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THE QUEST FOR THE PERFECT STUDY OR MY FIRST 1138 DAYS AT CNA

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AUTHOR'S NOTE

This paper is a first draft of what I had planned to say at a seminar with my colleagues at CNA. Most of the management staff heard me out in a first dry run on October 1 and 5, 1976. As a result of their suggestions, and of my own reactions from giving the paper, I had planned to make some revisions. In particular, I was not satisfied with the sections on standards and on analytical problems and pitfalls.

Of course, the press of other matters delayed these revisions. I did hold separate small seminars on the first half of the paper with the Marine Corps Operations Analysis Group, the Systems Evaluation Group, and the editors at CNA.

Now I have been called away from CNA and will not be able to make those revisions, or to hold the full-scale seminar. Not knowing quite what to do with the draft, but not wanting to abandon what is in some sense an account of three or four of the most intellectually stimulating years of my professional career, I am offering it with only minor revisions as a CNA Professional Paper.

I want to express my gratitude to those who bore with me -- and I hope were not bored by me -- during the dry run. I extend them my apologies for not having reacted to their suggestions. It is not that I did not welcome them, but simply that I have now missed the boat. Thus, the faults in this paper are mine, all mine.

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UNANNOUNCED

THE QUEST FOR THE PERFECT STUDY
OR
MY FIRST 1138 DAYS AT CNA

For a couple of years now, people have told me that I ought to hold a seminar on the CNA review process -- on what CNA expects from its analysts, and what it seems to be getting. That struck me as a good idea, and now I've finally gotten around to doing it.

The reason it struck me as a good idea is that we ought to be taking advantage of the review process as a way of learning from past mistakes. But we don't -- at least, not as much as we should.

The reason we don't is that you can't get copies of my review papers -- or, at least you're not supposed to. The only reviews you're supposed to see are those of your own studies. Even Group Directors aren't supposed to see my reviews of other groups' work.

That may be a dumb idea, but there's a reason for it. Early in my career here, I turned out a very unfavorable review of one study. The thing became a best-seller. Everybody and his brother wanted to see what I'd said about poor old Joe Blow. But some analysts took exception to that idea. They felt it was unfair to pillory the guy before his peers, particularly since he hadn't had a chance to defend himself.

I could argue that open criticism is a normal and important part of the research process. But it certainly hadn't been my intention to embarrass anyone, and after that experience, I changed the distribution rules so that the only people who can get copies of my reviews are Dave Kassing, the Group Director in question, and his Study Director. Any other copies require my OK, or the Group Director's OK.

That does have the advantage that I feel freer to tell it like it is without worrying so much about embarrassing people. But it obviously cuts down the opportunities for the rest of the staff to learn from their colleagues' efforts, and to build up a sense of what's expected of CNA reports.

So the point of this seminar is to mitigate that problem. What I propose to cover is this:

- THE CNA REVIEW PROCESS
- PRESENTATION
- STANDARDS
- ANALYTICAL PROBLEMS & PITFALLS

First I'd like to talk about the CNA review process itself -- its purpose, how it's organized, some ground rules, and such.

After that, I plan to talk about presentation -- organization, writing, graphics, and other matters, including a most important one, the role of your editor.

After that -- standards -- some "no-nos" and "yes-yesses" for all CNA reports.

And, finally, I'll talk about some general analytical problems and pitfalls I've run across in CNA studies.

Uninterrupted, covering all that would take me about two hours. But I don't want to do it uninterrupted. This is a seminar, not a lecture. I've always tried to make it clear that it's open season on my reviews; I expect my victims to fight back if they disagree with me. By the same token, I want to get your reactions to what I have to say now.

So I have no idea how long all this will take, but clearly longer than you want to sit still, and much longer than I can talk. So what I plan to do is go for about an hour today and see how far we get. If anybody's left at that point, we can take up where we left off at another session, and maybe even make it to the end.

In thinking about this seminar, it seemed to me that a lot of my points would be clearer if I cited examples, and more believable if those examples were from real CNA drafts. During my first 1138 days at CNA, I personally reviewed and wrote reviews on 27 major studies. I've been back through all those memos, and culled the examples I'm going to use. Now, some of you will probably be able to figure out which studies they came from, and maybe even whose work it is that I'm criticizing. But my purpose is not to embarrass anyone, and I hope you will concentrate on the issues rather than trying to guess who the authors were. In the words of my former boss, Alain Enthoven: "What's important is what's right, not who's right."

And let me say one other thing before you think I'm trying to pillory anybody, or before any of you are tempted to guffaw too loudly at your colleagues. Just as my quest for the perfect study is quixotic, there's just no such thing as the analyst who never goofs. If you drive a car long enough, you're going to dent a fender. If you invest long enough, someday you're going to pick a loser. If you talk long enough -- as I plan to do today -- you're going to say something you'll wish you hadn't. And if you make enough analyses, you're going to do some dumb things. We all do, and even if you can figure out who's responsible for some horrible example I may cite, remember that one swallow maketh not a summer -- the guy who made that goof may have done some pretty brilliant things too.

OK -- now to the CNA review process. Why do we have it? Why, to make sure that our work meets the standards of analytical quality expected of us by the University of Rochester, of course. What are those standards? I don't know. Fortunately, they didn't tell me when I was hired, so I feel free to guess. My guess is:

> THE QUALITY OF CNA STUDIES SHOULD BE AT LEAST AS HIGH AS THAT OF PH.D. DISSERTATIONS ACCEPTED BY THE UNIVERSITY OF ROCHESTER.

By that I do not mean to set any standards, one way or the other, for length, importance, trail-blazing, or analytical sophistication. But I do think that implies standards in terms of analytical rigor, documentation, reproducibility, and general competence in the field.

That's the "go/no-go" gauge. Beyond that -- to borrow a phrase -- I have a dream, and I like to think that the review process can help make it come true. That dream is for CNA to be recognized -- and recognized justly -- as the best organization of its kind in the world. That means better than the Beltway Bandits, of course, and it means better than RAND, and better than anything the Brits have, or the Russians -- if they really have any.

That's not an impossible dream. Some of you have been in this business longer than I have, but I've been in it for 22 years. I've seen it from inside the aerospace industry, I've seen it from inside the Systems Analysis shop in OSD during the heyday of the Whiz Kids, and I've seen it here. During that time, I've known an awful lot of analysts and, based on that experience, I think we have enough sheer talent, right here at CNA to be Number One.

I don't know how you really measure who's Number One. Perhaps we already are. But it's not clear that we are. On the other hand, if we can bring the quality of our work up from where it is now to where it should be -- given the quality of our people -- there won't be any doubt that we're Number One.

So much for the dream. How does the review process work? Partly, by holding "teach-ins" -- "pre-natals" at the beginning of studies, and others as the studies progress. Those are all oral briefings. They are part of the review process, and they serve their purpose. But they're no substitute for a careful, detailed review of the finished product. I know of no way to make sure that a report meets our standards without reading it and thinking about what it says. What I'm going to talk about here is the result of doing that 27 times.

As for the ground rules of the review process, they start with the requirement that any CNA Study (but not Research Contribution or Memorandum) has to come to the Office of the President for review before publication. It is sent to me by the Group Director, and he is held responsible for its quality -- whether he's reviewed it or not. Thus, any criticisms I may have -- so long as they're valid -- are held against the Group Director as well as the Study Director.

In that regard, I want to make it clear that I am not supposed to furnish a centralized review service for the 5 operating groups. The understanding is that no report is to be sent to me for review until the Group Director feels it's ready for publication in all respects -- and that includes final editing. All I'm supposed to do is make sure that nothing has slipped by him.

The problem with that, of course, is that Group Directors get pressed for time, and they seem to be tempted to send me some reports before they've really scrubbed them down. My counter to that is to send copies of every review to Dave Kassing — and he's the one who does the Group Directors' year-end reviews. The basic review is supposed to occur in the operating groups, not on the 12th floor.

Either Charlie Woods or I will then review the report, unless we're both already tied up. In that case, I will ask a member of the professional staff -- or occasionally an outside consultant -- to make an ad hoc review for me.

I resort to that only when doing anything else would delay publication too much. But I don't like to do it, for two reasons. First, there is the problem of

loading all that additional work on the ad hoc reviewer and giving him the problem of reporting to two people, only one of whom writes his year-end review. The other reason is that those reviews leave me with an uneasy feeling. There's no real way that I can tell how good the review is without doing a parallel review myself -- which would defeat the purpose of the ad hoc review in the first place. I can give it a quick look-through, but I either have to take most of it on faith or do it over myself. So I can't tell you how good those ad hoc reviews have been. I don't really know.

My dislike for ad hoc reviews was the reason we hired Charlie Woods to help me on a full-time basis. Charlie had read all my reviews while he was here as the DNM, so he was familiar with the process. When he turned into a reviewer, he and I did independent parallel reviews until I was confident he was doing a first-rate job. His arrival has greatly reduced the need for ad hoc reviews, but there will still be some. If any of you would like to take on an ad hoc review, let me know, and I'll be glad to put you on my list of volunteers. I think review is the best job in the place. Try it -- you'll like it.

Next is the matter of how long these reviews take. The record is nothing I'm proud of; 27 studies in 1138 days is an average of 42 calendar days each. Some have taken a lot longer. The record was the review of one draft report of 506 double-spaced pages. It took me 114 calendar days. I ended up with a series of review memos on the various sections of the report that totaled 62 single-spaced pages. That was about 1/4 the length of the report itself, and even longer than some of the reports I get to review.

Was all that necessary? I don't know. That's a fundamental problem in the review process, and I don't know what the answer is. I just don't know how carefully a report should be reviewed. Any fool can tell you it's possible to spend too much time on a review, or too little. The way I judge how long to spend is to work the report over until I'm satisfied that I've caught all the problems I'm likely to catch, and that the rest is OK. But that's just a matter of judgment, and there's never any guarantee that you're not letting major goofs get by. Sometimes it will take me a month to notice a major goof that I never even suspected up to then.

Of course, there are practical limits to what a reviewer can hope to take on. I suppose you could say that much of my review work consists of trying to reproduce the analysis -- not all of it, of course, but enough examples so that I can understand the methodology and confirm the calculations. But what happens when the analysis is based on a complex computer calculation? Should I review the computer program? Usually that's not feasible. In those cases, sometimes I'll talk to the analyst and see what he's done to convince himself that the program is doing what he thinks it is. Or I can ask for some trial runs of parts of the program that can be checked for reasonableness. But big computer programs make this reviewer nervous.

Sometimes it's not just computer programs that are hard to check. One study I reviewed used a very large expected-value model, the various events in the model being defined by 29 separate equations -- all properly spelled out in an appendix. The only way to check those was to re-derive them independently. That would have taken a lot of time. What I did was to talk to the analyst.

and he convinced me he had run enough reasonableness checks to catch any major goofs. So I didn't try to derive them independently. I assume they're right, but I still don't know. Perhaps I should have further delayed that study by taking the time to derive those equations, but it so happens that this particular study was the record one I mentioned earlier -- the one that ended up taking 114 days in review -- even without that extra checking I perhaps should have done.

So, how long and detailed the review should be is an enigma to me, and generally a frustration to the Group Directors because I err on the side of length. One possible way to speed things up is to review working papers before the study is published. We do try to do that when we get a breathing space, but that doesn't happen often, and you always run the risk that what you've reviewed will be changed before the study is published.

Another reason for the relatively long review time is that I commit all my comments to writing. That takes time, but I think it's the only way to do it. First, it forces a discipline on me. Putting it down on paper makes me think more carefully about what I'm saying. It's like writing up a study. I'm sure you've run across the same phenomenon I used to, back when I was doing studies myself, instead of just kibitzing. You think you're all finished, but when you try to put it down on paper, you find out you can't explain it clearly. Then it dawns on you that your analysis still has a hole in it. The test of being able to get it down clearly on a piece of paper is a good one.

The second reason for writing it down is to make sure the Group Director and his study team understand what the problems are. That gives them something specific to work from -- either to refute what I say or to repair the study. That refutation is important. I don't want any analyst -- no matter how junior -- to blindly accept what I say. Like anybody else, I make mistakes. And I'm not Superanalyst. I doubt that there's a single area in which some -- or even many -- analysts in CNA don't know a lot more than I'll ever know. I'm not a acoustician, or a statistician, or a mathematician. I try to get help from such experts, but I'm really just a guy who's been given the responsibility for review. My only advantage over you is that I get to see a lot more analyses per year than you are likely to. So if you get a review from me that you think is nuts, tell me. The idea is to deliver good work to our sponsors -- not to cater to my ignorance.

In writing these reviews, when we can, we try to split the memo into three parts. The first part collects all the issues of fundamental importance -- issues that could affect the results in a major way. Following that come all the other issues that are more than just trivia. The trivia are in the third section, an appendix: errors in addition that don't affect the answers, calling things by the wrong name, and so on -- things that ought to be fixed, but don't cause significant problems. By splitting things up that way, which isn't always easy, we try to draw attention to the key points instead of hoping the Group Director will pick them out of the hash.

After we discuss each problem, we also try, as much as we can, to suggest what ought to be done. The idea is not to tear things apart but to suggest how to fix what needs to be fixed. There have been a couple of exceptions to that

in the past, when a study was so bad that I judged it to be beyond reasonable repair. My memo then didn't try to suggest improvements; it just told the Group Director why I thought his work was hopelessly wide of the mark. But, fortunately, that doesn't happen often, and Charlie and I do try to be constructive rather than destructive in our comments.

There is one other thing you should understand about these review memos. Those of you who've gotten them in the past already know this; the others should be warned. The memos tend to be negative in tone; they concentrate on what's wrong with the draft report rather than what's right about it. Perhaps that's because it takes so much space to talk about what I think is wrong, why I think that, and how I think it might be improved, that I hesitate to take any more space talking about what's already fine as it stands. Or perhaps it's just because I'm heartless. I was struck, in re-reading those 27 memos, by how seldom I ever said really complimentary things.

Perhaps I'll remember that in the future, and try to be a bit less cold-blooded, but if I don't, and you get what seems to be a blast from me, something you interpret as a slur on your analytical ability and intelligence, and you think I don't understand that it's much harder to turn out good work than to criticize -- don't be too upset. The general lack of kudos doesn't mean anything. If I really do mean to tell you that I think your work stinks, you won't have to do any reading between the lines to figure that out.

After the review is finished, I send it to the Group Director and then discuss it with him and the study team if they don't agree with what I've said, or if there are questions about what to do about revisions. I generally take a quick look through the revised report, but I must admit that those looks have been more of a lick-and-a-promise than any really careful re-checking. The reason is that I'm busy enough with original reviews, and I don't want to get into the business of following Group Directors around to make sure they're not cheating. The process certainly is vulnerable to the Group Director who wants to beat the system, but I wouldn't want to work at CNA if I didn't respect the integrity of my colleagues.

So much for the process itself:

- Every study gets a formal review.
- We're not a central review service -- Group Directors are supposed to submit studies ready for publication.
- We sometimes resort to ad hoc reviews.
- Reviews tend to take a long time.
- Comments are written:
  - They concentrate on what's wrong.
  - They're supposed to be constructive.

• PRESENTATION

Now I'd like to turn to the subject of presentation, by which I mean everything to do with getting your message to the people we work for. I'm going to start by saying something obvious:

> THE BEST ANALYSIS IN THE WORLD IS ABSOLUTELY WORTHLESS IF THE READER DOESN'T UNDERSTAND IT, OR MISUNDERSTANDS IT, OR DOESN'T BELIEVE IT, OR DOESN'T READ IT.

If you think that analysis is the only game in town, and that presentation is just so much housekeeping you have to go through at the end, you're wrong.

If you think that excellence in presentation is any easier or any less important than excellence in analysis, you're wrong.

If you think your professional development is complete when you can correctly analyze any problem, but you can't write with clarity and elegance, and you don't understand what can and should be done with tables and graphs, and you don't know how to organize a report so that your reader follows you like a dog follows a bitch in heat, you're wrong.

And if you think I feel free to stand up here and make all those flat statements because I've mastered all those problems myself, you're wrong.

Some CNA reports are well presented. But too many aren't. Why? Perhaps one reason is that few CNA analysts have ever been on the receiving end. They've never been in the position of sitting behind a Pentagon desk with an "IN" box stacked a foot high with reports as thick as telephone books and as hard to fathom as Gorshkov's "Sea Power of the State" in the original Cyrillic, and a boss who wants an answer now. Try that sometime,and you'll get a whole new perspective on the importance of presentation.

Another reason for poor presentation may well be that we just don't work anywhere near as hard on developing our presentation skills as we do our analytical skills.

I've been on the PDP Committee for about a year and a half, but I've yet to see so much as a single application from an analyst who wants to take a course in how to write. Everybody wants to go study advanced statistics, or physics, or econometrics; nobody wants to learn how to use the English language. Yet a lot of the writing that crosses my desk is so bad that I don't know whether to laugh or cry.

I've talked to lots of editors about this. They tell me that the people who already write well are the ones most open to editorial suggestions. The ones who resist and resent that advice most are precisely the ones who need it most. What have you done lately to improve your writing skills? Do you try to learn anything when an editor takes the time and trouble to blue-pencil your draft? Have you read the copy of Strunk and White we gave you? Have you read the copy of "Gobbledygook Has Gotta Go" that we gave you? Have you tried the "Write Formula" from Page 7 of that book on any of your own reports? Try it. You may not like it, but it's good for you -- like grits.

And, speaking of editors, I have two points -- one about editors helping analysts, the other about analysts helping edirors. Don't let your pride of authorship get in the way of turning out a well presented report. Let your editor help. Our editors were hired as experts in their field, just as you were. That being the case, if I were king, I'd make a rule. That rule would say that in matters of presentation -- and particularly regarding the use of the English language -- the editor's word is final because there's no point in hiring experts if we ignore their advice. But there would be two exceptions to that rule.

If the analyst can show that the editor's version would change the intended meaning, then the editor and the analyst have to try again. But the analyst shouldn't feel too smug -- the editor wouldn't have misinterpreted the draft if the analyst had written it clearly in the first place.

The second exception would be when the analyst can show that his original version is clearly better-written than the editor's version. If that happens too often, the editor should be fired.

The other point is about analysts helping editors. That has to do with giving the editor a draft that at least meets minimum standards of organization and clarity. Don't expect editors to make silk purses out of sows' ears. Editors shouldn't have to spend hours and hours puzzling over what you're trying to say, and probably getting it wrong in the process. We can't afford enough editors to handle that much extra work, and it shouldn't be necessary in the first place. So try to help the editors. Work on your own writing ability. Learn something every time they polish your work. Read those little books we handed out. Use the "Write Formula", and keep at it. Don't settle for scores much lower than 70.

Beyond helping the editors, there are other reasons for working on your writing. It's a challenge, and it's a great satisfaction to turn out a good piece of writing. But knowing how to write well is also an invaluable analytical tool. Once you've established high standards for your own writing, if you find that you still can't explain a piece of work clearly and completely, you'll know that your work's not finished, and that you need to go back and fill in the holes in your argument. But if you don't take pride in your own clarity of exposition and you expect the editor to make it all sound convincing, you may never notice all the holes in your own argument. So learn to write clearly -- not just for the editor's sake, but for your own.

I'd like to show you some examples of the kind of writing that crosses my desk. First, a short one:

"EXPANDED USE OF VLF AND SPACE BORNE SYSTEMS ARE
ALSO UNDER EXTENSIVE EXPANSION"

That kind of treatment of the English language gives me the distinct impression that sometimes I get reports no editor has ever seen. How about this?

WE JUDGE THAT 5 GROUPS WOULD STRIKE AT THE SAME TIME FROM DIFFERENT DIRECTIONS, STARTING AT 300 N.MI., AND LAUNCHING MISSILES FROM 180 N.MI.

What's wrong with that, you ask? Well, what's wrong with it is that it's not what I got. What I got was this:

"OPERATIONALLY, AN ATTACKING FORCE WOULD VERY PROBABLY BE BROKEN UP INTO VARIOUSLY TIMED GROUPS, EACH FOLLOWING VARYING HEADINGS ENROUTE TO THE TARGET AREA; BUT WITH TIMING AND MANEUVERING SUCH THAT THE 5 ATTACKING GROUPS WOULD ARRIVE AT THEIR RESPECTIVE AXES SIMULTANEOUSLY AT RANGES OF ABOUT 300 N.MI. FROM THE TASK FORCE CENTER (TFC); AND PROCEED TOWARD THE TFC, LAUNCHING THEIR MISSILES APPROXIMATELY SIMULTANEOUSLY AT A RANGE OF 180 N.MI."

The two semi-colons are incorrect -- they should be commas -- which makes this a single 71-word sentence. The "Write Formula" score is 54. The "Write Formula" score of what you see in the first version is 75, it's gotten rid of more than 60% of the words, and I think it says the same thing. The first version tells your busy reader what he needs to know; the second makes him struggle with a lot of gobbledygook.

Here's another example, this time with the original first.

"SINCE THE 1950s, WHEN IT BECAME APPARENT THAT THE MAJOR OIL-CONSUMING COUNTRIES, PARTICULARLY JAPAN AND WESTERN EUROPE, WERE GOING TO BECOME INCREASINGLY DEPENDENT ON IMPORTED OIL, ONE APPROACH SUGGESTED TO MINIMIZE THE POSSIBLE IMPACT OF SUPPLY INTERRUPTIONS WAS THAT OF GEOGRAPHIC DIVERSIFICATION OF SUPPLY SOURCES. BY DIVERSIFYING SOURCES, THE IMPORTING COUNTRIES WOULD BE ABLE TO ABSORB MORE EASILY A SUPPLY DENIAL FROM ANY POLITICALLY VOLATILE SOURCE. THE FACT MOST DAMAGING TO THIS POLICY OF DIVERSIFICATION IS THAT THE GEOGRAPHICAL CIRCUMSTANCES FAVORING THE EXISTENCE OF PETROLEUM FORMATIONS IS (sic) FAR FROM UBIQUITOUS. IT IS, THEREFORE, THE SHIFT IN THE CENTER OF GRAVITY OF WORLD INDUSTRY OPERATIONS MADE POSSIBLE BY THE SPECTACULAR DEVELOPMENT OF OIL SOURCES IN THE MIDDLE EAST WHICH HAS RESULTED IN THE CURRENT AND PROJECTED CONFIGURATION OF OIL SUPPLY AND DEMAND PATTERNS. ONLY BY MAINTAINING A RELATIVELY LOW DEMAND FOR IMPORTED OIL WILL INDIVIDUAL NATIONS BE SPARED THE CRUCIAL STRATEGIC DEPENDENCE ON THE MIDDLE EAST FORECAST FOR SUCH AREAS AS WESTERN EUROPE AND JAPAN."

It's not that that's so opaque that you can't understand it. It's a bit high-falutin' with its "geographic diversification", and its "geographical circumstances favoring the existence of petroleum formations", and its "configuration of oil supply and demand patterns". But the real problem is that it takes so long to get through what it's trying to say that the poor reader is likely to miss the point altogether. Here's an alternative:

MOST OF THE OIL AVAILABLE FOR EXPORT LIES IN THE MIDDLE EAST. SINCE THE NATIONS OF WESTERN EUROPE AND JAPAN MUST IMPORT OIL, THEY HAVE NOW BECOME VULNERABLE TO MIDDLE EAST POLITICS.

You're right -- it doesn't tell you everything the first version did. Frinstance, it doesn't say that people have been thinking about the problem since the '50s. It doesn't say that development of Middle East oil has been "spectacular". And it doesn't specifically say that you could avoid the problem if you limited oil demand. But so what? While some people may find all that very interesting and learned, it was totally irrelevant to the study. By using the shorter version, you cut 4 out of every 5 words -- thus saving your busy readers some of their valuable time -- and boost the "Write Formula" test score from a dismal 50 up to a satisfactory 71, thus making it easier for your busy readers to understand what you're trying to say.

In addition to that kind of writing problem, there's also the problem of banalities:

"THE STATE OF THE ART IN THESE AREAS (SIZE, HULL MATERIAL, SPEED. DEPTH, ETC.) NATURALLY IMPOSES BOUNDARY CONDITIONS ON THE DESIGN OF FUTURE SUBMARINES."

"TO EXAMINE THE ROLES THAT SUBMARINES CAN PLAY IN THE FUTURE U.S. NAVY. IT IS NECESSARY TO UNDERSTAND THE ROLES THAT THE U.S. NAVY WILL ASSUME IN THE SAME TIME FRAME AS A PART OF THE ENTIRE DEFENSE FORCE OF THE UNITED STATES."

"THE NUMBER OF FUTURE NAVAL MISSIONS DEVELOPED IN CHAPTER IV THAT ARE EXPECTED TO BE PERFORMED WITH A SUBMARINE PLATFORM DEPENDS ON THE OPERATIONAL ENVIRONMENT, THREAT, STATE OF TECHNOLOGY, ETC., OF THE FUTURE."

"IN THE 1980-1990 PERIOD, NEW AND IMPROVED ELECTROMAGNETIC AND ACOUSTIC COMMUNICATIONS LINKS WILL HAVE EVOLVED FROM CURRENT RESEARCH AND DEVELOPMENT EFFORTS."

"MOREOVER, CONSIDERATION SHOULD BE GIVEN TO ESTABLISHING COMMUNICATIONS REQUIREMENTS WHICH ARE CONSISTENT WITH BOTH FUTURE MISSION OBJECTIVES AND SUBMARINE COMMUNICATIONS TECHNOLOGICAL CAPABILITY."

Who could argue with any of that? But who's going to stay awake long enough to plow through it? What's likely to be the reaction of the busy reader to CNA's forcing all those platitudes on him?

Our reports are far more effective and useful when they're short, punchy, and clear. Not so short that they're cryptic, but not so long that they're soporific. Three obvious banalities are as effective as five milligrams of Nytol.

There's another kind of problem we run into in our writing-- tailoring our writing to our audience. It's very tough to draw the line in some cases. Clearly, we should draw the line at naval illiteracy:

NAVAL ILLITERACIES

"THESE (SONAR) DETECTION REGIONS WERE PLOTTED ON A MAP."

"KN./HR."

"A SHIP OF 20,000 TONS GROSS WEIGHT"

Generals use maps; admirals use charts. "Knots per hour" is a unit of acceleration equivalent to about a sixty-nine-thousandth of a g -- not a unit of speed. (Incidentally, the abbreviation for "knot(s)" is "kt".) And "gross weight" is a term that applies to aircraft or possibly fat people; the term for ships is "displacement".

It's easy to draw the line in these cases, but the line eventually gets fuzzy. Some terms, such as ICBM, are so common that most reasonably well-informed laymen understand them. Almost all of our audience probably understands what "CVA" means, or "SSBN", or "JCS". Of course, you don't have to explain them. But when you get to "MFCS", you'd better think about explaining that. Charlie Woods ran into "MGDAT" in a CNA report, and I'm absolutely certain you'd better explain anything as esoteric as that. I can offer no general rules here except to use common sense. Don't fill your report with tedious definitions of terms that every one of your readers already understands, but even worse is to toss around a lot of unfamiliar and undefined terms. And try not to be pedantic:

LIMACON

How many of you recognize that word? If any of you do, how many Naval officers in the Pentagon would you expect to recognize it?

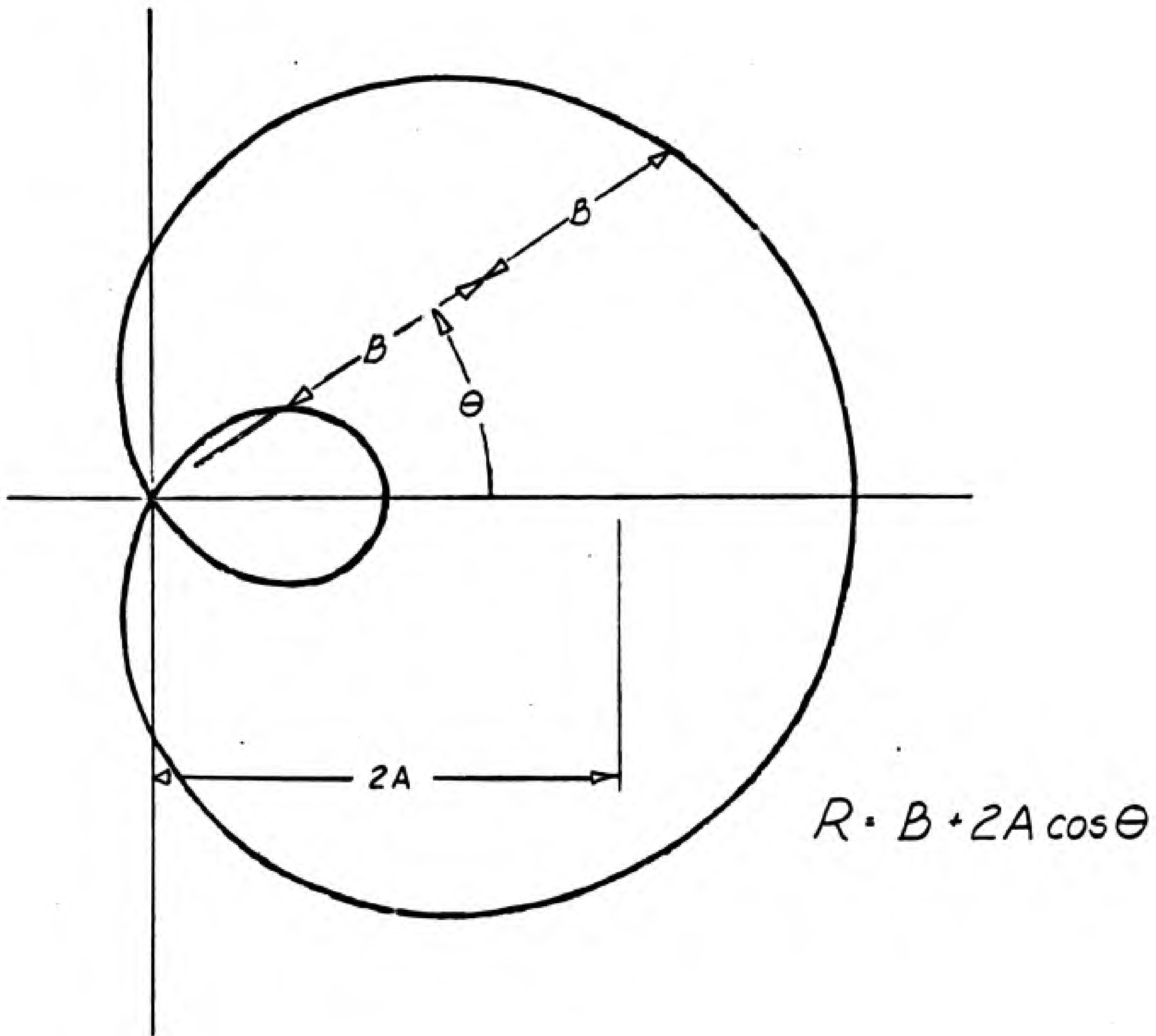
Well, I ran across that word in a CNA report, and I didn't know what it meant. Judging by the context, it referred to some sort of a curve. It wasn't clear whether it referred to the shape of a polar plot of hydrophone gain versus aspect angle, or whether it referred to the shape of the hydrophone itself.

I've got two dictionaries in my office: the 1975 revision of the Random House Collegiate and Webster's Seventh New Collegiate. There are no limacons there. The closest you can get is limacine -- which means "pertaining to or resembling a slug" -- which gardeners think is a thing like a snail without a shell, but which we engineers know is a mass that weighs about 32 pounds at the Earth's surface. But that's no help, so I went to the big, 2700-page Webster's Unabridged and tracked down the elusive limacon. It turned out to be not a limacon, but a limaçon, with a cedilla under the "c", and this is Webster's definition:

> "....A PLANE CURVE CONSISTING OF THE COLLECTION OF POINTS OBTAINED BY TAKING A FIXED DISTANCE IN BOTH DIRECTIONS ALONG A HALF LINE FROM A FIXED POINT ON A CIRCLE MEASURED FROM ITS SECOND INTERSECTION WITH THE CIRCLE."

Clear enough? No. Anybody who can visualize what in hell that means in less than five minutes gets a gold star. And how many Naval officers, given that they don't know what a limaçon is, but do care, and have easy access to the fat version of Webster's, will keep trying at this point?

I tried Burrington's next, and then Hudson's. No soap. But it is in Eshbach -- with the mathematical expression and a pretty picture. So that you'll all know what a limaçon is, here's roughly what it looks like:



So the thing turned out to be the shape of the polar plot of hydrophone gain vs. aspect angle. That's an illustration of making unnecessary work for the reader. There was nothing wrong with using the term "limaçon" -- that, I presume, was a precise definition of the curve the text was talking about. The problem was in recognizing the likely limits of the audience, and just explaining what a limaçon is. I think that verges on pedantry, a nice definition of which is in Margaret Nicholson's "Dictionary of American-English Usage", slightly modified here:

> "PEDANTRY MAY BE DEFINED...AS THE SAYING OF THINGS IN LANGUAGE SO LEARNED OR SO DEMONSTRATIVELY ACCURATE AS TO IMPLY A SLUR ON (NAVAL OFFICERS) WHO ARE NOT CAPABLE OF OR NOT DESIROUS OF SUCH DISPLAYS."

She goes on to make an important point about judging exactly what's pedantic and what's not.

> "THE TERM (PEDANTRY), THEN, IS OBVIOUSLY A RELATIVE ONE;MY PEDANTRY IS YOUR SCHOLARSHIP, HIS REASONABLE ACCURACY, HER IRREDUCIBLE MINIMUM OF EDUCATION, AND SOMEONE ELSE'S IGNORANCE..."

The best I can say here is to urge that you avoid the temptation to appear learned. It may be fun to demonstrate your erudition, to trot out your favorite little-known term, but remember that you're liable to pay a price for that. Some Naval officers won't even notice, and a few may be awed, but the rest will get mad at what they take to be a snow job. That's likely to reflect poorly on CNA, and it's almost certain to interfere with getting your message across. I don't mean that you should write like you're trying to get through to a bunch of apes. But ask yourself whether you're trying harder to demonstrate than to communicate.

So how's this?

> "ALTHOUGH THE SIDE-LOOKING RADAR IS SUPPOSED TO GIVE COURSE AND SPEED OF THE CONTACT AS WELL AS ITS DIMENSIONS, THE SHIP'S ASPECT RELATIVE TO THE RADAR CAN FOUL THIS UP."

I'm not making this up. That's from the draft of a formal CNA study report submitted to me as being ready for publication. Don't be pedantic and stuffy, but don't be sassy, either, especially with such terms as "foul up," which can be interpreted as euphemisms for obscenities.

That's all I'm going to say about writing at this point, though -- as you may have guessed -- it's a subject near and dear to me. I don't mean to imply that nobody at CNA writes well. Some do, but lots don't, and it's an area where CNA can make big improvements.

Next, under the general subject of presentation -- the question of how a CNA report should be organized. I think it's helpful to view our readership as being divided into three levels:

ADMIRALS

ADMIRAL'S STAFF

ANALYTICAL SPECIALISTS

At the top level are the senior admirals or Marine Corps generals. These are the people closest to the decisions, and they're the guys we're really trying to reach. But they have their limits -- almost always limits on the amount of time they have to read reports, and sometimes limits, too, on their analytical background. So they usually pass the report on to their staff and ask them to check it out and report back.

That's the second level. There's likely to be more time available on that level -- when the admiral says "read it for me", it gets read. So they'll plow through the thing to see how we came up with our answers and then tell the admiral whether they think that makes sense. But sometimes they'll worry about some of the analysis, and then they'll pass it along to the third level -- what I've called the "analytical specialists". These are the guys who presumably understand analysis reasonably well, and they may want to try reproducing our work to see if they get the same answers.

I recognize that that's sort of an idealized view of our readership, and that things don't always work that way. But I think it's at least plausible, and I think the organization of our reports should be tailored to it, like this:

ADMIRALS	SUMMARY
ADMIRAL'S STAFF	MAIN BODY OF TEXT
ANALYTICAL SPECIALISTS	APPENDICES

Aim your summary at the admiral. Aim the body of your text at his staff. Aim your appendices at the working-level analysts.

What should the summary say? It should describe what it is that you've analyzed, and it should cover any caveats or limitations that you want the admiral to understand -- such as important assumptions. Of course, it should cover your results. But, at least as important, it should explain <u>why</u> the results turn out the way they do. Just knowing what the answer <u>is</u> is a sort of bare minimum. Knowing what <u>drives</u> the answer is important if you expect to persuade the admiral that your answer's sensible, if you expect to give him some real insight into the problem, and if you expect him to use your results intelligently.

To cite a simple example, suppose you've compared a couple of different versions of an airborne early warning aircraft, and your report says "buy Version B". If you've got a really tame admiral, maybe he will, but most wouldn't. But if you say "Buy Version B <u>because</u> it's got twice the radar range, which gives it four times the area coverage, so you'll only have to buy a quarter as many aircraft, and that more than makes up for its slightly higher unit cost" -- maybe you'll get through to him.

Incidentally, the importance of understanding not just the "whats", but also the "whys" is one of the reasons that many people -- myself included -- are leery of big computer analyses. They're liable to give you a lot more answers than understanding. All kinds of subtle, but powerful -- and sometimes nutty -- things can happen inside the black box without your knowing it, and sometimes it's very hard to be sure you understand why the answers turn out as they do unless you do an awful lot of dissection.

OK -- that's what ought to go into the summary. What shouldn't? One thing that shouldn't is methodological detail. Another is any attempt to rigorously defend your analysis. All that kind of thing belongs in the body of the report and the appendices. This section is written for the admiral, and he's not going to audit you for stupidity, incompetence, or fraud. That's what his staff is for. You've got three things to worry about:

- HOW DO I LURE THE ADMIRAL INTO READING IT IN THE FIRST PLACE?
- HOW MUCH SHOULD I TRY TO COVER?
- HOW DO I GET HIM TO REMEMBER WHAT'S IN IT?

There are several things you can do to get the admiral to read your summary. One is a sort of psychological ploy: put the summary in a separate volume. Obviously, that gets silly when the report's only 25 pages long to begin with, but it makes a lot of sense when it's a thick report. Looking at a 300-pager sitting in your IN box is enough to discourage the best of us. The admiral is likely to skip Step 1 -- reading the summary -- and go directly to Step 2 -- giving it to his staff. But if we pull out a skinny little 5- or 10-page summary and drop that in his box, maybe he'll pick it up and read it.

And that leads to the second thing you can do: keep it short. There's probably some threshold length for any given admiral on any given day. If you exceed that length, he won't read even a separate summary volume; if you're below that threshold, he will. It's impossible to know where that threshold will be; err on the side of brevity.

So, if you stick it in a separate volume and keep it short, maybe he'll start reading it. But that's not enough. To get him to finish it, instead of giving up part way through, make sure you write well. No gobbledygook. Easy to follow. Interesting. It's very hard to do all that. But if you're successful, perhaps you'll meet the first of the three challenges.

Then you have to decide how much to cover. Here's one way of looking at the problem. There you are, about to write up the results of a year's work. You know the analysis like the back of your hand. But six months from now, some of the details will have begun to fade and get fuzzy in your mind. That process will continue until, say, five years from now, you'll probably have forgotten just about everything but the most central points. That's what you should put in the summary section. Try to envision the likely decay in your own recollection of the study until you figure out what's left just before the whole screen goes blank, and concentrate your summary on that.

There's another way of looking at it that I think would make sense to a newspaper reporter. Most newspaper articles are written so that the guts of the story are in the first paragraph, with succeeding paragraphs getting into more and more detail. There are two reasons for that -- first, so that the guy who's in a hurry doesn't have to wade through introductions, digressions, and details to understand at least the basics of the story, while the guy who's interested and has the time can read until he's seen as much as he's interested in. The other reason is that when the reporter writes the story this way, the editor can just cut it off at any paragraph -- depending on how he decides to allocate the space in the paper -- and everything before that point will still stand on its own and make sense.

As a frinstance, here's a 29-paragraph lead story in the Times on the day I happened to be drafting this paper. First paragraph:

PARAGRAPH 1:

"LEADERS OF THE UNITED AUTOMOBILE WORKERS CALLED A STRIKE TODAY AGAINST THE FORD MOTOR COMPANY, STARTING AT MIDNIGHT."

That's the central fact of the story. Here's the 12th paragraph:

PARAGRAPH 12:

"FORD OFFICIALS SAID THAT ABOUT 150,000 OF THE 1977 MODELS WERE TO HAVE BEEN PRODUCED BY THE STRIKE DEADLINE. THAT WORKS OUT TO ABOUT 21 CARS FOR EACH OF THE NATION'S 7000 FORD DEALERS."

Much more detailed. Here's the last paragraph -- or at least the last one before the editor cut it off:

PARAGRAPH 29:

"FORD WANTED TO REDUCE THE ESCALATING COST OF THE WORKERS' HEALTH-INSURANCE PROGRAMS, FOR WHICH IT PAYS THE PREMIUMS."

Again, more detailed and, apparently, right at the threshold of what the Times considers fit to print, or possibly what the Times could print to fit the space available.

So another thing you can do when you're trying to figure out what to cover in your summary is to think how you'd write up the story if you were a newspaper reporter. Then put what would be the first paragraph or so into your summary.

- HOW DO I GET THE ADMIRAL TO REMEMBER WHAT'S IN THE SUMMARY?

OK, now you've done what you can to get the admiral to read the summary, and you've decided what to cover. How do you get him to remember what's in it? One thing you can do is to try to make your points through statements that are simple, short, clear, and meaty. Here's a statement from the summary of a draft report on a CNA wargame -- which I've disguised a bit:

"AS EXPECTED, THE (DINGBATS) PROVED TO
BE QUITE VULNERABLE TO AIR ATTACKS."

Well, that's short, but it's not too meaty. Just what does "quite vulnerable" mean? And is that supposed to be a bit of incidental information, or is it likely to be a real problem? Actually, according to the results in the body of the report, the summary could have said this:

"HALF THE DINGBAT LOSSES AND ALL THE NON-FATAL DAMAGE
WERE CAUSED BY ENEMY AIRCRAFT."

That's not much longer, but it's a lot punchier. It hits the admiral right between the eyes with the idea that aircraft are likely to be a major problem for the Dingbat, and it gives it to him in a simple, quantitative way. I think he'd be more interested in that second version, and more likely to remember it.

There are other things you can do to make it easier for him to remember. One has to do with tablesmanship -- and I'll talk more about that later. But here's an example from the summary section of the report covering Atlantis I and II. (I can't disguise this one without obscuring the point. But I certainly don't mean to throw rocks at that particular summary, which was perhaps the best I've seen yet.)

Atlantis I looked at the use of expanded convoys, and Atlantis II looked at the use of a protected lane across the Atlantic, as alternative ways of helping to get cargoes to Europe during a NATO war. It's the protected lane version I'll talk about now. The spectacular and controversial part of that was a series of large concrete platforms -- very much like the oil drilling platforms they build in Norway now -- spotted all the way across the Atlantic. These were to be used like a series of very hard ASW carriers, complete with maintenance facilities. There were a lot of other novel concepts, too -- surface-effect ships, decoys, helo platforms on merchant ships, Captors, moored buoy fields, and such. But the things that caught everybody's eye were those big concrete platforms.

The summary volume of the report had a fairly conventional table, showing the 10-year system costs for all the pieces -- and there were a lot of them. I thought it might be better to have a different kind of cost table -- much simplified, showing only major aggregations of categories, arranged in decreasing order of cost. Here it is -- but with the numbers -- though not their relative size -- changed to keep this unclassified:

10-YEAR SYSTEM COSTS			
SHIPS	$ 6755	ZILLION	(54.7%)
AIRCRAFT	4163		(33.7%)
CONCRETE PLATFORMS	460		(3.7%)
BUOYS	407		(3.3%)
CAPTOR BARRIERS	303		(2.5%)
MERSHIP HELO PLATFORMS	160		(1.3%)
DECOYS	97		(.8%)
TOTAL	$12345	ZILLION	(100.0%)

Well, what does that get you? First, it shows clearly where the lion's share of the money goes -- to the aircraft and, especially, to the ships. You can see that they account for 7 or 8 times as much as all the rest put together. And, second, you can see that in spite of their notoriety, the big, spectacular concrete platforms account for less than 4% of the total.

Of course, that kind of table alone isn't enough; you also need to highlight the key points by a series of short statements -- as I've just tried to. I think that this display is fairly easy to grasp and retain.

Of course, this kind of table is vulnerable to distortion -- unscrupulous analysts can seem to prove different things by choosing their categories carefully. As a matter of fact, in this particular case, I probably should have broken out the "ships" category into conventional ships and surface-effect ships to make it clear that it was the surface-effect ships that cost so much. The way I've shown it here may still be a bit opaque.

Another thing to watch out for in organizing your report is redundancy. For example, you shouldn't try to build a summary by lifting statements from the main body of the text and pasting them together in the front of the report. There are a couple of reasons for that. One is that it probably won't work. The summary is supposed to be very carefully drafted, and written with the greatest economy you know how. A pastiche of odd statements culled from here and there in the body of the report is unlikely to come together into anything like a tightly knit summary.

Another reason is primarily cosmetic. The admiral, even if you can get him to read the summary, probably won't read the main text, but his staff man will read both sections, and having to plow the same ground twice is likely to make him impatient. Frinstance, here are some quotes from a single draft report:

> "COMPARISONS ARE MADE ...OF THE USE OF...SENSOR SYSTEMS IN ...MID-OCEAN DETECTION OF ENEMY SURFACE SHIPS..."
>
> "THE STUDY ASSESSES...SENSOR...SYSTEMS...FOR...TACTICAL AND STRATEGIC DETECTION OF ENEMY SURFACE SHIPS IN MID-OCEAN AREAS..."
>
> "FOR SURVEILLANCE OF REMOTE OCEAN AREAS...SENSORS ARE MORE ECONOMICAL..."
>
> "SENSOR-AUGMENTED SYSTEMS OFFER COST ADVANTAGES IN ...SURVEILLANCE OF MID-OCEAN AREAS..."
>
> "THE STUDY ASSESSES THE RELATIVE COSTS OF SENSOR...SYSTEMS...FOR MID-OCEAN DETECTION OF ENEMY SURFACE SHIPS..."
>
> "THE ANALYSIS OF SENSORS IN MID-OCEAN SURVEILLANCE COMPARED THE COSTS OF (OTHER) SYSTEMS WITH THE COSTS OF SENSORS FOR...SURVEILLANCE OF REMOTE OCEAN AREAS..."

Those quotes don't all say exactly the same thing, but there is a certain monotonous tone about them. They lead to a strong sense of deja vu. So, at least for cosmetic reasons, try to avoid being redundant. Also try to avoid it as a way of cutting down the size of the report. Don't feel that every section -- the summary, the main body, and the appendices -- has to stand entirely on its own. I think it's safe to assume that anybody who reads the main text will have read the summary first, and that anybody who reads the appendices will have read the main text first. That should save your having to repeat a lot of things in the various sections.

I've got one thing left to cover under "organization": avoid the chronological format. Sometimes people write up their analysis almost like a diary -- first I did this, and then I did that, and so on and on, until you get to the end. That may be interesting to the author, but it's rarely the most economical or most easily understood way of getting your message across. The reader's interested in what you found out -- not the history of your life at CNA. Don't use the chronological format.

Instead, when you've gotten through the analysis -- or at least when you think you have because your attempts to write it up haven't shown you the holes yet -- put down your pencil and think about your whole analysis without worrying about when you finished the various parts. Try to find some order in which you can introduce and explain the various pieces so that A is the logical starting place, and B follows easily from that, and then C comes naturally after that, and so on. Always keep in mind the difference between you -- steeped in all the gory details of your own work -- and the customer -- to whom this will all be new.

A movie director doesn't just splice together all the scenes in the order they were filmed and expect the audience to figure out the story. Don't you do that, either.

Next under presentation, I'd like to move on to the subject of graphics. I'll cover a lot of small but important points -- quickly, I hope. First, on tables, don't make them hard to read, like this one (which is modeled after one in a CNA report):

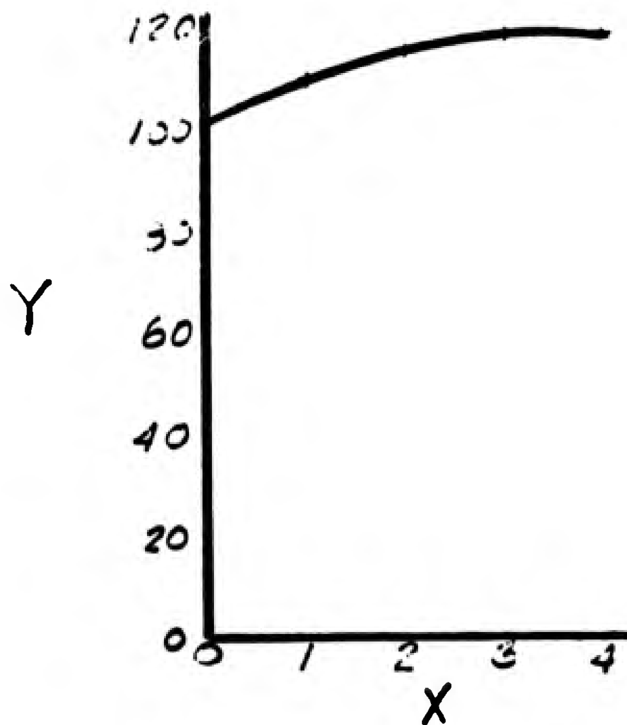
	FY 73	FY 74	FY 75	FY 76	FY 77	FY 78	FY 79	FY 80
RADAR JAMMERS	4	13	3		5	21		
WEB BELTS				105		19		
AIR-CONDITIONED TRACTORS			1	2	1			
TURBOGENERATORS		41					16	
GALVANIZED ANCHOR CHAINS	7						17	
COMMAND-ACTIVATED SONOBUOYS			31	31				
PELICAN HOOKS								
TURBOENCABULATORS					3	17	37	
BORON FILAMENT AILERONS								50
CONFORMAL LINEAR ARRAYS		19		91				
SOFT SOAP	1			7			17	1
TELEMETER FLUID		21			29			
ANALOG-TO-DIGITAL CONVERTORS			3			8		
DIGITAL-TO-ANALOG CONVERTERS				3			8	
TOTAL	12	94	38	239	38	65	95	51

The problem with this table is that you need a ruler to be sure you know which lines some of the numbers are supposed to be on. It's too hard for the eye to track across all those endless wastes of paper without straying up or down to the next line. So fill in the blanks, like this:

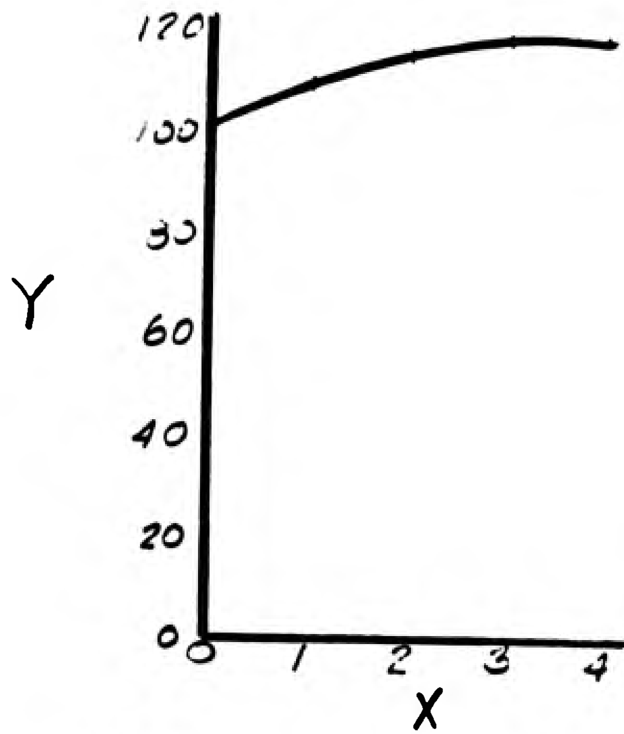
	FY 73	FY 74	FY 75	FY 76	FY 77	FY 78	FY 79	FY 80
RADAR JAMMERS	4	13	3	-	5	21	-	-
WEB BELTS	-	-	-	105	-	19	-	-
AIR-CONDITIONED TRACTORS	-	-	1	2	1	-	-	-
TURBOGENERATORS	-	41	-	-	-	-	16	-
GALVANIZED ANCHOR CHAINS	7	-	-	-	-	-	17	-
COMMAND-ACTIVATED SONOBUOYS	-	-	31	31	-	-	-	-
PELICAN HOOKS	-	-	-	-	-	-	-	-
TURBOENCABULATORS	-	-	-	-	3	17	37	-
BORON FILAMENT AILERONS	-	-	-	-	-	-	-	50
CONFORMAL LINEAR ARRAYS	-	19	-	91	-	-	-	-
SOFT SOAP	1	-	-	7	-	-	17	1
TELEMETER FLUID	-	21	-	-	29	-	-	-
ANALOG-TO-DIGITAL CONVERTORS	-	-	3	-	-	8	-	-
DIGITAL-TO-ANALOG CONVERTERS	-	-	-	3	-	-	8	-
TOTAL	12	94	38	239	38	65	95	51

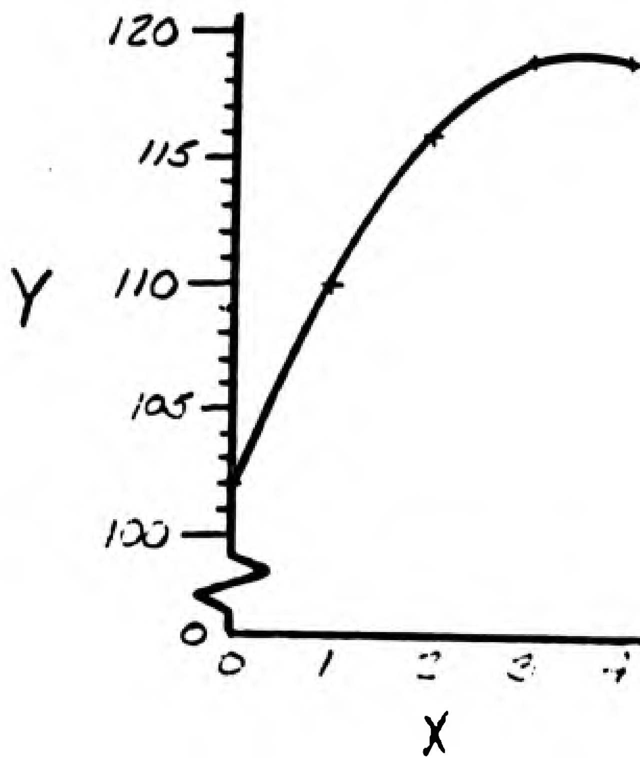
Ah ha! No pelican hooks in any year!

Here's another problem:



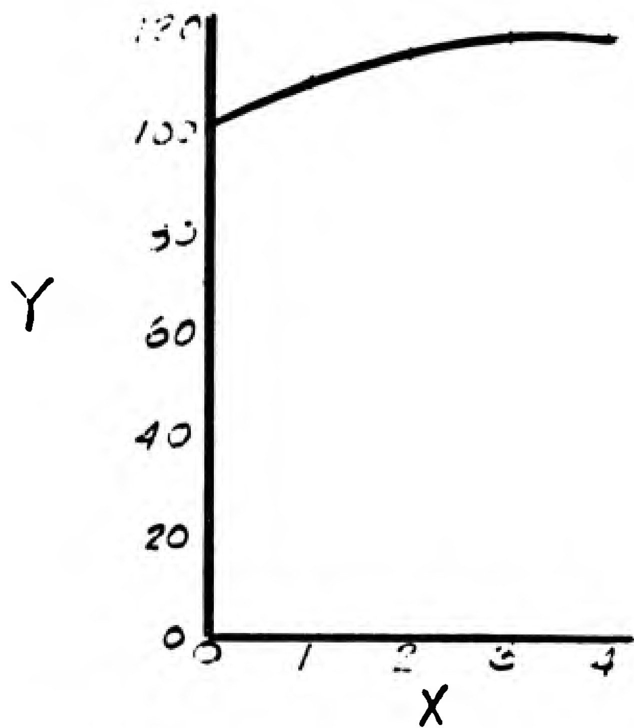
Here's a plot of "Y" -- think of that as successful ship deliveries -- versus "X" -- think of that as the amount of money we put into ASW. Note all that wasted space. Makes the plot hard to read, you say. OK, why not just cut off the the bottom part of the plot, like this:


120
100
60
40
20
0
Y
0
1
2
3
4
X

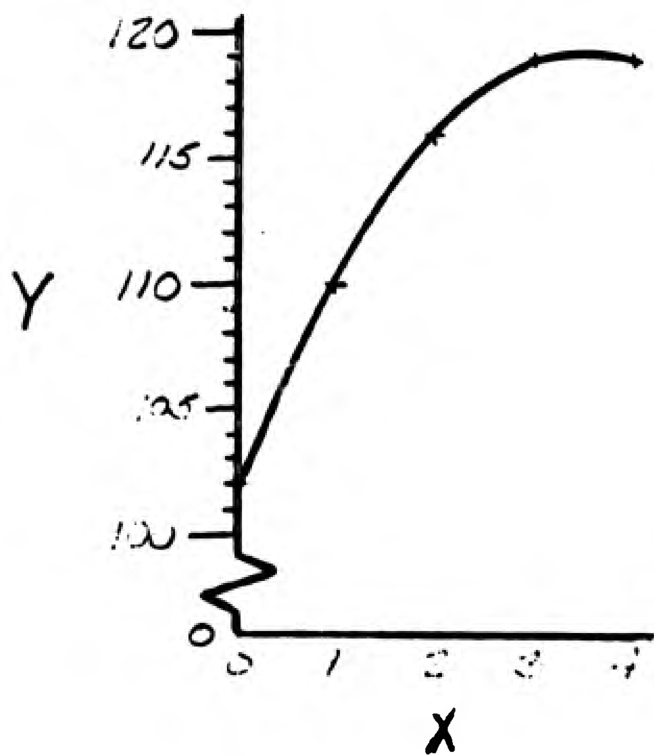

120
115
110
105
100
0
Y
0
1
X

Now you can see the effect more clearly. But is that a good idea? Almost never. It's misleading. It's the kind of plot you see in "Time" magazine that makes changes look much bigger than they really are. One very good reason for using a plot -- as opposed to a table -- is to give the reader a quick picture -- a general sense of some relationship. But if the ordinate doesn't start at zero, then the quick picture it gives you is almost certain to be misleading. In this case, it looks like more spending on ASW greatly increases the number of ship deliveries -- just see how steep that slope is. But if you use the original plot instead, then you'll see that the real improvement is actually much more modest.

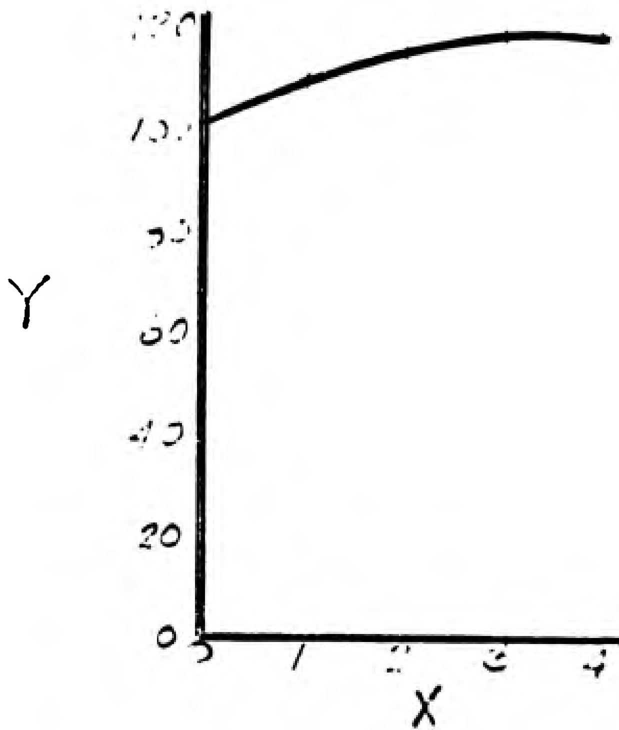
Some may claim that the truncated plot makes it easier for you to read the exact numbers. True enough, but if precision is what you have in mind, use a table, like this:



X	Y
0	102
1	110
2	116
3	119
4	119

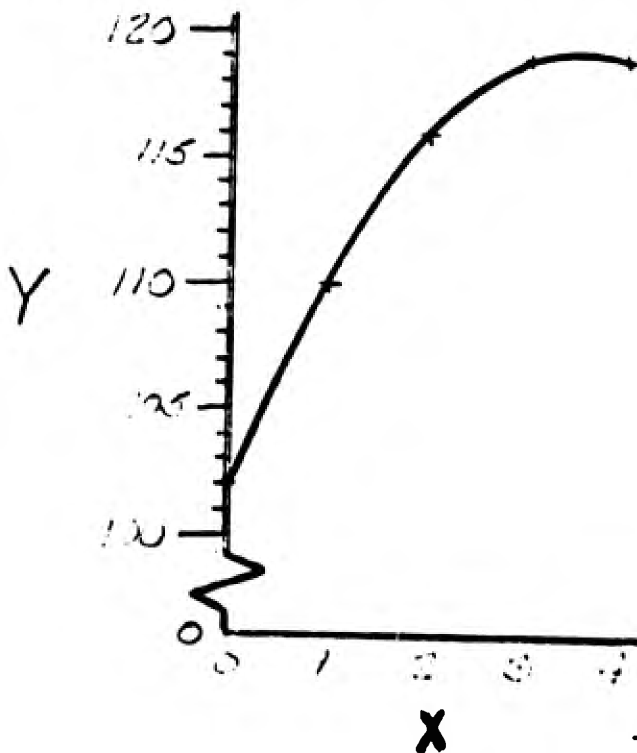


There are your numbers.



X	Y
0	102
1	110
2	116
3	119
4	119

Using an ordinate that doesn't start at zero is almost as misleading as this table:



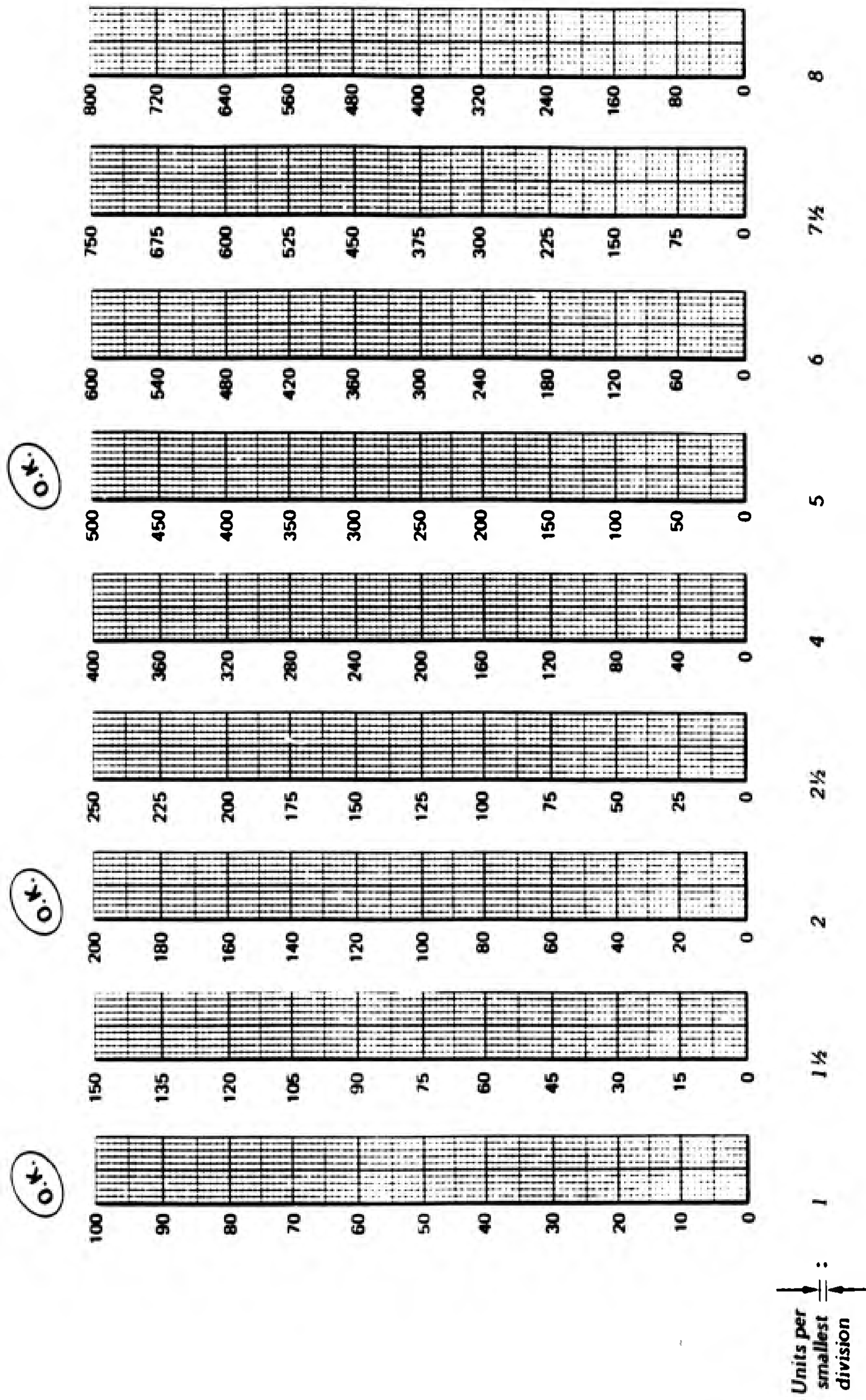
X	Y-100
0	2
1	10
2	16
3	19
4	19

If the idea is to show what happens when I spend more on ASW, and you're willing to lop off the bottom of the upper plot on the grounds that nothing changes down there, then why not lop off the hundreds digit of the upper table? Nothing changes there either.

Start your ordinates at zero unless you have a very compelling reason not to. And trying to make changes look more important than they really are is not a compelling reason.

Having said all that, now I'm going to contradict myself. I said that the advantage of a plot is that it gives you -- or, at least, it gives some of us -- a general sense of some relationship. And I said that if you wanted to display precise data, use a table. Well, that's not always practical. Sometimes, particularly when you've got a complex function, a table covering all the possible values of the independent variable would get so big that it's better just to show it on a plot, and let the reader pick off whichever point he's interested in.

OK, but when you do that -- and, actually, any time you work up a plot -- don't use bastard scales. People's hands are pretty much tied when they use log paper, but linear paper opens up the possibilities. The reason people use bastard scales is usually that they're trying to fill up the graph paper and make the plot as big and clear as possible. But in doing that, they come up with oddball scales -- such as 7 units per division. That may expand the plot to fill the paper, but it makes it almost impossible to read, and it greatly increases the chance of making errors when you're drawing the graph.

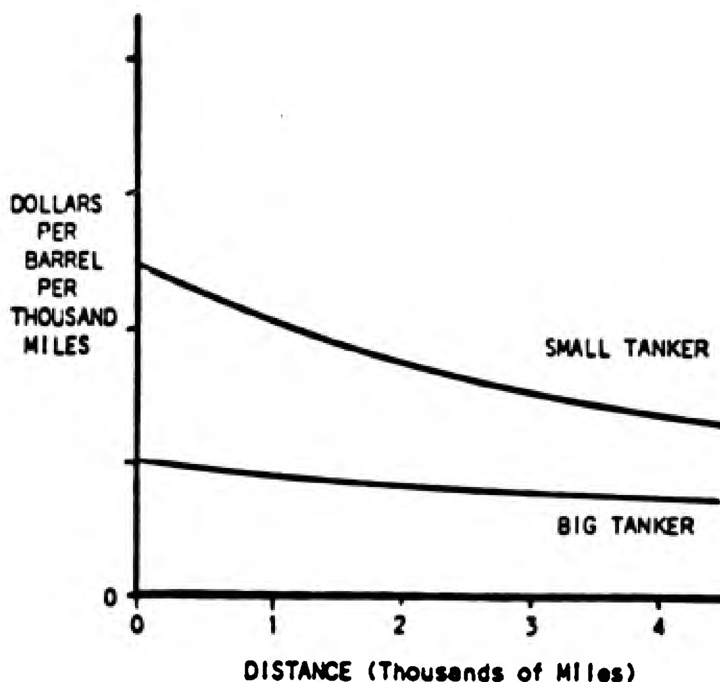

Units per smallest division:
1
1½
2
2½
4
5
6
7½
8
O.K.
O.K.
O.K.
0 10 20 30 40 50 60 70 80 90 100
0 15 30 45 60 75 90 105 120 135 150
0 20 40 60 80 100 120 140 160 180 200
0 25 50 75 100 125 150 175 200 225 250
0 40 80 120 160 200 240 280 320 360 400
0 50 100 150 200 250 300 350 400 450 500
0 60 120 180 240 300 360 420 480 540 600
0 75 150 225 300 375 450 525 600 675 750
0 80 160 240 320 400 480 560 640 720 800

There are only 3 legitimate scales: 1, 2, or 5 units to each of the fine divisions on the graph paper. Or those scales times 10 to any integral power. They're the only ones that are easy to read. This one -- 2-1/2 units per division -- is common, but it's a strain to read. Don't use it, and certainly don't use any of the others.

Another point. Sometimes we use computer-generated plots, where the machine nicely draws, labels, and ticks off the ordinates and abscissae and then spots points on the plot. Points for different conditions are represented by different symbols. That's nice, and it saves you a lot of work, but sometimes it's hard for the reader to untangle the various symbols and see the trends the plot is supposed to show, particularly if the symbols tend to lie on top of each other. Save the reader some work: fair curves through the points before you stick it in the report -- if the computer hasn't already done that, too.

A graphical small point: sometimes reports will include scale diagrams, for example, to show the layout of a bunch of SAM batteries and their coverage. If you do that, draw the scale right on the diagram, like a cartographer's legend. Don't settle for something such as "1/4 inch = 1 n.mi.". Paper changes size in lots of reproduction processes, and your quarter inch is likely to end up representing something other than a nautical mile. It can give me problems when I review the draft, and it can give Graphics problems, too.

Next, be careful to have the plot demonstrate the point you're trying to make, as directly as possible. Here's an example:

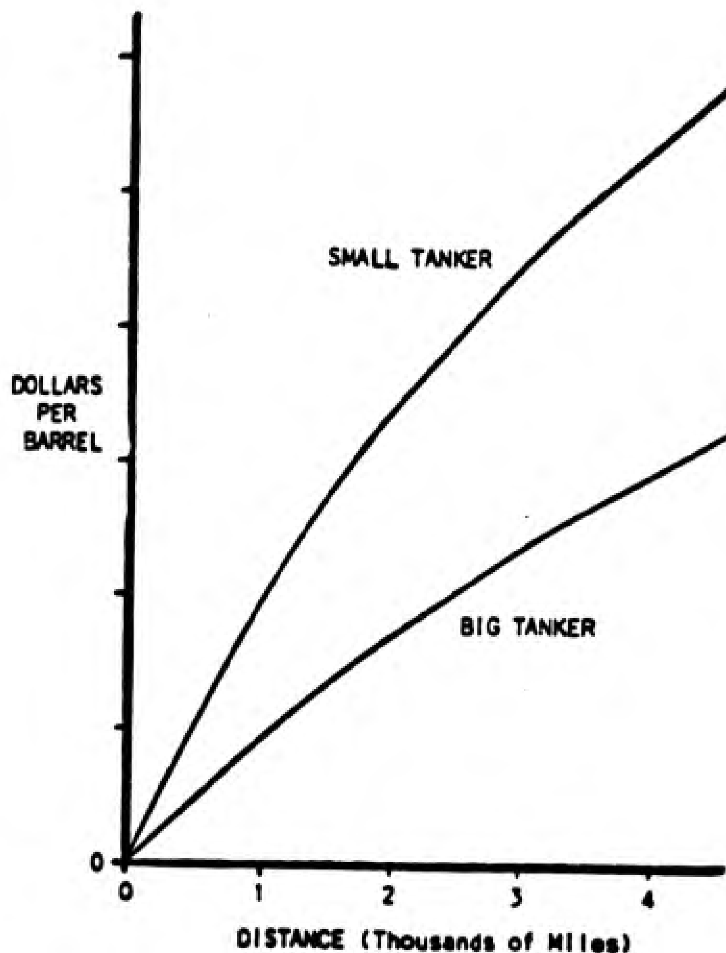


This is from a CNA report that was discussing the advantages of big tankers, and what you see here is modeled after the plot that was included as a way of demonstrating the following point, and I quote:

> "...the longer the haul, the greater the cost savings by using large tankers."

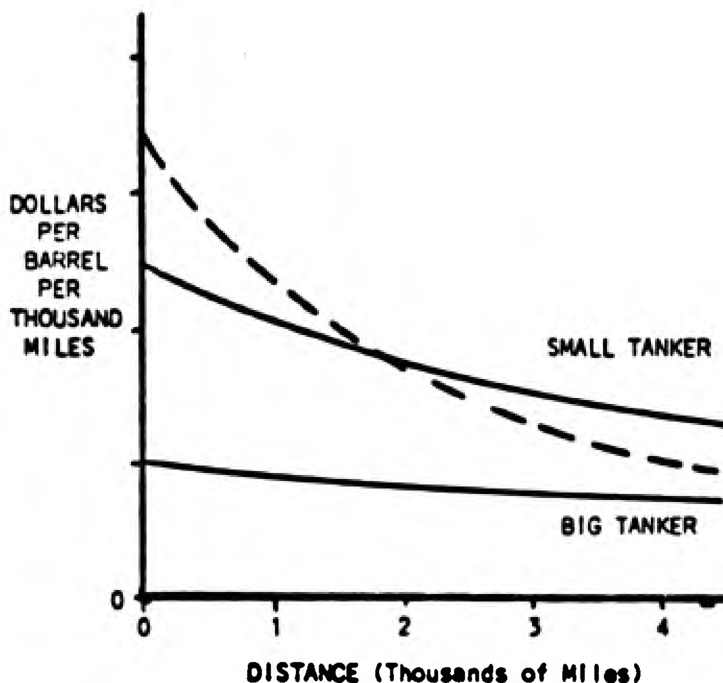
So you look at the plot, and you see that the abscissa is the distance scale, and the ordinate is the cost scale, and the small tanker is more expensive than the big tanker. But the contention was that the cost saving from using the big tanker grows with distance. Yet on this plot the difference between the two curves gets smaller at the longer distances -- not bigger. That's not much of a demonstration of the point, and it's likely to shake the reader in his attempt to follow you.

Actually, the plot doesn't contradict that statement at all -- it just looks at first glance as if it did. Sharp-eyed plot-readers will note that the dependent variable isn't cost per barrel, it's cost per barrel per thousand miles. To see whether the contention is correct, you have to pick points off the plot, multiply them by the particular distances involved, and come up with a new plot which shows the cost per barrel -- rather than cost per barrel per thousand miles. If you do that, this is what you get:

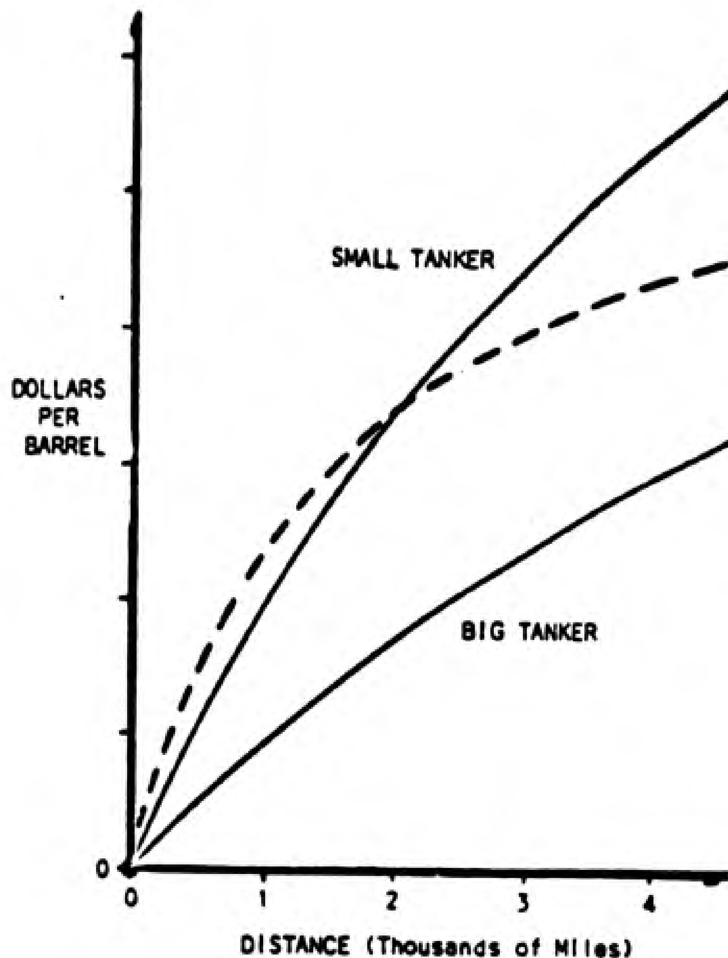


Sure enough, the gap between the two curves does widen with increasing distance, so the contention was correct. But this was the plot that should have been in the report to begin with. The other plot was correct, but it didn't demonstrate the point at a glance.

Perhaps you think that most people should have been able to see that anyway from the original plot. I don't think that's so obvious. Suppose I showed you this kind of a curve -- the dashed line -- for the small tanker:



It's not so all-fired different to the naked eye. Would you say that the cost saving still increases with distance? Or decreases? Well, here's what it does:



It increases up to about 2000 miles, and decreases after that.

You may be very good at mental transformations of curves, but don't assume your reader is. If you're trying to demonstrate some point, use a plot that does that in the simplest, most direct, least ambiguous way you can think of. You can't force the reader to follow you -- he can quit trying anytime he wants to. So,at least,make it as easy as possible for him to follow you.

One last point on graphics -- the use of codes. Sometimes analysts use codes as a shorthand way of differentiating between various cases they're analyzing. In one CNA study, "A" referred to targets at 35,000', "B" to targets at 45,000', and so on. In another "R4" referred to Indonesia, and other symbols referred to other places in the world. That may be convenient for the analysts, but it's likely to be rough on the reader. Try to avoid carrying over to your final report the codes you used in your analysis. It's a pain in the neck to have to learn a new language, or to have to keep referring to some table like a Rosetta stone. If at all possible, spell these things out in plain English on your plots and tables, and in your text, too.

In a lot of places, of course, you do have to use symbols -- in mathematical expressions, for example. In those cases, at least try to make the symbols as intuitively logical as you can. In one CNA report, there were a lot of references to an outer area and an inner area. The symbol used for the inner area was A_0 (A-sub-zero), and the symbol for the outer area was A_1 (A-sub-one). But that's easy to forget, and when the text referred to A_0 (A-sub-zero) -- the inner area -- you could just as easily read it as A_O (A-sub-oh), and naturally associate that with the outer area. A small thing, of course, but like people say, it's the little things that make the difference -- in this case, the difference between clarity and confusion.

OK, I'm almost at the end of what I plan to say about presentation, and I'll wind it up with some miscellaneous points. Most are pretty simple, but the first one is very important: exorcism. Be an exorcist.

In the course of most studies, analysts run down some blind alleys. Few studies are 100% efficient, with every piece of work turning out to be essential to the study. Sometimes a piece of work just bombs out, and the problem ends up being solved some other way. But sometimes the piece of work's OK -- it's just that it turns out not to be essential to the study. Perhaps the study takes a new tack, and then you don't need the answer to some question you've been working on. Or you figure out a better and simpler way of answering some question specifically, but you're left with a beautiful general analysis that answers all kinds of other questions, too -- none of which has any bearing on the study.

What do you do then? Let all that work go to waste? Of course not -- assuming the work's potentially valuable. So you grind it into the study, right? Wrong. Exorcise it. Throw it out of the study. If you want to commit it to posterity, publish it as an RC. But don't clutter up the study with anything that's not essential to the finished product. And that applies right down to the details. such as a table in one CNA draft report showing the characteristics of a whole bunch of aircraft, most of which weren't considered in the analysis.

Cluttering up a study is a dumb thing to do. You make it thicker, and that discourages readership. You force your remaining readers to plow through stuff they shouldn't have to plow through. You interrupt the logical flow of your exposition. For the health of your study, exorcise regularly.

Here is an excerpt from the draft of a CNA final report that illustrates another kind of problem:

"SENSITIVITY ANALYSIS WILL BE DONE ON
BASE AVAILABILITY IN THE FINAL REPORT."

That's an illustration of the hazard of just taking working papers and binding them into the final report. To read about something that's going to be done before the final report is written, when it's the final report you're reading, gives the impression of sloppy work, and tends to make the reader wonder whether the analysis might not be just as sloppy. So be careful about just binding in old work.

As a matter of fact, be careful about any kind of trivial mistake like that -- things that just slip by. They may seem trivial to you, but they make you look careless, and they put the reader off. He can't tell whether it's just an innocent, isolated slip, or whether it's a symptom of a slipshod, slapdash, superficial approach to your work. Could you really blame a guy for being put off if he saw this in a CNA report?

> "THE PERCENTAGE RULE OR SUPERIOR INTERCEPTORS AGAINST THE SOVIETS OBTAIN A REASONABLE ESTIMATE OF POSSIBLE OUTCOMES."

Sure, it's only a typographical glitch. But it looks horrible. Proofreading is a terrible chore, but it's one of the things that's got to be done if CNA is to be clearly the best think tank in the world.

So much for presentation. I wanted to talk about that first not because I think it's more important than anything else, but because it struck me -- in reviewing those 27 reports -- that it's probably the most neglected art in CNA, and that we pay much, much less attention to it than it deserves.

• STANDARDS

I'd like to move on now to my third major topic: standards, "no-nos" and "yes-yesses" for CNA reports. I mentioned earlier the general standard that CNA reports should be of at least the quality of Ph.D. dissertations accepted by the University of Rochester, in terms of analytical rigor, documentation, reproducibility, and general competence in the field. I'd like to start by talking about reproducibility.

First, a sort of flat statement: all CNA studies should be as reproducible as it is reasonably possible to make them, and studies which don't meet that criterion don't get my approval.

Why should we strive for reproducibility? I'll start with a philosophical reason. CNA is a research organization. In the research community, one of the classical tests of one researcher's findings is to have other researchers try to reproduce his results. If they can, those results gain credibility. If they can't, they don't. In that way, good research tends to be added to our store of knowledge -- or at least our store of beliefs -- and bad research tends to send the researcher back to his lab to try again.

That's why CNA reports -- as products of research -- should be reproducible. Specifically, there are three things reproducibility does for us. First, it establishes confidence on the part of the reader. When you explain your methodology, he can follow what you've done and understand your analysis. When you give him the inputs you cranked through your methodology, he can repeat the analysis himself and make sure you know how to add and subtract. He's much more likely to believe you if you show him how to get the same answers himself.

The second thing it does is preserve your work. That not only allows some future analyst to save a lot of futzing around, but it gives him a base on which he can build new things instead of reconstructing old things. Save the drawings and you won't have to reinvent the wheel.

The third thing it does is a bit unpalatable, but it's good for you, too -- like the "Write Formula" test. It opens your work to informed criticism. That may hurt at times, but it prevents you from making the same goofs over and over. And it adds to the general progress of our analytical I.Q.

My flat statement a moment ago was that all CNA studies should be as reproducible as it's <u>reasonably possible</u> to make them. What's "reasonably possible" mean? It's obviously subjective, and I don't know any way to define it rigorously. Clearly, attempts to make reports reproducible can be carried to extremes. There's no point in expanding the final report to include a couple of thousand pages of computer inputs and outputs. But, at the other extreme, there's no point in statements such as this:

> "IN THIS PROJECTION, THE ANALYSES MADE BY OTHERS HAVE BEEN USED AND SUPPLEMENTED BY ESTIMATES WHICH HAVE BEEN REVISED BY THE AUTHOR AS NEW DEVELOPMENTS AND CONSIDERATIONS WARRANT."

Modifications of referenced works may be entirely appropriate, but they have to be spelled out in enough detail for the reader to be able to use them as you did.

That brings up another point about references. Digging out a reference so that you can reproduce the analysis is a pain in the neck. It may be easy for you to do it -- we've got a remarkable library here -- but it's not so easy for most of our readers. If you can bolt into your report the key parts of the reference so that the reader doesn't have to dig it out himself, do it. A reasonable compromise is to include whatever algorithms or data the reader will need from the reference to reproduce your results, but omit the reference's derivations and details. Giving the reader enough so that he can at least repeat your analysis is reasonable; trying to defend every reference back to Newton's Principia is not. I will grant that there may be times when copying even the algorithms and data is impractical from the point of view of sheer bulk. But even in these cases, there are things you can do to help. If it's important enough, you could even consider binding microfiche cards into your report. Or you can at least make it clear where the ambitious and skeptical reader can get hold of the reference.

There really are three phases in making your work reproducible: first, your methodology; second, your input data; and, third, your output data. I reviewed one CNA study and sent back a review memo pointing out that the results seemed to be laced with calculation errors. It turned out that they weren't: it was just that the calculation process had been explained so incompletely that I couldn't get the right answers. It's important to make your methodology clear and unambiguous.

One thing that's a great help along these lines is to include a sample calculation. Take the reader by the hand, as it were, and lead him all the way through the process one time from inputs to outputs. If you're using an expected-value model with conditional probabilities linked in series and parallel, draw a flow diagram if the thing's complex and has lots of branches and jumps. That's useful not only for reproducing the answers, but also for explaining the logic.

As to input data, I reviewed a CNA wargame where there were certain conclusions drawn on the basis of what happened in the game. But what happened was not detailed closely enough to enable the reader to derive the same conclusions himself. You might not want to burden the reader with a complete chronology of everything that happened, but it's not unreasonable to think about at least detailing whichever events have a bearing on the conclusions you draw.

As to output data: one very large CNA report presented its findings in the form of bar charts. That's fine for a quick appreciation of the results, but somewhere in the report -- probably an appendix -- the actual numbers should have been cited. Otherwise, the reproducer has to compare his precise numerical results with your fuzzy graphical results, and he can't be sure whether any differences he may find are real or just the result of careless plotting, or inaccuracies in scaling off the bars, or whatever.

So reproducibility is a "yes-yes" for CNA reports. Perfect reproducibility is sometimes impractical, and we shouldn't create monsters in the attempt. But the fact is that monsters haven't been the problem in those 27 reports that unreproducibility has.

Next, I'll run through some "no-nos". An obvious one is brochuremanship. In our business, people talk a lot about unbiased analysis -- and generally get that confused with unbiased analysts. In my book, the unbiased analyst is most likely to be the guy who doesn't know anything about the problem. Once any of us has dug into some question, we're liable to develop some opinions. For example, my opinions on VTO aircraft are no secret. You can call me biased on that subject -- like most that I've worked on -- but I think we all have biases of that kind. We have biases about nuclear propulsion, about mutual assured destruction, about AFEES tests, and God-knows-what else. <u>But</u> it is very important to keep those personal biases -- whether they be right or wrong or something in between -- from influencing our analyses and our reports.

These biases sometimes lead to brochuremanship in CNA reports. An analyst will plow through a study and become convinced in the process that the Dingbat is the greatest thing since Greek fire. That's OK; the problem is that in the flush of his enthusiasm he then writes up his report as the Dingbat Manufacturing Corporation Sales Department would -- lots of dramatic adjectives -- and perhaps assertions about the Dingbat that aren't supported by the cold results of his analysis.

Don't do that. That's a no-no. CNA is not in the business of selling anything other than the value of our analytical help. That doesn't mean that a CNA report has to be dry as dust. It doesn't have to sound like a report to the Royal Society of Lepidopterists on population trends in the Clouded Yellow butterfly. But putting some spark into your writing isn't the same thing as brochuremanship. You should avoid anything that even sounds as if you were reporting your own personal preferences instead of the analysis that may have led you to them. That kind of thing is totally at odds with the concept of CNA as an independent analytical organization. Beyond that, you deny yourself the real impact of a clear, factual statement, devoid of hucksterism.

Another problem I've noticed is a tendency to skate on technologically thin ice. One CNA draft report asserted that you could make a surface-to-air missile with a 250-mile range by putting Phoenix missile parts into a Firebee II airframe, and using over-the-horizon radar for mid-course guidance. I found that pretty hard to swallow. Among the problems that were overlooked were the weight and balance of the modified Firebee, how the Phoenix radar antenna could be fitted into the Firebee's skinny nose, structural masking of the warhead, a thousand-to-one mismatch between the Phoenix and OTH radar frequencies, the intolerably low data rate and accuracy of an OTH radar, not to mention its complete lack of altitude tracking and, finally, how we could expect an 1885-pound Firebee to outrange the 14,000-pound Bomarc -- the longest-ranged SAM that's ever been built. It would be a lead pipe cinch for any competent engineer to demolish assertions like those, and possibly CNA's reputation along with them. That's a particularly egregious example of skating on technologically thin ice, but not the only one I've seen in CNA reports.

The point here is to restrain yourself in matters where your technological expertise is limited, not to mention where it's likely to be more limited than your reader's. Get some help from someone in CNA who is technologically competent. Failing that, try to find an outside consultant. Failing that, stay out of that area.

Here's another no-no:

> "THERE IS NO SHORTAGE OF PROJECTIONS.....FOR PURPOSES OF THIS APPENDIX, HOWEVER, ONLY SELECTED STATISTICS ARE PRESENTED. IT IS HOPED THAT THIS SELECTION WILL HELP TO BRING OUT THE MAIN SUBJECTS OF INQUIRY INTO SHARPER FOCUS."

The problem here might have been simply the author's unfortunate choice of words. But it sounds as if he were picking and choosing from the various available projections so as to make the "evidence" seem to support whatever it was he wanted it to support. I call that "sly eclecticism", and it's a no-no.

When there is conflicting data, face that openly in your report. Dealing with uncertainty is a way of life in this business -- or should be.

Another no-no:

> "HOWEVER, FOR THE BULK OF OUR ASSUMPTIONS, WE BELIEVE THAT EITHER WE HAVE MADE A CLEARLY REASONABLE ASSUMPTION, OR OUR RESULTS AREN'T SENSITIVE TO THAT ASSUMPTION."

If the last no-no was sly eclecticism, this one is the self-serving assertion. It sounds like the "You gotta believe.." commercials from Eastern Airlines. Only our customer doesn't gotta believe. Telling him you happen to think your assumptions are reasonable tells him nothing, and certainly isn't persuasive if he doesn't like them. Instead, try to demonstrate why you think your assumptions are reasonable -- after all, if you're convinced, something must have convinced you, so tell him what it was. But if you're not really convinced, don't assert that you are. Deal with that as you would any other uncertainty.

Closely related to that is the "Center-for-Naval-Guesses Syndrome". Remember that this is the Center for Naval Analyses, not Guesses. Making a guess when you could make an analysis that would give you a better answer is a no-no. For example, if you say that the price of oil is likely to rise in the future and therefore you've assumed that consumption will drop by "x" percent, that's making a guess -- the same guess one CNA study made. Making an analysis is estimating the price elasticity of the demand for oil and then calculating the effect on consumption of any particular rise in price. It may well be that history won't bear you out, but at least you've done the best you can. Pulling some number out of the air is not doing the best you can.

Related to that no-no is the no-no of idle speculation. Base what you have to say on your analysis. When you run out of analysis, you've run out of things to say. One page of a particular CNA draft had 3 "mights", 2 "coulds", and one each "possible", "possibility", "may", and "perhaps". And that was a double-spaced page, too. Try to avoid that -- your reader is probably just as good at idle speculation as you are.

One last no-no has to do with the Study Directive and the Study Plan: don't grossly violate either one. It's not unusual for a study to take a different direction -- for good reason -- after you're partway into it. If the change is a major one, get the Directive or the Plan revised. If the change is really minor, don't worry about it. If it's something in between, or you're not sure, pass the buck upward. But don't surprise the customer with an analysis of zebras when you told him you were going to work on aardvarks.

I've got one other -- neither a no-no nor a yes-yes, but a maybe-maybe, depending on the situation. Sometimes you'll be forced by the pressure of time, or whatever, to wrap up your work and write the report, but there are still things that you think should be looked at. Maybe you're still not satisfied with some of the analyses you've done. Or maybe there are other cases that should be addressed. There's an alternative to just forgetting about all that. It takes an admirable amount of humility, but it can be a real contribution.

Right up in the front of the report, explain the situation openly. Tell the reader that this analysis has been taken this far and no farther. Don't apologize for that -- this shouldn't be a mea culpa. Then point out the limitations. Say what it is that troubles you about it, and what effect that might have on the results you show. Suggest what future analysts might do if they were interested in carrying on the work. What I'm suggesting is a sort of ready-made, built-in critique of your own work. Its purpose is not just to fend off criticism, but to be open and honest with the reader, to protect him from misunderstanding your work, and to help future researchers carry on where you left off. It's not an acceptable way of covering yourself if you've done a lousy job, but it's a great idea when a conscious decision has been made for good reason to chop off an analysis someplace short of nirvana.

Of course there are lots of other no-nos and yes-yesses. Do think about what you're doing. Don't crunch numbers for the fun of it. Do document what you do as you go along. Don't make any wheels-up landings. The list is endless, but the ones I've talked about are the ones that seem not to be so widely appreciated.

• ANALYTICAL PROBLEMS & PITFALLS

I'd like to turn now to my last main section -- "Analytical Problems and Pitfalls". I don't mean this to be an encyclopedic discussion of how systems analysis should be done. I'm just going to talk about how it has been done here at CNA when it hasn't been done well.

Some of the things I'll talk about will bore you. You may even feel insulted, because I'm going to mention some things that nobody in CNA should have to have explained to them. But I'm going to do it anyway as a way of making it clear that a lot of our analytical problems have nothing whatsoever to do with pre-Colombian methodology -- they have to do with a lack of ordinary care and craftmanship.

Perhaps chief among these are columns of numbers that don't add up to what's shown at the bottom. Think of that. Nobody who doesn't sit at my desk could possibly believe how often that happens. For goodness' sake, check all your tables -- and all your other numerical calculations, too. Then check to see that your typist got it right. Don't submit a report that looks like an unmade bed.

Another problem is internal inconsistency. Sometimes it's one number in the text, and another in an appendix. Sometimes it's a "tis" in one place and a "taint" in another. But whatever it is, it's unacceptable. I suppose it happens because different parts of a report tend to get written up at different times, sometimes by different people, and it's hard to take a snapshot of the whole analysis at one time. The only answer is for the Study Director to check the finished report from beginning to end. I know that's a dull job; I know that the Study Director's main wish in life at that point is to be rid of the monster. But it's part of the job.

Another problem has to do with precision. One CNA report dealt with prepositioning as an alternative to mobility. After going through a long costing analysis, the report said that the cost for a particular option was "4 billion, 851 million, 819 thousand dollars". That's 7 significant digits. Yet a significant portion of that cost -- a part dealing with how much extra gear you'd have to preposition to replace what would be lost to enemy air attack -- traced back to an aircraft availability factor that was cited to one significant-digit's worth of precision.

In another study, the cost of several alternative aircraft was examined in exquisite detail, including items such as the cost of transition training for the pilots, the cost of peculiar training equipment, component rework costs, and many similar fine-grained details that implied great precision. Yet the production costs were estimated on the basis of the average cost per pound of three earlier aircraft, without any accounting for production numbers, learning curves, or inflation. Obviously, there's no point in beating yourself to death trying for great precision in some of the numbers when you're willing -- or forced -- to accept crude estimates for the rest.

All that brings me to the related problem of rounding off numbers. Rounding's a good idea -- don't show meaningless digits -- but the process has its perils. One is an unnecessary loss of precision. Some analysts have a tendency to round after each step in a calculation. Depending on the luck of the draw, that's likely to result in an answer that isn't the same as if you had carried all the excess digits through the whole calculations, and then rounded at the end.

ROUNDING AT EACH STEP:

FIRST FACTOR:	2/6 = .33333	ROUND TO .3
SECOND FACTOR:	(.3)(.7) = .21	ROUND TO .2
PRODUCT:	(.3)(.2) = .06	

ROUNDING AT END:

$$\frac{(2)\ (.3)\ (.7)}{6} = .07$$ (ROUNDING NOT NECESSARY IN THIS CASE)

Rounding can also make it hard to reproduce your work. In one CNA report, a sweep rate calculation came out at 2780 n.mi.^{2}/hr., but later calculations used the number 2800. That's likely to confuse the reader. He may spend a lot of time trying to find where the 2800 came from, especially if he's punched 2780 into the keyboard of his MK 1, Mod 0 brain, and switched it to the auto-search mode. When he eventually notices the 2780, he'll probably guess that you must have rounded that up to 2800, but he can't really be sure.

Things like that may not be serious errors, but they certainly are unnecessary, and it's silly to take a chance on derailing your reader's train of thought. If there's some good reason for you to round off at an intermediate point, show the rounding explicitly, right next to the raw figure. In most cases, though, it's much better to save your rounding till the end.

You can also give the reader fits when you show rounded tables of intermediate results when you've actually saved your rounding till the end. Then, when the reader puts together your intermediate results, he can't quite come up with your final answers. The differences may be small, but it shakes his confidence. What he wants to see flash up on his calculator is exactly the same number you show in the report -- not something that's just close.

The next problem is wanton averaging. Sometimes you can tell the reader all he really needs to know by citing an average figure instead of the whole data base. But that can be carried too far. One CNA report said that a particular missile range parameter increased an average of a half mile with each thousand-foot increase in target altitude. But if you looked at the data, you could see that the figures that averaged out at .5 actually varied between .001 and 1.26. That's wanton averaging, and it can badly mislead a busy reader.

As a long-time advocate of back-of-the-envelope analysis, I hesitate to bring up this next one. But it is true that analysis can be oversimplified. As an

example, one CNA study calculated the number of VP aircraft it would take to monitor a barrier on the basis of the average radius from the base to the various stations on the barrier. That's simple, but it's not right. The number of aircraft required to maintain one on station is a very non-linear function of radius. Obviously, if a station is at the maximum radius of the aircraft, it takes an infinite number of aircraft to maintain it. So picking the average range to plug into the calculation results in an understatement of how many aircraft it takes to do the job. What you save on the closer stations is not as big as the extra it costs you on the farther stations.

In another case, a CNA report on a wargame estimated how many of the submarines detected by SOSUS would have been missed if the game had been played with a lower SOSUS detection probability. The analyst's approach to that question was to run through all the SOSUS contacts reported for a given submarine during the game. For each such contact, he would consult a random number table. If the number was less than the probability of losing a contact -- given the assumed change in SOSUS performance -- he would knock that contact out. At the end, if any of the original contacts survived this process, he would say that this particular sub would still have been detected. If no contacts survived, he would say that the drop in SOSUS performance would have moved this sub from the "detected" to the "undetected" category. He repeated the process for each sub, and thereby came up with his answer.

That's an example of what I'd call a "poor man's Monte Carlo" analysis. The problem is that any such single calculation throws you on the mercy of the luck of the draw. Maybe you'll get a representative answer, but maybe you'll come up with a fluke. To make any Monte Carlo analysis significant, you've got to make a lot of runs. The solution, in this case, would have been to make a straightforward expected-value calculation.

A similar sort of oversimplified analysis was used in a report analyzing how accurate a radar would have to be to vector interceptors. The approach in this case was graphical -- the whole thing was laid out on a chart, with the radar here, the real position of the interceptor here, its measured position there, the target here, and so on. But the probabilistic complexities of the problem result in so many possible geometries that any such graphical solution is hopeless. You'd need a separate plot for every combination of target and interceptor position, heading, and speed, and for each radar scan time during each intercept. For each target position, you'd have to lay out enough different measured points to account for the distribution of errors. You'd have to do the same for each interceptor position. Then you'd have to see what happened at the next time interval when the radar made its next scan, and so on. Then you'd have to try different interceptor and target headings and speeds, and try it at shorter radar ranges, or longer, or at different azimuths. And so on. To get even a rough idea of the effect of radar accuracy on the success of the intercept would take more plots than you could draw on all the paper at CNA.

So while I urge you to use the simplest analytical method you can, and to avoid cluttering up your report with tour-de-force analyses of baroque complexity, watch out for oversimplified analyses like these examples.

Another problem is the non-credible approximation. Here's an example from a CNA report. A recce aircraft is flying from A to B, investigating any contacts it makes along the way. Each time it gets a contact, it has to spend a certain amount of time checking it out. The ground rules are that the wider the sweep width, the more contacts it will make. The question is, what is the average speed between "A" and "B"? The report came up with the following expression:

$$V_{av} = V_o \left(1 - \frac{W}{120}\right), \text{ WHERE: } V_{av} = \text{AVERAGE SPEED FROM "A" TO "B"}$$

V_o = AIRCRAFT CRUISE SPEED

W = SWEEP WIDTH (N.MI.)

That expression may be OK for small values of "W", but it doesn't take any great mental gymnastics to figure out that if the sweep width is 240 n.mi., this expression implies that the aircraft doesn't slow down at all -- it just flies backwards.

If you're going to use some approximation, try to base it on a plausible algorithm. Non-credible approximations like that one shake the reader's faith. They make it look as if you've just pulled something out of the air without thinking about it. So if you have to use some approximation that's not inherently plausible, the burden of proof is on you to explain why you're doing that, and then to cite specific limits over which the approximation is valid. In the example above, that would mean saying something such as "this approximation is accurate within ± 5% for sweep widths up to 25 n.mi.".

But even then you may have to be careful, especially if you have a complex approximation. One CNA study estimated the percentage of SAC bombers that would be killed in a surprise raid by Soviet SLBMs. It accounted for five independent variables, and the "acceptable ranges" for each were clearly stated. But with that many variables, there are lots of combinations. Here's one set:

INPUT PARAMETER	MINIMUM ALLOWABLE	EXAMPLE	MAXIMUM ALLOWABLE
TOTAL NUMBER OF RVs	0	500	1000
RV ARRIVAL RATE (NO./MIN.)	20	100	200
NUMBER OF SAC BASES	20	150	250
BOMBER REACTION TIME (MIN.)	2	3	8
BOMBER ESCAPE TIME (MIN.)	0.5	0.7	2

If you plugged those into the approximation, you came out with the answer that the percentage of bombers killed was -1.6%. Tilt.

A non-intuitive approximation, if you must use one, should have stated limits, but if you can still get goofy answers using numbers within those stated limits, it passes from the merely non-intuitive to the non-credible.

The next problem is the Trusting Soul Syndrome. One of the hallmarks of a good analyst -- like a good newspaperman -- is a certain amount of skepticism. He should have a sort of red light in his head that goes on any time a funny-looking number passes before his eyes. One CNA analysis where that didn't happen involved a calculation of what it would cost the Soviets to resupply submarines with torpedoes from supply ships in remote locations. The idea was to avoid long submarine transits back to Russia, which not only would be non-productive, but also tough on the submarines as they passed through all our barriers. To make the cost calculation, the analyst looked up the intelligence on some Soviet submarine tenders, which he picked as representative of likely resupply ships. He read there how many torpedoes each of them could carry, and from that point, knowing how many Soviet submarines he had deployed and how fast they used up torpedoes, it wasn't hard to figure out how many sub tenders the Russians would need for the job, and thus the cost. But the cost came out very high.

This would be simpler if I could cite the actual numbers involved, but I can't and still keep this unclassified. So I'll have to talk around the numbers. What should have lit the red light in the analyst's head was the extraordinarily small number of torpedoes cited in the intelligence data as the capacity of the Soviet submarine tenders. In fact, the submarines it was supposed to support carried three times as many torpedoes per ton of displacement. That's clearly nuts. You'd normally expect that ratio to be the other way 'round. But the analyst didn't question that, presumably because he had the intelligence data to back him up.

If he had questioned it, though, he would have found out that the intelligence data didn't refer to how many torpedoes you could carry on a ship the size of a Soviet sub tender if that's what you wanted to do. What it actually referred to was the number of torpedoes the sub tender could handle in its rework facility -- tearing them down and rebuilding them. So don't just accept the printed word. If you see something that looks fishy, be sure it's you that's nuts and not the publication before you accept it.

Here's another example. We monitored a field test in which six separate squads of Marines fired at pop-up targets. There were two kinds of squads -- big ones and small ones -- and there were three of each in this test. Here are the results:

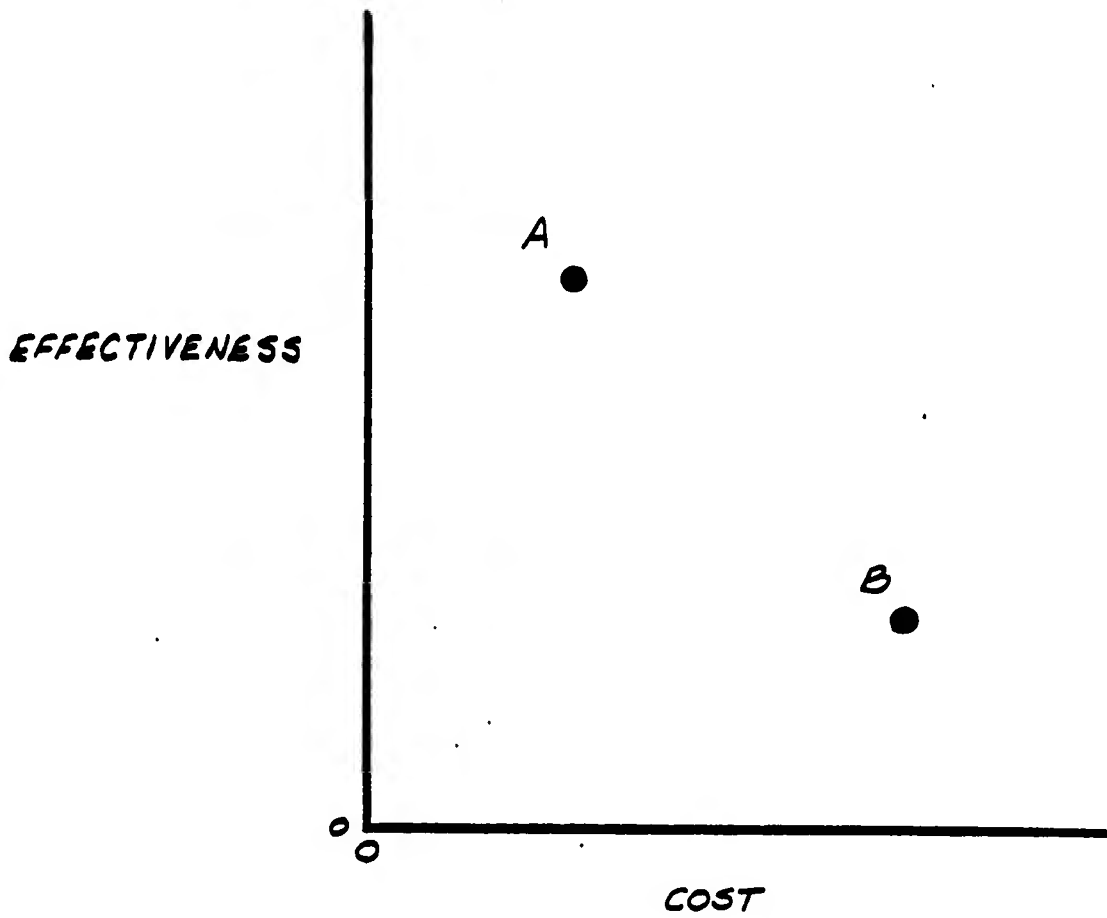
NUMBER OF HITS ON POP-UP TARGETS

SMALL SQUADS		BIG SQUADS	
S-1:	2052	B-1:	1646
S-2:	1934	B-2:	1531
S-3:	1733	B-3:	1510
MEAN:	1906		1562
STD. DEV.:	161		73

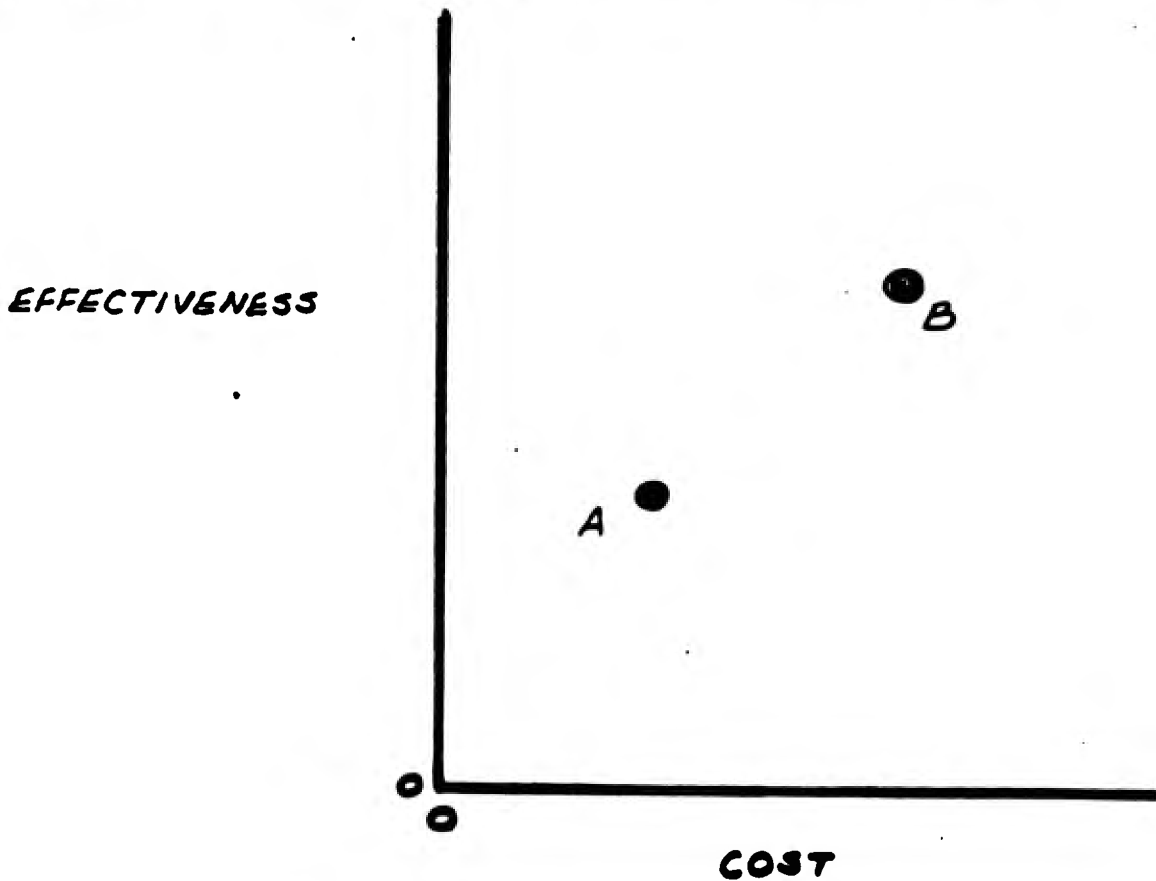
Not only is the mean score higher for the small squads than for the big squads, but the worst small squad got more hits than the best big squad. That's counter-intuitive. The Trusting Soul says "That's what the data shows". The Skeptical Analyst says "Hey, that's nuts. I'd better dig into this to see if something was wrong with that test before I draw a whole lot of conclusions".

Well, those examples are sort of nuts-and-boltsy, and I'd like to move on to some problems that are a bit more conceptual. One is the use of cost-effectiveness ratios. They are a trap; don't use them. If people had used cost-effectiveness ratios to decide how many Minuteman missiles the United States should have, we'd never have the thousand we have now. The first missile goes against the target with the highest payoff. Each additional missile you buy gets you less. So if all missiles cost the same, you'd get the best possible cost-effectiveness <u>ratio</u> with the first missile, and buying more would just make things worse. (Of course, you can complicate the argument by considering the effects of R&D costs and the production learning curve -- but I can guarantee that the cost-effectiveness <u>ratio</u> curve will peak out well below 1000 Minuteman missiles for any reasonable set of assumptions.)

The ideal form of the answer is not the cost-effectiveness ratio, but the whole curve of effectiveness versus cost. That's great if you can get that far, but usually you'll end up with just a few points representing different alternatives on the menu. If those points end up like this, you've got it made:



Alternative "A" is clearly better than Alternative "B" -- it costs less and it's more effective. But what happens when the points show up like this?

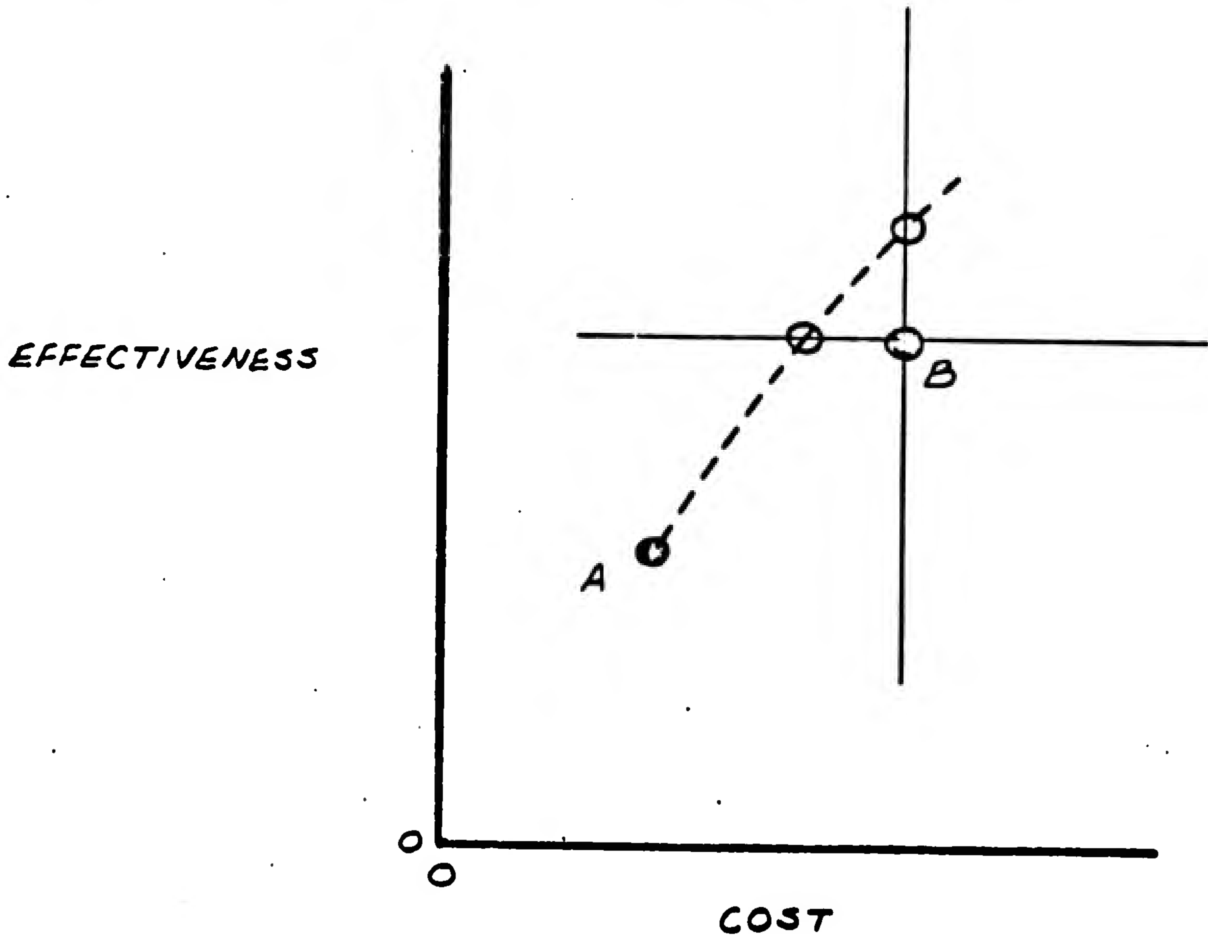


When that happens, the situation is ambiguous. There's no way you can choose between "A" and "B" without more information. The choice is easy, of course, if you say you can't afford "B", or if you say that "A" doesn't meet your minimum "requirement" for effectiveness. (I'm going to talk about "requirements" next.) But that's bureaucrat-talk, not analyst-talk.

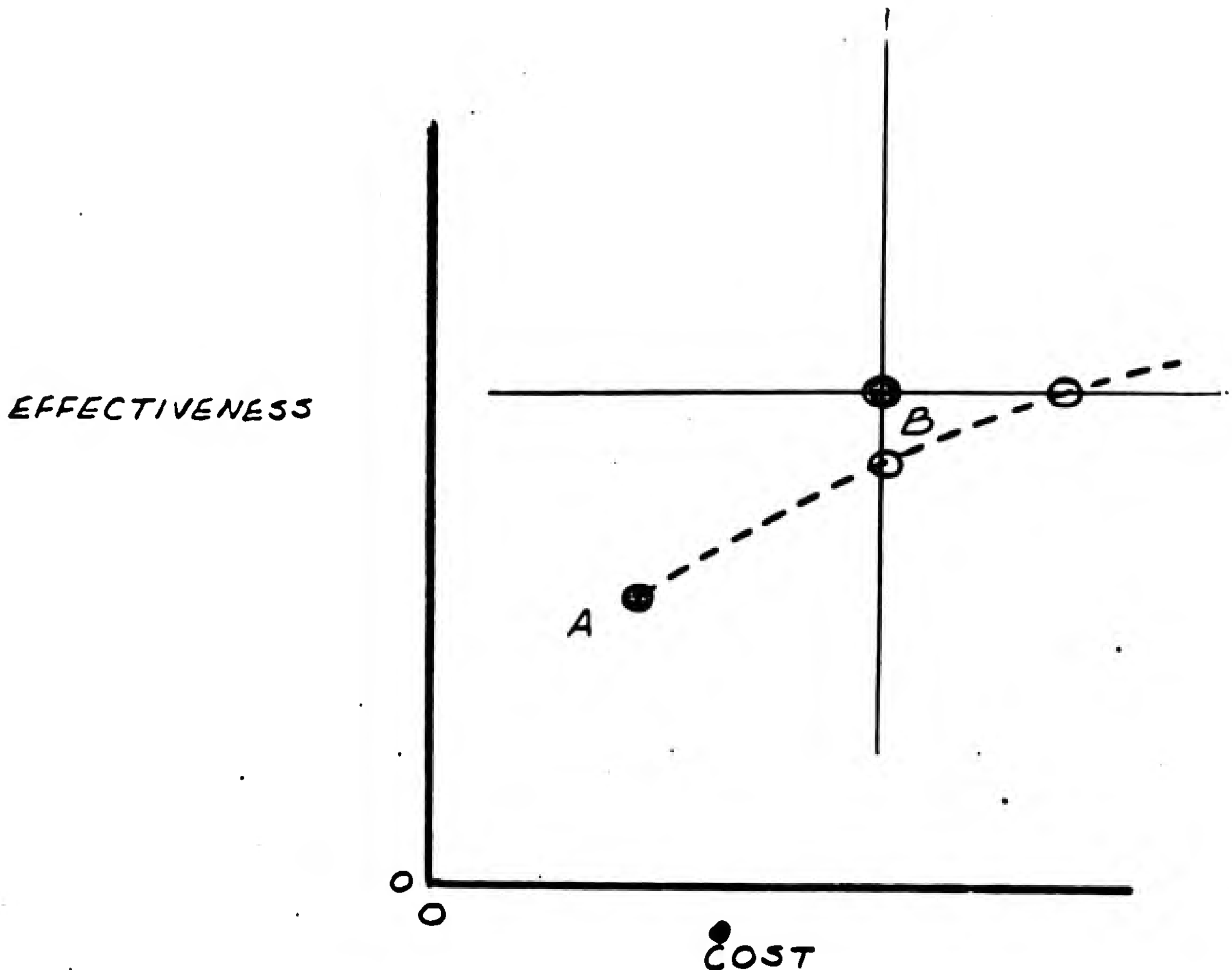
The reason you can't make the choice is that effectiveness and cost are incommensurables -- they're apples and oranges. If you buy "A" instead of "B", then you can take the money you saved and buy something else with it, say, schools. Then the question becomes "Are 17 schools more important in the general scheme of things than delivering 3 extra shiploads of equipment to NATO if a war should break out in 1983?" That's a hopeless question.

The only thing you can do to make a completely self-contained question out of this is to compare two systems that either cost the same -- so you don't have to worry about buying something like schools -- or have the same effectiveness -- so you don't have to compare education with ship deliveries.

What you've got to do is somehow shrink System B or expand System A until you get both alternatives to cost the same or to be equally effective. The question is, when you do that, will it come out like this:



so that "A" is the clear choice, or like this:



so that "B" is the clear choice?

I know that sounds like a page out of first principles of systems analysis, but that fundamental concept apparently does not come naturally to some CNA analysts. It should. It should be as ingrained and natural as the distinction between mass and weight is in the mind of a physicist.

I said I was going to talk about requirements next, so I will. "Requirements" is a word that's used in the Pentagon in a very special way that has led directly to one of the most perverse illusions in the defense business. (Pardon me while I climb on my soap box for a minute.)

In the Pentagon, the word "requirement" can refer to a piece of paper that defines the characteristics of a new piece of hardware. It's the thing that OpNav sends to NavMat telling them to develop a new Dingbat. Or it could refer to a commander's estimate of the forces he needs. Or other things. That's OK, provided it's recognized for what it is.

The problem is that people confuse the word with another word -- "requirement" -- which is spelled and pronounced the same way. This other word comes from the real world, and is used with reference to such things as the voting age in federal elections. If you are 17 years and 364 days old, you cannot vote because you do not meet the requirement. If you are 18 years old, you can because you do. Being 18 years old is a requirement.

So the two words mean different things. In the real world, it's a matter of yes or no. In the Pentagon, it's a matter of desire, or yearning, or even craving, but it's not a question of yes or no; it's not a matter of catastrophe on one side of the line and Elysium on the other -- even though many people act as if it were.

My advice is to wire that red light in your brain directly to microswitches in your eyes and ears that will operate whenever that word shows up. When the light lights, ask yourself whether you're dealing with a true requirement or not. Shape your analysis accordingly, and write your report without using that accursed word unless you really know what you're doing. End of speech.

The next subject is Arbitrary Probabilities. Among the more popular arbitrary probabilities are .5, .9, .95, and .99. These are best-sellers, but aficionados know lots of others. I have seen CNA reports where results are presented in terms of the number of Dingbats it takes to get a .9 probability of doing some particular military task. That's OK as far as it goes, but usually that doesn't go far enough. Usually, the .9 -- or whatever the number is -- has been chosen arbitrarily by the analyst. But it doesn't help the decision-maker an awful lot. If he's sharp enough, he'll then ask, what does it take to get a .95 probability, or a .5 probability, or -- as a matter of fact -- any probability? Preempt such questions by showing the data over a range of probabilities so that he can not only see what the specific answers are, but how they change as you demand higher and higher probabilities of success.

The next one is BOP analyses. BOP stands for Best, Optimistic, and Pessimistic. The idea is that you should cope with uncertainty by showing not only what you think will happen, but also what will happen if the fates are kind to you, and what will happen if things turn sour.

In CNA, there is a tendency to be what some people refer to as "conservative". That's a sort of ambiguous term, but it usually means taking the pessimistic estimates for your side and the optimistic estimates for the enemy. By doing that, the analyst thinks he's acting more responsibly. "Let the industry salesmen make the wild claims for hardware, and let the doves minimize the enemy's forces; we'll be the rock of incorruptibility", says that analyst. But don't you ever do it for that reason.

If you're going to make a full BOP analysis, and show all three results, fine. But if you're only going to show one, then use best estimates -- not pessimistic, and not optimistic. If you use pessimistic estimates, you tend to paint the picture blacker than it should be, and the decision-maker may despair of solutions that are actually within his grasp. If you use optimistic estimates, you tend to paint such a bright picture that the decision-maker may blissbully ignore serious problems that he should worry about.

But what about the analyst who makes a "conservative" analysis because he's deliberately building an a fortiori case? He's going to show you how good one alternative is by pointing out that even though he's stacked all the cards against it, it still looks best. That's a nice dramatic trick, and sometimes it's a useful way of coping with uncertainty -- just pick the extreme values. But be careful. One of your readers may find a hole someplace in your analysis, correct it, and come up with a different winner. Maybe the only reason it now comes out that way is because all the assumptions are still stacked. OK, you can do it over with more-balanced assumptions, and put your winner back in first place. But you may never get the chance. You may never know who's taken what action on the basis of his revision of your work. So unless you're both confident and gutsy, it's better to use best estimates throughout.

Next, a few points on costing or, more specifically, peacetime costs and wartime costs. For most of our work, the costs we ought to worry most about are peacetime costs, not wartime costs. That's not because we're deliberately being shortsighted, or because costs have no further meaning once war is declared. Not at all. The reason we should concentrate on peacetime costs is simple: we're trying to help the Navy Department decide how to allocate peacetime dollars. When that allocation is made, it's written down in the FYDP (the Five Year Defense Plan); all those dollars you see there are peacetime costs. But concentrating on them -- which we should do and which the Navy Department certainly does -- is not the same as keeping one's head in the sand. These peacetime costs are the costs of maintaining a wartime capability, and that's what we have to do in peacetime.

Sometimes we forget that. We did in a prepositioning study. The cost of the prepositioning included the costs of building the site, stocking it, defending it, and a lot of such elements. But one of the alternatives moved the sites far to the rear so that they wouldn't be so vulnerable. This alternative was then charged with the costs of moving the equipment forward from the rearward sites to where it would have been with conventional sites.

That wasn't right. All the other costs were peacetime costs. If you picked that alternative, those were costs you would be certain to incur, war or no war. But the cost of moving the gear forward was a cost you would incur only if war broke out. Adding it to the peacetime costs was not logical. Once you crossed that line, there were lots of other wartime costs you could think about adding -- turning on the lights in the warehouses so that you could see to unload them, paying death benefits to the guards who were killed in the first attack, and so on.

What should have been included in the case of the rearward sites was only the extra peacetime costs of maintaining the capability to move the gear forward if war broke out. That might mean buying trucks, keeping a staff of drivers ready to use them, and things of that kind -- costs you would be certain to incur in peacetime, but not costs you would be certain to incur only in the inherently uncertain event of war breaking out.

Generally, that doesn't cause a big problem, once the concept is clearly recognized. If the cost goes into the FYDP as soon as the program is approved, include it. But sometimes questions don't resolve themselves as neatly as that. We did one such study, which I'll paraphrase as being a question of whether it would be better to detect enemy surface ships by using an over-the-horizon radar or by using barriers of short-life sonobuoys. The costing was done on an annualized basis throughout. The procurement cost of non-expendable items, such as over-the-horizon radar, or the aircraft used to plant the sonobuoys, was annualized by dividing it by the expected life of the system. Thus, a million-dollar Dingbat with a 10-year life would be charged at an average rate of $100,000 per year. To that was added the procurement cost of a year's supply of expendables -- such as sonobuoys. And to that was added a year's peacetime operating cost for whatever you'd bought, and that gave you the total.

Annualizing procurement costs does introduce some troublesome artificialities, but if you're willing to overlook that, the rest of the costing methodology sounds plausible on the surface. But there's a big hole in it related to the peacetime/wartime cost problem, even though all those costs were clearly peacetime costs. The hole has to do with the assumption that you'd buy a one-year supply of expendables, such as sonobuoys. It probably seemed natural to the analyst at the time, since everything else was on an annual basis. But whether the system with expendables looks better than the one without expendables depends very much on that one-year assumption. Obviously, if you buy only a month's supply of expendables, or a week's, the system that uses them will look cheaper, but the system that doesn't use them won't be affected.

There's no way I know of to handle that problem other than to treat the duration of the war as a variable. Then the answer will turn out something like "'A' is cheaper than 'B' for any war lasting less than 6 months, but more expensive for any war lasting longer than that." Of course, it could be simpler if it turned out that the system with expendables was more expensive for any length of war. Or it could be a lot more complex if it turned out that the cross-over point was so far into the war that you'd have to think about whether you'd really have to buy all those expendables in advance, as opposed to building more when the war starts.

But regardless of those possible complications, the point was that this study's results were valid for only one narrow assumption about how long the war would last, and the conclusions could have been turned upside down for wars of other lengths. The problem lay not in the comparability of costs, but in the comparability of wartime capability.

Now, having said all that about how it's peacetime costs we're interested in, let me show you a quote from a draft report, which might seem to agree precisely with what I've been saying:

> "...NO ATTEMPT HAS BEEN MADE TO PREDICT WARTIME USAGE BECAUSE IN SUCH A SITUATION COST IS NOT A PRIME CONSIDERATION."

But that's not what I said. I said that we should concentrate on peacetime costs because we're trying to help people build good peacetime budgets. But I didn't say that wartime costs are unimportant. Wartime cost may not be <u>the</u>

prime consideration because cost alone is rarely the only consideration. But it surely is one prime consideration, even in wartime. The cost of Dingbats in wartime -- assuming the Dingbat is something you'd produce during a war -- is a surrogate for all the goods and services allocated to the production of Dingbats, as opposed to the production of anything else, such as aircraft, ships, rifles, or any of the other things needed to fight a war. And in war, as in peace, the available total of goods and services is limited. Ergo, a cheaper Dingbat is desirable even in wartime -- not because we'd be unwilling to spend more money during a war, but because it would let us produce more of the other things that we also need.

So while we concentrate on peacetime costs during peace, we shouldn't assume that wartime costs are never important.

So much for costing. Here's a quick one. One very nice CNA study looked at two alternative and novel ways of doing a job that's usually done another way. The study was very interesting and imaginative, and the two alternatives were thoroughly explored and compared. What was the problem, then? The problem was that there was no reference case to orient the reader. He could see that Alternative A was better than Alternative B in some respects, and all that, but he couldn't really see how either alternative compared -- in either cost or effectiveness -- with the traditional way of doing the job. It did look like both A and B were more expensive and more effective than the traditional way, but you couldn't really be sure, and you certainly couldn't guess whether the traditional way might not be as good as either of the novel ways if you were willing to pump that much extra money into it. So it's a good idea to include at least one case, if you can, that will orient the reader to something he's likely to be familiar with.

Next subject: realism. Sooner or later, analytical representations of the real world are bound to run up against the stops. Our job is to try to make that happen later, rather than sooner. Nobody expects perfection, but they do expect our best efforts in that direction. Lots of times, though, our analyses will be based on tactics that are at least unlikely.

As an example, in one study a question was how long a submarine with antiship missiles could stay within range of a carrier after the carrier went to All Ahead Full. In calculating that, the assumption was made that the submarine would parallel the carrier's course. That's OK if one's directly astern of the other, but if the sub's off the carrier's beam, his optimum course -- if he is trying to stay within range as long as possible -- isn't parallel to the carrier's, but at some angle toward the carrier. That's a little harder to model, but it can make a big difference in terms of how long the sub can keep the carrier within range. That was a case of forcing unrealistic tactics on the enemy.

Another common problem has to do with statistical inference. One study asserted that non-whites tend to be assigned to occupational fields where the promotion rates are inherently slow. That may be so. Maybe whites and non-whites are inherently equally promotable, while some occupational fields are inherently slower with promotions than others, and that's where the

non-whites tend to be assigned. But, maybe that's not so at all. Maybe there are equal promotion opportunities in all occupational fields, but non-whites -- because of discrimination, perhaps -- are not promoted as fast as whites. Then, whichever fields get the higher proportions of non-whites will automatically be those that show a slower promotion rate.

Of course, given enough data, you could sort that out statistically. But this report didn't do that. It merely associated the incidence of non-whites with the promotion rate, and jumped to the assertion. It's an easy mistake to make. I caution you to be very careful with statistical inference. Don't confuse correlation with cause-and-effect. Don't try to pry more out of the data than is really there, even -- or especially -- if what you pry looks interesting.

An old stand-by in the analytical business is the sensitivity test. This may be a case where long familiarity has tended to breed contempt. A couple of quick examples. In an analysis that paralleled the one I described a moment ago, the question was how long it would take a carrier to get beyond the passive sonar range of a trailing enemy sub. The analysis assumed one value for the submarine's passive sonar range, made the calculation, and then used that "escape time" all through a series of later calculations. Obviously, passive sonar range is a highly variable parameter that depends on a whole lot of things. So, clearly, the results should have been checked for sensitivity to reasonable variations in sonar range.

Another study that needed more sensitivity analysis was one that looked at what would happen if the Soviets tried in the future to interdict the oil SLOC between the Persian Gulf and the U.S. (SLOC means sea lines of communication.) To figure that out, you need to know how much oil is likely to be flowing through the SLOC, and to do that you need to estimate the growth in U.S. oil consumption between now and then. The report said that most estimates were that the annual growth rate would be between 3% and 5%. The study was based entirely on the 5% figure. But if the 3% figure had been picked, the oil flow from the Persian Gulf to the U.S. would have been cut roughly in half. That clearly would have changed the results of the study. An 8% growth would have almost doubled the flow. A 1% growth in oil demand would have meant no Persian Gulf imports at all, and that would have ended the study.

When you run into things like that -- figures you can't really be too sure of, but which obviously drive the answers all over the plot -- make a sensitivity analysis. When you run into figures you can't really be too sure of, but it's not clear that they have any important effect on the results -- make a sensitivity analysis. When you don't know whether you should make a sensitivity analysis -- make a sensitivity analysis.

I'm going to conclude with five examples of faulty analysis -- a small horror gallery. I'll put the worst first. The mission was to intercept Backfires. There were two candidate interceptors: the F-14 with 6 Phoenix missiles, and the "F-X", a 13,000-pound fighter with 4 Sidewinders and a very simple fire control system. The result of the analysis was that the F-X would kill 69% more Backfires than the F-14, on a plane-for-plane basis, not just an equal-cost basis.

Why did it turn out that way? There were two big factors: missile kill probability and aircraft availability. The analyst assumed that the single-shot kill probability of the F-X's Sidewinder was about twice that of the F-14's Phoenix. And he assumed that the availability of the F-14 -- because of its complex fire control system -- was much lower than that of the F-X. (I don't remember now how much lower.) Those two factors were the guts of the analysis.

To see how ridiculous that was, accept the idea that availability is solely a function of the complexity of the fire control system, and accept the idea that the kill probability of the Sidewinder is twice that of the Phoenix. If you believe that, then make the F-14 look more like the F-X. That's not hard: rip out the AWG-9 fire control and put in the F-X's rudimentary system, take off the 6 Phoenixes and replace them with 4 Sidewinders, and voila!, a 69% increase in the effectiveness of the F-14. But if you believe that, you'll believe anything.

Aside from his questionable estimates of availability and kill probability, among the things the analyst overlooked were:

- Differences in radar range between the F-14's AWG-9 and the F-X's rudimentary system.
- The F-14's simultaneous multiple-shot capability.
- Order-of-magnitude differences in missile range.
- All-weather capability versus no all-weather capability.
- The combat radius you might expect from a 13,000-pound F-X as opposed to a 75,000-pound F-14, and the difference in time you could use afterburner power without running out of fuel, and
- The credibility of the reader.

I cite that one not as an illustration of the kind of analysis I see everyday around here. Because I don't. That was one of the worst I've ever seen, and I've seen some pips. I cite it only as an illustration that it can happen here.

Here's one that's not quite as bad, although it did come from one of our senior analysts at the time. The problem was to calculate the probability that a U.S.-bound tanker in an oil SLOC would be shot at by a waiting Soviet submarine on any given day. The ground rules were that some of the tankers leaving the port were bound for the U.S., and the rest were bound elsewhere. In this game, the waiting Soviet submarine had to chase down any tanker it detected. If it was U.S.-bound, he shot it; if it wasn't, he didn't. (Don't ask me how he could tell the difference, but he could once he got close enough.)

The analytical procedure was this: 1) Figure the number of non-U.S.-bound tankers leaving on that day. 2) On the basis of the width of the SLOC and the detection range of the sub, calculate what fraction of those tankers are detected. 3) On the basis of how long it takes to chase down a tanker and see whether it's U.S.-bound or not, calculate how many hours a day the sub spends on these wild goose chases. 4) On that basis, calculate how many hours are left in the day to chase down U.S.-bound tankers. 5) Then, on the same basis as before, calculate the number of U.S.-bound tankers leaving the port that day, and the probability that they'll be detected by the sub. 6) Knowing the probability of detecting a U.S.-bound tanker, how long it takes to chase him down if he does detect it, and how many hours are left in the day to do it, you've about got your answer: the expected number of U.S.-bound tankers that get potted on that day is thus-and-such. What's wrong with that?

What's wrong with it is that it assumes that the non-U.S.-bound tankers are always the first to attract the sub, and that he goes after his real target -- the U.S.-bound tankers -- only in his spare time after he's finished his wild goose chases. By that rationale, if there are enough non-U.S.-bound tankers to keep him busy all day long, the U.S.-bound tankers will go scot-free. The proper solution is left as an exercise for the student.

Here's one that's a little more subtle. Everybody knows the usual expression for the probability of kill given "n" independent shots:

$$P_k = 1 - (1 - P_{k_{ss}})^n$$

WHERE: $P_{k_{ss}}$ = SINGLE-SHOT KILL PROBABILITY

n = THE NUMBER OF SHOTS

In the analysis in question, the single-shot kill probability was .17. There were 100 aircraft in the raid, and the defenses had time to fire 10 missiles. So the average number of missiles shot at any given aircraft is .1. Plug that into the expression above, and you get .0185.

$$P_k = 1 - (1 - .17)^{.1} = .0185 \qquad (.0185)(100) = \underline{\underline{1.85}}$$

If that's the kill probability facing each aircraft, and there are 100 aircraft, the expected number of kills must be 1.85, right? Wrong.

$$(.17)(10) = \underline{\underline{1.70}}$$

If each missile has a .17 chance of scoring a kill, and the enemy fires 10 missiles, then he should expect to kill 1.7 targets, not 1.85. (That's assuming he's smart enough not to fire two missiles at the same target at the same time, which would tend to make the number 1.7 even lower.) Why the difference in the answers?

The problem is -- in real world terms -- that you can't fire a tenth of a missile at anything. In fact, you can't fire any non-integral number of missiles at anything, and that expression is good only for what you can do in the real world; it's good only for integral values of "n". It gives you especially bad answers for values of "n" less than one, and for single-shot kill probabilities close to one. For example, if the $P_{k_{ss}}$ had been .9 instead of .17, that expression would have predicted 20.6 kills, instead of 9, the correct number.

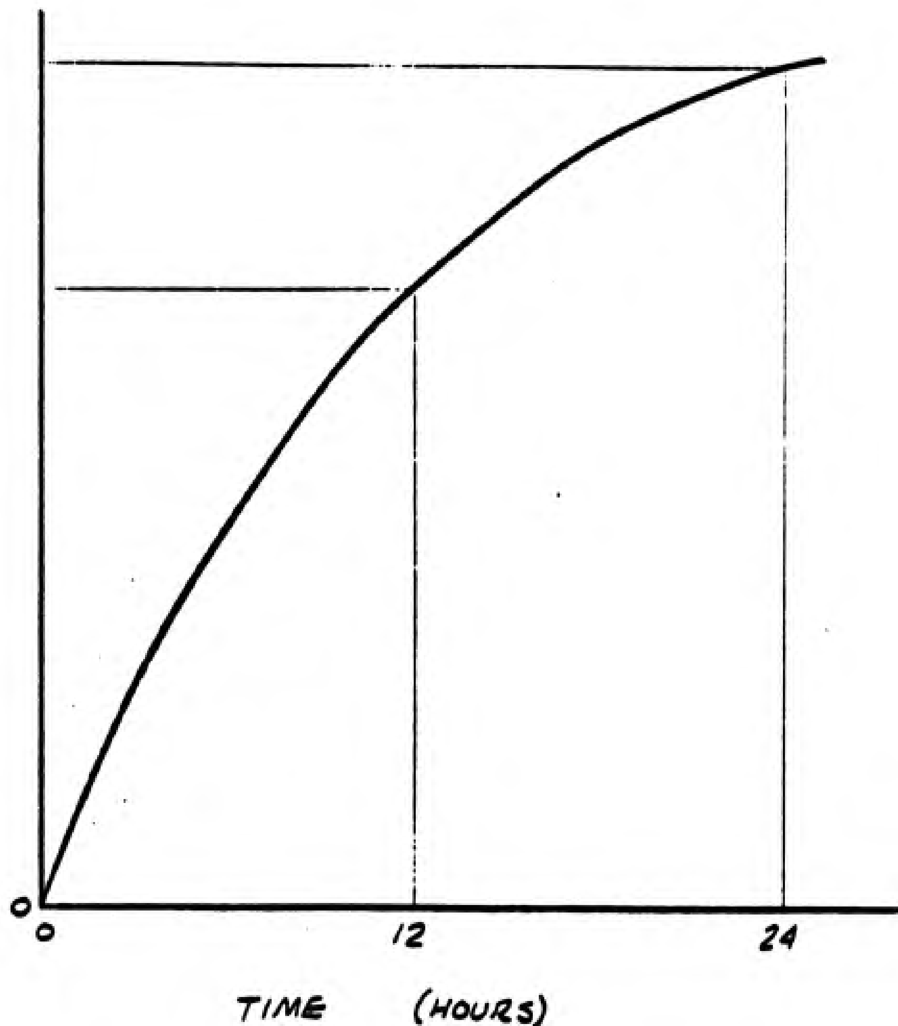
While I'm on the subject, as a sort of aside, there's another problem we sometimes run into with non-integral numbers. Some people are put off by non-integral numbers -- not just because they give you goofy answers when you plug them into expressions like that last one -- but because they're unhappy with analyses that assume you buy 3.7 carriers, or that you build 21.4 prepositioned warehouses, or that you send 44.8 aircraft on a mission because your inventory is 56 and they're 80% available. So people tend to adjust those numbers to integers and carry on with the analysis.

That can lead to problems in interpreting the data. Where you should be getting a nice smooth curve, the thing turns lumpy and hard to interpret. When you can, therefore, save the fractions and preserve the shape of the curve. If you then want to convert your answers into real-world, integral possibilities, fine. Or if you're interested only in real-world, integral possibilities and don't care about easily-interpreted trends, forget what I've said. But in any case, ask yourself what it is you want to find out about before you decide whether to stick to integers or not.

My next-to-last example has some elements of the previous one, in that it involved the daily probability of something happening. In this case, it was the expected number of subs killed per day by an MSS buoy field/VP combination or, more precisely, the probability that a sub would be killed during a 24-hour period.

The drill was that the MSS field would listen for the sub. When it made a detection, the VP would take off and fly out to the field. The next step was for the MSS to redetect the sub so that the VP could start localization. The assumption was that if the MSS didn't get this second detection within 12 hours, the VP would give up and go home. The rest of the process isn't important. The important point is how the analyst handled this much.

He started with a curve of the cumulative probability that the MSS field would detect the submarine as a function of time, like this:



Since he was interested in the daily rate, the analyst picked the probability that the MSS field would make its initial detection off this plot at the 24-hour point. For the probability of the second detection -- the one the VP needed to begin localization -- he picked the probability for the 12-hour point, since that's how long the VP would stick around before giving up.

That sounds plausible on the surface, but it's not. An easy way of seeing the fallacy is to think about what would have happened if the analyst had repeated the same calculation for the expected number of subs killed per half-day, instead of per day. Surely that should be just half the daily rate. Yet it's obvious from the shape of this curve that that's not the answer you'd get. You'd get an expected value that's more than half the daily rate.

The fundamental illogicality is the implicit assumption that the first detection, if it occurs at all, always occurs after precisely 24 hours of waiting, and

that the second detection, if it occurs at all, always occurs after precisely 12 hours of waiting. Of course that's not right -- either detection can occur at any point during the waiting period.

The proper solution would have been to use an expected-value model based on a logic-tree that accounts for the various branches and probabilities.

The last example I have is one that bears on what I was talking about earlier -- realism. When you build an analytical model, it's a good idea to try to visualize what would actually happen in the real world. In this case, the hypothesis was that Soviet Bear reconnaissance aircraft would fly out of Cuba and look for ships in the Atlantic. For reasons I won't go into, we wanted to put a U.S. aircraft on his wing -- a sort of airborne tattletail operation -- for the whole time he was in his designated search area.

The calculation was that once the Bear had reached the corner of the search area nearest Cuba, he had 10 hours of cruise time (that's not the actual number, of course) before he had to be back at that corner if he was going to make it safely back to Cuba.

There was a parallel calculation that a particular kind of U.S. aircraft could meet the Bear at the corner, and stay with him for 5 hours (that's not the actual number either) before he had to head for home.

The analyst was interested in how many U.S. aircraft it would take to do the job. So he divided the Bear's 10 hours in the area by the U.S. aircraft's 5 hours, and came up with 2. Logical enough -- one U.S. aircraft tails the Bear for the first 5 hours, and is then relieved by a second U.S. aircraft which takes the last 5 hours. Right? Wrong.

The only way that can be right is for the Bear to be kind enough to return to the starting corner at the 5-hour point for the tattletail turnover. If he doesn't do that, but instead arranges to be at the farthest corner of the search area at the 5-hour point we lose both our aircraft. The first one can follow the Bear out to the corner, but he hasn't enough gas left to get home from way out there. The turnover was supposed to occur at the starting corner, which was close to the U.S. base. Of course, we lose the second one too, for the same reason, if he's dumb enough to try it.

That analysis suffered from a lack of realism. The analyst didn't think enough about what he was doing. And it wasn't just a matter of you-can't-get-there-from-here or, rather, you-can't-get-back-from-there. There were other practical considerations. If you don't know where the Bear's going to go, how do you know when to send a relief aircraft? If the Bear flies back toward the U.S. base, you'll probably send the relief too soon. If the Bear flies away from the base, you'll probably send the relief aircraft too late. And so on. The real message should have been: if you really want to fly wing on a Bear, pick an aircraft that can stay up with and as long as a Bear.

Well, Lord knows I've gone on long enough. I hope I haven't bored ycu to death, and I hope I haven't preached too much. I ought to end with exhortations about not throwing rocks at your fellow analysts when you may be living in a glass house yourself, and not making any wheels-up landings in the future, and how most of our work is really much better than this, but we should still try harder to be Number One. But you've stuck with me much longer than I had any right to expect, for which I thank you.

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