from data analysis: be on the lookout for unforeseen effects. Non-epistemic observation is a phrase that philosophers have used for the phenomenon that one may have observed something without knowing it at the time. The top category, which is the most active category, in which one performs some act of making or doing is sometimes referred to in philosophy as *verum factum*, knowing through doing.

It is instructive to take what is now commonly accepted as the iterative and cyclical nature of the scientific method and quality improvement (see the Shewart–Deming chart) and trace back the ideas several hundred years. Here are two of my favourite quotations. Claude Bernard (1865), in his book on experimental medicine, states, "An experiment differs from observation in this, that knowledge gained through observation seems to appear of itself while that which an experiment brings us is the fruit of an effort that we make with the object of knowing whether something exists or does not exist." And a very Baconian quote from Jevons (1874), a principal critic of Bacon himself, reads, "It may readily be seen that we pass upwards by insensible gradations from pure observation to the determinant experiment. . . . The successful investigator must combine diverse qualities: he must have clear notions of the results he expects and have confidence in the truth of his theories and yet must have that candour and flexibility of mind which enable him to accept unfavourable results and abandon mistaken views." It is the responsibility of statisticians of whatever type to rescue the practical side of the philosophy of science, particularly as embodied in the design and analysis of experiments, and deliver it to the world. This leaves an admirable role for the academic statistician, who may have a theoretical bent, which is to try to continue to understand the dynamic nature of the whole process.

Rejoinder

David Banks

I thank all the discussants and am pleased that their spectrum of opinion confirms my belief that important controversies underlie the current practice of industrial statistics. From the comments, my sense is that statisticians with experience in manufacturing industries find broad agreement with the paper, in contrast to academic statisticians whose research is in industrial statistics. Between these extremes, traditional academic statisticians tend to be favorable, but industry statisticians in quasi-academic environments (the Bell group) are less so. If there is a latent variable that explains this ordering, it may reflect the degree to which these researchers are expected to simultaneously justify their work on two criteria: statistical merit and practical value.

There was strong consensus on statistical education. I think most agree that the best single thing academic statisticians can do for industry is to revise program requirements in the terminal M.S. degree. The current curricula contain enormously too much inference and probability, but insufficient exposure to a breadth of toolkit topics. The precise content of an ideal curriculum is debatable, but the discussants have persuaded me that it should give conceptual coverage of survey sampling and categorical data.

TQM evoked more diversity. Many discussants were uncomfortable with my claim that the TQMperor has no clothes. I concede that the ubiquitous expression of the contrary view may have seduced me into overstatement. A possible compromise is that the TQMperor has really nice underwear. Specifically, I can agree that TQM has useful but staggeringly simple ideas. Their connection to statistics is generally slight. A manager who does not incorporate what is good is doomed. Managers should assess each TQM precept thoughtfully, in the context of their own situation, and not expect that adoption of TQM is sufficient, or even primary, for their survival.

Stitching together several of the discussants' comments, TQM may be successful because it creates a corporate climate in which statistical reasoning can flourish. Insofar as statistical thinking replaces wishful thinking by factual evaluation, it can only benefit industry. In particular, decision makers should have a grasp of the principles of experimentation, the critical weighing of evidence and random variation.

Some discussants felt that the overview of industrial statistics was narrow and incomplete. Doubtless, it was incomplete. And I agree that it spoke more to conventional manufacturing than to the R&D environ-

ment, or to the quasi-academic environment at the highest statistical stratum in the telecommunications field (my thanks to Drs. Cnaan, Duffy, Nair and Pregibon for partially correcting this imbalance). However, my preference was to emphasize issues that concern the majority of industrial statisticians. These people confront TQM propaganda regularly, often employ the conventional tools described in Section 3 and may soon be called in on the newer problem areas described in Section 4. Many industrial statisticians have only an M.S. degree, or work through people who hold such a degree, and Section 5 presents feedback one hears regarding facets of the M.S. education that are useful, neutral or must be unlearned.

Beyond these common themes, each discussant contributed unique perspectives. I address these individually, by discussant. Space limitations preclude much exploration of areas of substantial agreement, of which there are far more than I had expected. But there is still considerable disagreement, and I am glad that all participants entered so freely into the debate. And I feel compelled to point out that the quality and substance of their contributions offers a counterexample to the TQM dictum about the vital few and the trivial many.

PROFESSOR CNAAN

I am grateful to Professor Cnaan for her strong seconding of some of the more controversial opinions in the paper, especially since she has direct experience of the industrial milieu.

Regarding the short shrift I gave to statistics for industrial R&D, I hope the preceding remarks offer explanation, if not justification. Regarding the pharmaceutical industry specifically, I agree that it has features which distinguish it from general manufacturers and that its relatively sound footing is due to the several advantages which Cnaan so clearly describes. It would be interesting to compare statistical practices between the automotive and pharmaceutical industries, broken out by product development, product manufacture and higher management.

Professor Cnaan's description of the research activity in biopharmaceutical statistics is quite encouraging. I am less optimistic than she about the magnitude and breadth of that work, but then she is much better informed about it than I. Nonetheless, I would suggest that the establishment of new publications, such as the *Biopharmaceutical Report*, may mark a field's maturity rather than its intellectual fecundity. Similarly, the papers Cnaan mentions, regarding a unified approach to safety analysis and statistical education as it relates to the biopharmaceutical industry, seem to support my thesis that the activities in that area are unlikely to profoundly change the face of statistics. But I do not doubt that these represent good and useful work which could have substantive value for business. Insofar as such research helps industry succeed, I strongly advocate its pursuit.

DR. DUFFY

As she emphasizes, Dr. Duffy's experience at Bellcore is probably different from that of statisticians who practice in more conventional environments. This makes her comments doubly valuable.

In her second section, I did not understand the thrust of the part on collaboration and consultation. Most people's ideas on this are sufficiently similar that little needs to be said; nonetheless, her synopsis is a clear capsule of the prevalent thinking. Although it seems tangential, I would distance myself from her summary only to the extent of emphasizing that there is a continuum between collaboration and consulting, rather than a dichotomy. My real confusion arises from her segue in the final paragraph, regarding her sense of my view of one's co-workers. She seems to feel that in-house teaching of simple statistics precludes the education that occurs during consulting or collaboration, which was not my intention.

In the third section, I have the feeling that she is trying to stuff me into a semantic box. Apparently my skepticism of the intellectual importance of recent publications in experimental design and control charts has been misinterpreted to mean that industry should ignore those areas. Rather, it is my hope that academic journals will begin to ignore those areas and that industry will proceed to adapt such well-studied methods to their needs. I do not see this tailoring activity as vigorous research—it has financial value, but not intellectual value. Thus the activity should be richly rewarded by industry, but it needs and merits no academic kudos. Dr. Duffy asks: "I wonder if Banks believes that a statistical problem must have intrinsic mathematical interest to qualify as research?" My answer is yes; solutions to problems that do not have statistical interest do not constitute statistical research. Obviously, Duffy wants to use a much larger definition of research than I, but the point she criticizes in this aspect of my article concerns the co-occurrence of real statistical research and honest industrial problems. I was not attempting to point out lines of research which might enrich industry but be statistically sterile.

Also, I regret that she finds my optimism regarding the possibility of fundamental contributions by academic statistics to industrial problems "more than a bit amusing." Drs. Hahn, Nair, Pregibon and Duffy herself list areas in which the power of the academic perspective is only beginning to be applied. I think research statisticians can and are developing new un-

derstandings of nonparametric regression, high-dimensional response surfaces, speech recognition, neural networks, software reliability and so forth. Achievement in these areas will not happen in a partnerless vacuum, but their intellectual cores are clearly and entirely statistical. Duffy sees small hope of swift progress on these fronts, favoring an incremental model of advance. I agree that evolutions are more common than breakthroughs, but hope people will continue to set their sights high. If Duffy seriously doubts that academic statisticians can make fundamental contributions to industry, then her views are far more controversial than mine.

I thank Dr. Duffy for her update on progress in software reliability and for her discussion of the interface between statistics and computing.

- I am sorry she feels I overlook computing—its importance is a tub I am happy to thump whenever asked, although the sentiment has become a bromide. If the centrality of computing to high-dimensional response surface analysis, geometric conformance and so on, was so implicit that the reader was misled into thinking I saw no role for computing in statistical research on industrial topics, then I am grateful for this opportunity to clarify.
- Duffy's reading (and uncontextual quotation!) of what purports to be my view of the value of data visualization is uncharacteristically ungenerous. While discussing things a terminal Master's degree student should know, I stated that graphics are alleged to be useful in making presentations to upper management. That comment disparaged management, not graphics, and ought not be selected out as a judgment on high-level research in data visualization.

I think Duffy's emphasis upon using the computer to solve real problems is correct. I also applaud her willingness to overlook discipline definitions and boundaries—being finicky about such points is a barrier to problem solving.

Regarding education, Dr. Duffy and I find much more agreement. And in her miscellaneous comments, I like her plan to modify software interfaces to make thoughtful analyses more natural. I also agree that industry's eager emphasis on profit can be distasteful to researchers accustomed to the academic ideal of intellectual cooperation and amity.

DR. HAHN

Dr. Hahn's obvious relish of the article, both in his concurrences and his objections, is what makes this kind of writing fun. I am grateful for his gusto.

Regarding "And Now for Some Reservations," I agree with most of the softenings and cautionings Hahn makes. But there are three of his five points (paraphrased in italics) on which I want to put a bit of spin.

- *Few statisticians really believe that the Japanese "use sophisticated quality control methodology."* My paper says this belief is most common at the interface of statistical and management communities, and I suspect that pure statisticians have a more balanced grasp of the level of Japanese sophistication than pure management. But there are knowledgable and prominent statisticians who disagree with Dr. Hahn and myself (e.g., Professor Wu asserts the direct opposite in the first point of his discussion). In any case, my real point about the perceived sophistication is that this argument is used to justify research funding, when the same money might more efficiently be spent in teaching elementary statistics.
- *Use of customer information on noncompliant parts is reactive, whereas we ought to be proactive.* I agree that the reactive approach of the past is insufficient, but I do not think it should be entirely discarded. The proactive strategy is needed to improve outgoing quality and manage the daily process control, but it depends on model assumptions that may become invalid. When the customer provides feedback that the number of bad parts has increased, then that information should surely take precedence over estimates based upon control charts or inline measurements. I would be reluctant to advise any company to stop capturing estimated rates of bad parts from the customer.
- *Mathematical proficiency generally implies good practical skills.* When I suggested the association was negative, I was only semiserious. Subsequent reflection leaves me more convinced of my point than before. I hope Hahn is correct, but optimism is undermined when I glance at the authors in the most recent issue of *The Annals of Statistics*. With a few conspicuous exceptions, I am doubtful that most could handle the range of problems that arise in statistical practice, allotting due attention to the practicability and relevance of their solutions.

On other issues, Dr. Hahn's cautious view of partial least squares' popularity is probably wise, and he has persuaded me that some M.S. graduates need to know survey sampling. I think we actually agree about the desirability of exposure to time series—probably we both feel an M.S. student needs the ideas more than the derivations.

Regarding the "Future Directions" section, I like Hahn's examination of the failure of academic statisticians to capture strategic territories, such as neural networks. To some extent, it is unavoidable; if we hold ourselves to a theorem-based standard of understand-

ing, we often lag behind clever people who get out and do sensible but theoretically intractable things. But that limitation can also be a strength.

I tend to agree (in varying degrees) with most of the other points in "Future Directions." There is an interesting conjunction between Dr. Hahn's vision of computer software becoming ever more user friendly, despite the potential for abuse, and Dr. Duffy's plan to build interfaces predisposed to avoid abuse. I also like Hahn's list of intriguingly difficult challenges, but some, such as the conduct and evaluation of analytic studies, probably require considerably more definition before any solution strategy can emerge.

Dr. Hahn's recommendations in the "We Need Broader Interactions" section are sound and should be pursued. Probably, to a greater degree than Hahn already appreciates, academic statisticians collaborate around the university departments. As he emphasizes, it is fun, interesting and sometimes leads to better work. But I was startled by his belief that academic statisticians are starved for real problems or that we do not have long mental lists of problems we want to come back to some day. Perhaps our groups are most similar in that we are both always pressed for time.

PROFESSOR HOGG

Professor Hogg's discussion makes me eager to read his "A quality journey" paper. He appears to own an arsenal of anecdotes that speak to the issues I have addressed. Moreover, he shows a great deal more patience with packaging than I possess, which better enables him to appreciate the incidental excellences of TQM.

It is delightful that someone who has spent so much time in the scholarly scrutiny of TQM finds considerable room for agreement with the points that I make. In that same constructive spirit, I emphasize that the strengths Professor Hogg notes in TQM are not to be scorned, and any company that does not acquire them faces Darwinian doom. But I think we both agree that much more needs to be done, and the simple lessons of TQM must be followed by more complex adaptations to the economic ecology.

Professor Hogg's views on statistical education repay attention, especially since he has been one of the strongest champions for radical change in the M.S. programs. I find his arguments for the inclusion of a significant component of quality culture topics unpersuasive, but that is my only reservation. I have no doubt that the program he proposes would be enormously more effective for industry than what is currently in place.

I also note that Professor Hogg was the only one of us to mention the shortfall in the educational quality of the U.S. workforce. This issue is not explicitly statistical, but his discussion points out the possibility that statistical predigestion of data may mitigate this economic handicap. Additionally, it is implicit that the U.S. school system is a conspicuous example of an institution whose platform is burning and which has failed to learn even the simple TQM lessons for improvement.

DRS. NAIR AND PREGIBON

I am sorry that Drs. Nair and Pregibon's reading left them with the impression that I am pessimistic about the future of industrial statistics. That so much is currently done so poorly fills me with the conviction of future potential.

Like Dr. Hahn, they argue that statisticians have ceded several interesting problem domains to the engineers without contest. I agree with both the premise and the conclusion that we should be more proactive; however, I note that there are several agenda here. On the one hand, it is clearly desirable to make better IC wafers and speech recognizers—this will benefit industry. On the other hand, it is not clear that participation with industry (specifically, in the kinds of research problems they suggest) will yield much benefit for statistics. It is a glib and unexamined platitude that real problems engender pathbreaking statistics. Sometimes this happens, but we must resist the false inference that because a problem is practical and may enrich stockholders, it is therefore likely to produce new understanding that advances our field. Often, applied work simply produces a rechurning of socially useful but insipid statistics.

I agree with Nair and Pregibon that the creation of a fact-based, statistically sophisticated corporate culture is a great step forward. If TQM is the necessary bandwagon to achieve this, then by all means, people should hop on. But there are other ways, and this particular bandwagon may predispose its passengers to overlook useful ideas.

Also, I am grateful to these discussants for sketching aspects of education and training for industry that occur in Japan. This is an area that was not addressed in my article, but which has clear relevance in assessing the value of training programs in U.S. corporations.

DR. ORCHARD

Dr. Orchard's management experience balances the seasoning in the discussion. His third, fourth and sixth points emphasize the requirement for courtesy among all partners in an enterprise, between suppliers and customers, managers and employees and even statisticians and clients. It may be that this lesson is the most important insight we can glean from Japan. Relentless use of courtesy can drive out fear, open lines of communication and foster teamwork. For statisticians,

the chief implication is that we must talk with our clients and collaborators, rather than lecture them on the inadequacies of their data or their need for a short-course.

Regarding Dr. Orchard's fifth point, I fear that the good ideas in Dr. Deming's philosophy are not exhaustive and that an industry which assiduously seeks the local solution of disseminating the Deming perspective throughout its ranks will be surpassed by competitors that pursue global strategies. Industry ought not ignore what is worthwhile, but blind Demingism closes the door to more sweeping improvements.

DR. ROBINSON

I very much like Dr. Robinson's distinction between management statisticians and problem-solving statisticians. Clearly, these groups face different professional challenges, even though the same person may commonly wear both hats. In particular, their relationships with TQM are likely to be different.

Robinson points out that I do not directly address the question of whether statisticians should take a role in changing the managerial climate. This is true, in part because I cannot imagine any argument whereby someone would conclude that a statistician (or engineer, or accountant or any knowledgeable professional) should avoid such a role. Recent history has documented patterns of management error that demand the activity of anyone capable of positive intervention. From the perspective of this imperative, statisticians have some unique tools, but tools from other areas are also needed. I do not foresee statisticians becoming the philosopher-kings of corporate city-states, but I hope upper management echelons will grow wise enough to include people with formal training in decision making under uncertainty and a sturdy sense of what data can tell.

Regarding Robinson's list of the characteristics of industrial statistics that bear on comparisons with other areas of application, I am in close agreement with virtually all of his points. However, regarding "Importance," I do not concur that management statistics is the most important of all areas of statistical consulting, because I do not agree that the willingness of customers to pay is the appropriate criterion for judging one's work.

PROFESSOR WOODALL

I hope my longstanding friendship with Bill Woodall has not evoked a level of restraint in his commentary that misleads readers into overestimating his acquiescence with my arguments. But I am delighted that at least one of the discussants undertook a cogent rebuttal of my views of academic research on control charts.

Were I the kind of polemicist who stooped to cheap rhetorical tricks, I would restate Professor Woodall's justification of control charts as an argument from economic necessity, on the grounds that they enable process-ignorant people (or computers) to better manage product critical operations than they could otherwise do. Since unskilled people (and computers) are cheaper than experts, American industries can hope to improve their balance sheets by ramping up the incompetent with a course in control chart rules and then relinquishing to them the task of daily process oversight. I would then close by stressing Woodall's concession that control charts are "no substitute for knowledge of the process."

But such casuistry is unworthy of the subject, and Woodall's points are important. There is a dearth of skilled people and much pressure to automate control so that only anomalous process behavior is directed to expert attention. My sense is that it is dangerous to walk far down this road, since:

- Regular inspection of routine time series charts is a key method whereby a process engineer becomes a process expert.
- Handing process-ignorant people an oversimple tool, such as control charts, and then teaching them that it gives official statistical pronouncement on when to declare a process emergency, will surely undermine either process productivity or the credibility of our profession.

I gather that in Japan, it is expected that managers and workers be intimately familiar with the processes they control.

Professor Woodall slightly misinterprets my perspective when he states that "Much of Banks' criticism of research on control charting appears to stem from the assumption that no decision rules are needed with a time series plot to form a control chart." Rather, my view places much importance upon the use of good decision rules, but I do not believe the off-the-shelf decision rules available from control chart research provide more than a first-order approximation to the rules that are needed. The appropriate decision rule for evaluating a time series plot depends sensitively upon the process and should not be developed without the participation of process experts. The decision rule that confers competitive advantage in the production of sheet aluminum is different from that which applies to the production of sheet glass, but statisticians persist in cutting the same 3σ measure of cloth for all customers. A good decision rule is probably complex and conditional upon knowledge of recent perturbations in process inputs and controls.

Professor Woodall's correction about the EWMA chart is appreciated, and we may be in generally closer agreement than he suspects. Davis and Woodall (1988)

show laudable skepticism of one of the canonical decision rules, and I would encourage the extension of that skepticism to the entire corpus. Although my description of their work as overparticular is perhaps extreme, it still strikes me as straining at gnats when there is an important camel to swallow. Similarly, I do not discount the value of modeling, but I am alarmed when its conclusions are applied ubiquitously, with essentially no assessment of the validity of the model for the application in hand. Modeling has carried control charts as far as it can, and new improvement can best derive from:

- the development of methods for tuning decision rules according to the past performance of particular processes, and
- the use of control chart ideas for diagnosing process flaws and identifying appropriate recovery action.

Both of these directions depend upon the use of specific process histories rather than conventional model-based research.

Regarding the textbooks, I welcome the comparison of someone who has taught both Ryan and Montgomery. We agree that Ryan's exercises are thin. Although Ryan often directs the reader to other sources for details on particular topics, my concern vis-à-vis Montgomery is that at least Ryan does not omit key areas, such as regression. But, following the spirit of my critique of control charts, Professor Woodall's experience of the teaching process with both books may be a better guide than my automatic evaluation of their relative coverage.

PROFESSOR WU

It is unfortunate that Professor Wu considers my paper irretrievably biased. As the former director of one of the most important consortia, the Institute for Improvement in Quality and Productivity at the University of Waterloo, he enjoys a unique perspective on the commerce in quality improvement between academics and industry.

Regarding Professor Wu's first point, he appears to have misread the relevant section. Section 2 does not assert that statistics plays a small role in product quality, but rather that statistics played a small role in Japan's economic rise. Surely statistics contributed a bit, but the lion's share of credit belongs to intelligent management. Japan found a trajectory of strategic industrial build-ups that matched capital, resources and markets to the evolving international economy. This happened because management planned in terms of decades rather than quarters, confronted problems rather than cosmeticizing them, valued long-term research and paid serious attention to its customers. In fact, Japanese management was quite unimaginative—they mostly did only the textbook things that competent management ought to do. Their success testifies to the failure of U.S. management, not Japanese acumen.

I have the sense that Professor Wu would prefer me to say that Japan's achievements reflect their management's commitment to the use of simple and sophisticated statistical techniques. I agree that their management generally has such commitment and that a corporate culture which weighs evidence more than internal politics is a good thing. But to credit Japan's success to SPC is simplistic.

Regarding Wu's second point, we both agree that the seven quality tools are simple. He emphasizes the selection process, and JUSE certainly did a good job. But it is evidence of the blindness TQM mythology creates when Professor Wu accepts their judgment uncritically. Professor Hogg has made a strong argument for the inclusion of a runs chart and (implicitly) some form of the process capability index; in contrast, the checksheet is valuable but not directly statistical. I would like to see the control chart disenshrined, and the Pareto diagram modified to account for costs, rather than numbers. And there is nothing magic about the number seven. It might be worthwhile to know the eighthmost important tool on JUSE's list—it could give us a competitive advantage.

Regarding Wu's third point, and specifically his defense of Taguchi's parameter design, he seems to be putting unvoiced opinions in my mouth. My assessment of the future of Taguchi's work in subsection 3.3 is markedly more sanguine than corresponding comments on classical experimental design in subsection 3.1. But I do think that statistical research in parameter designs is nearing its zenith. Applied research will continue to be done and I hope the ideas find a permanent place in the corporate toolbox, but I expect the fertility of such work for statistical growth to decline.

Championing the importance of traditional design, Professor Wu points to his paper with Chen in *The Annals of Statistics* and his work on combinatorial challenges in supersaturated designs which is about to appear. He also mentions a very short and engaging paper by Lin (1993), which I recommend to anyone contemplating use of a supersaturated design. I see two grounds on which to assess such work: the practical payoff to industry and the degree to which it advances mathematical statistics. Lin's work has value on the first criterion and is attractively unpretentious on the second. But it is hard to imagine that the technical results in Chen and Wu's paper will affect the growth of U.S. industry, and I invite readers to examine their paper and decide for themselves the eminence it will likely attain.

Implicit in this discussion (and also in Dr. Duffy's

distaste for the "propensity to be ever enamored of what is new") is the fascinating problem of measuring progress in a scientific field. I think most people agree that progress is not uniform and that physics, mathematics and chemistry have enjoyed different periods of florescence which historians of science can clearly identify. Citation analysis purports to offer a basis for such measurement, but there are significant methodological problems; moreover, in forecasting emergent and declining specializations in statistics, we lack historical retrospective. I know no better guide than educated taste and so remain skeptical that experimental design and control charts will develop in new and important directions. But the topics I list in Section 4 seem promising, in part because, as Wu indicates, they are still in their infancy.

Respecting Professor Wu's fourth point, he knows far more about the business of extracting money from industry than I, and I should defer to his experience. I agree that most statisticians have not been as conspicuous in this as computer scientists, and I do not doubt his comment that statisticians who have secured industry funding must work hard to keep the money coming. My chief concern is that the research services academic statisticians want to supply may offer a poor match with the real, but less intellectual, needs of the industries whose support is sought. From this perspective, exotic academic research on "control charts and oddly balanced designs" may be an example of a TQM failure to properly assess the customer's requirements.

Finally, I reassure Professor Wu that Carnegie Mellon shows intellectual respect for work in industrial statistics. Properly, it reserves its greatest respect for work that also enriches the field of statistics.

PROFESSOR WYNN

Professor Wynn urges the breakdown of barriers between the engineering and statistical disciplines. I agree with him (and Drs. Duffy and Hahn) that this leads to better industrial performance. Sometimes it also leads to better research science and new statistical problems. In any case, every industry statistician should be prepared to roll up their sleeves and learn whatever engineering they need in order to make their collaborations and consultations more effective.

The philosophical aspects of Professor Wynn's defense of experimental design are intriguing [even to a philosophically challenged person such as myself, whose eyes glaze when a savant pulls (the magic number) seven modes of intervention out of the air and constructs a theory from them]. But whatever separate paths we follow, I think we reach similar conclusions. Wynn sees a bit more that can be done in unifying the diversity of experimental design strategies, but concurs that industry needs an intuitive appreciation of statistical experimentation far more than it needs another paper on an arcane design.

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