A Challenge for Researchers
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In the science Computer Science (as opposed to mathematical CS), we face a rather special challenge: We Build What We Measure. We don't really have Nature to test our theories for the structure of systems. This makes it very difficult to know what is principle and what is an artifact of the engineering decisions.

This has lead to a somewhat "any thing that works is good" attitude and has contributed to the master craftsman approach that seems to dominate recently. History indicates that artisan approaches come to rely on tradition and ultimately stagnate. We are already seeing this.

This is not science. In science, we construct and disprove theories. Those theories not yet disproven are working models. In software systems it is possible to make almost anything "work." The question then is how do we choose our theories without nature to test against? Is it a popularity contest? Which company has the most money to put behind it? This too is not science. What criteria should we use? After much thought on this problem looking for a neutral basis for criteria, I was drawn to Newton's *Regulae Philosophandi* of 1726 (as paraphrased by Gerald Holton):

1. Nature is essentially simple; therefore, we should not introduce more hypotheses than are sufficient and necessary for the explanation of observed facts. This is a hypothesis, or rule, of simplicity and *verae causae.*

2. Hence, as far as possible, similar effects must be assigned to the same cause. This is a principle of uniformity of nature.

3. Properties common to all those bodies within reach of our experiments are assumed (even if only tentatively) as pertaining to all bodies in general. This is a reformulation of the first two hypotheses and is needed for forming universals.

4. Propositions in science obtained by wide induction are to be regarded as exactly or approximately true until phenomena or experiments show that they may be corrected or are liable to exceptions. This principle states that propositions induced on the basis of experiment should not be confuted merely by proposing contrary hypotheses.

Or as a corollary, what problems or insights has the model produced that were not considered in its initial formulation. What predictions does it make? (Taking the view of Robert MacArthur, the great analytical biologist, to describe what is without making predictions or explaining issues not yet uncovered is Natural History, not Science.)

For the last decade or more, the research community has been considering the problem of a new theory or architecture for networking. They seem no closer now that they were when they started. To focus the thinking, we issue this challenge:

The IPC Model developed by Patterns in Network Architecture appears to fulfill Newton's criteria and has made not only predictions, but unexpected predictions about the nature of networking. At present, there appears to be no other theory or architecture that is simpler and has the explanatory breadth of the IPC Model that is not isomorphic to it.

The challenge is to:

- Either disprove this claim or limit its scope, or
- Improve the model to broaden its scope
- Find a simpler or more universal model
- Further explore the properties of the IPC model