WHAT IS THIS CASE ALL ABOUT?

W. C. Clark

October 1973 WP-73-7

What Is This Case All About?

W.C. Clark

"Counsellor, what is this case all about?" (No. 1)

Our perspective on the problem leads us to considerations of persistence, or the lack thereof, in systems.

But "persistence" is a totally aggregate and dichotomous empirical concept. In fact, we are interested in the relative abilities of systems to persist in the face of various stresses. We therefore turn our enquiries to a (proposed) general behavioral attribute of systems which we call resilience. This "resilience" is probably not formally different from the more conventional concept of "stability-in-the-large."

Much of our present work on "resilience" has concerned analysis of systems with alternative, non-zero, attractor domains or equilibrium states, the characteristics of boundary conditions separating those domains. This emphasis may provide useful and novel insight of sorts, but it should not be mistakenly taken as our major interest.

For, in fact, what we really ultimately want to do this year is:

(1) characterize in empirical detail the systems behavior we term resilient;

(2) identify the structural (organizational) attributes of the system which result in the observed resilient behavior;

(3) synthesize a predictive theory of the evolution (development) of our sorts of systems under specified patterns of history.

Note several things about the above:

-- The third point, our ultimate goal, is a--or the--general systems theory problem. Our potential handle on GST is our ability to specify the characteristics which make our sorts of (adaptive?, self-organizing?) systems unique and therefore let us work with defined special cases of GST. There is a parallel here to May's notion that we must be careful to specify which of the many formally "possible" cases (of community matrices) are, in fact, relevant to us.

-- We will know we have answered point two when we can design both resilient and non-resilient systems on a priori grounds.

-- Concerning the first point, we must be careful in deciding which sorts of systems we are interested in. We have done so on an ad hoc basis in our work proposal (ecological, anthropological, economic), but still have no formal criteria for choice.

-- Finally, we should beware of following points 1, 2, 3 in sequence. We know we can always find examples to support anything; hence Eddington's disinclination to "put much faith in facts which have not been explained by theory."

In retrospect, we've said all this before. I just need occasional enforcement.

"What is this case all about?" (No. 2)

(Dynamic system structures...)

* The problem of dynamic change in system structure, as opposed to our present focus on change in system state given static structure...or, What is Mao up to?

* There are two related ways in which resilient (or "preadapted") systems originate. The first is the obvious tautology; non-resilient systems which arise for whatever reason will be "filtered out" by an environment requiring resilient behavior.

The second concerns environmental heterogeneity and the time constants of various candidate systems. In any changing environment you will always find somebody (species, systems) still solving yesterday's problems today. This is not efficient, but it does mean that when yesterday's problem again becomes relevant, there will be someone around who has a lot of practice at finding solutions to it (cf. Lewontin's "Historicity"). Of critical importance is clearly the relative frequency (rate) of former problem re-occurence as compared to the rates of problem-solving-unit "growth" during "good" (problem-present) times, and attrition during "bad" (problem-absent) ones.

If we include the notion of a minimum "density" of problem-solving-unit necessary to act as a good-times propagule, then the characteristic growth and attrition rates described above are sufficient to define the minimum frequency of problem

reoccurrence necessary to sustain existence of the relevant unit (cf. diatoms in streams).

---

* All this means that we really have two classes of levers available to us as we attempt to manipulate system organization; viz. we can change structural time constants, and we can alter the frequency of problem occurrence, i.e. the environment.

Having recognized this, we should, in principle, be able to "starve" an entire mode of organization (i.e., a style of problem solving) out of existence merely by reducing the suitable frequency of problem occurrence below its threshold level. Similarly, we can nurture any "desired" mode simply by increasing the relevant frequency of problem occurrence.

---

*The reason this problem-frequency-manipulation does not constitute a simple solution to system organization problems is that in shifting the problem-frequency, you initiate a series of changes in the distribution of dominant problem-solving strategies which in turn feeds back on problem-frequency. The situation becomes a control theory "searching" problem; on a priori grounds we can only conclude that there may exist a characteristic, in some sense resonant, rate at which one can apply particular transient stresses leading to particular transient shifts of problem-frequency, which is sufficient to move the entire system state and structure in a desired direction. It is likewise plausible that by applying

such stresses at different rates or frequencies, different final states could be reached.

Perhaps this is the theory underlying Mao's cultural revolution.

"What is this case all about?" (No. 3)

We are missing ethology (n.b., a CSH topic of interest...) in our spectrum;

(genetics)-ecology-ethology-anthropology-anthropological economics-(economics)

As there are essentially ecological factors (e.g., functional response curves) imparting "resilience" or whatever to assemblages of beasts, so there are essentially behavioral ones. It is only by extending our ecological arguments <u>through</u> their ethological variants that we can make coherent contact with anthropology/anthropological economics...

Every new field we decide to consider "relevant" to our efforts stretches us just so much thinner--forces us into areas of which we are irredeemably ignorant. We hope to avoid producing either another dilettante's "theory of living systems," or a narrow treatment of specific ecological phenomena. In order to do this, by definition, we must find a way of capitalizing on our areas of special understanding and expertise (i.e., ecology), while creatively incorporating what we can in some sense "securely" gain from the other fields.

It seems to me that our <u>theory</u> must therefore be developed almost completely from the ecological side. That is, we should be most wary of including in our own central dogma ideas which do not have at least retrospective roots in ecology. This, of course, cannot and should not amount to a total prohibition, but exceptions should be conscious, pre-meditated exceptions.