

REVIEW OF STRETTON “THE POLITICAL SCIENCE”: A CRITIQUE OF FUNCTIONALISM

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Stretton’s case seems to be:

- (1) that a significant part of sociology is still hung up on a scientific program that is fruitless.
- (2) That this sterile scientism is being forced into the students of sociology (p. 398).
- (3) That a fruitful model is available to sociology.

Let me speak firstly about his second point as it is the only one on which I feel uneasy about his treatment. He repeatedly asserts this and obviously feels strongly about the effects this sort of teaching is having on the new generation. But he provides no evidence. I have been pretty well out of touch with the teaching scene for the past twelve years so it may be that the situation is so obvious as to require no documentation. Many of the textbooks on sociological theory certainly seem to have the bias of which he emphasizes.

Perhaps someone who knows the academic scene could bear witness to Stretton’s assertion. Let me turn now to the first part of Stretton’s case. Here he has taken adequate steps to back his assertion. He took the guidance of two professional sociologists, Martin and Zubrzycki, in selecting leading theoretical contributions in their field. Two of those chosen, Merton and Parsons are certainly doyens of sociology. Furthermore, he presented a most telling quote from Wilbur Moore:

“It is this addiction to discontent and the search for a better future that I want to explore here, particularly as this actively of the laity relates to the enterprise of sociology as the generalizing science of man’s social behaviour. Have we, in short, any obligation as social scientists to start taking account not only of the changeful quality of social life but also of the fact that some portion of that change is deliberate? [ASR, 31, 1966].”

As Stretton observed 1966 is a little late in the game to be “starting to take account of purposeful social action”. This, I think, is enough evidence for that part of his case. I do not think a counting of heads in the profession or a content analysis of sociological publications would have added much.

How then does he handle the second part of this charge namely that the scientific program of Merton and Parsons is fruitless. Firstly he is good enough to the reader to point out that the battle he is going to reenact was fought and won in the middle of the 1950s. Because the fruits of victory have not filtered down to the academic masses, he has to take on the tedious task of talking ‘old hat’. He does it well. In tackling Merton first I think he has chosen the right target. Merton’s functional approach has far wider acceptance amongst sociologists than has Parson’s theorists and is a more careful, self-conscious analysis of the weaknesses and potential ideological bias of functionalism.

I will try to recapture and list points because, like a good analyst, he has gone back to scratch to make his own direction observations.

1. Against the claim that this kind of analysis is value free he has points out that the selection of measure of system properties will be a function of the purposes of the study. To select ‘persistence’, ‘stability’, ‘integration’ or ‘harmony’ as the criteria of system performance is already a value judgement (p. 176-7).
2. Against Merton’s claim that by adding the notion of dysfunction he had completed the structure of functionalism and negated the charges of conservatism Stretton rightly points out that many social changes are made in order to better an existing good situation not just as a reaction to presenting dysfunction. This is surely an important class of social changes for social science theory to start thinking about?
3. And closely allied to the point just made, Stretton shows that Merton’s functionalism ignores the environment as a possible source of autochthonous changes in, challenge to and opportunities that he tries to ‘cram everything *inside* the system’ (p. 178).
4. Where Merton seeks to encompass some problems by allowing that the system can be treated as just an aggregate of parts Stretton rightly comments that this is then no system. Merton has in effect admitted to a class of social phenomena collective behaviour outside the scope of his system concept.
5. Stretton senses and gnaws away at the fact that Merton’s model allows for no dialogue between the system and the concrete individual. He is not successful in formulating this concern. He seems to have overlooked Parson’s very explicit

recognition that in this kind of closed system analysis the individual exists only insofar as he is functional or dysfunctional to the system.

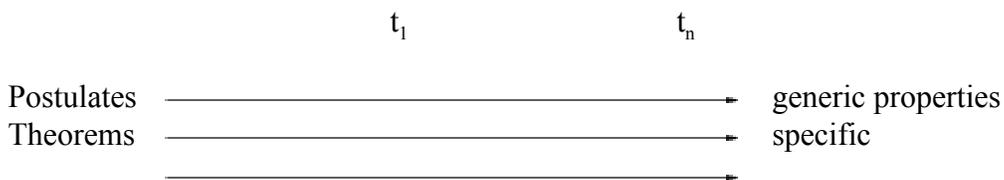
6. And lastly, Stretton points out, by way of the Tumin-Moore debate on unequal social rewards, the barrenness of the system criteria of 'survival'. He might have sharpened his point by showing that this is just a special case of the sterility in the social sciences of the criteria of equilibrium: although the criteria is inherent to closed system thinking.

To this critical points I should like to add one other that he makes more specifically about Parsons and Easton but which he apparently sees as characteristic of all the systems theories he discusses.

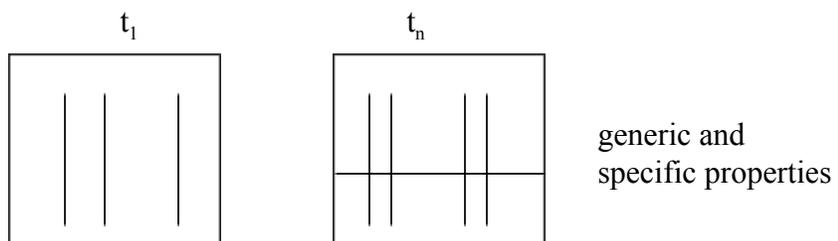
This is that the theoretical aim of these theories is primarily the development of theory itself – increased elegance, etc. The aim is not understanding and central over reality.

Stretton puts this point in various ways and in varying contexts. For my taste he sometimes comes too close to rejecting altogether the goals of theoretical rigour and comprehensiveness. However, I think I understand what he is driving at. He is not insisting that every bit of theoretical construction be anchored and proven to be anchored in concrete realities. He would be happy to accept even Easton and Parsons if their theories were generally fruitful. His point I think was most clearly formulated by F.S.C. Northrop in his article "The Impossibility of a Theoretical Science of Economic Dynamics", (*Quarterly Journal of Economics*, November 1941).

In Theoretical Economic Science



In Theoretical Physical Science



postulates

theorems

Whether this was true of pre-war economics theory is irrelevant just now. I do think the picture Northrop presents is true of the social systems theories Stretton is concerned about. Parsons would argue “so what: this is how science is built”.

Now, if we can take this as a not unfair representation of Stretton’s critique of this kind of systems theory we may now consider his general conclusion namely:

“Static systems may be cut but the static’s tool kits is yet to be replaced” (p. 183)”.

Here I think it might be advisable to take the less optimistic view that despite its transvestite proclivities closed systems models are still very much with us and consorting with the tool kit.

Perhaps Stretton’s good nature lowered his professional guard when it came to throwing yet more stones at the old conservative image of Parsons. At least he appeared to have been beguiled by the picture of Parsons in open systems garb despite the fact that when Parsons talks about environment he only means the next inclusive system and thus remains firmly embedded in the part-whole logic of closed systems.

Maybe Stretton thinks that old theories like old soldiers don’t have to be killed they will simply fade away. I cannot really believe that he thinks this as he is in other parts of the book so concerned with the usefulness to the teaching profession of a theory even when it is full of holes.

Further theoretical development will be necessary to drive out the old, and this does not necessarily mean more pretentious theories – theories of even a grander scale.

Stetton’s criticisms were negative and did not stem from an alternative theory. (His positive recommendations were about the practice of social science so I will leave consideration of them till we finish the theoretical bit). While Stretton lacked theory to drive out the disease he did, like a good clinician, put his finger on what would mean health for the patient. It was as he puts it a theory that would allow for human choices behaviour:

“My choices seem neither perfectly predictable nor perfectly inscrutable. They are among the causes of action: they are unlike most causes in nature”. (p. 164)”.

There is more of this in a similar vein but that will do. It gives us a vantage point from which to look back at systems theory.

If a theory which allowed for multiple and variously interrelated causes and effects, i.e. a systems theory, if such a theory could take into account choice, purposefulness and allow independent action to the environment then it would be close to what Stretton sees as necessary. With these properties the theory would permit a dialogue between individuals and a system; and it would entail some specification of the purposes of a systems study if environmental complexity were to be kept within the bounds of empirical study. Of course one can hardly fail to get the impression from Stretton that it is futile to expect any such development in systems theory.

It is however, unfortunate that Stretton was not guided to relevant development that were occurring outside sociology departments. From his own theorizing he should have guessed that the development were likely to occur outside departments of sociology and not necessarily in history.

The shortcomings in closed system theory that Stretton pondered on in the late sixties had already led Sommerhoff to produce his mathematical model of goal directed behaviour in 1950. This received very close attention from Ernst Nagel's group in Columbia and by 1956 he had himself used it to reanalyze Merton's functionalist model. As distinct from Stretton's feel of certain discrepancies Nagel quite rigorously demonstrated that Merton's model could not account for even goal directed behaviour because:

- a. His initial systems parameter were not independent (p. 312-313).
- b. His inability to introduce the individual as something more than just a role carrier renders his distinction of manifest and latent functions 'vacuous' (p. 318).
- c. The relative value of different functional requirements for a system can be established only by reference to the special material characteristics of a system. A generalized solution cannot be expected.
- d. There is no recognition of the E which must be an independent co-producer for any goal directed behaviour.
- e. There is no recognition that a real system has only a limited range of states.
- f. There is no recognition that the laws relating parts of a functional system (the f's of mathematical representation) may be changing, i.e. that goal-directed systems have histories that are highly pertinent to the function of that systems.

In short, Merton's system theory was of a class of functioning systems best represented by a working clock (Anatol Rappaport actually said this of Parson's theory in a Conference with Parsons and all present apparently thought "marvelous achievement, how scientific we are getting").

Stretton rightly rejects this reductionism. Sommerhoff and Nagel go a good way to meet Stretton's requirements but not far enough. A model of goal-directed behaviour is still not a model of purposeful behaviour let alone ideal-seeking behaviour.

My point is that while Stretton did a very good job almost bare-handed as it were, a better more methodical and more constructive job had already been done some 15-20 years ago.