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Comment on Giuseppe A. Micheli/1

Gestalt Switches in the Idea of Context: Some Informal Comments

by Thomas K. Burch

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Introduction

“The unexamined life is not worth living” [Plato, after Socrates]. Science needs to be examined too. We have literary critics, art critics, theatre critics, etc. Science critics? We’re supposed to be self-critical, but often we are not. We need scholars like Professor Giuseppe Micheli to step back and take a hard look at what we’re doing. And much of what we do in social science follows fad and fashion, or well-worn but narrow disciplinary paths. Much empirical work is opportunistic: “I have a new data set or a new statistical technique; I can get a paper out of that.” There also is a good deal of armchair theorizing, seen as transcending the need for empirical test.

Micheli’s paper is broad and deep. I’m not sure I fully understand the import of all of his ideas. But reading it was a bracing reminder of facets of our disciplines that are often overlooked. Let me mention four, in no particular order and without much elaboration.

1. **There is no “truth” in science, much less absolute truth**

Micheli: “In the social sciences no illusion that we can discover the truth is permitted....” This statement should be put on the bulletin board over every social scientist’s desk. I would go further and make the same claim with regard to all sciences and all human knowledge.

Some contemporary philosophers of science have more or less dropped the notion of scientific truth. An early work by Nancy Cartwright (a scientifically trained philosopher of science) is entitled *How the Laws of Science Lie* [Cartwright 1999]. Ronald Giere, in his *Science Without Laws* [1999] argues that no scientific model or theory can truthfully portray reality, since it must have limits, whereas reality is infinite – or at least indefinite – in extent and complexity. Our theory or model is only an approximation. In another of Micheli's phrases, it's a "proxy for reality."

In place of the concept of "truth," Giere would substitute a more pragmatic test: Does the theory or model correspond to observations on the real world *a)* closely enough, *b)* in certain relevant respects *c)* for the purpose(s) of our scientific investigation? If so, we have good and useful knowledge but not Truth. I'll return to the topic of purpose later.

2. Social science is seriously lacking in unity and consensus on fundamentals

Nancy Cartwright entitles one of her recent works *The Dappled World: A Study of the Boundaries of Science* [Cartwright 1999]. Her argument is that our view of physics as a completely integrated discipline, with all of its parts subsumed under an over-arching theory, is not correct. It is rather a patchwork of theories, concepts and empirical findings. Some are expressed in elegant theories and mathematical models; but many are not.

The situation is all the more extreme across the social sciences generally and within some disciplines, especially sociology. Some would attribute this to subject matters that are arguably more complex. In any case, opportunities for controlled experiment are limited by ethical and practical considerations, making definitive tests of hypotheses difficult. Yet we sometimes express the view that, after all, we're all doing the same thing, trying to develop a science of human behavior, social structure, and culture.

Micheli's Table 1 is a powerful reminder that in fact we till different fields. I am reminded of an advertisement I once received for a text on sociological theory, highlighting the fact that it presented some twenty different "theoretical perspectives" – in a sense twenty different sociologies.

Imagine a physics text which presented several different views on the laws of motion, or a biology text which presented several different versions of DNA or cell

anatomy, or a chemistry text which provided several different formulas for ordinary water. We would find them confusing and unsatisfying.

These differences, coupled with a belief in “truth,” occasion nasty interdisciplinary and intradepartmental squabbles. Are such differences inevitable? To some extent – but I believe that much could be done if we were to make serious attempts to find consensus on some fundamental concepts and principles. We also need to accept that there is no one correct way of doing social science. The strongest results will come from a synthesis of empirical research and theory, qualitative and quantitative, cross-sectional and historical.¹ If anything can lead to consensus on fundamentals it is triangulation using different perspectives and methods.

3. Shortcomings of “variable analysis”

Although I am a demographer and have done my share of multivariate analyses of census and survey data, I believe that common practice among demographers and other quantitative social scientists often goes too far in this direction. As many others have pointed out, the emphasis is on new data and new technique. Serious attention to theory often is lacking, although lately there has been some improvement in this regard. In social science departments, quantitative analysis is equated with statistics; mathematical and computer modeling may not be taught at all (the fact that some social science students, notably sociologists, are selected for a lack of interesting or training in mathematics is partly responsible for this.) This has led to a relative neglect of computer modeling, a tool, among other things, for the statement and manipulation of complex theoretical models.

Late in my career, it became increasingly clear to me that our census and survey data, typically collected or supported by governments (greatly constrained with respect to the kinds of questions that could be asked) represented only the tip of the behavioral iceberg. Even from a “variables analysis” perspective, too many variables have to be left out – formally, relegated to the error term. I once commented to colleagues at Statistics Canada that to really understand the divorce process, they would need to collect data on extramarital affairs. Needless to say they did not follow-up on my suggestion.

Generally speaking, these “variables analyses” try to prove the researcher’s hypotheses. Negative findings are played down, whereas in a well-structured argument

¹ In my view, this list cannot contain the pair “critical and objective.” As soon as moral or ethical values enter the picture, the level of conflict rises. But in any event, one has left the realm of empirical science.

they are the most powerful. This is Popper's idea of the need to falsify hypotheses, based on the inescapable asymmetry of conditional logic.

4. Abduction

Micheli mentions the process of abduction several times, notably in connections with Kepler's search for the principles underlying the orbit of Mars. I welcome this because the concept does not seem to me to receive the attention it deserves. In my social science training, it was all about induction and deduction. Empirical studies led by induction to generalizations which were or became laws (through abstraction and further generalizations.) Explanation consisted in deduction from these laws to the explicandum – so-called nomothetic explanation. The whole process of science became somewhat mechanical, an application of well-defined logical rules.

In fact, many of the great scientific discoveries in all fields have been the product of human imagination. Charles Sanders Peirce, generally credited with the philosophical development of the concept, at some point referred to it as "guessing," which is a good description. In the face of some phenomenon to be explained, especially one that is somewhat surprising, the scientist thinks "For that to have happened, there must be this kind of entity or mechanism at work." Mendel thought this way about genes, as did the "inventors" of DNA; John Dalton and Ernest Rutherford thought this way to "discover" the basic model of the atom, and Einstein in formulating his theories of relativity. As empirical scientists, they were clear that their guesses had to be tested, by themselves or others. But the original guess or conjecture was a consummate act of creative imagination, not empirical research or formal logical inference.

In the sociology I have known (and in some other social and behavioral disciplines) the process has been split, leaving schizoid disciplines. Armchair theorists and social critics fail to test their guesses, or even to see the need to do so. Empiricists are wont to dismiss theoretical speculation as "making things up." Logical positivism and behaviorist psychology have limited scientific work to "observables," forgetting the fact that many of the most important scientific theories deal with things that have never been seen, but are "known" only through their effects. We have never seen gravity, and still don't really know what it is.

Some additions. I would have welcomed discussion of two other points, although I realize they could not have been discussed length given an already weighty paper.

First, Micheli has little to say about the role of biology in human behavior. Surely, we have moved beyond the point where mention of this provokes outcries of racism, Nazism, eugenic sterilization, and the like. Modern genetics, ethology, evolutionary biology, and neuroscience all point to a substantial role for heredity in human behavior, in the species and in individuals.² We are not *tabula rasa*, shaped at will by parents, society and culture. There are deeper behavioral tendencies within.

Social science flirted with Freud in the Twentieth century, but he is now in disfavor, partly because of his narrow focus on sex. The analytic psychology of Jung and his followers holds greater promise of deepening our understanding of human action, with a coherent system of thought regarding the role of both the personal and the collective unconscious – the latter found in human potentialities and tendencies inherited largely unchanged since we evolved as hunters and gatherers.³ If universals are to be found in social behavior, this realm is their origin.⁴

Second, there is need for greater emphasis on the centrality of the purpose(s) of any scientific work and its outcome in judging its quality as scientific knowledge. This is mentioned above in § 1, in reference to Giere's view of scientific knowledge.⁵ If I am setting out to do some research and look at Micheli's Table 1, how do I decide which logics of action, contextual effects, and research designs to use? Personal habit, disciplinary custom, ideological bias – none of these is a very good guide. The proper choice depends on the purpose of my research. This is a pragmatic view, one that sees all of our scientific concepts, models and theories as tools, not sacred truths. A tool that is good for one purpose (e.g., a hammer – for driving nails) is useless for another purpose (e.g., cutting a board.) Newton's basic laws of motion are fine for putting satellites into orbit. They approximate our everyday world of space-time well enough for many practical purposes. But at the scale of the universe or at the subatomic scale, the approximation breaks down. So must it be with social science knowledge.

One possible basis for a gestalt switch may be a switch in the aims of an investigation. Instead of trying simply to predict, a researcher may shift to a focus on explanation. Rather than seeking knowledge for its own sake, he or she may switch

² For a discussion of these disciplines as they relate to human psychology, see Stevens [2002].

³ Jung's theories are an example of a lifetime of abduction in the context of clinical practice and self-analysis. For an interesting discussion of the relevance of Jung to sociological theory, especially Weberian action theory, see Walker [2012].

⁴ It is ironic that my former teacher Marion J. Levy, Jr. spent his career looking for social universals and constants, but defined sociology in a way that sharply limited the prospects of success: the study of variations in social action and structure within the limits imposed by human heredity and the non-human environment [Levy 1952]. But what first makes us human is our common evolutionary heritage, which provides the driving force behind our shared behaviors. Cultural and behavioral differences, as colorful as they are, are variations on a basic theme.

⁵ This idea is developed to great effect in an old but excellent work by Meehan [1968].

to a more practical aim such as policy guidance. In each case, different concepts and different methods are in order.

Micheli's emphasis is on a gestalt switch of a different kind – not a shift of purpose but a shift from one model to another in a search for the most satisfactory for the same purpose, what modern philosophy has termed 'inference to the best hypothesis.' Although refined in science, this process is as old as the human neocortex – a couple of million years old. It is the process of trial and error: "If that doesn't work, I'll try something else."

Professor Micheli's analysis of these issues and his rules of thumb should inspire and assist us to become more nimble and more humble social scientists, as we move crabwise towards a better understanding of human society.

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Abstract: Professor Micheli's systematic unfolding of the concept of "Gestalt switch" and its use to outline different definitions of context, linked to different theories of action, speaks for itself. Rather than discussing it further, I focus on other ideas in his paper dealing with the nature of social science and of social scientific knowledge – ideas which seem to me to need more widespread recognition. These deal with: truth; disunity; variable analysis; abduction. I also comment on two ideas whose discussion – or at least mention – I would have welcomed: biology and human behavior; purpose of investigation as a criterion for judging the quality of scientific knowledge.

Keywords: Scientific truth, abduction, disunity of social science, purpose in scientific research, biology.

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