HOW WE DO SCIENCE—OR DO WE? SEVEN POINTS.*

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1. WE HAVE KNOWN HOW TO DO SCIENCE FOR THOUSANDS OF YEARS

I begin with this point because I believe we sometimes operate under the myth that science as we know it began in the Renaissance. There is no doubt that some elements of what we call Western science emerged from the cultural practices of that period. But even the most stripped down definition of scientific activity leads to the conclusion that the scientific approach to understanding the universe is ancient.

As a starting point, I take Russell's straightforward observation: '. . . the test of scientific truth is patient collection of facts, combined with bold guessing as to laws binding the facts together.' Stated in less eloquent contemporary terms, I would say that science is the search for the fundamental elements that make up the universe and for the principles that govern those elements in their combination. We break down observationally unitary activities (speech, matter, personality) into separate and irreducible elements and then ascertain what allows those elements to combine and interact. The scientific approach therefore assumes that the observed complexity of the universe arises from the principled interaction of primitive elements. It follows that these elements are not always perceived directly, and that the principles that govern them may be quite abstract. The scientific frame of mind thus assumes that apparently discrete appearances are in reality complex combinations, and that there is no simple causal relationship among the parts.

Consider as an example of this approach to understanding the universe an early theory of personality. In this theory, the wide variety of observable personality types was viewed as emerging from the interaction of what I will call 'basic type' and 'character traits'. The basic types can be represented by a circular array, and the character traits by individual glyphs that are placed in various positions and combinations on the basic array. Since there are *n* basic types and *p* character traits that can be placed around the basic array in virtually any spot or grouping, the result is a theory that represents and predicts a wide variety of personalities differing in many small degrees. This contemporary-sounding approach to analyzing and defining personality is Babylonian

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(Chaldean) astrology; its beginnings date from approximately 4,000 BC.

Babylonian astrology thus structured its theory of personality as a modern science would. Obviously, the theory of causal relations between basic types and season of birth and between character traits and the position of stars and planets in the heavens has not been supported by experimental research in the intervening six thousand years; any observed congruences or predictions based on the method are chance phenomena (see, for example, Gauquelin 1979, who does, however, make claims about other types of birth-related correlations). This fact is independent of the overall sophistication of the theoretical approach, and is not in my estimation a trivial one. Human thinking was not in its infancy at the time this theory emerged. More importantly, the association of planetary influences with variable character traits, which may have followed from observed correlations between seasonal changes and/or lunar phases and behavior, allows for a sophisticated view of the subtle variability of personality.

It matters little to the point I am making here that the predictive power of the theory in its haruspic form is by modern standards inadequate, showing only chance levels of success. I am not saying that how we do science has not improved. I do believe that the central scientific notion of describing natural phenomena in terms of the interaction of elements and principles has a long history and is still essential to how we think scientifically today. Diverse and perverse applications of the methodology were as common then as now—power to a priestly class, money-making activities, misinformation, credulity, and the like; all are associated with contemporary scientific practice as well.

It is also the case that the history of science as we practise it is the convergence of theory structure with the practice of experimentation— that observation and practice also form part of the human scientific tradition. What we sometimes dismissively call technology existed successfully for many millenia. This was the 'experimentation' of the past. It could not have been carried out without some level of theorizing about the properties of elements involved. Humans sailed for millenia before the complex and subtle physics behind sailing was 'understood'. But theory and practice were united by simultaneous modelling of the phenomena in application.

It has been suggested (Ohala, this forum) that what defines science is its openness, the public forums of debate, criticism, and counterproposal. But surely this is true of the arts as well, and we do not hallow them with the label 'scientific'. Furthermore, openness, public forums of debate, criticism, and counterproposal characterized the theological debates of 12th century Paris; I suspect that few of us would characterize the topic of these debates as science. I would be inclined to suspect that the one thing that sets post-Renaissance science apart from earlier forms of inquiry is our insistence on validation—would be inclined, that is, if validation was not such a difficult animal to produce, given our skill at defending our theories by interpretating data in various ways, and given the inordinate role that prestige still plays in allowing theories central stage space (see below).

2. THE FALLACY PERSISTS THAT THERE IS A RIGHT WAY TO CARRY OUT SCIENCE

A common criticism of other peoples' research is that it is 'bad science'. Such attacks are frequently couched as negative observations about the way people arrive at their conclusions. What I'll call here the inductive/deductive wars have been carried on for generations in various clothing. Often cited is Raffaelo's splendid painting, The School of Athens, in which we see Plato and Aristotle striding toward us deep in discussion, the elderly Plato pointing upwards, and a bearded, vigorous Aristotle pointing downward. Rationalism and Empiricism? Nominalism and Realism? Cartesianism and Lockism? Sapir and Bloomfield? Chomsky and Skinner? Whatever form the debate takes, it seems to tilt back and forth across the fulcrum of how we view the world itself: that there are accessible ideal states that underly and determine the messy surface of things, or that there is essentially the surface of things which can be sifted through to discover the way things work, but never some thing itself.

Such radical differences in outlook (so radical and persistent in human thought that I suspect there is some genetic basis for them that is triggered by something like early experiences with large animals or harsh toilet training) manifest themselves inevitably in pronouncements about method. Consider Bloomfield: 'The only useful generalizations about language are inductive generalizations.' (Bloomfield 1933, p. 20). Rationalists (Cartesianists, Realists, Platonists, etc) can be equally firm in their opposing convictions.

Such extreme positions, thought to be valuable heuristics in scientific debate (on which more below), inevitably lead to hardening of the arteries and an insistence that conclusions reached by other approaches must inevitably be wrong because of the way in which they go about doing science. But each of these extreme positions is vitiated by an inherent fallacy.

The inductive fallacy is that there can never be enough data, no matter what we do. Not only can we never be certain, we can never even know with reasonable certainty that the next datum will not completely change the way we view all the other data. The deductive fallacy is that we do not even require data to construct a productive hypothesis.

What we really do is a mix of both, plus flying by the seat of our pants, relying on intuitions—those messages from our subconscious about stuff it has been thinking about in our absence, plus remembering things that someone else said years ago but that we now understand because we have discovered them for ourselves, even tying together motley strands of thought that we have collected and suddenly see in relation to each other thanks to one more piece of thread, be it an item of data (inductive) or another idea (deductive) that pushes us over the threshold of understanding.

What we really do is observe (making use of what we know about the world, our experience), experiment, report, get criticized, and then—only then— begin to wash away the mud and gravel of criticism (personal, institutional, etc) to see if there are any nuggets of genuine insight. And of course that criticism may be our own. We then obtain prestige by showing that we can collect data or construct experiments or have interesting ideas and write them up properly. Once we have collected enough prestige we can begin to play for bigger stakes, to win or lose. There may be wrong ways of doing science, but there is no one right way.

3. THERE IS NOTHING WRONG WITH A WEIRD HYPOTHESIS

At this point in our era this observation may be less than startling, but it was not so long ago that whole fields of study that now obtain generous amounts of funding and produce graduate theses were considered unworthy of attention. For those who point up, the work of those who point down is often considered an unproductive waste of time, and vice-versa.

But hypotheses propel science, and they stick with us in metamorphised and presumably increasingly sophisticated forms. The astrological notion of external influences determining behavior and personality is an early form of the nurture hypothesis, and was not a bad hypothesis for its time. We assume that successive hypothesizing will become informed by experience over time and so not be wasteful re-inventing of the wheel. But the Viconian spiral is a better metaphor for what happens. Various forms of nature/nurture, of mentalism, of behaviorism, have persisted for generations. The appearance of Skinnerian behaviorism was seen as a weird and unwholesome hypothesis (along with other mechanistic trends early in the century) since it threatened time honored notions of mind along with a world view that encompassed notions of soul and spirit. The revival of mentalism in linguistics by Chomsky and its accompanying emphasis on genetic transmission was considered without empirical foundation. Contemporary genetic science gives nativism at least a peg to hang on and renders its initial weirdness less so. (This change of paradigm cannot be too exaggerated. High school psychology courses in the late 1950's nourished students on a strict Behaviorist regimen, dismissing unexplained behavioral phenomena as 'instinct'. Without contemporary genetics, certain properties of behavior appear inexplicable.)

The problem lies not so much in the hypotheses themselves, but that in the social structures manifested by our brains, we place too much emphasis on leaders and followers, centers of attention, grant-getters, winners and losers. The very prestige we must obtain in order to be heard turns into an encumbrance for the field as a whole when some players acquire too much of it. The result is the perpetuation of some hypotheses for the wrong reasons at the expense of others.

Of course, we might say that this is the sociology of science, and it has not kept research from coming up with the right answers over the long haul. The social matrix of our particular science in the second half of the twentieth century provides a testing ground for humans that has always been part of our societies. The best ideas survive, the poor ones fall by the wayside. But I have suggested above that this is not necessarily the case. The tough, aggressive, and prestigious survive and thrive, but their ideas are not necessarily the best. As Cuppy 1950 notes about Attila, just about all he accomplished was that the people he killed stayed dead. At least science has provided for the resurrection of ignored good ideas through libraries.

Science does not have to live by the rule of the junior high school playground. Diversity is the key to survival in nature. Things have improved in linguistics, but hegemony is hard to break.

4. WE OPERATE WITH WEIRD HYPOTHESES FOR GENERATIONS

Not only is a weird hypothesis something that should not be nipped in the bud, it is often the essence of a discipline's survival.

One of the most striking examples of a persistent and valuable weird hypothesis was the notion of parallel light rays. Unproven and in the strictest sense untestable, the hypothesis made possible the whole technology of Western celestial navigation. Navigation was in a sense its test, which the hypothesis has served well for several hundred years.

There is at least lip service given to the notion of a useful weird hypothesis. A geocentric universe is accepted as having been useful for a while, since we knew no better. The Ether theory helped hold the universe together for a time, and so on. But the history of science as it is actually practised is replete with such heuristics ossifying into dogma defended against all comers, not only by administrators of priestly grants with a vested interest in them, but by the very scientists themselves who should have an open mind and sharp eyes for new weird theories. We often become locked into a Popperian nightmare of defending our hypotheses at whatever the cost because that is supposed to be the way science is done. And so old theories rarely become extinct; rather, new, alternative schools arise while the old ones await their turn for resuscitation.

5. THE CRUCIAL EXPERIMENT IS NOT DEAD

It has become a truism that the notion of a crucial experiment is dead. The crucial experiment can always be re-interpreted, redone, overhauled, confronted with ever new data, and of course, the crucial experiment itself often raises a whole new set of questions. But I think that the ghost of the crucial experiment still hovers.

We operate as if with a hangover of the crucial experimentation fantasy. First, we still rely too much on machines to make decisions. What I call the fallacy of the machine is the belief that we go to a machine to discover some truth about the universe, as if the machine was a flashlight and we were out looking for interesting things in the dark, when in fact we design machines to discover what we are looking for. The machine does not provide the answer, it provides the kind of data we want. This is not to say that there can't be serendipity effects as one discovery leads to another or as a machine is put to novel (or 'weird') uses—VOT studies emerged from spectrography in an unexpected way, for example. But submitting to the authority of machines will not solve our problems until the machines can start coming up with useful hypotheses.

The 'naive falisificationalism' that Popper rightly discounts was described somewhat wickedly by Einstein as 'verification by little effects'.

> "It is really strange that human beings are normally deaf to the strongest arguments while they are always inclined to overestimate measuring accuracies." Letter of May 12, 1952 to Born, from *The Born-Einstein Letters*, cited in Feyerabend, 1975, p. 57.

For Einstein, the interlocking of a theory's various parts and arguments as a whole made the strongest case for the theory. This is often misinterpreted and misused as an argument from 'elegance'.

A second hard lesson we have to learn sooner or later is that self-proclaimed wholly empirical theories are subject to the same conceptual, organizational, and verificational flaws as wholly 'rationalist' arguments. Poor design, weakly collected data, poorly understood statistical measures and their application, and downright misunderstanding of the issues at stake all contribute to the same kind of hall-of-mirrors argumentation among experimentalists as can be heard at any syntax conference. Einstein was right: we understand something when we understand the whole thing.

6. REDUCTIONISM IS ONLY VALUABLE WHEN IT IS APPROPRIATE

An inescapable dilemma of modern science arises from our own technological power. We keep reducing our primitives. This is fine as long as there is an end in sight and we are not simply reshuffling the deck. Reductionism is a handy club for any side. It is easy enough to argue that an opponent's categories are meaningless because they can be reduced to lower-level primitives (Ohala 1974). It is just as easy to say that such an approach is mere reductionism but that the link between reductionism and generality is not justified (Pierrehumber 1990).

Hierarchical systems such as nature guarantee that a blind or mechanistic reductionism will not provide us with satisfying answers to scientific questions. In any hierarchical system, units at one level are made up of units from the level below. In being incorporated into the higher level, the immediate lower-level units lose the validating power they had at their own level. Contrastive features keep morphemes apart, but once questions of the meaning and function of each morpheme come into play, the distinctive power of the feature is no longer relevant to the explanation. Explanations of why vowel harmony does not cross word boundaries are inappropriately reduced to phonetic questions though the answer lies in the nature of words and word boundaries themselves and is properly described and explained at that level. *Appropriate reductionism* is the key. It's really not turtles all the way down.

Practitioners of science have known for a long time that we are looking for the best answer, not the absolute truth. And yet we all at times act as if we are still looking for a final truth. Perhaps we need that faith to go looking. But theories are like tools, and each theory does something another theory cannot do. Each theory serves the degree of idealization required.

7. SYSTEM, THEORY, AND PARADIGM

Although Newton legitimized the image of the scientist picking up pebbles along the vast beachfront of the universe, we must do more than that (as he did). Without a broader view of what we are doing—how our data and hypotheses fit together with other pieces of our own work, or with what other people are doing—we are indeed just collecting pebbles.

Kuhn 1986 notes that a true scientific definition of gold defines 'gold' precisely as equal to atomic number 79, and not gold's 'superficial' properties such as yellowness and ductility. But of course '79' is meaningless unless it forms part of a coherent theory, a system of elements and principles (primitives and relations)—unless it is 'atomic number 79', with all that the qualifiers imply about a theory of atomic relations. Otherwise it is an arbitrary label. We require a paradigm in which to work (' . . a set of recurrent, quasi-standard illustrations of various theories in their conceptual, observational, and instrumental applications. These are the community's paradigm.' [Kuhn 1970]) but we also require a general view of how all the pieces might fit together. Although in fact we work across many paradigms and hypotheses simultaneously, a dominant one in the field as well as dominant counterparadigms, temporary and sub-paradigms, we must hold hypotheses on a larger scale. We shouldn't burrow.

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