Kuhn, Consciousness, and Paradigms

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The Schwartzreport tracks emerging trends that will affect the world, particularly the United States. For EXPLORE it focuses on matters of health in the broadest sense of that term, including medical issues, changes in the biosphere, technology, and policy considerations, all of which will shape our culture and our lives.

Something very profound is happening in science, something not seen in more than a century is occurring: the paradigm of science is changing. Consciousness, particularly nonlocal non-physiological consciousness, is becoming mainstream. The world view of materialism is increasingly inconsistent with the reported experimental data in a spectrum of disciplines, as any search of PubMed, Academia.edu, or Researchgate will quickly reveal. I think it is time to retire the limitation and go where the data goes.

I believe materialism did not arise from scientific findings but was the result of a science culture that formed as a result of the pronouncements of the Council of Trent (1545–1563), the principal one being a prohibition against science studying "spirit," which is to say consciousness, on pain of death. It is a taboo that lasted for centuries, and for more than three centuries it tortured and killed. An unknown but large number of doctors, scientists, herbalists, particularly village herbal women, philosophers, alchemists and others were tortured and killed, often by being burnt alive. I have described this elsewhere in these pages.[1] Materialism is a self-imposed limitation not a scientific absolute. There is nothing in science that precludes consciousness being studied like anything else, and there is much to urge that it should be studied. In non-Christian countries like China, consciousness has always been and remains a part of science. In China religion has been stripped of any power in government, and so the study of consciousness is not burdened by its beliefs, although it is incorporated anthropologically as Shamanism has been in the West.

I have been involved with this transition for almost half a century, smiling and leaning toward a world view that incorporates consciousness. As the process has gone along what I have found most interesting, yet least noted in much of the academic discussion is that the transition, is as much a cultural movement as a scientific one.

No one understands this better than the late Thomas Kuhn, M.Taylor Pyne Professor of Philosophy and History of Science of the Princeton University and, later, Laurence S. Rockefeller Professor of Philosophy at MIT. His 1962 exegesis, *The Structure of Scientific Revolutions*, is arguably the most important book about the history and philosophy of science ever written.

Today there is hardly a college offering a course in the history and philosophy of science that does not cover his book. In it he lays out the nature of the interactive relationship between science and culture very clearly, and the role this interaction has in the development of scientific understanding.

Kuhn begins by saying, "The developmental process [of science] has been an evolution from primitive beginnings—a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature. But nothing… makes it a process of evolution toward anything. Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal?… The entire process may have occurred as we now suppose biological evolution did without benefit of a set goal, a permanent fixed scientific truth of which each stage in the development of scientific knowledge is [an improved] exemplar".[2]

He then goes on to describe, accurately, I think, the culture of science, saying that those who are drawn to science and who become scientists are a special community dedicated to solving certain very restricted and self-defined problems whose relevance is defined by a world view or paradigm.

Paradigms are, as Kuhn argues, absolutely essential to science, although ultimately they become self-limiting. Without the set boundaries provided by the paradigm, no observation has any greater importance or weight than any other. Without this differentiation western science is impossible. The benefit it confers is that with boundaries comes depth, and with depth comes detail.

The narrowness of this definition increases as a science matures and manifests itself in increased subspecialization; one is not simply a chemist but an *organic* chemist. It should be obvious then, to quote Kuhn again, that "one of the reasons why normal science seems to progress so rapidly is that its practitioners concentrate on problems that only their own lack of ingenuity should keep them from solving… intrinsic value is no criterion for a puzzle, the assured existence of a solution is".[4]

This efficiency in puzzle solving collectively is "normal science." Obviously, this normal science is accumulative, but does it also seek the Copernican leaps, the insights that will change the course of history? No, it specifically does not. Normal science, in fact, is specifically not interested in the very thing it is popularly supposed to be obsessed with doing.

The reality is that the efficient solution of problems requires an agreed-upon limit to what is attempted. To reach such an agreement—the paradigm—demands a special kind of education, one that does not so much teach the student about "truth" as condition the aspirant, through the academic degree stages of initiation, into a commonly shared body of experience. Anthropologically, socially, it is not much different

from initiation through man or womanhood shamanic ceremonies into an Amazonian healing cult. Like their non-technological Amazonian cousins, fledgling scientists conclude such an education only after demonstrating their competence, in this case through examinations and papers, showing that they have learned what enterprise, and only what enterprise, is supported by their group's world view.

But achieving this acceptance always and everywhere comes at a cost. For the modern scientist it requires the acceptance of some fundamental compromises, not the least of which is a highly selective presentation of the past. For example, few young PhDs in physics today would know of Newton's interest in alchemy. Yet it was from that interest and context that much of modern mathematics and physics sprang, and it was the perspective from which Newton viewed his work.

Western science has very little relevant past, excepting the careers of the teachers of those now practicing and perhaps their teachers, except for those specifically interested the history and philosophies of science. If a science's paradigm has changed, past research, particularly if it operated under the earlier rules, is *unscience* by definition.

Under the terms of the present paradigms of science alchemy, Newton's sovereign fascination is nonsense. The only thing the past has to offer are the laws or rules that have crossed the borders separating one world view from another; and these can be expressed in their most condensed form since the context in which the researcher who formulated them lived, or the philosophy that motivated him, is of no interest or help to a present-day investigator. Also, because a paradigm is a world-view specific to a discipline, anyone outside of that paradigm-attained discipline is a layman. An MD is no more a member than is a plumber of the paradigm community of astrophysicists.

To seek the discovery of new phenomena unaccounted for by the paradigm, or to attempt the theoretical ground its discovery leads to, threatens the paradigm, functionally a synonym for science. A researcher engaged in threatening activities is practicing anti-science and is soon isolated to the status of nonscientist. Even the most prominent can be destroyed when a critical collegial consensus emerges against them. While they may be performing scientifically such tasks as measuring accurately or experimenting and recording results carefully, if their basic premise lies outside of the paradigm, what they are doing is not science. The critical difference is paradigm, and history is replete with researchers and clinicians whose careers were ruined or stunted when they threatened the prevailing worldview. Revolutionary advances and normal science are often socially incompatible, and to pursue radical lines requires real professional bravery.

How a scientist communicates his research to others is also worth considering. Until well into the twentieth century scientists usually presented their major findings in books issued to the public, lay and scientist alike, and it is a popular myth that they still do so. The fact is, however, that the days of Darwin's *The Origin of the Species*, or Newton's *Philosophiae Naturalis* and *Principia Mathematica* are over. One development which characterized twentieth century science was that ideas and propositions were communicated to peers not by books but through papers, seminars, professional journals and, increasingly, internet discussion groups and electronic journals.

If one must publish to survive, papers which vary from the paradigm stand little or no chance of being published, and professional survival becomes problematic. In recent years this has meant that those interested in nonlocal informational interactions, as well as other fields such as chaos physics, or for many years discussions of Bell's Theorem, either circulated never-to-be-published "pre-publications" or wrote a non-paradigm book which, by its form, was suspect. Practically, this meant either limited circulation, or the risk of loss of standing. The fact that in the social sciences, books debating philosophy and publicly proposing new theories are still being written is an indication that here the paradigm-achieving process continues.

All this does not mean that books have no place in science, for they most definitely do. If the book is no longer the primary vehicle for the presentation of original work, the form has other tasks. Textbooks are currently the main processing mechanism used to condition aspiring scientists. It is essentially pedagogical propaganda, and for this reason textbooks are molded to a very specific pattern. They report only the research that supports the paradigm and its normal science techniques; rarely are alternative explanations of reality and the research that produced those explanations presented.

The findings concerning alternative medicine or even nutrition, for example, until recently had almost no place in modern medical training and were treated only slightingly, if at all, in medical text books.

Textbooks fail to address the full complex of developments which led to present understanding. Textbooks also help in another aspect of initiation. As the paradigm-achieved sciences have matured, one sign of their maturity has been the development of a jargon which, anthropologically, functions much like a sacred language. Like sacred languages everywhere, it is often incomprehensible to anyone outside the paradigm, *even though the material itself might not be*. It is a point which is not lessened, even as one acknowledges the importance of jargon in stopping incorrect theory building, and respects the significance of compressed notation in scientific communications.

How does the paradigm-achievement process occur? After a period in which a variety of points-of-view compete, certain theories begin to draw adherents and schools (of thought) are formed. Gradually, this phase gives way to a next stage of development where one school "gains status" by being more successful in solving what the discipline has set up as its most acutely pressing tasks. This does not mean that this school's theory and techniques are more "truthful" or that they can solve all problems. It only means that the school is more efficient and successful at solving the critical problems in question. Indeed, since by definition a paradigm is a set of boundaries, the victorious school and its theories are only designed to solve a selected, and limited, list of puzzles.

Once a view has proved successful, the school it represents draws adherents from the other schools until a kind of

critical mass is achieved. At this point one set of theories predominates and becomes the entire discipline's paradigm. Obviously, though, not all members of a discipline accept the dictates of the dominant school; some have a vested interest in their alternative theories. What happens to them? If they persist in clinging to their now "unscientific" views, they are drummed out of a community increasingly uninterested in what they have to say.

Having achieved paradigm, a discipline becomes a science and begins to practice what Kuhn calls "normal science." At this plateau, as he points out rather harshly in my view, "The scientific enterprise as a whole does from time to time prove useful, opens up new territory, displays order, and tests long-accepted belief. Nevertheless, the *individual* engaged on a normal research problem is almost never doing any one of these things [emphasis Kuhn]".[2] He finds himself instead working from a different motivation, the desire to demonstrate that he is capable of solving a problem within the paradigm that no one has ever solved before, or has not solved as elegantly. Kuhn says, "On most occasions any particular field of specialization offers nothing else to do, a fact that makes it no less fascinating to the proper sort of addict…. Scientists normally [do not] aim to invent new theories, and they are often intolerant of those invented by others".[3]

In the late 1970s, after interviewing Kuhn several times on the phone, I drove up to Princeton and spent an afternoon with him. As we talked about what blocked consciousness research from greater acceptance, Kuhn surprised me by saying he felt the sociology of science was closely akin to a priesthood, and growing more so as the materialist paradigm was threatened. He advised me to consider the language of parapsychology as a limitation not a strength, because he felt technical terms were one of the ways a scientist demonstrated (s)he was a member of the tribe, or revealed s(he) was not. *Structure* goes into this at some length. He felt the terms of parapsychology, psi, psychic, anomalous, telepathy, etc. all tended to make the field a pariah, because no one outside of parapsychology used such terms.

It made a strong impression on me and from that day forward I have preferred instead, nonlocal consciousness, that aspect of consciousness that is non-physiologically based. The term was coined in 1981 by physician and researcher Larry Dossey, and I think it is descriptively accurate. Also useful I think are, nonlocal awareness, nonlocal perception and perturbation.

And these terms are gaining currency. Why? Perhaps because Planck's dictum that "consciousness is the fundamental" is gaining ever widening acceptance in a spectrum of sciences from biology to medicine to physics.

An interdisciplinary collegiality of men and women interested in what has come to be called Postmaterial Science is taking form; the founding of the Academy for the Advancement of Postmaterialist Sciences, announced in this journal, and the Society for Consciousness Studies, in both of which, in full disclosure I am involved, constitute the latest manifestations of this emergence.

The impulse behind the trend is not to replace the insights of materialism, or the rigor of protocol and analysis, simply to include consciousness as fundamental. A seed crystal has formed around which a community holding a more comprehensive view can coalesce.

Of course there have always been individuals in science who considered consciousness appropriate for scientific inquiry. In the 17th century the French Academy of Science tested the dowsing skills of uber-peasant Jacques Aymar.[4] Newton was as interested in alchemy as he was gravity. Benjamin Franklin was the first person in the modern sense to explicitly study a non-local task, the healing power of Mesmer's "animal magnetism." In designing his methodology he also invented the blind protocol, and became the first parapsychologist.[5]

But these individuals however famous some of them became are still outliers. Because of the Council of Trent (1545-1563) which resulted in the Roman Catholic Church taking for itself all matters involving "spirit," read consciousness, upon risk of death the social reality up until the 19th century was to abjure the study of consciousness. How powerful was this interdiction? The last man killed by the Inquisition was a teacher named Cayetano Ripoli, who was garroted for teaching Deism to his class in Spain in 1826. But change was coming.

Just 56 years later the interest in what today we would call nonlocal consciousness had reached a sufficient critical mass that in the UK, a group of physicists, philosophers, psychologists, and clerics decided that science could and should study consciousness and the experiential phenomena so universally reported across the span of human history. In 1882 the Society for Psychical Research was formed for that explicit purpose. Two years, later in 1884, the American Society for Psychical Research. In France, Germany, and other countries over the last years of the nineteenth, and the early years of the 20th century, similar societies were founded.

And all of this paralleled the development of psychology and psychiatry. Two streams of consciousness research and clinical practice emerged. One centered on mental and emotional processes the other focused on what today we would call non-local consciousness. This schism occurred, I think, because materialism was still the dominant paradigm.

From the beginning, though, there has been a lot of cross-over between these communities. Psychologist and philosopher Williams James, whose book *Varieties of Religious Experience*, first published in 1902 and still read and respected today, comes to mind from those early years. As does Adolf Bastian's theory *Elementargedanke*, which morphed into Carl Jung's Collective Unconscious, and then into Joseph Campbell's Mono-myth. But it has not been the dominant paradigm for either psychology or psychiatry. the Council of Trent Taboo against science studying consciousness still obtains.

Kuhn makes an important distinction between a science and a discipline that is also relevant here. A group may call themselves by a name and society may come to acknowledge both their name and their mission, but this acceptance does not make their practice a science. To become a discipline is a social phenomenon, not a scientific one. The thing that differentiates a discipline from a science is paradigm. A discipline is either paradigm-aspiring or paradigm-achieved—at which point it attains the

status of a science. The difference between these states is not trivial, and one of the central obstacles against the acceptance of consciousness research is that in Kuhnian terms, consciousness research as a field is more discipline than science.

In 1969, after an impassioned challenge issued to the mainstream by anthropologist Margaret Meade, that the American Association for the Advancement of Science admitted the Parapsychology Association as a section, and parapsychology could be truly said to have entered the greater community of science.

In the early 1970s anthropology was riven with controversy over the dissertation of a young anthropologist at the University of California—Berkeley, Carlos Castenada. The orthodoxy of the day saw shamanism as a tribal form in which some individuals, or families became powerful through the manipulation of tribal rituals and beliefs. Castenada offered a radically different view: that shamans were people who through the rituals—read protocols—of the shamanic path entered genuine altered states of consciousness where therapeutic intention and nonlocal perception were possible. It was a view supported in psychology by Charles Tart who in 1972 published in *Science*, a now classic paper arguing much the same thesis.[6]

Over the last half century that view has substantively altered anthropology's worldview. This dispute was what led a medical anthropologist, Joseph Long and myself to apply for and receive approval from the American Anthropological Association to hold the Rhine-Swanton Symposium on Parapsychology and Anthropology at that year's annual meetings. While we were in Mexico City, helped by Norman Emerson, father of Canadian Archeology, and chairman of the Department of Anthropology at the University of Toronto we created what 45 years later has became The Society for the Anthropology of Consciousness, a section of the American Anthropological Association.

In 1982, an interdisciplinary group organized largely by physicists, but including researchers from other disciplines started the independent Society for Scientific Exploration. In 1989, I and three others began the interdisciplinary International Society for the Study of Subtle Energies and Energy Medicine (ISSEEM). Based on a single mailing of approximately 8500 names in the research community, within 12 weeks approximately 1200 scientists and clinicians from a wide range of disciplines, 55% of whom held PhDs or MDs, responded with an application for membership. All of these organizations, with the exception of ISSEEM, today are thriving with robust memberships, newsletters, websites, journals, and regular conferences.

At the same time researchers, including Professors Jessica Utts of the University of California—Irvine, Richard Bierman of the University of Amsterdam, Dean Radin at the Institute of Noetic Sciences, Patrizio Tressoldi of the Università di Padova, Roger Nelson of Princeton, Edwin May for many years head of the SRI/SAIC laboratory, and James Spottiswoode, have essentially created a sub-specialty in mathematics carrying out post-experiment analyses of studies, seeking to understand the nature and functioning of nonlocal consciousness.

Across the world it has become standard for laboratories doing this research to have Institutional Review Boards to assure methodological rigor. Several studies have also shown that double and triple blind protocols are used more often in parapsychology than any other branch of science.[7,8]

There are also now more than a dozen working *ad hoc* computer network groups and discussion groups of scientists and clinicians exploring topics from meta-analysis to the psychotherapeutics of experienced anomalous trauma. Indisputably there is interest among a broad base of well-trained men and women who are willing to devote at least some of their professional time to these efforts and the networks which have evolved to serve this commitment. By any calibration, although materialism is still the dominant paradigm the social evidence suggests that the materialist paradigm is going into crisis, exactly as Kuhn described.

The idealized compact science has with society is the promise to explain without bias, politics, ideology, or religion, and superficially it would seem that nonlocal consciousness events should be of pre-eminent interest. They constitute the most broadly experienced mystery for which the culture seeks an explanation, because at sometime in their lives almost everyone has experienced *deja vu*, had a precognitive dream, or a premonition that came true. It is this larger cultural context which gives these issues an importance extending beyond the boundaries of the science or any one discipline. Conversely, it is this universality and social context which gives these phenomena their unique place in the culture. The mysteries of subatomic particles are of great interest to scientists, and of enormous importance, but average men and women are not confronted with them in their normal lives as they are with altered states of consciousness and the froward phenomena called psi.

Where, then, does an Einstein, a Newton, and, in a slightly different way, a Jung come from? And how does an extraordinary researcher's work, which is genuinely radical and not simply an extension of normal science, get into the mainstream? The answer is that the seed of innovation lies within the dynamic of normal science. There is a kind of Metamorphosis Mechanism contained within the very being of a paradigm.

Since it is by nature narrow and rigid —and this should not be construed as a pejorative description because the vast bulk of research could be practiced in no other way—normal science always produces anomalies in the course of its work, and as it proceeds inevitably to reach its boundaries the encounters with anomalies increase. The reason is simple: before paradigm is achieved, clearly nothing can be anomalous; after paradigm, a great deal will be. As the limits of paradigm are reached, what lies beyond is that much closer.

Normal science, however, abhors anomalies since they are not tailored to the scheme by which it defines the universe. At first, then, anomalies are ignored on the assumption that later normal science research will deal with them when either instrumentation or theory articulation, or both, are improved. If this does not happen,

an attempt is made to extend the endangered theory in the hope that an extension of the paradigm's accepted propositions will bring the anomalies back into the fold.

In the beginning of a paradigm's lifespan better instrumentation or theory extension does eliminate most of the anomalies by making them conform; some, though, will not conform, no matter how artful the experiment or ingenious the development of the original premise. Most scientists are happy to leave these anomalies in a state of limbo. Everyone knows they are out there, lurking on the edges of the paradigm like hungry beasts around a campfire, but scientists assume, mostly correctly, that most problems can still be contained within the paradigm, and so for a time at least normal science continues, and the paradigm provides a reasonably secure framework.

However, as normal-science research continues to get closer to the edge of the "known" it pushes so intensely, and with such specific focus, that its explorations produce just the opposite effect from that desired. Not only does such research fail to strengthen the paradigm, which was its original purpose, but it produces still more anomalies. Ironically, at the end of the paradigm's lifespan the better the instrumentation the more intractable the challenge presented by anomalies. These begin to cluster until so many exist that not only theory but the paradigm itself is called into question. When this happens, the science enters a state of crisis from which there is no turning back.

There is extraordinary resistance in the scientific trenches to this final phase—in an individual it might be called denial. Scientists hate crisis even more than anomalies. Researchers delay retooling as long as they can, since it is expensive, involves much aggravation, and threatens careers and hard-won status. Paradigm crisis is the last stage in a process of scientific death. When it becomes irresistible, and the limits of the paradigm's lifespan are acknowledged by a critical consensus of its practitioners, several significant events take place.

First, the perception of the universe espoused and represented by the paradigm begins to go out of focus. As this happens the rigid restrictions that have dominated normal science go slack because researchers in the community become less dogmatic and secure in what dogmatism does remain. This insecurity is reflected in the papers and seminars that normally reinforce the community's perception of itself and what it can and cannot do. Books appear again as major vectors of communication. Debates on the philosophic foundation of the community take place—an activity that is almost nonexistent in the normal science period, since a steady-state philosophy is taken to be a given.

Most of all, crisis allows the reexamination of problems that were formerly assumed to be either unsolved or unscientific. To do this, what Kuhn calls "extraordinary research" is begun. This research, as in the pre-paradigm period, begins to cause fragmentation and then a reassertion of schools.

When this stage is reached, two segments of the community become its critical practitioners: the most senior and the most junior. The latter are important because they will probably be the ones to engage in the extraordinary, indeed, revolutionary, research that will relieve crisis. They have been in the community the shortest length of time, have the smallest vested interest in the past way of doing business, and are most open to alternate perceptions. As Max Planck so famously put it, "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents die and a new generation grows up that is familiar with it".[9]

The seniors are important for another different reason. The fact that extraordinary research can articulate a new paradigm does not mean that it has solved all the puzzles that its formulation represents. By definition it cannot, since that more mundane task lies within the domain of *normal*, not extraordinary, science. Consequently, although juniors may make the breakthroughs, it is the graybeards around whom the emerging crisis schools will form. Because there are few answers and only new puzzles, practitioners within the community align themselves with new theories not only on the basis of intellectual scientific merit but also (and this is almost never admitted even when it is recognized) on faith in a particular senior.

One other source can produce revolutionary innovators. Occasionally, researchers from one paradigm group find themselves attracted to puzzles that have primary significance for another group. Because they are not fully conditioned to the paradigm of this field and have less vested interest in its maintenance, these investigators function very much like juniors; but they have a great mastery of research skills. Extraordinary advances are often the result of this interdisciplinary contact. Consider the impact on archeology and anthropology that resulted from the development, by two researchers from outside of those fields, of Carbon-14 dating by Frank Libby, a professor of chemistry, who won the Noble Prize in 1960 for his discovery, and the dendrochronological correction, developed by Charles Ferguson, which followed.

Regardless of whether the innovators are juniors or investigators from other paradigms, however, the final result is the same. Gradually, as in the preparadigm days, one school emerges supreme, the world is redefined, a new paradigm is established, extraordinary research is suppressed as "unscientific," and normal science can begin "the mopping up operations [that] are what engage most scientists throughout their careers".[2] Revolution is over and the cycle begins again. And although to an outsider it may appear that things are much the same (and they are in the sense that the same words in most cases are still used and many of the old solutions are still valid), there has been the most fundamental change possible. The world of that scientific community has profoundly altered; its universe, and how that universe operates, is radically different.

The change is not without price, however, because one of the first orders of business under the new paradigm is the rewriting of all the textbooks, and the obliteration of much of the past and many reputations; revolutions are therefore invisible except as highly distorted hero-worshiping of a select few past researchers—many of whom are the very people who caused the paradigm

change. Ironically, they are presented not as revolutionaries, men and women who tore an earlier world apart, but simply as evolutionary practitioners whose vision made science's knowledge move more rapidly forward—but still in the same channel. It may be pleasant and good for morale that paleontologists, for instance, trace their professional genealogy back through the 20th century Kenya-born Englishman Louis S.B. Leakey, to the 18th century Frenchman Georges Cuvier, to the 15th century Italian Leonardo da Vinci, to the sixth-century B.C. Greek, Archelaus, assuming an unbroken continuum of research. But this is a fiction made possible only by distorted hindsight. In truth, these men operated either under no paradigm or under radically different paradigms. The only valid continuum is that they each represent an attempt to solve similar puzzles in the context of their own age.

With this as background, how do we, as parapsychological experimentalists and theorists interested in these anomalous regions of science chart our course? First, we must recognize that we are in the paradigm-attaining phase. This is not a time for delusions of certainty. Our principal challenge is the issue of mechanism. We can measure effects but we cannot explain how they are achieved. We do not even know whether there is one mechanism or several. Second, there is no universally accepted theory about what we are trying to do; third, we do not all agree on what constitutes the significant facts.

To complete the process of establishing a new paradigm, here are the tasks I see that lie before us:

- Determination of what the significant facts are. The reality is that we do not know what all the significant facts are, in some cases we may not know any of the significant facts.
- Development of competent working theories that can be tested through competent experimentation.
- Matching facts with theory. If there is common acceptance here, we can move out over our agreed upon playing ground. If facts are not acknowledged as matching theory, then the cycle of searching for the match must begin again.

To achieve these three goals certain strategies seem required as well as certain tactics flowing from those strategies.

The first strategy is identifying and acknowledging anticipated criticism; rather than shying from criticism we should see it as a guide to the potholes on our road.

The second strategy is the incorporation of critical considerations into protocol design, and discussion through peer-review of what those critical considerations are. Interdisciplinary peer-assistance, prior to a study's execution, I believe, is the key to developing experimental protocols and hypotheses that will withstand all but the most ideologically based criticism leveled at researchers working in the area of anomalous phenomena

If we incorporate and work within the dynamic tension that exists not only among our disciplines and sciences, the synthesis coming out of such research will give us levels of insight that might otherwise be unattainable. The great test that we, as parapsychologists, will face, is the successful resolution of our dynamic tension. What happens if it is not successfully resolved? Organizations schism. We have an opportunity here to learn from past experiences, to offer and incorporate those strategies that lead to really excellent experimental work. If I were asked to advise the kind of interdisciplinary research team I envisage, my principal suggestions beyond those points I have already made would be:

First, pay attention to all possible variables, even those that seem irrelevant or seem to be assumed. It cannot be over-stressed in looking at nonlocal consciousness phenomena, that you record everything you can quantify. For instance, recent insights into the effect of geomagnetic field strength on human performance, beginning with the work of Laurentia University's Michael Persinger, and carried on by Dean Radin, James Spottiswoode, Edwin May, and others, suggest a massive confounding variable may not be properly accounted for not only in parapsychological experiments, but in all experiments affected by psychophysical variations. And the Local Sideral Time (LST) effect proposed by Spottiswoode after meticulous examination of past data sets would have been impossible if researchers in the past had not recorded the exact date and time at which they carried out their experiments—even though, at the time, the information was of only marginal significance to them.

Second, include a careful description of the people who are associated with the occurrence of these phenomena, as well as those who may be affected by them. Who are they? What do they think is happening? How do they explain what they are subjectively experiencing? If they see themselves as active agents of the phenomenon, from which tradition, if any, do they come; in what context do they place themselves? To the degree that one can do so, a complete picture of all human factors ought to be a standard procedure in this kind of research. We must also recognize and report everything we can concerning the full spectrum of researcher/participant interactions, because we know now that observer and latency effects play a powerful role.[10] We are only at the threshold of understanding the full nature of these exchanges, and we must always be conscious of the possibility that there may be operative channels of informational and energetic interaction of which we are not now aware. The development of the DAT argument by Edwin May, Jessica Utts, and James Spottiswoode illustrates this very clearly.[11] One can never know, in the beginning, when one first enters *terra incognita*, what will prove to be significant in the end.

The only way we can meet the demands of those who will come after us is to give them the gift of accurate and comprehensive reportage, even when it does not seem to make any sense or to be relevant. It is amazing if you go back through the literature in this field, how difficult it is to figure out exactly what people did when they carried out their research. What was that piece of equipment they used? Exactly what model, with what modifications? Who was in the room?

Third, as soon as—but not before—an avenue of research has proven its worth, I would urge the development of common procedures, consistent procedures, something which is common in more mainstream arenas of science, but not as prevalent in the anomalous world we study. One of the most significant

tools in anomalous research to emerge recently is the retrospective meta-analysis: the capacity to look across many laboratories, many clinics working in a worthwhile, that is, theory enlightening avenue of research and say, "overall, this is what this line of experimentation has produced." We are deeply indebted to few in our field, notably Roger Nelson, Dean Radin, Jessica Utts, and the late Chuck Honorton, for their pioneering work in this arena. Such analyses as they carried out are immeasurably more difficult, with less confidence in the end product, when consistent and common procedures are not present.

Finally, let me touch on a critical consensus so pervasive and powerful that for most of us it is taken as a given; the tacit understanding which lies at the core of all sciences—for by now it should be clear that science is not one thing, but many—as well as the culture which is their collective context. While each discipline which has achieved the status of science has a world view distinctly its own, chemists varying slightly from biologists and so forth, there is also what might be called the *Metaparadigm.* For although each science has apartness, it also shares certain primary assumptions with all the other disciplines that recognize one another as having attained paradigm level.

There is an entire hierarchy of science, one that begins with the individual researcher; goes on to the school (sometimes literally the institution with which the researcher is affiliated); then to a discipline; then a paradigm-achieved discipline (or science); finally, a multiscience community made up of the disciplines that have achieved paradigm and share in a metaparadigm.

Each level of membership in the greater whole implies agreement on several critical assumptions, and like the Mobius strip, the paradoxical twist is that the metaparadigm is at once the pinnacle and the base of this consensus. In the case of the current metaparadigm —which, because it is the scientific expression of materialism, I will call the *Grand Material Metaparadigm*—there are at least four of these critical assumptions relevant to this analysis. They are: (1) the mind is solely the result of physiological processes; (2) each consciousness is a discreet entity; (3) no communication is possible except through the defined five senses; and (4) consciousness dwells entirely within the time/space continuum.

Western science in its present form can be practiced because it accepts these world perceptions; without them, it could still be science, but not as most scientists accept it today. Essentially, all sciences which accept the limitation of a metaparadigm are, in aggregate, that metaparadigm's normal science. Under the rules, then, by which the metaparadigm's normal science is practiced, although specific techniques may vary from discipline to discipline, it is always presumed that: (1) the researcher and the experiment can be isolated from affecting each other except in controlled and understood ways; and (2) since the experiment exists in a time-space continuum, the conditions under which it is carried out can be duplicated and the experiment replicated by any other researcher if it is valid.

All of this, the common techniques, the various levels of the collective, the fundamental assumptions which often go unspoken, seem to irresistibly argue for what I will call the myth of gradualism; the idea of gradual incremental change. Yet both that myth and the materialism its supports are refuted by the undeniable reality of scientific change, and how it actually comes about. Those individuals who produce extraordinary research do so not by force of intellect or will alone, although these are important, but because they have had, as they explicitly report, insights arising from nonlocal consciousness *at the same time that there was a crisis.*

It is on this point that most commentators describing the development of scientific breakthroughs are uncomfortably silent. John Mihalasky invokes intuition as an overt explanation, but tentatively,[12] and Kuhn notes only that it represents a change in gestalt, a change in "beingness." "Normal science," he says, "ultimately leads only to the recognition of anomalies and to crises. And these are terminated not by deliberation and interpretation, but by a relatively sudden and unstructured event like a gestalt switch. Scientists then often speak of the 'scales falling from the eyes' or of the "lightning flash" that "inundates" a previously obscure puzzle, enabling its components to be seen in a new way that for the first time permits its solution."[13] To someone interested in the field of non-local informational interactions this wording is virtually identical to the reports they have received from percipients such as healers or remote viewers.[14]

Kuhn is also willing—since the evidence is so great that it cannot be denied —to invoke the inspiration of dreams, although how this actually works he does not venture to say. In fact, he seems so uncomfortable with the moment of genius that he makes only one speculation on the nonintellectual aspect of puzzle solving. He notes, "No ordinary sense of the term 'interpretation' fits these flashes of intuition through which a new paradigm is born. *Though such intuitions depend upon the experience, both anomalous and congruent, gained with the old paradigm, they are not logically or piecemeal linked to particular items of that experience as an interpretation would be* [emphasis added]."[1]

What makes these key figures revolutionaries, then, is not just the quality of their work. They are also revolutionaries because of the source, mechanism unknown, from which their information derives. At the deepest level the process by which the information is obtained is as revolutionary as the information itself.

However, it would be a mistake to see intellectual excellence and intuitive insight as the only criteria for success as a "paradigm shifter." A careful analysis of the process also suggests that some kind of inter-connectedness between breakthrough researchers and their peer communities is involved, a kind of interactive collective awareness that comprises the critical consensus. When this consensus is absent even brilliance can be ignored. Nowhere is this more clearly illustrated than in the work of geneticist Barbara McClintock who showed us how part of evolution worked but it took three decades before everybody else could understood what she had seen, and why it was so important, resulting in her belatedly being awarded the Nobel Prize.[15]

As Gunther Stent demonstrates, if an intuitive researcher is premature, no matter how great the insight, the response of peers is indifference at best,

and martyrdom at worst.[16] Only when intuition and crisis are correctly juxtaposed can the necessary change in gestalt occur. I believe we are seeing the first stages of this process at the metaparadigm level. If I am correct, how might this new metaparadigm be defined? What kind of world view will it represent? What contributions can we make towards exploring the new realms which may lie before us?

Based on research being carried out across the spectrum of the sciences, I believe there are five relevant descriptors helping to define what the new metaparadigm might look like. They are: (1) Only certain aspects of the mind are the result of physiologic processes. (2) Consciousness is causal, and physical reality is its manifestation. (3) All consciousnesses, regardless of their physical manifestations, are part of a network of life which they both inform and influence and are informed and influenced by; there is a passage back and forth between the individual and the collective. (4) Some aspects of consciousness are not limited by the time/space continuum. (5) The ultimate goal of organic evolution cannot now be scientifically defined. We simply lack the necessary data to reach such a conclusion, but Darwinian Survivalism may be only one aspect of evolution's totality.

How we respond to the task of proving—or disproving—these ideas will determine whether this area of science is, in fact, on its cutting edge or its fringe. Being cognizant of the process of scientific development as we do so will help us make insightful choices, and a new paradigm will emerge.

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