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The Care of the Self and Blind Variation:

An Ethnography of the Empirical in Two Sciences.[i]

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1. An Ethnography of the Empirical

In this paper, I want to present two stories about kinds of empiricism; about the ways a science, experimental high energy physics, understands and enacts empirical research, and about how this understanding differs from that of another science, molecular biology. An ethnography of the empirical in different sciences has never been written. One reason for this surely lies in the fact that the meaning of empirical procedure is thought to be common to all experimental sciences, describable in terms of a few injunctions, and spelled out in any textbook introduction to the respective field. By studying scientific laboratories, the new sociology of science overcame the textbook image of science[11], but it did not break away from the assumption that all sciences conform to similar procedures, exemplify similar attitudes to the empirical world, and form part of one culture. It also focussed on the role played by contingencies, interpretation and negotiation in the creation of scientific knowledge. What got left out of the picture was the construction of the empirical machineries involved in this creation.

What are the differences in the empirical procedures of experimental high energy physics and molecular biology? The question itself may seem impossible to answer. After all, the experimental natural sciences deal with their matters in a deep sort of way. The facts they produce are intricate in the making, the things they handle are handled in detailed and complicated ways, the chains of processing involved are infinite and divided into many components. The task to see through the thick growth of experimental manipulations in search for the cultural switchboard that sets the directions is overwhelming, and the sociological revenue may be unclear at first. The help I enlisted was that of the comparative method, which I used less as an asset in generalizing results than as a humble supplier of frameworks of seeing: I looked at high energy physics equipped with a good view of molecular biology, and at molecular biology from the viewpoint of high energy physics. Through such a comparative optics, an ethnographer can discern, not the essential features of a field, but differences between fields, which seem far more tractable anyway ethnographically speaking than essential features. The focus of observation was on the rough build of the empirical machineries at work in the two science; it was not on the level on which single screwdrivers are fidgeted with when they are pointed at individual screws. To characterize these machineries, I shall use the analogy of "blind variation and selection by success" to describe the referent oriented epistemics of molecular biology. My general picture of experimental high energy physics will look different. I shall use the analogy of a closed system that interacts with the world only mediated through interactions with itself to designate what one might call the liminal and recursive epistemics of high energy physics. In designating these differences, I do not draw on philosophical labels such as realism, instrumentalism, pragmatism, conventionalism and the like. This project attempts to provide a richer description of epistemic practice, something like, if I may misuse a term Geertz once made popular, a thick theory of knowledge. If anything is suggested with respect to the philosophy of science than that it might be a mistake to treat the method of science as if it were all one piece. The disunity of the practices discerned is found on the level of their orientation toward and treatment of signs, of their relation to themselves, of the forms of alignments they institute between subjects and natural objects, of their general approach to capturing and engaging truth effects in inquiry. It is also located in how they set up, and shift, and include the referent - whether they attempt to form, with the referent, a common life world or leave the work of dealing with the referent to an interposed machine.

2. The Closed Universe of Particle Physics

There is an analogy which I think appropriately describes the **'truth-finding'** strategy of particle physics. This is the analogy of the brain as an **informationally** closed system. The neurophysiology of cognition is based on results developed in the 19th century according to which states of arousal in a nerve cell in the brain represent only the intensity, but not the nature of the source of the arousal. Maturana and Varela (e.g. 1980) applied these results to the experimental study of perception. They concluded that perception must be seen as a cognitive process which is energetically open but informationally closed. Perception is accomplished by the brain, not the eye, and the brain can only construe what it sees from signals of light intensity which arrive at the retina. In order to form a picture of the nature of the source of these signals, the brain makes reference to its own previous knowledge and uses its own electro-chemical reactions. Phrased differently, in perception the brain only interacts with itself and not with an external environment. It reconstructs the external world in terms of internal states, and in order to accomplish this the brain "observes" itself. Consciousness, according to this theory, is a function of a nervous system capable only of recursive self-observation.

I want to argue that like the brain, particle physics operates within a closed circuitry. In many ways, it operates in a world of objects separated off from the environment, a world entirely reconstructed from within the boundaries of a complicated multi-level technology of representation. A detector is a kind of ultimate seeing device, a sort of microscope that provides for the first level of these representations. The representations themselves show all the ambiguities that afflict any world composed of signs. Yet particle physics is perfectly capable to derive truth effects from its representing **operations**. I want to specify more concretely three aspects of this world before I go on to discuss the strategies particle physics has developed in moving within its boundaries: 1) its experience of objects as signs and the associated technology of representation; 2) Its turn towards the negative - the character of these signs as simulators and deceivers; and 3) the issue of the 'meaninglessness' of measurement which is part of particle physics technology of representation.

2.1 When Objects are Signs

In particle physics experiment, natural objects (cosmic particles) and quasi natural objects (debris of particles smashed in particle collisions) are admitted to experiments only rarely, perhaps for a few periods of several months in an experiment that may last anywhere between eight to sixteen or even twenty years. The proposals for UA1 and UA2, the two large collider **experiments** at CERN were approved in 1978, after several years of preparatory work, and both experiments were dismantled in **1991,** although analysis on some of the experiments' data continues. During the upgrade period in which the detectors were rebuilt and which lasted from the early 1980s to the end, the experiments had 4 "runs" (data taking periods) between 1987 and 1990 of about 4 months each. Thus experiments deal with the objects of interest to them only very occasionally, while most of the experimental time schedule is spent on design, installation, testing and other work outlined below. Second, these objects are in a very precise sense "unreal" or "**phantasmatic**"[iii]; they are too small to be ever seen except indirectly through a detectors, too fast to be captured and contained in a laboratory space, too dangerous to be handled directly. Furthermore, they usually occur in combinations and mixtures with other components which mask their presence. Third, most subatomic particles are very short-lived, transient creatures which exist only for a billionth of a second. They are subject to frequent metamorphosis and to decay which makes their existence into one that is always already past, always already history rather than present.

Now these phantasmatic, historical constantly changing occurrences can only be established indirectly, through the signs they leave when they fly through different pieces of equipment. Physicists deal with them through a technology that creates and exploits representations on three levels. The first level of representations results when particles interact with detector materials through, for example, liberations of electrons and the emission of light by electrons. The work on this level is done by the particles themselves. The experiment is involved through designing and building the apparatus in which the particles register. Physicists, however, don't start with the particles, they start with the detector. A second level of representation involves "representations of the detector", that is "offline" manipulations of the signals extracted from detectors after data have been taken which reconstruct the events in the detector and slowly mold these signals into a form that echoes the particles of interest to physicists. The signs produced by detectors are strewn all over different pieces of equipment and generally meaningless without further elaboration. They must be assembled, interpreted to have certain (energy) meanings, and coordinated to yield consistencies between different representations, that is "tracks". In a sense they must first be put together and brought into shape as signs before their analysis can begin. Physicists' representational vocabulary, their reference to energy and track "reconstruction", to electron "identification", and more generally their implementation of a "production" program that performs the major portion of the work of "producing", from signals meaningless in themselves, signs that can be associated with physics events, exemplifies this work. But this work is not the whole story either. There is a third level of representations: from the reconstruction of events in the detector physicists create "variables" which are no longer interpreted in terms of the signs that register in detector materials but are designed and analyzed in terms of physics distributions and models (e.g. expected distributions for certain kinds of particles).

2.2 The Antiforces of research

The representations physicists deal with are non-arbitrary; in accordance with their own use of the word, they are signals. If there is no relationship between a sign and the object it stands for (a tree and the word "tree") then there is also, in this respect anyhow, no problem. Problems arise when the relationship is thought to be there and one wants to use it to be pointed back to the objects. Frequently and not only in high energy physics, the passage between signal and object is uncertain, strewn with obstacles, and difficult to control.

The obstacle collider experiments face with their sign catching instruments is that the signs of interesting events are muffled and smeared by signs of other occurrences in the detector. These other signals derive from uninteresting parts of events, from other classes of events, or from the apparatus itself - from the signals it emits in addition to signals evoked by real objects. Furthermore, there are limitations of the apparatus which affect the signal. All of these phenomena are a threat to interesting events. They may falsify their signature, misrepresent their character, jeopardize their identification. They deceive detectors and analysts about the presence of events, tamper with the shape of their distributions, and substitute false information for the real one. They are tricksters, fakers and impostors, or just plainly deteriorating factors - factors that worsen the results that one could get in a better world. They aggravate the analysis and cause infinite problems to researchers.

There are forces which stand out in this picture. The most insidious force surely is what the physicists call background: competing processes and classes of events which fake the signal. The physicists in proton-antiproton collider experiments see themselves as "buried in background": "The nature of the problem is to deal not really with the signal so much as the background. You have to deal with the horrible case that you didn't want to see". Their task as they see it is to get the proverbial needle out of the haystack. The signs of the events of interest are almost muted by the background. If you think of these signs in terms of footprints, it is as if millions and even billions of different animals had stampeded over a trail from whose imprints one seeks to discern the tracks of a handful of precious animals - those one is really looking for in the experiment. In the search for the Z zero at CERN in the early 1980s, less than one event was retained out of every 10 000 000 000 (10 to the tenth) interactions[iv]. In the search for the top quark during the upgrade of UA1 and UA2, UA2, for example, expected of the order of 40 top events in six million selected electron triggers (electron candidates), which is already a vastly reduced number compared with the number of interactions.

2.3 The 'meaninglessness' of measurement

An internal universe of signs of "external" occurrences in which these signs are buried in other signs and appearances - these were the first two aspects which feed into the analogy of the closed universe. Let me raise a third issue, one that lies at the core of the universe considered. In fact, it might be its most crucial component; it sets high energy collider physics of the kind described apart from many other sciences. In many fields, measurements, provided they are properly performed and safeguarded by experimenters, count as evidence. They are held to be capable of proving or disproving theories, of suggesting new phenomena, of representing more or less interesting, more or less publishable "results". This holds irrespective of the fact that measurements are theory-laden, prone to raise arguments in crucial cases, and sometimes subject to re-interpretation. What I have in mind is the role of measurements as, one might say, end of the line verdicts; verdicts to which experimental work leads up to in intermediary and final stages, from which it takes its clues, at which it pauses and starts afresh. In high energy collider physics, however, measurements fall short of these qualities. They appear to be curiously immature beings, more defined by their imperfections and shortcomings than by anything they can do. It is as if high energy physics recognized all the problems with measurements philosophers and other analysts of scientific procedures occasionally point their finger at. As if, in addition, they had pushed one problem to its limit and drew a conclusion which other sciences have not taken: that measurements are to be considered as no more than a stage in a cycle of stages, that they are to be pushed back behind the lines of what counts as a result, that they are not to be displayed in public unless accompanied by other elements. Purely experimental data, as physicists say, "mean nothing by themselves". Not only are there few quantities which can be measured relatively directly, but even those which can cannot be taken as they are. They must be further refined by or in some other sense combined with non-measured quantities, such as theoretical ratios and Monte Carlo simulations. As one physicist put it a little indignantly at my insinuation that one might "just measure" the mass of the W: "You cannot read off a detector how big the mass of a particle is like you can read the time off a watch!"

For example, with respect to the strong force coupling constant, Alpha S, in effect a measure of the probability for the emission of a force-carrying particle, what is interesting is not the experimental value but "the theoretical ratio in relation to the experimental ratio for a given detector configuration." This, of course, sounds much more complicated than a simple experimental measurement. And it is. First, one must determine the ratio between the number of W plus 1 jet events divided by the W plus 0 jet events, which, with the search for the top in the experiment studied, one could measure; second, one has to assemble a Monte Carlo program that includes all necessary theoretical calculations and simulates the detector, the "fragmentation", that is the break up of quarks and gluons into jets, the underlying event, etc. From this one obtains the same ratio as the experimental one in theory. The theoretical ratio is a function of, among other things, the coupling constant. It increases when the coupling of relevant particles increases. The experimental ratio, on the other hand, is a constant. The "real" alpha s derives from intersecting the experimental value with the monte carloed curve of the theoretical ratio.

Measurements in HEP always walk on crutches. They are a sort of amputated quantity; a quantity that, without the non-measured parts that are missing from it, is not worth much as an experimental result. It is not a final figure that can stand on its own but a position in a structure of relations to which the other positions must be filled before the whole becomes useful. With respect to the analogy of the closed universe this means that measurements are placed firmly - and obviously - inside the ranks and components of the experiment rather than outside. They are not cast as external evaluations of internal propositions, not even as outposts through which one can make independent contact with the world, but rather as elements and stages that are held in check and turned into something useful only through the interaction of these elements with other features of the experiment.

3. The Structure of the Care of the Self

How does a science like high energy physics nonetheless derive truth effects from the appearances it deals with? The answer is, in a nutshell, that it substitutes for the care of objects the care of the self (Foucault 1986). By this I mean the preoccupation of such an experiment with itself, with observing, controlling, improving and understanding its own components and processes. Confronted with a lack of direct access to the objects it is interested in, caught within a universe of appearances, and unwilling to trespass beyond the boundaries of its liminal approach, high energy collider experiments have chosen to switch, for large stretches of the experiment, from the analysis of objects to the analysis of the self.

3.1 Self-Understanding

This can be seen, for example, by merely looking at an experiment's expenditure of time. More time in an experiment is spent on designing, manufacturing, and installing its own components, and in particular on predicting their performance and understanding every aspect of their working than on anything to do with data. Time expenditure, however, is only one indicator. Another more significant aspect perhaps is the importance credited to self analysis in practices and discourse at all points of experimental activities. This is codified in the native terminology and prescription of "understanding" each aspect of the experiment, for example in understanding the behavior of the detector, which comprises a major portion of the care of the self. The detector is an apparatus that is self-created and assembled within the experiment. Nonetheless, the behaviour of this apparatus, its performance, blemishes and ailments are not self evident to the physicists. These features must be learned, and the project of understanding the behaviour of the detector spells this out (see exhibit 1).

(Exhibit 1 about here)

What exactly does one mean by understanding the behaviour of the detector? First, in the words of physicists, this means "knowing when some physics process of some kind happens in (the detector) what comes out of it". It is "being able to do a perfect mapping" of it, and "trying to unfold what has happened between an input and an output" of results. Understanding the behavior of the detector begins when its first components, like the silicon uncertainties which surround it. It delimits the properties and possibilities of the objects which dwell in this region through the properties of the objects which interfere with them and distort them. Of course if one asks a physicist in this area he or she will say that the goal of it all remains to catch the (positive, phenomenal) particles which are still on the loose, to measure their mass and other (positive, phenomenal) properties, and nothing less. All other things are ways and means to approach this goal. There is no doubt that this goal is indeed what one wishes to achieve, and occasionally succeeds in achieving, as with the nobel prize winning discovery of the vector bosons at CERN in 1983. My point is by no means to deny such motivations or their gratification. However, what interests one when one works one's way into a culture is precisely by what ways and means a group arrives at its gratifications. The upgrading of liminal phenomena, the torch that is shone on them, the time and care devoted to them, is a cultural preference of some interest. For one thing it extends and accentuates what I called high energy physics negative and self referential epistemics. For another, there seems to be a majority of fields by whom the preference is not shared, among them molecular genetics. Third, it is quite remarkable how much one can do by mobilizing negative knowledge.

3.4.1 Knowing one's Limitations: Efficiencies and Acceptance, Errors, and Limits

There are three areas in which the liminal approach is most visible, the area of errors and uncertainties, and the area of corrections, and the area of limit calculations. Limit calculations are analyses in which the goal is to identify the boundaries of a region within which a certain physical process can be said to be unlikely. Limit analyses offer a way out of negative results: If the top quark for which one searches in one's data is not there, it is at least possible to say "up to a certain mass for which we have searched the terrain the top is unlikely to occur. Limit analyses are perhaps the most frequent output of collider experiments. Even in experiments designed to produce precision mass measurements of known particles, such as the LEP experiments at CERN, limits may be the most frequent result: LEP is said to produce "a stream of papers where they produce limits on all sorts of things." Added to the calculation of limits are the calculations of more indirect limitations, the analyses of corrections and of errors and uncertainties physicists perform. Corrections are ways of putting to work all the knowledge the experiment has gained about itself through the care of the self. Corrections refer, mostly, to the calculation of efficiencies and acceptance - figures which indicate whether, if an event is produced in a detector, this event is identified and how well it is identified. With typical analysis, for example an analysis published on the search for the top quark, each particle that is part of the signature of the top will have a string of several (I counted up to 9) efficiencies attached to its identification (particle e- for w for top, track, vertex-finding eff., background cut effs,s). However, self-knowledge too is subject to limitations. To a substantial degree, errors and uncertainties, the 2nd stronghold of the liminal approach, are ways of addressing, on a second level, the blemishes of the above analysis of (efficiency and acceptance)limitations. All sciences, presumably, recognize some measurement errors, but few have such elaborate conceptions of systematic errors and the urge to pursue them into their finest details. Systematic errors point to a systematic problem, like using a ruler that is too short in a measurement of the length of some object, but the problem is unknown. As one physicist put it,

> "the systematic error is just a way to measure our ignorance...(it)is our way to try and estimate what we've done wrong. And if we knew what we did wrong we could correct it rather than to allow for it in the error."

There is a lot of ignorance high energy physics takes stock of in a typical analysis, as the lists of error terms in a published and unpublished analysis show (see Exhibit). Interestingly, the difference between a first and a more refined analysis with higher statistics (more data) often concerns shifts in the error and correction portion, but not in the sense that the list of error terms becomes shorter but that it becomes longer and the terms more precise (some errors may turn into corrections). That measurements are subject to long lists of corrections and have long tails of error terms highlights once more their status as figures which count for nothing if they are not surrounded by an (albeit quantitative) account of their circumstances, conditions, expectancies, differences, etc. Consider an example of the origin of one such error term, one in which physicists feel they have to take into account the difference between different theories. In early 1991, about 45 sets of structure functions which describe the density of quarks and gluons (Partons) within the proton and are needed for calculating the number of expected events in a proton-anti-proton collision were available. They involve different assumptions and count as different theories about how to extrapolate a few available low energy data to higher energies:

> "One structure function might lead you to this value, another to that, etc. If these values would result from measurements you could construct a broad Gaussian out of this with an average and a sigma...But these are not measurement errors, these are different theories, and for the moment we have no way of telling which is right and which is wrong. All of these values are equally probable..."

What physicists do in this situation is that they apply, preferably, all of these functions to their cross section measurements. The variation between different structure functions (the spread between the curves) in regard to the contribution of a particular quark or quark combination to the cross section is then interpreted as the theoretical systematic error or uncertainty associated with the structure function.

To fields which are used to different preferences, this procedure of turning variations between answers to a problem into an error and uncertainty estimation is quite stunning. The mere fact that several theories about the same phenomenon are available in an area counts as an error, and the deviations between the predictions of these theories are used as a resource in estimating the size of the error. Scientificity consists in considering all theories one can get hold of, provided they are not completely outdated by recent measurements. Would sociologists or philosophers care to consider the variability between different theories on a subject as a source for making a calculation of their theoretical error? Different theories in sociology - or in molecular biology - give rise to scientific arguments, and to the formation of different groupings of scientists divided along their theoretical preferences, but never to error calculations. Would it make sense to these fields to require that the dispersion of these different theories should somehow be ascertained, so that we know, if not what is right, than at least how far we might go wrong? Of course sociologists and biologists do not make primarily quantitative predictions. But this is hardly enough to account for the dispreference. There is little concern for exploiting liminal phenomena in these areas, whereas in physics there is.

4. Molecular Biology and its Intervening Technology

Experimental high energy physics is marked by a loss of the empirical; recall the non-encounterability of the objects of interest, the diminished role of pure measurement, the construction of the evidential domain as meaningful only when it is firmly embedded in theoretical predictions, phenomenological laws, and Monte Carlo simulations. Recall also the care of the self in lieu of setting up reactions with the outside world, and the highly sophisticated exploitation of liminal phenomena and objects. Molecular biology, on the other hand, constitutes itself as a system open toward natural and quasi-natural objects. It shows none of the interest of high energy physics in self-understanding and none of its virtuosity in separating off and relating to its own components. Instead, it shows a different virtuosity. It upgrades and enhances natural objects in a continuous stream of experimental action.

In experimental high energy physics, experience appears to provide no more than an occasional touchstone which hurls the system back upon itself, and "success" may well depend on how well - and how intricately - the system interacts with itself. Molecular biology, on the other hand, appears to base progress upon maximizing contact with the empirical world. If, in high energy physics experiments, natural and quasi-natural objects are admitted to the experiment only rarely, in molecular biology they are sought out and encountered on a day-to-day basis. If in high energy physics experiments it seems no longer the phenomenon itself which is at issue but rather its reflection in the light of the internal megamachinery which envelops and tracks down physical occurrences, in molecular biology the phenomena assert themselves as independent beings and inscribe themselves in scientists feelings and experience. Experimental high energy physics can be characterized in terms of a negative, self referential epistemics built around sign systems. In molecular biology, on the other hand, the epistemic culture is orientated toward positive knowledge built from the manipulation of objects in an analog regime that continuously turns away from sign processes.

Three aspects of this preference stand out in our observations of experimental work. One is the close circuit established between scientists and objects through the massive presence of objects in the laboratory and the modes of organization linked to these objects - a mode of organization wherein objects are embedded in processing programs which transforms these objects. The second aspect refers to the further enhancement of objects and experience in what one might call an analog regime - a regime whose components include the embodied functioning of the scientist, visual scripts and the narrative culture of the laboratory. The third aspect which stands out is the preference for 'blind variation' and natural selection by success as a strategy deployed in molecular biology when problems arise. Natural and quasi-natural objects are not only present in the lab on a continuous basis, they are also set up as a selection environment to which experimental strategies propose alternatives for selection when things don't work out. In the following, I shall very briefly summarize some of these aspects.[vii]

4.1 A technology of intervention

Consider first the maximization of contact with the empirical world through the massive presence of objects in the laboratory and the practices of dealing with these objects. Molecular biology laboratories are archetypal in the way they feature testtubes and pipettes, samples of specimen and chemical reactions, small scale instruments and craftlike, manual work. Molecular biology does not process signs, it processes substances and organisms in a multitude of steps and substeps. The technology in terms of which work proceeds is not a technology of representation, but of intervention. Non-life materials are subject to almost any imaginable intrusion and usurpation. They are smashed into fragments, evaporated into gases, dissolved in acids, reduced to extractions, run over columns, mixed with countless other substances, purified, washed, spun round and centrifuged, inhibited and precipitated, exposed to high voltage, heated or frozen, and reconstituted. Cells are grown on a lawn of bacteria and raised in media, incubated and inoculated, counted, transfected, pipetted, submerged in liquid nitrogen and frozen away. Animals are raised and fed in cages, infused with solutions, injected with diverse materials and cut open to extract parts and tissues, they are weighted, cleaned, controlled, superovulated, vasectomized and mated, they are anesthetized, operated on, killed, frozen, and cut into sections and slices, and they will have dispensable parts such as tails cut off to test their genetic make up.

4.2 The analog regime

Through this technology of intervention (a term taken over by Hacking), natural objects and quasi natural objects are included in a common life world in which they thrive, resist, perform their functions etc. in direct and often intimate relationships with scientists and technicians. The laboratory is a kind of second nature biotope, a laboratope, as a student of mine calls it, in which certain things (cells, mice, microorganisms) grow and develop, live through reproductive cycles, and infect and affect each other interspersed with human beings who try to (try to!) arrange, and control some of these processes. The notion of a laboratope stresses the labored nature of this living together; nature is not romantically imitated in the lab; it is split apart, rearranged and disfigured at the same time as it is laboriously reconfigured. Common **life-worlds** are build through co-presence, which **Schütz** saw as an important feature of face-to-face situations, through co-temporality, the possibility of conjoint time, and the possibility of conjoint statuses for human agents and non-human entities or objects; in other words, they are built through structural features of the arrangement; we need not assume shared beliefs or some other form of unity. There are some interesting **structural alignments** which I want to point out. For example, molecular biologists function vis-a-vis natural objects often in a sort of analog mode. By this I mean something like the opposite of a digital functioning (Collins 1991), of the kind of repetitive, dividable, tractable and above all fully describable mode of operation which can be automated and which is sometimes required of factory workers. Analog functioning, on the other hand, is functioning that shuns or refuses description and even cognition. Analog processors are automats too, but because of the dynamic, adaptable, and non-cognized nature of the processing they perform, they are less likely to be modelled by Artificial Intelligence programs. The concept of analog functioning is of course something of a recasting of the notion of tacit knowledge Polanyi once described, a notion that incorrectly stresses, I think, the knowledge aspect rather than the embodied skills aspect, of experts like molecular biologists.

Molecular biologists meet sensory objects as sensory performers; they register things without consciously marking them off, they act upon things in a conversation of gestures (Mead), not a conversation of words, they emphasize and constantly draw upon "experience" with these objects, without being able to spell out, and without caring to codify, what this experience consists off. There is a native discourse on the role of the analog body in research, a discourse embedded in an abundance of instructions which stress embodied experience - which advice practitioners to perform all kinds of experimental activities in person, by themselves, which warn about misinterpretations of results that occur when one has not been present at experiments, and which advise to set aside the time that is needed to "handle", i.e. embody, any method of dealing with objects. There is also the preference for travelling to places where objects are dealt with successfully instead of learning them from lab protocols, and, if they are asked to solve an experimental problem, to solve it by displacing themselves and attaching themselves to the problem situation. What interests me is the distrust in the mind as being able to figure things out at a distance, and in language and communication as supplying, for this purpose, the necessary information. Through this distrust, molecular biologists act inclusionary toward natural objects; they align themselves with them (or they are aligned by them) by letting their processing capacities **be** triggered by situations rather than by mental events, and by substituting "behavior" for cognized, premeditated action.

Molecular biologists analog mode of operation enhances the features and reactions of natural objects. The common life world is a trick, so to speak, that allows this science to adjust on a day to day basis to these features and reactions. There are other forms of enhancement of the phenomenal world; for example the method of appresenting this world and the circumstances which surround it when it is not present through visually recalling its features. When molecular biology produces signs, the activity of decoding becomes equal to opening a window upon the phenomenal reality that supposedly gave rise to the sign; one asks what happened in the lab, which steps were taken, which procedures turned out how, etc. Appresenting is also notable in molecular biologists ways of dealing with invisible objects - the invisible objects are constantly rendered visible through drawing them on paper and blackboards - and in technical discussions, in which scenic descriptions appresenting laboratory objects substitute for detached measurements and technical terms (e.g. you don't talk in grams to indicate a quantity, you talk in laboratory dishes). I am not suggesting, by the way, that in physics, appresenting is never used, or that the analog mode of functioning is unnecessary. I am suggesting, however, that these processes when they occur are usually detector (i.e. equipment) related, and that other epistemic strategies, the ones outlined before, are superimposed upon them.

4.2 'Blind' variation and selection by Success

There is another way of enhancing natural objects in molecular biology which I alluded to in the beginning, and which I want to now sketch out briefly in concluding. Natural objects and processes are also set up in molecular biology work as selection environments to which experimental strategies propose alternatives for selection. This is particularly visible when things don't work out, a common occurrence in all laboratory work. This is also the case where the analogy of blind variation comes into the picture. What is the point of the analogy? In evolutionary biology, mutations introduce variations in the genetic material which can be passed on to descendant molecules or organisms. If a given organism always reproduced itself perfectly, its descendants would never change, and evolutions would be impossible. Which mutations are beneficial and survive is determined by natural selection - the differential advantage bestowed on those organisms whose qualities - introduced by variation - are more effective in a given environment. Mutations, of course, are "blind"; they are random errors not pre-adapted to the environmental conditions which they encounter.

If there is a general strategy molecular biologists adopt in face of open problems, it is a strategy of 'blind' variation joined with a reliance on natural selection. They vary the procedure that produced the problem, and let something like its fitness - its success in yielding effective results - decide the fate of the experimental reaction. Variation is 'blind' in a very precise sense. It is not based on the kind of detailed investigation and understanding of the problem that was so popular with high energy physicists. Confronted with a malfunctioning reaction, a problem of interpretation of data, a string of methods that do not seem to work, molecular biologists will not set out, like physicists, to find, through a "study", the reason for the difficulty. Instead, they will try out several variations and rely on the fact that these will result in the end in workable evidence. Note that in physics understanding and self-convictions are based upon demonstrable data points which detail the crucial aspects of the difficulty. Nothing of the sort of demonstrable data points are necessary or sought after in molecular biology.

4.3 'Blind' Variation and the Care of the Self

"Blind" variation is a strategy of dealing with the resistance of natural objects, equivalent to the master strategy in physics of self-analysis and self-understanding. Let me dwell for a moment on this equivalence. It is important to realize that molecular biology's preference for 'blind' variation and selection by success by no means implies that this method is any less effective than the physics' care of the self and negative epistemics. In fact, molecular biology by all standards has been very successful in the last 20-30 years, and seems bent on remaining successful in the foreseeable future. Moreover, from a molecular biology perspective, it is not at all clear that a strategy like the one adopted by experimental high energy physics would work. Molecular biologists will argue that their attempt 'to understand' a life organism of which little is known quickly reaches its limits, and since the machinery used in molecular biology is largely the life machinery of the cell and of organism reproduction, attempts at 'self understanding the tools and components of the experiment are jeopardized by the same limitations as investigations of the subject matter of molecular biology. Furthermore, they will argue that liminal knowledge, so useful in physics to correct for errors and systematic problems, may be less useful with an intervening technology. If an inadequately construed vector (plasmids or viruses which serve to transport and replicate DNA) generates the wrong protein, this cannot be subtracted out of the experiment through remedial calculations - the vector has to be remade until it performs. Biochemical reactions as used in experiments are not formulated mathematically, and hence cannot be calculated with in the ways the reactions in a detector can be computed. What it all boils down to is that for molecular biology to behave like experimental high energy physics many components of its system would have to change in synchrony with other components. In other words, it would involve a change of the whole epistemic culture. The argument is not that this is impossible. It is just that any central component of a system is often sustained by other components. It is rendered effective by them and works in conjunction with them. 'Blind' variation works with the massive presence of small objects, the intervening technology of molecular biology, its many ways of placing a premium upon empirical reality while de-emphasizing work with representations.

5. Conclusions

There are several things one can say in concluding this paper. For example, one can highlight the uses high energy physics makes of reflexivity. Reflexivity has been the rage in anthropology, and science studies, and literary criticism, and other fields in recent years. It is usually discussed, epistemically speaking, as a monster that must somehow be kept at bay, a serious challenge of which Kuhn (1992) says in a recent paper that our inability to answer it is a grave loss to our understanding of scientific knowledge. Yet in high energy physics we have a field that has long turned reflexivity into a principle of knowing, that brings into focus the possibilities of informationally closed systems in exploiting internal mechanisms and knowledge of the self, and that continuously curls back upon itself while instituting threefold hierarchies of observation elaborated toward the inside, rather than the outside of the system (observation through transitional objects, observation of transitional objects through the experiment, and observation of these observations through error calculations). Perhaps it would be time to ask if we have to have foundations, whether we cannot build a theory of knowledge from circular foundations? Molecular biology, while involving different types of circularities, chooses another road to the referent. It sets up long frontlines in which it engages the other side of the referring activity in analog, "body-to-body" exchanges, thus including natural objects in a system in which they are continually enhanced through assuming the status of a selection environment, through appresentation, and through the willingness of scientists of meeting them on a sensory, object level. This raises the question of the local ontologies different sciences institute in meeting the referent, a question I only touched upon by mentioning transitional objects like the detector or the analog functioning of scientists.

There are other issues one could mention, for example what it means to look from a cultural perspective on both sciences' handling of signs. Signs are prominently present in all sciences, a fact recognized by semiotic and communication oriented perspectives in science studies. Yet from a praxeological perspective on culture, what matters is not their presence but how they are featured, inserted into different processes and dealt with in scientific practice.they are treated very differently in the epistemic practice. Cultural systems of behaviour, as we know, construe the world in which they live differently. If they involve sign processes as they invariably do, the question is nonetheless on what, figuratively speaking, they place their bets and stake their money - signs or not signs. They may construct their world out in terms of these sign processes, or continuously construct it away from such processes. They may choose to combine the care for signs with an elaborate care of the self, or they may show a preference for mechanisms which reduce representations and minimize the interaction with the self. Both methods go by the name of "empirical" and "experimental". Nonetheless, the disunity of these strategies is apparent: the disunity of the two disciplines involved and of "the method of science". Different sciences of this kind feature different epistemic cultures - different ways to approach the world and different ways to derive sources of epistemic profit.

References

Barger, V.D. and R.J.N. Phillips, 1987, Collider Physics. Redwood City, CA: Addison- Wesley Pub. Comp.

Collins, H. and S. Yearley, 1991, "Epistemological Chicken", in A. Pickering (ed.), Science as Practice and Culture. Chicago: University of Chicago Press.

Foucault, M., 1986, "Of Other Spaces", Diacritics 16: 22-27 (transl. from the French by J. Miskowiec).

Knorr, K.D., 1977, "Producing and Reproducing Knowledge: Descriptive or Constructive? Toward a Model of Research Production", Social Science Information 16: 669-96.

Knorr Cetina, K., 1981, The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science. Oxford: Pergamon Press.

Knorr Cetina, K., 1994a, forthcoming, Epistemic Cultures: How Scientists Make Sense.

Knorr Cetina, K., 1994b, forthcoming, "Laboratory Studies: The Cultural Approach to the Study of Science", in S. Jasanoff, G.E. Markle, J.C. Petersen and T.J. Pinch (eds.), Handbook of Science, Technology and Society. Los Angeles: Sage.

Kuhn, T., 1992, "The Trouble with the Historical Philosophy of Science", Robert and Maurien Rothschild distinguished lecture, Harvard University: Department of the History of Science.

Latour, B. and S. Woolgar, 1979, Laboratory Life: The Social Construction of Scientific Facts. Beverly Hills: Sage.

Lynch, M., 1985, Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory. London: Routledge and Kegan Paul.

Maturana, H. and F. Varela, 1980, Autopoeisis and Cognition: The Realization of the Living. Boston.

Traweek, S., 1988, Beamtimes and Lifetimes: The World of High Energy Physics. Cambridge: Harvard University Press.

Turner, Victor, 1969, The Ritual Process. Chicago: Aldine Publishing Company.

Notes

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[ii] The first laboratory studies were conducted in the late seventies and led to be a new understanding of scientific research (see Knorr Cetina 1994b). Examples for such laboratory studies are Knorr Getina (1977, 1981); Latour and Woolgar (1979); Lynch (1985); Traweek (1988).

[iii] One physicist described them to me in German as "irreale Gegenstände", as irrational objects (somewhat like irrational numbers).

[iv] See Barger and Phillips (1987:31)

[v] For an example which exemplifies this attitude, see Knorr Cetina (1994a: chapter 3).

[vi] Victor Turner uses the term to characterize periods during which the status of ritual subjects is ambiguous, as in rites of transition performed by native tribes. See Turner 1969).

[vii] For a detailed analysis of the experiential regime see Knorr Cetina (1994a: ch. 4).