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## Discussion Paper Series

DP 64/03

*Nationality and the Methodology of the Natural Sciences*

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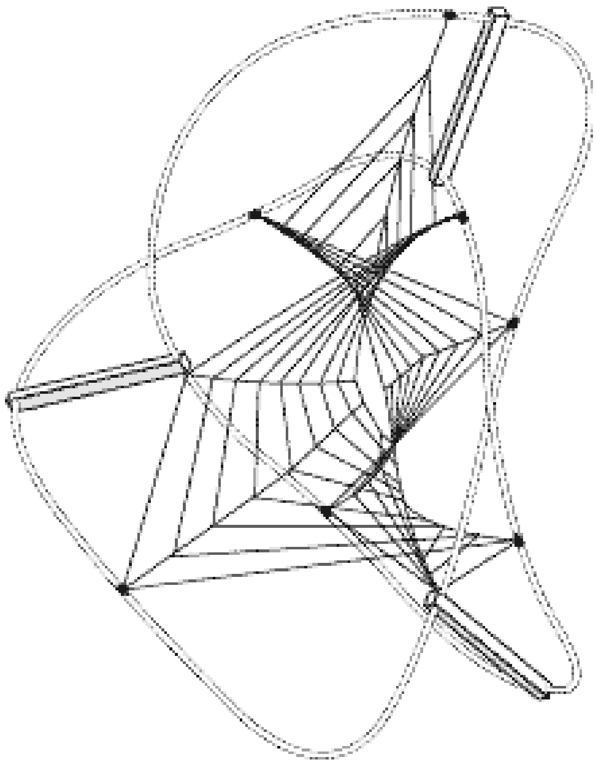

Editor: Max Steuer

# Nationality and the Methodology of the Natural Sciences

Rom Harre

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# Nationality and the Methodology of the Natural Sciences[1]

*Basic Distinctions*

The place of the nation-state/cultural domain in science

The nation-state can be an object of study for some social sciences, and the more complex object, the nation-state/culture can also be such a research object. The material world exists independently of the existence of any and all nation-states and associated cultures. The research object is the same regardless of where and when it is investigated. However, the material world is of such complexity and diversity that different aspects are available or not available to investigators who approach their studies from different points of view. This leads to the question: In what ways are the natural sciences influenced by and/or related to the nation-states and/or cultural matrices in which they are practiced?

I will look into two ways in which there have been national/cultural influences on the natural sciences. One of these is the way *reasons for believing* what someone has contributed are related to the nation and/or culture form in which they lived. The other is the matter of *national/cultural styles* of ways of researching. These include not only how the empirical data are acquired, but also the systems of concepts with which they are interpreted and explained.

Nationalism and National Styles

It is worth noticing the distinction between the influence of the national location of natural scientists on their work, methods and concepts, and the influence of racism and political ideologies on what science is acceptable. In the former category we must recognize the distinction between nationalistic epistemology — only the members of this nation/cultural entity can produce genuine knowledge, e.g. German unwillingness to

[1] This is the text of the paper presented on June 27, 2002 at a two-days workshop on 'Methodological Nationalism', organized by The Centre for the Study of Global Governance and The Centre for Philosophy of Natural and Social Science, LSE.

admit Russian chemists might have been first in the field with structural chemistry, e.g. Butlerov v. Kekule; and national-cultural epistemology, a certain style or method of work becomes established in a particular nation/cultural matrix, e.g.. American preference for interventionist procedures, e.g. experimental studies of genetics, v. European observational studies on natural populations. This is interesting since this cultural preference difference extends to practical medicine beyond natural science.

Racism and ideological epistemology in the culture of the natural sciences are both irrational and self-defeating. For example, the Nazi condemnation of `Jewish science' deprived the Germans of subatomic physics, for good or ill, and the Lysenko/Stalin prohibition of Darwinian evolutionary concepts destroyed Soviet biology for fifty years. I propose to say almost nothing about ideological epistemology, something about nationalistic epistemology and most about national-cultural epistemology. So, I am not concerned with trans national ideological influences, e.g. Lysenko and acquired characteristics, or within nation racist influences, e.g. Nazi rejection of `Jewish science'.

*Reasons for believing*

Epistemology is concerned with critical examinations of reasons for believing items of putative knowledge in this or that context and with respect to this or that subject matter. Bruno Latour and others have raised the question of whether the character, nationality, sex, professional status or age of the one who enunciates a proposition expressing some possible item of knowledge should be taken into account in analyses of the means of scientific knowledge production. When the center of research in some field seems firmly established within the boundaries and institutions of one native state, the very fact of being within the hegemonic group lends authority to a person's claims. Being outside that group, in particular working in the scientific institutions of another less-regarded nation-state weakens that authority. Here are two examples of what I mean.

A German or a Russian. Butlerov and structural chemistry

Philosophers use the term `reify’ to refer the treatment of an abstract entity as a thing, a constituent of the world of material reality. In the mid-nineteenth century chemistry the type formulae, once ubiquitous as classificatory categories of complex molecules, were gradually superceded by structural formulae. Structural formulae first appeared as convenient representations of molecule constitutions in terms of their reaction products. Thus, the same compound might have several representations depending on the way it was analyzed. Soon these `structures’ began to be reified and to be treated as pictures or realistic though abstract representations of the arrangements of atoms in space. They were more than heuristic devices to guide research or classificatory schemes to summarize reactions.

The idea of structure as a prime attribute of the chemical molecule served to resolve all sorts of puzzles. It was the final step in the exploitation of the distinction between atoms, the smallest units that could enter into compounds, and molecules, the smallest units in which compounds could exist. For example, there was a very large number of hydrocarbons, compounds of carbon and hydrogen only. By the eighteen sixties it had become commonplace to think of some carbon compounds as based on chains of carbon atoms and others on rings. A hexagon consisting of a closed chain of six carbon atoms proved to be a particularly useful image. Since hypotheses that these were the structures of the molecules under study were the result of inferences from chemical behaviour, it was possible to interpret them as no more than helpful visual aids. It was another step altogether to take these structures to be genuine maps of real molecular terrains, shapes in physical space.

The story of the transformation of structural diagrams from heuristic device to iconic model involves the outsider status of the originator of the idea of chemical formulae as structural representations as a reason for setting aside his claim to having originated the idea. There is no doubt at all that A. M. Butlerov, then in Kazan, was the first to enunciate a realist reading of structural formulae, and to provide the necessary

supporting principles. The type theorists felt free to write several different formulae for the one substance, each identifying a particular set of the products of different chemical operations. Butlerov realized that if a diagram was to be interpreted as an iconic model of the spatial arrangement of the atoms in the molecule then each substance could have one and only one formula. Each distinct diagrammatic formula must represent just one molecule. In general, then, he argued: "…the chemical nature of a compound molecule depends on the nature and quantity of its elementary constituents and its chemical structure." (Butlerov, quoted in Hartley, 1971). According to Partington (1964:550) Butlerov had claimed his formulae represented `la position topographique des atomes'. Yet, Turkevich (1965: 42) remarks that Butlerov dismissed Kekulé's hypothesis `that spatial positions of the atoms in addition to their nature, determined the properties of organic compounds'. At the same time, he says that Butlerov supported the interpretation of structural formulae as expressing `chemical structure' rather than `physical structure'. The implication being that he opted for their formal rather than their spatial significance

However, by 1858, the German chemist F. A. Kekulé had abandoned his view that structural formulae were only summaries of chemical reactions. In later years, he told the story of how the idea of real molecular structures had come to him in 1854 while dozing in a London bus.

> I fell into a reverie. The atoms were gamboling before my eyes. I had always seen them in motion, these small beings, but I had never succeeded in discerning the nature of their motion. Now, however, I saw how, frequently, two smaller atoms united to form a pair, how a larger one embraced two smaller ones …. But look! What was that? One of the snakes [chains of atoms] has seized hold of his own tail, and the form whirled mockingly before my eyes' (Quoted in Partington: 537).

The German chemical establishment seemed to have refused to acknowledge Butlerov's priority in presenting an unambiguous version of the structural theory of atoms in the molecule, really arranged in space as the formulae presented them. They embarked on a campaign of vilification of the Russian chemists inspired by Butlerov. The

underlying offence, in their eyes, was the impertinence of a provincial in proposing a radical theoretical concept owed, they thought, to no less a person that Kekulé himself. Butlerov was famous for his easy going nature and his tact. His only known response to the German claims to priority is a paragraph in his report to Kazan University about a conference he had attended in Germany. Here is what he said:

> One feature of these German congresses is particularly striking to us foreigners, so strange that it cannot be passed over in silence; it is an aspiration to assert their own nationality at every opportunity (quoted in Leicester, 1966: 17).

His pupil, V. V. Markovnikov (1838 – 1904) was not so reticent. He published an article on the history of the structural hypothesis and set out on a counter-attack, with little immediate result. True, Kekulé had at first seen the move from type theory to more specifically structural formulae as impractical. He had once asserted that, "It is clear that one cannot determine the positions of atoms in a specific compound by a study of metamorphoses…", that is, by a study of the results of substitutions of hydrogen atoms by organic radicals. In whatever way that problem was to be resolved, he became increasingly sure that structural formulae were pictures of the spatial arrangements of atoms in the molecule. By mid-century the realist interpretations of structural formulae were well established, though the seminal role of Butlerov's insights was scarcely mentioned.

<u>Hodgkin, a British woman or a US man: structure of penicillin</u>

As is regrettably often the case in the scientific world, other claimants to the crown of `having established the three-dimensional molecular structure of penicillin' jockeyed for position as the true discoverer. Dorothy Hodgkin's work was quietly sidelined by some, and ignored by others (Ferry, 1998: 215). A particularly galling example for her was a prominent review by Robinson on the studies of penicillin. Ostensibly, it covered work up to 1944. In fact, later work *was* included but not that of

the Oxford team. Similar incidents marred her route to the Nobel Prize (Ferry, 1998: 286 – 290). Even *afte*r she had won the prize for her work on the structure of vitamin $B_{12}$ and penicillin there were some reluctant to acknowledge her priority. This quote is to be found in Sourkes (1967: 241):

> Chain and his colleagues at Oxford began the long and difficult task of elucidating the structure of penicillin; the problem was subsequently taken up elsewhere, chiefly in the United States.

The correspondence between Dorothy Hodgkin and her friends and collaborators, as reported in Ferry (1998) is very revealing not only of the maneuverings of others but also of her own personal thirst for fame!

**National/Cultural Sciences**

In the social sciences, the research object often is the nation-state/culture itself. Sometimes these are coincident in time and space, sometime not. When they are not, I think that it is the culture that is the usual object of research. In the case of the natural sciences, the nation-state/culture is at one remove from the research object, for the most part. Ecology, physical geography and other earth sciences sometimes identify research objects that respect national boundaries or the temporal spans of political organizations such as the communist regime in East Germany, but the research objects of physics and chemistry do not include these features of the social world as component attributes.

Nevertheless, there are some influences from the national/cultural location of the researchers on the content and also on the methods of research. Military demands and economic necessities are well known and heavily researched influences on the content of research programs. I want to look at something subtler, the matter of national style and its expression in methods of research as well as choice of classificatory and explanatory concepts.

There is one well known and often quoted instance of a distinction in national style that I mention only in passing. Pierre Duhem is famously quoted as saying that

when he opened a book of physics by an English author, expecting to find high level abstract representations of laws of nature, he found himself instead in a workshop — referring thereby to the propensity for English physicists to work from iconic, even mechanical models towards mathematical formulations. Lord Kelvin's gyroscopic ether atoms, and Clerk Maxwell's rotating tubes of force were the sort of thing Duhem had in mind. I want to look at three other cases of national/cultural style in physics.

*British Mathematics 1600 - 1900. The calculus*

The differential and integral calculus was developed about 1660 as the culmination of a century of effort by mathematicians on both sides of the English Channel (La Manche). However, it made a simultaneous appearance in the works of two great mathematicians, Isaac Newton and Gottfried Leibniz. To whom should the prize of priority be awarded? The dispute was long and bitter. Yet, in hindsight, it is clear that each author had a very different conception of what had been discovered and within what framework of concepts (Cajori, 1911).

At first sight it might look as of the Newtonian approach based on the representation of fluents and fluxions, and the Leibnizean approach based on ratios of infinitesimal increments of space and time as ds/dt was a distinction without a difference. Each arrived at the correct procedures for differentiating and integrating functions. Newton and the British mathematicians who followed him used conceptions based on instantaneous velocity and while Leibniz used concepts based on average velocity.

Newton took lines to be fluents described by moving points, and the instantaneous velocities of such points at point locations to be the `first fluxions'. Leibniz used the concept of average velocity, distance/time, thus coming up with the definition of the differential coefficient as the slope of the curve F(st) $\in s/\in t$ which becomes ds/dt as $\in t$ tends to 0.

What was the difference in the two outlooks? Newtonian mathematics took continuity as given, and thus found no difficulty with the concepts of instantaneous velocity and instantaneous acceleration. Leibniz approached instantaneous velocity as a

special case of average velocity. However, this required not only a concept of limit shared with the Newtonians, but also an implausible concept of the denominator of a fraction tending to 0 while the value of the fraction tended to some finite quantity.

This was certainly a very clear national difference, but was it part of some more complex and wider cultural matrix? It certainly fits with the Bosvichean metaphysics of powers and continuous (non-atomistic) conceptions of physical interactions that was common to Davy, Faraday and others in Britain, e.g. Baxter in Scotland. In 1749 R. J. Boscovich had published his *Theoria* with the declared intent of reconciling Newtonian and Leibnizean physics. While the concept of the fluxion of a fluent emphasized continuity, Newton had also made much of the idea of a universe of discrete, material and incompressible atoms, extended in space, acting only by contact. He never resolved the clash of this general metaphysics with gravity, seemingly acting at a distance. By abandoning the extended material atom in favour of point centers of power mutually interacting at all degrees of spatial displacement, Boscovich sketched a general field theory of continuous interaction. Davy knew of Boscovich's work, and devoted a chapter of his *Consolations in Travel* to a confession of allegiance to Boscovichean metaphysics. Faraday followed his one time mentor in basing his experimental researches on a Boscovichean ontology.

Greene explicitly claimed as the English point of view the dynamicist metaphysics that supplanted a material orientation with causal powers. This was long before the general English acceptance of Boscovichean metaphysics. Greene declared the English position to be distinct from the Italian scientific metaphysics of Galileo and the French metaphysics of Descartes. In this example we have a clear case of a national style that dominated mathematical practice, theorizing in physics and, indirectly, the thrust of experimental research, particularly the experimental researches undertaken by Michael Faraday. There seems to be a complex cultural object, including `British science'. As methodologists, we need to add `being British', for example, to our catalogue of reasons for believing. As sociologists of science we must recognize, as Greene did, a higher-order object of study, national science, identified by a certain style.

I have abstracted one strand from a more complex history to make this point. The story of British science and its contrasts with `Continental science' is more complicated. Leibniz too espoused and defended a Law of Continuity as a general metaphysical principle guiding one's formation of concepts in mathematics and physics. Newton's followers were corpuscularians, even if they were agnostic about whether there were minima naturalis, true material atoms. In his *Origins of Forms and Qualities* Boyle offered a wide range of experimental `proofs' of the corpuscularian character of material stuffs. For example, he pointed out that the idea that qualities are drawn up as quasi-substances from the earth by the roots of plants is refuted by the fact that grafting pear onto a black thorn stock, one gets sweet pears. The fruit of the blackthorn is bitter. Therefore, the material basis of sweetness must be a consequence of the rearrangement of corpuscles by the process of ripening the fruit. In general, matter was lumpy and discontinuous in a continuous spatio-temporal void.

*Natural Religion: The British way of linking Theology and Natural Science*

In the first of Leibniz's letters that opens the Leibniz-Clarke correspondence, Leibniz deplores the British penchant for Natural Religion. According to the British point of view, God's hand should be discernible in his handiwork, the material world. Both Newton and Boyle explicitly declare that their aim in pursuing scientific investigations of material nature is, as Newton put it, [to persuade] `considering men [of] the existence and attributes of God'. Having rejected the authority of the church and its claim to exclusive rights to interpret religious texts, the English in particular, were inclined to reject that authority in matters of science. Natural religion is one more step in that direction.

Leibniz claimed to have shown that this world was the only possible world by the a priori route of deducing its necessity and its main characteristics from God's nature, in accordance with the laws of logic. Only one possible world is non-self-contradictory, has a sufficient reason for being thus and so, meets the criterion of the identity of indiscernibles, that is, is unique, and displays maximal compossibility. Since God must create what he can create on pain of imperfection, there is only one possible world.

Let me illustrate the style of Natural Religion with an extraordinary example, Whiston's proof of the existence of God from Newton's gravitation law. The law

$$F\ (\text{gravity}) = G\ m_1.m_2/d^2$$

relates the force of gravity to the masses and so to the volumes of the gravitating bodies, since mass is proportion to $r^3$ for a spherical mass. Action by contact would be proportional to the surface area of any material body, which, for a sphere, is proportional to $r^2$. So it follows that gravity cannot be the effect of a material agent, acting by contact. It must therefore be the effect of the activity of an immaterial agent. This can be none other than God.

Once again, we can see that as methodologists we must include national-cultural affiliation among reasons for believing, while as sociologists of science, we must attend to a specific cultural object, the national-cultural tradition which includes such reasons for believing.

*Niels Bohr and the Sources of his Philosophy of Physics*

For half a century, the debate between Einstein and Bohr concerning the meaning of the EPR(What is this?) thought experiment has been the source of a radical misunderstanding of Bohr's philosophy of physics. In the nineteen fifties this debate was seen as a clash between a scientific realist, namely Einstein, and a positivist, namely Bohr. In hindsight, it is quite clear that Einstein had very little idea of Bohr's point of view. Just as in Britain of the seventeenth and eighteenth centuries, in nineteenth and twentieth century Denmark there was a unique national philosophical tradition that we must recognise as a distinct cultural object.

In 1852 Soren Kierkegaard published his *Either/Or.* Though the argument was set in the domain of moral philosophy the thesis had far-reaching applications. There are no a priori binary distinctions which automatically distinguish two exclusive categories of moral standards. One can only make judgments of right and wrong, good or evil, after the distinction between right and wrong has been taken up. In one context, there is no distinction to be drawn between good and evil, since the difference has not been recognized. In another context, where it is recognized judgments in accordance with it

can be made. It is not hard to see that here we have an ancestor of Bohr's famous concept of complementarity.

In the context of experimental physics we have an application for the distinction *either wave or particle*. This distinction comes along with the impossibility of constructing a wave detector and a particle detector at the same place at the same time. In the context of theoretical physics, we have no grounds for declaring that physical reality must be both wave-like and particulate. It is neither. The distinction is applicable only to experimental set-ups. Bohr is a scientific realist, but his conception of material nature is Kantian. The Noumenal World is misdescribed if we try to use empirical concepts beyond the bounds of sense.

What justification do we have for ascribing these ideas to Niels Bohr? Bohr himself had great difficulty expressing his insights verbally. My Danish colleagues tell me that there is the same difficulty for the reader whether Bohr was writing in Danish or English. This is not surprising in the light of the originality and depth of Bohr's thought. However, once on(e) is aware of the background to all this in the Danish philosophical tradition, and takes note of the Kantian terminology Bohr uses for certain key concepts, his writings suddenly become quite clear.

We know that Bohr was greatly impressed by the novel *The Adventures of a Danish Student* by P. M. Möller. The story centers round the problem of knowing oneself. Only one's actions can be observed, how one manifests oneself in the world, situation by situation. We should also note how much Bohr derived from his father's writings, even on occasion by direct quotation. Christian Bohr's comments on the study of physiology include the telling Kierkegaardian thought that the physiological response of an animal can only be considered in relation to a specific context of elicitation. Niels quotes the very paragraph in which Christian Bohr expresses this thought. [2] The German romantic, Schiller, seems to have provided further inspiration for Bohrian complementarity. There can be no doubt that Niels Bohr's way of looking all intellectual enterprises, including quantum mechanics, is a direct descendent of the Danish intellectual tradition, touched at important points by Kantian and pos-Kantian German philosophy and literature.

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[2] I owe all this personal information about Niels Bohr to my colleague Steen Brock of Aarhus University.

*Interim Conclusion*

I have already pointed out that for sociologists of science there are research objects that are bounded as temporal and nation-state/cultural units, admittedly with fuzzy edges and contestable limits. Thus, as a sociologist or even as a philosopher of science, one studies British Mathematical Physics or Danish (Bohrian or its caricature version, the `Copenhagen Interpretation') Quantum Mechanics. However, these `objects' are also the targets of study by science students. As an undergraduate I studied Leibnizean, not Newtonian mathematics, more is the pity. Later I studied Copenhagen Quantum Mechanics. Now I know that there is another nation-state/cultural object, Bohrian Quantum Mechanics to be studied. These objects of study are components of complex cultural-historical matrices. Learning physics or learning biology one is learning X-physics or Y-biology, where the variable can be substituted for by several different cultural referents.

**Methods of enquiry**

When one is presented with the result of a research endeavour, one can assess the status and relative importance of it by asking the question `By whom, when and when(Do you mean 'where'?) was this research undertaken?' The three `W-questions' lead us straight to nation-state/cultural matrices and traditions once again. In this section, our foil will be the research strategies and their implicit groundings of American scientists. The geographical boundaries of the United States will not serve to bound the cultural matrix involved, since in at least one important case the research was carried out within the United States by a scientist who brought his cultural presuppositions with him.

Indigenous American culture is shot through with the idea of `taking action'. Interventionist strategies are evident in American medicine, in sharp contrast to the `Help Nature heal' strategies of British medicine. There is very little of the `wu wei' attitude of Taoism to be seen in American practices, even though the ultimate Taoist epigram `If it ain't broke don't fix it' is of American origin. In science, and particularly in biology, this

national-cultural imperative shows itself as an American penchant for experimental, laboratory based studies in contrast to European natural history style of enquiry.

To illustrate the difference between an interventionist, experimental research programme and one based on naturalistic observation, I will offer three brief case studies: American v. Russian evolutionary genetics; American v. British studies of natural selection; and American v. European studies of animal behaviour.

*Case 1: The Drosophila Model*

Some 60,000 papers have been published to date on the genetics of the common fruit fly, *drosophila melanogaster*. This rapid breeding species first attracted the attention of American biologists in the United States. The story begins with the work of W. E. Castle in 1901. He used laboratory-breeding populations, following the distribution of characteristics generation by generation, much as Mendel had done with peas in the Abbey garden. In 1907, a major research program into Lamarckianism by was begun by F. Payne in T. H. Morgan's laboratory in Columbia University. Again, it was strictly and perhaps necessarily laboratory based. Payne kept 40 generations of the insects in the dark. If Lamarck's thesis that the environment directly affected the genetic material had been correct surely in that span of generational `time' the eyes of later generations would have changed or even atrophied through disuse. Payne found no change at all.

We must see this project and many later ones that adopted the laboratory method as a nation-state/cultural tradition. Payne was a student of Moenkhaus at Indiana University, who, in turn, had been a student of Castle. Studies by Americans from the time of Castle have almost all been laboratory based in populations in artificial environments.

The story of Russian research into evolutionary genetics with *drosophila m*. displays a very difference(different) nation-state/cultural tradition. Russians worked in a well-established natural history tradition, which we can see as part of a pan-European style in biological research. They studied natural populations of *drosophila m.* This work culminated in and around the years 1926 – 7. It was led by S. S. Chetverikov. In all probability the idea of using *drosophila m.* as the research object had been suggested by the earlier American studies.

This line of research was stopped dead by Lysenko, the advocate of Lamarckism. Stalin's paranoiac adherence to his version of Marxist-Leninism was sympathetic to Lamarckian biology. He listened to Lysenko. Most of the leading biologists were shipped off to Siberia on his orders, where some even tried to continue their work in exile. The beneficiaries of this persecution were in the West. Probably the most important evolutionary biologist of the Twentieth Century, T. Dobzhansky, came from the Ukraine to the United States, bringing the Russian natural history tradition of field studies with him. Delighted by the countryside of the Western United States Dobzhansky continued to base his work on the genetics of *drosophila m*. but in the Russian style, studying natural populations in the rural areas he enjoyed. His great book was published in 1937.

The contrast I wish to draw is not to be read as expressing a judgment one way or the other on the virtue of the laboratory method vis à vis fieldwork. In the human sciences, with which I am not concerned in this paper, the situation is rather different. American social psychology, for example, lags far behind the context sensitive work in the European tradition.

*Case 2: Natural Selection: The Empirical Proof of Darwinian evolutionary theory.*

In what must surely be the ultimate example of research within the `natural history tendency'(,) Drs Grant and Grant from London University set about a study of natural selection as it takes place in the wild (Weiner, 1996). Every year for ten years they returned to one of the Galapagos Islands, made famous by Darwin, to collect data on the successive populations of finches. The variation, island by island, of the beaks of these birds was one of the most telling pieces of evidence that Darwin took to support his evolution by natural selection hypothesis. The Grants and their assistants captured, weighed, ringed and measured the beak of *every* finch hatched *each* year as it grew to adulthood. They were thus able to relate the size of the brood in any one year to the characteristics of those birds that survived to breed as adults in the next generation. This was not a statistical survey of a sample of a population. The data were the results of measurements of the entire population, bird by bird. Each beak shape and size is adapted to a certain size, type and shape of seed. By counting the seeds on sample areas of the

feeding grounds of their birds, they accumulated the data necessary to test the Darwinian hypothesis. Birds with beaks better suited to a certain pattern of edible seeds would tend to have more offspring, thus generation by generation changing the physical shape of the beaks of the whole population.

This was not a study of a model of a natural system and the processes that go on in it. It was a study of the very thing itself. The Grants' team was collecting *complete* record(s) of finches' beaks generation by generation.

It stands in contrast to the study of evolutionary genetics in captive populations in artificial model worlds along a different dimension from that in which Dobzhansky's studies stood in relation to captive *drosophila m*. genetics. Yet, in both cases the foil is the American cultural imperative to control and master nature by the experimental pattern based on a methodology of dependent and independent variables

*Case 3: Primate Psychology*

Primate studies display the same contrast. Harry F. Harlow (1962) of the University of Wisconsin, carried out extensive laboratory based studies of the psychology of chimpanzees and other primates. For example he tried to partition to `variables' involved in the affiliation between infant and mother, by constructing a surrogate `mother' out of a towel and a nipple. By varying various conditions in which infants were related to the surrogate he hoped to identify the causal factors that led to affiliation. Some progress in understanding the relevant factors was made, but the downside of this and other experiments was the subsequent psychological difficulties suffered by the animals reared in schematic and deprived environments.

Just as in the first two cases cited in this section, the work by Jane Goodall (1971) in the Africa forest observing populations of wild chimpanzees, revealed a great deal that was invisible to Harlow. Rearing practices, disciplining of infants, peer relations, the means by which status is established and maintained, the organization of hunts and much else was revealed in studied in the wild. This distinction is also reflected in animal behaviour studies in general between laboratory based by Americans and observational ethology of the great Europeans. Nikko Tinbergen, for most part, followed the lives and

behaviour patterns of insects, birds and fish in their natural habitats. Konrad Lorenz lived with and among his animals and birds. He did not discover imprinting by a laboratory study of affiliations processes. It happened as he went around his geese. von Frisch discovered the bee dance by which information about the distance and direction of a food supply is conveyed to other bees by closely and patiently watching a hive (For a summary of the history of ethology c.f. Tinbergen,1969: 238 – 268).

Again, I must underline the point that these examples are not intended to rank laboratory studies and field work in biology according to some scale of competence or fruitfulness. Much can be learned in the laboratory in which environments are schematized and variables controlled. Much can be learned by studies of animals (and plants) in their natural habitats, sometimes over many years. Among the great figures of the past Darwin exemplifies, for the most part, the latter, while Gregor Mendel exemplifies the former.

**Conclusion**

While it is clear that there is British Mathematics, German Chemistry, Danish Physics and European and American Biology as socio-cultural objects, the natural sciences, in the very long run, seem to distill a higher order body of knowledge from the nation-state/cultural matrices in which research is carried out day by day. Methodologists need to acknowledge that there are local traditions not only in choice of concepts for theory making and classifying the phenomena of science, but also in what are favoured as empirical methods. As a student progresses through the layers of the education system for the most part, and the calculus is an exception, the work encountered becomes more and more a matter of national or cultural traditions.

The sociologist of science *must* take the nation-state/cultural scientific activity and its results as the primary research object , acknowledging what ethnomethodologists have called the indexicality of scientific bodies of knowledge and taklen-for-granted procedures.

The natural sciences, as human products, are not so different from the social sciences.

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