Jean Piaget Society

20th. Annual Symposium: "Piaget in Retrospect and Prospect"

Philadelphia, Pennsylvania, June 1990.

The Structure of Knowledge and the Knowledge of Structure.

Rolando García

(Keynote address at the opening plenary session)

Instituto de Salud Colectiva
Universidad Nacional de Lanús

Physical intuition and the constitution of matter.

This revolution implied a drastic change in the basic concepts of science. It was not only that some concepts were substituted by others, although this was also the case. In the Theory of Relativity, for example, "space" and "time" were replaced by "space-time". That seemingly superficial substitution had profound implications.

The new concept required renouncing customary ideas associated with the concepts of space and time as found in what is usually referred to as "direct intuition". Neither Newton nor his followers thought the day would come when those intuitions were questioned. The idea of a "real" absolute space where all bodies are located had already been questioned by Leibniz(1720)[*], but his critique never came to shake Newtonian physics.

Even one of the most brilliant minds of the century, Heisenberg, had difficulties in accepting the new conceptions. In one of his autobiographical books Heisenberg (1972) writes of a dialogue he had in his youth with Wolfgang Pauli in which he declared that while the mathematical frame of Einstein's theory did not cause him any problems, he found it incomprehensible "that a moving observer might understand the word 'time' differently from a stationary observer."

In spite of this statement, in a very short time Heisenberg himself was to become one of the principal actors

Instituto de Salud Colectiva
Universidad Nacional de Lanús

of the greatest drama in the history of physics. I say "drama" because it meant the definitive renunciation of the use of our intuition in the comprehension of the constitution of matter. More than that, from this moment onwards, the very notion of "understanding" would change in meaning.

In this respect I would like to refer in some detail to a dialogue between the young Heisenberg and the great Danish physicist Niels Bohr. It is well known that Bohr enunciated a theory of the atom, still taught today in secondary schools, according to which the atom is conceived as a planetary system in miniature. At the centre of the atom is the nucleus containing most of the mass and surrounded by much lighter electrons (particles with a negative electric charge) that move around it as the planets around the sun. From the point of view of classical physics, such an image of the atom cannot be sustained because if the atoms were indeed thus constituted, matter would not be stable.

At the beginning of 1922 Bohr was invited by Sommerfeld to Göttingen to deliver a series of lectures on atomic theory. Bohr was impressed by the questions posed by young Heisenberg, then a student, and invited him to walk with him so they could discuss the issues after the lectures. During the discussion, Heisenberg expressed his doubts concerning the conception of the atom as consisting of particles charged with electricity - the electrons - that actually move around the nucleus (Heisenberg, 1972, pp. 64/65).

Instituto de Salud Colectiva
Universidad Nacional de Lanús

Bohr's reply merits quoting in full but I shall give only the gist of it:

"Because matter is stable, Newtonian physics cannot be correct at a sub-atomic level; at most it can give us a starting point[3]. And, for the same reason, there cannot be a visual description of the structure of the atom, since such a description - precisely because it is visual - has to be based on the concepts of classical physics, concepts which no longer enable us to apprehend phenomena. You must realize - Bohr continued - that the attempt to formulate a theory of this sort is a task that is _a priori_ impossible. Because we have to say things about the structure of the atom, but we do not have a language which can make what we want to say comprehensible."

Heisenberg insists - "but what then do the images of atoms that you have discussed and even justified in your lectures of the past few days mean? What is it that you really wanted to say?"

"These images - Bohr replied - have been deduced or, if you prefer, 'guessed' on the basis of experimental facts; they are not the fruit of theoretical calculations. I hope that these images describe the structure of atoms _as well_ - but only _as well_ - as

Instituto de Salud Colectiva
Universidad Nacional de Lanús

possible in the visual language of classical physics. We should realize that we cannot use this language here except as poets do. They also do not seek to represent facts in a precise manner, but hope only to create images in the spirit of their audience and to establish connections at the level of ideas." (ibid).

This dialogue between two of the greatest revolutionaries in the history of ideas is more eloquent for the epistemologist than any treatise on the situation of science in our century. Bohr's comments posed what appear to be insurmountable problems: if physical theory is to be accepted and it implies renouncing the concepts of solid matter, trajectories in space-time and causality, how could it be validated by measurements with instruments that assume these concepts? What kind of world is the world of physics if none of our basic physical intuitions are applicable to it? What kind of "knowledge" is "physical knowledge"?

Physical theory and Mathematics: Russell and Carnap.

It was Russell, one of the great philosophers of this century who attempted to provide an answer to the above questions and to extract the consequences for the theory of knowledge. In 1927 the same year that saw the culmination of the research on the structure of the atom - with the quantum theory of Heisenberg - Bertrand Russell published his book The Analysis of Matter (Russell, 1927), a volume not much

Instituto de Salud Colectiva
Universidad Nacional de Lanús

read by physicists because it is very philosophical and not much read by philosophers because it contains a lot of mathematics. Russell's theory can be summarized in a simple sentence: "Our knowledge of the physical world is only a knowledge of structure". In plain terms, Russell's claim meant that physics can tell us nothing about the nature of the physical world beyond the structural relations represented in the mathematical framework of physical theories.

It should be clear that this formulation is radically different from the position inherited from the seventeenth century. Galileo (1632) had stated that,

> the great book of the universe (...) cannot be comprehended unless one first learns to understand the language and read the letters with which it is composed. It is written in the language of mathematics and the characters are triangles, circles and other geometrical figures without which it is humanly impossible to comprehend a single word of it.

Galileo's position consists of showing the necessity of expressing natural phenomena (which are studied by means of observation and experimentation) in mathematical statements. Mathematics thus appears as the <u>language</u> in which the laws of nature are expressed, a language that gives them greater precision, allows interrelationships to be clearly

Instituto de Salud Colectiva
Universidad Nacional de Lanús

established and opens up the possibility of making predictions. But the laws refer to a world that is "out there", and which is known just as it is through observation and experimentation. The tradition of Galileo was taken up and carried to its ultimate conclusion by one of the epistemologies typical of our century: logical empiricism. Its chief exponent was Rudolf Carnap.

Let us now look briefly at why neither Russell or Carnap provided a solid foundation to their epistemological theories. Russell's The Analysis of Matter and Carnap's work, Der Logische Aufbau der Welt, published in 1928 are the most relevant works to be considered*.

We will begin with Russell. The thesis that "we only know the structure of the physical world" was demolished a year later by M.H.A. Newman (1928), a mathematician not very inclined towards epistemology, in an article published in Mind. Newman develops his logical argument with the utmost clarity, but I will not present it in full as what is important for the present discussion is his conclusion that Russell's theory is either trivial or false. The fundamental reason is that if we only know the structure of the physical world, we know nothing. Because given any structure, all sets, whatever the nature of their elements, having the same cardinality (the same number of elements, if they are finite sets), can be organized in a manner satisfying the structure. Therefore, if we do not know anything else, there

Instituto de Salud Colectiva
Universidad Nacional de Lanús

is no basis to assert that a given structure is in fact the structure of our world.

Newman sent a copy of his article to Russell, who replied with a pathetic letter:

Dear Newman,

Many thanks for sending me the off-print of your article about me in Mind. I read it with great interest and some dismay. You make it entirely obvious that my statements to the effect that nothing is known about the physical world except its structure are either false or trivial, and I am somewhat ashamed at not having noticed the point for myself.

It is clear that the Achilles heel of Russell's theory was the word "only". To be able to say that the structure that we know is the one that corresponds to our physical world, we need to know something more than the structure.

Russell never mentioned Newman's objection in his later works. He was to formulate the position once again, in a slightly modified form in his reply to Nagel, in the volume The Philosophy of Bertrand Russell (P.A. Schilpp, (Ed.). 1944):

"The inference by which physicists pass from percepts to physical objects (which we are assuming valid) only enable us to know certain facts about the structure of

Instituto de Salud Colectiva
Universidad Nacional de Lanús

the physical world as ordered by means of causal relations, compresence*, and contiguity. Beyond certain very abstract mathematical properties, physics can tell us nothing about the character of the physical world." (Russell, 1944, p. 706)

In his last philosophical work, (Russell, 1948), the theme is not explicitly mentioned, but there are indirect references that make evident changes in Russell's position. For example, in the chapter entitled "The World of Physics" he states:

"Mathematical Physics contains such an immense superstructure of theory that its basis in observation tends to be obscured. It is, however, an empirical study, and its empirical character appears most unequivocally where the physical constants are concerned." (Russell, 1948, p. 27).

Another indication of change is seen in the chapter entitled "Structure", which concludes in the following manner:

"Considerations deriving from the importance of structure show that our knowledge, especially in physics, is much more abstract and much more infected with logic than it would seem. There is, however, a

Instituto de Salud Colectiva
Universidad Nacional de Lanús

> very definite limit to the process of turning physics into logic and mathematics; it is set by the fact that physics is an empirical science, depending for its credibility upon relations to our perceptive experiences." (Russell, 1948, p. 256).

Russell does not mention how these relations are established. The kind of construction he had proposed in previous works, particularly in Russell (1914), could not be sustained. In my view, he was prevented from going beyond them by his empiricism (despite the fact it did become weaker over time). Perhaps for this reason the final chapter of this last philosophical work is entitled "The Limits of Empiricism". In it he states "it must be admitted that empiricism as a theory of knowledge has proved inadequate". And he adds - perhaps as a consolation - "...though less so than any other previous theory of knowledge". (Russell, 1948, p. 507).

I would hazard a guess that Russell arrived at the very doors of constructivism, but did not dare to enter.

Let us now look briefly at Carnap's position. Carnap (1928) makes some statements that may appear to parallel the former Russellian structuralism, and even have some "Piagetian flavour". Let us take one example:

> "If science is to be objective, then it must restrict itself to statements about structural properties,

Instituto de Salud Colectiva
Universidad Nacional de Lanús

> and...it can restrict itself to statements about structures, since all objects of knowledge are not content but forms and can be represented as structural entities." (Carnap, 1928, p. 107 of the Dover edition)

But the "Piagetian flavour" disappears when we go closely into the bases for such a statement.

Carnap's thesis, according to which "science only deals with the description of structural properties of objects" should be placed in the context of his more encompassing thesis that "each scientific statement can in principle be so transformed that it is <u>nothing but</u> a structure statement" (the emphasis is ours). Carnap adds a surprising and revealing assertion: "this transformation is not only possible, it is imperative. For science wants to speak about what is objective, and whatever does not belong to the structure but to the material (i.e. anything that can be pointed out in a concrete ostensive definition) is in the final analysis, subjective". (Carnap, 1928, p. 29 of the Dover edition).

It is well known that in Carnap's empiricist epistemology, knowledge starts from perceptions leading to "ostensive definitions" and the problem he had to face was how the knower went from there to structural properties. His resolution led to a truly monumental construction, but it fell short of the target. It must be said that his was an honourable failure. With admirable honesty Carnap recognized

Instituto de Salud Colectiva
Universidad Nacional de Lanús

his failure and made no further attempts. In fact in his later courses on the philosophy of physics (Carnap, 1966) no trace of this resolution remains, although his empiricist convictions stayed intact.

Carnap and Russell were not the only ones who arrived at a structuralist conception of science from empiricist positions. The most conspicuous example within present-day philosophy of science is Quine. A peculiar kind of empiricist, to be sure, having repudiated what he calls the "dogmas" of empiricism. The following statement, for instance, does not differ from Russell's: "Structure is what matters to a theory, and not the choice of its objects" (Quine, 1981, p. 20). Although Quine recognizes that Ramsay and Russell both held this position before he did, he points out that they referred only to theoretical objects, and adds on the same page, "I extended the doctrine to objects generally, for I see all objects as theoretical". This statement receives further clarification in another chapter: "Bodies and our knowledge of them are related only structurally and causally and not by a sharing of qualities" (Quine, 1981, p. 177).

Quine is quite aware of the roots of the difficulties faced by both Russell and Carnap in trying to carry out the programme that he defines as "translation of all discourse about the external world into terms of sense data, set theory, and logic" (Quine, 1981, pp. 83/84). This is clearly indicated in the same text. Although recognising that

Instituto de Salud Colectiva
Universidad Nacional de Lanús

"Carnap achieved remarkable feats of construction, starting with sense data and building explicitly, with full Principia techniques and Principia ingenuity toward the external world", Quine concludes: "One must in the end despair of the full definitional reduction...and it is one of the merits of the Aufbau that we can see from it where the obstacles lie" (Quine, 1981, p. 84). He ends up his comments with a most remarkable assertion: "The empiricist's regard for experience thus impedes the very program of reducing the world to experience". (Quine, 1981, p. 85).

However, Quine like Carnap, takes ostensive definitions as the starting point of knowledge, but he makes no attempt to bridge the gaps in the Aufbau because, he says in another essay, "the project of a rational reconstruction of the world from sense data...is an attractive idea...(but) I am convinced, regretfully, that it cannot be done" (Quine, 1981, pp. 22/23).

Quine's way out is his well known naturalism. Science is taken for granted and what is left of the epistemological problems faced by Russell and Carnap is displaced to the problem of how we build the language of science. In the chapter "Empirical Content" (Quine, 1981, p.24), Quine says:

> "What sort of thing is a scientific theory? It is an idea, one might naturally say, or a complex of ideas. But the most practical way of coming to grips with ideas, and usually the only way, is by way of the

Instituto de Salud Colectiva
Universidad Nacional de Lanús

words that express them. What to look for in the way of theories, then, are the sentences that express them. There will be no need to decide what a theory is of when to regard two sets of sentences as formulations of the same theory; we can just talk of the theory formulations as such.

The relation to be analyzed, then, is the relation between our sensory stimulations and our scientific theory formulations: the relation between the physicist's sentences on the one hand, treating of gravitation and electrons and the like, and on the other hand the triggering of his sensory receptors.

So, his analysis begins "by looking at the sentences most directly connected with sensory stimulation" (p.25). This road is not free from difficulties either (see, for instance, the essay by P.F.Strawson in Hahn and Schilpp, 1986). But, even if successful, it is hard to reject the idea that the fundamental epistemological problems are dismissed rather than solved.

Piagetian epistemology has been able to overcome these difficulties and to offer a solution to what appeared to be an insurmountable dilemma presented by modern Physics. We have been forced to renounce intuitive concepts to describe the constitution of matter. The traditional sense of "description" is not applicable to events inside the atom. We only have a mathematical theory to account for them. Yet,

Instituto de Salud Colectiva
Universidad Nacional de Lanús

"structural descriptions" without empirical support, which is necessarily based on intuitive concepts, cannot single out any particular world as our world.

Genetic epistemology offers a way out of this dilemma based on the analysis of the common origins of causal relations and logic operations. Piagetian theory of causality is well known and I will not dwell on it. The structure is present at the very beginning, but it is not hanging in the vacuum. The path from action, at the sensorimotor level, as intermediary between "objects" and cognitive instruments, to physical theory is long and laborious. A reconstruction of it is far beyond the scope of this paper.

II.- THE VALIDITY OF PIAGETIAN EPISTEMOLOGY.

This brings me to the second theme of my paper. What is the epistemological theory of Piaget? In the analysis of the characteristics and the validity of genetic epistemology there are two different questions that require particular attention:

(a) The epistemological theory as an organized totality that may provide an explanation for the development of cognitive systems.

(b) The basic assumptions of the theory, making it a scientific theory, in that they confer on it an empirical character, whose areas of experimentation are

Instituto de Salud Colectiva
Universidad Nacional de Lanús

genetic psychology and the historical-critical analysis of the history of science.

Point (b) has been dealt with extensively in a volume on which I was fortunate enough to collaborate closely with Piaget (1983/89), Psychogenesis and the History of Science.

The first aspect - the theory as a global system or totality - will be the focus of my examination of the validity of genetic epistemology. My insistence on referring to the theory as an organized totality has two reasons. The first is concerned with the name "theory". This word covers too wide a spectrum in scientific writing today, ranging from systems with a high degree of formalization to sets of opinions and ideas that attempt to explain a given and highly restricted phenomenon.

Without entering into the question of its degree of validity, I believe that it can be said that Piaget's theory constitutes a coherent body, with a high degree of integration, that appears as a system explaining the fundamental processes that intervene in the development of knowledge, both at the individual level and in the history of science.

In this sense I call it an "organized totality" and I apply to it the thesis of Pierre Duhem, elaborated by Quine (with which I agree entirely) with regard to the validation of theories. Although he has referred to this thesis in numerous contexts, Quine formulated it in its sharpest -

Instituto de Salud Colectiva
Universidad Nacional de Lanús

almost provocative - way in his well known paper "Two dogmas of empiricism". Quine's formula is:

> "Our statements about the external world face the tribunal of experience not individually, but only as a corporate body". (Quine, 1953, p. 41).

The thesis is clear and totally anti-Popperian in two senses. In the first place, it rejects the idea that a theory is a conjunction of statements that can be separately corroborated or invalidated, such that when one of the statements is refuted, the whole is refuted. Secondly, a theory is an organized body and when it fails in one application it is not at all obvious what it is that has failed and why it failed in this particular application.

Let me develop this second point further. <u>All</u> empirical theories have failed in some particular seemingly critical application, even in extensive fields of application that appear to enter naturally into their domains. For example, Newton's theory of motion/gravitation cannot explain the "anomalous" movement of the smallest of the planets - Mercury - although it did permit predicting the existence of a planet unknown to man at the time. Neither does Newtonian physics apply at a sub-atomic level. But these are not "proofs" that "the theory is false", and that it should be discarded. What does falsification mean when these failed predictions are set side by side with our ability to send a space vehicle to Neptune based on these "laws"? The calculations as to its path and the time to be

Instituto de Salud Colectiva
Universidad Nacional de Lanús

taken by the journey are all made with the tools of the solid theory that is Newtonian mechanics - and the vessel reaches Neptune, within the time period calculated!

The question that should be asked with respect to a theory - assuming it is a theory formulated in an acceptable fashion and has been successfully applied in a given number of instances - is not therefore, whether it is true or false, but rather what is its domain of application? In which situations does it cease to be valid? What are the factors not taken into account by the theory that should be taken into account when it fails?

III.- EQUILIBRATION THEORY AND PRIGOGINE'S DISSIPATIVE STRUCTURES.

Following Quine's conception of theories, I will consider genetic epistemology as a "corporate body". It is a theory about the development and evolution of a system, namely the cognitive system. The detailed analysis of any part of the theory would be entirely out of the scope of this paper. Instead, I will focus on what could be considered the core of Piagetian conceptions: the theory of equilibration. Again, I will not attempt any detailed analysis. My aim will rather be an evaluation of the consistency of the theory as a whole within the context of present day ideas on the development and evolution of systems which, like the cognitive system, are natural systems (that is, not the result of laboratory experiences), complex (that is, having heterogeneous constituents in

Instituto de Salud Colectiva
Universidad Nacional de Lanús

continuous interaction with each other) and <u>open</u> (that is, in continuous interaction with the external environment).

Piaget's (1985) central ideas about the development and evolution of the cognitive system are presented in <u>The equilibration of the cognitive structures</u>, with the subtitle, <u>The central problem of development</u>. Throughout the remainder of this chapter I will refer to that problem as "equilibration" or less often "dynamic equilibrium".

I will begin by recapitulating very briefly the fundamentals of the equilibration theory and then examine how it fits into a general theory of complex systems. The objective is twofold. Firstly I will attempt to show how the main features pointed out by Piaget as being characteristic of the evolution of the cognitive system are in fact general characteristics of open systems. Secondly, I will provide some examples of the fruitfulness of making comparative analyses of the evolution of quite different open systems with the evolution of the cognitive system as depicted in Piaget's equilibration theory.

One of the fundamental tenets of Piagetian epistemology is the assertion that the development of the cognitive system is neither continuous growth nor a linear process. The existence of stages is an expression of these two facts. The cognitive system can be viewed as an open system whose dynamics are determined to a large extent by exchanges with the environment. The system evolves through periods of nearly steady state conditions (the stages) such that the

Instituto de Salud Colectiva
Universidad Nacional de Lanús

components of the system remain in dynamic equilibrium (equilibration).

New knowledge consists of assimilating objects or events to the previous schemes and structures of the subject. Assimilation implies an integration of new contents (objects, events) into an existing system. The progress of knowledge thus consists of new forms of organizing such contents. This requires new coordinations and modifications of pre-existing schemes (accommodation).

Conflicts, gaps, contradictions, i.e. the impossibility of accommodation of existing schemes and structures to new contents, may ultimately result in disequilibration or de-structuration of the system. Re-equilibration results from constructing new structures, new coordinations, new operations which accommodate without conflict the same contents that acted as perturbations leading to the disruption of the former structure.

The theory of equilibration thus summarized was not meant to be just a metaphorical description of the stages found by Piaget in his psychogenetic researches. Far from it, it was proposed as a coherent theory of cognitive development that has enough explanatory power to account for the mechanisms that govern both the growth of knowledge and change of structure.

From early on, Piaget had the clear intuition that many features of his theory had a wider domain of applicability than the field of knowledge. He cautiously expressed this in

Instituto de Salud Colectiva
Universidad Nacional de Lanús

his book on equilibration (1985) where he points out that "cognitive equilibria are quite different from mechanical equilibrium" and that "they differ still more from thermodynamic equilibrium...which is a state of rest after the destruction of structure." But he adds: "they are closer (however) to those dynamic steady-state conditions referred to by Glandsdorff & Prigogine (1971) with the exchanges that maintain a functional and structural order in an open system".

The subject of "similarities" between cognitive development, as described by Piaget's cognitive theory, and the self-organization of dissipative systems was taken up again in the discussions we held during the preparation of our joint publication, the book entitled Psychogenesis and the History of Science and in the last chapter we identified five "close analogies". Subsequent work convinced me that they were more than superficial analogies.

Let us then consider some systems with quite different natures which, I would maintain, give support to the above assertion of "more than superficial analogy". My presentation here is only indicative. A more detailed analysis is included in García (1990).

Instituto de Salud Colectiva

A laboratory experiment showing successive reorganizations of a simple system

Universidad Nacional de Lanús

Long before Prigogine formulated the theory of dissipative systems, there were a number of laboratory experiments with open systems, particularly in the field of fluid dynamics, that exhibited "strange behaviour" having characteristics of what we now know, after Prigogine, as "dissipative systems".

Perhaps the most impressive of such experiments were carried out by D. Fultz at the University of Chicago beginning in the period 1946-47. I had the fortune, as a student, of witnessing such an experiment. I must confess neither we - the students - nor the professor understood at that time what was going on in the laboratory. The experiments were later the object of careful studies by Fultz and other researchers in the fifties.

Essentially, it consisted of a rotating annulus filled with fluid (Figure 1). The internal border of the annulus is kept at a constant temperature, whereas the external border may be subject to a strictly controlled varying temperature so that a temperature gradient is established within the fluid. The experiment may proceed by either varying the magnitude of the temperature differential in the annulus or varying the rotation rate. Let us consider the first case.

Starting with a constant rotation and no temperature differential, the fluid acquires a laminar motion symmetrical about the rotation axis. Looking at the tank from above, the streamlines appear as closed circles (Figure). Then the temperature of the outer border is increased <u>in a linear and continuous manner</u> (constant rate).

Instituto de Salud Colectiva
Universidad Nacional de Lanús

The following evolution or development if you wish, can be observed:

-Firstly, the speed of the flow will increase, but always with closed circular streamlines.

- As the temperature gradient continues to be increased, the streamlines are disrupted. There is a transition period of disorderly motion until an unsymmetrical flow pattern develops and becomes stable.

- After a period of time (with temperature gradients increasing <u>linearly and continuously</u>) a new disruption of the streamlines takes place. The flow is disorganized until a new stable pattern develops.

- The same kind of events are repeated a number of times. The evolution may be described as a succession of "stages". All of them are characterized by a nearly steady, slowly drifting wave pattern. The number of waves increase from one stage to the next (from two to about five).

- Finally the flow pattern will consist of a number of irregular eddies drifting around.

The analogies with the development of the cognitive systems referred to above are very striking in this experiment. The temptation is great to apply the same language of the equilibration theory:

- The continuous injection of heat from the outside is the "perturbation" that the system has to "assimilate".

Instituto de Salud Colectiva
Universidad Nacional de Lanús

- At each "stage", the assimilation takes place by a simple kind of "accommodation" of the structure consisting of an increasing speed of the fluid motion <u>within the same flow pattern</u> (same structure).

- When the perturbation (injection of heat) increases beyond a certain threshold, no "accommodation" of the existing structure may assimilate it. The system is disequilibrated.

- Reequilibration takes place when a more complex structure is set in.

- The "perturbation" (heat flow) is acting in a continuous and linear manner, but the system reacts <u>discontinuously</u> by a succession of complete reorganizations.

- It should be pointed out that a "repetition" of the experiment would reproduce the general characteristics of the structures, but not exactly the same patterns.

Intensive research on physical, chemical and biological systems carried out by a large number of researchers in the last few decades has shown a high degree of generality of these steps in the evolution of open systems. As a general rule in physical systems, instabilities set in when the gradients generated by external forces exceed certain values. In Garcia (1990) I maintain that in the cognitive system social factors take the place of "external forces".

Instituto de Salud Colectiva
Universidad Nacional de Lanús

The evolution of natural systems of high complexity.

Since 1976, I have been doing research on natural systems of far greater complexity than the one just described. The accumulated experience has provided enough evidence, in my opinion, that the above mentioned analogies between the rotating fluid experiments and the cognitive system are much more than mere superficial similarities. The systems I have been dealing with share with the cognitive system the three characteristics already mentioned: they are natural, open, complex systems.

The studies have been carried out within the framework of a program called "Interdisciplinary Research on Complex Systems" and consisted of a number of "case studies" on agrarian regions of Mexico and Argentina, sponsored by the United Nations Research Institute for Social Development - UNRISD and the International Federation of Institutes for Advanced Study - IFIAS (García et al., 1988a; García et al., 1988b). Studies prior to these were undertaken in Africa (Sahelian region) within a program called "Drought and Man", sponsored by IFIAS (García, 1981).

In all cases the systems under analysis were composed of essentially three subsystems in interaction: a physical subsystem (soil, climate, hydrology), an agro-productive subsystem (crops and their associated technology) and a socio-economic subsystem (the agrarian community, the production relations, the acting economic forces). The evolution of such systems was analyzed with particular

Instituto de Salud Colectiva
Universidad Nacional de Lanús

reference to the periods of significant changes due to modifications of "boundary conditions" such as: climatic anomalies, introduction of new crops, changing technologies, economic crises, etc. These systems are clearly quite beyond the range of the dissipative systems analyzed by the Prigogine school.

The importance we assigned to this work was that in our study cases, contrary to the case of the fluid dynamic experiment, the center of analysis was the interaction between the physical environment and society, and in most cases, the human factor played the most important role.

At the start, there was little hope that with such extreme complexity the systems would exhibit in their evolution any of the characteristics of Piagetian equilibration theory. We even doubted that such systems might behave as "totalities" in the sense analyzed by Piaget.

It was a striking "discovery" to find what had been learned through equilibration theory and dissipative system theory served as guidance in our research. In fact, the interactions between the physical environment and society (in selected agrarian regions) did act as a system (a totality in the Piagetian sense). The whole system went through "stages", i.e., periods of stabilized production, living conditions, etc. Disequilibration took place under strong perturbations of the type indicated above. But then the <u>whole</u> system would be reorganized: new kinds of

Instituto de Salud Colectiva
Universidad Nacional de Lanús

production and/or new technologies; new socio-economic relations; new interactions with the physical subsystem.

<u>The value of comparative analyses</u>.

The accumulated experience with both natural systems and laboratory experiments would seem to provide enough evidence to assert that the "similarities" with the equilibration theory of cognitive systems are the manifestation of deep processes that determine to a large extent the functioning of all open systems. Therein the usefulness of comparative analyses - I will present one example of how Piaget's equilibration theory organizes questions germane to open systems.

In his book on equilibration, Piaget introduces the concept of "equilibration majorante" referring to the equilibration processes conceived of as "a structuration oriented towards improved equilibrium conditions" (p. 36). And in the "Conclusion" he refers to it as "the central concept....in the explanation of cognitive development (in the history of sciences as well as in psychogenesis)" (p. 170).

Piaget presents two main questions arising from this conceptualization:

- Since, according to the definition, reequilibration involves actions having a general teleonomic character, how is the election of the objectives to be explained? (page 170).

Instituto de Salud Colectiva
Universidad Nacional de Lanús

- How to explain the mechanisms of the "equilibration majorante" in its twin aspects of construction and increasing coherence (p. 171).

Teleonomy and "newness" of the structures arising in reorganizations are indeed two dominant problems in equilibration theory and they are also problems central to the analysis of open systems beyond cognitive systems.

I will not enter into the analysis of the answers given by Piaget to these questions. My only purpose here is to show that the same kind of questions present themselves in the quite different types of open systems we have analyzed and there equilibration theory appears to yield very precise answers.

Let us consider once again the rotating fluid experiment. The function performed by the fluid is to transport the heat from the outer to the inner border. In each one of the "stages" we described above (starting with helicoidal motions, changing into wave patterns with an increasing number of waves) the fluid motions adopt the most efficient flow structure that will transport the heat for the given temperature difference up to that moment.

At each stage the increase in temperature difference is "assimilated" by increasing the fluid speed without changing the flow pattern. As the temperature difference continues to increase, this "accommodation" mechanism does not suffice to transport enough heat, which as a consequence is then accumulated at the outer border establishing a strong

Instituto de Salud Colectiva
Universidad Nacional de Lanús

temperature gradient that de-stabilizes the motion. The flow becomes irregular until the "next" pattern is established which is able to transport the heat at a higher rate leading to a new stabilized stage. This is clearly a teleonomic process and Piaget's question "How to explain the election of the objective?" is equally applicable here. How does the fluid "know" which structure of motion it has to adopt to do a better job of transporting the heat? The answer is simple and eliminates any suspicion of teleology. When the flow becomes unstable, the irregular, random motion opens the way to all possible flow patterns. However, most of them are unstable and only some trajectories of the fluid particles will be stable for the temperature gradient existing at that moment. These are the kind of trajectories that remain and that establish the new structure of the flow.

After this explanation we can make sense of anthropocentric or teleological expressions such as: the fluid "learns" by trial and error; the fluid "adopts the best structure to do the job"; the fluid "creates new structures", and so on.

We may also go back now to Piaget's texts with some clues to uncover the meaning of some cryptic statements. In my own experience, the above explanation helped me to understand, for instance, Piaget's view of the form in which "the possible" fulfills its role in cognitive development (as expressed in his posthumous books on The Possible and The Necessary). In the "General Conclusions" of the volume

Instituto de Salud Colectiva
Universidad Nacional de Lanús

on "The Possible", Piaget (1981) asserts that the results reported there renewed the equilibration model "by explaining the mechanism of reequilibration by an internal dynamic, specific to the possible, and such that each possible novelty constitutes at the same time a construction and an opening". The stability theory of the fluid experiment provides a model, in my opinion, where this somewhat cryptic statement acquires a clear meaning.

IV.- CONCLUDING REMARKS

In the preceding sections of this paper I attempted to present Piagetian genetic epistemology as a coherent theory deeply immersed in some of the most fundamental problems that have been the concern of science in our century. We referred to two of such problems:

- The first was about the relation between empirical facts and mathematical theory in the interpretation of the physical world. Here, Piagetian constructivistic epistemology, with its conceptualization of a genetic structuralism, provided an answer to the questions that empiricism (whether of the Carnapian or the Russellian brands) left unsolved.
- The second problem was related to the increasing evidence that open systems, subject to continuous interaction with the "environment" through exchanges of matter, energy, information, etc., evolve through a series of successive stages of self-organization, quite

Instituto de Salud Colectiva
Universidad Nacional de Lanús

in line with the Piagetian theory of equilibration of cognitive structures.

I submit that no other epistemology has similar "credentials" as a scientific theory of the structure of knowledge and as a solid foundation for the theories with which science endeavours to explain the structure of the world.

Instituto de Salud Colectiva
Universidad Nacional de Lanús

References

Carnap, R. (1928). Der Logische Aufbau Der Welt . The quotations are taken from the edition The Logical Structure of the World, published by the University of California Press, 1969.

Carnap, R. (1966). Philosophical Foundations of Physics, Basic Books Inc.

Demopoulos, W. & Friedman, M. (1985). "Bertrand Russell's The Analysis of Matter: Its historical context and contemporary interest". Philosophy of Science, Vol. 52, number 4, p. 621.

Dugas, R. (1954). La Mécanique au XVII^{e} Siècle. Neuchatel, Switzerland: Editions du Griffon

Galileo (1632). Dialogo sopra i due massimi sistemi del mondo. English edition: Dialogue concerning the two chief world systems. University of California Press, 1953.

García, R. (1981). Nature Pleads Not Guilty. Oxford: Pergamon Press.

Instituto de Salud Colectiva
Universidad Nacional de Lanús

García R., (1990) "Structural evolution of open systems: the case of cognition", invited paper presented to the Congress on Evolution and Cognition (Bergamo, Italy, October, 1990). To be published.

García, R. et al. (1988a). Modernización en el Agro: ¿Ventajas Comparativas para Quién?. Mexico City: Centro de Investigación y de Estudios Avanzados del I.P.N..

García, R. et al. (1988b). Deterioro Ambiental y Pobreza en la Abundancia Productiva. Mexico City: Centro de Investigación y de Estudios Avanzados del I.P.N.

Glandsdorff & Prigogine, I., (1971). Thermodynamic Theory of Structure, Stability and Fluctuations. London: John Wiley.

Hahn & Schilpp, P.A. (1986). The Philosophy of W.V. Quine. The Library of Living Philosophers, Vol. XVIII. La Salle, Illinois: Open Court.

Heisenberg, W. (1972). La Partie et le Tout. Paris: Editions Albin Michel. (Original work published in German as Der Teil und Das Ganzen).

Instituto de Salud Colectiva
Universidad Nacional de Lanús

Leibniz, (1720). The quotation is taken from Dugas (1954) who reproduces a text included in Recueil de pièces sur Philosophie, la religion naturelle, l'histoire, les mathématiques, etc., par Messrs Leibniz, Clarke, Newton et autres célèbres. Amsterdam: De Sauzet éd.

Newman, M.H.A. (1928). "Mr. Russell;s Causal Theory of Perception". Mind, Vol. 37.

Piaget, J. (1985). The Equilibration of the Cognitive Structures. Original edition: L'Equilibration des Structures Cognitives. Paris: P.U.F., 1975.

Piaget, J. (1981). L'évolution des possibles chez l'enfant. Vol. I of Le possible et le nécessaire. Paris: P.U.F.

Piaget, J. (1983). L'évolution du nécessaire chez l'enfant. Vol II of Le possible et le nécessaire. Paris: P.U.F.

Piaget, J., & García, R. (1989) Psychogenesis and the History of Science. University of Columbia Press, translated from Psychogenèse et Histoire des Sciences. Flammarion: Paris, 1983.

Instituto de Salud Colectiva
Universidad Nacional de Lanús

Quine, W.V. (1953). "Two dogmas of empiricism", reprinted in From a Logical Point of View. New York: Harper & Row Publishers.

Quine, W.V. (1981) Theories and Things. Cambridge, Mass.: Harvard University Press.

Russell, B. (1914). Our Knowledge of the External World. London: George Allen & Unwin.

Russell, B. (1927). The Analysis of Matter. London: George Allen & Unwin. Dover edition, 1954.

Russell, B. (1944). "Reply to criticisms", in P.A.Schilpp, (Ed.) (1944).

Russell, B. (1948). Human Knowledge, its Scope and Limits. New York: Simon & Schuster.

Schilpp, P.A., (Ed.) (1944). The Philosophy of Bertrand Russell. Volume V of The Library of Living Philosophy. Chicago: Northwestern University.

Strawson, P.F. (1986). "Reference and Roots", in Hahn & Schilpp, P.A., (1986).

Instituto de Salud Colectiva
Universidad Nacional de Lanús

[1] The author is very grateful to Prof. Peter Pufall and Prof. Harry Beilin for many valuable comments that helped to improve the original version of this paper.

[2] In his polemic with Clarke, Leibniz says: J'ai marqué plus d'une fois que je tenais l'Espace pour quelque chose de _purement relatif_, comme le Temps, pour un _ordre des Coexistences_, comme le Temps est un _ordre des Successions_ (Third Letter).

[3] Bohr refers here to the fact that, according to Newtonian physics, a particle with an electric charge having a cyclic motion would emit radiation and therefore would lose energy, so that all elctrons should finally collapse onto the nucleus.

[4] Just before I wrote the last version of this paper, a colleague brought to my attention an important article by W. Demopoulos and M. Friedman (1985) where this subject is analyzed in considerable detail. My criticism of these authors is that they don't pay attention to the profound epistemological differences between Russell and Carnap concerning the structural conception of physical theories. As indicated above, Carnap follows Galileo's tradition as to the role of mathematics in empirical science. This is not Russell's position.

[5] As defined by Russell (1927, p. 294), two events have a relation called "compresence" if they overlap in space-time. This concept is used by Russell to define a "point", or rather a "point-instant" as a group of events such that any two members of the group are compresent, and no event outside the group is compresent with every member of the group.

Instituto de Salud Colectiva
Universidad Nacional de Lanús