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THEORETICAL PRACTICE

"Without revolutionary theory there can be no revolutionary movement. This idea cannot be insisted upon too strongly at a time when the fashionable preaching of opportunism goes hand in hand with an infatuation for the narrowest forms of practical activity."

Lenin

THEORETICAL PRACTICE editorial

> 'The external application of a concept is never equivalent to a theoretical practice ... However, and this is a thesis essential to Marxism, it is not enough to reject the dogmatism of the application of the forms of the dialectic in favour of the spontaneity of existing theorectical practices, for we know that there is no pure theoretical practice, no perfectly transparent science which throughout its history as a science will always be preserved .... from the threats and taints of idealism, that is, of the ideologies which besiege it; we know that a 'pure' science only exists on condition that it continually frees itself from the ideology which occupies it, haunts it or lies in wait for it. The inevitable price of this purification and liberation is a continuous struggle against ideology itself, that is, against idealism .. '
>
> L. Althusser, For Marx, Allen Lane London 1969, p 170.

Theoretical Practice is a Marxist-Leninist theoretical journal. Many of our readers find this position paradoxical, for they cannot recognise in Theoretical Practice the conventional shape and substance of Marxist theory. Where in it does one find concrete analysis of our present situation and its history, the application of Marxist theory to the crucial struggles of our age? The answer is that one does not find in it the empiricist conception of theoretical practice from which such positions derive. Theoretical Practice's work is philosophical. Again, for many of our readers this statement will be paradoxical, for they cannot recognise in it the conventional shape and substance of Marxist philosophy. The 'conventional' form of Marxist philosophy is constituted by two opposed positions, 'orthodoxy' and 'revisionism'. The 'orthodox' position, represented by Stalin's Dialectical and Historical Materialism (but also to be found in the philosophical positions of Stalin's political opponents Bukharin and Trotsky), starts from the system of Dialectical Materialist Naturphilosophie, an ontological theory of the basic constituents of the universe. Orthodoxy conceives the relations of Marxist philosophy and Marxist scientific theory as a relation of derivation or application in which the science is an instance subordinate to the philosophy. Dialectical Materialism establishes a priori the universal laws of the nature and development of things, and the laws have only to be applied to particular regions of reality, eg. history, to produce scientific knowledge of those regions. Sciences are therefore effects of philosophy; they are founded outside their own scientific practice. The revisionist position, represented by the importation of neo-Kantian, humanist, idealist and existentialist philosphies into Marxism, starts from the position

that Marxism is a science of 'empirical discoveries', and that 'mechanical' materialism, the materialist Naturphilosophie of orthodoxy, is an inadequate epistemological foundation for Marxism. Revisionism conceives classical Marxism as a blind and fortuitous empiricity which needs methodological shoring-up, a philosophical guarantee and proof that it is indeed a valid knowledge. Necessarily, this ' act of foundation' is accompanied by ' the act of criticism' that is, the rejection of those aspects of Marxism which do not meet the conditions of validity demanded by the philosophical system which is imported to 'save' Marxism. This act of 'foundation-criticism' is constantly accompanied by an emphasis on method: '... Marxism ... does not imply the uncritical acceptance of the results of Marx's investigations. It is not the 'belief' in this or that thesis, nor the exegesis of a 'sacred' book .... (but) on the contrary refers exclusively to method.' (G. Lukács, History and Class Consciousness, Merlin, London 1971, p 1 Lukács' emphasis.) Marxism is thus subordinated to a philosophical position which is produced outside of it, which is produced by the philosophical ideologies of the bourgeoisie. These opposed philosophical positions form a unity, a unity constituted by their common empiricist conception of science (science works on given real objects) and their common speculative conception of philosophy (philosophy is a guarantee of the empiricities of science it establishes their right to the title of knowledge). This unity, the unity of an ideological problematic, is in no way connected with Marxist scientific theory; it is a bourgeois importation into and deformation of Marxism. This ideological problematic installs within Marxism a field of empiricist misrecognition of the forms of Marxist scientificity. It is an idealist appropriation of Marxism which subordinates its scientific discourse to the discourse of bourgeois philosophy. This 'Marxist' philosophy must be opposed by a materialist philosophical practice - materialist in the sense that is defends the existence of the only true materialism, the production by the sciences of the knowledge effect, the appropriation of the concrete in thought. Theoretical Practice's philosophical practice consists, in part, of its defence of Marxist scientific theory against the ideologies which besiege it. This philosophical practice consists in the exposure of the source of these philosophical ideologies, the extra-theoretical instances of ideological and political positions opposed to the proletarian position in politics and theory. Theoretical Practice's philosophical practice is a political and partisan struggle.

This special issue of Theoretical Practice is not an excursion into the philosophy of science; it is not a diversion from the main direction of our work. It is the position of Theoretical Practice that its work represents a simultaneous double intervention in the theoretical and the political. It is a political intervention in theory from the position of a politics, Marxism-Leninism. It is a theoretical intervention in politics from the position of a science, Historical Materialism. The task Theoretical Practice has set itself is to assist in the recommencement of the scientific practice of Historical Materialism and the development of Marxist-Leninist political practice.

Marxist Theory and Politics

It is the position of Theoretical Practice, which we have maintained in previous editorials, that these two instances of Marxist practice, the theoretical and the political, represent each for the other an essential condition of its existence as an instance. We have maintained that the unity of 'theory and practice', of theoretical and political practice, is not a pre-given unity. It is not a unity of the fusion of these instances in the 'praxis' of a subject, be it a class, an organisation or an individual. Marxist theory and the political practice of the workers' movement must be combined in specific forms of organisation: in our era, the Marxist-Leninist party. But this combination itself is not an automatic and guaranteed unity; it is fraught with the threat of those distortions of theoretical and political practice we call deviations. The relation of these two instances, the theoretical and the political, is maintained in the face of the threats that deviations pose, by the practice of a third instance, that of philosophy: Marxist philosophy. Marxist philosophy is a political practice of intervention in the realm of theory and a theoretical practice of intervention in the realm of politics. Marxist philosophy does not exist outside of this space of intervention in these two domains, it does not exist outside of the theoretical-ideological conjunctures which impose threats to theoretical and political practice.

Inevitably, these instances of Marxist practice are related, are a unity with a decisive political effectivity only in Marxist-Leninist parties. But in this country such a party does not exist, nor are its conditions of existence immediately given. Theoretical Practice has set itself the task of assisting in the development of the theoretical and political conditions for such a party. Until such a party exists the unity of the theoretical and the political can only be a unity of positions, and not of positions and mass practice led by the Marxist-Leninist vanguard. In this situation philosophical practice is doubly important. Without the effective presence of party organisation and mass struggle, of the discipline and tasks that this presence enforces, we face a greatly magnified threat of theoretical and political deviations.

Deviations are positions and practices which destroy the necessary conditions of Marxist-Leninist theoretical and political practice. Deviations always distort the relations between theory and politics and ideologically misrepresent such relations in theory. Deviations always represent a certain combination of theoretical and political errors, and they have their source in class positions which are opposed to the proletarian position, in bourgeois and petit-bourgeois ideology.

Theoretical practice and its products are never reducible to effects of a class outlook, nor are they the product, more or less mediated, of class 'experience'. Theoretical practice is the effect of its own conditions as a distinct instance and it cannot be guaranteed by the 'sociological' characteristics of its practitioners. Nevertheless, the scientific practice of Marxist theory does have its political conditions of existance. Anti-proletarian political positions necessarily have specific theoretical effects on Marxist scientific practice. The knowledge of Historical Materialism could only be produced on the condition that its supports, in particular Marx broke with the ideological positions of the petit-bourgeoisie and adopted the political position of the proletariat. Bourgeois ideological positions are epistemological obstacles for Marxist scientific

practice. Marxism is not a neutral bundle of wares which can be peddled to whoever finds it 'useful', a science fit for artisans and bankers alike as the Austro-Marxists maintained.

The Marxist science of Historical Materialism is thus necessarily connected with political positions which represent a certain class. Just as a correct political position is an essential condition of scientific practice, so a scientific theoretical position is essential for a correct political practice. Opportunism, spontaneism, economism, ultra-leftism and the whole catalogue of deviations from Marxist-Leninist political position are necessarily connected with erroneous theoretical positions. These positions always distort the nature of the relation between scientific theory and political practice. Marxist theory is essential for a correct proletarian politics. It is not essential because it produces the answers to the problems of acting in the concrete political situation in a ready-made form. Historical Materialism provides, through its concepts and its knowledges of the structural conditions of the current situation, the means to think out the characteristics of that situation, the political tasks it enforces, and the political line and actions it requires. This connection between the general theory of modes of production, the theory of the capitalist mode of production, and the theory of a particular social formation (which is always a combination of more than one mode of production with a complex superstructure), and the current situation of the here and now, is possible only because of the particular character of Marxist scientific theory and the Marxist mode of explanation. The Marxist concept of the social totality as a complex totality, and the Marxist concept of structural causality, do not establish a dislocation between an abstract theory of 'social structure' and the concrete existence of particular 'events'. The concept of the structure as an effectivity in its elements, of its presence/absence in the specific conjuncture, provides the theoretical conditions for thinking the mode of presence of the structure in its elements.

Historical Materialism does not have this political current situation, the precise conjuncture of class forces for its object. The object of Historical Materialism is an object in knowledge. To act in the realm of knowledge is not to act in the realm of the political. No scientific analysis ever took up arms in an insurrection. No proletarian militant ever acted in, eg, the Capitalist Mode of Production, but always in a particular social formation at a particular time. It is Marxist political practice which has the current situation of the political conjuncture for its object.

The notion that the current situation will, by the inevitable logic of a history, reduce itself to the 'ideal' order of relations in theory, will act out theory, makes theory into an abstraction of the real and a premonition of an abstract demiurge which lies behind the real and which will ultimately reveal itself as its presence to itself in the real when the sojourn of its secret history in the shadows is at an end. It is a distortion which replaces Historical Materialism with a theory of historical evolutionism, by a theory of a history with a subject which comes to be in that history and ends that history with its being in the real. This theoretical position conflates the scientific analysis of the capitalist social formation with the political practice of overthrowing it. Its historicism reduces politics to a

moment of an historical evolution: the tasks of politics are accomplished by the logic of an evolution which necessarily brings their fulfilment to pass.

The pragmatist position, the ideological representation that forms a couple with evolutionism, insists that the theoretical and the political are separated by an unbridgeable gulf, that theory is an abstraction of reality which can never grasp it in its immediacy and complexity. Politics becomes a matter of horse-sense and insight, and accommodation to the exigencies of the here and now is the only connection between an abstract strategy, founded on the abstractions of theory, and the concrete political acts of tactics, founded on the contingency of real events.

Marxist-Leninist political practice is only effective on the condition that it is guided by theory. But the theoretical is effective in politics only at the level of the political. It is effective only insofar as the conditions of practice in the current situation are thought. Theory intervenes in political practice as a means, as an instrument, in political practice's transformation of its own object.

Lenin's political practice was founded upon the concept of this distinction between the objects of Historical Materialism and of Marxist political practice. Lenin produced the theory of the conditions, the forms and the object of Marxist political practice. In doing so, he developed an essential political instrument of that practice, the knowledge of the conditions of its own effectivity.

The Leninist theory of a practice is a scientific theory of a particular kind; it is not a theory of an instance in the social formation, but rather a theory of a particular practice, the political practice of the Marxist movement which takes place at that level. Leninism does not represent a theory of the political level in the social formation; that level is the object of a regional theory of Historical Materialism.

Lenin was a 'pragmatic', a 'successful' politician, not only because he thought out the 'current situations' that the Bolshevik party faced in a masterly fashion, a fashion that has been equalled only by Mao Tse-Tung, but also because, like Mao, he developed the concepts to think the practice that must take place if that situation is to be seized and acted upon politically. The Leninist theory of political practice, a theory active at the level of that practice, is an essential condition of that practice. It is this theory that is most misrepresented in the present conjuncture. It is deformed to the point of its absence. Hence the need for a philosophical recovery of Leninism as well as of Historical Materialism. However, the philosophical recovery of Leninism is possible only on the basis of an adequate knowledge of the science from whose logic and concepts it is derived. The philosophical defence and demarcation of Historical Materialism is our first task.

Science and politics are distinct instances of Marxist practice; they have different objects and therefore their relations as practices are external relations. Their relations are not internal relations whereby the one can generate the other as a subordinate or derived effect of itself. Marxist-Leninist scientific practice is not simply and automatically present in political practice, nor is a correct political position simply and automatically present in theory. Philosophical practice is a third instance, separate from

the instances of the theoretical and the political, which represents the one instance alongside the other in a practice of intervention in conjunctures where the relation of these instances and the effectivity of each is threatened by ideological positions which induce political misrecognitions in theory and theoretical misrecognitions in politics.

Theoretical Practice's theoretical work is philosophical in this sense. It is an intervention in a particular conjuncture. We have attempted to specify the characteristics of this conjuncture: the dominance of revisionist political and theoretical positions in the British revolutionary movement and, on a wider, scale, the absence of a correct conception of Historical Materialism and of a scientific practice of Historical Materialism. We have maintaimed that the philosophical recovery of the scientific concepts of Historical Materialism is the dominant task to be undertaken in the struggle against revisionism and an essential pre-condition for the creation of a Marxist-Leninist party. Philosophical struggle must be directed toward this primary objective, the struggle for the political and theoretical positions necessary for the scientific practice of Historical Materialism. We have stressed the importance of the work of Althusser and his collaborators in this task and the necessity to start from the concepts which they have made available for this task. The position we take here on our philosophical practice is derived from the analysis developed in Althusser's text Lenin and Philosophy.

The basis of Marxist philosphical practice is to draw lines in theory, to demarcate between the positions of Marxist science and the ideological positions which

are opposed to them. Philosophical practice consists in this struggle in theory; it has no object but to effect the political-theoretical separation of these Marxist domains from ideology. Philosophical practice is not a science, and it does not have an object in the sense in which the sciences have an object. It does not produce scientific knowledge, but merely represents knowledges and their conditions which already exist prior to it. Marxist philosophy does not pre-exist or give rise to Historical Materialism, but rather it is an effect of the existence of Historical Materialism. Marxist philosophy does not pre-exist the Marxist-Leninist position in politics, but rather it is an effect of the existence of that politics. It is induced into existence by the conjuncture which Marxist scientific practice faces, a conjuncture which threatens Historical Materialism with the loss, the ideological misrepresentation, of its object. It is induced into existence by the effects of the political struggles to which that crisis in Historical Materialism is directly connected, the struggle between Leninism and Revisionism. This threat to Historical Materialism necessarily implies serious dangers for Marxist-Leninist political practice, the destruction of its theoretical conditions of existence. The matter that is at stake in philosophical struggle is no mere abstract or speculative question, but a political question of the first importance.

Bachelard, Freud and the Theoretical History of the Sciences

In the Preface to Vol. II of Capital, Engels unhesitatingly makes use of the history of Chemistry to develop an important epistemological point about the unrecognised production of the phenomenon of surplus value in the discourse of political economy: the latter's production of the answer to a question it did not pose. Engels is demonstrating the relation between Marxism and political economy by means of an illustration drawn from the relation between Phlogistic and Lavoisierian chemistry. But this is no mere chance selection of a happy example: Engels is presenting an important epistemological thesis in the form of this 'example'. He is arguing that the history of the sciences is a history that can serve to reflect Marxist scientific practice, that Marxism is a science, and therefore its scientificity can be thought, in part, by those conceptual generalities which think the process of production of all scientific knowledges.

In the present conjuncture the recourse to those generalities and to the history of other sciences thought through those generalities is no idle recourse to erudition or to a convenient source of 'illustrations'. It is a recourse which we must adopt to think the scientificity of Marxism and to disinter its origins. It is a necessity in that we are faced with a task that is nothing less than the recommencement of scientific Marxism. This task does not mean simply learning to take up from the point where Marx, Engels or Lenin 'left off'. The practice of Marxism, like all scientific practice, is not auto-reflective, nor are the concepts which think the object of Marxist science all present in an adequately reflected form as concepts. No science is immediately readable. No science's problematic establishes a structure of immediate recognition. It does not reproduce ideological givens and it does not produce itself as a given. The concepts and the object of

Marxist scientificity are not merely buried beneath ideological misrepresentation and misrecognition. A philosophical recovery of Marxist science is not the paring away of a husk of ideology to reveal a given science of truth immediately present to itself. Any such conception of Marxist philosophy installs the empiricism of an innocent reading of the scientific discourse of Marxism.

Marxist philosophy is indeed an effect of the existence of the science of Historical Materialism, but it is an effect of the necessity of a reflection of that science's concepts and object in order that the form of its scientificity and its conditions be known. Therefore the situation which induces Marxist philosophy as an effect does not guarantee the efficacy of Marxist philosophy. If the concepts and object of Historical Materialism were given in a form accessible to the immediacy of an innocent reading there would be no need for philosophy. Marxist philosophy does not find the concepts it must reflect already reflected. It must employ non-Marxist instruments in order to fulfil its task in respect of Marxism.

In respect of its political conditions Marxist philosophy is not in the same situation. The proletarian position in politics is not merely an effect of theory but also of the class struggle. The positions of Marxist-Leninist politics are, in part, present in a much more direct form in the history of the workers' movement and in the contemporary political struggle between Leninism and Revisionism.

Thus the necessity of a theoretical but not of a political recourse outside of Marxism. This theoretical recourse is not the recourse of revisionism, that is, the recourse to the practice of giving philosophical

'foundations' to science; of basing it upon the non-scientific. The difference between Marxist philosophical practice and revisionist philosophy lies in their different political points of departure in respect of Marxism and the corresponding difference of the non-Marxist positions to which they have recourse. Revisionism unhesitatingly gravitates to the ideological philosophies which are elaborations of bourgeois ideology.

But can there be any theoretical positions which are not either bourgeois or proletarian? How are we to tell whether this recourse outside of Marxism is revisionist or not? There can be theoretical positions which are not the expressions of class ideologies. Any claim to the contrary rests upon the conception of science as the expression of a class subject. It is a disastrous sociologistic ideology which destroys conditions of any scientific work, which abolishes the realm of knowledge in favour of a mystical class experience. Those theoretical positions which are not derived from the class ideology, which are not governed by the structure of ideology in general, are the positions of the sciences. Those philosophical positions which are not simply governed by the structure of ideology in general are the philosophical positions of the materialisms which defend the scientificity of the sciences. We stress the plural: materialisms - in contradistinction to the ideological Naturphilosophie of Materialism which claims a knowledge of the nature of matter independent of the sciences.

In giving prominence to the work of Bachelard we do so for three reasons:

i. that he insists that knowledge is an effect of the problematics of the sciences and that the materialities of the sciences are the products of their theory materialised in scientific instruments;

ii. that he adopts a consistent materialist position of defending the sciences against realist and idealist notions;

iii. that he bases his position in philosophy upon the clear conception of the closure of ideological philosophies, that no philosophical system can ever guarantee scientific knowledge or legislate its conditions.

In giving prominence to the work of Freud and in using Freudian conceptual forms we are using the concepts of a science, psycho-analysis, to reflect the concepts of Historical Materialism. This recourse is legitimate because the object those Freudian concepts think is similar to the object that the concepts of Historical Materialism think, in that it is a complex totality. There is a strict and limited epistemological homology between Marxism and psycho-analysis which does not imply that the social formation is reducible to the psychical formation but that both are thought by sciences in a similar manner.

The recourse outside of Marxism demonstrates, and is based upon, the possibility of thinking the mechanism of the production of scientific knowledges and the histories of the operation of that mechanism as a scientific generality. This recourse demonstrates that the space of this generality and the concepts which think it can be nothing other than a scientific theory of the history of the sciences. By means of this recourse it is possible to represent the conditions which Marxism shares with the other sciences and to distinguish Marxism from ideological representations of its conditions as a science.

Bachelard, Freud and Lacan are not Marxists; but neither are they mere ideologists. Their work is of the first importance for Marxism in the present conjuncture. Equally, Marxist theory is of crucial importance in developing the consistent materialist position in their work and in the formulation of their own concepts. Other sciences are as much in need of a firm materialist philosophical defence as is Marxism. The 'natural' sciences have not escaped the effects of idealist philosophical interventions and appropriations. Thus in this issue we publish an important section of Lecourt's first book on Bachelard, a book which puts into practice a materialist reading of Bachelard, and we also publish an article by Ben Brewster on the relations between Bachelard and Althusser. Barry Hindess' article on the concept of model in mathematics is based upon Alain Badiou's attempt to apply concepts developed in the defence of Marxist theory to the problem of idealist incursions into the science of mathematics.

The study of the history of the sciences has a specific theoretical role in respect of the science; it cannot be relegated to the status of an exercise in which ideological positions write 'histories' in which they recognise themselves, histories constructed by the reproduction of their ideological point of departure in the form of a description. The theoretical history of the sciences is invaluable to Marxism and the other sciences as a source of epistemological reflection. This history has not yet been written. Fichant's piece in this issue is an important step toward the theoretical conditions for such a history; a theory of what it is that it is a history of. The article by Tony Cutler represents a good introduction to Fichant's work and considers a crucial concept of such a history, the concept of epistemological break.

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Further sections of Jacques Rancière's The Concept of 'Critique' and the 'Critique of Political Economy' will appear in later issues of Theoretical Practice.

Correction to Theoretical Practice No. 2

At the beginning of page 29 the following was omitted:

"14. 'Matter is a philosophical category.'

'The sole property of matter with whose recognition philosophical materialism is bound up is the property of being an objective reality.' Lenin, Materialism and Empirio-criticism, p 130, and pp 260-61.

15. The 'epistemological contract', the insistence upon a "

At line 5 of page 20 the number to note 16 was omitted:

"community (16)"

Definitions[1]

by MICHEL PECHEUX and ETIENNE BALIBAR

I

In the historical process of the formation of scientific physics, we shall call the point of 'no return' (in François Regnault's words) from which the science begins the epistemological break.

This point in history can be situated in Galileo's works on falling bodies. After these works, indeed, any recovery (or any reworking) of Aristotelian and scholastic cosmological and physical notions becomes factually impossible. On the other hand the elaboration of the concepts of physics (instantaneous velocity, acceleration) and of mathematics (infinitesimal calculus) required even for the exposition of these statements of Galilean 'dynamics' becomes factually necessary. (2)

The term 'point of no return' constitutes the adoption of a position in the polemic between a 'continuist' position in epistemology and in the history of the sciences (Brunschvicg and the permanent spectacle of the human mind manifest in science; Duhem and the question of precursors.....) and a 'discontinuist' position which can be appropriately designated by the names of Bachelard and Koyré.

The discontinuist position rejects the notion that 'knowledge' ('savoir') is a continous development, from 'common sense' to 'scientific knowledge', from the dawn of science to modern science. (3)

The term beginning marks the distinction from what is sometimes called the origins of a science: to speak of a beginning means that the break which constitutes a science necessarily takes place in a definite conjuncture, in which the origins (the philosophies and theoretical ideologies which define the space of the problems) undergo a displacement towards a new space of problems.

II

The improvements, corrections, critiques, refutations, negations of certain ideologies or philosophies logically preceding the epistemological break in physics will be called revampings (4) (or intra-ideological ruptures).

The series of terms: 'improvements .... negations' designates the existence of a process of accumulation which necessarily precedes the moment of the break and determines the conjuncture in which the latter will be made (cf. the 'impetus physics' developed by the 'Parisian' school of the 14th century, Benedetti's physics). This means that the break takes place at the point in the space of theoretical problems which is overdetermined by the accumulation of successively proposed ideological configurations (in this case: the definition of movement).

In other words the process of accumulation must be understood not as a phase of pre-scientific aberration

pure and simple about which there could be nothing to say, but as the period of formation of the conjuncture in which the break takes place. In the course of this formation elements linked to the economic base (relations of production and process of production), to the juridico-political superstructure of the society and to practical ideologies (5) intervene according to historically governed modalities, and it is the condensation of these elements which determines the historical conditions of the break.

At the same time, this means that the concept of the break has nothing to do with the voluntarist project of effecting a 'leap' out of ideology into science, with the inevitable religious connotation attached to this project, and the impossible 'heroes of science' which it implies. To take the example which serves as a guiding thread here, Galileo's name is an ill-chosen unit, for a science is not the product of one man alone. Galileo is the effect and not the cause of the epistemological break which is designated by the term 'Galileism'.

III

It is convenient to make certain distinctions between the epistemological effects produced by the break.

Firstly, the break has the effect of rendering impossible certain philosophical or ideological discourses which precede it, in other words, of leading the new science into an explicit rupture with them: the epistemological rupture thus appears as an effect (of a 'philosophical' nature) of the break (which reminds us correlatively that a rupture with ideology is not enough to produce an epistemological break).

Secondly, the break has the effect of making validations, invalidations or segregations within the philosophies implicated in the conjuncture in which it takes place. In a word, lines of demarcation (6) are traced on the basis of it, in the conflictual terrain of philosophy.

Finally, the break has the effect of determining a relative autonomy of the new science which corresponds to it: after the break, the new science depends on its own continuation, and is in some sense in question with respect to that continuation. As we know, this continuation on which hangs the fate of a nascent discipline, depends on the possibility of instituting an experimental procedure adequate to it(7). It also depends on intra-scientific ruptures, or to use François Regnault's expression, recastings of the theoretical problematic which intervene in the history of a science (Einstein).

To conclude, let us stress that the error which consists in confusing mere intra-ideological ruptures (or revampings), the epistemological break (including its ruptural effect) and intra-scientific ruptures (or recastings), imagining that every recasting is a new break and that the break is only a primary recasting, amounts to the annihilation of the effectivity of the very concepts of break and rupture and in practice surrenders the terrain to the 'continuist' position outlined above. (8)

Notes.

(1) Printed in M. Fichant and M. Pêcheux Sur L'Histoire Des Sciences (François Maspero Paris 1969) pp. 8-12.

(2) The majority of historians speak of 'Galilean dynamics'. This expression can cause difficulties if it is taken literally. It is important to remember that it is only with Newton that dynamics is truly founded. Canguilhem writes 'Newton's science, the model of all science of the epoch, perfected the science of Galileo', Galilee, la Signification de l'Oeuvre et la Leçon de l'Homme in G. Canguilhem Etudes D'Histoire et De Philosophie Des Sciences Second

Edition Vrin Paris 1970).
(3) These few phrases from Koyré, extracts from Etudes Galiléennes (Hermann Paris 1966 p. 50 cf. Metaphysics and Measurement, pp. 30 - 31) clearly designate in this respect the discontinuist position, through the example of pre-Galilean impetus.

'.... The notion of impetus is .... a very confused notion. Basically, it merely translates into 'scientific' terms a conception founded on everyday experience, on a given of common sense.

'Indeed, what is impetus, forza, virtus motiva, if not, so to speak, a condensation of muscular effort and vigour? Thus it accords very well with the 'facts' - real or otherwise - which form the experimental basis of mediaeval dynamics; and in particular with the 'fact' of the initial acceleration of the projectile; it even explains this fact: is not time needed for the impetus to take hold of the mobile? Everybody knows that in order to jump an obstacle one has to 'make a take-off'; that a chariot which one pushes, or pulls, starts slowly and little by little increases its speed: it, too takes off and gathers momentum; just as everybody - even a child throwing a ball - knows that in order to hit the goal hard he has to place himself at a certain distance from it, and not too near, in order to allow the ball to gather momentum.'

On this point see the whole of M. Fichant's contribution to Sur L'Histoire Des Sciences, L'Idee D'Une Histoire Des Sciences (op. cit pp. 49-143 and below).
(4) The term 'démarquage' is here translated 'revamping' in order to signify that the intra-ideological ruptures referred to reproduce the invariant combination of ideology and in no way represent epistemological transformations. (trans).
(5) The term practical ideologies is defined thus by L. Althusser.

'Practical ideologies (for example morality, religion ....) are complex formations of montages of notions-representations-images on the one hand and of montages of behaviour-conduct-attitudes-gestures on the other hand. The ensemble functions as a set of practical norms which govern the attitudes and the concrete positions which men take up in respect to the real objects and the real problems of their social and individual existence, and of their history.'
(6) cf. also on this point L. Althusser Lênine et La Philosophie (François Maspero Paris 1969) translation to appear in L. Althusser Lenin and Philosophy and Other Essays (NLB 1971).
(7) cf. A. Koyré's article An Experiment in Measurement in his Metaphysics and Measurement (London Chapman and Hall 1965).
(8) The concept of break is applied here above all to the history of physics. The adequate utilisation of this concept for the analysis of the scientific constitution of any other discipline depends on each occasion on an epistemological labour on the history of the discipline considered, transferred to the differential field of the history of the sciences.

Bachelard's New Problematics

by DOMINIQUE LECOURT

Introduction

Dominique Lecourt's book L'Epistémologie Historique de Gaston Bachelard (2nd Edition Vrin Paris 1970), sets out the basic Bachelardian concepts of what he calls 'historical epistemology', yet this title is not without ambiguity, and is not accessible to any literal reading of 'history'. The historicity of Bachelard's epistemology refers to his concept of dialectic. The function of this concept is to delineate a philosophy adequate to the sciences from an ideological philosophy. The 'dialectical' nature of the former derives from its grasping the essential 'openness' of scientific discourse in contradistinction to the closure of 'knowledge' represented by ideological philosophy. The 'historicity' of Bachelardian epistemology enables it to comprehend the invariance of the discourse of ideological philosophy. In the latter Bachelard argues the invariant of 'knowledge' is the comprehending subject confronting the entity which has the character of a given empirical entity. In place of this comprehension/entity (subject-object) relation, Bachelard 'substitutes' the relation comprehension-extension. These categories demonstrate that in science the thought object is constructed, and therefore comprehension is a function of the concept which thinks the thought object. The dissolution of the entity as the object of science dissipates the myth of immediate comprehension.

However, the operation of this dialectic occurs after the break; the historicity of Bachelard's epistemology refers to the openness of constituted science. Thus, there is no theory of the epistemological break in Bachelard. The production of science is not grasped as the inauguration of a new practice in a break with ideology. In this sense constituted science is a given for Bachelard.

Thus, while we may fully endorse Lecourt's characterisation of the first of the new problematics, 'historical epistemology as an articulated system of concepts', his characterisation of the second problematic 'the history of the sciences as an object of theoretical thought' must be qualified by the observation that while Bachelard (through his concept of recurrence) has an immensely progressive role in the history of the sciences he does not successfully found the object of that discipline (c/f M. Fichant's paper).

The 'philosophical spectrum' to which Lecourt refers is reproduced in the translator's note to the text. Bachelard seeks in the use of this 'spectrum' to demonstrate the hierarchisation of philosophical doctrines. The 'inner' doctrines are submitted to the 'outer' in the unity of both sides of the spectrum. The blanks, situated on a different plane, indicate the philosophy adequate to think the sciences referred to above, which is thus conceived as partaking of none of the ideological invariants

of the philosophy of realism-idealism.

Text

Now, therefore, has come the time of the 'anabaptist philosophers', which Bachelard prayed for in his Philosophy of No! (La Philosophie du non, P. U. F., Paris, 1940; translated Orion Press New York 1968)

For these new epistemologists will still be philosophers - in a certain sense. This sense is extremely precise: we shall see that it is determined in the blank space which I have left in Bachelard's 'philosophical spectrum'. *

And they will be 'anabaptist' in that they will forswear all the beliefs, all the dogmas of traditional philosophy. We now know that we are in no danger if we take these terms completely literally.

They will establish themselves on this territory, still untouched at the point of Bachelard's coming; scientific knowledge itself, in its actual practice, is both the ground and the horizon of this territory.

But these philosophers will already be historians, and the historians of this country will necessarily be philosophers. Indeed we have already seen History appear in person in Bachelard's work on the concepts of traditional philosophy in the light of scientific thought; we have seen it rise from the darkness in which classical philosophy and epistemology had buried it. There is a theoretical necessity in this appearance as there was in the repression. But only a careful and exact scrutiny of the articulation of the new concepts of the new discipline can bring this necessity to light.

1. A Nonphilosophical Philosophy

It is in its very refutation that the necessity of philosophy appears. Indeed, insofar as we have recognized that philosophy was defined not by its object but by its function, and defined this function as an intervention in the area of the sciences, to that extent to annihilate what I shall call the 'philosophical instance' it is essential that the conditions which give it strength be themselves suppressed.

Given the analyses developing Bachelard in the last chapter, this means that all ideology - moral, political or religious, - must be driven out. In other words, science must be installed in an ideological vacuum - that is, in a social vacuum. [1] This operation can be performed in thought by building a utopia. But Gaston Bachelard leaves utopias to poets; it is with this world that the epistemologist is concerned.

Another way of annihilating the philosophical instance would be to suppress all science; in that case philosophy would be, as it were, 'an ambassador without an embassy': philosophy would then have lost its purpose, and would vanish. But the sciences do exist.

So there must be a discipline of philosophy because, in fact, sciences coexist with ideologies. But philosophy must be reversed: it must no longer be the spokesman of ideologies vis-à-vis the sciences - rather its task must be to neutralize their discourses and so to hinder the emergence of obstacles as far as possible. At the very least, it will set itself the task of distinguishing within given discourses between what derives from scientific practice and what originates in ideological discourses.

It is just this function of vigilance which Bachelard assigns to the new epistemology. 'Escorting' the progress of the sciences, its constant concern will be to 'sort out the philosophical interests' which arise in the scientist's route.

In other words, it treats problems completely alien to traditional philosophy; it sets questions which Philosophers cannot - or will not - see as interesting. It goes without saying that these 'problems' may vary; insofar as a science progresses, the 'values' which it secretes change and the footholds which it gives to ideology shift. On the other hand, the emergence of a new science may change the theoretical conjuncture. Finally, the dominant position of a given science in this theoretical conjuncture may come back into question: I am thinking in particular of mathematical physics which was dominant in Bachelard's time, but had not always been and perhaps will not continue to be for ever.

For all these reasons, the new discipline will be an 'open' philosophy, Bachelard asserts that 'The philosophy of scientific knowledge must be open' .... 'it will be the consciousness of a mind which founds itself by working on the unknown.' [2] Merely stating these principles shows that it is so open that, if the evolution of the scientific conjuncture demanded it, a Nonbachelardian epistemology in the Bachelardian sense of the term could be conceivable.

Being open, the new philosophy will be non-systematic; it will reject that tendency to become a system which Bachelard saw as a characteristic of traditional philosophy. In Bachelard's writing this is not a matter of the ill-founded reproaches which common sense directs against philosophy, but of an imperative which stems from the very nature of scientific knowledge. Science is not a unity: between the different branches of scientific knowledge development is uneven. Bachelard makes this position clear at the beginning of the La Formation de L'Esprit Scientifique (Vrin Paris 1938). There cannot therefore be, to use his expression, any unitary epistemology. Or better: it is at the level of each concept that the precise tasks of the philosophy of the sciences are posed.[3] Thus we must build a 'differential philosophy'; the new discipline will be a philosophy of the concept.

Finally, this new discipline will be attentive to the real conditions of scientific work, to the specificity of the different regions of science and to the evolution of their inter-relations, and vigilant as to the insertion of scientific knowledge into the world of culture; in short, it will be a historical philosophy.

One last word on my method of exposition before leaving these generalizations for the details of the organization of Bachelard's concepts. Indeed, it goes without saying that the order I have adopted is in no sense historical; I do not claim to display first of all the formation of the concepts, so as then to display them at work. This is clear enough from the fact that I have continually borrowed from all of Bachelard's works. A more genuinely historical treatment I am reserving for the last part of the book (not translated here). The analysis I carry out here is at present situated on a quite different plane. My aim can be described as to display the logical architecture of Bachelard's epistemology. Or better still: I hope I have shown the prerequisites of historical epistemology, which is itself - in a sense still to be specified - a prerequisite of epistemological history. What follows

is an account of how these prerequisites form a coherent and coordinated doctrine. One should not be surprised to rediscover as principles some concepts which we have already seen in a polemical form. It is clear also that since Bachelard's epistemology is more mature in his later works, I shall appeal primarily to them and shall not need to recall the earlier texts in which the same concepts are already at work, but in an imprecise and even an irresolute form. I shall make such a return only in those few cases in which the evolution of the concept has assigned it a clearly different meaning in the later works.

2: Dialectic

Exactly such a concept is that of dialectic, which undergoes a certain evolution between the first and the last works. Nonetheless, one must beware of seeing in this evolution a reversal of its meaning. It would be better to say that the function of the concept changes and that as a result its meaning swings from one side of the notion to the other.

Hence, to clarify things we must throw light on the function of the concept: its place is in the end to be found in the dialogue between the Mathematician and the Physicist, the purveyor of hypotheses and theories, and the master of experiments. A dialogue that cannot be grasped, as we have seen, without occupying that central position - so difficult to win - which Bachelard assigns to epistemology.

What is the exact meaning of this? An exchange of information whose final result is to adjust theory and experiment. But since we cannot have recourse to a fixed object, this adjustment must be thought not as a formal adequation but as a historical process. In a history which implies no security, no destiny promising theory that it will always find the means to realise itself. This history, then, is a dangerous one, and in it the two protagonists must unite their efforts.

Another word for this danger: failure. At any given moment the language of the physicist and that of the mathematician may be in contradiction. Philosophy will hasten to see a 'crisis' of science in this. For the mathematician and the physicist, it will just be the chance for some work: for the former to review his theories, and formulate other hypotheses; for the latter to refine his experiments and check his instruments. In short, a reorganization of knowledge will take place; it is this reorganization which Bachelard calls dialectic.

What he means to designate by this term is thus the specifically progressive procedure of scientific thought. But we have seen that in order to think the particular style of this movement, it was necessary to unleash a lively polemic against 'realist' philosophies: this is undoubtedly why the concept of dialectic in the earlier works is bent more in the direction of the break which experiment imposes on knowledge as it passes from one state to the next. Whereas in the last works, it is rather the progressive character of the latter moment which is emphasised. One is certainly entitled to think that this variation, which does not cast doubt on the meaning of the concept, is related to the fact that at the close of his work Bachelard had available other concepts with which to think the aspect of the 'break'; I shall return to this.

It is enough to note that under this definition, the concept of dialectic does not coincide with any of the concepts designated by the work dialectic in traditional philosophy. I will not rehearse here the proof of this which Georges Canguilhem has given in his article on 'The dialectic

and the philosophy of No' (in Etudes d'Histoire et de Philosophie des Sciences, 2nd Edition Paris, Vrin 1970); but I would like to point out that when we take account of the situation of Bachelard's epistemology with respect to previous Philosophy, this concept could not be the equivalent of any philosophical concept whatsoever. I hope that I have shown this in my first chapter.

3: Technical Materialism

Scientific thought, therefore, progresses by oscillations, by reorganizations of its bases proceeding from its summit; but this movement takes place only in and by scientific experimentation. That is by placing itself in the position of the other interlocutor (the mathematician). Here then is what scientific experiment in its technical detail forces one to think - a task unknown to philosophers, and a task for which Bachelard lays down the principles.

The texts in which Bachelard opens up this theory of scientific instruments as 'materialised theories', and of their assembly, are famous. His theses form a completely new body of doctrine, which he calls 'trained materialism' or 'technical materialism' ie the study of the material which science uses for the organization of its experiments.

This body of concepts developed progressively in Bachelard's thought, its essential base a reflection on the role which instruments play in microphysics. Its form, its field and its tasks are laid down in Le Rationalisme Appliqué: (1st Edition P. U. F. Paris 1949) but it is interesting that as early as 1927 in his Essai sur la connaissance approchée, (1st Edition Vrin, Paris 1928) Bachelard insisted on the role of instruments in physical knowledge which he thought the philosophers underestimated.

But if theories materialize themselves in this fashion, and if epistemology must therefore watch over the construction of a 'Trained materialism', this is in order to produce phenomena which are strictly defined as scientific phenomena; in order that no ideological intervention can be made in the functioning of scientific knowledge under the cover of natural observation.

Bachelard gives a parody appellation to this production of specifically scientific phenomena: phenomenotechnics, which is radically incompatible with a phenomenology that can only talk about phenomena, never produce any. In the Nouvel Esprit Scientifique (P. U. F. Paris, 1934) Bachelard asserts that 'the true scientific phenomenology is therefore essentially a phenomenotechnics. It reinforces what shows through behind what appears. It instructs itself by what it constructs (.....) Science raises up a world not by a magical force immanent in reality, but rather by a rational force immanent to the mind. '[4]

And he puts it still more sharply in his La Formation de l'Esprit Scientifique: 'Phenomenotechnics extends phenomenology. A concept has become scientific insofar as it has become technical, ie that it is accompanied by a realisation technique. '[5]

Thus the essential element of the activity of scientific thought is to produce couplings of the abstract and the concrete via the installation of theoretically defined instruments and via assemblages of apparatuses following programs of rational realization. Or again, to use another of Bachelard's expressions, to concretize the abstract.

It is at the centre of this process, unthinkable for the philosopher, that the thought of the epistemologist must

install itself.

Consequently, experience again becomes a central philosophical theme but with a completely new meaning. Thus Bachelard writes: 'A well conducted experiment always has a positive result. But this conclusion does not rehabilitate the absolute positivity of experience as such, for an experiment can be one only if it is complete, which can be the case only if it has been preceded by a well worked out project, starting from an achieved theory. In the end, experimental conditions are the same as preconditions of experimentation. '[6]

The 'objects' of these experiments must also be understood in a new manner. Amongst other examples, Bachelard gives this one in L'Activité Rationaliste de la Physique Contemporaine: (P. U. F. Paris, 1951) 'The meson, at the junction of the most abstract theory and of the most painstaking technical research, is now a particle with that double ontological status required of all the objects of Modern Physics. '[7]

So one can understand why Bachelard concludes: 'If one is to hold one's position at the centre of the working mind and of worked matter, one must abandon many philosophical traditions of the native translucence of the intellect and of the reality of the sensory world. '

4: Application

What is now clear is that we have determined the epistemological disciplines which, at the level of scientific activity, will fill the blank spaces which we left in the spectrum. We may call them 'Applied Rationalism' on the one hand, and on the other, 'Technical Materialism'.

But to give them these names - as Bachelard does - is right away to set up in each of them a distinction which produces a fertile reciprocity between the two doctrines.

Indeed, in other words, as far as Rationalism is concerned, (that is to say, as far as the production of concepts is concerned), even at this stage attention must be paid to the conditions of application of the concepts, or as Bachelard puts it, one must 'integrate into the concept its conditions of application'. Such a rationalism, then, is not unitary or monolithic, but already divided; or to put it better, it is a dialectical rationalism.

As far as 'Technical Materialism' is concerned, this means that the problems of assembly must integrate into their solution the theoretical conditions of their formulation.

The two disciplines are thus not only coordinated, but reciprocal. This reciprocity in its turn permits an important distinction between what I shall call problems of scientific research and those which one could more strictly call problems of experimentation.

The first effect of this distinction is the devaluation of the notion of 'method'. Or rather, the idea Bachelard often recalls vis-à-vis Descartes, that the notion of 'general scientific method' is vacuous, a notion which lacks the real movement of knowledge. The texts which are essential in this respect are in the Nouvel Esprit Scientifique and in the Speech on 'The Philosophical Problem of Scientific Methods' (delivered at the Congrès d'Histoire des Sciences in 1949) which I have quoted. This title is itself meaningful, since it clearly signals that according to Bachelard, there is no one method, but methods, specific to each science, and even to each determinate epoch of any given science.

What interests Bachelard - and it is more intelligible when one has understood the function of epistemology - is not the system of concepts with which the scientist rationally reconstructs the order of his research; this is the topic of all Discourses on Method. But rather the reality of research, with its hesitations, its setbacks, its mistakes, in a word, at its 'summit', in Bachelard's words, ie at the level of the difficult formulation of problems.

5: Problematic

As early as 1927 Bachelard asserted that the sense of the problem is the sinew of scientific progress; in his later work he continuously deepened this idea. Its most fully achieved expression is found in Le Rationalisme Appliqué, where Bachelard introduces the new concept of problematic to cover within the structure of the new epistemology what he had already attempted to think in terms of the mathematical metaphor of a (structured) field (corps de problèmes) - just as he had attempted to think the set of concepts of technical materialism within the metaphor of a field structured by two 'operations': experiment and definition (corps d'expérience et de prêcaution). [8] Benefitting by the other concepts of Applied Rationalism the concept of problematic is richer.

It is the positive notion which 'stands in for something else', according to the terminology we have proposed - for the philosophical idea of the 'given'; it resorbs the traditional notion of doubt, which is a correlate of the notion of general method. Let us make clearer this last point: Bachelard opines - against Descartes - that if one admits the existence of a general method of scientific knowledge, the doubt which is the first moment of that general method can never achieve specificity. In other words, it is purely formal, it does not allow the production of any correction, and hence of any knowledge. We may read, for example, in the Formation de l'Esprit Scientifique: 'Descartes' confidence in the clarity of his image of the sponge is symptomatic indeed of his inability to install his doubt at the level of the details of objective knowledge, to develop a discursive doubt which could unpick every joint of the real and every corner of the image. '[9] I can add that my study has proved that all this depended in the last analysis on the philosophical idea of looking outside knowledge for an object to serve as its foundation.

Bachelard's concept of problematic takes into account precisely the disqualification of the philosophical notion of object. It could be said to connect the notions of given and of doubt on another terrain: that of knowledge as a process of objectivation. Bachelard writes: 'Universal doubt irreversibly pulverizes the given into a heap of heteroclite facts. It corresponds to no real instance of scientific research. Against the parade of universal doubt, scientific research demands the setting up of a problematic. Its real starting point is a problem, however ill formulated. The scientific ego is then a program of experiments; while the scientific non-ego is already a constituted problematic. '[10] Thus for the scientist there can be no general unknown; the indeterminate unknown is of no interest to him; all his effort is precisely to specify the unknown. It is at the level of these specifications that the new epistemologist must pursue his task, which is always a double one, of defending the scientist from the intrusion of extra-scientific notions, and of instructing himself concerning the style of the progress of a given science at a given moment in its history.

6: Scientific Borrowings

But Bachelard allows us to go further in the - necessarily formal - determination of the structure of all production of scientific concepts. He shows, indeed, especially in Le Rationalisme Appliqué, that the problematics of the different sciences are not wholly independent of each other, but only relatively autonomous, and that zones of overlap may appear. What he calls transrationalism[11] and shows at work with respect to piezo-electricity is of interest insofar

as it enables us to pose the elements of a theory of scientific loans.

Bachelard writes that transrationalism establishes itself at the end of prolonged theoretical labour, by the intermediary of algebraic organization. It has nothing to do with some vague correspondence established by unprincipled empiricism at the starting point of knowledge. [12] On the contrary, it is at the level of a technical organization refined by the determination of ever more precise - and hitherto unnoticed - variables, that 'interferences' between domains of rationality can arise.

We must admit that the principles which Bachelard gives us have not been applied to a large enough number of examples for us to get a precise idea of the mechanisms which govern the details of these scientific loans. But at least, formally, the principles have been laid down and the field cleared. All that is left is to get down to work...

Let us end this exposition of the major concepts of the new epistemology with a point to which Bachelard has accorded the greatest importance, ever since the Formation de l'Esprit Scientifique: this organization of the production of scientific concepts does not take place in the pure space of disembodied minds. It is materialized in the form of institutions, meetings, colloquia...

As a result there is constituted what Bachelard calls a 'city of science': and he constantly draws our attention to the extremely social character of modern science. Bachelard therefore attempts to assess the cohesion of this city and its effectivity.

Its effectivity: by means of 'communications' which take place within it and which Bachelard suggests should be considered as a 'mutual pedagogy,'[13] theories circulate more rapidly, and permit an acceleration of discoveries.

Bachelard writes in L'Activité Rationaliste: 'The isolated worker must admit that 'by himself he could not have made that discovery...'[14]

In turn the city's cohesion allows the elimination of every aberration related to the subjective character of any particular researcher. Modern science is freed from all those reveries which encumbered the science of previous centuries. In this sense, it is more difficult for epistemological obstacles to form - hence, it would appear, the acceleration of scientific time in our days - although their appearance is inevitable, as we have formally demonstrated.

The conclusion: it is the city of science which creates its own norms. It is the city which maintains the criteria of objectivity and of truth. We can grasp this function, as Bachelard shows, in the technical region of the city: there one can read in material form, the general characteristics of the city of science.

Just so, in Le Matérialisme Rationnel, Bachelard shows that in contemporary chemistry the 'reagent', a mass-produced item, standardized according to universal norms, is a good illustation of the social character of modern science. [15] He demonstrates the same point with respect to the homogeneity of metals in Le Rationalisme Appliqué.

We conclude, then, that the city of science stands in for the Reason of the philosophers, but elsewhere; on the other hand, it is strange to see Bachelard attempting in Le Rationalisme Appliqué to found the apodicticity of scientific values in a vocabulary of a psychologistic kind. He attempts - very ingeniously - to show that the social character of Science is first of all an inter-subjective character, that this intersubjectivity of objective knowledge produces a division within the subject and that the obligation we feel when we come into contact with a scientific value is located in this division.

It is as if Bachelard hoped in this way to resolve a problem whose very terms were forbidden to him ever since he broke with the conception of a norm-producing Reason like the one constituted by the philosophical problematic. We must ask if, at the end of his thought, Bachelard was not suddenly stricken by 'philosophical guilt'. These researches would then be an attempt, marginal to his work, to get back to the ground of Philosophy and justify himself in that region.

Thus, as a result of the epistemological work of Gaston Bachelard, we may assert that (to use a different vocabulary) the concept of a theoretical mode of production [16] has been erected; in it the formal principles, invariant with respect to every theoretical mode of production, are laid down and put to the test in the cases of the Physics and Chemistry of the early 20th Century. After seeing by what sort of polemical labour the field of this new concept was cleared, we have now seen what are its internal articulations.

However, it appears that when Bachelard had constructed the concept of theoretical mode of production, he thereby was in a position to think the transition from one given mode of production to another. Even if in his work he did not treat this problem in all its generality, one can still see it at work in certain specific notions.

That is how he founds a new concept of the History of the Sciences.

7: The Concept of the History of the Sciences

Bachelard thought this concept for itself only in his last works, and at a 'lecture at the Palais de la Découverte'; but in a practical state it is present in his thesis of 1927: A study of the evolution of a problem in Physics: the propagation of heat in solids.

This text begins with these words: 'It is easy to believe that scientific problems follow each other historically in an ascending order of complexity, without always making the effort to move in thought so as to confront the problem as it appeared to the primitive observation.'

The entire novelty of the enterprise is inscribed in this sentence. This novelty is polemically asserted against a positivist 'history', which Bachelard explicitly controverts: positively, it is defined as an effort to move to a previous viewpoint. Or better: this effort is in no sense aesthetic, it is not a question of reliving the past, but of judging it: for 'once the solution is found, its clarity lights up the previous data.'

So the first characteristic of this History is its normativity: Bachelard repeats this more than once. He maintains it against 'the spontaneous hostility of the historian to every normative judgment'. This leads straight to the second characteristic: the judgment produced will be recurrent. It is for this reason, according to Bachelard that the history of the sciences cannot be a history 'just like all the others.'

The first effect of this double character: a whole type of research is disqualified - the attempt to discover precursors for every scientific discovery. Thus in Rationalisme Appliqué, Bachelard comments on those who saw Hegel as a precursor of Maxwell: 'There is nothing in the philosophy of Hegel or Schelling to prepare the synthesis of the domains of electricity and optics... The foundations are established by recurrence. We see the base by starting

from the summit.' [17] In a similar manner, in Activité Rationaliste he comments on those who claim that since Raspail proposed a planetary image of the atom in 1855, he was a precursor of Rutherford and Bohr. [18]

Bohr and Rutherford did not propose an image, but a concept; Bachelard has shown that there can be no continuity between the two. The History of the Sciences can make its judgements only when informed by epistemology.

But from what source does epistemology inform itself? The source as we have seen, is living science, as it engages in research. The consequence immediately follows: recurrence cannot be once and for all, but must constantly be remade. So Bachelard writes in his Lecture: 'Insofar as he is informed by the modernity of science, the historian of the sciences will introduce finer and finer nuances, and more and more of them, within the historicity of science... It would appear that a luminous History of the sciences cannot be completely contemporaneous with its unfolding.'

It follows that one must vigilantly beware of false recurrence - the search for precursors is an example of this - that one must proceed with tact, as Bachelard puts it; but so also must one affirm the progressive value of the scientific past.

Historical epistemology teaches us that science progresses by means of jerks, sudden mutations, reorganizations of its principles; in short, by a clear dialectic. It is for this reason that the History of the Sciences must itself be dialectical: it will fasten especially on those 'critical' moments in which the bases of a science are reorganized.

The History of Sciences will see in the principles which are abandoned the effect on the practice of the science of certain 'epistemological obstacles', which epistemology will teach it to characterize. From then on, one can understand that Bachelard is led to distinguish two types of critical moment:

i. the moment in which at one point at least, in a given domain, the texture of pre-existing ideology is torn and scientificity is installed. This is what he calls the moment of the rupture;

ii. the moment, after the entry into scientificity, when a given science reorganizes its bases: this moment is styled recrystallization or reorganization.

The result of this distinction is to cut the history of the sciences into two: indeed, moving from reorganization to reorganization one finds on the one hand a clear and rapid History of positivities;[19] on the other, a more slowly moving History of the negative. This is Bachelard's distinction between ratified History and lapsed History.

But it goes without saying that the task of the historian of the sciences is to pay attention to both, and to observe carefully their reciprocal relations. Indeed, this should be clear enough to him if he is the epistemologist he should be.

Such, reduced to their logical form, are the characteristics of the new discipline of which Bachelard gives us the principles. We have seen how each of these characteristics is an effect of a concept of the new epistemology. We may assert that once it had become historical (in the sense of taking for its object the historicity of the concepts produced by scientific knowledge), epistemology 'enveloped' in a Spinozan manner a new concept of the history of the sciences and a new discipline predicated on that new concept.

Footnotes

1. This reasoning, in the pursuit of which I follow Bachelard could, I suggest, acquire precise theoretical status in the structure of the Marxist science of history: 'historical materialism'. Cf. L. Althusser's piece in Cahiers Marxistes-Leninistes No. 11 April 1966: Matérialisme Historique et Matérialisme Dialectique.
2. Philosophie du Non, p. 9; Philosophy of No. p. 9.
3. Philosophie du Non, p. 14; Philosophy of No. p. 12.
4. Le Nouvel Esprit Scientifique, p. 13 (10th Edition, P. U. F. Paris 1968).
5. La Formation de l'Esprit Scientifique, p. 61 (7th Edition Vrin Paris 1970)
6. Le Nouvel Esprit Scientifique, p. 9.
7. L'Activité Rationaliste de la Physique Contemporaine: Here one must examine the unity of this long paragraph. It contains a very precise illustration of the theses which we are defending. The first lines read:
'The existence of the meson poses philosophical problems which would themselves take a whole book to examine, for one would have to evoke cosmological problems which are posed in terms quite different from those of previous cosmologies. (...) One would have to completely remould simplistic ideas about the relations between hypothesis and experiment. Indeed, the hypothesis of the meson was initially a mathematical hypothesis, and not an image related to experiment. (...) One could with as much justification call the philosophy of the meson: from mathematical theories of the nucleus of the atom to aeronautical experiments on cosmic rays.' (My emphasis - DL).
8. I take the liberty of noting in this regard that these metaphors borrowed from mathematics are not isolated in the work of Gaston Bachelard. One could even say that the framework of the vocabulary of his philosophy is scientific in character. The framework of traditional philosophy is moral, legal or religious in character; noting this fact, I dare assert that here we have an index of the novelty of Bachelardian philosophy; a philosophy which refuses to carry extra-scientific ideological values must start by defending itself against them at the level of the words which it uses. This is another reason for the dépaysement one feels in reading Bachelard.
9. La Formation de l'Esprit Scientifique, p. 79.
10. Le Rationalisme Appliqué, p. 51 (Third Edition P.U.F. Paris 1966).
11. Le Rationalisme Appliqué, pp 121 and 129.
12. Le Rationalisme Appliqué, p 133: 'The question is then no longer one of defining a general rationalism which will gather together the elements common to all regional rationalisms. By such methods one would find no more than the minimum rationalism of everyday life. On the contrary, the point is to multiply and refine those structures: which, from the rationalist point of view, must be expressed as an activity of structuration, as a determination of the possibility of multiple axiomatic systems corresponding to the multiplicity of experiments ... Integral rationalism can then be no more than a domination over different basic axiomatic systems. And it designates rationalism as a dialectical activity, since axiomatic systems are dialectically articulated to one another.' There is no point in rehearsing here a commentary on this remarkable text: the whole of this study is an attempt to do so.
13. L'Activité Rationaliste, p. 6. On the same page one may read this passage of anti-philosophical polemic:
'The School - in the sciences - does not hesitate. The School - in the sciences - sweeps its pupils along. Scientific Culture imposes its tasks, its line of growth. Philosophical utopias can do nothing in this area. Idealism displays nothing. One must go to school - to school as it is

- to school as it develops, in the social thought which transforms the school. '

14. Le Rationalisme Appliqué, p. 23.
'The real scientist is always in the position of the pupil. ' The implication is that: 'The philosopher is always playing the pedagogue. '

15. Le Matérialisme Rationnel, p. 78 (Second Edition P. U. F. Paris 1963).

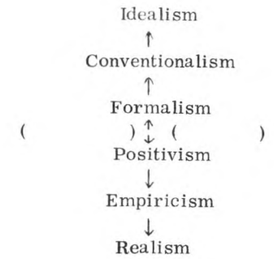
16. Louis Althusser has proposed this concept.

17. Le Rationalisme Appliqué: p. 153. Bachelard writes: 'Still, Schelling was able to think that the luminous aspect of certain electrical phenomena was an index of the unity of principle of light and electricity. It is thus very clear that Schelling's unification is superficial. ' It is not achieved in the correct perspective of an Applied Rationalism: 'It initiates no constructive thought; it cannot promote any technique. '

18. L'Activité Rationaliste, p. 69. Raspail wrote: 'Imagine a series of carriages in motion - but that one cannot see the locomotive: this movement could be explained just as well by the hypothesis of traction as by that of propulsion; the locomotive could just as well be supposed to be placed before as behind the train. ' Such is the justification Raspail gave for his 'atomic astronomy'.

19. As expressed in the Lecture, this appears as the 'liquidation of the past'; the most regular example is therefore the history of mathematics.

*Translator's Note
Bachelard's philosophical spectrum:



This is reproduced from Bachelard's Le Rationalisme Appliqué, P. U. F. Paris, 1949, p. 5) This diagram appears near the beginning of section 5 of the Part i of Lecourt, the section entitled 'the topology of philosophy'. Lecourt paraphrases Bachelard's meaning as follows: on both sides of this symmetrical pattern, the axis of which could be reversed without changing the meaning of the terms, each of the 'inner' doctrines is subordinated to, 'lives in the light of' the extreme terms: Realism and idealism.

ALTHUSSER AND BACHELARD

by BEN BREWSTER

In Lenin and Philosophy, Althusser castigates the Otzovists, the Bolshevik adherents to Mach's philosophy of empirio-criticism: 'The Otzovists were infatuated with a fashionable philosophy or philosophical fashion, "empirio-criticism"... They said that Marxism had to rid itself of that pre-critical metaphysics "dialectical materialism", and that in order to become the Marxism of the 20th century, it had at last to furnish itself with the philosophy it had lacked... empirio-criticism'. (1) Such criticisms are not restricted to the Russian Machists, of course. They can also be applied to the early Lukács and Korsch in the 1920's, grafting the neo-Kantian and neo-Hegelian philosophies of the Heidelberg school onto Capital, (2) and to Sartre and Merleau-Ponty in the 1940's attempting to combine their existentialist philosophy with a Marxist politics. Could they not also be applied to Althusser himself? Althusser is often charged with attempting to reconcile Marxism with a 'philosophical fashion', ie, with 'structuralism'. But since in such arguments 'structuralism' always remains a classificatory chimera, this charge is only damning for those for whom a refutation follows from the mere desire for refutation. However, could not the charge be brought more seriously with reference to Bachelard and Bachelardian epistemology? Bachelard is not often referred to by Althusser, but he, and a number of other thinkers who can be said to have worked in parallel with him, continued his work or borrowed from it (Cavaillès, Canguilhem, Foucault) are always referred to with respect to one of Althusser's most crucial concepts, the concept of the epistemological break. The specific problems of this concept are discussed by Antony Cutler elsewhere in this issue of Theoretical Practice. The problem here is what relation between Althusser and Bachelard is implied by the considerable philosophical debt revealed in that article; and, in particular, is the relation of the same type as that between Bogdanov and Mach, the early Lukács and Lask, or Sartre and Husserl?

What is at stake here, and in all the other examples I have cited, is the relationship between the Marxist science of history and Marxist philosophy, though not all the theorists mentioned would recognize the distinction in the classical historical-materialism/dialectical-materialism form. Hence to examine Althusser's relation to Bachelard, it is first essential to outline his relation to Marx and the classical works of Marxism-Leninism, at the risk of repeating what is obvious to those who have any knowledge of his work. Here, too, it is worth comparing his position with those of other Marxists and revisionists. Marx's most consistent and repeated philosophical claim is well known - his adherence to materialism. Later developments of Marxist philosophy have always hinged on what attitude to take or what interpretation to give to this claim. Revisionists like Bernstein and the Russian Machists rejected materialism, the former because of his attraction to Kantian ethics, the latter because they believed, following Mach, that the recent 'revolution in physics' had made the category of

matter redundant. The young Lukács and Sartre avoid this issue by asserting that the philosophy of Marxism is specific to the human world, that it defines a methodology for the science of history, that this philosophy was outlined in Marx's Early Works before being applied to a concrete object in Capital. For Althusser, too, the crucial question is the relation between Marxist science and Marxist philosophy. But far from the philosophy preceding the science, the constitution of the science involved the rejection of the philosophical tradition in which Marx was educated, and which his Early Works still represent. The new philosophy of dialectical materialism could only emerge later as a result of the emergence of the new science, historical materialism; and the concepts of this new science could only emerge clothed in words left over from the old philosophy or borrowed from other disciplines. Hence the texts embodying the new science can only be read symptomatically, as the effects in discourse of a new practice of science, a process of production of historical knowledge effects rather than a consciousness of history to itself, and this necessity for a symptomatic reading applies a fortiori to Marx's methodological or philosophical statements, which indicate the break without fully grasping it conceptually. (3) However, this scientific revolution had to have philosophical effects, like all scientific revolutions, because the ideology it replaced, the Hegelian and post-Hegelian philosophy of history stipulated and founded a certain general epistemological problematic: the epistemology linking subject and object in an empiricist or speculative relationship. The new concept of the object of history, the theory of the social formation as a process without a subject, necessarily removed the ground out from under this problematic, posing a new, non-empiricist, non-speculative epistemology for the science of history, and redefining the philosophical instance itself no longer as the guarantor and founder of scientific truth, but as the age-old battle-field of idealism and materialism, in which the emergence or recasting of concepts in the sciences re-aligns the fronts in philosophy without affecting the nature of the struggle. Engels had grasped at this new philosophy in Anti-Dühring and the Dialectics of Nature, where he attempted to think the philosophical effects of the science of historical materialism; however, he tended to interpret it in the light of the materialist natural philosophies of Büchner, Vogt, Moleschott and Haeckel, though aware of their limitations, as an ontology, an exhaustive account of the basic characteristics and laws of movement of matter, without which empirical scientific statements are impossible, and this interpretation, without Engels' sensitivity, later became the 'orthodox' interpretation of dialectical materialism in the international Communist Movement. Only Lenin, by distinguishing between the philosophical category of matter and the scientific concepts of matter, and between relative and absolute truth, in Materialism and Empirio-Criticism, rescued Marxist philosophy from this language of naturalistic metaphysics and made the first steps in a truly Marxist philosophical practice.

Bachelard's approach to the philosophy of the sciences is in many ways analogous to that of Althusser. Just as Althusser attempts to discover the effects in philosophy of the emergence of a new science, historical materialism, Bachelard's project is to discover the effects in philosophy of the recrystallization of concepts in modern physics and chemistry associated with the name of Einstein. The empirio-criticists were, of course, responding to the 'crisis' which was eventually resolved by this re-crystallization. But instead of attempting to find a philosophical solution to this 'crisis' in a 'theory of knowledge' combining positivism with neo-Kantianism, or of resorbing the scientific

revolution which in fact resolved it back into such a general theory of knowledge, as his contemporary Émile Meyerson did, Bachelard argued that this revolution had not taken place against one existing philosophy (mechanical materialism) with the assistance of another ('agnostic' idealism), but without benefit of philosophy at all, against all existing philosophies. Moreover, this was not an unfortunate exception, but the rule. Sciences are produced in opposition to philosophies, including especially those apparently unphilosophical philosophies, empiricism and positivism. The truth of a scientific statement is not founded on any general philosophical principle of truth, on any philosophical guarantee. There is no 'theory of knowledge', there are as many epistemologies as there are sciences producing knowledge effects. These principles apply to supposedly non-empirical formal disciplines like mathematics and logic as well as to the 'empirical' sciences of nature. There is no philosophically defined world of things-in-themselves which empirical science appropriates either asymptotically (Duhem) or piece-meal (Kelvin). Nor is there any philosophically defined Evidenz or consciousness to which all scientific statements can be reduced. The materiality of the real world, ie, its existence independently of thought, and the possibility of its appropriation by the sciences (the primary categories of materialism), are sufficiently confirmed by the practice of the sciences themselves, by their ability to inscribe their theories in experimental forms, in what Bachelard calls a phenomeno-technics. (4) This, too, is as true of supposedly formal disciplines like mathematics and logic as it is of the traditional 'experimental' sciences. (5) Sciences are not sciences of the (sensorily) observable world, explaining the regularities in the natural world available to the senses, they themselves produce their objects and phenomena in their theories and their materialization in experimental proofs. The world of micro-physics is not the world of sensory perception, it is a scientific construction, just as non-Euclidean geometry does not correspond to our experience of space; but this makes them no less 'real' than the Newtonian mechanics and Euclidean geometry which Kant believed corresponded to the only possible conceivable world.

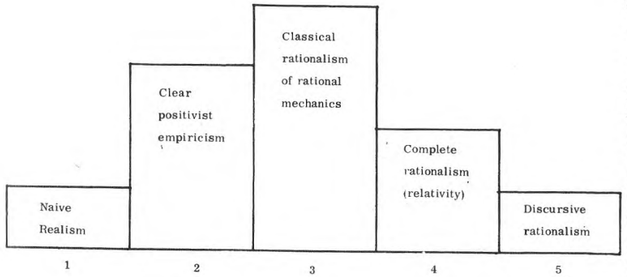
This introduces a second Bachelardian theme. Sciences are born by breaking with pre-existing modes of thought, by overcoming the 'epistemological obstacles' secreted by those modes of thought, and they progress discontinuously by further such breaks, overcoming further epistemological obstacles arising from the resorption of their new concepts by traditional modes of thought. Bachelard gives no general theory of epistemological obstacles, contenting himself with illustrative examples. The contents page of La Formation de l'Esprit Scientifique (1938) gives a representative list: immediate experience, general knowledge, the abusive extension of imagery, pragmatic knowledge, substantialism, realism, animism, etc. Three things are important about epistemological obstacles for our purposes. First, once a science has been constituted, they arise inside the science: 'It is not a matter of considering external obstacles such as the complexity or fleetingness of phenomena, nor of blaming the weakness of the human senses and mind: delays and disturbances occur intimately in the very act of knowing, by a kind of functional necessity'. (6) Second, they are a kind of trap for scientific knowledge and concepts set by the thought habits of everyday life and experience: concreteness, realism, utilitarianism, anthropomorphism, etc: 'Over-familiar scientific ideas become charged with too much psychological concreteness, they collect too many analogies, images and metaphors and lose little by little their abstraction vector, their fine abstract

tuning.' (7) Bachelard constantly counterposes the abstraction necessary to scientific thought to the 'revery', the dream-like character of everyday experience, and particularly of art and poetry. As is well known, he wrote two series of books, one devoted to questions of scientific epistemology, the other questions of artistic revery. To constitute itself a science must break away from revery, but the latter does not thereby lose its right to exist; the domains of knowledge and art are simply separated. Third, the most characteristic epistemological obstacles, realism and animism (idealism) are also the two poles of what Bachelard calls the 'philosophical spectrum'. (8) The psychological power of the obstacles gives a foothold to the philosophies which claim to guarantee the knowledges produced by the sciences, while really only battening on to and supporting the epistemological obstacles produced at each stage of scientific development. Philosophies are produced as a result of scientific advance with the aim of re-uniting the world of knowledge and experience which each new science and each new scientific advance shatters. Hence philosophies can be defined in a spectrum around ongoing science in terms of their displacement from science.

Next, science is, according to Bachelard, characterized by a 'dialectic' (not to be confused with the traditional philosophical use of the term), (9) a dialogue. This is not, as in the case of most hypothetico-deductive theories of knowledge, a dialogue between the scientist and nature, the scientist asking questions, nature answering yes or no. It is a dialogue between two complementary aspects of scientific practice, the rational formulation of hypotheses and their technical application in experiments. It is a dialogue because experiments are the materialization of invented phenomena, not mere sensory observations, and would be impossible without the prior mathematical formulation of the possibility of such phenomena, while experimental

failure is the precondition of theoretical reconstruction, whether this is mere correction or a major recasting (refonte) of the theory. A science progresses unevenly by this dialectic of reason and application. (10)

Thus a science is not the exhaustive investigation of a closed domain defined a priori by sensory experience, philosophical fiat or scientific hypothesis. Once it has made its first break with common-sense experience and the theoretical modes of thought anchored in common-sense experience by the leap known as an epistemological break, its future is completely open. The dialectic of reason and application ensures that it will go through a series of recastings or recrystallizations, each of which will redefine the basic concepts used by the science. Thus the Newtonian concept of mass has no perfect equivalent in Einsteinian phsyics where a body can have different masses at different velocities and in different directions, and the concepts of position and velocity in Newtonian physics have no equivalent in modern sub-atomic physics. However, there is a life-line linking the new concepts to the old in the mind of the scientist, the 'epistemological profile', which measures the extent to which each scientist's use of the concept at any given time corresponds to each phase of the development of the theory in which the concept has been used. Thus Bachelard gives the following example for his own use of the concept of mass:(11)



Epistemological profile of my personal notion of mass

This schema thus implies an ideal sequence of recastings, each characterized by a specific form of rationality, with a psychological support in the mind of the scientists.

Lastly, this epistemology implies a novel conception of the history of the sciences. Scientific knowledge is not accumulated piecemeal, so the history of the sciences cannot be a record of discoveries. Nor, despite the ideal sequence in the epistemological profile, can the future developments of a science be predicted, and hence a teleological history of the precursors of the final truth towards which science is tending is equally impossible. Each break or recasting of knowledge redefines the past history of scientific and ideological knowledge by rejudging it, which Bachelard calls 'recurrence', thus creating a double history, a history of error, of theories ruled out by the advance of science (histoire périmée) and a history of the progressive developments of the science since the first epistemological break (histoire sanctionnée). The same

arguments indicate that there can be no history of science in general, only a history or histories of sciences. (12)

This brief sketch of Bachelard's concepts is further developed in the chapter from Lecourt's book L'Epistemologie historique de Gaston Bachelard (Paris, Vrin. 1969) published in translation in this issue of Theoretical Practice. It is clear that Bachelard's philosophy closely corresponds to Althusser's in many respects. But before going on to give a comparison of the two, it is important to stress the limitations of Bachelard's epistemology, limitations which have one ultimate source: the epistemologist and the historian of the sciences are located by Bachelard only with respect to the development of the science in question.

The first result of this one-sidedness is the constant intrusion of psychologism. [13] Scientific knowledge only exists in opposition to, by overcoming, epistemological obstacles. But to theorize epistemological obstacles, ie, what prevents the existence of science, a theory of something other than science is required: a theory of ideology, ie, a theory of the ideological instance in the social formation. Hence a theory of the history of a science cannot be wholly independent of the theory of history in general, historical materialism. But Bachelard attempts to think the problem of epistemological obstacles solely from the side of science and the scientist. He does so in a classical way, by an appeal to the psychology of the scientist. Epistemological obstacles have no historical location; they are assumed to be universal and natural products of the human mind. (14) A 'psychology' is necessary to explain them, what Bachelard called a 'psychoanalysis'. Bachelard first developed the theses of this 'psycho-analysis' in La Formation de l'Esprit Scientifique and La Psychanalyse du Feu in 1938. The second of these books, now translated into English as The

Psycho-analysis of Fire (Routledge and Kegan Paul, London 1964), takes the example of fire, with all its psychic attraction, the object of ideological speculation for millenia, but utterly destroyed as a concept by the Lavoisierian theory of combustion. Despite this destruction, fire retains its attraction, its mental power, in revery. The Psycho-analysis of Fire was only the first of a series of books which analysed the psychic power of the other 'elements' of pre-scientific thought, and then developed a general 'poetics of revery', a poetics of imagery, which, as Mary Ann Caws points out, comes close to Breton's surrealist theory of poetry, as well as to Jung's theory of archetypical images. (15) Thus the natural tendencies of the mind are anti-scientific. Science is a constant struggle against the psychological traps lying in wait for each scientific concept, the epistemological obstacles.

This psychologistic conception of the scientific mind has effects throughout Bachelard's work. First, on his view of philosophy. Bachelard's philosophical statements are all polemical, all directed against the claims of other philosophies over the sciences, from the stand-point of a philosophy secreted in the development of each science. Traditional philosophies lodge in the niches provided for them by the ideological obstacles that the mind creates for the concepts of the science, forming the philosophical spectrum. The true epistemology of the science in question only has a fleeting existence in the science's rejection of the claims of these philosophical hangers-on. Hence there is no positive role for philosophy: philosophy has no history, it is a kind of wake left behind by the development of the sciences.

So far these criticisms of Bachelard have not directly exposed any internal inconsistency, nor indeed are they incompatible with a certain conception of Marxism, which similarly refuses philosophy anything more than an imaginary reality, reducing knowledge to a psycho-physiological faculty of the human brain. (16) However, the ahistorical nature of the psychological obstacles to be overcome in the establishment of a science has its inverse in a necessary set of stages, a hierarchy of rationalities in the constitution of scientific concepts: the epistemological profile. Three consequences follow. First, an evolutionist conception of the history of the sciences in which each passes through the necessary stages of the profile - but then what of the concept of recurrence and the double history of the sciences? Second, each science is seen in isolation according to its place in the evolutionary scheme, and each science is essentially similar in kind - but then what of the claim that each science constitutes its own epistemology and no history of science in general, only histories of sciences exist? Third, a general theory of scientific rationality is set up corresponding to the last phase, that of discursive reason - but then what of the attack on philosophy for attempting to pre-found the truth of scientific statements?

Another contradiction arises from the individualism of this psychology of errors. Bachelard correctly states that, as a practice, a science cannot be individual, it is not founded, like Cartesian doubt, on the reduction of knowledge to an individual cogito and then its reconstruction on the ground cleared by this reduction. On the contrary, scientific knowledge is a 'cogitamus', a collective activity, uniting collective rational activity and collectively controlled experimentation. But Bachelard conceives error as individual, for, although it is archetypical, the psychology of epistemological obstacles

manifests itself in the psyche of the individual scientist. Hence he comes to believe that the socialization of a science, its incorporation into scientific institutions, the cité scientifique, is sufficient to guarantee science against the aberrations of the individual psyche. Hence the progressive sequence of rationalities implied by the epistemological profile is guaranteed by the institutionalization of science in the historical development of scientific institutions. The evolutionism of the epistemological profile is accompanied by a historicism of the social conditions of existence of the scientific mind. (17)

Thus Bachelard's attempt to abolish philosophy in a non-philosophical philosophy in fact restores an evolutionist and historicist theory of knowledge. The philosophy of knowledge achieves its re-entry by the classical back-door of psychology. How this re-entry is achieved is revealed by the way examples function in Bachelard's work. The latter is full of convincing demonstrations based on brilliantly analysed examples from the history of the sciences. Every scientific development is examined ex post, recurrently, as a relationship between the concepts of the science and its constructed object, completely rejecting the subject-object couple of the traditional theory of knowledge. But whenever Bachelard makes a general statement about the sciences, he substitutes a constitutive subject in place of the concepts term in the couple used in the examples, a subject which first appears in negative form as the psychological subject of error, but then positively as the scientific 'mind' of the advanced phases of the epistemological profile. In the concrete examples, Bachelard merely takes up a position on a particular scientific development from within the science in which it takes place. In making a general statement he is forced to take up a position outside particular sciences, and in the absence of any theory of the outside of science, he lapses into a traditional philosophy of knowledge with its categories of subject and object. (18) This contradiction can only be resolved by situating science with respect to the ideologies with which it breaks, i.e., by a theory of 'epistemological obstacles' as part of the ideological instance of the social formation, and with respect to the world views engaged in what is ultimately a class struggle, the struggle between materialism and idealism, i.e., by a new theory of philosophy. But to do so, Bachelard would have had to take into account the epistemological break achieved by the science of historical materialism and situate himself within the new practice of philosophy, dialectical materialism, which is an effect of that break.

However, if the history and philosophy of the sciences require a historical-materialist theory of ideology, this theory is precisely the sector of historical materialism which has hitherto suffered the most from historicist (Hegelian) and positivist (Enlightenment) interpretations. In a recent article, 'Ideology and the Ideological State Apparatuses', (19) Althusser has laid the basis for a genuinely historical-materialist theory of ideologies. This theory is a double one. Ideology in general is transhistorical, it is the mechanism which represents the imaginary relation of individuals to their real conditions of existence whatever the mode of production and form of class rule dominant in the social formation. The basic transhistorical role of this ideology in general is to constitute individuals as subjects, as supports for economic and political practice. Central to this role are the 'practical ideologies' according to which the simplest actions of 'everyday life' are experienced, reinforced by the rituals of religion and ethics embodied

in the institutions of ideological practice, the ideological apparatuses.

It is this transhistorical character of ideology which accounts for the value of so many of Bachelard's examples despite the psychologism of his explanations. In particular, it accounts for the predominance of empiricism (realism) and idealism as obstacles, for various forms of this couple are characteristic of the transhistorical structure of ideology in general. However, ideology cannot be reduced to this transhistorical structure: ideologies are historical, they change with the other instances of the social formation (i.e., they are determined in the last instance by the economy), and with the emergence and development of the different sciences. As well as subject-constituting practical ideologies, the ideological instance contains theoretical ideologies justifying and reflecting the historically changing economic and political practices, and theoretical ideologies of application and education vis-à-vis the emerging scientific practices. In class societies these ideologies are unified by the dominance of the ideology of the ruling class, the ruling ideology. Finally there are those theoretical ideologies known as philosophies which, as we shall see, arise as a result of the emergence of the sciences in the form of a struggle between materialism and idealism.

Hence sciences arise and develop in this complex space of practical ideologies, theoretical ideologies, philosophies and other sciences. This immediately makes the histories of the sciences both more concrete, and more differentiated. No universal pattern of development need be prescribed; indeed, the different sciences must develop differently, in response to their different ideological environment. As Michel Pêcheux argues (following an unpublished article by Etienne Balibar) (20) the ideologies most closely linked to a science are the ideologies of its technical application and of the teaching of the discipline. Moreover, the relation of each science to its ideologies of application and education will be different: Canguilhem shows that the practice of medicine has always occupied a far more significant role with respect to biology than engineering has with respect to physics. (21) This is because of the different place in the social formation, and hence in the ideological instance of the latter, occupied by the technical ideologies of medicine (essentially concerned with the reproduction of labour power) and engineering (essentially concerned with the advance of the productive forces). Thus there is a political differential between sciences and between their extra-scientific effects.

Among these extra-scientific effects, as we know, are philosophical effects. As is well-known, Lenin argued in Materialism and Empirio-Criticism that philosophy is and always has been partisan: 'Recent philosophy is as partisan as was philosophy two thousand years ago. The contending parties are essentially... materialism and idealism'. (22) Where practical ideologies constitute individuals as subjects, theoretical ideologies recognize such subjects as constitutive of the world, of nature (alchemy, astrology), history (empiricist and historicist theories of history) and man (psychology). In turn, individuals can recognize themselves as the subjects constituting the domains of theoretical ideologies. But the world of which these subjects are recognized to be constitutive in theoretical ideologies is always this world, i.e., the world of the contemporary ruling ideology. Hence the necessary and universal character of the subject constituted by the mechanism of practical ideologies is attributed to what Marx called das Bestehende, the existing stage of affairs as defined by ideology. The emergence of a science, whose 'objects' are not the objects of the ideological subjects, not the objects of 'this' world, threatens this economy and thus the dominance of the ruling ideology. Hence it evokes a struggle in a new arena denying (idealism) or affirming (materialism) the possibility of such a practice and such knowledge, and hitherto always resolving the struggle in favour of idealism by supposedly 'founding' the new knowledge in the subject of an empiricist-idealist theory of knowledge. This new reality, which does not exist before the existence of a science, is philosophy. Although the line of battle in philosophy is a changing one, changing with the emergence of new sciences and mutations in existing sciences, the domination of idealism in this battle has been assured hitherto by the acceptance by both parties in it of an empiricist-idealist theory of knowledge. The immediate objective of such struggles is the development of the sciences, but the ultimate aim is to ensure or undermine the continuing dominance of the dominant ideology, the ideology of the ruling class. Hence the emergence of a science is a political event, and the struggle against its ideological resorption is a political struggle, a struggle for materialism against idealism, wherever and whenever it occurs. However, this is not to deny that the emergence of different sciences have different effects in philosophy, and that these are more or less directly political. This is most especially true of historical materialism. The philosophies of history which it made impossible by its epistemological break, philosophies resumed in Hegel's philosophical system, are especially important in that they contain directly a theoretical space for the empiricism-idealism couple so basic for the transhistorical practical ideology of all social formations. Hegel's Philosophy of History is incomplete without its pendant Logic and Phenomenology. Hegel, like all historicists, idealists and empiricists, assumes that the subject of historical science is the subject of the historical process itself. The constitution of historical materialism with its concept of the social formation as a process without a subject, (23) as a complex structure in dominance, demanded a new epistemology, like any other science. But at the same time, it demanded a new conception of philosophy as an instance in which ideologies and sciences are represented alongside politics in a Kampfplatz and not as a general 'theory of knowledge'. (24) It is no longer possible to justify externally the epistemological procedures of the new science by a modification in the basic ideological schema of subject and object; this schema itself is ruled out, leaving only the struggle for and against the new scientific epistemology, the struggle between materialism and idealism as materialist philosophy draws the demarcation at present, a struggle which is essentially political.

It is a struggle between materialism and idealism because the new scientific practice, the new dialectic of rational development and phenomeno-technics, has constructed and demonstrated in its practice the existence of a new form of matter, while its rejection on philosophical grounds literally asserts the claims of thought against matter, claiming that the new matter cannot exist if it cannot be thought according to the present criteria of thought as laid down by philosophy, and its resorption by so-called philosophical 'foundation' merely grants the new form of matter the privilege of membership of the society of thinkable objects on condition that it creates no disturbance in the world of respectable and respected truths. It is a political struggle because the science

materialism defends against idealism threatens the unity of the ideological world which assures the dominance of the ruling ideology, and because the struggle against the idealist resorption of the science is a struggle to ensure that these disruptive effects are not neutralized by the domination of idealism in philosophy. In the specific case of the emergence of historical materialism, this political character is tripled: the destruction of the ideologies of history threatens the ruling ideology at its very heart, the justification of the economic and political status quo; the destruction of the empiricism-idealism couple makes possible a philosophy, dialectical materialism, which knows its own political character; and historical materialism and dialectical materialism together for the first time make possible a scientific political practice on the basis of the unification of the concrete analysis of a concrete situation with the strategic class positions of the proletariat. (25)

These theses are of necessity summary and dogmatic in form. However, they do enable us to answer the question raised at the beginning of this article. Althusser is not comparable with any of the revisionists either in his attitude to Marx or in his attitude to the bourgeois philosopher from whom he has learnt. Althusser does not dogmatically accept the texts of Marx and Engels, but neither does he simply reject Marx's philosophy, substituting a more fashionable one in its place, as the revisionists do. On the contrary, he reads the texts of Marx and Engels as the effects in discourse of a scientific practice, and Marx's philosophical and methodological statements as the first philosophical effects of that practice, as Marx's indications of the novelty of his scientific practice in the field of history and his recognition of the revolutionary epistemological consequences of the scientific revolution he had initiated. On the other hand, Althusser does not dogmatically reject Gaston Bachelard as a bourgeois philosopher, but neither does he substitute his philosophical system for the one supposedly lacking in Marx and Engels, as the revisionists do. He reads this philosophy as a materialist, as Lenin advised Marxists to read Hegel, accepting it and applying it insofar as it is a defence of the sciences against the claims of traditional (idealist) philosophies, i.e., insofar as it is a materialist philosophical practice, but criticizing and replacing the ideological effects in it of Bachelard's assumption that epistemology only relates to the sciences and not also to politics, his ignorance of the historical-materialist theory of ideology and the resultant psychologism and ideological philosophical resorption - errors which derive in the last instance from Bachelard's petty-bourgeois class position. Althusser is not a Bachelardian Marxist but a materialist friend of Bachelard. The philosophical tasks Althusser's work has set us is not the exegesis of a Bachelardian philosophy, but the development of a Marxist-Leninist one, from Marxist-Leninist texts, with all the help we can obtain from Bachelard and from any other bourgeois philosophers.

Notes

1. Louis Althusser: Lenin and Philosophy and Other Essays, NLB 1971, pp 30-31.
2. Cf. Theoretical Practice Number one, pp 15-16.
3. A failure to grasp this fact is the commonest basis for criticisms of Althusser's position, even where, as in the case of a recent article by L. Callender in The Irish Communist (July 1971), a serious attempt is made to read something Althusser has written. Apparently through ignorance of the rest of Althusser's works, Callender fails to grasp the nature of the critique of Marx's statements as to the relationship between himself

and Hegel that Althusser makes in Contradiction and Overdetermination; he is thus reduced to repeating these same statements against Althusser and imputing his failure to accept them at face value to his non-proletarian class situation.
4. Cf. Gaston Bachelard: Le Nouvel Esprit Scientifique (Paris PUF 1968 - first edition 1934), pp 12-13, and La Formation de l'Esprit Scientifique (Paris Vrin 1965 - first edition 1938), p 61: 'Science realises its objects without ever finding them already in existence. Phenomeno-technics extends phenomenology. A concept has become scientific insofar as it has become technical, insofar as it is accompanied by a technique for its realisation'.
5. Cf. Barry Hindess's article in the present number of Theoretical Practice and Alain Badiou: Le Concept de Modèle (Paris Maspero 1969).
6. La Formation de l'Esprit Scientifique, op. cit., p 13.
7. Ibid., p 15.
8. See the translator's note to Lecourt's article in this number of Theoretical Practice.
9. Cf. Georges Canguilhem: Etudes d'Histoire et de Philosophie des Sciences (Paris Vrin 1970), pp 196-207.
10. 'Realism and rationalism are in constant negotiation. Neither is enough by itself to constitute scientific proof; in the realm of the physical sciences there is no place for an intuition of the phenomenon indicating at one stroke the foundations of the real; nor is there place for an absolute and definitive conviction imposing fundamental categories on our methods of experimental research' (Le Nouvel Esprit Scientifique, op.cit., p 9).
11. Gaston Bachelard: The Philosophy of No (English translation New York, Orion Press 1968 - first French edition 1940), p 36.
12. Cf. Gaston Bachelard: 'L'Actualité de l'Histoire des Sciences', Editions du Palais de la Découverte, (Paris, October 1951) and Fichant's article in this issue of Theoretical Practice.
13. Cf. Dominique Lecourt: 'De Bachelard au Matérialisme Historique', L'Arc no.42, 1970, pp 12-13.
14. Of course, a large part of La Formation de l'Esprit Scientifique deals with a social obstacle to science, the fashion for electrical toys and games in eighteenth-century Europe, but this is never theorized as such; it is seen as the social extension of a psychological obstacle and as the failure to institutionalize scientific activity socially in specifically scientific institutions which would control psychological speculation and enable the obstacle to be overcome.
15. Mary Ann Caws: Surrealism and the Literary Imagination (The Hague, Mouton 1966). Caws is wrong, however, to imply that Bachelard's shift from condemnation of revery as an obstacle to science to praise of revery as poetic activity was accompanied by a relaxation of his hostility to the imagination in science (ibid., p. 16). As François Dagognet remarks in his book Gaston Bachelard, sa vie, son oeuvre (Paris PUF 1965), p. 59: 'Bachelard never ceased to deepen the opposition between the two worlds of science and onirism, so much so that these universes came to correspond negatively to one another'. This is not the place for an analysis of Bachelard's poetics; suffice it to say that it suffers from the same defect as his scientific epistemology - the lack of an adequate conception of ideology. For Bachelard, revery, the day-dreaming of imagery, is poetic activity par excellence. The poet's job is to give free rein to his mind as it drifts from potent image to potent image. (This serves once again to distinguish Bachelard's 'psycho-analysis' from Freudian psycho-analysis. Bachelard's attempts are

all to explain the psychic power of the images of reverie, Freud's to reveal the psychic forces behind the apparent trivia of the content of (night) dreams: 'A good deal of the contempt in which dreams are held is due to the preference... shown in their content for what is indifferent and trivial. Analysis does away with the misleading appearance upon which this derogatory judgement is founded. If the content of a dream puts forward some indifferent impression as being its instigator, analysis invariably brings to light a significant experience and one by which the dreamer has good reason to be stirred', On Dreams, Standard Edition, Vol. V, p 656. In analysing poetry as day-dreams, Bachelard attempts to locate the power of an image by reducing it to its archetype. In analysing (night) dreams, Freud attempts to explain the occurrence of trivial dream content in relation to a powerful experience. Bachelard's inclination towards Jung rather than Freud is unmistakeable.) Hence Caws' comparison with Breton's surrealism. But if surrealism is reduced to a repertory of images, it becomes a kind of expanded neo-classicism. The real contribution of surrealism to a scientific aesthetics is its stress on the unconscious element in aesthetic activity, i.e., its decentring of the artist to the function of a support for an aesthetic practice. Art thus becomes not a voyage through and in ideology, but a practice on it, as science is, but in a different way. Cf. Althusser's 'Letter to André Daspre' and 'Cremonini, painter of the abstract' in Lenin and Philosophy and Other Essays, op. cit., and Pierre Macherey: Pour une Théorie de la Production Littéraire (Paris Maspero 1966).

16. For example, Claude Lévi-Strauss; cf. Maurice Godelier: 'Histoire et Pensée Sauvage', Annales 1971, p 553; translated in New Left Review 69, Sept-Oct, 1971.

17. Cf. Georges Canguilhem: 'Founding the objectivity of rational knowledge on the union of experimental workers and the validity of rationalism on the consistency of a co-rationalism; founding the fertility of my learning on the division of the ego into an ego of existence and an ego of super-existence, i.e., of co-existence within a cogitamus, is as a whole an ingenious attempt, filled with conviction, but not wholly convincing' (Etudes, op. cit., p 205).

18. The impossibility of philosophical abolitions of philosophy, which are, of course, by no means confined to Bachelard, is the central theme of the work of Jacques Derrida, particularly his De la Grammatologie (Paris Miniut 1967). Cf. also Althusser's comments on the 'vulgar materialism' of Lévi-Strauss in Lenin and Philosophy and Other Essays, op. cit., pp 59-60.

19. Cf. Louis Althusser: 'Ideology and Ideological State Apparatuses' in Lenin and Philosophy and Other Essays, op. cit.

20. Michel Pêcheux: 'Idéologie et Histoire des Sciences,' in Fichant and Pêcheux: Sur l'Histoire des Sciences (Paris Maspero 1969), pp 40-41. Cf. also the articles by Thomas Herbert in Cahiers pour l'Analyse numbers 2 and 9 (1966 and 1968).

21. Cf. Georges Canguilhem: Le Normal et le Pathologique (Paris PUF 1966), p 175.

22. V.I. Lenin: Collected Works (London Lawrence and Wishart 1960-), Vol. 14, p. 358.

23. In fact, paradoxically enough, Marx was able to get this concept from Hegel by a materialist reading. Cf. Louis Althusser: 'Lenin before Hegel', Lenin and Philosophy and Other Essays, op. cit., and 'Sur le rapport de Marx à Hegel' in Hegel et la Pensée Moderne, ed. Jacques d'Hondt, Paris PUF 1970.

24. In a paper given to Jean Hyppolite's seminar in January 1968, Althusser defines the new philosophy in the following terms: 'This rejection (of the traditional philosophy of knowledge - BB) leads to a new conception of philosophy - and not just a new conception, but also a new modality of existence, what I shall call a new practice of philosophy: a philosophical discourse which speaks from somewhere else than classical philosophy did. To make this comprehensible, let me invoke the analogy of psycho-analysis. (1) The point is to carry out a displacement = to make something move over in the internal disposition of philosophical categories. (2) Such that philosophical discourse changes its modality - speaks differently, which creates the difference between interpreting the world and changing the world. (3) Without philosophy disappearing nonetheless.
'Apparently it is the most conscious discourse there is. In fact it is the discourse of an unconscious. The point is no more to suppress philosophy than it would be to suppress the unconscious in Freud. What is required is, by working on the phantasms of philosophy (which underly its categories), to make something move over in the disposition of the instances of the philosophical Unconscious, so that the unconscious discourse of philosophy find its place - and speaks at the top of its voice about the very place assigned to it by the instances which produce it.' ('Sur le rapport de Marx à Hegel' op. cit., p. 97).

25. For the relationship between philosophy, politics and theory, cf. Louis Althusser: 'Philosophy as a Revolutionary Weapon', Question 5, Lenin and Philosophy and Other Essays, op. cit., pp 21-22. Crucial here, too, of course, is the whole of 'Lenin and Philosophy' in the same volume, a text of which the present article is no more than a commentary.

The Idea of a History of the Sciences

by MICHEL FICHANT

I. The Problem of the History of the Sciences

In giving the title 'the problem of the history of the sciences' to this lecture intended for an audience of research scientists, my aim has simply been to draw attention to a paradoxical situation which suffices to show that the history of the sciences is not a matter of course: this situation is the same as the one we know in which philosophy pretends to provide the sciences with the forms of their progressive constitution, of their development.

'Problem' is to be understood in an objective sense which is signalled by the use of the word in the singular. It is not a question of the problems which the history of the sciences may pose to those who seek to practice it: but it concerns the very existence of a history of the sciences, and consequently, the existence of a distinct practice which corresponds to it in the world of theoretical practices.

This 'problem' can be refracted through a certain number of distinct and convergent questions.

For whom is there a history of the sciences?

For whom does this history create a problem? 'for whom', that is to say: in what space of theoretical discourse, as a function of what real state of this discourse? To pose such questions is already to estimate that there is a certain relation of proximity (voisinage) between the history of the sciences and philosophy: that is, the problem posed is that of its interest rather than its object; (2) interest to be understood in an objective and systematic sense: every theoretical discipline must correspond to what Kant calls an 'interest of reason'.

Similarly, there is the question, deliberately using a term just as vague as the term proximity used above:

In what does the history of the sciences 'affect' science? Inversely: in what respect is the history of the sciences affected by science?

I shall situate this question by beginning with the fact of the indifference of scientists to the history of the sciences. There are certainly exceptions: anyone who has read Bourbaki's Elements D'Histoire Des Mathématiques or Van de Waerden's Erwachende Wissenschaft among others, will know this. These exceptions pose, in passing, the problem of their existence, - and of what that existence bestows on the concept of the history of the sciences. But they remain precisely exceptions in the midst of a general, average indifference.

Hence a last question: does this factual indifference signify a difference in principle, a polemical difference between the concept of science and the problematic concept of the history of the sciences, - and what would be constitutive of the latter?

A certain number of discourse lay claim in fact to the appellation 'history of the sciences': there are no grounds for looking for the concept other than in the critics reading of these discourses. Let us distinguish them, at least provisionally in respect of their origins, measured by the theoretical formation of their authors. We should then find the scientists' history already evoked, a philosophers' history which contains both the paradigmatic employment of the history of the sciences to the ends of a philosophy (from Kant - Preface to the second edition of the Critique - to Brunschvicg), and the constitution of the history of the sciences as a philosophical discipline (Comte, Bachelard, Koyré), and lastly a history which, for want of something better, we shall call the history of historians of science when the latter claim self-determination and independence with respect to the scientists, as to the philosophers (Sarton and all the Anglo-Saxon authors).

Is this difference founded? How are these three histories articulated with one another?

A. The History of the Sciences Practised by Philosophers

Like every 'philosophical history' this history began in the 18th century; it arose from a generalisation of the practice of history understood in the sense of the collection of documents and 'memoirs' for the purposes of the conservation of contemporary science (cf. in this respect Fontenelle's work in the permanent Secretariat of the Académie des Sciences from 1699). The Academies, the Societies, the scientific journals consolidated the present of learning (a perpetuable dated memoir): henceforth the same function was to be exercised with respect to the elements from which learning proceeds, even if this meant by separation from them.

The consciousness of living a revolution of science was founded on the fact of the double triumph of Newtonian physics and infinitesimal mathematics: in both cases the 18th century believed it possessed the refutation of cartesian philosophy: Newton's physics against the fiction of vortex physics, - infinitesimal analysis against the limitations of algebraic geometry.

However, it was that philosophy which continued to justify this consciousness of revolution. (3) This is apparent firstly in the character of a history of the sciences understood as verification of a philosophy of progress. Whatever the model of the idea of progress (cf. below) it presupposes the unity of science and the uniformity of its development. But the rupture from which the new physics, the new cosmology, the new mathematics arose could not be circumscribed, it put the totality of learning at risk, because there was a unity of all the parts of this learning, founded on the unity of the knowing mind: the whole of the 18th century reiterates the statement of the First of the Règles pour la Direction de L'Esprit: the unity of the mind founds the unity of learning.

However, even more profoundly, the consciousness (even in part illusory) of a reawakening and of a recommencement of science simultaneously involved the will to conserve its effects (and thus the practice of the 'memoirs' and the collections of documents) - and a consciousness of the past as such whose permanence tradition could no longer maintain: that is the condition of possibility of a history as the narrative of an orientated development. It was certainly from cartesian philosophy that the theoretical formulation of this rupture was drawn in the 18th century even if it seemed that it was an anti-cartesian science which accomplished it practically.

The idea of progress, as a law of the development of learning adds to the two pre-suppositions already indicated (unity of learning, uniformity of its course) that of a solidarity between the history of science and general history, which allowed models and examples for the latter to be discovered in the former. Thus understood, progress was expressed according to two representations, most frequently interwoven, as accumulation, or as evolution or development. Whichever of the two is referred to, a Cartesian example will invariably be found.
(a) The model of Accumulation: the unity of learning is that of a spatial scheme, of an empty space to be filled in by successive additions: 'For such is the nature of truths', writes Fontenelle, 'that they are always prepared to receive other truths among them, and to leave them, as it were, places which they have only to come and take'. (4)

There is simultaneously chance, in that the schema is undifferentiated, and pre-adaption, since every truth not yet known has its already designated place, to which the space is an aspiration. The unity of learning is that of a mechanism which completes itself by complicating itself, the complication being only a filling in, the completion of lacunae, the inscription of blank spaces.

A Cartesian illustration: the chain (catena scientiarum), with the qualification (which is important) that it is no longer a question of proceeding through it in order, but adding absent links as they present themselves.
(b) The model of evolution was formulated in two languages: - in a ' cosmological' language which attributes a sort of force of attraction to concepts and truths which associates them in a system. (5)
- in a 'biological' language in which evolution must be understood as the development of what is pre-formed or pre-enveloped. Progress is then the bringing to light of what was contained in a germ. History, like the living, can know neither metamorphoses, mutations, crises, nor innovations. (6)
A Cartesian illustration: the seeds of truth which it is sufficient to leave to their spontaneous momentum, with the qualification that the consciousness of obstacles is weakened by the guarantee of success.

In both cases, in both models, history was written as a table (tableau): the succession is entirely visible in a single perspective, or at least re-visible: the table is a review of discoveries and progress. In the table there is simultaneously a co-presence of the elements and themes and a homogeneity of the frameworks: co-presence and homogeneity - once again we have the classical concept of history which was to find its speculative fulfilment in the Hegelian concept as it has been restored by Althusser (Reading Capital Part 2).

More precisely: on what is based the homogeneity and the co-presence of the moments one in relation to the others in this history? It is the fact that there is simultaneously a unity of learning and a homogeneity of learning with nature. Whatever model may be chosen to characterise the progress of the sciences, this progress is always represented as a profound harmony with a natural order of things. The course of history is defined by the necessary sequence and the solidarity of the parts of a Nature which forms a whole. Fontenelle: 'All nature is one and it is everywhere in the same disposition.' D'Alembert: 'For he who could encompass it in one glance, the universe would, if I may dare say so, be no more than one unique fact and one great truth'.

In the Histoire de L'Astronomie Ancienne (1775) Bailly

defined the two limits defining the field of action of history (7): the beginning of the sciences is the moment when we divide up the variety of nature in order to study its 'riches' separately. The distinction between the different sciences is engendered by our inability to consider the universe in its entirety at the outset. But, in order to be fully explained, the smallest phenomenon must be envisaged in all the perspectives of the diverse sciences simultaneously: 'There is no astronomical phenomenon which does not belong at the same time to all these sciences ... To analyse in order to comprehend (connaitre), to re-unite what we have separated in order to imitate or to describe nature, that is our route.'

The second limit is constituted by the ideal order in which we present a constituted science when expounding its 'elements', in other words, the necessary sequence of the truths which compose it from the simplest to the most complicated. This order is simultaneously distinct from nature, of which it is only an imitation, and from the history of our discoveries, which follows a 'contrary order', the complication here preceding the simplicity which is only discovered little by little. To say that the order of the history is contrary to that of the elements is not to say that it is the same order gone through in reverse: for the elements form a 'continuous chain' while in the history 'nature does not in any way develop in sequence with our observations: it allows itself to be perceived intermittently and in parts'. Moreover, the elements eliminate tentative experiments and errors, while the history must relate such difficulties and checks.

In this way the field of history and the principle of its progress are delimited: between the infinite variety, the countless wealth of nature, which escape us and which we divide up in order to bring it into proportion with our perspectives, - and the regular and methodical order of the 'elements' (8); the history describes this oscillation of learning divided and separated into distinct sciences because separated from the Nature which is its unique object, between the profound and elusive unity of Nature and the transparent and abstract unity of the elements.

It is important for our purposes to stress at this point that the Comtian theory of the history of the sciences takes up and sytematizes the same elaboration of the concept of progress, for which it aspires not only to give models but also to reveal the governing principle (loi). In France at least, the history of the history of the sciences is in large measure inseparable from the history of positivism. (9) At the end of the 19th century a historian as scrupulous as Paul Tannery, the details of whose work appear rather indifferent to any concern with philosophical generalisation, was insistant to situate himself expressly under the patronage of the positivist doctrine, understood as a strict fidelity to its founder. (10) However, Comte's work belongs to the 18th century precisely insofar as it perfected its essential themes, which means simultaneously gave them finished form and ultimate systematicity and rejected them as no longer contemporary.

By conceiving progress as evolution, that is to sayas the actualisation of a virtuality, Auguste Comte did indeed intend to found the possibility of the history of the sciences on the consciousness of the fact that where the system of knowledge is concerned positivism completes the development which carries each of our knowledges in its time to a positive state. Indeed only the foundation of the ultimate scientific discipline - sociology or the theory of humanity - allows the history of the sciences to be situated in its true place, because it at last reveals the unity of science.

In the letter which he addressed to Guizot in 1832, calling for the creation of a chair of the history of the sciences at the Collège de France, Comte is perfectly explicit on this point: 'It is only in our time that such a chair could fittingly be established, since, before our age, the various fundamental branches of natural philosophy had still not in any way acquired their definitive character and had not manifested theirnecessary relations .... In this state of our intelligence, human science insofar as it possesses positivity can be envisaged as a unity and consequently its history can be conceived from then on. Impossible without this unity, the history of the sciences tends reciprocally to render scientific unity more complete and more tangible.' (11)

The history of this unity is then valid as a principle of classification: the development of learning is the temporal manifestation of the classification possible solely at its end, since each science accedes to positivity in its time, according to the degree of complexity of its object. Thus it can be said that there is simultaneously:
- a presence of history in every instantaneous section: the synchrony makes contemporaneous states which are not of the same epoch, sciences which in relation to their own peculiar temporality are not contemporaneous with one another.
- teleology towards a moment of effective co-presence, since as each science tends towards its positivity, they all tend together towards an epoch in which they will all be equally positive. It is this epoch which marks the moment when the history of the sciences is possible.

This remark can be clarified by analysing the function fulfilled by the distinction between the order of historical exposition and the dogmatic order in the Deuxieme Leçon of the Cours de Philosophie Positive (12): the former presents successively the knowledges in the order in which they were obtained, whereas the latter systematises them from the point of view of a mind of today, 'concerned to recreate science in its totality'. Two fundamental clarifications demonstrate the new utilisation of this classical opposition:

(1) a science at its beginning knows no other order of exposition than the historical: a Greek geometer learnt his science 'by the successive study of the very small number of original treatises produced up to that time.' In contrast, for a science in its maturity, the historical order becomes impracticable and the dogmatic order simultaneously possible and necessary, through the abbreviation effect produced by the reworking of previous discoveries from the point of view of new conceptions.

(2) but the study of a science according to the historical order has nothing to do with the knowledge of its real history. The historical order is triply abstract: it separates the various parts of each science; it separates each science from other sciences; it separates the progress of the sciences from that of techniques and from social history. Inversely, real history must not only demonstrate, as we have seen, the unity of science, but also integrate the history of the sciences into that of humanity: 'Hence, it follows that the true history of each science, in other words the real formation of which it is composed, can only be known by studying, in general and direct manner, the history of humanity.' (13) Two conclusions follow from this:

- the first is 'that a science cannot be fully known as long as its history is not': it is necessary to understand thereby that the history of a science is only possible when it can and must be presented, along with all the others,

according to the dogmatic order, the historical order having become impossible. A science detached from its history, such is the condition of a real history of science.

- in the second place 'this study must be conceived as entirely separated from the particular and dogmatic study of the science, without which even this history would be unintelligible'; the true place of the history of the sciences lies in the presentation of the human history of which it 'constitutes the most important part.'

But of what will there be a history? A science at its beginning is confused with its history while a mature science separates itself from it and identifies itself with a system which, in accordance with the regularity appropriate to the positive state must now extend itself by successive acquisitions and accumulations. If there is no history except a history of something in itself incomplete, there can only be a history of the pre-scientific, the theological or the metaphysical: the development of a positively constituted science is closed within the enclosed unity of definitively founded learning. Or it is necessary to say that what is historical in science is what forbids it from having an autonomous development: the development of a science is not dissociated from the history of practical ensembles and social formations: and hence the narrative of this development only escapes this science to be integrated into another, that is to say into sociology.

The philosophical history of the sciences knows other roads than these dead-ends taken by Comte. It is then a question of finding in the science the confirmation of a philosophy (which may as easily be that of Meyerson as that of Brunschvicg). Such a history is a logical history in which the succession is a revelation of the mind to itself. It is also a critical history, which judges and selects, defines strong and weak points, regressions and break-throughs. There is, therefore, a scansion of historical development, but one which receives its norm from a philosophy which only esteems itself as the logical result of history in order the better to reject all real confrontation with it. The theories and the concepts of science are only expressions of the mind or intelligence, the unique and fictional personage of an apologetic history.

We shall come back on the history of the philosophers through the analysis of Bachelard's contribution later in this exposition.

B. A Scientist's History

I have already indicated that the history of the sciences might on occasion be practised by the scientist himself. Bourbaki's admirable Elêments d'Histoire des Mathématiques bears witness to what an exemplary success can be in this domain. However, my purpose is to pursue a different analysis here, aiming to show the difficulties of a history of the sciences created by a scientist when his approaches are determined either by a spontaneous scientist's philosophy or even by an elaborated philosophy which he adopts for himself, rather than by his actual scientific practice.

To my mind that is the significance of the considerable historical work of Pierre Duhem (1861-1916). Its quality derives not only from the extent of the information it brings to bear, but also and above all from the mastery which the scientific culture of the author bestows on his exposition. However, the objective which inspires this history, and which founds its relevance for Duhem himself is philosophical, ideological and apologetic through and through.

All the work Duhem devoted to the study of ancient and

mediaeval science was aimed at the elimination of the myth of the Renaissance, of the constitution of classical scientific knowledge ex nihilo through the rejection of religious philosophies and theology. Hence he has to establish that the concepts for which modern science is honoured had been stated, formed, preconstituted by the Middle Ages; this aim, which is Duhem's intention as a historian, is stated in the preface to Origines de La Statique, and as it should be, in the form of a conclusion and of a result:

'The study of the origins of Statics has led me ... to one conclusion; the further and the more varied directions in which I pushed ahead my historical researches, the more this conclusion imposed itself on me with growing force; hence I therefore venture to formulate it in its full generality: mechanical and physical science, which is justly the pride of modern times, descends by an uninterrupted sequence of barely perceptible improvements from the doctrines professed in the schools of the Middle Ages; the so-called intellectual revolutions have most often merely been slow, long prepared evolutions; the so-called renaissances merely reactions, often unjust and sterile ones; the respect for tradition is an essential condition of scientific progress'.

A first thesis directly concerning the history of the sciences can be drawn from this text: there are neither revolutions nor ruptures in this history.The history of a science is an evolution and this evolution is slow: thus Mechanics:'The development of Mechanics is, therefore, really an evolution; each of the stages of this evolution is the natural corrollary of the stages which have preceded it, it is pregnant with the stages which will follow it';'The tree of science grows extremely slowly' (L'Évolution De La Mécanique, 1903, pp 346 and 3). This characterisation of the evolution given by Duhem provides the historian's working procedure: to demonstrate what is the natural fruit of the past in a scientific work of a given epoch, - and also, to indicate the germs of the future.

The thesis is presented in the work which Duhem devoted to La Théorie Physique, Son Objet, Sa Structure (1906): hence it does not state an accidental aspect of the development of science, it is directly linked to the very characterisation of the notion of scientific theory. Its proof occupies section 2 of chapter VII of Part II of the book: 'History shows us that no physical theory has ever been created out of whole cloth. The formation of any physical theory has always proceeded by a series of retouchings which from almost formless first sketches have gradually led the system to more finished states ... A physical theory is not the sudden product of a creation; it is the slow and progressive result of an evolution' (The Aim and Structure of Physical Theory. Atheneum New York 1954, p. 221).

The opposition creation, evolution is specified in a study of the formation of the theory of universal attraction: 'We can follow the slow and gradual transformations through which the theoretical system evolved; but at no time can we see a sudden and arbitrary creation of new hypotheses' (p. 252). Presented in this way the alternative reveals everything fallacious in it, as if the choice was between an incoherent succession of instantaneous and unfounded innovations - and the slow, tranquil course of continuity. The fact that scientific revolutions must be prepared, that they follow the maturation of an epistemological situation and its decadence, is what they have in common with all revolutions; furthermore, there is no arbitrary decision in them, but a necessity which, far from precluding innovation, on the contrary calls for it and founds it as

such. (14)

Duhem adds two specifications to his thesis: the acceleration of the course of history does not erase its continuity, it simply 'condenses' the evolution (p. 253). On the other hands, if there is no revolution, nor is there an origin of scientific knowledge which would signify an initial rupture. In another language: if there is no recasting in the course of the development of a constituted science, neither is there a rupture instituting science. The internal continuity of the history of science also implies the continuity of scientific knowledge and everyday knowledge. (15)

However, it is the actual use made of the thesis of continuity rather than its abstract statement which makes possible the determination of its epistemological significance. In fact, history written in the style of continuity will be a search for precursors. It is no accident that Duhem is principally concerned with the formation of Copernicus' system and with seeking precursors of Galileo (cf. Tome 3 of his Études Sur Léonard de Vinci). What is really at issue? To show that Copernicus' cosmological system is the result of a slow sequence of transformations which 'gradually' prepare for it; and that Galileo, too, is a continuer who has been presented as a founder by a false conception of history.

Thus on Galileo's contribution to Dynamics:

'If all the historians of Mechanics are to be believed, in the Discorsi Galileo had overturned the bases of Peripatetic Dynamics from top to bottom in order to raise modern Dynamics on new foundations: but, in these same Discorsi, he borrows a lemma from a statics which takes as its principle Aristotle's axiom, and this lemma is not intended to prove some accessory and unimportant theorem; its object is the demonstration of a proposition which Galileo regards as 'the most essential theorem' for the establishment of the science of movement he proposed' (Origines de la Statique I, 260).

The lemma in question is the law which postulates the equality of velocities obtained by bodies falling from a certain height, whatever the inclination of the fall (free fall or fall on an inclined plane). Galileo, Duhem tells us, refers for the justification of this lemma, in the Third day of the Discorsi, to the pamphlet Della Scienza Mechanica, which takes as its axiom Aristotle's principle according to which the speed of bodies is proportional to the force which causes their movement.(16) Similarly, the central notion of momento, 'as it is conceived by Galileo is an idea still completely impregnated with peripatetic Physics'. Furthermore, when the Parisian philosophers of the 14th century say that the impetus which maintains the movement of a body is eternally conserved they thus prepare for the statement of the principle of inertia under a different formulation, etc.

Hence the conclusion: 'the opinion which makes (Galileo) the creator of modern Dynamics is a spurious legend' (Origines De La Statique P. 261).

For in fact the thesis of historical continuity rests on a more profound thesis, a thesis which involves the nature of physical theory; this thesis in its turn responds to the need to derive from science a conception of physical explanation which conserves the rights of metaphysics and the legitimacy of a religious philosophy. Duhem the 'Christian scientist' intends to show that science is compatible with the metaphysics which he professes (and why not, if it gives him good reasons), as Duhem the Christian historian intends to establish the good cause of the Catholic Church

in the history of science. His article of 1905, Physique de Croyant (reprinted in the second edition of La Théorie Physique and in The Aim and Structure of Physical Theory) expresses this project very clearly.

In this article, Duhem recalls his epistemological thesis about the nature of physical theory: 'What is a physical theory? A group of mathematical propositions whose consequences are to represent the data of experiment: the validity of a theory is measured by the number of experimental laws it represents and by the degree of precision with which it represents them, if two different theories represent the same facts with the same degree of approximation, physical method considers them as having absolutely the same validity; it does not have the right to dictate our choice between these two equivalent theories and is bound to leave us free' (Aim and Structure, op. cit. p. 288).

Thus, the sole criteria of choice is the simplicity and the convenience of the hypothetical order of the theory, not its conformity to an unknowable ontological order. For either the theory agrees with the totality of our experiments, but that does not prevent another theory agreeing just as well, - or else it does not agree, but in this case the discordance cannot be located: all the hypotheses of the theory are solidary and thus cannot be verified or falsified separately.

Here, again, my aim is not a direct examination of Duhem's epistemology, in which it is easy to recognise the habitual themes of formalism and empiricism, what could be paradoxically designated a metaphysical positivism. Duhem is a positivist in his conception of science in order to provide a place for his metaphysics.

Rather, what is important for me is to demonstrate the consequences of this thesis in the practice of the history of the sciences, and here too we must return to Duhem's assessment of Galileo's work. For Duhem has written under the title Sozein ta phainomena, Essai sur La Notion de Théorie Physique de Platon à Galilée a work which makes the link between his epistemology and his history of the sciences: it involves both a reprise of history at the level of the norms of explanation which scientific theories propose, and a confirmation by history of the systematic book on Physical Theory. Thus a history whose theme is epistemological comes and duplicates the history of scientific theories and in some sense controls it.

How is this control achieved? Duhem's goal in this work is to show:

- that his conception of physical theory corresponds to a tradition which has been elaborated à propos of astronomy since Antiquity.

- that the norms of physical theory have always been the same throughout the history of science, even if it is the case that certain scientists misinterpret the significance of the theories which they produce.

It can be said on the first point that Duhem finds his exact formulation of what a physical theory is in a text by Geminus (1st century B. C.) which commentators of Aristotle relate (op. cit. p's 9 - 10) (18)

Geminus distinguishes between first principles which concern the real nature of things and which are the object of Physics and secondary constructions which are the geometrical representations of the apparent and observable order of these same things, and which constitute the concern of Astronomy. The astronomer receives his principles from the Physicist and has as his task, within the framework set by the latter, to find geometrical lines composed from circles which will render the appearances correctly, which 'will save the phenomena'. These hypotheses of the Astronomer are neither true nor false; once they are compatible with principles posed by the Physicist (and which do have pretension to truth), and once they permit calculation, they are legitimate. Thus, several mathematical hypotheses are possible, while the essence of things is unique. The Physicist postulates that the World is spherical, that the Earth is immobile at its centre: the Astronomer then shows that an account of the movement of the planets can be given by diverse combinations, of eccentrics (in this case one says that the Planet is carried in a great circle whose centre is itself in rotation around the Earth), or else of epicycles (in this case one says that the Planet moves on a small circle itself borne along on a circumference whose centre is the Earth). No choice can be made between these two conceptions which equally account for the same observations.

Duhem considers the role attributed to the astronomer by Geminus is the one which belongs for him to physical theory as he conceives it. And like Geminus he considers that beyond theory, there is room for another knowledge, which is more than a convenient classification schema, and relates to the reality of the order of things. The distinction between the Astronomer and the Physicist becomes in Duhem the distinction between science and metaphysics, or physical theory and cosmology:

'By asserting that physical theory tends towards a natural classification in conformity with the order in which the realities of the physical world are arranged (the physicist) has already exceeded the limits of the domain in which his methods can legitimately be exercised; all the more reason why this method cannot disclose the nature of this order, or tell what it is. To make out the nature of this order exactly is to define a cosmology; to display it to us is to expound cosmological system; in both cases it is doing work not essential to the Physicist but to the metaphysician' (Physique de Croyant op. cit. p. 229).

Better still: Duhem adds that the Cosmology which is rendered most acceptable (by analogy, not proof) by the most perfect Physical theory (for him, Thermodynamics) is Aristotle's cosmology, reduced to its essential affirmations' and, 'rid of the outworn demoded scientific clothing covering it' (ibid p. 310 cf. L'Evolution De La Mecanique p. 345). This completes the circle of the demonstration: the ideal of physical theory has always been the same from Geminus to Thermodynamics and the Cosmology most compatible with the state of the theories has also always been the same, including, as we have seen, and despite anything he himself may have said about it, in Galileo.

We verify once again the idea that Duhem's positivism and formalism with respect to physical theory are the counterpart of his metaphysics: it is a matter of delineating the limits and conditions of scientific learning in order to subordinate it to another learning. (19)

From this Duhem can go on to pronounce on the work of Coperincus and on that of Galileo. The philosophy which comes to intervene beside the sciences as the guardian angel of Christian ideology can now intervene in the history of the sciences to denounce the lapses in it from conception of physical theory. The major lapse is realism.

Copernicus was a realist in affirming as a cosmological truth the movement of the Earth around the Sun. Against him, his editor Oslander was correct to recall that the

astronomer cannot make pronouncements at this level. In the preface he added to On the Revolutions of the Celestial Orbs he was correct to write:

'Now, as different hypotheses are sometimes offered to explain one and the same movement (thus for the movement of the sun, eccentricity and the epicycle), the astronomer will adopt by preference that one that is easiest to understand. Perhaps the philosopher will stress its verisimilitude more; however, no one can attain or teach anything certain unless it has been revealed to him by God. Let us therefore allow these new hypotheses to be made known among the old ones which are in no way more probable than they, especially as they are both admirable and simple and bring with them an immense treasure of the most scientific observations. And let nobody expect anything certain from astronomy about these hypotheses since astronomy can give us nothing of this kind....' (trans. Koyré pp. 29 - 30).

Galileo is realist: Cardinal Bellarmino is right as against him when he writes to one of his supporters: 'It seems to me that our Holy Father and Signor Galileo would be acting prudently if they were content to speak ex suppositione and not in an absolute manner ... To say that by supposing that the Earth is in movement and the Sun immobile all the appearances are saved more satisfactorily than they could be by eccentrics and epicycles is certainly quite right; that presents no danger at all and is enough for the mathematician. But to wish to affirm that the sun really remains immobile at the centre of the World, that it turns solely on itslef, without running from East to West, that the Earth occupies the Third Heaven, and that it revolves with great speed around the Sun, is something dangerous; it threatens... to prejudice the faith' (cit Duhem, Sozein ta phainomena, pp. 128-129).

The Osiander-Bellarmino argument is clear: Copernicus' system is a geometrical construction of the same kind as those of the eccentrics and epicycles, neither more nor less true, though doubtless more convenient. And, for Duhem, - 'today we have to recognise and to declare that Logic was on the side of Osiander, Bellarmino and Urban VIII and not on the side of Kepler and Galileo; that the former had understood the precise scope of experimental method and that in this respect, the latter were mistaken' (ibid. p. 136).

The sad thing is that this very clear argument is absurd and illogical; as chance would have it, Galileo revealed the sophistry it contains in a text of 1615, translated with a commentary by Maurice Clavelin (Revue D'Historie Des Sciences XVII, 1964. cf. the extract in (21))

Galileo's argument is crystal clear: the Copernican revolution does not consist in the proposition of a new system of secondary hypotheses about the phenomena, - but in the proposition of a new system of first principles about the real cosmological order. The equivalence (in relation to 'saving' the phenomena) is only valid between the geometrical constructions, not between the antagonistic cosmologies. From the point of view of the categories of their own theory of sciences, Osiander and Bellarmino are mistaken in displacing the Copernican revolution from its true place. It is true that Copernicus continues to use geometrical methods similar to those of the Ancients, combining eccentrics and epicycles. From the mathematical point of view, Copernican astronomy is still linked to the primacy of circular movement; but the real movements of the planets (including the Earth) thus constructed define the framework of a new cosmology.

It is therefore not possible to dismiss as equivalent what

Duhem, with an appreciable nuance in the choice of epithets, calls the 'impenitent realism' of Galileo, and the 'intransigeant realism' of the Holy Office. Nor is it possible to declare in the style of continuist history:

'In spite of Kepler and Galileo, we believe today, with Osiander and Bellarmino, that the hypotheses of Physics are no more than mathematical artifices designed to save the phenomena: but thanks to Kepler and to Galileo it will be demanded of us to save all the phenomena of the inanimate universe at once' (Duhem Sozein ta phainomena p. 140) For this sentence is contradictory: in order to 'save all the phenomena at once' it was necessary to establish that the same Mechanics and the same Dynamics governed the Earth and the Heavens, the sublunar world and the planets; it was necessary to break the ancient and mediaeval image of the ordered and hierarchised Cosmos and with it the very idea of Science held by Aristotle and his commentators, which only has meaning for such an image of the World. 'To save all the phenomena at once', meant, for classical science, to reject the ontological hierarchy of the forms of knowledge which it implied. Even if this transformation took more than a century, from Copernicus to Newton, it defined a revolution which no continuism can explain.

The history of the sciences must evaluate Galileo's epistemological choice: it can, with Duhem, condemn it in the name of a formalist and conventionalist philosophy of science. We have seen the implications. For if the history of the sciences appears to provide Duhem with the confirmation of his epistemology, this is because, quite to the contrary, the latter has established at the outset the apologetic programme which the former will have to fulfil. The conditions of another history of the sciences will only be found in another epistemology.

II. The concept of recurrence

A. The preceding remarks, which, I remind the reader, have no other aim than to establish negatively the obstacles in an ideology of science which oppose the construction of the concept of its history, suggest a characterisation of these obstacles in three statements deriving from the philosophy of the sciences.

(1) Science is a unity. Here I refer to Althusser's paper, thesis 26: 'every philosophy which presents itself as a philosophy of science is an ideological philosophy ... The expression 'science' (in the singular) is neither a philosophical category nor a scientific concept, but an ideological notion. 'Science' is an ideological notion. The object which it designates does not exist: 'science' does not exist. On the other hand the expression 'science' is symptomatic of the existence of an object different from the one it designates: 'sciences' (in the plural) exist.

(2) The development of science is continous and uniform. This follows from what has gone before: continuity and uniformity are the temporal phenomenon of the essential unity of science.

(3) The third statement provides an interpretation of what science is in its essence. We have seen that this interpretation could be in turn positivist, pragmatist or conventionalist. The unity of these three interpretations is the empiricism which lies in the definition of the pseudo-object adopted by the history of the science conceived as a history of methods and results.

B. If every attempt to justify the ideological themes of the philosophy of the sciences is removed from the history of the sciences, it has to seek its concept and the rules of the construction of its object elsewhere. Let us assume (to avoid any ambiguity) that the term 'philosophy of the sciences' designates an ideological project in the same way as the term 'theory of knowledge' does. Where the

theory of knowledge develops the implications of the couple (doublet) 'subject-object', the philosophy of the sciences discusses the couple 'Unity-Plurality' or 'Specificity-Generality'. Therefore, philosophy of the sciences will henceforth be taken to mean a discourse external to its object and which deforms that object.

If Science (in the singular) does not exist, the history of Science does not exist either. Histories of sciences alone are possible. Three consequences follow:

First consequence. - The history of a science can only find the concept of its object in the science of which it is the history. I will illustrate this essential thesis by a commentary on an admirable text of Cavaillès in the introduction to his Remarques sur la formation de la théorie abstraite des ensembles (1938):

'Of all histories the history of mathematics seems the history least related to what it serves as a vehicle for; if there is a link it is, a parte post merely satisfying curiosity, not an understanding of the result: the after explains the before. The mathematician has no need to know the past as it is his vocation to reject it: to the extent that he rejects the authority of tradition, to that extent alone is he a mathematician, that is to say a revealer of necessities. However, with what means does he work? The work which negates the history is accomplished in history. A double relation: with problems studied and posed at a certain time - the choice of rebellion - , with the already existing methods, the raw materials with which to forge the new instrument. In both cases the arbitrariness of the individual or the style of a mileu are insufficient explanation: even if mathematics were conceived as a system in itself, the meanderings of the process of revelation would have some relation with the structure of the portions revealed. In other words, there is a mathematically founded objectivity of mathematical development.' (pp 27 - 28 in J. Cavaillès Philosophie Mathématique, Paris Hermann 1962).

It is this last sentence which gives us a formula for the construction of the concept of the object of the history of mathematics.

(a) The development of mathematics is an objective development.

(b) This objectivity is not any objectivity but a singular and typical one: 'there is an objectivity etc.'

(c) This singularity is relative to what this development is a 'vehicle' for. At first it seems that this development has only rather loose and extrinsic relations with what it is a vehicle for. Nonetheless this is an appearance, which means that one situates oneself from the point of view of the results. However, these results have to be obtained and this obtaining is neither random nor arbitrary but relative to a system of means.

(d) It is these means which ensure the real link between the development of mathematics and mathematics itself. What is this link? It is necessary to think it as a relation between a founding and something founded. It is mathematics which founds the objectivity of its development. Cavaillès establishes this by an allusion to a limit case: the case in which mathematics is conceived as a system in itself, eg as eternal truths. Even in this case, the temporal stages of the formulation of these eternal truths would correlate with the concatenation (the structure) of these truths. This limit case is not Cavaillès' thesis, it is invoked here merely to strengthen the argument.

'There is a mathematically founded objectivity of mathematical development': this statement is a philosophical statement. This truism involves two others: it is not a mathematical statement; it is not a statement of the history of mathematics. It is a statement which defines the relation between mathematical statements and the statements of the history of mathematics - ie, between a history and its object. These truisms have to be formulated in order to avoid the misconception which would see in this statement a reduction of the history of a science to that science, an absorption of the history of mathematics into mathematics. What is founded mathematically is not mathematical development. Mathematical development is not the development of a pre-given structure, mathematics in itself (other things being equal, the way that for Comte, order is revealed by progress). What is founded mathematically is the objectivity of this development, in other words a philosophical category which belongs to the theory of this development and is confused with the construction of the concept of the history of mathematics.

Second Consequence - Inversely, it can be said that there is no initial definition of a science, or that the definition of a science is its history, not the historical pre-existing the treatises, nor the review of results and discoveries - but its real history, the real conditions of the production of its concepts.

Cavaillès again: 'Mathematics is a development. All that we can do is to attempt to understand its history in order to situate mathematics among other intellectual activities, to attempt to discover certain characteristics of the development.' (23)

A science is not born by defining an object, by encountering an object, or by imposing a method. It is born by constituting a body of concepts with their rules of production. By this very fact, the development of a science is the formation of the concepts and theories of that science. Not only will different sciences have different forms of development, but within the nominal unity of a single science, concepts or theories may have different developments, types of constitution or formation which cannot be reduced to a single model.

Third Consequence - Lastly, the history of a science implies an epistemology. By an epistemology we mean, in opposition to the universalizing project of the philosophy of the sciences and the theory of knowledge, the theory of the specific production of the concepts and of the formation of the theories of each science.

The true point of contact between the epistemology and the history of the sciences is more difficult to delimit. There is nothing to ensure in advance that this contact can be established in the same way in all cases. However its recognition enables us to reply to the questions posed initially of the relation between the history of a science and that science, and of the interests at stake in this relation.

The philosophy of the sciences engenders a teleological history of the sciences: each 'stage' of science leads to the following stage for which it prepares, and extends the previous stage which it perfects. However, the relation installed by teleology, because it is established post factum and remains external to the terms which it relates, only masks the fortuity and radical contingency of the success of facts and accumulated results of the science. It is crucial to substitute straight away an epistemology of recurrence for this teleological philosophy.

C. The Concept of Recurrence.

The elaboration of the concept of recurrence forms the kernel of the theory of the history of the sciences, just as the concepts of the break and the recasting form the kernel of the theory of the sciences or epistemology.

(Cf Balibar and Pêcheux Definitions in this issue).

I will proceed in two stages:

- an analysis of the theory of the history of the sciences implied in Bachelard's epistemology.
- an exposition, borrowed from Heinrich Scholz, of a paradigm application of recurrence to the method of the history of the sciences; this exposition forms the third part of this essay. (Not published here see Sur L'Histoire des Sciences pp 116-139).

The epistemology of recurrence marks the rupture with the history of the sciences as it is ordinarily conceived by the scientist, as he sometimes expounds it in his historical introductions. In appearance the history in these historical introductions is recurrent: setting out from what is currently true, it chooses the dates of its progressive arrival, separates the grain from the chaff - in other words, what conforms or is equivalent to what is currently and what is destroyed or rejected by it.

This point can be specified by comparing it with the quite similar problems raised by the reading of the works of the Young Marx. Althusser has shown (cf. For Marx) how this reading is invalidated by a simultaneously analytic and teleological pre-judgement; this pre-judgement founds a dissociation between elements which are 'already' materialist and 'Marxist' and elements which are 'still' idealist, Hegelian and Feuerbachian, which pre-supposes that it is possible to reduce an ideology to its elements independently of the problematic which confers a meaning on them, - and to classify these elements according to the norms provided by what this ideology is deemed to have 'become' later.

The history of a science encounters an analogous deformation of its object. In order to constitute the field of study of that object it must first substitute what we shall call recurrence for teleology, or, if you wish, a 'good' recurrence for the 'bad' recurrence of the historical reviews.

Teleology is the extrinsic link which founds the before on the after by reducing the before to the after, in the forms of preformation, prefiguration and anticipation. Hence the concern to seek for 'sources' and ancestries and the hunt for precursors. (24) But it can also be argued that the after is reduced to the before, since everything was in a sense already there, enveloped in the shades of pre-existence. Then why history? - unless perhaps as a moralising discourse, a lesson in patience and modesty; as Bergson said, we have to wait for the sugar to dissolve. But nothing has really happened.

Teleology, or 'bad recurrence', is a regressive analysis because it is regressive, it must presuppose a continous and homogeneous historical time: such a continous and homogeneous environment is necessarily implied by the equivalences which the regression establishes. Therefore there are no breaks or recastings. Because it is an analysis one of its results is the division of the body of concepts, methods and theories by which it dissociates the true which has been confirmed - because it was already there - from the false and the illusory.

This bad recurrence depends on the same empiricism as the one our first indications have already enabled us to discern. It is time to disengage its moments abstractly:
(1) Firstly, this analysis treats the statements of science as things; it dissociates them, separates and reduces them, links them together as container to content or cause to effect. This reduction masks two confusions:
(a) That of the statements of the science with the object which they refer to.
(b) That of this object (the object of the science) with things offered to a perception, whereas the object of science is a theoretical, constructed, object, an object 'in thought' and not a concrete thing given as the support of its perceivable properties.
(2) This analysis rests finally on the confusion of the real and knowledge, in an empiricist mode which confers the properties of the real onto knowledge. Science is the disclosure and formulation of the real. The price of this empiricism is firstly nominalism - the reduction of the concept to the word: the presence of a word or of a synonym in a text will amount to the presence of the concept which this word henceforth designates for us; eventually it is the liquidation of the concept itself reduced to a description or a résumé of experience (eg Descartes describes the reflex movement: he therefore possesses the concept of reflex). Empiricism is also a formalism, in other words it conceives the statements of science not as the registration and production of a concept, but as the formalization or formulation of a pre-existant real. (25) The successive formulations only 'translate' this real diversely, without affecting it in itself.

My promised examination of the Bachelardian theory of the history of the sciences will enable us to specify this idea of a recurrent history. Two fundamental texts will be referred to:

L'Actualité de l'Histoire des Sciences, a lecture at the Palais de la Decouverte in 1951 (quoted as HS) (26)

L'Activité Rationaliste de la Physique Contemporaine, the first chapter Les récurrences historiques. Epistémologie et Histoire des Sciences. La dialectique onde-corpuscle dan son développement historique. (quoted as AR)

The problem posed is to know how an epistemology of rupture, a theory of the novelty effect of contemporary science, a philosophy of science in action can think its relation to the history of science: 'the modern point of view thus determines a new perspective on the history of the sciences, a perspective which poses the problem of the current (actuelle) effectivity of this history of the sciences in scientific culture' (AR p. 24)

This new perspective on the history of the sciences is precisely recurrent history (the expression appears in AR p. 26)

The singularity of the history of the sciences lies in its object: indeed, the latter presents itself in the first place as the progress inherent in the very concept of science: a history of the decadence of science belongs to a history of customs, civilisation etc but not to a history of science. For 'science could not be the cause of a regression of learning'. (HS p. 6). (27)

However, examined more closely, this progress is not a linear course, a conservation, or an englobement: the reality of this progress is the 'dialectic of the liquidation of the past'. (HS p. 7) Progress proceeds by erasion and recasting, by what Bachelard calls dialectic. (28)

The history of the sciences cannot therefore be a 'history like the others'. (HS p. 6) It is not only the narration of events, but a duplicated history in which the unfolding of values duplicates that of facts. (HS p. 10) This duplication implies a reflexive 'judged history, judged in the detail of its texture' (HS p. 8 and AR p. 24). This judgement is only possible by reference to the actuality of the science: the latter provides the values and the axes. The history of the sciences therefore pre-supposes the filtering or critical function of an epistemology directly informed by

the actuality of science; that is, the material of the historical narrative is here made up of an ensemble of judgements which have laid claim to truth: the history will constitute its object by judging the claims of these judgements, on the basis of contemporary scientificity: The history of the sciences is at the very least a tissue of implicit judgements about the value of scientific thoughts and discoveries.' (HS p. 8)

The same critical duplication makes it possible to distinguish between an outdated history, (histoire périmée), a history of thoughts which have become unthinkable in current rationality, and a sanctioned history (histoire sanctionnée), a history of thoughts which are still current or can be made so, confirmed by the science of today. Hence the elaboration of the concept of the 'current past' (AR p. 25), the true aim of a history of the sciences sure of the epistemological dignity of its object: from the point of view of this current past there are in the history of science concepts eternally, valid: eg Huygens' construction of refraction. (AR p. 36). In contrast, Descartes' physics is abandoned to its 'historical solitude' (AR p. 35). Thus 'the dialectic of the liquidation of the past' is translated into another dialectic, that of 'obstacles' and 'epistemological acts'. But it is in respect of recurrent reflection that the acts are confirmed as such and the obstacles recognised as overcome or avoided.

Even when the history of the sciences is reduced to that of the lives of the scientists, it always contains a symptomatic recognition of the necessity of this evaluation: this impure history, which is only accidently a history of the sciences, and misrecognises its object, tells of the struggles of genius thwarted, misunderstood, consecrated: it thus admits the reality of evaluation even as it perverts it. (HS p. 27)

While defining the history of the sciences as a critical history we should remember that this concept was elaborated by Nietzsche in the second of the Thoughts out of Season, On The Use and Abuse of History. Critical history judges and chooses, validates and rejects, remakes the past to suit the present. Bachelard refers to this explicitly, quoting Nietzsche (AR p. 24): 'You can only explain the past by what is highest in the present.' I will return to the possible implications of this reference.

Although the definition of recurrent history relates to a very general epistemological thesis, it does not act in a uniform way with respect to every science. Bachelard notes that its employment is more indecisive in physics or in chemistry than in mathematics and cites the example of aberrant recurrences which misrecognise the concepts and their true genealogy. He draws a lesson from this, a lesson from which we shall disengage a thesis, 'Real tact is necessary in handling possible recurrences... This assimilation of the past of the science by the modernity of the science may be ruinous when the science has not yet achieved the hierarchy of values which characterises in particular the science of the 19th and 20th century.' (HS p. 10) The thesis which emerges from this lesson in modesty and methodological flair is the following:

The use of historical recurrence is only legitimately founded if the science concerned has itself attained the level of rigour which makes it possible to recognise the hierarchy of epistemological values and through it to discern of the real state of the genealogy of the concepts.

The answer to the initial question is thus that in recurrent history 'the consciousness of modernity and the consciousness of historicity are rigourously proportional' (HS p. 9). An implication follows which although at first sight paradoxical, is nevertheless inevitable: the history of a science is never completed for a given epoch. The history of the sciences has to be taken up again and redone in order to keep at the level of current science: 'In following the ideal of modernist tension which I propose for the history of the sciences it will often be necessary to redo, to reconsider the history of the sciences.' (HS p. 10). The history of the sciences would pay a high price - the destruction of its interest and finally of its object - if it lagged behind the epistemological revolutions, just as the philosophy of the sciences pays a high price - the insignificance of a discourse rendered vacuous - for lagging behind scientific revolutions.

I have followed Bachelard's texts closely, to make them say nothing but what they do say, and to remain faithful to this very precise thought. It remains to pose some questions which involve the interpretation of these texts, and their scope for the actual elaboration of the history of the sciences.

The distinction between the outdated history and the sanctioned history, the history of the current past, justifies the general indifference of the scientists to history, in a sense distinct from that of psychological considerations, insofar as the current work of science implies the refusal to adhere to what is given, including the cultural given which transmits established knowledge.

However, the problem of the history of the sciences is not to explain this acquisition as an ensemble of results, it is to describe the real processes of the production of knowledges. In this respect, there is no established knowledge for the history of the sciences: its object is not given, it must be constructed and reconstructed, whenever a science happens to provide us with a new light on the conditions of the production of its concepts. However, these recommencements of the history of the sciences do not signify the dissolution of its object. Now, if Bachelardian epistemology is, as we have stated, a history in action, its history continually threatens to dissolve into the current epistemology. That is, to read Bachelard, it seems that the historicity of science is biassed much more towards its future than towards its past. Science is historically situated because it knows itself to be non-definitive and awaits its rectification, it includes a consciousness of its next rectification; but the past is only authentically the past of science if it is conserved as a current past, with the ahistorical index of a 'forever': an epistemological act and ever-current. Therefore, there is a history in the strict sense, that is to say, a discourse on a past recognised as such, only of obstacles, errors, erasions and rectifications: but this is no longer the history of science, it is the history of recognised error. Either the object of the discourse is sciences, but that gives an epistemological but ahistorical discourse - or, the discourse really is a history, but its object only has the negative epistemological value of an obstacle or a deviation.

This difficulty appears in another way if we refer to certain texts in which Bachelard illustrates one of his essential themes: that of the usually confused distinction between the fundamental and the primitive. The simplicity of the primitive is a false simplicity which derives from the hidden simplicity of the fundamental, the simplicity of a law with complex, manifold applications. This theme is illustrated in Chapter 5 of Le Rationalisme Applique by the history of Pythagoras' theorem.

The initial simplicity is to prove the theorem on an isoceles right-angled triangle, then on any right-angled triangle. The Greek geometer constructs squares on the sides of this triangle. Then begins the history of the vari-

ations which lead eventually to the fundamental simplicity: the theorem is re-discovered if instead of squares, regular polygons are constructed (eg equilateral triangles); but here the regularity is only an inscription or similarity: the theorem is re-discovered again if any similar figures, rectilinear or curvilinear, are constructed on the side of the right-angled triangle. From this point, in particular, it is possible to construct similar right-angled triangles which are similar to the triangle on whose sides they are constructed (fig. 1), then simply to divide the initial right-angled triangle into two similar right-angled triangles, by dropping a perpendicular (fig. 2) 'Immediately.... the proofs for other figures ebb away given the prime obviousness of the diagram obtained at the end of our variations.' (29) 'Pythagoricity' is an intrinsic property of right-angled triangles and has nothing to do with the construction of squares, which has thus has the benefit of an undeserved historical privilege suppressed by recurrent culture. 'The notion of an epistemological privilege is seen to appear... It should have been predicted. It should have been predicted that pythagoricity was inscribed in the right-angled triangle without any supplementary figure at all .... Epistemology thus situates us in a logical time, a time of correctly situated reasons and conclusions, in a logical time which no longer has the delays of real chronology.' (30)

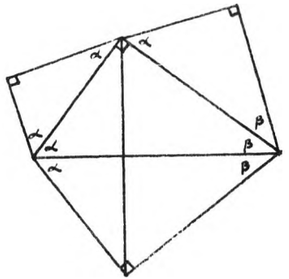


Fig. 1

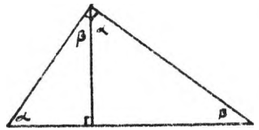


Fig. 2

In this way historical time is abolished in the logical time of deserved privileges, in the time which epistemology recreates. The sluggishness and the opacity of historical time disappear in the transparency and 'delectable rapidity' of logical time. This logical time therefore takes off from real chronology in order to reverse its axis according to well founded evaluations. However, the sequence of variations which we followed initially is itself an ideal construction whose concatenation is pedagogic (31) not historical. We were outside history from the start, or at least, removed from any pre-occupation to restore the articulation of its moments as they were actually realised.

Let us remember the distinction of logical and historical time in this text, the ultimate duplication instituted by the recurrent approach; let us also remember how it can lead to a sort of dissolution of actual history. The need to find the terrain of this history suggests posing two problems

implied by the Bachelardian conception of recurrent history:

(1) The first problem arises from the objection which is addressed to all ' critical history'. This objection was formulated by Nietzsche when he defined such a history as an 'unjust and cruel' history: 'Sometimes it becomes necessary to see to what extent everything is unjust and deserves to perish; age is then submitted to criticism ... It is always a dangerous initiative ... For since we are the fruit of past generations, we are also the fruit of their aberrations, their passions, their errors, even of their crimes. We may condemn these errors and think ourselves exempt from them, but that does not affect the fact that we stem from them.' (32)

No doubt critical history according to Bachelard finds the norms of its jurisdiction or of its sanction in the current rationality of the work of science. However, all things being equal, it is open to the same interrogation: the current past may be isolated from the outdated and excluded past, but it remains true that it was possible once for this falsity to have been held to be true, and to have been thought indissociably from what remains true for us. Copernicus maintained the dogma of circular movement even though his cosmological principle of heliocentrism opened the field of the new cosmology, and thereby of the new physics which lead to the concept of celestial mechanics. Is it necessary to separate what is science here and the endurance of an illusion or of an obstacle there? The fact that the recurrent approach confines Descartes' physics in its 'historical solitude' does not abolish its actual presence in the necessary concatenation of the themes of physics even if it reveals there more obstacles to surmount than acts of foundation. (32)

It is necessary to understand 'to what extent superseded notions, attitudes or methods were in their time an advance, and consequently how the superseded past remains the past of an activity for which we should retain the name scientific.' (33)

(2) The second problem is epistemological: it relates to the specificity of what the history of the sciences is a history of, not the chronological recording of results conceived as events, but a restitution of judgements and concepts whose claim to truth has been held to be legitimate. What is true in the current sense should not be confused with the true of a previous state of sciences, even in the case where this state is conserved in its current past. The truth of a theory, the validity of a method, the functioning of a concept, as they are recuperated and evaluated by current science, does not coincide with the truth, the validity, the functioning of what only appears to be the same concept, the same method, the same theory in another age of science. When, in his Eléments d'Histoire Des Mathématiques, Bourbaki declares that 'the history of the concept of truth in mathematics belongs to the history of philosophy and not that of mathematics', he is only partly correct; for although philosophy does define the concept of truth in mathematics, what it says only has meaning through a precise relation to what mathematicians do, and these mathematicians themselves are not obliged to be indifferent to it. This relation can have the rigour which grasps each element of the concept with precision, or be relaxed into diffuse generalities, - but that means precisely that in order to judge it, we have to recognise that the concept of mathematical truth resides nowhere but in the processes set to work by the production of mathematical concepts and the construction of mathematical theories. In order that there is a 'mathematically founded objectivity of mathematical development', there must be a mathematical concept of truth, which is not

given, but must be constructed from all the propositions which the mathematicians of an epoch hold to be true.

Therefore, there is truth in the sense of Eudoxus, truth in the sense of Leibniz, truth in the sense of Dedekind or Cantor, and these truths are not mutually exclusive, but neither do they imply one another, such that a true proposition in the sense of Eudoxus can remain such in the sense of Dedekind. The historian has not only to take note of this equivalence, he must also think the mutation of meaning which underlies it.

His task is therefore to construct the real time of science without reducing it to the transparency of a play of closed forms. But this construction is founded on what can be called logical time, the discursive concatenation of intelligibility. The laws of formation which order logical time are active in the temporal unfolding of learning, but this unfurling offers to its dissolution into logical time a resistance which attests to the reality of history and of its own temporality.

Notes.

(1) The text published here is an edited version of the text of M. Fichant L'Idée D'une Histoire Des Sciences published in M. Fichant and M. Pêcheux Sur L'Histoire Des Sciences (Paris F. Maspero 1969), Fichant's text occupies pages 51 - 129 of that text, this edited version constitutes pages 54 - 66, 71 - 84 and 96 - 114 of that text.

(2) 'Since these lectures a collection of Georges Canguillhem's articles has appeared under the title Etudes D'Histoire et De Philosophie Des Sciences, which would seem to contradict our assertion. However, it is remarkable that the author poses firstly the question under the generality and indefiniteness of an 'of what': 'Of what is the history of the sciences a history?' (p. 9) and he declares later 'The object of the historian of the sciences can only be delimited by a decision which assigns relevance (intérêt) and importance to it (p. 18).

The thesis of this article can be resumed in saying: (1) the object of the history of the sciences is not the object of science. (2) the relation of the history of the sciences to its object is not the relation of a science to its object. (3) and yet the history of the sciences maintains a relation with the science simultaneously paradoxical and essential. It is this relation which we shall seek to specify here.

(3) For a justification of this thesis, - the Cartesian origin of an historical discourse on science and philosophy - cf. Belaval's book Leibniz Critique de Descartes, Ch. 2, Révolution et Tradition. cf. previously, Brehier La Philosophie et Son Passé pp. 28 - 29: 'It has been said and repeated to repletion that Descartes' rationalism was essentially anti-historical, breaking all links with the past and making philosophy begin, at least in intention, from the current advances of each individual or rather from the intemporal and ever present enlightenment of true ideas. In one sense there is nothing more false ... Descartes is not really the enemy of history, but rather of the commentary, who ever gives the past this jurisdiction on the present effectively rejects history.'

(4) Fontenelle Préface Des Eléments De La Géométrie De L'Infini, Oeuvres Tome VI, p. 38. This concerns Cavalieri. Fontenelle shows how he could justify the Geometry of indivisibles in demonstrating its compatability with previous methods: 'One can still convince oneself of this through a certain natural order, through a simple liason which is found between old and new propositions.'

(5) Fontenelle Préface Sur L'Utilite Des Mathématiques et De La Physique, Oeuvres Tome VI, p. 75: A propos 'detached fragments' of which the collections of the Academy consist, Fontenelle notes 'Perhaps the time will come when these separate links will be joined in a harmonious body; and if they are as one suspects, in some way they will assemble themselves from themselves. Many separate truths as soon as they are sufficient numerically present their relations and mutual dependence to the mind in such a living way that it seems that after having been detached by a species of violence one from the other they naturally seek to re-unite themselves' (my emphasis).

(6) cf. Fontenelle Préface Des Eléments De La Géométrie De L'Infini Oeuvres Tome VI p. 42: 'There is an order which giverns our progress. Each knowledge only develops after a certain number of preceding knowledges have developed and when its time of emergence (son tour pour eclore) has come' (my emphasis). 'An order which governs our progress': this will be Auguste Comte's formula. It is necessary to point out that this model of emergence (eclosion), at the moment fixed by a law of development is incompatible with the spatial model of adjunction: in an organised body there are no empty places to fill. Elsewhere, Fontenelle presents the 'necessary succession of discoveries' in demonstrating that each scientist is situated at a point of departure which is the end-point of his predecessors. cf. Analyse Des Infiniment Petits, by Monsieur Le Marquis De L'Hospital, preface by Fontenelle (1787 Edition): 'In a word, it does not appear that the Ancients were able to do more in their time: they have done what our good minds would have done in their place: and if they were in ours, it is to be thought that they would have the same views as we. All that is a consequence of the natural equality of minds and of the necessary succession of discoveries.' (p. IX) 'To speak only of Mathematics which alone is in question here, M. Descartes will begin where the Ancients had finished, and will commence by the solution which Pappus said remained in its entirety.' (pp. X - XI) 'In the absence of this calculus, that of the celebrated M. Leibniz has emerged; and this geometer - scientist began where M. Barrow and the others had left off.' (p. XIII)

(7) cf. the text in Appendix A (not printed here to be found in Sur L'Historie Des Sciences op. cit. pp 142 - 144)

(8) On this notion, cf. D'Alembert's article in the Encyclopedia reproduced in Cahiers Pour L'Analyse No. 9.

(9) I leave to one side the memorable history of the steps taken by August Comte to obtain the creation of a Chair of the History of the Sciences at the Collège de France. It would belong in a study of the place of the history of the sciences in the institution of the university and in the tradition of education.

(10) cf. Tannery Memoires Scientifiques Tome X p. 158: '.... It is clear that excellent specific works can be done in the history of the sciences without any pre-occupation with positivist doctrines. However, if it is a question of the general history of the sciences, the possibility or the appropriateness of treating it such cannot be denied; no other total conception but that of Auguste Comte can be adopted; it remains indubitably the case that from the point where this immortal thinker became alone he who had sought to submit this history to laws, it is certainly necessary for whoever wished to treat of it either to be for or against him. Now, I am for him and with him against those who have combated him and also against those who have deviated from him, such as Littré.' The

opposition explicitly made between specific works in the history of the sciences and the general history of the sciences will be noted: the latter pre-supposes the possibility of a single law. In this sense, it is necessary to be for or against he who alone had furmulated this law, to either accept it, or to produce another. It could be said that the situation of the problem expresses a positivism even more profound than that to which Tannery adheres in his reply: in effect this position assimilates generality to legality. There is only a general history of the sciences if all the sciences form sequences submitted to the same order of laws.

(11) Cited by Littrê Auguste Comte et Le Positivisme.

(12) Auguste Comte Cours De Philosophie Positive (Paris Bachelier 1830) p. 77 cf. the Positive Philosophy of Auguste Comte 'Freely Translated and Condensed by Harriet Martineau' pp. 24- 26, and Sur L'Histoire Des Sciences op. cit. Appendix B pp. 145 - 148.

(13) Ibid p. 81 cf. also the Schleicher Edition (1934) Tome IV p. 238.

(14) cf. Koyré Etudes D'Histoire De La Pensée Scientifique p. 176 and ff.

(15) The articulation of this consequence is made by Duhem in the first lines of his work Le Système Du Monde (Tome 8): 'The genesis of a scientific doctrine is not an absolute beginning; as elevated as the train of thoughts may be raised which have prepared, suggested, signalled this doctrine, one always comes back in turn to opinions which have prepared, suggested, signalled' (p. 5). The choice of a point of departure is arbitrary and depends in a large measure on lacunae in our information. cf. Appendix D (Sur L'Histoire Des Sciences op. cit. p. 155), Bachelard's text characterising continuism as an illusions of beginnings.

(16) cf. Also De L'Accélération Produite Par Une Force Constante Congrès de Philosophie 1904 p. 888. Koyré remarks justly (Etudes Galileenes p. 76) that Duhem's interpretation attributes the notion of force to Aristotle which is alien to him. Similarly, even when Galileo employs the same words as his 'precursors' (for example, the word impetus), he designates a completely distinct concept by it (ibid. p. 93).

(17) That has been definitively made by Koyré in his Etudes Galiléenes. Thus p. 10 Note 1 'For the history of scientific thought, the scientific conception, the popular conception of the 'Renaissance' proves to be profoundly true.'

(18) Not printed here; see the note De Galilée à Duhem (Sur L'Histoire Des Sciences op. cit. pp. 85 - 87).

(19) cf. Koyré's clarification, La Revolution Astronomique (Paris Hermann 1961) pp. 84 - 85: 'The opposition between mathematicians and philosophers (that is to say physicists) leads finally to a purely pragmatist and phenomenalist epistemology, which despairing being able to determine the real movements of the celestrial bodies, merely assigns to astronomy the task of constituting a system of calculation permitting the prediction and ordering of the phenomena. The famous Platonic injunction: Sozein ta phainomena, salvare apparentias which means in the first place: rediscover the intelligible structure of what appears, transforming the meaning and becoming the device of a science which renounces knowledge of reality and which derives from appearances alone ... In effect, the attitude of the ancient and mediaeval 'positivists', generally modernised and misinterpreted by the modern positivist historians, did not consist in the adoption of a new scientific ideal, but saw that the essence of things and their real causal relations remain inaccessible to our knowledge, in despair or renunciation of the possibility of attaining to this truth, either in a determinate domain of the real, astronomy, or (the sceptics and nominalists) in all domains of knowledge of nature. Ancient and mediaeval positivism continually demands a devaluation of science, which merely treats of phenomena (appearances) in relation to what treats or will treat of the real. Consequently, it is opposed to modern positivism which does not deny knowability, but the very existence of a world of realities underlying appearances, and which glorifies itself in its anti-realism.' (cf. also Etudes D'Histoire De La Pensée Scientifique p. 75). Clearly, Duhem's positivism is also linked to the devaluation of scientific knowledge. It is not to be ignored that Koyré has always been faithful to Meyerson's epistemology, as an affirmation of the realist character of science. It can perhaps be said that this epistemology does not entirely merit the disrepute under which it has fallen since Bachelard: between Duhem and Meyerson, it is clearly recognisable to whom the advantage must be attributed. Essentially, moreover, in the whole of Koyré's work, the reference of the practice of the historian to epistemological norms will be constant and conscientious. In view of the results, there are no grounds to be contemptuous of Meyerson, as Koyré's master.

(20) Preface honestly anonymous, deriving from which fact it was for a long time attributed to Copernicus, who was unable ever to read it, for it seems he died before the end of the printing of his work.

(21) Not published here see the note De Galilée A Duhem in Sur L'Histoire Des Sciences op. cit. pp. 88 - 93.

(22) The word does not appear too strong to me when I read at the end of Physique de Croyant: 'The only conclusion which the facts allow is that the Catholic Church has strongly contributed, in many circumstances, that moreover it contributed energetically to keeping human reason on the right road, even when this reason strives to discover the truths of the natural order' (op. cit. p. 472). I continually maintain that my polemic does not intend in any way to depreciate the quality of the monumental work of collection of documents effected by Duhem.

(23) Communication a La Societe Francais de Philosophie, published in the bulletin of 1946. This text published in Appendix E (Sur L'Histoire Des Sciences op. cit. pp. 156-159).

(24) cf. G. Canguillhem op. cit. p. 20 and ff.

(25) It goes without saying that 'formalism' is taken here in the philosophical sense, which has nothing to do with the scientific, logico-mathematical concept of formalism. Only a misconception authorises the justification of a philosophy of the formalist style (like that of 'logical positivism' or of a certainstructuralism) by the development of formal methods in mathematics and of the formalism of mathematical logic. Though incomplete, an epistemology like that of Cavaillès will attest to the contrary: it is because the 'progress' (or the process of science) is 'material or between singular essences' (and not a passage from one form to another and even less from an unformed material to an imposed form) that only a 'philosophy of the concept' can present the doctrine of science, cf. the final pages of Cavaillès Sur La Logique et La Théorie De La Science, Second Edition (Paris Presse Universitaires De France 1960) which correctly takes up (p. 77) the problem of history.

(26) cf. Appendix D (Sur L'Histoire Des Sciences op. cit.

p. 155)

(27) It is not without interest to observe that Tannery in his book on Greek Geometry (Paris Gaunthier-Villars 1887) in his aim to resolve the problem of the 'future of science' through history, he questions himself on the decadence of Greek mathematics: 'The true problem which is imposed today in this history is to specify the circumstances and determine the causes of past decadence, with a view to knowing the precautions to be taken to avoid a future decadence.' It is true that the problem of the future of science is epistemologically false and only has meaning in relation to a positivist ideology which creates an obstacle to the constitution of a rigorous history of the sciences.

(28) I leave to one side, supposing it known, the formation of the Bachelardian concept of dialectic. cf. G. Canguillhem's article Dialectique et Philosophie Chez Gaston Bachelard in Canguillhem op. cit. pp. 196 - 210.

(29) op. cit. pp. 95 - 96.

(30) I make it clear that I use this text to my own ends which are not those to which Bachelard principally assigns it, with whom it is a question of illustrating a critique of Meyerson explanation. In this respect he cannot be reproached for historical arbitrariness which in no way affects his purpose.

(31) cf. pages 86, 87, p. 89 where the word is stressed.

(32) Thoughts Out of Season II (Edinburgh and London T. N. Foulis 1909) p. 29.

(32) cf. in this respect Koyré's clarification in Newtonian Studies (London Chapman and Hall 1965) pp. 53 - 54. 'The comparison, or confrontation, of Newton and Descartes, somewhat on the Plutarchian pattern, was very often made in the 18th century. It is no longer done. And we can understand why: Cartesian science, for us, belongs entirely to the past, whereas Newtonian science, though superseded by Einstein's relativistic mechanics and contemporary quantum mechanics, is still alive. And very much so. But it was very different in the 18th century, at least in its first half. Then Cartesian philosophy, which in the latter part of the 17th century inspired most of the scientific thinking of continental Europe, was still an active force; Newton's influence was practically restricted to England. It is well known that only after a long and protracted struggle against Cartesianism did Newtonian physics, or, to use the term by which it designated itself, NEWTONIAN NATURAL PHILOSOPHY, gain universal recognition in Europe'. It will be possible to determine the programme of the history of the sciences more effectively in fixing the conditions which must situate the use of recurrent judgement.

(33) Canguillhem op. cit. p. 14.

THE CONCEPT OF EPISTEMOLOGICAL BREAK

by ANTONY CUTLER

The concept of epistemological break is a crucial concept in Althusser's work. Its relevance is necessarily not restricted to the theory of the production of knowledge, for it is a key concept in the formation of a theoretical history of the sciences. It is through this epistemological break that the new practice which is the object of the history of the sciences is constituted. This concept is then not merely polemical, that is it takes up a discontinuist position within this history, but also theoretical. Theoretical, for it delineates the specificity of a particular discontinuity, that between science and ideology. It is from this specific theoretical function that the necessity of an epistemologically informed history derives, this history which judges the scientificity of its object is necessarily epistemologically informed in so far as it is a history 'of the sciences'. Following Bachelard I shall designate a crucial specific concept of the history of the sciences 'recurrence'. However, the specific effects of these words, discontinuity, recurrence, remains at the level of polemic and thus of ideology; the task is to specify the concepts of discontinuity and recurrence and to articulate their effects as concepts; this is the object of this paper.

Every vulgar empiricist history departs from an unthought discontinuity, the discontinuity of the events themselves, their difference in so far as they are different points in 'time'. It is this immediate empirical discontinuity which enables an ideological continuity to appear, the similarity/continuity of events as events. It is this characteristic of the empiricist history of science (1) which enables this history to be one of 'results and methods', that is of scientific 'events' which are given. Yet here is there not an immediate paradox? This ideological discourse is a 'history' of 'givens' yet surely are not 'methods' means of production, are they events 'in the same sense' as 'results'? What is meant by 'method' in this context?

The ideological history of science disposes of two notions of method. Firstly, method can be seen as a formalisation of givens; (2) in this respect the method is itself an event because like a result is is a discovery. The formalisation 'discovers' the relations between givens and as such is a discovery/event. This is because like all atomistic 'events' the formalisation is always radically exterior to the given, proceeding 'after the event'. Secondly, method is conceived as technique, as material technique, as a tool of discovery. However, again the method is radically exterior to the means of production of the result, for the concept of method in this case is of a tool which is an 'extension of perception'. As such, the tool as perceiving 'subject' contains its discoveries

within it, ie what it will 'see'. Method in this conception is a result for we are never informed of the place of the tool within the discourse of the science, the structure of its means of production, that is its status as materialised theory. It is precisely from such empiricism that the history of technique and the history of scientific instruments is conflated, for if 'methods' are 'extensions of perception' no distinction between the two fields can be drawn. (3)

The Double Continuity - The Concept of Continuity in the Ideological History of Science

The category of continuity in the empiricist history of science is dependent on conditions of existence which reflect the double articulation of the pre-requisites of any mechanical causality. (4) That is, what is 'required' is (a) the speculative theory of a given (eg homo oeconomicus) and (b) the planar space, necessarily homogeneous, within which objects/events 'interact'. The former is provided in the discourse of the ideological history of science by the category of knowledge (continuity of common sense and scientific knowledge). At this point the characteristics of this constituted space must be examined.

The space defined is that of 'knowledge', the knowledge of knowing, perceiving subjects. Knowledge is a unity in this conception, it cannot be other than itself because it is grounded on an invariant combination, knowing subject - perceived object. Thus, the objects within this discourse are unified as 'perceptions', 'ideas'. Of course, within this subject-object structure variants are produced. These variants are ever-already given because of the 'division of the real' between essential and inessential. 'This structure (that of an empiricst reading) concerns precisely the respective positions in the real of the constitutive parts of the real: the inessential part and the essential part. The inessential part occupies the whole of the outside of the object, its visible surface; while the essential part occupies the inside part of the real object, its invisible kernel.'(5) This division of the real thus assures the possibility of variation of essential and inessential within a basic structure of perception for the invisible is no more than a 'temporary blindness' before the discovery of the inner light.

This 'anthropology' of the knowing subject installs the space where the objects inter-relate in this ideological discourse. That is to say it establishes a continuity. This continuity allows quantitative accretion, ideas can thus be 'added', a possibility allowing both formalisation/synthesis, the representation of givens in a particular relation, and 'influences' the possession of an 'idea' by a 'precursor' and the relation between them and the one who 'receives'. However, the constitution of this space also involves a reduction. In any given text the presence of the word must be equivalent to the presence of the concept. The inevitability of this reduction stems from the subject-object invariance. The concept can be no more than the resumé of experience, the idea is thus a 'composite perception'. Given this, the presence of the word is the presence of the concept because the presence of the word is the condition of the space and thus of the history. Condition of the history because outside of the word there is no means by which ideas can be conveyed, that is there is no continuity of ideas. This categorical necessity is felicitously also anthropologically founded, for the presence of the word indicates the presence of a human subject whose characteristics within the subject-object invariance is to constitute the objectivity of the object 'for himself' through the medium of the word and thus

'for others' and thus 'for history'.

This history is the history of subjects who are 'present to themselves', a phenomenon which enables a displacement in the vulgar history of ideas. The displacement of history on to the technical practice of empiricist historiography. The 'problem' of history within this anthropologically constituted space is that of whether or not a certain 'idea' can be attributed to a certain subject, whether or not certain 'words' are present in certain texts, whether certain words in texts can be related to one another ('problem of influences'). The effects of this displacement lie in the absences of this discourse, in what is left 'unthought'. The effect of the category of 'oversight', the subject either 'sees' or does not, his 'vision' partakes of his unity as subject, the subject's invisible is his absence, it is visible 'for someone else'. (6) The effect of the category of 'honesty of the subject', the problem of which subject made a certain discovery. The creation of this linear space raises the problem of its crucial governing category.

Temporality

The concept of time within the empiricist history of ideas has not been thought so far in this analysis. The characteristic of this time already established is merely that of its continuity, that is to say of its homogeneity, the homogeneity of its points or events. However, to leave the analysis at this point would be to fall into empiricism. To the treatment of the 'immediacy' of the empiricst history as a given rather than an effect of a process of production. Althusser has demonstrated how this ideological immediacy can be reproduced in theoretical discourse through the Hegelian concept of time. (7) 'It is well known that Hegel defined time as 'der daseinde Begriff' ie as the concept in its immediate empirical existence. Since time itself directs us to the concept as its essence, ie since Hegel consciously proclaims that historical time is merely the reflection in the continuity of time of the internal essence of the historical totality incarnating a moment of the development of the concept (in this case the Idea) we have Hegel's authority for thinking that historical time merely reflects the essence of the social totality of which it is the existence. That is to say that the essential characteristics of historical time will lead us, as so many indices to the peculiar structure of the social totality'. (8) The homogeneity of time is the co-presence of the elements (they are all 'in the same time') and their equivalence/presence to themselves (they are all equally elements). As such, each element immediately expresses the totality, it reflects the essence of the totality in its immediate existence. It is through this concept of expressive totality that the 'immediacy' of everyday experience is re-produced. The immediate ideological perception of time is of an historical present (the subject is at a point in time) in which all the elements of his recognition 'accompany' him, they are 'objects' present with the subject in 'his' present. The expressive totality thus reproduces as a theoretical ideology this practico-social ideology of temporality,'... The Hegelian idea of time is borrowed from the most vulgar empiricism, the empiricism of the false obviousness of everyday practice...'.

The empiricist history of science takes up this ideological notion of time in its conception of science as a unity - the unity of the elements of science as results. The most vulgar empiricist variant of this ideology has the equivalence of elements in their equivalence as empirical discoveries. Any scientific discipline is thus a totality of equivalent observations at a particular point in time, there is no unevenness there are merely 'problems', objects to be ob-

served in the future, the results of which will produce a new equivalence of empirical discoveries. At a higher level of 'sophistication' the introduction of teleology displaces this co-presence into the future, into the 'table' of the sciences to be 'filled in' by the future development of science. (10) However, the same empiricism reigns for the spaces in the table can only be determined on the basis that they will be observations of the real, the spaces in the table will reflect the 'spaces' within nature. If the co-presence is not immediate in the sense of 'present now' there will be a point where this even co-presence is realised which will mark 'the completion of science'. (11) In each case science is conceived 'in essence' as an 'absolute knowledge', there is or will be a time in which the totality of what can be known is known.

The Empiricist History of Science and the 'Entrance of Philosophy'

It has been demonstrated above that the categories deployed in the empiricist history of the sciences fail to found an adequate history of science and reflect the reproduction of vulgar empiricist notions. A theoretical history of science cannot be a history of given results but rather must.

But philosophy has long recognised the difficulties of the empiricist history of science. Before examining the necessity of the concept of the epistemological break for a history of the production of knowledge, we must examine the notions of the history of science held by traditional 'philosophy of science', in particular, by Husserl. Husserl is not, of course, in this country at least, regarded as a traditional philosopher of science. Nevertheless, his position is exemplary in this respect for, as we shall see, he is the philosopher who most rigourously reflected the role philosophy has traditionally assumed vis-a vis the sciences.

The specificity of the philosophical discourse derives from the postulate of the radical incompleteness of science. Superficially then it escapes the bogus evenness postulated by the ideological history of science. The science is precisely not an even presence to itself, however; philosophy poses no unevenness within the science, on the contrary the dislocation refers to that between science and 'knowledge' in the 'problem of knowledge'. Yet, as Althusser has pointed out, (12) this is a pseudo-problem, designed simply to re-produce a pre-existing answer, for in any such philosophy knowledge is already constituted (in a particular variant of the subject-object relation) all that is required is to 'find' this pre-existent structure. Similarly, the concept of foundation of science is 'founded' on this pre-given knowledge (subject-object) relation. Furthermore, if science is 'knowledge' it must be founded and thus 'science' can be none other than the science created by philosophy, a particular determination of the subject-object relation.

This circle is most apparent where the 'radical' nature (its postulated distance from exisiting science) is most clear, the case of Cartesian scepticism. 'The rationalist cogito which tends to affirm the thinking subject in an activity of apodictic thought must function as an emergence over an existence already affirmed more or less empirically. Only a fortuitous world could, in a constructive reflection, succeed the word destroyed by universal doubt. Without assuming a right to a detour through the notion of a creator God it is indeed impossible to see what guarantee there could be after a totally destructive doubt of reconstructing precisely this real world a propos of which fundamental doubt had previously been raised. The cartesian universe could say to the philosopher: you would

not find me again if you had truly lost me.'(13) Thus, the Cartesian circle, the rediscovery of the 'universe' through the philosophical reprise on the science of its epoch. The cogito which surreptiously poses contingency has already 'opted' for the necessity of an originally constituted subject-object relation, knowledge.

If, however, there is a philosophy of science which practises within the circle of the theory of knowledge there is a philosophical reprise which attempts to think this circle 'from the inside'. In this discourse the role of science in the 'theory of knowledge' is thought in terms of a postulated 'crisis in science'. At this point, 'The high point of consciousness and honesty was reached precisely with the philosophy (Husserl) which was prepared to take theoretical responsibility for the necessary existence of this circle, ie, to think it as essential to its ideological undertaking; however, this did not make it leave the circle...'(14) The 'crisis in science' directly reflects this circle because the crisis is not within science but between 'science' and 'knowledge'. '... It is known that for Husserl the critical significance of that situation derives less from some epistemological conflict inherent in the internal development of the sciences than to a divorce between on one hand the theoretical and practical activity of the science in the very brilliance of its progress and success and on the other hand its meaning for life and the possibilities of its being related to the totality of our world.'(15) If this circle is to exist then 'science' can be no more than the construction of philosophy, it can only exist as the radical alterity of an unthought technical practice, philosophy conceives the science as producing results whose foundation lie in philosophy, the problem of the means of production of knowledge (problematic of the theoretical history of the sciences) is displaced on to the problem of foundation (category of the philosophy of science).

'... The original relation between logic and the sciences (16) is inverted in a remarkable manner in modern times. The sciences take their independence, they elaborate without being capable of fully satisfying the mind of critical auto-justification, highly differentiated methods whose fecundity from the practical point of view was certainly assured but whose achievement (Leistung) was not finally understood conspicuously (avec evidence).'(17) The problem of knowledge, first displacement, leads then to the inevitable correlate of a second displacement, a collapse into the petty-bourgeois ideology of the 'tragedy of culture', the problem of the specialisation of modern science denies the possibility of 'anyone' (18) enjoying the full richness of 'its' knowledge. The resolution of this crisis is in fact the production of an essential section in knowledge, the realisation of a system of mutually reflecting elements. 'If we construct the Idea of a completely rationalised empirical science, ie of a science that has progressed so far on its theoretical side that every particular incorporated in the same is referred back to its most universal and most fundamental grounds, it is then clear that the realisation of this Idea is essentially dependent on the cultivation of corresponding eidetic sciences; not only then on that of the formal mathesis which is related similarly to all the sciences, but in particular, on that of the material-ontological disciplines which analyse out the essential being of Nature.....' (19)

The postulate of the incompleteness of science with respect to (philosophical) 'knowledge' leads to the necessity of situating philosophy 'alongside' the science. Both take their place in the essential section of knowledge. However, philosophy does not merely occupy a space

here, it creates that space in denegating its own creation. 'Knowledge' is a category of philosophy here so philosophy is not merely 'alongside' the science in the table of knowledge, it creates the table itself, the table in which it (philosophy) is represented as an instance. This denegation is founded on the fact that the science is a creation of philosophy insofar as it exists as a discourse which is specific, which means in this context, lacking in auto-illumination, this illumination which can only be provided by philosophy. The 'problem of knowledge' poses this crisis because for it science in its existing form is the alienation of consciousness, it is not self-reflecting characteristics of the knowing subject. A specific partial knowledge it lacks the characteristic of relating the universal to the particular in realising the universal in the particular.

The source of this reprise lies in the ambiguity of the philosophical category of object '.... in the first place it is obvious that an empirical science, whereever it finds grounds for its judgement through mediate reasoning, must proceed according to the formal principles used by formal logic. And generally, since like every science it is directed towards objects, it must be bound by the laws pertaining to the essence of objectivity in general.' (20) The crucial postulate is that science deals with objects and therefore is governed by objectivity in general. Here, the entrance of vulgar empiricism into the philosophical discourse and the speculation/empiricism couple must be sought in the operation of the category of object. Althusser has demonstrated the importance of the distinction between the word object (conflation of real object and thought object) and the concept of thought-object, a distinction blurred by the 'play on words'. (21) Here is such a case, with the added 'clarification' that 'objects' and not 'object' are the reference. What is significant in this text is the unity of 'objects' (the object of 'science') and the dependence of the sciences on 'objectivity in general'. Here the play on words allows the following logic to emerge. Because the science deals with 'objects' its objectivity is governed by a philosophical objectivity, that of objectivity in general, the existence of the latter depends on the former, but the science has a specific thought-object, its objectivity cannot be objectivity in general. Thus, the governance of philosophy on the sciences in the discourse of ideological philosophy, can only be established by the correlative process of substituting objectivity in general for the specific objectivity of the thought-object of the science. This surreptious substitution enables the category of 'science' to appear, for science in general can exist as a partial knowledge, a determination of knowledge but not the totality of the determinations of knowledge. (22) The crucial importance of the Althusserian distinctions real concrete/concrete in thought, real object/thought object become apparent at this juncture. These distinctions are embodied in the three generalities (a) in the postulate of generality I as already 'in thought' ie the science does not work on the 'real' object, (b) the radical mutation between generalities I and III, (c) the objectivity of the science inhering in the definition of its object by its means of production, generality II, (concepts) (d) the objectivity of any particular science inhering necessarily in the specificity of its object, that of generality III, a specificity which is never given but, on the contrary derives from an objective process of production. (23)

To express the radical distinction between science and ideological philosophy two Bachelardian theses may be invoked.

Thesis I. For the philosopher the word signifies an entity,

for the scientist the word is a concept inscribed within a system of realtions defining a thought-object. (24)

Thesis 2. The unity of comprehension and extension in science, and the 'primâcy' of the latter over the former. 'It is necessary to substitute a study of extension for a study of comprehension.' (25)

Comprehension and extension co-exist within science precisely because comprehension is never perception of a 'fixed' object or entity. On the contrary, comprehension in science is always a function of the system of hierarchized concepts in a problematic which define the thought-object. The effect of the epistemological break is to produce a pre-given comprehension-extension in the relation concepts-object. The thought-object is then radically distinct from the empirical entity (real object); there is no immediate relation to it, it is always defined by a system of concepts. By the same token, the possibility of an essential section through a scientific discourse is denied, for the thought-object is always a function of a particular set of concepts which define that object. Comprehension is not even a 'moment', for comprehension is defined by extension in combination, that is comprehension can only exist within a system of concepts in science, that is within a problematic. There is no way in which fixed places can exist within the science, any point in time reveals only an uneven development of the concepts which think the object, science cannot partake of the finitude of asymptotic knowledge (a finitude of appearance) or of realism (the finitude of 'fit' between thought and the world).

Philosophy thus recovers the essential categories of the empiricist history of science, the unevenness it has posed is merely a moment (the moment of crisis) before the pre-given evenness of 'knowledge' is established. The essential section is in this case the realisation of 'knowledge' through philosophy. The category of 'crisis in science' involves the construction of a history of knowledge, this construction is built around the category 'origin' of science, for no crisis is possible without an origin, that is a crisis is a departure from an origin.

'..... culture and tradition of truth (26) are marked by a paradoxical historicity. In one sense, they may seem to be disengaged from all history, since they are not intrinsically affected by the empirical content of real history .... This emancipation can be confounded with a liberation in respect of history in general.....

But in another sense which responds to Husserl's intention, the tradition of truth is the most pure and profound history. The unity of pure meaning of that tradition is apt to found this continuity without which there could be no authentic history, thinking itself and projecting itself as such, but only an empirical aggregate of finite and accidental unities.' (27)

Hence, for Husserl, not only is the history of science the history of knowledge but also the history of knowledge is the possibility of history per se. There are two postulated temporalities, (a) the necessary 'time' of continuity, the tradition, (b) contingent empirical time where there is no continuity. Yet there is an immediate paradox, the necessary history is that of a tradition, its necessity derives from its linearity. In some sense all points on the plane are 'the same'. The tradition is nothing more than the reproduction of the 'origin'. So, at the point where this time is constituted it is abolished for, to use a metaphor, the line is contracted to a point, the point of the origin. If, in this discourse, we talk of a science the problem is that we must always talk of the 'same thing',

but if this science is to have a 'history' it must have begun at some point; however, at the very moment where the characteristics of this 'origin' are identified the history is abolished. However, the empirical time is a non-history because it is merely the collection of contingencies, the 'privilege' of the origin cannot be that of a 'first point', an empirical origin, for this would reduce the necessary history to a contingent history, and thus abolish the conditions of any history whatever. The privilege of this origin is that of an essence, it is governed by the ideological conception of synchrony.

'The synchronic is contemporaneity itself, the co-presence of the essence with its determinations, the present being readable as a structure in an 'essential section' because the very present in the very existence of the essential structure. The synchronic, therefore, pre-supposes the ideological conception of continous-homogeneous time.' (28)

The category of origin reproduces again the vulgar empiricist notion of time, for it is nothing other than the presence of knowledge to itself in all its determinations. The privilege of the origin is the privilege of the knowing subject. Once this 'saturated' time has been defined the other times merely become negations of this time. The 'time' of science is necessary only to found a crisis but it is essentially an empty, partial time. Its existence is two-fold, as partiality (as the moment of crisis), that is as only 'part of the essential section', yet again this very crisis is abolished, for a crisis could only exist if an essential section were already effected, that is if a theory of knowledge had been constituted, thus the time of science is the time of the tradition of knowledge itself. The concept of temporality is inscribed within the circle of the theory of knowledge. The same is true for the contingent time of empirical events, it functions only as the 'outside' of this necessary history. The 'philosophy of science' is equally incapable of founding a scientific history of the sciences.

Two Concepts of Recurrence

For M. Fichant, 'The elaboration of the concept of recurrence forms the kernel of the theory of the history of the sciences.' (29) However, as Fichant points out, in effect we are faced with two concepts of recurrence, the Bachelardian concept which remains ideological, and the scientific concept. (30) The theoretically progressive nature of the Bachelardian concept cannot be denied, it is important, however, to recognise its limits. The progressive side of this concept derives firstly, from its denial of any empiricist history of the sciences. The history will always be an epistemologically grounded history departing from scientific rationality. That is to say this history will always be one 'of science', for it is epistemologically informed at every point. The crucial importance of the Bachelardian concept derives from the fact that it never departs from givens, and as such the events of any history are never equivalent as elements, on the contrary at every point there is a division between scientific objectivity and the 'tissue of tenacious errors' which constitute ideology. However, the recurrence of this history is that of recurrent judgement from the point of view of the existing rationality of the science. As such, this history will be dualistic, a sanctioned history (that of 'eternal' scientific truths) and the history of 'errors'. The former are defined insofar as they are not in contradiction with existing scientific rationality, the latter insofar as they are. It is at this point that the limitations of the Bachelardian concept appear, limitations which reflect the limitations Bachelardian epistemology in general.

These limitations of necessity plunge the Bachelardian history into empiricism. The history of science in the hands of Bachelard will be a valorised history but what status can we accord to the object of these judgements? To select out 'elements' from the history of the sciences is precisely to separate out these elements from their particular problematic even if these elements are concepts and not the vulgar elements/discoveries of the empiricist history of science. Similarly, the point of departure of the judgement is always a present existant science, the correlate is a fall into relativism, science is the science of the present, the history of the sciences that of a 'current past' (Bachelard). The science of a present which defines this recurrent judgement will thus plunge not only science itself but also the history of the sciences into an inescapable relativism. Even 'eternal truths' will be relative to existing scientific rationality. The possibility of a history founders on the absence of the concept of that history, for the recurrent judgement must conflate the epistemological break with the re-organisation of the problematic of the science (31) and so the concept of epistemological break will be removed from the corpus of concepts of the history of the sciences insofar as it is continually shifted 'forward'.

This limitation reveals a general limitation of Bachelardian epistemology deriving in the last instance from a political limitation. In the division between the sanctioned history and the 'history' of the 'tissue of tenacious errors', the determination of these two histories remains a question which is not posed. '... the conjunction of the two histories and their reciprocal determination remains in the shadow.' (32) The impossibility of posing this question in Bachelardian discourse derives in the last instance from petty-bourgeoise ideology.

The 'tissue of tenacious errors' is thought through what Bachelard calls a 'psychoanalysis of objective knowledge.' This recourse, as Lecourt has demonstrated involves psychologism. This descriptive 'psychoanalysis' is a substitute in Bachelard for the theory of the instance of ideology. The tissue of errors is dissipated within the 'scientific community' (conceived as 'inter-subjectivity') (33), thus relating these errors to a division within the scientist as subject. The entrance into relation with other scientists within this community dissipates these errors, they are deposited at the door of the hall of science.

The effect of this ideological construction on the Bachelardian concept of epistemological break is to reduce it to a descriptive category. The break has no mechanism of production, it remains rooted in a descriptive comparison of scientific and pre-scientific. Thus, the specificity of the break, the particular conjuncture in which it occurred cannot be grasped in Bachelardian epistemology. The break is conceived as being with a continual atemporal set of obstacles and it is notable that the only instance to which these obstacles are related is that of education (conceived without reference to the state). (34) The scientific community remains a given for Bachelard enforcing a history departing from this given, a necessity which ensures the impossibility of the concept of the problematic of the science of the past and the conflation of the epistemological break and the re-organisation of the problematic pointed out above.

This reprise involves a correlative reduction of the concept of philosophy in Bachelardian epistemology, for while 'effects' of philosophy may be grasped in this discourse, the concept of a specific instance of philosophy disappears. Philosophy is interiorised within the descriptive field of epistemological obstacles, its

specificity is merely that of conveying epistemological obstacles through the operation of the category of 'general culture'. Thus, in this conception if philosophy has no history it is because the epistemological obstacle has no history, there is a conflation of the instance of ideological philosophy and the instance of ideology in general. As a result, philosophy must be seen as an instance which constitutes an obstacle in the conjuncture of the break rather than an instance created by the dislocations (double dislocation order of discourse/order of exposition, science/revolution in the theoretical) inaugurated by the break. As a result Bachelardian epistemology is incapable of thinking the political/theoretical conditions of its own exstence.

Epistemological Break and the Theoretical History of the Sciences.

The limits of Bachelardian epistemology appear founded on the absence of the possibility of defining the nature of the autonomy of science, for as has been demonstrated this autonomy always appears as a given in the Bachelardian discourse. Science is not a practice for Bachelard, for the epistemological break remains a descriptive category. In contrast, in the Dialectical Materialist concept of science the epistemological break is the mechanism of production of a new practice. This is possible firstly because Dialectical Materialism situates the epistemological break in a defined conjuncture of the over-determined relation of instances (theoretical, political, economic, ideological.)

However, it must be stressed that as the history of the sciences is not a history 'like all others', so this conjuncture is not a conjuncture 'like all others'. Furthermore, we cannot think the specificity of this conjuncture merely by pointing out that it inaugurates a 'new practice' but the new practice of science. That is, it inaugurates a practice which is not relatively autonomous but autonomous. Marxist science demonstrates that the inauguration of a new practice which is relatively autonomous is an effect of a particular conjuncture; the instance of the political level was an effect of the conjuncture produced in the rupture between primitive communism and class society.

To think this specificity we must analyse the conditions of possibility of the production of this new relatively autonomous instance. This possibility derives from the fact that the conjuncture is an effect, ie it is produced. But this effect is not the empiricist-positivist effect of the 'interaction' of the empirical elements, for if this is maintained the conjuncture effect is structured only by its empirical elements. Structuration is reduced to the contingent 'pattern of interaction' of these elements and in the same way, the structuration 'in dominance' of the elements is not governed in any way: the dominance is contingent. Marxism rejects this empiricism. The structure in dominance must itself be structured. The Marxist concept of structure refers not to the play of the empirical elements but always to a structure ('in dominance') which is already structured. The structuring governs the structuration of the structured (structure in dominance) and therefore is capable of changing the dominant instance in that structure, or 'displacing the dominant'. The effect of the inauguration of a new relatively autonomous practice takes the following form: - the displacement of the dominant produces an effect whereby the conditions of existence of the totality are transformed, that is, the re-structuration of the totality has the inauguration of the new practice as its condition of existence. That is, the new practice overdetermines the totality and is

necessarily overdetermined by it, the new practice is relatively autonomous.

The conditions of the inauguration of a new autonomous practice (science) are different. This inauguration does not correspond to the displacement of the dominant, that is, sciences may arise in formations with different hierarchies of dominance, different modes of production. Correlatively, the inauguration of the new practice of science is not a condition of existence of the totality. The conjuncture of the epistemological break is effected in a situation of the 'overdetermined relation of instances' but the inauguration of the practice of science is not a condtion of existence of those instances, for if it were, the inauguration of the instance of science would eradicate the instance of ideology but this is impossible for if science were to have this effect it would have to cease to be science for it would be overdetermined by the other instances. So, science is absolutely autonomous, it is not part of the social formation, it is not 'in the superstructure.' Yet, here it will be argued we have maintained a transcendental autonomy for science, we have produced a Platonic dualism, a dichotomy between the real and the ideal. This is not the case, for the effective existence of science is not derived from a transcendental assumption, on the contrary, the effective existence of science is its effective continuation as a practice. This effective continuation is in no way guaranteed externally. However, the determinants of the attenuation of the continuation of science do not arise in science itself. They arise because science is represented in the social formation as an instance but not as the instance of science. That is, science cannot have the character of determinant in the last instance, for science has two modes of existence in the social formation, as an effect and as a representation. In the former case, the effect of science is always in combination with another practice, eg in its combination with politics its intervention is determined by a prior displacement of the dominant by the determinant instance, the economy. (The mode of existence as representation has been outlined above.)

Science is represented in an instance which is 'other than itself', it is represented in philosophy. Here we face extremely difficult problems to whose answers only indications can be given.

This representation in another instance is not simple (it is not a reflection) for the representation is effected through the space of the new instance (philosophy). The constitution of this space derives from the instance which poses, in this space, a new question: that of the nature of knowledge. (More specific indications on the mechanisms of this process are given below.) It is the posing of this question which engenders the necessary double representation of the representation of science, for science may be represented as knowledge, that is, as the practice which produces knowledge or, it can be represented as 'posing the question of knowledge'. In the latter case, the representation of science is in the space of the ideological practice of philosophy, for science is represented as posing a pseudo-problem, for insofar as there is knowledge in this sense, it is scientific knowledge. Thus, the representation is effected through a denegation, that is the problem of science which is posed explicitly (eg science as a partial knowledge) is denied in the question which is effectively posed, that science creates a 'problem of knowledge', for once this 'science' has posed the 'problem' it no longer exists.

In contrast, in the first case the representation is effec-

ted in the space of the 'new' practice of philosophy, the new practice inaugurated by Lenin. Lenin's break with previous philosophy inheres in thinking (in a practical state) this representation of science, as science. This is achieved through the distinction of philosophical categories and scientific concepts, for the representation of matter in philosophy and in science is distinguished. This 'intervention' is doubly articulated, 'Philosophy is a practice of political intervention carried out in a theoretical form.' (35) 'It intervenes essentially in two privileged domains, the political domain of the effects of the class struggle and the theoretical domain of the effects of scientific practice.' (36) Althusser describes this intervention in the theoretical domain of the effects of scientific practice in the following terms: 'The Marxist-Leninist revolution in philosophy consists of a rejection of the idealist conception of philosophy (philosophy as an 'interpretation of the world') which denies that philosophy expresses a class position, although it always does so itself, and the adoption of the proletarian class position in philosophy, which is materialist, ie, the inauguration of a new materialist and revolutionary practice of philosophy which induces effects of class division in theory.' (37)

This intervention 'induces' effects of class division in theory because it articulates the demarcation science/ ideology (theoretical articulation of the new practice of philosophy) in combination with the demarcation Marxism/ bourgeois class position. To illuminate this double articulation we must turn to Althusser's answers to Maria Antonietta Macciocchi's questions in the interview given in 1968 and published under the title Philosophy As A Revolutionary Weapon.

Macciocchi asks 'You have said two apparently contradictory or different things:(I) - philosophy is fundamentally political; (2) - philosophy is linked to the sciences. How do you conceive this double relation?' (38) Althusser's replies may be taken up in the following theses, (a) class positions which confront each other in the class struggle are represented in 'practical ideologies' by 'conceptions of the world'; (b) these conceptions of the world are represented in the domain of theory (sciences and theoretical ideologies) by philosophy, philosophy represents the class struggle in theory: (c) philosophy as a representation of 'conceptions of the world' in the domain of theory is engendered by science, this 'representation' comes 'after the event' of science; (d) Marx founds a new science, the science of history (e) this science produces knowledge of ideology and thus knowledge of philosophy.

These theses demonstrate the nature of the 'new' practice of philosophy, for the specificity of the new science in respect of the domain of the theoretical derives from the fact that this new science produces completely new knowledge, knowledge of ideology, knowledge of philosophy. Previous sciences did not produce that knowledge, they could not engender a knowledge of philosophy, but historical materialism produced that knowledge and thus Dialectical Materialism became the first philosophy which could distinguish between science and ideology. It could draw lines of demarcation because it represented that knowledge produced by Historical Materialism. Thus, this knowledge of ideology was represented by Lenin in a double demarcation, between science and ideology (the new practice of philosophy represents the difference between science and ideology), and between philosophy and science (the new practice of philosophy represents the difference between philosophy and science). But this theoretical articulation is in combination with a political articulation for philosophy represents the protetarian class struggle

in theory because it represents the effect of the new science (Historical Materialism) in politics, that is, that the new science engendered the possibility of scientific socialism. That is as a politics articulated in combination with science, revolutionary politics was no longer restricted to a basis in ideological positions (utopian socialism, revisionism), a unity of ethical ideology (the ethical imperative to socialism) but was based on the combination of class position and scientific theory. This new combination is represented in the necessity to draw lines of demarcation in respect of this combination, in contrast to the pre-marxist ideology of demarcation on the basis of ideological position alone. Revisionism denies the possibility of this combination for it 'replaces' it with the postulate of the division between science (contemplative, positivist) and politics (unified around ideology, the need for 'faith', the postulate of the compatability between Marxism and religion etc.)

This modality of the philosophical intervention is grounded on a specific effect of the new science at a particular level. That is to say, the effect of knowledge of ideology which is represented at the level of the domain of the theoretical. However, philosophy is a double intervention, for it also represents scientificity in politics.

Again, this is a representation of an effect of science at a particular level. The effect of the science here is the effect of producing a concrete analysis of the conjuncture. This effect.... is represented philosophically in the distinction abstract/concrete, in the demarcation between Marxist and revisionist positions (deviations). Revisionist deviations reproduce an abstract analysis which ignores the balance of forces in any particular conjuncture, its speculative character reproduces ideological philosophy's 'interpretation of the world'. This can be seen clearly in Lenin's criticism of Kautsky's concept of 'ultra-imperialism'. Kautsky outlines his position in the following terms '..... Cannot the present imperialist policy be supplanted by a new, ultra-imperialist policy, which will introduce the joint exploitation of the world by internationally united finance capital in place of the mutual rivalries of national finance capitals? Such a new phase of capitalism is at any rate conceivable. Can it be achieved? Sufficient premises are still lacking to enable us to answer this question.' (39) Lenin replies, 'The question has only to be presented clearly for any other than a negative answer to be impossible. This is because the only conceivable basis under captialism for the division of spheres of influence, interests, colonies, etc, is a calculation of the strength of those participating, their general economic, financial, military strength, etc.' (40) The abstract 'analysis' is invalid because its speculation is effected without reference to specific balance of forces, its possibility is the possibility of no particular situation, in contrast, the concrete analysis allows the development of a strategy in respect of the specificity of the 'current situation'.

Thus, on the basis of this analysis we may signal the radical difference between the conjuncture effect in politics and philosophy (instances in the social formation) from what we have called conjunctural effect below. The latter effects are purely internal to the science itself and are quite distinct from the point of conjuncture of the break outlined above (eg in the conjuncture effect of the point of the break, the area overdetermined is transformed into the conjunctural effect of the science, knowledge of a particular area of theoretical problems which is no longer limited to those problems because those concepts are thought with an appartus of concepts thinking the specific

thought object of the science and thus capable of extension).

The break is in no way an origin, for the break cannot be referred to any essential unity of subject-object. The category of origin depends on this unity for its 'privilege' depends on the original moment having the character of an 'essential moment', of the 'knowledge' produced at that moment having the character of the 'essence' of knowledge in general. This is because the moment of the 'origin' is the moment of irreducible subject-object relation, that is of the presence to itself of knowledge in all its determinations. Historical Materialism recognises this relation (subject-object) at the interior of its discourse not as the irreducible kernel of a theory of knowledge but as an effect of the practice of ideology, the work of the category of subject to impose an immediate recognition/misrecognition structure. The relation subject-object is no longer a given but is thought at the interior of a theoretical discourse.

The conjuncture of the epistemological break is not an origin. The conjuncture of the break is characterised not by the presence of knowledge to itself in all its determination, in which case the break would be conceived as a voluntarist 'leap into science' but on the contrary by a series of dislocations. This is in no way contingent, it is not a question of 'good' and 'bad' conjunctures, the good producing felicitously an 'even' generality III, the bad an uneven. It is necessary, that is necessary in respect of the apparatus of concepts which think this necessity (the concepts of theoretical practice, the concept of the generalities). This is easily demonstrated for if the evenness of generality III is maintained then the epistemological break collapses again into the origin. The reason for this unevenness derives from the fact that the discourse of the science proceeds by particular demonstrations, the specificity of the object of science inaugurates a specificity of demonstration in relation to specific concepts, and this specificity precludes speculative discourse.

The operation of the concept of epistemological break involves the posing of a process where science derives from a break with ideology, not any ideology but the structure of 'successively encountered configurations'; the break is effected primarily in the specific theoretical space of those configurations. The conjunctural effect means that the totality of the science is not problematised; a break is around the theoretical objects constituted in a certain space.

The definition of a scientific object is therefore effected primarily in this theoretical space (for example, in Galileo the central theoretical space of the concept of movement is not transformed in the break but rather the means of thinking movement). The inauguration of the order of discourse of a science is not a once and for all order but the order of a specific conjuncture within scientific discourse, to think otherwise would be to fall back into the idealist illusion of the essential section.

The conjunctural effect of the unevenness of generality III involves a dislocation between the science and the concepts adequate to think that science. The specificity of the space in which the break is effected involves the non-presence of the order of discourse of the science as an object of that science (conjunctural effect I). Similarly, this specific theoretical space involves a dislocation between the object of the science and the concepts adequate to think that object, which are thus interiorised within the science 'in a practical state', (conjunctural effect 2). These two conjunctural effects define the specificity of the mode of materiality of scientific discourse, that is, order of exposition (eg the development of 'forms' in Capital), order of proof. These two orders reflect the specificity of the object of

science insofar as they present demonstrations in a specific theoretical space and insofar as the mode of demonstration is that of exposition/development and proof (demonstration of the effectivity of concepts in a specific theoretical area). These orders are deployed to produce the knowledge effect.

Correlative with these two conjunctural effects is the effect of the concept of practice. The break is effected by an apparatus of concepts involving the interiorisation of the subject within the process of production of knowledge. Thus, the conjunctural effect signalled by the concept of practice is that of the specificity of the break defined not by a subject but by concepts, ie the materiality of thinking a scientific thought-object. The concept of practice cannot be co-present with the category of constitutive subject. This co-presence is impossible because the presence of the constitutive subject denotes the existence of an 'essential moment', that is a privileged point for which any other 'moment' is either a re-production of the essential moment or a deviation from it. However, as it has been demonstrated above the moment of 'deviation' has no independent existence for it is itself a 'moment' of the essential moment (all crises in science are moments of the non-presence of the teleologically necessary essential section). In the same way, the necessary interdependence of the structure in dominance and determination in the last instance is denied by the category of constitutive subject, for the distinction has no meaning for an idealist 'theory of knowledge', that is the essential determination of an idealist theory is always determinant and empirically present. This 'epistemological privilege' of the conjunctural effect must be maintained against any anti-humanist ideology of science which necessarily lacks this concept. Anti-humanism departs from a denial of the constitutive human subject in the absence of an interiorization of the subject within a practice, thus this denial of a particular subject substitutes itself for the theory of the operation of the category of subject and its effects. As a result, the concept of totality remains unthought and the denial of the constitutive human subject involves the substitution of 'extra-human' constitutive subjects conceived on the 'model' of the human subject (automata, nature etc.) (41) In the most literal sense the anti-humanist ideology is a denegation of the subject. (42)

The conjunctural effects outlined above do not exhaust the effects of the epistemological break for these effects engender a new space that of the instance of philosophy. This 'space' is occupied by the Kampflatz of philosophy, the struggle between materialism and idealism. At this point an outline of the relation of the terms of this struggle and the necessary dislocations and unevenness engendered between them by the epistemological break must be demonstrated.

The new 'Space' and its Constitution.

To demonstrate the nature of this new space we shall take up three necessary dislocations engendered by the epistemological break and the respective positions of materialist and idealist philosophies in respect of this dislocations.

(a) Dislocation I: order of discourse/order of exposition. In respect of this dislocation the position of idealist philosophy is thought through the category of 'occultation' (Husserl) or technisation of science. This dislocation is represented in idealist philosophy as an absence of immediate presence of essence in existence. That is to say, the necessary dislocation within the scientific discourse (text) is displaced on to a dislocation between science and

philosophy, science conceived as a partial (technical) knowledge whose determinations lie in philosophy. Idealist philosophy exhibits the unity of empiricism and speculation. Empiricism for idealist philosophy departs from a given effect of the dislocation, non-transparency of the order of discourse in the text, speculation for this effect is interiorised within the speculative category of partial knowledge, founded on the speculative essence of the subject (subject as present to himself, knowledge as reflected essence in existence). In contrast to this philosophical reprise materialist philosophy affirms the necessity of the dislocation through the concept of knowledge effect, specific effect of science, order of exposition as necessary form of material existence (materiality) of the knowledge effect of science. 'The theory of the knowledge effect has as its object the thematization of the unity-difference, the 'dislocation' (Reading Capital p. 68) between the order of combination of concepts in the system and their order of presentation in scientific discourse; all the difficulties relate to the fact that the second order is not in any way the route to the first nor its repetition, but its existence, existence determined by the same absence of the system, and by the immanence of this absence: its non-presence at the interior of its very existence.' (43)

(b) Dislocation II: science/revolution in the theoretical.
The intervention of idealist philosophy in respect of this necessary dislocation is thought under the category 'crisis in science'. Again idealist philosophy demonstrates the same empiricism/speculation unity. The necessary dislocation between the instances of science and philosophy, '.... Philosophy is not a science. Philosophical categories are distinct from scientific concepts.' (44), deriving from conjunctural effect and the specificity of the scientific object is taken as a given by idealist philosophy. This is thought under the speculative category of crisis in science as alienation of consciousness. This 'crisis' may be that of the progressive development of science within the field of its specific thought-object (materialist category, development of science) as a dislocation (a moment) between science and 'our world' (Husserl). Or the crisis may be thought as a mutation between the philosophical instances themselves, the development of science (eg re-organisation of the problematic between classical mechanics and relativity theory) is seen by idealist philosophy as the 'end of materialism'. This crisis engenders a 'period of doubt (Poincaré) (45) yet as has been demonstrated this 'doubt' is merely that of an already constituted idealist 'theory of knowledge', in this particular instance that of a sensationalist-idealist theory of knowledge. In contrast to this ideological reprise materialist philosophy demonstrates the distinction between the re-organisation of the problematic and the instance of philosophy. 'Matter disappears' means that the limit within which we have hitherto known matter disappears and that our knowledge is penetrating deeper: properties of matter are likewise disappearing which formerly seemed absolute, immutable, and primary (impenetrability, inertia, mass, etc) and which are now revealed to be relative and characteristic only of certain states of matter. For the sole 'property' of matter with whose recognition philosophical materialism is bound up is the property of being an objective reality, of existing outside the mind.' (46) Materialist philosophy seizes the materiality of the distinction of instances fused in the speculative essence of 'knowledge' in idealist philosophy.

(c) The uneven development of generality III.
This reprise of idealist philosophy in respect of the unevenness of generality III derives from the operation of the couple subject-object as an ideological displacement of the relation concepts-object. Idealist philosophy conceives this unevenness under the category of partiality/alienation of consciousness. The ambiguity of the word object allows the surreptious substitution of the empirical object for the thought object. Idealist philosophy thinks the appropriation of the (empirical) object under the form of specific (partial) knowledge, requiring the supplementation of the other 'aspects' of the determination of the subject-object relation. The effect of the process of displacements is that the finitude of the thought-object is displaced onto the knowledge. The unity of speculation/empiricism appears again in the couple multiple determinations of knowledge/given empirical object. 'Need I comment that the theoretical characters cast in this ideological scenario are the philosophical Subject (the philosophizing consciousness), the scientific subject (the perceiving consciousness), and the empirical subject (the perceiving consciousness) on the one hand; and, on the other the object which confronts these three Subjects, the transcenental Object, the pure principles of science and the pure forms of perception; that the three Subjects for their part are subsumed under a single essence (eg, this identification of the three Objects as it is seen with significant variations, in Kant, as well as Hegel and Husserl, depends on a persistant identification of the object perceived and the object known);.....' (47) The materialist intervention grasps the materiality of the of the mode of production of the knowledge effect, the relation concepts-object against the idealist recourse to the real concrete and its dependence on a speculative 'essence of knowledge.' (48)

Conclusion
In conclusion the central role of the concept of epistemological break for a theoretical history of the sciences derives from the following effects:

(a) The epistemological break thinks the development of the science, as an objective development (interiorization of the subject, concept-object relation as constitutive relation.)

(b) The epistemological break defines a singular objectivity, that of a specific science (finitude of the object as specific knowledge effect).

(c) The specificity of this development defined by the concept of epistemological break engenders the necessity of thinking the objectivity of a particular science through its specific concepts-object relation. The history of the sciences takes the specific objectivity of the specific sciences within the differential field of the sciences as its object. The category of 'general' objectivity of development is not one of its concepts, it is a philosophical category, a distinction only possible through the effects of the concept of epistemological break. M. Fichant expresses this relation in respect of the science of mathematics. 'There is a mathematically founded objectivity of mathematical development (49): this statement is a philosophical statement. This truism involves two others: this is not a mathematical statement; it is not a statement in the history of mathematics. It is a statement which defines the relation between mathematical statements and the statements of the history of mathematics - therefore between a history and its object. It is necessary to formulate these truisms in order to avoid the misconception which would make us see in this statement, the reduction of the history of a science to that of the science itself, an absorption of the history of mathematics into mathematics. What is founded mathematically is not mathematical development. Mathematical development is not the development of a pre-given structure which will be mathematics in itself..... what is founded mathematically is the objectivity of this

development . . . . . ' (50)

In the last instance, the function of the concept of epistemological break is to mark out the space between the theory of the production of knowledge and the history of the sciences on one hand and the idealist 'theory of knowledge' on the other. 'A doctrine of science cannot be presented by a philosophy of consciousness but by a philosophy of the concept. ' (51)

Notes.

(1) The phrase 'history of science' will denote an ideological discourse, following the usage of M. Fichant in his text published in this issue.
(2) A topic taken up in Barry Hindess' paper published in this issue.
(3) The crucial importance of the Bachelardian concept of 'phenomeno-technique' may be signalled here, see Dominque Lecourt L'Epistemologie Historique de Gaston Bachelard Second Edition (Paris J. Vrin 1970) pp. 66 - 69, for an index of sources to this concept in Bachelard's work see the reference to 'phenomeno-technique' in the index of Bachelard's principle concepts, op. cit. p. 110. See also, the Introduction to Bachelard's Le Matérialisme Rationnel Second Edition (Paris Presse Universitaires de France, 1963). On the distinction between the history of technology and the history of scientific instruments see A. Koyré Du Monde de L'à peu Près A L'Univers de la Précision Critique, Tome IV: No 28, Sept. 1948, and for a brilliant study on the impact of scientific discovery on the production of scientific instruments see A. Koyré Concept and Experience in Newton's Scientific Thought in his Newtonian Studies (London Chapman and Hall 1965) pp. 40-43 and in particular p. 43 footnote 1.
(4) On this point see L. Althusser and E. Balibar Reading Capital (London New Left Books, 1970).
(5) Althusser and Balibar op. cit. p. 37 (emphasis in original).
(6) ibid pp. 18 - 24.
(7) ibid Part II section 4.
(8) ibid p. 93 (emphasis in original).
(9) ibid p. 96.
(10) M. Fichant op. cit.
(11) It should be noted, however,that the image of the table is related in the case of 18th century philosophy to an asymtotic conception of knowledge, another variant of empiricism, see Althusser and Balibar op. cit. pp. 81 - 82.
(12) Althusser and Balibar op. cit. p. 53.
(13) G. Bachelard Le Rationalisme Appliquée Third Edition (Paris Presse Universitaires de France, 1966) p. 51 (emphasis in original).
(14) Althusser and Balibar op. cit. p. 53.
(15) J. Derrida Introduction to E. Husserl L'Origine de la Géométrie (Paris: Presse Universitaires de France, 1962) p. 10.
(16) Husserl refers to Plato's notion of logic governing science.
(17) E. Husserl Logique Formelle et Logique Transcendentale (Paris: Presse Universitaires de France 1965) (second edition) pp 4-5 (my emphasis).
(18) ibid p. 6.
(19) E. Husserl Ideas (London: Collier Macmilliam, 1962) p. 58 (emphasis in original).
(20) ibid p. 57 (my emphasis).
(21) Althusser and Balibar op. cit. p. 40.
(22) For the relation of this reprise to the 'Early Works' of Marx see Jacques Ranciere The Concept of Critique . . . (Part One) Theoretical Practice No. 1, Jan. 1971.
(23) See L. Althusser On the Materialist Dialectic in his For Marx (London: Allen Lane 1969) and Althusser and Balibar op. cit. p. 90.)
(24) D. Lecourt op. cit. p. 23.
(25) G. Bachelard Essai Sur La Connaisance Approchée (Paris: J. Vrin 1928) cited in Lecourt op. cit. p. 23.
(26) scientific knowledge.
(27) Derrida op. cit. p. 48.
(28) Althusser and Balibar op. cit. p. 96.
(29) M. Fichant op. cit.
(30) The basic distinction between these two concepts derives from the fact that the Bachelardian concepts sets the parameters of recurrence in respect of the current problematic of science, thus the 'science of the past' exists only as correct elements of a past problematic which cannot be grasped and simultaneously as 'part of the current problematic'. In contrast the scientific concept of recurrence functions as a tool defining the nature of this problematic of the science of the past.
(31) This conflation does not derive from the failure to think the past of science per se in Bachelardian discourse, but from the failure to think the problematic of past science
(32) D. Lecourt 'Du Bachelard Au Matérialisme Historique' in L'Arc 42 p. 13.
(33) ibid.
(34) ibid.
(35) L. Althusser Lénine Devant Hegel cited in P. Hirst Althusser and Philosophy Theoretical Practice No. 2 (emphasis in original) April 1971 p. 24.
(36) ibid.
(37) ibid. (my emphasis).
(38) Philosophie Comme Arme De La Révolution La Pensee April 1968 Question 5 p. 51 English translation New Left Review 64 Nov/Dec 1970.
(39) K. Kautsky article in Die Neue Zeit April 30 1915 cited in V. I. Lenin Imperialism, The Highest Stage of Captialism in Lenin Selected Works Vol. 1 (Moscow: Progress Publishers 1967) p. 769.
(40) ibid p. 771.
(41) On this point see Alain Badiou Le Concept de Modèle (Paris: F. Maspero 1969).
(42) see the Glossary, Althusser and Balibar op. cit. p. 312.
(43) A. Badiou Le (Re) Commencement Du Matérialisme Dialectique Critique Tome XXIII No. 240 May 1967 p. 453. See also M. Gane Althusser in English Theoretical Practice No. 1 Jan 1971 p. 8.
(44) L. Althusser Lénine et Philosophie (Paris F. Maspero 1969) p. 35.
(45) V. I. Lenin Materialism and Empirio-Criticism Fourth Edition (Moscow: Progress Publishers 1967) p. 241.
(46) ibid p. 248.
(47) Althusser and Balibar op. cit. p. 55 (my emphasis).
(48) It should be noted in this respect that any recourse to the real concrete in the definition of the finitude of the scientific object represents an idealist reprise, this recourse has had a great deforming effect on Marxism, see Althusser and Balibar op. cit. pp. 81-2 and F. Engels Letter to Conrad Schmidt in K. Marx and F. Engels Selected Correspondence Second Edition (Moscow: Progress Publishers 1965) pp. 481-485.
(49) Jean Cavaillès Philosophie Mathématique (Paris Hermann 1962).
(50) Fichant op. cit.
(51) Jean Cavaillès Sur Le Logique et La Théorie de La Science Second Edition (Paris: Presse Universitaires de France 1960) p. 78.

MATERIALIST MATHEMATICS

by BARRY HINDESS

The practice of a science is a theoretical practice: a process of transformation of a determinate given raw material (representations, concepts, facts) into a determinate product. (1) As such it differs from other, non-theoretical, practices in the type of object (raw material) which it transforms, in the type of means of production which it sets to work, and in the type of products which it produces (knowledges). The objects of the sciences are theoretical objects, the objects of concepts: the object of a science is the object of its concepts. These concepts do not correspond to a given real object of which they are merely an abstraction. The process of production of knowledge takes place within knowledge. It does not take place by an act of abstraction whereby a given subject extracts the essence of a given real object. (2)

Scientific practice is its own criterion. It contains within itself definite protocols with which to validate the quality of its product, ie the criteria of the scientificity of the products of scientific practice. Once they are truly constituted and developed the sciences 'have no need for verification from external practices to declare the knowledges they produce to be 'true', ie to be knowledges. No mathematician in the world waits until physics has verified a theorem to declare it proved, although whole areas of mathematics are applied in physics: the truth of his theorem is a hundred per cent provided by criteria purely internal to the practice of mathematical proof, hence by the criterion of mathematical practice, ie by the forms required by existing mathematical scientificity.' (3) The same holds for the results of every science; they themselves provide the criterion of the validity of their knowledges.

The internality of the forms of proof and of demonstration is characteristic of all the sciences. There is no difference in this respect between those sciences commonly called 'experimental' and the rest. In fact the distinction between experimental, or empirical, sciences (physics, chemistry, biology, etc.) and the formal, or non-experimental, sciences (logic, mathematics) belongs essentially to empiricist philosophies of science: ie to philosophies which seek an extra-scientific guarantee of the truth of scientific knowledge. (4) In these conceptions experimentation is presented as an operation of comparison, which may be more or less direct, between scientific theory on the one hand and the given real object on the other. Scientific instruments are presented as extensions of the senses which, under suitably controlled conditions, enable the scientist to see the given real object more clearly, to measure it more precisely, and so on.

Such conceptions present a difference between the sciences as resulting from a specific difference between their objects: the experimental sciences are concerned with real objects: the formal sciences are not - they are concerned with formal or ideal objects (Husserl) or they are not concerned with objects at all (Russell, Carnap, Popper). (5) Within the 'experimental' sciences there is a related distinction between 'real' experiments and 'thought' experiments: the former being characterised by the effective presence of the real, the latter by its absence. These conceptions displace and distort a real difference between the objects of the different sciences: the object of mathematics is the object of mathematical concepts, the object of physics is the object of the concepts of physics, and so on. The objects of the different sciences are distinct objects not because some are real and others are not, but because they are the objects of different problematics.

Experimentation is a form of the theoretical practice of the sciences. (6) Scientific instruments, the instruments of experimentation, are means of scientific production. A scientific experiment is a determinate operation performed on a determinate raw material with determinate means of production. All sciences are experimental. Mathematics is an experimental science. (7)

All variants of the empiricist problematic , ie all conceptions of knowledge as a process that takes place between a given subject and a given real object, (8) must assign to mathematics a special, either privileged or underprivileged, place among the sciences - since it is a science with no corresponding real object. Such assignments necessarily involve specific ideological distortions of mathematics and of all of the other sciences (misrepresentation of theory as the specifically mathematical part of science, correlative misrepresentation of experiment as essentially non-theoretical). Mathematics is presented as a tool, an ideal language (Carnap), and thus merely as an instrument of the other sciences; as an abstract representation of the real (Kant) leading to attempts to found mathematics on the non-mathematical (Frege, Russell, Husserl); (9) and so on.

These ideological distortions can produce real obstacles to scientific practice: the various 'crises' in mathematics induced by ideological interventions following upon, eg, the introduction of irrational numbers at the time of Plato, imaginary numbers and infinitesimals in the 16th and 17th centuries, and, more recently, the development of set theory by Cantor and others. (10) This last 'opened a 'crisis in the foundations' of a rare violence, which continued to shake the mathematical world for more than 30 years, and seemed at times to compromise, not only all recent acquisitions, but even the more classical parts of mathematics.' (11) Brouwer and his school, to take just one example, attempted a complete refounding of mathematics upon sound 'intuitionist' principles. For Brouwer mathematics is identical to the 'exact' part of our thought, based on the primary intuition of the sequence of natural numbers (the integers) There is no possibility of translating our exact intuition into any language, not even the mathematical language of formal systems. A demonstration is conclusive, not because it follows specified rules of deduction, but because each of its steps is immediately evident to our intuition. (12) Intuitionism introduces an ideological division of the continent of mathematics into parts that are safe (guaranteed by intuition) and parts that are not: the latter contain, eg the bulk of Cantor's set theory, the whole of transfinite arithmetic, large areas of analysis.

The effects of such ideological incursions threaten the very existence of mathematics as a science. Its defense takes place on a number of levels: scientific repudiation of, eg various doctrines of limitation (Godel's demonstration that, if the theory of sets without the 'doubtful' axiom of choice is consistent, then the theory with this axiom is also consistent) or of the 'logicist' programme

of founding mathematics upon logic; (13) the philosophical critique of ideologies by mathematicians - whose 'spontaneous' philosophy often conflicts with their explicit philosophical allegiance; (14) the materialist practice of philosophy consists precisely of the philosophical defense of the sciences against ideological incursions.

The Concept of Model is an example of the materialist practice of philosophy. In this important text Badiou reflects the object and concepts of mathematics as a science, through the reflection of a crucial region of mathematics: mathematical logic and, within this, the theory of models. Ideological representations of this region open up the whole continent of mathematics to realist and speculative misrecognitions. In particular, the positivist category of model, plays a crucial role in contemporary neo-positivist epistemology and philosophy of science. Badiou's object is to demarcate between the scientific concept of model and philosophical and ideological representations of this concept.

A point on terminology: it is necessary to make a rigorous distinction between ideological notions, scientific concepts, and philosophical categories.

A concept is a scientific concept. If the object of a science is the object of its concepts, a concept is always a concept of such an object. The same word may appear in the discourse of more than one science. In that case it represents different concepts: different because they are the concepts of different objects.

'Philosophy is not a science, and it has no object, in the sense in which science has an object.' (16) Philosophy consists in the ideological representation and reflection of the sciences through the elaboration of scientific concepts into philosophical categories (idealism) and in the defense of scientific practice against such ideological incursions through the elaboration of categories which provide firm epistemological foundations for scientific practice (materialism). Idealist philosophical categories necessarily involve a specific ideological distortion of scientific concepts- a necessity imposed by the invariant structure of the empiricist problematic which always counterposes a given subject to a given real object. (17) Each science is constituted in rupture with ideology in the production of a new, open and specific, theoretical problematic. The defense of scientific practice is the defense of an open, specific problematic (the problematic of a specific science) against the closed problematic of the ideologies.

Ideological notions are neither concepts nor categories. They appear in discourse which is purely ideological, they are not the result of materialist or idealist philosophical elaborations of scientific concepts into categories. Purely ideological representations of science are not categorical. (18)

A materialist epistemology of mathematics must distinguish four significant uses of the word 'model': (19)

'(1) notion: knowledge is the representation of the real-empirical-given by means of models.
(2) concept: (mathematical) theory of models.
(3) category 1 (positivist): the real- empirical furnishes semantics for the syntax proposed by the 'pure' sciences. Experimentation is an evaluation-realisation.
(4) category 2 (dialectical materialist): all sciences are experimental. Mathematics is a doubly articulated process of production of knowledge (1. by the system of the hierarchy of concepts; 2. by the order of inscription of the proof) (20). Model designates the conceptual articulation with respect to a particular experimental apparatus: a formal system. 'Formal system' thus designates the experimental articulation or inscription. Articulation 2 is enveloped by articulation 1: the understanding of formal mathematical constructions is deployed in the conceptual practice of mathematics itself.' (21)

These differences between the concept and the notion of model, and between the materialist and positivist categorical representations of the concept, are established in Badiou's text. He begins with an examination of the notion of model and of the positivist category of model. The former (the purely ideological use of the word) has little to do with any scientific concept, but it occupies a central place in the methodical discourse of several contemporary pseudo-sciences - especially economics and the so-called behavioural sciences. The semantics of logical positivism on the other hand involves a categorical elaboration of the scientific concept of model. A preliminary sketch of the concepts of model, formal system, syntax and semantics, which belong to the discourse of mathematical logic, enables Badiou to establish the precise difference between the positivist category and the notion.

The positivist category is the product of a specific philosophical elaboration of the scientific concept of model. Only a construction of this concept can establish the specific character of the deformation, circumscription and limitation of the scientific concept by this idealist categorical elaboration. This paper follows Badiou's text in presenting a sketch of the construction of the concept together with an epistemological commentary. The text closes with a discussion of mathematical experimentation - the possibility of which is denied in the positivist category - and with the introduction of the materialist category of model.
This paper is a presentation of, and commentary upon, the ideological notion, scientific concept, and philosophical categories examined in Badiou's text. It does not attempt to follow his exposition.

1. The ideological notion and the positivist category of model.

(i) the empiricist conception of knowledge

'The empiricist conception of knowledge presents a process that takes place between a given object and a given subject. At this level, the status of this subject (psychological, historical, or otherwise) and of this object (discontinuous or continuous, mobile or fixed) is not very important. This status only affects the precise definition of the variants of the basic problematic, ... The whole empiricist process of knowledge lies in fact in an operation of the subject called abstraction. To know is to abstract from the real object its essence, the possession of which by the subject is then called knowledge. Whatever particular variants this conception of abstraction may adopt, it defines an invariant structure which constitutes the specific index of empiricism.' (22)

Within the empiricist problematic scientific knowledge appears as the formal representation of its given real object: what is abstracted from the real object is the logical or mathematical form of its scientific representation. The presupposed difference of given facts and the logical form of their representation is the common motor of an infinite variety of more or less sophisticated ideological discourses.

Carnap, for example, explicitly poses the difference between the formal sciences (logic and mathematics) and the empirical or factual sciences. There are no 'formal' or 'ideal' objects of the formal sciences corresponding to the 'real' objects of the empirical sciences. 'The formal sciences do not have any objects at all; they are systems

of auxiliary statements without objects and without content.' (23) The value of the formal sciences is that they enable us to supplement the language of pure observation with 'theoretical' terms. A scientific theory may contain terms that have no direct empirical referent but which are nevertheless related to observation terms by the mathematical structure of the theory: that is, 'observable' consequences can be deduced from statements containing 'theoretical' terms. The language of science can be divided into two parts: the observation language (L_O) and the theoretical language (L_T). 'The L_O uses terms designating observable properties and relations for the description of observable things or events. The L_T, on the other hand, contains terms which may refer to unobservable events, unobservable aspects or features of events ....' (24) This distinction poses immediate problems for empiricist semantics which is concerned, eg with relations between 'expressions of a language and their designata': (25) how may new theoretical terms be introduced legitimately into the language of science? what are the criteria for the significance (ie meaningfulness) of theoretical terms and of theoretical sentences? etc. All of Carnap's semantic analysis culminate in the problem of the relations between the observation language, L_O, and the various 'artifical' language of the formal sciences. The notions of 'empirical science', 'formal science', 'semantic analysis', 'reducibility', the method of intension and extension, etc, serve both to pose the initial difference between the formal and the factual sciences and to articulate the steps of their relationship.

Carnap's various articulations of the initial difference/correlation are not reducible to a simple ideology of the given. His articulations are opposed by other variants: Quine effaces the distinction between theoretical and observation languages: Hempel retains a form of this distinction but doubts whether a sharp distinction between meaningful and meaningless can be drawn; etc. (26) The differences between the variants of the empiricist problematic must not be confused with the differences between a science and ideology (27) or between one science and another. A science 'can only pose problems on the terrain and within the horizon of a definite theoretical structure, its problematic'; (28) its concepts are the concepts of that problematic and its object is the object of those concepts. The discrete problematics of the sciences are distinct from each other and from the empiricist problematic of the ideologies. The ideologies are, on the contrary, precisely variants of the same invariant structure of the ideological problematic.

Badiou compares the ideologies to variations on a musical theme: different, 'but with a difference which relates one to another as variations of the same theme. The (infinite) system of differences between variations is the effect the (unique) difference between the theme and ... the field of possible variations' (29). The ideological discourses are variations 'on a theme that is not given': which does not appear as one variation amongst the others precisely because it is the invariant structure of the variational field. Each variation can then appear as the theme in person, with every other variation appearing as the product of mistakes. errors, confusions. It is then possible for a philosophical variant to present itself as not taking sides, as being above the Kampfplatz which characterised all 'pre-critical' philosophy. Any such philosophy is a theoretical denegation of its own practice, a gigantic theoretical effort to present this denegation in a coherent discourse. (30)

'In the theoretical mode of production of ideology (which is utterly different from the theoretical mode of production of science in this respect), the formulation of a problem is merely the theoretical expression of the conditions

which allow a solution already produced outside the process of knowledge because imposed by extra-theoretical instances and exigencies (by religious, ethical, political or other 'interests') to recognise itself in an artifical problem manufactured to serve it both as a theoretical mirror and as a practical justification.' (31)

The empiricist problem of scientific knowledge concerns the relation between the given facts and their scientific representation. In each variant the terms in which the problem is formulated are determined by the specific articulation of the presupposed difference between the given facts and the form of their representation. More precisely, each articulation of the presupposed difference contains the solution to its own specific formulation of the problem: the problem is hand-picked by this solution. The posing of the problem is a simple repetition since it is 'the problem that had to be posed if the desired ideological solution was to be the solution to this problem'. (32) Ideological discourse is characterised by the denegation of this repetition. The system of variations appears to be the product of the variations themselves rather than the variations being an effect of the structure of the basic problematic: denegation of the problematic, presence of its variations.

(ii) the epistemology of models

The epistemology of models is a variant of the basic ideological problematic in which scientific knowledge is presented as knowledge through models. Given an empirical domain, in which facts are 'carefully observed and described, without allowing any theoretical preconception to decide whether some are more important than others', (33) the scientist constructs models to account for the observed facts. Models are reconstructions of the 'order' of the facts - the validity of the model is determined by its 'fitting' the order of the facts. Science has a theoretical and a non-theoretical moment. The latter is the moment of observation, the former that of model-building. Von Neumann lists the theoretical requirements of a good model as follows:

'The definition must be precise and exhaustive in order to make a mathematical treatment possible. The construct must not be unduly complicated so that the mathematical treatment can be brought beyond the mere formalism to the point where it yields complete numerical results. Similarity to reality is needed to make the operation significant. And this similarity must usually be restricted to a few traits deemed 'essential' pro-tempore - since otherwise the above requirements would conflict with each other'. (34)

For Von Neumann theoretical activity consists in the construction of models which confront the empirical domain in question. How does one choose among the multiplicity of models? In effect the facts decide for themselves: 'the best model will always be that which is true, that is, the simplest possible model which, while being derived exclusively from the facts under consideration, also makes it possible to account for all of them. Therefore, the first task is to ascertain what those facts are.' (35)

In the epistemology of models science is reduced to the fabrication of a plausible image. The presupposed difference is between the opacity of the given facts and the creative activity of the scientist. Their articulation is achieved through the notion of model: the model resembles the given facts and is the product of the creative activity of the scientist. This 'epistemology' has a double significance:

(1) 'it effaces the reality of science as a process of production of knowledges, a process which at no point

confronts the pre-existence of a real with ideal operations, but which develops, in the interior of a specific historical materiality, from demonstrations and proofs.
(2) it clouds the distinction between the production of knowledge and the technical regulation of concrete processes. Especially in economic 'models', the technical subjection to the conditions of production appears as the timeless necessity of a 'type' of economy, of which the model exemplifies the beneficient constraints.' (36)

(iii) syntax and semantics

The concept of model belongs to a branch of mathematical logic. It relates two distinct mathematical domains: a formal system and its domain of interpretation; and two aspects of logic: syntax and semantics. Logical positivism proposes a doctrine of science which makes categorical use of the scientific concept. Where mathematical semantics is concerned with relations between two mathematical domains, positivist semantics is concerned with relating the theory of a science (which it identifies with a formal system)and its given real object (which it identifies with the domain of interpretation). The positivist category involves a specific displacement of the intra-mathematical relations of the concept.

A formal system is a game played on a set of marks (X, Y, Z, =, U, a, b, c, ... etc). The game concerns finite sequences of these marks and is governed by rules of formation and rules of derivation. The formation rules divide the sequences that are well formed (eg: a=b) from those that are not (eg: ab=). The rules of derivation operate on well formed sequences: they allow one to 'deduce' theorems from an initial set of axioms. In any worthwhile game there is at least one sequence that is well formed but not a theorem - otherwise the derivation rules are redundant. Such a formal system is said to be coherent. (37) These two sets of rules define the syntax of the formal system.

Formal systems are not constructed simply as a way of passing the time. They are produced, eg, in an attempt to isolate the deductive structure of an existing scientific domain - the various 'axiomatisations' of arithmetic, logic, geometry, etc. In order to verify that a formal system does indeed express this structure it is necessary to establish a correspondance between sentences of the formal system and those in the scientific domain concerned. The rules of this correspondance provide the semantics of the system.

If the rules assign a 'true' sentence in one domain to each deducible sentence in the other, the domain of interpretation is a model for the formal system: If, to each true sentence of the model there corresponds a deducible sentence in the system, then the system is complete for the model. These and other semantic properties are investigated in the theory of models.

(iv) Meaning and Necessity

the positivist category of model

Logical positivism identifies the formal dimension of science with the syntax of the language of science. The real or empirical supplies the domain of semantic interpretation. Theory, for example, may be subjected to a double evaluation: syntactic - deducibility and consistency; semantic-measurement, experimentation, testing. This double constraint on the language of science is reflected in the title of Carnap's Meaning and Necessity. (38) In this book he starts with the semantical concepts of truth and L-truth (logical truth). The distinction between the logical and factual is made by means of the notion of state-description.
'A class of sentences in S (an object language) which contains for every atomic sentence either this sentence or its negation, but not both, and not other sentences, is called a state description in S, because it obviously gives a complete description of a possible state of the universe of individuals with respect to all properties and relations expressed by predicates of the system.....
The connection between these concepts and that of truth is as follows: there is one and only one state description which describes the actual state of the universe; it is that which contains all true atomic sentences and the negations of those which are false. Hence it contains only true sentence; therefore, we call it the true state-description. A sentence of any form is true if and only if it holds in the true state-description.' (39)

Since state-descriptions represent possible worlds (40) a definition of L-truth is suggested by Leibniz' conception that a necessary truth must hold in all possible words. This leads Carnap to the following definition:

'A sentence Si is L-true (in S) = def. Si holds in every state-description (in S). A sentence is factual or a synthetic or contingent truth if it is true but not L-true.'

Carnap reproduces and refines traditional philosophical categories (analytic/synthetic, necessary/contingent) 'which have long been used by philosophers without being defined in a satisfactory way'. The new 'satisfactory' definitions appropriate the concepts of the mathematical theory of models - in particular the mathematical distinction between logic and mathematics. These will be discussed below.

Semantic constraints in Carnap's system are represented by (1) rules of formation; (2) rules of designation for the descriptive; ie non-logical, constants; (3) rules of truth: eg 'an atomic sentence in S consisting of a predicate followed by an individual constant is true if and only if the individual to which the individual constant refers possesses the property to which the predicate refer; (4) rules of ranges which determine for every sentence in S whether or not it holds in a given state-description. (41)

It is clear from the character and function of the rules of truth and of designation that observation and measurement are essential semantic operations. In the language of physics, Sp, all measurement results in a rational number (ie expressible as a finite sequence of decimals) since the concrete operations of measurement are necessarily finite. Semantically S_p requires to be based on rational numbers only. However, from the point of view of syntax, any limitation to rationals involves considerable complication. For example the elementary operator of square root, which plays an essential role in electro-magnetic or gravitational field theory, is inadmissible since, more often than not, rational numbers have no rational square root. Thus from the point of view of syntax, of the formal manipulation of the theory, it is preferable to use real numbers (in which infinite sequences are possible). The opposition between empirical investigation and mathematical necessity appears in the types of constraint which semantics and syntax exercise on the language adopted. The articulation of these constraints is reproduced, in classical arthimetic in the articulation of the real numbers on the rationals.

It is not difficult to found an epistemology on the difference/correlation of syntax and semantics. This shares with the vulgar epistemology of Levi-Strauss, von Neumann, Wiener et al, the posing of the difference between the formal and the empirical. On the other hand it inverts the original conception. For von Neumann the theoretical construct is a model relative to a given empirical domain. In this conception rigour is possible only at the theoretical level, the level of the model itself: there can be no rigorous

definition of the (extra-theoretical) relationship of similarity between the model and its facts. For positivist semantics the model is an interpretation of the formal system; the empirical provides the models for theoretical constructs.

More significant is the relationship of the two conceptions to the sciences. Von Neumann's conception is purely notional: it is not the product of a philosophical elaboration of scientific concepts. Logical positivist semantics, on the other hand, rests explicitly on the science of mathematical logic. Semantics is a matter of precise and unambiguous rules of correspondence: between one mathematical domain and another in the science; between a theoretical domain and its given real object in logical positivism. In neither case is there room for the arbitrary play of similarity and difference - the 'semantic' rules of the epistemology of models. In the science the completeness of a formal system, is at least in principle, demonstrable or refutable. One of Gödel's theorems establishes the incompleteness of any formal system of arithmetic (eg the Russell-Whitehead axiomatisation) which has classical recursive arithmetic as a model. The domain of interpretation is a mathematical domain: its properties are used in investigations of the properties of semantic systems. In logical positivist semantics, on the other hand, the domain of interpretation has no theoretical properties - since it consists precisely of the given facts. Semantic properties are properties of the theory (ie of the formal system) alone.

The semantics of logical positivism involves a categorical use of the concept of model - althought the word model does not always appear. Only an examination of the concept can demonstrate that this categorical use of model involves a deformation of the concept. That is, that positivist semantics is not supported by the science on which it pretends to rest. (42)

2. The scientific concept of model

The following construction uses an elementary calculus containing only unitary predicates: ie if P is a predicate then P(x) is a well-formed expression of the calculus, but P(x,y,), P(x,y,z,), etc, are not well-formed. It presupposes no mathematical knowledge. (43)

(i) syntax

(a) Alphabet:

individual constants: a, b, c, a', b', c', ...;
individual variables: x, y, z, x', y', z', ...;
predicates: P, Q, R, P', Q', ...;
connectives: negation -;
implication $\rightarrow$;
quantifiers: universal U;
existential E.

Informally: the individual constants designate 'objects' and the predicates are properties. Individual constants and predicates are not interchangeable. Variables 'represent' unknown constants, 'places' where any constant may be written. Quantifiers may be 'read' as follows:

(Ex)P(x): there exists an x with the property P;
(Ux)P(x): all x have the property P.

Negation and implication are read in the obvious fashion.

(b) Formation rules:

P(a), P(x), etc are well-formed expressions;
if A and B are well-formed expressions, then - A and A $\rightarrow$ B are well formed;
if x is free in A then (Ux)A and (Ex)A are well-formed (note a variable is free in a well-formed expression if it is not governed by a quantifier, otherwise it is tied - eg in (Ex)(P(y) $\rightarrow$ Q(x)) y is free and x is tied.)

Informally: well-formed expressions are grammatical sentences 'describing' properties of objects, etc. The rule governing the use of U and E eliminates trivial redundancies and nonsenses. To borrow an example from Carnap, assume that (Ux)(H(x)$\equiv$(F. B)(x)) is well-formed (it reads:'All human beings are featherless bipeds and vice-versa). Then (Ex)(Ux)(H(x)$\equiv$(F. B)(x)) is not well-formed (it reads: 'there is an x such that, for all x, if x is human it is a featherless biped and vice-versa').

(c) Deduction rules:

If A and B are well-formed expressions and if $\vdash$ indicates that the following formula has been deduced, then we have two deductive schemata:

generalisation:
$$\frac{\vdash A}{\vdash (Ux)A}$$

separation:
$$\frac{\vdash (A \rightarrow B) \quad \vdash A}{\vdash B}$$

These may be read: if A then, for all x, A; and, if A implies B and if A, then B. These deduction rules may appear 'obvious' from the informal readings. For example, an intuitive or 'common-sense' reading of the sign (implication) leads directly to the rule of separation. Such informal readings are necessarily ideological. The alphabet, rules of formation and deduction, must not be interpreted as providing a neat expression of what is 'intuitively' clear or obvious or evident. In logic, as in other branches of mathematics, what is clear to 'intuition' is frequently false. It seems clear, for example, that 'the whole is greater than the part', yet there are no more integers (ie 1, 2, 3, 4, ...) than there are perfect squares (ie 1, 4, 9, 16, ...). In the present formal system there are 'intuitively obvious' deductions that cannot be made. Consider the following sequence:

$$\frac{\vdash (A \rightarrow B) \quad \vdash -B}{\vdash -A}$$

This may be read: 'if A implies B and if not B then not A'. It corresponds to an 'intuitive' rendering of implication. Nevertheless the conclusion, -A cannot be deduced from the axioms, (A $\rightarrow$ B) and -B, by means of the rules introduced above without the use of further axioms.

The formation and deduction rules establish the mechanical, or effective, character of deduction in the formal system.

(d) Axioms

Once deduction rules have been introduced it is necessary to choose initial formulas or axioms. This choice characterises the formal system in question since all other rules are universal. Given the axioms deduction is possible. A finite sequence of formulas is a deduction if each formula is (a) an axiom, or (b) is deduced from preceding formulas in the sequence. The first formula in any deduction is therefore an axiom. Every formula in a deduction is a theorem of the system.

Thus if: $\vdash$ P(x) and $\vdash$ (Ux)P(x) $\rightarrow$ - Q(a) are both axioms.
then $\vdash$ P(x) (axiom)
$\vdash$ (Ux)P(x) (generalisation)
$\vdash$ (Ux)P(x) $\rightarrow$ -Q(a) (axiom)
$\vdash$ -Q(a) (separation)
is a deduction and -Q(a) is a theorem. (44)

(ii) semantics

Semantics concerns the correspondence between a system and its domain of interpretation. At this point positivist semantics introduces the king of France, Sir Walter Scott (who sometimes doubles as the author of 'Waverley'),

featherless bipeds and other such objects. Nothing is more indistinct than the empiricist notion of a set as a collection of objects, defined according to the arbitrary whim of the collector. The mathematical (ie scientific) theory of models uses set theory or some other mathematical domain to construct its domain of interpretation. There are several, more or less formalised, axiomatic set theories (45) consisting, for example, of:

(i) an alphabet: a, b, c, d, ... (called elements)
(ii) another alphabet: A, B, C, ... (called sets)
(iii) two marks: $\in$, $\subset$ ($a \in C$ reads a is a member of C, $A \subset B$ reads A is a subset of B)
(iv) various axioms, rules of deduction, etc.

Collections of objects (eg 'all the letters in all the printing presses on the earth', 'all featherless bipeds') are not sets in the sense of any mathematical set theory. (46) Note that the system of marks which appear in set theory is quite distinct from all those appearing in syntactic systems. The concepts of model concerns the correspondence between two 'games' of marks on paper. The materiality of marks ensures the materiality of mathematical experimentation.

(a) structure:
A structure consists of the following apparatus:
a set V, called a universe, with elements u, v, w, ...
Thus $u \in V$, etc;
a collection of subsets, possibly empty, of V: pV, qV, ...
Thus if $u \in pV$, then $u \in V$, etc;
two marks: T and F. (these marks may be read as true and false).

(b) interpretation in a given structure.
All interpretation requires a function, f, which assigns:
(i) to each individual constant of the system an element of the universe V. eg f(a) = u;
(2) to each predicative constant a subset of the collection which defines the structure eg f(P) = pV.

The function f and the marks T and F establish rules relating syntactic deducibility (the fact that A is a theorem) and semantic validity (the fact that A is valid for a structure).

(c) evaluation of formulas.
rule 1; P(a) = T if and only if $f(a) \in pV$. , otherwise P(a) =F

This reads: a has the property P if and only if the element corresponding to a belongs to the subset corresponding to P.

rule 2: -A = T if and only if A = F, otherwise -A =F.
rule 3: $(A \rightarrow B)$ = F if and only if A = T and B = F, otherwise $(A \rightarrow B)$ = T.

That is: an implication is false if and only if the antecedent is true and the consequence false.

There are two rules for quantifiers. If x is free in B let $B(^{a}/_{x})$ be the expression obtained by substituting a for x in B.
rule 4: (Ex)B = T if and only if there is at least one a such that $B(^{a}/_{x})$ = T, otherwise (Ex)B = F.
rule 5: (Ux)A = T if and only if $A(^{a}/_{x})$ = T for all individual constants, otherwise (Ux) A = F.

Note that rules 4 and 5 do not provide for the evaluation of formulas containing free variables. If x is free in B then B cannot be evaluated by means of the above rules, the evaluation of (Ex)B and of (Ux)B is not the evaluation of B. Suppose B contains several free variables, x, y, z, ... A is closed instance of B is a formula of the type $B(^{a}/_{x})$ $(^{b}/_{y})$ $(^{c}/_{z})$.... Then B is <u>valid</u> for a structure if, for every closed instance B' of B, B = T. Thus if B has one

free variable, B is valid if and only if (Ux)B = T.

These rules establish a procedure for evaluating any given formula. Starting with elementary formulas, P(a), which can be evaluated directly, the rules provide for the evaluation of any complex formula once the shorter formulas it contains have been evaluated. Thus the evaluation of -B proceeds from the evaluation of B, that of (Ex)B from $B(^{a}/_{x})$, and so on. For any number n, the evaluation of formulas of n+1 marks follows from the evaluation of formulas of n marks or less. Thus if any formula of 10 marks or less can be evaluated, then any formula of 11 marks can be evaluated, then any formula of 12 marks, and so on. It follows that any all formulas can be evaluated.

Suppose that some formulas could not be evaluated using the rules, and that the shortest such formula <u>M</u> contains m marks (where m is a positive integer). Then <u>M</u> is of the form -A, or (Ex)B, or (Ux)C, or D E (where A is a formula of length m-1, B is of length m-4, etc). Since <u>M</u> cannot be evaluated it follows that there is at least one shorter formula that cannot be evaluated. Thus, since all elementary formulas P(a) (ie all formulas of length 4) can be evaluated (by rule 1), the assumption that there is a shortest formula that cannot be evaluated leads to a contradiction.

This demonstration is significant for two reasons:

1. the rigorous construction of the concept of model, with evaluation as a moment of this construction, requires that all sequences can be 'measured' by the integers - that is they must have whole numbers of marks. The concept of model rules out continuous formal languages - in which, eg sequences were measured by the real numbers and could be of any 'length' (ie consisting of marks and parts of marks). In a continuous formal system it would be impossible to show that the evaluation rules allowed for the evaluation of all formulas. The assumption that there are formulas that cannot be evaluated by the rules leads to a contradiction only if there must be a shortest such formula - any non-empty set of finite/positive integers must have a smallest member. The real number system does not guarantee the existence of such a smallest member. A formal system having, say, the real number system as a model cannot be continuous. The relationship between a formal system and one of its models is fully specified by the function, f, (which, in the present example, assigns to individual constants and predicates of the system 'corresponding' elements and subsets of the structure), and the evaluation rules. There is no question of the formal system having to 'resemble' its model by, eg, sharing the same properties.

2. the concept of model is based explicitly on the mathematics of sets and implicitly on the mathematics of the integers (particularly the axiom of induction). This recourse to an existing mathematics - set theory, or some alternative domain of interpretation, classical recursive arithmetic (for the integers) - is absolutely essential. The construction of the concept of model requires the use of an existing mathematics: it takes place entirely within science. There is no reconstruction of mathematics starting from scratch, no foundation of mathematics upon the non-mathematical.

(d) conservation of validity.

All of the above is wasted unless the two deduction rules conserve validity. There is an epistemological lesson to be had from the verification of conservation. Consider the rule of generalisation. Suppose it does not conserve validity. Then there is a formula A which is valid while (Ux)A is invalid. If (Ux)A is invalid there is a closed

instance (Ux)A' of (Ux)A such that (Ux)A' = F. Rule 5 implies that there is a constant, a, such that $A'(^{a}/_{x}) = F$. $A'(^{a}/_{x})$ is a closed instance of A. Therefore A is invalid. The original hypothesis leads to a contradiction and must be rejected. I leave the rule of separation to the reader.

The above verification relies upon the principle of non-contradiction: no statement can be both true and false (ie not both A and -A). It uses a logic 'in the practical state': ie a logic which functions as a means of mathematical production, as a scientific instrument. It is itself the product of scientific work. Thus, in addition to the two mathematical domains presupposed (set theory and the classical arithmetic of the integers), the construction of the concept also involves the use of a logic. An idealist appropriation of the concept must reduce this logic either to the basic principles of all thought (eg a transcendental logic) or to rules of inference tried and tested by experience (J. S. Mill, Popper etc). (47) The distinction between mathematics and logic is examined below.

(e) model.
Since the rules of deduction conserve validity it follows that, if the axioms of a formal system are valid for a structure, then all theorems are also valid. This leads directly to a definition of model:

A structure is a model of a formal theory if all axioms of the theory are valid for the structure.

(iii) Mathematical and Logical Axioms.
A formal theory may contain axioms that are valid for all structures and axioms that are invalid for some structures. The first type are logical, the second are mathematical. Of the following axioms the first three are logical, the fourth is not.

1. $A \rightarrow (B \rightarrow A)$
2. $(-A \rightarrow -B) \rightarrow (B \rightarrow A)$ (48)
3. $\lceil A \rightarrow (B \rightarrow C) \rceil \rightarrow \lceil (A \rightarrow B) \rightarrow (A \rightarrow C) \rceil$
4. $(Ex)(Ey) - \lceil (P(x) \rightarrow -P(y)) \rightarrow (-(-P(y) \rightarrow P(x)) \rceil$

2 Suppose	$(-A \rightarrow -B) \rightarrow (B \rightarrow A)$	$= F$	(1)
rule 3	$-A \rightarrow -B$	$= T$	(2)
	$B \rightarrow A$	$= F$	(3)
rule 3 on (3)	B	$= T$	(4)
	A	$= F$	(5)
rule 3 on (2)	$-A$	$= F$	(6a)
	or $-B$	$= T$	(6b)

(6) contradicts at least one of (4) and (5).

Thus 2 is valid for all structures. Axioms 1 to 3 and the above syntax define one of the most important logical systems: the first order predicate calculus. The calculus of propositions is valid for all structures. However the validity of this calculus does not imply that its axioms are either axioms or theorems in all formal systems. If a formal system has a model then the axioms are valid for this model. This implies that the negations of the axioms are not valid for the model. The semantic rules then show that these negations are not theorems of the system.

This illustrates the essential distinction between deducability and validity, and therefore between syntax and semantics. Deducability implies validity (by definition of model) but validity does not imply deducability. At most the semantic rules can determine that, in any formal system that has a model, certain formulas cannot be refuted. In particular, the predicate calculus is not refutable in any formal system that has a model. The syntax of a formal system is not 'governed' by the predicate calculus or by any principles of logic.

A formal system, a system of inscriptions or marks controlled by specified rules of combination and separation, is an experimental apparatus, ie a mathematical experimental apparatus. Bachelard notes, in the case of physics, that the true principle of identity is that of the identity of scientific instruments. (49) In mathematics the system of marks, on paper or blackboard, is a scientific instrument. The study of algorithms or axioms, of what is deducible or calculable, of the properties of axiomatic systems, is governed by the control of the identity of marks. The only principle governing the operation of the formal system is the principle of the invariance of marks. Note that the substitution of, eg, a constant for a variable in certain situations in no way contravenes this principle. In the formal systems of this paper the appearance of the formula (Ux)(P(x) Q(x)) in a deduction allows one to deduce, and therefore to write, P(a) Q(a). This substitution of a for x involves writing a new formula in a deduction. The mark x remains an x and the mark a remains an a. There is no changing of one mark into another.

The first order predicate calculus is valid for all models - that is a property, which can be demonstrated, of the set of axioms which define this calculus. The predicate calculus does not 'govern' or 'control' or in any other way affect deduction in a formal system. Deduction is governed by explicit rules of deduction and by the invariance of marks, and by nothing else. There is no logic which underlies, or provides the foundation for deduction in formal systems. Logic cannot therefore 'found' mathematics. There are no eternal or trans-historical principles of logic which underlie all thought.

4 $(Ex)(Ey) - \lceil (P(x) \rightarrow -P(y)) \rightarrow (-(-P(y) \rightarrow P(x)) \rceil$

This formula is not valid for a structure whose universe consists of one element. Thus it is not valid for all structures: it must be mathematical.

Suppose it is valid. Then, by rule 4, there is a constant a, such that
$(Ey) - \lceil (P(a) \rightarrow -P(y)) \rightarrow (-(-P(y) \rightarrow P(a)) \rceil = T$ (1)

By rule 4 again, there exists a constant b, such that
$(P(a) \rightarrow -P(b)) \rightarrow (-(-P(b) \rightarrow P(a))) = T$ (2)

In a universe with one element a and b must correspond to this one element u. The evaluation of P(a) is then identical to that of P(b). In formula (2) P(b) can be replaced by P(a) without affecting the evaluation of the formula (since both 'translate' into $u \in pV$). This leads to the formula:
$-\lceil (P)(a) \rightarrow -P(a)) \rightarrow (-(-P(a) \rightarrow P(a))) \rceil$

This formula is never valid. This can be seen by examining the two possible cases P(a) = T and P(a) = F.
If P(a) = T
rule 2 $-P(a) = F$
rule 3 $(-P(a) \rightarrow P(a)) = T$
rule 2 $-(-P(a) \rightarrow P(a) = F.$ (3)

Again starting with P(a) = T
rule 2 $-P(a) = F$
rule 3 $P(a) \rightarrow -P(a) = F.$ (4)

Apply rule 3 to (3) and (4)
$\lceil (P(a) \rightarrow -P(a)) \rightarrow (-(-P(a) \rightarrow P(a))) \rceil = T$
rule 2 $-\lceil (P(a) \rightarrow -P(a)) \rightarrow (-(-P(a) \rightarrow P(a))) \rceil = F.$
The same result is reached starting from P(a) = F.

Thus 4 must be rejected if the universe consists of one element. The formula prescribes a particular multiplicity for the structure. It ensures that the structure must contain at least two elements if it is to be a model of the system. The former does not differentiate between models.

Any structure can be a model for the system. Formal logic makes no distinction between the concepts of model and of structure. This property of logical systems, or rather its ideological appropriations, provides the basis for the supposed 'transhistoricity' or 'universality' of logic. It also provides the basis for Carnap's 'semantic' distinction between truth and L-truth (cf. I(iv) above).

Mathematical axioms distinguish between structures. Formal mathematics distinguishes between the concepts of structure and of model. The concept of logic is precisely constructed according to the couple it forms with that of mathematics. This opposition reproduces in syntax the semantic distinction of model and structure. Mathematical axioms govern the difference of models, logical axioms govern their unity.

(iv) Some results in the theory of models

1. A theory which is an extension of the first order predicate calculus (the formal system defined by the 3 logical axioms of the preceding section) is coherent if and only if it has a model.

This result is of fundamental importance. Coherence means that there is a formula A which cannot be deduced from the axioms, ie that some formulas are not theorems. With this result it is possible to show that the calculus of propositions (as in (i) and (iii) above but with n-ary predicates) is a complete formal logic. In other words every formula that is valid for all structures is a theorem of the calculus. In the calculus of propositions every formula of its negation is a theorem.

This property defines the 'ideal' upon which Husserl based his category of formal system. It has an axiom system 'distinguished by the circumstance that any proposition that can be constructed, in accordance with the grammar of pure logic, out of the concepts occurring in that system, is either 'true' - that is to say an analytic (purely deducible) consequence of the axioms - or false - that is to say: an analytic contradiction -; tertium non datur. (50)

A theorem established by Godel (51) in 1931 demonstrates that any formal system capable of generating elementary recursive arithmetic contains an undecidable formula: that is, neither the formula nor its negation can be deduced from the axioms. The formal system of arithmetic, and therefore the vast bulk of mathematical formal systems, are 'inadequate' in terms of Husserl's ideal. Completeness, decidability, coherence, etc, are properties that may or may not be possessed by a given formal system; these properties are investigated by the theory of models. Husserl imposes, as a norm for mathematics, a property of weak formal systems (ie incapable of generating recursive arithmetic). It can be demonstrated that 'strong' formal systems (which are capable of generating recursive arithmetic) do not have this property. Husserl's norm, an ideal for all mathematical theories, would present the whole of mathematics as a number of discrete, isolable and masterable formal systems: (52) ie whose essence can be 'grasped' or mastered' by a knowing subject - the subject that is opposed, implicitly or explicitly, to the given real object in all variants of the empiricist problematic. This norm is imposed by the structure of the empiricist problematic: it is not imposed by the exigencies of mathematical production, ie by the problematic of that science.

If the calculus of propositions is valid for all structures what is the status of formal systems in which an axiom or a theorem of this calculus is denied? Suppose, for example, that $-(-(-A) \rightarrow A)$ is a theorem of a formal system. If such a system contains the axioms of the calculus of propositions then it is incoherent. Otherwise, since this calculus is

valid for all structures, the system has no model: more precisely it has no model in terms of the set theory used in the constructions of this paper - that is, in a set theory which contains the axiom of choice (see below). There are other set theories - in which, for example, the negation of the axiom of choice is an axiom. The theory of models discussed in this paper is a theory of one specific set theory. A formal system containing the above theorem has no model in this theory. It may still be coherent. The supposed principles of logic, eg the principle of contradiction, are universally valid within the domain of one theory of models belonging to one specific set theory.

2 Any formal system which is an extension of the calculus of propositions (ie in which its axioms are axioms or theorems), has a denumerable model (roughly: the elements of the model can be put into a one-to-one correspondence with the integers; the model is not 'bigger' than the set of integers). Thus a formal theory aiming to isolate the deductive structure of a non-denumerable domain (say, the geometry of a Euclidean plane) has a denumerable model. The 'discrete' character of a formal system (all formulas having an integral number of marks) does not prevent it from dealing with continuous domains.

3 If the theory of sets without the axiom of choice has a model then the theory obtained by adding this axiom has a model.

Very schematically, the axiom of choice asserts that for any set whose members are sets there is a set containing exactly one element from each of the member sets. A version of this axiom plays a crucial role in many of the constructions and proofs in Principia Mathematica . This axiom appears rather 'risky' and, for a time, many mathematicians refused to use it in their proofs. This led to a 'finitist' or constructivist movement in mathematics. One could not affirm the existence of an object, eg the set 'supplied ' by the axiom of choice, without showing how it could be constructed. Thus it was impossible to consider the set of all members of an infinite collection.

The above result, together with 1, guarantees that if set theory without the axiom of choice is coherent then set theory with this axiom is coherent. The axiom of choice is no more risky than other axioms of set theory. It follows that 'constructive' or 'finitistic' proofs have no claim to any kind of mathematical priority or superiority. Of course all deductions within formal systems are constructive: otherwise they are not deductions. The constructive norm for mathematics suggested by Hilbert, Bernays and others, would reduce the whole of mathematics to formal systems.

III Epistemological Results

(i) mathematical experimentation and the positivist category of model

The construction of the concept of model depends more or less directly on several existing mathematical domains: in particular, upon arithmetic and set theory. The latter enters directly as the domain of interpretation. The concept of model does not relate the theoretical to the non-theoretical, the theory of a science to that science's given real object. It relates one mathematical domain to another. The instruments of the correspondence between a formal system and its model presuppose the concepts of set, sub-set, function, and so on. Semantics is an intra-mathematical relation between an experimental domain (the formal systems) and certain mathematical products that are accepted and taken for granted. The operation of a formal system establishes a proof of deducability in any domain which is a model.

Semantics is an experimental protocol in mathematics: not

in the sense of positivist epistemology where semantics supplies the experimental moment corresponding to a correlative 'formal' theoretical moment; on the contrary the formal systems are the experimental moments, the materiality of mathematical proofs. This materiality is rigidly controlled by the rules of formation and deduction and by the identity of marks - a mark retains its identity throughout a deduction, there can be no substitution. In a very strict sense deduction is a mechanical operation.

'A formal system is a mathematical machine, a machine for mathematical production, situated in the process of this production.' (53) The means of scientific production are scientific products. The formal system as a means of mathematical production is the product of 'informal' set theory and recursive arithmetic.

The ideological question of the 'foundations' of mathematics generates two forms of answer: set theoretic foundations with basic notions such as set, inclusion (one set in another) and the 'logical' notions of union, complement, product etc; combinatorial (or arithmetical) foundations in which basic notions are words (finite strings of symbols), combinatorial function (of which arguments and values are words - eg a product function with arguments (abcd) and (efgh) has a value (abcdefgh). The concept of model provides a mathematical articulation of the set-theoretical and the combinatorial domains. Neither the one nor the other can supply the basis for a foundation of mathematics.

There is nothing in the concept to legitimate its export from the field of mathematical experimentation into the positivist articulation of theory and fact. That articulation is an ideological recovery of an intra-mathematical articulation of set theory and arithmetic.

The positivist epistemology of Carnap bends mathematical concepts to the service of an empiricist ideology. 'Formal Languages' and 'empirical facts' are confronted as two heterogeneous domains, and one provides 'models' for the other. The confrontation is then 'thought ' as a correlation. Carnap's 'Model' designates an extra-theoretical domain waiting to be formalised. His category of model is a combination of empiricist notion and scientific concept in which ideology is dominant and science enslaved.

(ii)the materialist category.

A rigorous reading of the concept of model establishes a dividing line between two categorical usages of the concept: one has just been examined; the other belongs to the theory of the history of the sciences. In the materialist theory of the production of scientific knowledge the concept of model provides a key to decipher the experimental dialectic of mathematical production. In particular it disposes of idealist doctrines of 'pure', 'formal' or ' a priori' knowledge which have usually found their safest refuge in mathematics. (54)

The use of models in the production of proofs of relative coherence and independence suggests the epistemological import of the concept. If T is a formal theory and A is a well-formed expression using the alphabet and formation rules of T, let (T+A) be the theory obtained by adding A to the axioms of T. (T+A) is said to be coherent relative to T, if the coherence of (T+A) follows from that of T.

An example of such a theorem, but not its proof, is given in II (iv) above. If the theory of sets without the axioms of choice is coherent then so is the theory with the axiom of choice. This purely syntactic result requires a semantic proof: it is proved by means of the theory of models. Consider another example. (55)

Let GE be the formal theory of euclidean geometry. If it

is coherent it has a model. In euclidean plane geometry replace the axiom of parallels: 'through any point not on a line there is exactly one line parallel to the given line', by the axiom: 'through any point not on a line there is no line parallel to the given line'. Call this GRP (Riemannian plane geometry). Is GRP coherent?

The coherence of GE establishes that of GRP. A 'model' of GRP can be constructed by means of the model of GE (which exists because GE is coherent). Take a sphere in this model as the universe of the model of GRP. Then the function f:

assigns to points of GRP a pair of diametrically opposite points of the shere - ie The elements of the model are pairs of points;
assigns to lines of GRP great circles of the sphere (a great circle gives the shortest distance between two points on the surface, the plane of a great circle passes through the centre of the sphere);
interprets the relation between lines of 'having a common point' in the same way as the corresponding function for GE.

This structure is a model for all the axioms of GRP including those which it shares with GE. Thus if GE is coherent so is GRP. It follows that the axiom of parallels is independent of the other axioms of GE. If not, then any model of (GE-A) would be a model of GE also - since the deduction rules conserve validity a deduction of A from (GE-A) would establish the validity of A for the model. But GRP is a model for (GE-A) and A is invalid for it. If A and -A were both valid for GRP then any formula would be valid, GRP would then be a model for all geometries.

The production of this model of Riemannian geometry defended retrospectively the mathematical production of 'new' geometries against Kantian and neo-Kantian counter-attacks. (56) The proof administered by the model also transforms retrospectively the status of the multitude of vain attempts to demonstrate the axiom of parallels. The defeat was necessary, not accidental. The model puts an end to the practice it judges.

These and other instances suggest to Badiou a categorical usage of the word model. He proposes the term model 'for the status assigned retrospectively to early practical instances by their experimental transformation at the hands of a determinate formal apparatus'. (57)

Thus the use of GE as a model transforms its status from the geometry to one instance of geometry amongst others. Set theory without the axiom of choice provides a model for the theory with the axiom and also for a set theory in which choice is denied. The theory of models becomes a theory of a sub-domain of one instance of set theory.

'The category of model thus designates the retroactive causality of formalism upon its own scientific history, the joint history of an object (generality I & III) and its usage (generality II)'. (58) The category of model belongs to the history of formalisation. It marks the site of the internal reconstruction of a science. Badiou's text closes with this gesture towards a materialist history of mathematics. It is a history that has yet to be produced.

Notes.

(1) 'On the Materialist Dialectic' in L. Althusser For Marx Allen Lane, London 1970.
(2) L. Althusser 'From Capital to Marx's Philosophy' in L. Althusser and E. Balibar Reading Capital NLB, London 1970. pp. 34 - 40. See also section 1 (i) below.
(3) Reading Capital p. 59.

(4) cf. Reading Capital pp. 54 - 60.
(5) See below for reference to these authors.
(6) of the third part of D. Lecourt's article in this issue.
(7) This is discussed in sections II(iii) and III of this paper.
(8) See note 2.
(9) R. Carnap 'Foundations of Logic and Mathematics' in International Encyclopedia of Unified Science vol. 1, no. 3. University of Chicago Press, 1939. See also the discussions of Carnap in part I below; E. Kant Prolegomena translated by P. G. Lucas, Manchester University Press, 1953 pp. 36 - 52 (Main Transcendental Question, first part: How is pure mathematics possible?) - see also the discussion of Kant in B. Russell. An Essay on the Foundations of Geometry, Dover Publications, New York 1956. G. Frege The Foundations of Arithmetic, Basil Blackwell, Oxford 1953; B. Russell & A. N. Whitehead Principia Mathematica, Cambridge University Press, London 1950. E. Husserl Formal & Transcendental Logic, M. Nijhoff, The Hague 1959.
(10) See the relevant sections in N. Bourbaki Eléments d'Histoire des Mathématiques, Hermann, Paris 1960; and, for set theory, J. Cavaillès 'Remarques sur la Formation de la Théorie Abstraite des Ensembles' in his Philosophie Mathématique, Hermann, Paris 1962 pp. 25 - 177.
(11) N. Bourbaki op. cit. p. 46.
(12) A. Heyting 'The Intuitionist Foundation of Mathematics' in P. Benacerraf & H. Putnam Philosophy of Mathematics, Basil Blackwell, Oxford 1964, pp. 42 - 49 & A. Heyting Intuitionism. North Holland Publishing Co, Amsterdam 1956. The intuitionist ideological partition of the continent of mathematics should not be confused with 'intuitionist' mathematical systems (ie systems satisfying certain 'constructive' criteria).
(13) See parts II and III of this paper.
(14) It is necessary, in effect, to distinguish the 'spontaneous' philosophy of the scientist from another 'spontaneous' philosophy which is the 'product of importation' (ie ideological importation). D. Lecourt. L'Epistemologie Historique de Gaston Bachelard, J. Vrin, Paris 1969, p. 26. Compare, for example, J. von Neumann's writings (eg 'The Formalist Foundations of Mathematics' in Benacerraf & Putnam op.cit. pp. 50-54) with vulgar epistemology of J. von Neumann and O. Morgenstern. The Theory of Games and Economic Behaviour, Princeton University Press, 1954.
(15) Alain Badiou Le Concept de Modèle, Maspero, Paris 1969.
(16) L. Althusser 'Lenin before Hegel' in his Lenin and Philosophy and other essays NLB, London 1971 p.
(17) See I(i) below.
(18) of I (ii) the epistemology of models, below.
(19) Of course there are other uses, eg a planetarium, graphs representing non-spatial processes, automatic modelling living organisms, etc. On the latter see G. Canguilhem 'The Role of Analogies and Models in Biological Discovery' in A. C. Crombie (ed) Scientific Change Heinemann, London 1963. pp. 507 - 20.
(20) Reading Capital pp. 67 - 8.
(21) A. Badiou op. cit. pp. 60, 62.
(22) Reading Capital pp. 35 - 6.
(23) R. Carnap 'Factual and Formal Science' p. 128, in H. Feigel & M. Brodbeck, Readings in the Philosophy of Science Appleton - Century - Crofts, New York 1953.
(24) R. Carnap 'The Methodical Character of Theoretical Concepts' Minnesota Studies in the Philosophy of Science Vol. 1, 1956.
(25) R. Carnap Introduction to Symbolic Logic, Dover Publications, New York 1958, p. 79.
(26) K. Hempel, 'Empiricist Criteria of Cognitive Significance' in his Aspects of Scientific Explanation Prentice

Hall, New York 1965 & W. V. Quine 'Two Dogmas of Empiricism' in his From a Logical Point of View Harvard University Press, Cambridge, Mass. 1953.
(27) cf Antony Cutler 'The Epistemological Break' in this issue.
(28) Reading Capital p. 25.
(29) A. Badiou op. cit. p. 11.
(30) L. Althusser, 1971 op. cit. p. 64
(31) Reading Capital p. 52.
(32) Reading Capital p. 52.
(33) C. Levi-Strauss Structural Anthropology London 1968 p. 280.
(34) J. von Neumann & O. Morgenstern op. cit.
(35) Levi-Strauss op. cit. p. 281.
(36) A. Badiou op. cit. p. 22. This cannot be too strongly emphasised. What is at stake here is the theory of models as an ideology of knowledge. There is also a technical use of 'models' in eg the technical practice of planning in the socialist countries. The 'model' is then a technical means with which to compound the different data with a view to obtaining a certain goal. In economics the effect of the epistemology of models is a confusion between the technical instrument that a model is, or may be, and a scientific theory. This confusion of scientific theory with its technical application is condemned by Stalin in his 1952 text Economic Problems of Socialism in the USSR.
(37) If it is not coherent then, eg both $A \rightarrow B$ and $-(A \rightarrow B)$ are deducible from the axioms.
(38) R. Carnap Meaning and Necessity Chicago University Press 1947. For a rather different neo-positivist variant of this problematic, cf. K. R. Popper 'Why are the calculuses of logic and arithmetic applicable to reality?' Conjectures and Refutations RKP, London 1963, pp. 201 - 215. Part of his answer contains the assertion: 'Insofar as a calculus is applicable to reality, it loses the character of a logical calculus and becomes a descriptive theory which may be empirically refutable; and insofar as it is treated as irrefutable, ie as a system of logically true formulae, rather than a descriptive scientific theory, it is not applied to reality' p. 210.

There is no necessary correspondence between our language and reality: 'We are all most intimately acquainted with a world that cannot be properly described by our language, which has developed mainly as an instrument for describing and dealing with our physical environment - more precisely, with physical bodies of medium size in moderately slow motion. The indescribable world I have in mind is, of course, the world I have 'in my mind' - the world which most psychologists (except the behaviourists) attempt to describe rather unsuccessfully with the help of what is nothing but a host of metaphors taken from the languages of physics, of biology, and of social life.' p. 213.
(39) Carnap 1947. pp. 9 - 10.
(40) Note that since the language of physics, Sp, is a four-dimensional co-ordinate system (ie 'a standard individual expression in Sp will consist of four standard real-number expressions' p. 79) these definitions of truth and of L-truth presuppose Newtonian space and time. Carnap's notion of 'the actual state of the universe' is incompatable with relativity and with wave-mechanics - though both were well developed before Carnap produced the present text. Contemporary physics deals with material processes that cannot be described in one-dimension of time and three dimensions of space. cf. G. Bachelard: L'Activité Rationaliste de la Physique Comtemporaine, P. U. F., Paris 1951, and L'Experience de l'espace dans la physique contemporaine, P. U. F., Paris 1937.
(41) These last rules govern the use of logical connectives:

$\forall$, . , $\exists$, etc.

(42) This semantics also rests on Newtonian physics (see note 40) in spite of the supposed 'timeless' character of logic in this conception. In that respect its foundations are a little more solid - or would have been up to about the time of Laplace. The use of dead science against the living is characteristic of idealist epistemology.

(43) It is impossible to avoid vulgarisation in the present context: this, of course, opens the door to ideology. I present here an absolute minimum to make the epistemological point. It should be clear that logic texts written by or for philosophers are not to be relied upon. Those available in English are generally by logical positivists or empiricists or else by those 'ordinary language' philosophers who do philosophical logic.

Roger Martin. Logique Contemporaine et Formalisation, P. U. F. Paris 1964 is generally reliable. Among texts in English those written by or for mathematicians are to be preferred - but beware of those in which 'the dead King of France', 'the author of Waverley' and like individuals appear (see 'Semantics' below). Carnap uses the following as an example of a sentence that is true but not L-true: 'The proposition that Scott is a featherless biped is equivalent to the proposition that Scott is human.' 1947 p. 188.

The following rigorous texts require perseverance in abstraction but little mathematical training.

P. J. Cohen Set Theory and the Continuum Hypothesis, Benjamin, New York, 1966.
M. Davis Computability & Solvability, McGraw Hill, New York 1958.
S. C. Kleene Introduction to Metamathematics, van Nostrand, New York 1966.
E. Mendelson Introduction to Mathematical Logic, van Nostrand, New York, 1966.
R. M. Smullyan Theory of Formal Systems, Princeton University Press 1961

(44) A formal system is effective if it is possible to decide by a method fixed in advance, and in a finite number of steps, if an expression is well-formed and if a sequence of formulas is a deduction. Effectiveness is or is not a property of a formal system - depending on the axioms and the formation and deduction rules. In this paper all systems are effective.

(45) See discussion of set theory in N. Bourbaki op. cit. and J. Cavaillès op. cit. and J. Cavaillès op. cit.

(46) The paradoxes that wrecked the early Russell's 'foundation' of mathematics depended upon his use of the notion of a class as a collection of any objects whatever. See his attempts to avoid the paradoxes by means of the 'vicious circle' principle, etc, in Principia Mathematica, especially chapters II and III of the Introduction, and the Introduction to the second edition.

The rigorous construction of the mathematical concept of set and its differentiation from the empiricist notion of a collection of objects has been a considerable scientific achievement. (again see Bourbaki & Cavaillès on this). Even Cantor could write of 'a collection into a whole of definite, well distinguished objects of one perception or of our thought' G. Cantor Gesammelte Abhandlungen. Springer, Berlin 1932 p. 282

(47) see note 38.

(48) With this axiom -A is deducible from (A $\longrightarrow$ B) and -B of II (i)(c).

(49) G. Bachelard 1951 op. cit. especially chapter 2.

(50) E. Husserl op. cit. p. 96.

(51) There are many expositions. The account given in

Martin op. cit. is excellent and not too technical for the non-mathematician. See also B. Rosser 'An Informal Exposition of proof's of Godel's theorems and Church's theorem' Journal of Symbolic Logic, IV (1939) pp. 53 - 60. The reader should be warned of elementary expositions or accounts of the theorem which interpret it as establishing limitations to mathematical thought. E. Nagel and J. R. Newman Godel's Proof, R. K. P. London 1959, is just such an elementary exposition - it has just been reissued as a paperback. Piaget interprets the theorem as showing 'that the axiomatic method has certain inherent limitations' (J. Piaget. Structuralism R. K. P. London 1971. p. 31) In both cases the 'limitation' allows humanism to rush in sweeping all thought before it. What the theorem shows is, not that MAN is irreplaceable, but that the partition of well-formed sequences into those that are deducible and those that are not puts at least one valid formula, A, and its negation, -A, into the same part (not deducible). That is all.

(52) of J. Cavaillès Sur la Logique et la Theorie de la Science, second edition P. U. F. Paris 1960 especially pp. 44 - 78.

(53) A. Badiou op. cit. p. 54.

(54) The early work of Russell provides a particularly interesting example. See his An Essay on the Foundations of Geometry op. cit. (first published, London 1897).

(55) The following is due to Poincaré. It is discussed by Badiou pp. 64 - 66. Many other examples could be taken from the history of 19th century algebra in which transformation groups and even arithmetic provide 'models' for groups, rings, fields, etc.

(56) Much of the early development of non-euclidean geometry is concerned to show that, as against Kant's position, geometrical axioms are empirical (therefore not a-priori), eg Helmholtz' articles on geometry in Mind vols. 1 and 3. An anusing, but not entirely reliable account appears in Russell, 1956 op. cit.

(57) A. Badiou op. cit. p. 67.

(58) A. Badiou op. cit. p. 67.

A BRIEF RESUME OF THE ARCHAEOLOGY OF KNOWLEDGE

by ATHAR HUSSAIN

The text by Foucault translated here is a generalized response to a series of questions posed to him by the 'Cercle Epistémologique' of the Ecole Normale Supérieure in 1968. (1) The response, as a reading of it will make obvious, is enigmatic and systematically avoids a number of questions. It should be regarded as a tentative response, since the arguments put forward here have been further elaborated and in some cases transformed in Foucault's later book L'Archéologie du Savoir (Gallimard, Paris 1968; to be published in English by The Tavistock Press).

Foucault describes himself as an 'archaeologist of learning' (savoir) in order to establish the distance between his enterprise and on the one hand the so-called 'history of ideas', and on the other the history of the sciences practised by Bachelard, Canguilhem and Althusser. Foucault uses the term archaeology in opposition to a specific form of history, ie, idealist and empiricist history. In contra-distinction to the history of ideas:

1) The Archaeology is not a search for thoughts, representations, themes that are hidden or manifest in the discourse. Instead, it is concerned with discourse as a practice governed by specific rules. Hence the importance of the distinction between documents and monuments in the Archaeology. A document is defined as a collection of signs or signifiers which refer to an externally given object. A document can be said to be in a relationship of exteriority with the referent. The treatment of the discourse as a document always leads to a search for the essence of the discourse. In opposition to a document, a monument is in a relationship of interiority to its object. The Archaeology treats the discourse as a monument; it is neither an allegorical nor an interpretative discipline.

2) The Archaeology is not in search of continuous transitions from what precedes a discourse to what follows it. It seeks to register the occurrence of ruptures within the order of the discourse. Unlike the history of the sciences as practised by Bachelard, Canguilhem and Althusser, the Archaeology deploys its own criteria for the identification of discursive events. For example, it does not have any concept of the epistemological break, ie, an event in discourse defined by the epistemolgical criterion of scientificity. Discarding the notion of the slow maturation of learning does not condemn the Archaeology to a narrative of heterogeneous opinions; it is not a doxology but a differential analysis of the modalities of discourses.

3) The Archaeology is not a sociology, psychology or, more specifically, an anthropology of creation. For example, it does not recognize the sovereignty of an author's oeuvre. The notion of a creative subject as the raison d'être of an 'oeuvre' is alien to the Archaeology. Instead, it seeks to define the forms and types of discursive practices whose end-product any oeuvre is.

4) Lastly, the Archaeology does not seek to trace the intentions, motives, etc, of the speaking subject. It is not the discovery of the primordial germ of a discourse. It is the re-writing (reécriture) of a discourse, ie, a rule-governed transformation of something which has already been written.

The Archaeology of Learning seeks to destroy all the received notions of the unity of the discourse in a particular space or domain, eg, psycho-pathology, psychiatry, grammar, political economy, etc. After having performed this destructive task, it sets up epistemological protocols by which to establish the unifying base (called the 'rules of formation') for a given set of statements (called a 'discursive formation'). Foucault lists the following four procedures used to specify discursive formations in Madness and Civilisation, (2) La Naissance de la Clinique, (3) and The Order of Things. (4)

A. Discursive formations can be described on the basis of the objects, not the object, of a discourse or statements in a particular space, eg. discourse on madness. The characteristic feature of discourse on madness should not be sought in an object called 'madness' that is preserved through time. The rules of formation of the set of statements should be sought in the social sanctions, legal measures and religious casuistry that delineated the objects of discourse on madness.

B. Discursive formations can be described on the basis of the types, not the type, of statements in a particular domain. For example, the types of modalities of statements in clinical discourse in the 19th century were determined by protocols of diagnosis and prescription, experimentation in clinical laboratories, internment and regulated observation of patients in hospital, concern about the maintenance of public health, etc.

C. Discursive formations can be described on the basis of a series of concepts deployed in a particular domain, eg, classical grammar, and linguistics in the 19th century. The Archaeology does not seek to embed the concepts deployed in a particular domain into a coherent conceptual structure; instead, it seeks to define a common system which accounts for the emergence, dispersion and heterogeneity of the concepts in a particular domain, eg, classical grammar, natural history.

D. Lastly, discursive formations can be described in terms of the strategic possibilities offered by a particular theme. For example, in the 18th century, the evolutionist theme in natural history was analysed on the basis of the common ancestry of the species that form a continuum. In the 19th century, however, the evolutionist theme concentrated on the modalities of the interaction between an organism and the environment which determines the conditions of life of that organism. The unifying base of the discursive formation lies in the strategic choices offered by a theme, eg, evolution, or the formation of values or prices in the political economy of the 18th and 19th centuries.

Foucault formulates the concept of the discursive formation to answer the following question: What is the base that characterizes the unity, co-existence and heterogenity of statements in the domains called clinical medicine, political economy, grammar, or psycho-pathology? He goes on to argue that even a cursory reading of these disciplines would show that they lacked the unity of a single object of

discourse, a particular type of modality of statements, a coherent conceptual architecture or the effective presence of one identical theme. Discursive formations, as is obvious from this brief list of the rules of their formation, designate a system of dispersions of statements defined by the difference in the objects of discourse, modalities of statements, concepts deployed in a particular domain and thematic choices. The rules of formation are the conditions of emergence, co-existence, modification, conservation and disappearance of statements in discursive formations. For the purposes of illustration, the rules of formation can be taken to be homologous with the 'generative grammar and transformational rules' in post-Chomskyan linguistics.

Discursive formations are not co-terminous either with sciences or with theoretical ideologies. For example, the types of statements used in clinical medicine are determined jointly by sciences, eg, physiology and anatomy, and by ideologies, eg, the social status of the doctor, the institutionalisation of clinical treatment, etc. The rules of formation governing a discursive practice are not all of a discursive nature. For example, the objects of discourse on madness are delineated by discursive practices such as psycho-pathology and psychiatry, but also by non-discursive legal, economic and religious practices. It is clear that on its own terms, and with its own meagre resources, the Archaeology is trying to elaborate on the order of determination specified by historical materialism, ie, by the relationship between the infrastructure, the modes of production, and the legal and ideological superstructures. (5)

In his discussions of learning (savoir), Foucault points out that there is something between science and experience, namely, learning. But although certain passages from Althusser's For Marx might, if taken out of context, suggest that historical materialism treats ideology as the complement of science, this is not the case. Althusser does not argue that the birth of a science signalled by an epistemological break means the immediate disappearance of the ideology inhabiting the domain of the new science. Foucault rightly attacks the mechanistic notion of the epistemological break, ie, the suggestion that the birth of a science is a break from 'tenacious, solidary and positive errors'. Unless the history and philosophy of the sciences possess a concept to think the errors described by Bachelard, these errors will be reduced to the psychological weaknesses of the subject producing knowledge. Bachelard, being a bourgeois philosopher, did not have the support of the science of history, ie, of historical materialism, and in consequence lacked the concept of ideology required to confer a material status on the 'tenacious, solidary and positive errors'. The questions asked by the Cercle are governed more by the Bachelardian problematic than by the problematic of Althusser's more recent work, for they restrict themselves to the relations between 'tenacious, solidary and positive errors' and sciences, rather than formulating their questions in terms of the relationship between ideologies, practical and theoretical, and sciences. Foucault's attack on the mechanical inversion of the couple continuity-subject which characterized the history and philosophy of the sciences before Bachelard into the couple discontinuity-objects should be seen as a justified attack on the Bachelardian problematic.

The Archaeology of Learning sets itself up in a terrain different from that of the History of the Sciences. The differentia specifica of the Archaeology raises a number of thorny problems for the Archaeologist. If, as Foucault claims, the Archaeologist is epistemologically neutral, ie, does not recognize as pertinent the distinction between the

scientific and the non-scientific, then one of two things inevitably follows:

1. The Archaeology is restricted to an analysis of non-scientific discourse - a perfectly legitimate and important enterprise which has been forced into the background by Bachelard's emphasis on keeping pace with the 'modernity' of the sciences.
2. The Archaeology is not restricted to non-scientific discourse, but it is condemned to ideological blindness, ie, it is unable to register the moment when theoretical practice 'establishes a science by detaching if from the ideology of its past and by revealing this past as ideological' (For Marx p. 168, cit. L'Archéologie du Savoir p. 12). The main problem can be formulated in the following terms: unless the Archaeologist possesses the concept of the epistemological break, a concept which does not have to be identical to the one used by Bachelard, his claim to the detection of discursive events remains without adequate theoretical foundation and is in consequence false.

To give an example of the effect of the absence of a concept of epistemological break in Foucault, let me take his discussion of the development of political economy. Foucault characterizes the discourse of political economy in the 18th and 19th centuries by the following two strategic choices offered by the problem of the determination of prices:
i) the price of a commodity is determined by the demand for that commodity;
ii) the price of a commodity is determined by the labour embodied in that commodity.

If the Archaeologist restricts himself to partitioning discourses on political economy according to the alternative chosen, then he is unable to see that the strategic choice is itself ideological, as was shown by Marx. In Capital, the prices of particular commodities in the capitalist mode of production are neither determined by the demand for those commodities, nor by the labour embodied in them. The umbilical cord connecting the price of any particular commodity with its value is severed in the Third Volume of Capital.

The usefulness of this interview can be defined as follows:

1. it provides an extremely effective and corrosive attack on 'empiricist-idealist' history and on the so-called 'history of ideas';
2. it specifies the problems involved in the periodization of history - on this particular problem two of Foucault's earlier books, Madness and Civilization and La Naissance de la Clinique have a great deal to offer;
3. it elaborates the effects of social, economic, religious and theoretical practice on the discursive practice in specific domains, eg, discourse on madness and clinical discourse.

Notes.

(1) First published in Cahiers pour l'analyse no. 9 Paris Summer 1968.
(2) Histoire de la folie, Plon, Paris 1961; abridged translation as Madness and Civilization, The Tavistock Press, London 1965.
(3) P. U. F. Paris 1963; to be published in translation by The Tavistock Press.
(4) Les Mots et les choses, Gallimard, Paris 1966; translated as The Order of Things, The Tavistock Press, London 1970.
(5) For a further elaboration, see Dominique Lecourt: 'Sur l'archéologie et le savoir', La Pensée no. 152, August 1970, pp. 69 - 87.

ON THE ARCHAEOLOGY OF THE SCIENCES

QUESTIONS TO MICHEL FOUCAULT

Our sole intention in asking these questions of the author of Madness and Civilization, Naissance de la Clinique and The Order of Things, was to get him to state the critical propositions on which the possibility of his theory and the implications of his method are founded. The 'Cercle' proceeded by requesting him to define his replies in relation to the status of science, to its history and its concept.

On the epistemè and the epistemological rupture

Since the work of Bachelard the notion of epistemological rupture has served to designate the discontinuity, which the history and philosophy of the sciences claim to detect, between the birth of every science and the 'tissue of tenacious positive, solidary errors' which in retrospect is recognised to have preceded it. The prototypical examples of Galileo, Newton and Lavoisier, but also those of Einstein and Mendeleev, illustrate the horizontal perpetuation of that rupture.

The author of The Order of Things detects a vertical discontinuity between epistemic configuration of one epoch and the next.

We ask him: what relations are maintained between that horizontality and that verticality? (1)

The archaeological periodization delimits within the continuum synchronic sets which group learnings together in the pattern of unitary systems. By so doing it erases the difference that, for Bachelard, at each moment separates scientific from non-scientific discourses and assigns to each of them their specific temporalities and reduces the simultaneity and co-existence of the two discourses to a superficial effect.

We ask: if the archaeologist wishes to erase this difference? If he seeks instead, to distinguish between two registers, whether or not they are hierarchical?

If it is the case that one obtains an epistemic configuration by articulating chosen pertinent characteristics in a set of statements, we ask:

- what governs the selection, and justifies, for example, the following sentence: 'Only those who can cannot read will be surprised that I have learnt such a thing more clearly from Cuvier, Bopp and Ricardo than from Kant or Hegel.' (The Order of Things Chapter 9 sec 1, p. 309)?

- What validates the configuration thus obtained?
- Is there a sense in which one can ask what defines an epistemè in general?

We ask further: Does the Archaeology recognise the concept of science - a concept that consists in more than just the diversity of its historical forms?

On Reading

What use of the letter does the Archaeology presuppose? That is to say what operations are to be carried out on a statement in order to decipher, through what it says, its conditions of possibility, and to make sure that one reaches the non-thought which outside it, inside it, gives rise to it and systemizes it?

If one takes a discourse back to its non-thought, does that make it pointless to describe its internal structures and to recompose its autonomous functioning? What is the relation that obtains between these two concurrent systematisations? Is there an 'Archaeology of philosophical doctrines' to be opposed to the technology of philosophical systems as practiced by Gueroult? (2)

The example of Descartes might be relevant here (Histoire de la folie, pp. 54-57).

On Doxology (Theory of Opinion)

How does one define the connexion articulating the epistemic configuration with the conflicts of opinion that take place on its surface?

Has the level of opinions only negative properties: disorder, separation and dependence?

Is the system of opinions characterizing an author not subject to a law of its own which allows one to establish the rules governing the varieties of doxological system in an epistemè, the presence of a particular opinion implying or excluding certain other inside the same system?

Why should the connexion between the systems of opinion always take the form of conflict?

On the forms of transition.

Concerning the forms of transition that ensure the passage from one broad configuration to another, Chapter 6 of part III of The Order of Things explains that, while in the case of Natural History and General Grammar 'the mutation came about abruptly ..... The mode of being for money and wealth, on the other hand, because it was linked to an entire praxis, to a whole institutional complex, had a much higher degree of historical viscosity' (p. 180).

We ask: what theory can have as its object the general possibility of such a viscosity?

How and according to what relations (causality, correspondence etc) can a form of transition be determined by such viscosity?

Are all the discontinuities between succeeding configurations in principle of the same type?

What is the motor that transforms one configuration into another? Does the principle of Archaeology imply a demotion of this question?

On Historicity and Finitude

We ask the author of Madness and Civilization, Naissance de la Clinique and The Order of Things, how he would define the point from which he can lift the epistemic earth. When he states that in order to speak of madness 'a language without support is necessary', that in clinical medicine something has started to change today, or simply that 'the end of man is imminent', what status would be confer on this pronouncement itself?

Today, is he able to clarify his own configuration?

If one called an author's 'historicity' his belonging to the epistemè of his epoch and 'finitude' the name that an epoch - notably ours - would give to its own limits, what relations or non-relations according to him would obtain between that historicity and that finitude?

Would he accept it if he were offered the choice between a radical Historicism (the Archaeology could predict its own reinstatement in a new discourse) and a kind of absolute knowledge (which some authors might have forseen independently of epistemic constraints)?

CERCLE D'EPISTEMOLOGIE
Alain Badiou, Jacques Bouveresse, Yves Duroux, Alain Grosrichard, Thomas Herbert, Patrick Hochart, Jean Mathoit, J-A. Miller, J-C. Milner, Jean Mosconi, Jacques Nassif, Bernard Pautrat, Francois Regnault, Michel Tort.

Notes.
(1) We refer, in this question, to the following passage from Canguilhem's article on Foucault's book (Critique no: 242, pp. 612 - 3): 'Concerning a theoretical learning, is it possible to think it in the specificity of its concept without reference to some norm? Among the theoretical discourses conducted in conformity with the epistemic system of the 17th and 18th centuries, some, like Natural History were discarded by the epistemè of the 19th century, but others were integrated into it. Newtonian Physics did not pass away with the Physiology of animal economy even though the former served the latter as a model. Buffon was refuted by Darwin, if not by Etienne Geoffroy Saint-Hilaire. But Newton is no more refuted by Einstein than by Maxwell. Darwin was not refuted by Mendel and Morgan. The sequence Galileo, Newton, Einstein, does not contain ruptures similar to those revealed in the sequence Tournefort, Linné, Engler, in botanical taxonomy'.

(2) Martial Geroult is Professor at the 'College de France' and the author of books on Fichte, Leibniz, Descartes, Berkeley and Spinoza.

MICHEL FOUCAULT:

A Reply to the Cercle d'Epistemologie

A curious intersection. For decades now historians have, by preference, devoted their attention to long periods of time. As if, beneath the political peripeteiae and their episodes, historians undertook to bring to light the stable and resilient equilibria, the imperceptible processes, constant re-adjustments, the tendential phenomena which culminate, then recede after secular continuities, the movements of accumulation and slow saturations, the great immobile and mute shelves that the tangle of traditional accounts had hidden beneath a thick coating of events. To conduct this analysis, historians deploy the instruments which they have partly fashioned and partly received: models of economic growth, quantitative analysis of the flows of exchange, profiles of demographic growth and regression, and the study of climatic fluctuations. These tools have enabled them to distinguish, in the field of history, various sedimentary strata; the linear successions which until then had been the object of research, were replaced by a series of transverse overlaps. From political instability to the deliberation proper to 'material culture', the levels of analysis have multiplied; each level has its specific ruptures; each contains a periodicity which belongs only to itself. And the units become broader the further one descends towards the deeper strata. The old historical question (what link to establish between discontinuous events) is replaced, from now on, by a series of difficult interrogations: which layers should be isolated from each other? What type and criteria of periodisation need to be adopted for each of them? What system of relations, (hierarchy, dominance, tier-arrangement, univocal determination, circular causality), can be established between them?

Now, in about the same period, in those disciplines which are called the history of ideas, sciences, philosophy, thought and also literature (their specificity can be left aside for the moment), in those disciplines which, in spite of their titles, on the whole escape the work of the historian and his methods, attention has displaced from the vast units forming an 'epoch' or 'century' towards the phenomena of rupture. Beneath the great continuities of thought, beneath the massive and homogeneous manifestations of the spirit, and beneath the stubborn development of a science struggling from its beginnings to exist and complete itself, attempts are made to detect the occurrence of interruptions. G. Bachelard has charted out the epistemological thresholds which interrupt the indefinite accumulation of knowledges; M. Geroult has described the enclosed systems, the closed conceptual architectures which partition the space of philosophical discourse; G. Canguilhem has analysed the mutations, displacements and transformations in the field of validity and the rules for the use of concepts. As for literary analysis, it is the internal structure of the oeuvre - on a still smaller scale the text - that it examines.

But this crossover should not give us any illusions. We should not take on trust the appearance that certain historical disciplines have moved from continuity to discontinuity, while others - really history as such - were moving from the swarm of discontinuities to broad and uninterrupted units. In fact what has happened is that the notion of discontinuity has changed in status. For history in its classical form, discontinuity was both the given and the unthinkable: it was both what presented itself in the form of scattered events, institutions, ideas or practices; and what had to be evaded, reduced, effaced by the historian's discourse in order to reveal the continuity of the concatenations. Discontinuity was that stigma of temporal dispersion which it was the historian's duty to suppress from history. It has now become one of the basic elements of historical analysis. It appears in this analysis with a triple role. First it constitutes a deliberate operation of the historian (and no longer what he receives willy-nilly from the material he has to deal with): for he must, at least as a systematic hypothesis, distinguish between the possible levels of his analysis, and establish the periodizations which suit them. It is also the result of his description (and no longer what has to be eliminated by the action of his analysis): for what he undertakes to discover is the limits of a process, the point of change of a curve, the reversal of a regulatory movement, the bounds of an oscillation, the threshold of a function, the emergence of a mechanism, the moment a circular causality is upset. Finally, it is a concept which his work constantly specifies: it is no longer a pure and uniform void interposing a single blank between two positive patterns; it has a different form and function according to the domain and level to which it is assigned. A notion that cannot but be rather paradoxical: since it is both instrument and object of the investigation, since it delimits the field of an analysis of which it is itself an effect; since it makes it possible to individualize the domains, but can only be established by comparing them; since it only breaks down units in order to establish new ones; since it punctuates series and duplicates levels; and, in the last analysis, since it is not just a concept present in the historian's discourse, but one that he secretly presupposes: on what basis could he speak if not on that of this rupture which offers him as an object history - and its own history.

To be schematic, we could say that history and, in a general way, historical disciplines have ceased to be the reconstitution of the concatenations behind the apparent sequences; they now practise the systematic introduction of discontinuity. The great change which characterizes them in our

day is not the extension of their domain to economic mechanisms which which they have long been familiar; nor is it the integration of ideological phenomena, forms of thought, types of mentality: they were already being analysed in the nineteenth century. It is rather the transformation of discontinuity: its transition from obstacle to practice; an internalization into the discourse of the historian which means it need no longer be an external fatality that has to be reduced, but rather an operational concept to be utilized; an inversion of sign thanks to which it is no longer the negative of historical reading (its underside, its failure, the limits of its power), but the positive element which determines its object and validates its analysis. We must be prepared to understand what has become history in the real work of the historians: a certain controlled use of discontinuity for the analysis of temporal series.

It is clear why much is still invisible in this fact which is contemporaneous with us and yet which historical learning has born witness to for nearly half a century. Indeed, if history could remain the chain of uninterrupted continuities, if it ceaselessly linked together concatenations which no analysis could undo without abstraction, if it wove obscure syntheses always in the process of reconstitution around men, their words and their deeds, it would be a privileged shelter for consciousness: what it takes away from the latter by bringing to light material determinations, inert practices, unconscious processes, forgotten intentions in the silence of institutions and things, it would restore in the form of a spontaneous synthesis; or rather, it would allow it to pick up once again all the threads that had escaped it, to re-animate all those dead activities and to become again their sovereign subject in a new or restored light. Continuous history is the correlate of consciousness: the guarantee that what escapes from it can be restored to it- the promise that it will some day be able to appropriate outright all those things which surround it and weigh down on it, to restore its mastery over them, and to find in them what really must be called - leaving the word all its overload of meaning - its home. The desire to make historical analysis the discourse of continuity and the desire to make the human consciousness the originating subject of all learning and all practice, are the two faces of one and the same system of thought. This system conceives time in terms of totalization, and revolution never as anything but a coming to consciousness.

However, since the beginning of this century, psychoanalytical, linguistic, and then ethnological, research has dispossessed the subject (ie the 'Human consciousness' as the constituting subject of history - trans.) of the laws of its desire, the forms of its speech, the rules of its action, and the systems of its mythical discourses. Those, in France, who are securely in control, constantly reply: 'yes, but history ... history which is not a structure, but a prosess of becoming; not simultaneity, but succession; not a system but a practice; not a form, but a never-ending effort of a consciousness coming back to itself, and attempting to regain control of itself right down to the most basic of its conditions; history, which is not discontinuity but long and uninterrupted patience.' But in order to chant this contestatory litany, it was essential to divert attention from the work of historians, that is, refuse to see what is actually happening in their practice and discourse; close one's eyes to the great mutation of their discipline; remain obstinately blind to the fact that perhaps history is not a better shelter for the sovereignty of consciousness, less perilous than that of myths, language or sexuality; in short, for the sake of salvation, it was essential to reconstitute a History which is no longer being done. And if

this history could not offer enough security, the development of thought, knowledges, learning and the development of a consciousness forever close to itself, indefinitely bound to its past and present in all its moment, was asked to save what had to be saved: who dares strip the subject of its recent history? Every time the use of discontinuity becomes too visible in an historical analysis (particularly if it is concerned with knowledge) the cry goes up: history murdered! But do not make a mistake here; what is mourned for so loudly is in no sense the obliteration of history, but the disappearance of that form of history which was secretly, but in its entirety, transferred to the synthetic activity of the subject. All the treasure of the past had been hoarded in the ancient citadel of this history. It was believed to be strong, because it was sanctified, and it was the last bastion of philosophical anthropology, But historians went elsewhere long ago. They can no longer be counted on to protect the privileges or to reaffirm once again - however necessary it might be in the present troubles - that history at least is living and continuous.

The Field of Discursive Events

If one wants to apply the concept of discontinuity systematically (ie to define it, to use it in as general a way as possible and to validate it) to these domains - so uncertain of their frontiers and so indecisive in their content - which are called the history of ideas, thought, science, knowledges, a certain number of problems arise.

Firstly the negative tasks. It is essential to break free of a series of notions which are connected with the postulate of continuity. Doubtless, they do not have a very rigorous structure, but their function is very precise. Such is the notion of tradiiton which makes it possible both to register all innovations with respect to a system of permanent coordinates and to give a status to a set of constant phenomena. Such is the notion of influence, which gives a more mystical than substantial support to the facts of transmission and communication. Such is the notion of development which makes it possible to describe a sequence of events as the manifestation of one and the same organising principle. Such is the symmetrical and inverse notion of teleology or evolution towards a normative stage. Such are the notions of the mentality or spirit of an epoch, which make it possible to establish a community of meanings, of symbolic ties or a play of resemblances and reflection between simultaneous or successive phenomena. All ready made syntheses, all groupings which one accepts before any examination, all those ties whose validity is accepted beforehand, should be abandoned.

There is no longer any need to consider as valid the lines of demarcation between disciplines or the groups with which we have become familiar. As they stand, one cannot accept either the distinction between the broad types of discourse, not that between forms or genres (science, literature, philosophy, religion, history, fictions, etc). The reasons are blindingly obvious. We are ourselves uncertain of the use of these distinctions in the world of our own discourse. This is true a fortiori when one is concerned to analyse sets of statements which were distributed, scattered and generally characterised in a completely different manner: after all, 'literature' and 'politics' are recent categories which can only be applied to medieval or, even classical culture by means of a retrospective hypothesis and by a play of new analogies or semantic resemblances. Neither literature nor politics nor, consequently, philosophy and the sciences, were articulated in the field of discourse in the seventeenth and eighteenth centuries, as they were in the nineteenth century. Anyway, it is clearly necessary to

recognise that these divisions - those which we accept today, or those which are contemporary to the discourses studies - are always themselves reflexive categories, principles of classification, normative rules and institutionalised types; they are in turn facts of discourse which merit analysis alongside other facts, which certainly have complex relations with them, but do not have intrinsic characteristics which are autonomous and universally recognisable.

But, above all, the units which must be questioned are those which appear most immediately: those of the book and the oeuvre. At first sight they cannot be removed without extreme artificiality; they are given in a most certain manner, either by a material individualization (a book is a thing which occupies a determinate space, has its economic value and itself marks the limits of its beginning and end with a number) or by an assignable relation (even if in certain cases it is rather problematic) between discourses and the individual who has put them forward. But the unity of a book is not a homogeneous unity: the relations that exist between different mathematical treatises are not the same as those existing between different philosophical texts. Further, the edges of a book are neither clear nor rigorously delineated. No book exists by itself, it is always in a relation of support and dependence vis-a-vis other books; it is a point in a network- it contains a system of indications that point, explicitly or implicitly, to other books, other texts, or other sentences. If one is concerned with a book of Physics, or with a collection of political speeches, or with a science-fiction novel, the system of indications and consequently the complex relations of autonomy and heteronomy will differ.

Finally, as a last measure to put out of circulation the unreflected continuities by means of which the discourse that one seeks to analyse, is half secretly organized in advance, it is crucial to renounce two postulates which are bound together facing one another. The one assumes that it is never possible to find the irruption of a genuine event in the order of discourse; that beyond every apparent beginning there is always a secret origin - so secret and primordial that it can never be entirely recaptured in itself. So much so that one is led fatefully through the naiveté of chronologies, towards an indefinitely distant point, never present in any history. The point itself could only be its own emptiness; all beginnings from that point could only be recommencements or occultations (strictly speaking both at one and the same time). Linked to this is the thesis that every manifest discourse secretly rests on an 'already said'; but that this 'already said' is not just a phrase already pronounced, a text already written, but a 'never said' - a disembodied discourse, a voice as silent as a breath, a writing which is only the void left by its own inscription. These two themes which function to guarantee the infinite continuity of the discourse and its secret presence to itself in the action of an absence which is always one stage further back, must be renounced. Each moment of the discourse must be welcomed in its irruption as an event; in the point where it appears; and in the temporal dispersion which allows it to be repeated, known, forgotten, transformed, wiped out down to its slightest traces, and buried far from every eye in the myriads of books. There is no need to retrace the discourse to the remote presence of its origin; it must be treated in the action of its occurrence.

Once these preliminary forms of continuity, these unregulated syntheses of the discourse are set aside, a whole domain is set free. An immense domain, but one which can be defined; it is constituted by the set of all effective statements (whether spoken or written) in their dispersion

as events, and in the instance which is peculiar to each of them. Before it is dealt with as a science, a novel, a political discourse, or the work of an author or even a book, the material to be handled in its initial neutrality is a population of events in the space of discourse in general. Hence the project of a pure description of the facts of discourse. This description is easily distinguished from a linguistic analysis .... The question asked by linguistic analysis, concerning a fact of discourse, is always: according to what rules has this statement been constituted and consequently, according to what rules could other similar statements be constructed? The description of discourse asks a different question: how is it that this statement appeared, rather than some other one in its place?

Similarly, it is clear why this description of discourse is opposed to the analysis of thought. There too a system of thought can only be reconstituted from a definite set of discourses. But this set is treated in such a manner that one attempts to rediscover, beyond the statements themselves, the intention of the speaking subject, his conscious activity, what he meant, or even the unconscious pattern that emerges against his will in what he says or in the hardly discernable cracks in his explicit utterances. At any rate, it is a matter of reconstituting another discourse, rediscovering the barely audible, murmuring, endless utterance which animates the voice which is heard from within and re-establishing the tenuous and invisible text which skims through the interstices of the written lines and occasionally jostles them. The analysis of thought is always allegorical in relation to the discourse which it uses. Its question is invariably; what then was being said in what was said? But the analysis of discourse is directed to another end: it is concerned to grasp the statement in the narrowness and singularity of its event; to determine the conditions of its existence, to locate as accurately as possible its limits, to establish its correlations with the other statements with which it may be linked, and to show what other forms of articulation it excludes. It does not look beneath what is manifest for the barely-heard mutterings of another discourse. It must show why the discourse could not be other than it is, what makes it exclusive of other discourses and how it takes up a position among other discourses and in relation to them which no other could occupy. The real question of the analysis of discourse could therefore be formulated as follows: What is this regular existence which comes to the fore in what is said, - and nowhere else?

One might ask what ultimate use is this suspension of all accepted units, this obstinate pursuit of discontinuity, if it is no more than a matter of releasing a cloud of discursive events, of collecting them and preserving them in their absolute dispersion. In fact, the systematic destruction of merely given units makes it possible, firstly, to restore to the statement its singularity as an event: it is no longer regarded merely as the intervention of a linguistic structure, nor as the episodic manifestation of a deeper significance than itself; it is dealt with at the level of its historical irruption; an attempt is made to direct attention at the incision it constitutes, the irreducible- and often minute - emergence. However banal it is, however unimportant its consequences may seem, however quickly it is forgotten after its appearance, however little understood or badly deciphered one would think it, however quickly it may be devoured by the night, a statement is always an event which neither language nor meaning can completely exhaust. A strange event, certainly: first because on the one hand it is linked to an act of writing or to the articulation of a speech, but on the other hand opens for itself a residual existence in the field of a memory, or in the

materiality of manuscripts, books and any other form of record; then because it is unique like every other event, but is open to repetition, transformation and re-activation; finally, because it is linked both with the situations which give rise to it, and to the consequences it gives rise to, but also at the same time and in quite another modality, to the statements which precede it and follow it.

But the instance of the statement-event has been isolated with respect to language and thought not in order to deal with it in itself as if it were independent, solitary and sovereign. On the contrary, the aim is to grasp how these statements, as events and in their so peculiar specificity, can be articulated to events which are not discursive in nature, but may be of a technical, practical, economic, social, political or other variety. To reveal in its purity the space through which discursive events are scattered is not to undertake to establish it inside a break (coupure) which nothing could cross; it is not to close it in on itself; nor, a fortiori, to open it to a transcendence; on the contrary, it is to acquire the freedom to describe a series of relations between it and other systems outside it. Relations which have to be established - without recourse to the general form of language or to the individual consciousnesses of the speaking subjects - in the field of events.

The third advantage of such a description of the facts of discourse is that releasing them from all the groupings which present themselves as natural, immediate and universal unities makes it possible to describe other unities, but this time by a set of controlled decisions. Given that the conditions are clearly defined, it might be legitimate, on the basis of correctly described relations, to constitute discursive ensembles which would not be new but would, however, have remained invisible. These ensembles would not be at all new, because they would be made up of already formulated statements, between which a certain number of well-determined relations could be recognized. But these relations would never have been formulated for themselves in the statements in question (unlike for example those explicit relations which are posed and pronounced by the discourse itself when it adopts the forms of the novel, or is inscribed in a series of mathematical theorems). But these invisible relations would in no way constitute a kind of secret discourse animating the manifest discourses from within; it is not therefore an interpretation which could make them come to light, but rather the analysis of their coexistence, of their succession, of their mutual dependence, of their reciprocal determination, of their independent or correlative transformation. All together (though they can never be analysed exhaustively), they form what might be called, by a kind of play on words, for consciousness is never present in such a description, the unconsciousness, not of the speaking subject, but of the thing said.

Finally, a more general theme might be outlined on the horizon of all these investigations: the theme of the mode of existence of discursive events in a culture. What has to be brought out is the set of conditions which, at a given moment and in a determinate society, govern the appearance of statements, their preservation, the links established between them, the way they are grouped in statutary sets, the role they play, the action of values or consecrations by which they are affected, the way they are invested in practices or attitudes, the principles according to which they come into circulation, are repressed, forgotten, destroyed or re-activated. In short, it is a matter of the discourse in the system of its institutionalization. I shall call an archive, not the totality of texts which have been preserved by a civilization or the set of

traces that could be salvaged from its downfall, but the series of rules which determine in a culture the appearance and disappearance of statements, their retention and their destruction, their paradoxical existence as events and things. To analyse the facts of discourse in the general element of the archive is to consider them, not at all as documents (of a concealed significance or a rule of construction), but as monuments; (1) it is - leaving aside every geological metaphor, without assigning any origin, without the least gesture towards the beginnings of an archè - to do what the rules of the etymological game allow us to call something like an archaeology.

Discursive Formation and Positivities

Initially, it seemed to me that certain statements could form a set insofar as they referred to one and the same object. After all, statements concerning madness, for example, are not all on the same formal level (they are far from all obeying the criteria required for a scientific statement); they do not all belong to the same semantic field, (some come from medical semantics, others from legal or administrative semantics; others use a literary vocabulary), but they are all related to that object outlined in different ways in individual or social experience which can be designated as madness. Yet it is easy to see that the unity of the object does not allow the individualisation of a set of statements and the establishment of a descriptive and constant relation between them. This for two reasons. Firstly, the object, far from being what it is possible to define a set of statements in relation to, is rather constituted by the set of those formulations; it would be wrong to look for the unity of the discourse of psycho-pathology or psychiatry in 'mental illness'; it would certainly be wrong to ask of the very being of this illness, of its hidden content, of its truth, dumb and shut in on itself, what it has been possible to say of it at any given moment: rather mental illness has been constituted by the set of what it has been possible to say in the group of all the statements that named it, delineated it, described and explained it, give account of its developments, indicated its diverse correlations, judged it, and eventually allowed it to speak by articulating, in its name, discourses which were to pass for its speech.

The characteristic relation which permits the individualisation of a general unity of statements concerning madness is, therefore: the rule of the simultaneous or successive appearance of the various objects which are named, described, analysed, valued, or judged in it; the law of their exclusion or mutual implication; the system which governs their transformation. The unity of discourse on madness is not founded on the existence of the object 'madness', or on the constitution of a unique horizon of objectivity; it is the series of rules which make possible, during a given period, the appearance of medical descriptions (with their object), the appearance of a series of discriminatory and repressive measures (with their particular object), and the appearance of a set of practices codified in prescriptions or medical treatments (with their specific objects); it is thus the set of rules which takes account of the object's non-coincidence with itself, its perpetual difference, its deviation and dispersion reather than of the object itself in its identity. Over and above the unity of discourses on madness, it is the pattern of the rules which define the transformations of these different objects, their non-identity through time, the break which is produced in them, and the internal discontinuity which suspends their permanence. Paradoxically, to define the individuality of a set of statements does not consist of individualising its object, fixing its identity, or describing the characteristics which it per-

manently retains; on the contrary, it is to describe the dispersion of these objects, to grasp all the interstices which separate them, to measure the distances reigning between them - in other words, to formulate their law of distribution.

The second criterion which could be used to constitute discursive sets is the type of enunciation used.

It had seemed to me for example that from the beginning of the nineteenth century, medical science was characterized less by its objects or concepts (of which the former remained the same while the latter were entirely transformed) than by a certain style, a certain constant form of enunciation: a descriptive science could be seen coming into existence, medicine seemed to be formalizing itself as a series of descriptive statements. But here too it proved necessary to abandon this initial hypothesis. I had to admit that clinical medicine was just as much a set of political prescriptions, economic decisions, institutional settlements and educational models as it was a set of descriptions; that at any rate the latter could not be abstracted from the former, and that descriptive enunciation was only one of the formulations present in clinical discourse as a whole. In fact, the unity of the clinical discourse is not a determinate form of statements, but the set of rules which simultaneously or successively made possible not only purely perceptive descriptions, but also observations mediated through instruments, protocols of laboratory experiments, statistical calculations, epidemiological or demographic observations, institutional settlements and political decisions. This whole set cannot be subject to a unique model of linear concatenation. It is rather a question of a group of diverse enunciations which are far from obeying the same formal rules, from having the same exigencies of proof, from maintaining a constant relation to truth, and from having the same operational function. What must be characterised as clinical medicine is the co-existence of those dispersed and heterogeneous statements; it is the system which governs their distribution, the support which they give to each other, the way in which they imply or exclude each other, the transformation that they undergo, and the pattern of their arisal, disposition and replacement. A temporal coincidence can be established between the appearance of the discourse and the introduction of a privileged type of enunciation in medicine. But the latter does not have a constituent or normative role. A set of diverse enunciational forms are unfolded beside and around this phenomenon; and it is the general ordering of this unfolding which constitutes, in its individuality, the clinical discourse. The rule of formation of these statements in their heterogeneity, in the very impossibility of their integration into a single syntactical chain, is what I shall term enunciational divergence (l'écart énonciatif). And I shall say that clinical medicine is characterised, as an individualized discursive set, by the divergence or the law of dispersion which governs the diversity of its statements.

The third criterion by which unitary groups of statements could be established is the existence of a series of permanent and internally consistent concepts. It might be supposed, for example, that the analysis of language and of grammatical facts made from Lancelot to the end of the eighteenth century depended on a definite number of concepts whose content and use were established once and for all: the concept of judgement defined as the general and normative form of every sentence, the concepts of subject and attribute grouped together in the more general category of the noun, the concept of the verb used as the equivalent of the logical copula, the concept of the word

defined as the sign of a representation. In this way it would seem possible to reconstitute the architecture of classical grammar. But here again, limitations appear immediately. One has to admit that new concepts appear some of which may be derived from the ones I have listed, but others of which are heterogeneous and some even incompatible with them. Must we then admit that grammar only apparently constitutes a consistent set; and that this set of statements, analyses, descriptions, principles and consequences, and deductions, is a false unity, though it survived under this name for more than a century?

In fact, it is possible to define a common system beneath all the more or less heterogeneous concepts of classical grammar which explains not only their emergence, but also their dispersion and, eventually, their incompatibility. This system is not constituted by concepts any more general and abstract than those that appear on the surface and are openly manipulated there; it is constituted rather by a set of rules of formation of concepts. This set is itself divided into four subordinate groups. There is the group which governs the formation of those concepts which permit the description and analysis of the sentence as a unit in which the elements (the words) are not merely juxtaposed, but related to one another. This set of rules may be called the theory of attribution. There is also the group which governs the formation of those concepts which permit a description of the relations between the different signifying elements of the sentence and the different elements of what is represented by these signs. This is the theory of articulation. The theory of designation governs the emergence not only of such concepts as that of the arbitrary and conventional sign, but also that of the spontaneous and natural sign, immediately charged with expressive value. Finally, the theory of derivation accounts for the formation of a very dispersed and heterogeneous series of notions; the idea of an immobility of language which is only subject to change as a result of external accidents; the idea of a historical correlation between the development of language and the individual's capacities for analysis, reflection and knowledge; the idea of a circular determination between the forms of language, those of writing, learning and science, those of social organisation, and, finally, those of historical progress; the idea of poetry understood not only as a particular use of vocabulary and grammar, but as the spontaneous movement of language shifting in the space of human imagination, which is, by its very nature, metaphorical. These four 'theories' - which are four formative schemata of concepts - have describable relations between them: they assume each other; they oppose each other in pairs; they derive one from the other and, in elaborating their logical sequence, they link up the discourses, which can neither be unified nor superimposable, into a single pattern. They form what may be called a theoretical network. This term must not be understood to mean a group of fundamental concepts which could regroup all the others and permit their replacement in the unity of a deductive architecture, but rather the general law of their dispersion, heterogenity and incompatibility (whether simultaneous or successive): the rule of their insurmountable plurality. And it is only permissible to recognise an individualisable set of statements in general grammar, insofar as all the concepts which appear, are interconnected, intersect, interfere with and follow each other, are hidden and scattered in it, are formed from one and the same theoretical network.

Lastly, one might attempt to constitute units of discourse on the basis of an identity of opinions. The 'human sciences' are so condemned to polemic, so open to the play of

preferences or interests, so permeable by philosophical or ethical themes, so apt in certain cases to political utilization, also so near to certain religious dogmas that it is legitimate in the first instance to suppose that a certain thematic might be capable of binding together a set of discourses, of balancing it like an organism which has its needs, its internal power and its survival capacities. For example, might not one consitute everything which belonged to evolutionist discourse from Buffon to Darwin as a unit? First, this theme is more philosophical than scientific, closer to cosmology than to biology; it has rather guided investigations from afar than named, discovered and explained results; it always presupposed more than was known, but on the basis of this fundamental choice, it made obligatory the transformation into discursive learning what was outlined as a hypothesis or as an exigency. Might one not speak in the same way of the physiocratic idea? An idea which postulated the natural character of the three ground rents beyond any proof and before any analysis; which therefore presupposed the political and economic primacy of landed property; which ruled out any analysis of the mechanisms of industrial production; which implied in return the description of the circulation of money inside a State, of its distribution between different social categories, and of the channels whereby it returned to production; which finally led Ricardo to consider the cases in which this triple rent did not appear, the conditions in which its formation was possible, and therefore to denounce the arbitrary character of the physiocratic theme?

But such an attempt leads one to make two opposing but complementary observations. In one case, the same fact of opinion, the same thematic, the same choice is articulated on the basis of two completely different series of concepts, two completely different types of discourse and two completely different fields of objects: the evolutionist idea, in its most general formulation, is perhaps the same in Benoît de Maillet, Bordeu or Diderot, and in Darwin; but in fact what makes it possible and consistent is not at all of the same order in both cases. In the 18th century, the evolutionist idea is a choice made on the basis of two well determined possibilities: either it is admitted that the common ancestry species forms a completely pre-given continuity interrupted and in some sense torn apart only by natural catastrophes, by the dramatic history of the earth, by the upheavals of an extrinsic time (in which case it is this time which creates the discontinuity, ruling out evolutionism); or on the other hand it is admitted that it is time that creates the continuity, the changes in nature which compel species to take characters different from those that were pre-given: such that the more or less continuous table of the species is like the outcrop of a whole stratum of time beneath the eyes of the naturalist. In the 19th century, the evolutionist idea is a choice which no longer involves the constitution of a table of species, but rather the modalities of the interaction between an organism, all of whose elements are solidary, and an environment which provides it with its real conditions of life. One 'idea' only, but based on two systems of choices.

On the other hand, in the case of physiocracy, one can say Quesnay's choice depends on exactly the same system of concepts as the contrary opinion upheld by those who might be called the utilitarians. In this period, the analysis of wealth contained a relatively limited series of concept and one which was generally agreed upon (everyone defined money in the same way as a mere sign without any value except through the practically necessary materiality of that sign; everyone explained price in the same way by the mechanism of barter and by the quantity of labour necessary to obtain the commodity; everyone determined the price of a given labour in the same way by the cost of the

upkeep of a worker and his family while the work was being done). But on the basis of this single conceptual system there were two methods of explaining the formation of value depending on whether the analysis was made on the basis of exchange or on that of the remuneration of the working day. These two possibilities inscribed in economic theory and in the rules of its conceptual system gave rise to two different opinions on the basis of the same elements.

It would finally be quite incorrect to look for the principles of the individualisation of a discourse in matters of opinion. What defines the unity of natural history, for example, is not the permanence of particular ideas such as that of evolution; what defines the unity of economic discourse in the eighteenth century is not the conflict between the physiocrats and the utilitarians, or between the owners of landed property and the partisans of commerce and industry. What permits the individualisation of a discourse and gives it an independent existence is the system of points of choice which it offers from a field of given objects, from a determinate enunciational scale; and from a series of concepts defined in their content and use. Therefore, it would be inadequate to look for the general foundations of a discourse and the overall form of its historical identity in a theoretical option; for a similar option can re-appear in two types of discourse, and a single discourse can give rise to several different options. Neither the permanence of opinions through time nor the dialectic of their conflicts is sufficient to individualise a set of statements. To do that, one must be able to register the distribution of points of choice, and define, behind every option, a field of strategic possibilities. If the physiocrats' analysis is a part of the same discourse as the utilitarians' analysis, it is not because they lived during the same period, nor because they confronted one another in the same society, nor because their interests were entangled in the same economy, but because their two options derive from one and the same distribution of points of choice, in one and the same stategic field. This field is not the total of all the conflicting elements, nor is it an obscure unity divided against itself and refusing to recognise itself in the mask of each of its opponents; it is the law of formation and dispersion of all possible options.

To sum up, we have here four criteria enabling us to recognise discursive units which are not at all the traditional units (whether 'text', 'work', 'science'; whatever the domain or form of the discourse, whatever the concepts it uses or the choices it manifests). These four criteria are not only not incompatible, they demand one another: the first defines the unit of a discourse by the rule of formation of all its objects; the next by the rule of formation of all its syntactic types; the third by the rule of formation of all its semantic elements; the fourth by the rule of formation of all its operational eventualities. All the aspects of discourse are thus covered. And when it is possible, in a group of statements, to register and describe one referential, one type of enunciational divergence, one theoretical network, one field of strategic possibilities, then one can be sure that they belong to what can be called a discursive formation. This formation groups together a whole population of statement-events. Obviously, neither in its criteria, in its limits, or in its internal relations, does it coincide with the immediate and visible units into which statements are conventionally grouped. It brings to light relations between the phenomena of enunciation which had hitherto remained in darkness, and were not immediately transcribed on the surface of discourses. But what it brings to light is not a secret, the unity of a hidden meaning, nor a general and unique form; it is a controlled system of differences and dispersions. This four-level system which governs a discursive formation and has to explain, not its common elements but the play of its divergences, its inter-

stices, its distances - in some sense its blanks rather than its full surfaces - that is what I propose to call its positivity.

Learning

At the outset the problem was to define units which could be legitimately installed in such a disproportionate domain as that of statement-events other than the hastily admitted forms of synthesis. I tried to give an answer to this question that would be empirical (and articulated into precise inquiries) and critical (since it concerned the place from which I was posing the question, the region which situated it, the spontaneous unity within which I could believe I was talking). Hence the investigations into the domain of the discourses which installed, or claimed to install, a 'scientific' knowledge of living, speaking and working men. These investigations have brought to light sets of statements which I have called 'discursive formations' and systems which should explain these sets called 'positivities'. But have I not in toto purely and simply produced a history of the human 'sciences' - or, if you will, of the inexact knowledges whose accumulation has not yet managed to consitute a science? Am I not still caught in their apparent divisions and in the system they pretend to adopt for themselves? Have I not made a kind of critical epistemology of these patterns which cannot firmly be said really to deserve the name of sciences?

In fact, the discursive formations which I have separated or described do not precisely coincide with the delimitation of these sciences (or pseudo-sciences). Undoubtedly I opened my inquiry into the history of Madness on the basis of the existence at present of a discourse which calls itself psycho-pathology (and which some may regard as having pretensions to be scientific); undoubtedly I undertook to analyse what it was possible to say about wealth, money, exchange, about linguistic signs and the functioning of words, in the seventeenth and eighteenth centuries on the basis of the existence of an economics and a linguistics (whose criteria of scientific rigour may well be contested by some). But the positivities obtained at the end of the analysis and the discursive formations that the group together do not cover the same space as these disciplines, and are not articulated as they are; to go further, they cannot be superimposed on what is was possible to regard as a science, or as an autonomous form of discourse in the period under study. Thus the system of positivity analysed in Madness and Civilization does not explain, either exclusively or in a privileged way, what doctors were able to say about mental disease at the time; rather it defines the referential, the enunciational scale, the theoretical network, the points of choice which made possible the very dispersion of medieval statements, institutional controls, administrative measures, literary expressions and philosophical formulations. The discursive formation, constituted and described by the analysis, goes far beyond the account that might have been given of the pre-history of psycho-pathology or of the genesis of its concepts.

In The Order of Things, this situation is inverted. The positivities obtained by description isolate discursive formations which are narrower than the scientific domains recognised in the first instance. The system of Natural History permits the explanation of a certain number of statements about the resemblences and differences between beings, the constitutions of specific and generic characteristics, the distribution of relationships in the general space of the table; but it does not govern the analyses of involuntary movement, nor the theory of genera, nor the chemical explanations of growth. The existence, the autonomy, the internal consistency and the limitedness

of this discursive formation is precisely one of the reasons why a general science of life was not constituted in L'âge classique (ie, the 17th and 18th centuries.) Similarly, the positivity which governed the analysis of wealth in the same period did not determine every statement about exchange, commercial transactions and prices: it left out 'political arithmetic' which did not enter the field of economic theory until much later, when a new system of positivity had made the introduction of that kind of discourse into economic analysis both possible and necessary. Nor does general grammar explain all that it was possible to say about language in L'âge classique (whether by exegetes of religious texts, by philosophers, or by theoreticians of literary works). In none of those three cases was it a matter of discovering what men could have thought about language, wealth or life at a time when a biology, an economics and a philology were slowly and stealthily constituting themselves; nor was it a matter of finding out the errors, prejudices, confusions or even fantasies still mixed up with the concepts on their way to formation; nor was it a matter of knowing the price in breaks and repressions which a science, or at least a discipline with scientific pretensions, had to pay in order to constitute itself at last on such impure ground. It was a matter of bringing out the system of that 'impurity' - or rather, for the word can have no meaning in this analysis, of explaining the simultaneous appearance of a certain number of statements whose level of scientificity, form and degree of elaboration may well seem heterogeneous to us in retrospect.

The discursive formation analysed in La Naissance de la Clinique represents a third case. It is much broader than medical discourse in the strict sense of the term (the scientific theory of illness, of its forms, of its determinations and of therapeutic instruments); it englobes a whole series of political reflections, reform programmes, legislative measures, adminstrative settlements, and ethical considerations, but on the other hand, it does not include everything which it was possible to know in the period studied about the human body, about its workings, its anatomico-physiological correlations, and about the disturbances which may occur in it. The unity of clinical discourse is in no sense the unity of a science or of a set of knowledges attempting to acquire a scientific status. It is a complex unity: the criteria by which we can - or think we can - distinguish one science from another (eg, physiology from pathology), a more developed science from one which is less so (eg, biochemistry from neurology), a really scientific discourse (such as hormonology) from a mere codification of experience (such as semiology), a real science (such as micro-biology) from a science which was not a science (such as phrenology), could not be applied to it. Clinical medicine constitutes neither a false science nor a true one, although in the name of present day criteria we may assume the right to recognise the truth of certain of its statements and the falsity of certain others. It is an enunciational ensemble which is both theoretical and practical, descriptive and institutional, analytical and prescriptive, made up of inferences as well as decisions, of assertions as well as degrees.

.... The discursive formations are neither current sciences in gestation, nor sciences formerly recognised as such, then fallen into desuetude and abandoned as a result of the new requirements of our criteria. They are unities of a different kind and on a different level from what is called today (or was once)called a science. In order to characterise them, the distinction between scientific and non-scientific is not pertinent: they are epistemologically neutral. As for the systems of positivity which ensure unitary grou-

ping, they are not rational structures, nor are they patterns, equilibria, oppositions or dialectics between forms of rationality and irrational constraints; the distinction between the rational and its opposite is not pertinent in describing these unities; they are not the laws of intelligibility, but the laws of the formation of a whole set of objects, types or formulation, concepts, and theoretical options which are invested in institutions, techniques, collective and individual behaviour, political operations, scientific activities, literary fictions and theoretical speculations. The set, thus formulated from the system of positivity, and manifested in the unity of a discursive formation is what might be called a learning. Learning is not a sum of scientific knowledges - since it should always be possible to say whether the latter are true or false, accurate or not, approximate or definite contradictory or consistent; none of these distinction is pertinent in describing learning, which is the set of the elements (objects, types of formulation, concepts and theoretical choices) formed from one and the same positivity in a field of a unitary discursive formation.

We are now dealing with a complex pattern. It can and must be analysed both as a formation of statements (when considering the population of discursive events which are part of it); as a positivity (when considering the system which governs the dispersion of the objects, the types of formulation, the concepts and the opinions which come into play in these statements); as a learning (when considering these objects, types of formulation, concepts and opinions as they are invested in a science, a technical recipe, an institution, a fictional narrative, a legal or political practice, etc). Learning cannot be analysed in terms of knowledge; nor can positivity in terms of rationality; nor can the discursive formation in terms of science. And it is impossible to ask that their description be equivalent to a history of knowledges, a genesis of rationality or the epistemology of a science.

It remains true nonetheless that it is possible to describe a certain number of relations between the sciences (with their structures of rationality and the sum of their knowledges) and the discursive formations (with their system of positivity and the field of their learning). For it is true that only formal criteria can decide about the scientificity of a science, ie can define the conditions which make it possible as a science, but they can never account for its factual existence, ie its historical appearance, the events, episodes, obstacles, dissensions, expectations, delays, and facilitation which have been able to stamp its actual destiny.

Under the general term of the 'conditions of possibility' of a science, two heteromorphous systems must be distinguished. The first defines the conditions of the science as a science: it is relative to its domain of objects, to the type of language it uses, to the concepts which it has at its disposal or which it is seeking to establish; it defines the formal and semantic rules which are required for a statement to belong to the science; it is instituted either by the science in question, insofar as it poses its own norms for itself, or by another science, insofar it imposes itself on the former as a model of formalisation; at any rate, these conditions of scientificity are internal to the scientific discourse in general, and cannot be defined other than through it. The other system is concerned with the possibility of a science in its historical existence. It is external to the science and the two cannot be superimposed. It is constituted by a field of discursive sets which have neither the same status, units, organisation, nor the same functioning as the sciences to which they give rise. These discursive sets should not be seen as a rhapsody of false knowledges, archaic themes and irrational figures which the sciences; in their sovereignty, definitively thrust aside into the night of a prehistory. Nor should they be imagined as the outline of future sciences which are still confusedly wrapped around their futures, vegetating for a time in the half sleep of silent germination. Finally, they should not be conceived as the only epistemological system to which those supposedly false, quasi- or pseudo-sciences the human sciences, are susceptible. In fact, the system is concerned with patterns which have their own consistency, laws of formation and autonomous disposition. To analyse discursive formations, positivities and the learning which corresponds to them is not to assign forms of scientificity, but rather to run through a field of historical determination which must account for the appearance, retention, transformation, and, in the last analysis the erasure of discourses, some of which are still recognised today as scientific, some of which have lost that status, some have never pretended to acquire it, and finally, others have never attempted to acquire it. In a word, Learning is not science in the successive displacement of its internal structures, but it is the field of its actual history.

Concluding Remarks.

The analysis of discursive formations and of their system of positivity in the element of learning only concerns certain determinations of discursive events. There can be no question of constituting a unitary discipline replacing all other descriptions of discourses and invalidating them en bloc. Rather it is a question of giving a place to different, already familiar and often long practised types of analyses: of determining their level of functioning and effectivity; of defining their points of application; and finally of avoiding the illusions to which they can give rise. To bring into existence the dimension of learning as a specific dimension is not to reject the various analyses of science, it is to unfold as broadly as possible the space in which they can come to rest. Above all, it is to give notice to two forms of extrapolation, which have symmetrical and inverse reductive roles: epistemological extrapolation and genetic extrapolation.

Epistemological extrapolation should not be confused with the (always legitimate and possible) analysis of the formal structures which may characterize a scientific discourse. But it suggests that these structures are enough to define for a science the historical law of its appearance and unfolding. Genetic extrapolation should not be confused with the (always legitimate and possible) description of the context - whether discursive, technical, economic or institutional - in which a science appeared; but it suggests that the internal organization of a science and its formal norms can be described on the basis of its external conditions. In one case, the science is given the responsibility of explaining its own historicity; in the other, various historical determinations are required to explain a scientificity. But this is to ignore the fact that the place in which a science appears and unfolds is neither this science itself distributed according to a teleological sequence, nor a set of mute practices or extrinsic determinations, but the field of learning, with the set of relations which traverse it. This ignorance can in fact be explained by the privilege granted to two types of sciences, which serve in general as models whereas they are surely limit cases. There are indeed sciences of such a type that every episode of their historical development can be described as a movement of lateral extension, then of repetition and generalization at a higher level, such that each moment appears either as a special region, or as a definite degree of formalization; sequences are abolished in favour of proximities which do not reproduce them; and

dates are removed in order to reveal synchronies which know no calendar. This is clearly the case of mathematics, in which Cartesian algebra defines a special region in a field which was generalized by Lagrange, Abel and Galois; in which the Greek method of exhaustion seems to be contemporary with the calculus of definite integrals. On the other hand, there are sciences which can only secure their unity through time by the narration or critical repetition of their own history: if there has been one and only one psychology since Fechner, if there has been only one sociology since Comte, or even since Durkheim, it is not insofar as it is possible to assign a single epistemological structure (as tenuous as is conceivable) to so many diverse discourses; it is insofar as sociology or psychology have at each moment located their discourse in an historical field which they themselves had traversed in the critical mode of confirmation or invalidation. The history of mathematics is always on the point of crossing the boundary of epistemological description; the epistemology of 'sciences' like psychology or sociology is always on the edge of a genetic description.

That is why, far from constituting privileged examples for the analysis of all other scientific domains, these two extreme cases rather threaten to lead to an error: the failure to reveal both in their specificity and in their relations the level of epistemological structures and the level of determinations of learning; the fact that all sciences (even ones as highly formalised as mathematics) presuppose a space of historicity which does not coincide with the interaction of its forms; but that all sciences (even ones as heavy with empiricities as psychology and as far from the norms required to constitute a science) exist in the field of a learning which does not merely prescribe the sequence of their episodes, but which determines their laws of formation according to a describable system. On the other hand, there are 'intermediate' sciences - such as biology, physiology, political economy, linguistics, philology, for example - which ought to provide the models: for with them it is impossible to fuse the instance of learning and the form of science into a false unity, or to elide the moment of learning.

It is possible on this basis to situate a certain number of legitimate descriptions of the scientific discourse in their possibility, but also to define them in their limits. Descriptions which are not directed towards learning as an instance of formation, but to the objects, forms of enunciation, concepts, and finally to the opinions to which they give rise. Descriptions which will, nevertheless, only remain legitimate on the condition that they do not pretend to discover the conditions of existence of something as a scientific discourse. It is thus perfectly legitimate to describe the series of opinions or theoretical options which emerge in a science and à propos a science; one must be able to define, for a historical period or determinate domain, what are the principles of choice, in what way (by what rhetoric or dialectic) they are manifested, hidden or justified, how the field of the polemic is organised and institutionalised, what are the motivations which may characterise the individuals; in short, there is room for a doxology, the description (sociological or linguistic, statical or interpretative) of the facts of opinion.

Finally, it is possible and legitimate to define, by a regional analysis, the domain of objects to which a science addresses itself. And to analyse it either on the horizon of ideality which the science constitutes (by a code of abstraction, by rules of manipulation, by a system of presentation and potential representation) or in the world of things to which those objects refer. For if it is true that

the objects of biology or of political economy are indeed defined by a particular structure of ideality peculiar to these two sciences, and if these are not purely and simply the life in which individual human beings participate, or the industrialisation which they have fashioned, nevertheless, these objects refer to experience or to a definite phase of capitalist evolution. But it would be incorrect to believe (through an illusion of experience) that there are regions or domains of things which present themselves spontaneously to an activity of idealisation and to the work of scientific language; that these things unfurl themselves in the order in which history, technology, discoveries, institutions and human instruments have managed to constitute them or bring them to light; that all scientific elaboration is only a certain way of reading, deciphering, abstracting, decomposing and recomposing what is given either in a natural (and consequently generally valid, experience or in a cultural (and consequently relative and historical) experience. There is an illusion which consists of the supposition that science is grounded in the plenitude of a concrete and lived experience; that geometry elaborates a perceived space, that biology gives form to the intimate experience of life, or that political economy translates the processes of industrialisation at the level of theoretical discourse; therefore, that the referent itself contains the law of the scientific object. But it is equally illusory to imagine that science is established by an act of rupture and decision, that it frees itself at one stroke from the qualitative field and from all the murmurings of the imaginary by the violence (serene or polemical) of a reason which founds itself by its own assertions: ie, that the scientific object brings itself into existence of itself in its own identity.

If there are, at the same time, both relations and a break between the analysis of life and the familiarity of the body, suffering, sickness and death; if there are ties and separation between political economy and a particular form of production; if, in a general way, science refers to experience and yet detaches itself from it, it is not a matter of univocal determination, nor of a sovereign, constant and definitive break. In fact, these relations of reference and separation are specific to each scientific discourse, and their form varies through history. This is because they are themselves determined by the specific instance of learning. The latter defines the laws of formation of scientific objects and by the same action specifies the connections or oppositions between science and experience. Their extreme proximity and their unbridgeable distance is never given at the outset, it finds its principle in the morphology of the referential; it is this which defines the reciprocal disposition - the confrontation, opposition, their system of communication - of the referent and the object. Between science and experience, there is learning no longer as an invisible mediation, or as a secret complicit pander between two distances so difficult to reconcile and unravel at the same time. In fact, Learning determines the space in which science and experience can be separated and situated one in relation to the other.

Note.

(1) I am indebted to Georges Canguilhem for the idea of using the word in this sense.

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