

This article was downloaded by: [Macquarie University]

On: 25 August 2008

Access details: Access Details: [subscription number 731690693]

Publisher Routledge

Informa Ltd Registered in England and Wales Registered Number: 1072954 Registered office: Mortimer House, 37-41 Mortimer Street, London W1T 3JH, UK



Economy and Society

Publication details, including instructions for authors and subscription information:

<http://www.informaworld.com/smpp/title-content=t713685159>

From Bachelard to Althusser: the concept of 'epistemological break'

Etienne Balibar

Online Publication Date: 01 August 1978

To cite this Article Balibar, Etienne(1978)'From Bachelard to Althusser: the concept of 'epistemological break'',Economy and Society,7:3,207 — 237

To link to this Article: DOI: 10.1080/03085147800000013

URL: <http://dx.doi.org/10.1080/03085147800000013>

PLEASE SCROLL DOWN FOR ARTICLE

Full terms and conditions of use: <http://www.informaworld.com/terms-and-conditions-of-access.pdf>

This article may be used for research, teaching and private study purposes. Any substantial or systematic reproduction, re-distribution, re-selling, loan or sub-licensing, systematic supply or distribution in any form to anyone is expressly forbidden.

The publisher does not give any warranty express or implied or make any representation that the contents will be complete or accurate or up to date. The accuracy of any instructions, formulae and drug doses should be independently verified with primary sources. The publisher shall not be liable for any loss, actions, claims, proceedings, demand or costs or damages whatsoever or howsoever caused arising directly or indirectly in connection with or arising out of the use of this material.

ECONOMY AND SOCIETY

Volume 7 Number 3 August 1978

Contents

Etienne Balibar From Bachelard to Althusser: the concept of 'epistemological break'	207
Ernesto Laclau Introduction to Cerroni	238
Umberto Cerroni Democracy and socialism	241
<i>Review article</i>	
Laurence Harris The science of the economy	284
Correspondence	321

Published quarterly for the Editors by Routledge & Kegan Paul Ltd.,
London, Henley and Boston

ECONOMY AND SOCIETY

Editorial Board

Talal Asad *University of Hull*

Terence J. Johnson *University of Leicester*

Ernesto Laclau *University of Essex*

Mary McIntosh *University of Essex*

Keith Tribe *University of Keele*

Harold Wolpe *University of Essex*

Sami Zubaida *Birkbeck College, University of London*

Claude Meillassoux *Centre National de la Recherche Scientifique, Paris,*
(Corresponding Member)

G. Carchedi *University of Amsterdam* (Corresponding Member)

Contributions are welcomed by the Editors. All contributions, correspondence, and other material dealing with the editorial matter of this journal should be sent to The Editors, *Economy and Society*, Routledge & Kegan Paul Ltd., Broadway House, Reading Road, Henley-on-Thames, Oxon RG9 1EN, England. Notes on the form that contributions should take are available from the Editors at this address.

Books for review should be sent to Terence J. Johnson, Department of Sociology, The University, Leicester and not to the Publishers.

Economy and Society is published quarterly in February, May, August and November. The annual subscription is £8.50 (US \$20.00) for individuals; £9.50 (US \$20.00) for institutions; £6.00 (US \$15.00) for members of the British and American Sociological Associations (please use the Associations' special order forms). All back issues are available at £9.50 (US \$18.00) per volume or £3.00 (US \$8.00) per issue. All prices include postage; American subscription rate includes air service.

Subscriptions together with remittances should be sent to *Economy and Society*, Routledge & Kegan Paul Ltd., Broadway House, Reading Road, Henley-upon-Thames, Oxfordshire RG9 1EN, England or *Economy and Society*, Routledge & Kegan Paul Ltd., 9 Park Street, Boston, Mass. 02108, U.S.A.

Printed in England. Second Class postage paid at New York, N.Y.

U.S. Mailing Agent: Air and Sea Freight Inc., 527 Madison Ave., New York, N.Y. 10022.

From Bachelard to Althusser: the concept of 'epistemological break'

Etienne Balibar

In response to your invitation, I want if only schematically to enter into the dossier of our discussions a few reflections on an event which has given rise to very animated discussions in French philosophy for the last fifteen years. A certain *encounter* between Marxist philosophy on the one hand and works in the history of sciences and in epistemology on the other has taken place around the category of 'epistemological break' that has been put forward by Louis Althusser.

I do not claim that this question is necessarily of interest outside French frontiers. But I am sure it will be granted me that the general problem underlying Althusser's enterprise is not a problem peculiar to French philosophy. It can be designated as follows. What type of philosophy of sciences is capable of discussing, recognising and proving the scientific character of historical materialism (Marxism) and, in a more general way, the scientific character of a revolutionary mode of scientific knowledge concerning society, practice, and human conduct? The question is not posed solely with respect to Marxism but also, of course, with respect to psycho-analysis. But we can also present the problem in a reciprocal way. What are the ideological, and hence in the last analysis political, positions that are invested in all philosophy of sciences or epistemology, even though that sort of philosophy is concerned only with 'traditional' and recognised disciplines such as mathematics or biology? No doubt it is the reality of these problems as encountered by our contemporaries that explains the echo of Althusser's works in a number of countries other than France and explains the *way* he has generally been perceived — as a philosopher seeking either *to import a Marxist point of view into epistemology* or *to 'renovate' Marxism with the help of epistemological categories*, precisely by means of the 'epistemological break', the notion on which the controversies, the attacks, and the defences concentrate.

But, no sooner had I begun work on an exposition of this question for you than I had to take account of the fact that its objective was of inordinate length. I had to restrict its scope; and what I present to you today constitutes only a beginning of an

approach to the problems in question. For obvious historical reasons I shall today try to reflect on the transformation which has been worked between the problematic of Gaston Bachelard and that of Louis Althusser.

Althusser claims to have 'borrowed' the concept of 'epistemological break' from Bachelard. Bachelard is the author of what in this perspective has been called a 'historical epistemology', the effects of which on the practice of the history of sciences were felt immediately. At the same time, even if I do not speak of it in detail today, I want to try to take account in advance of what Althusser has called his 'self-criticism'. Some people have insisted on seeing in that self-criticism only the persistence of his errors, indeed the adoption of symmetrical errors. (Cf. Althusser 1976a pp. 114, 190).

Section 1

As I was saying, Althusser claims to have borrowed the concept of 'epistemological break' from Bachelard. How exactly do things stand with this initial relation? They are not as simple as they seem, and one might wonder if it is not a typical case of 'false recognition' in the Freudian sense of the term. It seems to me that in reality it is instead an original concept which Althusser introduced between 1960 and 1965, a concept which, it is true, owes 'something' to Bachelard and which does indeed rest on certain common philosophical presuppositions but which in fact has a quite other object and opens a quite other field of investigation.

Indeed, not only does Bachelard not speak of 'epistemological break' but also he does not speak much of 'epistemological rupture'. What are constantly present in Bachelard are the idea and the word 'breaking' and 'rupture' alternating in a little regulated way with the idea and word 'revolution', 'clean separation', 'mutation', 'deep discontinuity', indeed 'recasting of knowledge', and finally 'division' in the scientific mind. In short it can be said that the idea of *discontinuity* here seems to constitute the sole true and stable core under different metaphors. Hence the inevitable problem: in what site is this discontinuity installed, and between which terms or processes is it installed?¹

It is here that we must refer to the system of concepts that comprises Bachelardian epistemology. Canguilhem has clearly shown that 'it is by means of the invention of the concept of *epistemological obstacle* that Bachelard has shown himself to be an inspired innovator in the history of sciences.' (Canguilhem 1968 p. 174) This is because from the start Bachelard challenged the myths of empiricism about the progressive continuity of know-

ledge, challenged those myths in all their forms, including the form of the speculative empiricism of an eternal Reason. In so doing he opened to epistemology in an effective way the field of a real problem which had no anticipated and already given solution. This was the problem of studying, in the light of the information that alone can provide an effective practice of current science, the 'epistemological acts' which are necessary and which are nonetheless unpredictable, the 'epistemological syntheses' which are without true precedents and by means of which the science of the real progresses indefinitely by surmounting its epistemological obstacles.

Hence the unity of epistemology and the history of sciences. In fact, a problem like the one Bachelard opened can be treated and resolved only on the ground of history, if it is true that *the structure of epistemological obstacles is always specific* and, more than that, that it is a structure of 'epistemological acts' through which knowledge can surmount those obstacles. Such acts are always a *singular unity* of theoretical forms (in Bachelard, mathematical forms above all) and of precise experimental techniques, a unity realised in determinate scientific *concepts* which cannot be interchanged. That is why the history of sciences as indicated and to a certain extent practised by Bachelard is, contrary to a philosophical history of Knowledge in general, a regional history which follows the (moving) specialisation of scientific disciplines. But it is at the same time, contrary to the simple chronology of discoveries, a theoretical history which substitutes problems for the retrospective evidences and illusions of the scientist.²

We should note at the same time that this conceptualisation culminates in fact in posing *the primacy of epistemology over the history of sciences* in their relative fusion. There is, as it were, a double decomposition which is always occurring as a result of this unitary project. History of sciences is possible only as the application of an epistemological theory, on condition of course that, contrary to all previous philosophies of science, that theory is not a theory of the permanence of Reason (or of Experience) but that it is a theory of unpredictable, not finalistic, historicity of knowledge. And seeing that such a dialectic is shown only, as we have seen, in the singularity of its realisations, seeing that the sole achievement of dealing with it 'in general' would have the immediate effect of reversing it into speculation, it is very necessary that in its turn epistemology be constituted with historical problems and examples as its starting point, without thereby becoming purely and simply merged with history of sciences. Bachelard, it seems, consciously takes on this *circle*. He takes it on because this specular

decomposition of epistemology and history of sciences under the primacy of epistemology, which poses *in a general way* the impassable 'regionality' of scientific knowledge, is organically linked with *his treatment of the problem of philosophy*. If he criticises all the philosophies of science which seek a philosophical 'foundation' for science or which seek their own foundation *on* science, he is at the same time looking for a philosophy which is 'ultimately adequate' to effective science and which reproduces within itself, I mean in its style and method, indeed in its practice and strategy, the revolutionary characteristics of the modern 'new scientific mind', *beyond* empiricism and beyond substantivist metaphysics and formalist positivism. Now, as has been clearly shown by Lecourt, it is in this place that are concentrated precisely the difficulties of Bachelard's problematic, to such a degree that he has been able to surmount them only by allowing a historical epistemology to teeter about in a naturalistic philosophy of the Imaginary which is also an imaginary philosophy of Nature. (Lecourt 1974)

It is necessary here to emphasise two particular aspects of Bachelardian epistemology in which its innovatory force is manifested at the same time as the depth of its contradictions. These two aspects are thrown into particular relief if at the same time we compare this epistemology with other enterprises that have sometimes been compared with it, in particular Kuhn's theory of 'scientific revolutions'. I want to discuss first the radical division between 'common knowledge' and 'scientific knowledge' and secondly the distinction between 'lapsed history' of a science and 'ratified history' of a science.

'Common knowledge' and 'scientific knowledge'³

In reality, the 'rupture' or the Bachelardian discontinuity is not directly a historical discontinuity which is immediately inscribed in a chronology, however reasoned that chronology may be. We say that it is both *more* and *less*.

It is more because if effects of discontinuity must be capable of being marked in the course of the history of a given concept and of a given scientific problem, the Bachelardian problem is not restricted in any way to registering the difference between a *before* and an *after*. What interests Bachelard is not the simple 'fact' of discontinuity, not simply the fact of a change in language or in referential which turns thoughts of a time into the non-sense, the non-thoughts for another time, which turns discourses of a time into the literally incomprehensible non-discourse for another time. Rather it is the complex mechanism of that difference which is always inscribed *at several levels* in the system of several activities

and corresponding intellectual forms. It is inscribed not only at the level of *theoretical* formulations but also at the level of *technical* activity (well beyond just experimental laboratory technique) and at the level of (academic) pedagogic activity. It might even be said that the Bachelardian 'rupture', which remains a purely *epistemological* rupture, that is, specifically tied to *knowledge*, manifests all its effects, is effectively realised, only in the field of activities concerned with technological application and with teaching necessary to knowledge itself. Hence the remarkable idea that what characterises scientific thought is not *abstraction* as such but on the contrary the realisation of abstraction *in* the concrete, the production of 'abstract-concrete' technical objects, concrete *in that* they incorporate and make objective theoretical abstractions 'function'.⁴

We say, then, that for Bachelard it is not so much the rupture that intervenes in the already given, already defined domain, the domain of 'knowledge', as if we could *know what knowledge is* and what it is to 'know' *before* posing for ourselves the question of continuity or discontinuity. It is rather the contrary that alone has sense. Only because in this set of inseparable activities of research, applications, pedagogies, there is in fact a discontinuity, a series indeed of correlative discontinuities issuing from a chain reaction, we can identify there what we call 'knowledge'. *The very category of knowledge is the expression of this transition, of this antithetical relation, of this process of rupture.*⁵

But I also said that the Bachelardian thesis is also less than historical. To understand that it is enough to investigate briefly this category of 'common knowledge' which has an essentially *negative* import and which is a clear mark of its pregnancy of a purely *rationalist* point of view. Already the word should put us on the alert. Common knowledge is merely 'non-science' and under this representation it combines, whether one likes it or not, both the strongly valorised (that is, devalorised) sociological notion of a pseudo-knowledge of The Ignorant, The Lay, in which we remain so long as we have no right of entry to the 'Scientific City'; and the 'epistemological' notion of primary Error or Illusion. But as unjustifiable as they are absolute in their ascendancy at the bottom of the gloomy cave in which knowledge finds its alienated origin. That is why Bachelard immediately rediscovers, in connection with 'common knowledge', the classical philosophical notions by means of which philosophy from Plato to Kant and beyond has represented illusion and error: *the immediate, sensation, opinion* (as hasty generalisation), uniquely formal *abstraction*. And this, as we know, leads to the redoubling of the rationalist critique in a

theory of the original imaginary which is the (anthropological) condition of possibility of all illusion and all errors. Hence the consequence that, in the very moment that Bachelard states 'the positive role of error', error is more than ever incapable of a proper history and, equally, of a differentiated analysis which is distinct from a mere illustration or classification. It is 'common' in all senses of the term. In other words, if there is discontinuity and rupture, *there is no contradiction*. And in consequence there is no real ground on which there could be combat between or transformation of prescientific knowledges and scientific knowledges nor on which, as a result of such combat, there could be developed the concepts that are the practico-theoretical unities of scientific knowledge. Once again, the even more paradoxical result is that Bachelard inscribes his 'rupture' in a differentiated, articulated, field of practices. So what can the status of prescientific ('empirical') techniques be for Bachelard? Is their effective history, which *also* realises a certain 'hold' over nature, left to reduce to the undifferentiated of 'sensations', 'opinions' and 'immediate' imaginary representations? And what can be the status, similarly, of the *pedagogical forms* which are pre-existent to the diffusion of scientific knowledge and which scientific knowledge reinvests, by transforming them, it is true, but only up to a certain point? Here it is enough to think, for example, about a concrete case such as *medicine* to understand why Bachelard's continuators, like Canguilhem and, in another sense, Foucault could not take up the Bachelardian conceptualisation as it stood.

'Lapsed history' and 'ratified history'

We find the same difficulty in connection with the concept of *recurrent history* of scientific disciplines, hence with the distinction between 'lapsed' history and 'ratified' history. This will allow us to throw better light on the stakes of this problem. (Cf. Bachelard 1951 pp. 21–49)

To go right to the most critical point, I shall say that this distinction is crucial in that it inscribes at the very level of the history of sciences an incontrovertible and *materialist* thesis. Lecourt has clearly shown that Bachelard's epistemology itself breaks with the idealism of all 'theory of knowledge' in that for it the objectivity of scientific knowledge *is not a problem*. Objectivity is not the name of a 'critical' questioning followed by the reassurance of a fictitious 'guarantee'. Rather it is *posed* initially, as a fact, not a simple fact but one which is not to be doubted of. In other words, contrary to the whole tradition which flounders about interminably in the obvious incompatibility between the idea of an *objectivity*

of sciences (hence the idea of a truth in their results) and the idea of their *historicity* (hence the idea of the 'relativity' of their results, theories, concepts and givens of fact), Bachelard shows from the start that only *objectivity of scientific knowledge permits the rigorous thinking of its history*. To take up Canguilhem's formulations, only objectivity permits the history of sciences to be torn from the lazy dilemmas of 'chance' and 'logic' and permits the demonstration that the historicity of science is not just that of its 'external' conditions (politics and sociology of scientific institutions) but also *that of the production of its concepts*. (Cf. Canguilhem 1968 Introduction). This is because the objectivity, which first resides in the posing of problems before it resides in the answers it brings them, introduces (under experimental, mathematical and logical forms developed at each stage in the progression of knowledge) right into the very heart of scientific activity a material constraint with which the 'mind' cannot freely play and which is translated both by *the necessity of rectification* and by continuity, better, by *the compatibility* of the successive acquisitions of scientific knowledge under the condition, in a precise way, of its successive 'recastings' or syntheses.

An admirable example of this materialism is given by Bachelard in the first chapter of *Activité rationaliste de la physique contemporaine* (Bachelard 1951) in connection with the history of theories of light between Descartes and De Broglie's undulatory mechanics.⁶

But this conceptualisation, which is at the centre of Bachelard's epistemology, calls for several comments.

In the first place, it maintains a remarkable relation with the representation which scientific records 'commonly' give of their own practice. We know that this representation is expressed in particular in a privileged way in 'historical records' of theories or discoveries which pedagogically often precede the treatises of a discipline or which biographically succeed the 'active' career of a researcher in order that the contribution can be reflected upon and situated. Bachelard has a no less polemical relation with this 'history of scientists' than he has with the empiricist and eclectic history of the 'historians of the sciences' of his time, and he does it by putting weight on their scientific activity itself. Bachelard's approach is to reform the conception which the scientists' records have of the history of their science by bringing home the lessons of their practice within that history. The historical record of the scientists is indeed often none other than the museum, not to say the cemetery, of the lapsed theories which are invoked as the distant 'origins' of modern knowledges in order to exhibit in current science the completion of an immemorial project of the human Mind or the solution that has finally been found to an Enigma of

the Universe. But in fact their current practice is the intellectual and technical laboratory in which *certain* theories, more exactly, *certain concepts* are ratified and constantly reactivated to the extent that they are always working to produce new objective knowledges and are in this way themselves *reproduced qua* knowledges.

This position will be expressed by saying that the distinction between lapsed history and ratified history reflects, in a conception of the history of sciences 'adequate' to scientific practice, *the very objectivity of the distinction between truth and error* indicated by that practice. And, to anticipate a formulation later proposed by Althusser (1974), we shall say that the necessity of correcting a 'spontaneous' representation by confronting it with the objectivity of *its own* practice is the mark of a *conflict internal* to the 'spontaneous philosophy of the scientists' records' and one which is necessary to their practice. And we shall say that Bachelard is not content here to redouble scientific labour in a speculative way but that at his level he intervenes actively by taking up a position for the materialism of the scientists against one of the forms of their idealism.

But we must also see the internal weakness scoring through Bachelard's position. It should be clear already that 'lapsed' history, no more than the 'common knowledge' with which it is tendentially indented or of which it represents the instance in the course of recurrent history, does not have its own consistency, a genuine reality. On the one hand, Bachelard shows 'the importance of a dialectic peculiar to scientific thought' in which 'it is ceaselessly necessary to form and reform the dialectic of lapsed history and ratified history' (Bachelard 1951 p. 25) and in which as a result *the differences* within what recurrently appears to us as the 'prehistory' of a modern science themselves call for a historical analysis. Witness the example briefly invoked here by Bachelard of the epistemological difference between the concept of 'phlogiston' and the concept of 'heat' in the eighteenth century, an example which is even more remarkable in that the whole of positivist historiography has always amalgamated the two under the heading of 'substantivist hypotheses' belonging to 'metaphysics'. (Cf. Bachelard 1951 pp. 25–6) Nevertheless, this historical analysis of the 'lapsed', of the 'prescientific', which is necessary to the realisation of a true historical dialectic, is completely impossible for Bachelard. Here again is *the other aspect of the rationalist couple of 'truth' and 'error'*, the materialist side of which we saw a moment ago, namely that what is on the side of error is pure negative and has no real history, having at the very most an anthropological explanation. To put these things in a different way, the Bachelardian

demonstration that science has, as such, a history (in the strong sense: dialectical) opens into the thesis *that only science has a history*. But in that case Bachelard cannot prevent his conception of the 'autonomy' of scientific development rejoining the conception against which he himself is constantly engaged in struggle. He cannot prevent the 'progressivity' of scientific knowledge appearing as the expression of an internal 'logic' and its 'normativity' as that of an implicit teleology.⁷

Section 2

We are now more in a position to appreciate the meaning of the transformation worked by Althusser under the form of 'borrowing'. To put it in a few words first, Althusser's operation is double, although in his eyes one of its aspects takes precedence over the other.

What Althusser thinks he is doing above all is *applying* the Bachelardian conception of the 'rupture', rechristened 'epistemological break', *in a new field*, the field of historical materialism, the scientific discipline inaugurated by Marx and Engels and constituted historically from the starting point of a certain *transformation* of preexisting theoretical ideologies (the famous 'sources' of Marxism listed by Engels, Kautsky and Lenin). In doing this Althusser was providing himself with the means of registering and analysing in an explicit theoretical discourse an epistemological 'fact' recognised by Marxism itself but still permanently in the grip of the equivocation of philosophical formulations which in fact belong not to Marxist theory but to its ideological 'prehistory'. In the last analysis, we can indeed say that it is the element of materialism belonging to the work in the Bachelardian position (i.e. assertion of scientific objectivity) which in this way permits Althusser to intervene in the struggle of materialism and idealism which is waged within contemporary Marxist philosophy. We can even say that, from the simple fact that he proceeds to this *extension* of the import of the Bachelardian concept in a domain of objectivity which Bachelard in no way envisaged, indeed a domain which was excluded by the privilege he accorded to the mathematical disciplines, Althusser reinforces the materialist element in Bachelard. He extricates himself at one go from the idealising tendency linked with the privilege of mathematics and mathematical physics. But, the form of this initial borrowing also signifies that Althusser is not concerned to examine in detail the internal contradictions of the Bachelardian problematic. He is therefore (at that moment) incapable of knowing with precision

where in Bachelard the demarcation of materialism from idealism is played. We may expect that the Bachelardian epistemology will, without his realising it and for the price of that borrowing, bring him a 'revenge' the cost of which will be born by Marxism.

Indeed, Althusser cannot stop this idea of an application (or of a borrowing) producing the following line of reasoning: what permits the identification of an 'epistemological break' in the constitution of historical materialism is that historical materialism *in its turn* presents the already referenceable characteristics in the 'theoretical practice' of other sciences. Thanks to the 'historians of science', 'we know' that 'epistemological breaks' have *already* intervened in these other sciences. Marx opens the 'continent of History' to objective knowledge, as the Greeks opened the 'continent of Mathematics' and as Galileo and his contemporaries opened the 'continent of Physics'. In other words, Althusser is held fast in the relation of speculative 'guarantee' which he himself never ceased to describe. In order to be able to think Marx's 'epistemological break' he must anticipate its form by invoking epistemological breaks typical of mathematics, physics and chemistry, following a famous comparison which Engels worked between Lavoisier and Marx. *'Bachelard' is the uncriticised guarantor of that anticipation.* The 'scientificity' of Marxism, contested by its opponents and abandoned to their objections by a number of its proponents, is then 'demonstrated' with these examples as starting points. At least, that is the justification. From this very fact, the examples become models. It is then no longer possible to escape the hypothesis, in one form or another, of an essence of SCIENCE ITSELF, the object of a general Theory which cannot be absolutely distinguished from a theory of knowledge or of a Science of the sciences, even though Althusser directs his entire explicit polemic against such an idea and in particular against the precise form it had taken in the 'Stalinist' Third International, namely the idea of Dialectical Materialism as general philosophy of nature.

To clarify this point, a quick comparison can be made with other currents in contemporary epistemology. Accordingly, it is clear — and recognised in practice by the party in question himself — that the concept of 'criterion of falsifiability' in Popper's works is an *ad hoc* concept intended to *exclude* Marxism and psychoanalysis from the 'domain' of science, not merely from current science but from all possible science. The question can be asked if, in a symmetrical way, the concept of 'epistemological break' in Althusser's works is not the *ad hoc* concept intended in advance to *include* Marxism and psychoanalysis in the field of science. That the two objectives are contrary, and that in one case the criterion is logical

while in the other it is historical, changes nothing in this fundamentally analogous approach. Or rather, the idea implicit in Althusser would become that as soon as one passes from an (external) 'logical' criterion to an (internal) 'historical' one, *the final result is contrary*. Anyhow, the consequence is *the definition of (Marxist) philosophy as Theory (of the history) of theoretical practices*. Althusser today denounces the major expression in this position of his initial 'theoreticism'.

But Althusser's enterprise can also be read differently, so that it is made to appear in a very different light. It appears not as the borrowing or the application of a *fully developed* notion but as the constitution of *a new notion with incomplete and contradictory material as its starting point*. Let's leave on one side the question of the 'proof' in support of the authentically scientific (or not) character of Marxist theory. Or rather, since Althusser never ceased to be convinced of it and to affirm it, let's start from *the existence* of a scientific Marxist theory with its own concepts. That is in no way the same as the idea of their completion or perfection. The objective, then, will no longer be to think the Marxist distinction between the ideological and the scientific under the epistemological categories of truth (objectivity) and error (the prescientific). On the contrary, it will be to tear the category of 'rupture' from the internal contradiction which affects it in a 'historical epistemology' which is itself unsuccessful in breaking definitively with the project of a philosophy of Science. It will be *materially to implant the history of sciences in the field of plain history*, something which can be done only by means of the concepts of a scientific theory of that history even in its beginnings. In the first place there is the concept of ideology. Error, Illusion, the Imaginary, and so on, and ideology in the Marxist sense are worlds apart. To be precise, there is an epistemological break: ideology is in no way *the Marxist name* for these philosophical categories. So, we must not say, through a false symmetry, that it is now a question of thinking the epistemological couple: Truth/Error *under* the Marxist categories of Science and Ideology. Rather we must say that for a historical theory of sciences definitively to escape the idealism of the philosophies of Being (or Truth) and of Nothingness (or Error), it is a question of radically eliminating this last term and *transporting the whole problem of the constitution of sciences into the field of the materialist (historical) theory of ideologies*.⁹

Althusser's own demands must therefore be applied to himself. Not only can we not be content to take him at his word in his declarations of borrowing and his acknowledgements of his debts, but we must also apply to him the very principle which, from his first article onwards (Althusser 1970 'On the Young Marx'), he

claimed to have applied to Marx. We are not allowed to judge an *isolated* concept; by itself it has no sense. On the contrary, we are to examine a system of interdependent concepts. That does not mean that we seek to confer on this system a 'coherence' which it quite certainly does not have. On the contrary, it means seeking the fault in the working of this system in relation to its own objectives.

I shall formulate some comments in connection with two points. First, what type of historical 'event', according to Althusser, is an epistemological break? Secondly, what problems are posed by the conjunction, in the analysis of the break, of the concepts of *ideology* and *practice*? These comments will lead me to indicate in my Conclusion some of the reasons why, in my view, Althusser is justified in maintaining in the face of all opposition, that is, against people who would like to see him make a self-criticism other than the one he proposes, that 'the essential question is that of Marxist *philosophy*.'¹⁰

The break as historical event

In the very introduction of the term Althusser wanted to characterise the break as an event, to the point, in particular, of giving it a date with, in the case of Marx, an extreme, perhaps excessive, precision. It is true that at the same time Althusser always notes that the possibility of fixing a date has at the very most an indicative value. The date is merely the index of the event. In particular, Althusser always forbids himself the pure and simple identification of the theoretical event which is the break (the emergence of concepts without precedents from a new scientific problematic, the concepts themselves having a material historical existence) and a biographical fact. For example, he forbids the identification of the emergence of the concepts of historical materialism with a fact of the intellectual biography of Marx or a group of young revolutionary intellectuals. Rather it is what, in particular in connection with Marx, Althusser emphasised as the 'contingency of the beginnings' of a science and as the historical necessity of that contingency. The formation of revolutionary concepts is produced in the element and in the course of an individual and collective theoretical revolution, even though it is not in the last analysis its causes. In consequence it cannot be explained simply with the succession of the stages of that evolution as its starting point. As event produced by a conjunction of historical causes, theoretical and non-theoretical, the epistemological break does indeed signify the 'encounter' of preexisting tendencies which by transforming

itself in a reciprocal way will produce a new result. It also signifies that this encounter and transformation must be *worked* thanks to the action, the 'labour' of the men who provide the supports, that is, the labour of such an individual or such individuals. But it does not signify that an individual evolution or labour can either immediately *identify* its historical process from which results the event of the break or do so at the decisive (ultimate) moment of that process. Between the two, a *difference* always subsists which is not reducible, even 'in theory'.¹¹

This first comment, however, leads us to one which has a more general import, for it counters that it is impossible to discuss the concept of break without taking account — in the example of Althusser himself — of the definition of *history*.

There is in Althusser another remarkable occurrence of the metaphor of *essential section* [*coupe*] or of *break* [*coupure*]. (Do people know about this?) We are dealing with the place in *Reading Capital* where Althusser proposes an 'outline of a concept of historical time' (Althusser 1975, subtitle to Ch. 4, p. 91) by means of a critique of the current empiricist conception of time, of which the opposition: synchronic/diachronic is a simple variant. (Cf. Althusser 1975 pp. 95–6, 107–8) Now this empiricist conception is expressed in complete form in the possibility of what Althusser calls the 'essential section': the possibility of 'reading' in the immediate of a *present* (or of an instant) the whole system of determinations of a historical phenomenon by means of the social totality, taking account of the 'place' which it occupies itself.

It is true that it might be thought that what Althusser wants to exclude is only the idea of an 'essential section' of the Hegelian type which would allow one to fasten on the principle of unity of *all* aspects of social life — the economic, arts, sciences, politics, morality and so on — in just one simple 'figure' of historical development. That would not prohibit — quite the contrary — the characterisation, for each of these different levels, of the events of which they are the site, and specially those events in the strong sense that are *epoch-making* in the history of sciences, or the history of arts, or of politics, and so on, (the revolutions) by means of the intermediary of such an essential section. It is remarkable that this interpretation, which ends up simply replacing the banal image of a unique time in history with the scarcely less banal image of a superposition of *times out of phase*, is rejected in a precise way by Althusser. It is because the idea of autonomous histories in this sense is absurd. Sciences, ideologies or politics do not each have *their* linear 'history', the after-the-event effected sum of which — as complex, uneven and out of phase as you could wish — constitutes history *tout court*. If the sciences, production, the arts, and so on,

have a history, that is, a process of relatively autonomous transformation, it is precisely *because of* their reciprocal determination – reciprocal determination even though at the same time Althusser was trying to define historical contradictions as ‘overdetermination’. So, and for exactly the same reasons, the ‘essential section’ is just as impossible at the level of the ‘part’ as it is at the level of ‘the whole’. If the concept of epistemological break has a sense as the concept of a decisive event in the history of theoretical formulations, it is on condition that the ‘break’ resembles neither closely nor remotely the idea of an ‘essential section’, that is, the idea of an absolute present (or of an absolute instant) in which history is in play.

But this has considerable consequences. It means that the immediacy of the fact (or event) under which Althusser represents the epistemological break in no case has anything to do with the empirical or speculative immediacy of time. Since it is not itself a determination of time, *it has nothing of substance, either, to do with the dilemma of continuity or discontinuity*. In this way, if it can be pedagogically useful to show that a representation of the history of sciences in terms of ‘epistemological break’ itself breaks with existing ideologies of the continuity of the progress of knowledge, that critique must in no case inveigle us into concluding that the dialectic of the history is essentially characterised by its own *discontinuity*. The one is as ideological as the other, it is true to say. And we now have all the proofs we could wish for of the fact that, under one or the other form, what is here given free rein is the good old idea of a *subject* of history, whether it is psychological, sociological or speculative.

In truth, the practical stake of this conception appears as soon as attention is given to the consequences of a formula which ceaselessly and insistently comes back to Althusser, the formula which designated *the break* as ‘the beginning of a process which has no end’. What changes progressively in Althusser is the characterisation of that process (in connection with Marxism). First it was tendentially ‘reduced’ to the process of constitution and development of a scientific theory (Althusser 1970, 1975). Today it is tendentially identified as the process of ‘fusion’ of that theory and the revolutionary workers’ movement (Althusser 1976a). This is not a slight difference. But the formula itself, and the idea it expresses, do not change. They do not cease designating the fact that the break must be thought, however strange it seems, not simply as an event but as a process. And it must be thought not only as a process but as a tendential process which, as we shall see, is internally contradictory. This is why, contrary to what certain critics of Althusser have thought, he finds no difficulty in admitting

that the epistemological break is a 'continuing break' and not a result definitively completed in the instant.^{1 2}

We say that the epistemological break as thought by Althusser is characterised both by its *irreversibility* and by its *incompleteness*, the one being as important as the other, the two being thought together, in a contradictory way. What needs clarification here, by going back to an irreversible 'knowledge effect' produced in Marx's theoretical labour, by examining its contradictory link with the practical 'sanction' which the revolutionary workers' movement brought to that labour, is exactly that incompleteness of Marx's 'break', and as a result *the transformation of the conditions of its relative completion* in today's conditions. The irreversibility of Marx's break does not permit of any 'guarantee'. It is wholly suspended from the pursuit of the critical labour which produced it, labour which *without it* could not even have been envisaged. In short, when, in the 1960s, Althusser was repeating in connection with Marx the 'habemus enim ideam veram' of Spinoza, it did not look much like a way of saying to Marxists, 'Rest assured that, in spite of your errors or set-backs, the future belongs to you because you have a true idea! You've got truth in the bag!'

But Althusser's critical definition does permit of another theoretical consequence. Contrary to every conception of a rupture, mutation or 'revolution' which is uniquely regulated on the image of discontinuity, the definition at no moment implies the necessity of representing *the terms in which the break is effected* as 'invariants'. Intervening in the theoretical controversy over the 'young Marx' Althusser showed that in order to recognise the break it is necessary to identify the different, incompatible, theoretical *problematics* to which belong, on the one hand, the notions and above all the questions typical of ideological prehistory and, on the other hand, the concepts and problems typical of the scientific theory of social formations. In addition it is necessary to study the relation of these theoretical problematics to what Althusser at this time calls the given *ideological field*, that is, the system of organically linked ideological formations at such a time in the history of a social formation, for it is this *relation* that permits the understanding of the *contradictions* of a theoretical practice. But at no moment do Althusser's definitions imply that either the theoretical problematics or the ideological field itself should be considered as 'invariants'. It is indeed exactly the contrary. The one and the other are given as effects of a certain process to be rediscovered.

It is here, no doubt, that the comparison with other contemporary tendencies that have sometimes been likened to Althusser's work in the name of the 'discontinuity' of knowledge becomes quite revealing. In Kuhn and Foucault alike the theme of the dis-

continuity of knowledge is indeed essentially tied to that of invariance.

It is manifest in Kuhn, since his whole endeavour is to show that, on condition that the field of scientific disciplines is well 'marked out' [*découper*], it is always possible to describe the history of sciences as alternation, the correlation of revolutionary crises and an activity of 'puzzle solving' in the essentially invariant framework ('the preformed box') which a certain paradigm determines. This invariance is itself both the cause of the revolutionary crisis through the accumulation of phenomena perceived as 'a-normal' and the end towards which the crisis moves, according to a very simple adaptive model.

It is also manifest, and in a much more interesting way, in Foucault, at least in that part of his work which begins with *Madness and Civilisation* and which provisionally culminates in *The Order of Things*. (Foucault 1967, 1970) Foucault and Althusser both make use of a quasi-identical formulation to characterise, in the one case the relation of a theoretical thought to its problematic and in the other case its relations to the *episteme* characteristic of an epoch and which transversally governs different disciplines. They both speak of the necessity of investigating not only given individual thoughts or discourse but also the system which takes account of *the possibility of those thoughts or discourses* and which as a result assigns them impassable interior limits. But for Foucault the relation between discourses and episteme is explained by a *variational mechanism* according to a double play of criteria: variation in different disciplines and in their particular 'objects' (so: work, life, language), and variation in antithetical positions which, within each discipline, 'bifurcate' with certain typical 'heretical points' of the episteme in force as their starting points. Now, to speak of a variational mechanism is also necessarily to speak of an invariant preexisting or immanent to those variations. That is why in the last analysis we are obliged in this respect to set Foucault's endeavour among the 'variants' of culturalism, regardless of the heterodoxy of that. (Cf. Canguilhem 1967) Canguilhem compares Foucault with the American theorists of 'basic personality' except that, and this is not insignificant, Foucault's invariant is non-normative. In another way, we might ask if today since the problem of the hospitals has been joined with the problem of the prisons in Foucault's enquiry, with a view to completing the sense of the word 'discipline' by its *power* dimension, the invariant has not become in fact explicitly normative, productive of norms.¹³

But in Althusser the relation of a theoretical problematic to its particular effects or realisations is not thought in variational terms, nor, therefore, can he have anything invariant as ultimate anchorage

point. Even if we say that it is there a question of an excessive metaphorical extension, we must agree that the idea of (theoretical) *relations of production* to which Althusser refers here has nothing to do with the couple: variant/invariant. So I believe that it is possible, at the very least, to credit Althusser with the originality — in relation to his contemporaries — of having attempted the elaboration of a *theory of discontinuity without invariance*, a theory of discontinuity thought not as the counterpart of an invariance but as the counterpart of a tendential transformation, *as a relation between terms which are processes of a different nature*, instead of being *different invariants but of the same nature*. It will be granted me perhaps that from this point of view Althusser's formulations on the opposition of science and ideology merit examination. Equally it will perhaps be granted me that in these conditions Althusser's belonging to a 'structuralist' current, of which the couples: synchrony/diachrony and variation/invariance are constitutive, becomes completely dubious, not to say absurd.

I would add this. It was even more improbable that the Althusserian theory of the epistemological break should culminate in a structuralism of invariance considering that it was entirely elaborated *against a particular case of that conception* — not the variational model but the simpler model of the 'reversal' of the dialectic, out of which, by exploiting a few of Marx's and Engel's phrases without posing the question of what those phrases owed to Feuerbach, official and even non-official Marxism made the very theory of the constitution of historical materialism.

It is enough to have grasped this point to understand how much Althusser's conception is diametrically opposite to any idea of 'normal science'. The stake is not small: it is in fact political. In the case of historical materialism, the existence of a 'normal science' or a 'normal course of science' can be presented as a received fact only with difficulty. It can be thought in its turn only as a norm to be realised, to be imposed, indeed. In other words, it becomes the watchword of a normalisation of science.

The antithesis of science and ideology

The fundamental categories Althusser uses to think such a contradictory process are the categories of *ideology* and *practice*. They concentrate, it seems to me, both what makes his position in relation to epistemology new and the difficulties of that position. They permit an understanding of why the definition of philosophy and its role in relation to scientific knowledge is the strategic point of the discussion.

The category of ideology is evidently central since it permits an

immediate understanding of the necessary unity between the two sides of Althusser's enterprise: the 'general' side of a theory of theoretical practice and the 'specific' side of historical materialism. Ideology is a concept of historical materialism which only it has been able to discover and which it alone can develop. Marxism, according to Althusser, is a revolutionary scientific theory not only as an objective analysis of social production but also as objective analysis of ideology and ideologies. The two things are inseparable. It is therefore in historical materialism that our knowledge of ideology must be sought and if necessary developed in order to apply it to the problem of epistemology and in that way to transform that problem completely. And this transformation is necessarily radical, for an obvious reason. As Althusser has never ceased to stress, *ideology is a social instance*, which is totally irreducible to the epistemological dimension of an error, an illusion or a misrecognition. In given historical conditions, ideology produces 'misrecognition' effects but it cannot be defined as misrecognition, that is, *through its (negative) relation to knowledge*. And this holds not only for what Althusser first designates the 'ideological field' of an epoch and for what he will later designate the 'practical ideologies' linked to a given material base. It holds equally for the 'theoretical ideologies' within which and starting from which scientific knowledge is developed by means of a process of historical break.

So, when Althusser defines the discovery, by Marx, of the initial concepts of historical materialism (mode of production, class struggle, ideology, and so on) as an 'epistemological break', it is indeed a question of marking out the incompatibility between these concepts and the problematic of juridical ideology, economic and moral ideology, to the last of which the 'theoretical humanism' of alienation belongs in its entirety. But it is also immediately a question of a complete transformation of the notion of epistemology. On the basis of that notion, if the term is to be retained, Althusser in effect places a relation of antagonism between two terms, *one* of which — science — is defined in the field of knowledge whilst the *other* — ideology — is defined outside that field and without initial relation to it, as a system of social relations, more precisely, as a system of the 'lived' or 'imaginary' relations in which individuals live the economic, political, etc., relations on which they depend. (Cf. Althusser 1970 pp. 233–4 and Althusser 1976b p. 101f.) *The relation between science and ideology is therefore in all respects an unequal and heterogenous relation in which the two terms cannot be spontaneously associated or 'work' directly on each other without the intervention of a third term, practice.*

We are truly on the most delicate point here. We know that the definition of science as 'theoretical practice' is one of the definitions that has attracted the most objections. Althusser's position has been denounced as reducing practice to theory, worse, to 'pure theory'. This is an evident paradox because it is quite explicit that the thesis he maintains is exactly the contrary: theory is itself a practice, one form of practice among others, necessarily social; necessarily linked, then, to all other social practices. But we cannot avoid an investigation of the reasons for this ambiguity in Althusser himself.

Without going into detail, I want here to maintain a hypothesis which runs exactly contrary to the one generally accepted. I do not at all think that Althusser has ever given in to any temptation whatsoever to pose the primacy of theory over practice, and as a result he is from the start diametrically opposed to all scientism and all positivism. To be convinced of this it is enough to remember two things. First, in spite of having had, as he put it after Rousseau, 'the weakness of believing in the power of consequences' (Althusser 1976a p. 191 and cf. Althusser 1976b p. 154), Althusser arrived at the definition of philosophy as 'Theory of theoretical practices' which was ineluctably evocative of a Science of sciences. But, secondly, in that very moment he put all the weight of his argumentation on the thesis that the materialist dialectic, in its specificity, 'absent' or only sketched in the theoretical formulations of Marxism, had to be researched *in the revolutionary practice* of the workers' movement (Lenin, Mao) where it appeared in the flesh, 'in the practical state'. He was on the point of *drawing* from this analysis of the practical dialectic of historical revolutions the keys to his philosophical 'reading' of *Capital*, intended to bring *Capital's* theoretically revolutionary meaning to the surface. In other words, Althusser never ceased to state and put to work the idea of the primacy of practice over theory. Practice precedes theory and permanently goes beyond it.

His thesis amply confirms that *the category of practice is the fundamental category* of the materialist dialectic and that it is necessary to develop it with a view to including in it, with every justification, the process of knowledge. The 'knowledge effects' Althusser speaks of are practical effects. It has not been sufficiently noted, it seems to me, that if, after Marx and contrary to empiricism, Althusser states that 'the process of knowledge unfolds entirely within knowledge', that is, that the object of knowledge can never be *confused* with the real object, he never states, on the contrary, that the 'knowledge effects' are purely theoretical effects.¹⁴

In reality, what produces the difficulty in Althusser's position, including its *political* difficulty, is not the supposition of a pure

theory but *the implicit admission of the idea of a 'pure practice'*. It is in this way that Althusser's initial position can be described as 'unilateral', not dialectical. In what sense? There is no pure theory, because theory is never definitively installed outside the field of ideology. (Cf. Althusser 1970 p. 167f. and Althusser 1974 p. 98) Althusser explicitly rejects this thesis, in particular by maintaining against many Marxists of all opinions the idea that *there can be no 'end of ideology' in general in history*.¹⁵ The contrary would have been bewildering because, as we know, Althusser's number one problem is that of the ideological struggle being waged *within* Marxism more than a hundred years after its constitution. In contrast, it can be said that Althusser in fact postulates a 'pure practice', that is, a practice which is *pure activity of material transformation* and which is applied, according to conditions, to the production of means of subsistence (transformation of nature by means of a given means of production), to the production of knowledges (transformation of a primary ideological material by means of theoretical means of production, producing a specific form of 'appropriation' of reality), and indeed to the revolutionary transformation of existing social relations. I think that this is the main point. A contradiction is manifested in these first texts. In spite of his non-positivist, non-rationalist definition of ideology, Althusser in fact presented practice and ideology as two antithetical terms, radically exterior to each other. *He displaced on to the couple of practice and ideology the abstract (rationalist) opposition of truth and error*, in such a manner that if practice, *qua* theoretical practice and *qua* revolutionary practice, *transforms ideology*, it is impossible to say if it is genuinely affected by it in turn or 'transformed' by it.

What has steadfastly prevented the most 'critical' of Althusser's readers, with few exceptions, from recognising in this point the root of a deviation which he himself calls 'theoreticist'? (Althusser 1976a p. 142) No doubt the disarming tautology that 'theoreticism' should be a matter for theory . . . But above all it is the fact that that contradiction has been masked, in Althusser's text, by a constant *denegation*. On the one hand, he has never ceased to write, in black and white, that from the point of view of Marxism there is no 'pure production', that any process of material transformation of nature is effected *under social relations* which determine not only the exterior form but also the technical organisation and internal tendencies, and which for all known history are relations of class struggle. On the other hand, extending the category of practice to the analysis of theoretical practice, Althusser takes for his model (which he immediately 'generalises') precisely the description Marx gives (in *Capital*) of the 'labour process', an

abstract and provisional description prior to all analysis of determinate social relations and which itself has reference only to the ideological notion of the (practical) opposition of Man and Nature. On this point I come back to 'On the Materialist Dialectic' where Althusser develops the concept of 'theoretical practice' according to the schema of the 'Three Generalities' (Althusser 1970 p. 183f.) and to 'Sur le rapport de Marx à Hegel' which was published as an Appendix to *Lénine et la philosophie*. (Althusser 1972) I think we see there both the marks of that denegation and the seriousness of its stake, namely to think the emergence of the revolutionary theory of Marx as a real transformation and not as a simple intra-ideological 'reversal'.¹⁶

But there is more. Far from breaking with a certain Marxist tradition, this tendency of Althusser on the contrary derives from it directly. (This explains the embarrassment of critics who try to oppose them purely and simply.) It derives directly from the opposition posed by Marx from *The German Ideology* and *Theses on Feuerbach* up to the celebrated formulas of the Preface to *A Contribution to the Critique of Political Economy*, and assuredly beyond, between ideology as 'form of consciousness', world of ideas, true or false but always unreal representation, and practice as 'real' transformation, with the tendential indetermination, step by step, of the categories of practice, labour, production and history.

But Althusser is here in contradiction with himself, with his own definition of ideology. For that definition implies that all (social) practice develops within ideology, under ideological relations which determine it, better, which overdetermine it, since all practice is also, already, determined in the last instance by relations of production – including scientific practice, of course. It is a definition of ideology which presents the interesting property of being, subject to certain corrections, the only possible Marxist definition and being nonetheless absent in Marx, or rather figuring in him only in 'the practical state', in the implacable critique to which Marx subjects bourgeois economic, juridical and philosophical ideology.

Starting from there we can understand, I think, why in the end Althusser had to recast his definition of the 'epistemological break'. Not that that correction came of exclusively theoretical motives. On the contrary, it had an essentially political determination. But the system of initial concepts, unstable and contradictory within itself, in a precise way had to reveal its contradiction under the test of a certain political practice. For as much as we envisage it at the theoretical level, it seems that in fact the profound crisis of Marxism opened up today brings us back not only to the deviations of such or such of its contemporary variants but also to the

internal limits of Marx's thought. By the systematic exploitation of the possibilities of interpretation opened by such or such of Marx's non-unified formulations, these variants can do no other than confront Marxist theory with its own limits. Or rather, they *produce* those limits, which were given only virtually. In particular they confront us without possible retreat from now on with the difficulty of a 'theory of ideology' which Marxism both permanently demands and fails to develop really. How are we to articulate in a concrete analysis *the system of (unequal) double determination on which all social practice depends?* By double determination we mean the determination by contradictory tendencies of *relations of production* (since all society is in the last analysis reproduction of the conditions of a certain mode of production), and determination by the specific contradiction of *ideological relations* (since individuals and groups are made the 'bearers' of the relations of production and of class struggle only in ideological forms, according to determinate ideological behaviours). Coming after other aspects of the political conjuncture of that time (in particular the crises of the 'socialist camp'), the 'events of May-June 1968' in France and their profound contradiction – the typical 'dislocation' between the acuteness of the social crisis and the forms of ideological revolt in which it was lived – could only emphasise once again the difficulty.

Conclusion

For a provisional conclusion to these comments, I want to return to the question of philosophy. Why does Althusser state today that, while formulating a totally erroneous definition of philosophy, he was right to maintain that the essential question was indeed that of Marxist philosophy? Or even, why, seeing that in a certain state of Marxism he was posing the correct thesis that everything relates theoretically to philosophy, did he inevitably have to end up with an erroneous definition of philosophy, *the furthest* perhaps from its true status?

To say that everything depends theoretically on philosophy is to see the two *distinct* disciplines of historical materialism and Marxist philosophy (materialist dialectic) as one quite solidary whole. It is to refuse to reduce the one to the other in one or the other way. (Cf. Balibar 1977) It is to state not that the correlative 'play' of these two disciplines can stand instead of (revolutionary) practice but that practice requires theoretical development (since theoretical non-development blocks practice and causes it to regress). It is to state not that a 'purely philosophical' elaboration can stand instead

of the production of scientific knowledges (hence the production of concrete analyses) with a bearing on our current history but that the production of those knowledges has itself a philosophical elaboration as internal condition. In actual fact, to the precise extent that philosophy is *distinguished* from a (pure) science, it alone can represent *within the science* and within its 'theoretical practices' the instance of the *non-theoretical* social practices and of class struggle which informs them. In this sense, the internal combination, within a same theoretical practice, within philosophy and within science – philosophical labour and scientific labour – is alone able to give rise to *the class theoretical point of view* (in one bloc) which is the unique 'secret' of historical materialism. That is exactly what Marx's 'break' shows us, under an unfinished but decisive first form. It is the result of a practice conducted *under determinate ideological relations*, relations which are extraordinarily contradictory in that they see the first historical forms of proletarian ideology being stated *against* the dominant ideology (in particular bourgeois juridical ideology) *within* bourgeois ideology, by distorting, combining and reversing its elements in order to turn them against themselves. It can be effected therefore only under the condition of a philosophical revolution which pushes the contradiction to extremes and extracts from it a theoretical matrix irreducible to any form whatever of consecrating the existing state of things.

Today these theses seem to us simple to expound. They were not simple to produce, by virtue of the inextricable theoretico-political combination in which the Marxists of our time have had to work and still have to work. If I may be allowed, here once again, to rely on some verbal encounters which cannot be chance, I shall recall that on the threshold of his first texts Althusser had to take a position against a quite different conception of the 'break', the one resumed by the too well known watchword of 'bourgeois science or proletarian science'.¹⁷ It might seem, therefore, that his objective had been *to substitute one break for another* by means of a mechanical reversal: to substitute an 'epistemological break' for a 'class break'. Hence the substitution, for a theory of class struggle, of a theory . . . of theory! Hence, finally, by a scarcely surprising swinging back of the pendulum, the necessity of '*reintroducing*' the class struggle into a problematic from which he had been initially straining to exclude it! The result is a zero, a worthless circuit.

It was possible for things to appear that way, yes, but only on the condition of remaining indifferent to certain problems at least. To exemplify that fact, consider that the 'leftist' antithesis between 'bourgeois science' and 'proletarian science' (and its less aggressive

assorted variants) was never anything but the formal reversal — and hence the substantive conservation — of an opposition or of a 'rupture' typical of bourgeois ideology and philosophy, the opposition of 'natural sciences' and 'moral sciences', or of nature and history, or of objectivity and practice, or of things and human persons, or of necessity and freedom, etc. Again, consider that today more than ever it is necessary to revolutionary practice to affirm the necessity and justifications of an authentically scientific Marxist research against the permanent temptation of deducing *a posteriori* the 'theoretical premises' of a politics of circumstances, that is, in fact deducing its pragmatic justifications.¹⁸ The result is that, formerly just as today, the problem has never been simply *the affirmation or the denial of 'class struggle'* and its primacy. It has always been to know *how* class struggle determines theory, a 'particular case' which too many cheap philosophies put to us as being 'evident' when in all probability it is the most difficult of all. Whatever the already real difficulty of analysing the determination by class relations in the theoretical practice of mathematics or physics, it is much more difficult to analyse it in the theoretical practice of historical materialism in which the 'political' is *omni-present*.

In this respect you will note the 'reversal' which finally affects the two hypotheses posed from the start by Althusser. The *first hypothesis* is that the 'break' of which Marxism is the continuing effect is in the last analysis *a philosophical revolution*, a 'theoretical revolution in philosophy'. Even though Althusser was led — by Marx's writings — to characterise the break as a simple foundation for science, he did not cease to state that the break represented *the unity of the two aspects* (a new science, a new philosophy) and that, in that unity, the principal aspect from the point of view of the rupture effected with the former ideology was the philosophical aspect, the aspect which was to *think* the rupture as such. Indeed, and this is the *second hypothesis*, the proper 'object' of philosophy was the distinction, the process of demarcation of science and ideology, of the scientific and the ideological within the same practice. Althusser has not ceased to maintain this. If Marxism requires a proper philosophy, a true philosophical revolution (of which the official Marxist philosophies are merely the sad caricature), if there cannot be a science *'tout court'* (science of an already given object, history), it is because it is constituted only in and through the struggle against the dominant ideology and because that ideology does not reduce to the negation of its own 'truths' (under the form of error and illusion). It is because the dominant ideology — with its internal contradictions — has a real historical necessity and does not disappear purely and simply in the face of

scientific knowledge, not even in the face of the socialist revolution.

When Althusser thought it was possible to recapture from Marx's formulation an *immediate* opposition of ideology and practice, that is, of the *reproduction* of the state of existing things and its transformation, he defined that philosophical revolution as being itself 'scientific' in another more general sense of the term. At the extreme of this position, he had to pose that 'every science, in the relationship it has with the ideology it emerged from, can only be thought as a "science of the ideology"'. (Althusser/Balibar 1975 Part I, 12, p. 46) In this way, then, there was formulated the impossibility of scientific positivism the thesis of which was instead that all science is *ignorance* of the ideology from which it emerged! By our formulation we must understand that 'every science' is first, a *critique* of ideology, secondly, a *recognition* of the (historical) necessity of ideology, and thirdly, theoretical *knowledge* of its mechanism, the production of misrecognitions and illusions. And as a result 'every science' brings with it a philosophical revolution which thinks the modality under which it is effected, for a given scientific continent, that triple operation of critique-recognition-knowledge. But that thesis, it is true, formally abolishes all necessity of recourse to an instance of external guarantee (to a 'truth criterion') for purposes of explaining the 'change of ground' operated by scientific knowledge in its own practice. That thesis also has as a consequence the fact that all the epistemological breaks are valid and are copies of the same model. We have seen that this is a guarantee all the same, at a second degree.

When Althusser drew the consequences of the fact that the 'breaks' are, on the contrary, specific and irreducible to a uniform process of theoretical practice,²⁰ he was able to pose clearly that historical materialism is 'science' in an original sense of the term which was *itself revolutionary*. He had reflected in a systematic way on the singularity of the Marxist science of social formations. It was not a rupture with Ideology *in general* but a rupture with *a* determinate ideology — dominant bourgeois ideology — under its different forms. And yet it was the only theory that could effectively go beyond a simple critique of ideologies in order to recognise their historical necessity and broach the analysis of the general mechanism of ideology. At that point his two initial hypotheses had not to disappear but to change profoundly with respect to content. Instead of being the necessary completion of the epistemological break, its interior sanction, the philosophical revolution had to become, *in the* (singular) *case of Marx*, its initial condition. So the pursuit of this philosophical revolution (beyond Marx's formulations, 'Hegelian' or 'anti-Hegelian') had to appear as the continuing condition of the development of Marxist scientific

theory (meaning its recommencement and its recasting), for as much as that development *always depends* on the vicissitudes of the 'fusion' of scientific theory and revolutionary practice. So Marxist philosophy itself ceased to be able to be conceived as a 'new practice of philosophy' (Althusser 1976a) which, in the given historical conditions, engages in 'class struggle in theory' (and also against certain received conceptions of class struggle), in order to loosen and dismantle the domination of the dominant ideology in its concentrated form. Now it is rather *philosophy* — as social practice and instance, the 'site' of a certain form of class struggle — that *becomes an object* in its turn in the field of historical knowledge, without any privilege with regard to foundation or final completion. It is necessary to *know* the nature of philosophy, that particular historical form of ideology, in order to transform its practice and be able to put it to work.

Finally, to come back from a word, philosophy, to the point of departure for this exposition, the epistemological break, no doubt such a thesis *can* — though it remains to be proved in the facts — assist the displacement of the question of the relation between other sciences and the ideological conditions of their own formation, then of their development. This is because the thesis suggests that it also represents in a very general way a process such as contradictory practice, 'rupture' with determinate ('prescientific') ideological formations but rupture effected in the framework of new (successive 'scientific ideologies') ideological formations whose role of obstacle or motor in the production of concepts, in the articulation of research, in technological application and in scientific pedagogy appears, then, as the fundamental problem of the history of sciences. Indeed, taking account of the *system of differences* formerly designated, in a programmatic way, by the category of 'epistemological break', it surely seems that such an enquiry cannot develop independently of the advance of historical materialism itself.

Translated by Elizabeth Kingdom

Notes

* This is a communication to the *Secundo Coloquio Nacional de Filosofia*, 3–7 October 1977, Monterrey, Nueva Leon, Mexico. A short appendix on Thomas Kuhn has been omitted in this translation.

1. The term 'epistemological rupture' is to my knowledge systematically defined by Bachelard only in one truly fundamental passage in Bachelard 1970 p. 104f.

2. One aspect of Bachelardian epistemology which has had important consequences will be held to be central. It is the thesis according to which *the concept* is the *unit par excellence* typical of scientific knowledge because it is

also the singular unity of (mathematical) theory and experimental technology: '... to incorporate the conditions of application of a concept in the very meaning of the concept' (Bachelard 1938 p. 31) not in an arbitrary *isolation* of concepts but in their interrelation, their 'interdefinition'. (Cf. Bachelard 1949 pp. 51f, 144f) By this means Bachelard tears epistemology from the undefined commentary of ('inductive') relations between 'theory' and 'the facts' in order to propose for it the study of historical relations between constitution (and then transformation) of concepts and posing of problems.

3. Chapter VI of Bachelard 1949 p. 102f is specially devoted to this theme. Similarly, cf. Bachelard 1953 Conclusion p. 207.

4. 'The contemporary epoch consummates in a precise way the rupture between common knowledge and scientific knowledge, between common experience and scientific technique... the technique which constructed the electric light bulb truly broke with all lighting techniques in use throughout the world until the nineteenth century. In all former techniques it was necessary to *burn* a material. With the Edison lamp the technical skill is to *stop* a material burning. The former technique is a technique of combustion. The new one is a technique of non-combustion. ... in electrical science there is the institution of a 'non-natural' technique, one which does not take its lessons from an empirical examination of nature. ... We can indeed, therefore, state that the light bulb is an object of scientific thought. Under this heading, it is for us a very simple but very clear example of an *abstract-concrete* object. To understand its functioning we have to make a detour which will take us into a study of the *relations* of phenomena, that is, into a rational, algebraically expressed science.' (Bachelard 1949 pp. 102, 105–6)

5. It is in this strong sense, far removed, we see, from a scientific position, that we must take Canguilhem's trenchant formulation: 'There is no vulgar knowledge... a knowledge which is not scientific is not a knowledge. I shall maintain that "true knowledge" is a pleonasm: so also "scientific knowledge"; so also "science and truth"; and that all those are the same thing.' (Canguilhem 1964–5) And it seems to me that it is in a closely allied sense that Althusser for his part has not ceased to maintain that the 'criteria of the practice' for knowledge is *internal to the theoretical practice*, on condition that we remember that by definition a science is not a closed circle of ideas but a practice opening on to other practices and on to its own development.

6. Bachelard shows us that in this history there is a rupture which, in a recurrent way, that is, starting from the successive stages in the constitution of the current science, stages correspondingly characterised by revolutionary 'syntheses', can be assigned with certainty. This rupture separates a *lapsed* history of theories of light culminating in Descartes from a *ratified* science which begins with Huyghens' construction of the concept of wave-surface so as to solve the problem of double refraction. It is a rupture that is even more remarkable for the fact that, contrary to a non-critical legend, it does not intervene *between* a (geometric) Cartesian conception and a (dynamic) Newtonian conception, but within the Cartesian conception itself, the contradiction of which it accordingly manifests. Bachelard tells us that there is no escaping the necessity of this choice: from the point of view of what current science (that is, not just the science of today, a simple chronological reference, but effective science, drawn from the source of its practice) teaches us as objective truth, Descartes' physics is 'forever lapsed' whilst something from Huyghens's physics is 'forever ratified'.

7. Cf. the conclusion of Canguilhem's article: 'A history of sciences which treats a science in its history as an articulated succession of *facts of truth* does not have to be preoccupied with ideologies. It is understood that the historians

of this school abandon ideology to the historians of ideas or, worse, to the philosophers.

A history of sciences which treats a science in its history as an elaborated purification of *norms of verification* also cannot be concerned with scientific ideologies. What Bachelard distinguished as lapsed history of sciences and ratified history of sciences must be both separated and intertwined. The sanction of truth or objectivity itself brings condemnation on the lapsed. But if what must later become lapsed is not first offered to the sanction, the verification has no basis for making the truth appear.

So the separation of ideology and science must make it difficult to place in continuity in a history of sciences certain apparently conserved elements of an ideology and the scientific construction which destituted the ideology; for example, to seek anticipations of *The Origin of Species* in *d'Alembert's Dream*.

But the intertwining of ideology and science must make it difficult to reduce the history of a science to the platitude of a historical record, that is, a picture without shadows in relief. The historian of sciences must work and present his work on two registers. If it is not so worked and presented, for want of not recognising the specificity of scientific ideology and not giving it a place, . . . the history of sciences itself risks being nothing more than an ideology, in the sense this time of false consciousness of its object . . . To want to do only the history of truth is to do an illusory history. Suchodolski is right on this point: history of the sole truth is a contradictory notion.' (Canguilhem 1970)

8. On the 'borrowing' Althusser did from Bachelard, cf. Althusser 1970 pp. 32, 168, 185; Althusser 1976a p. 190; Althusser 1976b p. 154.

9. This has implications for the study of pieces or fragments: a contradictory dialectic of sciences and ideologies which is 'stripped of the reassuring idealist simplism according to which, in the same way that kindness is never a loss but always finds its reward, the scientific question is never without an answer but always finds its answer. Reality has a bit more imagination. There are questions which will never have an answer because they are imaginary questions with no correspondence to real problems. There are imaginary answers which leave the real problem they evade without a true answer. There are self-styled sciences which are merely the scientistic impostor of a social ideology. There are non-scientific ideologies which in paradoxical encounters give rise to true discoveries — as fire shoots out from the impact of two foreign bodies. Accordingly, the entire complex reality in all its determinations — economic, social and ideological — comes into play in the intelligence of scientific history itself.' (Althusser 1964)

10. 'At that time I said: the essential question is that of Marxist *philosophy*. I still think so. But, if I did see (in 1960–5) *what* the essential question was, I now see that I did not understand it very well . . . I defined philosophy as "Theory of theoretical practice", thus conferring on it, by the use of the single term "theory", the same status as a science. In *theoretically* overestimating philosophy, I underestimated it *politically*, as those who correctly accused me of not "bringing in" the class struggle were quick to point out. . . . If I now propose a different formula: "philosophy is, in the last instance, class struggle in theory". it is precisely in order to be able to give *both* the class struggle (the last instance) *and* the other social practices (among them scientific practice) their due in their "relation" to philosophy.' (Althusser 1976a pp. 149–50)

11. This thesis immediately situates Althusser outside any philosophical problematic of historical 'mediation' in which the long term processes of history are precipitated into events through the mediation of individuals or

groups or through the 'series of mediations' thanks to which it is hoped to be able to demonstrate that the course of the history and practice of individual or collective 'subjects' mirror each other. He has been charged with that enough. The thesis *already* features, in connection with the 'break', in Althusser's initial position (cf., for example, Althusser 1970 pp. 64, 163, 231) and it permits him the analysis of the development of the contradictions between Marx's 'political', 'scientific' and 'philosophical' conceptions. Of course, it is *reinforced* by the rectification to which he later proceeds for purposes of correcting the reduction of the break to a simple epistemological theoretical fact. (Cf. Althusser 1976a p. 123)

12. 'everything is in play between the rigour of a single thought and the thematic system of an ideological field. Their relation is this *beginning* and *this beginning has no end*' (Althusser 1970 p. 64). 'So something *irreversible* really does start in 1845: the "epistemological break" is a point of *no return*. Something begins which will have no end. A "continuing break", I wrote, the beginning of a long period of work' (Althusser 1976a p. 66). "This scientific discovery is a theoretical and political event unprecedented in human history. And I would specify: this event is irreversible. . . . For Marx's scientific discovery has been since the very beginning and has become more and more the object and the stake of a fierce and implacable class struggle. . . . A great deal of work and a very long ideological and political struggle were needed before the union [of the Labour Movement and Marxist Theory] could take place and acquire a historical existence. The very conditions of its realization and existence mean that this Union cannot be a once-and-for-all victory'. (Althusser 1970 pp. 151–2)

13. '... we must go further than the unmentioned presence of his *potential thoughts*, to his problematic, that is, to the constitutive unity of the effective thoughts that make up the domain of the existing *ideological field* with which a particular author must settle accounts in his own thought' (Althusser 1970 p. 66). And cf. the whole of Althusser/Balibar 1975 Part I where explicit reference is made to Foucault. [There does not seem to be an exact correspondence between the following extract Balibar gives from Foucault 1966 and any single passage in Foucault 1970. I have therefore translated the French myself and supplied a relevant extract from Foucault 1970. *Translator*] 'It is indeed possible to write a history of thought in the classical epoch by taking these debates as points of departure or as themes. But then one will be doing only a history of opinions, that is, the choices worked according to individuals, surroundings, social groups; and a whole method of enquiry is implicated. If one wishes to understand an archaeological analysis of knowledge itself, then these are not the debates to serve as the guiding thread or to articulate the business. We must reconstitute the general system of thought the network of which, in its positivity, makes possible a play of simultaneous and apparently contradictory opinions. It is this network that defines the conditions of possibility of a debate or of a problem and it is this network that is the bearer of the historicity of knowledge, etc.' (Foucault 1966, pp. 89–90). Cf. 'After examining these problems and the discussions they give rise to, it is simple enough for the historians to reconstruct the great controversies that are said to have divided men's opinions and passions, as well as their reasoning. . . . It would be pointless to go back over the presuppositions inherent in such a method.' (Foucault 1970 p. 126)

14. Cf. this famous passage in Notebook M: '... the scientifically correct method. The concrete is concrete because it is the concentration of many determinations, hence unity of the diverse. It appears in the process of thinking, therefore, as a process of concentration, as a result, not as a point of departure,

even though it is the point of departure in reality and hence also the point of departure for observation [*Anschauung*] and conception. . . . The totality as it appears in the head, as a totality of thoughts, is a product of a thinking head, which appropriates the world in the only way it can, a way different from the artistic, religious, practical and mental appropriation of this world. The real subject retains its autonomous existence outside the head just as before; namely as long as the head's conduct is merely speculative, merely theoretical' (Marx 1973 pp. 101–2). And compare this, in particular, with this passage from Lenin on the 'criterion of practice' in which he shows that the idealist/pragmatist separation of a domain of practice and a domain of theory is absurd from the scientific point of view because 'in practice, which serves us as a criterion in the theory of knowledge, we must include also the practice of astronomical observations, discoveries, etc.' (Lenin 1968 p. 143)

15. 'Only an ideological world outlook could have imagined societies *without ideology* and accepted the utopian idea of a world in which ideology (not just one of its historical forms) would disappear without trace, to be replaced by *science*' (Althusser 1970 p. 232). I am drawing attention to the fact that this thesis, posed from the start, clarifies the often contested necessity for Althusser's including in his research on the 'ideological state apparatuses' an outline of a 'theory of Ideology in general' (which, as we have seen, is not a general theory of the opposition of Ideology and Science). Every discourse of historical ideologies, and especially every 'Marxist' discourse on ideologies, includes explicitly or implicitly a definition of ideology in general which permits that discourse to think its particular conditions of possibility and its particular historical place. And, according to an ineluctable alternative, there is inscribed there *either* the utopia of the end of ideologies (for example under the form of a definition of ideology as alienated, inverted reflection of the real), *or* the materialist thesis of an indefinite process of transformation of (and in) ideology. (Cf. Althusser 1976b p. 67f.)

16. Here it would be appropriate to discuss in detail the passage in which Althusser is concerned with precisely these theses of Marx on knowledge as 'appropriation of the world' and has developed a definition of thought as 'the historically constituted system of an *apparatus of thought*, founded on and articulated to natural and social reality.' (Althusser/Balibar 1975 p. 41)

17. 'In our philosophical memory it [the period of great political and ideological battles in the Communist Party after the War] remains the period of intellectuals in arms, hunting out error from all its hiding-places . . . and slicing up the world with a single blade, arts, literatures, philosophies, sciences with the pitiless demarcation of class — ' (Althusser 1970 p. 22) Today we will refer to Lecourt's book (Lecourt 1976) and to his Introduction to Bogdanov's works (Lecourt 1977).

18. 'All this is clearly played "over the head of" the concept of the dictatorship of the proletariat, that is, played in the theoretical sense. For the abandonment of a theoretical concept (which — is it necessary to repeat it? — cannot be thought by itself, quite alone, but makes a body with a set of other concepts) cannot be the object of a political decision. Since Galileo every materialist has known that the fate of a scientific concept which objectively reflects a real problem with multiple implications cannot be the object of a political decision.' (Althusser 1977 pp. 32–3)

19. The expression resumes an important theme in Spinozist philosophy and was proposed by Macherey (1965).

20. In a too brief text, Pêcheux introduced this rectification. When it is extended from its original domain to new domains, a revolutionary scientific problematic does not immediately produce scientific concepts but on the

contrary creates 'epistemological obstacles' (Fichant/Pêcheux 1969). To make this point more precise, we must follow Canguilhem's example and place in the centre of the history of sciences the problems of 'importation' and the conditions of 'naturalisation' (or non-naturalisation) of scientific concepts from one discipline to another, these being the recasting of Bachelardian 'regionality'.

References

- Althusser, L.** (1964) Foreword to Macherey, P.: 'La philosophie de science de G. Canguilhem', *La Pensée*, 113, February.
- (1970) *For Marx*, Vintage Books, Random House, New York.
- (1972) 'Sur le rapport de Marx à Hegel', *Lénine et la philosophie*, Paris.
- (1974) *Philosophie et philosophie spontanée des savants*, Paris.
- (1976a) *Essays in Self-Criticism*, New Left Books.
- (1976b) *Positions*, Paris.
- (1977) 22nd Congress of the PCF, Paris 1977.
- Althusser, L. and Balibar, E.** (1975) *Reading Capital*, New Left Books, London.
- Bachelard, G.** (1938) *La Formation de l'esprit scientifique*, Paris.
- (1949) *La Rationalisme appliqué*, Paris.
- (1951) *L'Activité rationaliste de la Physique contemporaine*, Paris.
- (1953) *Le Materialisme rationnel*, Paris.
- Balibar, E.** (1977) 'A nouveau sur la contradiction', in the collection *Sur la Dialectique*, CERM, Editions Sociales.
- Canguilhem, G.** (1964–5) 'Philosophie et science', broadcast by *Télévision scolaire*.
- (1967) 'Mort de l'homme ou l'épuisement du Cogito', *Critique*, July.
- (1968) *Etudes d'histoire et de philosophie des sciences*, Paris.
- (1970) 'Qu'est-ce qu'une idéologie scientifique?', *Organon*, 7.
- Fichant, M. and Pêcheux, M.** (1969) *Sur l'histoire des sciences*, Paris.
- Foucault, M.** (1966) *Les mots et les choses*, Paris, [translated as Foucault 1970].
- (1970) *The Order of Things*, Tavistock, London.
- Lecourt, D.** (1976) *Lyssenko, histoire réelle d'une 'science prolétarienne'*, Paris.
- (1977) Introduction to Bogdanov: *La science, l'art et la classe ouvrière*, Paris.
- Lenin, V.I.** (1968) *Materialism and Empirio-criticism*, Collected Works Vol. 14, Lawrence and Wishart, London.
- Macherey, P.** (1965) 'A propos de la rupture', *La Nouvelle Critique*, May.
- Marx, K.** (1973) *Grundrisse*, Pelican Marx Library, London.