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# The Order of Things

AN ARCHAEOLOGY OF THE HUMAN SCIENCES

MICHEL FOUCAULT

*Translated from the French*

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## Publisher's Note

A literal translation of the title of the French edition of this work (*Les Mots et les choses*) would have given rise to confusion with two other books that have already appeared under the title *Words and things*. The publisher therefore agreed with the author on the alternative title *The order of things*, which was, in fact, M. Foucault's original preference.

In view of the range of literature referred to in the text, it has not proved feasible in every case to undertake the bibliographical task of tracing English translations of works originating in other languages and locating the passages quoted by M. Foucault. The publisher has accordingly retained the author's references to French works and to French translations of Latin and German works, for example, but has, as far as possible, cited English editions of works originally written in that language.

## Foreword to the English edition

This foreword should perhaps be headed 'Directions for Use'. Not because I feel that the reader cannot be trusted – he is, of course, free to make what he will of the book he has been kind enough to read. What right have I, then, to suggest that it should be used in one way rather than another? When I was writing it there were many things that were not clear to me: some of these seemed too obvious, others too obscure. So I said to myself: this is how my ideal reader would have approached my book, if my intentions had been clearer and my project more ready to take form.

1. He would recognize that it was a study of a relatively neglected field. In France at least, the history of science and thought gives pride of place to mathematics, cosmology, and physics – noble sciences, rigorous sciences, sciences of the necessary, all close to philosophy: one can observe in their history the almost uninterrupted emergence of truth and pure reason. The other disciplines, however – those, for example, that concern living beings, languages, or economic facts – are considered too tinged with empirical thought, too exposed to the vagaries of chance or imagery, too age-old traditions and external events, for it to be supposed that their history could be anything other than irregular. At most, they are expected to provide evidence of a state of mind, an intellectual fashion, a mixture of archaism and bold conjecture, of intuition and blindness. But what if empirical knowledge, at a given time and in a given culture, *did* possess a well-defined regularity? If the very possibility of recording facts, of allowing oneself to be convinced by them, of distorting them in traditions or of making purely speculative use of them, if even this was not at the mercy of chance? If errors (and truths), the practice of old beliefs, including not only genuine discoveries, but also the most naïve notions, obeyed, at a given moment, the laws of a certain code of knowledge? If, in

short, the history of non-formal knowledge had itself a system? That was my initial hypothesis – the first risk I took.

2. This book must be read as a comparative, and not a symptomatological, study. It was not my intention, on the basis of a particular type of knowledge or body of ideas, to draw up a picture of a period, or to reconstitute the spirit of a century. What I wished to do was to present, side by side, a definite number of elements: the knowledge of living beings, the knowledge of the laws of language, and the knowledge of economic facts, and to relate them to the philosophical discourse that was contemporary with them during a period extending from the seventeenth to the nineteenth century. It was to be not an analysis of Classicism in general, nor a search for a *Weltanschauung*, but a strictly 'regional' study.<sup>1</sup>

But, among other things, this comparative method produces results that are often strikingly different from those to be found in single-discipline studies. (So the reader must not expect to find here a history of biology juxtaposed with a history of linguistics, a history of political economy, and a history of philosophy.) There are shifts of emphasis: the calendar of saints and heroes is somewhat altered (Linnaeus is given more space than Buffon, Destutt de Tracy than Rousseau; the Physiocrats are opposed single-handed by Cantillon). Frontiers are redrawn and things usually far apart are brought closer, and vice versa: instead of relating the biological taxonomies to other knowledge of the living being (the theory of germination, or the physiology of animal movement, or the statics of plants), I have compared them with what might have been said at the same time about linguistic signs, the formation of general ideas, the language of action, the hierarchy of needs, and the exchange of goods.

This had two consequences: I was led to abandon the great divisions that are now familiar to us all. I did not look in the seventeenth and eighteenth centuries for the beginnings of nineteenth-century biology (or philosophy or economics). What I saw was the appearance of figures peculiar to the Classical age: a 'taxonomy' or 'natural history' that was relatively unaffected by the knowledge that then existed in animal or plant physiology; an 'analysis of wealth' that took little account of the assumptions of the 'political arithmetic' that was contemporary with it; and a 'general grammar' that was quite alien to the historical analyses and works of exegesis then being carried out. Epistemological figures, that is, that were not superimposed on the sciences as they were individualized

<sup>1</sup> I sometimes use terms like 'thought' or 'Classical science', but they refer practically always to the particular discipline under consideration.

and named in the nineteenth century. Moreover, I saw the emergence, between these different figures, of a network of analogies that transcended the traditional proximities: between the classification of plants and the theory of coinage, between the notion of generic character and the analysis of trade, one finds in the Classical sciences isomorphisms that appear to ignore the extreme diversity of the objects under consideration. The space of knowledge was then arranged in a totally different way from that systematized in the nineteenth century by Comte or Spencer. The second risk I took was in having wished to describe not so much the genesis of our sciences as an epistemological space specific to a particular period.

3. I did not operate, therefore, at the level that is usually that of the historian of science – I should say at the two levels that are usually his. For, on the one hand, the history of science traces the progress of discovery, the formulation of problems, and the clash of controversy; it also analyses theories in their internal economy; in short, it describes the processes and products of the scientific consciousness. But, on the other hand, it tries to restore what eluded that consciousness: the influences that affected it, the implicit philosophies that were subjacent to it, the unformulated thematics, the unseen obstacles; it describes the unconscious of science. This unconscious is always the negative side of science – that which resists it, deflects it, or disturbs it. What I would like to do, however, is to reveal a *positive unconscious* of knowledge: a level that eludes the consciousness of the scientist and yet is part of scientific discourse, instead of disputing its validity and seeking to diminish its scientific nature. What was common to the natural history, the economics, and the grammar of the Classical period was certainly not present to the consciousness of the scientist; or that part of it that was conscious was superficial, limited, and almost fanciful (Adanson, for example, wished to draw up an artificial denomination for plants; Turgot compared coinage with language); but, unknown to themselves, the naturalists, economists, and grammarians employed the same rules to define the objects proper to their own study, to form their concepts, to build their theories. It is these rules of formation, which were never formulated in their own right, but are to be found only in widely differing theories, concepts, and objects of study, that I have tried to reveal, by isolating, as their specific locus, a level that I have called, somewhat arbitrarily perhaps, archaeological. Taking as an example the period covered in this book, I have tried to determine the basis or archaeological system common to a whole series of scientific 'representations'

or 'products' dispersed throughout the natural history, economics, and philosophy of the Classical period.

4. I should like this work to be read as an open site. Many questions are laid out on it that have not yet found answers; and many of the gaps refer either to earlier works or to others that have not yet been completed, or even begun. But I should like to mention three problems.

The problem of change. It has been said that this work denies the very possibility of change. And yet my main concern has been with changes. In fact, two things in particular struck me: the suddenness and thoroughness with which certain sciences were sometimes reorganized; and the fact that at the same time similar changes occurred in apparently very different disciplines. Within a few years (around 1800), the tradition of general grammar was replaced by an essentially historical philology; natural classifications were ordered according to the analyses of comparative anatomy; and a political economy was founded whose main themes were labour and production. Confronted by such a curious combination of phenomena, it occurred to me that these changes should be examined more closely, without being reduced, in the name of continuity, in either abruptness or scope. It seemed to me at the outset that different kinds of change were taking place in scientific discourse – changes that did not occur at the same level, proceed at the same pace, or obey the same laws; the way in which, within a particular science, new propositions were produced, new facts isolated, or new concepts built up (the events that make up the everyday life of a science) did not, in all probability, follow the same model as the appearance of new fields of study (and the frequently corresponding disappearance of old ones); but the appearance of new fields of study must not, in turn, be confused with those overall redistributions that alter not only the general form of a science, but also its relations with other areas of knowledge. It seemed to me, therefore, that all these changes should not be treated at the same level, or be made to culminate at a single point, as is sometimes done, or be attributed to the genius of an individual, or a new collective spirit, or even to the fecundity of a single discovery; that it would be better to respect such differences, and even to try to grasp them in their specificity. In this way I tried to describe the combination of corresponding transformations that characterized the appearance of biology, political economy, philology, a number of human sciences, and a new type of philosophy, at the threshold of the nineteenth century.

The problem of causality. It is not always easy to determine what has

caused a specific change in a science. What made such a discovery possible? Why did this new concept appear? Where did this or that theory come from? Questions like these are often highly embarrassing because there are no definite methodological principles on which to base such an analysis. The embarrassment is much greater in the case of those general changes that alter a science as a whole. It is greater still in the case of several corresponding changes. But it probably reaches its highest point in the case of the empirical sciences: for the role of instruments, techniques, institutions, events, ideologies, and interests is very much in evidence; but one does not know how an articulation so complex and so diverse in composition actually operates. It seemed to me that it would not be prudent for the moment to force a solution I felt incapable, I admit, of offering: the traditional explanations – spirit of the time, technological or social changes, influences of various kinds – struck me for the most part as being more magical than effective. In this work, then, I left the problem of causes to one side;<sup>1</sup> I chose instead to confine myself to describing the transformations themselves, thinking that this would be an indispensable step if, one day, a theory of scientific change and epistemological causality was to be constructed.

The problem of the subject. In distinguishing between the epistemological level of knowledge (or scientific consciousness) and the archaeological level of knowledge, I am aware that I am advancing in a direction that is fraught with difficulty. Can one speak of science and its history (and therefore of its conditions of existence, its changes, the errors it has perpetrated, the sudden advances that have sent it off on a new course) without reference to the scientist himself – and I am speaking not merely of the concrete individual represented by a proper name, but of his work and the particular form of his thought? Can a valid history of science be attempted that would retrace from beginning to end the whole spontaneous movement of an anonymous body of knowledge? Is it legitimate, is it even useful, to replace the traditional 'X thought that . . .' by a 'it was known that . . .'? But this is not exactly what I set out to do. I do not wish to deny the validity of intellectual biographies, or the possibility of a history of theories, concepts, or themes. It is simply that I wonder whether such descriptions are themselves enough, whether they do justice to the immense density of scientific discourse, whether there do not exist, outside their customary boundaries, systems of regularities that have a decisive

<sup>1</sup> I had approached this question in connection with psychiatry and clinical medicine in two earlier works.

role in the history of the sciences. I should like to know whether the subjects responsible for scientific discourse are not determined in their situation, their function, their perceptive capacity, and their practical possibilities by conditions that dominate and even overwhelm them. In short, I tried to explore scientific discourse not from the point of view of the individuals who are speaking, nor from the point of view of the formal structures of what they are saying, but from the point of view of the rules that come into play in the very existence of such discourse: what conditions did Linnaeus (or Petty, or Arnauld) have to fulfil, not to make his discourse coherent and true in general, but to give it, at the time when it was written and accepted, value and practical application as scientific discourse – or, more exactly, as naturalist, economic, or grammatical discourse?

On this point, too, I am well aware that I have not made much progress. But I should not like the effort I have made in one direction to be taken as a rejection of any other possible approach. Discourse in general, and scientific discourse in particular, is so complex a reality that we not only can, but should, approach it at different levels and with different methods. If there is one approach that I do reject, however, it is that (one might call it, broadly speaking, the phenomenological approach) which gives absolute priority to the observing subject, which attributes a constituent role to an act, which places its own point of view at the origin of all historicity – which, in short, leads to a transcendental consciousness. It seems to me that the historical analysis of scientific discourse should, in the last resort, be subject, not to a theory of the knowing subject, but rather to a theory of discursive practice.

5. This last point is a request to the English-speaking reader. In France, certain half-witted 'commentators' persist in labelling me a 'structuralist'. I have been unable to get it into their tiny minds that I have used none of the methods, concepts, or key terms that characterize structural analysis.

I should be grateful if a more serious public would free me from a connection that certainly does me honour, but that I have not deserved. There may well be certain similarities between the works of the structuralists and my own work. It would hardly behove me, of all people, to claim that my discourse is independent of conditions and rules of which I am very largely unaware, and which determine other work that is being done today. But it is only too easy to avoid the trouble of analysing such work by giving it an admittedly impressive-sounding, but inaccurate, label.

## Preface

This book first arose out of a passage in Borges, out of the laughter that shattered, as I read the passage, all the familiar landmarks of my thought – *our* thought, the thought that bears the stamp of our age and our geography – breaking up all the ordered surfaces and all the planes with which we are accustomed to tame the wild profusion of existing things, and continuing long afterwards to disturb and threaten with collapse our age-old distinction between the Same and the Other. This passage quotes a 'certain Chinese encyclopaedia' in which it is written that 'animals are divided into: (a) belonging to the Emperor, (b) embalmed, (c) tame, (d) sucking pigs, (e) sirens, (f) fabulous, (g) stray dogs, (h) included in the present classification, (i) frenzied, (j) innumerable, (k) drawn with a very fine camelhair brush, (l) *et cetera*, (m) having just broken the water pitcher, (n) that from a long way off look like flies'. In the wonderment of this taxonomy, the thing we apprehend in one great leap, the thing that, by means of the fable, is demonstrated as the exotic charm of another system of thought, is the limitation of our own, the stark impossibility of thinking *that*.

But what is it impossible to think, and what kind of impossibility are we faced with here? Each of these strange categories can be assigned a precise meaning and a demonstrable content; some of them do certainly involve fantastic entities – fabulous animals or sirens – but, precisely because it puts them into categories of their own, the Chinese encyclopaedia localizes their powers of contagion; it distinguishes carefully between the very real animals (those that are frenzied or have just broken the water pitcher) and those that reside solely in the realm of imagination. The possibility of dangerous mixtures has been exorcized, heraldry and fable have been relegated to their own exalted peaks: no inconceivable amphibious maidens, no clawed wings, no disgusting, squamous epidermis, none