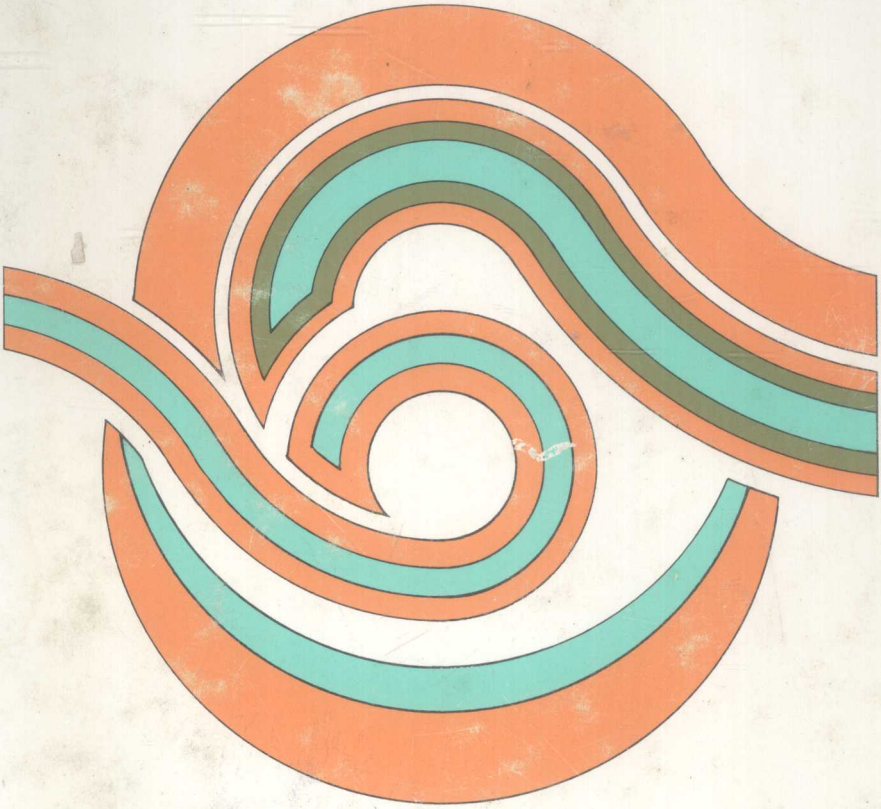
Studies in Rationality and Social Change

Explaining Technical Change

Jon Elster



Cambridge University Press

Jon Elster

EXPLAINING TECHNICAL CHANGE

A Case Study in the Philosophy of Science

Cambridge University Press
Cambridge – London – New York –
New Rochelle – Melbourne – Sydney

Universitetsforlaget
Oslo – Bergen – Tromsø

Published in collaboration with Maison des Sciences de l'Homme, Paris

Published by the Press Syndicate of the University of Cambridge
The Pitt Building, Trumpington Street, Cambridge CB2 1RP
32 East 57th Street, New York, NY 10022, USA
296 Beaconsfield Parade, Middle Park, Melbourne 3206, Australia
and in Scandinavia by Universitetsforlaget, Kolstadgt. 1, Oslo 6, Norway

First published 1983

Cambridge University Press ISBN 0-521-24920-1 hard covers excluding Scandinavia
Cambridge University Press ISBN 0-521-27072-3 paperback excluding Scandinavia
Universitetsforlaget ISBN 82-00-06334-8 hard covers. Scandinavia only
Universitetsforlaget ISBN 82-00-06454-9 paperback. Scandinavia only

Library of Congress catalogue card number: 82-9702

British Library Cataloguing in Publication Data
Elster, Jon
Explaining technical change. – (Studies in rationality and social change)
1. Technological innovations. – Social aspects
I. Title II. series
306'.46 T173.8

Printed in Norway by
Tangen-Trykk A/s, Drammen

EXPLAINING TECHNICAL CHANGE

STUDIES IN RATIONALITY AND SOCIAL CHANGE

STUDIES IN RATIONALITY AND SOCIAL CHANGE

Editors: Jon Elster and Gudmund Hernes

Editorial Board:
Fredrik Barth
Amartya Sen
Arthur Stinchcombe
Bernard Williams

Explaining technical change (1983)

Contents

Preface and acknowledgements

The occasion for writing this book was provided by Bernt Schiller of the University of Linköping (Sweden), who asked me to write a textbook in the philosophy of science that could be suitable for their doctoral programme 'Technology and social change'. I am grateful for the suggestions and comments offered by him and his colleagues along the way. I should also like to thank the following for their comments on an earlier draft: G. A. Cohen, Aanund Hylland, Michael MacPherson, Nathan Rosenberg, and an anonymous referee of Cambridge University Press. Acknowledgements for comments on the Appendices are given at the appropriate places. Appendix 1 was originally published in *Social Science Information* 18 (1979).

Part I of the work can be read as an introduction to the philosophy of scientific explanation. The Achilles heel of this part, clearly, is the chapter on causal explanation. My competence in these intricate matters is not high, but since the chapter was required by the overall architectonics of the book I felt I should state my views even when they are not strongly grounded. I hope I have avoided saying too much that is obviously wrong, but the reader may justifiably feel that some of what I say is not very interesting or not as tightly argued as he might wish.

Part II is *not* to be read as an introduction to the theory of technical change. It is subordinated to the epistemological purpose of showing how the distinctions and propositions of Part I can be applied to a specific set of empirical problems. Here the danger is that my exposition of the theories may be too compact for the non-specialist and too sloppy for the specialist. To the first, I can only offer the advice to look up the original works. To the second I make a plea that ambiguous statements be taken in their most plausible sense. Even so, there will probably remain some statements that are plainly wrong, of which the reader can justly complain.

J.E.

General introduction

The study of technical change is uniquely well suited to epistemological analysis. It is located at the interface of social science and the natural sciences, and so might be expected to be relevant to the discussion of 'the unity of science'. It bridges the gap between pure science and everyday affairs, and might therefore be expected to throw light on how theoretical knowledge relates to the observable world. Technical change – the manufacture and modification of tools – may have played an important role in the evolution of intelligent life on earth, comparable to that of language. During the course of human history, social institutions have emerged and disappeared largely in response to changes in productive and destructive technology. Moreover, technical change offers a challenge to analysis in that it is fundamentally unpredictable. 'If I knew where jazz was going, I'd be there already', Humphrey Lyttelton is reported to have said. Similarly, any attempt to explain technical change sooner or later comes up against the paradox of turning creativity into a dependent variable.

In this book I first set out the main varieties of scientific explanation, and then look at some central theories of technical change from the vantage point provided by that discussion. This enables me to deal with what are, I think, the two main approaches to technical change. First, technical change may be conceived of as a rational goal-directed activity, as the choice of the best innovation among a set of feasible changes. Secondly, technical change may be seen as a process of trial and error, as the cumulative addition of small and largely random modifications of the production process. Any serious student of technology will agree that technical change exhibits both these aspects, but there are strong differences in emphasis between the contending explanations.

This main dichotomy cuts across many of the other relevant distinctions that can be made in this domain. I believe, for instance, that neoclassical and Marxist theories of technical change at the level of the firm

share an emphasis on the rational-actor approach. True, Marxists have not engaged in much detailed modelling of technical change at the micro-level, but there are a number of historical studies by Marxists who argue that the entrepreneur uses innovation as a weapon in the class struggle. The neoclassical economists explain technical change in the light of profit maximization, whereas Marxists tend to argue that power rather than short-term profits is at stake. Within both traditions technical change is explained in the light of the goal to be achieved, although they impute different goals to the entrepreneur.

On the other side of the main dichotomy we find the 'evolutionary' theories of technical change, which emphasize past history rather than future goals in the explanation of why firms currently use the techniques they do. Typically the proponents of these theories look at technical change as more or less closely analogous to evolution by natural selection. It is instructive, therefore, to consider the socio-biological studies of animal tool behaviour which explain technical progress as a literal rather than a metaphorical instance of biological evolution. These theories set out to explain not only specific inventions, but also the emergence of genes for inventiveness. Interestingly, tool behaviour turns out to be closely related to play behaviour – a reminder that creativity is of the essence in technical change.

Technical change may be studied at various levels of aggregation, and for various time spans. Neoclassical and evolutionary theories tend to study change at the levels of the firm and the industry, in contradistinction to the large-scale historical syntheses offered by Schumpeter and Marx. Schumpeter probably is the most influential single writer on technical change, its causes and consequences. Again, this may be because he emphasized creativity and disequilibrium, rather than trying to fit technical change into the pattern of routine profit maximization. He praised capitalism not because of its efficiency and rationality, but because of its dynamic character – to be explained in terms of irrational expectations and dreams of founding private dynasties. Marx also insisted on the uniqueness of capitalism in that, in contrast to all earlier modes of production, it does not oppose technical change, but rather depends on it. Yet in his materialist conception of history he also, somewhat inconsistently, argued that the development of the productive forces is the major determinant of social change in all modes of production.

It ought to go without saying that I am not offering an introduction to theories of technical change. In most cases, the reader will have to go elsewhere for detailed expositions of the various theories discussed in Part II. The discussions of Marxism in Ch. 7 and in Appendix 2 are somewhat more detailed, because I happen to know more about this tradition than about the others. But even in this case my attention is directed by epistemological concerns rather than by an informed interest in the substantive issues. This is a case study in the philosophy of science, not a bird's eye view of science. Again, it ought to go without saying that I am not out to tell economists or historians how to do their work. I believe, however, that philosophers of science can be of help in distinguishing true from spurious foci of disagreement within the empirical disciplines. Empirical work conducted in isolation from the philosophy of science may be no worse for that, whereas the philosophy of science atrophies if it is not in close and constant touch with the development of current thinking on empirical matters. Yet the asymmetry is not so radical as to make philosophy of science totally parasitic.

This will be admitted by many scientists with respect to problems of verification and falsification. Few will contest the statement that Popper's methodology of science – basically an injunction to scientists to stick their necks out – has had a valuable influence. At the more technical level, the issue of statistical inference has been discussed by philosophers of science and scientists working in parallel and sometimes in tandem. My concern here, however, is with the structure of scientific explanations. Whereas I believe that the problems of verification are basically the same in all disciplines, I shall argue that the differences in their subject matters impose different strategies of explanation. I shall distinguish between causal, functional and intentional explanations, corresponding – broadly speaking – to the physical, biological and social sciences respectively. While recognizing that causal explanation in some sense or senses is more basic than the other modes, I shall argue that there nevertheless is room and need for the latter.

The dichotomy between rational-choice theories of technical change and evolutionary theories corresponds – and once again I have to speak very broadly – to the distinction between intentional and functional explanation. It will turn out, however, that in many respects the exceptions to this statement are the more interesting cases. In fact, not only science, but philosophy of science as well, has most to offer at the level of

detailed analysis where such glib generalities break down. Technical evolution differs from biological evolution in that the changes are far from totally random, but to some extent directed; they are also screened by a mechanism in which human intentionality plays a crucial role. Similarly, not all intentional models of technical change qualify as rational models – for there may be cases when the underlying expectations are not rationally formed. The lack of rationality in the expectations may be due to complex strategic interactions, or to a fundamental uncertainty about the future, or to both. It is perhaps the interaction between these two sources of ignorance that lends a unique flavour and depth to the issue of explaining technical change. Taken separately, both games without a solution and decisions under radical uncertainty create havoc with rational-choice models. When they both operate in a given choice situation, the result is close to chaos. Out of this chaos the evolutionary theories emerge as the more likely to explain actual technical progress. But here I go beyond my self-imposed limitations, so instead let me turn to matters more within my competence.

PART I

Modes of Scientific Explanation

Introduction to part I

The philosophy of science, generally speaking, has two main tasks. One is to explain the features that are common to all the sciences (or at least all the empirical sciences), the other to explain what sets them apart from each other. To begin with the second task, there is a long tradition of distinguishing between the natural sciences and the humanities *(Geisteswissenschaften)*. Within the natural sciences one may distinguish, furthermore, between the study of inorganic nature (or *physics)* and the study of organic nature *(biology)*. Within the humanities as traditionally defined there has developed a cleavage between the *social sciences* (which I define so broadly as to include linguistics, history, and psychology, in addition to the more obvious disciplines) and the aesthetic disciplines or *arts*. Now these distinctions by subject matter are not in themselves very interesting. Their relevance, if any, must stem from their being correlated with other classifications. I shall discuss three such ways of classifying the sciences: according to *method,* according to the underlying *interest,* and according to mode of *explanation.*

A widely held view is that the sciences are to be distinguished from each other according to their characteristic methods. The natural sciences, on this view, employ the hypothetico-deductive method, the arts use the hermeneutic method, and the social sciences the dialectical method. It is not always clear whether these are methods for theory construction or for theory verification, except that the hypothetico-deductive method clearly is of the latter kind. Let me briefly and without much argument state my opinions of this view. (i) The hypothetico-deductive method is *the* method for verification in all empirical sciences. If the hermeneutic method is understood as a procedure for verification, it can only be a sub-species of the hypothetico-deductive method. To be precise, the hermeneutic method is the hypothetico-deductive method applied to intentional phenomena, with some peculiar features due to

the nature of these phenomena.[1] (ii) If the hermeneutic method is seen as a method for theory formation, it coincides with the notion of intentional explanation. (iii) The dialectical method as a procedure for verification invokes some kind of appeal to 'praxis', i.e. to the idea that social theories can simultaneously be agents of change and explanations of change. This, however, is ambiguous in that it can mean either that the theories are self-fulfilling or that they are instrumental in bringing about some desired change. I believe that the notion vaguely underlying most uses of the phrase 'the unity of theory and praxis' is that the theory should be both self-fulfilling and useful, but this, unfortunately, is normally not possible.[2] (iv) The dialectical method as a tool for theory formation can also be understood in several ways, the most interesting of which involves the notion of psychological and social *contradictions*. These, however, can be made intelligible in the standard causal-cum-intentional language of the social sciences.[3]

The upshot of this – excessively condensed – discussion is that there are no grounds for distinguishing between scientific disciplines according to their methods of verification, with the exception mentioned in note 1. Nor should hermeneutics or dialectics be thought of as methods for theory-formation that are somehow *sui generis*. In my view, there is equally little substance in Jürgen Habermas's theory that the sciences differ mainly in the interests they serve.[4] By his account, the natural sciences serve a technical interest, the hermeneutic sciences a practical interest, and the social sciences an emancipatory interest. Now this may be tautologically true, contingently true or contingently false, according to how the terms are further defined. As far as I can understand, the most reasonable reading of the view makes it come out as false. Each of the three scientific disciplines can serve each of the three interests, although perhaps to different degrees and (above all) in different ways. I do not want to enter into further discussion of this issue, since I believe that by any reading the theory is singularly unhelpful for the practising scientist – and this means that it fails the acid test for any philosophy of science. The language of interests is simply too coarse-grained and too external to scientific practice to mesh well with the fine grain of actual research.

I now proceed to sketch my own account of how the sciences differ from each other. I shall argue that the most illuminating and fertile distinction is between various modes of scientific explanation, which again are closely linked to strategies of theory-formation. Only certain