

# MAURICE DOBB

Theories of  
value and  
distribution since  
Adam Smith

Ideology and  
economic theory

THEORIES OF VALUE AND  
DISTRIBUTION  
SINCE ADAM SMITH

IDEOLOGY AND ECONOMIC THEORY

BY  
MAURICE DOBB

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M.D.

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## INTRODUCTORY: ON IDEOLOGY

## I

Around the question of the so-called 'ideological' element in economic theory (as of social theory in general) there has been in recent times increasing debate. In entering this debate at this stage it would be tedious to start by embarking on a genealogy and a quest for unique and precise definition of the term. On this let it suffice for the moment to say that, while the concept bears a close relationship to the Hegelian 'false consciousness' that serves to obstruct man's vision of himself and of his conditions of existence, it is not to be taken as synonymous therewith, still less as referring exclusively to the delusive element in thought (as some have indeed used it). Its central reference, undoubtedly, is to the historically-relative character of ideas, whether this be regarded as no more than an element or aspect of them or as characterising them in their entirety. But such historical relativity may embrace both insight and partiality,\* and this perhaps from the very nature of the situation, in a way that defies any complete analytical separation. It will be this reference, at any rate, that we shall have principally in mind in what follows. Perhaps it is unnecessary to add, further, that when the word, 'ideology', is used, this must necessarily refer to a whole *system* of thought, or coordinated set of beliefs and ideas, which form a framework, or higher-level group of related concepts, for more specific and particular notions, analyses, applications and conclusions. As such it will generally be related to certain activities and policies, but not necessarily in any simple and obvious or direct manner; and to

\* Cf. Oskar Lange, *Political Economy*, Eng. ed. (Warsaw and London, 1963) Vol. 1, pp. 327-30. In a 'Note on ideology and tendencies in economic research', *International Social Sciences Journal* (UNESCO), Vol. xvi, No. 4, 1964, p. 525, Oskar Lange wrote: "Ideological influences do not always lead to the apologetic degeneration of social science. Under certain conditions they may be a stimulus of true objective research."

those who conduct discussion at the higher (or more general) level the relation may not always be fully conscious, still less explicit. At its most general an ideology constitutes or implies a philosophical standpoint, in our present context a social philosophy, provided that this is not given too formal or methodological a connotation.

In the field of political economy or economic theory, the rôle of ideology (and by implication its definition) has been treated in various ways. First, it has been contrasted with the scientific core of the subject, by implication being regarded as the intermixture or accretion of ethical postulates and so-called 'value judgements'. Thus it is an alien element in what should be designed as an objective and 'positive' enquiry, which although inevitably encroaching upon most people's thinking about practical affairs, deserves to be purged by more rigorous analysis and precise definition. Thus the proposition that on a competitive market factors of production are priced according to their marginal or incremental productivity is sometimes contrasted with the statement that people *ought* to be rewarded according to their contributions to production, and the latter abjured as an unscientific intrusion; or again theories about how income-distribution is in fact determined are contrasted with postulates about what income-distribution ideally should be. Sometimes, while this contrast between an ideological and scientific element in a theory or doctrine is preserved, the boundaries of the former are more widely drawn to include other classes of statement than purely ethical ones: we find them, for example, called 'metaphysical' statements,\* which

\* Thus Professor Joan Robinson has answered the question, "what are the criteria of an ethical proposition as opposed to a scientific one?" by saying: "if an ideological proposition is treated in a logical manner, it either dissolves into a completely meaningless noise or turns out to be a circular argument". These she seems virtually to identify with "metaphysical" propositions, which are admittedly "not without content" and although "they do not belong to the realm of science yet they are necessary to it" and, in social sciences at least, have "played an important, perhaps an indispensable, role". "Whether or not ideology can be eliminated from the world of thought in the social sciences, it is certainly indispensable in the world of action in social life." The classical notion of 'value' is here classed as "one of the great metaphysical ideas in economics" (Joan Robinson, *Economic Philosophy* (London, 1964) pp. 7-9, 29 *seq.*). Cf. also the

have no proper place in a scientific theory since they are incapable of being tested or disproved; nevertheless they perform a function in persuading people to adopt certain attitudes and to undertake certain activities.

Joseph Schumpeter, who among modern economists has made what is, perhaps, the fullest and most serious contribution to the discussion, has adopted a rather different – one could perhaps say less *simpliste* – attitude to this question.\* What he has justly singled out and aptly called ‘vision’ – vision, *i.e.* of the complex shape of reality and of the nature of the problems confronting mankind in any given historical situation – is inevitably ideological. Thus ideology “enters on the very ground floor, into the pre-analytical cognitive act” and with the necessary start of theory “with material provided by our vision of things”; “this vision [being] ideological almost by definition”, since “it embodies the picture of things as we see them”.† The reason adduced for this seems to be less the historically-conditioned perspective of the observer, inevitably limited as this must be by time and place and position in society, than emotional commitment prompting men to draw pleasing pictures of themselves and of their kind – the fact that “the way in which we see things can hardly be distinguished from the way in which we wish to see them” (although it is added that “the more honest and naive our vision is, the more dangerous is it to the eventual emergence of anything for which general validity can be claimed”). Hence he concludes that, while “Political Economy” and “Economic Thought” generally‡ must almost

dismissal of the whole classical (especially Ricardian) notion of ‘real value’ as being “metaphysical” in Gunnar Myrdal, *The Political Element in the Development of Economic Theory*, trans. P. Streeten (London, 1953) pp. 62–5. For comment on this, see, on the other hand, R. L. Meek, *Economics and Ideology and other Essays* (London, 1967) pp. 210–15.

\* Schumpeter incidentally starts by denying that ideology is to be equated with ‘value judgements’: “an economist’s value judgements often reveal his ideology but they are *not* his ideology” (*History of Economic Analysis* (New York and London, 1954) p. 37).

† *Ibid.*, pp. 41–2.

‡ Political Economy is defined as “certain unifying (normative) principles such as the principles of economic liberalism, of socialism, and so on”, leading to the advocacy of “a comprehensive set of economic policies”; Economic Thought as



inevitably be ideologically conditioned, "economic analysis" proper can be treated as independent and objective – a hard core of formal techniques and instruments that are governed by supra-historical standards and rules to be discussed and assessed independently, and about which a separate history can be composed to answer unambiguously such questions as whether and "in what sense there has been 'scientific progress' between Mill and Samuelson".\*

This Schumpeterian view, qualified and hedged about with numerous reservations as it is, is clearly connected with the more crude and forthright 'box of tools' view of economic analysis as being (in its modern form at least) purely instrumental, concerned with techniques capable of application to a wide variety of purposes and situations. As such it has no interest in normative judgements; and it is unconcerned with the specific purposes to which it is put – whether to clarify the problems of a profit-making monopoly or those of the planners of a socialist economy. This conception of the pure economist's rôle has, naturally, been furthered by the vogue of mathematical methods and forms of statements in economics, to the point even of purifying the subject of notions, elements or relations incapable of being quantified and expressed in an equational system.

Of this attempt to separate economic technique from its product it is, surely, appropriate to say simply this. *Either* the 'analysis' of which Schumpeter speaks is a purely formal structure without any relation to economic problems or sets of questions to which it is being designed as an answer (or aid to answering) – in which case it does not constitute a set of propositions or statements with any economic content – *or* else it is a logical system designed as the vehicle of certain statements *about* economic phenomena or activities. If it is, indeed, the former it cannot be identified with the history of economic

"the sum total of all the opinions and desires concerning economic subjects, especially concerning public policy . . . that, at any given time and place, float in the public mind" (*ibid.*, p. 38).

\* *Ibid.*, pp. 38–9. In a like vein Professor J. J. Spengler has confidently stated that "whatever be the effects of ideology, they tend to diminish in importance as economics matures and achieves scientific autonomy" (in R. V. Eagley (ed.) *Events, Ideology and Economic Theory* (Detroit, 1968) p. 175).

theories such as those we review below; since these theories, as we shall see, are very much concerned with economic statement, if at a fairly general level. If it is the latter of our stated alternatives, it surely *cannot* be separated from the answers to questions that it frames, and hence from the actual (or supposed) shape of the economic *problems* with which it is designed to deal – and this however ‘rarified’ or abstract the structure of statement happens to be. In this case it is impossible to claim for it ‘independence’ of the economic content and meaning of the propositions which *are* (on Schumpeter’s own admission) ideologically conditioned, and hence impossible to regard it as supra-ideological. Theoretical analysis and generalisation invariably build upon classification, in the sense of using what has first been classified as its material units or counters; and what is classification but a drawing of boundaries between discrete objects, which are in turn derived from the structural pattern one senses (or deems to have discovered) in the real world? Schumpeter himself makes this clear in his very definition of ‘Vision’ – “the thing that comes first . . . in every scientific venture”: as he emphasises, “before embarking upon analytic work of any kind we must first single out the set of phenomena we wish to investigate, and acquire ‘intuitively’ a preliminary notion of how they hang together or, in other words, of what appear from our standpoint to be their fundamental properties”.\*

To say this is not to deny that a separate study could be made of economic theory in its analytical aspect alone and even a history be written of this aspect *per se*, viewed as the perfecting of a technical apparatus (as one could write the history of any other technique).† But what is dubious is whether if this were

\* Schumpeter, *History of Economic Analysis*, pp. 561–2.

† Presumably this would be concerned with such questions as to the time and occasion when the differential calculus was first employed as a vehicle of economic statement, with discussion perhaps of its appropriateness for certain uses and its inappropriateness for others; and similarly with the use of ‘difference equations’ as distinct from differential, or of matrix algebra; perhaps also the concept of elasticity and the geometry of certain types of curve and the types of theory to which they are relevant. But it has to be remembered that implicit in techniques may be certain axioms which conceal a certain philosophical approach (*cf.* the footnote on page 7).

done one could regard it as a study of some separable and definable section of the subject itself: *i.e.* as a set of propositions or statements which the analytical apparatus has been designed to carry. This would seem to be an altogether different matter. Admittedly it may be hard to divide one's talk about the analysis as an instrument, and one's assessment of its rôle, from the particular use to which this analysis is put. But there surely is a difference, and a crucial one, between discussion of the syntax of sentences and of the content of particular statements that are cast in any given syntactic form. What is highly questionable is whether in economics, or in any branch of social science, if one pays attention to the economic *content* of a theory as distinct from its analytical framework, any part of the theory can preserve the independence and neutrality claimed (and with some reason) for the formal analysis itself.\* Such content must consist of some kind of statement about the shape and functioning of actual economic processes, however particularised or however generalised the statement claims to be. This must, surely, be the case unless reference is being made to some entirely imaginary land of Cathay. Here the simile of a picture or map seems applicable, and no longer that of a tool or instrument. Into its formation Schumpeter's 'vision' must essentially enter; the statement which theory enshrines, *qua* 'picture' or 'map', being entirely dependent upon and relative to such 'vision',† and the latter, as Schumpeter himself so wisely

\* There is, however, the view that has been expressed, *e.g.* by Professor F. A. Hayek, that the propositions of economic theory have a universal and necessary character akin to that of "synthetic *a priori* propositions"; the objects that form the subject matter of social sciences being "not physical facts" but wholes "constituted" from "familiar categories of our own minds". Since economic principles or laws are not empirical rules, then they presumably have, according to this view, both independence and neutrality, alike in content and in form. Cf. citation and comment on this view in the present writer's *Studies in the Development of Capitalism* (London, 1946) p. 27, n. 2. Perhaps it was something like this which Marshall had in mind when he spoke (in connection with the theoretical basis of Free Trade) of "economic truths as certain as those of geometry" (*Official Papers* by Alfred Marshall (London, 1926) p. 388).

† One would suppose, indeed, that this was the implication of Schumpeter's statement that we have quoted, to the effect that ideology "enters on the very ground floor, into the preanalytic cognitive act", to which is added: "Analytic work begins with material provided by our vision of things, and this vision is ideologic almost by definition" (*History of Economic Analysis*, p. 42).

stresses, being always relative to a particular time and social place in the process of history. No examination of economic theory, still less historical examination of *systems* of theories, seems to be justified in denying or ignoring this relativity. A mathematical 'model' can be (and should be, *inter alia*) examined in its purely formal aspect, as a consistent structure. At the same time, *qua* economic theory, its very structure is relevant to the statement it is making about reality – to its diagnostic quality. In choosing one structure in preference to another, the model-builder is not only providing a scaffolding or framework within which human thought can operate, but is laying emphasis upon certain factors and relationships and excluding others or casting them into the shadows;\* and in doing so he can be judged to be distorting or illuminating reality, and thus affording an unsound or a sound basis for interpretation and prediction – more likely perhaps he is illuminating some corners or facet of reality, or certain situations that recur, at the same time as he is obscuring, or totally concealing, others. This is not to say, of course, that any such distortion or partiality is part of the conscious intention of the model-builder, who may indeed have chosen its shape for purely formal reasons, because he regarded it as intellectually ingenious or aesthetically pleasing. But in the degree that he is influenced by its economic implications – in the degree, that is, to which he is trying to be an economist – its shape and projection will be influenced by his vision of the economic process, and by whatever socio-historical conditions shape and limit his mental picture of social reality.

If, however, some economic statements, at any rate those at the most general level, are capable of expression in purely mathematical form, it might seem as though the 'picture' of economic reality they embody must be of too abstract a character to be affected by 'ideological' influences in any marked degree and even less to carry in themselves any particular bias or partiality. Hence the content of the statement as

\* Even the choice of technique may not be without material implication (*e.g.* continuity).

well as its form could here be qualified as 'ideologically neutral' and 'supra-historical', in sufficiently high degree at least for any historically-relative element in its make-up not to matter and to be justifiably ignored. It has often been said that a system of simultaneous equations carries *per se* no causal implication. All that such a system does is to provide a description of a situation as a set of interrelations: a situation composed of a cluster of internally related elements and treated as being isolated, comparatively speaking, from what lies outside it, at least to the extent of not interacting with the latter. But it does no more than this.

As such, however, a description of this kind does not amount to an *explanation*, in the sense of depicting the situation as an economic process that works in a certain way and is capable of being acted upon and influenced.\* For this purpose the equational system must be made to tell us something *more*; and this 'something more' almost inevitably has a causal form, whether as complex mutual interaction of a set of variables or as the simpler type of uni-directional causal linkage.† This is quite commonly done, in fact, even with what lay claim to be purely formal systems depicting a *catena* of interrelations and no more; an *order* of determination being implied as soon as some of the variables are treated as exogenously determined from outside

\* In this context let us not forget the statement of Wittgenstein: "In life . . . we use mathematical propositions *only* in order to infer from propositions which do not belong to mathematics to others which equally do not belong to mathematics" (*Tractatus logico-philosophicus* (London, 1922) p. 169).

† It has been stated that "the formulation of causal relations in terms of functional interdependence is precisely the aim of the more advanced sciences which have got beyond the imprecise concepts of cause and effect" (T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory* (London, 1938) p. 71). On the other hand, Mario Bunge has said that "the disclosure of interactions need not always *exhaust* the problems of determination unless an extreme symmetry is at stake", and that, for example, "the usual interpretation of quantum mechanics does not sweep out causes and effects, but rather the rigid causal nexus among them" (so-called "quantum indeterminacy" being "a consequence of the idealistic hypothesis inherent in modern positivism"). It is added that "a causal interpretation of a mathematical form . . . does not belong to the mathematical symbols but to the system of relations linking the signs with the physical, chemical, biological . . . entities in question. Sometimes such an interpretation is not made explicitly but is taken for granted" (Mario Bunge, *Causality* (Cambridge, Mass., 1959) pp. 14, 76-7, 164).

the system, or else treated as constants, and hence specified as *data* (implicitly or explicitly) and the others as being dependent on the internal relations of the system or as the 'unknowns' awaiting a solution.\* This is certainly true of the Walrasian system of general equilibrium, despite assertions (or at least implications) sometimes made to the contrary. Walras himself, as we shall later see, did not refrain from speaking of "forces [which] are the primary cause and conditions of the variation of prices" or of the prices of the productive services of factors as being "determined in the market for products".† In the case of those dynamic 'models' that have played so large a rôle in modern theories of growth, the system of interrelation-

\* Cf. F. Zeuthen, *Economic Theory and Method* (London, 1955) p. 23: "If we have a special economic science, it is because there exists a particularly intensive connection within the circle of phenomena which are generally termed economic, so that in a great part of the work of investigation these can with advantage be considered as mutually interdependent variables, whereas a series of other phenomena . . . are to a lesser extent influenced by the economic phenomena and therefore with a very good approximation may be taken as data." Cf. also Professor Gautam Mathur who has been rightly concerned to emphasise the incorrectness of asserting "that in an equilibrium system there are no causal relations, because such a situation is described by a set of simultaneous equations." This is an incorrect interpretation because "every equation depicting an economic relation has got one or two direction signs, which we neglect in print, but which must not be lost sight of when we analyse the solution of simultaneous equations" (*Planning for Steady Growth* (Oxford, 1965) p. 70).

† L. Walras, *Elements of Pure Economics*, ed. W. Jaffé (London, 1954) pp. 146-8, 422. R. Bentzel and B. Hansen ('On Recursiveness and Interdependency in Economic Models', *Review of Economic Studies*, Vol. xxii, 1954-5, pp. 153 *seq.*) have argued that the apparent "interdependency" (merely) of a Walrasian system "only arises because the system is a static equilibrium system" and "a static equilibrium system only expresses the conditions for an unspecified dynamic system to be in equilibrium, i.e. to repeat itself". Such a system "is a derived model . . . Static equilibrium assumptions can at most be *special* hypotheses and can never be accepted as a general argument for interdependency" (pp. 160-1). Cf. also J. L. Simon, 'The Concept of Causality in Economics', *Kyklos*, Vol. xxiii, 1970, Fasc. 2, pp. 226-44, who says incidentally that "in economics, a statement that is deduced from, compatible with, and logically connected into the general framework of systematic economics, is much more likely to be considered causal than is a statement that stands alone without logical connections to the body of economic theory. This is because the theoretical connection provides support for belief that the side conditions necessary for the statement to hold true are not restrictive ones and that the likelihood of 'spurious correlations' is not great" (p. 241). Analogously P. W. Bridgeman in *The Logic of Modern Physics* (New York, 1928) speaks of "the causality concept" as being "a relative one, in that it involves the whole system in which the events take place" and as applying "to sub-groups of events separated out from the aggregate of all events" (pp. 83, 91).

ships is so interpreted as to depict the interaction between variables as of a particular kind and having a certain direction; this being of considerable significance for the stability or instability of the equilibrium to which the system tends. The particular interpretation that gives the theory its essential character and its practical implications comes from introducing additional hypotheses (sometimes by imputing particular values to particular variables) that were no part of the schema in its pure form. Moreover, the mere definition of what is the proper sphere of relevant interrelations (and hence the boundaries of a theoretical system) can be crucial, as we shall see: crucial in distinguishing different ways of locating the determining influences.

Associated with the notion of economic theory as a purely formal structure, *qua* theory of general equilibrium, is that of the 'reconciling' rôle of such generalised economic analysis with respect to rival and opposed (and less general) theories that formerly agitated rival schools. This is a view of which a good deal has been heard recently, at least in certain circles, and is evidently germane to any critical examination of the history of economic thought. One example of it is the attempts made soon after the appearance of Keynes's *General Theory* to display the different emphasis and conclusions of Keynesian and pre-Keynesian doctrine as depending upon different values or 'shapes' implicitly assumed for certain parameters or generalised functional relationships (and in some cases implicit assumptions of independence). Thus the *General Theory* of the *General Theory* should represent the contending doctrines as special cases of the more comprehensive, and 'true', form of statement. What 'reconciliation', however, seems to have amounted to in this case was little more than the statement that one kind of mechanism characterised one type of situation and another kind of mechanism was appropriate to a different situation (*e.g.* where some special 'boost' factor sufficed to maintain full employment and/or full-capacity working). Perhaps a more pertinent example is the suggestion that has recently been heard (following renewed interest in the classical approach) to

the effect that there is no real opposition between what had traditionally been rival theories of value of Ricardo and Marx, on the one hand, and of Jevons and the Austrian school, on the other: in any equational system of general equilibrium (*e.g.* of Walrasian type) both quantities of labour expenditures and consumers' substitution-ratios (or marginal utilities) will need to be included, and with appropriate interpretation emphasis can be thrown upon the determinative influence of either the one or the other.\* Increasing formalisation of the subject is thus identified with increasing neutralisation, so far as the intrusion of ideological influence is concerned, and is accordingly held to exemplify the scientific progress in the subject that Schumpeter sought to find in historical examination of the march of economic analysis *per se*. If this progress in analytical techniques involved some restriction on the boundaries of the subject as compared with those more generously drawn by the classical pioneers, this was something to be applauded and not deplored: at most it was to be regarded as a cost well compensated for by the resulting gain in scientific rigour.

All that can be shortly said, I think, about such an allegedly 'neutral' corpus is that when carefully formulated and analysed it will be found to be extremely thin in factual content: *i.e.* its apparent neutrality is because it contains very little in the way of factual statement about economic situations or processes and their behaviour – so little, perhaps, as to evoke serious doubt as to whether it is entitled to rank as an economic theory at all in the sense of a theory that *explains* social action and behaviour.

\* Cf. the statement of Leif Johansen, 'Marxism and Mathematical Economics' in *Monthly Review* (New York) January 1963, p. 508: "For goods which can be reproduced on any scale . . . it is very easy to demonstrate that a complete model still leaves prices determined by the labour theory of value even if one accepts a marginal utility theory of consumers' behaviour"; and his elaboration of this point in a paper, 'Some Observations on Labour Theory of Value and Marginal Utilities', *Economics of Planning*, Vol. 3, No. 2, September 1963, pp. 89 *seq.* (where the passage from the *Monthly Review* is cited). The implication here is that *both* quantities of labour and marginal utilities enter into the equations defining equilibrium. But it is to be noted that Professor Johansen emphasises the distinction that, while prices are related to labour-expenditures (being proportional thereto when compositions of capital are equal), "the marginal utility functions interact with the prices . . . only in determining the *quantities* to be produced and consumed of the different commodities".



To qualify as the latter it must be so framed as to show how certain results or events are *determined*; and a system of equilibrium defined in terms of a set of equivalences or identities may well amount to no more than a series of tautologies.\*

To question the status of an apparently 'neutral' corpus of theory of this type is not equivalent to denying the existence of certain high-level generalisations that apply to a variety of economic situations, including even situations pertaining to different institutional systems. Marxist writers, for example, have always granted that there are general statements, even 'laws', that apply to all modes of production or socio-economic systems, or at any rate to all systems embodying a common characteristic such as commodity-production for sale in a market, and hence some form of division of labour and exchange.† Again, to take an example from modern 'growth models' (e.g. the von Neumann model), there are certain interrelationships between quantities in economic growth that will apply to any economic system, given only a minimum of common assumptions as to prices and price-flexibility, technical possibilities and supply-elasticities. But it does not follow at all that these consist merely of analytical statements about an (undefined) equilibrium of interrelated variables: if so, as we have seen, their significance for practice would be distinctly trivial, and even as a framework for more concrete statement they could probably be ignored without much loss of illumination. Restriction of this kind certainly does not apply to the type of general statement to which we have been referring, about exchange-

\* As, for example, Dr L. Pasinetti has shown profit-theories of the Irving Fisher type (in terms of 'a rate of return on capital') to be (in *Economic Journal*, Vol. xxix, No. 315, September 1969, pp. 508 *seq.*, and especially pp. 511, 525, 529).

† Oskar Lange, for example, in his *Political Economy*, Vol. 1 (Warsaw, 1963), after distinguishing "technical and balance laws of production" from "laws of human behaviour" and "laws of interplay of human actions", and emphasising that the first of these have "the widest application in history", speaks of "common economic laws" applying to "different social formations" in addition to "specific economic laws of a given social formation" (*ibid.*, pp. 58-68); citing in this connection Engels's postscript to Vol. III of *Das Kapital*, where it is stated that, since "commodity exchange appears in the period before written history", "the law of value reigned supreme for a period of time lasting from five to seven thousand years". Also cf. Marx's letter to Kugelman of 11 July 1868.