

# History of Economic Thought

## EC311

### Writing Assignment 1 School of Economic Thought

"Mainstream" economic thought shifts over time due to social (political, religious, business interests) and technical forces. Micro and macroeconomics are different perspectives with different issues and methods but they share similar values. Values currently shared include economic growth, efficiency in production, efficacy of markets, minimal government intervention, private property rights and an emphasis on those things and events that can be quantified and measured in pecuniary terms.

In microeconomics, the current mainstream can be characterized as neoclassical economics. In macroeconomics the mainstream lies between the "new classical" and Keynesian economics. It falls closer to the new classical view with overtones of monetarism and rational expectations.

A "school of economic thought" is a phrase that is often used to characterize particular approaches to economic analysis. While the concept of a school is frequently used, its specific meaning is elusive. Joseph Shumpeter (*History of Economic Analysis*, 1954, Chapter 4) describes a sociology of economics.

*The reader will have no difficulty in perceiving the relation which exists between the definition of a science as a technique that develops in a social group professionally devoted to its cultivation and the ideological aspects of the methods and results that emerge from the 'scientific' activities of such a group. Evidently there must be a certain amount of cohesion between its members, at least when the group has attained a sufficiently definite existence. A corporative spirit that produces explicit or subconscious rules according to which the members recognize each other and admit certain individuals and exclude others. In noticing a few of the phenomena to which these facts give rise we shall complete the little that can be said here on the subject of the sociology of science.*

*If it be possible at all to imagine an individual who no matter for what reason embarks for himself and by himself upon the investigation of any of those sets of phenomena that have ever become the objects of scientific efforts, it should also be possible to realize a very simple yet very fundamental truth. Our individual must first recognize the phenomena on which he is going to work and he must recognize them as being somehow connected with one another and distinct from others. This recognition is a cognitive act. But it forms no part of the analytic work. On the contrary, it supplies the object or material on which analysis works and is therefore a prerequisite of it. The analytic work itself then consists of two different though inseparable activities. The one consists of in conceptualizing the contents of the vision. By this we mean the fixing of the elements into precise concepts that receive labels or names in order to retain their identity, and in establishing relations (theorems or propositions) between them. The other consists in hunting for further empirical data (facts) with which we enrich and check the ones originally perceived. It stands to reason that these two activities are not independent of one another but that there must be an incessant give and take between them. Attempts at conceptualization invite the hunt for further facts and the new facts*

*discovered must themselves be inserted and conceptualized. In an endless sequence both activities improve, deepen, and correct the original vision and also each other's results. We do try at any given stage of our scientific endeavors to construct schemata or systems or models by which to describe as best we can the set of phenomena we are interested in, which are then developed 'deductively' or 'inductively.' But they are provisional by nature and are always relative to the stock of facts we command. This is indeed a very imperfect description of scientific procedure but it brings out a fact that will be emphasized again and again in these pages: there is not and there cannot be any fundamental opposition between 'theory' and 'fact finding,' let alone between deduction and induction. It will be one of our tasks to show why the appearance of such opposition has emerged nevertheless.*

*In practice, of course, no scientific worker ever goes through all the stages of the work beginning with an independent vision of his own. Intuitive perception of novel aspects is indeed never absent so long as science is really alive. But vision of the kind that produces novel methods or propositions or else leads to the discovery of novel facts-which then enter the science in the form of new hypotheses or restrictions- only adds to and perhaps partly displaces existing scientific structures, the bulk of which is handed from generation to generation as a matter of course. And practically always it isn't society as a whole or even a random collection of members that hands on the stock of scientific knowledge but a more or less definite group of professionals who teach the rising generations not only their methods and results but also their opinions about the directions and the means of further advance. In a majority of cases competence in doing scientific work cannot be acquired, or can be acquired only by individuals of quite exceptional originality and force. From any source other than the teaching of recognized professionals. Let us briefly glance at some of the consequences of this fact.*

*First of all, it should be observed that this social mechanism is tremendously labor saving. By means of it any beginner who follows the advice received and who does the work assigned to him acquires knowledge of facts, grasp of problems, mastery of methods with an economy of energy that should set the bulk of his force free for exploration of lands that lie beyond the boundary line at which the competence of the teacher ends. There should be no reasonable doubt about it, therefore, that primarily the social mechanism glanced at is not only favorable to the development of conceptual apparatus and to the accumulation of factual knowledge but even that it supplies the most potent motive power of what is usually referred to as scientific progress. Obviously, however, there is also another side to the medal. Teaching in any established science stereotypes the mind of the tyro and may stunt such originality as he may have. This has another and less obvious consequence. Owing to the resistance that an existing scientific structure offers, major changes in outlook and methods, at first retarded, then come about by way of revolution rather than of transformation and elements of the old structure that might be permanently valuable or at least have not yet had time to yield their full harvest of result are likely to be lost in the process. There is thus plenty of justification, just as there is for the resentments of the revolutionary, for the propensity of a certain type of mind to emphasize continuity and to defend old insights against new ones. Many examples of this will be noticed in this book.*

*Second, the fact that existing structures once established tend to persist accounts in the field of scientific endeavor as it does in others for a phenomenon that is not easy to explain, the phenomenon of 'generations.' Consider a population with constant age distribution in which moreover the number of people that enter scientific vocations are equal to the number of people who retire. A given profession, say the profession of scientific economists, would then also display a constant age distribution. It is no doubt possible to construct sub-groups whose outlooks and methods may be expected to develop and there is no problem whatever in the antagonism of these age groups that we might observe. But this is not the problem of scientific generations for we also observe that at any given time a majority of the people in all the age groups display certain similarities of attitude so that, for example, it is possible to speak of a generation of 1880-1900 and to contrast it with the generation of 1920-1940 although younger and older men presumably differed in the first period as much as they did in the second. There would be no point in this if change in methods and results proceeded at an even rate. In the case of economists one might be tempted to explain this phenomenon by the change in social and economic conditions and by the consequent change in the practical problems that attracted attention in the two periods. But we find the same phenomenon in sciences that work on invariant environments. It is precisely this which gives us the clue to the nature of the problem and at the same time to its solution. Problems and methods not only change because environments change. They also change in consequence of the [fact that the] analytic work that is embodied in a given structure of a science has a way of resisting change.*

*Third, the professionals that devote themselves to scientific work in a particular field and even all the professionals who devote themselves to scientific work in any field tend to become a sociological group. This means that they have other things in common besides the interest in scientific work or in a particular science per se. In most cases they teach the science which they are trying to bring up and to make their living by teaching. Naturally, this will tend to evolve a social and economic type. The group accepts or refuses to accept co-workers also for reasons other than their professional competence or incompetence. In economics this grouping took long to mature but when it did mature it acquired much greater importance than it did in physics. We shall see how in most countries writers on economic topics hail from all sectors of society. There were indeed factors that made for grouping at an early time, the most important instance being the Catholic scholastic doctors, but all the rest consisted of types that came from anywhere in the scales of social rank or of income brackets. In England, this was so even in the first half of the nineteenth century. In such cases we must use the word profession with a proviso. In England there was at the time indeed a profession of economists in the sense that there were writers on economic topics who mutually recognized their professional competence. But later on the association of scientific work with teaching produced an economic profession in a fuller sense of the word and this economic profession developed attitudes to social and political questions that were similar also for reasons other than similar scientific views. This similarity of conditions of life and of social location produced similar philosophies of life and similar value judgements about social phenomena. It would be unnecessary to dwell on the consequences of this were it not for the fact that it was closely associated with the phenomenon of scientific schools. [Shumpeter, pp. 45-47]*

Larry Laudan in *Progress and Its Problems* [1977] uses Thomas Kuhn's "scientific paradigm and Imre Lakatos' "scientific research programmes" to develop a concept of "research traditions." He begins by characterizing the use of the term 'theories.'

*In the standard literature on scientific inference, as well as in common scientific practice, the term "theory: refers to (at least) two very [different] types of things. We often use the term "theory" to denote a very specific set of related doctrines (commonly called "hypotheses" or "axioms" or "principles") which can be utilized for making specific experimental predictions and for giving detailed explanations of natural phenomena. Examples of this type of theory would include Maxwell's theory of electromagnetism, the Bohr-Kramers-Slater theory of atomic structure, Einstein's theory of the photoelectric effect, Marx's labour theory of value, Wegner's theory of continental drift, and the Freudian theory of the Oedipal complex.*

*By contrast, the term "theory" is also used to refer to much more general, much less easily testable, sets of doctrines or assumptions. For instance, one speaks about "the atomic theory," or "the theory of evolution," or "the kinetic theory of gases." In each of these cases, we are referring not to a single theory, but to a whole spectrum of individual theories." [Laudan, pp. 71-72]*

After discussing Kuhn's concept of "paradigms" and Lakatos' "scientific research programmes," Laudan describes the nature of "research traditions."

*We have already referred to a few classic research traditions: Darwinism, quantum theory, the electromagnetic theory of light. Every intellectual discipline, scientific as well as nonscientific, has a history replete with research traditions: empiricism and nominalism in philosophy, voluntarism and necessitarianism in theology, behaviorism and Freudianism in psychology, utilitarianism and intuitionism in ethics, Marxism and capitalism in economics, mechanism and vitalism in physiology, to name only a few. Such research traditions have a number of common traits;*

- 1. Every research tradition has a number of specific theories which exemplify and partially constitute it: some of these theories will be contemporaneous others will be temporal successors of earlier ones;*
- 2. Every research tradition exhibits certain metaphysical and methodological commitments which, as an ensemble, individuate the research tradition and distinguish it from others;*
- 3. Each research tradition (unlike a specific theory) goes through a number of different, detailed (and often mutually contradictory) formulations and generally has a long history extending through a significant period of time. (By contrast, theories are frequently short-lived.)*

*These are by no means the only important characteristics of research traditions, but they should serve, for the time being, to identify the kinds of objects whose properties I would like to explore.*

*In brief, a research tradition provides a set of guidelines for the development of specific theories. Part of those guidelines constitute an ontology which specifies, in a general way, the types of fundamental entities which exist in the domain or domains within which the research tradition is embedded. The function of specific theories within the research tradition is to explain all the empirical problems in the domain by "reducing" them to the ontology of the research tradition. If the research tradition is behaviorism, for instance, it tells us that the only legitimate entities which behavioristic theories can postulate are directly and publicly observable physical and physiological signs. If the research*

*tradition is that of Cartesian physics, it specifies that only matter and minds exist, and that theories which talk of other types of substances (or of "mixed" mind and matter) are unacceptable. Moreover, the research tradition outlines the different modes by which these entities can interact. Thus, Cartesian particles can only interact by contact, not by action-at-a-distance. Entities, within a Marxist research tradition, can only interact by virtue of the economic forces influencing them.*

*Very often, the research tradition will also specify certain modes of procedure which constitute the legitimate methods of inquiry open to a researcher within that tradition. These methodological principles will be wide-ranging in scope, addressing themselves to experimental techniques, modes of theoretical testing and evaluation and the like. ...Put simplistically, a research tradition is thus a set of ontological and methodological "do's" and "don'ts." To attempt what is forbidden by the metaphysics and methodology of a research tradition is to put oneself outside that tradition and to repudiate it. ... By breaking with the ontology or the methodology of the research tradition within which he has worked, he has violated the strictures of that research tradition and divorced himself from it. Needless to say, that is not necessarily a bad thing....*

*Although it is vital to distinguish between the ontological and methodological components of a research tradition, the two are often intimately related, and for a very natural reason; namely, that one's views about the appropriate methods of inquiry are generally compatible with one's views about the objects of inquiry....*

*So a preliminary, working definition of a research tradition could be put as follows: a research tradition is a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain. [Laudan, pp. 78-81]*

George Stigler comments that;

*A school within a science is a collection of affiliated scientists who display a considerably higher degree of agreement upon a particular set of views that the science as a whole displays. It is essential to a school that there be many scientists outside it, or the school would have no one with whom to argue. Schools have received little study, and the following remarks are only casual impressions*

*A school must have a leader, because the consensus of its members will normally be achieved and maintained by major scientific entrepreneurs. In some instances, such as the Ricardian school, the chief bond has, in fact, been the admiration for the leader. I doubt whether a scientific school based on substantive scientific views can long survive the death of its leader, except in the improbable event of the appearance of a new leader of comparable stature. ...*

*If the school is united on methodology rather than substantive doctrines, its life will be longer, but less influential. A methodology is usually not very confining with respect to substantive questions; so new members of the school can more easily adjust to new problems and new challenges. ....*

*A school may be based upon policy views rather than upon economic analysis or scientific methodology, and then its life will normally become even longer. Marxism is perhaps as much a political party as a school, but its longevity as a school is due to the fact that it is not a scientific body of knowledge (although its works have scientific content).*

The concept of a school of thought is a concept that is used to organize thoughts about the nature and structure of economic analysis. It is a loose, intuitive concept that shifts depending on its use. In many ways it is a stereotype that describes the ideological and methodological nature of a group. It may be used to describe the general nature of the problems addressed or the conclusions or policy recommendations of a related group of economists.

Using the notion of a "school of economic" thought, write a short paper (about 10 pages + bibliography and references) about a recognized school.

Your hypothesis should:

1. relate the school to the reasons for the emergence of the school, or
2. relate the role of the school to the development of mainstream economics, or
3. relate the importance of the school to a particular period of history, or
4. identify the role of events in shaping the nature of the school, etc.

The body of the paper should include:

1. the important elements of the school,
2. its ideological position,
3. the forces that contributed to its development (or demise),
4. the major contributors to the school,
5. the "hard core" of the school (using Lakatos),
6. the major tenets of the school,
7. major detractors,
8. policy recommendations
9. and other elements you feel are important to understanding the nature of the school.