

Milton Friedman and Monetarisms
Part 1
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1. Introduction.

In this paper I trace the intellectual history of Milton Friedman and his relationship with the so-called monetarist doctrine. There are several senses to this epithet - which I distinguish below - as there are several dimensions in Friedman's economic theory. Some of them are very well connected, and some of them are independent of each other. Also one can distinguish between Friedman's doctrinaire influence and action on economic policies, which was outstanding with its equally outstanding research impact for which he was awarded the John Bates Clark Medal (1951) and the Nobel Prize (1976).

2. Intellectual Origins of Milton Friedman.

Coming from an immigrant Jewish family of Hungarian origin, his parents were not educated and had permanent economic difficulties. His father died when he completed high-school, but, in spite of that, he was encouraged by his mother and sister to complete his studies on his own account. Following that, he supported himself with a mix of part-time jobs and fellowships. He took his Bachelor degree from Rutgers University, a public university of New Jersey. He took two Majors, one in Mathematics and another in Economics. His first project was to attend graduate courses toward a degree in Actuarial Science. Having received two alternative opportunities through fellowships - one for studying Mathematics at Brown University and the other for studying Economics at the University of Chicago - he readily chose the latter. This was in 1932, in the middle of the Great Depression, when one fourth of the labor force was unemployed.

According to his own account, the major intellectual influences in his Rutgers (1928-32) and Chicago (1932-33) years were from his professors. At Rutgers Friedman attended classes with Arthur Burns (then doing his doctoral dissertation for Columbia) and Homer Jones (then doing his doctoral dissertation for Chicago). At Chicago, his professors were Frank Knight, Jacob Viner, Henry Schultz, and Lloyd Mints.

Burns¹ "shaped my understanding of economic research, introduced me to the highest scientific standards, and became a guiding influence on my subsequent career." (Friedman, 1976), whereas Jones² "introduced me to rigorous economic theory, made economics exciting and relevant, and encouraged me to go on to graduate work.... Arthur

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¹ Arthur Burns co-authored, with Wesley C. Mitchell, the classic *Measuring Business Cycle*. He also was the main influence in economic matters in the Eisenhower (1953-61) and the Nixon (1969-74) administrations, either as a member or as the chairman of the Council of Economic Advisers (1953-56). Besides that, he was chairman of the Federal Reserve Bank (1970-78).

² Holmer Jones became research director of the Federal Reserve Bank of St. Louis and was responsible for its "St. Louis model" based in the Fisher equation.

Burns and Homer Jones remain today among my closest and most valued friends" (Friedman, 1976).

Knight³, Viner⁴, Schultz⁵, and Mints⁶ were bright scholars. Together with his graduate classmates, who had come from a wide variety of countries, Friedman found a very cosmopolitan milieu there in 1932-33. He also met Rose Director, with whom he married some years later, and formed a very prolific research partnership. He was also a pupil in Paul Douglas's⁷ course. Fundamentally, however, as in his own words:

"My teachers, who were Jacob Viner, Frank Knight, and Lloyd Mints, taught us that what was going on was a disastrous mistake by the Federal Reserve in reducing the money supply. It was not a natural catastrophe, it was not something that had to happen, it was not something which had to be allowed to run its course. There were things which should be done. Jacob Viner, from whom I took my first course in pure economic theory as a graduate, had given a talk in Minnesota in which he very specifically called for expansive policy on the part of the Federal Reserve and the government. Therefore the Keynesian Revolution didn't come as a sudden light from the dark showing what you could do about a situation that nobody else seemed to know how to do anything about." (Snowdon & Vane, 1999, p. 125).

Henry Schultz, who was a friend of Harold Hotelling⁸, got a fellowship for Milton Friedman in Columbia, in order to proceed in his graduate courses, after he had finished his MA in Chicago. That was in 1934. In 1976, for his Nobel autobiography, Friedman wrote:

"The year at Columbia widened my horizons still further. Harold Hotelling did for mathematical statistics what Jacob Viner had done for economic theory: revealed it to be an integrated logical whole, not a set of cook-book recipes. He

³ Frank Knight wrote the classic *Risk, Uncertainty and Profit* (Knight, 1921). where, among other breakthroughs, he anticipated the Keynesian notion of uncertainty as differentiated from the concept of risk. Nowadays this notion of uncertainty is referred as Knightian uncertainty, which is based on an absolute ignorance of the future.

⁴ Jacob Viner was, according to Blaug (1985, p. 256) "the greatest historian of economic thought that ever lived". He wrote the classics *Studies in the Pure Theory of International Trade* (Viner, 1937), *The Customs Union Issues* (Viner, 1950), where he formulated the twin concepts of "trade creation" and "trade diversion", and *Cost Curves and Supply Curves*, where he clarified the Marshallian cost curves for the modern student.

⁵ Henry Shultz wrote *The Theory and Measurement of Demand* (1938) which is a classic. Friedman, who would be his research assistant while he was writing this book some years later, did not have a high regard for his classes, although, retrospectively acknowledged his contribution. See Friedman & Friedman (1998).

⁶ Lloyd Mint (1950) was one of the leading scholars in this period in the so-called Chicago School of Economics.

⁷ Paul Douglas wrote the *The Theory of Wages* (1934), formulated with Charles W. Cobb the famous Cobb-Douglas production function, and was elected a U.S. Senator as a Democrat for the state of Illinois, in 1948. He remained a Senator until 1966. For a brief biography see Blaug (1985, pp. 53-55).

⁸ Harold Hotelling, author of a very few but very much influential articles (Hotelling, 1929, 1931, 1938) was a "pioneer of mathematical economics and the inventor of many statistical methods now found in textbooks on multivariate analysis..." (Blaug, 1985, p. 97).

also introduced me to rigorous mathematical economics. Wesley C. Mitchell, John M. Clark and others exposed me to an institutional and empirical approach and a view of economic theory that differed sharply from the Chicago view. Here, too, an exceptional group of fellow students were the most effective teachers.

Wesley C. Mitchell⁹ was, of course, the great founder of a long tradition in business cycle research in the US which yielded the diffusion indexes while John M. Clark¹⁰ was also a business cycle researcher. Coming back to Chicago one year later, he assisted Henry Schultz in his work on the measurement of demand. There he met George Stigler, who would be a lifetime friend and future colleague at the Chicago faculty¹¹ and W. Allen Wallis with whom he also became a lifetime friend.¹²

The first paper by Friedman was published in the *Quarterly Journal of Economics* in 1935.¹³ When Wallis went to Washington, DC, in 1935, Friedman went after him to work in the Natural Resources Committee, where he participated in the design of a large study of consumption and budget. This professional experience in the government was one of the elements which led him in the direction of the permanent income concept which would eventually yield his work *A Theory of the Consumption Function*.

"The other came from my next job - at the National Bureau of Economic Research, where I went in the fall of 1937 to assist Simon Kuznets in his studies of professional income. The end result was our jointly published *Incomes from Independent Professional Practice*, which also served as my doctoral dissertation at Columbia. That book was finished by 1940, but its publication was delayed until after the war because of controversy among some Bureau directors about our conclusion that the medical profession's monopoly powers had raised substantially the incomes of physicians relative to that of dentists. More important, scientifically, that book introduced the concepts of permanent and transitory income." (Friedman, 1976).

During the war, Friedman worked first (1941-1943), at the U.S. Treasury Department, "on wartime tax policy", and secondly (1943-45) at Columbia University "in a group headed by Harold Hotelling and W. Allen Wallis, ... as a mathematical statistician on problems of weapon design, military tactics, and metallurgical experiments." (Friedman, 1976). While at the Treasury Friedman commented a paper by William Salant, published in the AER. (Friedman, 1942). This was the first Friedman's writing on inflation. It was written some two years after he had read *The General Theory*.¹⁴ After its publication in the

⁹ His most known works are Mitchell (1927) and Burns & Mitchell (1946).

¹⁰ Author of Clark (1935).

¹¹ Stigler was contemplated with the Nobel Prize of Economics in 1982.

¹² W Allen Wallis became later adviser in economic affairs in several Republican administrations and *Under Secretary of State for Economic Affairs* in Reagan's years.

¹³ Friedman (1935).

¹⁴ The following was drawn from Snowdon & Vane (1999, pp. 125-26). "S&V: Can you recall when you first read the *General Theory* (1936) and what your impression were of the book? *Friedman*: I can't really answer that, I may be able to tell you if I look in my original copy of the *General Theory* as I sometimes had a habit

AER, he reprinted that comment with substantial modifications as *Discussion of the Inflationary Gap* in Friedman (1953).

So, we can demarcate the formative intellectual years of Milton Friedman as the period between 1929 - when he enrolled at Rutgers - and 1945 - when he got his PhD from Columbia University. These were - as the Chinese say - "interesting years", encompassing the Great Depression (1929-33), the New Deal, and World War II.

3. Early Criticisms to 20th Century Emerging Theories.

After the war, he published two articles criticizing theoretical tendencies that emerged at that time. In Friedman (1946) he criticizes, strictly on methodological grounds, the book by Lange (1944). The latter is a very good example of the general equilibrium approach. So the criticism that Friedman makes there may be interpreted as his main objections to what he will subsequently call "erroneous general equilibrium analysis". We will return to this topic below. In Friedman (1947) the subject of his criticism is the book by Abba Lerner (1944).

The latter was a very influential book in this period. In some respects, it epitomizes the spirit of the emerging post-war technocratic vision in mainstream economics in the US. This kind of vision was consistent with the dominant assumptions of the period in which the Keynesian posture was orthodoxy - from the end of WW II to the end of the 1960s. The point of departure in Lerner's book are the technical conditions for the maximization of social welfare, which at the time had already been shown by Pareto (1897) and Barone (1908), and were being discussed in Lange & Taylor (1938). Were these conditions capable of being implemented in a collectivist society? The conditions were satisfied in a decentralized economy with perfect competition. According to Friedman "Lerner writes as if it were possible to base conclusions about appropriate institutional arrangements almost exclusively on analysis of the formal conditions for an optimum. Unfortunately, this cannot be done." (Friedman, 1947, in Friedman, 1953, p. 316).

The grand vision of Lerner's book is that in the "controlled economy" - a precursor of what Samuelson would call later the "mixed economy"¹⁵ - the following pragmatic principle should hold:

"The fundamental point of the controlled economy is that it denies both collectivism and private enterprise as principles for the organization of society, but recognizes both of them as perfectly legitimate means. Its fundamental principle of organization is that in any particular instance the means that serves society best should be the one that prevails. Perhaps a better name would be the *service economy*, since it is the question of which method *serves best* that determines which is to be used." (Lerner, 1944, p. 5).

of marking in my books the date when I bought them and how much money I paid for them. Yes here it is. I bought it in 1938 and paid \$ 1.80 cents for it (*laughter*). That's probably when I first read it but I can't remember my impressions, it's a long long time ago, but I do remember that in the early 1940s I wrote a book review in which I was very critical of the Keynesian analysis contained in the book that I reviewed.

¹⁵ For instance, Samuelson (1973, p. 43).

According to Friedman:

"Much of *The Economics of Control* is devoted to presenting the formal reasoning underlying this broad result (Pareto optimality) and to developing in detail its implications for various sectors of the economy - the allocation of goods among consumers, the allocation of resources among industries, ... This part (price theory) of the book is novel in exposition, though not in substance. Motivated by the question how society ought to work rather than how it does work, Lerner puts primary emphasis on the human wants and technical possibilities to which society must adjust rather than on the market expression of these wants and possibilities. The result is a highly unusual organization of topics... The exposition is novel not only in organization but also in style. Most of it is entirely abstract, yet Lerner uses graphs sparingly and mathematics not at all. He uses words, abbreviated substitutes for words, and simple arithmetical examples. The resulting exposition seems to the reviewer to have most of the disadvantages of a strictly mathematical exposition (it is abstract and artificial and requires sustained attention and retention of symbols) and none of the advantages (it is neither brief nor rigorous). (Friedman, 1947, in Friedman, 1953, pp. 303-04.)

Friedman's first major attempt to a positive contribution - after his work with Kuznetz - was in partnership with Leonard J. Savage (Friedman & Savage, 1948). In this work they formalized a model to explain why some individuals may be risk-averse in a range of income and show a preference for gambling in other ranges of income.¹⁶ Following that, his next publications were an article (Friedman, 1949) and his book (Friedman, 1953) on Positive Economics.

In the article on the Marshallian Demand, he shows that there were two interpretations of the Marshallian demand curve. One that included, in the *ceteris paribus* clause, (1) the prices of every other commodity; (2) tastes and preferences of the group of consumers considered, and (3) their money income. This he called "the current interpretation". The other one included in the *ceteris paribus* clause the (1) prices of every closely related commodity ; (2) tastes and preferences of the group of consumers considered, and (3) the real income, instead of the money income. This he called "the alternative interpretation".

The "current interpretation" involves the so called "income-effect" and the "substitution-effect" which add up to the "total-effect" of a *ceteris paribus* price-change or money income change. At the time Friedman wrote this article (1948) this interpretation was already accepted by the "mainstream". In his article he tries to persuade the reader that the "alternative" interpretation is more consistent with (i) the spirit and the letter of Marshall's writings and (ii) the overall textual evidence in Marshall's work. He makes a very detailed exegesis of the several editions of the *Principles*, concluding that the only evidence there in favour of the "current interpretation" is in the 3rd (and last) edition in the

¹⁶ For a critical review of this work see Hirsleifer & Riley (1992, pp. 26-28)

commentary about Giffen goods. He considers this to be totally inconsistent with the previous work, and attributes this to the influence of the "current interpretation" on Marshall's own thought.

He recognizes the legitimacy of the current interpretation and attributes it to the vagueness and imprecision in Marshall's writings. But he also accuses the authors that were responsible for this interpretation¹⁷ - including Schultz, Samuelson, Triffin, Joan Robinson, and even he himself - of a lack of attention in reading what Marshall had really meant. In his discussion in this article he raises methodological questions that will be more thoroughly analysed in Friedman (1953). One relevant comment is the following:

"The distinction commonly drawn between Marshall and Walras is that Marshall dealt with `partial equilibrium`, Walras with `general equilibrium`. This distinction is, I believe, false and unimportant. Marshall and Walras alike dealt with general equilibrium; partial equilibrium analysis as usually conceived is but a special kind of general equilibrium analysis - unless, indeed, partial equilibrium analysis is taken to mean erroneous general equilibrium analysis...The important distinction between the conceptions of economic theory implicit in Marshall and Walras lies in the purpose for which the theory is constructed and used. To Marshall...economic theory is `an engine for the discovery of concrete truth`....Economic theory, in this view, has two intermingled roles: to provide `systematic and organized methods of reasoning` about economic problems; to provide a body of substantive hypotheses, based on factual evidence, about the `manner of action of causes`. In both roles the test of the theory is its value in explaining facts, in predicting the consequences of changes in the economic environment. Abstractness, generality, mathematical elegance - these are all secondary, themselves to be judged by the test of the application. The counting of equations and unknowns is a check on the completeness of reasoning, the beginning of analysis, not an end in itself." (Friedman, 1949: in Friedman 1953, pp. 90-91).

4. The Methodological Issues.

In *Essays in Positive Economics* (1953), he collects several previously published articles - including all those above mentioned, except his work with Kuznetz and the one with Savage - and a new one, dealing exclusively with methodological questions. This is Part One - Introduction (*The Methodology of Positive Economics*) where his main thesis on this subject is thoroughly presented.

In this essay Friedman (1953) begins with a laudatory quotation of John Neville Keynes¹⁸ (1891) where the latter makes a distinction between "a *positive science*" and "a *normative or regulative science*", the first dealing with "what is" and the second dealing

¹⁷ See Friedman, 1949, note 41.

¹⁸ John Maynard Keynes' father and, also, a philosopher and economist of Cambridge. He was a friend of Alfred Marshall. He was born in 1852 and died in 1949, after his son, who died in 1946.

with what "ought to be". After this distinction he gives examples - the effects of the introduction of a minimum wage and of stabilization policies - where

"...currently in the Western world, especially in the United States, differences about economic policy among disinterested citizens derive predominantly from different predictions about the economic consequences of taking action - differences that in principle can be eliminated by the progress of positive economics - rather than from fundamental differences in basic values, differences about which men can ultimately only fight." (Friedman, 1953, p. 5).

In *The Methodology of Positive Economics* Friedman's main aim is the formulation of rules, within a Popperian conception of logical positivism, for the implementation of tests of hypotheses' falsification in the field of economics. In this essay of 1953 Friedman does not quote Karl Popper, although he discusses what several of great economists of the past and their contemporaries thought on methodological issues in economics, statistics, and econometrics.¹⁹ This is easy to explain. Although Popper (1945) had already been translated to English, the book where he presents his first formulation in the theory of knowledge (*The Logic of Scientific Discovery*) would be translated only in 1959. But Friedman knew very well Karl Popper and his ideas, since they discussed them at least on an annual basis at the Mont Pelerin Society, as Stigler (1988, cap. 9) shows.

The Methodology of Positive Economics is divided into six sections. The first examines the relation between positive and normative economics. In this regard Friedman states his optimistic belief²⁰ - already shown in the quotation above - that

"a consensus on 'correct' economic policy depends much less on the progress of normative economics proper than on the progress of a positive economics yielding conclusions that are, and deserve to be, widely accepted. It means also that a major reason for distinguishing positive economics sharply from normative economics is precisely the contribution that can thereby be made to agreement about policy." (Friedman, 1953, pp. 6-7).

The second section deals with positive economics. Friedman analyses the content and the form of what a scientific theory (positive science) is. Its main ingredients are a "language" and a set of "hypotheses" which are not truistic. "Viewed as a language, theory has no substantive content; it is a set of tautologies" (Friedman, 1953, p. 7). On the other hand,

"Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to 'explain'. Only factual evidence can show whether it is 'right' or 'wrong' or, better, tentatively 'accepted' as valid or 'rejected'. As I shall argue at greater length below, the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are

¹⁹ This point is raised by Blaug, 1980, p. 104.

²⁰ For a critical assessment of Friedman's position on this issue see Blaug (1980, chapter 5).

contradicted (frequently or more often than predictions from an alternative hypothesis): it is accepted if its predictions are not contradicted; great confidence is attached to it if it has survived many opportunities for contradiction. Factual evidence can never prove a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been confirmed by experience." (Friedman, 1953, pp. 8-9).

But since "observed facts are necessarily finite in number; possible hypotheses, infinite" (Friedman, 1953, p. 9) he suggests some principles to discriminate among the latter. These - drawn from accumulated experience - are "simplicity" and "fruitfulness". "Logical completeness and consistency are relevant but play a subsidiary role.." (Friedman, 1953, p. 10). The problem with social sciences - generated by the impossibility of performing laboratory's experiments - is not an unsurmountable obstacle ["no experiment can be completely controlled, and every experience is partly controlled" (Fried., idem, p. 10)] but it produces a higher degree of complexity in the design and the interpretation of tests. There are instances in which even this complexity is lowered by the directness, dramaticity, and convincingness of the evidence as it is in the case of "inflations on the hypothesis that a substantial increase in the quantity of money within a relatively short period is accompanied by a substantial increase in prices." (Fried., idem, p. 11).

Normally the task is more difficult. This fosters "a retreat into purely formal or tautological analysis". (Fried., idem, p. 11) . A footnote (number 9) inserted in the text on this page refers to Lange (1944). Then, Friedman goes on to write:

"But economic theory must be more than a structure of tautologies if it is to be able to predict and not merely describe the consequences of action; if it is to be something different from disguised mathematics. And the usefulness of the tautologies themselves ultimately depends, as noted above, on the acceptance of the substantive hypotheses that suggest the particular categories into which they organize the refractory empirical phenomena." (Friedman, 1953, pp. 11-12)

Following that Friedman discusses the role of empirical evidence in the process of constructing and testing the validity of hypotheses. These two stages are interrelated by successive and joint steps in which the results of a second stage (validation) serve as inputs to the generation of new hypotheses for a new first stage, and so on. This is an "apparently straightforward process", but the above mentioned complexity in the social sciences

"makes it tempting to suppose that other, more readily available, evidence is equally relevant to the validity of the hypothesis - to suppose that hypotheses have not only implications but also assumptions and that the conformity of these assumptions to reality is a test of the validity of the hypothesis *different from* and *additional to* the test by implications. This widely held view is fundamentally wrong and productive of much mischief." (Friedman, 1953, p.14).

In the third section of the essay Friedman answers the question "Can a hypothesis be tested by the realism of its assumptions?". In order to do this he constructs three examples - one from physics, one from biology, and one from human behavior. In all of them he shows that tests that result in the acceptance of the tested hypotheses may be explained by totally unrealistic assumptions and that the answer to the posed question is no.

After the description of his third example - "predictions of shots made by an expert billiard player" explained by the "hypothesis that the billiard player made his shots *as if* he knew the complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles, etc, describing the location of the balls, could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas" (Friedman, 1953, p. 21) - he goes on to write:

"It is only a short step from these examples to the economic hypothesis that under a wide range of circumstances individual firms behave *as if* they were seeking rationally to maximize their expected returns... and had full knowledge of the data needed to succeed in this attempt; *as if*, that is, they knew the relevant cost and demand functions, calculated marginal cost and marginal revenue from all actions open to them, and pushed each line of action to the point at which the relevant marginal cost and marginal revenue were equal. Now, of course, businessmen do not actually and literally solve the system of simultaneous equations in terms of which the mathematical economist finds it convenient to express this hypothesis, any more than ... billiard players explicitly go through complicated mathematical calculations... The billiard player, if asked how he decides where to hit the ball, may say that he 'just figures it out' but then also rubs a rabbit's foot just to make sure; and the businessman may well say that he prices at average cost, with of course some minor deviations when the market makes it necessary. The one statement is about as helpful as the other, and neither is a relevant test of the associated hypothesis." (Friedman, 1953, pp. 21-22).

According to Friedman, the confidence that one has in the hypothesis of maximization of returns²¹ is based on "evidence of a very different character." In his words:

"Let the apparent immediate determinant of business behavior be anything at all - habitual reaction, random chance, or whatnot. Whenever this determinant happens to lead to behavior consistent with rational and informed maximization of returns, the business will prosper and acquire resources with which to expand; whenever it does not, the business will tend to lose resources and can be kept in existence only by the addition of resources from outside. The process of 'natural selection' thus helps to validate the hypothesis - or, rather, given

²¹ This discussion on the hypothesis of maximization of returns is motivated by a reference that he had made (p. 15 and note 13) to a "lengthy discussion on marginal analysis in the AER some years ago". (Friedman, 1953, p. 15). The participants in that discussion were Lester (1946, 1947), Machlup (1946, 1947), Stigler (1947), Oliver (1947), and Gordon (1948).

natural selection, acceptance of the hypothesis can be based largely on the judgement that it summarizes appropriately the conditions for survival. " (Friedman, 1953, p. 22).

"An even more important body of evidence for the maximization-of-returns hypothesis is experience from countless applications of the hypothesis to specific problems and the repeated failure of its implications to be contradicted. This evidence is extremely hard to document; it is scattered in numerous memorandums, articles, and monographs concerned primarily with specific concrete problems rather than with submitting the hypothesis to test. Yet the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and be widely accepted, is strong indirect testimony to its worth. The evidence *for* a hypothesis always consist of its repeated failure to be contradicted, continues to accumulate so long as the hypothesis is used, and by its very nature is difficult to document at all comprehensively. It tends to become part of the tradition and folklore of a science revealed in the tenacity with which hypotheses are held rather than in any textbook list of instances in which the hypothesis has failed to be contradicted". (Friedman, 1953, pp. 22-23).

Following that, in the fourth section (*The Significance and Role of the "Assumptions" of a Theory*), he discusses two of the the three positive roles that, in his view, the "assumptions" play : "(a) they are often an economical mode of describing or presenting a theory; (b) they sometimes facilitate an indirect test of the hypothesis by its implications; and (c) , as already noted, they are sometimes a convenient means of specifying the conditions under which the theory is expected to be valid." (Friedman, idem, p. 23).

In the fifth section (*Some Implications for Economic Issues*) Friedman initially criticizes several works and, finally, states his positive contributions. In both parts he critically analyzes the economic literature of the time in its methodological aspects. In the first part he defends orthodox economic theory from the criticisms that were made to it based, in his view, on the confusion between the realism of assumptions and analytical relevance. In the second part he discusses the method used by Marshall (1920), clarifying some points that were not well understood by his critics, and makes a comparison between the approaches of imperfect competition in Chamberlin (1950) and Robinson (1933) with the Marshallian tradition of analysis, concluding by the superior "scientific" maturity of the latter.

The criticisms of orthodox theory which are discussed by Friedman came from several authors. Both Veblen (1898) and Oliver (1947) have the same argument; they satirize the *homo oeconomicus* designed in the assumptions of economic theory as a total distortion of how men and businessmen behave. Friedman`s answer is that since this is "not supplemented by evidence that a hypothesis differing in one or another of these respects from the theory being criticized yields better predictions for as wide a range of phenomena"²⁰ it is beside the point.

With reference to Henderson (1938), Andrews & Meade (1938), Harrod (1939), Hall & Hitch (1939), Lester (1946) and Gordon (1948), which criticize "the maximization-of-returns hypothesis on the grounds that businessmen do not and indeed cannot behave as the theory `assumes` they do"²² and who do present evidence Friedman dismisses this evidence. In his words:

"The evidence cited to support this assertion is generally taken either from the answers given by businessmen to questions about the factors affecting their decisions - a procedure for testing economic theories that is about on a par with testing theories of longevity by asking octogenarians how they account for their long life - or from descriptive studies of the decision-making activities of individual firms. Little if any evidence is ever cited on the conformity of businessmen`s actual market behavior - what they do rather than what they say they do - with the implications of the hypothesis being criticized, on the one hand, and of an alternative hypothesis, on the other." (Friedman, 1953, p. 31)

Then Friedman argues that realism - in the sense of descriptive accuracy - cannot be an attribute of a theory. The attempt to describe all the details of a phenomenon "is certain to render a theory utterly useless."²³ As he had argued before what a theory in its simplified description (abstract representation) of reality must capture are the relevant aspects of the phenomenon to be explained. "Why is it more `unrealistic` in analyzing business behavior to neglect the magnitude of businessmen`s costs than the color of their eyes ? The obvious answer is because the first makes more difference to business behavior than the second;..." (Friedman, 1953, pp. 32-33).

After illustrating the confusion between analytical relevance and descriptive accuracy with an example drawn from an article published in the AER²⁴, he writes:

"A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure.... If a class of `economic phenomena` appears varied and complex, it is, we must suppose, because we have no adequate theory to explain them.... A theory is the way we perceive `facts` and we cannot perceive `facts` without a theory." (Friedman, 1953, p. 34).²⁵

²² Friedman, 1953, p. 31.

²³ Friedman, 1953, p. 32.

²⁴ He quotes Alexander (1951) on the business cycle : "economic phenomena are varied and complex, so any comprehensive theory of the business cycle that can apply closely to reality must be very complicated." (Alexander, 1951, p. 872, Apud Friedman, 1953, p. 33).

²⁵ At the end of this paragraph he quotes John Stuart Mill`s "justly ridiculed statement" as being in the same class of the Alexander quotation of note 24: "happily, there is nothing in the laws of value which remains (1848) for the present or any future writer to clear up; the theory of the subject is complete". (Mill ,1929, p. 436. Apud Friedman, 1953, p. 34).

In the second part of this fifth section Friedman argues that this confusion between analytical relevance and descriptive accuracy is also present in the emergence of models of imperfect competition. In defending the use of such models economists argue that Marshall assumed "perfect competition" and so his analysis should be replaced by the more realistic assumptions of "imperfect competition" or "monopolistic competition". In response Friedman writes:

"The reader will search long and hard - and I predict unsuccessfully - to find in Marshall any explicit assumption about perfect competition or any assertion that in a descriptive sense the world is composed of atomistic firms engaged in perfect competition. Rather he will find Marshall saying: 'At one extreme are world markets in which competition acts directly from all parts of the globe; and at the other those secluded markets in which all direct competition from afar is shut out, though indirect and transmitted competition may make itself felt even in these; and about midway between these extremes lie the great majority of the markets which the economist and the business man have to study'. Marshall took the world as it is; he sought to construct an 'engine' to analyse it, not a photographic reproduction of it." (Friedman, 1953, pp. 34-35).

Friedman goes on describing Marshall's procedure. The hypothesis that he constructs is that in many instances it is useful to group firms into what he called "industries". The basic criterion of this grouping is the dominance of similarities over differences. The problems he wanted to deal with were shocks from demand or the supply of factors and their effects on these industries and firms. In Friedman's words:

"The abstract model corresponding to this hypothesis contains two 'ideal' types of firms: atomistically competitive firms, grouped into industries, and monopolistic firms. A firm is competitive if the demand curve for its output is infinitely elastic with respect to its own price for some price and all outputs, given the prices charged by all other firms; it belongs to an 'industry' defined as a group of firms producing a single 'product'. A 'product' is defined as a collection of units that are perfect substitutes to purchasers so the elasticity of demand for the output of one firm with respect to the price of another in the same industry is infinite for some price and some outputs. A firm is monopolistic if the demand curve for its output is not infinitely elastic at some price for all outputs. If it is monopolist, the firm is the industry." (Friedman, 1935, p. 35).

In commenting such a Marshallian procedure, Friedman enumerates the methodological elements which he developed in the former sections of this essay and which are exemplified by it. There is no descriptive purpose in the ideal types. They are constructed in order "to isolate the features that are crucial for a particular problem."²⁶ The hypothesis consists not only of the abstract model but also of the rules used in defining its elements. "No observed demand curve will ever be precisely horizontal, so the estimated elasticity will always be finite. The relevant question always is whether the elasticity is

²⁶ Friedman (1953, p. 36).

`sufficiently` large to be regarded as infinite..."²⁴. Also a firm may be treated as a competitive one in one problem, and as a monopolist one, in another problem. All depends on the definitions which are designed according to particular problems.

Friedman identifies the source of misconception with reference to Marshall in his analysis in which "a group of firms is affected by common stimuli, and in which the firms can be treated *as if* they were perfect competitors."²⁷ He concedes that

"It should be highly desirable to have a more general theory than Marshall's, one that would cover at the same time both those cases in which differentiation of product or fewness of numbers makes an essential difference and those in which it does not...To perform this function, the more general theory must have content and substance; it must have implications susceptible to empirical contradiction and of substantive interest and importance. The theory of imperfect or monopolistic competition developed by Chamberlin and Robinson is an attempt to construct such a more general theory. Unfortunately, it possesses none of the attributes that would make it a truly useful general theory. Its contribution has been limited largely to improving the exposition of the economics of the individual firm and thereby the derivation of implications of the Marshallian model, refining Marshall's monopoly analysis, and enriching the vocabulary available for describing industrial experience." (Friedman, 1953, p. 38).

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²⁷ Friedman (1953, pp. 37-38)

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