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## Friedman's Methodological Stance and Popper's Situational Logic

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### 1.- The Challenge of Comparing Friedman With Popper

It has already been argued by Frazer and Boland (1983) that, interpreted in an instrumentalist fashion, Milton Friedman's well known and much criticized 1953 paper on "The Methodology of Positive Economics"<sup>1</sup> proved to be convergent with Popper's falsificationist philosophy of science<sup>2</sup>. I think that this comparison is flawed. For one can assuredly contest this interpretation in view of the fact that Popper always opposed any kind of instrumentalist philosophy of science<sup>3</sup>. It is not even clear that what Friedman has to say on the intricate question of the status of theories and on the function of tests has anything to do with what Popper criticizes under the general category of "instrumentalism". What I will do here is not so much to directly challenge the Frazer-Boland interpretation as to replace it with a completely new comparison of both methodologies in order to get a new look at Friedman's methodological stance. Of course, one has to focus only on the arguments that really are comparable and leave aside those that could easily be proved to be fundamentally different, if not plainly incompatible. But, as I will try to show, the exercise is worthwhile, for this new comparison will force us to reinterpret, at least partially, what seem to me to be the two central tenets of Friedman's methodological doctrine. Of course, in order to find convergence between Friedman's and Popper's views on economic methodology, we have to read

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<sup>1</sup> See Friedman, M. (1953), "The Methodology of Positive Economics", in *Essays in Positive Economics*, Chicago: The University of Chicago Press: 3-43. Republ.. in *Readings in Microeconomics*, William Breit and H. M. Hochman (eds.), 2nd ed., New York: Holt, Rinehart and Winston, 1971: 23-47.

<sup>2</sup> See Frazer, W.J., Jr. and Boland, L.A. (1983), "An Essay on the Foundations of Friedman's Methodology", *American Economic Review*, 73 (March): 129-44. See also Frazer, W.J. (1984), "Methodology: Reply", *American Economic Review*, 74: 793-794.

<sup>3</sup> The best place to look for his classical objections against instrumentalism is the *Postscript*. See Popper, K.R. (1956/1983), *Realism and the Aim of Science, From the Postscript to the Logic of Scientific Discovery*, vol. III, W.W.Bartley,III (ed.), Totowa, N.J., Rowman and Littlefield, chap.1, sections 11 to 16, pp.107-158. See also *Conjectures and Refutations*, chap. 3.

somewhat more cautiously both Friedman's essay and Popper's epistemological writings. We have especially to carefully re-read what the latter says in the *Logic of Scientific Discovery*, but also take a new look at the arguments he puts forward in *Poverty of Historicism* and in his well known 1967 text on the "Rationality Principle"<sup>4</sup>. I am aware that it has already been argued by D. Wade Hands that Popper's philosophy of social science, and especially his views on "situational logic", cannot be made consistent with Popper's philosophy of natural science<sup>5</sup> - even if one cannot miss that *Poverty of Historicism* was partly written precisely to radically undermine any methodological dualism. But even if that was the case<sup>6</sup>, it could still be possible to think that Friedman's view is plainly consistent with Popper's situational logic (the one which Hands calls Popper's) and this is precisely what I intend to show. Accordingly, the burden of the present paper is to show that if we take into consideration Popper's argumentation about the methodological status of the Rationality Principle (RP from now on) in economic analysis, we don't have any other choice but to recognize the overlap (at least a partial but, nevertheless, very important overlap) of Friedman's methodological stance with Popper's epistemological conclusions about economics.

We can say that, on the face of it, two major objections were raised against Friedman, each one pertaining to what I tend to see as the two correlated and interconnected positions of a very cohesive methodological stance. Those theses are the two major tenets of one and the same fundamental doctrine. The first one concerns Friedman's thesis about the illegitimacy of searching for "direct tests" of what is called, by him and by his opponents as well, the "assumptions" of a particular theory. What is at stake here is nothing other than a general logical and epistemological theory of hypothesis testing, a philosophical theme that occupies a very central place in Popper's philosophy of science, be it natural or social science<sup>7</sup>. The second one, which seems to have been to a majority of commentators even more unacceptable on purely epistemological and methodological grounds, states that economic science can work perfectly and do its job when relying on the

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<sup>4</sup> See Popper K.R. (1967), «La rationalité et le statut du principe de rationalité», in Emil M. Claassen, dir. de la publ., *Les Fondements Philosophiques des Systèmes Économiques*. Paris: Payot, 1967, pp.142-150. The original english version only appeared fifteen years later: cf. "The Rationality Principle", in David Miller, ed., *Popper Selections*. Princeton: Princeton University Press, 1982, pp. 357-365.

<sup>5</sup> See Hands, D. W. (1985), "Karl Popper and Economic Methodology: A New Look", *Economics and Philosophy*, 1, 1: 83-99.

<sup>6</sup> I argued elsewhere that, in fact, it was not, even if I granted that Popper's analysis was, generally speaking, quite debatable on some other grounds. See my «Confuting Popper on the Rationality Principle» (*Cahiers d'épistémologie* no 9012, Département de philosophie, Université du Québec à Montréal).

<sup>7</sup> See Popper, K.R. (1959), *The Logic of Scientific Discovery*. 2nd ed., New York, Basic Books, in particular section 12: "Causality, Explanation, and the Deduction of Predictions", pp.59-62.

“unrealistic” hypothesis of maximization of expected return (MH from now on). Friedman's argument, which he developed exclusively in relation to the theory of enterprise, has been said to be a logical mistake, if not a methodological blunder, for, as it was argued using clearly Popperian arguments, scientific knowledge cannot work in any domain whatever with false propositions. Following Popper, this is said to be so on one hand because false propositions entail everything, contrary to scientific statements which preclude at least a certain kind of event and forbid some sorts of states of affairs in conjecturing that they do not take place anywhere in our world and at any moment in our history. It is also said to be so on the other hand because propositions known to be false cannot be part of any logically sound and practically acceptable explanation argument whatever, be it a D-N, an I-S, a “reason based” explanation (as opposed to a “causal” one) or even a “propensity based” argument (as Popper would now have it done).

Nevertheless, I would like to stress that on each of the two major points to which I just alluded, whatever the differences may be on some other important philosophical topics, Friedman's views may be said to be in complete agreement and in full harmony with Popper's. This is what the challenge of comparing Friedman's with Popper's methodological doctrines is all about. And if I were to show convincingly that those two approaches converge to one another and support themselves mutually, we would find ourselves committed to say that they either collapse together, or that it is, in fact, possible to vindicate both at the same time and with the same argumentation. Indeed, if it has seemed acceptable to see in Popper's situational logic a sound methodological approach for economic analysis<sup>8</sup>, then it would appear that we can find new grounds to ensure Friedman's overall methodological position. In a sense, one could reasonably argue that the case against the plausibility of having a “popperian reading” of Friedman's methodological doctrine has been completely misconstrued by Frazer and Boland. We must nevertheless recognize that they tried to show some epistemological agreement between Friedman and Popper, and this is also what I would like to prove. My own analysis is no more concerned by any biographical data that would purport to prove that Friedman was under Popper's influence or that they were, in fact, in contact exchanging ideas and arguments about

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<sup>8</sup> But there has been, of course, and for more than fifteen years now, a lot of criticism of Popper's 1967 thesis on the RP status. See in particular, apart from Hands (1985) cited above in note 3, Glück, P. & Schmid, M. (1977), «The Rationality Principle and Action Explanations: N. Koertge's Reconstruction of Popper's Logic of Action Explanations», *Inquiry*, 20: 72-81; Koertge, N. (1979), "The Methodological Status of Popper's Rationality Principle", *Theory and Decision*, X: 83-95.; Latsis, S.J. (1983), "The Role and Status of the Rationality Principle in the Social Sciences", in R.S. Cohen and M.W. Wartofsky, eds., *Epistemology, Methodology and the Social Sciences*. Dordrecht: Reidel, 1983, pp.123-151; Schmid, M. (1988), «The Idea of Rationality and its Relationship to Social Science: Comments on Popper's Philosophy of the Social Sciences», *Inquiry*, 31: 451-69. See also Latsis, Spiro J. (1972), "Situational Determinism in Economics", *The British Journal for the Philosophy of Science*, 23 (1972): 207-45. Koertge, Noretta (1974), "On Popper's Philosophy of Social Sciences", in K. F. Schaffner and R. S. Edren (eds..) *PSA 1972, Vol. XX: Boston Studies with Philosophy of Science*, Dordrecht, Reidel: 195-207. Koertge, N. (1975), "Popper's Metaphysical Research Program for the Human Sciences", *Inquiry* 18: 437-62.

economic methods. My analysis is intended to be purely “internal” and has nothing to do with circumstantial evidence: it has to do exclusively with the pure logic of concepts and with the validity of philosophical arguments. This said, we must notice that Friedman's methodological essay was written and published before the English translation of Popper's *Logic of Scientific Discovery*, which, as we all know, only appeared in 1959. It would then be quite impossible to argue that the appearing of the “popperian bible” could have had any direct influence on Friedman's methodological and epistemological thinking at the time he was preparing his essay. Hence, we can't be surprised not to see the name of Popper cited anywhere in Friedman's essay.<sup>9</sup> Does that establishes beyond any reasonable doubt that Friedman was unaware of Popper's work? I don't think so. And it appears to me that the missing link could well be Friedrich Hayek, who was at the London School of Economics until 1950, who had Popper offered a readership at the LSE in 1945 (Popper arrived in London to stay in January 1946)<sup>10</sup> and who had left London for the University of Chicago where he could surely discuss general methodological matters with Milton Friedman. We know that Hayek had read Popper and that he was, as he acknowledged overtly, profoundly influenced by Popper's philosophical views exposed in the *Logic of Scientific Discovery*.

## 2.- On the Technical Notion of Theory Testing

In order to establish my first point, I will argue that Friedman actually accepts the central and more crucial theses of what critical rationalism is concerned with as a general philosophy of science. Refutationism, as I shall argue, comes with a very precise conception of theory testing. And Friedman, on what seems to be completely independent grounds, puts forward an argument that is very similar to the one Popper formulates.

Popper's *refutationism* is a methodological prescription (and as such, it implies a lot more than just using the logical rule of *modus tollens*) which states that one is allowed to consider the theory under examination as practically falsified if and only if two general conditions are satisfied. First of all, there must be, at least in principle, an event (i.e. a sort of phenomenon or a kind of event and not a singular occurrence only) that is forbidden by the conjecture which was formed to explain some observable situation, for example, an experimental result or a social outcome. If this is

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<sup>9</sup> As a matter of fact, Friedman cites very few philosophers (I noted the names of only those three: Kurt Gödel, J.S. Mill and David Hume) but he refers to a lot of economists (I noted the names of Lester, Machlup, Stigler, Oliver, Gordon, Haavelmo, Marschak, Robinson, Koopmans, Cooper, Hurwicz, Alchian, Veblen, Henderson, Harrod, Hall & Hitch, Meade & Andrews, Hart, Alexander, J.N. Keynes, Savage, Marshall, Chamberlin, Bishop and Triffin).

<sup>10</sup> See *Unended Quest*, (Lasalle, Ill.: Open Court Publ. Co., 1974/1976), section 26.

necessary, it is far from sufficient: it only defines what "refutability" is all about. We must also talk about concrete or practical "refutation". This is why, as a second general condition, Popper states that, if the hypothesis under examination is contradicted by some well-established observable fact, in order to consider it as practically refuted and to be forced to discard it at least temporarily (until maybe a new observation forces us to reconsider), some other conjecture must prove at the same time to be a good contender or a reliable challenger to the first one, and this contender hypothesis has to be corroborated by the very same test that serves to put the first one into question. The popperian doctrine of theory testing is a form of generalized deductivism assuming that *the only way to test a scientific hypothesis* (i.e. a universal proposition) in order to see if it is wrong or if it can be assumed to be on the right track is to infer from it in conjunction with a set of auxiliary assumptions some singular statement which would seem to be well established and of which the truth value can be looked at as unproblematic for the time being. Popper's doctrine of theory testing is modeled along the lines of what Francis Bacon used to call "experimentum crucis", this procedure being redefined by Popper to meet Duhem's criticism of it.<sup>11</sup>

We have to take into consideration that, for both Friedman and Popper, economics is, and has to develop as, a "positive science" (Friedman) or as an "empirical domain of research" (Popper), like the natural sciences. When Friedman argues that economic knowledge has to be established on the basis of facts rather than otherwise, when he writes that as a positive science, «its task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances» and that «its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields» (Friedman 1953, p.4), Friedman is in complete agreement with the popperian view of empirical science. "Positive science" for Friedman means exactly what "objective knowledge" means for Popper<sup>12</sup>.

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<sup>11</sup> See Duhem, P.M. (1906), *La Théorie physique, son objet, sa structure*, Paris: Marcel Rivière (new ed., Paris, Librairie J. Vrin, 1981). Engl. transl., *Aim and Structure of Physical Theory*, New York: Atheneum, 1954.

<sup>12</sup> The example of a theoretical controversy discussed by Friedman, the one concerning minimum wage, is a very good one to expose the general methodological point of view that Popper would surely have accepted to endorse. For, like Friedman, Popper would no doubt consider that the consequences of implementing a legislation to fix a minimum wage is an empirical matter rather than an ideological one. I have no doubt at all that in the case under discussion, Popper would propose the same argument as the one that Friedman puts forward and would say that, on the one hand, «(P)roponents believe (predict) that legal minimum wages diminish poverty by raising the wages of those receiving less than the minimum wage as well as of some receiving more than the minimum wage without any counterbalancing increase in the number of people entirely unemployed or employed less advantageously than they otherwise would be» and that, on the other hand, «(O)pponents believe (predict) that legal minimum wages increase poverty by increasing the number of people who are unemployed or employed less advantageously and that this more than offsets any favorable effect on the wages of those who remain employed.» (Friedman 1953, pp.5-6)

But scrutiny of Friedman's 1953 essay reveals that there is here more than just coincidence in words or phrases, for Popper and Friedman seems to have exactly the same conception of what "the aim of science" is. This is to say that one of the very central methodological tenet of Friedman's doctrine is also one which Popper would feel completely comfortable with. As long as economics constitutes a positive science, «(its) ultimate goal (...) is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed.» (Friedman 1953, p.7) Inasmuch as a theory presents itself as a «body of substantive hypotheses designed to abstract essential features of complex reality» (*ibid.*, p.7), it has two fundamental features: it can be viewed either as a language, a set of categories, or as a body of substantive hypotheses and then «theory is to be judged by its predictive power for the class of phenomena which it is intended to "explain"». (*ibid.*, p.8) As Friedman puts it, «(F)actual evidence alone can show whether the categories of the "analytical filing system" have a meaningful empirical counterpart, that is, whether they are useful in analyzing a particular class of concrete problems.» (*ibid.*, p.7). And, as a further precision, Friedman adds that «(O)nlly factual evidence can show whether it is "right" or "wrong" or, better, tentatively "accepted" as valid or "rejected"». (*ibid.*, p.8) This shows that Friedman's "predictionism" is not so much a defence of instrumentalism as it is a defence of the only legitimate method for theory testing. What has to be seen here is that Friedman argues against any kind of conception that would look at economics as a mere system of classification of our social experience and that would treat it as a mere formal system, as a system of tautologies, as a kind of discourse that could be true *a priori*.

But it has been said that Friedman was "reducing" economic theorizing and modeling to the construction of a mere predictive device. What he says in fact is that «the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience.» (*ibid.*, pp.8-9). Popper says nothing else when he states what the "empirical content" of a scientific theory amounts to: a theory has content inasmuch as it has "observable consequences". One can even say that, for Popper, all empirical theories "reduce" to the factual propositions that we can logically derive from them. They inform us about the world if and only if they make a difference for observation. As it seems, Friedman does not say anything else than that. He even advances one step further onto Popper's path when he goes on to say that «(T)he hypothesis is rejected if its predictions are 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.» As in the popperian methodology, there can only be two outcomes here: either the hypothesis or theory gets "corroborated", because all repeated tests fail to show that it is incorrect, and then it gets conserved as seemingly now being the best available approximation to truth; or it gets falsified and, ultimately, rejected for a better one if there is one available. Of course, Friedman is not speaking of "corroboration" "falsifiability", "falsification" and "verisimilitude", but no one can deny that

his methodological arguments are absolutely popperian in spirit and in inspiration, if not in verbal expression. Moreover, there cannot be a more popperian epistemological statement than the following one: «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.» (*ibid.*, p.9) Here, Friedman can be seen as making a case against “confirmation” defined as a degree of rational credence and interpreted as an eventually increasing state of subjective certitude<sup>13</sup>. He is clearly arguing, in fact, for what Popper calls “falsifiability”.

So, whatever the rest of Friedman's methodological doctrine is, he proves to have adopted a rather anti-verificationist mood of thought and seems to endorse an openly refutationist way of assessing what is the main function of observation and experiment in positive science. This Friedmanian doctrine seems in fact to go quite blatantly against all “positivist” philosophy of science<sup>14</sup>. This fact has to be qualified as a very meaningful one when we take into consideration that Friedman's essay was being written during the heyday of logical empiricism, and that it was precisely at the University of Chicago that the new “positivist turn” was taking place after the collapse of the Vienna Circle in 1935, and when we also take notice that it was also at the University of Chicago that the project of having an all-encompassing “Encyclopedia of Unified Science” was launched and intellectually steered by Rudolf Carnap, Otto Neurath and Charles Morris. And in precisely that institutional context, Friedman's methodological declaration cannot help but take on a new historical dimension.

### 3.- The Poisonous Question of Using “Unrealistic Assumptions”

But there is a real testing problem in economics, and both Popper and Friedman acknowledge this difficulty. While Popper seems to maintain that what demonstrates the real explanatory power of a bold conjecture is its capacity to help us predict new observable facts (for, otherwise, a theory could be seen as merely *ad hoc*), Friedman only insists on the logical fact that a theory does not have *prima facie* to be tested on new facts for it can be tested on old ones which have not yet been noticed and which statistical analysis could reveal to us. But here is no solid ground for a dispute between the two

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<sup>13</sup> We know that Popper was the first to talk about the “confirmation” of scientific theories, but with an openly anti-verificationist mind. And when he saw that Carnap was using this very same word in a logico-probabilistic and subjectivistic sense, Popper finally chose to avoid this term and began instead to speak of the “corroboration” of theories. This terminological change occurred during the nineteen-fifties, when Popper was establishing the English version of his *Logik der Forschung*.

<sup>14</sup> This is the way we have to read the following statement: «The evidence *for* a hypothesis always consists of its repeated failure to be contradicted...» (Friedman 1953, p.23).

thinkers. The general testing problem in economics has to do with the practical impossibility (first acknowledged by Carl Menger in his 1883 book) of disentangling the observable facts so that one could clearly see what pertains exclusively to economics and what to other social or psychological factors. And it is even more difficult to break down all economic factors so as to isolate only those ones that a proposed theory or model would urge one to seek for. A very similar kind of methodological difficulty is indeed identified by Popper when he takes for granted that only *kinds* of events, as opposed to *token-events* ("occurrences" as Popper says), can be explained and predicted in economics. As we know, this line of argument is borrowed by Popper from Hayek who speaks in terms of "explanation in principle" (or "pattern explanation") as compared with explanation of singular situations or outcomes.

Because it is really difficult in economics to find new evidence in order to test the models and theories that we formulate, and because it is even more difficult to isolate the only economic factors the efficiency of which we would like to attest, it has been thought that one could proceed otherwise. That is, by way of directly assessing the general plausibility of what the theorist is assuming when trying to explain observable events. Besides the *implications* of the theory, it was asserted that it could be possible to evaluate the credibility and legitimacy of the theory by way of contrasting what it states with what seems to be the case at first glance - in other words by way of contrasting it with "immediate evidence" or with "common sense knowledge". Of course, it follows from the first argument exposed that this way of testing a theory hasn't anything to do with scientific method. The very idea of what has been labelled the "assumptions of a theory" is clearly, from Friedman's point of view, an empty one: it has no methodological content at all. But if we were to try to make sense out of it, then we would have to say, furthermore, that as long as it "assumes" anything, a theoretical framework has to have "analytical relevance" and must not bother at all with having or not having "descriptive accuracy" (Friedman 1953, p. 33). This is certainly the main target of every one who wishes to oppose Friedman's methodological stance. And I would like to show that on this very same point, once again, Friedman meets in complete agreement with Popper. But the point doesn't concern refutationism anymore and one has to ask what are precisely those "assumptions" economic theory is using. Some examples of universal propositions playing this putative explanatory role could be said to be Smith's formula concerning "the pursuit of one's own personal interest", or Marshall's conjecture about the reality of "perfect competition", or again, it could also be, at it seems, Popper's formula of the RP ("Every economic (or social) agent always acts in a way that is adapted to her situation as she sees it"). What is clearly at stake here is whether Friedman's defence of the "maximization of utility hypothesis"<sup>15</sup> and Popper's legitimization of the "rationality principle" are convergent or not. I will try

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<sup>15</sup> Friedman writes, in fact, about the "maximization of expected returns" as an hypothesis in the theory of the firm and criticizes those who continue to speak of the "maximization of profit" as if it was the same concept and as if it was the way economists would still account for the rule businessmen follow in fixing prices.



to show that there is convergence, and that each time the defence and illustration of the considered hypothesis is done on quite analogous grounds.

I must say first that, to my mind, there is no way to differentiate, in first approximation at least, the theoretical meaning of those two assertions: both Popper's RP and Friedman's MH seem to be "assuming" one and the same situation on the part of economic agents (be they consumers or entrepreneurs), i.e. that every decision-maker is always regarded by the scientist who tries to explain and understand his behavior as having tried, in one way or another, to make the best or the most appropriate decision for him in the circumstances in which he finds himself, depending on his goals, on the means available to him in the precise conjuncture in which he is, and, of course, on the general constraints (budget, information, context, etc.) with which he has to deal with. Second, I submit that what seems to have been universally rejected by almost all commentators, economists or philosophers, is that for Friedman, this MP is, as a theoretical proposition, still a workable one for economic understanding and explanation even if it is recognized by him to be an "unrealistic" one, which is generally understood as meaning "thoroughly false".

But, be that as it may, nobody has yet counter-argued by calling attention to the fact that this is also one of the main theses Popper puts forward in his 1967 article. Popper takes great care in telling us explicitly that the RP is evidently a false statement when considered as a psychological law<sup>16</sup>, but that it nevertheless constitutes an indispensable conjecture as long as we wish to be able to explain anything going on in society. The RP may well be false, says Popper, but it constitutes, on the face of it, a sufficiently near-to-the-truth (or verisimilar) empirical hypothesis (should we perhaps say "assumption"?) that has to be used as the only available "law of animation" of any social system taken as a whole. Popper's thesis is that it is what has to be methodologically accepted if one is to be able to make sense of social issues, and in particular of the economic decisions of entrepreneurs and consumers. This comes down to saying that we can't bother with the falsity of the RP if we want to explain, in the framework of a situational logic, individual action in general. One cannot but see how close Friedman's and Popper's arguments are on this precise point.

Once this general line of reasoning has been recalled, we can try to reinterpret the main reason why, for Friedman, it is strictly impossible or unacceptable to argue as if a theory or a general explanatory hypothesis in economics could be assessed by directly inspecting the plausibility of its axiomatic content. The general idea of having a theory axiomatized and eventually formalized, be it in physics or in economics, is precisely to not have any kind of common or everyday intuition interfere

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<sup>16</sup> Popper's exact words are: «the rationality principle seems to me clearly false - even its weakest zero formulation». (Popper 1967/1982, p. 360)

with the logical content of the set of propositions involved in the deductive process of explanation. That being the explicit aim of the theoretician, it would be quite inappropriate to criticize the whole theoretic enterprise on the basis that it is alien to the "concreteness of the real life". For, after all, what "reality" is in fact cannot be discovered unless one renounces his immediate perceptions and, often enough, even his favorite convictions. As Popper would have it, personal beliefs have nothing to do with science, for authentic knowledge is a process without a personal subject, and this is true not only for the layman but also for natural scientists and indeed for economists as well.

If one was to read Friedman as saying that economists never have to bother with the truth of what they say - as long as it works out so as to be fruitful enough on predictive grounds - and if one was to interpret Friedman's essay as allowing economists not to feel concerned with the logical validity and the empirical "truthlikeness" of the theorems that may be derived as consequences of the axiomatic system that they more or less boldly conjecture in order to explain and predict a certain class of observable phenomena, one would also be committed to say that Friedman's methodological stance is at odds with Popper's situational logic. But that would seem to me to be far away from the central problem which Friedman's 1953 essay comes to grips with. What has been read as meaning that the economist may, if it so pleases him, use axioms that he knows for sure to be false, is a very inaccurate understanding of what Friedman is really arguing for. For him, the question is not so much to concede in a kind of scandalous confession that economic theorizing is grounded on false assumptions and that this mortal sin doesn't change anything as far as he is concerned, but to challenge the very fact that a theory is, properly speaking, a set of "assumptions" that can be read out of the system of axioms themselves. If Friedman is nonchalant about the presumed fact that the "assumptions of the perfect competition model" are psychologically unrealistic, it is not because he finds it legitimate for scientists to work with false theoretical presuppositions. It is, on the contrary, because he believes that no scientific theory at all has any "assumption" whatever that we would have to take into consideration when assessing its truth value, as opposed to the "implications" that the repeated and severe testing process has the function to permit us to document and to establish firmly.

Maybe the whole debate would have been more easier to cut short if one had only seen that, so long as we understand "assumptions" to be what we now call "presuppositions", then all assumed propositions are necessarily a proper subset of the logical consequences of the theory. Using Popper's terminology, which he borrowed from Tarski, a theory has a "content" so long as it has "consequences": all the empirical content of a theory is reducible to the set of the observable propositions that it is possible to infer from this precise theory with the aid of any other valid statement (initial conditions, auxiliary hypotheses, boundary conditions) that one is willing to use in coordination with it. So far as this is true, there isn't any methodological distinction to make between "assumptions" and "implications" of a theory: the only "presuppositions" of the theory that the theoretician takes to be an

explanation of some situation or outcome are necessarily part of the "consequences" of the set of propositions he looks to as being plainly explanatory. This comes down to saying that there isn't any storable methodological difference between the "presuppositions" of a particular theory, or what we may logically deduce directly from it, and its "observable entailments", or what may be inferred from it when applying it in a complex system of reference and coordinates, be it a natural or a social one. As long as assumptions derive from theories, they are part of what we have to regard as the proper consequences of them. This is, on my view, what is really at stake in Friedman's argument targeted against all those who are ready to take at face value what businessmen and entrepreneurs think they're doing or not doing when they fix the price of whatever goods or services they offer on the market. We can now see what Friedman wants to say when he challenges the fact that economic theories "assume" nothing but what can be deductively derived from them as logical and observable consequences. It is absolutely impossible to derive from economic theory that entrepreneurs are aware of what is *really* going on when they make decisions, and the only way to know what is effectively going on is to use the theoretical set of axioms and theorems in order to discover new information about reality and to predict yet unobserved events. And even then, that will never "prove" that the theory is true, it will at most show that it is perhaps on the right track in trying to approximate what the truth is.

Then, that the alleged "assumptions" of perfect competition or perfect monopoly give «a false image of the reality» (Friedman 1953, p. 15) does not matter at all, for this image is not "the scientific one", to use van Fraassen's phrase. It is only an epistemic mirage, a non-knowledge. It is out of question, if we are to test, for example, the economic theory of the firm, to ask entrepreneurs whether «they in fact reach their decisions by consulting schedules, or curves, or multivariable functions showing marginal cost and marginal revenue» (*ibid.*) for this in no way contradicts any statement that we could derive from the theory under test. Of course, because for Friedman «(T)he evidence *for* a hypothesis always consists of its repeated failure to be contradicted...» (Friedman 1953, p.23), if statements referring to what entrepreneurs and businessmen spontaneously say they're doing could logically be derived from economic theory, and if those statements were to be in plain contradiction with this theory as such, then we would have to say - and surely Friedman would agree to say - that the theory being tested on this empirical basis has to be declared flawed. And it would have to be declared practically refuted if and only if some other theory could offer a better explanation of this observed fact. But as long as it's not the case, for both Popper and Friedman takes for granted that there is no such available contender of the RP or of the MH in economic theory, there seems to be no other conclusion possible: entrepreneurs' and businessmen's opinions, as measured by sample surveys and Gallup polls, do not matter at all for the economic theory of the firm.

#### 4.- What Should We Conclude From This Comparison?

I have contented myself with showing that, on two important points, the friedmanian methodological stance is plainly convergent with Popper's refutationism and situational logic. In the past, those two points gave raise to much criticism of what has been seen as an incoherent, illogical, pervasive, unreasonable and, still, defenceless epistemological credo. But relating this doctrine to Popper's falsificationism and to his situational logic, especially in view of the status he recognizes to the RP in economic analysis, we find some new grounds to somewhat credit the MH with more methodological and epistemological authority. For Popper explicitly states that the RP is false. To maintain that the RP is false is merely to take for granted that some agents do not «always act in a manner appropriate to the situation in which they find themselves» (Popper 1967, p. 361). But, as it seems, we may nevertheless "assume" (or conjecture) that every agent acts in an adaptative way so long as this explaining principle has an empirical content and scope, so long as it is not being believed that it means that social agents are really psychologically rational, and so long as we can use this best available approximation-to-the-truth in order to explain, by way of model building, explanations of economic behavior and social action in general. As for Friedman, he would say that it does not matter that the MP is "unrealistic" so long as it is testable and so long as it gives way to fruitful predictions which permit us to discover something. If Popper's argument is sound, then it can help Friedman's methodological stance to hold more firmly. But, consequently, I should perhaps add that, if not, it could also be surely criticized for the same reasons as those that could be maintained against Popper's philosophy of social sciences.