THE POLITICS OF POSITIVISM: DISINTERESTED PREDICTIONS FROM INTERESTED AGENTS

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1. POLITICS IN «THE METHODOLOGY»

Of the six sections composing «The Methodology of Posive Economics», the first one («The Relation between Positive and Normative Economics») is apparently the less discussed in the F53 literature, probably as a result of being the shortest one and the less relevant for the realism issue, all at once. In view of Milton Friedman's subsequent career as a political preacher, it seems difficult not to wonder whether this first section ruled it the way the other five directed Friedman's scientific performance. After all, the role of prediction in defining positive economics was already advanced therein: when an economist predicts, her results are «independent of any particular ethical position or normative judgments». This is also why positive economics is a politically relevant discipline: as long as the differences about economic policy *–among disinterested citizens–* derive only from different predictions about the economic consequences of taking action, these differences could be eliminated by the progress of positive economics. Our plan in this paper is to present, in the first place, the role of *political motivations* in the development of Friedman's methodological stance. As we will discuss in §2, Friedman was involved in the policy-making process right from the beginning of his professional career, and could experience at first hand the relevance of economic predictions in generating a consensus not only among politicians or the public opinion, but among the profession itself. Conversely, Friedman could also appreciate how difficult was to reach a consensus on a particular policy when the economists disagreed on its practical consequences. In this respect, as we will see, «The Methodology» attempted at guaranteeing the political efficiency of economic research. However, the sociological turn in science studies suggests to question on what basis can we deem a prediction *neutral*. Is it simply that economists produce these positive predictions *disinterestedly*, even while deeply engaged in political debates? In §3, we analyse how Friedman himself produced predictions immediately before «The Methodology» was drafted, and how this procedure lies at the core of his Marshallian approach, which he contrasted to the Walrasian strategy on the grounds of its higher political relevance. Yet, from a sociological viewpoint, it is precisely this Marshallian strategy which seems most objectable: the way Friedman deals with theoretical concepts in economics in order to obtain predictions makes them particularly capable of manipulation by a not so disinterested economist. We will see that «The Methodology» does not provide any defence whatsoever to counteract.

Finally, we argue that the «knowledge» produced by economist can only gain the trust of lay audiences if the latter know that the activity of the former is constrained by an appropriate *methodological contract* (ZAMORA BONILLA 2002), and we suggest that F53

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can be best understood as an attempt to persuade the economics profession to adopt a certain *methodological contract*, an agreement which ensures that supporting economic research is a rational for citizens and politicians (§4). Nevertheless, our own case will not be long, as a peculiar manuscript we add by way of conclusion makes it much better than we originally did (§5).

2. PREDICTING FOR THE POLICY MAKERS

Whereas only a minor section of «The Methodology» was devoted to normative issues, a significant part of Friedman's professional experience in the lapse 1935-1945 took place in Washington, quite close to the political arena. We are going to examine the various contexts in which Friedman came to experience the connection between prediction and politics, which, in our opinion, provide the drive behind F53's first section. We discuss Friedman's first stay in Washington in the New Deal years, paying also attention to the debate on the political relevance of statistical economics in the early 1930s. We will also consider how Friedman developed a position of his own early in the 1940s, while appointed at the Treasury. By the end of the decade, it had became a standard view among the profession –if we are to judge it by an AEA report which Friedman coauthored in 1947. Finally, we exemplify the role Friedman assigned to prediction as a consensus-generating tool by means of an analysis of two of the papers compiled in his *Essays in Positive Economics*.

Let us then start in 1935. A growing demand for young economists sustained by *New Deal* policies gave the young Friedman an opportunity to enter a depressed labor market (FRIEDMAN & D. FRIEDMAN 1998, p. 60). Paradoxical though it may seem now, he was appointed at an agency aimed at the promotion of economic planning at the highest levels of government, the National Resources Committee (BRINKLEY 1995, pp. 245-50; LEE 1990). Under the supervision of Hildegarde Kneeland, Friedman joined an NRC task force assigned to coordinate the Study of Consumer Purchases (SCP), a big statistical survey intended to quantify consumer incomes and expenditure in the United States in the period 1935-1936 (NRC 1938, 1939). Or, in other words, to provide a statistical answer to the debate on the causes of the Great Depression which stirred up the 1932 presidential campaign. At its roots –Roosevelt had argued then– lied the maladjustment between industrial production and the purchasing-power of would-be consumers, and the way out consisted simply in raising the latter (BRINKLEY 1995, pp. 69-72; BALISCIANO 1998, pp.160-65). Six years after Roosevelt's election, the NRC income report went in print. It still addressed the underconsumption issue in its preamble (NRC 1938, p.1), even if the policies so far implemented had worked to the detriment of consumers (BRINKLEY 1995, p. 71). Though credited among the contributors, Friedman's memoirs skip the political implications of the SCP, focusing instead on the statistical challenges involved therein (FRIEDMAN & D. FRIEDMAN 1998, pp. 61-66).

New Deal policies required government intervention in the American economy to a scale never seen before. Needy of figures (DUNCAN & SHELTON 1992, p. 322), the American administration multiplied its statistical production, and most economists were appointed with a view to conduct empirical research, their lack of technical qualification notwithstanding (HOTELLING 1940, p.458). As a result of the year he spent in Columbia with Harold Hotelling, Friedman was certainly familiar with the subtleties of the methods introduced by Ronald Fisher in the 1920s, but sampling design was a newborn discipline, almost unknown to most federal agencies[1]. The SCP gave him thus the opportunity to face the challenges of applying theoretical statistics to the design and implementation of a large-scale survey, which were not small (KNEELAND *et al* 1936; SCHOENBERG & PARTEN 1937). Indeed, the SCP gave none the less than Jerzy Neyman the opportunity to devise a double sampling procedure to correct the one applied by the NRC team, to which he was exposed during his stay at the Department of Agriculture Graduate School in 1937. Friedman himself posed the problem during one of Neyman's lectures[2]. In sum, New Deal planning fostered the completion of Friedman's statistical training, both on a practical and theoretical sides. It is quite plausible that he had then become aware of the political relevance of statistical economics. However, it is also possible that he already knew that statistical economics could also make economists differ.

In 1932, Roosevelt recruited three Columbia professors as campaign advisors. Among the members of this *Brain Trust*, as they came to be known, there was an economist, Rexford Tugwell, who had proclaimed the year before in the AEA meeting the end of the *laissez faire* era (TUGWELL 1932). According to Tugwell, it was the federal government responsibility to adjust production and consumption through price control, securing the purchasing power of salaries. Moderate voices demanded a mere *indicative* planning, an ineffective solution for Tugwell since it did not eliminate *uncertainty* (TUGWELL 1932, p. 85)

It was certainly the end of free entrepreneurial activity, as Frank Knight –Milton Friedman's «revered teacher»– had conceived of it in his 1921 classic *Risk, Uncertainty and Profit*[3]. Economic planning required *perfect information, which* was, for Knight, unattainable: the decision of each economic agent is essentially unique and thus unpredictable. Individual freedom was at stake if the planners were to reach their goals (KNIGHT 1932, p. 448).

Against those who conceived of economics as a mere technique of prediction and control, Knight warned that only the short-time view of the price problem, as contained in static economics, is to be deemed scientific. Unfortunately, static economics did not allow for far reaching predictions as required by planners, whereas those derived from institutional approaches to economics, amounts to nothing more than sheer «philosophy of history» (KNIGHT 1924, p.146). These were hardly good news to announce in the times of the Great Depression: still as an undergraduate student in Chicago, Friedman could attest for the reactions it aroused in 1932 when Knight delivered his tongue-in-cheek lectures on «The

[1] On the introduction of mathematical statistics in the USA, precisely in 1933, cf. STIGLER 1996. The use of mathematical statistics by Federal agencias in that same year is discussed in DUNCAN & SHELTON 1992, p. 321. For an overview of the development of sampling techniques cf. SENG 1951 and DESROSIERES *et al.* 2001. For an extended discussion of the methodological challenges herein involved as Friedman could have met them, cf. TEIRA 2003a.
[2] Cf. NEYMAN 1938, p. 328. On the context: cf. REID 1998, pp. 137-38 –Friedman's contribution is mentioned in p. 148. For an extended discussion cf. TEIRA 2003a.
[3] On this particular point, cf. PRADIER & TEIRA 2000, 2002.

Case for Communism» before an over reactive audience (FRIEDMAN & D. FRIEDMAN, p. 37).

By the end of the 1930s, in sum, Friedman had already experienced how predictions were requested from the economists with a normative purpose (namely, state intervention) and the objections raised against economics assuming such a role. As for his own position concerning statistics and economic planning, little is known but still in 1938 Aaron Director could fool around Friedman's «very strong New Deal leanings –authoritarian to use an abusive term» (*apud* FRIEDMAN & D. FRIEDMAN 1998, p. 81).

In the 1940s, Friedman's stance began to take shape. By 1942, he was a self-proclaimed «thorough Keynesian»: the terms *money* or *monetary policy* seldom appeared in his writings (FRIEDMAN & D. FRIEDMAN 1998, p. 113). The year before, he had been summoned again to Washington, this time appointed as the *Principal economist* at the Treasury's division of tax research (FRIEDMAN & D. FRIEDMAN 1998, pp. 105-25). Having completed the draft of his dissertation at the NBER that same year, Friedman was an accomplished expert in both income and consumption studies, a profile highly demanded again by the Federal Government. Hitler's Germany had already proven how menacing State intervention could be if economic planning was taken to its last consequences. Roosevelt's liberals were now inclined to intervene through more *compensatory* tools, in Alan Brinkley's words: i.e., a combination of Keynesian fiscal measures and enhanced welfare-state mechanisms (BRINKLEY 1995, pp. 154-64). In a crude war context, stabilizing consumption required active taxing policies that prevented inflation. Friedman's role at the Treasury obliged him to take part in the real policy making debate, which involved frequent congressional testifying, consulting services to many a senator, speech drafting and so forth. Friedman argued for a demand oriented approach to income analysis, which conferred theoretical significance to the available statistical data (e.g., SHOUP *et al*. 1943, pp. 111-53) –an approach already exemplified in his own dissertation: see below §3.

By the end of the 1940s, the two main theses later on stated in «The Methodology…»'s first section were already explicit in Friedman's texts: statistical economics was affirmed as a politically relevant discipline, not only for being instrumental in the policy-making process, but also for its virtue as a consensus-generating device. We should take into account that this was not a minority view among the profession by then, as it may be noticed in a report delivered by the AEA Committee on Public Issues in 1950. Friedman and three coauthors[4] were assigned to present to a general audience the state of the art concerning the problem of economic instability. One of the first topics they addressed was the relevance of economics in dealing with inflations and recessions, *its forecasting limitations notwithstanding*. Taking sides against Knight's indictment on institutional economists, the AEA committee proclaimed:

> A second misconception is contained in the proposition that effective stabilization policies can be designed only if we understand fully all the causes of fluctuations. Partial knowledge can be very useful for deciding how to act. (DESPRES *et al*. 1950, p.512)

[4] Together with Emile Despres, Albert Hart y Paul Samuelson. The committee had been appointed in 1947.

Even if the calculations involved in planning were out of reach for any statistical economist, the policy-maker could still benefit from concrete forecasts on the evolution of certain economic variables. On the basis of this partial knowledge, the profession could reach a consensus on which policies were most convenient to the country (DESPRES *et al.* 1950, p. 505). I.e., they deserved to be trusted by the public opinion, their usual discrepancies notwithstanding (p. 511).

The connection between partial knowledge, predictions and consensus among the profession was even more explicit in another paper by Friedman alone on the stability issue –published the year after the committee formed. In «A Monetary and Fiscal Framework for Economic Stability» (FRIEDMAN 1948b) he argued for an economic agenda which would lead to the attainment of a number of consensual values such as *freedom, efficiency,* and *economic equality* (FRIEDMAN 1948b, p. 134), on which not many *disinterested citizens* would disagree. If the profession could be persuaded first of the convenience of the agenda –«the greatest common denominator of many different proposals»–, it may be presented as a «minimum program for which economists of the less extreme shades can make common cause» (FRIEDMAN 1948b, p. 135). Friedman's agenda consisted of four main points, being its core stated as follows:

> The essence of this fourfold proposal is that it uses automatic adaptations in the government contribution to the current income stream to offset, at least in part, changes in other segments of aggregate demand and to change appropriately the supply of money. (FRIEDMAN 1948b, p. 139)

The case for this proposal was thoroughly presented two years later in «The Effects of a Full-Employment Policy on Economic Stability: A Formal Analysis» (FRIEDMAN 1951). Full-employment policies were usually analysed on the basis of a four-variable model: consumption (*C*), income (*Y*), investment (*I*) and government expense (*G*).

$$Y = C + I + G$$
$$C = f(Y)$$

The income level Y_0 which will bring about full employment depends directly on government expense, which must counterbalance *I*. Stability is therefore the stability of income Y. The discussion of the effects of discretional government action proceeds in a way inspired by «the theory of statistics rather than economic theory» (FRIEDMAN 1948b, p.121), more precisely by the statistical analysis of variance[5]. Income at *t Z(t)* is decomposed into two variables, the first one *X(t)* measuring the level attained without a full-employment policy, the effects of which being measured by the second one *Y(t)*. The analysis of income stability would thus work analogously to that of variance:

> For X or Z the variance measures the fluctuations in income in the absence or presence of a countercyclical policy. For Y the variance may be regarded as measuring the magnitude of the countercyclical action taken. (FRIEDMAN 1948b, p.123)

[5] FRIEDMAN 1949, p. 951, from which the idea grew out (FRIEDMAN 1948b, p.121n.)

In this way, it would be possible to analyse in which conditions the variance of Z is less than the variance of X, obtaining Y's optimal value.

Therefore, Friedman concludes, the variance of Y and its coefficient of correlation with X allow us to classify any full-employment policy according to its effects on economic stability. Yet, statistics would not allow any planner to control the cyclical variations at will. Quite the opposite: only automatizing the response, Friedman argues, would minimize the delay between the variation to counteract and the action which would counteract it –or, in statistical terms, to approximate the value of the correlation between X and Y to the desired –1. Therein lies, according to Friedman, the *positive* superiority of a monetary and fiscal programme based on automatic reactions over discretional policies, beyond their normative convenience to each particular citizen.

These two papers exemplify thus the role that «The Methodology»'s first section assigned to prediction in the generation of public consensus about particular policies: as long as the divergence lies in the policy to be chosen, it may be resolved on a purely consequentialist basis through statistical analysis. Given that positive economic predictions are independent of any normative commitment on the part of their authors, they deserve to be trusted by any *disinterested citizen* when a decision on the policy to adopt is at stake[6]. In spite of its brevity, «The Methodology»'s first section reflects thus a significant part of Friedman's professional experience, a real methodological concern on the role an economist could play in promoting public policies without losing her scientific integrity. The question we are going to address in the next section is how «The methodology» contributes to such a goal.

3. HOW TO DISAGREE ON PREDICTIONS

In the lapse between meeting Hotelling in Columbia (1933) and getting back to Chicago (1946), Friedman's statistical qualification grew incessantly: on the practical side, he extended his training first at the NRC (1935-1937) and later at the NBER (1937-1940), where he collaborated with Simon Kuznets while completing his dissertation; on the theoretical side, he could also benefit from his two years appointment at the Statistical Research Group (1943-1945)[7]. Friedman engaged also in a collective effort to renew the teaching of statistics in American higher education, a task he undertook first during his brief stay in Wisconsin and later on in Chicago, where he contributed to the creation of a separate Department of statistics (OLKIN 1991, pp. 125-127). Friedman was therefore extremely well qualified not only to advocate for predictive theories, as he did in «The Methodology», but to settle a methodological standard on what should count as a good prediction. He did not, however, neither in his 1953 paper nor afterwards[8]. The methodological dilemma that arises therefrom may be stated as follows: can we really base

[6] Yet, although unmentioned in «The Methodology», attention should be also paid to the fact that *professional consensus among economists on the most convenient policies* is somehow required for any general consensus to be reached (FRIEDMAN 1948b.; DESPRES *et al.* 1950). Or conversely, as Rose D. Friedman put it: «It is not difficult to see why different layman might have different predictions about the economic consequences of certain actions when the experts disagree» (FRIEDMAN & D. FRIEDMAN 1998, p. 217). For layman and experts alike, the way to attain consensus is the same one: predicting. This topic will be taken up again in §4.
[7] For further details, cf. FRIEDMAN & D. FRIEDMAN 1998, pp. 67-76 and 125-47; a methodological analysis in TEIRA 2003b
[8] Cf. TEIRA 2003a for an extended discussion.

a consensus, both public and professional, on predictions without any criterion to assess them? «The Methodology» proves to be extremely effective in discriminating politically relevant vs. irrelevant economic theories, once we assume that a predictively empty theory is *eo ipso* politically useless. However, lacking such a criterion, the particular predictive procedure that Friedman had in sight seems to be particularly capable of manipulation today, suspicious as we have become after the sociological turn in the study of science. In order to show it, let us examine first how Friedman achieved predictions.

By way of example[9], let us focus on a NBER joint monograph, part of which was also submitted to Columbia as a doctoral dissertation by Friedman. Friedman and Simon Kuznets finally published it under the title *Income from Independent Professional Practice* in 1946. Its methodological relevance was emphasized by Friedman himself, who declared in 1946 to E. B. Wilson that its chapters 3 and 4 were his most successful pieces (STIGLER 1994, p. 1200): the balance between demand theory and statistical analysis exemplified a particular combination of the approaches to economics developed in Chicago and Columbia which came to distinguish Friedman's own style. Besides, the study was explicitly aimed at reaching «conclusions relevant for public policy», educational in this case (FRIEDMAN & KUZNETS 1945, p. v). Friedman's expertise in income analysis was now applied to discern whether professional workers «constitute a "noncompeting" group»; i.e., whether their number (and hence their income) was exclusively determined by the «relative attractiveness of professional and nonprofessional work», or rather by the number of prospective students who count on the particular resources required to pursue the career in question (FRIEDMAN & KUZNETS 1945, p. 93).

Among the five professions considered, medicine and dentistry provide particularly suitable data. Given that both professions require similar abilities and training, we should expect the prospective practitioners to choose between them mainly on the basis of their respective «level of return» (FRIEDMAN & KUZNETS 1945, p. 123). Did the differences in average income between doctors and dentists correspond thus to the equilibrium level, or do they rather point out to an entry restriction –such as the licenses imposed by the American Medical Association?

In our opinion, the most salient methodological feature in these two chapters lies in the way demand categories are redefined so that predictions can be derived thereof. Instead of searching for an empirical counterpart that corresponds to its theoretical definition, Friedman restates demand theory so that it corresponds with the statistical variables at hand: e.g., careers would be chosen by according to the average expected income as reflected in the arithmetic mean (FRIEDMAN & KUZNETS 1945, pp. 65, 145); prices and expected income would approximate the commodities offered by doctors and dentists, in the absence of a definite unit to measure them (p. 155); regression analyses performed separately on the 1934 and 1936 data for *per capita* income of both professions, their number per 10.000 population and general *per capita* income (pp. 161-173) would do as proxy to discern whether the difference between their average incomes lies at the equilibrium level. Though admitting that none of these variables would account for the behavior of any particular individual, Friedman and Kuznets argue that they may

[9] For an extended analysis cf. TEIRA 2003b, where *A Theory of Consumption Income* is also appraised.

nevertheless explain the behavior of the group of prospective entrants as a whole (p. 96). In short, even if there is not a perfect correspondence between theoretical concepts and the data, it is preferable to relax our definition of the former so that predictions may be derived.

Indeed, the alternative at hand (rigorous definitions without an empirical counterpart) seemed to Friedman methodologically undesirable. Friedman opted for avoiding «schizoid concepts», i.e., those «thoroughly competent in the field of deductive analysis but utterly incompetent for quantitative analysis» –as Friedman claimed in a joint paper with Allen Wallis of the same period (WALLIS & FRIEDMAN 1942, p. 176). The more mathematical constraints are imposed by the definition a theoretical concept, the less value it will have for the organization of empirical data (WALLIS & FRIEDMAN 1942, p. 186). This sort of «taxonomic theorizing» was precisely the mark of the Walrasian approach, which was canonically criticized in Friedman's 1946 review of Lange: no empirical counterpart will correspond to those abstract taxonomies in the data (FRIEDMAN 1946, pp. 286-89).

The core of the dichotomy between Walrasian and Marshallian approaches seems to lie in the way theoretical concepts are defined: Walrasian *taxonomies* are opposed to Marshallian *filing cases*. Whereas theoretical concepts in the former were defined by purely logical means, producing entirely abstract taxonomies without correspondence in the data, in the latter they were redefined with a view to obtain a classification of the data. We may better understand what this classification issue had to do with predictions if we take into account Friedman's predictive technique of choice: linear regression enhanced by the analysis of variance.

It was probably Hotelling who introduced Friedman to this technique in Columbia, given that he counted among the few in the USA who were then abreast of Ronald Fisher's recent breakthrough in statistical inference (FRIEDMAN 1992, p. 2131; STIGLER 1996). According to the teaching of Fisher[10], for a regression curve to be traced the first step consisted in classifying data in a *contingency table*, so that every observed figure was *filed* under a given variable. On the basis of this classification, contingency correlations –and eventually regression– may be calculated. Therefore very strict definition of the variables would make unfeasible the classification of the data, preventing thus predictions from being obtained. Handling variables in a more Marshallian spirit would allow the economist to render a theory predictively more fruitful.

Let us return now to the opening question of this section: in view of all this, why did not Friedman set up a standard in order to assess predictions? Part of the answer may be that such a standard could have affected the very definition of the variables involved, restricting its predictive capacity. On the other hand, it seems as if «The Methodology» aimed more at discriminating between predictive and non-predictive approaches than at characterizing good predictions. For Friedman, choosing between «Marshall» and «Walras» was not an entirely methodological issue: it depended also on «the purpose for which the theory is constructed and used» (FRIEDMAN 1953b, p.90).

[10] In this respect, an extension of Galton and Pearson's approach, also adopted by Hotelling later on. Allen Wallis still used it in his 1956 manual (WALLIS & ROBERTS 1956). For a superb analysis of this intellectual tradition, cf. ARMATTE 1995.

Part of Friedman's purposes was to derive politically relevant predictions, as we saw in section two. In this respect, a rough prediction is better than none: even if the theoretical relevance of the variables analyzed by Friedman and Kuznets was at most conjectural, they did not hesitate in blaming on the medical licenses the difference observed in the average income between medicine and dentistry[11]. Conversely, if policy conclusions were always to rest on a prediction, Walrasian approaches were inherently useless for the policy-maker, as Friedman often pointed out to[12]. Irrespective of its purported precision as a demarcation criterion, prediction proves to be extremely discriminating when it comes to the political efficiency of an economic theory.

However, in the aftermath of the science wars, the mere claim that such a criterion may pick out theories being at once positive and politically useful is clearly suspicious. Friedman's definitional strategy put in question a central claim in our methodological received view: *theoreticity*. In order to guarantee that each theoretical term had a clear empirical (*positive*) counterpart rigorous definitions were called for. From this point of view, it is not strange that Walrasian axiomatizations have been so often appreciated by philosophers of science (e.g., MONGIN 2003). However, when rigor in definitions is relinquished with a view to facilitate predictions, the sociological challenge to the neutrality of scientific classifications seems most threatening. According to a tradition dating back to Durkheim and Mauss[13], our most basic classifications of sense-data are inescapably biased by social interests, since the concepts we use to this effect are rooted in our daily practice. There is no disinterested ground on which to base scientific concepts. Now, in our opinion, even those who disagree with Bloor should concede that Friedman's methodology does not provide such a disinterested ground. Quite the contrary: for those economists whose daily practice consists in obtaining predictions with a political view, it is essential that the definition of theoretical concepts is loosed so that the data they are provided with could be classified and predictions follow. Yet, once the rigor of the definition is relaxed, who could guarantee that it will not be manipulated in order to obtain interested predictions?

By way of example, let us recall that the conclusions of Friedman and Kuznet's analysis were called in question by the American Medical Association. Apart from a few years delay, it took a «Director's comment» by Carl Noyes (FRIEDMAN AND KUZNETS 1945, pp. 405-410) to be added before the NBER accepted the publication. The note's major concern was to state certain «reservations» over the scientific validity of the results obtained in chapters 3 and 4. More precisely, to argue that it could have been different, had the authors' introduced additional distinctions (e.g., specialists vs. general practitioners) in their analysis of the data: i.e., *different classifications*. In this respect, Friedman's methodological theses on prediction as the real mark of value-free science fail to provide an adequate defense against objections on the possibility of manipulating the classification of the data according

[11] «The analysis is necessarily conjectural and our quantitative results are only a rough approximation. But the problem is real, and a rough approximation seems better than none» (FRIEDMAN & KUZNETS 1945, p. viii).
[12] E.g., FRIEDMAN 1946a, p.300; 1947a, p.316
[13] After all, *categorization* is a central concern in many a sociological approach to science: the starting point was E. Durkheim and Marcel Mauss 1903 essay «Primitive Classification». BLOOR 1982 vindicated its relevance for the *Strong program*. Philip Mirowski introduced this tradition in the sociology of economic knowledge in his *More Heat than Light* (MIROWSKI 1989, p. 412).

to the particular predictions you want to derive thereof. How much *precision and conformity with our experience* a prediction should render to rule out any possible bias?

Friedman was obviously not unaware of this particular bias –Ronald Fisher himself had the occasion to warn him when the Friedmans visited him in Cambridge, only a year after the publication of «The Methodology» (FRIEDMAN & D. FRIEDMAN 1998, p. 244). We may wonder why he left this objection unanswered. For the moment, the only available clue is the one provided by Rose Director Friedman:

> I have always been impressed by the ability to predict an economist's positive views from my knowledge of his political orientation, and I have never been able to persuade myself that the political orientation was the consequence of the positive views. My husband continues to resist this conclusion, no doubt because of his unwillingness to believe that his own positive views can be so explained and his characteristic generosity in being unwilling to attribute different motives to others than to himself. (FRIEDMAN & D. FRIEDMAN 1998, pp. 218-19)

That is to say, a principle of mutual methodological trust, which «like most ideals» is «often honored in the breach» (FRIEDMAN 1967 p.5). In the end, the production of positive predictions of political use would require a sort of tacit normative commitment on the part of the economist: «to serve the public interest», even if self-interestedly (FRIEDMAN 1986, p. 9). The obvious question is *why?* A tacit consensus may have existed still early in the 1950s among a cohort of American economists who had been attracted to the profession in the lower reaches of a big depression to be subsequently recruited by the government to fight it (FRIEDMAN & D. FRIEDMAN 1998, p.34). However, it is dubious whether this consensus still exists today. As we pointed out in section 2, Friedman's claim was quite the opposite: consensus on the best economic policy available, both among professionals and general audiences, was to be grounded on predictions, so that its normative neutrality should be guaranteed right from the beginning. If we still expect economics to play such a consensual role in our public sphere, we should somehow restate this previous tacit consensus among the profession. A reexamination of «The Methodology» in a contractarian light might provide several clues to achieve it.

3. MAKING THE CONTRACT EXPLICIT

Let us then reason *as if*. What if we re-read Friedman's methodological case as if it were a plea addressed to his fellow economists in order to gain the trust of the public opinion on the results attained by the profession? The problem would then be how economists can signal their lay fellow citizens that the advice provided by the former is scientifically sound. Stated in terms of the economics of information, Friedman's plea may be thus restated as the economists' part in *a contract* (as «agents») with the *consumers* of their science (the «principals»). The parties to the contract find themselves in an *asymmetrical* situation: those who produce knowledge can appreciate better the true quality of their output (or their own capabilities as producers) than those who consume it. Therefore, the demanders' desideratum would be that the contract were designed in such a way as to oblige the suppliers to work efficiently in the production of items (of knowledge) having the highest possible utility for them.

The principals' main difficulty is that they are often incapable to control by themselves whether the contract is being complied with by the agents or not, or to what extent it is. Even more, they are unable to design by their own means something like an «optimum» contract: ironically, the principals would perhaps need to hire an economist to design for them an optimum contract. Due to this inability of the principals, to gain the trust of these, scientists would be forced to set up the contract as an agreement *among themselves*, which would serve as the *constitution* of their discipline. Under its rule, the consensus they reach and/or their past technical success would do as *signals* of epistemic quality. However, this also opens room for *moral hazard* problems, since, being the consensus among experts the only signal users may often have of the quality of a researcher or of the «knowledge» produced by her, the losers in the race for a discovery could simply deny that real knowledge has been attained by other colleagues, preventing as a result the constitution of that consensus.

As we saw in section 2, throughout the 1930s and the 1940s, Friedman often found himself acting as the agent for many different principals: first as a member of the NRC task force researching on consumer incomes and expenditures for the Roosevelt administration and later as the *Principal economist* at the Treasury's division of tax research. It may be said that in this case the principal's trust in the use of statistical economics had already been gained at least a decade before Friedman came to Washington –e.g., during the Hoover administration: cf. BARBER 1985, pp. 8-9. However, as we pointed out before, different views on the political use of statistical economics were in dispute among the profession in the early 1930s, a debate which was not completely closed by the time the AEA commission addressed the problem of economic instability late in the 1940s. The fact that Roosevelt could have initially ignored Keynes himself may indicate that choosing the *right* economist was still difficult, and the disagreement among the profession certainly did not contribute to simplify the choice. Both the AEA report and Friedman's parallel paper coincided in the need of a professional consensus that ensured that the best policy could be implemented. Furthermore, Friedman was arguing for a particular procedure to reach it, i.e., to judge each alternative according to its predictive reliability. His 1953 proposal of a *methodological constitution* for the economic profession took this rule as its cornerstone. Once this rule was adopted, economics would turn into a positive endeavor –in our terms, a trustworthy alternative for every principal.

A contractarian assessment of Friedman's proposal requires further precisions on our notion of a «social scientific contract». Basically, the aim of such a contract is to render compatible the incentives of the people supporting scientific research (i.e., the desire for *useful* knowledge), on the one hand, and the incentives of researchers themselves. These are mainly conceived of as seeking for recognition in return for their discoveries, as well as for other sorts of privileges that may result thereof. There are two possible sources of divergence between the researchers' interests and those of their «customers»: firstly, it may happen that the opportunity arises for the scientist to obtain recognition without producing real discoveries; secondly, their research domains may be really worthless for the customers. But the incentives of the researchers themselves may also be in mutual conflict, not only because of the competition to attain better results, but because a scientist may simply opt for denying public recognition to her *rivals* even when she believes that they really deserve it. A well-designed social scientific contract is, then, a set of *norms* that make researchers behave in a «honest» way towards those who support the production of science (producing useful knowledge) and towards their colleagues (e.g., acknowledging their discoveries when it is due). Though different scientific communities or schools may have different «social contracts», their essential and common elements should be the following ones:

a) A set of *methodological rules*, telling what actions of a researcher (among them, particularly, what assertions) is she entitled or committed to perform; these norms are of an *inferential* nature, in the sense that they connect the previous entitlements and commitments of a researcher, or by others, with a specific action which can be seen as their 'consequence'; needless to say, this type of norms has been the main traditional topic of the philosophy of science.

b) A set of *scoring rules*, which, formally understood, is a function transforming the entitlements and commitments acquired by a researcher into *publicly expressed 'scientific merit'* (or her 'score'); these norms serve basically to define the *significance* of a scientific item as well as the *competence* of a researcher;

c) Lastly, a set of *resource allocation rules*, which determine what and how many scientific resources are to be apportioned to a researcher according to her absolute or relative merit; violations of these rules can also modify a researcher's score if she is considered as responsible of the misallocation.

Traditional views of the «social contract» of science had, in the first place, an almost absolutely naive conception of the second and third types of rules. As to the former, it was assumed that scientists could be relied on for assessing the score of their colleagues; concerning the latter, the deference of the public towards the scientists' decision on whom among them deserved being funded was simply presupposed. Friedman was probably not far from sharing this «positivist» view techniques (FRIEDMAN 1986, p.8). However, after the social turn in science studies, trust in science seems to require an explicit commitment to scoring and allocation rules, if scientists scientists and non-scientists are to reach a reasonably efficient and self-supporting equilibrium in the game of science.

In this respect, the conclusions of section 2 seem devastating. It may happen, for instance, that a *quack* succeeded in deceiving the public opinion producing apparently good predictions though theoretically irrelevant (FRIEDMAN 1991, p. 36). If scoring rules existed, scientific economists would penalize the deceiver, but that would require in turn that a clear standard to assess predictions were settled. After our discussion in section 2, we know that there is no such one. Friedman seems rather to opt for relying exclusively on the personal integrity of each economist in producing predictions, on a purely reciprocal basis. As for the orientation of lay audiences in funding economic research, Friedman seems to doubt its very possibility[14], his previous methodological plea notwithstanding.

[14] E.g.: «There is no satisfactory solution to the dilemma posed by the propositions: (1) there is a body of "positive" economics that can yield reliable predictions of the consequences of change; (2) there are "experts" in positive economics; (3) differences about the desirability of governmental policies often reflect different beliefs about the consequences of the policies –conclusions of positive economics– rather than different values; (4) there is no simple litmus test by which a citizen can decide who is an "expert" and who is a

4. FRIEDMAN REWRITTEN?

Fortunately, other methodologists are now facing this challenge. By way of example and conclusion, we would like to transcribe here a singular piece we came across while surfing the Internet a few weeks ago. The manuscript, signed by an uncertain *M. Menard*, was posted in an old site on Friedman, no longer active –as a matter of fact, the domain was cancelled soon afterwards. It deserves, however, attention from a contractarian viewpoint, as it rewrites a substantial part of Friedman's 1953 paper in exactly the spirit of our reconstruction. *Stat rosa pristina nomine...*

THE ECONOMICS OF A POSITIVIST METHODOLOGY

1. THE RELATION BETWEEN POSITIVE AND NORMATIVE ECONOMICS

Confusion between positive and normative economics is to some extent inevitable. The subject matter of economics is regarded by almost everyone as vitally important to himself and within the range of his own experience and competence; it is the source of continuous and extensive controversy and the occasion for frequent legislation. Self-proclaimed 'experts' speak with many voices and can hardly all be regarded as disinterested. The conclusions of positive economics seem to be, and are, immediately relevant to important normative problems, to questions of what ought to be done and how any given goal can be attained. Laymen and experts alike are inevitably tempted to shape positive conclusions to fit strongly held normative preconceptions and to reject positive conclusions if their normative implications -or what are said to be their normative implications- are unpalatable.

It seems that positive economics should in principle be independent of any particular ethical position or normative judgments, for it deals with 'what is', not with 'what ought to be'. Its task, it has been sometimes argued, is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances, and so, its performance should be judged by the precision, scope, and conformity with experience of the predictions it yields. In short, positive economics should be an 'objective' science, in precisely the same sense as the physical sciences are assumed to be. Of course, the fact that economics deals with the interrelations of human beings, and that the investigator is himself part of the subject matter being investigated in a more intimate sense than in the physical sciences, raises special difficulties in achieving objectivity at the same time that it provides the social scientists with a class of data not available to the physical scientist. But, after all, neither the one nor the other makes surely a fundamental distinction between the goals and methods of the two groups of sciences.

More importantly, normative economics cannot be independent of positive economics. Any policy conclusion necessarily rests on a prediction about the consequences of doing one

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"quack"; yet (5) even though the patient is incompetent to choose the physician, there is no alternative in a free society.» (FRIEDMAN 1975, p.x) thing rather than another, a prediction that must be based on positive economics. Governments wanting to improve the living conditions of their citizens, or wanting at least to maximize the chances of being reelected by favoring a majority of their constituency, need to be confident about the likely consequences of the policies that 'experts' are recommending. It is then both in the interest of the economist and of the politician having a corpus of substantive hypotheses, which are successful when judged by its predictive power. On this basis, the judgment has been ventured that currently in the Western world differences about economic policy among citizens who try to be disinterested derive predominantly from different predictions about the economic consequences of taking action -differences that could perhaps be eliminated by the progress of positive economics- rather than from fundamental differences in basic values, differences about which men face ultimately a choice between fighting or showing tolerance. For example, in the debate about the so called 'basic income', no one seems to deny that the ultimate goal is to attain an acceptable standard of living for all, but strong differences arise when predictions on whether this goal would be attained or not with the application of that policy, and at what costs, begin to be drawn. The same takes place in numerous other examples. Hence, the argument follows, achieving consensus about the relevant consequences of each economic policy would reduce to a high extent the dissensus existing about alternative courses of political action.

This is, of course, a 'positive' statement to be accepted or rejected just on the basis of empirical evidence, but this evidence could only be achieved if positive economics 'advanced' as much as to generate a degree of consensus on empirical predictions much higher than what has been attained until now. On the other hand, an apparent consensus about economic predictions can derive either from an 'appropriate' scientific methodology, or from any political strategy to silence those making different predictions, and the same dilemma arises regarding a possible consensus about the normative implications of those predictions. If this judgment is valid, it means that the normative relevance of positive economics (i.e., the usefulness of its empirical predictions for the fostering of our social values) can only be assessed through an analysis of the conditions under which economic 'knowledge' is generated, discussed and applied, in order to understand whether the *actual* conditions can be expected to generate a corpus of predictively useful generalizations about economic phenomena, or whether some amendments of those conditions are both desirable and feasible.

2. POSITIVIST ECONOMICS.

Any normative judgment presupposes some particular values, for there is no such a thing as an absolutely impartial evaluation. When we describe someone as 'disinterested', we simply mean that, in the assessment of positive as well as of normative propositions, he is adopting a *moral* perspective which does not only take into account his own private interest and opinions, but also the welfare and the opinions of other people. However, since different individuals may obviously have different moral values, the judgments of 'disinterested' evaluators need not coincide, although a higher degree of agreement can be expected than in the case of 'interested' agents. The first essential point in making any normative pronouncement as 'experts' is, then, to openly declare the moral values which underlie that judgment. In the argument elaborated in this paper, these values reduce to the following: *the point of view that must be adopted in order to evaluate the methodological practices of economists is essentially the point of view of the citizens*. After all, policy makers have to use the best available economic knowledge in order to promote the welfare of the members of the political community. Under a democratic regime, this demand transforms easily into a necessity, for those parties most capable of putting into use the 'right' economic knowledge will tend to be the winners in the political competition. The task left for us, economists, is then that of 'supplying' those theories, models or hypotheses which can be most efficient in the formulation of 'right' economic policies, i.e., policies that *actually* promote the interests of the citizens of a democratic society.

As a methodological ideal, the positivist conception of science is certainly more appropriate for orienting the practice of economics towards that goal than the methods which, according to a common interpretation, are customarily employed in our discipline. Committing surely a blatant oversimplification, current economics is usually described as divided into two camps: on the one hand, 'mainstream' economics would basically be organized as a kind of disguised mathematics, in which the production of *theorems* is seen as a much more important job, and much more praiseworthy, than the discovery of *regularities* about the empirical world. Of course, the formal models those theorems are about are usually devised as abstract representations of possible economic circumstances, but no much effort is put -the criticism follows- in discussing how can one know whether a concrete empirical situation is better represented by means of a specific formal model or by a different one. So, the practical usefulness of this model building activity is in general rather low, save perhaps in producing Nobel Prize winners. On the other hand, a heterogeneous bunch of 'heterodox' approaches would permanently complain about the lack of 'realism' of the models produced by mainstream economists; unfortunately, the positive contributions of these critical schools either consist in mere descriptive explorations of the 'ontology' to which the orthodox models fail to respond, or in the development of formal models presumably more 'realistic' but in the end no more efficient in the discovery of true empirical regularities.

From a positivist point of view, the common mistake of these two approaches, at least in their unadulterated forms, would be the presumption that *truth* is the basic value of scientific research, instead of *predictive capacity*. Mathematical economists glorify theorems because, as pure formal statements, these are necessarily true, whereas heterodox economists criticize the models from which those theorems are derived because their assumptions are clamorously false. But, clearly, what makes of a theorem an interesting one is not just its truth, for all of them are equally true but not equally relevant, and likewise, the practical relevance of a model does not depend on the realism of its assumptions, but only on the accuracy of its empirical predictions.

In general, economists, lacking the capacity to experiment with most of the parcels of reality they study, have traditionally developed a method which is based on introspection and casual observation for guaranteeing the truth of the *premises* from which their argument start, and on logico-mathematical deduction for guaranteeing that those arguments are truth-preserving, and hence, that their *conclusions* are also true. But we may doubt that, from the point of view of practical relevance, this strategy is optimal. For, in order to successfully apply economic knowledge to reality, the empirical accuracy of the conclusions is much more important than the descriptive truth of the premises, and hence, a theory with false assumptions (such as simplifications and idealisations) but from which many accurate conclusions follow is much more useful than a theory with only true axioms but from which just a couple of trivialities can be derived, even if the first theory has a few false empirical implications as well.

Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, for a hypothesis is significant if it 'explains' much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances around the phenomena to be explained and permits valid predictions on the basis of them alone. In general, both the process of abstraction (for constructing hypotheses) and the process of prediction (for testing their validity) demand that the theory is mathematically well organized. Mathematisation is just an economical tool for achieving a great deductive power with a tractable set of premises; it must not be pursued for its own sake, and less still has it to be hoisted as the dominant criterion for judging the validity of an economic theory. In the end, predictive success must be the ultimate criterion, if economics wants to become a socially relevant discipline.

Of course, all this does not mean that political pressures must be the exclusive, or even the most important guide of economic research. As in any other field of science, sheer curiosity is often the most powerful stimulus towards discovery. The truly significant point is that the method employed for selecting the 'best' theories or models, even if this selection is carried out with no interference from politics, should favour those hypotheses with the highest predictive success. Only if economists follow this 'positivist' method will they advance towards the ultimate *practical* goal of their science, which is the development of a theory that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed, and will they also satisfy their *intellectual* desire of revealing how superficially disconnected and diverse phenomena are manifestations of a more fundamental and relatively simple structure.

3. SOME IMPLICATIONS FOR AN ECONOMICS OF ECONOMICS.

The abstract methodological issues we have been discussing in the past section serve to describe the ideal form economic knowledge should have, 'ideal' from the point of view of the common citizens, who, in a sense or another, are supporting economic research. We have referred in passing to some ways economics is usually practiced, which, all our gross simplifications considered, do not seem to correspond to this ideal. I have not examined to what extent the spread of these 'wrong' economic methods depend on the specific academic organization of inquiry within the field of economics. Perhaps all intellectual practices have some kind of 'internal live' which makes some ideas to grow no matter what the institutional setting where they disseminate is. Indeed, past proponents of a positivist method for economics have put the question as if all that mattered were the *logical persuasiveness* of their proposals, and not so much the *interests* of the people to which the proposals affect. In this sense, they have actually preached with the voice of the methodologist, rather than from the attitude of a 'positivist economist' like the one they pretended to be.

However, the assumption of single-minded pursuit of self-interest by individuals in competitive 'industries' (and the academia can be taken to be such an 'industry') has worked very well in a wide variety of hypotheses, and it is therefore likely to seem reasonable to us, economists, that it may work in this case as well. The epistemologist or methodologist, on the other hand, is accustomed to a very different kind of model for explaining the spread and the working of scientific ideas, in part because he uses a different body of indirect evidence (the 'logical connections' between ideas, rather than the phenomena of interdependent choice economics is usually about), but of course, neither the evidence of the economist nor that of the epistemologist is conclusive: the decisive test of any hypothesis is just whether it works for the phenomena it purports to explain. So, I propose to analyze the question about the means to establish an 'optimum' economic method from the point of view of economic theory itself.

Seen from this economic viewpoint, a fundamental problem for the instauration of an appropriate economic method is that of *assymetric information*: the people for whom the 'right' method is appropriate (the citizens) are not those who can better appreciate what the power of each alternative method is (the economists). The desideratum for the demanders of economic knowledge (the citizens-principals) would be that of designing a contract so that the suppliers (the economists-agents) worked efficiently in the production of items of knowledge having the highest possible utility for the former. The principals' main difficulty, however, is that they often cannot control by themselves whether the contract is being obeyed by the agents or not, and usually they even ignore how to design something like an 'optimum' contract; they have trust some economists for designing it, and this clearly leads to a vicious circle, for how to know who are the 'good' economists in the first place?

We need also take into account that the citizens-principals do not form a homogeneous group: a few of them have much more political power than the rest, and all have interests, values and beliefs which are in conflict with those of many others. *Collective action* problems arise as well, hence. For example, perhaps the people who had the capacity to enforce an 'optimal' contract in economics would prefer that no real empirical progress were made in this field, in order to keep using the aura of mathematical certainty of some branches of economics to justify before the public some policies designed to favour the most privileged. This possibility -well documented in the history of our discipline-, and in general the plurality of interests existing within our societies, suggest that this plurality should be somehow reflected within the 'optimal' contract.

In this situation, the economists-agents are forced, if they want to gain the principals' trust, to establish the contract as an agreement among themselves, as a *'disciplinary contract'*, so to say, and to use this consensus as a 'signal' of the quality of their 'output'. Unfortunately, this opens also room for 'moral hazard' problems, since, being often the consensus among experts the only signal users may have of the quality of a researcher or of the 'knowledge' produced by her, the losers in the race for a discovery could simply deny that real knowledge has been attained by other colleagues, preventing as a result the constitution of that consensus. The possibility also exists that a majority of the agents 'sign' a contract which is efficient for them, but not for the citizens. In principle, the interests of the economists may be incompatible with those of the citizens because of two reasons at least:

first, scientists may have sometimes an opportunity of attaining their goals without producing real discoveries (for example, by inventing their data), and second, they can devote their effort to fields or problems which are of no obvious value for the people supporting science (for example, by looking just for 'elegant' equilibrium solutions). The 'disciplinary contract' of scientific research has to be seen, then, as a set of *norms* which would ideally make researchers behave in a 'honest' way towards the citizens (producing useful knowledge) and towards their colleagues (e.g., acknowledging their discoveries when it is 'due').

I want to suggest that a good task for future economists will be to devise some alternative, hypothetical 'disciplinary contracts' for the practice of economics, exploring their properties both analytically (by way of game theory, economics of information, social and public choice, etcetera) and, as long as possible, empirically (by way of institutional and experimental economics, as well as history of economics), and subjecting these proposals to severe discussion and criticism not only by the economists themselves, but also by part of political or civilian organisations. To some extent, the result of this discussion would count as an empirical test of the *positive* predictions of those hypothetical contracts about the *normative* judgements they would induce. Many economists will certainly doubt that this task is necessary at all, for they are persuaded that, in the long run, academic competition leads by itself to the adoption of a 'right' disciplinary contract, as much as political competition tends to select those parties who choose the 'right' economic advisors. But the progress experimented during the last fifty years in our knowledge of how to efficiently solve people's main economic problems has been so tiny, and the reasons are so clear to suspect that, under many realistic circumstances, both academic and political competition can lead to stagnation rather than to progress, that the effort of devising a new disciplinary contract for economics is worth the trouble.

M. Menard

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