*A Feasible and Objective Concept of Optimality: the quadratic loss function and U.S. monetary policy in the 1960s**

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Resumo

A introdução de programação linear e função de perda quadrática em economia monetária nos anos 1960 imprimiu marcas no intenso debate sobre o instrumento de política monetária ótimo. Tais métodos foram amplamente utilizados em outras áreas do conhecimento por tornarem simples as soluções para problemas complexos, algo principalmente importante se considerados os computadores da época. Este argumento é também apresentado para explicar sua adoção em economia monetária. Em tal narrativa, Henri Theil e Herbert Simon são citações recorrentes por suas provas do princípio de "equivalente certeza". Este trabalho argumenta que a caracterização do comportamento dos bancos centrais através de funções de perda quadráticas inaugurou um modo uniforme e objetivo de se discutir políticas ótimas, o que estabilizou tal discurso. Para se compreender melhor este processo, é necessário analisar como tais funções originaram-se na literatura de "operations research" e "management science" às quais Modigliani e Simon contribuiram. A tese aqui sustentada é a de que soluções simples eram apenas um dos atrativos a explicar o amplo uso de funções de perda quadráticas em economia monetária.

Palavras-chave: função de perda quadrática, política monetária ótima, equivalente certeza, Henri Theil, Herbert Simon

Abstract

The introduction of linear-quadratic methods in monetary economics in the 1960s tinged the intense debate about the optimal monetary policy instrument. These methods were widely used outside monetary economics because they delivered easy solutions to complex stochastic models. This same reason explains the success of quadratic loss functions according to the conventional wisdom among monetary economists. In this traditional narrative, Henri Theil and Herbert Simon are often cited by their proofs that models with quadratic objective functions have the certainty equivalence property. This attribute made the solution of these models feasible for the computers available at that time. This paper shows how the use of a quadratic loss function to characterize the behavior of central banks inaugurated an objective or uniform way of talking about optimality. In this respect, the discourse on optimal monetary policy stabilized. Moreover, a richer account of the quadratic approach to monetary policy debate emerges by analyzing how quadratic loss functions were used in operations research and management problems by groups of scientists that included economists like Modigliani and Simon. I argue that feasibility is only one important factor that explains the wide popularity of quadratic functions in monetary economics.

Keywords: quadratic loss function, optimal monetary policy, certainty equivalence, Henri Theil, Herbert Simon

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* A more detailed version of this paper is available from the author upon request.

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1 Introduction

In the late 1960s, Milton Friedman and the so called monetarists veered the debate about policy-making away from the prescriptions of aggregate demand management based on fiscal instruments. The propose to rely mostly on monetary policy to achieve low inflation and stable growth was part of an intense controversy among economists and policymakers.

In this contest, several issues were intertwined. For example, questions about the stability over time of the money demand function, about which instruments the monetary authority should use to conduct monetary policy (should it be a price, interest rate, or should it be a quantity, a monetary aggregate?), concerns about the lags of monetary and fiscal policies, about the way economic policy should be conducted (following rules or in a discretionary fashion; adjusting the instruments gradually or not), among others, all pervaded theoretical and policy-oriented discussions at that time.

At the same time, new mathematical methods designed to improve the decision-making process in operational research and management problems were brought to monetary economics, shaping the discussion on optimal monetary policy. This paper is concerned with the particular notion of "optimality" that emerged out of the above mentioned debate in the U.S. circa the 1960s.

In order to implement an "optimal" monetary policy, it is necessary to characterize its "optimal" instrument. In spite of seeming a trivial technical task, the specification of the "optimal" instrument was, on the contrary, a source of a wide contention among economists and policy makers in the 1960s, involving all sorts of arguments. In this debate, there were three main positions taken by different groups of economists, as described by Poole (1970).

To have a better idea on how intensely the "instrument problem" was debated in the 1960s and also how far it was from coming to an end, the words of Alan Holmes (in Federal Reserve Bank of Boston (1969, page 65)), Senior Vice President of the Federal Reserve Bank of New York, are illustrative:

> "The debate over whether the Federal Reserve should rely exclusively on the money stock –somehow defined– as an indicator or a target of monetary policy, or both, continues unabated. While the debate has shed some light on the role of money in monetary policy and the role of monetary policy in the overall mix of policies that affect the real economy, there has been perhaps as much heat as light. And the light that is being generated from the many research studies that have stemmed from the debate is very often dim indeed."

Instead of identifying the members of each of the previously identified groups and making a genealogical account of each of the three positions outlined before, the present paper takes those groups and standpoints as the background for discussing a particular notion of optimality that arose among monetary economists circa 1960, with the use of a quadratic loss function[1]. The paper also shows that this concept of optimality was used to support different arguments on the "instrument contest." More specifically, it analyzes how the use of a quadratic loss function became a widespread and consensual way for talking about optimality of monetary policy among otherwise divergent economists involved in policymaking, like the monetarists and their opponents like Franco Modigliani, James Tobin, Paul Samuelson, and Arthur Okun.

In order to construct a more comprehensive history of the quadratic loss function it is crucial to recognize that it is an episode on the more general movement towards formalization of economics, the increasing use of stochastic dynamic methods, and the consolidation of neoclassicism in the

[1]For a broader picture of the American monetary thought, see Mehrling in Morgan and Rutherford (1998, pages 293-306).

post World War II period. Different aspects of these processes are discussed by several papers in Morgan and Rutherford (1998) and by Klein (2000, 2005), Mirowski (2002), and Weintraub (2002). Dynamic programming problems were not only related to gunners in aircrafts facing moving targets, but also to scheduling tasks so to perform them in the minimum time possible, among several other examples, as presented in the second section of this paper. I shall argue that this latter problem is more relevant for understanding the meaning and usage of the expression “loss function” in monetary economics. The third section introduces the main economists who contributed to the introduction of a quadratic loss function in the monetary policy debate of the 1960s, for whom Simon (1956) and Theil (1957, 1964) are important references. This historical account is contrasted to the perceptions of the quadratic loss function among monetary economists. Finally, concluding remarks close the paper.

2 A quadratic world outside monetary economics

The urge for planning strategic decisions was one of the characteristics of the period around World War II. This period witnessed an outburst of linear-quadratic methods and electronic computers to deal with various kinds of decision problems. A common characteristic to all these problems was the necessity to economize resources in designing strategies to achieve a desired goal.

Another characteristic of this period was that choices under uncertainty became a central issue to decision makers in general and to economists in particular[2]. As a consequence, parsimonious models were desirable not because they were more “elegant” in theoretical terms, whatever that means, but because they required less information to be known *a priori* by decision makers. The crucial advantage of linear-quadratic models, in this sense, was to be parsimonious in a very convenient way: in providing easy solutions to complex models, which in turn, could be computed by hand of by the computers available at that time.

In the 1950s, the scope of application of linear methods expanded from military needs to a broader audience of social scientists (economists included), mathematicians and engineers involved in several non-military problems like how to schedule different tasks so to minimize the time spent on them, what is “the most economical way of transporting materials between various sources and destinations” (Smith Jr. (1956, page 156)), how to distribute the different types of efforts expended on given tasks to obtain the maximum desired result, or how to produce, hire workers, and manage inventories so to “optimally” respond to uncertain demanded quantities, especially for highly seasonal products like heating oil.

As I shall show later, the notion of optimality developed in this period was closely related to economizing resources and information and was practically implemented by postulating decision makers who minimized quadratic “loss functions.”

The mentioned widening of the scope of linear-quadratic methods from military circles to social sciences, to business and to the government was part of an increasing employment of mathematics in these different areas of human action. Linear programming provided a convenient way of combining three features of decision making, following Smith Jr. (1956), perceived as desired in that period: model building (i.e., having an “appropriate” representation of the situation being studied), mathematics (i.e., the tools for manipulating the model), and computation (how to actually carry out the model manipulation).

In the 1950s and early 1960s the research projects on dynamic programming and on quadratic “loss functions” were carried out in many institutions, among of them the RAND Corporation (with Richard Bellman), the Carnegie Institute of Technology (Carnegie Mellon University, since 1967[3]; with Franco Modigliani, Herbert Simon, William Cooper, Charles Holt and their graduate

[2] This connection is explored by Klein (2000, 2005) and Mirowski (2002).

[3] The Carnegie Technical Schools were founded in 1900 by the industrialist Andrew Carnegie. In 1912, it changed its name to Carnegie Institute of Technology (CIT). In the interwar period, by designing interdisciplinary research

students, John F. Muth and Albert Ando, among many others), the Cowles Commission (with Jacob Marschak, Kenneth Arrow, besides Simon and Modigliani as research consultants in the early 1950s), and the Central Planning Bureau of the Netherlands (with Henri Theil) stand out in the narrative of the present paper. Among the sponsors of those research projects we find mainly the Office of Naval Research (ONR)[4] and the Department of Air Force, in the United States.

The group of scientists in the newly created Graduate School of Industrial Administration (GSIA, in 1949) at Carnegie Institute of Thechnology, with financial support of the ONR, is key to the present narrative, as explored later. The GSIA was an important institution in the network of a diverse group of economists, mathematicians, and the new generations of operations researchers and management scientists. The group at GSIA also interacted with the researches at Cowles –as many of them, including Simon and Modigliani, were Cowles' research consultants in the early 1950s–, and with Henry Theil, who was working at the Central Planning Bureau of the Netherlands but visited Chicago several times in the 1950s and 1960s and was also in contact with the group at GSIA.

In this same period operations research was firming its scientific character devoid of strong military connotations. The Operations Research Society of America (ORSA) was created in 1952, the same year in which it started publishing the *Journal of the Operations Research Society of America*[5]. It was "then inhabited mostly by mathematicians, physicists, and engineers," and, according to Simon (in Ijiri and Whinston (1979, page xxiii)), "did not appear (at least to those of us who were then outside) too hospitable to econometricians and other social scientists who brought knowledge of business institutions and marginal analysis to management problems." An analogous society was formed in England at that time.

Paralleling the consolidation of operations research as a non-military science, the 1950s also witnessed the establishment of a scientific community to deal with increasingly complex managerial problems. The Institute of Management Sciences (TIMS) was officially founded in December of 1953, with the objective of identifying, extending, and unifying "scientific knowledge that contributes to the understanding and practice of management," as stated in its constitution, published in the first issue of TIMS' journal, *Management Science*, in 1954[6]. William Wager Cooper, an important figure in the present narrative and then a professor at the Carnegie Institute of Technology, was TIMS' first president[7].

Among the new management scientists there was a clear desire to distinguish themselves from the old management practitioners, who, according to them, were problem-oriented. The new scientists saw in adopting the new mathematical and computational techniques a way to do so, under the guidance of the Institute of Management Sciences and influenced by von Neumann[8].

It was in the same temper of planning decisions (made by the government or by businessmen) in an uncertain world through the guide of mathematical models that a quadratic loss function was introduced, in articles published mainly in journals like *Operations Research*, *Management Science*, but also in *Econometrica*, *Naval Research Logistics Quarterly*, and *The Annals of Mathematical Statistics*[9]. It was a period in which a "scientific" and "objective" notion of optimality was con-

programs devoted to industrial and governmental problems, CIT developed and strengthened its ties with the industry and government. In 1967, the Mellon Institute gave an endowment of $ 60 million to the Carnegie Institute of Technology leading to the creation of the Carnegie-Mellon University.

[4]As argued by Klein (2005), the ONR published the *Naval Research Logistics Quarterly* and was the main institution financing projects on inventory control, as those in the Carnegie Institute of Technology, but also financed a project on the theory of decision-making under uncertainty at Cowles Comission. This project was directed by Marschack, who invited Simon to collaborate with it (see Koopmans (1952)).

[5]In 1956, ORSA changed its journal name to *Operations Research*.

[6]TIMS and the Operations Research Society of America were merged to create INFORMS, the Institute for Operations Research and the Management Sciences, in 1995.

[7]See Cooper's very illustrative presidential; Cooper (1955, especially page 183).

[8]See Flood (1956, pages 179-180).

[9]It is important to emphasize that the focus of the present paper is in how quadratic loss function was incorporated by monetary economists. To this narrative, the important and vast literature on quadratic objective functions in econometrics and statistics (like minimizing the sum of the squared residuals, as done in ordinary least squares) is

structed and used, a notion closely related to economizing resources and information. A feasible concept of optimality was introduced with quadratic "loss functions," in line with the main characteristic of operations research: to be "concerned with optimizing the performance of a system regarded as given for the purpose of the problem" (Halsbury (1955, page 239).)

A quadratic function was understood as an approximation to the "true" objective function and the conditions necessary for having a good approximation were, in general, carefully discussed in the seminal papers outside monetary economics. The main advantage of this approximation is that it renders to the model being built the so-called "certainty equivalence" property, which provides an easier and feasible way of solving complex dynamic problems. In this period, "good" algorithms used to solve a model were those that minimize the number of calculations that a computer would need to execute. This was the main feature of feasibility at that period. Postulating a quadratic loss function would do the trick and turn difficult problems solvable in computers.

In order to appreciate the different meanings of a "loss function," as it was introduced outside monetary economics, and how it was deeply connected with the characteristics of the 1950s and 1960s described before, the next two sections discuss the works of Modigliani, Simon, and others at Carnegie Tech and those of Henri Theil at the Central Planning Bureau of the Netherlands.

2.1 Planning Production and Employment at Carnegie Tech

In 1946, William Cooper, who graduated in economics from the University of Chicago in 1938, left his position as instructor of economics at the University of Chicago during the war (1944-1946) and went to Carnegie Institute of Technology as a (assistant) professor of economics and management science. At Carnegie, Cooper, George Leland (Lee) Bach, economics department head, Edward Schatz, instructor in electrical engineering, and the Carnegie Tech's Provost Elliot Dunlap Smith devised "an imaginative undergraduate curriculum in industrial management to replace the old-fashioned programs in industrial engineering and commercial arts that Carnegie was then offering."[10] William Larimer Mellon, founder of his family Gulf Oil branch, saw this program as a perfect opportunity to implement his beliefs about "young men with combined technical and business training in high-technology industries."[11] The William L. and Mary T. Mellon Foundation donated $ 6 million to create the Graduate School of Industrial Administration (GSIA) in 1949, having George Lee Bach as its first dean. Was with this same spirit of grounding the new management science on mathematical and computational methods that lead Cooper to be one of the founders of the Institute of Management Science (TIMS), as already mentioned. In 1952 GSIA also started a long-term cooperation with the Ford Foundation, which financed substantial part of the programs in behavioral sciences and in economic development and administration, for example.

In September of 1949, Cooper brought Herbert Simon, who referred to himself as a "mathematical social scientist"[12], from the Illinois Institute of Technology –where he was a professor of political science– to become a professor of administration and head of the department of industrial management of Carnegie Tech. Simon's contributed to consolidate the new program at GSIA. Simon had a contact with operations research and management problems before he went to GSIA. After graduating in political science at the University of Chicago in 1936, he was Clarence E. Ridley's research assistant on decision-making in the field of municipal administration (at the International City Managers' Association). After and getting his Ph.D. also in political science at the same university in 1942 (Simon (1992)), Simon worked as a consultant to the International City Managers' Association and the Institute for Training in Municipal Administration from 1942 to late 1940s.

Herbert Simon and William Cooper had a close friendship[13] that started at the University of

not relevant and, thus, not considered here.

[10]Herbert Simon, "A Yankee from Chicago" in Ijiri and Whinston (1979, page xix)

[11]Simon, in Ijiri and Whinston (1979, page xix).

[12]Simon (1992). For more on Simon, see Sent (2005) and references therein.

[13]See Simon in Ijiri and Whinston (1979, page xvii).

Chicago in the 1930s, when both were undergraduate students there[14]. Cooper and Simon worked together in the Bureau of Public Administration, at the University of California, Berkeley[15]. Later, in the 1940s, Cooper and Simon attended the seminars at Cowles Commission, at Chicago, [16] and went to the Carnegie Institute of Technology.

The GSIA at Carnegie Tech was the center of diffusion of linear-quadratic techniques to the military, as well as to government and businessmen[17]. It was there that many of the main actors in the present narrative were working and discussing the use of quadratic loss functions in the mid to late 1950s (influenced, of course, by the work of Arrow et al. (1951) and Bellman[18]). Franco Modigliani joined the GSIA in 952, after a three-year period as Associate Professor and Professor of Economics at the University of Illinois (Chicago). Modigliani already knew Simon (and Koopmans, Marschak, Arrow, among others) from the period when he was an associate of the Cowles Commission, from 1948 to 1950 approximately. The group composed by Modigliani, Simon, Holt and several of their graduate students, as Muth, Ando, and many others, worked for about five years (from 1953 to almost 1958) to the Office of Naval Research and produced more than fifty memoranda as part of their project, called "Planning and Control of Industrial Operations," contracted with the ONR. Modigliani stayed at Carnegie Mellon until 1960, when he went to MIT.

In 1953, after spending a year at LSE, Merton Miller joined the GSIA group at Carnegie Institute. It was there that he started working with Modigliani in the first paper of the now known "Modigliani-Miller Theorem" on corporate finance. More important to the present paper is the fact that Miller stayed at Carnegie Institute until 1961, when he went to the University of Chicago. There, he became Poole's main advisor. As we shall show, William Poole is a very important actor launching quadratic loss function in the monetary debate of the 1960s in the U.S. Therefore, it is clear how important diffuser of linear-quadratic methods the group at Carnegie Institute of Technology was, particularly in monetary economics.

Out of the more than fifty reports produced by the research group at Carnegie Tech, three articles are of great relevance in the present paper. In the first paper, Holt et al. (1955), they discuss the problem of minimizing the costs imposed to the firm by the fluctuations in consumer's orders when firms are unable to have precise estimates of their cost functions. They had a paint company as their case study. The authors first formalized carefully and quantified this decision problem by using a quadratic cost function. In a second step, they calculated the implied linear decision rule and showed how production executives could avoid losses (incur lower costs) by employing this rule instead of "prevailing rule-of-thumb and judgement procedures."[19] The proposed method was easily implementable and implied more profits to the firms. What music could be better to businessmen ears?

Besides providing businessmen an implementable scientific way of deciding how much to produce and how much labor to employ, which resulted in lower costs than the actual decision procedure followed by them, two other characteristics of this first paper are important to the present narrative of the quadratic loss function. The first is the use of a quadratic cost function as an approximation to the "true" function. And the second feature is an specific criterion to judge how good the approximation was.

With respect to the first characteristic, the authors followed the main idea of the prevailing operations research and management science approach, that of "optimizing the performance of a system regarded as given for the purpose of the problem" (Halsbury (1955, page 239)), and instead

[14]Simon went to the University of Chicago in 1932 and graduated in political science in 1936, while Cooper was a Chicago student in 1934 and graduated in economics in 1938.

[15]See: Simon, Divine, Cooper, Chernin. *Determining Work Loads for Professional Staff in a Public Welfare Agency.* Berkeley, 1941. Simon published a summary of this report in *Public Management*, vol. 23, pages 172-175, 1941.

[16]See Simon in Ijiri and Whinston (1979).

[17]For more on the research conducted at GSIA in its beginning, see Cooper (2002).

[18]See Klein (2005) for a detailed discussion about the early works on the "inventory problem."

[19]Anshen et al. (1958).

of having profit maximization as the decision criterion they treated “the scheduling of production and employment from the point of view of the production manager. We assume that sales volume and price are beyond his control, so that revenue is given, and the maximization of profits becomes the minimization of costs. We should emphasize that “costs” are interpreted broadly to include any relevant considerations to which the decision maker chooses to attach importance.” (Holt et al. (1955, page 7))

In implementing the cost minimization criterion the authors found convenient to resort on a quadratic approximation to the cost function, a mathematical form that “is both sufficiently flexible to approximate a wide range of complex cost relationships, and sufficiently simple to allow easy mathematical solution.” [20]. Such a function has the general features the “true” cost function has: “the cost of inventory is high when inventory is large, and high also at the other extreme when inventory is so small that there are frequent runouts of particular products which cause back orders and a high penalty for delayed shipments to customers.”[21] Among several functional forms that matched these features of production cost, the quadratic function perfectly fitted the criterion of delivering easy mathematical solution, because it generates linear decision rules (for production and employment, in the present case) with the property of “certainty equivalence.” This property implies that in order to follow the obtained decision rules the decision maker needs only to know the expected value of the stochastic variables, but no information about the whole probability distributions is required.

The second feature of Holt et al. (1955) relevant to the present narrative is the specific criterion to evaluate how good the proposed approximation to the cost function is. The authors first carefully justify that the several nonlinear components of the cost of a painting factory studied by them (cost of hiring and layoffs; overtime costs; inventory, back order and machine setup costs) can be “reasonably” approximated by quadratic functions. Holt et al. (1955, see page 9) discreetly used a local argument, that the quadratic function “can give a tolerable approximation” over “the range in which the work force are expected to fluctuate.” However, no mention was made to any argument like using a Taylor expansion of the true function around a given point, which was common in mathematics and engineering as a way to simplify some of their problems. However, in the book they published later, while justifying the use of a quadratic cost function, Holt et al. (1960, page 12) connected their assumption to a broad mathematical literature, including the Taylor expansion, by stating that they “have used a mathematical decision structure (...) that fits these objectives [of flexibility, of being a good approximation and easily implementable] and permits us to draw on a large body of mathematical and statistical theory (linear difference equations, Taylor series, Lagrange multipliers, and probability theory).”

However, in contrast to a Taylor-type argument, in which the quality of the approximation of a given degree depends on the distance of any point to the point of approximation, or to arguments of “goodness of fit”, Holt et al. (1955, page 15) argued that a good approximation to the decision problem is that delivering minimal costs.

In the second paper, Holt et al. (1956), now coauthored by John Muth instead of Herbert Simon, the authors showed how the linear decision rules used in the first paper are optimally derived from a quadratic cost function involving inventory, overtime, and employment costs. Optimality here is equivalent to minimizing the expected total cost. Besides this, the authors demonstrated how the numerical coefficients of the rules are computed for any set of cost parameters. This is a much more technical paper than the first one, and the proposed method for solving the derived linear system of equations is a particular case of the general method developed by Muth (1955), but also with insights and concerns on the stability of the computational solution coming from Modigliani (1954). The authors explicitly stated the usefulness of the “certainty equivalence” property and the use of quadratic loss (cost) functions[22].

[20]Holt et al. (1955, page 7)

[21]Holt et al. (1955, page 7).

[22]See Holt et al. (1956, pages 160-161)

It is important to stress here that the notion of optimality is a notion of economizing information in a very particular sense. A decision rule is "optimal in the sense that it is the best action that can be taken on the basis of information currently available" (Holt et al. (1956, page 176)), but the assumption that the objective or loss function is quadratic waives the decision makers the necessity to know the whole distribution of the stochastic variables of their model. The only required information to base an optimal decision are the expected value of the unknown variables. In other words, a quadratic loss function converts the set of all relevant information currently available (i.e., the whole distribution of the random variables) into the smaller set of the expected value (first moment of the distributions) of the stochastic variables of the model.

Finally, the third paper from the research group at GSIA discussed here was published in 1958, in the Harvard Business Review, illustrating their main arguments with the case study of the paint company[23]. The purpose of the authors was to persuade their audience (businessmen and academics related to management science) about the importance of the new mathematical methods that they employed to solve management problems.

To do so, they verbally explained the main advantages of their method, as well as its limitations, trying to generalize them as much as possible, with appealing arguments for the period, like: "Better decisions within a company contribute directly to its profits. Even more broadly, better decisions in many companies can increase the efficiency with which the nation uses its resources –a fact of growing importance as our arm race with the Soviet Union intensifies."[24] In their conclusion, they stress that "the general technique is applicable to scheduling in any plant in which the relevant costs may be approximated by U-shaped curves. Decision problems in areas outside production would also appear to be candidates for the application of mathematical decision rules of the type described. (...) And with ingenuity management will undoubtedly discover still other applications in the future."[25]

In conclusion, linear-quadratic methods became the reference to sound decision-making in the United States, during the 1950s. They were then applied not only to military needs but also reached a broader audience of social scientists and engineers involved in non-military problems. In the 1960s, there was a great effort in "bridging the gap" between the quantitative analysis approach used in those different problems and "the process by which governments formulate their economic policies."[26] Holt (1965, pages 254-255, italics in the original) could not be more emphatic in supporting the idea that quantitative methods should be applied to economic policymaking:

> "*If* the values which are sought by the citizenry and the responsible elected officials can be determined –that is if the most abstract ingredient in the decision-making process can somehow be subjected to objective study and quantitative estimation– the economists can take an entirely different approach in their contributions to the decision-making process.
>
> The problem of maximizing the accomplishment of a welfare function subject to the constraint of the economic relationships can be posed. And when the problem is stated in mathematical language it can be solved by formal methods to determine the action alternatives that are efficient –that is, effective in achieving the desired ends. (...)
>
> (...) This general approach has proved feasible in operations research on business and military decision problems, and it has the potential to tremendously increase the contributions of economists to national economic policy. We are *not*, however, visualizing a benevolent dictatorial technocracy run by professional economists; rather, we are foreseeing a day when economists will be better able to offer sound advice on a professional level to politically responsible decision-makers."

[23] Anshen et al. (1958), which had a suggestive headline: "Here is another area where the new statistical techniques can subtract guesswork, add precision, divide risk, and multiply efficiency."

[24] Anshen et al. (1958, page 51).

[25] Anshen et al. (1958, page 58).

[26] Holt (1965, page 252). Theil also contributed to this volume edited by the Brookings Institution, from a conference of the Social Science Research Committee on Economic Stability on the subject of quantitative planning of economic policy.

In the 1950s and 1960s, the main proponents of linear-quadratic (or "quantitative") methods in the United States, like Herbert Simon and William Cooper, for example, had a good transit in military, governmental and industrial circles. In a different context, "[a]t this same time, Tinbergen and Theil were independently developing very similar techniques for national planning in the Netherlands"[27], dealing with problems of offering "sound advice" to the policy-making authority, as discussed next.

2.2 Henri Theil and a dictatorial welfare maximizer

Before discussing the contributions of Henri Theil to the subject of quadratic loss functions, welfare maximization, and certainty equivalence property, a brief sketch of his life in the 1950s and 1960s[28], might enrich their understanding. Theil was born in Amsterdam on October 31, 1924. After being caught by the Germans and sent to forced labor in a factory in Germany circa 1943, Theil took up economics and abandoned his first studies of chemistry, physics and mathematics at the University of Amsterdam. However, he followed lectures in mathematical statistics and was an assistant to the most prominent Dutch statistician, Professor David van Dantzig. He obtained his Ph.D. in 1951, with a dissertation on inventories and consumer demand theory. In 1952 he started working at the Central Planning Bureau, where he stayed only for about a year. This office was an institute for economic advice to the Dutch government. After that, Theil worked at the International Statistical Center in Calcutta, India.

In 1953, invited by Jan Tinbergen, Theil took over Tinbergen's classes in econometrics at the Netherlands School of Economics, in Rotterdam (now Erasmus University), where he proposed to start a special program in quantitative economics. Supported by Tinbergen, Theil proposed the program to the university and the senate of the Netherlands School of Economics agreed, but put the condition that the program "would not confine itself to economic applications, but would also give attention to business problems, which implied that operations research had to be included in the program." (Kloek (2001, page 264)) However, the final authorization was given only in 1957, since the proposed change required royal consent. In 1955 and early 1956, Theil visited the University of Chicago, where he also proposed the establishment of a research institute in econometrics, with the feeling "that at the Central Planning Bureau it was impossible to give adequate attention to problems of econometric methodology." (Kloek (2001, page 264)) Again with Tinbergen's support, the Econometric Institute started its activities at Chicago in September 1956. In 1966 Theil accepted a joint appointment at the Graduate School of Business and Department of Economics, at the University of Chicago.

Once again, the influence of planning on the economic theory produced at the 1950s and 1960s is clear. Theil (1954) was obviously involved in advising the Dutch government on economic matters and took a welfare approach to a static problem, in spite of Arrow's impossibility theorem, recently published as a monograph of the Cowles Commission, in 1951 (see Arrow (1951).) This theorem basically states that the social decision mechanism resulting from the aggregation of rational individual preferences cannot exhibit the same properties of the latter unless it is dictatorial. This theorem was questioning that a "well-behaved" social welfare function could be derived from aggregating the individual utility functions. More important, such a function would have to exhibit the ranking of only one individual, if it has to exist. Theil (1954, page 61) chose to attribute the government the dictatorial role and to avoid discussions about the impossibility theorem.

For the story Theil (1954) created to justify the dictatorship role of a government being advised by economists, these professionals were able to only report consequences of different policy actions in different scenarios, with no normative judgement on their (sound) advice. He goes on with his econometric approach to economic decision making by borrowing from Tinbergen (1952) the concept of instruments, the variables directly controlled by the government, and targets, or "indirect variables"

[27]Simon (1992).

[28]See Kloek (2001) and Barnett (2001)

as he called them, those variables that the government is interested in affecting. With this distinction Theil represented the policymaking problem as maximizing the "decision-maker's desires," represented by the expected value of a welfare function that has as arguments the "instruments," subject to the economy's behavior, represented as a system of linear equations[29].

Therefore, the policymaker's problem is analogous to the usual consumer's problem, both represented as a constrained maximization. Throughout the paper there are several rhetorical comparisons of the welfare function attributed to the government with the "utility function of the traditional theory of consumer's behaviour" (Theil (1954, page 64).) Moreover, Theil (1954, page 69), once more mimicking the consumer's demand theory, discussed several geometrical representations of the von Neumann-Morgenstern social welfare function for different economic policy problems to conclude that "[i]t appears from the four diagrams that the indifference curves can presumably be well approximated, for rather wide intervals, by means of quadratic functions"[30]

Theil describes two possible approaches to maximizing the welfare of the (dictatorial) government. In the first, called "choice alternative," it is assumed that there is no uncertainty and "that instruments and indirect variables are the arguments of a *social welfare function*, corresponding to the preferences of the policy maker" (Theil (1954, page 64, italics in the original).) The second is the "probability alternatives" approach, in which an econometric (stochastic) model is used to guide policymakers, who should not only compare the different alternatives they face, but should also attach to each alternative a probability of its occurrence. Economizing the relevant information required from policymakers also appear in Theil (1954, page 66) when he explained that setting random variables at their mean was an approach already used in the Central Planning Bureau of the Netherlands.

In sharp contrast with the case of the research group at Carnegie Tech, we see Theil looking for a theoretical justification to the "trick" already employed at the Central Planning Bureau, that of arbitrarily setting the random variables at their mean or their mode, to be able to use a stochastic model to advise governmental decisions. A quadratic objective function, in this case a social welfare function, was the main hypothesis, among others, delivering the certainty equivalence property[31]. However, once again a quadratic function is seen as an "appropriate" approximation which should be applied with care.

Henri Theil had also contributed to the certainty equivalence-quadratic loss function literature in other articles and books. In Theil (1957), he shows how the proof derived by Simon (1956) for the case of a dynamic model can be generalized and simplified, by following a rather econometric approach to it. Theil (1961) has a rather methodological approach to the problem of programming under uncertainty, and he compares different approaches to this problem, including the certainty equivalence case. Further discussion on quadratic loss functions and certainty equivalence property is found in Theil (1958, 1964). Most of his work, especially his 1964 book and his *Econometrica* paper of 1957, together with Simon (1956), became the main references on quadratic loss functions and certainty equivalence property for monetary economists, as discussed in the next section.

Theil developed his ideas about certainty equivalence while at the Central Planning Bureau

[29]Theil's approach differs from to that of Harry Markowitz and his portfolio selection theory. Theil (1961, page 127) explicitly cites Markowitz's approach as an exception to the customary assumption that agents maximize expected utility. Markowitz (1952) understood that, ignoring market imperfections, if investors maximized the expected value of their objective function (the future dividends of stocks), they would invest all their wealth in a single stock, that with higher expect return. Such hypothesis should be rejected because "diversification is both observed and sensible." He thus introduce risk consideration with his idea that investors "consider expected return a desirable thing *and* variance of return an undesirable thing" (page 77).

[30]Theil (1954, pages 75-76). The author seems to argue here that the quadratic welfare function is a good approximation to the "true" function in a global sense, in contrast to the local arguments employed by the group at GSIA, Carnegie Institute of Technology. However, Theil (1964, pages 4-5) explicitly states that the quadratic function can be obtained by a second-order Taylor expansion of the "true" function. In this book, he also uses first-order Taylor expansions to prove some of his results, using, therefore, local arguments.

[31]For a summary of those conditions, see Theil (1954, page 81).

of Netherlands, under influence of Tinbergen. Not only the denomination of “instruments” and “targets” was borrowed from Tinbergen by Theil (as well the general concern of having as many instruments as targets to achieve), but also his approach of deriving a policy model from a “structural model of the economy by eliminating the irrelevant endogenous variables.”[32] Moreover, Theil implements Tinbergen’s (1935, page 306) desire of deriving the “optimal” policy in a systematic way:

> “Having indicated, at least in principle, how the consequences of a given business cycle policy are to be found, we can now enunciate the “last” problem in this field, that is, which policy is the “best” one, the “optimal”? To answer this question, we should dispose of some general ophelimity function and calculate for which policy a course of events would occur giving a maximum to this general ophelimity function. Of course, at this moment any practical calculations of this sort are impossible. I shall not try to give them here.”

Theil (1965, page 19) criticizes the “standard practice of Western countries” in attacking the decision-maker’s problem “very informally. (...) Intuition and “feeling” are frequently the most important ingredients of the procedure. The apparatus of economic theory will usually be employed, of course, but the difficulty is that this theory gives little guidance as to the quantitative impact of the measures taken.” Theil proposed an alternative to this unsatisfactory situation “from the standpoint of economic analysis” by representing the policymaking as a constrained maximization problem, with a quantifiable (and “objective”) criteria of optimality, as Tinbergen would like to have available at his time. Economists were finally able to give sound advice to policy authorities.

In spite of being in the Netherlands in the 1950s and part of the 1960s, Theil did interact with Simon and the group at GSIA, Carnegie Institute of Technology. In the 1955 he visited the Carnegie Institute before going to Chicago and, more important, he exchanged letters with Simon commenting Simon’s papers and memoranda to the ONR, including Simon’s (1956) proof of the certainty equivalence property in a dynamic model. Theil sent his 1954 paper to Simon in a letter of June of 1955 (see Theil (1955a)), he also asked Simon to send him a copy of Holt’s paper to him and to “Professor Verdoorn, also of the Central Planning Bureau, [who] is interested in the same field, and he too would appreciate very much if you could send him copies of the two papers.”[33]

At that point Simon had already submitted his 1956 paper to *Econometrica*, what happened in March of 1955 (see Simon (1955a)). However, the revised and final version was sent by Simon to the journal only in October of that same year (see Simon (1955b)) and he was aware of Theil’s contribution to the topic[34].

The analysis of how quadratic loss functions became used to deliver the desired certainty equivalence property shows that the quadratic function was seen as an approximation to the “true” objective function. The main attractive was the easily implementable or feasible solutions obtained in models with quadratic objective function. Quadratic functions were suitable to policymakers with few time to devote to the decision making process and having no access to joint distributions of random variables, but only to few of their moments. Besides that, the cases discussed in this section reveal that different meanings of “loss function” were present in the different cases, all concretely specific to the problems discussed.

Easy analytical solutions and an “objective” way of talking about optimal policies were also very important elements for monetary economists participating in several debates in the 1960s in the United States. These elements partially explain the widespread adoption of quadratic loss functions in monetary debates since late 1960s, as discussed next.

[32] Hickman (1965, page 8).

[33] See also Theil (1955b).

[34] Answering a letter from Robert Strotz, the editor of *Econometrica* at the time, of September of 1955 with criticisms from the referees to Simon’s draft, Simon (1955c) by the end wrote: “I will await with interest your decision on the paper. If it is accepted, I should like to have the manuscript returned so that I can make the change mentioned above, and insert a reference to the Theil paper which was called to my attention after I had submitted the manuscript. In any event, I hope a decision can be reached with somewhat greater speed than was in evidence on the first round.”

3 The leading actors in the monetary debate

The "instrument contend" and its approach through assuming a quadratic loss function were more than mere theoretical curiosities. The period after the World War II witnessed the "increasing use of professional economists in government, not only in the Council of Economic Advisers but as presidential aides, specialists throughout the executive branch, and even as cabinet officers"[35] in the United States. The point is not that American economists started discussing policy issues, but rather that they did it inside the "government system, especially the executive branch," as stressed by Goodwin and Herren in Goodwin (1975, page 35).

At the same time, in the 1950s and 1960s, every president of the United States "has faced inflation as a major problem of his administration and has to construct a policy for its solution,"[36] keeping an eye on unemployment and the balance of payment consequences of such policies.

The administration of President John Kennedy (1961-1963) "brought a new level of involvement of professional economists to the top ranks of the executive branch of government"[37]. Kennedy's first Council of Economic Advisers had Walter Heller as chairman (from 1961 to 1964) and Kermit Gordon and James Tobin as members[38]. Robert Solow and Arthur Okun were part of the Council staff and contributed with reports and discussions of different topics. Paul Samuelson and Walt Rostow also contributed to the discussions about the main goal of Kennedy's economic policy, especially the wage-price policy[39].

During the 1950s, Seymour Harris organized the Treasury consultants' meetings, strengthening the communication and interaction between professional economists and the Treasury. After this successful initiative, the Board of Governors of the Federal Reserve System arranged a small-scale version of the Treasury consultants' meetings. At that point, the Board felt that, "while it is in close and active contact with financial and business groups in the community, it has not had very effective contact with academic economists on monetary issues. The main purpose of these informal sessions, therefore, would be to give the academic economists a chance to say what we think on some of the important issues facing the Board, and to give the Board members a chance to explore these views with us."[40]

Franco Modigliani participated in several academic consultants' meetings in the Federal Reserve Board since 1964 (until early 1980s). Several times Tobin, Poole, Samuelson, and Friedman, among several others, were invited to discuss different monetary issues.

The idea that economists should find the "optimal" instrument for monetary policy was partly a reaction to the understanding that monetary policy could, in first place, be used to keep the economy close to a desired path. This was one of the four main features that, according to Tobin (1974), characterized the "new economics" brought to Washington in the early 1960s[41]. The other three novelties were the conviction that, against the Mitchell-Burns-Moore tradition, business cycles were not inevitable and that government policy could and should keep the economy close to a path if steady real growth at a constant target rate of unemployment; the elimination of the taboo on deficit spending and an active use of fiscal and monetary policies mix to reach long-run goals; and

[35] Craufurd Goodwin in Goodwin (1975, page 6).

[36] Craufurd Goodwin in Goodwin (1975, page 1).

[37] *Idem.*

[38] Tobin (1974, page 6, footnote 3) adds that "when I returned to Yale in August 1962, I was replaced by Gardner Ackley. But I maintained close consulting relations with the Council for several more years."

[39] See Barber in Goodwin (1975).

[40] Bach (Bach). Bach was then a professor at Stanford's Graduate School of Business and was asked by the Board to organize the academic meetings.

[41] As stated by Tobin (1974, page 6): "[t]he economics brought to Washington by the Kennedy Council of Economic Advisers and other academic economists close to the new President was not new to economic literature and classrooms. It was not new in the practice of governments in Scandinavia, Holland, and Britain. Much of it was not new to official doctrine in this country, having been set forth by the first Councils of Economic Advisers, chaired by Edwin Nourse and Leon Keyserling for President Truman, and, in lesser degree, by the Eisenhower Councils as well. But it was new to Washington nonetheless."

a growth-oriented policy prescription.

The recent literature on the "instrument problem" and the characterization of the central bank's behavior by means of quadratic loss functions[42] generally tend to cite Sargent and Wallace (1975) as the seminar paper on this topic. Sargent and Wallace (1975), on their turn, followed a strategy used by Poole (1970) (and Poole (1970) only, not Sargent and Wallace (1975), cites Theil (1964) to justify the assumption of a quadratic loss function[43]).

William Poole, as already anticipated, was a Chicago Ph.D. student. He graduated in 1966 having Merton Miller as the chair of his thesis committee. Miller was at Carnegie Tech before going to the University of Chicago in 1961. Poole was an economist of the Board of Governors of the Federal Reserve System from 1969 to 1970, when he became senior economist, position occupied until 1974. In this period he was also lecturing at The American University (1970-1971) and at George Washington University (1971-1973), as well as Harvard (fall of 1973). In 1969 he published a version of his 1970 paper as a working paper of the Federal Reserve Board (see Poole (1969)), which is practically the same version published in the *Quarterly Journal of Economics*. He also discussed a first draft of his paper at the December 1967 meetings of the Econometric Society.

Poole (1970) compared three types of monetary policy: one that uses interest rate as instrument and another that has a monetary aggregate as instrument and another which is a combination of the previous two. He used an IS-LM framework to show that the "instrument problem" can only be properly analyzed in a stochastic model since in a static model all instruments are equivalently optimal. Then, in a stochastic version of the IS-LM model, Poole (1970) discussed the conditions upon which a given instrument is "optimal". He concluded that this is an empirical issue and that it depends on several parameters of the model, among them the variances of the different shocks disturbing the economy. This means that the choice of the instrument depends on the type of uncertainty faced by policymakers.

Poole (1970) was not the only one to approach the "instrument problem" with a quadratic loss function. Kareken (in Federal Reserve Bank of Boston (1969, pages 57-63)) has the same approach as Poole (1970) and the same type of conclusion: that the choice of the instrument depends on the type of uncertainty hitting the economy. In fact, Kareken was verbally arguing what he had proven in a paper not published already at that time, but available from him upon request. This paper, which is in fact a short note, was published in the *Journal of Money, Credit and Banking* in 1970 (see Kareken (1970).) In the particular case that "the monetary authority is certain about the values of the structural coefficients", the optimality condition is the same as Poole's: the policy leading to the lowest variance.

What is most relevant is the fact that an "objective" way of talking on optimal monetary policy arouse with the linear-quadratic approach. Objectivity here is used in the sense of what Daston (1992) denominated "aperspectival objectivity" and in the sense of Porter (1994): a standard way of exchanging knowledge and approaching a given problem so that individual idiosyncracies (or from a group of individuals) are eliminated. The main conclusion of Kareken (1970) is that no matter how economists believe the economy works the method he proposed should be always applied to judge what policy is optimal.

> "What I have done then in this note is (by example) suggest a procedure for deciding how the System [i.e., the Federal Reserve] should operate. Nor is fatal that economists are not all agreed on the structure of the U.S. economy. **Whatever structure has been estimated, the procedure I have suggested can be used**."
> Kareken (1970, page 390, emphasis added)

[42] For references on the vast recent literature on quadratic loss functions, see Woodford (2003) and Svensson (2003) and references therein. It is important to stress that part of the current literature on quadratic loss function, as Woodford (2003), justifies its employment as a second-order Taylor approximation to the "true" welfare function of the economy, in general given by the representative consumer's utility function. It is clear that this argument has a different nature than those supported by the group at Carnegie Tech and much closer to Theil (1964).

[43] However, Sargent and Wallace (1975) cite Chow (1970) who cites Theil (1958) and Holt et al. (1960).

In contrast to Poole (1970) and Kareken (1970), Sargent and Wallace (1975) were concerned with the problem that different monetary instruments have different implications to the existence of equilibrium in a model, depending on the type of public's expectations about prices considered, if rational or adaptative expectations. Their main conclusion is that under rational expectations the probability distribution of output is the same for all deterministic money supply rules adopted by the government, while interest rate rules, in this case, lead to price level indeterminacy. They also show that the opposite result holds for the case of adaptative expectations.

However, more than their results, two other features of their paper are important to the present narrative. Sargent and Wallace (1975) set up an *ad hoc* stochastic model with structure somewhat similar to the IS-LM model, but with dynamic equations involving lagged variables. The essence of the model is the same that Sargent was recently working on (see Sargent (1971) and Sargent and Wallace (1973)): it was a linear model. Moreover, they defined optimality according with the minimization of a quadratic loss function, with the following justification:

> "In order to discuss policy within the context of an ad hoc model, we must adopt an ad hoc loss function. The most familiar such function is the quadratic loss function."
> Sargent and Wallace (1975, page 244)

Sargent and Wallace (1975) were here following a type of argument that became common after Lucas' (1976) critique, that a model should be "derived explicitly from a theory in which agents optimize subject to constraints," so to make possible "to apply the standard welfare economics based on Pareto optimality" (Muench and Wallace (1974, page 331)[44].) In the absence of such a model, "some *ad hoc* criterion must be adopted" as currently done in the control theory literature with the assumption of a quadratic loss function.

This search for macro models of policy analysis based on microfoundations is an important element for a historical account of the recent literature on quadratic loss function, as Woodford (2003) for example. However, such account is out of the scope of the present work. The focus here is rather in how different economists employed the quadratic loss function to support a particular monetary policy characterized as "optimal" in the 1960s (and early 1970s).

Among economists involved in policymaking in the 1960s, there were two broad groups. The monetarists, with Friedman, Kareken, and Poole, among others, and the "anti-monetarists", like Tobin, Modigliani, Okun, and Solow, for example. The dispute among them about the role of money in the economy was described by Friedman (in Taylor (2001, page 120)) as the "radio AM/FM debates [Ando and Modigliani versus Friedman and Meiselman]." There was also a clear distinction between monetarists and the so-called "Yale Tradition" in macroeconomics. As Tobin and Shiller argue (in Colander (1999)), this tradition referred to people around Tobin, with a sharply different understanding of how the economy operates than that of the monetarists. Nonetheless, a quadratic loss function was taken as a mere tool to evaluate alternative policies for all these economists. Tobin (in Federal Reserve Bank of Boston (1969, pages 79-80)) stated that the procedure Kareken was using to evaluate different monetary policies was correct, is spite of disagreeing with his representation of the way the economy works:

> "(...) I think John is going about the problem the right way, namely, to try to find some rules of policymaking that will minimize the variance of the objective of Federal Reserve policy around its target. He contrasted two policies – one was to fix M and the other was to fix interest rates – and he asked under what circumstances can you say one of them is preferable to the other. (...)
>
> One problem with Kareken's model is that it assumes that the Federal Reserve can know the structure well enough to know, on the average, what combination of quantity of money and reserves will produce what interest rate and will be geared to the target for GNP. (...) The actual problem the Federal Reserve faces is more complicated."

[44]Lucas' critique was already known by Muench and Wallace (1974), who cites his article as (to be) published in the Journal of Money, Credit and Banking in 1973. However, Lucas' article was only published later, in 1976.

Another interesting example is Okun (1972) with his somewhat length discussion on several of the intertwined arguments about economic policymaking mentioned in the introduction of the present article. He carefully supported the activist way of conducting monetary policy and had no concerns in assuming a quadratic "pain or welfare loos" function. Okun mentioned that a modular function would also work to get a version of the certainty equivalence in qhich the random variables are set to their median, rather than the mean. Moreover, Okun (1972) dismissed the monetarist proposal of a constant money growth on the grounds that this policy does not deliver the lowest welfare loss. He presented a numerical exercise in which he used a simplified version of the St. Louis Fed econometric model, "because [it] represent[s] the most thorough and carefully articulated quantification of the monetarist view,"[45] and a quadratic loss function with the deviation of output to its target as argument. Okun (1972, page 151, italics in original) concludes that a policy maker "should *not* keep monetary growth steady, but rather should step it up when GNP falls below the target path and reduce it when GNP exceeds the target."

Curiously enough, Okun's paper was discussed by Kareken and Charles Holt, among others. As reported by the journal[46], in the general discussion, "Holt stressed the need to develop formal tools for implementing an activist policy, combining and complementing them with judgemental devices. Formalizing the decision problem requires an objective statement of the various targets and the relative weight placed on them and of the penalties associated with various instruments. Delving into those issues would help distinguishing the areas in which we can deal with the various problems by formal decision rules from those in which we have an intuitive grasp of problems that we cannot quantify precisely."[47] Okun agreed with Holt's comments.

Therefore, we see a group of economists (Tobin, Okun, and Kareken, for instance) that disagreed in several aspects about how to conduct the monetary policy, all seemed to accept or approve that a quadratic loss function was the "right way" to characterize an optimal policy. In this respect, it is not possible to associate the use of quadratic loss functions with only a particular argument or group of participants of the monetary debate in the 1960s, as Poole (1975) tried to do[48]

The consensual view among Tobin, Okun, Poole, Kareken, and Sargent and Wallace, for example, about the way to characterize an "optimal" policy did not imply that the quadratic loss function was unanimously seen as a perfect approximation to the policymaker's problem. Such function treats symmetrically positive and negative errors of the same magnitude: a policymaker equally dislikes and output 10% above its target as one 10% below. Formal criticisms to the symmetry implicit in the assumption of a quadratic welfare function were arising in economics in the 1970s, as suggested by the papers of Friedman (1972) and Waud (1976). Some might argue that Brainard (1967) was also criticizing the "standard approach" of quantitative policy evaluation, which can be characterized as assuming a quadratic loss function and that uncertainty enters the model only through stochastic disturbances. In fact, the author called attention for a more complex problem (already discussed by Theil (1964)): that of an uncertain response of target variables to policy instruments (what Friedman (1953b) called the "multiplier uncertainty"). His conclusion, a Markowitz-type argument, is that, in this case, the optimal policy implies diversification of instruments, i.e., it uses all instrument available even in the case of only one target. It is worth pointing that Brainard (1967, page 413)

[45] Okun (1972, page 150).

[46] The *Brookings Papers on Economic Activity* has the habit of publishing the main paper and the comments and discussion following it. In this part, it publishes the text of the specified discussants of the paper and also notes on the general discussion with the audience.

[47] Okun (1972, page 152).

[48] For the author, the lack of a "package of reforms in the macro area" in the CEA Report of 1975 "reflects two failures in macro policy analysis in the economic profession in general. The first is that the advocates of discretionary policy have generally refused to be pinned down on formulations of optimal policy reaction function. (...). There is no hope of improving discretionary policy without the explicit specification of policy reaction functions that are to be followed until evidence accumulates that better policies are available." (Poole (1975, page 547)) Okun (1972) would probably be identified as member of the advocates of discretionary policies (or an anti-rules), but he does support his argument with a quadratic loss function, as shown before.

followed "Theil in assuming that the policy-maker maximizes the expected value of a quadratic utility function"[49]. However, all those arguments did not prevent the use of quadratic loss functions to be long lived.

There is an interesting question to explore: why did Modigliani, Tobin, and Friedman, for example, avoid to use systematically formalize the discussion about monetary policy in the 1960s in terms of a quadratic loss function (in spite of not opposing to its employment in this discussion in which they participated actively)? One argument is that the quadratic loss function (and the certainty equivalence derived from it) was a new mathematical tool dominated by "young" economists trained in a "different tradition" than that of Modigliani, Tobin, and Friedman. However, this is hardly supportable. Modigliani was obviously deeply familiar with linear-quadratic methods that he and the group at Carnegie Tech applied to the inventory problem. Tobin was in close contact with Brainard and Okun who did discuss, in some occasions, the optimal monetary policy in terms of a quadratic loss function. Friedman, on the other hand, was involved with operations research problems at the Statistical Research Group, at Columbia University, during the World War II. It was there that he was familiarized with the quadratic loss function and the linear-quadratic methods, as he explained in a letter to the author (Friedman (2005))[50].

Yet another argument that is hardly supportable is that the notion of optimality embedded in the quadratic approach was the only one available at that time. This was clearly not the case. For example, Bailey (1956), following lines advanced by Friedman (1953a) and strongly associated to Friedman (1968), had a welfare approach to monetary policy, by arguing that the way to judge alternative policies could be by assessing the different levels of inflation they would generate. Inflation was not seen as a painless way of financing government expenditures, but rather as a tax imposed to consumers. However, that was not a sufficiently attractive way of discussing short-run alternatives to monetary policymaking at that period[51].

Another possible answer to the question previously stated is that Modigliani, Tobin, and Friedman were able to discuss "optimal" monetary policy in term of minimizing variances and of tradeoffs between unemployment and inflation, as summarized by a Phillips curve. All this could be casted in the form of a quadratic function in deviations of output from its target and deviations of inflation from its target. Flexibility of this mathematical apparatus to interpret "optimality" and "stabilization" is key here. The relative weight of these arguments is exactly related to the mentioned tradeoff. As Poole (1970) and Kareken (1970) first argued, the optimal instrument is that delivering the lowest variance of output.

Moreover, an argument about finding a policy that minimizes the variance of economic variables (such as inflation and output) would hardly sound "unnatural" to economists discussing economic policymaking and advising policy authorities. In fact, "in the most general (nonoperational) sense, the goal of macroeconomic policy is clear and undisputed. It is to keep the economy as close as possible to some desired path of evolution."[52] It is the same idea as that stabilizing the business cycle "with respect to a certain number of key variables, all of which are measured quantitatively."[53]

[49]So that he would be able "to compare our findings directly with the familiar certainty equivalence results."

[50]When asked if he was familiar and in accordance with the quadratic function and why had he never employed this approach to the "instrument contest" of the 1960s, Friedman (2005) wrote: "I must confess that in the 1960s I did not get much involved in considering the quadratic approach to debates about the optimal monetary instrument. The quadratic approach was familiar to me from the operations research during the war, but somehow or other I do not remember ever having seriously explored its use for the purpose of monetary policy. (...) I never made a conscious decision whether to employ the quadratic approach or not. If a problem had come up for which that seemed an appropriate approach, I probably would have used it but I have no recollection of thinking about the subject in a systematic way."

[51]See, for example, Chow (1970, page 301).

[52]Holbrook and Shapiro (1970, page 40).

[53]Theil (1965, page 18). Just as another example, Tinbergen (1935, page 306), after recognizing that calculating what policy would maximize a general ophelimity function was then impossible, stated: "I shall only make some remarks on the frequent and, I think, justifiable assumption that stabilization of business cycles in employment is the optimum policy."

In conclusion, one might think that the analytic simplicity provided by a quadratic loss function was the main attractiveness for its widespread usage in monetary economics. No doubt that this was an important factor. However, I think that the ability of talking about minimizing variances and about unemployment-inflation tradeoff was an equaling appealing property of that quadratic function, which became a widespread tool employed to discuss optimality in monetary economics. Monetary economists could disagree in terms of how the economy works, but not in terms of how to evaluate a monetary policy adopted by the government. To use Weintraub's (1991, page 120) words, "I want to suggest that there is another way that the sequence of papers" on quadratic loss function and certainty equivalence property "can be read, and that is as an attempt, by members of a particular community, to narrow the possibilities for disagreement among community members."

4 Concluding Remarks

Given that Sargent and Wallace (1975) is considered to be a seminal paper in the "instrument contend" literature and that it followed Poole (1970), who cites Theil (1964), the history of quadratic loss function in the monetary debate seems to start with these papers and book. In fact, an economist making a literature review on this topic might likely argue that the first insights on how to deal with economics decisions in an uncertain world came from Simon (1956) and Theil (1957), both article published in an important economics journal, *Econometrica*. However, as showed in the second section of this paper, such account misses important nuances coming from how mathematical methods made their way from military to industrial and governmental uses during the 1950s and the 1960s in the United States.

Additionally, this narrative also misses the important dynamics among the three competing neoclassical "schools", according to Hands and Mirowsky (in Morgan and Rutherford (1998, pages 260-292)), MIT, Chicago, and Cowles Commission, on one hand, and Carnegie Institute of Technology, on the other. The research group at GSIA, which was in contact with Cowles and Chicago, clearly stands out as a center disseminating linear-quadratic methods not only for industrial and governmental uses, but also to professional monetary economists discussing the optimal policy design[54].

During the 1950s and the 1960s there was a great interaction between economists and other scientists, as the newly grouped management scientists. It was common to see cooperation among them and one publishing in the journals of the other. Modigliani, Simon, Cooper are just few among several examples. This period witnessed the emergence of the industrial use of linear programming and the quadratic approach to maximize the performance of a given system. Economizing resources and information, in the particular way of minimizing the set of relevant information to base an optimal decision, was a main characteristic of such a period of intense planning.

Clearly, the feasibility introduced by linear-quadratic methods in dynamic programming problems under uncertainty was one important attractive for launching them into monetary economics. However, one should not forget that these methods became widespread in monetary economics exactly in a period when professional economists started to be massively used in government, mainly in the executive branch. At the same time, since World Ward II, mathematics, stochastic models under uncertainty, and quantification were elements highly appreciated by decision scientists in general, and (orthodox) economists willing to give sound and objective advice to policymakers in particular. Linear-quadratic models was then seen as a great opportunity to potentially provide policymakers a laboratory in which they could test alternative policies, following specific rules and criteria to judge them:

> "Through simulation testing we can compensate, at least partially, for the fact that the laboratory for these decision problems is the national economy where any errors will necessarily

[54] The group at Carnegie Tech was also important in the development of the rational expectation hypothesis, as developed by Sent (2002) and references therein.

be large. We can set up an economic model on a computer and perform experiments with various alternative actions. An extensive testing program of this type is strongly to be recommended to validate the model, the welfare function, and the decision analysis."
Holt (1965, page 264)

Moreover, the flexibility of a loss function to encompass "traditional" arguments in monetary debates, as minimizing variance and as the unemployment-inflation tradeoffs, was an equally appealing feature of a quadratic loss function.

If the literature outside monetary economics was careful in pointing out possible limitations of a quadratic loss function, monetary economists saw themselves creating an objective tool for discussing optimality. It was not uncommon to find statements such as the one of Sargent and Wallace (1975) presented in the previous section, that an ad hoc model calls for an ad hoc loss function, or such as:

"Despite well-known objections, I assume that the monetary authority's utility function is

$$U = -(y - \tilde{y})^2$$

where y is nominal output for the policy period and $\tilde{y}$ is the desired or target value of y."
Kareken (1970, page 385)

Quadratic loss function was a tool to be employed in any model. Objectivity has arisen in monetary economics. Different from the other scientists using quadratic loss function in the 1950s and 1960s, monetary economists disregarded possible limitations and had an unified understanding of a "loss function." The discourse on optimality, to use Weintraub's (1991) concept, has stabilized. If such a function was previously related to the time wasted in scheduling tasks or with the production costs, "loss function" for monetary economists was merely a uniform description of central bank's preferences, so that optimality could be characterized in a objective and quantifiable way.

References

Anshen, M., C. Holt, F. Modigliani, J. Muth, and H. Simon (1958). Mathematics for Production Scheduling. *Harvard Business Review 36*(2). March-April.

Arrow, K., T. Harris, and J. Marschak (1951). Optimal Inventory Policy. *Econometrica 19*(3), 250–272.

Arrow, K. J. (1951). Social Choice and Individual Values. *Cowles Commission for Research in Economics, Monograph* (12).

Bach, G. L. Letter to Franco Modigliani. Box PS1, folder "1964, Jan. 30".

Bailey, M. J. (1956). The Welfare Cost of Inflationary Finance. *Journal of Political Economy 64*(2), 93–110.

Barnett, W. (2001, November). A Conversation with Henri (Hans) Theil: His Experiences in the Netherlands during the Second World War. *Working papers at WUSTL* (0111001).

Brainard, W. (1967). Uncertainty and the Effectiveness of Policy. *American Economic Review 57*(2), 411–425.

Chow, G. C. (1970). Optimal Stochastic Control of Linear Economic Systems. *Journal of Money, Credit and Banking 2*(3), 291–302.

Colander, D. (1999). Converstations with James Tobin and Robert Shiller on the "Yale Tradition" in Macroeconomics. *Macroeconomic Dynamics 3*, 116–143.

Cooper, W. W. (1951). A Proposal for Extending the Theory of the Firm. *Quarterly Journal of Economics 65*(1), 87–109.

Cooper, W. W. (1955). Presidential Address to TIMS. *Management Science 1*(2), 183–186.

Cooper, W. W. (2002). Abraham Charnes and W. W. Cooper (et al.): a brief history of a long collaboration in developing industrial uses of linear programming. *Operations Research 50*(1).

Daston, L. (1992). Objectivity and the Escape from Perspective. *Social Studies of Science 22*, 597–618.

Federal Reserve Bank of Boston (1969, June). *Controlling Monetary Aggregates*, Conference Series no. 1. Federal Reserve Bank of Boston.

Flood, M. M. (1956). The Objectives of TIMS. *Management Science 2*(2), 178–184.

Friedman, B. M. (1972). Optimal Economic Stabilization Policy: An Extended Framework. *Journal of Political Economy 80*(5), 1002–1022.

Friedman, M. (1953a). Discussion of the Inflationary Gap. In *Essays in Positive Economics*, pp. 251–262. Chicago, University of Chicago Press. Fifth impression, paperback.

Friedman, M. (1953b). The Effects of a Full-Employment Policy on Economic Stability: A Formal Analysis. In *Essays in Positive Economics*, pp. 117–132. Chicago, University of Chicago Press. Fifth impression, paperback.

Friedman, M. (1968). The Role of Monetary Policy. *American Economic Review 58*(1), 1–17.

Friedman, M. (2005, March 25). Letter to the author.

Goodwin, C. (Ed.) (1975). *Exhortation and Controls*. Washington, D.C.: The Brookings Institution.

Halsbury, E. O. (1955). From Plato to the Linear Program. *Journal of the Operations Research Society of America 3*(3), 239–254.

Hickman, B. G. (1965). Introduction. In B. G. Hickman (Ed.), *Quantitative Planning of Economic Policy*, pp. 1–17. Washington, D.C.: The Brookings Institution.

Holbrook, R. and H. Shapiro (1970). The Choice of Optimal Intermediate Economic Targets. *American Economic Review 60*(2), 40–46.

Holt, C., F. Modigliani, J. Muth, and H. A. Simon (1960). *Planning Production, Inventories, and Work Force*. Englewood Cliffs: Prentice-Hall.

Holt, C., F. Modigliani, and J. F. Muth (1956). Derivation of a Linear Decision Rule for Production and Employment. *Management Science 2*(2), 159–177.

Holt, C., F. Modigliani, and H. Simon (1955). A Linear Decision Rule for Production and Employment Scheduling. *Management Science 2*(1), 1–30.

Holt, C. C. (1965). Quantitative Decision Analysis and National Policy: how can we bridge the gap? In B. G. Hickman (Ed.), *Quantitative Planning of Economic Policy*, pp. 252–269. Washington, D.C.: The Brookings Institution.

Ijiri, Y. and A. Whinston (Eds.) (1979). *Quantitative Planning and Control –essays in honor of William Cooper on the occasion of his 65th birthday*. New York: Academic Press.

Kareken, J. H. (1970). The Optimum Monetary Instrument Variable: comment. *Journal of Money, Credit and Banking 2*(3), 385–390.

Klein, J. (2000). Economics for a Client: the case of statistical quality control and sequential analysis. In R. Backhouse and J. Biddle (Eds.), *Toward a History of Applied Economics*, Annual supplement to vol. 32?? of History of Political Economy. Durham: Duke University Press.

Klein, J. (2005). *Constructing Duality: How Applied Mathematics Became a Science of Economizing From 1940 to 1960*. Forthcoming.

Kloek, T. (2001). Obituary: Henri Theil, 1924-2000. *Statistica Neerlandica 55*(3), 263–269.

Koopmans, T. C. (1952, July 1). Letter to Herbert Simon. In *Herbert Simon Collection*. Carnegie Mellon University.

Lucas, Jr, R. E. (1976). Econometric Policy Evaluation: A Critique. *Carnegie-Rochester Conference Series on Public Policy 1*, 19–46.

Markowitz, H. (1952). Portfolio Selection. *The Journal of Finance 7*(1).

Mirowski, P. (2002). *Machine Dreams: Economics Becomes a Cyborg Science*. Cambridge: Cambridge University Press.

Modigliani, F. (1954, August). An Alternative Method for Inverting the Quadratic Decision Matrix. In *ONR Research Memorandum n.o 22*. G.S.I.A., Carnegie Institute of Technology.

Morgan, M. and M. Rutherford (Eds.) (1998). *From Interwar Pluralism to Postwar Neoclassicism*, Annual supplement to vol. 30 of History of Political Economy, Durham. Duke University Press.

Muench, T. and N. Wallace (1974). On Stabilization Policy: Goals and Models. *American Economic Review 64*(2), 330–337.

Muth, J. F. (1955, May). *Locally Optimal Decision Rules*. Unpublished.

Okun, A. M. (1972). Fiscal-Monetary Activism: Some Analytical Issues. *Brookings Paper on Economic Activity 1972*(1), 123–172.

Poole, W. (1969, December). Optimal Choice of Monetary Policy Instruments in a Simple Sthocastic Macro Model. Special Studies Paper - Division of Research and Statistics - Federal Reserve Board , no. 2.

Poole, W. (1970, May). Optimal Choice of Monetary Policy Instruments in a Simple Stochastic Macro Model. *Quarterly Journal of Economics 84*(2), 197–216.

Poole, W. (1975). The 1975 Report of the President's Council of Economic Advisers: Long on Analysis, Short on Policy. *American Economic Review 65*(4), 539–547.

Porter, T. M. (1994). Objectivity as Standardization: the rhetoric of impersonality in measurement, statistics, and cost-benefit analysis. In A. Megill (Ed.), *Rethinking Objectivity*, pp. 197–237. Durham: Duke University Press.

Sargent, T. (1971). The Optimum Monetary Instrument Variable in a Linear Economic Model. *The Canadian Journal of Economics 4*(1), 50–60.

Sargent, T. and N. Wallace (1973). Rational Expectations and the Dynamics of Hyperinflation. *International Economic Review 14*(2), 328–350.

Sargent, T. and N. Wallace (1975). "Rational" Expectations, the Optimal Monetary Instrument, and the Optimal Money Supply Rule. *Journal of Political Economy 83*(2), 241–254.

Sent, E.-M. (2002). How (not) to Influence People: The Contrary Tale of John F. Muth. *History of Political Economy 34*(2).

Sent, E.-M. (2005). Simplifying Herbert Simon. *History of Political Economy 37*(2).

Simon, H. A. (1951). A Formal Theory of the Employment Relationship. *Econometrica 19*(3), 293–305.

Simon, H. A. (1952). On the Application of Servomechanism Theory in the Study of Production Control. *Econometrica 20*(2), 247–268.

Simon, H. A. (1955a, March 23). Letter to Robert H. Strotz. In *Herbert Simon Collection*. Carnegie Mellon University.

Simon, H. A. (1955b, October 3). Letter to Robert H. Strotz. In *Herbert Simon Collection*. Carnegie Mellon University.

Simon, H. A. (1955c, September 6). Letter to Robert H. Strotz. In *Herbert Simon Collection*. Carnegie Mellon University.

Simon, H. A. (1956). Dynamic Programming under Uncertainty with a Quadratic Criterion Function. *Econometrica 24*(1), 74–81.

Simon, H. A. (1992). Autobiography. In A. Lindbeck (Ed.), *Nobel Lectures, Economics 1969-1980*. Singapore: World Scientific Publishing Co.

Smith Jr., L. W. (1956). Current Status of the Industrial Use of Linear Programming. *Management Science 2*(2), 156–158.

Svensson, L. E. O. (2003). What is Wrong with Taylor Rules? Using Judgment in Monetary Policy through Targeting Rules. *Journal of Economic Literature 41*(2), 426–477.

Taylor, J. B. (2001). An Interview with Milton Friedman. *Macroeconomic Dynamics 5*, 101–131.

Theil, H. (1954). Econometric Models and Welfare Maximisation. *Weltwirtschaftliches Archiv 72*(1), 60–83.

Theil, H. (1955a, June 15). Letter to Herbert Simon. In *Herbert Simon Collection*. Carnegie Mellon University.

Theil, H. (1955b, August 26). Letter to Herbert Simon. In *Herbert Simon Collection*. Carnegie Mellon University.

Theil, H. (1957). A Note on Certainty Equivalence in Dynamic Planning. *Econometrica 25*(2), 346–349.

Theil, H. (1958). *Economic Forecasts and Policy*. Amsterdam: North-Holland Publishing Company.

Theil, H. (1961). Some Reflections on Static Programming under Uncertainty. *Weltwirtschaftliches Archiv 87*(1), 124–138.

Theil, H. (1964). *Optimal Decision Rules for Government and Industry*. Amsterdam: North-Holland Publishing Company.

Theil, H. (1965). Linear Decision Rules for Macrodynamic Policy. In B. G. Hickman (Ed.), *Quantitative Planning of Economic Policy – a conference of the Social Science Research Council Committee on Economic Stability*, pp. 18–42. Washington, D.C.: The Brookings Institution.

Tinbergen, J. (1935). Annual Survey: suggestions on quantitative business cycle theory. *Econometrica 3*(3).

Tinbergen, J. (1952). On the Theory of Economic Policy. In *Contributions to Economic Analysis*. Amsterdam: North-Holland Publishing Company. vol. I.

Tobin, J. (1974). *The New Economics One Decade Older*. Princeton: Princeton University Press.

von Neumann, J. and O. Morgenstern (1947). *Theory of Games and Economic Behavior*. Princeton: Princeton University Press. 2nd ed.

Waud, R. N. (1976). Asymmetric Policymaker Utility Functions and Optimal Policy under Uncertainty. *Econometrica 44*(1), 53–66.

Weintraub, E. R. (1991). *Stabilizing Dynamics: constructing economic knowledge*. Cambridge: Cambridge University Press.

Weintraub, E. R. (2002). *How Economics Became a Mathematical Science*. Durham: Duke University Press.

Woodford, M. (2003). *Interest and Prices: foundations of a theory of monetary policy*. Princeton: Princeton University Press.