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How Macroeconomists Lost Control of Stabilization Policy: Towards Dark Ages*

Jean-Bernard Chatelain[†] and Kirsten Ralf[‡]

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Abstract

This paper is a study of the history of the transplant of mathematical tools using negative feedback for macroeconomic stabilization policy from 1948 to 1975 and the subsequent break of the use of control for stabilization policy which occurred from 1975 to 1993. New-classical macroeconomists selected a subset of the tools of control that favored their support of rules against discretionary stabilization policy. The Lucas critique and Kydland and Prescott's time-inconsistency were over-statements that led to the "dark ages" of the prevalence of the stabilization-policy-ineffectiveness idea. These over-statements were later revised following the success of the Taylor (1993) rule.

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1 Introduction

This paper presents a longitudinal study of the transplant of key ideas and mathematical tools from negative-feedback control in engineering and applied mathematics to macroeconomic stabilization policy. This movement evolved parallel to the "rules *versus* discretion" or "stabilization policy ineffectiveness" controversy from 1948 to 1993. In particular, we observe a fast transplant of classic control and optimal control to stabilization policy in the 1950s and 1960s, followed by a long delay to transplant robust control and stochastic optimal control to optimal state estimation and optimal policy. The paper re-evaluates the Lucas critique and time-inconsistency argument which contributed to the bifurcation with diverging paths between control *versus* the modeling of stabilization policy by mainstream macroeconomics in the 1970s and 1980s.

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Adam Smith (1776) believed that demand and supply are always self stabilizing due to a negative feedback mechanism in the private sector. In the 1930s emerged Keynesian macroeconomic stabilization policy where the policy maker uses negative feedback mechanisms with monetary or fiscal policy instruments. Friedman (1948) started the rules *versus* discretion controversy, when he proposed fiscal rules that do not vary in response to cyclical fluctuations in business activity, so that it is the private sector's negative-feedback mechanism that stabilizes the economy, and not the policy maker. He defined "discretion" as Keynesian state contingent policy where the policy instruments change with respect to the deviation of policy targets from their set points. Discretionary policy was used in the US in the 1950s and 1960s, and the proponents of rules were losing in the controversy during these two decades.

In parallel to the development of Keynesian stabilization policy, the field of applied mathematics and engineering developed the tools of classic control during 1930-1955 (Bennett (1996)). This field gains maturity and autonomy while creating a world association with a first IFAC conference in 1960. Between 1955 and 1990, it has an impressive and fast rate of new discoveries: optimal control, optimal state estimation with Kalman filter, stochastic optimal control, robust optimal control, Nash and Stackelberg dynamic games. These discoveries were readily applied for many devices with numerical algorithms using the development of computers at the time. Aström and Kumar (2014) survey the research field of control which is based on negative-feedback rules stabilizing a dynamic system:

Feedback is an ancient idea, but feedback control is a young field... Its development as a field involved contributions from engineers, mathematicians, economists and physicists. It represented a paradigm shift because it cut across the traditional engineering disciplines of aeronautical, chemical, civil, electrical and mechanical engineering, as well as economics and operations research. The scope of control makes it the quintessential multidisciplinary field. (Aström and Kumar (2014), p. 3)

There was a strong demand for control tools for firm-level planning and for macroeconomic stabilization policy in the 1960s (Kendrick (1976), Kendrick (2005), Neck (2009) and Turnovsky (2011)). Barnett describes the related controversies surrounding the model of the Federal Reserve Board in the 1970s (Barnett and Serletis (2017)):

The policy simulations were collected together to display the policy target paths that would result from various choices of instrument paths. The model was very large, with hundreds of equations. Some economists advocated replacing the "menu" book of simulations with a single recommended policy, produced by applying optimal control theory to the model. The model was called the FMP model, for Federal Reserve-MIT-Penn, since the origins of the model were with work done by Franco Modigliani at MIT and Albert Ando at the U. of Pennsylvania, among others. That model's simulations subsequently became an object of criticism by advocates of the Lucas Critique. The alternative optimal control approach became an object of criticism by advocates of the Kydland and Prescott (1977) finding of time inconsistency of optimal control policy. (Barnett and Serletis (2017), p. 7-8)

The Lucas (1976) critique and Kydland and Prescott's (1977) time-inconsistency argument convinced a sufficient number of macroeconomists that, *although negative-feedback*

mechanism and optimal control should be used for modeling the private sector, negative-feedback mechanism and optimal control cannot be used for stabilization policy by macroeconomists and practitioners of monetary and fiscal policy.

In the 1970s, Lucas, Kydland and Prescott, labeled as new classical macroeconomists, took sides for rules in the rules *versus* discretion controversy. On the one hand, incorporating control tools into macroeconomics was obviously a scientific progress, and new classical macroeconomists invested heavily, like Keynesian macroeconomists, in learning these tools in the 1960s. On the other hand, control tools were supporting negative-feedback mechanism driven by policy makers, hence discretion. Therefore, the tools of control were pivotal in the rules *versus* discretion controversy.

Efficient multi-disciplinary research tools can be imported from one field of research to another one. However, the scientists in the field of arrival are free to bias their choice of the tools to be imported from the field of origin, if they are taking side in a scientific controversy. This selection bias entails the risk of the inconsistency of the imported subset of tools with respect to the field of origin.

The new-classical economists put forward their normative rational expectations theory with theoretical demonstrations using a Kalman filter. They claimed it is impossible to estimate parameters of the transmission mechanism when there is reverse causality of the feedback rule. The new-classical economists suggested importing time-inconsistency into dynamic games. They simulated models using the linear quadratic regulator for the private sector. All these approaches are using tools from the field of control.

But, in addition, following the complete guidelines of the field of control where the accuracy of the measurement of the transmission mechanism is a key element, they could have attempted the falsification of their theory estimating parameters with a Kalman filter. They could have attempted to devote a lot of resources to identification strategies when facing reverse causality in systems of equations. They could have determined optimal feedback policy based on these estimation of state variables using stochastic optimal control. They could have searched for a policy that could be robust to some ranges of uncertainty on parameters of the transmission mechanism.

But using these tools would have been inconsistent with their research agenda where they were taking sides in a scientific controversy. They biased the use of some tools of control in order to support their prior view on the side of “rules” in the controversy. The temporary success (for two decades) of their selection bias restricted the demand and delayed the use of the tools of control for stabilization policy.

This helps to reconsider how, in order to convince a sufficiently large subset of the community of macroeconomists, *the authors rhetorically generalized some valid statements of their papers stretching them to extreme conclusions, which, in turn, were false statements.*

The Lucas critique (1976) over-stated that it is impossible to identify parameters in a dynamic system of equations with reverse causality. Despite the Lucas critique, Kydland and Prescott (1982) over-stated that the US economy during 1950-1979 behaved *as if* stabilization policy had never been implemented (policy instruments were pegged) or *as if* the policy instruments did not have an effect on policy targets. Kydland and Prescott (1977) over-stated that policy maker’s credibility can never be achieved using negative-feedback rules according to optimal control.

Simulations using private sector micro-economic foundations and auto-regressive exogenous shocks were rhetorically presented as the magical “scientific” solution to answer the Lucas critique, to avoid time inconsistency and to describe business cycles data.

These rhetorics marginalized or delayed for at least a decade attempts to model stabilization policy transplanting the new tools of robust optimal control facing parameter uncertainty and stochastic optimal control with feedback rules reacting to estimates of state variables using a Kalman filter with macroeconomic time-series. This outcome is labeled “dark ages” by Taylor (2007):

But after this flurry of work in the late 1970s and early 1980s, a sort of “dark age” for this type of modeling began to set in. Ben McCallum (1999) discussed this phenomenon in his review lecture, and from the perspective of the history of economic thought, it is an interesting phenomenon. As he put it, there was “a long period during which there was a great falling off in the volume of sophisticated yet practical monetary policy analysis”. (Taylor (2007))

In order to explain the selection of tools imported from control on behalf of the new-classical macroeconomist side in the “rules *versus* discretion” controversy, our method is to use as a reference model the simplest model of control. We translate the mathematical arguments of the most cited papers in this controversy into the framework of this single model. This helps to understand how different definitions of discretion and how different hypothesis on the persistence of the policy targets in the transmission mechanism matter, even though they were not highlighted so far in the history of the rules *versus* discretion controversy.

The structure of this study is as follows:

Section 2 frames the original classical economists’ view of self-stabilizing markets in the framework of Ezekiel’s (1938) Cobweb model. Friedman’s (1948) and Kydland and Prescott’s (1977) rules *versus* discretion controversy is presented in the framework of the simplest first order single-input single-output model of control.

Section 3 documents the fast transplant of classic control from Phillips (1954b) and resurrection with the Taylor (1993) rule. It mentions the fast transplant of optimal control to stabilization policy in the 1960s and of a Kalman filter for rational expectations theory. By contrast, It mentions the very limited use of Stochastic Optimal Control using simultaneously Kalman-filter estimations for determining optimal stabilization policy in the linear quadratic Gaussian model. Finally, it emphasizes the long delay before transplanting robust control, dealing with the uncertainty on parameters.

Section 4 re-evaluates the claim of the Lucas (1976) critique that it is impossible to identify the parameters of the transmission mechanism when there is reverse causality due to a negative-feedback rule. The Lucas critique is not resolved by microeconomic foundations, by Sims’ (1980) vector autoregressive models, by Kydland and Prescott’s (1982) real business cycle, nor by Lucas’ (1987) welfare cost of business cycles. Kydland and Prescott (1977) section 5 has a specific definition of discretion assuming a Lucas critique bias, which is not related to time-inconsistency.

Section 5 re-evaluates the time-inconsistency argument and the impossibility of policy maker’s credibility leading to the impossibility to use negative feedback grounded by control. Firstly, it credits time-inconsistency to Simaan and Cruz’ (1973b) first contribution with Kydland (1975, 1977), Calvo (1978) and Kydland and Prescott (1980) as followers. Secondly, it highlights that the inflationary bias in Barro and Gordon’s (1983) static model is distinct from Calvo’s (1978) dynamic time-inconsistency.

Section 6 explains how Taylor (1993) rhetorically translated Friedman’s (1948) rules

versus discretion controversy in his “Semantics” section to the advantage of the negative-feedback mechanism of his Taylor rule.

Section 7 concludes that the “rules *versus* discretion” controversy on macroeconomic stabilization policy biased and delayed the efficient transfers of knowledge from another field of research (the field of control), and by doing so, it delayed scientific progress.

2 Rules *versus* Discretion and Control

2.1 Self-Stabilizing Markets: Smith (1776) and Ezekiel’s (1938) Counter-Example

The main underlying disagreement of the debate starting with Friedman’s (1948) ‘rules *versus* discretion’ and continuing with the new-classical macroeconomists’ attack against stabilization policy during the 1970s and 1980s, is the question which forces stabilize the economy. Relating this to optimal control, i.e. finding a control law such that an objective function is optimized in a dynamical system, the question is whether it is the private sector’s behavior alone that leads to an economic equilibrium or whether there is a need for economic policy in the form of government intervention. As a prominent example, optimal control of the private sector, using negative feedback, stabilizes the markets in Kydland and Prescott’s (1982) business cycle model.

As has been noted by Mayr (1971) it is even possible to interpret Adam Smith’s (1776) self-regulating local stability of supply and demand market equilibrium as a negative-feedback mechanism.

When the quantity brought to market exceeds the effectual demand, it cannot be all sold to those who are willing to pay the whole value.... Some part must be sold to those who are willing to pay less, and the low price which they give for it must reduce the price of the whole. (Adam Smith’s (1776, chapter 7))

Mayr (1971), however, did not relate his control translation of Smith to the Cobweb model (Ezekiel (1938)). In the Cobweb model, the deviation of the market price p_t from its natural (equilibrium) price p^* is a decreasing function of excess supply, the difference between supply x_t^s and demand x_t^d :

$$p_t - p^* = F(x_t^s - x_t^d) \text{ with } F < 0.$$

Conversely, the difference between the market price and the natural price is the signal that tells the producer whether to increase or decrease his production. Excess supply increases with the price, including a time lag to adjust supply:

$$x_{t+1}^s - x_{t+1}^d = B(p_t - p^*) \text{ with } B > 0.$$

Excess supply does not depend on its own lagged value $x_t^s - x_t^d$. There is no persistence of excess supply in the case where the price is set to its equilibrium value: $p_t = p^*$.

If at any time it [the supply] exceeds the effectual demand, some of the component parts of its price must be paid below their natural rate. If it is rent, the interest of the landlords will immediately prompt them to withdraw

a part of their land; and if it is wages or profit, the interest of the labourers in the one case, and of their employers in the other, will prompt them to withdraw a part of their labour or stock from this employment. The quantity brought to market will soon be no more than sufficient to supply the effectual demand. (Adam Smith's (1776, chapter 7))

Smith concludes that this feedback mechanism implies that market prices tends towards the natural equilibrium price:

The natural price, therefore, is, as it were, the central price, to which the prices of all commodities are **continually gravitating**. Different accidents may sometimes keep them suspended a good deal above it, and sometimes force them down even somewhat below it. But whatever may be the obstacles which hinder them from settling in this center of repose and continuance, they are **constantly tending towards it**. (Adam Smith (1776, chapter 7))

As opposed to Smith's (1776) intuition, however, the convergence result in the Cobweb dynamics is only valid under a specific condition for price elasticities of supply and demand:

$$x_{t+1}^s - x_{t+1}^d = BF (x_t^s - x_t^d) \text{ requires } -1 < BF < 0.$$

Feedback can bring local stability (negative feedback) or local instability (positive feedback) within the private sector. Although Ezekiel (1938) does not cite Smith (1776), he uses the same word ("gravitate") for describing classical economic theory:

Classical economic theory rests upon the assumption that price and production, if disturbed from their equilibrium **tend to gravitate back toward that normal**. The cobweb theory demonstrates that, even under static conditions, this result will not necessarily follow. On the contrary, prices and production of some commodities might tend to fluctuate indefinitely [case $BF = -1$], or even to diverge further and further from equilibrium. [case $BF < -1$] (Ezekiel (1938), p. 278-279)

When $BF = 0$ ($F = 0$ or $B = 0$) the adjustment towards the equilibrium following an excess supply or excess demand shock takes only one period. The demand-first equation can be interpreted as a proportional feedback rule, where the price plays the role of the feedback-policy instrument of the private sector. Since the Cobweb model assumes zero open-loop persistence of supply, if the price elasticity of demand and therefore the parameter F was to be chosen optimally, it would be set to an infinite elasticity ($F = 0$). Then the optimal policy of the private sector is to peg the price at its optimal value $p_t = p^*$.

2.2 Positive *versus* Negative Feedback in a First-Order Two-Inputs Single-Output Linear Model

Before going into details on the rules-*versus*-discretion controversy in the next section, we will clarify the concepts of positive and negative feedback and their relation to controllability and local stability. For this we introduce explicitly a policy maker and consider

the monetary policy transmission mechanism as a “*first-order two-inputs single-output*” linear model as used in dynamic games (Simaan and Cruz (1973a)). First-order stands for one lag of the policy target in the transmission mechanism. The first input is a policy instrument decided by the private sector (for example output or consumption x_t). The second input is a policy instrument decided by the policy maker (for example, nominal funds rate i_t). The single output or single policy target can be inflation π_t (instead of the price level p_t in the cobweb model). The policy target and the policy instruments are written in deviation of their long run equilibrium values:

$$\pi_{t+1} = A'\pi_t + B'x_t + Bi_t + \varepsilon_t \text{ with } A' \geq 0, B' \neq 0, B \neq 0, \pi_0 \text{ given.} \quad (1)$$

Additive disturbances are denoted ε_t and are assumed to be identically and independently distributed. If $B' \neq 0$, the private sector’s policy instrument is correlated with the future value of the policy target. Then, this first-order linear model is Kalman (1960a) controllable by the private sector. If $B \neq 0$, the policy maker’s policy instrument is correlated with the future value of the policy target. Then, this first-order linear model is Kalman (1960a) controllable by the policy maker. \square Both, the private sector and the policy maker behave according to proportional feedback rules given by:

$$x_t = F'\pi_t \text{ and } i_t = F\pi_t \text{ with } F' \in \mathbb{R} \text{ and } F \in \mathbb{R}. \quad (2)$$

In a first step, substituting the private sector’s feedback rule in the transmission mechanism implies:

$$\pi_{t+1} = A\pi_t + Bi_t + \varepsilon_t \text{ where } A = A' + B'F'. \quad (3)$$

For A' and B' given and if the values of the policy instrument x_t are not constrained (for example by a endowment constraint), the private sector can choose any real value for F' and therefor also for $A' + B'F'$. As a consequence $E_t\pi_{t+1}$ can take any target value.

Consider the case where the policy maker pegs its policy instrument to its long run value ($i_t = 0$). As we never measure negative auto-correlation for macroeconomic time series, we can assume that $A \geq 0$. We also never measure zero auto-correlation (no persistence) for macroeconomic time series. Nonetheless, we also consider the case of zero persistence $A = 0$ in this paper, because it played a crucial, but unnoticed role in the rules *versus* discretion controversy. Then, three outcomes are possible for the private sector’s feedback:

Case 1 $A = A' + B'F' = 0$ for $F' = \frac{-A'}{B}$. The value of the private sector’s feedback rule parameter F' implies no persistence of the policy target following a random shock without persistence.

Case 2 $0 < A = A' + B'F' < 1$ for $\frac{-A'}{B} < F' < \frac{1-A'}{B}$ if $B' > 0$. The value of the feedback rule parameter F' implies persistence with stationary dynamics of the policy target following a random shock without persistence.

Case 3 $A = A' + B'F' \geq 1$ for $F' > \frac{1-A'}{B}$ if $B' > 0$. The value of the feedback rule parameter F' implies a diverging trend with non-stationary dynamics of the policy target following a random shock without persistence.

Negative feedback and positive feedback mechanism are defined in the following way:

¹As a reminder, a model exhibits Kalman controllability if the policy instruments have a direct or indirect effect on the policy target.

Definition 1 *Negative-feedback rule parameters F' are such that $0 \leq A' + B'F' < A'$, which implies $B'F' < 0$.*

Since any disturbance automatically causes corrective action in the opposite direction, the parameters B and F have opposite signs.

Definition 2 *Positive-feedback rule parameters F' are such that $0 \leq A' < A' + B'F'$, which implies $B'F' > 0$.*

Proposition 1 *Negative feedback does not imply local stability for the private sector's policy-rule parameters F' such that $0 \leq 1 < A' + B'F' < A'$. The requirement for negative feedback and local stability is therefore that the private sector's policy-rule parameter F' satisfies: $0 \leq A' + B'F' < \min(A', 1)$. Conversely, positive feedback does not imply local instability for the private sector's policy-rule parameters $0 \leq A' < A' + B'F' < 1$.*

In a second step, the policy maker chooses $A = A' + B'F'$ and B . Substituting the policy maker's feedback-rule in the transmission mechanism implies:

$$\pi_{t+1} = (A + BF)\pi_t + \varepsilon_t. \quad (4)$$

For A and $B \neq 0$ given and if the values of the policy instrument are not constrained (for example by a zero lower bound for funds rate) so that the policy maker can choose any real value for F , the policy maker can target $E_t\pi_{t+1}$ at any real value because he can choose any real value $A + BF$.

The condition for the policy maker's negative-feedback and stabilizing policy-rule parameters is given by the set of parameters F satisfying: $0 \leq A + BF < \min(A, 1)$.

2.3 Rules *versus* Discretion

Even though the main focus of the paper is the period after 1948, it is worth mentioning Simons' (1936) article on "rules versus authorities" as a predecessor of the literature on policy rules. According to Simons (1936), authorities are not necessarily only related to a policy maker's negative-feedback behavior, but also to any random or erroneous policy maker's decision, such as Gold standard "rules" or trade wars. First of all, a 100% reserve requirement should be set so that private banks and shadow banks cannot create money. This would eliminate the financial instability due to a banking crisis. Secondly, a rigid public rule on central bank public creation of money should be fixed. Thirdly, free market competition in the real sector should prevail.

Based on these ideas, Friedman (1948) defines rules *versus* discretion as follows:

[For government expenditures excluding transfers,] no attempt should be made to vary expenditures, either directly or inversely, in response to cyclical fluctuations in business activity. ... The [transfer] program [such as unemployment benefits] should not be changed in response to cyclical fluctuations in business activity. Absolute outlays, however, will vary automatically over the cycle. They will tend to be high when unemployment is high and low when unemployment is low. (Friedman (1948), p. 248)

In addition to Simons' (1936) 100% reserve requirements by private financial institutions, Friedman (1948) also advocated zero public debt and allowed money supply to move cyclically in order to finance cyclical deficits or surpluses. He shifted to a fixed money-supply growth-rate rule in Friedman (1960): “*The stock of money [should be] increased at a fixed rate year-in and year-out without any variation in the rate of increase to meet cyclical needs*”. Rules can be Friedman’s (1960) fixed k -percent growth rate of money supply ($m_t = 0$), no public deficits ($s_t = 0$), an interest rate peg ($i_t = 0$), or an exchange rate peg ($e_t = 0$). These definitions are the same as in Kydland and Prescott (1977). They will change, however, at the end of the 1970s (see section 6). Using our framework, rules are defined as follows:

Definition 3 *A policy maker follows a “Rule” whenever he pegs his policy instruments to their steady state values ($F = 0$ and $i_t = 0$), with policy target dynamics $\pi_{t+1} = A\pi_t + \varepsilon_t$.*

Condition 1 *In order to have stable dynamics for the policy target with “Rules”, the private sector always decides to stabilize the value of the policy-rule parameter F' : $0 \leq A' + B'F' < 1$ (case 1 and case 2).*

One could interpret this behavior of the private sector as Smith’s (1776) implicit hypothesis of market clearing.

Negative-feedback counter-cyclical fiscal policy evolved with Keynes’ (1936) “General Theory” and the idea that the equilibrium is not automatically reached by market forces alone, but that there exist situations where a government intervention is necessary.

Definition 4 *“Discretion” is a policy that responds to cyclical fluctuations of the deviations of the policy variables from their long run target values ($i_t = F\pi_t$ with $F \neq 0$), with policy target dynamics $\pi_{t+1} = (A + BF)\pi_t + \varepsilon_t$.*

Condition 2 *In order to have stable dynamics for the policy target and policy maker’s negative-feedback, the policy maker decides policy rule parameter F to satisfy $0 < A + BF < A < 1$ in case 2, or to satisfy $0 < A + BF < 1 < A$ in case 3. In case 1, $0 \leq A + BF \leq A = 0$, a discretionary policy with $BF \neq 0$ adds persistence with respect to a policy pegging the policy instrument to its long run value ($F = 0$): negative-feedback cannot be achieved with $F \neq 0$.*

Using the above framework, table 1 summarizes distinctive features of the rules *versus* discretion (1948-1993) controversy.

Table 1: Rules *versus* Discretion, Stabilization Policy Ineffectiveness *versus* Negative-feedback Controversy (1948-1993).

	“Rules”, “Laissez-faire”	“Discretion”
Policy	Ineffective	Effective
Proponents	Friedman, Barro, Gordon	Phillips, Taylor
Fischer’s label	Inactive (Passive)	Activist
Feedback	No feedback, Peg	Negative feedback
“Rule”	$i_t = F\pi_t = 0$ with $F = 0$	$i_t = F\pi_t$ with $F \in D_{nf}$
Transmission	$\pi_{t+1} = A\pi_t + Bi_t + \varepsilon_t$	$\pi_{t+1} = A\pi_t + Bi_t + \varepsilon_t$
Controllability	Not necessary: $B \in \mathbb{R}$	Necessary: $B \neq 0$
Optimal	$A = 0$	$A > 0$
Phillips curve	Static: $A = 0$	Accelerationist: $A = 1$
Dynamics	$E_t\pi_{t+1} = A^t\pi_0$	$E_t\pi_{t+1} = (A + BF)^t\pi_0 < A^t\pi_0$
Target π_t persistence condition	Private sector necessarily stationary dynamics: $0 \leq A < 1$	$0 \leq A + BF < \min(A, 1)$ $BF < \min(0, 1 - A)$ F opposite sign of B
Lucas critique	A not a function of F	$A + BF$ function of F
Time inconsistent	Static model: irrelevant	Time inconsistent if π_t jumps
Control label	Open loop	Closed loop

Prominent supporters of the “rules” side are Friedman (1948, 1960) and Barro and Gordon (1983a and b). Their implicit assumption is case 1 ($A = 0$) and static models without lags of the policy target (Blanchard and Fischer (1989), p.581). Barro and Gordon (1983a and b) consider a static Phillips curve ($A = 0$).

Prominent supporters of the “discretion” side are Phillips (1954b), Taylor (1993, 1999). They implicitly assume a dynamic model with trend (case 3: $A > 1$) and possibly with stationary persistence (case 2: $0 < A < 1$). In case 2, persistence A is not too small so that the reduction of persistence subtracting $BF < 0$ is not negligible. Taylor (1999) and Fuhrer (2010) consider an accelerationist Phillips curve ($A = 1$), which may be related to trend inflation in the 1970s in the USA. Volcker’s discretionary monetary policy during 1979-1982 reversed non-stationary trend inflation into stationary inflation $0 \leq A + BF < 1 \leq A$. The rule parameter F is a bifurcation parameter, for given values of the parameters A and B of monetary policy transmission mechanism.

In control theory, the policy responses are always conditional on the transmission mechanism. It does not make sense to put forward a policy rule without specifying the policy transmission mechanism. For Nelson (2008, p.95), Friedman and Taylor agreed "*on the specification of shocks, policy makers’ objectives and trade-offs. Where they differed was on the extent to which structural models should enter the monetary policy decision making process.*"

3 The Transplants of Control Tools from Engineering to Stabilization Policy

Smith did not refer to engines as an analogy when he explained the negative-feedback mechanism in the *Wealth of Nations* (see Mayr (1971)) even though he knew Watt and engineers’ machines using negative-feedback. The concept of negative feedback was used by economist most of the time without a reference to engineer’s techniques until classic control emerged. With respect to the modeling of macroeconomic stabilization policy during the period of the rules *versus* discretion controversy 1948-1993, there have been

three stages of implementation of control theory according to Zhou, Doyle and Glover (1996) and Hansen and Sargent (2008). The first one is classic control without a loss function in the 1950s, with proportional, integral and derivative (P.I.D) policy rules. The second one is optimal control including a quadratic loss function with a Kalman linear quadratic regulator, optimal state estimation with a Kalman filter and stochastic optimal control merging both methods with the linear quadratic Gaussian model in the 1960s. The third stage is robust control which takes into account uncertainty on the parameters of the policy transmission mechanism in the 1980s.

3.1 The Fast Transplant of Classic Control

Tustin (1953), an electrical engineer at the University of Birmingham, mentions to have started in 1946 applying classic control methods used in electrical systems to Keynesian macro-models (Bissell (2010)). Phillips (1954a), an electrical engineer hired at the London School of Economics, wrote a two page book review on Tustin (1953) in the *Economic Journal*. He built the hydraulic computer MONIAC (Monetary National Income Analogue Computer) in 1949 (Leeson (2011)). The MONIAC is a series of connected glass tubes filled with water where the flow represented GNP and the feedback system represented the use of monetary and fiscal policy. Phillips (1954b (from Phillips' PhD) and, 1957) used proportional, integral and derivative (P.I.D.) rules of classic control to stabilize an economic model using negative-feedback mechanism (Hayes (2011)). Taylor's (1968) master thesis merged Phillips' (1961) model of cyclical growth with Phillips' (1954b) proportional, integral and derivative negative-feedback stabilization rules. Thirty-nine years after Phillips (1954b) and twenty-five years after Taylor's (1968) P.I.D rules, the Taylor (1993) rule is a proportional (P) feedback rule of classic control. Taylor, in Leeson and Taylor (2012)), explains why he took sides with discretion:

I viewed policy rule as a natural way to evaluate policy in the kinds of macroeconomic models which I learned and worked on at Princeton and Stanford. It was more practical than philosophical or political. (Leeson and Taylor (2012))

We now highlight how classic control takes sides with “discretion”: The policy maker targets his preferred persistence of the time-series of policy targets, which determines his preferred speed of convergence of these policy targets to their long run equilibrium. For example, a central bank could do an inflation persistence targeting of an auto-correlation of inflation of 0.8, satisfying a stability and negative feedback condition: $0 \leq \lambda^* = A + BF^* = 0.8 < \min(A, 1)$. This decision is called in classic control “pole placement”, because λ is a pole or a zero of the polynomial at the denominator of the Laplace transform of the closed-loop system.

Accordingly, the policy maker decides on a policy rule parameter $F^* = \frac{\lambda^* - A}{B} = \frac{0.8 - A}{B}$ in the case of a proportional feedback rule. For example, if there is a negative marginal effect of the funds rate on inflation ($B < 0$), the policy rule F^* is an affine decreasing function of the inflation persistence target λ^* .

Taylor's (1999) transmission mechanism is an accelerationist Phillips curve (where x_t is the output gap and a is the slope of the Phillips curve) and an investment saving (IS) equation:

$$\pi_{t+1} = \pi_t + ax_t, a > 0 \text{ and } x_t = -b(i_t - \pi_t), b > 0. \quad (5)$$

So that the transmission mechanism of monetary policy is such as $A = 1 + ab > 1$:

$$\pi_{t+1} = (1 - B) \pi_t + B i_t \text{ with } B = -ab < 0. \quad (6)$$

The Taylor principle states that the funds rate should respond by more than one to deviation of inflation from its long run target ($F > 1$). The Taylor principle corresponds to the classic control condition for negative-feedback rule parameters such that $0 < A + BF < \min(1, A)$, for models such that $B < 0$ and $A = 1 - B > 1$ (Taylor (1999)):

$$0 \leq A + BF = 1 - B + BF < 1 \text{ and } B < 0 \Rightarrow 1 < F < -\frac{B}{A} = -\frac{B}{1 - B}. \quad (7)$$

The upper bound condition on F corresponds to zero persistence of inflation.

3.2 The Fast Transplant of Optimal Control

The second step in the transfer of methods used in engineering to economic modeling is the introduction of optimal control in the 1950s, see Duarte (2009) and Klein (2015). In optimal control, a quadratic loss function is minimized subject to linear dynamic equations. Using a certainty-equivalence property, normal disturbances with zero mean can be added to the model according to Simon (1956) and Theil (1957).

In the 1950s, Simon, Holt, Modigliani and Muth came to the *Graduate School of Industrial Administration* at the *Carnegie Institute of Technology* in Pittsburgh. Holt had come from an engineering background at M.I.T. and Simon's father was an electrical engineer after earning his engineering degree in Technische Hochschule Darmstadt.² (Leeson and Taylor (2012)). They applied control methods to microeconomics by computing variables for production, inventories and the labor force of a firm. Optimal linear decision rules from linear quadratic models were computed for specific economic models of firm's production by Holt, Modigliani and Simon (1955) and Holt, Modigliani and Muth (1956), (Singhal and Singhal (2007)). Holt (1962) developed an optimal control model to analyze fiscal and monetary policy.

Kalman (1930-2016), an electrical engineer, wrote the key paper for solving linear quadratic optimal control (linear quadratic regulator, LQR).³ He extended the static Tinbergen (1952) principle, namely that there should be as many policy instrument as policy targets, to a dynamic setting. Kalman's (1960a) controllability definition is such that a single instrument can control for example three policy targets, but in three different periods (Aoki (1975)). Masanao Aoki (1931-2018) was a Japanese professor of engineering at UCLA and California, Berkeley from 1960-1974, before switching fields to economics. Kalman's (1960a) linear quadratic regulator sets the solution of stabilization policy facing a quadratic loss function solving matrix Riccati equations. Textbooks include Sworder (1966) and Wonham (1974) among others.

The diffusion of control techniques to macroeconomics was complementary to the development of large scale macroeconomic models:

Professor Bryson was offering a course in the control theory in 1966 that caught the attention of a small group of economics graduate students and

²Phillips' and Taylor's fathers were also engineers, (Leeson and Taylor (2012))

³He was invited to participate in the world econometric congress in 1980 in Aix en Provence, but his paper was not published in *Econometrica*. He later received the highest honor of US science, the US medal of science in 2009.

young faculty members at Harvard... Rod Dobell, Hayne Leland, Stephen Turnovsky, Chris Dougherty, Lance Taylor and I persisted... Two control engineers who had shifted their interest to economics – David Livesey at Cambridge University and Robert Pindyck at MIT – developed macroeconomic control theory models (in 1971 and 1972).... In May of 1972, a meeting of economists and control engineers was arranged at Princeton University by three economists (Edwin Kuh, Gregory Chow and M. Ishaq Nadiri) and a control engineer. The meeting which was attended by about 40 economists and 20 engineers was to explore the possibility that the application of stochastic control techniques, which had been developed in engineering, would prove to be useful in economics as well (Athans and Chow, 1972).... Another British-trained control engineer, Anthony Healy, who was teaching at the University of Texas at the time, took an interest in economic models and applied the use of feedback rules to a well-known model that had been developed at the St. Louis Federal Reserve Bank (FRB). (Kendrick (2005), p. 7-8)

In the ongoing debate “discretion” *versus* “rules”, optimal control – with a policy maker minimizing a loss function – was used by macroeconomists in favor of “discretion”, whereas those in favor of “rules” model the private sector’s optimal behavior with stationary policy targets ($0 < A = A' + B'F' < 1$). Nonetheless, even in this case, optimal control by the policy maker is still able to decrease the loss function further.

Optimal control is filling a gap in the “pole placement” method of classic control where the criterion for choosing the persistence λ^* of the policy target is not explicitly stated ($0 < \lambda^* = A + BF^* < 1$). Kalman’s optimal control uses a quadratic loss function with the possibility of discounting future periods with a factor β and non-zero quadratic cost of changing the policy instrument, $R > 0$, in order to ensure concavity. The relative cost of changing the policy instrument (R/Q) represents e.g. central banks’ interest-rate smoothing or governments’ tax smoothing. In the case of the private sector, it corresponds to households’ consumption smoothing or firms’ adjustment costs of investment. Maximize

$$-\frac{1}{2} \sum_{t=0}^{+\infty} \beta^t (Q\pi_t^2 + Ri_t^2), \text{ with } R > 0, Q \geq 0 \text{ and } 0 < \beta \leq 1, \quad (8)$$

subject to the same transmission mechanism as the one of classic control.

Solving this linear quadratic regulator yields two roots of a characteristic polynomial of order two, one of them stable, the other one unstable. For this reason, Blanchard and Kahn (1980) call this solution “saddlepath stable”, in a space which adds co-state variables (Lagrange multipliers) and state variables (policy targets). This implies that the dynamics of the state variables is stable, exactly like in classic control.

Optimal persistence λ^* is a continuous increasing function of the relative cost of changing the policy instrument: $\lambda^* \left(\frac{R}{Q} \right)$. The relation between the persistence of the policy target and policy-rule parameter, $A + BF^* = \lambda^*$, is the same for optimal control as for classic control. Unless the targeted persistence does not belong to the interval: $\lambda^* \in \left] 0, \min \left(A, \frac{1}{\beta A} \right) \right[$, a simple rule derived from classic control with an *ad hoc* targeted persistence λ^* is observationally equivalent to an optimal rule $\lambda^* \left(\frac{R}{Q} \right)$, with identical predictions and behavior of the policy maker. A reduced form of a simple rule parameter

F corresponds to an optimal rule $F\left(\frac{R}{Q}\right)$ with preferences $\frac{R}{Q}$. There exist preferences of the policy maker that “rationalize” an estimated value of a simple rule parameter F to be a reduced form of an optimal rule parameter $F\left(\frac{R}{Q}\right)$.

Proposition 2 *A rule pegging the policy instrument ($i^* = 0, F = 0$) is **optimal** for a quadratic loss function (which is bounded if: $\beta A^2 \neq 1$), with a first-order single policy maker’s instrument single-policy-target transmission mechanism:*

(i) *for stationary persistence of the policy target $0 < A < 1/\sqrt{\beta}$ and for a zero weight ($Q = 0$) on the volatility of the policy target in the policy maker’s loss function.*

(ii) *for zero persistence of the policy target $A = 0$ and a positive weight $Q \geq 0$ of the volatility of the policy target in the loss function.*

Proof. (i) With zero weight on the policy target ($Q = 0$), the relative cost of changing the policy instrument is infinite ($R/Q \rightarrow +\infty$), which corresponds to maximal inertia of the policy. Only the first of these two cases allows $F^* = 0$. In the second case, $F^* = 0$ corresponds to $\beta A^2 = 1$ with unbounded utility (see appendix).

$$0 \leq F^* = \frac{\lambda^* - A}{B} \leq \frac{-A}{B}, \text{ if } 0 < A < \frac{1}{\sqrt{\beta}}, B < 0, 0 < \beta \leq 1.$$

$$\frac{\frac{1}{\beta A} - A}{B} \leq F^* = \frac{\lambda^* - A}{B} \leq \frac{-A}{B}, \text{ if } A > \frac{1}{\sqrt{\beta}} \text{ and } B < 0, 0 < \beta \leq 1.$$

If the feedback parameter is zero ($F = 0$), optimal policy corresponds to a rule ($i^* = 0$). The policy target dynamics $\pi_t = A\pi_{t-1}$ is not taken into account in the expected loss function.

(ii) For $A = 0$ and for $B \neq 0$, the system is controllable with a non-zero effect of the policy instrument on the policy target:

$$\text{Min } (\beta Q \pi_t^2 + R i_{t-1}^2) \text{ subject to: } \pi_t = B i_{t-1} \Rightarrow \text{Min } \left(\left(\beta Q + R \frac{1}{B^2} \right) \pi_t^2 \right).$$

For each period with identical repeated optimizations, a rule ($i^* = 0 = \pi^*$) is the optimal solution, whatever the magnitude of the relative cost of changing the policy instrument $R/Q > 0$. ■

The weakness of static analysis, however, was stated by Phillips (1954b) in a seminal paper on stabilization:

The time path of income, production and employment during the process of adjustment is not revealed. It is quite possible that certain types of policy may give rise to undesired fluctuations, or even cause a previously stable system to become unstable, although the final equilibrium position as shown by a static analysis appears to be quite satisfactory. (Phillips (1954a, p.290))

“Rules” are optimal for a *static* model $A = 0$, but in a *dynamic* model where $A \geq 1$, rules lead to instability and huge welfare losses. In addition, the hypothesis $A = 0$ cannot explain the observed persistence of all macroeconomic time series, which are stationary if $0 < A < 1$ or non-stationary if $A \geq 1$. It cannot explain the variations of the non-zero persistence of inflation ($A + BF > 0$), such as the acceleration of inflation or the

disinflationary period during 1973 to 1982 with observed changes of the Fed's policy behavior during Volcker's mandate.

Optimal policy (rules) in a repeated *static* model of the transmission mechanism ($A = 0$, as with e.g. a Phillips curve) is never a modeling shortcut with results extended by analogy to optimal policy in *dynamic* models of the transmission mechanism ($A \geq 1$, as with e.g. an accelerationist Phillips curve or a new-Keynesian Phillips curve) where discretion is optimal.

The above proposition for "rules" is bad news for the proponents of "rules" because "rules" are sub-optimal in dynamic models with policy makers using optimal control. Therefore, it was top of the research agenda of the proponents of "rules" in the controversy with discretion (that is, the opponents of Keynesian stabilization policy) to seek opportunities in order to add distortions in order to "prove" that optimal control techniques could not be used by policy makers for stabilization policy.

In addition, the above proposition renders useless Friedman (1953) results recently put forward by Forder and Monnery (2019) as a contribution for his Nobel prize. He considers the simplest static model for the policy transmission mechanism:

$$\pi_{CL} = \pi_{OL} + i.$$

The policy target randomly deviates from its zero equilibrium value with the value π_{OL} if the policy instrument i is set to zero (open loop). After a change of the policy instrument i , the closed loop value of the policy target is equal to π_{CL} . The policy maker's loss function is the volatility of the closed loop policy target $\sigma_{\pi_{CL}}^2$. The loss function is obviously minimized $\sigma_{\pi_{CL}}^2 = 0$ for this optimal negative feedback rule: $i = -\pi_{OL}$, which implies $\pi_{CL} = 0$, $\rho_{i\pi_{OL}} = -1$ and $\sigma_i = \sigma_{\pi_{OL}}$. Friedman's (1953) condition for sub-optimal *negative feedback* rules is obtained after substitution of the transmission mechanism in the loss function:

$$\sigma_{\pi_{CL}}^2 = \sigma_{\pi_{OL}}^2 + \sigma_i^2 + 2\rho_{i\pi_{OL}}\sigma_i\sigma_{\pi} < \sigma_{\pi_{OL}}^2 \Rightarrow -1 \leq \rho_{i\pi_{OL}} < -\frac{1}{2}\frac{\sigma_i}{\sigma_{\pi_{OL}}}.$$

Friedman (1953) discusses verbally the effect of lags of the transmission mechanism. His mathematical formulation of the model, however, has no lags. As mentioned by Phillips (1954b), a negative feedback rule for a static model can drive instability in a dynamic model.

3.3 A Conflict between the Kalman Filter and Stochastic Optimal Control?

Additionally to the linear quadratic regulator (LQR, Kalman (1960a)) Kalman developed a filter-estimate that recursively takes in each period the new data of the current period into account (Kalman (1960b)). This filter is used, for example, in the global positioning system (GPS) since its inception in 1972.

It is fitting that at the conference [1st world IFAC conference, Moscow, 1960] Kalman presented a paper, 'On the general theory of control systems' (Kalman, 1960) that clearly showed that a deep and exact duality existed between the problems of multivariable feedback control and multivariable feedback filtering and hence ushered in a new treatment of the optimal control problem. (Bennett, 1996, p. 22).

Optimal trajectories derived from the linear quadratic regulator (LQR) and optimal state estimation using Kalman’s filter for linear quadratic estimation (LQE) were unified in the linear quadratic Gaussian (LQG) system. It is one of the main tools of Stochastic Optimal Control, see Stengel (1986) and Hansen and Sargent (2008). The Kalman filter has been quickly implemented in rational expectations theory, even though an insufficient emphasis on measurement can be observed.

The transplant of stochastic optimal control and the Kalman filter to macroeconomics was very fast. For example, during his Ph.D. (1973) in Stanford, Taylor participated in seminars that discussed books linking control and time-series, such as Whittle’s (1963) *Prediction and Regulation* or Aoki’s (1967) *Optimization of stochastic systems*, see Leeson and Taylor (2012). Hansen and Sargent (2007) confirm:

A profitable decision rule for us has been, ‘if Peter Whittle wrote it, read it.’ Whittle’s 1963 book *Prediction and Regulation by Linear Least Squares Methods* (reprinted and revised in 1983), taught early users of rational expectations econometrics, including ourselves, the classical time series techniques that were perfect for putting the idea of rational expectations to work. (Hansen and Sargent (2008), p. xiii)

Independently of Kalman (1960b), Muth (1960) updated conditional expectations based on new information in a simple model. Hansen and Sargent (2007) highlight that Muth (1960) is a particular case of Kalman’s (1960b) filter estimation. In the early 1970s, following Muth (1960), Lucas solved several rational expectations models using variants of a Kalman filter (Boumans (2020)). Most of the time, Lucas used a Kalman filter as a tool for solving theoretical models of rational expectations, instead of using it for what the Kalman filter is designed for in the field of control, namely the practical empirical estimation of the state variables and of the parameters of the transmission mechanism using time series.

Based on the observations of a few Lucas’ papers in the early 1970s and following Sent (1998), Boumans (2020) infers that engineering mathematics did not unify optimal trajectories and optimal state estimation, whereas they were in fact unified in the early 1960s in stochastic optimal control:

Engineering mathematics, however, is not one and unified field. This paper shows that the mathematical instructions come from informational mathematics, which should be distinguished from control engineering. (Boumans (2020)).

It may appear that the Kalman filter and stochastic optimal control are distinct approaches because Lucas used a Kalman filter in theoretical papers in the early 1970s and later dismissed stochastic optimal control for policy makers in the Lucas (1976) critique. In basic engineering textbooks, such as Stengel (1986), however, optimal trajectories (Boumans’ (2020) “control engineering”) and optimal state estimation including the Kalman filter (Boumans’ (2020) “informational mathematics”) are merged into stochastic optimal control:

Not surprisingly, the control principle of chapter 3 and the estimation principles of chapter 4 can be used together to solve the stochastic optimal control problem. (Stengel (1986), p. 420)

This artificial split between policy makers' optimal trajectories and optimal state estimation done by Lucas was in practice driven by the rules *versus* discretion controversy. This explains Lucas' change of perspective and tools. Optimal control, and especially stochastic optimal control with a policy maker reestimating in each period the optimal policy is clearly a method favored by macroeconomists in the camp "discretion". Hence, Lucas, as a proponent of "rules", took the opportunity to break the dual approaches unified by stochastic optimal control in two parts. The advocacy of particular theoretical views taking sides in a controversy and the selection of mathematical tools from control goes hand-in-hand. Scientific tools are part of the scientific rhetorics in order to convince a majority of scientists to opt for one side of the controversy.

The present study, then, provides a rationalization for rules with smooth monetary policy, exactly as did the earlier studies of Lucas, Sargent and Wallace, and Barro. Similarly, it rationalizes the analogous fiscal rule of continuous budget balancing and rules to stabilize the quantity of private money, such as larger reserve requirements for banks. (Lucas (1975), p. 1115)

Barnett (2017) identifies the different emphasis on measurement as the main methodological difference between rocket science and macroeconomics, with both fields using control methods. William A. Barnett had a BS degree in mechanical engineering from MIT in 1963. He worked as an "rocket scientist" at Rocketdyne from 1963 to 1969, a contractor for the Apollo program. He then got a Ph.D. in statistics and economics from Carnegie Mellon University in 1974 at the same time as Finn Kydland. He worked for the models of the Fed soon after.

The different emphasis on measurement is very major, especially between macroeconomics and rocket science... In real rocket science, engineers are fully aware of the implications of systems theory, which emphasizes that small changes in data or parameters can cause major changes in system dynamics. The cause is crossing a bifurcation boundary in parameter space... But when policy simulations of macroeconomic models are run, they typically are run with the parameters set only at their point estimates. For example, when I was on the staff of the Federal Reserve Board, I never saw such policy simulations delivered to the Governors or to the Open Market Committee with parameters set at any points in the parameter estimators' confidence region, other than at the point estimate. This mind-set suggests to macroeconomists that small errors in data or in parameter estimates need not be major concerns, and hence emphasis on investment in measurement in macroeconomics is not at all comparable to investment in measurement in real rocket science. (Barnett (2017), p.22)

Lucas, Kydland and Prescott did not increase the emphasis on measurement with respect to modelers at the Fed in the 1970s. They had prior theoretical views taking sides for "rules" in the debate on rules *versus* discretion. As well as optimal control, using a Kalman filter for optimal state estimation using macroeconomic time series would give a chance to challenge these prior views. Hence, it was a rhetorical strategy in the rules *versus* discretion controversy to challenge measurement and identification strategies.

Lucas (1976) presented as impossible the econometric identification of parameters in the case of reverse causality due to feedback rules. When confronting their model

with data, Kydland and Prescott (1982) decided not to use Kalman-filter optimal-state estimations and avoid identification issues, but did simulations. The increased distance between econometricians and new-classical macroeconomists with respect to identification issues in econometric estimations using a Kalman filter, led to a selection bias with respect to the tools and the practice of control. In the field of control, the measurement of the transmission mechanism is viewed as much more important than theoretical models based on a priori hypothesis taking sides in a controversy. This increased the gap between the macroeconomics of stabilization policy and the field of control in the 1980s.

Kalman, who worked for the Apollo program, emphasized that the most important issue for stabilization of dynamic systems is the *accuracy of the measurement* and the estimation of parameters of the system of policy transmission mechanism (A and most importantly B and its sign) instead of a priori modeling of the real world. For Kalman, measurement is above all necessary for optimal policy rules, so one should not “separate” filtering estimation with data from optimal trajectories found with control. One should not separate macroeconomic theory from econometrics using macroeconomic time-series. By contrast, the linear quadratic gaussian (LQG) system merging optimal policy and Kalman filter effective estimation with macroeconomic time series has not been widely used during the the 1980s.

3.4 The Delayed Transplant of Robust Control

The third stage of control theory, namely robust control (Zhou et al. (1996), Hansen and Sargent (2008)), emerged in the following of Doyle’s (1978) two-pages paper presenting a counter-example where the Kalman linear quadratic Gaussian (LQG) model has not enough guaranteed margins to ensure stability.

Robust control assumes that the knowledge of the transmission parameters is uncertain on a known *finite interval* with a given sign. In our example, it may correspond to $B_{\min} < B < B_{\max} < 0$. An evil agent tries to fool as much as possible the policy maker, which reminds of Descartes (1641) evil demon:

I will suppose therefore that not God, who is supremely good and the source of truth, but rather some malicious demon, had employed his whole energies in deceiving me. (Descartes (1641))

The policy maker is the leader of a Stackelberg dynamic game against the evil agent. The policy maker is minimizing the maximum of the losses in the range of uncertainty on the parameters. Kalman’s solutions of matrix Riccati equation for the linear quadratic regulator remain instrumental for finding the solutions of robust control.

For monetary policy, the uncertainty on parameters was put forward by Brainard (1967), so that, indeed, this is a very important issue. But the transplant of the new methods of robust control was delayed in the 1980s. Very soon after robust control tools emerged, Von Zur Muehlen (1982) wrote a working paper applying robust control to monetary policy which was never published in an academic journal. No major conference took place between leaders in the field of robust control and renowned macroeconomists like the ones described by Kendrick (1976) for stochastic optimal control in the early 1970s. Instead of at most three years for the spread of new tools from the field of control, one had to wait until the end of the 1990s, more than fifteen years. For now two decades, Hansen and Sargent (2008, 2011) transplanted robust control tools into macroeconomics (Hansen and Sargent (2008)), with still a limited number of followers in macroeconomics:

When we became aware of Whittle’s 1990 book, “*Risk Sensitive Control*” and later his 1996 book “*Optimal Control: Basics and Beyond*”, we eagerly worked our ways through them. These and other books on robust control theory like Basar and Bernhard’s H^∞ “*Optimal Control and Related Minimax Design Problems: A Dynamic Game*” Approach provide tools for rigorously treating the ‘sloppy’ subject of how to make decisions when one does not fully trust a model and open the possibility of rigorously analyzing how wise agents should cope with fear of misspecification.’ (Hansen and Sargent (2008), p. xiii)

The explanation of the delay for the transplant of robust optimal control is that the policy ineffectiveness arguments spread and took over for a while among influential macroeconomists, according to Kendrick (2005):

However, it was a problem in the minds of many! As a result, work on control theory models in general and stochastic control models in particular went into rapid decline and remained that way for a substantial time. In my judgment, it was a terrible case of ‘throwing the baby out with the bath water’. The work on uncertainty (other than additive noise terms) in macroeconomic policy mostly stopped and then slowly was replaced with methods of solving models with rational expectations and with game theory approaches.... I believe that the jury is still out on the strength of these effects and think that there was a substantial over-reaction by economists when these ideas first became popular. (Kendrick (2005), p.15)

We now re-evaluate the Lucas critique and the time-inconsistency theory that are at the origin of the long delay for using robust control methods and the relative scarcity of applied macroeconomic estimations using a Kalman filter jointly with optimal policy with the linear quadratic Gaussian model.

4 The Impossibility to Identify the Parameters of the Policy Transmission Mechanism

4.1 Lucas (1976) Critique

In popular terms, the Lucas critique states that (Keynesian) macroeconomic relations change with government policy. The Lucas critique is a parameter identification problem in a system of dynamic equations including reverse causality due to a feedback control rule equation. The model of the last section of the paper of the Lucas critique (Lucas, 1976) can be stated in the form of the first-order single-input single-output model. We add our notations of the previous sections in brackets in the Lucas (1976) quotation. The corresponding notations are: for the policy target: $y_t = \pi_t$, for the policy instrument: $x_t = i_t$, and for the parameters of the transmission mechanism: $\theta = (A, B)$ which also includes additive random disturbances ε_t . In order to avoid confusion with the control notation F for the policy rule parameter, we change Lucas’ notation of function $F(\cdot)$ into $D(\cdot)$:

I have argued in general and by example that there are compelling empirical and theoretical reasons for believing that a structure of the form

$$y_{t+1} = D(y_t, x_t, \theta, \varepsilon_t) \text{ [our notation: } \pi_{t+1} = A\pi_t + Bi_t + \varepsilon_t]$$

$D(\cdot)$ known, θ fixed, x_t arbitrary will not be of use for forecasting and policy evaluation in actual economies... One cannot meaningfully discuss optimal decisions of agents under arbitrary sequences $\{x_t\}$ of future shocks. As an alternative characterization, then, let policies and other disturbances be viewed as stochastically disturbed functions of the state of the system, or (parametrically)

$$x_t = G(y_t, \lambda, \eta_t) \text{ [our notation: } i_t = F\pi_t + \eta_t]$$

where G is known, λ is a fixed parameter vector, and η_t a vector of disturbances. Then the remainder of the economy follows

$$y_{t+1} = H(y_t, x_t, \theta(\lambda), \varepsilon_t) \text{ [or } \pi_{t+1} = (A + BF)\pi_t + \varepsilon_t + B\eta_t]$$

where, as indicated, the behavioral parameters θ [or $A + BF$] vary systematically with the parameters λ [or F] governing policy and other “shocks”. **The econometric problem in this context is that of estimating the function $\theta(\lambda)$ [or $A + BF$].** In a model of this sort, a policy is viewed as a change in the parameters λ [or F] or in the function generating the values of policy variables at particular times. A change in policy (in λ [or F]) affects the behavior of the system in two ways: first by altering the time series behavior of x_t ; second by leading to modification of the behavioral parameters $\theta(\lambda)$ [or $A + BF$] governing the rest of the system. (Lucas (1976, p.39-40))

The closed loop parameter ($A + BF$) determines the persistence of policy targets. It depends on the policy rule parameter F as in all closed loop models including feedback mechanism in the field of control. The econometric problem is the identification of parameters A , B and F in a system of dynamic equations with reverse causality. In addition, in the scalar case, the true parameters B and F have opposite signs for the case of a negative-feedback mechanism ($BF < 0$).

Lucas (1976) over-stated that this parameter identification problem cannot *in principle* be addressed.

The point is rather that this possibility [of feedback rules] cannot *in principle* be substantiated empirically. (Lucas (1976, p.41))

Because the identification of estimates of A and B cannot in principle be substantiated empirically, then it is better to use rules such as $i_t = 0$ and $F = 0$ instead of “discretion” with a feedback rule parameter $F \neq 0$.

This reverse-causality parameter identification problem is identical to the one of the private sector’s demand and supply price elasticities with respect to quantities, when there is negative feedback of supply on demand (section 2.1, Smith (1776) and Ezekiel’s (1938) cobweb model). The supply of goods should increase when price increases, whereas the demand of goods should decrease when price increases. But there is *only one covariance* for observed prices and quantities with a *single sign*.

According to Koopmans (1950), the identification of B and F finding opposite signs can *in principle* be substantiated empirically, if one finds strong and exogenous instrumental variables with distinct identification restrictions for both the transmission mechanism (demand) and the feedback equation (supply). Finding these instrumental variables with their identification restrictions is far from being easy with macroeconomic data. One answer is instrumental variables allowing identification via identification restrictions such as exogenous monetary policy shocks, exogenous inflation shocks or lags of some explanatory variables in vector autoregressive (VAR) models which include policy-target (inflation) and policy-instrument (funds rate) equations or lags of explanatory variables.

Although Lucas (1976) does not mention microeconomic foundations, it is often erroneously claimed that microeconomic foundations are required because of the Lucas (1976) critique. From private sector’s demand and reverse causality of supply with negative feedback, the private sector’s behavior estimates of elasticities are also subject to the Lucas (1976) critique. If the private sector’s behavior is modeled with microfoundations (section 3.2, intertemporal optimization using optimal control), the representative household’s behavior includes an optimal policy-rule equation F' besides the law of motion of the private sector’s state variables with parameters (A', B') as in section 2.3. The Lucas critique applies as well: there is a system of two equations with reverse causality and opposite signs for these two parameters: $B'F' < 0$. The private sector persistence parameter $A' + B'F'$ is not a structural parameter. Therefore, private sector’s microfoundations are never an “answer” to the parameter identification problem of the Lucas (1976) critique. Dynamic models of the private sector alone face also the Lucas critique, even without policy intervention where $BF = 0$ using section 2.3 notations.

Lucas’ rhetorical over-statement was successful, according to Blinder’s interview in July 1982 reported in Klammer (1984):

This is a case where people have latched upon a criticism. All you have to do in this country (more than in other places) right now is scream mindlessly, “Lucas critique!” and the conversation ends. That is a terrible attitude. (Klammer (1984), p.166)

Instead of fostering a research agenda on identification issues with reverse causality in applied econometrics, it convinced a number of macroeconomists that applied macroeconomics was to be dismissed and hopeless. Macroeconomic theorists favored theory with the private sector’s microeconomic foundations using simulations, instead of tackling thorny identification issues in econometrics.

4.2 Sims (1980) faces the Lucas critique: The VAR Price Puzzle for Identifying B and F with Opposite Signs

Several Keynesian applied econometricians, including Blinder, investigated the stability of reduced form estimates $A + BF$ without finding much change on several key equations from 1960 until 1979 or 1982 (Goutsmedt et al. (2019)). But it is possible to have unchanged reduced form persistence parameters, even when the “structural” parameters did change from period 1 (before 1973) to period 2 (after 1973): $A_1 + B_1F_1 = A_2 + B_2F_2$. The difficult step is to identify separately A , B and F for each period, knowing that there is a reverse causality with opposite signs ($BF < 0$) of the policy instrument and the policy target in the transmission mechanism (parameter B), on the one hand, and in the policy rule (parameter F), on the other hand.

The Lucas critique was used as an explanation for the forecasting errors of large scale models such as the FMP model, for Federal Reserve-MIT-Penn. Another critique of large scale forecasting models was put forward by Sims (1980) with his vector auto-regressive models (VAR). Sims (1980) warned that some identification restrictions (for example, constraining some parameters to be zero) were not tested in macroeconometrics in the 1970s.

But soon an unexpected anomaly of VAR models appeared, labeled the “price puzzle” (Rusnák et al. (2013)). We highlight that the VAR price puzzle is closely related to the Lucas (1976) critique. Impulse response functions obtain that inflation increases, following a shock increasing the funds rate. This is the opposite estimate of the expected sign of the true model: $\widehat{B} > 0 > B$. Indeed, impulse response functions are such that the funds rate increases, following a shock increasing inflation, with the expected sign: $\widehat{F} > 0$. The identical sign for \widehat{B} and \widehat{F} comes from the fact that the sign of the covariance between the policy instrument and the policy target does not change with one or two lags. The price puzzle is such that $\widehat{B} > 0$ and $\widehat{F} > 0$, so that there seems to be a positive feedback mechanism on inflation persistence of policy $\widehat{A} + \widehat{B}\widehat{F} > A$, even during Volcker’s disinflationary policy with negative-feedback mechanism.

Estimating the correct sign of B is of utmost importance for policy advice. Negative feedback reduces the persistence of the policy target according to the condition: $0 < A + BF < A$. This implies $BF < 0$. The sign of F should be the opposite of the sign of B . If one estimates wrongly that $\widehat{B} > 0$ whereas its true value is $B < 0$, the policy advice will be $F < 0$, so that $\widehat{B}\widehat{F} < 0 < BF$. Therefore, the policy advice will wrongly increase the persistence of the policy target:

$$0 < A + \widehat{B}\widehat{F} < A < A + BF \text{ if wrong estimated sign } B < 0 < \widehat{B}. \quad (9)$$

Several econometric techniques have been used to reverse the sign of the “price puzzle” (Rusnák et al. (2013)). Perhaps some of them correctly managed to answer the Lucas (1976) critique for some data set. Walsh’s (2017) textbook restricts the description of the price puzzle measurement issue to half a page. Walsh’s (2017) textbook demonstrates that it has been easier to develop a large variety of theoretical micro-founded macroeconomic models conflicting among themselves than to provide accurate measurements of A , B and F .

4.3 Kydland and Prescott (1982) face the Lucas’ critique

For a representative agent of the private sector, Ramsey (1928) models a negative-feedback mechanism for optimal savings or optimal consumption x_t with consumption smoothing (related to the notations R/Q for the linear quadratic approximation). This idea was formalized again independently by Cass, Koopmans and Malinvaud in 1965 (Spear and Young (2014)). Kydland and Prescott (1982) stuck to this idea adding auto-correlated productivity shocks z_t .

Their model is a stock-flow reservoir model where the stock of wealth or the stock of capital k_t has an optimal set point k^* (De Rosnay (1979), Meadows (2008)). The stock of capital is controlled by saving/investment inflows and it faces depreciation outflows. If the level of the stock is below its long run optimal target, consumption decreases proportionally according to a negative feedback proportional rule:

$$x_t = F'k_t + F'_z z_t, k_{t+1} = (A' + B'F')k_t + (A'_{kz} + B'F'_z)z_t \text{ with } 0 < A = A' + B'F' < 1 ,$$

where both the consumption flow and the capital stock are written in deviation from their long run target. Consumption also responds to an exogenous auto-correlated productivity shock z_t . To become rich, a poor representative households with capital below its long run target ($k_t < 0$) *only needs to save more* which implies his optimal consumption is below its long run target ($x_t < 0$). Hence, the poor representative household replenishes his reservoir storing his stock of wealth to its optimal level ($k_t = 0$).

When the relative cost of changing consumption (R/Q) increases, the policy rule parameter decreases (consumption is less volatile) and the persistence of capital or wealth increases A or equivalently, the speed of convergence of capital towards equilibrium decreases.

Kydland and Prescott (1982) send us back to Smith's (1776) view that a negative-feedback mechanism always works within the private sector with supply and demand interaction. This time, the negative-feedback mechanism is related to the investment/saving market. It is a typical model including microeconomic foundations which faces the Lucas critique⁴.

(1) There exists a problem of parameter identification due to a private sector reverse causality feedback rule. In particular, it is difficult to find properly identified opposite signs for optimal behavior of the private sector with negative feedback such that: $B'F' < 0$.

(2) They assume and do not test the identification restrictions such that $BF = 0$. They assume that there is no transmission mechanism of macroeconomic policy ($B = 0$) and no policy maker's feedback rule ($F = 0$). This amounts to consider that the autocorrelation of the policy target $A + BF$ is exogenous. But, if one does not test whether $BF = 0$ or not, one cannot exclude $BF \neq 0$ so that the persistence of the policy target is a reduced form parameter which depends on the policy rule parameter F according to $A + BF$. This is exactly the Lucas (1976) critique, as highlighted by Ingram and Leeper (1990):

Kydland and Prescott's model assumes that policy doesn't affect private decision rules. There is no policy evaluation to perform. Alternatively, if policy does affect private behavior, then the parameters Kydland and Prescott calibrate are reduced-form parameters for some underlying model embedding monetary and fiscal policy. Thus, if there is any policy evaluation left to perform, Kydland and Prescott's calibrated parameters must be functions of policy behavior and should change systematically with policy. (Ingram and Leeper (1990), p. 3)

Kydland and Prescott (1982) have chosen simulations and calibrations in order to avoid the econometric measurement of A' , B' , F' , B , and F . To solve and simulate their model, Kydland and Prescott (1982) used Kalman (1960a) LQR solutions of a Riccati equation. Without Kalman's (1960a) solution of optimal control for the private sector, real business cycles would not have been computed. Lucas selected a Kalman filter for his theoretical papers, but rejected optimal control for policy makers doing "discretion", Lucas (1976). Kydland and Prescott (1982) selected optimal control for the private sector only, but rejected Kalman filter estimations. Because Kydland and

⁴Sergi (2018) provides the history of the Lucas (1976) critique in relation to real business cycles models and dynamic stochastic general equilibrium models which followed Kydland and Prescott (1982) paper.

Prescott (1982) expected their model to be rejected by econometrics, they decided to avoid a measurement strategy based on a Kalman filter, as opposed to researchers in the field of control. Behind this seemingly inconsistent use of tools of control by Lucas and Kydland and Prescott, there remains a common motive: support “rules” in the rules *versus* discretion controversy. Write complicated and technical papers, imported from the tools of control, with a crucial hypothesis (here $BF = 0$) which handicaps policy maker’s negative-feedback discretion with respect to rules.

Only when we have considerable confidence in a theory of business cycle fluctuations would the application of public finance theory to the question of stabilization be warranted. Such an extension is straightforward in theory, though in all likelihood carrying it out will be difficult and will require ingenuity. (Kydland and Prescott (1988), p. 358)

In relation to Kydland and Prescott’s (1982) restriction $BF = 0$ of no stabilization policy during post-war business cycles, Blinder states in an interview in July 1982:

One important aspect to the debate is how stable an economy would be without an active government... Look at a time series chart (in my stagflation book) of the year-to-year fluctuations of GNP. They are remarkably smaller in the ’50s and ’60s, the “Keynesian” years, than they were before... I claim I know why: that there was active fiscal and monetary policy to iron business cycles beginning after World War II, but not before. So I think discretionary policies have been used and they have often worked. (Klamer (1984), p. 165)

Lucas (1987) confirms Blinder’s observations for the case of consumption time series, which has in general lower volatility than GNP:

In the period prior to the Second World War, and extending as far back in time that we have usable data, the standard deviation (logarithmic deviations from trend) of consumption was about three times its post-war level. (Lucas (1987), p. 28)

Although Simons’ (1936) and Friedman’s (1948) 100% reserve requirement for banks was not enacted, the ’50s and ’60s were decades without banking crisis following the Glass-Steagall act of 1933, separation of investment and retail banking and the Bretton Woods conference, 1944, with capital controls and international financial stability. Financial regulation and high productivity growth are likely to have been complementary to active stabilization policy in order to damp business cycles fluctuations during these two decades.

4.4 Lucas (1987) faces the Lucas critique

Lucas (1987, chapter 3 and 2003) evaluates the cost of aggregate US consumption fluctuations by its certainty equivalent loss of consumption. He uses the second order Taylor development of a discounted separable utility function with constant relative risk aversion of the representative household. This certainty equivalent is one half of the coefficient of absolute fluctuations aversion (or relative risk aversion) γ times the variance of the log of U.S. aggregate consumption around its trend denoted σ_x^2 . Lucas (1987) considers the following range of estimates $\hat{\gamma}$ for the coefficient of constraint relative risk aversion:

$$\frac{1}{2}\gamma\sigma_x^2 \text{ with } 1 \leq \hat{\gamma} < 20 \text{ and } \hat{\sigma}_x = 0.013.$$

Lucas (1987) is the transcript of Yrjö Jahnsson lecture in May 1985 and Lucas (2003) is the transcript of January 2003 presidential address at the American Economic Association, eighteen years after for the same evaluation. Lucas (1987) evaluates the certainty equivalent cost of post Second World War fluctuations of consumption to \$8.50 year 1983 per person (\$22 year 2019). This is an extremely low cost. But it is also a conservative estimate of welfare because Lucas' (1987) utility does not include the leisure benefits during recessions due to reduction of working hours which partially offsets the loss of consumption in Kydland and Prescott's (1982) utility. Lucas (1987, p.28) mentions "I would guess that taking leisure fluctuations into account more carefully would reduce the estimate in the text still further."

Lucas' (1987) low estimate of the variance of detrended consumption σ_x^2 since the Second World War faces the Lucas critique for the following reason: It is a reduced-form estimate σ_x^2 *which depends on stabilization policy parameters* F . For example, if the log of U.S. aggregate consumption around its trend is a stationary autoregressive process of order one, its variance decreases if there is negative feedback due to stabilization policy.

$$x_t = (A + BF)x_{t-1} + \varepsilon_t \text{ and } 0 < A + BF < \min(A, 1)$$

$$\sigma_x^2 = \frac{\sigma_\varepsilon^2}{1 - (A + BF)^2} < \frac{\sigma_\varepsilon^2}{1 - A^2} \text{ if } 0 < A < 1 \text{ and } BF < 0.$$

Lucas (1987) notes the difference of the volatility of consumption before and after Second World War:

Fluctuations in the pre-Second World War, especially combined as they were with an absence of adequate programs for social insurance, were associated with large cost of welfare... In the period prior to the Second World War, and extending as far back in time that we have usable data, the standard deviation (logarithmic deviations from trend) of consumption was about three times its post-war level. Since this number is squared in the formula (8), the implied cost estimates are multiplied by nine, becoming like one-half of 1 per cent of total consumption. As deadweight losses go, *this is* a large number. (Lucas (1987), p.28)

But he does not attribute the change of the volatility of consumption σ_x^2 to the changes of the policy-rule parameter F related to the introduction of Keynesian stabilization policy. His conclusion therefore may face the Lucas (1976) critique:

I find the exercise instructive, for it indicates that economic instability at the level we have experienced since the Second World War is a minor problem, even relative to historically experienced inflation and certainly relative to the costs of modestly reduced rates of economic growth (Lucas (1987), p. 31).

Blinder's argument (interview in July 1982 in Klamer (1984)) is to compare the US economy after the Second World War with stabilization policy ($BF < 0$) to the US economy before this period without stabilization policy (with e.g. $BF = 0$). One would

have saved up to 8 times the cost of consumption fluctuations using Lucas (1987) utility and estimates: \$68 year 1983 (\$175 year 2019) per person, still a small benefit. Then, Blinder (1987) changes the scale of Lucas evaluation of the cost of post-war-II US business cycles as follows:

Now change the utility to the Stone-Geary form: $U = \log(C - \$1500)$. Here, a 4 percent drop in consumption reduces utility by 8.3 percent... Finally, let the cycle instead reduce the consumption of 10 percent of the population by 40 percent while the other 90 percent loses nothing. (Note I am allowing very generous unemployment insurance here.) With the Stone-Geary utility function, mean utility declines 16.1 percent. (Klamer (1984), p. 165)

Since then, the great financial crisis in 2007 and its jobless recovery changed the figures of the standard error of US detrended aggregate consumption since the Second World War. Twenty-two years after Lucas Yrjö Jahnsson lecture in 1985, Barro (2009) uses Epstein-Zin-Weill utility for a representative agent and estimates that economic disasters with at least 15% loss of GDP over several consecutive years has a frequency of 2% for 35 countries during the 20th century:

Society would willingly reduce GDP by around 20 percent each year to eliminate rare disasters. The welfare cost from usual economic fluctuations is much smaller, though still important, corresponding to lowering GDP by about 1.5 percent each year. (Barro (2009))

4.5 Kydland and Prescott (1977, section 5): Systematic Bias of Estimated Parameters

Following the Lucas (1976) critique, because the identification of estimates of A and B cannot in principle be substantiated empirically, then it is better to use “rules” where $i_t = 0$ and $F = 0$ instead of “discretion” with a feedback rule parameter $F \neq 0$. Accordingly, Kydland and Prescott (1977) set a new definition of discretion where the policy maker always measures the parameters of the transmission mechanism with cumulated systematic errors which finally leads to the instability of the policy target.

Definition 5 *A discretionary policy is a policy for which the policy rule is optimal under the incorrect assumption that the observed persistence of the policy target is invariant to the policy-rule parameter used in the previous period.*

The policymaker uses control theory to determine which policy rule is optimal **under the incorrect assumption** that the equilibrium investment function is invariant to the policy rule used... Econometricians revise their estimate of the investment function, arguing that there has been structural change, and the policymaker uses optimal control to determine a new policy rule. (Kydland and Prescott (1977), section 5).

To describe the private sector, a model with convex adjustment costs of investment is used. Because of the optimal behavior of the private sector, “rules” pegging the policy instrument to its long-run value (where $i_t = 0$ and $F = 0$) has stable dynamics ($0 < A = A' + B'F' < 1$). For optimal taxation, the policy maker’s instrument is investment

tax credit. Because $A \neq 0$ and $Q \neq 0$, “rules” are sub-optimal with respect to optimal control discretion by the policy maker obtaining unbiased estimates of the policy rule parameter A (section 3.2). Assuming an increasing bias of the policy-rule parameter A , Kydland and Prescott’s (1977) simulations lead to the increased volatility of the policy target in the discretion case when compared to the rules case:

For one example (...), after the third iteration, however, performance deteriorated, and the consistent [optimal control] policy to which the process converged was decidedly inferior to the passive policy for which the investment tax credit was not varied... For another example, the iterative process did not converge. Changes in the policy rule induced ever larger changes in the investment function. The variables fluctuated about their targeted values but fluctuated with increased amplitude with each iteration.... Such behavior either results in consistent but suboptimal planning or in economic instability. (Kydland and Prescott (1977), p. 485)

We translate their argument using the first order single-input single-output model. In each period, the policy maker uses $A + BF_{t-1}$ instead of A for determining his policy rule parameter F_t at the current period. The policy maker sets an optimal policy F_t which belongs to the stability set for the erroneous $A' = A + BF_{t-1}$. This implies that the sequence of policy-rule parameters F_t is strictly increasing or decreasing:

$$|(A + BF_{t-1}) + BF_t| < A < 1 \Rightarrow |B| |F_{t-1} - F_t| < 0.$$

After some iterations up to a date t_1 , the “discretionary” policy rule F_t may belong to a set with a larger persistence $|A + BF_t|$ and volatility of the policy target than the one obtained with “rules” (where $i_t = 0$ and $F = 0$) persistence equal to A :

$$|A + BF_t| > A, \text{ for } t > t_1.$$

Because the policy maker estimates with an increasing bias the open-loop persistence A of the policy target, discretion implies higher volatility of the policy target than rules. This result does not assume rational expectations with forward-looking variables, nor time-inconsistency in a Stackelberg dynamic game, which is defined in the next section.

5 The Impossibility of Policy Maker’s Credibility and therefore of Optimal Control.

5.1 Simaan and Cruz’ (1973b) Time-Inconsistency of Stackelberg Dynamic Games

Kydland and Prescott’s (1977) expressed their dislike of discretionary policy in the abstract of their famous paper on rules *versus* discretion:

... economic planning is not a game against nature but, rather, a game against rational economic agents. We conclude that there is *no* way control theory can be made applicable to economic planning when expectations are rational. (Kydland and Prescott (1977), p. 473)

This radical statement put forward the specificity and the autonomy of macroeconomists with respect to the field of control. Following this statement of such a divide as a major scientific contribution, surveys and historiography of the time-inconsistency literature done by economists do not mention Simaan and Cruz's (1973b) original contribution, for example Hartley (2006). Even Tabellini (2005) mentions Simaan and Cruz only in a footnote.

The point-of-view from the field of control is markedly different. The field of control was subsidized by the army in the 1940s and during the cold war. War is related to non-cooperative dynamic games, so army funding was not limited to games against nature. Dynamic Nash games (Isaacs (1965)) used tools related to the field of control. Dynamic games flourished in engineering and applied mathematics departments using control tools. It was marginal in economics departments. Cruz, a professor of electrical and computer engineering at university of Illinois, published with his student, Simaan, an important result on Stackelberg dynamic games (Simaan and Cruz (1973b)). The paper was partly funded by the US Air Force. Cruz (2019) describes the following sequence, from the point of view of the field of control:

R.P. Isaacs (Isaacs 1965) is the originator of differential games. Y.C. Ho (Ho 1970) clarified the connection between control and differential game theory. C.I. Chen and J.B. Cruz (Chen and Cruz 1972) were the first to consider dynamic Stackelberg games [with an economic example]. M.A. Simaan and J.B. Cruz (Simaan and Cruz 1973a, Simaan and Cruz 1973b) reframed dynamic Stackelberg game theory, providing mathematical proofs for various results and showed that the Stackelberg strategies do not necessarily satisfy Bellman's principle of optimality. This violation of the principle of optimality was renamed as time inconsistency in economic policy (Kydlan and Prescott 1977), and later Kydlan and Prescott won the Nobel prize in economics in 2004 in part on the basis of their paper. M.A. Simaan and J.B. Cruz (Simaan and Cruz 1973c) extended the dynamic Stackelberg concept to many players. J.B. Cruz (Cruz 1975) introduced the dynamic Stackelberg concept to the economics community, and he extended the Stackelberg strategy to more than two levels (Cruz 1978). (Cruz (2019), p.83)

Simaan and Cruz (1973b) is one of the key building blocks for Kydlan and Prescott's (1977) later success. Theoretical work done by two electrical engineers was the initial trigger of Kydlan and Prescott's (1977) and Prescott's (1977) papers denying the use of control for stabilization policy.

Stackelberg dynamic games can consider a quadratic loss function for the leader and the follower. In this case, their solutions are extensions of Kalman's linear quadratic regulator solutions, solving an algebraic Riccati equations.

The leader takes into account the follower's marginal conditions. However, like the tower of control interacting with the pilot of an airplane, the leader can order the change of the decision variables of the follower (the pilot in the cockpit). These decision variables are jump variables. Hence, the policy maker can anchor the initial value of these jump variables optimally, minimizing the loss function with respect to the follower's decision variable at the initial date.

In our example, let us assume that prices and inflation are private sector's "jump" decision variable, with unknown initial condition, instead of assuming a given initial value π_0 . This initial transversality condition is optimally decided by the policy maker, where

the anchor parameter $P^*(Q/R)$ of the jump variable on predetermined variables z_0 is derived from optimization. It depends on the policy maker's preferences:

$$\gamma_0^{*0} = \left(\frac{\partial L}{\partial \pi} \right)_0 = 0 \Rightarrow \pi_0^{*0} = P^*(Q/R)z_0 \text{ and } \gamma_t^{*0} > 0 = \gamma_0^{*0} \text{ for } t = 1, .. \quad (10)$$

The time superscript besides a star *0 indicates it is a date-0 optimal plan. This plan is valid for a list of future dates starting at date $t = 0$ which corresponds to the time-subscript. Simaan and Cruz's (1973b) result is that if the policy maker re-optimizes at any future period indexed by t , it contradicts the optimal plan decided at date zero: $\gamma_t^{*t} = 0 < \gamma_t^{*0}$. They conclude:

If the starting time is fixed, the leader's closed-loop Stackelberg control is the best control law (among all other admissible closed-loop controls) that he can announce prior to the start of the game, but it does not have this same desirable property from any other starting time (Simaan and Cruz's (1973b), p. 625).

In a personal communication, Simaan recalls the genesis of this result:

In 1972 while I was a PhD student exploring the application of the Stackelberg Strategy to dynamic games, I had extensive discussions with my advisor Professor Cruz about my observation and conclusion, at the time, that the closed-loop controls in Stackelberg dynamic games did not satisfy Bellman's Principle of Optimality and that the controls had to be recalculated again at every instant of time along the optimal trajectory. Initially, he had some doubt about the validity of this result, but after long discussions, we were convinced. The results were published in a follow up paper on Stackelberg dynamic games in 1973. Unfortunately, at that time, we did not make the connection with macroeconomics, but fortunately, several years later, Kydland and Prescott made the connection and published their famous 1977 paper.

Coincidentally, in 1977 I was a faculty member in electrical engineering at the University of Pittsburgh, which is right next door to Carnegie Mellon University. I had no idea that Kydland and Prescott were at CMU and I am not sure if they were aware that I was at the University of Pittsburgh about five minutes' walk from their offices. I discovered their work on time inconsistency several years later and was very happy that they received the Nobel Prize for it in 2004. (email received 14th July 2020.)

The transplant of Simaan and Cruz' (1973b) time-inconsistency of Stackelberg dynamic games to economics was quickly done by Kydland (1975, 1977), Prescott's Ph.D. student at Carnegie Mellon University, where he completed his PhD in 1973 on "Decentralized Economic Planning". Cass first encouraged Kydland to expand an idea about non-cooperative and dynamic games into a thesis (Kydland (2014)). In a paper based on a chapter of his PhD dissertation, Kydland (1975) acknowledges his marginal contribution with respect to Simaan and Cruz (1973a, b):

From the viewpoint of game theory this section does not really offer any new results... The dominant player problem, on the other hand, has only recently received a little attention in the game literature, and the two interesting papers by Simaan and Cruz [1973a, 1973b] should be mentioned (Kydland (1975), p. 323).

After Kydland's (1975, 1977) citations, Kydland and Prescott (1980 p.80 and 86) and Calvo (1978, equation A.10, p.1425 and A.21 p.1427) use the initial transversality condition at the origin of time inconsistency without citing Simaan and Cruz (1983b, equation 11, p.621). Calvo (1978) correctly states that:

We encounter time inconsistency even when the government attempts to maximize the welfare of the representative individual, that is to say, in a context where there is not a shade of malevolence or dishonesty at play. (Calvo (1978), p. 1422)

In Simaan and Cruz (1973b), Calvo (1978) and Kydland and Prescott (1980), there is time inconsistency and the policy maker knows the transmission mechanism parameters which do not change over time. Therefore, he has unbiased estimates of these parameters, as opposed to the policy maker in Kydland and Prescott (1977), section 5, which is not related to dynamic time-inconsistency.

Although the optimal discretionary policy at date 0 is sub-optimal with respect to all other optimal discretionary policies with an optimization starting at any future date, it is always less sub-optimal than rules which peg policy instrument to their long run value, for dynamic models if $A > 0$.

Kydland and Prescott (1980) rename the Stackelberg dynamic game as Ramsey optimal policy, in reference with Ramsey's (1927) static model of optimal taxation.

In his paper on optimal taxation, Ramsey (1927, p. 59) briefly considers the dynamic problem, but because it is 'considerably more difficult' essentially assumes the dynamics away. (Kydland and Prescott (1980), p.84)

Kendrick (2005) mentions Kydland's talk in 1975:

Since the idea of forward variables like those above was of obvious importance in dynamics and control, we invited Lucas to give a talk at the Society of Economic Dynamics and Control (SEDC) conference in Cambridge, MA in 1975. He declined to come, but Finn Kydland did accept our invitation. Finn's talk at that meeting was well attended and listened to carefully. The reaction of the control engineer standing next to me was typical – "no problem – you just have to treat it like a game theory problem". (Kendrick (2005), p. 15)

Ljungqvist and Sargent (2012) judge that Prescott's (1977) "pessimism" on optimal control has been overturned:

Prescott (1977) asserted that recursive optimal control theory does not apply to problems with this structure. This chapter and chapters 20 and 23 show how Prescott's pessimism about the inapplicability of optimal control theory has been overturned by more recent work. The important contribution by Kydland and Prescott (1980) helped to dissipate Prescott's initial pessimism. (Ljungqvist and Sargent (2012), chapter 19)

Ljungqvist and Sargent (2012) expose the optimal program in the linear quadratic case of a Stackelberg dynamic game. This is the same optimal program as the one presented in Simaan and Cruz (1973b), Kydland (1975, 1977), Calvo (1978) and Kydland and Prescott (1980). "More recent work" only assumes a policy maker's commitment constraint as initially proposed by Calvo (1978), published the year after Prescott (1977):

It is clear that no inconsistency arises if the government optimizes at t_0 , say, and abides by the dictates of that policy for all $t \geq t_0$; so one possible proposal could be constraining the government to do just that for a given t_0 . (Calvo (1978), p. 1422)

The contribution of “more recent work” was to *forget* Prescott’s (1977) and Kydland and Prescott’s (1977) *over-statement* of the inapplicability of optimal control for stabilization policy.

Finally, how large are the relative gains of re-optimization on future dates $t > t_0$ with respect to initial optimal policy on date t_0 ? Firstly, if the initial shock is small and the policy target remains close to its long run equilibrium, the optimal path hardly deviates after re-optimization. Secondly, if the initial shock is large and if one re-optimizes soon after, the optimal path hardly deviates. Thirdly, if the relative cost of changing the policy instrument (R/Q) is very large or very small, the optimal path hardly deviates after re-optimization (Chatelain and Ralf (2016)).

If there is a fixed cost of future re-optimization, re-optimization is more likely to happen (1) following large shocks π_0 although one may maintain the linear-quadratic approximation (2) in the middle of the duration of the way back to equilibrium and (3) if $R/Q \approx 1$: the cost of changing the policy instrument is of the same order of magnitude than the cost of the volatility of the policy target.

5.2 Barro and Gordon (1983) Steady State Bias in the Policy Maker’s Loss Function

As opposed to Calvo’s (1978) time-inconsistency in dynamic Stackelberg games, Kydland and Prescott (1977, section 3) and Barro and Gordon (1983a, 1983b) assume that the policy maker’s utility is different from the private sector’s utility (there is a “*shade of malevolence or dishonesty at play*” by the policy maker) and that the policy transmission mechanism is a static model ($A = 0$).

Definition 6 *Optimal discretion is such that, firstly, one assumes $A = 0$ in the policy transmission mechanism, secondly the policy maker has a long run target value of the policy target ($\pi_D > 0$) which is **systematically different** from the long-run set point value of the policy target in the transmission mechanism, contrary to “rules”.*

The policy target is unemployment U_t (Barro and Gordon (1983a)) or output x_t (Barro and Gordon (1983b)) and their policy instrument is inflation. The transmission parameter is a static Phillips curve: $U_t = B\pi_t$ with $B_U < 0$ or $x_t = B_x\pi_t$ with $B_x > 0$. We translate their model in our benchmark model with our notations: $\pi_t = B i_{t-1}$ with $B < 0$ and $A = 0$.

Proposition 3 *The joint assumptions $A = 0$ (static model), a non-zero weight on the volatility of the policy target $Q > 0$ and a distorted long run objective for the policy target $\pi_D > 0$ (inflationary bias) implies that optimal discretion is necessarily sub-optimal with respect to rules which are optimal assuming $A = 0$ and $\pi_D = 0$*

Proof. Optimal discretionary policy is obtained by minimizing in each period the expected loss function. The first order condition is:

$$\frac{\partial}{\partial \pi_t} \left(\beta Q (\pi_t - \pi_D)^2 + R \left(\frac{\pi_t}{B} \right)^2 \right) = 2\beta Q (\pi_t - \pi_D) + 2 \frac{R}{B^2} \pi_t = 0.$$

As expected, this leads to a suboptimal solution denoted (π_D^*, i_D^*) with a policy maker's distorted steady state $\pi_D > 0$ when compared to the solution of "rules" $(\pi_D^* = 0, i_D^* = 0)$ which is optimal for a static model ($A = 0$) with a policy maker's non-distorted steady state $\pi_D = 0$:

$$\pi_D^* = \frac{\beta Q}{\beta Q + \frac{R}{B^2}} \pi_D > 0 = \pi^* \text{ and } i_D^* = \frac{1}{B} \frac{\beta Q}{\beta Q + \frac{R}{B^2}} \pi_D < 0 = i^*.$$

■

The discretionary equilibrium of the policy target π_D^* increases with the steady state inflationary bias π_D in the central bank's preference and with the weight of the policy target Q and decreases with the weight on the policy instrument R . The same level of π_D^* is obtained for a lower inflationary bias π_D compensated by a higher Q/R ratio. Assuming a zero weight ($Q = 0, R > 0, \pi_D \geq 0$) on the variance of the policy target with respect to the policy maker's biased long run target, is equivalent to assume a non-zero weight and no inflationary bias ($Q \geq 0, R > 0, \pi_D = 0$).

Instead of directly assuming that a conservative central banker has an exogenously set lower inflationary bias $\pi_{D,C}$ in the loss function $0 < \pi_{D,C} < \pi_D$ which would be too simple for a publication in a top journal, Rogoff's (1985) contribution is to assume that a conservative central banker has an exogenously lower relative weight on the policy target Q/R for an unchanged exogenous inflationary bias π_D in the loss function.

Barro and Gordon (1983a) mention that if ever the policy maker with a distorted steady state (exogenous inflationary bias) would choose "rules" ($\pi^* = 0$), he would have an incentive to deviate creating surprise inflation. They mention that this solution does not hold in a rational expectations equilibrium (π_D^*, i_D^*) .

Barro and Gordon (1983a) justify their exogenous steady state inflationary bias assumption by this single sentence:

In the presence of unemployment compensation, income taxation, and the like, the natural unemployment rate will tend to exceed the efficient level - that is, privately chosen quantities of marketable output and employment will tend to be too low. (Barro and Gordon (1983a), p. 593)

Taylor, however, is not convinced by this argument:

In other well-recognized time inconsistency situations, society seems to have found ways to institute the optional (cooperative) policy... The superiority of the zero inflation policy is obvious... It is therefore difficult to see why the zero inflation policy would not be adopted. (Taylor (1983), p.125)

Blinder (1998) comments on Barro and Gordon's (1983a) positive interpretation of their model explaining high inflation during 1973-1979:

Barro and Gordon ignored the obvious practical explanations for the observed upsurge in inflation - the Vietnam War, the end of the Bretton-Woods system, two OPEC shocks, and so on - and sought instead a theoretical explanation for what they believed to be a systematic inflationary bias in the behaviour of central banks. (Blinder (1998), p.40).

A version of Barro and Gordon (1983a) was soon included in graduate textbooks of macroeconomics in order to explain Kydland and Prescott's (1977) time inconsistency. For now four decades, which included a global disinflationary trend and the emergence of inflation targeting, graduate students around the world have exams related to the unlikely statement that central bankers have a pro-inflation bias in their loss function:

Though it didn't hurt them in academia, Kydland and Prescott's timing was exceptionally poor from a real world perspective. As mentioned earlier, the late 1970s and early 1980s witnessed sharp and painful disinflations in the US, the UK, and elsewhere. Neither Paul Volcker nor Margaret Thatcher (the Bank of England was not independent then) succumbed to the temptation posed by time inconsistency; they probably never even heard of it. It turned out that Kydland, Prescott, and other academics were prescribing how to fight the last war just as the next war was getting underway. (Blinder (2020), p.31).

6 Taylor (1993) Translates the Rules *versus* Discretion Controversy

The perception of rules *versus* discretion changed fundamentally with the introduction of the Taylor rule in the 1990s. Like Barnett and Serletis (2017), Kendrick (2005), and Turnovsky (2011), Taylor confirms the gains of the proponents of the policy ineffectiveness claim in the 1980s:

His (Ben McCallum (1999)) lecture also describes how research work on monetary policy rules waned considerably in the 1980s, except for the work of small groups "toiling in the vineyards". I call it "dark ages" in another paper; it seemed like everyone interested in the new rational expectations methods in the 1980s was working on real business cycle models without a role for monetary policy. (Leeson and Taylor (2012))

Taylor refers to mainstream macroeconomists belonging to top US academic circles in the 1980s, for example, the ones invited to NBER conferences, as well as their PhD students. By contrast, in the Fed and in other central banks, policy advisers still used models involving policy maker's negative feedback response. But they had less and less access to publishing their results in top academic journals. In continental Europe, in particular in France and Belgium, macroeconomists around Drèze, Benassy and Malinvaud were involved in the heyday of disequilibrium macroeconomics in the 1980s. They considered that real business cycles models were not relevant.

During the "dark ages" of the 1980s, Taylor followed the strategies of dissenters in scientific controversies with the outcome described by Latour (1987, p.137): "*From a few helpless occupying a few weak points, they end up controlling strongholds.*" His strategy targeted policy makers at the Fed, in order to convince them that "rules" can also be a specific "feedback rules".

One may be surprised that Taylor (1993) dedicated the first section of his paper to semantic issues. Semantic issues matter in a scientific controversy in other sciences than mathematics (which always proceed with exact mathematical definitions).

Friedman (1948) and Kydland and Prescott (1977) originally *translated* “stabilization policy ineffectiveness” and “the fixed setting of the policy instrument” *into* “rules”. They translated “negative feedback policy” *into* “discretion”. The word “rules” is related to more virtues than “discretion”, which has a wide range of negative polysemic connotations such as the abuse of power or arbitrary, opportunistic, careless, thoughtless or non-predictable random behavior.

For the proponents of stabilization policy, these negative connotations are unrelated to the expected virtues of stabilization policy aiming to improve social welfare. For example, Buiter (1981) translated “negative feedback policy” into the oxymoron “flexible rules”:

This paper analyses an old controversy in macroeconomic theory and policy: ‘rules versus discretion’ or, more accurately, fixed rules (rules without feedback or open-loop rules) versus flexible rules (contingent rules, conditional rules, rules with feedback or closed-loop rules). (Buiter (1981), p. 647)

In their textbook, Dornbush and Fischer translated “negative feedback policy” into “activist monetary rule”:

The growth rate of money is high when unemployment is high and is low when unemployment is low. That way, monetary policy is expansionary at times of recession and contractionary in a boom. (Dornbush and Fischer (1984), p. 343)

The fixed setting of the policy instrument is translated into “*inactive policy*” (Blanchard and Fischer (1989), p.582) or *passive policy*, which may carry the negative connotations of policy makers who may be lazy and useless bureaucrats being paid for doing nothing.

The *translation* of the definition of the word “rule” by *its opposite* in the controversy is a dissenters’ tactical move:

Tactic four: rendering the detour invisible... People can still *see* the difference between what they wanted and what they got, they can still feel they have been cheated. A fourth move is then necessary that turns the detour into a progressive drift [e.g. “activist policy”, “systematic policies”], so that the enrolled group still thinks that it is going along a *straight* line, without ever abandoning its own interests... I should now be clear why I used the word *translation*. In addition to its linguistic meaning (relating versions in one language to versions in another one), it has also a geometric meaning (moving from one place to another). Translating interests is at once offering *new interpretations* of these interests and channeling people in other directions. (Latour (1987), p. 116-117)

In the Harry Johnson lecture at the Money Macro and Finance Research Group conference in Durham, September 1997, Taylor (1998) defines “*translational economics*” in a more narrow sense than Latour (1987):

The process of finding ways for the research in the biology laboratories to be applied in medicine to improve people’s health is called ‘translational biology’. Analogously the term translational economics might usefully designate the process of finding ways to make academic research in economics applicable to improving the performance of an economy. (Taylor (1998), p. 7)

Taylor used his position in the Council of Economic Advisers from June 1989 to August 1991 during the George H.W Bush administration:

As a member of the CEA I felt that I had an opportunity to move the policy ‘ball’ at least a little bit in the direction of the policy rule ‘goal line’, an opportunity that would not exist outside the policy arena. One plan of action was to use the public forum offered by the annual Economic Report of the President [1990] to make the case for monetary policy rules. (Taylor (1998), p. 8-9)

In a first step, Taylor translated Keynesian “negative feedback policy” into “systematic policy”:

When we wrote about policy “rules” in the 1990 *Economic Report to the President*, for example, we said “systematic” policies instead of rules so as not to confuse people who might think that a rule meant a fixed setting for the policy instrument. (Leeson and Taylor (2012))

McCallum (2015) mentions:

At the time I was a member of the Carnegie-Rochester Conference’s advisory board. One of our duties was to suggest fruitful topics for future conferences. In that capacity, at the board’s planning meeting for the November 1992 conference, some months earlier, I had suggested that a paper should be commissioned that would develop some method or criterion that would permit an outside researcher to determine whether an actual central bank’s actions over some significant span of time should be regarded as resulting from a policy rule, rather than being “discretionary”. This suggestion met with approval by Allan Meltzer and the other participants, and then we quickly agreed that the best person to write the paper would be John Taylor—who Allan subsequently contacted and signed up for the conference.

Well, as it turns out, the paper that John delivered did not actually do this. Instead it proposed and (very effectively) promoted a specific rule. So at the time of the conference, I, evidently, must have been somewhat put off by the change in focus. (This change might have been arranged with Allan; about that I do not know.). (McCallum (2015), p.2).

Secondly, in Taylor (1993) first section “Semantic issues” of a paper first presented in November 1992 at a meeting in Pittsburgh of the Carnegie-Rochester conference on public policy, and then published in the resulting conference volume in the spring of 1993, Taylor (1993) translated the “negative feedback policy” into its opposite in the Friedman (1948) controversy: “policy rules”.

There is considerable agreement among economists that a policy rule need not be interpreted narrowly as entailing fixed settings for the policy instruments. Although the classic rules versus discretion debate was usually carried on as if the only policy rule were the constant growth rate rule for the money supply, feedback rules in which the money supply responds to changes in unemployment or inflation are also policy rules... A policy rule is a contingency plan that lasts forever unless there is an explicit cancellation clause. (Taylor (1993), p. 198).

Taylor (1993) proposed the following new definitions of rules *versus* discretion. He fixed rounded values of the parameters of a feedback rule which was roughly predicting the Federal funds rate in the last six years (1987-1992):

Definition 7 *Taylor (1998, 2017): “Rules” follow systematically a Taylor (1993) negative-feedback “rule” where the real federal funds rates has a 2% set point and responds in proportion to inflation deviation from 2% and to output gap with given parameters equal to 1/2.*

$$i_t = \pi_t + \frac{1}{2}(\pi_t - 2) + \frac{1}{2}x_t + 2 = 1.5\pi_t + 0.5x_t + 1.$$

Definition 8 *Taylor (1998, 2017): “Discretion” is any other monetary policy which deviates from a Taylor (1993) negative-feedback rule, measured by the discrepancies (or the residuals) of funds rate with respect to Taylor (1993) rule predictions.*

The discrepancies between the [Taylor rule] equation and reality could be a measure of discretion, either for good or for bad. (Taylor (1998), p. 12)

Taylor’s (1993) policy paper was using a rhetorical presentation in order to convince the Fed’s policy makers, not academics. A feedback rule with fixed parameters can be easily understood. His rule had soon early users among a sufficiently large set of influential practitioners (in this case, at the Fed) and of private sector analysts of policy making (Koenig et al. (2012)).

Taylor’s (1993) plots 24 quarterly observations of Fed funds rate from 1987 to 1992 and its forecast according to his rule. No statistical tests are presented. The rounded parameters of his rule seem arbitrary (the formal constraint is that they involve only the numbers 1 and 2). There is no information on the transmission mechanism, which is a crucial information for designing negative feedback policy rule. There is no mention of reverse causality and nor of endogeneity issues related to this forecast. The paper was published in a conference volume by invitation along with comments by McCallum (1993), so that peer-review was limited.

We mention Lucas, Kydland and Prescott overstated their results in the rules versus discretion controversy. Taylor (1993) may have overstated the response of Fed’s fund rate to inflation during 1987-1992 in his policy rule. Let’s check the robustness of Taylor’s (1993) rule estimating this policy rule with currently available and revised quarterly data from FRED database for inflation and congressional budget office (CBO) output gap, still for 24 observations from 1987 to 1992. The inflation rule parameter estimate is below one (0.75) instead of 1.5 in the Taylor rule, so that the Taylor principle is not satisfied. The output gap rule parameter estimate is 0.75 instead of 0.5. The share of unexplained variance related to discretion is 30%.

Inflation has a very low persistence with a very low autocorrelation coefficient: $\rho_\pi = 0.4$. By contrast, Fed funds rate and CBO output gap are highly persistent, close to unit root with $\rho_i = 0.96$ and $\rho_x = 0.935$. A regression of Fed funds rate on its lagged value and current output gap leads to an $R^2 = 962\%$: the share of unexplained variance of the policy rule related to discretion is less than 4%. Including inflation in the regression only increases the R^2 by 0.0013%, which may correspond to overfitting. Inflation persistence is low because of private sector’s negative feedback mechanism and because of a divine coincidence where leaning against output gap fluctuations stabilizes the component of inflation correlated with output gap. A policy maker’s instrument should respond more

to more persistent target variables (Ashley *et al.* (2020)). Rounding the figures of the estimates in order to introduce the number 2 for a key parameter, one may have proposed an alternative definition of "rules":

Definition 9 *Rules follows systematically a persistence-dependent negative feedback rule where Fed funds rate responds to its lagged value and to current output gap, with Fed's funds rate long run sensitivity to the output gap equal to 2.*

$$i_t = 0.8i_{t-1} + 0.4x_t \text{ with } 2 = \frac{0.4}{1 - 0.8}$$

Definition 10 *"Discretion" is any other monetary policy which deviates from this persistence negative-feedback rule, measured by the discrepancies of funds rate with respect to the forecast of persistence-dependent rule. For the period 1987-1992, discretion corresponds to the unexplained share of variance is around 5% in the estimation of the policy rule.*

The unexpected side-effect of the interest paid by policy makers to Taylor's interest rate rule was fed back three years later into academia with citations in academic papers starting in 1996. These papers soon led to the emergence of the three equation new-Keynesian theoretical model where the Taylor rule is the third equation (Kerr and King (1996), Clarida, Gali and Gertler (1999)). Google Scholar citations of Taylor (1993) increased by at least 40 citations each year from 1997 to 2008. The success of the Taylor (1993) rule tilted the balance to formerly "discretion" in the original "rules *versus* discretion" controversy, with the gain of the original negative feedback "discretion" being renamed "rule".

Essentially, when the usefulness of discretion was rediscovered, the notion of rule got the credit... This peculiar *re-definition* of 'rule' not only renders the terminology bogus, but also the substance of rules vs discretion Mark I. (Bibow (2004), p. 559).

The Taylor rule marked the return of classic control into top level macroeconomic research. However, advisors using models at the Fed never stopped using feedback rules. Even though optimal rules were considered too complex to be disclosed to the press, forty years later after the initial use of optimal control with the Fed FMP model, Fed's optimal control approach in the FRB/US model made the headlines of financial newspapers, following Yellen's (2012) speech. Yellen was at the time Vice Chair of the board of governors of the Federal Reserve System:

First, the FRB/US model's projections of real activity, inflation, and interest rates are adjusted to replicate the baseline forecast values. Second, a search procedure is used to solve for the path of the federal funds rate that minimizes the value of an assumed loss function, allowing for feedback of changes in the federal funds rate from baseline to real activity and inflation. For the purposes of the exercise, the loss function is equal to the cumulative discounted sum from 2012:Q2 through 2025:Q4 of three factors—the squared deviation of the unemployment rate from 5-1/2 percent, the squared deviation of overall PCE inflation from 2 percent, and the squared quarterly change in the federal funds rate. The third term is added to damp quarter-to-quarter movements in interest rates. (Yellen (2012), p. 16).

7 Conclusion

The present paper studied the use of control theory in macroeconomic models with a special emphasis on the debate of monetary policy and – in particular – the question of “rules *versus* discretion”. Whereas the 1960s were dominated by a belief in the effectiveness of discretionary stabilization policy, the rhetorics changed in the subsequent years. The Lucas (1976) critique and Kydland and Prescott’s (1977) time inconsistency convinced a sufficient number of macroeconomists that *the negative-feedback mechanism of classic and optimal control cannot be used for stabilization policy*, whereas it should be used to model the private sector. Using rhetorical over-statements in their final conclusions, helped to convince their readers of the “stabilization policy ineffectiveness claim”. These influential papers paved the way to Kydland and Prescott’s (1982) real business cycle theory, assuming that stabilization policy had no effect on US postwar business cycles.

Furthermore, these influential papers succeeded in delaying the expected fast transplant of the robust control, linear quadratic Gaussian estimations and Ramsey optimal policy in the 1980s to nearly twenty years. Our analysis stops in 1993 with the emergence of the Taylor (1993) rule, which contributed to the revision and the decline of the stabilization policy ineffectiveness claim. At the end of the nineties, negative-feedback rules in backward-looking models were used at the Fed, and Ramsey optimal policy and robust optimal control gained interest in monetary policy analysis.

Also in the mid-1980s, however, another controversy started, namely the debate “commitment *versus* discretion”. As a starting point the model of Oudiz and Sachs (1985) can be seen. They modeled a policy maker’s behavior who re-optimizes in each period, as if he is replaced by another policy maker at the end of the period. As this policy maker lives only one period, he sets a probability equal to zero to the expectations of the policy target. Omitting expectations, he does a static optimization of an otherwise dynamic process for the policy target. Phillips (1954) already mentioned that static optimization, when wrongly applied to a dynamic model, can lead to a positive feedback rule which implies exploding paths of the policy target. Therefore, a set of additional mathematical restrictions are added on the private sector’s behavior in order to eliminate these unstable paths (Chatelain and Ralf (2021)). Oudiz and Sachs (1985) solution was initially labeled “time-consistent” policy. At the beginning of the 1990s, it became the new benchmark model describing “discretion” (Clarida, Gali, Gertler (1999)).

The shift from the original definitions of “rules” versus “discretion” to these models of “commitment” versus “discretion” brought semantic issues. Firstly, the mathematics of this new “discretion” model implies a state-contingent positive feedback “rule”. Conversely, Ramsey optimal policy under commitment has a state-contingent negative feedback “rule”, which corresponds to the original definition of “discretion” by Friedman (1960).

When discussing discretionary policies, a macroeconomist involved into policy making at the end of the seventies may misunderstand a macroeconomist having done his PhD at the end of the nineties. The older macroeconomist will describe Volcker’s policy as discretionary policy, with a strong negative feedback effect against inflation. The next generation macroeconomist will describe Volcker’s policy as commitment (able to sharply decrease the expectations of inflation) instead of discretion.

In a second approach, Currie and Levine (1985) defined a “simple rule” dynamic path including a proportional feedback rule when the policy target is a jump variable without an initial condition. Although the policy maker does not optimize, this equilibrium solu-

tion can be a reduced form of Oudiz and Sachs' (1985) "discretion" model (Chatelain and Ralf (2020b)). Their "simple rule" solution also implies positive feedback rule parameters and exploding paths of the policy target. As in Oudiz and Sachs' (1985) solution, a set of additional mathematical restrictions on the private sector's behavior are assumed in order to eliminate these unstable paths (Chatelain and Ralf (2020a and 2020b)).

"Time-consistent" and "simple rule" dynamic paths with positive-feedback rule parameters advocate the *opposite of control* which favors negative feedback rule parameters for stabilization policy as obtained with Ramsey optimal policy under commitment. Other than in the above discussed "rules" paradigm with no policy feedback in Friedman (1960), Kydland and Prescott (1977, 1982), Barro and Gordon (1983a)) due to confidence in the stabilizing force of the private sector, in the subsequent "time-consistent" or "simple rule" paradigm, positive feedback rules were favored with a set of mathematical restrictions on the private sector's behavior, eliminating unstable paths, as in Clarida, Gali, Gertler (1999), among many other papers. *In both cases, macroeconomists lost control.* The latter story, however, deserves to be treated in a separate paper.

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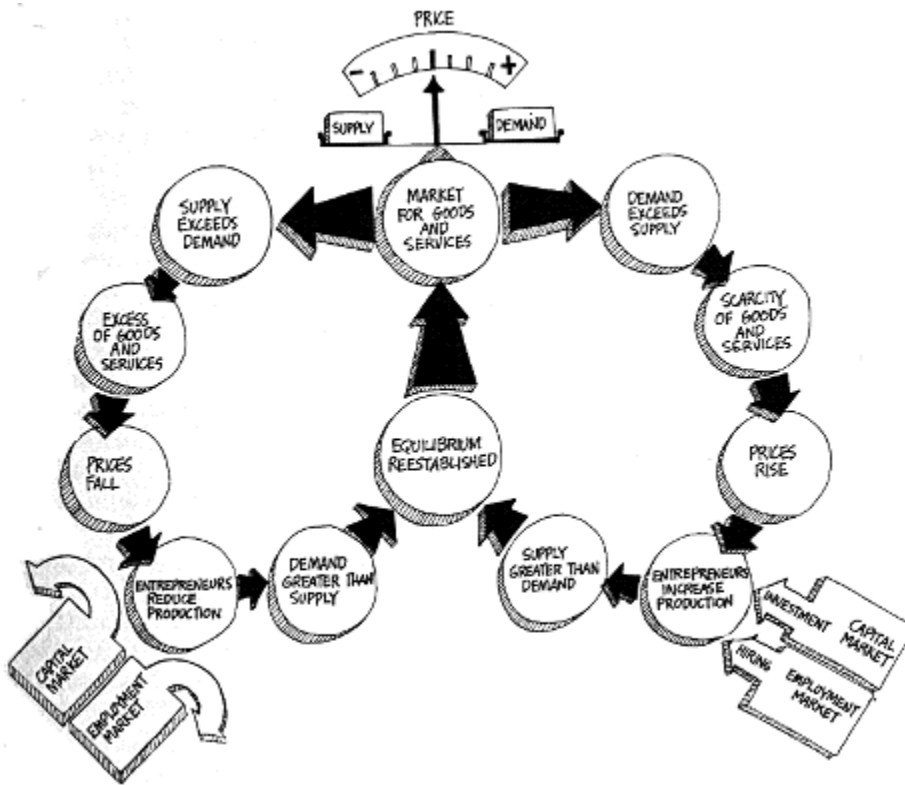
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FIGURES NOT FOR PUBLICATIONS:

Section 2.1 (Self stabilizing markets): Figure 1 : Adam Smith (1776) feedback of supply on demand (De Rosnay (1979), *The Macroscope*, p.31).



Section 2.1, Figure 2: Block diagram for Smith feedback of supply on demand (Mayr (1971, p.11).

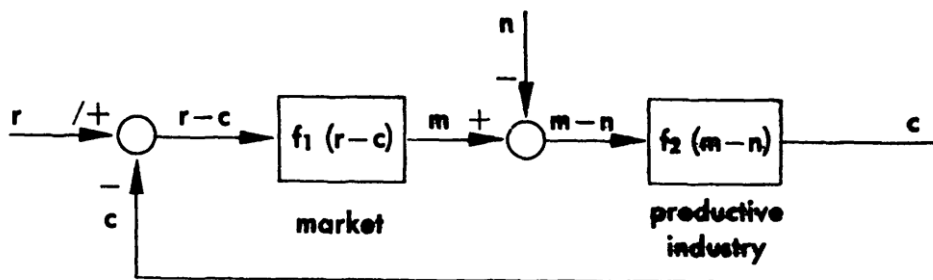
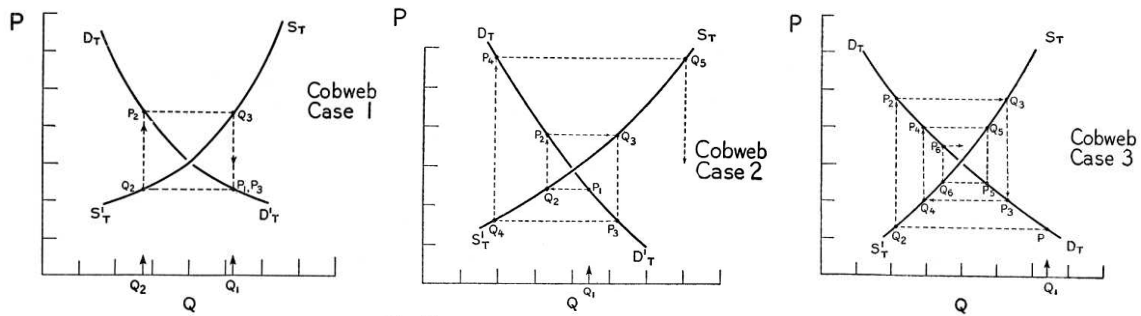
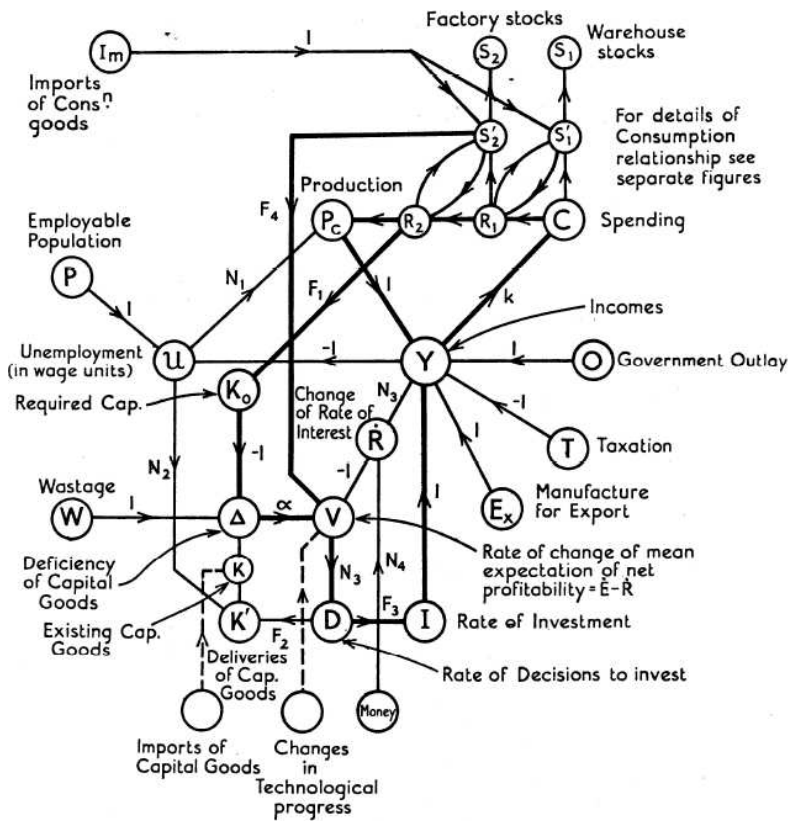


FIG. 4.—Adam Smith’s general theory of supply and demand (r = demand for any given commodity, c = supply, m = market price, and n = natural price [i.e., actual cost]).

Section 2.1. Figures 3a,b,c in Ezekiel (1938, p. 264) cobweb, Case 1 non diverging cycle and Case 2 diverging cycle cases are counter examples with positive feedback of market mechanism. Case 3 converging case with negative feedback mechanism of supply as in Adam Smith (1776)



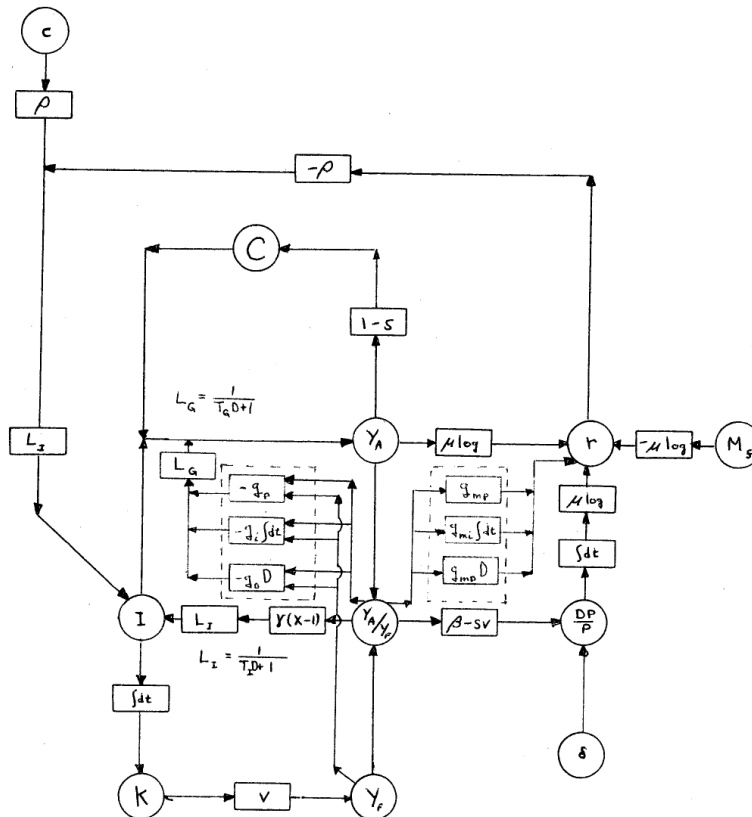
Section 3.1 (Classic control). Figure 4 : Signal-flow block diagram of a Keynesian model by Tustin (1953).



Section 3.1. Video “Making Money Flow, the MONIAC” Phillips Machine at the Central Bank of New Zealand (09/09/2017):

<https://www.youtube.com/watch?v=rAZavOcEnLg>

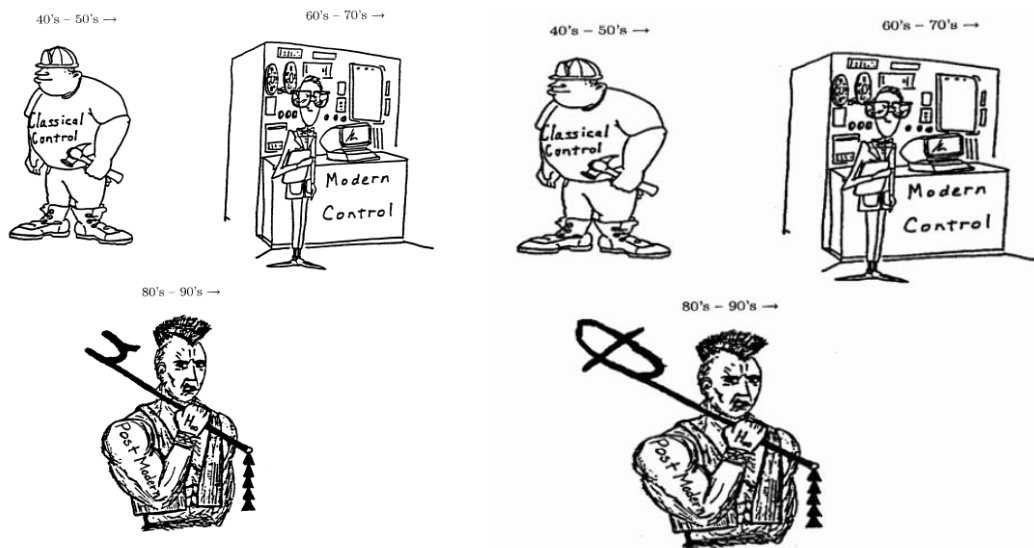
Section 3.1 (Classic control). Figure 5 : Signal flow block diagram in Taylor (1968) master thesis. The blocks into dash lines correspond to proportional integrative derivative (PID) feedback rules.



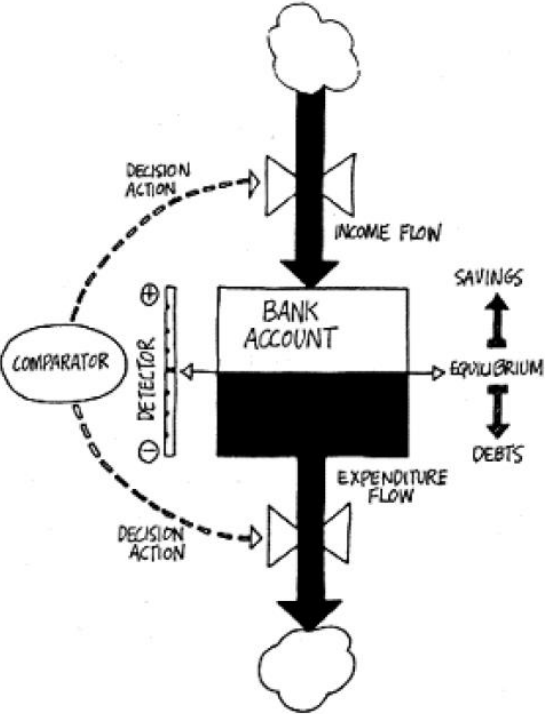
Section 3: Video: Interview with Professor Kalman on the occasion of the Nordic Process Control Award 2015 (21/01/2015). At 20:00 *“Everything depends on how accurately you measure the thing that you want to control”*. At 21:40 *“Modelling is sometimes done a priori, you think it should be like that, but that’s useless unless you have real measurements, that’s where the main problem is... the basic issue is the accuracy of the measurements”*.

<https://www.youtube.com/watch?v=BRS9msPIVYU>

Section 3: Transplants of the three stages of control: Figure 6a and b: A picture history of control (Zhou, Doyle, Glover (1995, figure 1.1, p.35)) on the left replicated in Hansen and Sargent (2008) on the right who changed the Greek letter μ by the Greek letter θ . The post-modern gentleman is for robust control.



Section 4.3. Optimal Saving in Real Business Cycles Model. Figure 7 : Ramsey (1928) feedback mechanism of optimal saving as a reservoir model of the stock of wealth (De Rosnay, The Macroscopic (1979)).



Section 5.1: Dynamic Stackelberg Games. Figure 8a,b,c : Five equicost contours for static two-players contrasting Nash versus Stackelberg game (Chen and Cruz (1972, figure 2), replicated in Simaan and Cruz (1973a, figure 4) with 4 equicost countours and in Kydland (1975, figure 1) with 3 equicost contours.

Figure 7 : Chen and Cruz (1972) : 5 equicosts

Figure 8 : Simaan and Cruz (1973a) : 4 equicosts

CHEN AND CRUZ: STACKELBERG SOLUTION FOR GAMES

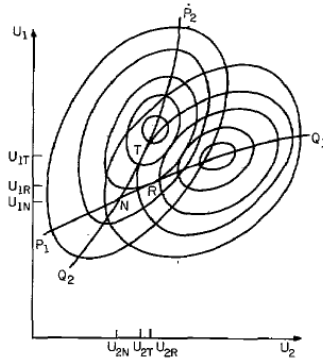


Fig. 2. Equicost contours for two-player nonzero-sum game. Player 1 chooses u_1 and Player 2 chooses u_2 . N is Nash solution, R is Stackelberg solution with Player 2 as leader, and T is Stackelberg solution with Player 1 as leader.

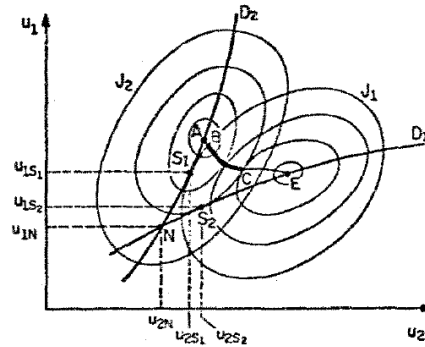


Fig. 4. A game with Nash and Stackelberg solutions.

Figure 9 : Kydland (1975) : 3 equicost contours.

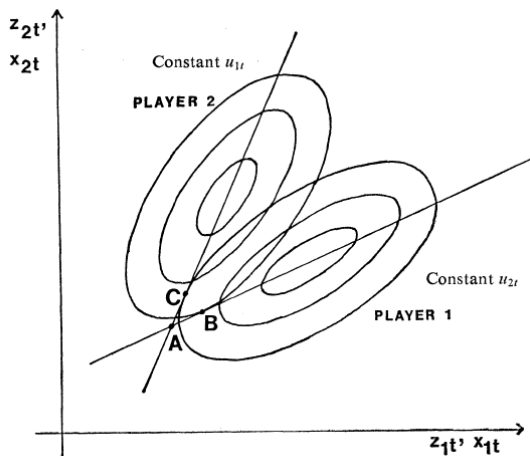


FIGURE 1

Section 6: The return of classic control with a proportional negative feedback interest rate rule, soon to become the "Taylor rule". Figure 9 : Federal funds rate and the forecast of the Taylor rule using 24 quarterly observations between 1987Q1 and 1992Q4. The gap between the two curves corresponds to the new definition of discretion proposed by Taylor (1993).

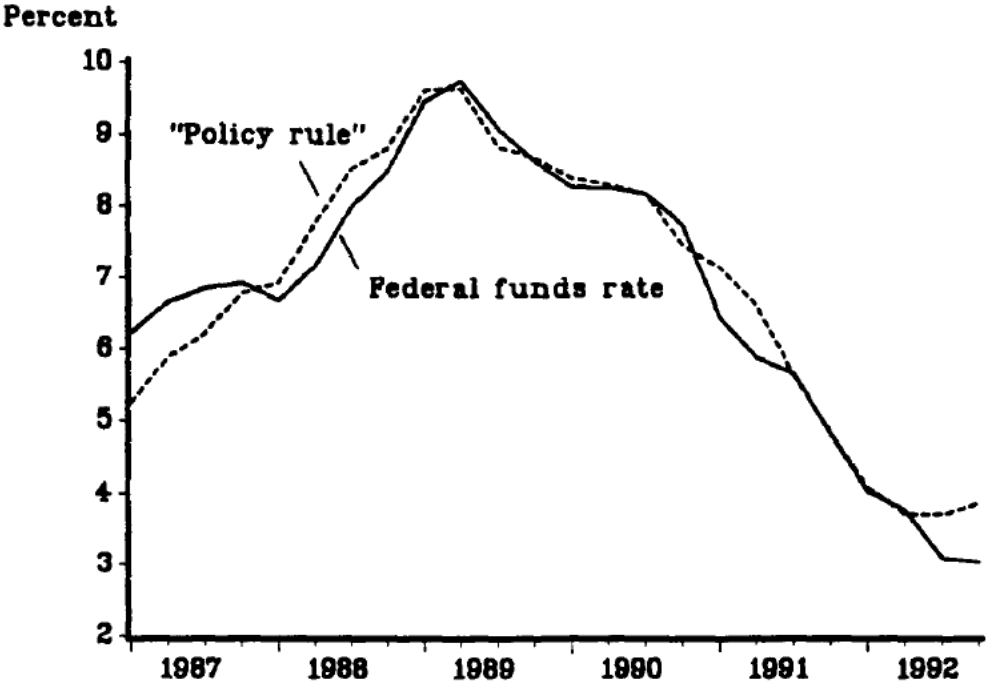


Figure 1. Federal funds rate and example policy rule.