Challenges in the Study of Optimal Monetary Policy

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Rahapolitiikan ja suhdanneteorian keskiöön on muodostunut viime vuosikymmeninä joukko uskomuksia, joita keskuspankit käyttävät politiikkansa ohjenuorina ja joista vallitsee jonkinlainen yhteisymmärrys tutkijoiden keskuudessa. Muutokset rahaoloissa vaikuttavat taloudelliseen toimeliaisuuteen lyhyellä aikavälillä, mutta pitemmällä aikavälillä työllisyyden ja tuotannon määräävät reaaliset tekijät. Rahapolitiikan toteuttamisessa on noudatettava sääntöjä ja noita sääntöjä on viestittävä yleisölle. Mikroperusteiset mallit ovat välttämättömiä politiikkavaihtoehtojen tutkimuksessa. Tutkimus käsittelee optimaalisen rahapolitiikan tutkimuksen lähihistoriaa ja mahdollisia tulevia kehityssuuntia Phillips-käyrän ja yksinkertaisten rahapolitiikkasääntöjen ympärillä.

Rahapolitiikan tutkimus viime vuosikymmeninä on pitkälti ollut vastaamista kahteen 1970-luvulla muotoiltuun haasteeseen: Lucas-kritiikkiin ja säännöistä vapaiden politiikkojen dynaamiseen epäjohdonmukaisuuteen. Lucas-kritiikkiin vastatakseen taloustieteilijät ovat pyrkineet kehittämään malleja, jotka samanaikaisesti ovat sisäisesti johdonmukaisia ja vastaavat empiirisiä tosiseikkoja mahdollisimman tarkasti. Siitä huolimatta että yleisimmissä rahapolitiikan analysointiin käytetyissä malleissa on paljon yhteisiä elementtejä ne voivat silti johtaa hyvin erilaisiin politiikkasuosituksiin. Tavanomaisin tapa tunnustaa dynaamisen epäjohdonmukaisuuden ongelma on johtaa politiikkasuositukset sitoutumiseen kykenevälle ja kykenemättömälle keskuspankille. Tällainen binäärisyys ei vastaa kovin hyvin todellisuutta.

On mahdollista, että lähitulevaisuudessa makrotaloustieteen tutkijat joutuvat vakavasti harkitsemaan epätavanomaisempia lähestymistapoja ja metodeja näiden kysymysten tutkimisessa. Talouden perimmäisten relaatioiden etsintä Lucas-kritiikin kestävän mallin muodostamiseksi on vaarassa johtaa yhä monimutkaisempiin malleihin, joissa epävarmuus politiikkasuositusten pitävyydestä kumuloituu jokaisen uuden yhtälön myötä. Mitä tulee dynaamiseen epäjohdonmukaisuuteen, yleisön ja rahapolitiikan toteuttajien välistä peliä kuvaa paremmin epätietoisuus toteuttajien todellisista preferensseistä kuin toteuttajien kykenemättömyys sitoutua tavoitteisiinsa.

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1. Introduction

How can science help the people govern themselves rationally? In this thesis I discuss the most important intellectual challenges economists have faced and continue to face when studying and conducting monetary policy and, more importantly, seeking ways to make policy better.

The focus will be on simple targeting rules[1], especially inflation targeting and price level targeting. There are two reasons why I think simple rules deserve more attention than other types of monetary policy guidelines, such as optimal reaction functions. First, simple rules are more likely to be robust to uncertainty about the structure of the economy – a policy fine-tuned to produce optimal results in a given structure may perform poorly in another. Not only is the structure of the economy difficult to estimate at any given time, modern economies change more frequently than it is practical to change the guidelines of monetary policy.

The other reason for concentrating on simple rules is that the objectives of the central bank are almost always defined as a simple targeting rule of a sort. An example is section 2A of the Federal Reserve Act: "The Board of Governors of the Federal Reserve System and the Federal Open Market Committee shall maintain long run growth of the monetary and credit aggregates commensurate with the economy's long run potential to increase production, so as to promote effectively the goals of maximum employment, stable prices, and moderate long-term interest rates." It states the objective of the central bank in terms of targets, not instruments, and it does so in a relatively simple fashion. There is still a whole lot of interpretation to do to turn this into policy, but when communicating their own interpretation of their mandates central bankers still keep within the realm of simple targeting rules.

I speak of simple rules instead of mandates because I believe that creating a mandate is as much a question of political science as it is of economics. I present this case briefly by looking at two data points, the Fed and the Bank of England in the recent recession (including the subsequent sluggish recovery). The Fed is said to have a dual mandate, with the price level and the employment objectives being nominally equal.[2] The recent FOMC minutes from June 2012 suggest that the Fed is either considering its mandate to be hierarchical with precedence given to the price level target or

[1] Targeting rules and other relevant terms are defined below.
[2] The third objective stated in the Act, moderate long-term interest rates, come largely as a result of price level stability

it is simply giving an almost trivial weight to its employment target. The FOMC participants have estimated that the long-run level of unemployment is in the range of 5.2 to 6 percent with expectations for 2012 and 2014 ranging from 8.0 to 8.2 and from 7.0 to 7.7 for the two years respectively. Thus the FOMC believes that the level of unemployment is currently substantially larger than the natural level and will continue to be so for at least two years. The following quote from the minutes also belies an idea of the nominal target's precedence:

> "Looking beyond the temporary effects on inflation of this year's fluctuations in oil and other commodity prices, almost all participants continued to anticipate that inflation over the medium-term would run at or below the 2 percent rate that the Committee judges to be most consistent with its statutory mandate. In one participant's judgment, appropriate monetary policy would lead to inflation modestly greater than 2 percent for a time in order to bring unemployment down somewhat faster. "

The mandate of the Bank of England (BoE) looks to be defined as hierarchical, with the principal priority being keeping inflation on track to the 2 % target set by the Chancellor of the Exchequer. To be more precise, the BoE's strategy defines two "core purposes": monetary and financial stability. Financial stability as an objective is not discussed in this thesis, but I will note that the BoE does not seem to be using monetary policy tools to provide financial stability and so this objective can safely be ignored here, as the interest here is with monetary policy and not with whatever central banks happen to do.

Despite this precedence of the inflation target the UK CPI inflation rate has been consistently above 2 % since the end of 2009 and even reached over 5 % at the end of 2011. In the September 2011 Inflation Report the BoE estimated that "the chances of inflation being above or below the 2% target in the medium term are judged to be roughly equal".

As we see there is more to central bank objectives than mere mandates. Some degree of discretion is actually a part of most central bank mandates. Thus when defining mandates one should probably be able to anticipate how that discretion will be used. This thesis will concentrate on the relatively simple world of simple monetary policy objectives, although some organizational and social psychological issues of central banking are touched upon in section 5.4.

The substantial discretion over a significant sector of economic policy has resulted in an exceptionally close relationship between science and practical policymaking. The current paradigm

of monetary policy is inflation targeting (or, strictly speaking, flexible inflation targeting). Inflation targeting has many qualities: an announced numerical inflation target, an implementation of monetary policy that gives a major role to an inflation forecast and high degree of transparency and accountability (Svensson 2008). Inflation targeting has been considered a success and the qualities above are, I believe, desirable qualities in any monetary policy regime. These qualities have probably helped solve or at least ameliorate some of the issues that have troubled central bankers and the public at different points in history: variable interest rates, variable inflation rates and uncertainty about policy objectives and future actions.

There are still open questions within the current regime. Should monetary policy be concerned with asset prices, and if so, which asset prices? What is the optimal target rate of inflation? How should central banks communicate? This thesis concentrates more on a single framework than a given question. Given a structure of the economy and a loss function of the central bank, what can we say about the relative feasibility of given targeting rules? An example of such a problem is whether price-level targeting is preferable to inflation targeting. As this example will be studied more closely in section 3.2 and will be referred to later, it is worthwhile to illustrate the difference between the two and the relevance of the question.

Simply put the difference between the two is that in inflation targeting bygones are bygones. In price-level targeting, if inflation undershoots (overshoots) a target at a given period, this is compensated by higher (lower) inflation in the next period – the policy objective is a price level path. In inflation targeting the policymaker aims for the same rate of inflation each period, even if there have been misses in the past.

History knows of only one explicit price level targeting regime, implemented in Sweden in 1931–1937 (see Berg & Jonung 1999). The current inflation targeting countries all target inflation, not price level, although according to Bernanke & Mishkin (1997), "[i]n practice, central banks tend to compensate partially for target misses, particularly at shorter horizons". The question of horizon length is discussed in King (1999). He argues that the difference between inflation and price level targeting is that of degree, not of quality. The operational inflation target can be denoted as

$$\pi^t = \pi^A - \frac{1}{H}\left[\frac{P_t - P_t^*}{P_t^*}\right],$$

where π^A is the average inflation rate implied by the price level target, π^t is the target inflation rate of the period t, P_t is the price level at the beginning of period t, P_t^* is period t price level target and

H is the policy horizon. If $H = 1$, the price level is brought back to target path in a single period, and as $H \to \infty$, the policy regime comes to resemble pure inflation targeting. Thus there is a close connection between this issue and that of optimal policy horizon (for an analysis of these issues in a single framework, see Smets 2000). Since this particular policy problem serves only to illustrate methodology, the issue of horizon lengths will not be discussed in this thesis and the focus will be on comparisons between pure inflation and pure price level targeting. For an analysis of a hybrid regime mixing both targets, see Batini & Yates (2003).

The focus of this thesis is the method of the study of the relative optimality of simple monetary policy rules, the word method understood broadly as the way research questions are set and solved. How is the economy modeled? How is the issue of time inconsistency addressed? How is uncertainty about the true structure of the economy addressed? To what extent can these problems be solved? I will first discuss some of the themes that have occupied my mind while writing this thesis. I will then give a brief description of the thesis, followed by a note on vocabulary.

This thesis does not aim to contribute to the substantial issues discussed here, such as the relative optimality of inflation targeting and price level targeting. This is more of a literary review, with (hopefully) thoughtful comments and critiques on the literature. I do not seek to determine who is right and who is wrong, but rather highlight the choices that are made and must be made when studying these issues. Two themes are especially relevant.

Firstly, economic research is not a simple issue of finding a relevant problem and researching it. The methods that are thought to constitute proper economics often restrict the set of problems and possible answers beforehand – a prime example of this is methodological individualism. Secondly, resources and rewards within the scientific community are finite and economists who study agents facing tradeoffs face tradeoffs themselves, such as realism and tractability of models. Even more fundamentally this is a question of optimal resource allocation. Should macroeconomists concentrate on finding better microfoundations to satisfy the Lucas Critique of should they rather create better empirical models, possibly for different times and places?

In section 2 the New Classical and New Keynesian Phillips Curves are introduced. Knowledge of these two relations is imperative for they are the most commonly used and contested single structural equations in the study of monetary policy. In section 3 the framework of the analysis of simple monetary policy rules is described with an example. In section 4 I present arguments considering the limitations of this framework and in section 5 I present some approaches how the

problems discussed in section 4 can be overcome or circumvented, and at what cost. Section 6 concludes.

The vocabulary referring to the parts of the framework of monetary policy used here follows the writings of Svensson (e.g. Svensson 2005, Svensson & Woodford 2006). This thesis is concerned with the study of optimal monetary policy, i.e. the best possible policy given the structure of the economy, the instruments available and the goals of the policy. There are numerous levels to optimal monetary policy. This thesis concentrates on, though does not restrict itself, to the theoretical concepts most similar to the mandates given to central banks and their explicit interpretation of those mandates. The reason for this is that what interests me is how scientific knowledge can be used in the political process governing these mandates.

An obviously relevant concept is a *monetary policy rule*, interpreted broadly as a "prescribed guide for monetary-policy conduct". This is contrasted with discretionary policy making . It must be noted here that discretion is used in more than one way in macroeconomics (see e.g. McCallum 2004). Here I use it to refer to a policy conducted subjectively; a policymaker chooses the policy she thinks is best without an external mandate. Although there may be a clear pattern to her actions, this is an *ex post* realization of her own preferences. She is, to echo Weber, not a bureaucrat. Alternatively discretion can be used to refer to policymaking conducted as a sequence of unrelated decisions. This is the more common use of the word in this thesis, and is the subject of section 4.1.2.

The central bank's mandate consists of one or more economic variables and their target levels. To study these mandates, they are translated as *loss functions*, which give the loss – the inverse of utility – of the policymaker as a function of endogenous *target variables*. Another set of variables relevant to monetary policy are *instrument variables*, which are the variables the central bank controls and uses to influence target variables.

Specifically this thesis, as does most of the literature on optimal monetary policy, restricts itself to *targeting rules*. These specify a condition the central bank's target variables (or forecasts thereof) need to fulfill. An alternative would be instrument rules, which are simple mappings from observable or estimated variables to instrument setting. Examples of instrument rules are the Friedman k%-rule, which specifies the rate of growth of the money stock as constant, and the Taylor rule, which specifies the central bank's nominal interest rate as a function of inflation, desired inflation, equilibrium real interest rate, GDP and potential GDP.

All policy rules imply a *reaction function*, which specify the central bank's instrument as a function of variables observable to the central bank at the time it sets its instrument. These are thus similar to instrument rules, but these generally change with the structure of the economy. For example, when the functional relationship between inflation and the money supply changes, this requires a change in the reaction function of a central bank the loss function of which includes inflation.

I follow the convention of using the term rule even though in many instances the term objective would be more proper. In the framework of section 3 policies are differentiated by their respective loss functions, a loss function being basically the translation of an objective into something more subjective. A rule then is nothing but a description of how the loss function is minimized so it is the objective that determines the rule. Thus there is little real threat in confusing rules and objectives since they are so tightly linked together.

2. A Short Introduction to the New Classical and New Keynesian Phillips Curves

An important part of the study of optimal monetary policy is the Phillips Curve. A Phillips Curve denotes a short-run relationship between a nominal variable, usually inflation, and a real variable, usually unemployment or output or their deviations from a trend or a "natural" level. The two most important Phillips Curve specifications are the New Classical and the New Keynesian Phillips Curve. Their historical origins are touched upon in section 4.1.1.

In this section I will go through these curves' microfoundations in a concise and descriptive manner. I hope this exposition, based largely on Woodford (2003), will help understand some of the possible differences in conclusions these two curves may imply for optimal monetary policy presented in section 3.2. Understanding the microfoundations of these curves is important also because they are primarily microfounded equations, not necessarily equations that work well empirically.

An important facet of the literature on the microfoundations of the short-run relation between nominal and real variables is that it actually relates the inflation rate to real marginal costs, not directly to unemployment or production. From this a relation between production or unemployment and the price level is derived, probably since unemployment and output are easier to understand as welfare-relevant variables and more easily observable. For this reason discussion is still heavily

centered on the output gap concept, which is the right-hand-side variable of choice also in this thesis. A discussion of the role of real marginal costs will take place below.

The New Classical Phillips Curve is defined as

$$\pi_t = \kappa(Y_t - Y_t^n) + \pi_{t|t-1},$$

where π_t is the inflation rate[3], κ is a parameter denoting the responsiveness of the inflation rate to the output gap, defined as the difference between output Y and its natural rate Y^n, and $\pi_{t|t-1}$ denotes inflation expectations in period $t-1$ for period t.

The New Keynesian Phillips Curve is defined as

$$\pi_t = \kappa(Y_t - Y_t^n) + \beta\pi_{t+1|t},$$

where β is a discount parameter and $\pi_{t+1|t}$ denotes inflation expectations in period t for period $t+1$. It must be noted that the value of the term κ is not necessarily the same in the two equations.

Woodford's presentation of the New Classical Phillips Curve is a retrofit of a sort. Similar and identical short-run aggregate supply relations have been used quite extensively since the 1970s with varying narratives of microfoundations. The advantage of Woodford's presentation is that both curves are derived from the same analytical framework, which makes comparing the assumptions behind them easier.

Both equations are based on optimizing monopolistic producers facing price-setting constraints. The monopolistic market structure means that each supplier produces a differentiated good, for which there exists imperfect substitutes. This market structure is necessary for without it suppliers would be price-takers and there would be no price-setting behavior, as in the standard model of perfect competition where prices are set by the Walrasian auctioneer, and sticky prices would result in unboundedly large changes in sales.

Three output concepts can be distinguished in the New Keynesian framework. The efficient level of output is what would prevail if markets were perfectly competitive and prices and wages were perfectly flexible. The natural level of output is what would prevail if markets were monopolistically competitive but prices and wages were perfectly flexible. Lastly there is the actual, observed level of output.

[3] Denoting price level in period t with p_t, $\pi_t \equiv p_t - p_{t-1}$

The efficient level of output is the relevant benchmark for welfare analysis, i.e. what matters for welfare is not the gap between actual output and the natural rate of output but that between actual output and the efficient level of output. In light of this it is peculiar that the output gap used in analyses relates to welfare.

What determines the value of κ, which determines how much fluctuations in nominal spending affect real activity? The exact form of κ for each curve can be found in Woodford (2003). In short, κ is affected by price-setting constraints and the degrees of preference for variety and strategic complementarity. I will explain what these constituents describe in an intuitive and qualitative manner rather than giving numerical estimates of them and κ. κ is a function of price-setting constraints and strategic complementarity, and in the case of the NKPC also the discount parameter. I shall first explain the concept of strategic complementarity and the factors affecting it in the New Keynesian framework, and then describe how price-setting constraints are modeled in the two Curves.

Strategic complementarity describes how individual price-setters react to a change in aggregate demand[4]. They can be thought of as an amplification channel for nominal disturbances. The word "strategic" refers to the game-theoretic origins of the concept. Suppose that the economy is hit by a nominal shock. What is the optimal strategy for each price-setter? Specifically how does the optimal price depend on the pricing decisions of other agents? If an increase in other agents' prices increases the agent's optimal price, it can be said that there is strategic complementarity.

Strategic complementarity is a real phenomenon, i.e. it arises from non-nominal sources and it exists whether or not there are nominal rigidities, and as such it cannot account for the *existence* of non-neutrality. But coupled with constraints in price-setting, which by themselves are likely to be an inadequate explanation for the *degree* of observed non-neutrality, they are the cornerstone of the modern understanding of non-neutrality of money.

The factors contributing to strategic complementarity are various. I will discuss this issue only from the point of view of models presented in Woodford (2003), for a more general list of all the possible factors affecting firms' price responses to changes in aggregate demand see the references in Bakhshi et al. (2003).

[4] There are actually two concepts describing this: strategic complementarity and real rigidities. The two come from different intellectual traditions. Strategic complementarity was a concept offered as an explanation for unemployment based on non-nominal factors and thus alternative to Keynesian and New Classical traditions of the time (1980s). Real rigidities is a name given by Ball & Romer (1990) to describe the responsiveness of an agent's desired real price to nominal fluctuations in an economy with money.

One is preference for variety. Each firm has a monopoly over the single good it produces, and these goods are imperfect substitutes for each other. Preference for variety is a term describing the degree of substitutability between different goods, originating from Dixit & Stiglitz (1977). The greater the preference for variety, the greater is κ and the more the adjustment to an aggregate shock falls on output. This is because a greater preference for variety entails a greater degree of monopoly power for each firm and a smaller elasticity for the firm's product, which dampens the firm's price reaction to a change in demand.

Another factor is the intertemporal elasticity of private expenditure. The greater this elasticity is, the smaller is the degree of strategic complementarity and thus the smaller the response of output to aggregate fluctuations. One can think of this as analogous to the preference for variety-theme discussed above. Preference for variety induces monopoly power for each firm in a given time period, and it is this monopoly power which dampens firms' price reactions. Intertemporal elasticity of substitution can be thought of as preference for temporal variety: the smaller the elasticity the more consumers want to spread out their consumption between time periods. Thus a smaller intertemporal elasticity means that producers within each time period have greater monopoly power vis-á-vis producers in other time periods.

Additional factors are the less intuitive degree of diminishing returns to labor in the production function and the degree of increasing marginal disutility of work. Lastly, and this is quite an important factor, is the assumption of factor specificity. Factor specificity means that at least some of the inputs used in production are not perfect substitutes across firms or industries. Factor specificity is an additional source of strategic complementarity (Woodford 2005b).

The curves differ in their treatment of price-setting constraints. In the New Classical specification, it is assumed that a fraction of prices are set one period in advance, while the rest of the prices are set each period with full information about current demand and cost conditions. The New Keynesian version utilizes the Calvo (1983) pricing model, where a fraction of prices remain fixed each period with each price having an equal probability of being revised in any given period.

Expectations in both specifications are assumed to be formed rationally. In the definition of Sargent (2008), "rational expectations is an equilibrium concept that attributes a common model (a joint probability distribution over exogenous variables and outcomes) to nature and to all agents in the model. The rational expectations equilibrium concept makes parameters describing agents' belief disappear as components of a model, giving rise to the cross-equation restrictions that offer rational

expectations models their empirical power." Rational expectations is therefore a modeling technique and a central one at that. The implication of non-rational expectations for the optimal monetary policy framework is discussed in section 4.2.2.

Empirically both curves have fared rather unsatisfactorily. The NCPC makes the false prediction that anticipated changes in the money supply (or the interest rate) have no effect on real variables. This is considered to be a false prediction (Mishkin 1980). The NKPC on the other hand predicts that there is no inertia in the inflation rate, leading to the possibility of "disinflationary booms", which are not only counterintuitive but rarely seen in real life (see for example the references in Galí & Gertler 1999). The issue of credibility is, however, very important here and provides a plausible explanation for the negative real effects of disinflationary periods. This will be discussed in section 4.1.2.

To account for the lack of persistence in the NKPC, two types of specifications have been proposed and used. Clarida, Galí & Gertler (1999) make the specification of an autocorrelated error term

$$\pi_t = \beta\pi_{t+1|t} + \kappa y_t + u_t$$

$$u_t = \rho u_{t-1} + \varepsilon_t,$$

where u_t can be interpreted as a "cost-push shock". This is a shock that affects the relation between the natural and efficient levels of output. The addition of the cost-push shock also creates a trade-off between inflation and output gap stabilization – without it the model exhibits what Blanchard & Galí (2007) call "divine coincidence", in which the optimal policy description is stabilizing inflation only, as this coincidentally stabilizes the output gap also.

An alternative presentation, due to Galí & Gertler (1999) is the "hybrid New Keynesian Phillips Curve", where the error term is i.i.d. but a lagged inflation term is included in the equation

$$\pi_t = \omega_f\pi_{t+1|t} + \kappa y_t + \omega_b\pi_{t-1} + \varepsilon_t,$$

where ω_f and ω_b denote the shares of forward-looking and backward-looking price setters in the economy respectively.

There is an ongoing debate discussing the empirical relevance of this specification (see the Journal of Monetary Economics 2005 Issue 6). An important part of this discussion is the role of real marginal costs and measuring them appropriately. It's also worth noting that how good a model is

depends naturally on its particular use. Galí & Gertler (1999) argue that a more theoretically correct model with real marginal costs as the right-hand-side variable fits the data better than a more traditional Phillips-curve relationship with production or unemployment as the right-hand-side variable. This may be true (though univariate models may still outperform Phillips curves in out-of-sample prediction (see Stock & Watson 2008)), but when analyzing optimal monetary stabilization policy the crux of the issue is welfare analysis and real marginal costs are welfare relevant only to the extent that they affect other variables, such as employment and output.

3. Simple targeting rules

In this section I will provide a short introduction to the study of optimal monetary stabilization policy using simple rules. In section 3.1 the basic approach is explained verbally and some notions of historical development are made in anticipation of section 3 discussing the limits of this approach. In section 3.2 an example of the application of this approach is given as the problem of inflation targeting vs. price level targeting is discussed. The examples given in later sections will usually refer to this particular theme.

3.1. Introduction to the Framework

In this section I will introduce the framework in which simple monetary policy rules are analyzed. First the inapplicability of the more "natural" method, straightforward optimal control is argued. After this the role of objectives and welfare is discussed. Finally some remarks are made concerning the structure of the economy in these models.

How can economics help society determine what is good monetary policy? Economics is about optimization, and naturally an application of optimal control theory comes first in mind. The problem of optimal monetary policy would be posed as a constrained minimization problem, in which the objective function is a central bank loss function and the constraints describe the workings of the economy. The loss function and these constraints form a model. The solution to the problem is the loss-minimizing path of the control variable, i.e. an instrument rule.

There are severe problems in applying optimal control methods to determine optimal monetary policy – although this is certainly not to say they are useless. Let us forget the pitfalls of applying optimal control methods to forward-looking models for now; these are discussed in section 4.1.2.

There are two additional problems with the method. First, it tends to yield complex results. For example Orphanides & Williams (2008) calculate optimal policy for an economy with a New Keynesian Phillips Curve with indexation and an "IS" curve with adjustment cost or habit as

$$i_t = 1.17i_{t-1} + 0.03i_{t-2} - 0.28i_{t-3} + 0.17\pi_{t-1} + 0.03\pi_{t-2} + 0.01\pi_{t-3} - 2.47u_{t-1} + 2.12u_{t-2} - 0.32u_{t-3}$$

, where i is the interest rate, π is the inflation rate and u is the unemployment rate with the subscript denoting time. The equation thus includes three lags of each state variable. It is certainly easier to communicate, defend and legitimize a more general target, such as "price stability" than a reaction function such as the one above.

Perhaps the more important reason is that optimal control often does not yield very robust results. A reaction function which is optimal in one environment may perform poorly in a different environment, even if these two environments were not very far from each other. Simple monetary rules are generally more robust to such changes or errors. This is a separate issue from that of defining mandates broadly so as to enable central banks to use their discretion to respond to changing circumstances. It is assumed here that the central banker follows the simple rules given to her. One could even argue that there is no central banker as a decision-making agent, only a rule.

Thus whereas the traditional optimal control framework aims to find certain (instrument) rules to provide the optimal behavior of a given system, the simple (targeting) rules framework aims to compare the performance of different targeting rules in a given system. Optimal control methods are used in the latter framework as the central bank implicitly uses these to derive its reaction functions, so the difference is not so much the method as the purpose of the analysis, although the purpose of the analysis also in part defines the method.

As mentioned in the introduction different monetary policy rules are actually different loss functions. Thus when comparing the relative optimality of different policies one is actually comparing the performance of central banks with different loss functions within a given economy. An obvious question here is how performance is measured. There are two ways to do this.

The first is to assume a "social loss function" or the "actual loss function" to serve as the basis for ranking different policies. Why shouldn't the actual loss function be also central bank's loss function? The reason for this is that there may be some other rule which can replicate optimal performance when the central bank is not able to commit to a certain policy (in the time

inconsistency sense). For example even though society's preferences correspond to an inflation target, they may still be better off with a price level targeting central bank when the central bank is unable to commit (this is the case in Svensson 1999).

The second way is to choose some variables of interest and examine their behavior under different policies. For example one could compare the variance of both inflation and unemployment under inflation and price level targeting (Vestin 2006). The only welfare criterion here is a sort of Pareto criterion: only a policy which provides lower variance for either inflation or unemployment without increasing variance in the other can be said to be better than the other policy.

The structure of the economy can be seen as a constraint from the point of view of the optimizing agent, the central bank. The complexity of the model can be seen as the distance between what the central bank controls and what it wants to control. I will present the most important elements of some of the most common models in an increasing order of complexity.

At minimum the model consists of the loss function and a single constraint. In these models the variables of interest, usually inflation and production, are assumed to be directly controlled by the central bank. The constraint is simply an aggregate supply function describing the relation between inflation and production, i.e. a Phillips Curve. An additional function describing the formation of expectations is needed where expectational operators are used. Models in this fashion are Svensson (1999) and Vestin (2006).

A layer of complexity can be added by assuming that the central bank does not control inflation and unemployment directly but rather influences these by changing the rate of interest. The additional variable is the level of interest and the additional equation is an aggregate-demand relation, which is sometimes called the intertemporal IS-equation. Another way of understanding this is that the central bank controls aggregate demand, often interpreted as nominal GDP, and it is the aggregate-demand-relation that determines how a change in the interest rate is translated into a change in prices and production. This is the canonical approach of the new Keynesian Phillips Curve (Clarida et al. 1999).

The model described above, with interest rate and aggregate demand, can be reduced to the one before it, with just the Phillips Curve, in all but one case. As it is a rare case, this step of simplification is usually taken and if the aggregate demand relation is kept in the model, the constraint seldom binds. The special case is that of zero lower bound on the rate of interest, or the liquidity trap, as it is sometimes called. Zero lower bound is simply a constraint on values that the

interest rate can take; namely that it cannot be negative. Analysis in this case is technically more difficult, as it involves non-linear constraints, but it is nonetheless possible to draw some conclusions (Eggertsson & Woodford 2003, Woodford 2011).

Finally one can assume that the central bank does not control the level of interest but only the money supply. This leads to an addition of an equation describing how the interest rate is defined in the money market.

The order in which different variables are added into the models can be seen as an implicit ranking of different variables' marginal efficiency in making the model more useful and the point at which a researcher has stopped adding variables in her model is her optimum, where the marginal utility of adding a layer of complexity equals the marginal cost. Another way to interpret the addition of constraints is from the planner's perspective. As the economy grows more complex, the power of the planner over the economy diminishes. Thus in the most simple family of models we see the central bank controlling inflation and unemployment directly. Eventually one winds up with the only thing central banks actually have at their disposal, namely the monopoly in the issuance of base money.

In all the structures presented above, however, the aggregate-supply relation is the most important. By this I mean that formulations of all the other elements in the model are fairly standardized and different results are usually results of different assumptions about the aggregate-supply relation. Exceptions are indeed exceptions, such as the zero lower bound and the ensuing need for aggregate demand modeling.

I will end this section by a quote from Williams (1999), as it anticipates some of the themes of section 4.

> "What is a good monetary policy rule for stabilizing the economy? Confidence in model-based answers to this question has waxed and waned over the last three decades. By the 1970s, application of optimal control techniques to estimated macro models appeared to provide a precise answer based on a concrete description of policy makers' preferences and the law of motion of the economy. This approach then came under attack from two sides. Lucas (1976) decried the fact that the structural parameters of the macroeconomic models used for policy evaluation were assumed to be invariant to policy, contradicting the notion of optimizing agents. Moreover, Kydland and Prescott (1977) argued that such policies were in any case time

inconsistent, that is, a policy maker would find it advantageous to deviate from the 'optimal' policy rule." (Williams 1999)

The contemporary analysis tries to sidestep these pitfalls. Lucas critique is taken into consideration by using models derived from agents' optimization, although the constraints of optimization are often assumed to be model-independent, such as Calvo pricing. The rules vs. discretion issue is addressed by deriving results separately for cases with and without commitment (Svensson 1996) or, more usually, stating that commitment is an unrealistic case and focusing on discretion only (Dittmar, Gavin & Kydland 1999). Some critique of the way these critiques are taken into consideration in contemporary research is offered in section 4.1.

3.2. An example: New Classical and New Keynesian Phillips Curves and Inflation Targeting and Price Level Targeting

In this section I will illustrate the analysis of optimal monetary stabilization policy in the framework described above through an example: is price-level targeting preferable to inflation targeting? I will specifically highlight the issues of microfoundations and commitment as these will be discussed later in more detail. I will first contextualize this debate and then proceed to discuss some of the research.

The inflation-targeting regimes that have been established by numerous Western countries are generally thought to have been quite successful. Scientists however need problems and there has been an increasing interest in searching for ways to improve these regimes (or alternatively replace them with better regimes). One venue of research has compared the merits of inflation targeting and price-level targeting.[5] I will give a simple example to illustrate the difference between the two policies. Let's assume, for example, that the inflation target is two percent per annum. A fact is that the central bank cannot control inflation over the short term; a two-percent-inflation targeter is likely to under- and overshoot its target from time to time. Suppose the inflation rate at year *t* has been 1,5 percent. An inflation targeting regime implies a constant inflation target, meaning an inflation target of 2 percent at year *t+1*. A price level targeting regime, however, implies a constant

[5] The Government and the Bank of Canada recently renewed their monetary policy agreement. When preparing the new agreement the possibility of replacing the inflation target with a price-level target was considered and extensively studied, but eventually rejected. On this, see Bank of Canada 2011

price level target, meaning an inflation target of (approximately and depending on the horizon) 2,5 percent at year *t+1*, to make up for the previous year's undershooting.

It is worth emphasizing how the terms of debate in monetary policy have shifted. As Taylor (1994) writes:

> "Describing the nature of the trade-off between inflation and output or unemployment has long been difficult and controversial. The Friedman-Phelps hypothesis, that there is no long-run Phillips Curve trade-off between inflation and unemployment, has clearly won over most macroeconomists, but the debate has continued over what, if any, trade-off remains. The subtle notion that an uncertain short-run trade-off, but no long-run trade-off, exists between inflation and output has proved more difficult to analyze and describe."

Since Taylor's article, the short-run trade-off has been analyzed and described enough to conclude that it certainly exists for certain Phillips Curves, although whether it exists in reality, i.e. whether such models are realistic is more contentious. Specifically in the issue of inflation targeting and price level targeting the relevant trade-off is not between levels (or rates of change) as in the old long-run trade-off, but between the variability in inflation and output.

The literature on this question concentrates on the relative optimality of inflation targeting and price level targeting in economies characterized either by the New Classical Phillips Curve (NCPC) or the New Keynesian Phillips Curve (NKPC).[6] As discussed in section 2, the NKPC needs to be augmented somehow to account for the sluggish behavior of inflation, and it is specifically these persistence-augmented versions of the model that are used. I will restrict my attention to three articles: Svensson (1999), Dittmar & Gavin (1999) and Vestin (2006). There are two reasons for this restriction. First is that my aim is merely to illustrate the approach, not to settle the actual issue, and to that end these articles suffice. The second is that these are the most important papers in the literature and their approaches are relatively similar, making them easy to compare. For more references on this literature, see Côte (2007).

In the "conventional wisdom"[7] regarding this question it was acknowledged that price level targeting would likely be better than inflation targeting with regard to questions other than monetary

[6] A notable exception to this is Ball, Mankiw & Reis (2005).
[7] This conventional wisdom was defined be Svensson (1999) who also, in part, overturned this and so the conventionality of it should probably be taken with a grain of salt.

stabilization, but these benefits were deemed to be relatively small. As Fischer (1994) argues, although price-level targeting would remove the long-term uncertainty in the price level other mechanisms to this end exist already (such as indexed bonds and inflation-contingent wage contracts) and the fact they are not widely in use suggests that the costs of this long-term uncertainty are not substantial. On the costs side it was argued that as periods of above-average inflation would need to be followed be periods of below-average-inflation and vice versa price level targeting would result in larger inflation variability (and thus larger output variability, via the Phillips curve) in the sort run relative to inflation targeting.

Svensson (1999) showed that under certain conditions (discretion, forward-looking expectations and a certain amount of a certain type of persistence) price-level targeting provides a "free lunch" – lower inflation variability at no cost to output variability. This result was then studied under alternative specifications by Dittmar & Gavin (1999), Barnett & Engineer (2001) and Vestin (2006). I will now give a brief overview of the results and the intuition behind them, after which I examine the Phillips curve specifications of Svensson (1999), Dittmar & Gavin (1999) and Vestin (2006).

Svensson showed that for an economy described by a Lucas supply curve price-level targeting dominates inflation targeting if output is sufficiently persistent and the central bank is unable to commit. This is the case even if society's preferences are such that the representative agent cares about inflation variability, and not price level variability. The result resembles that of Rogoff (1985) in that society can be made better off by appointing a central banker with different preferences from those of the representative agent if the central banker is unable to commit.

In this case the result stems from the fact that a price level target is a history dependent variable, unlike inflation. This history dependence helps the central bank take account of the dynamic nature of monetary policy. It does not necessarily mean that the the problems of time inconsistency are solved and that the ideal solution, inflation targeting under commitment, can be replicated. Vestin (2006) finds that for an economy characterized by a NKPC price level targeting under discretion replicates inflation targeting under commitment only if there is no persistence. Price level targeting nonetheless dominates inflation targeting under discretion.

When it is assumed that a central bank is unable to commit, price-level targeting seems to dominate inflation targeting for economies characterized either by a NCPC or a NKPC, at least for sufficient, empirically plausible degrees of persistence. With NKPC price-level targeting dominates regardless

of the degree of persistence (Vestin 2006). With NCPC price-level targeting dominates if ρ >0.5 in the specification given below, where the left-hand-side variable is output and persistence is introduced with a lag, the coefficient of which ρ is (Dittmar et al. 1999). An important caveat is that the expectation formation in Svensson (1999), Dittmar & Gavin (1999) and Vestin is purely forward-looking.

The analyses leading to the above conclusions suffer from a few shortcomings I find to be prevalent problems in modern macroeconomics. The Lucas critique, examined in more detail in section 4.1.1, has led to a search for models (or in this case Phillips curves) with both consistent microfoundations and empirical accuracy. One result of this search has been the NKPC and the NCPC which Woodford (2003) derives meticulously from certain micro-level utility functions and constraints. But as noted in section 2, these curves display too little persistence in inflation and two different specifications for additional persistence, an autocorrelated error term and a lag of inflation, have been used to improve on the issue. Barnett & Engineer (2001) call these "exogenous" and "endogenous" persistence respectively.

The problem with these specifications, at least from the point of view of Lucas critique, is that they are not derived from the micro level and thus somewhat undermine their entire purpose. I will return to this issue in section 4.1.1. For now I wish to highlight another problem which has resulted from uncertainty regarding the true functional form of the Phillips curve and the sources of persistence.

Svensson (1999) and Dittmar & Gavin (1999) use the following relation

$$y_t = \rho y_{t-1} + \alpha\left(\pi_t - \pi_{t|t-1}\right) + \varepsilon_t,$$

where y_t is the deviation of output from the target level (which is assumed to be set so that no long-run inflation bias exists), ρ and α constants ($0 \leq \rho \leq 1$ and $\alpha > 0$) defining the degree of persistence in output and the responsiveness of the output gap to unanticipated inflation respectively, π_t is the inflation rate, $\pi_{t|t-1}$ denotes inflation expectations in period $t-1$ for period t and ε_t is an i.i.d. temporary supply shock with zero mean and variance σ^2.

Dittmar & Gavin (1999) also examine the behavior of price level targeting and inflation targeting under what they call a New Keynesian Phillips Curve, which they define as

$$y_t = \rho y_{t-1} + \alpha\left(\pi_t - \pi_{t+1|t}\right) + \varepsilon_t,$$

The equations are, as Dittmar and Gavin aptly put it, "deceptively similar". The only difference is that in the New Classical version anticipated inflation enters as the previous period's expectation of period t inflation, whereas in the New Keynesian version it enters as the current period's expectation of period $t + 1$ inflation. Expectations are assumed to be formed as a rational response to the central bank's known policy rule.

Vestin's (2006) specification of the New Keynesian Phillips Curve is of the form

$$\pi_t = \beta\pi_{t+1|t} + \kappa y_t + u_t,$$

where $u_t = \rho u_{t-1} + \varepsilon_t$. Note that constants such as ρ and α do not necessarily take the same values in different equations.

The specifications are obviously different. The first difference is how anticipated inflation enters the equation as noted above. The second difference is the persistence specification. Svensson (1999) and Dittmar & Gavin (1999) use the lag of the left-hand-side variable, whereas Vestin (2006) uses an autocorrelated error term. The third difference is the specification of the direction of the relation, which is always a little arbitrary in the field of macroeconomics, where simultaneous determination is the norm. Svensson's and Dittmar & Gavin's specifications are better described as supply curves since their left-hand-side variable is output.

Whether the relationship is written as a supply curve or a Phillips curve makes no difference when the function includes no lags and the possible error term is i.i.d., as in the NKPC and NCPC as they were presented in section 2. But as we have noted, researchers have found it necessary to include either a lag of the left-hand-side variable or an autocorrelated error term to add persistence to the model, thus making this issue of functional form non-trivial. For example compare the NKPC specifications of Dittmar & Gavin (1999)

$$\pi_t = a(y_t - \rho y_{t-1}) + \pi_{t+1|t} + \varepsilon_t,$$

which is presented here as rearranged so as to make inflation the left-hand-side variable, and Vestin (2006)

$$\pi_t = \beta(\pi_{t+1|t} - \rho\pi_{t|t-1}) + \rho\pi_{t-1} + \kappa(y_t - \rho y_{t-1}) + \varepsilon_t,$$

which is rearranged so as to make the error term i.i.d.[8] These two specifications clearly depict two different economies and they also imply different sources of persistence with possibly different consequences for policy. All this uncertainty leads naturally to the question of robustness. How can we tell whether price-level targeting is better than inflation targeting or not if we are not certain of the true model of the economy? These issues are discussed in section 4.2.1.

The last aspect I find slightly problematic is how the issue of time inconsistency is included in these analyses. Kydland and Prescott (1977) showed that the presence of forward-looking expectations in the constraint equations, such as these Phillips Curves, cause a time inconsistency problem for the central bank. A (possibly time- and state-contingent) strategy (a policy rule), is said to be time inconsistent if an agent (a central bank) finds it optimal from the point of view of some initial period t_0 but finds it suboptimal in some subsequent period t (Klein 2008). The classical example is that the central bank promises to keep the interest rates at a path that produces zero inflation. As the price setters believe this and set their prices accordingly the central bank is tempted to lower interest rates to raise employment levels forcing the price setters to sell at non-profit-maximizing prices (since the interest rates affect optimal price setting). Doing this the central bank loses credibility, leading the price setters to anticipate inflation even when the central bank would not renege on its promises.

This issue is another source of uncertainty, since there is no consensus whether central banks should best be thought of as being able to commit or not. The standard practice is either computing results for both cases (as in Svensson 1999) or for the case which one thinks is more realistic (as in Dittmar & Gavin 1999, who assume the central bank is unable to commit). In section 4.2.2. I discuss to what extent the whole binary commitment/discretion-issue is an accurate representation of reality.

4. Limitations of the current approach

In this section I will give a summary critique of the approach outlined and applied in section 3. Where available, I present some proposals researchers have made to improve on some of its shortcomings –which in the case of more fundamental critiques looks more of a revolution than a reformation. The discussion is brief, but it ought to leave the reader with a feeling that central banking is still as much art as it is science and why we need not only good research and good institutions but also good central bankers.

[8] These are derived in Appendix A.

The discussion will, I hope, also convey the following idea. While the two themes discussed in section 4.1., the Lucas critique and time inconsistency, used to represent the frontier in the study of optimal monetary policy, diminishing marginal productivity of scientific research has led to them being constituted as boundaries and new frontiers being opened elsewhere. The analogy with geography is quite functioning, as far as analogies go. Kristof (1959) sketches their differences in geography as follows (emphasis in the original): "The *frontier* is *outer-oriented*. Its main attention is directed toward the outlying areas which are both a source of danger and a coveted prize. --- The *boundary*, on the contrary, is *inner-oriented*. --- While the frontier is inconceivable without frontiersmen – "an empty frontier" would be merely a desert – the boundary seems often to be the happiest, and have the best chances of survival, when it is not bothered by border men." And lastly, "[t]he *frontier* is an *integrating factor*. Being a zone of transition from the sphere (ecumene) of one way of life to another, and representing forces which are neither fully assimilated to nor satisfied with either, it provides an excellent opportunity for mutual interpenetration and sway. --- The *boundary* is, on the contrary, a *separating factor*."

Section 4.1. takes an historical view of the approach as it outlines a critique of how the issues of Lucas critique and time inconsistency have been solved. These two were the dominant themes in the study of monetary policy in the last quarter of the 20th century and they have been explicitly dealt when building the method and its applications. This is the boundary, where the line between what is known and what we cannot know is the clearest. The boundary is a separating factor: there are models that satisfy the Lucas critique and there are models that don't (in principle); there is discretion and there is commitment (in principle).

Section 4.2. discusses the areas where the frontier of this patch of science is expanding more rapidly: non-rational expectations and model uncertainty. Although they are both quite old themes, only recently have researchers been able to model them satisfactorily. This is the frontier where the line between what is known and what we cannot know is constantly being blurred by and where we can see all the elements of what we will know with more certainty in a few decades.

4.1. Problems in solved issues

Two themes that dominated the study of optimal monetary policy in the last quarter of the 20th century: the Lucas critique and the problem of time inconsistency. Since they were presented in the 1970s no macroeconomist, theoretical at least, has been able to disregard them. This section

describes what they say, what contemporary practices did they critique, how current methodology deals with them and how well does it succeed in doing so.

A general note is in order. Although the two themes are discussed in two separate sections they are both features of a model with forward-looking behavior. It is the implications of the existence of expectations, not whether they are rational or not, that formed the frontier of macroeconomics from the late 1960's onward. The modeling of expectation formation is discussed in section 4.2.2.

4.1.1. The Lucas Critique

> "This essay has been devoted to an exposition and elaboration of a single syllogism: given that the structure of an econometric model consists of optimal decision rules of economic agents, and that optimal decision rules vary systematically with changes in the structure of series relevant to the decision maker, it follows that any changes in policy will systematically alter the structure of econometric models. " (Lucas 1976)

It is quite difficult to articulate the essence of the Lucas critique better than Lucas himself in the quotation above. But it is possible to give it some historical context. Lucas naturally refers to contemporary practices but says little about the historical developments in the field before his article and, needless to say, after his article, which are of much interest to us.

In principle the Lucas critique applies to all model-based policy analysis, but it is only in the realm of macroeconomics and even there only when discussing monetary policy that it is considered to be something to be reckoned with. The reason for this lies perhaps in the fact that systematic policy, modeled as "policy rules", is thought to be possible only for central bankers – and even that it dubious. Policy rules and their relevance are discussed in the next section.

The Lucas critique says that it can be dangerously misleading to estimate the effects of a change in the policy regime by estimating responses based on historical, old-regime data. To predict how different agents in the economy respond to monetary policy, it is not sufficient to know the magnitude of the change in the instrument and the prevailing economic conditions, for agents are forward-looking and they speculate on future policy actions. For example during the classical gold standard, and to a lesser extend during the Bretton Woods period, the effects of any policy actions

on currency markets were quite muted. This was due to the firm and widely held belief that keeping the external value of money fixed was the ultimate objective of central banks. The Lucas critique says that it would be wrong to conclude that the effects would be the same now that central bankers care more about inflation and unemployment than they do about exchange rates. (Ljunqvist 2008)

Let's write this argument down more formally so as to make it easier to refer to its parts later on. The presentation follows Lucas (1976), although here the problem is posed more specifically as a monetary policy problem. The motion of the economy is determined by a difference equation

$$y_{t+1} = f(y_t, x_t, \epsilon_t),$$

where the time period is denoted by t, y is the target variable, x_t is a control variable and ϵ_t is a vector of random shocks. The function f is taken to be fixed but unknown and it is the econometrician's task to estimate it. This is done by estimating the values of a fixed parameter vector θ, with

$$f(y_t, x_t, \epsilon_t) \equiv F(y, x, \theta, \epsilon),$$

and the econometric structure F being specified in advance. The estimated F then provides the dynamic constraints of the optimal control problem, which is quite straightforward to solve for a given cost functional assuming θ is known. The problem of model-based economic policy-making was thus to estimate θ. And this was no easy task. θ should include parameters from all the relevant behavioral relationships of the economy, ranging from supply of labor to foreign demand for domestic currency. The number of elements in θ was arbitrary as it varied from model to model but nonetheless quite large (typically numbering in the hundreds).

In the gold-standard example θ would include a parameter measuring the responsiveness of the foreign exchange value of the currency to control variables (the interest rate) and domestic economic conditions (unemployment). The estimated relation would be weak since during that period central bankers cared little for domestic economic conditions and instead used the interest rate to keep the foreign exchange value of the currency fixed (Eichengreen 2008). Also the functional form itself may change.

Lucas (1976) argued that the practice described above was useless for policy analysis. Sims (1982) presents the case from the point of view of the technical method, optimal control theory, and its use

in natural sciences on the one hand and in economics on the other. In economics it is harder to know the vector θ. Indeed, it would be most peculiar if there were different models with strikingly different amount of equations describing the motion of a space vehicle, for example.

Sims, who was critical of the structural models advocated by Lucas and Sargent and favored vector autoregressions offers very enlightening criticism of the Lucas critique in Sims (1982). Sims writes as follows:

> However, this abstract description of the problem of policy choice appears at first glance not to match the problems policymakers actually face. --- …in practice macroeconomic policymaking does not seem to be this sort of once-and-for-all analysis and decision. Policymakers ordinarily consider what actions to take in the next few months, and repeatedly use econometric models to project the likely effect of alternative actions. Furthermore, optimal policy should be a deterministic function of information available to the policymaker, but actual policy seems to include a large component that is unpredictable even to observers with the same information set as the policymaker.
>
> ---
>
> Policy is not made by a single maximizing policymaker, but through the political interaction of a number of institutions and individuals. The people involved in this process, the nature of the institutions, and the views and values of the public shift over time in imperfectly predictable ways.

Has there ever been a better and more sincere description of the antithesis to central bank independence and to the virtues of the inflation targeting regime as expounded by Bernanke & Mishkin (1997) among others? Viewed in this light, the accomplishment of Lucas (1976), and to equal extent Kydland and Prescott (1977), is shifting the terms of the debate. The responses to these articles made explicit many inferior practices in economic modeling and policymaking, which led to raising the bar for some aspects of central banking. I will give brief note of the history of structural models and how they led to the development of the equations used in the example of section 3.2. The aspect of commitment will be discussed in the next section.

The work to establish a Phillips Curve relation with microfoundations began before Lucas (1976), which gave this research project a formal justification. The main point of the Lucas critique is

present in Phelps (1967) , who with Friedman (1968) had argued against a permanent inflation-output-tradeoff and for a concept of a "steady-state" or "natural" level of unemployment. This concept was coupled with assumptions of continuous market-clearing, imperfect information and rational expectations leading to the Lucas Supply Curve (Lucas 1972, 1973). One interesting prediction arising from models structured around this was that only unanticipated changes in the money supply can have real effects, and for a time empirical evidence seemed to support the hypothesis (see the references in Mishkin 1980). This was however refuted by Mishkin (1980). Also constantly clearing markets with perfect competition was too outlandish an assumption for most macroeconomists (Blanchard 2008).

An alternative route was cleared by Fischer (1977) and Taylor (1979), who established a Phillips Curve relation based on constraints on wage setting, although the Calvo (1983) specification of price stickiness became the standard. Research was needed, however, to explain the existence and magnitude of nominal stickiness and its effect on real economic variables.

As explained earlier, price-setting agents can only exist in imperfectly competitive markets. Akerlof & Yellen (1985) and Mankiw (1985) took the task of explaining how sticky prices can be both privately efficient and socially inefficient – for if sticky prices were socially efficient, there would be no Keynesian theory of business cycles as we know it and if they were privately inefficient that would mean profit-maximizing firms were leaving money in the table. The eloquent answer given was that losses resulting from not changing prices constantly were first-order on aggregate real variables but only second-order on individual profits. Ball & Romer (1990) showed that nominal rigidities alone cannot account for the magnitude of real fluctuations observed. What is needed are real rigidities, which amplify the effects of sticky prices on real variables as explained in section 2.

The New Keynesian Phillips Curve can be derived from the Calvo model of dynamic pricing (Roberts 1995). The history of the New Classical Phillips Curve is not so straightforward and clear. The term itself was not used in the New Classical Macroeconomics research but is instead a later invention.

The Phillips Curve relation derived by Lucas (1973) is of the form

$$y_t = y_{n,t} + \theta\gamma(P_t - \bar{P}_t) + \lambda\left[y_{t-1} - y_{n,t-1}\right],$$

where y_t is the level of production, $y_{n,t}$ is the "secular" component of aggregate supply reflecting real aggregate variables, fundamentals such as capital accumulation and population growth, $P_t$is the

price level, θ, γ and λ are parameters and t-subscripts denote time. The observed level of production in each market consists of the secular aggregate component and a cyclical, market-specific component which is a function of relative prices and its own lagged value. $\bar{P}_t$ can be interpreted as the period t expectation of the price level given prior information. With a little rearranging it can be seen that this is identical to the persistence-augmented New Classical Phillips Curve used by Dittmar & Gavin (1999), which was discussed in section 3.2.

Some of the themes discussed in section 2 are of relevance here. It was shown that the attempt to derive a Phillips Curve relation with microfoundations, which would lead to a complete characterization of agents' optimization problems and thus pay heed to the Lucas critique, has been incomplete. I will first review some of the ad hoc-solutions made in the research and then describe some of the broader limitations and implications of the microfoundations literature.

A central part of New Keynesian macroeconomics is the Calvo pricing equation. It models time-dependent pricing and as such is clearly not based on optimization. It is easy to think of examples that would change the frequency of adjustment, such as hyperinflationary periods. That said, models with state-dependent pricing haven't improved the performance of the inflation equation much, though they can be made to correspond better to the stylized facts of microevidence on price changes (Dixon & Le Bihan 2012, Woodford 2009b).

Thus even the standard NKPC does not satisfy the Lucas critique. Nonetheless a more salient piece of evidence of this problem is the issue of persistence. As seen in section 3.2., there are different specifications used here with different theoretical justifications and different consequences for optimal policy. Whereas it is quite clear what Calvo pricing is and what it implies, there seems to be no consensus on what causes inflation persistence and how it should be modeled.

Fuhrer (2009) concludes that reduced-form persistence has changed over time, giving additional weight to the argument that it should be modeled from explicit microfoundations. Fuhrer quotes Barsky's (1987) evidence that during pre-World War I gold standard there was virtually no persistence in inflation. He then examines post-World War II data and concludes that persistence has diminished in the past few decades. This seems to be in line with the general narrative where central banks regained control of the inflation process after the period of "Great Inflation" of 1965–1982 (Bordo & Orphanides 2008).

Fuhrer (2009) also examines the sources inflation persistence in a framework where he separates "intrinsic" inflation persistence, which arises directly from price setting, and "inherited" inflation

persistence, in which the sluggish behavior of the driving process of inflation, which includes among other things output growth, is transmitted into inflation. He concludes that the persistence of the driving process has changed little over the sample period (1966–2008) and so it is likely that the change in reduced-form persistence is the result of some changes in the price-setting process. Although micro evidence gives some direction, the sources of this change are largely unknown. Within the standard framework utilizing Calvo pricing this could be due to rising frequency of price changes, a lower degree of indexation or diminishing role of rule-of-thumb price setters. Outside this framework it could be due to less frequent changes in the central bank's target inflation rate or better knowledge of the central bank's preferences due to increased transparency (this issue is discussed in section 4.2.2). It seems clear that more research is needed, especially at the micro level, on the microfoundations of nominal rigidities.

4.1.2. Time inconsistency

This section does not deal with the obvious ways in which time inconsistency is a relevant concept in macroeconomics and monetary policy. It does not discuss how monetary policy makers could commit better, nor does it discuss how time inconsistency may affect evaluations of past data. The key point in this section is that modeling monetary policy decision making in general and time inconsistency in particular is difficult, though in my view it is clear it should be given more effort.

Let us first define the concept. Klein (2008) writes "A (possibly time- and state-contingent) strategy is said to be time inconsistent if an agent finds it optimal from the point of view of some initial period 0 but finds it suboptimal in some subsequent period t." The drop in the optimality ranking of a strategy can of course be due to a change in preferences, as in fiscal policy conducted by changing coalitions (Persson & Svensson 1989). But time inconsistency is a relevant issue even when continuity in targets is valued, as in monetary policy.

Kydland & Prescott (1977) showed that the problem of time inconsistency arises in the presence of forward-looking behavior. When making decisions the policymaker either takes expectations as given or takes into account the constraint that actual policy should match expectations. The former corresponds to policymaking where each period optimal policy is established afresh, letting bygones be bygones. This is called discretion. The latter corresponds to policymaking where a decision rule is established and followed even when better actions are available given expectations.

This is called commitment. It is straightforward to show (see Kydland & Prescott 1977) that discretion leads to inferior results relative to commitment.

Policymakers who are able to commit are credible in their actions. This means that expectations of the instrument path are equal to policymakers' announcement of the instrument path. For example, assume that a central bank announces it will hold rates low for an extended period unconditional of inflationary pressures, which is what a central bank facing the zero lower bound constraint ought to do (Krugman 1998, Eggertsson & Woodford 2003). If its announcement is credible, the rate's futures markets will show no expectations of rate hikes during the extended period. If on the other hand the central bank lacks credibility, the market will expect the bank to renege on its promise later on.

There are basically two ways for a researcher to account for the issue of time inconsistency. The first is to simply assume that the policymaker is either capable of committing or it isn't. This is the elementary level of analysis, which is the most popular way of addressing the issue. All the research cited in section 3.2. was conducted in this fashion. Svensson (1999), Vestin (2006) present separate results for discretion and commitment and Dittmar et al. (1999) concentrate only on the discretion case.

This analysis presents cases which are not unrealistic in the sense that they are impossible, but neither of them seems to capture the reality which the central bankers of the Western world operate and which they operate in. Consider the discretionary case. The central bank cannot commit and it knows, and public knows, and both know that the other knows and so on. In this situation the central bank will not make any promises regarding its future behavior since it knows it lacks credibility. This is clearly not an accurate description of modern central banking, which targets inflation in the medium run and speculates constantly on its own future behavior.

Of the two, in my view reality resembles the commitment solution better, as long as commitment is understood to refer to targets or, even better, target forecasts, and not to instruments. Speaking against this judgment is the considerable variability in the expectations of survey forecasters (see Johnson 2002 for an analysis of international data spanning to mid-90's) and market forecasts (see Gürkaynak & Wolfers 2005 for U.S. data from the beginning of 00's). As I will explain below, it can well be argued that this is not an issue of commitment but of information regarding the central bank's preferences and to what extent it can be said that such preferences even exists.

The other way of modeling the issue is through the concept of reputation. Barro & Gordon (1983) argue that the unfavorable discretionary solution of the previous analysis is unlikely to arise in reality because the policymaker cares not only of the current values of its target variables, but also their future paths. A central bank considering engineering higher-than-expected inflation weighs the benefits – lower unemployment in the short run – to costs – a rise in inflation expectations which, assuming a natural rate of unemployment, corresponds to an upward shift in the Phillips Curve. Whether the result resembles more the discretionary or the commitment case depends mainly on the policymaker's discount rate. The higher the discount rate, the more valuable the present is relative to the future and the more the result resembles policymaking under discretion. This approach is rarer and the determination of reputation is not well understood. For these reasons I will concentrate on commitment and discretion as a binary choice.

I have two objections against the concepts of commitment and discretion as traditionally understood. Firstly the assumption of complete information, that the public knows what the central bank wants to do and the public knows the same things about the behavior of the economy as the central bank does, is of course false. Assumptions must be made to be able to model complex economies, but this assumption yields strikingly false predictions regarding the actual practice of monetary policy. The great role given to communication of monetary policy (Woodford 2005a) and the existence of a profession of "Fed Watchers" or "ECB Watchers"[9] is very salient. Although these professionals probably help reduce the uncertainty related to future central bank actions they do not eliminate it (Berger, Ehrmann & Fratzscher 2011).

Secondly, in my view the issue of time inconsistency has not made economic models much better. The Kydland-Prescott result has been extremely influential and important and it has probably made central banking better by making it more consistent and future-oriented, but commitment is difficult to model intelligently. This is mainly due to the fact that it is, as traditionally understood, an either-or issue. And not only is it an either-or issue, it is a variable which cannot be directly observed and the determination of which is largely political and institutional. Non-linearity, non-observability and extraeconomic determination; all of these are characteristics of a variable economists find it easier to take as a given. And so it has been taken, and that which is only assumed cannot be studied.

Mishkin (2004) describes how the previous secretive practices of central banks may actually have been attempts to solve the time inconsistency problem by shrouding itself from parliamentarians,

[9] These are journalists and financial market participants trying to obtain valuable information from the signals policymakers send, which include also its policy actions.

who have a shorter time horizon than is optimal for monetary policy.[10] Mishkin (2000) testifies from his own first-hand experiences that central bankers are "extremely adverse to falling into a time-inconsistency trap", but that time inconsistency issue still arise from political pressure. Thus communication is very relevant to this issue of time inconsistency, although this is a peculiar example. A more general example could be seen in the reluctance of central banks to accommodate negative supply shocks – an inflation-targeting central bank may feel its credibility would diminish if it were to lower interest rates in the face of an oil price shock even though this is generally regarded as the appropriate course of action.

In section 2 I went through some of the proposed solutions to account for the lack of persistence in the most common Phillip Curve models. Erceg & Levin (2003) show how one need not relax the assumption of rational expectations or rely on an ad hoc modification of the supply relation[11], as the most popular models do. In Erceg & Levin, "households and firms use optimal filtering to disentangle persistent shifts in the inflation target from transitory disturbances to the monetary policy rule". Thus the public faces a signal extraction problem.

A more realistic, and in my view more useful approach is to view the central bank not as a single central banker with a well-defined utility function but as an institution headed by people with diverse views. In this view the concept of information becomes crucial. How should the central bank communicate? How should the public interpret its communication? Attempts to answer these questions are discussed in section 4.2.2.

4.2. Problems in Unsolved Issues

In most analyses of optimal monetary policy, there is a great divide between the researcher and her research. Although the researcher herself might have doubts and caveats about her model and its implications, which she expresses in footnotes and seminars, the agents within her model are utterly confident about the model and have no reservations about using it to forecast the future and maximize utility. It is the study of departures from this that I review in this section.

I divide this section in two parts and I hope this will clarify rather than muddle the issues at stake. Section 4.2.1. reviews the literature conducted under the concept of robustness, which concerns

[10] Mishkin (2004) acknowledges that central banks are also bureaucracies maximizing power and prestige by avoiding accountability.
[11] Ball (1995) is an early model in this vein. Usually these models relate to central bank targets, but in Walsh (1999) the central bank also has private information about the workings of the economy and not just its own utility function.

itself with optimal monetary policy when the policymaker is uncertain about the true model of the economy. Section 4.2.2. reviews the literature conducted under the slightly misleading term "imperfect credibility". Before proceeding I wish to present some general ideas about how these themes relate to each other.

In the words of Sargent (2008), "[R]ational expectations is an equilibrium concept that attributes a common model (a joint probability distribution over exogenous variables and outcomes) to nature and to all agents in the model." The research in this section is about explicit departures from rational expectations. In the robustness literature, the policymaker does not know the true model, i.e. the model of "nature". In the imperfect credibility literature, the public does not know the policymaker's incentives. Of course in reality both are likely to apply simultaneously – that the central banker is trying to figure out how the economy works (e.g. how do the price setters form expectations) and the public is trying to figure out what the central bank wants to achieve (e.g. how much it dislikes inflation).

4.2.1. Model uncertainty

In this section I review some of the literature that models a policymaker who is uncertain of the structure of the economy and wishes to account for this uncertainty in a formal way. I will argue that although there are different approaches to robustness analyses, they can be seen as sharing the same structure in terms of problem-solving. It is informative, however, to first discuss why we have such uncertainty and how it is accounted for non-formally on a very general level.

We do not have an accurate theory of how the economy works nor do we have all the required data available to us. We build models which are simplifications of reality and try to make do with the data that is available – to give an example, the output gap is a crucial but a non-observable variable, so different proxies are used in its stead in empirical studies. Even if our knowledge of the structure of the economy gets better in time, policymaking cannot wait. It is also noteworthy that uncertainty is not merely a first-order problem. Not only is there uncertainty about the true structure of the economy, there is also uncertainty about how wrong our models are, uncertainty about how well we can test our models and so on. A more formal categorizing of types of uncertainty is given later.

In my view there are two general attitudes towards model uncertainty in monetary policy, the first of which is somewhat reasonable and second of which is likely to be wrong, although it seems

intuitively plausible. The attitudes are not mutually exclusive. The reasonable approach is to develop multiple models separately and test the behavior of different policies in these different models[12]. This could be called "crude robustness analysis". An analysis such as this was discussed in section 3.2., where inflation targeting and price-level targeting were studied under both New Keynesian and New Classical Phillips curves of sorts. This is a reasonable approach and the formal robust analyses discussed below are actually not that different in principle. The difference, and this is an important difference, is that in this crude robustness analysis no explicit probabilities are given to different models.

The second attitude is that of caution. There is a conventional wisdom, presented formally in Brainard (1967), that when in doubt, policymakers should dampen their instrument movements. This is also quite intuitive; when in doubt, the risks of doing too much seem to outweigh the risks of doing too little. Giannoni (2002), however, reaches an opposite conclusion: greater uncertainty leads to greater responsiveness for Taylor rules.

Caution in the face of uncertainty is also evident in ECB's thinking. Solans (2000), speaking as a Member of the Executive Board, said that "most central banks favour smoothness, especially in a scenario of uncertainty, for reasons of caution, consistency and credibility - the "three letters "C" argument", as we could call it". Duisenberg (2002) justified a policy decision at a press conference in the following words [emphasis added]: "As far as the balance between upside and downside risks is concerned, we believe that the risks are still somewhat on the upside, but *the uncertainties are too great to come to decisions already at this stage*." It is clear that the ECB considers uncertainty to lead to more muted responses to disturbances. Cogley & Sargent (2005) argue cautiousness in the face of model uncertainty can help explain the double-digit inflation rates of 1970s in the U.S.

The world is a complicated place. Economists build models which are tractable simplifications of it in order to predict and explain its dynamics and to make policy recommendations. In robustness analysis a model is best understood as a specification of a probability distribution over a range of possible relations between outcome variables and decision variables. With model certainty the probability distribution would collapse into a single function giving the true structure of the economy.

[12] The ECB's two pillars-approach to the analysis of risks to price stability can be seen as expression of this approach. As Issing (2003) writes "[T]he two-pillar framework makes explicit the complexity and uncertainty confronting monetary policy-makers and facilitates the cross-checking of information and complementary analytical perspectives." The argument is presented and justified formally in Beck & Wieland (2007).

Since a model is a specification it needs to be specified. This means defining what is known for certainty and what is not known for certainty. Let me give an example, or rather frame a familiar model in a new way. The New Keynesian Phillips Curve with an i.i.d. error term is defined as

$$\pi_t = \kappa(Y_t - Y_t^n) + \beta\pi_{t|t+1} + \epsilon_t.$$

The error term clearly signifies uncertainty: at any given period, we do not know the true functional form of the economy – this is the uncertainty. On average the functional form is the deterministic part on the right-hand side – this is what is known. And we also know – and this is important – that the degree of uncertainty is defined by the variance of the error term – this is what is known about what is not known. All problems of uncertainty in economics include the three elements: what we know, what we do not know and what we know about that which we do not know.

Batini et al. (1999) identify four types of uncertainty the policymaker faces: (i) temporary shocks, embodied as error terms in equations, as above, (ii) parameter values linking variables in the economy, i.e. the coefficients estimated in empirical works, (iii) the state of the economy (all the relevant variables are measured timely and with certainty) and (iv) the functional form of the economy. When the only type of uncertainty allowed is (i), uncertainty is said to be additive and a well-known certainty equivalence result holds: optimal policy is the same with or without this uncertainty.[13] This result is utilized in most studies of general targeting rules, including those discussed in section 3.2.[14]

Unfortunately all uncertainty is not additive. This is implicit in the literature discussed in section 3.2., where optimality results were calculated for different types of Phillips Curves – type (iv) uncertainty – and for different values of persistence parameters – type (ii) uncertainty. How does one deal with these types of uncertainty analytically? There are, in my view, two ways to do this: a non-formal way, which was already mentioned above, and a formal way. The most important difference between these two is that in the latter approach uncertainty is made more explicit and an unambiguous decision criterion for optimal policy is established. The same elements are however present in the former approach, which was taken in section 3.2, as well. Here a range of models (New Keynesian Phillips Curve and the New Classical Phillips Curve) with varying parameter values (for persistence) was established. There is a robustness criterion, but it is fairly restrictive:

[13] Uncertainty still affects welfare, naturally, since people are in general risk-averse and prefer certainty over uncertainty.

[14] Actually the certainty equivalence result holds strictly for backward-looking models, where the problem of time inconsistency is absent. Analogous results can however be derived for forward-looking models (Svensson & Woodford 2006).

the superiority of one policy over the other for the entire distribution of models and parameter values. As seen in section 3.2, neither policy (inflation targeting nor price level targeting) is optimal for all possible models and parameter values considered there.

The advantage of a formal analysis is that by being more explicit about uncertainty and the optimality criterion it is more probable to find an optimal policy for a given problem. I'll review the basic principles of the most popular technique of formal robustness analysis in economics, Hansen-Sargent robustness, and try to avoid technicalities. One reason for doing this is that I wish to highlight the similarities in objectives and thinking between formal and non-formal approaches to uncertainty. The presentation follows Williams (2008).

Policymaker has in mind the following nominal model of the economy.

$$x_{t+1} = Ax_t + Bi_t + C\varepsilon_{t+1},$$

where x_t is the state variable, such as inflation, i_t is the control variable, such as interest rate and ε_{t+1} is an i.i.d. shock. The nominal model is perturbed with a misspecification shock w_{t+1} which is used to represent alternative models and which may or may not be correlated with the state variable

$$x_{t+1} = Ax_t + Bi_t + C(\varepsilon_{t+1} + w_{t+1}).$$

The degree of misspecification is bounded by the parameter η, with

$$E_0 \sum_{t=0}^{\infty} \beta^t w'_{t+1} w_{t+1} \leq \eta.$$

Agents' preferences can be defined as a function of state and control variables

$$E_0 \sum_{t=0}^{\infty} \beta^t (x'_t Q x_t + i'_t R i_t).$$

The minimax approach can be represented as a game with the first player – "nature" or "alter ego" – choosing the perturbation process $w$so as to minimize the agent's utility, after which the agent chooses the control process i so as to maximize utility, given w.Technically this is represented by a Bellman equation for a two-player zero-sum game:

$$V(x) = \max_i \min_w \left\{ \left\{ x'_t Q x_t + i'_t R i_t + \beta\theta w'w + \beta E\left[V\left(Ax + Bi + C(\varepsilon + w)\right) | x \right] \right\} \right\},$$

where expectation is taken over the (Gaussian) shock ε and $0 < \theta < \infty$. The parameter θ, which can be thought of as a Lagrange multiplier for the bounded misspecification process. As in basic

consumer optimization, this parameter can be thought of as a “shadow price” or a marginal cost of the constraint. In this case it can be interpreted as a preference for robustness. The obvious question is how should this be determined? Hansen & Sargent (2010) suggest using past data to determine this parameter by determining which models within the constraint are difficult to distinguish. In the end though the minimax approach is a worst-case analysis with the worst possible perturbation determining the best possible policy.

Another possible approach in robustness analysis is Bayesian analysis. Here instead of a nominal model the researcher specifies a probability distribution representing her priors about the economy. Utility is then maximized over this probability distribution. Sims (2001) gives a Bayesian critique of the minimax approach. A policy maker maximizing utility with respect to the least favorable prior is not acting rationally. Sims argues that although modeling agents as non-rational may sometimes be warranted, positive analysis should be based on rational behavior. To use Sims’ example, we may sometimes postulate that a consumer buying a dish washer may face some computational limitations and thus act non-rationally, but we should expect our monetary policy to be conducted rationally. A related point is that by defining uncertainty as bounded disturbances around the nominal model the minimax approach may give undue weight to relatively minor (in terms of welfare) sources of uncertainty while ignoring more difficult issues.

On the flip side of these criticisms are the advantages of the minimax approach. Bayesian analysis is more strenuous as more parameters (the priors relating to different models) must be specified. From a normative point of view it is possible to argue that the object of monetary policy, as many other public policies, should be the prevention of disaster scenarios.

The Hansen-Sargent technique is in the tradition of optimal control. It has been argued in this thesis that the focus of the study of optimal monetary policy should be in the analysis of different simple rules, rather than deriving optimal instrument setting for a given circumstance. The robust control analysis does nonetheless offer an example of a way to analyze uncertainty, which can be useful also in the study of simple rules.

To once again take the issue of price level targeting vs. inflation targeting as an example, it was seen in section 3.2 that there are many issues over which there is considerable uncertainty. In both the minimax and Bayesian approaches a range of possible models would first be specified. In this case the range could be NKPC and NCPC with different degrees of persistence.

There is a clear counter-argument against the use of formal robustness techniques in the study of simple rules. Part of the reason why simple rules are studied is that they are often more robust to model specification errors (Levin, Wieland & Williams 1999). It seems suboptimal to first restrict one's analysis on the basis that this restriction helps overcome (at least some) robustness issues and then test for robustness within this restricted set.

Dealing with uncertainty is an unsolved issue in the study of optimal monetary policy in the sense that there is no set of "best practices", no principal method of analysis. This is not a technical issue. There are multiple techniques available and they produce similar results for given problems (Walsh 2004), as should be expected.[15] In my view the problem is that there is no agreed approach to uncertainty on a more general level. In section 5.3 I give my own thoughts on how this issue could and should be addressed.

One important issue I have not discussed with respect to robustness analysis is welfare. The study of optimal monetary policy usually begins with the quadratic loss function. As is often noted, this can be derived from microfoundations. If the target, the utility function, is derived from within the model or the framework, uncertainty about this model creates uncertainty about the target as well. This is the case with the celebrated quadratic loss function, which can be derived as an approximation to the utility function of households in the New Keynesian Phillips Curve framework.

4.2.2. Imperfect credibility and transparency

In this section I will review some of the literature that studies the consequences of the public not knowing for certain the objectives, incentives and view of the economy of policymakers.

Credibility is the equality of expectations regarding the policymaker's actions and the policymaker's announcements of its own actions[16]. The term "imperfect credibility" is slightly misleading, for it is imperfect information which is more relevant here in my view. With perfect information, credibility is a binary variable; you either have it or you don't. The public knows the policymaker's incentives and it knows whether it can or cannot commit. Either the policymaker

[15] Although there are fundamental differences between the minimax and the Bayesian approaches to robustness the procedures by which uncertainty is bounded can be thought of as creating priors, as in Bayesian analysis (Hansen & Sargent 2007).
[16] Blinder (1999) reviews the many ways in which the term is used in economics, and favors the one used here.

optimizes at time t_0 and follows the optimal path (commitment) or it optimizes afresh each period (discretion). Only in games of imperfect information can the actual policies taken resemble something lying between the commitment and the discretion solutions.

Basically in the imperfect credibility approach the public tries to extract the central bank's utility function and assessment of the economy, and their changes, from the bank's actions and communications. The more transparent a regime is, the more successful the public is at this task[17]. But why would the central bank's utility function or its assessment of the economy be imperfectly known? Why would the central bank want to be opaque? I can think of three sets of reasons.

Firstly there is political economy. Mishkin (2004) quotes an anonymous Fed official giving two rationales for opaqueness in central banking, both of which are issues of political economy and both of which are based on opaqueness shielding the central bank from political oversight and pressure. The public choice explanation for this is that the central bankers wish to maximize their own power and prestige by avoiding accountability. On the other hand it can be argued that most politicians have shorter time horizons than is optimal for monetary policy and thus the central banks are acting in the public's interests in resisting accountability.

Secondly there may be informational concerns. There are many models in which complete transparency is not optimal, and the degree of optimal transparency is affected by informational asymmetries between the central bank and the public. The seminal article in this tradition is Cukierman & Meltzer (1986).

Thirdly, and perhaps most importantly, complete transparency is both impractical and impossible. Mishkin (2004) argues against complete transparency on the grounds that it would be difficult to implement within the current structures. A related note is that in any monetary regime which includes discretion, a human element, complete transparency is impossible[18]. The central bankers' loss functions are crucial information for the public, but in reality central bankers do not have loss functions defined over the entire range of possible states of the world.

For example, the ECB has a clear preference for price stability, which it hammers into the public in its monthly press statements. Despite this there is great uncertainty over a large range of

[17] Faust & Svensson (2001) say transparency "is connected to" how easily the public can deduce central bank goals and intentions from observables, but I would define it as simply being that.

[18] It is noteworthy that in regimes where there seems to be no room for discretion, such as a fixed exchange rate, this theme is still relevant. In these regimes there is no uncertainty about behavior within the regime, but there is uncertainty about the fate of the entire regime. It would be foolish not to take into consideration how a prolonged period of high unemployment strains monetary policy regimes which are unable or unwilling to do anything about it.

possibilities. What would the ECB do if, for example, the interest rates on Italy's government bonds rose sharply? How would this depend on actions taken by national governments? Purchases of sovereign bonds can be justified and are justified from a simple inflation-targeting viewpoint as disruptions in these markets affect the monetary transmission mechanism (González-Páramo 2011). Thus even though there would be no uncertainty over ECB's preferences there could be uncertainty over its actions since it is not clear how the ECB views the monetary transmission mechanism and how it is affected by sovereign bond markets.

Whatever the reasons, there is no complete transparency in monetary policy anywhere and this has an effect on designing and studying monetary policy. Most importantly it casts doubt on the literature which assumes that public has perfect information regarding the central bank, employed in the example given in section 3.2.

A key task in the imperfect credibility literature has been explaining the behavior of inflation in the Volcker disinflation (Erceg and Levin 2003, Goodfriend & King 2005). Ball (1990) also touches upon the Thatcher inflation, though the empirical discussion in his article is quite brief. These articles, unsurprisingly, claim that the fact that the Volcker disinflation was accompanied by the Volcker recession can be explained purely or mostly by imperfect credibility, and that thus there might not be anything to fix in models of price-setting.

The issue of imperfect information can be illustrated by looking more closely at monetary policy decision making. An interesting recent article is Ball (2012). He uses the social psychology literature on group decision-making to study why the policies Ben Bernanke advocated in 2000-2003 as a remedy for an economy depressed at the zero lower bound were not implemented in the recent recession when Bernanke was the Fed chairman, economic output was depressed and the fed funds rate was at the zero lower bound. To review the article, Bernanke as a professor and Fed Governor wrote extensively on the subject in the beginning of 00's as Japan faced a similar situation, sometimes called a liquidity trap, where the conventional monetary policy instruments are inoperative. Bernanke (quoted in Ball) wrote:

> [Japan's] economy has operated below potential for nearly a decade. Nor is it by any means clear that recovery is imminent. Policy options exist that could greatly reduce these losses. Why isn't more happening? To this outsider, at least, Japanese monetary policy seems paralyzed, with a paralysis that is largely self-induced.

Specifically, Ball (2012) lists the following policy proposals Bernanke mentioned at separate occasions that would stimulate the Japanese economy: intentional currency depreciation, which Svensson (2003) calls the "foolproof way to escape a liquidity trap"; targets for long-term interest rates; money-financed tax cuts, which are colloquially known as a "helicopter drop" of money and higher inflation. Ball then goes on to discuss the policies actually taken by the Fed from December 2008 (when the Fed funds rate hit the zero lower bound) forward, which were limited in their effects and quite conventional compared to Bernanke's earlier proposals – though it is fair to admit that the expansion of the Fed's balance sheet in the crisis was quite unconventional by historical standards. Ball then tries to trace how and why Bernanke's views on the matter changed. He finds plausible explanations from the Fed's groupthink and Bernanke's personality.

Ball's analysis is very far from the conventional optimal monetary policy framework, but it is nonetheless worth a thought. A central assumption in economists' analyses of monetary policy is that there exists a well-defined central bank utility function, which takes as its inputs economic variables. This is true even in the imperfect credibility literature reviewed above, which maintains that although such a function is not perfectly known to the public, it still exists.

Ball's analysis echoes Milton Friedman's critique of the use of loss functions in monetary policy analysis. Friedman, in his comment to Fischer (1990, comment in the article) concentrates on the individual psychology of the policymaker from a public choice perspective:

> "The major comment is the omission of what I have increasingly come to regard as Hamlet on [the rules vs. discretion] issue, namely the public choice perspective. To illustrate, …you talk about a loss function for 'the policymaker' that includes solely inflation and the deviation of real output from a target level. If we bring this down to earth, these are likely to be only very indirectly related to the real objectives of the actual policymakers. From revealed preference, I suspect that by far and away the two most important variables in their loss function are avoiding accountability on the one hand and achieving public prestige on the other. A loss function that contains those two elements as its main argument will I believe come far closer to rationalizing the behavior of the Federal Reserve over the past 75 years than one such as you have used."

It is also worth noting that Friedman speaks of 'the policymaker' when speaking of the research he critiques and of 'actual policymakers', in plural, when speaking of the way policy is actually

conducted in his view. Monetary policy decisions are rarely if ever made by a single policymaker. The inner institutional design of the central bank – i.e. not relating to the central bank's relation to other institutions, such as the parliament – matters (see Blinder 2007 and the references therein).

This question tests the limits of economics as a discipline. Economics is a method-driven science. It is possible for individual researchers to employ methods and concepts borrowed from say, social psychology, but such research is likely to be poorly peer-reviewed, if it is peer-reviewed at all – Ball's (2012) is a working paper. Thus it is unlikely to see much research dealing with these issues without a methodologically separate research program, whose results would be difficult to compare with those of mainstream research.

It is a sad fate if something as relevant to monetary economics as making monetary policy decisions would be left in dark because of the limits in the discipline's methods. A possible solution which does not entail giving up methodological uniformity, which is undoubtedly one of the main strengths of economics, is actively shaping the reality so that it could be studied more easily. These issues are discussed in the next section.

5. Discussion: different ways forward

In section 4 I discussed some of the questions which are not taken into consideration in the literature on simple monetary policy regimes, which was discussed in section 3. In section 4.1 optimal policy conducted under uncertainty over the true model of the economy was discussed. There is evidence that even small departures from the policy maker's true model may result in large losses for the policy maker's utility. In section 4.2 the issues of microfoundations, credibility and imperfect information were discussed. Relaxing the assumption of perfect information provides plausible explanations for the phenomena the Phillips Curve literature has struggled to explain, such as inflation persistence and its variability across time and regimes.

In my view, there are five different ways to develop the interaction between theory and policy in central banking, and in the study of optimal monetary policy regimes in particular, all of which have their own strengths and weaknesses. These approaches are discussed in turn below. I have ordered them so that the ones which are most in line with current practices and thus most likely to be pursued are discussed first, followed by those which represent more exotic paths with greater prizes, greater uncertainty and greater dangers.

Sections 5.1 and 5.2 develop arguments for and against the idea that macroeconomists and central banks should devote their resources into finding realistic microfoundations and deriving from them an empirically accurate model suitable for policy analysis. Section 5.3 discusses what role uncertainty and its analysis should play in research and policy. Section 5.4 tackles the issue that there probably exists important human behavior which economics is ill-suited to study. Section 5.5 offers some thoughts on a difficult subject, the performativity of economics.

5.1. Simple, broadly interpretable models

The focus of this thesis has been in how different policy regimes, such as inflation targeting and price level targeting, can be analyzed. To do this properly, one would need a microfounded model satisfying the Lucas critique, which is difficult to establish. Yet it is high time to seriously and critically contemplate the extent to which this sort of analysis is needed, possible or useful. There is an alternative. Instead studying the properties of different regimes in different models, a country could keep the regime it has or choose the regime that seems to have performed relatively well and then focus on achieving the best possible performance within that regime.

It is easier to think about this question by first thinking about the empirical literature on inflation targeting. Inflation targeting is considered to be a successful policy regime in that the countries that have adopted it have experienced better inflation and output stability performance relative to non-targeting countries. Ball & Sheridan (2005) are critical of this description and argue that this is simply regression to the mean; targeters performed worse than non-targeters prior to implementing inflation targeting. As they say, "[J]ust as short people on average have children who are taller than they are, countries with unusually high and unstable inflation tend to see these problems diminish, regardless of whether they adopt inflation targeting. Once we control for this effect, the apparent benefits of targeting disappear." This conclusion is however disputed by Batini & Laxton (2007) and Vega & Winkerlied (2005), who use a larger set of countries.

In any case it seems that what is common for well-performing policy regimes is a nominal anchor, which the inflation targeting regime provides through an explicit numerical target for inflation. Inflation targeting is associated with not just an inflation target but also transparency and accountability, which are features available to other possible regimes as well. Also, it must be noted that the most important central bank in the world, the Federal Reserve, has performed relatively

well even without an explicit nominal target. Mishkin & Schmidt-Hebbel (2007) call Alan Greenspan the Fed's "strong nominal anchor" and speculate whether the bank would keep performing well under a less adept chairman.

Two important points arise from this discussion. Firstly, the importance of the choice of the nominal target for the performance of monetary policy is dwarfed by larger institutional questions. What matters more than the choice of the nominal target is how effectively the central bank can achieve its nominal objectives. Countries are constrained in their choice of monetary policy regime by two major factors. A non-independent central bank cannot commit to a nominal target, because in such a case the price level is determined by the fiscal authority's choice of seigniorage revenue. The feasibility of a nominal target is affected also by the short-run tradeoff between inflation and output, at least in a democratic country. A supply shock, such as a rise in oil prices, puts upward pressure on both inflation and unemployment. The pressure on unemployment is easily channeled into a pressure on the central bank to stimulate the economy despite the rise in inflation. Beside supply shocks, financial instability may impair policymaker's ability to implement any policy, at least if it is not willing or able to expand its set of policy instruments.

It is true that the greatest recent advances in monetary policy have come about by adopting a policy which anchors expectations regarding the future path of a nominal variable, but does it flexibly. It is likely that monetary policy practices will continue to converge in the future, because good policies are easy to copy. There are, however, open questions within the inflation targeting regime. A nominal target is a prerequisite for a determinate monetary policy regime, but it does not ensure optimal stabilization. How should the central bank respond to supply shocks? Should the central bank care about financial conditions, and if so, how?

The performance of most monetary policy regimes is dependent on the accuracy of the forecasts it generates.[19] Thus in these regimes policy performance can always be enhanced also by developing better forecasting techniques. If central bank resources are finite and the same resources can be applied either to developing a better policy or developing better resources, the central bank faces a trade-off between these two.

[19] Exceptions to this are regimes targeting only one variable, such as exchange rates or the growth of the money supply

5.2. More complex models

The research program behind the NKPC, one of the most prominent macroeconomic theoretical equations, has largely been driven by the need to create an empirically successful model with consistent and realistic microfoundations. As it stands the result seems half-baked: to make it more empirically successful some non-microfounded elements have been added in many analyses, at least partly defeating the purpose.

The first problem is that everywhere one looks there seems to be opportunities for more realistic micro structures in macro models. The depiction of financial markets as a "black box", simplistic price-setting behavior and a general lack of heterogeneity of agents and their behavior are only the most salient deficiencies of the microfoundations of the NKPC and other common pieces of macro models. If one were to interpret the Lucas critique literally the road ahead seems long indeed. What would be the consequences of better microfoundations?

There is a tradeoff between realism and tractability in economic models. Broader models are more realistic – or at least they have the potential to be more realistic – than smaller models, but they also make it more difficult to inspect model dynamics and derive policy prescriptions. Although empirical work is not a focus in this thesis, it should also be noted that broader models in general contain more variables that need to be estimated or proxied. Although these biases are likely to cancel each other out, at least in very large models, they are all in the end sources of uncertainty and variance.

In the next section I present arguments why devoting resources to create models with better microfoundations may not be optimal. The policymaker could instead pick a plausible nominal target and focus on conducting the best possible policy within that regime and in doing so the policymaker need not worry about microfoundations and instead use the best possible forecasting models. To end this section I want to note that the approach of Gordon (2011) can be seen as occupying the middle ground between these two approaches.

Gordon presents his view of post-1975 research of the Phillips Curve as that of two separate strands, the first of which is more micro-oriented and is represented by the NKPC and the second of which is more empirically oriented and is represented by the "triangle" model which includes

explicit supply shocks as explanatory variables.[20] The NKPC fares better for some times and environments (such as ends of big inflations, as documented by Sargent 1982) and the triangle model fares better for other times and environments (such as postwar US, as tested by Gordon). Gordon then argues that "the two approaches need to pay more attention to each other and to engage in a dialogue about which models apply to which episodes, and what factors would motivate a shift in relevance between the alternative models". Thus Gordon advocates that instead of having a single, all-encompassing model with microfoundations we should have two or more different models for different episodes with the micro understanding relating primarily to determining which models suits the current environment.

5.3 Development and application of robustness analyses

In section 4.2.1 I argued that there are two common approaches to uncertainty: muted policy responses and deriving optimal policy for multiple models. In this section I discuss two approaches that, in my view, build on the intuitions behind the approaches presented earlier.

Policy makers have certain attitudes toward uncertainty and when these attitudes are stated explicitly these attitudes and their justifications can be analyzed. A good example of this is the ECB's "two-pillar" strategy. The two-pillar strategy employs two analytical frameworks to assess the outlook for inflation over different horizons. The first pillar, economic analysis, uses a large number of indicators and models to predict inflation pressures over the short-to-medium-term. The second pillar, monetary analysis, uses the link between money and prices to examine these pressures over the medium-to-long-term.

Issing (2006) offers a retrospective account of the reasoning behind the choice of the two-pillar approach. The reasoning is basically based on two empirical regularities and uncertainty about their relation. The first is the long-run connection between inflation and money growth. The second is that this connection is not very clear in the short term and is affected by multiple factors, such as financial conditions and the price of oil.

[20] It may be useful to elaborate why this is actually quite a relevant difference. The NKPC in its theoretical form has inflation expectations and these need to proxied since there is no natural direct measure available. The usual solution, according to Gordon, is to use instrumental variables or 2SLS. To be theoretically consistent the first stage estimation can only include variables in the second stage equation, i.e. inflation and unemployment gap. Thus there is no role for explicit supply shocks in the NKPC. In the triangle model there are no expectations as explanatory variables.

Assenmacher-Wesche & Gerlach (2006) decompose inflation time series into a low-frequency and high-frequency components, the first of which corresponds to a "monetary pillar" in that inflation is determined by money growth, output growth and long-term interest rates and the second of which corresponds to an "economic pillar" in that inflation is determined by the output gap. They then show how these different causal variables have different explanatory power (coefficients and t-statistic values) at different frequency ranges. Although this is not a sufficient justification for a separate policy "pillar", as acknowledged by the authors, it does point to a role for money in monetary policy analysis (which, perhaps surprisingly it does not usually have). Woodford (2006) however argues that these phenomena are consistent with a standard New Keynesian model in which money has no causal role. According to Woodford the "cross-checking" conducted by the ECB may actually be deleterious:

> "-- there is at present little reason for the quest for such a robust framework to devote much attention to questions such as the construction of improved measures of the money supply or improved econometric models of money demand. For there is little intelligible connection between those questions and the kinds of uncertainty about the effects of monetary policy that are the actual obstacles to the development of more effective, more reliable, and more transparent ways of conducting policy"

How should uncertainty be addressed at the level of science and models, as opposed to practical monetary policy? The research reviewed in section 3.2 can be seen as addressing the uncertainties about the true functional form and parameter values of the models, but as such it seems quite inefficient. Why have multiple articles studying optimal monetary policy in a single model when one could have a single article studying optimal monetary policy over a range of models?

An important feature of robustness analyses is that ideally it is a method concerned with uncertainty, not disagreement. Robustness analysis is not a substitute for good, thorough modeling. In section 2 two different ways of incorporating persistence was introduced: a lag of the left-hand-side variable and an autocorrelated error term. There is no consensus here, and I argued that this is because there is no consensus about the microfoundations which would bring about such persistence, leading to "quick fixes" in econometric specifications first and in theory after that. In my view, and this view of robustness is in the spirit of Hansen & Sargent, the primary focus of robustness analysis should be in small disturbances around a single, nominal model. It is the task of positive theory to establish such a model and the task of econometrics to test the model and estimate

its parameters. Due to data limitations there would still be uncertainty about parameter values, but ideally there would be no disagreement.

5.4. New sources of data

Sometimes the fact that there are no plausible models causes problems. Sometimes it is the opposite: sometimes there are multiple plausible models which fit (some range of) past data. An example of such a period is the Volcker disinflation.[21] The disinflation was costly, contrary to the predictions of the basic NKPC, as unemployment rose significantly for a prolonged period. There are two plausible explanations for this. One is that agents in the economy are backward-looking and form expectations of future inflation by looking at past inflation. The other is that the policy announcements made by the Fed indicating it was going to reduce the level of inflation substantially over a short period of time were not unanimously believed.

Models based on both these explanations are plausible when tested with macro data (on sticky prices, see e.g. Fuhrer & Moore 1995, on imperfect credibility see e.g. Erceg & Levin 2003). They have, however, starkly different assumptions and they thus give rise to possibly different policy prescriptions. Price setters' backward looking behavior is usually taken as given[22] meaning that disinflations will be costly no matter what the central bank does. Imperfect credibility on the other hand implies that there is room for improvement in monetary policy. It is suggested that transparency can improve credibility, thus lowering the costs of disinflations.

A noteworthy feature of models that rely on imperfect credibility, such as Ball (1995), Erceg & Levin (2003) and Goodfriend & King (2005), is that they are all theoretical or simulating models, i.e. they do not actually use any data relating to the imperfect credibility of the central bank, rather they postulate a learning mechanism for the price setters and then calibrate this to fit past data. It is arguably difficult to find good data on this issue. It is perhaps ironic that the most used measures of uncertainty use come from surveys, a somewhat frowned upon method in economics. These measures are rarely used in scientific articles but central banks often report them in conjunction with survey expectations.

[21] The same argument applies to most other disinflations as well

[22] Although see Fuhrer (2009), where the decline in inflation persistence in the past decades is estimated to be due to price-setting behavior. In all likelihood this is at least in part a consequence of changes in monetary policy.

It should be noted that there is, however, a tradition of using surveys in economics, if not directly as inputs in models. At one point in economics there was a cottage industry in modeling nominal and real rigidities at the micro level. Multiple explanations for these were given. To generate data which could be used to differentiate between these explanations, Blinder et al. (1998) surveyed 200 firms on their price setting. Blinder (1990) presents some preliminary results of the survey and discusses the role of survey data in economics in general.

Should survey data then be used to measure and test imperfect credibility? Gürkaynak & Wolfers (2005) argue that the standard deviation of point forecasts of surveyed forecasters is best understood as disagreement and that this is a poor proxy for a more interesting variable, uncertainty. Gürkaynak & Wolfers study a novel source of data, macroeconomic derivatives, which are financial products the payoff of which is tied to macroeconomic outcomes and they define uncertainty as the state-price distribution of such a product. Unfortunately these markets, operated by Goldman Sachs and Deutsche Bank, were closed after only a few years, presumably because it was not profitable to these firms.

A final source of additional data is the narrative approach, pioneered by Friedman & Schwartz in their *Monetary History* and continued by Romer & Romer (1989). Here monetary policy shocks are identified from textual sources, such as the minutes of the FOMC. As discussed in section 4.2.2 monetary policy decisions are not made by a single individual but by a deliberative body, typically representing, at least nominally, different regions. Analyzing the decision making of this body may be of use in certain questions.

5.5. Embracing performativity

When the verisimilitude of the models used to examine optimal monetary policy is called into question, as has been done in this thesis, a natural response is to make the models more realistic. When the methods and data used to study the economy do not capture its essential features, a natural response is to develop new methods and new sources of data. There is, however, an alternative: making the reality look more like models.

Blinder (2000) writes:

> "George Stigler (1976, p. 351) wrote that "economists exert a minor and scarcely detectable influence on the societies in which they live." I don't believe that is true any longer. In fact, I want to make--and then defend-- an audacious claim that directly contradicts Stigler. Since he wrote those words, real-world economies, including ours, have changed in a number of ways that bring them into closer alignment with the pristine model envisioned by economic theory. In this curious sense, life has come to imitate art.
>
> But this essay is not meant as a piece of boosterism for economic theory. The arguments of economists, persuasive as they may have been, were not necessarily the main reason for the changes in real economies. Nor were all these changes unambiguously for the better. Nor do I claim that every previous discrepancy between the real world and the model has been resolved in the model's favor. However, I do perceive a general pattern, and it is an ironic one. One might have assumed that economists would have to adjust their models to fit reality, rather than the other way around. That's certainly the way it works in the natural sciences. Germs did not change their behavior to accommodate Pasteur's theory, nor was Mercury's orbit perturbed so that Einstein wouldn't be. But economists appear to have bent reality (at least somewhat) to fit their models. That's quite a feat."

Blinder argues that reality has come to resemble economic models. Why and how could this have happened? I can think of three reasons. First, there are the forces working in this world that have nothing to do with economists. Second, there is the application of economic models in real life markets and acknowledging policy and welfare analyses of economic models. Third, there are economists participating in the markets and commenting on policy beyond scientific research. It is often difficult to distinguish the last two.

The American sociologist MacKenzie (2006) argues that financial markets have come to resemble the models used to study them and that this is no coincidence.[23] This phenomenon he calls "performativity". He argues that this is a direct result of applying those models in real life and the modelers themselves being active market participants. His strongest example is that there are reasons to believe that the application of the Black-Scholes-Merton option pricing model and the involvement of many students of financial markets, especially from the University of Chicago faculty, lead to the improvement of the model in a predictive sense up to the 1987 crash. First the

[23] Peter Bernstein also makes that claim in his book *Capital ideas: the improbable origins of modern Wall Street*.

world drew closer to the model, only to drift away later, which testifies to the complex mechanisms involved in performativity and the possibility of "counter-performativity".

In my view the situation of monetary policy can be interpreted in similar terms. The scientific community has a large influence over policy. On one hand, this influence is exerted through scientific research and policy and welfare conclusions derived from models. On the other hand economists draw conclusions beyond models. The time inconsistency of optimal plans is a very strong result – and a very theoretical one. The Kydland-Prescott paper has had a major role in driving home the importance of commitment in monetary policy. But when Prescott (1996) says in an interview for the publication of the *Region,* the publication of the Minneapolis Fed, that "[a]n independent Fed, I think, is something that is a valuable commitment technology for Congress, for the same reason an independent judiciary is a good arrangement", he is drawing conclusions beyond his model.

The example I would give of the reality coming to resemble economic models in the field of monetary policy would be the development towards increased transparency in policy making in the last decades. The assumption of common information is prevalent in modeling monetary policy and transparency can be interpreted as the central bank sharing information about its own policy to the public. This much is clear. But why has this come to pass? I believe much of it has to with economists themselves. Although I cannot present a sufficient defense of this claim here, I will give a brief argument.

To begin with, if there has been a rapprochement of theory and practice in the issue of transparency and if it has something to do with theoreticians, the arguments are generally not based on models. This is because in almost all models of optimal monetary policy, the degree of transparency is an assumption, not a policy variable. The model-based study of the optimal degree of transparency is a footnote in the study of optimal monetary policy and in the discussion of the optimal degree of transparency.

Secondly, there are strong a priori reasons for believing that economists are willing and able to draw reality closer to models in this particular case. Able because, as emphasized many times earlier, economists exert large influence over policy. Willing because most economists are probably closer to Samuelson (1963) than Friedman (1953) on their views about the merits of unrealistic assumptions. Where Friedman claimed that realism of the assumptions is irrelevant, Samuelson was of the opinion that unrealistic assumptions are nothing but a dismerit for a model. It is pleasant if

one's model is realistic, and thus changing the world to make the model more realistic brings pleasure.

Thirdly, influential theoreticians have made strong arguments in favor of increased transparency (see e.g. Woodford 2005a) in venues close to monetary policy decision making.

How could economists influence the reality to make their models work better? Increasing the degree of transparency in monetary policy comes first in mind, for two reasons. Firstly opacity leads to ambiguity in empirical work, as illustrated in section 4.2.2, where I explained how Erceg & Levin (2003) were able to account for the observed persistence in inflation by relaxing the assumption of perfect knowledge rather giving up on the traditional price-setting models. Though it is clear that the NKPC doesn't fare that well empirically, it is difficult to say which of the model's assumptions are vitally wrong. One cannot change the past, but implementing full transparency in period t would result in "clean" data points from that period forward. Secondly most models used to study optimal monetary policy are based on the assumption of perfect information. Thus increasing transparency should result in increasing the relevance of the existing literature.

A second improvement would be increasing the degree of predictability of monetary policy. It was argued in section 4.2.2 that monetary policy is likely to be affected by individual and social psychological factors, which economics is ill-equipped to study. Reducing the influence of these factors would bring monetary policy theory and practice closer.

I can think of three objections to this line of action in research. First it may be argued that in general it is not the proper role of scientists to mould the world to suit their methods and particularly it is not why society has granted economists influence and power over economic affairs. Secondly, although the reforms might enhance the performativity of economists' models, they might still be welfare-reducing. For example, Milton Friedman argued in favor of more automaticity in monetary policy, but the Friedman k%-rule seems to be a monetary policy regime which requires a large degree of ineptitude on behalf of central bankers to be an optimal policy regime. Thirdly the strategy, especially if implemented poorly, might backfire. Increasing transparency deteriorates performance of monetary policy in some conditions, and if such deterioration were to happen in real life, it might provoke a backlash against transparency.

6. Conclusions

The study of a certain area of policy will always be intertwined with conduct of policy makers responsible for that area of policy. As such the study of monetary policy, to which much of modern macroeconomics relates to one way or the other, will always be intertwined with the peculiar actions and habits of central bankers past and present. This relation itself is rarely problematized and studied.

One thing which seems quite plausible is that the degree of consensus within the scientific community is proportional to the amount of influence it has over policy.[24] In 1978 the Federal Reserve Bank of Boston held a conference titled "After the Phillips Curve: Persistence of High Inflation and High Unemployment". Among the papers presented at that conference was one titled "After Keynesian Macroeconomics" by Robert E. Lucas and Thomas J. Sargent. This article is held up in some historical accounts as a symbol and sign of the nadir of Keynesian macroeconomics and the overreaching conclusions associated with it.[25] Lucas & Sargent write that:

> "Certainly the erratic "fits and starts" character of actual U.S. policy in the 1970s cannot be attributed to recommendations based on Keynesian models, but the inflationary bias on average of monetary and fiscal policy in this period should, according to all of these models, have produced the lowest average unemployment rates for any decade since the 1940s. In fact, as we know, they produced the highest unemployment since the 1930s. This was econometric failure on a grand scale."

In the opening remarks of the conference Frank E. Morris, the president of the Boston Fed remarked that

> "It is probably fair to say that economic policy is now being made in at least a partial vacuum of economic theory. Unlike earlier periods, no one body of theory seems to have a very broad acceptance. If Keynesianism is not bankrupt, as Messrs. Lucas and Sargent suggest, it is at least in disarray. Certainly, the confidence that I felt as a member of the Kennedy Treasury in our ability to use the Keynesian system to generate outcomes for the economy which were highly predictable has been shaken, and I believe a great many other people have also lost that confidence.

[24] For a different view, see Mankiw 2006, who argues that the influence of macroeconomics on policy depends on the policy relevance of the research and not the unanimity with it is viewed within the scientific community. From a practical point of view Mankiw concludes that the work of the past several decades looks like an unfortunate wrong turn. For a defense of practical relevance of macroeconomics of recent decades, see Woodford 2009a.
[25] These overreaching conclusions and the susceptible depiction of contemporary Keynesian macroeconomics are criticized by Benjamin M. Friedman in his comment to the article.

..

> My only problem with the rational expectations school and the Lucas Sargent paper is that they promise us a complete system ready for policy-makers in ten years. Obviously, ten years is a rather long time to wait-- I think we are all looking for a new synthesis in economic theory."

No complete system was ever delivered, but a new synthesis of a sort was eventually reached. This was not, however, based on the assumptions of perfect markets and imperfect information, as in Lucas (1972). The new synthesis, dubbed the "new neoclassical synthesis" combines elements from two very different research programs: the New Keynesian economics and real business cycle theories.

New Keynesian economics refers to a body of work done by macroeconomists in the late 1970s and 1980s in which economies with imperfect competition, nominal rigidities and real rigidities were shown to provide plausible explanations for the short-run non-neutrality of money (Dixon 2008). Real business cycle theories on the other hand assume neutrality of money and seek to explain fluctuations as optimal responses to changes in real factors, such as random changes in technology, taxes and government spending and regulation (McGrattan 2008). From the New Keynesian tradition the new synthesis has taken assumptions of imperfect competition and constrained price setting and from the RBC tradition explicit intertemporal optimization and, often, rational expectations.

Although there are fewer fundamental disagreements among macroeconomists today than there was at the end of the 1970s the new consensus is less confident of itself than the previous one. For example, although ECB uses DSGE models[26], it relies on many other models and methods in addition to these.

What are the reasons for this uncertainty? From a policy maker's perspective the two major factors are, in my view, the concept of a time-varying natural rate and the complexity of the monetary transmission mechanism. The idea that real variables such as changes in output and unemployment have natural rates which cannot be influenced by monetary factors has reduced the set of possible outcomes monetary policy can achieve.[27] Furthermore it is admitted that these natural rates are not

[26] On these see Smets et al. 2010

[27] However the possibility of hysteresis, the idea that output and unemployment gaps can affect their long-run levels, has been recognized by Bernanke 2012, among others.

constant and are difficult to measure accurately, making it difficult to distinguish cyclical and structural variation. Uncertainty about the monetary transmission mechanism is evident in the framework of the ECB, with its two-pillar approach, but this issue is acknowledged at the Fed as well. (Issing 2002, Mishkin 2007)

Policy makers are bureaucrats who in principle only conduct given policies and do not concern themselves with the choice of alternative regimes, although central banks do produce research on this issue. Thus the main focus of this thesis, comparing alternative monetary policy rules, is more a question of research than of practical policy. Here the major factor in decreasing certainty has undoubtedly been the Lucas critique, the argument that effects of a change in policy cannot be predicted with aggregated historical data.

To satisfy Lucas critique, a model should be derived completely from individuals' optimizing behavior in which policy is taken into account. Such models have not been empirically satisfactory and are often augmented with non-microfounded solutions for better performance. Particularly troublesome issues are persistence and time inconsistency, which may be related. Most microfounded models generate substantially less persistence than there actually is and time inconsistency seems to be a misleading concept in that it is not a question of credibility per se as it is of information. The resulting problems for the study of alternative policies are twofold. Firstly welfare analysis is impossible without microfoundations and uncertainty about microfoundations means uncertainty about welfare. Secondly it is difficult to analyze the behavior of the economy under different policies when the true model of the economy is not known.

In section 5 five different approaches to improving this situation were discussed.

First approach is that the Lucas critique can be sidestepped by giving up on the theoretical research of different policy regimes. The choice of a policy regime would be made based on comparative research of past and present regimes and a "trial and error" attitude. Once a policy has been chosen predictions within the policy regime could be made based on aggregated models.

In the second approach the quest for the true microfoundations is continued. The prize, a sort of "theory of everything" of macroeconomics would of course be immeasurably valuable, but it is doubtful whether it can be achieved. In any case it seems that unorthodox sources of data may be necessary to know the true micro structure. An additional problem is that as models become more complex, as they probably would in this approach, deriving optimal policy becomes more difficult.

The third approach consists of methods and procedures to cope with uncertainty and mitigate the risks it poses to policy. An optimal policy rule should perform well for a range of models, in case the policy maker's current model is not correct or the underlying model changes in time. Here the key issues are what the range of models and the criterion for model performance are. There are two different approaches for the criterion: the worst-case analysis of the minimax approach and the maximization of expected utility of the Bayesian approach. Aside from technical differences these approaches represent two different attitudes towards risk in general.

In the fourth approach new, even unorthodox sources of data will be analyzed and used as inputs in models and to discriminate between models. There are many aspects of monetary policy that are not captured by the current methodology. How do the organizational structure and the institutional environment of the central bank influence the choice of optimal policy rules? Analyzing new data would perhaps expand the methodological toolbox of economists. This would not be an entirely positive development, since economics is a very method-driven science and common methods enable researchers to comment and critique each other's' works.

The fifth approach suggests that if models do not correspond to reality, an alternative to adjusting the models, as in the second approach, is adjusting reality. It has been argued that reality has sometimes come to resemble the economic models used to study it and that economists have had a causal role in this. Economists could embrace this role and in doing so benefit the society by creating more accurate models. One possibility is increasing transparency in monetary policy: most economic models assume perfect information and increasing transparency is a way of making those models more realistic. There are of course obvious counter-arguments to such practices in science.[28]

Currently the most popular approaches are the first and the second: macroeconomists hold up microfounded models as an ideal yet recognize the need for a more practical approach. This is a sensible approach, although as the history of responding to the Lucas critique becomes longer and longer the "one true model" with only deep variables seems to be farther and farther – in the quotation of Morris above he indicates that Lucas & Sargent promised a complete system ready for policy makers in ten years.

With respect to the third approach, the existing literature on robust optimality is still quite technical and is probably of much use to practical policy, yet. There is quite a bridge to gap between practice

[28] The question of prescriptive vs. descriptive theories is an important theme in decision theory; see Tversky (1975) for a comment on the interpretation of the expected utility theory. The idea of active agency of researchers is not present in these discussions, however.

and science; between three C's of caution, consistency and credibility and solving Bellman equations of two-player zero sum games. Hopefully this gap will narrow in the future, with policy being based on something more than the Latin wisdom of *Omne trium perfectum* and science being more policy relevant.

The fourth approach is about expanding the range of possible data economists may use. In some cases collecting this data may require the use of non-standard methods – as in narrative analysis – and in some cases not – as in macroeconomic derivatives. As economics is a discipline largely defined by its method this sort of methodological expansion seems to be a risk to the conformity of the discipline and all its benefits, but it is not necessarily so. Even if one took the extreme view of Friedman (1953) and thought that the realism of assumptions does not matter and that all that matters is the models' predictive success non-traditional methods could still be useful. Piore (2006) discusses open-ended interviews and argues that although these cannot be used directly as empirical evidence, they can help when building theories and hypotheses. Scientists draw inspiration from anecdotal sources constantly and such open-ended interviews could be seen as just a systemic way of collecting anecdotes.

With respect to macroeconomic derivatives it should be remembered that their markets were cancelled because they were not profitable to the private companies operating them. On a more general level Snowberg, Wolfers & Zitzewitz (2012) note that prediction markets sometimes fail because there is not sufficient interest in the question. By failure the authors refer to an inability of the market to sustain itself, leading to low liquidity and poor information content of prices. If one conceives of these markets as potential sources of information, it can easily be argued that the inability to generate free of charge is not a failure of the entire mechanism. Employment surveys do not fail because they are not generated by the markets to be used freely by researchers. This is a relevant issue from the point of view of the fifth approach.

Performativity is an interesting concept and embracing it as a part scientific work would be quite provocative. In my view it is probably not necessary and in any case it may backfire, as MacKenzie's (2006) term "counter-performativity" indicates. Speaking of performativity and the relationship between science and practice is however important, especially so in a field where they are closely interwoven, for example monetary policy. As studying how institutions and organizations work is not a forte of economists these issues may be neglected in the study of optimal monetary policy.

All these approaches have their own respective risks and possible benefits and none of them is without an opportunity cost. Resources used to study optimal monetary policy are finite and they should be directed judiciously. One fundamental tradeoff in economic research is that between conformity and diversity, which both have considerable benefits. Conformity means a larger pool of researchers who are able to understand and critique each other's work, whereas diversity means looking at all the possible questions from all the possible angles with all the possible methods. As with all tradeoffs finding the optimal point in the possibility frontier is only the second-best alternative. The best is to push the possibility frontier further out.

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APPENDIXES

Appendix A: Rearranging New Keynesian Phillips Curve Specifications

Dittmar & Gavin (1999) and Svensson (1999) specify the New Keynesian Phillips Curve as

$$y_t = \rho y_{t-1} + \alpha(\pi_t - \pi_{t+1|t}) + \varepsilon_t \text{ (1)}$$

Vestin (2006) specifies the New Keynesian Phillips Curve as

$$\pi_t = \beta \pi_{t+1|t} + \kappa y_t + u_t \text{ (2)}$$

To compare these two specifications I will rearrange them so that both of them will have the inflation rate as the left-hand-side variable and an i.i.d. error term.

Rearrange (1) so that π_t is the left-hand-side variable. Add $(-\alpha\pi_t - y_t)$ to both sides and divide by $-\alpha$. The resulting equation is

$$\pi_t = a(y_t - \rho y_{t-1}) + \pi_{t+1|t} + \varepsilon_t \text{ (3)}$$

Then rearrange (2) so that the error term is i.i.d. The previous period's inflation rate is given by

$$\pi_{t-1} = \beta \pi_{t|t-1} + \kappa y_{t-1} + u_{t-1} \text{ (4)}$$

Multiply (4) by ρ to obtain

$$\rho\pi_{t-1} = \rho\beta \pi_{t|t-1} + \rho\kappa y_{t-1} + \rho u_{t-1} \text{ (5)}$$

Subtract (5) from (2) to obtain

$$\pi_t - \rho\pi_{t-1} = \beta(\pi_{t+1|t} - \rho\pi_{t|t-1}) + \kappa(y_t - \rho y_{t-1}) + u_t - \rho u_{t-1} \text{ (6)}$$

Since $u_t - \rho u_{t-1} = \varepsilon_t$ (6) can be written as

$$\pi_t - \rho\pi_{t-1} = \beta(\pi_{t+1|t} - \rho\pi_{t|t-1}) + \kappa(y_t - \rho y_{t-1}) + \varepsilon_t \text{ (7).}$$

Add $\rho\pi_{t-1}$ to both sides to obtain

$$\pi_t = \beta(\pi_{t+1|t} - \rho\pi_{t|t-1}) + \rho\pi_{t-1} + \kappa(y_t - \rho y_{t-1}) + \varepsilon_t \text{ (8).}$$