THE ROLE OF POLICY RULE MISSPECIFICATION IN MONETARY POLICY INERTIA DEBATE

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Abstract

Operational monetary policy rules are characterized by a parsimonious specification and are therefore prone to specification error when estimated on real data. I devise a policy rule estimation procedure, which is robust to marginal misspecification, and study the effects of specification error in least squares. I find the robust evidence of upward bias in policy inertia in least squares applied to most commonly used Taylor type rule. In effect, least squares learning of a central bank can lead to increasing monetary policy inertia over time.

Abstrakt

Operacionalizovaná měnově-politická pravidla jsou charakteristická svou parsimonní specifikací a proto jsou náchylná k chybě specifikace v odhadech na reálných datech. Je odvozen postup pro odhad měnových pravidel, který je robustní vůči marginální specifikační chybě, a jsou studovány efekty specifikační chyby v odhadu nejmenších čtverců. Výsledkem je robustní evidence o nadhodnocování parametru vyhlazování měnové politiky v odhadu nejrozšířenějšího pravidla Taylorova typu nejmenšími čtverci. Důsledkem je, že proces učení centrální banky založený na nejmenších čtvercích může vést k rostoucí strnulosti měnové politiky v čase.

Key words: Monetary policy inertia, policy rule
J.E.L. Classification: E4, E5

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1 Introduction

In recent years, monetary policy practice has been increasingly relying on a model based assessment of actual and future policy stance. Along with the increasing requirements posed on models in terms of level of sophistication and complexity, in order to provide the policy maker with more information about the consequences of its intended actions, one can observe a tendency of moving away from producing conditional in favor of unconditional macroeconomic forecasts. Taking the example of central banks that target inflation, among 22 banks in 2005 (Batini et al., 2005), eight [1] produce unconditional forecasts, while many of these launched this regime with conditional forecasts.

In effect, it enlarged the traditional pool of researchers to whom the accurate policy rule estimation is of obvious interest, i.e., the market participants trying to anticipate future policy rate changes, by one more group that now seeks accurate estimations of the policy rule to calibrate policy analysis models – the central bank's own research staff. The general concern is how to estimate and calibrate an operational policy rule and how to use it in practical policy implementation; in particular, how the identification depends on data frequency, policy rule specification, and method of estimation.

A policy rule estimated in limited specification (as it is usual for operationalized rules) through ordinary least squares might be misspecified and thus parameter estimates biased. The use of simple policy rules of the Taylor type, which predominantly involves inflation, inflation target, output gap, and neutral policy rate, is widespread in policy analysis frameworks. However, in reality, a broader set of variables (some even not directly measurable) determines the policy rate setting.

The misspecification and bias of policy rule parameter estimates, which might result in overstatement of policy inertia, is however likely to be lower with lower data frequency.

[1] Among inflation targeting countries (the year of inflation targeting implementation is given in parenthesis) that produce unconditional inflation forecast are the Czech Republic (1998), Chile (1991), Canada (1991), Columbia (1999), Norway (2001), New Zealand (1990), Peru (1994), and Slovakia (2005).

Although the decision about the key policy rate is carried out on a monthly basis, many operational models are quarterly and therefore we pay attention to the conceptual difference between quarterly and monthly data analysis to extract unbiased parameters of policy rules. Namely, we suppose that there might be a larger set of variables that determine the policy rate setting with monthly frequency than with quarterly frequency, since some of the determinants of monthly frequency are temporary and might get eliminated by quarterly averaging. Nevertheless, many variables enter the policy rule in gaps, thus averaging into quarters might eliminate some (but generally not all) of the additional temporary determinants, which are hard to quantify, but at the same time it could cause a loss of information in measurable objectives (for instance in inflation gap). Therefore, we compare estimates on both frequencies, test for some regularities, and discuss how the use of biased parameters might bear important consequences for policy implementation.

A great portion of the empirical literature has been devoted to studying the policy rule of the Federal Reserve System. For the purpose of the studied phenomenon, we distinguish studies according to the data frequency they analyze, i.e., monthly vs. quarterly.

For seminal papers using monthly data we go back to Rosett (1959), who suggested to apply essentially an ordered probit to address the rigid nature of the discount rate. A sequence of papers applying alternative discrete dependent variable models followed, including Feinman(1993) and Hakkio and Pearce (1992). Most recently, Choi (1999) derived a two-sided-type II tobit that accounts not only for the discrete nature of the discount rate but also for its partial censoring. It is rather apparent that the zero policy rate changes have the potential to be censored, which is Choi's conjecture; however, he also assumes that the non-zero policy rate changes are uncensored. The later assumption is, however, not entirely correct. The monetary authority adjusts its policy rate usually by a quarter of a percentage point since the council dislikes policy rate reversals, i.e., it aims at avoiding instability in financial markets (advocated by Goodfriend, 1991; Cukierman, 1989; and Rudebusch, 1995) and limits the number of large policy rate changes that could lead to a loss in credibility (see Goodhart, 1997). Thus, the outcome of the monetary policy decision meeting would be most often a quarter of a percentage point increase (decrease) in the policy rate even if the policy maker intended, based on fundamentals, to increase (decrease) by half a percentage point or more.

An example of such practice can be seen in a quotation from the minutes of the FED's FOMC meeting held on February 3-4, 1994:

"In the course of the Committee's discussion, a number of members endorsed a policy move that would involve only a slight adjustment toward a less accommodative degree of reserve pressure. These members recognized that evolving economic conditions might well justify a somewhat greater policy adjustment. They believed, however, that even a slight move at this time was likely to have a particularly strong impact on financial markets because it would be the first policy change after a long hiatus and indeed the first tightening action in about five years."

This implies that the *non-zero* discount rate changes are also potentially censored due to the presence of non-fundamental determinants.

In order to account for possible censoring of all policy rate changes, i.e., zero as well as non-zero, we develop a two-stage estimation procedure that combines the ordered probit and the censored regression.[2] Since the ordered probit delivers unbiased parameter estimates, we use these for deriving a censoring indicator (including non-censored observations) that we subsequently use in the censored regression. This procedure delivers unbiased coefficients and improves statistical efficiency of estimates. The marginal effects are constant, i.e., directly comparable to the calibrated linear policy rules, and thus it is advantageous for initial calibration, verification, and update of linear policy rules used in policy practice. Besides, it allows for treating the determinants of censoring as unknown. We provide a method verification on the Czech Republic, where unique data on implicit policy rate are

[2] We focus on monthly frequency models, which involve a discrete dependent variable since we consider these more correct from the point of view of the frequency of the actual decision process. We use the policy rate since the assumption that the continuous money market rate sufficiently approximates the discrete policy rate is in our view problematic due to the fact that money market rates contain the interpretation of the communication of a central bank by the market and thus a policy rate misspecification of a different kind remains a problem (inclusion of a speech index would be necessary, see Musard-Gies, 2005).

available. We further apply the method to the U.S. data set used by Choi (1999) and discuss the improvements in our new estimator.

In search for symptoms of quarterly policy rule misspecification, the recent literature is not consensual. On one side, the evidence of a low portion of predicted variance of future rates by the market is exposed as proof for non-inertial policy rules, see Rudebusch (2002). On the other side stands evidence on the size of shocks in policy rules that makes the future rates less predictable even with policy rate smoothing; see Soederlind et al. (2004). We bring additional evidence to this issue by comparing the projections of policy rate by a high-smoothing central bank and the financial market. In this way we prove, with an example of the Czech financial market, that policy rate inertia might be existing even if the market does not succeed in predicting the future rates.

Nevertheless, we consent that the empirically found policy rate inertia might be overstated in least squares estimation due to policy rule specification error. The misspecified policy rule causes bias in all coefficients, however prominently in the size of smoothing for two reasons. Firstly, the governing council fears policy reversals and therefore the additional variables (often omitted in estimation) are such that the council downsizes the implied size of a policy rate change, which results in higher policy rate smoothing. Secondly, the serial correlation in the omitted variables is naturally instrumented through the past dependent variable, thus omitted variables lead to overstatement of inertia (see Rudebusch, 2002). Therefore, we focus on the evidence on estimates of inertia and carry out a meta-analysis of available estimates in the literature with the aim of establishing some regularities with respect to data frequency and method of estimation. We suppose that the instrumental variable methods will deliver systematically lower inertia than ordinary least squares due to their robustness to marginal misspecification and that monthly inertial bias will be higher than quarterly. We make use of results in the following studies: Amato and Laubach (1999), Clarida et al. (2000), Lansing (2002), Levine et al. (2003), and Rudebusch (2002).

Our results confirm a statistically significant upward bias in monetary policy inertia by the least squares estimator applied to monthly as well as to quarterly data. The major policy implication is that least squares learning of a central bank might lead to increasing policy rate inertia over time due to systematic inertia overstatement in the update of a policy rule.

In section 2 I describe a model for the policy rate, and in section 3 I derive the estimation procedure for unbiased parameter estimates of policy rules. In section 4 I provide with the verification of the method and section 5 contains results of policy rule estimation on the U.S. data. Section 6 develops the analysis of policy inertia on quarterly frequency and section 7 concludes.

2 Policy rate model

The decision about setting the key policy rate is a result of a complex process. At every monetary decision meeting, the bank's governing council assesses the current and forecasted macroeconomic conditions (such as output gap, inflation, equilibrium interest rate which defines a basic set of measurable variables, here referred to as a core framework), and considering all other relevant information, it decides whether to adjust or keep the policy rates setting.

Since all (*zero* as well as *non-zero)* policy rate changes are potentially determined by more variables than are quantifiable in practice, the need for estimation of the policy rule using representation with a limited number of variables requires a special estimation treatment. If all true motives for the policy rate changes would be quantifiable, the policy rule would be easily estimated applying least squares to the full specification. However, some of the objectives that are pursued by the governing council are not straightforward to quantify and thus variable omission causes biased parameter estimates. Since the omitted variable is relevant and often impossible to quantify and determinants of the policy rate changes are in fact pertaining to the governing council, it resembles the censoring process when the council occasionally censors the usual core framework. Thus, we use censoring as a vintage for the policy rate setting process.

Let us define $\Delta i_t^* = i_t^* - i_{t-1}$, which represents the change in policy rate that would correspond to the quantifiable variables. Hence, the changes in the observed policy rate settings Δi_t might be only partially coinciding with the unobserved Δi_t^* due to an influence of some of the additional explanatory variables on Δi_t. Let the available set of explanatory variables X_t be a subset of the full set of explanatory variables Ω_t. Then it follows that

$$\Delta i_t = \Delta i_t^* + Z_t'\beta_0 = X_t'\beta_2 + Z_t'\beta_0 + u_{2,t}, \tag{1}$$

where $Z_t \equiv \Omega_t - X_t$.Thus omitting variables Z_t biases the estimates of β_2 in the least squares regression if $X_t'(Z_t'\beta_0) \neq 0$, which is often the case since shocks in explanatory variables are highly correlated; see arguments of Rudebusch (2002).[3]

If the matrix of regressors X_t contains the past dependent variable (policy rate smoothing), the bias is likely translated into an overstatement of the parameter pertaining to this variable. This is motivated by two reasons, one fundamental and one technical. The fundamental reason is that the motives of the governing council, mainly fear from policy reversals, are such that the council downsizes the implied size of the policy rate change and thus the censoring results in higher policy rate smoothing. The mechanical reason is that the serial correlation in the omitted variables is naturally instrumented through the past dependent variable and thus the omission of the variables Z_t potentially causes an increase in the smoothing term as well.

In practice, though, the aim is to estimate the following relation without bias, since the policy rate recommendation to the governing council should be based on quantifiable variables (core framework) and unbiased coefficients pertaining to them:

$$\Delta i_t^* = X_t'\beta_2 + u_{2,t}, \tag{2}$$

where β_2 represents the coefficients pertaining to the explanatory variables in X_t; $u_{2,t}$

[3] In the case of full specification, the estimate of β_2 is equal to $\widehat{\beta}_2 = (X_t'X_t)^{-1}X_t'\Delta i_t - (X_t'X_t)^{-1}X_t'(Z_t'\beta_0)$, while omitting Z_t leads to $\widehat{\beta}_2^* = (X_t'X_t)^{-1}X_t'\Delta i_t$. It follows that $\widehat{\beta}_2^* \neq \widehat{\beta}_2$ if $X_t'(Z_t'\beta_0) \neq 0$, (see Greene 2003).

denotes $i.i.d.$ random error $N(0, \sigma_2^2)$. If there is an omitted variable problem (vector of omitted variables Z_t), i.e., misspecification of the policy rule, we still can model the partially observed policy rate using the following formalization of the observation-by-observation censored model:

$$\begin{aligned}
\Delta i_t \leq \Delta i_t^* \quad & \text{if} \quad \Delta i_t^* \leq \Delta i_t - T_l \\
\Delta i_t = \Delta i_t^* \quad & \text{if} \quad \Delta i_t - T_l < \Delta i_t^* \leq \Delta i_t + T_u \\
\Delta i_t \geq \Delta i_t^* \quad & \text{if} \quad \Delta i_t^* > \Delta i_t + T_u
\end{aligned} \tag{3}$$

The thresholds T_u and T_l are equal to ± 12.5 basis points (b.p.) since the policy rate is predominantly adjusted by discrete changes of 25 b.p. If $X_t \equiv \Omega_t$, then all $\Delta i_t = \Delta i_t^*$ and the estimation can proceed with a linear estimator since there are no censored observations. Since we assign $\Delta i_t = \Delta i_t^*$ even if in fact $\Delta i_t - T_l < \Delta i_t^* \leq \Delta i_t + T_u$, the estimates will be unbiased, however inefficient since the variance $u_{2,t}$ will be constant but nevertheless higher, compared to knowledge of the continuous dependent variable. If we have, however, only X_t to our disposal, we need to remedy the misspecification bias. One can either try to account for all possible explanatory variables, or apply an alternative estimator that would be robust to the omitted, unobserved, and often hardly quantifiable variables (unknown censoring). In the next section we present such an alternative estimation method.

3 Estimation procedure

We design the following two-stage estimation procedure. The first stage can be described using an ordered probit, similarly to the frictions model by Rosett (1959). Let Δi_t be an observed discrete ordered policy rate response taking values $\{m_1, m_2, \ldots, m_n\}$, where m_j denotes a particular magnitude of observed change in policy rate. The change in implicit policy rate Δi_t^*, defined as $\Delta i_t^* = i_t^* - i_{t-1}$, is determined by

$$\Delta i_t^* = X_t' \beta_1 + u_{1,t}, \tag{4}$$

where $\beta_1$denotes the vector of coefficients corresponding to the explanatory variables in

X_t and $u_{1,t}$ stands for $i.i.d.$ random error $N(\mu, \sigma_1^2)$. In practice, the following propositions are likely to hold, i.e, $E(Z_t'\beta_0|X_t) = 0$ and $N_c/N \rightarrow 0$, since censoring by the council is occasional (if it was a regular practice, there would be likely an adjustment to the core framework) and further since the fear from policy reversals makes the council censor both from the right and from the left equally probable in a sufficiently long sample.

We can express the relation between the latent (implicit policy rate) variable Δi_t^* and the observed variable Δi_t as follows:

$$
\begin{aligned}
\Delta i_t &= m_1 && \text{if} && \Delta i_t^* \leq Tm_1 \\
&= m_2 && && Tm_1 < \Delta i_t^* \leq Tm_2 \\
&... \\
&= m_n && && \Delta i_t^* > Tm_n
\end{aligned} \tag{5}
$$

which means that at each of the m_j thresholds, denoted as $Tm_1 < Tm_2 < \ldots < Tm_n$, the magnitude of policy rate change m_j in observed policy rate discretely switches to a different one in an ordered manner. Since the policy rate changes usually by multiples of 25 b.p., the thresholds should take a value of multiples of $12.5\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}}$ b.p. The standard deviation of $X_t'\beta_1$ is denoted as $\sigma_{X_t'\beta_1}$ and similarly $\sigma_{\Delta i^*}$ stands for the standard deviation of Δi_t^*.

If the number of censored observations N_c is small relative to the number of observations in the sample $N_c/N \rightarrow 0$, i.e., censoring is occasional, the identified thresholds $Tm_i/\sigma_{X_t'\beta_1}$ will be close to the expected fixed values of $12.5/\sigma_{\Delta i^*}$ b.p. and its multiples. If N_c/N is large, the ordered probit will still estimate unbiased parameters (ordered probit parameter estimates are robust to marginal misspecification; see White, 1982). However, for the evaluation of the censoring indicator, one needs to use the precise values of multiples of $12.5\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}}$ b.p., since the underlying idea is to compare what the council should have done (based on core framework), conditional on the quantifiable variables (given that it adjusts the rate by multiples of a quarter of a percentage point) with what it actually did.

The maximum likelihood for the ordered probit is:

$$L = \prod_{t=1...n} \left\{ \begin{array}{c} [1-\Phi(X_t'\beta_1 - Tm_1)]^{I(\Delta i_t = m_1)} \\ [\Phi(X_t'\beta_1 - Tm_1) - \Phi(X_t'\beta_1 - Tm_2)]^{I(\Delta i_t = m_2)} ... \\ [\Phi(X_t'\beta_1 - Tm_n]^{I(\Delta i_t = m_n)} \end{array} \right\}$$

In the case that the data contains multiple sizes of changes (n is large), the ordered probit will deliver consistent but inefficient parameter estimates. Besides, the inconstancy (non-linearity) of the marginal effects of exogenous variables in ordered probit complicates their direct use for policy purposes. Therefore, we suggest using the consistently estimated parameters from ordered probit for evaluating the probabilities of censoring from the left, right, and of uncensored observations and performing a censored regression. The censoring respects the maximum likelihood of the alternative outcomes. Observations $\Delta i_t = 0$ are said to be censored as follows if[4]

$$\begin{array}{lll} \Delta i_t \geq \Delta i_t^* & \text{if} & P_1 > P_2 \text{ and } P_1 > P_3 \\ \Delta i_t \leq \Delta i_t^* & \text{if} & P_3 > P_2 \text{ and } P_3 > P_1 \\ \Delta i_t = \Delta i_t^* & & \text{otherwise.} \end{array} \quad (6)$$

The evaluation of the particular probabilities (for the case of three distinct sizes of policy rate changes, i.e., 0, 0.25, and -0.25), follows:

$P_1 = Prob(\Delta i_t^* \leq -0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}}|X_t'\beta_1) = 1-\Phi(X_t'\beta_1+0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}})$, $P_2 = Prob(-0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}} < \Delta i_t^* \leq 0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}}|X_t'\beta_1) = \Phi(X_t'\beta_1+0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}})-\Phi(X_t'\beta_1-0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}})$[5], $P_3 = Prob(\Delta i_t^* > 0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}}|X_t'\beta_1) = \Phi(X_t'\beta_1 - 0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}})$, and $\sum_i = 1, ...3\ P_i = 1$.

The equations (6) state that while observing no change in the announced policy rate, the policy rule implied a change, i.e., the probability of keeping the policy rate steady is smaller than both the probability of increasing and decreasing it. At the same time, if the largest probability is P_1 (P_2) among P_1,P_2, and P_3, we identify censoring from the left (right).

[4] If the model's policy rate is available, the censoring indicator would take a value of -1 if $\Delta i_t - \Delta i_t^* > \delta$; and -1 if $\Delta i_t - \Delta i_t^* < -\delta$, where δ is very small but sufficiently large to permit for estimation of the censored model; all remaining observations would be uncensored.

[5] Ideally, P_2 would be computed as $Prob(-0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}} < \Delta i_t^* < 0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}}|X_t'\beta_1)$; however, due to ease of derivation, we assume the presented approximation as satisfactory.

Similarly, in the case of observed non-zero changes $\Delta i_t \neq 0$, we have

$$\begin{aligned} &\Delta i_t \leq \Delta i_t^* \quad \text{if} \quad P_4 > P_1, \ P_4 > P_2, \ \text{and } P_4 > P_3 - P_4 \ \text{for} \ \ \Delta i_t > 0 \\ &\Delta i_t \geq \Delta i_t^* \quad \text{if} \quad P_0 > P_2, \ P_0 > P_3, \ \text{and } P_0 > P_1 - P_0 \ \text{for} \ \ \Delta i_t < 0 \\ &\Delta i_t = \Delta i_t^* \qquad\qquad\qquad\qquad \text{otherwise,} \end{aligned} \tag{7}$$

where $P_0 = Prob(\Delta i_t^* \leq -0.375\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}}|X_t'\beta_1) = 1-\Phi(X_t'\beta_1+0.375\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}})$ and $P_4 = Prob(\Delta i_t^* > 0.375\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}}|X_t'\beta_1) = \Phi(X_t'\beta_1 - 0.375\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}})$.

The conditions in (7) are also very straightforward. We evaluate the probability conditional on the quantifiable variables in X_t that the rates should be changed by more than the respective threshold, i.e., P_0 and P_4. Let us consider for instance the following situation. The probability that rates should be changed by more than the respective threshold exceeds the probability of keeping the rates steady, i.e. P_2, and decreasing the rates, i.e. P_1, and at the same time it exceeds also the probability of increasing by less than the threshold, i.e. $(P_3 - P_4)$. Then, the council censored the size of increase implied by the core framework, i.e., the council increased by less than would correspond to the recommendation based on the core framework.

In other words, the first stage identifies the observations, in which the operational policy rule does not meet the condition for no misspecification, $\Delta i_t = \Delta i_t^*$, i.e., the occasions at which the governing council censored the core framework.

In the second stage we complement the censored regression model by using the indicator of censoring derived on the basis of the first stage estimation. Besides improving the efficiency of estimates, in the presence of uncensored observations, the parameters will be constant and compatible with those calibrated in the lineal policy rules. The second stage of the model can be represented as follows:

$$\Delta i_t^* = X_t'\beta_2 + u_{2,t}. \tag{8}$$

The estimation of the censored regression follows the standard maximum likelihood method.

The likelihood function for the observation-by-observation censored regression model can be written as follows:

$$L = \prod_{t=1...n} \left\{ \begin{array}{c} \left[1-\Phi(X_t'\beta_2 - \Delta i_t)\right]^{I(I_t=-1)} \left[\sigma^{-1}\phi\left[(\Delta i_t - X_t'\beta_2)/\sigma\right]\right]^{I(I_t=0)} \\ \left[\Phi(X_t'\beta_2 - \Delta i_t)\right]^{I(I_t=1)} \end{array} \right\}.$$

The censoring indicator I_t is constructed as follows:

$$\begin{aligned} I_t &= -1 \;\text{ if }\; P_0 > P_2,\; P_0 > P_3, \text{ and } P_0 > P_1 - P_0 \text{ for } \Delta i_t < 0 \\ &= 1 \;\text{ if }\; P_4 > P_1,\; P_4 > P_2, \text{ and } P_4 > P_3 - P_4 \text{ for } \Delta i_t > 0 \\ &= 0 \quad \text{otherwise} \qquad \text{for } \Delta i_t < 0 \text{ and } \Delta i_t > 0 \\ &= -1 \;\text{ if }\; P_1 > P_2 \text{ and } P_1 > P_3 \qquad \text{for } \Delta i_t = 0 \\ &= 1 \;\text{ if }\; P_3 > P_2 \text{ and } P_3 > P_1 \qquad \text{for } \Delta i_t = 0 \\ &= 0 \quad \text{otherwise} \qquad \text{for } \Delta i_t = 0 \end{aligned}$$

where observations censored from the left, right, and uncensored are assigned $-1, 1$, and 0, respectively. For the evaluation of the particular probabilities (for the case of three distinct sizes of policy rate changes, i.e., 0, 0.25, and -0.25), see below equations (6) and (7).

4 Method verification: nearly laboratory data

The applicability of the proposed method is demonstrated using data for the policy rule of the Czech National Bank, which is one of the pioneers of explicit inflation targeting in the region of Central and Eastern Europe. The advantage of using the Czech example is mainly in the availability of unique data for the true (and real-time data)[6] determinants and calibrated coefficients of the policy rate i_t^*:

$$i_t^* = X_t'\beta_2, \tag{9}$$

[6] In this way we can avoid the argument of Lansing (2002) that estimated high policy rate inertia on revised data is misleading since estimations with real-time data on the output gap show much smaller policy rate inertia.

i.e., $X_t'\beta_2$, based on which the governing council has been advised to adjust policy rate i_t:

$$i_t = i_t^* + Z_t'\beta_0 = X_t'\beta_2 + Z_t'\beta_0 + u_{2,t}. \tag{10}$$

The matrix Z_t contains the potencially relevant variables that are considered by the governing council in rate decisions in addition to the variables in X_t (core framework). These variables are, however, omitted in the estimation.

4.1 Specification and Data

Although inflation targeting was implemented at the beginning of 1998, the Czech National Bank transited to an unconditional inflation forecast in early 2003. Since then, besides previously producing and publishing the inflation forecasts and announcing inflation targets, the policy rule became an integral part of the policy framework. As it has been disseminated in the Forecasting and Policy Analysis System (CNB 2003), the model's policy rate i_t^* obeys the following forward looking Taylor rule:

$$i_t^* = m_0 i_{t-1} + (1 - m_0)(r_t^{eq} + p_t^e + m_1(p_t^e - p_t^{tar}) + m_2 gap_t), \tag{11}$$

where m_0, m_1, and m_2 are calibrated parameters, and i_{t-1} denotes one period (month) lagged policy rate. The real equilibrium interest rate is denoted by r_t^{eq}, p_t^e labels the forecasted inflation in one year ahead, and p_t^{tar} denotes the corresponding inflation target. The output gap is denoted as gap_t.

The observed policy rate i_t is, however, determined as follows:

$$i_t = m_0 i_{t-1} + (1 - m_0)(r_t^{eq} + p_t^e + m_1(p_t^e - p_t^{tar}) + m_2 gap_t) + Z_t'\beta_0 + u_{2,t}. \tag{12}$$

In order to apply the ordered probit model most efficiently, we slightly transform the policy rule specification by subtracting from it the lagged policy rate, by defining the nominal equilibrium interest rate $i_t^{eq} = r_t^{eq} + p_t^e$, and implementing the restriction as $i_t^r = i_t^{eq} - i_{t-1}$.

These operations and omission of $Z_t'\beta_0$ transform the policy rule into the following form:

$$\Delta i_t = n_1 i_t^r + n_2(p_t^e - p_t^{tar}) + n_3 gap_t + u_{2,t} \tag{13}$$

where $n_1 = (1 - m_0), n_2 = (1 - m_0)m_1, n_3 = (1 - m_0)m_2$.

Besides the monthly two-week repo rate (policy rate), the data further comprises the quarterly deviation of the forecasted inflation from its target, output gap, and equilibrium nominal policy rate that we collected from the internal CNB's baseline forecast database for each quarterly inflation forecast. For the sake of using monthly observations on policy rate changes, we have interpolated the quarterly explanatory variables into monthly frequency through quadratic match-average. The time of our sample spans from 2003 January throughout 2005 December, which is motivated by the fact that since early 2003, when a policy rule recalibration took place, the calibration of the policy rule has not been changed. Descriptive statistics of the data used in the analysis are shown in Table 1.

Table 1: Data descriptive statistics

	Mean	St.D.	Max.	Min.
Two-week repo rate	2.14	.27	1.75	2.5
Policy neutral rate	3.62	.46	2.66	4.35
Inflation forecast deviation from target	−.86	.47	−1.63	−.12
Output gap	−1.17	.73	−2.44	−.39

The sample period is characterized by a negative output gap, inflation forecast under the target, and policy rates below their neutral level. As for the statistics on policy rate changes, the rate has been changed nine times out of 36 monthly meetings of the council. Three times the council decided to increase and six times to decrease the rate. All changes in the two-week repo rate were of the size of 25 b.p. At twenty seven meetings the rates remained on-hold.

4.2 Estimation results

We present four regressions. First, we estimated the equation (13) using the ordinary least squares, i.e., ignoring possible misspecification. Then we estimated the two-sided-tobit type II, allowing only zero policy rate changes to be potentially censored.[7] Next, we applied the two-stage procedure that consists of an ordered probit in the first stage, and then using the fitted values evaluated censoring indicator, I carried out the observation-by-observation censored regression. In this way, we accounted for possible misspecification of the policy rule and derived unbiased parameter estimates. And finally, the fourth regression is based on the observation-by-observation censored regression with the true indicator of censoring (the underlying model's policy rate minus the two-week repo rate). The results of parameter estimates are summarized in Table 2, along with the statistics pertaining to them.

[7] *Two-Step estimator for a two-sided tobit type II using Heckman's procedure.* The first step is the Ordered Probit (-1,0,1). The likelihood function reads

$$
\begin{aligned}
L = \prod_{t=1\ldots n} & \{[1-\Phi(X_t'\beta_1 - Tm_1)]^{I(\Delta i_t=-1)} \\
& [\Phi(X_t'\beta_1 - Tm_1) - \Phi(X_t'\beta_1 - Tm_2)]^{I(\Delta i_t=0)}\ldots \\
& [\Phi(X_t'\beta_1 - Tm_2]^{I(\Delta i_t=1)}\},
\end{aligned}
$$

where Tm_i denotes the tolerance ancillary parameters. The second step is the ordinary least squares with inverse Mill's ratio (λ_t):

$$\Delta i_t = X_t\beta + \gamma\widehat{\lambda}_t + \varepsilon_t + \eta_{H,t},$$

where ε_t denotes the model error and $\eta_{H,t}$ stands for the Heckman's approximation error, $\eta_{H,t} = \lambda_t - \widehat{\lambda}_t$. The estimate of λ_t is denoted as $\widehat{\lambda}_t$ and $\widehat{\lambda}_t = I_{(\Delta i_t=-1)} - \phi(X_t'b_1 - Tm_1)/\Phi(X_t'b_1 - Tm_1) + I_{(\Delta i_t=1)}\phi(X_t'b_1 - Tm_2)/\Phi(X_t'b_1 - Tm_2)$.
The vector of parameters b_1 is the estimate of β_1. We applied White's (1980) approach to derive consistent standard errors using the second step residuals e_i as $(Z_t'Z_t) - 1Z_t'Var(\varepsilon_t)Z_t(Z_t'Z_t) - 1$, where $Z_t'Var(\varepsilon_t)Z_t = \sum_{i=1,2,\ldots,n} e_i^2 z_i z_i'$. The z_i is an element of $Z_t = (X_t : \widehat{\lambda}_t)$.

Table 2: Estimates on monthly frequency

	$[i_t^{eq}$-$i_{t-1}]$	$[p_t^e$-$p_t^{tar}]$	$ygap_t$	(ps)-R^2	LL
OLS	$.06^{***}_{(.02)}$	$.07^{**}_{(.03)}$	$.04_{(.03)}$	0.3	–
OPROBIT	$5.6^{***}_{(1.9)}$	$-2.2^{*}_{(1.3)}$	$1.5^{**}_{(.7)}$	0.45	−14.32
OPROBIT (-1,0,1)	$5.6^{***}_{(1.9)}$	$-2.2^{*}_{(1.3)}$	$1.5^{**}_{(.7)}$	0.44	−14.32
Two-Sided-Hekit[f)]	$.08^{***}_{(.04)}$	$.12_{(.06)}$	$.05_{(.06)}$	0.78	–
CENREG[a,d)]	$.09^{***}_{(.02)}$	$.09^{***}_{(.035)}$	$.05^{*}_{(.03)}$	1[c)]	23.29
CENREG[b)]	$.09^{***}_{(.01)}$	$.10^{***}_{(.02)}$	$.05^{***}_{(.01)}$	1[c)]	23.36
MODEL[e)]	.09	.11	.04	1	–

Notes: Standard errors are given in parentheses. There are 36 observations. The stars denote significance as follows: *** 1%, ** 5% and * 10%.

Legend:

a) Regression with the indicator from ordered probit; σ=.11***(.01).

b) Regression with the true indicator of censoring; σ=.03***(.006).

c) Pseudo-R^2 was truncated at 1.

d) Corresponding ancillary parameters are: Tm_1=6.23***(2.8) and Tm_2=11.48***(4.1).

e) The CNB's policy rule calibration in the Forecasting and Policy Analysis System, converted to monthly frequency.

f) Corresponding ancillary parameters are: 6.23***(2.8) and 11.5***(4.05). The Inverse Mill's Ratio parameter: 0.073*(0.03). The *s.e.* in the parenthesis is computed using White's (1980) approach.

The model in the first stage identified the following thresholds: $Tm_1 = 6.23^{***}_{(2.8)}$ and $Tm_2 = 11.48^{***}_{(4.1)}$, which correspond to 1.4 times the standard error of fitted values $X_t'\beta_1$, ($\sigma_{X_t'\beta_1} = 4.45$). These thresholds seem very plausible, since for instance 1.4 times the standard error of the 3 Month Pribor during 2003 Apr. – 2005 Sep. ($\sigma_{\Delta i^*} \approx 0.105$) amounts to .147. Nevertheless, in order to evaluate the probabilities of policy rate changes of different sizes, we derive the following thresholds: $Tm_1^* = 0.125\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}} = 5.3$; $Tm_2^* = 0.375\frac{\sigma_{X_t'\beta_1}}{\sigma_{\Delta i^*}} = 10.6$. As for the construction of the censoring indicator,the council could have censored the size of the change in those cases when the council changed the rate in congruence with the probability that the fitted values from ordered probit exceed the thresholds Tm_1^* or Tm_2^*. Thus, if the fitted values from the ordered probit do not exceed $Tm_0^* = -5.3$ or do exceed $Tm_3^* = 26.3$, respectively, the rate should have been changed, according to the core framework, by more than it was observed. Such observations would be considered censored non-zero policy rate changes, otherwise uncensored. Similarly, in the periods when the council did not change the rates and the evaluated probability would suggest so, these periods would be labeled as censored policy rate changes at zero. All other situations would be considered as uncensored.

As it appears in the Table 2, the smoothing term (i_{t-1}) by OLS is excessive: the statistically significant difference between mean estimates by OLS and the censored regression (model's calibration) amounts to .04. Similarly, the remaining coefficients by OLS are accordingly lower (the difference for p_t^e-p_t^{tar} is .02−.03, and for $ygap_t$ it is .01). In addition, the parameter of the output gap ($ygap_t$) in OLS regression even appears statistically insignificant. These results imply that the policy rule is misspecified, namely that there are other relevant variables in the governing council's decision, i.e., $Z_t'\beta_0 \neq 0$ and that $X_t'(Z_t'\beta_0) \neq 0$.

In contrast to OLS estimates, the two stage procedure of ordered probit and observation-by-observation censored regression delivers unbiased and more efficient estimates of the parameters in the underlying policy rule specification. Additional knowledge of the true indicator of censoring yields a more efficient estimate of variance compared to the two stage procedure derived in this paper. The results for the two-sided tobit type II are partially insignificant due to the small sample of non-zero changes. The small sample is a general problem for this method since the second stage is performed on a subsample of non-zero policy rate changes that is often substantially smaller. Moreover, the model assumes only zero policy rate changes being potentially censored, and thus it omits the possibility of censored non-zero changes which might prove important, even though zero censored policy rate changes are likely to dominate. In addition, the model relies on the estimated selection rule and thus requires knowledge of its determinants. This is another and likely largest drawback of the method since some of the determinants of the selection rule are often not directly measurable.

5 Data and estimation results

As we aim at presenting a new method for estimating of a policy rule more accurately, we follow the benchmark specifications of the discount rate as in Choi (1999), since his model appears to be, to our knowledge the most advanced model to date. Hence the Benchmark I. specification (equivalent to the core framework) reads

$$\Delta i_t^* = \alpha_0 + \alpha_1 \Delta i_{t-1} + \alpha_2 i_{t-1} + \alpha_3 y_{t-1} + \alpha_4 \Delta y_t + \alpha_5 \pi_{t-1} + \alpha_6 \Delta \pi_t + \varepsilon_t, \qquad (14)$$

and the extended Benchmark I. for some additional potential objectives, which we label as Benchmark II., can be written as

$$\begin{aligned} \Delta i_t = {} & \alpha_0 + \alpha_1 \Delta i_{t-1} + \alpha_2 i_{t-1} + \alpha_3 y_{t-1} + \alpha_4 \Delta y_t + \alpha_5 \pi_{t-1} + \alpha_6 \Delta \pi_t + \\ & + \alpha_7 m_t + \alpha_8 s_t + \varepsilon_t, \end{aligned} \qquad (15)$$

where $\Delta i_t^* = i_t^* - i_{t-1}$, and $\Delta i_t = i_t - i_{t-1}$.The lagged official discount rate as the last day rate is denoted as i_{t-1} and the lagged difference of the official discount rate as Δi_{t-1}. The lagged percentage deviation of the industrial production index (87=100) from its trend is denoted as y_{t-1}, where the trend is derived as a geometric interpolation of benchmark rates (see Choi 1999). Similarly, Δy_t is the first difference of the gap in industrial production. Further, π_{t-1} is the lagged deviation of the y-o-y inflation from the target of 2% and $\Delta \pi_t$ is its first difference. And finally, the m_t stands for the y-o-y monetary aggregate M1 growth as a deviation from its Hodrick-Prescott trend and s_t stands for the difference of the lagged official discount rate from the Federal funds rate target set prior to the discount rate announcement (for further details, see Choi 1999). The residuals ε_t are i.i.d.

However, in both specifications, (14) and (15), there might be a problem of misspecification, i.e., some other true determinants Z_t of the policy rate i_t might have been omitted, likely due to the fact that these are often inquantifiable in reality, such that $Z_t'\beta_0 \neq 0$ and $X_t'(Z_t'\beta_0) \neq 0$.

5.1 Benchmark regressions

We first present the replication of the results for benchmark regressions as they were obtained by Choi (1999) and then apply our method (ordered probit and censored regression) to the same data set and specification and interpret the differences. In addition, we present a simple ordinary least squares estimate since if there is no censoring (no misspecification error), the ordinary least squares will be the unbiased estimator. Table 3 contains the results for Benchmark regression I.

As we can see from the table, the column titled Heckman's procedure (two-sided tobit type II.) denotes the replicated regression of Choi (1999). Restating his findings in the second step of the estimation procedure, all coefficients except for y_{t-1} have the correct sign ($\alpha_2 < 0$ and $\alpha_3, \alpha_4, \alpha_5$, and $\alpha_6 > 0$) and all variables except for Δi_{t-1} and y_{t-1} are statistically significant. Turning attention to the combined ordered probit and censored regression procedure, the results in the second column reveal that by generally permitting for all observations to be potentially censored (which is more corresponding with reality), all coefficient including α_3(y_{t-1}) preserve their correct sign and all variables appear statistically significant at the 1 percent significance level. Besides, there are number of coefficients that are statistically different in magnitude from Choi's estimates (testing whether Choi's parameter point estimate falls into an interval estimate of ordered probit and censored regression): y_{t-1}, i_{t-1}, and Δy_t, suggesting a bias in parameters of the two-sided tobit type II.

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Table 3: Benchmark regression I.

	Heckman's procedure	OP-Cenreg	OLS
First step			
Δi_{t-1}	$.270_{(.301)}$	$.523^{*}_{(.286)}$	–
i_{t-1}	$-.077^{*}_{(.044)}$	$-.09^{**}_{(.041)}$	–
y_{t-1}	$.137^{***}_{(.03)}$	$.109^{***}_{(.028)}$	–
π_{t-1}	$.097^{***}_{(.067)}$	$.102^{***}_{(.035)}$	–
Δy_t	$.789^{***}_{(.137)}$	$.69^{***}_{(.119)}$	–
$\Delta \pi_t$	$.306_{(.252)}$	$.434^{*}_{(.239)}$	–
Tl	$-1.799^{***}_{(.296)}$	-3.8/-2.9/-1.9/-1.8	–
Tu	$1.39^{***}_{(.279)}$	1.3/1.4/2/2.1/2.9	–
Tl_i^*	–	-2.8/-1.9/-.9/-.3	–
Tu_i^*	–	.3/.9/1.6/2.2/2.8	–
LL	-129.55	-189.27	–
Second step			
Intercept	$.129_{(.111,.111)}$	$.047_{(.046)}$	$.048_{(.052)}$
Δi_{t-1}	$.424^{**}_{(.12,.106)}$	$.347^{***}_{(.068)}$	$.141^{**}_{(.063)}$
i_{t-1}	$-.03_{(.017,.019)}$	$-.045^{***}_{(.008)}$	$-.017^{**}_{(.008)}$
y_{t-1}	$-.001_{(.011,.011)}$	$.033^{***}_{(.005)}$	$.016^{***}_{(.006)}$
π_{t-1}	$.033^{*}_{(.013,.012)}$	$.039^{***}_{(.007)}$	$.021^{***}_{(.007)}$
Δy_t	$.153^{***}_{(.038,.036)}$	$.208^{***}_{(.021)}$	$.141^{***}_{(.025)}$
$\Delta \pi_t$	$.211^{*}_{(.10,.12)}$	$.197^{***}_{(.046)}$	$.096^{*}_{(.05)}$
IMR/σ	$.296^{***}_{(.032,.033)}$	$.183^{***}_{(.013)}$	–
$R^2/Nob/DW$	.87/57/.72	.78/247/1.98	.26/247/1.79

Note: stars denote significance levels as follows: *10%, **5%, and ***1%. In Heckman's procedure, two standard errors are reported in the second step. The first pertains to the original estimate by OLS, and the second is the adjusted standard error through White's (1980) procedure; see Footnote 7.

The results for Benchmark regression II. offer similar picture, as presented in Table 4.

Table 4: Benchmark regression II.

	Heckman's procedure	OP-Cenreg	OLS
First step			
Δi_{t-1}	$-.372_{(.35)}$	$-.09_{(.317)}$	–
i_{t-1}	$-.281^{***}_{(.061)}$	$-.286^{***}_{(.055)}$	–
y_{t-1}	$.109^{***}_{(.035)}$	$.078^{***}_{(.032)}$	–
π_{t-1}	$.175^{***}_{(.043)}$	$.171^{***}_{(.039)}$	–
Δy_t	$.731^{***}_{(.15)}$	$.62^{***}_{(.13)}$	–
$\Delta \pi_t$	$.529_{(.285)}$	$.672^{***}_{(.262)}$	–
m_t	$.136^{*}_{(.075)}$	$.149^{**}_{(.071)}$	–
s_t	$-.75^{***}_{(.133)}$	$-.73^{***}_{(.12)}$	–
Tl	$-2.69^{***}_{(.388)}$	-5.3/-4.1/-2.8/-2.7	–
Tu	$1.17^{***}_{(.319)}$	1.1/1.2/1.9/2.1/3.3	–
Tl_i^*	–	-4.7/-3.1/-1.6/-.5	–
Tu_i^*	–	.5/1.6/2.6/3.6/4.7	–
LL	-105.69	-160.76	–
Second step			
Intercept	$.112_{(.106,.104)}$	$.018_{(.04)}$	$.143^{***}_{(.048)}$
Δi_{t-1}	$.171_{(.115,.114)}$	$.157^{***}_{(.054)}$	$.009_{(.058)}$
i_{t-1}	$-.065^{***}_{(.017,.016)}$	$-.064^{***}_{(.007)}$	$-.053^{***}_{(.009)}$
y_{t-1}	$-.014^{*}_{(.011,.008)}$	$.004_{(.005)}$	$.003_{(.006)}$
π_{t-1}	$.041^{***}_{(.012,.010)}$	$.034^{***}_{(.006)}$	$.03^{***}_{(.007)}$
Δy_t	$.096^{***}_{(.038,.035)}$	$.078^{***}_{(.019)}$	$.098^{***}_{(.023)}$
$\Delta \pi_t$	$.263^{***}_{(.094,.095)}$	$.143^{***}_{(.041)}$	$.127^{***}_{(.046)}$
m_t	$.059^{***}_{(.028,.027)}$	$.038^{***}_{(.01)}$	.019(.011)
s_t	$-.205^{***}_{(.033,.031)}$	$-.165^{***}_{(.017)}$	$-.144^{***}_{(.019)}$
IMR/σ	$.246^{***}_{(.042,.043)}/-$	$-/.146^{***}_{(.01)}$	–
$R^2/Nob/DW$	.89/57/.89	.78/247/1.92	.43/247/1.59

Notes: see Table 3.

The benchmark regression II. includes two additional explanatory variables, i.e., the money gap m_t and the measure of the misalignment of the discount rate and the market rate, s_t. Heckman's procedure delivers coefficients that all have the correct sign except for y_{t-1} and all variables appear significant, except for Δi_{t-1}. In the case of the estimates derived through the combined ordered probit and censored regression, all coefficients have a correct sign and all coefficients are statistically significant, except for y_{t-1}. In addition, the point estimates are statistically different in all three variables: $\Delta\pi_t$, m_t , and s_t, which again points at the biasedness of parameter estimates in the two-sided tobit type II., due to ignoring the non-zero censored observations.

In fact, the problem of misspecification can be seen by comparing the parameters of the OLS with those of the ordered probit and censored regression procedure. If there is minimal censoring (nearly fully specified model), the parameter estimates from OLS approaches those from the ordered probit and censored regression procedure. This indicates that the model is correctly specified and there is negligible misspecification error. A test based on comparing parameter estimates can be easily devised, for instance, on the platform of the Hausman specification test (Hausman, 1978). One can construct the Hausman m-statistics and test the following standard hypothesis. Under the H0: both the OLS and ordered probit and censored regression estimates are consistent and asymptotically efficient, while under H1: only the estimates from the ordered probit and censored regression procedure are consistent.[8] In both benchmark regressions, the Hausman test suggests misspecification in OLS estimates: $\chi^2_6(31.42) = 0.00$ and $\chi^2_7(183.35) = 0.00$, respectively.[9]

A simpler test can be applied as well. Since the past dependent variable plays a prominent role in revealing policy rate inertia and at the same time serves as an instrument for omitted variables, one can test just the estimate of the coefficient α_1,i.e. $\widehat{\alpha}_1$ by OLS against the

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[8] The m-statistics reads: $m = \widehat{q}'(\widehat{V}_{OLS} - \widehat{V}_{OP-Cenreg})^{-1}\widehat{q}$, where $\widehat{V}_{OLS}$ and $\widehat{V}_{OP-Cenreg}$ represent consistent estimates of the asymptotic covariance matrices of $\widehat{\beta}_{OLS}$ and $\widehat{\beta}_{OP-Cenreg}$, and $\widehat{q} = \widehat{\beta}_{OLS} - \widehat{\beta}_{OP-Cenreg}$.The m-statistic is then distributed χ^2 with k degrees of freedom, where k is the rank of the matrix $(\widehat{V}_{OLS} - \widehat{V}_{OP-Cenreg})$. A generalized inverse is used, as recommended by Hausman (1978).

[9] In the benchmark regression II., the statistically insignificant variable y_{t-1} was dropped for evaluation of the Hausman test statistics.

consistent estimate $\widehat{\alpha}_1$by OP-Cenreg. In both benchmark regressions, the simple t-test would suggest bias of the OLS estimates (see Table 3 and 4).

6 Policy inertia on quarterly frequency

Under the assumption of rational expectations of the financial market participants, the future policy rate changes of the monetary authority should be more predictable in the more distant future, the more the policy maker applies policy rate smoothing. Rudebusch (2002) provides evidence of a low portion of forecastable variability in policy rates by the market expectations (as many other authors, for instance Mankiw and Miron, 1986 or Fuhrer and Moore, 1995) and displays his evidence as proof of, in fact, the non-inertial policy rule (claiming that shocks are correlated and monetary authority is free of inertia). This argument, however, seems to be lacking wider recognition. The vast majority of the literature, for instance Goodhart (1999), Clarida et al. (2000), or McCallum and Nelson (1999), find high policy rate inertia in empirical investigations using various policy rule specifications.

6.1 Term structure evidence on policy inertia

In this section we display evidence that failure of the rational financial market participants to predict policy rate changes in the distant future might not be a clear proof of non-inertial behavior of the monetary institution. We put forward the observation of low forecastable variability of future policy rates by the monetary authority itself, by using an endogenous policy rate trajectory for predicting distant future policy rate changes. We assert that if the central bank itself can not predict future policy rates, despite very high degree of smoothing in the endogenous policy rate trajectory, how we could expect the market, even though rational, to do so. On the contrary, we believe that market rationality should be judged against the endogenous policy rate trajectory verbally communicated to the market by the central bank and not against the actual outcome of the policy rate at distant horizons.

6.1.1 Marginal regressions

We start with evaluating the forecastable variance of the future changes in policy rate by the market. We take the term structure of the forward rate agreements and test the predictability of the policy rate changes in a variety of forecast horizons. The following relation was tested using quarterly data covering the unconditional inflation targeting in the Czech Republic from October 2003 throughout January 2006:

$$i_{t+j} - i_t = \alpha_j + \beta_j (i^{FRA}_{t,t+j} - i_t) + \varepsilon_t. \tag{16}$$

The j stands for quarters and runs from one to four. The three month (interbank) interest rate from forward rate agreements set at time t for the period starting in j quarters is denoted as $i^{FRA}_{t,t+j}$. The inter-bank spot rate is denoted by i_t, α_j represents the average term premium for the respective period $t + j$ and β_j is the coefficient representing the relation between the realized and expected change in the rate.The error term ε_t is *i.i.d.*

We opted for estimating the slope of the yield curve at every particular horizon rather than tangency to it, since in this specification we can minimize the influence of time varying term premia embedded in the forward contracts. In all regressions, there is only one average term premium, which is captured by α_j. Such a specification is thus advantageous for the purpose of sensing the predictability of the future interest rates.

In order to perform a complementary test for the central hypothesis that *if the central bank smooths its policy rates, a large share of the variability of the policy rates at more distant horizons should also be forecastable*, we collected data for endogenous trajectories of the policy rate at each quarterly inflation-forecast round and evaluated the forecastable variance in the realized policy rate changes. The endogenous trajectory is based on the policy rule with a smoothing coefficient of .75. The smoothing in the policy rule seems to be rather close to the maximum smoothing of .8 that is justified by reasonable calibration of theoretical models. Rudebusch (2002) provides an interval $0 - .8$ for optimal smoothing, which is also consistent with the findings by Woodford (1999) or Levin et al.(1999), for instance. Therefore, a small portion of the future policy rate variability explained by the endogenous policy rate trajectory would be contradictory evidence leading to rejection of the central hypothesis. Hence, we estimate the following equation for the central bank:

$$i_{t+j} - i_t = \alpha_j + \beta(i^{ET}_{t,t+j} - i_t) + \varepsilon_t, \tag{17}$$

where $i^{ET}_{t,t+j}$ represents the future policy rate from the endogenous policy rate trajectory (mapping three months interbank rate) set at time t for j quarters ahead.

And finally, we also tested whether the market expectations are rational with respect to the monetary institution, i.e., whether the market successfully anticipates the endogenous policy rate trajectory of the central bank. For this purpose, we estimate another similar equation:

$$i^{ET}_{t,t+j} - i_t = \alpha_j + \beta(i^{FRA}_{t,t+j} - i_t) + \varepsilon_t. \tag{18}$$

6.1.2 Data and estimation results

Making use of the data from the internal documents of the governing council of the Czech National Bank about macroeconomic unconditional projections (containing the endogenous policy rate trajectory for j quarters ahead), which are being made public with a delay of six years, and data from the Bloomberg database about the forward rate agreements at corresponding frequency to match the quarterly projections, we estimated the relations (16) through (18).

The first result that follows from the regression (16), as displayed in Table 5, is that the interest rate at distant horizons is rather unpredictable by the market. In particular, we found a relatively large portion of the explained variability of the future realized policy rate development only at horizons up to two quarters. An exclusively high portion of explained variability can be recorded in the first quarter and somewhat lower in the second; however, as we move towards more distant quarters the share of explained variability drops literally to zero. Also, the slope coefficient is declining from unity rather rapidly, considering its insignificance already in the third quarter.

Table 5: Predictability of future rates

	*eq.*16			*eq.*17			*eq.*18		
horizon	slope	const	a-R^2	slope	const	a-R^2	slope	const	a-R^2
t,t+1Q	.89***	.02	.95	.73***	−.001	.56	.79***	.02	.59
	(.07)	(.02)		(.22)	(.06)		(.22)	(.06)	
t,t+2Q	.98***	−.09	.61	.7**	−.02	.51	1.02***	−.1	.66
	(.26)	(.09)		(.23)	(.1)		(.23)	(.09)	
t,t+3Q	.53	−.21	.17	.36	−.11	.10	1.06***	−.17	.67
	(.34)	(.17)		(.27)	(.17)		(.24)	(.18)	
t,t+4Q	.27	−.23	.001	.09	−.12	.001	1.11***	−.21	.73
	(.41)	(.29)		(.33)	(.23)		(.22)	(.15)	
t,t+1H	.93***	−.04	.7	.71***	−.01	.56	.91***	−.04	.64
	(.145)	(.044)		(.148)	(.054)		(.16)	(.05)	
t,t+2H	.37	−.23	.09	.22	−.14	.02	1.1***	−.19	.73
	(.232)	(.143)		(.191)	(.127)		(.149)	(.09)	

Notes: Standard errors are given in parentheses. Number of observations by horizons (1Q, 2Q, ... 2H) are as follows: eq B1 and B2: 9, 9, 8, 7, 18, 15; eq B3: 9, 9, 9, 9, 18, 18. The stars denote significance as follows: *** 1%, ** 5% and * 10%.

In order to control for the relatively small sample, we split the sample into two and estimated separately the predictability in the first half of the year ahead and in the second half and hereby confirmed our findings from quarter-by-quarter regressions; see bottom of Table 5.

The second result follows from the estimation of regression (17), presented in Table 5. It stipulates that the endogenous policy rate trajectory is not predicting the variability of the future policy rate any better than the market. The proportion of explained variability to total variability in the policy rate plummets to zero relatively quickly, similarly to the case of financial market forecasts. The slope coefficient diverges from unity relatively quickly as well.

This finding unveils that the central bank changes the trajectory relatively often, due to various shocks in the determinants and even a high smoothing term is not sufficient to keep the distant future predictable. On the contrary, it might be well the case that high smoothing induces high predictability at shorter horizons, due to the fact that it, in some sense, freezes the rates on their past values but does not provide a sufficient anchor for distant horizons. Even more specifically, in many standard monetary models, where shocks are persistent, see CNB (2003), time also possibly plays a role. The less the central bank responds with rates to a shock now, the more it has to respond in the future in order to reach the target of price stability. This would paradoxically even increase the variability in the projected future rates and consequently would make future rates at the distant horizons be less predictable.

The results for the final equation (see Table 5) show that the predictability of the endogenous trajectory by the market is very high along the entire considered horizon of the monetary policy. This suggests relatively effective communication of the governing council in directing the market regarding the endogenous trajectory, considering that the implicit policy trajectory is not directly shared by the central bank with the market.

The portion of explained variability reaches $65 - 75$ percent. In addition, the slope is rather close to unity and statistically significant, much along the horizons. This result shows that the market is rational with respect to the monetary authority that sets the policy rates. However, the assumption that a rational market predicts the future changes in policy rate with no systematic bias turns out rather inappropriate for this type of process. Rather, the rationality should be measured against the monetary policy maker, i.e., the endogenous policy rate trajectory, where we also find it. This further implies that the hypothesis that poor performance of the market in predicting variability in the future policy rate is a sign of low smoothing in the policy rate is not supported as we find it, even though we know for certain that the endogenous policy rate trajectory contains a very high smoothing coefficient of .75.

Thus our empirical verification lends support to the findings by Soederlind et al. (2004), who showed that monetary policy shocks are large and cause low predictability of future policy rates.

6.2 The evidence on overstated policy inertia

Even though the term structure is not likely to provide us with sufficient evidence on misspecification of empirical policy rules (wrong inclusion of past dependent variable, or at least overstated size of empirically estimated policy rate inertia), the robust estimator in the preceding sections (section 2 and 3) applied to monthly data (chapter 4 and 5) has shown that there is indeed a misspecification issue in monthly data that is responsible for overstating the true policy rate inertia in least squares estimates. It might be useful to analyze whether the misspecification issue appears significant on the quarterly frequency as well, since the operational frequency is usually quarterly, even though the policy rate decisions take usually place monthly.

First, we have estimated the policy rule for the Czech National Bank, equation (13) with OLS on quarterly frequency and compared with the converted monthly estimates (section 4) into quarterly frequency.[10] The results for policy inertia estimate are displayed in Table 6.

Table 6: Quarterly frequency policy inertia

	i_{t-1}
OLS– quarterly data estimation	$.82^{***}_{(.05)}$
OLS – derived from monthly estimate	$.82^{***}_{(.06)}$
Two-Sided-HEKIT– derived from monthly estimate	$.75^{***}_{(.07)}$
CENREG$^{a)}$– derived from monthly estimate	$.76^{***}_{(.06)}$
CENREG$^{b)}$– derived from monthly estimate	$.76^{***}_{(.03)}$
MODEL$^{e)}$	.75

[10] The conversion of estimates of inertia on monthly frequency into quarterly frequency is based on the following equivalence: $\rho_m^3 = \rho_q$, where ρ_m and ρ_q is the inertia estimate on monthly and quarterly frequency, respectively.

Notes: For a), b) and e), see notes to Table 2. Monthly m0 corresponds to the cube root of the quarterly m_0. Stars denote significance as follows: *** 1%, ** 5% and * 10%

As it appears in the table, the OLS quarterly estimate, as well as the converted monthly OLS estimates, unveil the misspecification issue by overstating the policy rate inertia by .7. In comparison to the model's calibration in quarterly frequency, estimated coefficients by censored regression show rather equal smoothing ($.75 - .76$ vs. .75), which confirms that the two-stage procedure of ordered probit, and censored regression performs well in identifying the consistent estimates of the true determinants of policy rule (among them policy smoothing as well).

The evidence from the Czech Republic suggests that the misspecification issue is present on both monthly as well as quarterly frequency. This might be connected to the fact that the Czech Republic is a small open economy, where some other factors can be assigned temporary importance, such as the exchange rate development. Such omitted variables might exhibit persistence and therefore do not average out by the construction of quarterly data from monthly data. Therefore, these variables might cause misspecification of the policy rule even on the quarterly frequency.

Now, let us consider the situation in a large and relatively closed economy, the U.S. economy. We have sorted the evidence on inertia estimates in various time samples and methods used for estimation of the FED's policy rule in a core framework specification.

We focus, in particular, on the systematic dependence of the size of estimated inertia on the method used. We suppose that the policy inertia will be lower for estimators such as GMM, IV, 2SLS or the two-stage procedure of ordered probit and censored regression, compared to the OLS, since these estimators either use instruments B_t so that $B_t'(Z_t'\beta_0) = 0$, or account for specification error directly, as the two-stage procedure.

We make use of 37 point estimates by Amato and Laubach (1999), Clarida et al. (2000), Lansing (2002), Levine et al. (2003), and Rudebusch (2002) and investigate the relation between estimated policy rate inertia in a core framework specification (inflation, output, and neutral policy rate). We study the systematic effect of the method used (ME= 1 if GMM, IV, 2SLS, OP-Cenreg and 0 for OLS), controlling for differences in sample and various definitions of variables. The time dummy is defined as follows: TM = 0 for the longest periods such as 1960-1979 or 1966-1997 and 1 for more recent periods, 1974-1995 or 1983-1997. Similarly, the indicator dummy is defined as follows: IN = 0 for GDP, CPI and GDP deflators, and 1 for alternative indicators such as unemployment and nonfarm indicators of output and its deflator. In this way, we defined a benchmark (the constant in our meta analysis) of OLS applied to GDP, its deflators or CPI, and the sample period of 1960-1979 or 1966-1997.

The results follow ($adj - R^2 = .39$, $DW = 2.01$)

$$\widehat{\rho}_i = .79^{***}_{(.02)} - .08^{***}_{(.02)} ME_i + .042^{*}_{(.025)} TM_i + .018_{(.02)} IN_i,$$

where $\widehat{\rho}_i$ is the $i - th$ estimate of inertia. A parsimonious regression (which improves the significance of estimates) is then ($adj - R^2 = .40$, $DW = 1.99$)

$$\widehat{\rho}_i = .80^{***}_{(.02)} - .084^{***}_{(.02)} ME_i + .043^{**}_{(.015)} TM_i.$$

It follows from the results that the bias in smoothing coefficient is significantly affected by the estimation method used. Using OLS for policy rule estimation biases upwards the policy rate inertia by .08 on quarterly frequency. The data sample turned out to be also a systematic factor for policy inertia. Namely, regression on more recent samples show typically higher inertia, ceteris paribus. In fact, we found that Volcker and Greenspan chairmanships were characterized by higher inertia by .04 than the average inertia. The alternative definitions of objectives did not prove to significantly influence the estimates of inertia in the literature.

Based on the results from the meta analysis, we draw the conclusion that the specification error is likely present at the quarterly frequency as well as at monthly frequency, even in a large, relatively closed economy such as the U.S. The size of overstatement of inertia is, however, of similar size (slightly lower) to the one found in the monthly regressions, converted into quarterly frequency, i.e., .14 (see section 4 and footnote 10).

7 Conclusion

In this paper, we contribute to the debate on policy rate inertia by pointing at the specification error of simple, operational policy rules of Taylor type. We develop an estimation procedure that is robust to marginal misspecification and provide an empirical application. We consider two extremes, a small open economy (the Czech Republic) and a large rather closed economy (the U.S.). We find robust evidence on specification error in the most commonly estimated specification of Taylor type policy rule in both countries. The misspecification biases coefficients in ordinary least squares regression and hence biases the inertia estimate upwards. In the analysis of the effect of data frequency, we find the bias being slightly smaller at quarterly frequency compared to monthly.

Placing our evidence on overstated monetary policy inertia by least squares into the context of practical policy making, we open a discussion on the consequences of least squares learning by a central bank. The learning based on least squares might cause policy to become increasingly inert over time. Ignoring the policy rule specification error, a central bank would believe that least squares describe their past actions and would recalibrate its models following the positivistic approach advocated by Taylor (1993). However, since least squares estimates are systematically biasing the inertia upwards, the future policy would become excessively inert.

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