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The methodology for assessing interest-rate policy rules: a reply

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This note is a brief reply to Watts (2021), who has been critical of the conceptual foundations and methodology in a discussion of the impact of different interest rate policy rules on inflation in Smithin (2020). The reply concludes that the case for a 'zero real policy rate of interest' (ZRPR), rather than a 'zero interest rate policy' (ZIRP), emerges unscathed.

Keywords: ZRPR, ZIRP, real interest rate rules, inflation instability, income distribution

JEL codes: B22, E43, E52, E58

1 INTRODUCTION

I am most grateful to the editors of the *European Journal of Economics and Economic Policies* for allowing me the opportunity to reply to Martin Watts's 'The methodology for assessing interest-rate policy rules: some comments' (Watts 2021), which is essentially a critique of my 'Interest rates, income distribution, and the monetary policy transmissions mechanism' (Smithin 2020). The latter paper was published in the November 2020 memorial issue of *EJEEP* in honour of the late Professor Basil J. Moore, a respected pioneer in the post-Keynesian theory of endogenous money and 'horizontalism'. In this reply, I would like to focus not so much on the mathematical details and/or questions of model specification which seem to have been the main focus of Watts's comment, but rather on the 'big picture', if I can put it that way. That is, on the fundamental questions of political economy and economic policy that I take to be involved. Watts has also made a number of other contributions on the technical issues in the past few years (Watts 2016; 2018), to which I have responded and made appropriate adjustments and emendations when necessary (Smithin 2016b; 2018; 2022). The reader is referred to the existing literature for the details.

From my point of view during this exchange it has seemed that one of Watts's main concerns has been to defend the policy proposal of a 'zero interest rate policy' (ZIRP), put forward by the school of modern monetary theory (MMT), against my alternative suggestion of a 'zero real policy rate of interest' (ZRPR). Watts is a core member of the MMT group (Kelton 2020; Mitchell et al. 2019; Wray 2012).¹ The ZIRP proposal is that the nominal policy rate of the central bank should be set at zero, whereas a ZRPR

1. In more recent correspondence, Watts has stated that he is not, in fact, an uncritical advocate of a ZIRP. But, in that case, I must confess it is difficult for me to understand exactly what the discussion has been about.

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would have the ‘real’ inflation-adjusted policy rate set at zero. I do not think, however, that a truly effective defence of a ZIRP can be achieved by finding mathematical errors, sign errors, misspecifications, typos, etc., in the various individual papers I have published. As a matter of fact, I am indeed very grateful that Watts has been prepared to go through my work in such detail and to point out any mistakes that can be found. (This is the only way to learn, improve, and ultimately progress!) However, I doubt that anyone reading Martin Watts’s commentaries on my work, including the most recent, will really be able to follow how all this relates to either the rules versus discretion argument in general, or to the further choice as to whether as matter of policy a real or nominal rule would be best. There must be some basic economic principles that ultimately decide this question, however well or otherwise individual authors express themselves. In my opinion, any resolution must take into account the obvious point that a nominal rule *per se* is compatible with any level of the inflation-adjusted real rate of interest (Lavoie/Seccareccia 1988; Smithin 1994; 1996).

Apart from any differences with the MMT school on the merits of a ZIRP versus ZRPR (and on some other specific policy issues) I believe that I share much of their general outlook on macroeconomic theory and policy. For example, I concur with what I have elsewhere called the ‘logically unassailable’ proposition of MMT (Smithin 2022), that the central government of an economy with its own sovereign currency, and either a floating exchange rate or a ‘fixed-but-adjustable’ exchange rate, faces no binding financial constraints.² Also, on the following two points (to quote Watts/Pantelopoulous 2022, in a forthcoming paper), that ‘interest rate policy should be subject to rules ... rather than remaining discretionary’ and, as a corollary, that ‘agile fiscal policy should be the main policy instrument’.³

In what follows, Section 2 describes the two main reasons why I have argued that a ZRPR rule of some kind would be a superior monetary policy rule to a ZIRP. Section 3 then briefly recapitulates the debate so far. Section 4 deals with some of the novel arguments introduced in Watt’s most recent contribution, and Section 5 offers some concluding remarks.

2 NOMINAL INTEREST RATE STABILITY DOES NOT GUARANTEE REAL INTEREST STABILITY, NOR A ‘FAIR’ DISTRIBUTION OF INCOME

In my paper in the memorial issue of *EJEEP* (Smithin 2020) it was suggested that a ZRPR (or for that matter any other real interest rate rule) on the part of a sovereign government would be conducive to inflation stability (but not price stability), financial stability, and general economic stability. *Low* or zero real rates of interest also promote higher economic growth, full employment, and higher real wages. I also made the further case that a ZRPR is a ‘near-optimal’ setting of the real policy rate of interest from the point of view of income distribution. That is, it will achieve as close an approximation as is possible to a fair distribution of income in a particular sense. The concept

2. I have been arguing in this way for a very long time, at least since the completion of my PhD thesis in 1982 (Smithin 1982). I do not claim that I fully understood all of the open-economy considerations at that particular stage, but in the closed-economy context the argument was already quite clear.

3. Watts (2021) complains that in my 2020 paper (Smithin 2020) I do not mention fiscal policy until the final page. To accept this as a criticism, however, would be to ignore much of my other published work and, by now, a quite extensive correspondence with Watts himself.

of ‘fairness’ invoked here is similar to, but not identical with, that attributed by Lavoie/Seccareccia (2016) to Luigi Pasinetti. In their definition, Pasinetti’s ‘fair’ rate of interest would have ‘the nominal rate of interest ... equal to the rate of growth of labour productivity plus the rate of inflation’ (ibid.: 211). A ZRPR is therefore less generous to the so-called rentier interests than was Pasinetti, but far more so than Keynes in *The General Theory* who advocated the ‘euthanasia of the rentier’ (Keynes 1936). For the purposes of the present discussion, the important point to note is that all of these proposals about fairness relate to the real distribution of income, real interest rates, real wages, etc. On the other hand, a ZIRP, in and of itself, is conducive neither to inflation stability nor to real economic stability – *nor* is it able to achieve the putatively fair distribution of income.

Using the same notation as that in Smithin (2020), we can let the symbol i_O^4 stand for the nominal policy rate of interest of the central bank and lower-case p for the inflation rate. Thus, a ZIRP may be characterized by setting:

$$i_O = 0. \quad (1)$$

On the other hand, a version of a real interest rate rule which would be transparent and easy to implement would be something along the lines of:

$$i_O = r_O + p_{-1}, \quad (2)$$

where r_O is the target for the real policy rate. If the target were zero, as would be the case in a ZRPR, this would reduce to:

$$i_O = p_{-1}. \quad (3)$$

The alternative rule, therefore, is simply that the nominal policy rate should be set equal to the inflation rate that was reported in the previous period.

At the time of writing the federal funds rate in the USA stands at 0.01 per cent (effectively zero) and the most recently reported inflation rate, according to press reports, has shown a sharp increase to around 5.4 per cent. Accepting these figures as accurate, this means that the inflation-adjusted real policy rate stands at (–) 5.39 per cent. In the last calendar year the inflation rate was around 1.4 per cent so that under a ‘near-ZIRP’ the real policy rate at the time would have been (–) 1.39 per cent. If the inflation rate were to increase to (say) 7.4 per cent in the future (a not unlikely scenario given the multiple possible sources of additional inflation in the endogenous money environment) the inflation-adjusted real policy rate would fall still further, to (–) 7.39 per cent. On the other hand, supposing there were to be a *deflationary* shock such that prices were *falling* at a rate of 5.4 per cent or 7.4 per cent, the effective real policy rate of interest would *rise* to (+) 5.41 per cent and (+) 7.41 per cent respectively. This is not a recipe for stability.

The alternative ZRPR would have had the nominal policy rate at 1.4 per cent in the previous period, 5.4 per cent currently, and 7.4 per cent in the future. Therefore, there is always at least some monetary response to inflationary or deflationary pressure.⁵ All the while, however, the effective real policy rate remains unchanged.

4. The central bank policy rate is usually an ‘overnight rate’ of some kind.

5. In the deflationary case there would need to be recourse to the currently fashionable notion of ‘deep negative’ policy rates (Watts/Pantelopoulou 2022).

3 THE STORY SO FAR

As I say, I basically agree with Watts's general argument that individual mathematical exercises, such as analytical solutions (where possible) to low-order difference or differential equations, are in themselves insufficient to decide the key questions of political economy. In fact, I would go further and argue that, nowadays, given the widespread use of numerical methods in the contemporary research environment (Collis 2018; Smithin 2022), the mathematical exercises are really no more than heuristic devices that may, or may not, be useful in explaining the underlying arguments to one's colleagues and students. Nonetheless, there is one partial mathematical result which up to now has not been disputed. This is that when/if financial markets are operating along the lines of Keynes's (1936) theory of the 'speculative demand for money', and the real rate of interest *is* allowed to change because the nominal policy rate is not adjusted appropriately, the rate of inflation cannot be stabilized.

I had originally made this argument, with a particular mathematical specification of the speculative theory, in a paper published in 2016 (Smithin 2016a) which was intended as a commentary on an exchange between two prominent members of the MMT school, namely Tymoigne/Wray (2015), and their 'critics' as represented by Palley (2015a; 2015b). The relevance of this result for a ZIRP policy in the given context is not disputed. At the same time, however, I also made the familiar assertion from my own writings that an alternative real interest rate rule would suffice to promote both inflation stability and general economic stability. This was not perhaps strictly pertinent to the exchange between Tymoigne/Wray and Palley, as neither camp advocates a real interest rate rule, but nonetheless seemed to be in place as an aside to the main discussion. Somewhat to my surprise, however, this last statement did attract some attention and debate from Martin Watts and others.

For instance, Watts has noticed that in the specific circumstances under discussion the simplest possible version of a real rate rule, that is a rule of the form $i_O = r_O + p$, will also fail to achieve the goal of inflation stability. Note that this is slightly different from the rule in (2) above, since to be able to implement it it would be necessary to make some assumptions about what the current inflation rate is actually perceived to be. Watts's contention does turn out to be formally (mathematically) correct in this particular case. Nonetheless, in terms of the underlying political economy, I don't think that this materially affects the general case for adopting a real rate rule. In the first place, it obviously does not overturn the argument *against* a nominal peg in the same circumstances. Secondly, and as shown in Smithin (2016b; 2018; 2022), the formal problem is easily remedied by the adoption of a real rate rule that is only slightly more sophisticated than the simplest version, but which nonetheless has the desired effect of stabilizing the real policy rate at the target level (zero or otherwise).

In correspondence, and in exchanges of draft papers and notes, etc., Watts has also made a more general critique of 'incomplete' models of the inflationary process by which, as I understand it, is meant precisely exercises of the sort undertaken in Smithin (2016a) and Smithin (2020), with relatively few equations or just a single equation. The point seems to be not only that a single mathematical exercise in itself is insufficient to decide the key questions of political economy (which is unobjectionable, and with which I fully concur), but also that even when one or more such exercises adds to the case against the policy that is favoured, the results should be discounted if not part of a complete and fully specified macroeconomic model. In the absence of any positive case made for the alternative recommendation, that argument does not seem to me to be methodologically sound. It ignores the wider context of the various 'partial'

exercises already undertaken in the literature, a context which does include a number of fully specified macro models such as my ‘alternative monetary model’ (AMM) in Smithin (2018; 2022), as well as contributions by other authors.

4 THE PHILLIPS CURVE, ADAPTIVE EXPECTATIONS, NAIRUs, ETC., IN MAINSTREAM MACROECONOMIC MODELS OF THE LATE TWENTIETH CENTURY

A novel feature of Watt’s most recent contribution (Watts 2021) is an example which does provide a positive argument that a ZIRP is compatible with inflation stability. Watts is able to solve a second-order difference equation which is stable for a certain range of parameters. This is achieved by employing a well-known device from the mainstream macroeconomics and monetary literature of the 1970s and 1980s, namely the assumption of an ‘adaptive expectations’ adjustment mechanism. However, I cannot think that this is particularly relevant as a commentary on my own work, as I do not accept that theoretical framework as a reliable guide to macroeconomic policy. I have provided a detailed comparison of my AMM and what I call the ‘textbook’ approach in a number of places, including most recently in Smithin (2022). Suffice it to say that I reject most of the textbook apparatus, including the focus on short-term inflation expectations itself, the idea of the ‘expectations-augmented Phillips curve’ or any sort of Phillips curve, notions of the ‘natural’ rates of economic growth, the rate of interest, and unemployment, and that of a unique NAIRU (non-accelerating inflation rate of unemployment).⁶

In effect, the use of an adaptive expectations mechanism simply adds a further extraneous difference equation to the dynamic system (which is why Watts is then able to proceed to the analytic solution for a second-order equation). As I recall, the various problems with adaptive expectations were widely discussed in the mainstream macroeconomics literature of the time, including the argument that an adaptive expectations mechanism is an arbitrary addition to the theoretical model, which generates neither the same expectation of inflation generated by the model itself nor those actually held by a consensus of the agents in any historical setting.⁷

On this same topic, however, I think that I would now readily concede that my treatment of the question of expectations in Smithin (2020) was confusing and misleading. This is therefore yet another instance in which I am quite sure that Watts’s commentary will have the effect of improving the quality of the debate in future. I ‘muddled the waters’ in a number of ways. For example, I made reference in a footnote to the mainstream ‘Taylor rule’, and Taylor’s idea of using the currently observed inflation as a ‘proxy for’ the expected inflation rate. The point that I was trying to make here was not an endorsement of this procedure, but simply that the Taylor rule itself was effectively couched in real terms. More importantly (and presumably because it seemed to me to be convenient to do so at the time), I did in fact employ a ‘forward-looking’ version of a ZRPR in the mathematical exposition. This was formally correct in that I treated the expected inflation rate as a *datum*. It was taken to be simply the in-house forecast of inflation published by the central bank

6. As far as the latter concept is concerned, I have usually tried to model macroeconomic phenomena as a series of cycles around positions of a moving equilibrium, with (in effect) each of the equilibria themselves representing a ‘NAIRU’. However, there is no unique NAIRU or natural rate, and no set patterns of co-movements of the individual macroeconomic variables.

7. See Collis (2018) for a discussion of the techniques whereby historical time series of the expectational variables can, in fact, be generated in an empirical exercise.

itself (regardless of the method by which they had arrived at it). The relevant difference equations will therefore have the same dynamic properties whether projected forwards or backwards. Nonetheless, I can certainly see how this specification would lead to confusion, taken out of context. More generally, as already explained above, I think that we should mainly focus on the behaviour of the *ex post* or inflation-adjusted real rate of interest, rather than that of the forward-looking expected real rate of neoclassical economics based on short-term inflation expectations. I take the view that ‘investment’ (or better, ‘firm’) spending depends mainly on the state of long-term expectation (as in Keynes) – and on *realized* profitability – which in turn depends on the inflation-adjusted real rate of interest.

5 CONCLUSION

I have found this ongoing exchange of views with Martin Watts very helpful in allowing me to deepen my understanding of the issues and to clarify my own position. Nonetheless, I think that the case for a ZRPR rather than a ZIRP emerges unscathed. Even in the situation explored in Watts’s most recent contribution, which is a case where a ZIRP is compatible with stability, the same would be true of a ZRPR and therefore provides no grounds for preferring a ZIRP to a ZRPR. There would still be grounds for preferring a ZRPR to a ZIRP, however, as this continues to be a ‘near-optimum’ monetary policy from the point of view of fairness in income distribution. In most of the other cases (some of which are discussed above), the ZRPR is able to handle other potential sources of inflationary instability with which a ZIRP is unable to cope.

Finally, I do not think that choosing a ZRPR over a ZIRP would have any implications for the overall viability of the MMT approach to monetary theory and policy.

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