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ORIGINAL PAPER

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A bootstrap-corrected causality test: another look at the money–income relationship

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Abstract Previous studies of the causal relationship between money supply and real output are based on asymptotic distributions. If the assumption of normality is not fulfilled and if ARCH effects are present, asymptotic distributions perform inaccurately. In this paper, we reinvestigate the potential causal relationship between money and output by applying an alternative methodology based on the leveraged bootstrapped simulation techniques using data from Denmark, Japan, Sweden, and the US. We find unidirectional causality from money to output for the sample countries except for Sweden for which causality is bi-directional. This finding of unidirectional causality between money and output supports monetary business-cycle models and reveals one important policy implication—that is, in looking for the sources of output fluctuations, money might be a major factor.

Keywords Money supply · Output growth · Granger causality · Leveraged bootstrap simulation

JEL Classification E51 · C32

1 Introduction

A well-known area of debate in macroeconomics literature has been the precise relationship between money and output (Blanchard 1990; Lucas 1996; Sargent 1996).

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The direction of causation between money and output is an important issue for many policymakers and economists since it is necessary if appropriate monetary policy is to be formulated. Does money influence output or is it vice versa? The purpose of this study is to explore the direction of causation between money supply and output by applying an alternative methodology.

There are two different theories, which explain the direction of causation.[1] The first, monetary-business-cycle theory, postulates that growth of the money supply causes output growth. Models in this category are known as new Keynesian models or sticky-wage models (Gray 1976; Fisher 1977; Taylor 1980). Another explanation put forward by monetary-business-cycle theorists for the non-neutrality of money stems from a class of models known as imperfect-information models (Lucas 1972, 1975; Barro 1976). According to these models, monetary changes can have real effects because individuals have limited information and thus may misperceive aggregate and relative changes.

The second, real-business-cycle theory, posits an opposite direction of causation between money and output. Real-business-cycle-models assign a causal role to real economic activity in affecting money supply. That is, an increase in output causes an increase in money, not vice versa. Shocks can affect supplies of real resources and relative prices that individuals expect to face over time. These shocks include technological innovations, other sources of productivity changes, environmental conditions, the world price of energy, developments in the labor market, and government spending and taxes. Thus, in real-business-cycle-models, output growth is determined by real shocks, not by money growth (Kydland and Prescott 1982; Long and Plosser 1983). Money is related to output because it reacts to the same real shocks that output responds to.

This paper seeks to find out which of these two theories mentioned above is more in accord with the data for Denmark, Japan, Sweden, and the US for the period 1961–2000. The choice of these countries is justified by the fact that these economies differ in size and are market-oriented economies with relatively unregulated capital accounts. Furthermore, the sample period does include boom period with improved government finances, external net borrowing, and full employment and a bust period with rapidly increasing unemployment and deteriorating government finances. Thus, the sample is not tranquil in a way that could favor any theory mentioned above.

This study is a first attempt to use an alternative methodology based on the leveraged bootstrapped simulation techniques to test for the causal nexus between money and output.[2] The procedure performs well when data generating process is characterized by non-normality, and when the sample size is relatively small.

The rest of this paper is organized as follows: Section 2 reviews previous empirical studies on money–output relationship. Section 3 describes the data set and methodology, and also presents estimation results. Conclusions and policy implications are presented in Section 4.

[1] For a survey of literature see Ahmed (1993) and Holmes and Hutton (1992).

[2] Granger's definition of causality is used here. That is, the objective is to find out whether the past value of one variable can improve the forecast of another variable or not.

2 Previous studies

The money-output relationship has been documented by rigorous empiricism in a number of studies employing a variety of data sets. Sophisticated empirical models have been devised to examine the implication of anticipated and unanticipated (Barro 1977), positive and negative (Cover 1992; Thoma 1994), and large and small monetary shocks (Ravn and Sola 1996) on output fluctuations. While some studies have supported unidirectional causality, running from money to income (e.g., Sims 1972; Hafer 1982; Devan and Rangazar 1987), other studies have provided evidence on unidirectional causality, running from income to money (e.g., Williams et al. 1976; Putman and Wilford 1978; Cuddington 1981; King and Plosser 1984). There is also empirical evidence of bi-directional causality between money and output for a number of countries (e.g., Hayo 1999).

However, the existing empirical evidence based on testing of causality between money growth and output growth is, at best, mixed and contradictory (Ahmad 1993). The instability of results in Granger causality test is likely to stem from whether the variables are modeled as (1) (log-) level variables or growth rates (Christiano and Ljungquist 1988) or (2) as trend—or difference stationary (Hafer and Kutan 1997).[3] Christiano and Ljungquist (1988) argue in favor of using level variables, since they find that power of the tests on growth variables is very low. Hafer and Kutan (1997) assert that if the variables are assumed to be trend stationary, money Granger causes output and if the variables are assumed to be difference stationary, output Granger causes money. Finally, for a large sample of countries, Hayo (1999) shows that, even after applying similar estimation methods, sample periods, and models, differences in Granger causality test results across countries do not boil down to the choice of variables in levels versus variables in growth rates.

3 Data, methodology, and estimation results

The data used in this study is yearly for the period 1961–2000 pertaining to the following variables—nominal money plus quasi-money and real GDP in domestic currency for Denmark, Japan, Sweden, and the US. In order to capture the effect of inflation, CPI is included in each model. Furthermore, the nominal short-run interest rate for each country is introduced into the system as a control variable for monetary policy as suggested by Sims (1980). The source of all data is *IFS CD-ROM*.

In this paper, our interest is focused on the causal nexus between the money and income. By causality we mean causality in the Granger sense. A variable Granger causes another variable if including it in the information set will improve the forecast of the second variable. It is widely accepted now that in the vector autoregressive (VAR) framework, the Wald test for testing the Granger causality may have non-standard asymptotic properties if the variables considered in the VAR are integrated. Toda and Yamamoto (1995) proposed solutions based on lag

[3] Other potential explanations for these mixed results could be the usage of different sample periods, the dimension of the VAR model, and the estimation methodology.

augmentation of the VAR model that guarantees standard χ^2 asymptotic distribution for the Wald tests performed on the coefficients of VAR processes with I(1) variables.

However, Monte Carlo experiments conducted by Hacker and Hatemi-J (2003) to investigate the properties of lag augmented tests for causality between integrated variables found that these tests do not have correct size when usual standard distributions are used but perform very well when bootstrap distributions are utilized, especially if non-normality and multivariate ARCH effects exist in the VAR model. For this reason we will use the leveraged bootstrapped tests suggested by Hacker and Hatemi-J (2003) in order to increase the probability of drawing valid inference in testing for the causal nexus between money and output. There are two other advantages in using this procedure: first, it has more precision particularly when the sample size is relatively small as in the case of the present paper, and second, the bootstrap procedure is not sensitive for the normal distribution because it is based on the empirical distribution of the underlying data.[4]

Consider the following n-dimensional vector autoregressive model of order p, VAR(p):

$$x_t = \nu + A_1 x_{t-1} + \ldots + A_p x_{t-p} + \varepsilon_t, \tag{1}$$

where $\varepsilon_t = (\varepsilon_{1t}, \ldots, \varepsilon_{nt})'$ is a zero mean independent white noise process with non-singular variance–covariance matrix Σ_ε.[5] In order to rule out explosive cases, we assume $E|\varepsilon_{it}|^{2+\tau} < \infty$ for some $\tau > 0$ (i=1,...,n). An important issue in this regard is the choice of the optimal lag length (p) in the VAR model because all inference in the VAR is of course based on the chosen lag length. To bring about this, we make use of a new information criterion introduced by Hatemi-J (2003). This information criterion is shown to perform well for choosing the optimal lag order, especially if the variables in the VAR model are integrated. The lag length that minimizes the following equation is chosen as the optimal lag order:

$$HJC = \ln\left(\det \widehat{\Omega}_s\right) + s\left(\frac{n^2 \ln T + 2n^2 \ln(\ln T)}{2T}\right), \quad s = 0, \ldots, p. \tag{2}$$

Where:

ln = the natural logarithm,

$det\widehat{\Omega}_s$ = the determinant of the estimated variance–covariance matrix of ε_t for lag order s,

n = the number of variables in the model, and

T = the number of observations utilized to estimate the VAR model.

It is well known in the literature that standard asymptotical distributions cannot be used to test for Granger causality. To remedy this shortcoming, Toda and Yamamoto (1995) suggest the following augmented VAR(p+d) model:

$$x_t = \nu + A_1 x_{t-1} + \ldots + A_p x_{t-p} + \ldots + A_{p+d} x_{t-p-d} + \varepsilon_t. \tag{3}$$

[4] See Hongyi and Maddala (1997).

[5] For tests of independency in the residuals of VAR models see Hatemi-J (2004).

Note that d represents the integration order of the variables. The kth element of x_t does not Granger-cause the jth element of xt if the following hypothesis is not rejected at a given significance level:

$$H_0: \text{ the row } j, \text{ column } k \text{ element in } A_r \text{ equals zero for } r = 1, \ldots, p. \tag{4}$$

It should be pointed out that the parameters for the extra lag(s), i.e. d, are unrestricted under the null hypothesis. According to Toda and Yamamoto (1995) these unrestricted parameters make sure that the asymptotical distribution theory can be utilized when tests for Granger causality are conducted between integrated variables. We make use of the following notations in order to describe the Toda-Yamamoto test statistic in a compact way:

$$W := (x_1, \cdots, x_T)\ (n \times T) \text{ matrix},$$
$$D := \left(\nu, A_1, \cdots, A_p, \cdots, A_{p+d}\right)\ (n \times (1 + n(p + d))) \text{ matrix},$$
$$Z_t := \begin{bmatrix} 1 \\ x_t \\ x_{t-1} \\ \vdots \\ x_{t-p-d+1} \end{bmatrix} ((1 + n(p + d)) \times 1) \textit{ matrix, for } t = 1, \ldots, T,$$
$$Z := (Z_0, \cdots, Z_{T-1})\ ((1 + n(p + d)) \times T) \text{ matrix, and}$$
$$\delta := (\varepsilon_1, \cdots, \varepsilon_T)\ (n \times T) \text{ matrix.}$$

Via this notation, the estimated VAR(p+d) model is written compactly as:

$$W = DZ + \delta. \tag{5}$$

Toda and Yamamoto (1995) introduce the following modified Wald (MWALD) test statistic for testing the null hypothesis of non-Granger causality:

$$MWALD = (C\beta)'\left[C\left((Z'Z)^{-1} \otimes S_U\right)C'\right]^{-1}(C\beta) \sim \chi^2_P. \tag{6}$$

Where:

$\otimes$ = element by all element matrix multiplication operator (the Kronecker product).

C = a $p{\times}n(1{+}n(p{+}d))$ indicator matrix. Each row of C acquires the value of one if the related parameter in β is zero under the null hypothesis, and it gets the value of zero if there is no such restriction under the null.

S_U = the estimated variance–covariance matrix of residuals in Eq. (5) when the null hypothesis of non-Granger causality is not imposed.

$\beta = \mathrm{vec}(D)$, where vec represents the column-stacking operator.

The MWALD test statistic is asymptotically χ^2 distributed, conditional on the assumption that the error terms are normally distributed, with the number of degrees of freedom equal to the number of restrictions to be tested. The number of restrictions is equal to p in our case. Hacker and Hatemi-J (2003) show via Monte Carlo simulations that the MWALD test statistic overrejects the null hypothesis, especially if the data generating process for the error terms is characterized by non-normality and autoregressive conditional heteroscedasticity (ARCH). To improve on the size properties of tests for causality under such circumstances, the authors propose using leveraged bootstrap simulations. The bootstrap method was originally introduced by Efron (1979) and it is based on resampling the underlying data to estimate the distribution of a test statistic. It has become a very useful tool to remedy cases when asymptotical distributions have low performance.

To perform the bootstrap simulations, we first estimate regression (Eq. 5) with the null hypothesis of no Granger causality imposed. For each bootstrap simulation we generate the simulated data, W^* , in the following way:

$$W^* = \widehat{D}Z + \delta^*, \tag{7}$$

here $\widehat{D}$ is the estimated value of the parameters in Eq. (5). That is:

$$\widehat{D} = WZ'(ZZ')^{-1}. \tag{8}$$

Note that the bootstrap residuals (δ^*) are based on T random draws with replacement from the regression's modified residuals, each with equal probability of $1/T$. The mean of the resulting set of drawn modified residuals is subtracted from each of the modified residuals in that set. This modification is done to ensure that the mean value of the bootstrapped residuals is zero. The modified residuals are the regression's raw residuals modified to have constant variance, through the use of *leverages*. The leverage adjustment is noted in Davison and Hinkley (1999). Hacker and Hatemi-J (2003) suggest this adjustment for multivariate equation cases. The modified residual through leverage adjustment for x_{it} is defined as

$$\varepsilon_{it}^m = \frac{\varepsilon_{it}}{\sqrt{1 - h_{it}}},$$

where h_{it} is the tth element of h_i, and ε_{it} is the raw residual from the regression for x_{it} (i=1, 2, 3, 4.). It should be mentioned that the T×1 leverages vectors for x_{1t}, and x_{jt} are respectively defined as (if the null hypothesis of x_{jt} does not Granger cause x_{1t} is tested):

$$h_1 = \mathrm{diag}\left(X_1\left(X_1'X_1\right)^{-1}X_1'\right), \text{ and}$$
$$h_j = \mathrm{diag}\left(X(X'X)^{-1}X'\right)\ for\, j = i-1 \text{ and } i = 1, 2, 3, 4.$$

Where $X = \left(W'_{-1}, \cdots, W'_{-p}\right)$ and $X_i = \left(W'_{i,-1}, \cdots, W'_{i,-p}\right)$. It should be pointed out that we have defined $W_{-L} = (x_{1-L}, \cdots, x_{T-L})$ where $W_{i,-L}$ is ith row of W_{-L} , i.e. it is a row vector of the lag L values for variable x_{it}. For the equation that determines x_{1t}, the explanatory variable matrix for the regression is X_1; this equation is restricted to have no Granger causality. For the equation that determines x_{jt}, the explanatory variable matrix for the regression is X; this equation permits all lags of all variables to be included. This is the case when the null hypothesis that x_{jt} does not Granger cause x_{1t} is tested. In the case of the null hypothesis that x_{1t} does not Granger cause x_{jt}, a corresponding procedure is conducted.

In order to calculate the bootstrap critical values, we run the bootstrap simulation 100,000 times and calculate the MWALD test statistic each time. In this way, we are able to produce the empirical distribution for the MWALD test statistic.

Subsequent to these 100,000 estimations we locate the $(\alpha)th$ upper quantile of the distribution of bootstrapped MWALD statistics and attain the α-level "bootstrap critical values" (c_{α}^{*}). We create the bootstrap critical values for 1, 5 and 10% significance levels, respectively. The next step is to calculate the MWALD statistic using the original data (not the bootstrapped simulated data). Then, the null hypothesis of no causality in the Granger's sense is rejected at the α level of significance based on bootstrapping if the actual MWALD is greater than c_{α}^{*} . The simulations are conducted in GAUSS.[6]

Before testing for causality, we conducted tests for integration order for each variable using the KPSS test (Kwiatkowski et al. 1992) and Perron (1989) test. The results are not presented here in order to save space but they are available on request. Based on the estimation results, we can conclude that the data-generating process for each variable is generally characterized by one unit root. As mentioned previously, the asymptotic critical values are not valid for causality tests when the

Table 1 Test results for causality in the Granger sense, applying leveraged bootstrap technique

The null hypothesis	The estimated test value (MWALD)	1% Bootstrap critical value	5% Bootstrap critical value	10% Bootstrap critical value
$GDP_{DEN} \neq> M_{DEN}$	2.933	7.956 (6.63)	4.173 (3.84)	2.983 (2.70)
$M_{DEN} \neq> GDP_{DEN}$	5.340**	7.828 (6.63)	4.233 (3.84)	2.917 (2.70)
$GDP_{JAP} \neq> M_{JAP}$	0.442	8.691 (6.63)	4.416 (3.84)	2.986 (2.70)
$M_{JAP} \neq> GDP_{JAP}$	8.748**	8.676 (6.63)	4.609 (3.84)	3.220 (2.70)
$GDP_{SWE} \neq> M_{SWE}$	6.938**	8.133 (6.63)	4.534 (3.84)	3.101 (2.70)
$M_{SWE} \neq> GDP_{SWE}$	8.597***	7.774 (6.63)	4.283 (3.84)	2.889 (2.70)
$GDP_{US} \neq> M_{US}$	3.289	14.789 (9.84)	8.596 (5.99)	6.355 (4.61)
$M_{US} \neq> GDP_{US}$	5.572*	12.718 (9.84)	7.489 (5.99)	5.502 (4.61)

The notation $A \neq> B$ implies that A does not Granger cause B

The notations ***, **, and * imply significance at the 1, 5, and 10% significance level, respectively, based on bootstrap critical values. The values presented in the parentheses are representing the asymptotic critical values. It should be mentioned the bootstrap critical values are quite higher than the asymptotic ones. Because the asymptotic chi-square critical values are 6.63, 3.84, and 2.7 at the 1, 5, and 10% significance level, respectively, if one restriction is tested. For testing two restrictions these critical values are 9.84, 5.99, and 4.61 at each corresponding significance level

MWALD represents the modified Wald test statistic as described in Eq. (6)

The lag order of the VAR model, p, was set to one in each case except for the US for which p was set to two. Also the augmentation lag, d, was set to one since each variable contains one unit root

Following Sims (1980), CPI and short-run interest rate were included into each model

[6] The program procedure written in Gauss to conduct leveraged bootstrap simulations as suggested by Hacker and Hatemi-J (2003) is available from the authors on request.

variables are integrated, especially when the assumption of normality is not fulfilled.[7] To remedy this problem, we have applied bootstrap simulation techniques to calculate our own critical values based on the empirical distribution of the data set, which does not require necessarily to be normally distributed. According to the results reported in Table 1, there exists uni-directional causality running from money to output in Denmark, Japan and the US. In the case of Sweden, bi-directional causality is found.[8] The reason for the bi-directional causality may stem from a certain class of non-linearities in the data as argued by Holmes and Hutton (1992) and Sephton (1995) who suggest that changes in money contain non-linear dynamic effects on changes in prices, interest rates, and income.

4 Summary and conclusions

Previous studies of the direction of causation between money and income, based on the Granger causality tests, have demonstrated mixed and contradictory results. We argue that the asymptotic critical values are not valid for causality tests when the variables are integrated, particularly if the assumption of normality is not fulfilled. Thus, we propose an alternative methodology based on the leveraged bootstrapped simulation technique to calculate our own critical values. Such critical values are subject to the empirical distribution of the data set which does not require necessarily to be normally distributed.

The evidence is provided for Denmark, Japan, Sweden and the US. Based on the estimated results, the authors find that Granger causality is uni-directional running from money to output in all the sample countries except for Sweden for which the direction of causality is bi-directional. However, our results are, more or less, in line with those of Seletis and King (1994); Artis (1992), and Hayo (1999). Generally speaking, the established uni-directional causality from money to output reveals one important policy implication—that is, in looking for the sources of output fluctuations, money might be a major factor.

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[7] It should be mentioned that we tested for multivariate normality in each case by the Doornik and Hansen (1994) test. The results showed that the null of normality can be rejected in each case except for the US. We also tested for multivariate ARCH effects by using a test suggested by Hacker and Hatemi-J (2005) and the results showed that except in the case of Japan there are no ARCH effects.

[8] It should be mentioned that the bootstrap critical values presented in Table 1 are quite higher than asymptotic critical values in all cases. However, the inference based on asymptotic critical values changes only in two cases. We estimated also the bootstrap critical values without leverage adjustments. These results, not presented but available on request, showed that the bootstrap critical values without leverage adjustments are close to the bootstrap critical values with leverage adjustments (except in the case of Japan).

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