EFFICIENT TESTS OF LONG-RUN CAUSATION IN TRIVARIATE VAR PROCESSES WITH A ROLLING WINDOW STUDY OF THE MONEY-INCOME RELATIONSHIP

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ABSTRACT. This paper develops a simple sequential multiple horizon non-causation test strategy for trivariate VAR models (with one auxiliary variable). We apply the test strategy to a rolling window study of money supply and real income, with the price of oil, the unemployment rate and the spread between the Treasury bill and commercial paper rates as auxiliary processes. Ours is the first study to control simultaneously for common stochastic trends, sensitivity of test statistics to the chosen sample period, null hypothesis over-rejection, sequential test size bounds, and the possibility of causal delays. Evidence suggests highly significant direct or indirect causality from M1 to real income, in particular through the unemployment rate and M2 once we control for cointegration.

Original version: Nov. 2004

This version: Feb. 2006

1. INTRODUCTION

We are interested in testing for the precise horizon at which fluctuations in the money supply anticipate growth in real disposable income. In order to do so, using a vector autoregression framework we develop a recursive technique for characterizing typically nonlinear causality chains for a trivariate process X, Y and Z in terms of linear parametric restrictions. This leads to a simple sequence of linear compound hypotheses for tests of multiple horizon non-causation when the auxiliary variable Z is scalar-valued.

A simple, efficient test procedure for multi-step ahead causation that can be employed to characterize causality chains and causal neutralization[1] has

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Keywords and phrases: multiple horizon causality; Wald tests; parametric bootstrap; money-income causality; rolling windows; cointegration.

JEL classification: C12; C32; C53; E47.

I would like to thank two anonymous referees and the editor, M. Hashem Pesaran, for helpful comments that lead to substantial improvements. All errors, if any, are of course mine.

[1] *Causal neutralization* from Y to X occurs when multiple causal routes at some time horizon $h \geq 2$ exist through Z, yet cancel each other out such that noncausation holds.

yet to be established. The fundamental problem lies in the inherently non-linear nature of parametric conditions for non-causality in VAR models, and the potential for asymptotic degeneracy of test statistics. See Sims (1980), Renault and Szafarz (1991), Lütkepohl (1993), Lütkepohl and Müller (1994), Lütkepohl and Burda (1997), and Dufour and Renault (1998). See Wiener (1956) and Granger (1969) for seminal contributions to the literature.

Let W_t be a k-vector process, $k \geq 2$, with trivariate partition $(X_t', Y_t', Z_t')'$. Dufour *et al* (2003) suggest analyzing the parameters of an h-step ahead VAR, say $W_{t+h} = \sum_{i=1}^{\infty} \pi_i^{(h)} W_{t+1-i} + v_{t+h}$, where $\pi_i^{(h)}$ are matrix-valued coefficients: see Section 2. It is easy to show Y does not linearly cause X at h-steps ahead *if and only if* the XY-block $\pi_{XY,i}^{(h)} = 0$ for all $i \geq 1$. Thus, a simple Wald test of linear zero restrictions is all that is required to test h-step ahead non-causality. Several limitations are noteworthy, however: a test of non-causality can only be performed for one horizon at a time; a new VAR model must be estimated for each horizon making cross-horizon comparisons particularly difficult; the method usually cannot be used alone to distinguish between simple non-causation (the total absence of indirect causal routes) and causal neutralization; and the procedure does not uniformly ensure a logical test conclusion[2]. Nevertheless, attractive features of this procedure are its relative ease of implementation and the fact that it can be used on a multivariate VAR process of arbitrary dimension.

Chao *et al* (2001) and Corradi and Swanson (2002) consider linear and non-linear out-of-sample tests of non-causality. Similar to Dufour *et al* (2003), this method can be applied to vector processes of arbitrary dimension, it only tests for non-causality at a particular horizon, and it cannot be used in a simple fashion to address causality chains.

In this paper we develop recursive parametric representations of causality chains for trivariate VAR processes in the case of one scalar-valued auxiliary variable Z. Two- or three-vector VAR's are still popular in the causality-in-mean and causality-in-variance literatures. See, e.g., Hiemstra and Jones (1994), Brooks (1998), Hong (2001) and Coe and Nason (2004). Moreover, a causality chain $Y \rightarrow Z \rightarrow X$ implies Y will eventually cause X if Z is univariate, and linear necessary and sufficient conditions for non-causation up to arbitrary horizons are available in all cases (see Theorem 2.1, below). This suggests a compact graph-theoretic notation for multiple horizon causation when Z is univariate. Cf. Studený and Bouckaert (1998) and Swanson and Granger (1997). See Section 3.2. Sequential test conditions in the presence of multiple auxiliary variables become substantially complicated when $h \geq 3$ and are therefore considered elsewhere (e.g. Hill, 2004).

[2]For example, in their study of monthly GDP (X), the federal funds rate (Y), the GDP deflator and non-borrowed reserves (Z), horizon specific tests suggest Y fails to cause X for horizons 1 and 2, and causes X at horizon $h = 3$. This is possible only if an indirect causal route $Y \rightarrow Z \rightarrow X$ exists. However, their test procedure reveals that Y fails to cause Z one-month ahead, a characteristic that implies *noncausation at all horizons*, which contradicts their conclusion.

We do, however, characterize the "compression" of information represented by VAR coefficients when auxiliary variables are omitted.

We make no attempt to consider causality and causal chains from the perspective of impulse response functions, forecast error variance decompositions, and so-called instantaneous causality. See Granger *et al* (1986), Granger (1988), Lütkepohl (1993) and Swanson and Granger (1997).

We apply our test procedure to the classic question of whether fluctuations in the aggregate money supply anticipate the growth of real income. See Sims (1972, 1980) and Christiano and Ljungqvist (1988) for seminal bivariate studies; Stock and Watson (1989) and Friedman and Kuttner (1993) for initial multivariate studies; and Sims *et al* (1990), Toda and Phillips (1993,1994) and Toda and Yamamoto (1995) who consider the impact of cointegrating relationships on tests of one-step ahead non-causation.

The studies of Thoma (1994) and Swanson (1998) are the most relevant to the one proposed here. Swanson (1998), in particular, controls for common stochastic trends and test sensitivity to chosen sample period, and uses standard and real-time data. Neither study, however, performs tests of multi-step ahead non-causation, neither controls for causal delays, and both ignore the possibility that the asymptotic distribution of the test statistic may be a poor proxy for the true small sample distribution.

We use monthly M1 and real disposable income for the period Jan. 1959-Dec. 2002, with the unemployment rate, M2, the price of oil and the spread between the Treasury bill rate and the commercial paper rate as auxiliary variables. In order to control for the possibility of VAR parametric evolution and the associated evolution of patterns of causality, and test sensitivity to the chosen sample period, we study causation over rolling sample windows of fixed and increasing length, with a minimum length of 324 months[3]. We employ conventional and bootstrap test techniques that are robust to unknown forms of cointegration, a la Toda and Yamamoto (1995), and derive an upper bound of the test size due to the sequential nature of the test method. This is the first such study (to the best of our knowledge) to employ simultaneously each method just described.

Using rolling windows we find significant evidence of a delay of 1-3 months before growth in monthly M1 anticipates growth in real disposable income, with the longest delay occurring through the unemployment rate. Once we control for cointegration, however, evidence suggests money causes real income 1 or 2-months ahead through M2, the unemployment rate and the price of oil, a result that strongly supports the major findings of Swanson (1998).

An arguably serious limitation of the present study is our use of the latest time series available, and not "real-time" data adjusted to account for

[3]Swanson's (1998) fixed window lengths are set at 10 and 15 years. The resulting degrees of freedom of the estimated parameters, after controlling for sample truncation due to the the presence of lags, is as low as 56. Nonetheless, the chi-squared distribution is used for all Wald tests, for all models and for all sample periods.

periodic updates. See Amato and Swanson (2001) who find that money fails to Granger-cause (1-step ahead) output when real-time data is used in VARs and VECMs, using standard in-sample and out-of-sample test procedures.

In Section 2 we define h-step ahead causation, and provide parametric characterizations of causality chains in Section 3. Section 4 develops the test strategy, Section 5 contains the empirical study, and Section 6 concludes with parting comments. All tables are placed at the end of the paper.

Due to space considerations all proofs, elaborations on causality chains, extended comments on the major empirical finds and all figures have been relegated to a technical appendix in Hill (2006).

For an m-vector process $\{W_t : t \in \mathbb{Z}\}$, let $W(-\infty, t]$ denote the Hilbert space spanned by the components $W_{i,s} : i = 1...m$, $s \leq t$. For Hilbert spaces A and B, we write $A + B$ to denote the space spanned by all components of A and B.

2. Causality Preliminaries

The following setup borrows heavily from Dufour and Renault's (1998) framework for Wiener-Granger causality. Consider some m-vector, stationary processes $\{W_t\}$ with trivariate representation $W_t = (X_t', Y_t', Z_t')'$, where X_t, Y_t, and Z_t have dimensions $m_x \geq 1$, $m_y \geq 1$ and $m_z \geq 0$ respectively, and $m = m_x + m_y + m_z \geq 2$. We assume $W_{i,t}$ has a finite variance for each $i = 1...m$. Denote by H the set of information available in all periods (e.g. starting conditions and constants). Let $I_{XZ} = I_{XZ}(t) = H + X(-\infty, t] + Z(-\infty, t]$ denote the set of information common to all periods and contained in past and present X and Z. Similarly, $I_W(t) = I_{XZ}(t) + Y(-\infty, t]$, all information contained in all periods, and in past and present X, Y and Z.

In principle none of the following results rely on the stationarity assumption. For example, we may allow time to be bounded in the finite past. For brevity, however, we consider only an unbounded past.

We say Y "*does not cause X at horizon $h > 0$*" (denoted $Y \overset{h}{\nrightarrow} X|I_{XZ}$) if the inclusion of past and present values of Y does not improve the minimum mean-squared-error forecast of X_{t+h} for any t. We say Y "*does not cause X up to horizon $h > 0$*" (denoted $Y \overset{(h)}{\nrightarrow} X|I_{XZ}$) if $Y \overset{k}{\nrightarrow} X|I_{XZ}$ for each $k = 1...h$. Finally, we say Y "*does not cause X at any horizon $h > 0$*" (denoted $Y \overset{(\infty)}{\nrightarrow} X|I_{XZ}$) if $Y \overset{h}{\nrightarrow} X|I_{XZ}$ for every $h > 0$.

2.1. Non-parametric Preliminaries.

The following results will expedite characterizations of causality chains in Section 3. Each process X, Y and Z are of arbitrary dimension unless otherwise noted.

Theorem 2.1 *i. If $Y \overset{1}{\nrightarrow} (X, Z)|I_{XZ}$, or $(Y, Z) \overset{1}{\nrightarrow} X|I_{XZ}$, then $Y \overset{(\infty)}{\nrightarrow} X|I_{XZ}$; ii. If $m_z \geq 2$ and $Z = (Z_1', Z_2')'$ for arbitrary sub-vectors Z_i, and if $(Y, Z_2) \overset{1}{\nrightarrow} (X, Z_1)|I_{XZ_1}$, then $Y \overset{(\infty)}{\nrightarrow} X|I_{XZ}$; iii. In order for non-causation $Y \overset{1}{\nrightarrow} X|I_{XZ}$ to be followed by causation $Y \overset{h}{\rightarrow} X|I_{XZ}$, for any $h > 1$, it is* *necessary for* $Y \xrightarrow{1} Z \xrightarrow{1} X$*; iv. If* Z *is scalar-valued and* $Y \xrightarrow{1} Z \xrightarrow{1} X$*, then* $Y \xrightarrow{h} X|I_{XZ}$ *for some* $h \geq 1$.

Remark 1: Results (i)-(iii) follow from Propositions 2.3 and 2.4 of Dufour and Renault (1998).

Remark 2: Cases (i) and (ii) simply state if Y does not cause X one-step ahead and a causal chain from Y to X, through Z, does not exist, then Y never causes X. Case (iii) states the converse: for Y to cause X at some horizon $h > 1$ a causal chain $Y \to Z \to X$ must exist. Case (iv) simply states that causation eventually occurs if a causality chain exists and Z is univariate. However, except when the auxiliary variable Z is univariate, a causality chain $Y \xrightarrow{1} Z \xrightarrow{1} X$ is generally not sufficient for causation $Y \xrightarrow{h} X$, $h \geq 2$, due to the possibility that multiple causal routes through the auxiliary variables Z may cancel each other out (causal neutralization). See Hill (2004).

2.2. **Parametric Preliminaries.** We assume $W_t = (X_t', Y_t', Z_t')'$ has a stationary autoregressive representation, and for the sake of notational brevity we assume all constants are identically zero:

$$W_t = \sum_{i=1}^{\infty} \pi_i W_{t-i} + \epsilon_t, \quad E[\epsilon_{t,i} W_{t-k,j}] = 0, \forall i, j = 1...m, \forall k \geq 1. \tag{2.1}$$

The innovations vector ϵ_t has a zero mean, it is covariance orthogonal to $W(-\infty, t-1]$, and has non-singular covariance matrix $E[\epsilon_t \epsilon_t']$. The coefficients π_i are real-valued $m \times m$ matrices, and the infinite series $\sum_{i=1}^{\infty} \pi_i W_{t-i}$ is assumed to converge in mean-square. We explicitly ignore the issue of cointegration although only slight modifications to the following discourse is required to include this case. Our empirical study, however, does control for cointegration of unknown form: see Section 5.

The distributed lag $\sum_{i=1}^{\infty} \pi_i W_{t-i}$ represents the best *linear* 1-step ahead forecast of W_t, but not necessarily the best 1-step ahead forecast, although the two coincide for Gaussian vector processes. The setup in (2.1) is fairly standard (e.g. Lütkepohl, 1991), but does not preclude the possibility of nonlinear, nor second-order, causal relationships. Throughout, therefore, the notation $Y \overset{(h)}{\nrightarrow} X|I_{XZ}$ strictly implies "linear predictive" non-causation[4]. See, e.g., Comte and Lieberman (2000).

[4]Most $L_2(\Omega, \mathfrak{F}_t, Q)$ processes of interest will have a representation (2.1) either in levels, or after some standard transformation, e.g. first differencing. Nonetheless, in tests not reported here we find that several processes used in the present study suggest highly significant patterns of smooth-transition autoregressive nonlinearity. See, also, Rothman *et al* (2001). Despite the inherent shortcomings associated with linear time series models, however, nonlinear models do not typically afford straightforward recursive parametric causal chain representations (e.g. the STAR model of Rothman *et al,* 2001), even though a consistent nonlinear out-of-sample test of non-causality at a particular horizon is available (Corradi and Swanson, 2002).

Under the maintained assumptions, above, it is easy to show an h-step ahead linear forecast of W_{t+h}, denote $\hat{W}_{t+h}|I_W(t)$, satisfies the recursion

$$\hat{W}_{t+h}|I_W(t) = \sum_{i=1}^{\infty} \pi_i \hat{W}_{t+h-i}|I_W(t) = \sum_{i=1}^{\infty} \pi_i^{(h)} W_{t+1-i}, \tag{2.2}$$

where $\hat{W}_{t+h-i}|I_W(t) \equiv W_{t+h-i} \forall i \geq h$, and $\{\pi_i^{(h)}\}_{i=1}^{\infty}$ satisfies the nonlinear recursion (see Dufour and Renault, 1998: eq. 3.8)

$$\pi_1^{(0)} = I_m, \quad \pi_j^{(1)} = \pi_j, \quad \pi_j^{(h+1)} = \pi_{j+1}^{(h)} + \pi_1^{(h)} \pi_j. \tag{2.3}$$

Consider the $(X', Y', Z')'$-conformable partition of the coefficient sequence

$$\pi_j^{(h)} = \begin{bmatrix} \pi_{XX,j}^{(h)} & \pi_{XY,j}^{(h)} & \pi_{XZ,j}^{(h)} \\ \pi_{YX,j}^{(h)} & \pi_{YY,j}^{(h)} & \pi_{YZ,j}^{(h)} \\ \pi_{ZX,j}^{(h)} & \pi_{ZY,j}^{(h)} & \pi_{ZZ,j}^{(h)} \end{bmatrix}. \tag{2.4}$$

For example, $\pi_{XY,j}^{(h)}$ denotes the $m_x \times m_y$ matrix of constant real numbers associated with the conditional causal influence from Y to X.

The following fundamental theorem is due to Dufour and Renault (1998: Theorem 3.1).

Theorem 2.2 *Let* $W_t = (X_t', Y_t', Z_t')'$ *satisfy* (2.1). $Y \overset{h}{\nrightarrow} X|I_{XZ}$ *if and only if* $\pi_{XY,j}^{(h)} = 0$, $\forall j = 1, 2, ...$

3. Causality Chains

In this section we provide a simple sequential characterization of causality chains. The representations will lead to a sequential test strategy in Section 4.

Because $Y \overset{1}{\nrightarrow} X$ and $Y \overset{1}{\nrightarrow} Z$ will imply non-causation at all horizons, $Y \overset{(\infty)}{\nrightarrow} X$ (cf. Theorem 2.1), we assume causation $Y \overset{1}{\rightarrow} Z$ throughout the present section, unless otherwise noted. Without loss of generality assume X and Y are univariate processes ($m_x = m_y = 1$)[5]. Assume $W_t = (X_t, Y_t, Z_t')'$ satisfies (2.1).

3.1. **Recursive Representations.** The coefficient recursion (2.3) renders the XY^{th}-block of π_j as

$$\pi_{XY,j}^{(h+1)} = \pi_{XY,j+1}^{(h)} + \pi_{XX,1}^{(h)} \pi_{XY,j} + \pi_{XY,1}^{(h)} \pi_{YY,j} + \pi_{XZ,1}^{(h)} \pi_{ZY,j}. \tag{3.1}$$

If non-causality up to horizon h is true, $Y \overset{(h)}{\nrightarrow} X|I_{XZ}$, then Theorem 2.2 dictates $\pi_{XY,j}^{(k)} = 0$ for each $k = 1...h$. Non-causation at the next horizon h

[5] Dufour and Renault (1998) prove that noncausation from vector process Y to vector process X is equivelant to noncausation from each scalar component Y_i to each scalar component X_j. Thus, it suffices to consider the causal structure from Y to X by considering the scalar components individually.

$+ 1$, $Y \overset{h+1}{\nrightarrow} X$, then also holds *if and only if*

$$\pi^{(h+1)}_{XY,j} = \pi^{(h)}_{XZ,1}\pi_{ZY,j} = 0, \forall j \geq 1. \tag{3.2}$$

Thus, non-causality up to some horizon $h \geq 1$ and subsequent causality at $h + 1$ can only occur if a causality chain exists, $Y \overset{1}{\rightarrow} Z \overset{h}{\rightarrow} X$ such that $\pi^{(h)}_{XZ,1}\pi_{ZY,j} \neq 0$, for some $j \geq 1$. If the auxiliary variable Z is scalar-valued and if $Y \overset{1}{\rightarrow} Z$, then (3.2) implies non-causation $Y \overset{h+1}{\nrightarrow} X$ *if and only if* $\pi^{(h)}_{XZ,1} = 0$. The coefficient recursion (2.3 leads to a simple characterization of $\pi^{(h)}_{XZ,1}$.

Lemma 3.1 *Let* (2.1) *hold and let* $m_z = 1$. *Assume non-causation* $Y \overset{(h)}{\nrightarrow} X|I_{XZ}$ *for any* $h \geq 2$, *and causation* $Y \overset{1}{\rightarrow} Z|I_{XZ}$. *Then* $\pi^{(2)}_{XZ,1} = \pi_{XZ,2}$ *for* $h = 2$, *and for any other* $h > 2$,

$$\pi^{(h)}_{XZ,1} = \pi_{XZ,h} + \sum_{i=1}^{h-1}\left(\pi^{(h-i)}_{XX,1}\pi_{XZ,i}\right). \tag{3.3}$$

The following theorem, based on Lemma 3.1, delivers a simple linear necessary and sufficient condition for non-causality up to horizon $h \geq 1$.

Theorem 3.2 *Let* (2.1) *hold and assume* $m_z = 1$. *Assume causation* $Y \overset{1}{\rightarrow} Z|I_{XZ}$.

i. *For all* $h \geq 2$, $Y \overset{(h)}{\nrightarrow} X|I_{XZ}$ *if and only if* $Y \overset{1}{\nrightarrow} X|I_{XZ}$ *and* $\pi_{XZ,k} = 0$, $k = 1...h - 1$;

ii.. *For all* $h \geq 2$, *if* $Y \overset{(h-1)}{\nrightarrow} X|I_{XZ}$, *then* $Y \overset{(h)}{\nrightarrow} X|I_{XZ}$ *if and only if* $\pi_{XZ,h-1} = 0$.

Remark 1: For any $h \geq 1$, non-causation through h-steps ahead $Y \overset{(h)}{\nrightarrow} X$ followed by causation $h + 1$-step ahead causation $Y \overset{h+1}{\rightarrow} X$ is feasible only if a causal chain $Y \overset{1}{\rightarrow} Z \overset{1}{\rightarrow} X$ exists (cf. Theorem 2.1) and *if and only if* $\pi_{XZ,i} = 0$, $i = 1...h-1$, and $\pi_{XZ,h} \neq 0$. Conversely, if a causal chain $Y \overset{1}{\rightarrow} Z \overset{1}{\rightarrow} X$ exists and Z is univariate, then it must be the case that $\pi_{XZ,h} \neq 0$ for *some* $h \geq 1$ and causation *eventually* occurs.

3.2. **Chain Representations.** The result that non-causation $Y \overset{1}{\nrightarrow} X$ and $\pi_{XZ,i} = 0$, $i = 1...h$, sequentially imply $Y \overset{(h+1)}{\nrightarrow} X$ when Z is univariate suggests a simple graph-theoretic representation of causality chains. See, e.g., Geiger and Pearl (1990) and Studený and Bouckaert (1998) for details on causal chain graph theory, and see Swanson and Granger (1997) for an application of the graph-theoretic approach to Wold-form innovations decompositions in a macroeconomic context.

Note, however, that the chain representation $Y \overset{1}{\rightarrow} Z \overset{1}{\rightarrow} X$ neither suffices to suggest causation *will* occur when Z is vector-valued, nor provides enough information concerning *when* causation takes place if Z is univariate. We, therefore, adopt a more concise notation. Write $Y \overset{1:h_{ZY}}{\rightarrow} Z$ to imply Y causes Z one-step ahead, such that $Y_{t-h_{ZY}}$ denotes the most recent occurrence of Y to enter into the best 1-step ahead forecast of Z. If $Y \overset{1}{\nrightarrow} X$ and if Z is univariate, then by Theorem 3.2 the chain graph[6]

$$Y \overset{1:h_{ZY}}{\rightarrow} Z \overset{1:h}{\rightarrow} X \tag{3.4}$$

provides the unambiguous interpretation that $Y \overset{(h)}{\nrightarrow} X$ and $Y \overset{h+1}{\rightarrow} X$.

When Z is multivariate, however, the chain notation $Y \overset{1:h_{ZY}}{\rightarrow} Z \overset{1:h}{\rightarrow} X$ is neither sufficient to convey whether, when nor how causation takes place. If Z is a 2-vector (Z_1, Z_2), for example, and $Y \overset{1}{\nrightarrow} X|I_{XZ}$, then $Y \overset{1:1}{\rightarrow} Z \overset{1:1}{\rightarrow} X$ need only imply $Y \overset{1:1}{\rightarrow} Z_1$ and $Z_2 \overset{1:1}{\rightarrow} X$, in which case non-causation $Y \overset{(2)}{\nrightarrow} X|I_{XZ}$ occurs: a direct path from Y to X does not exist at $h = 2$.

3.3. Multivariate vs. Univariate Z.

An important question arises concerning the information content that is lost when only one scalar auxiliary variable Z is employed. In some extreme cases, however, characterizations of causality from Y to X with one or multiple auxiliary variables are identical. Dufour and Renault (1998) present some theory on this point. In this subsection we present new results that help characterize the "compression" of information that results when auxiliary variables are omitted. We exploit the representations of this section in the empirical study, below, in order to address the shortcomings of 3-vector models when multiple auxiliary variables are available.

Let $\delta_j^{(h)}$ denote the VAR coefficients in the orthogonal projection of X_{t+h} onto the truncated linear sub-space comprised of past and present (X, Y, Z_1),

$$X_{t+h} = \sum_{j=1}^{\infty} \left(\delta_{XX,j}^{(h)} X_{t+1-j} + \delta_{XY,j}^{(h)} Y_{t+1-j} + \delta_{XZ_1,j}^{(h)} Z_{1,t+1-j} \right) + u_t. \tag{3.5}$$

Similarly, for each $i = 1, 2, ..$, denote by $\beta_{Z_2,j}^{1-i}$ the coefficients in the orthogonal projection of each vector $Z_{2,t+1-i}$ onto past and present information contained in (X, Y, Z_1):

$$Z_{2,t+1-i} = \sum_{j=1}^{\infty} \left(\beta_{Z_2X,j}^{1-i} X_{t+1-j} + \beta_{Z_2Y,j}^{1-i} Y_{t+1-j} + \beta_{Z_2Z_1,j}^{1-i} Z_{1,t+1-j} \right) + v_t. \tag{3.6}$$

Lemma 3.3 *Let $W_t = (X_t, Y_t, Z_{1,t}', Z_{2,t}')'$ where each $Z_{t,i}$ has arbitrary dimension $m_{z_i} \geq 0$. Then $Y \overset{(h)}{\nrightarrow} X|I_{XZ_1}$ if and only if $\delta_{XY,j}^{(k)} = 0$, $k = 1...h$, $j \geq 1$, where*

$$\delta_{XY,j}^{(k)} \equiv \pi_{XY,j}^{(k)} + \sum_{i=1}^{\infty} \pi_{XZ_2,i}^{(k)} \beta_{Z_2Y,j}^{1-i}. \tag{3.7}$$

[6]The chain $Y \overset{1:h_{ZY}}{\rightarrow} Z \overset{1:h-1}{\rightarrow} X$ depicts a *directed, acyclic* chain: the arrows depict the direction of influence, and the chains are inherently acyclic because causation occurs over unidirectional time. See, e.g., Geiger and Pearl (1990) and Studený and Bouckaert (1998).

Formula (3.7) implies *non-causation* from Y to X within the truncated system (X, Y, Z_1) (i.e. $\delta_{XY,j} = 0$, $\forall j$) and *causation* from Y to X in the complete system (X, Y, Z_1, Z_2) (i.e. $\pi_{XY,j} \neq 0$, for some j) may simultaneously be true. This is due simply to neutralization effects through the multiple causal routes from Y to X linked by the omitted set of auxiliary variables Z_2.

Conversely, consider the truncated system (X, Y, Z_1) with univariate Z_1, and suppose $Y \overset{1}{\rightarrow} Z_1 \overset{1}{\rightarrow} X$. Then Y must eventually cause X (i.e. $\delta^{(h)}_{XY,j} \neq 0$ for at least one $h \geq 1$ and at least one $j \geq 1$), even if Y *never* causes X in the complete system (X, Y, Z_1, Z_2) (i.e. $\pi^{(h)}_{XY,j} = 0$ $\forall h$, $\forall j$). This is due to the past and contemporary association between X, Y and Z_2. This simply points out a well known limitation of the use of VAR models in order to address "causal" orderings: Y may "cause" X because Y is contemporaneously associated with a common, omitted, process Z_2 that causes X.

If $\pi_{XZ_2,i} = 0$ for all i, then $Z_2 \overset{1}{\nrightarrow} X|I_{XYZ_1}$ and (3.7) dictates $Y \overset{1}{\nrightarrow} X|I_{XZ}$ *if and only if* $Y \overset{1}{\nrightarrow} X|I_{XZ_1}$. In general, we have the following result.

Theorem 3.4 *Let* $Z = (Z_1, Z_2')'$ *for some scalar* Z_1 *and vector* Z_2 *of arbitrary dimension* $m_{z_2} \geq 0$.

i. *If* $Z_2 \overset{1}{\nrightarrow} X|I_{XYZ_1}$, *then* $Y \overset{1}{\nrightarrow} X|I_{XZ}$ *if and only if* $Y \overset{1}{\nrightarrow} X|I_{XZ_1}$.

ii. *If* $(Y, Z_2) \overset{1}{\nrightarrow} X|I_{XZ_1}$, *then for any* $h \geq 1$, $Y \overset{(h)}{\nrightarrow} X|I_{XZ_1}$ *implies* $Y \overset{(h)}{\nrightarrow} X|I_{XZ}$, *and* $Y \overset{h+1}{\rightarrow} X|I_{XZ_1}$ *implies* $Y \overset{h+1}{\rightarrow} X|I_{XZ}$.

iii. *If* $(Y, Z_2) \overset{1}{\nrightarrow} X|I_{XZ_1}$ *and* $Y \overset{(h)}{\nrightarrow} X|I_{XZ_1}$, *then* $Y \overset{(h+1)}{\nrightarrow} X|I_{XZ}$ *if and only if* $\pi_{XZ_1,h} = 0$.

Remark 1: Results (*ii*) and (*iii*) are generalizations of Dufour and Renault's (1998) Proposition 2.4 (see also Theorem 2.1, above). They prove a more restricted implication that if Z satisfies the "separation" condition $I_{XZ} = I_{XZ_1} + Z_2(-\infty, t]$, where $I_{XZ_1} = H + X(-\infty, t] + Z_1(-\infty, t]$, then $(Y, Z_2) \overset{1}{\nrightarrow} (X, Z_1)|I_{XZ_1}$ is sufficient for non-causation at all horizons, $Y \overset{(\infty)}{\nrightarrow} X|I_{XZ}$.

Remark 2: If neither Y nor Z_2 cause X one-step ahead, then the chain graph $Y \overset{1:h_{Z_1Y}}{\rightarrow} Z_1 \overset{1:h}{\rightarrow} X$ has an unambiguous interpretation for either the reduced system $(X, Y, Z_1)'$, $Y \overset{(h)}{\nrightarrow} X|I_{XZ_1}$ and $Y \overset{h+1}{\rightarrow} X|I_{XZ_1}$, or the augmented system $(X, Y, Z_1, Z_2')'$, $Y \overset{(h)}{\nrightarrow} X|I_{XZ}$ and $Y \overset{h+1}{\rightarrow} X|I_{XZ}$.

4. Tests for Causation Through Arbitrary Time Horizons

We now construct a strategy for testing non-causality up to arbitrary time horizons by exploiting Theorem 3.2. We then analyze test size bounds due to the sequential nature of the test procedure. Each hypothesis detailed below entails a linear parametric restriction, and may simply be tested using a standard Wald statistic.

4.1. **Sequential Test.** Let π_{XY} denote the sequence of parameters $\{\pi_{XY,i}\}_{i=1}^{\infty}$, etc.

Step 1: **Test** $Y \overset{(\infty)}{\nrightarrow} X$

A fundamental question is whether Y ever causes X. Initially test both hypotheses (cf. Theorem 2.1)

$$H_0^{(\infty)} : Y \overset{1}{\nrightarrow} (X, Z) \Leftrightarrow \pi_{XY} = \pi_{ZY} = 0 \qquad \text{(Test 0.1)}$$

$$H_0^{(\infty)} : (Y, Z) \overset{1}{\nrightarrow} X \Leftrightarrow \pi_{XY} = \pi_{XZ} = 0 \qquad \text{(Test 0.2)}$$

If both hypotheses are rejected then proceed to test for horizon-specific non-causation.

Step 2: **Test** $Y \overset{1}{\nrightarrow} X$**,** $Y \overset{1}{\nrightarrow} Z$**, and** $Z \overset{1}{\nrightarrow} X$

The second most fundamental question is whether Y causes X one-step ahead:

$$H_0^{(1)} : Y \overset{1}{\nrightarrow} X \Leftrightarrow \pi_{XY} = 0 \qquad \text{(Test 1.0)}$$

If evidence suggests non-causation $Y \overset{1}{\nrightarrow} X$, then perform intermediary tests in order to characterize a causality chain, if any. Test

$$H_0^{(1.1)} : Y \overset{1}{\nrightarrow} Z \Leftrightarrow \pi_{ZY} = 0 \qquad \text{(Test 1.1)}$$

$$H_0^{(1.2)} : Z \overset{1}{\nrightarrow} X \Leftrightarrow \pi_{XZ} = 0 \qquad \text{(Test 1.2)}$$

If evidence supports either hypothesis, then evidence supports a broken causality chain and we deduce $Y \overset{(\infty)}{\nrightarrow} X$. If both Tests 1.1 and 1.2 are rejected, proceed to Step 3.

Step 3: **Test Multi-Horizon Causation** $H_0^{(h)} : Y \overset{(h)}{\nrightarrow} X$, $h \geq 2$

This step is reached only if evidence suggests non-causation $Y \overset{1}{\nrightarrow} X$ and a causal chain $Y \overset{1}{\rightarrow} Z \overset{1}{\rightarrow} X$. Theorem 3.2 dictates sequential evidence in favor of $\pi_{XZ,h-1} = 0$ is evidence in favor of non-causation up to horizon h. Simply test the linear compound hypothesis

$$H_0^{(h)} : Y \overset{(h)}{\nrightarrow} X \Leftrightarrow \ \pi_{XY} = \pi_{XZ,i} = 0, i = 1...h-1 \ \text{(Test } h.0\text{)}$$

4.2. **Size Bounds.** Due to the sequential nature of the test of $Y \overset{(h)}{\nrightarrow} X$ we require an upper bound on the test size. The problem of bounding the test sizes becomes quickly complex. For example, we reject 1-step ahead non-causation $Y \overset{1}{\nrightarrow} X$ only if we first reject both tests of non-causation at all horizons, $Y \overset{(\infty)}{\nrightarrow} X$ and then reject $Y \overset{1}{\nrightarrow} X$. We reject non-causation through two steps ahead $Y \overset{(2)}{\nrightarrow} X$ if we reject $Y \overset{1}{\nrightarrow} X$; or fail to reject $Y \overset{1}{\nrightarrow} X$, reject $Y \overset{1}{\nrightarrow} Z$ and $Z \overset{1}{\nrightarrow} X$, and reject the compound hypothesis $Y \overset{1}{\nrightarrow} X$, $\pi_{XZ,1} = 0$. Such trains of logic apply for further horizons.

For notational convenience let $\alpha_{\#.\#}$ denote the nominal size of Test #.#. Write $p^{(h)} \equiv P(rej.\ H_0^{(h)}|H_0^{(h)})$, the probability of an incorrect rejection of the hypothesis $H_0^{(h)} : Y \overset{(h)}{\nrightarrow} X$, and define

$$\begin{aligned}
p_1 &\equiv \min\{\alpha_{0.1},\ \alpha_{1.0} + (h-1) \times \alpha_{1.1}\} \\
p_2 &\equiv \min[\alpha_{0.2},\ \alpha_{1.0} + \sum\nolimits_{i=2}^{h} \min\{\alpha_{1.2},\ \alpha_{i.0}\}] \\
p_3 &\equiv \min[\alpha_{0.1},\ \alpha_{0.2},\ \alpha_{1.0} + (h-1) \times \min\{\alpha_{1.1},\ \alpha_{1.2}\}] \\
p_4 &\equiv \sum\nolimits_{i=1}^{h} \alpha_{i.0}
\end{aligned}$$

Lemma 4.1 *i. If* $Y \overset{1}{\nrightarrow} Z \overset{1}{\rightarrow} X$ *then* $p^{(h)} \leq p_1$; *ii. If* $Y \overset{1}{\rightarrow} Z \overset{1}{\nrightarrow} X$ *then* $p^{(h)} \leq p_2$; *iii. If* $Y \overset{1}{\nrightarrow} Z \overset{1}{\nrightarrow} X$ *then* $p^{(h)} \leq p_3$; *iv. If* $Y \overset{1}{\rightarrow} Z \overset{1}{\rightarrow} X$ *then* $p^{(h)} \leq p_4$. *Moreover, in general,*

$$P\left(rej.\ H_0^{(h)}|H_0^{(h)}\right) \leq \max\nolimits_{1 \leq i \leq 4}\{p_i\}. \tag{4.1}$$

Bound (4.1) generalizes every possibility for a false rejection of $H_0^{(h)}$. Let $h \geq 2$. If $Y \overset{1}{\nrightarrow} Z$, for example, then the conditions outlined in Theorem 3.2 are only sufficient for non-causation, but not necessary (because a causal chain does not exist). From formula (3.2) we may have $Y \overset{1}{\nrightarrow} X$, $\pi_{XZ,1} \neq 0$ and non-causation $Y \overset{(2)}{\nrightarrow} X$. In such a case, if a consistent test statistic is used then there is a probability one asymptotically that we reject $Y \overset{1}{\nrightarrow} X$, $\pi_{XZ,1} = 0$ and falsely deduce $Y \overset{2}{\rightarrow} X$. In cases (i) and (iii), the upper bound of the sequential test size embodies the probabilities of erroneous rejections of Tests 0.1 and 0.2 ($Y \overset{(\infty)}{\nrightarrow} X$) and Test 1.1 ($Y \overset{1}{\nrightarrow} Z$). Neither bound depends on the nominal horizon-specific sizes $\alpha_{h.0}$ because the parametric conditions of Tests h.0 are not necessary for non-causation when $Y \overset{1}{\nrightarrow} Z$. The probability bound of a Type I error in these cases can be controlled simply by setting the nominal size $\alpha_{1.1}$ of the test $Y \overset{1}{\nrightarrow} Z$ to a small value (e.g. .01).

4.3. **Rule of Thumb.** In practice, a simple rule will likely be applied. For example, put $\alpha_{0.1} = \alpha_{0.2} = \alpha$, the nominal size of the initial tests of $Y \overset{(\infty)}{\nrightarrow} X$; and $\alpha_{1.1} = \alpha_{1.2} = \alpha_{i.0} = \beta$ for each $i = 1...h$, the nominal size of tests of causality chains and $Y \overset{(h)}{\nrightarrow} X$. Then (4.1) reduces to

$$P\left(rej.\ H_0^{(h)}|H_0^{(h)} \text{ is true}\right) \leq \max[\min\{\alpha, h \times \beta\}, h \times \beta] = h \times \beta, \tag{4.2}$$

the standard Bonferonni bounds, depending only on the common β.

5. U.S. Income and Monetary Aggregates

We now investigate the causal relationships between aggregate money and income. For the period January 1959 - December 2002 we use the logarithm of monthly, seasonally adjusted, nominal M1 and M2 ($m1$, $m2$) and the logarithm of seasonally adjusted real disposable income (y). For auxiliary variables we use the logarithm of the West Texas Intermediate spot oil price (o), the civilian unemployment rate (u), the 90-day Treasury bill rate (r_b), the 90-day commercial paper rate (r_p) and the spread between the two rates ($rr = r_b - r_p$).

Except for the commercial paper rate, all data are taken from archives made publicly available by the Federal Reserve Bank of Saint Louis based on monthly announcements by the Board of Governors of the Federal Reserve System ($m1$, $m2$, r_b), Bureau of Economic Analysis (y), the Bureau and Labor Statistics (u) and Dow Jones & Company (o). The commercial paper rate was taken from the NBER data archive for the period 1959:01-1971:12, and from publications by the Board of Governors of the Federal Reserve System for the period 1972:01-2002:12. Seasonal adjustment, where applicable, was performed at the source. All data are based on the latest series available to the public in February 2003: we do not control for periodic updates (a la "real-time" data)[7].

In order to control for any apparent trend we pass all final (e.g. post-differenced) processes though a standard linear trend filter. In the case of income, for example, we use $y_t - \hat{\beta}_1 - \hat{\beta}_2 t$ where $\hat{\beta}$ denotes the ordinary least squares estimator. Weak evidence suggests the rate spread has a quadratic trend, but this is undoubtedly a spurious outcome given the chosen sample period. In any case, use of a quadratic or linear trend results in essentially identical test results, and identical conclusions.

Significant evidence suggests one positive unit root exists in each series, except for the rate spread r_b-r_p. The rate spread is likely $I(0)$, implying the process may represent one possible error correction term within a system of y, $m1$, $m2$, r_b, and r_p, with an error correction vector $(0, 0, 0, 1, -1)$[8]. Using industrial output y, aggregate money m, prices p, and the Treasury and commercial paper rates, Swanson (1998) finds in a rolling window framework the rate spread $r_b - r_p$ and the velocity of money $y + m - p$ are likely the only two error correction terms.

[7]Official (revised and non-preliminary) monthly disposable income and monetary aggregates are announced roughly 4-5 weeks after the end of the month. Monthly interest rates, averages of each calendar day in the month, are officially posted on the first business day of the following month. The spot price of oil is reported on the first Tuesday of the following month. The official civilian unemployment rate is reported on the first Friday of the following month. All day-specific announcements are for non-holidays.

[8]Stock and Watson (1993) similarly find evidence of cointegration among M1, industrial output, and the Treasury bill rate. Hafer and Jensen (1991) find evidence for cointegration between M2, real income and a short-term interest rate at quarterly increments, and conclude all evidence for cointegration vanishes once M2 is replace by M1.

Considering the amassed, yet uneven, evidence in support of integration within the individual processes and cointegration between money, income and interest rates, we implement two widely practiced VAR methods. We construct VAR models of de-trended first differences (except for the rate spread) in order to control for integration of order one: the processes are Δy, $\Delta m1$, $\Delta m2$, Δo, Δu, and rr. Second, we employ the excess-lag technique of Toda and Yamamoto (1995) and Dolado and Lütkepohl (1996) for VAR models of de-trended level processes in order to control for cointegration of unknown form. For this procedure, we specify a VAR(p) model in levels adding lags equal to the maximum order of suspected integration d (in this case, $d = 1$), and test only the first $p - d$ coefficient matrices[9].

There is ample evidence in the literature that standard Wald tests in multivariate models tend to lead to over-rejection of null hypotheses: see, e.g., Dufour *et al* (2003) and Dufour (2005). A parametric bootstrap method for simulating small sample p-values, however, has been shown to provide sharp approximations to the chosen significance level, although over-rejections may persist if the test statistic asymptotic distribution involves nuisance parameters (see, e.g., Andrews, 2000; Dufour and Jouini, 2005). For details on the parametric bootstrap see, e.g., Dufour *et al*, (2003), and see Dufour (2005) for a proof of first-order asymptotic validity. We perform standard and parametric bootstrap tests for each VAR method separately.

We perform sequential tests on 3-vector systems with real disposable income y, money $m1$, and one auxiliary variable chosen from the set $\{m2, u, o, rr\}$. VAR models are estimated using observations from the entire sample period, and observations from rolling sample windows of increasing and fixed width. VAR model orders are selected by minimizing the AIC over possible orders $p = 1...18$, subject to reasonably noisy residuals.

5.1. **Sample Period 1959-2002.** Extended test results for all auxiliary variables can be found in Table 1. For brevity, however, in the following we only discuss results based on the parametric bootstrap for models with the unemployment rate or M2.

5.1.1. ***Unemployment.*** We begin with the 3-vector process (Δy,$\Delta m1$,Δu). The chosen VAR order $p = 8$ minimizes the AIC. Larger orders lead to noisier residuals series, but result in qualitatively similar test results. In order to control for cointegration of unknown form the optimal order in levels is 9 based on minimizing the AIC, hence we use a VAR(10) model.

Both initial tests suggest money may anticipate income at some horizon (Test 0.1: .080, and Test 0.2: .040)[10]. We fail to reject the classic hypothesis

[9]Swanson *et al* (1996) demonstrate in a monte carlos study of tests of one-step ahead noncausation that the excess-lag method provides excellent empirical sizes, but tends to generate low power.

[10]Parenthetical values denote p-values derived from a parametric bootstrap.

$\Delta m1 \overset{1}{\nrightarrow} \Delta y$ (Test 1.0: .626), and sequentially reject at the nominal 5%-level only the compound hypothesis $\Delta m1 \overset{(4)}{\nrightarrow} \Delta y$[11].

If we perform each sequential test at the 1%-level, we fail to reject $\Delta m1 \overset{(5)}{\nrightarrow} \Delta y$ at a bounded 5%-level. If we perform each test at the level of the smallest compound test p-value (i.e. .032), then we reject $\Delta m1 \overset{(4)}{\nrightarrow} \Delta y$ at a bounded 13%-level. Weak evidence, therefore, suggests that fluctuations in the money supply anticipates growth in real income after a 3 month delay, $\Delta m1 \overset{4}{\rightarrow} \Delta y$. Graph-theoretically we deduce the chain $\Delta m1 \overset{1}{\rightarrow} \Delta u \overset{1:3}{\rightarrow} \Delta y$

For the excess-lag VAR(10) model in levels, we fail to reject initial tests of noncausality at all horizons. Moreover, evidence suggests a broken chain, $\Delta m1 \overset{1}{\rightarrow} \Delta u \overset{1}{\nrightarrow} \Delta y$ (Test 1.1: .000; Test 1.2: .376), further suggesting money never linearly anticipates income. Nevertheless, if we pursue tests at subsequent horizons and perform each sequential test at the 1%-level, then we reject $\Delta m1 \overset{(5)}{\nrightarrow} \Delta y$ at a bounded 5%-level. Once cointegration is controlled for, the level of significance of most tests, including the final sequential test, increases substantially.

5.1.2. ***M2***. Now consider the system (Δy,$\Delta m1$,$\Delta m2$). The minimum AIC order is $p = 6$ for first differences, however the lowest order at which we fail to reject the white-noise hypothesis for the residual series is $p = 10$. We opt for the VAR(10) model. Similarly, the optimal order for levels is $p = 7$, and the residuals are adequately noisy only if at least 11 lags are used. We therefore employ a VAR(12) model of excess lags in levels.

In the VAR(10) case we fail to reject $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ (Test 0.2: .198), suggesting fluctuations in M1 never anticipate real income growth. If we proceed to check individual horizons, we fail to reject $\Delta m1 \overset{1}{\nrightarrow} \Delta y$ (Test 1.0: .466), we find highly significant evidence for a causal chain, $\Delta m1 \overset{1}{\rightarrow} \Delta m2 \overset{1}{\rightarrow} \Delta y$, and reject the compound hypothesis of non-causation up to horizon h at the nominal 1%-level only for $h = 11$ (Test 11.0: .006), hence at a bounded 11%-level. This suggests $\Delta m1 \overset{1}{\rightarrow} \Delta m2 \overset{1:10}{\rightarrow} \Delta y$ and $\Delta m1 \overset{11}{\rightarrow} \Delta y$. Either money never causes real disposable income, or nearly one year passes before fluctuations in M1 will have an impact on real income through the non-M1 components of M2 (e.g. household savings and small time deposits).

For the excess-lag VAR(12) model in levels, we reject every null hypothesis at below the 1% level. We immediately deduce money anticipates real income one-month ahead. Similar to the model with the unemployment

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[11]We find significant evidence of a causal chain $\Delta m1 \overset{1}{\rightarrow} \Delta u \overset{1}{\rightarrow} \Delta y$ (Test 1.1: .000, Test 1.2: .026). Indeed, for each auxililary variable Z in models of either levels or differences, we find evidence in favor of $\Delta m \rightarrow Z$, with the level of significance below .1%. Thus, evidence strongly suggests the non-causality conditions of Theorem 3.2 are necessary *and* sufficient. We will, therefore, not comment on the issue below.

rate, significant evidence for causation expands sharply once cointegration is controlled for, supporting the major findings of Swanson (1998)[12].

5.1.3. ***All Auxiliary Variables.*** Based on an excess-lag models in levels, we fail to reject the hypothesis $Z \overset{1}{\nrightarrow} \Delta y$ (Test 1.2) for each scalar auxiliary variable $Z \in \{\Delta u,\ \Delta o,\ rr\} = Z_2$, say, where $Z_1 = \Delta m2$ (see Table 1). Moreover, we reject 1-month ahead noncausation from M1 to income in the truncated system $(\Delta y, \Delta m1, \Delta m2)$. Thus, M1 anticipates disposable income one-month ahead, and causal neutralization through the omitted variables is apparently impossible. Based on the ideas presented in Section 3.2, it is worthwhile, therefore, to check if the causality properties in the complete system $(\Delta y, \Delta m1,\ [\Delta m2, \Delta u, \Delta o, rr])$ are the same as in the truncated system $(\Delta y, \Delta m1, \Delta m2)$.

We estimated a VAR(12) excess lag model in levels[13], tested the joint hypothesis $(\Delta u, \Delta o, rr) \overset{1}{\nrightarrow} \Delta y$ (i.e. $Z_2 \overset{1}{\nrightarrow} X|I_{XYZ_1}$) and obtained a bootstrapped p-value of .229. From Theorem 3.4 we infer fluctuations in M1 causes real disposable income growth when only M2 is included, or when M2, the unemployment rate, the price of oil and the rate spread are included. Causal neutralization does not appear to be an issue. Of course, a classic 1-step ahead test of non-causation can be performed directly. A test of $\Delta m1 \overset{1}{\nrightarrow} \Delta y|I_{\Delta y,[\Delta m2,\Delta u,\Delta o,rr]}$ produces a bootstrapped p-value of .022.

Now consider the conclusions of Section 5.1.1 concerning the use of unemployment as an auxiliary variable. When the 3-vector $(\Delta y, \Delta m1, \Delta u)$ is analyzed, evidence suggests M1 *fails to cause* income 1-month ahead, yet when the complete set $(\Delta y, \Delta m1, [\Delta u, \Delta m2, \Delta o, rr])$ is analyzed evidence suggests M1 *causes* income 1-month ahead. Causal neutralization is evidently occurring via the omitted auxiliary variables. The association between $\Delta m1$ and the omitted auxiliary variables $(\Delta m2, \Delta o, rr)$, and the 1-month ahead causal impact that the omitted auxiliary variables $(\Delta m2, \Delta o, rr)$ have on Δy, evidently exactly offset the causal influence $\Delta m1$ has on Δy. Literally, fluctuations in the money supply do (evidently) linearly anticipate real income growth through the unemployment rate, but the effect is completely neutralized when a broad set of macroeconomic variates is considered.

[12]It should be pointed out that Swanson (1998) uses an industrial production index as "real income", aggregate prices and several measures of supply of money (M1, M2 and the Divisia measure of money) in a multivariate model, and control for cointegration of unknown form by use of the excess lag technique. We use real disposable income in a trivariate model (e.g. income, M1 and M2) similar in spirit to Boudjellaba *et al* (1992, 1994).

[13]Based on the AIC and Ljung-Box tests, the optimal VAR order for the compete vector process $(\Delta y, \Delta m1, \Delta u, \Delta m2, \Delta o, rr)$ is $p = 8$. In order to improve comparability with the above tests on the truncated system $(\Delta y, \Delta m1, \Delta m2)$, we employ a VAR(12) excess-lag model in levels.

5.2. **Rolling Windows.** Finally, we study patterns of causality from money to income over rolling sample periods of increasing and fixed length. Increasing windows begin and end with the sample periods 1959:01 - 1985:12 and 1959:01-2002:12, hence the initial window contains $n = 324$ months (before truncation due to lagging), and ends with $n = 528$ months for a total of 204 windows. We then fix the window length to 324 months, a sample size that corresponds to Stock and Watson's (1989) influential study. In this case, the initial sample period is 1959:01 - 1985:12 and the final period is 1971:11-2002:12, generating 205 windows.

Due to the large volume of tests required, we perform tests rather mechanically. VAR models of differences and levels (with excess lags) are both employed, and VAR orders are selected by minimizing the AIC over orders $p = 1...18$. For the excess-lag models we add one lag to the optimally selected order in lieu of evidence that the largest order of integration is one in any window. Although we collect residual white-noise test p-values for each window, the information is not used for model selection. We perform both standard and bootstrap tests of non-causality for each window for each VAR model in differences and levels, and keep a running count of rejections of the various hypotheses. Tests of the hypothesis that money never causes income are performed at the 5%-level. All other tests are performed at the nominal 1%-level. From Lemma 4.1 we infer the upper bound of the size of tests of $\Delta m1 \overset{(h)}{\nrightarrow} \Delta y$ is $.01 \times h$.

The criterion for detection of non-causation at all horizons ($\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$) is a failure to reject either Test 0.1 ($\Delta m1 \overset{1}{\nrightarrow} (\Delta y, Z)$) or Test 0.2 ($(\Delta m1, Z) \overset{1}{\nrightarrow} \Delta y$). We reject at $h = 1$ if we reject $\Delta m1 \overset{1}{\nrightarrow} \Delta y$; we reject $\Delta m1 \overset{(2)}{\nrightarrow} \Delta y$ if we fail to reject $\Delta m1 \overset{1}{\nrightarrow} \Delta y$, reject both intermediary tests, Test 1.1 ($\Delta m1 \overset{1}{\nrightarrow} Z$) and Test 1.2 ($Z \overset{1}{\nrightarrow} \Delta y$), and reject Test 2.0 ($\Delta m1 \overset{1}{\nrightarrow} \Delta y$, $\pi_{\Delta y, Z, 1} = 0$); and so on.

For a particular window we do not allow for rejection at multiple horizons: if we reject $\Delta m1 \overset{(h)}{\nrightarrow} \Delta y$ we stop the test procedure for the particular window. In this sense, our analysis concerns the earliest horizon at which causation takes place. We do, however, allow for simultaneous detection of non-causation at all horizons $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ and causation at some horizon, $\Delta m1 \overset{h}{\rightarrow} \Delta y$. We present window frequencies in which the two sets of tests contradict each other (i.e. detect $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ and $\Delta m1 \overset{h}{\rightarrow} \Delta y$). Horizon specific causality frequencies can be found in Table 2 for both increasing and fixed window length, and models of differences and levels.

5.2.1. ***First Differences, Increasing Windows.*** For VAR systems with the unemployment rate, sequential tests based on the parametric bootstrap detect non-causation at all horizons in fewer than 3% of all windows; causation 1-month or 2-months ahead is never detected; and causation 3 and 4 months ahead are detected in roughly 45% and 13% of all sample periods, respectively. In under 1% of all sample windows do we detect both non-causation at all horizons and causation at some horizon. Thus, there exists an unambiguous tendency for fluctuations in the money supply to anticipate growth in real disposable income after a discrete delay of 2-3 months as the unemployment rate adjusts. This both corroborates and strengthens evidence for causation at horizon $h = 4$ months within the complete sample period 1959-2002.

For VAR systems with M2 test evidence suggests both non-causation in all periods (over 96% of all periods), or causation 1-2 months ahead (23%-37% of all periods), with simultaneous detection of non-causation in all periods and causation in some period in roughly 60% of all windows. Such highly ambiguous evidence suggests extreme caution should be applied when interpreting tests of 1-step ahead non-causation in related money-income models with M2 (e.g. Boudjellaba *et al*, 1992, 1994; Amato and Swanson, 2001).

5.2.2. ***First Differences, Fixed Windows.*** A variable set with the unemployment rate $(\Delta y, \Delta m1, \Delta u)$ provides evidence of causation 3-4 months ahead, with a substantial increase in the number of windows suggesting causation exactly 4-months ahead. Allowing the sample period to increase (and thereby allowing the system to evolve toward a steady-state) suggests causation 4-months ahead occurs in only 13% of all windows. However, when we fix the sample size to 324 months (allowing for period-specific non-stationarity) we find evidence for causation at the same horizon in 56% of all sample periods. This pattern extends to the price of spot price of oil and the interest rate spread.

The most prominent characteristic is the significant increase in the number of windows providing any evidence of causation, except for the model with M2. Causation takes place between 1-5 months ahead through the unemployment rate in 94% of all fixed-length sample periods, compared to 58% when sample periods increase in length. VAR models with the unemployment rate again lead to a negligible frequency of contradictory test results.

5.2.3. ***Levels with Excess Lags, Increasing Windows.*** Once we control for cointegration a vastly different picture emerges. In over 53% and 40% of all sample periods for models with the unemployment rate and the interest rate spread, respectively, money linearly anticipates income 1-month ahead. Indeed, when M2 is the auxiliary variable direct causation from money to income is detected in 100% of sample windows, again supporting the major findings of Swanson (1998). Notice, however, that except for the model with M2, tests of non-causation at all horizons and at specific horizons are in substantial conflict.

5.2.4. ***Levels with Excess Lags, Fixed Windows.*** When the sample window is fixed at 324 months and cointegration is controlled for, evidence strongly points toward causality exactly one-month ahead (unemployment, M2), or causality 1-2 months ahead (oil). Similar to the case of increasing windows with levels, inclusion of M2 (unemployment) points to causation 1-month ahead in 96% (90%) of all windows. Only the model with M2 leads to a negligible frequency of contradictory test results (under 10% of all windows).

6. Conclusion

We develop a simple parametric recursion for VAR coefficients that, for trivariate processes with one scalar auxiliary variable, always allows for sequential linear parametric conditions for non-causality up to horizon $h \geq 1$. We develop a concise notation for causal chains and "chain graphs", and characterize the nature of information "compression" in VAR model parameters when auxiliary variables are removed. An empirical analysis of the money-income relationship reveals significant evidence in favor of linear causation from money to income, either directly when we control for cointegration, or indirectly after a delay of 1-3 months in models of first differences.

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Table 1

		Z = Δu^a		Z = $\Delta m2$		Z = Δo		Z = rr	
Test #	Hypothesis	Diff.	Level	Diff.	Level	Diff.	Level	Diff.	Level
Test 0.1	$\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y^b$	.080[e]	.000	.000	.000	.856	.020	.718	.000
Test 0.2	$\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y^c$	.040	.216	.198	.002	.560	.106	.166	.020
Test 1.0	$\Delta m1 \overset{1}{\nrightarrow} \Delta y$	.626	.148	.466	.008	.954	.026	.964	.032
Test 1.1	$\Delta m1 \overset{1}{\nrightarrow} Z$	.000	.000	.000	.000	.000	.000	.000	.000
Test 1.2	$Z \overset{1}{\nrightarrow} \Delta y$	.026	.376	.244	.000	.194	.516	.034	.526
Test 2.0	$\Delta m1 \overset{(2)}{\nrightarrow} \Delta y^d$	.450	.024	.082	.000	.890	.670	.912	.402
Test 3.0	$\Delta m1 \overset{(3)}{\nrightarrow} \Delta y$	.092	.012	.126	.000	.758	.854	.822	.396
Test 4.0	$\Delta m1 \overset{(4)}{\nrightarrow} \Delta y$	.032	.020	.128	.000	.742	.824	.892	.508
Test 5.0	$\Delta m1 \overset{(5)}{\nrightarrow} \Delta y$	.032[f]	.008[g]	.066	.000	.734[j]	.850[k]	.710[l]	.526[m]
...	...	-	-	....	....	-	-	-	-
Test 11.0	$\Delta m1 \overset{(11)}{\nrightarrow} \Delta y$	-	-	.006[h]	.000[i]	-	-	-	-
	Min. AIC order p	8	10	10	12	4	6	6	8
	Ljung–Box p-value	.045	.009	.370	.183	.004	.001	.009	.002

Notes: a. u = civilian unemployment rate, m2 = M2, o = spot oil price, rr = rate spread.

b. Equivalent test: $\Delta m1 \overset{1}{\nrightarrow} (\Delta y, Z)$; c. Equivalent test: $(\Delta m1, Z) \overset{1}{\nrightarrow} \Delta y$.

d. The test equivalent for each $\Delta m1 \overset{(h)}{\nrightarrow} \Delta y$ is: $\Delta m1 \overset{1}{\nrightarrow} \Delta y, \pi_{\Delta y,z,i}$=0, i=1...h-1.

e. p-values based on a parametric bootstrap.

f. Reject $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ at 10%-level, and reject $\Delta m1 \overset{(4)}{\nrightarrow} \Delta y$ at bounded 13%-level.

g. Fail to reject $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ at 10%-level; or reject $\Delta m1 \overset{(2)}{\nrightarrow} \Delta y$ at bounded 5%-level.

h. Fail to reject $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ at 10%-level; or reject $\Delta m1 \overset{(11)}{\nrightarrow} \Delta y$ at bounded 11%-level.

i. Reject $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ at 1%-level, and $\Delta m1 \overset{1}{\nrightarrow} \Delta y$ at 1%-level.

j. Fail to reject $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ at 10%-level; or fail to reject $\Delta m1 \overset{(5)}{\nrightarrow} \Delta y$.

k. Reject $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ at 10%-level, and reject $\Delta m1 \overset{1}{\nrightarrow} \Delta y$ at 5%-level, or fail to reject $\Delta m1 \overset{(5)}{\nrightarrow} \Delta y$ at bounded 5%-level.

l. Fail to reject $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ at 10%-level; or fail to reject $\Delta m1 \overset{(5)}{\nrightarrow} \Delta y$.

m. Reject $\Delta m1 \overset{(\infty)}{\nrightarrow} \Delta y$ at 5%-level, and reject $\Delta m1 \overset{1}{\nrightarrow} \Delta y$ at 5%-level or fail to reject $\Delta m1 \overset{(5)}{\nrightarrow} \Delta y$ at bounded 5%-level.

Table 2: Rolling Windows

Horizon Rejection Frequencies: *First Differences*

Increasing Width Rolling Windows					Fixed Width Rolling Windows				
Horizon	u	$m2$	o	rr	Horizon	u	$m2$	o	rr
0[a]	.029[b]	.9601	.637	.765	0	.029	.971	.240	.873
1	.000	.230	.000	.000	1	.172	.128	.049	.157
2	.000	.368	.157	.000	2	.005	.103	.382	.108
3	.451	.000	.015	.039	3	.201	.005	.000	.034
4	.128	.000	.000	.000	4	.559	.005	.000	.000
5	.005	.005	.000	.000	5	.005	.000	.000	.000
≥ 1[c]	.579	.598	.172	.039	≥ 1	.936	.240	.431	.299
0, ≥ 1[d]	.010	.574	.029	.039	0, ≥ 1	.029	.216	.108	.230

Horizon Rejection Frequencies: *Levels with Excess Lags*

Increasing Width Rolling Windows					Fixed Width Rolling Windows				
Horizon	u	$m2$	o	rr	Horizon	u	$m2$	o	rr
0	.000	.000	1.00	1.00	0	.409	.098	.366	1.00
1	.532	1.00	.059	.405	1	.902	.956	.615	.459
2	.234	.000	.668	.049	2	.024	.039	.171	.098
3	.010	.000	.000	.000	3	.044	.000	.000	.005
4	.000	.000	.000	.000	4	.000	.000	.000	.010
5	.176	.000	.000	.000	5	.000	.000	.000	.000
≥ 1	.951	1.00	.727	.454	≥ 1	.971	.995	.785	.571
0, ≥ 1	.951	.000	.727	.454	0, ≥ 1	.381	.093	.293	.571

Notes: a. h = 0 denotes non-causation at all horizons: values are window frequencies for which we fail to reject H_0^{∞}.

b. Values denote window frequencies based on bootstrapped p-values.

c. Window frequencies for causation at any horizon h $\geq$ 1.

d. Window frequencies for non-causation at all horizons, h = 0, and causation at some horizon h $\geq$ 1.

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