

# MACROECONOMICS AFTER A DECADE OF RATIONAL EXPECTATIONS: SOME CRITICAL ISSUES

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## Introduction

It has now been just over a decade since the start of the rational expectations revolution in macroeconomics. In saying that, I am accepting the conventional view that the first papers to be widely influential were those published in 1972 by Robert Lucas.<sup>1</sup> As is well known, these were soon followed by landmark pieces by Thomas Sargent (1973) (1976a), Sargent and Neil Wallace (1975), and Robert Barro (1976) (1977a), as well as others by Lucas (1976) (1977).<sup>2</sup> And, as is also well known, the revolution has been highly controversial because of the criticism of prevailing views that was implicit in the above-mentioned papers and explicit in others (e.g., Barro (1979), Lucas and Sargent (1978)).

Today the dispute seems to be less heated than it was a few years ago, with members of the leading schools of thought openly recognizing weaknesses in their own theories and strengths in those of others. Of course, major differences continue to exist, as consideration of recent papers by Taylor (1982), Kydland and Prescott (1982), and Sargent and Wallace (1982) will emphasize. But the terms of disagreement are no longer about the hypothesis of rational expectations—some version of the latter is utilized in almost all current research—but about the nature of the economy within which agents operate and form expectations.

In this regard, the portion of a macroeconomic

model that most strongly affects its policy-relevant characteristics is that pertaining to aggregate supply behavior. Accordingly, I will begin this presentation by discussing some competing theories of aggregate supply currently being utilized in rational expectations (RE) models, with emphasis on the distinction between “equilibrium” and “sticky-price” assumptions. This section will also include a brief description of a model that I find attractive and some discussion of the RE version of the natural-rate hypothesis. In the next section I will more briefly mention a few issues involving specification of the aggregate demand portion of macroeconomic models, with attention devoted to the role of the overlapping-generations framework. Finally, I want to consider a recent attempt to denigrate the importance of Lucas’s critique (1976) of traditional policy-evaluation techniques, an attempt that makes use of “vector autoregression” models. Throughout I will take it for granted that there is no need to spend time justifying the rational expectations assumption itself.

## Flexible and Sticky Price Models

It is of course widely understood that properties of RE models with multiperiod nominal contracts (e.g., Fischer (1977), Taylor (1979a)) are very different from those in which prices adjust fully within each period. Let us begin by considering which type is more useful for analysis of actual present-day economies.

In my opinion there is at least one reason for believing that some type of sticky-price model is needed to provide an empirically satisfactory description of quarter-to-quarter or even year-to-year fluctuations in prices, output, and other macroeconomic variables. In saying that, I have in mind several empirical regularities or “stylized facts” including the following:<sup>3</sup>

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<sup>1</sup> Specifically, Lucas (1972a) (1972b). Of course a few papers had previously been published using rational expectations in macroeconomic settings, but these did not have a great deal of impact.

<sup>2</sup> Important items were also produced by Fischer (1977), Taylor (1979a) (1979b), and others.

<sup>3</sup> Evidence supporting these facts appears in a large number of studies, including Sargent (1976a), Barro (1977a), Mishkin (1982), Sims (1980), Kennan and Geary (1982), and Gordon (1982).

(i) Output and employment magnitudes exhibit significant "persistence," i.e., positive serial correlation.

(ii) Output and employment magnitudes are strongly and positively related to contemporaneous money stock surprises.<sup>4</sup>

(iii) Output and employment magnitudes are not strongly and positively related to contemporaneous price level surprises.

(iv) Real wages do not exhibit countercyclical tendencies; indeed they appear to be mildly procyclical.

Furthermore, I have in mind a fact of a different kind, namely, that information concerning nominal aggregate variables—including money stock measures and various price indices—is available on a relatively prompt basis. The relevant point, then, is that this availability is hard to reconcile with fact (ii) in a flexible-price equilibrium model, for the existence of real effects of monetary shocks depends, in these models, upon agents' ignorance of contemporaneous values of nominal aggregates.<sup>5</sup> It was suggested by Lucas (1977) that this difficulty might be overcome if the "true" relevant monetary aggregate were unobservable and thus measured with error. King (1981) has shown, however, that if observations are available on a "proxy" variable that differs randomly from the true unobservable aggregate, output and employment should be unrelated to the proxy. Thus, according to these models, output and employment should be unrelated to movements in *measured* monetary aggregates, in contrast with fact (ii). King's analysis has been further developed and implemented by Boschen and Grossman (1983).<sup>6</sup>

A second reason for doubting the adequacy of flexible-price equilibrium models is provided by econometric studies which suggest that output fluctuations are induced by *anticipated* monetary move-

ments, as well as surprises.<sup>7</sup> These studies have some weaknesses<sup>8</sup> and there is not a strict one-to-one relationship between flexible-price equilibrium models and the absence of real effects from anticipated money movements. The relationship is close enough and the quality of the cited studies high enough, however, that the findings are troublesome for the flexible-price hypothesis.

In this regard I would like to emphasize that acceptance of the idea, that some kind of price-level stickiness is necessary for explaining observed time series data, does not require abandonment of the *equilibrium approach* to macroeconomic analysis. To see this, imagine a model in which nominal multi-period contracts are endogenously explained as the response of rational agents to adjustment, bargaining, or other "transactions" costs.<sup>9</sup> As Lucas (1980, p. 712) has recognized, such a model could be an equilibrium model—one in which all agents optimize relative to correctly-perceived constraints and in which the resulting supplies and demands are equated—though one without perfectly flexible prices. As such, it would incorporate the virtues of equilibrium analysis, including the intellectual discipline that it entails, a specification expressed in terms of policy-invariant relationships, and the possibility of basing policy choices on the utility of individual agents.

Indeed, such a model would seem to be precisely what is needed for the analysis of stabilization policy. As Fischer (1977, p. 204) acknowledged, it is likely that the format and length of nominal contracts agreed to by rational agents would change in response to major shifts in policy. So, even if existing contract models were capable of providing a good explanation of macroeconomic fluctuations within a single policy regime, they would tend to be unreliable if used to predict the comparative effects of alternative regimes.<sup>10</sup>

<sup>4</sup> Here and below I use the term "surprise" to refer to a one-period expectational error of the form  $m_t - E_t - 1m_t$ , in notation discussed below.

<sup>5</sup> This ignorance is required, to be more precise, in the three leading flexible price equilibrium models, namely, those of Lucas (1972a), Lucas (1973), and Barro (1981, pp. 42-50). It is possible that other such models do not have this property.

<sup>6</sup> The relevant point was mentioned by Barro (1981) and was very recently emphasized by Grossman (1982). Grossman recognizes, but does not accept, the possibility that money-output correlations are due to "reverse causation," i.e., monetary responses to output movements generated by shocks to technology or preferences, as suggested by King and Plosser (1982).

<sup>7</sup> See, for example, Gordon (1982) and Mishkin (1982).

<sup>8</sup> Movements in "natural rate" values of output or employment are assumed to be representable by trends, in contrast to the evidence given by Nelson and Plosser (1982). Also, the methods of overcoming the "observational equivalence" difficulty (Sargent, 1976b) are not entirely satisfying.

<sup>9</sup> The difficulty with this exercise comes in understanding why contracts are set in nominal terms without indexing.

<sup>10</sup> The problem is of course compounded in attempts to predict the effects of real-time **changes** in regimes because expectations are unlikely to adjust immediately to the new policy rule.

From the foregoing perspective, existing nominal contract models are best seen as incomplete models—ones that treat as fixed important parameters that would tend to be constant within regimes but to change across regimes. Even in their present state these models are of interest, however, so I would like to devote a few paragraphs to a comparison and discussion of the two most influential, those of Fischer (1977) and Taylor (1979a) (1980). For simplicity, I shall refer to two-period versions of each.

In both the Fischer and Taylor papers, a rudimentary aggregate demand function—one that makes the quantity demanded a fixed stochastic function of real money balances alone—is utilized, so no difference arises from that component. The wage-price or aggregate supply components are very different, however, despite the common feature of two-period, staggered, nominal wage contracts. Specifically, in each model nominal wages are set at the start of period  $t$  to apply to half of the workforce in periods  $t$  and  $t+1$ , but the values at which these wages are set are chosen according to different principles. In Fischer's model, the wages set for  $t$  and  $t+1$  will usually differ from each other and each is chosen, in light of current price-level expectations, so that the real wage is expected to clear the labor market in the relevant period. In Taylor's model, by contrast, the same value is set for periods  $t$  and  $t+1$  and is chosen to equal the average of the nominal wage rates expected to prevail for the other half of the workforce in  $t$  and  $t+1$ , with an adjustment added to take account of (expected) excess demand.

Prices, moreover, are assumed to move in unison with the average wage in Taylor's model, so that there is no systematic (or unsystematic) cyclical variation in the real wage. Fischer, on the other hand, assumes that firms select employment (hence, output) magnitudes in each period so as to equate the marginal product of labor to the observed real wage. Consequently, there is a tendency for the real wage to be high when employment is low.

Of these two models, Taylor's has attracted more attention and has been the more influential. One reason, undoubtedly, is that Taylor himself has produced a number of technically sophisticated and economically interesting applications involving actual data and policy issues of current concern. I suspect that there is an additional reason, however, which is the existence of a widespread belief that Taylor's model is substantially more consistent with crucial facts. In particular, it is believed that Taylor's model is more plausible than Fischer's because it generates

more persistence (for a given contract length) and does not yield the counterfactual implication that real wages move countercyclically. Consequently, I think that it is important to understand that neither of these observations is entirely compelling and that Taylor's model has some implications of its own that are theoretically unattractive.

With respect to the persistence issue, it should be kept in mind that there are several plausible ways of rationalizing persistence in any RE model. Among these are the existence of employment adjustment costs, the presence of finished-goods inventories, and the inability of agents to distinguish between permanent and transitory shocks.<sup>11</sup> Any of these features could be included in a variant of Fischer's model without altering the properties that his paper focussed upon. Furthermore, the relevant theoretical concepts involve output or employment measured *relative* to capacity (natural rate) values. But of course we do not possess direct observations on these relative magnitudes; the stylized fact (i) refers to raw measures of output and employment or to measures adjusted by the removal of a deterministic trend. And recent work by Nelson and Plosser (1982), which relies upon stochastic trend removal, suggests that there is much less persistence in the relevant adjusted series than the raw or deterministically-detrended measures have indicated.

Next, the countercyclical real wage in Fischer's model does not come from its wage-setting specification, but from an independent assumption regarding employment determination—i.e., that firms equate the marginal product of labor to the real wage. Now the counterpart of that relation in Taylor's model is the condition that the (detrended) real wage is constant. But that condition implies that product prices behave in the same way as average nominal wages, which also seems counterfactual.<sup>12</sup>

These arguments suggest that the above-mentioned reasons for preferring Taylor's model to Fischer's are not compelling. A point of equal or greater importance is that Taylor's model possesses a questionable feature, namely, a presumption that labor supply-demand behavior is fundamentally concerned with relative, rather than own, wages. As a result of this feature, together with contract staggering, the

<sup>11</sup> The last two features have been analyzed by Blinder and Fischer (1981) and Brunner, Cukierman, and Meltzer (1980), respectively, while the first has been emphasized most notably by Sargent.

<sup>12</sup> My argument is not that real wage movements induce business cycles, but that some systematic movements in real wages are observed.

model does not possess the *natural-rate* property as defined by Lucas (1972b).<sup>13</sup> That is, the model is one in which a suitably-designed monetary policy is capable of yielding a *permanent* increase in output relative to its natural-rate value: monetary policy can keep unemployment "low" forever.<sup>14</sup>

Having mentioned various shortcomings of the Fischer and Taylor models, let me now discuss an alternative that I find attractive, one which conforms to the natural rate hypothesis and also to all of the stylized facts mentioned above.<sup>15</sup> For the sake of simplicity and ease of comparison, the discussion will presume a rudimentary aggregate demand schedule. This can be expressed formally as

$$(1) \quad y_t = b_0 + b_1(m_t - p_t) + v_t \quad b_1 > 0$$

where  $y_t$ ,  $m_t$ , and  $p_t$  are logs of output, the money stock, and the price level while  $v_t$  is a white-noise disturbance. Also for simplicity, the log of the "natural rate" level of output,  $\bar{y}_t$ , is assumed to deviate from its previous value only by virtue of a white-noise disturbance,  $u_t$ :

$$(2) \quad \bar{y}_t = \bar{y}_{t-1} + u_t.$$

In addition—and again only for the sake of simplicity—I assume that output is perishable, so that no inventories are held.

The crucial aspect of the model is the way in which prices are determined. It is assumed that  $p_t$  is set, at the end of period  $t-1$ , at a level that is expected to make the quantity demanded in  $t$  equal to a weighted average of  $y_{t-1}$  and  $\bar{y}_t$ . Two basic ideas are involved in this assumption. The first is that firms find it optimal to meet all demands at the quoted price.<sup>16</sup> Second, firms experience adjustment costs whenever  $y_t$  differs from  $y_{t-1}$  but also suffer opportunity costs whenever there is any discrepancy between  $y_t$  and  $\bar{y}_t$ .

<sup>13</sup> With staggering, relative wages pertain to values set in different periods. If the relationship between such values depends upon output (relative to capacity), as Taylor's model assumes, then the latter variable will be affected by the trajectory of nominal wage settlements. I am indebted to Taylor for explaining to me that it is not an assumed concern for relative nominal wages, as opposed to relative real wages, that is responsible for this feature.

<sup>14</sup> Fischer's model, by contrast, does possess the natural-rate policy.

<sup>15</sup> This specification is mentioned, but not investigated, in McCallum (1980, p. 735).

<sup>16</sup> The analogous requirement would not seem extreme or unusual in a version of the model in which inventories are held.

Then if both of these cost functions are quadratic, producers will aim at some value between  $y_{t-1}$  and  $\bar{y}_t$  which we denote as  $\lambda y_{t-1} + (1-\lambda) \bar{y}_t$ , with the parameter  $\lambda$  ( $0 \leq \lambda < 1$ ) reflecting the *relative* costliness of output changes. Consequently, the price level is set at a value that satisfies (1) expectationally, with  $\lambda y_{t-1} + (1-\lambda) E_{t-1} \bar{y}_t$  inserted in place of  $y_t$ :

$$(3) \quad \lambda y_{t-1} + (1-\lambda) E_{t-1} \bar{y}_t = b_0 + b_1(E_{t-1} m_t - p_t).$$

Here, of course,  $E_{t-1}(\cdot)$  denotes the mathematical expectation of the indicated variable, conditional upon realizations of all variables in period  $t-1$  and earlier. The price-setting relation (3) can be expressed in various ways. One version that I have emphasized elsewhere takes the form of a modified expectational Phillips-Curve relationship, namely

$$(3') \quad p_t - p_{t-1} = \gamma(y_{t-1} - \bar{y}_{t-1}) + E_{t-1}(\bar{p}_t - \bar{p}_{t-1}),$$

$$\gamma = (1-\lambda)/b_1 > 0,$$

in which the relevant expected inflation rate is that pertaining to  $\bar{p}_t$ , the value of  $p_t$  that equates  $y_t$  to  $\bar{y}_t$  in (1).

The other main component of the model incorporates Fischer's scheme of nominal wage determination. Let  $w_t$  be the log of the average nominal wage in period  $t$  and let  $z_t$  denote the log of the real wage,  $z_t = w_t - p_t$ . Also let  $\bar{z}_t$  be the natural-rate value of  $z_t$ , which evolves over time as a random walk related to that generating  $\bar{y}_t$ :

$$(4) \quad \bar{z}_t = \bar{z}_{t-1} + \zeta_t, \quad E(u_t \zeta_t) > 0.$$

Then with half of the wage contracts prevailing in  $t$  having been set at the end of  $t-1$ , and the other half at the end of  $t-2$ , we have

$$(5) \quad w_t = (\frac{1}{2}) E_{t-1}(\bar{z}_t + p_t) + (\frac{1}{2}) E_{t-2}(\bar{z}_t + p_t).$$

Finally, to complete the system we suppose that the monetary authority sets  $m_t$  according to some policy feedback rule, utilizing data from periods  $t-1$  and before. Without specifying the form of the systematic component, we can write

$$(6) \quad m_t = E_{t-1} m_t + e_t,$$

thereby defining  $e_t$  as the (white noise) random component of policy behavior. In principle, equa-

tions (1)-(6) govern the evolution of the six variables  $y_t$ ,  $\bar{y}_t$ ,  $p_t$ ,  $m_t$ ,  $\bar{z}_t$ , and  $w_t$  (with  $z_t$  given definitionally as  $w_t - p_t$ ).

It is easy to see from equations (1), (2), (3), and (6) that, in this model, output conforms to the process

$$(7) \quad y_t - \bar{y}_t = \lambda(y_{t-1} - \bar{y}_{t-1}) + b_1 e_t + v_t - u_t.$$

Thus we can verify by inspection that stylized facts (i), (ii), and (iii) are mimicked by our model: output is positively related to monetary surprises but not to one-period price level surprises (as  $p_t = E_{t-1} p_t$ ), and both  $y_t$  and  $y_t - \bar{y}_t$  are positively autocorrelated. Furthermore, it can be shown that, for a wide class of specifications for the systematic component of monetary policy,  $z_t$  and  $y_t$  are positively correlated. Thus the model also conforms to the stylized fact (iv). And from (7) it is obvious that the natural-rate property obtains.

Indeed, it is clear from (7) that the famous *policy-ineffectiveness* proposition obtains in the model at hand. But while that result is useful as a counterexample to some mistaken notions about necessary conditions for validity of the ineffectiveness proposition, I do not think that very much should be made of it. The reason is that the result is not highly robust: while it holds if the aggregate demand specification (1) is changed to

$$(1') \quad y_t = \beta_0 + \beta_1(m_t - p_t) + \beta_2 E_{t-1}(p_{t+1} - p_t) + v_t,$$

it does not hold if instead we have

$$(1'') \quad y_t = \beta_0 + \beta_1(m_t - p_t) + \beta_2 E_t(p_{t+1} - p_t) + v_t.$$

Nor, more importantly, does it hold if the information set used in computing the expectation of  $p_{t+1}$  includes the current interest rate, as well as past values of all variables. This last specification would seem to be empirically relevant, given the existence of daily reports on interest rates in nation-wide markets.

But while I do not want to argue for the general validity of the ineffectiveness proposition, even as a matter of theory, I do want to mention parenthetically that many of the alleged theoretical demonstrations of its invalidity rely on a misinterpretation. The point is that the proposition asserts that the systematic components of monetary and fiscal policies have no influence on the evolution of output or

employment *relative to* their natural rate (capacity, full-information) values—not to the raw values themselves. The proposition is designed to pertain to issues about countercyclical *stabilization* policy, which has always been conceived of as a device for keeping output and employment close to their natural-rate values, not for altering the paths of the latter variables. A more extended discussion of this issue, including some examples of published misinterpretations, is presented in McCallum (1980, pp. 726-729).

The model outlined above can be extended in many ways—by including fiscal variables and/or inventory holdings, by positing more realistic processes for  $\bar{y}_t$  and  $\bar{z}_t$ , etc.—without altering its main properties. Thus it provides, in my opinion, an attractive and useful framework for thinking about macroeconomic fluctuations and stabilization policy. It has some weaknesses, however, that should be acknowledged. First, the implicit assumption that price changes are prohibitively costly within each period, but costless between periods, is extreme and difficult to justify except by definition of the “period.” And with that justification there is no guarantee that the periods so defined will correspond to the quarter-year periods in which most actual data is reported. Also, the *length* of a theoretical period could be affected by extreme conditions, such as those experienced during hyperinflations. Consequently, the period definition may not be fully policy-invariant.

Perhaps the most basic weakness of the model is the absence of any compelling explanation for the absence of indexing.<sup>17</sup> Why is it, in other words, that posted prices do not come with a proviso that automatically adjusts them in response to monetary surprises? The usual answer is that such arrangements are costly, but the validity of that answer is by no means self-apparent. The difficulty is, however, one that is not specific to this model. It merely reflects economists’ incomplete understanding of why contracts are often made in nominal terms. More generally, the above-mentioned flaws are a reflection of the fact that this model is incomplete, in the sense described above. An equilibrium rationalization of its price-setting arrangements has not been developed.

To conclude my discussion of issues involving aggregate supply, I would like to return to the subject of the natural rate hypothesis (NRH) and comment upon its present status. In particular, I want to emphasize that a number of influential researchers

<sup>17</sup> This issue was introduced by Barro (1977b).

in the Keynesian tradition<sup>18</sup> have in recent years expressed agreement with the NRH, yet have continued to conduct analysis in models that do not possess the NRH property.<sup>19</sup> A prominent example of a specification of this type is provided by models that incorporate the concept of a “nonaccelerating-inflation rate of unemployment” (NAIRU). Clearly, if there exists a stable negative relationship between unemployment and the acceleration magnitude (i.e., change in the inflation rate), then the unemployment rate can be permanently lowered by permanently accepting a higher rate of change of inflation—in contradiction to the NRH. Another example is provided by models that include demand and supply functions expressed in real terms together with a partial adjustment relation for a nominal price variable and the assumption that the transaction quantity is the smaller of supply and demand (or that demand is determining).<sup>20</sup> In such a formulation, there is an implied permanent tradeoff between the rate of change of the price variable and real excess demand.

Proponents of such specifications would no doubt admit that their implications regarding unemployment magnitudes under conditions of sustained accelerating inflation are implausible, but would presumably contend that the models are not intended to be applicable to extreme policies of that type. For predicting the consequences of less extreme policies, they would claim, the models are appropriate. It is not clear, however, that such a claim is justifiable. What is needed for the model's predictions to be plausible is that the policy followed be essentially the *same* as that of the sample period used in estimating the relationship. But to agree to that limitation is to admit that the model cannot be used for most interesting questions. In terms of Tobin's (1980, pp. 66-68) exercise, for example, I would say that a gradual but reliable and sustained decrease in the rate of growth of nominal GNP—or the money stock or any other nominal aggregate—is very *unlike* the policies of the past two decades. Thus the simulation predictions are not persuasive.

More generally, I would argue that the nonconformity of any model to the NRH property provides *prima facie* evidence of some implied form of irrationality and an associated vulnerability of the

model to the famous Lucas (1976) “critique.” In other words, nonconformity of any model to the NRH indicates that it will be systematically unreliable in predicting the consequences of alternative policy choices.<sup>21</sup> Other points concerning the Lucas critique will be discussed in the sections that follow.

## Aggregate Demand

To this point we have been concerned with issues involving aggregate supply behavior. Let us then more briefly consider some developments having to do with aggregate demand.<sup>22</sup>

As our previous discussion hinted, Lucas, Sargent, and other leaders in the RE area have advocated the use of aggregative general equilibrium models for macroeconomic policy analysis. The object of this strategy is to avoid the weaknesses of traditional macroeconomic models, weaknesses that were emphasized in Lucas's critique (1976). The hope is that it may be possible to develop models that are genuinely structural—i.e., policy invariant—by working “at the level of objective functions, constraint sets, and market-clearing conditions” (Sargent, 1982, p. 383). Since this equilibrium *approach* does not limit the user to flexible price models, it is almost impossible not to sympathize with it, at least at the level of principle. Adherence to the approach is not a guarantee of success, however: if a model is based on a poorly-specified objective function it will be a poor model, explicit maximization analysis notwithstanding.

Since this last qualification is obvious to the point of triviality, an example of how the approach can go astray may be of some interest. The example that I have in mind involves the application of a class of overlapping-generations (OG) models to problems in monetary economics. The class of OG models in question is that in which, although there is an inherently useless entity called “fiat money,” the specification excludes any cash-in-advance or money-in-the-utility-function feature that would represent a transactions-facilitating property for that entity. Accordingly, the entity does not serve, in these models,

<sup>18</sup> Including Tobin (1980), Modigliani (1977), and Gordon (1982).

<sup>19</sup> See Tobin (1980, pp. 66-68), Modigliani and Papanemos (1975), and Gordon and King (1982).

<sup>20</sup> This sort of formulation mars, for example, an interesting and otherwise attractive study by Smyth (1982).

<sup>21</sup> This is, I would suggest, the true message of Lucas (1972b) and one of the most basic messages of the RE revolution.

<sup>22</sup> Of course the distinction is not a clean one in equilibrium models, since agents in such models make factor supply and commodity demand choices simultaneously and in response to the same wealth and price variables. What is here meant by an “aggregate demand” topic is one that focuses attention on saving and/or asset-demand relationships.

as a medium of exchange; its only function is as a store of value.<sup>23</sup> Consequently, several striking and unusual conclusions are obtained when the entity is interpreted as money. For example, if the government causes the stock of money to grow at a rate even slightly in excess of the rate of output growth, the price level will be infinite (i.e., money will be valueless). Second, equilibria in which the price level is finite will be Pareto optimal if and only if the growth rate of the money stock is nonpositive. Third, "open-market" increases in the stock of money have no effect on the price level. I have argued at length, however, that these unusual conclusions obtain because of the model's neglect of the medium-of-exchange role (McCallum, 1983). If the model is modified so as to reflect this role for the entity called money, its unusual conclusions vanish. Consequently, the unmodified class of OG models evidently provides a misleading vehicle for the analysis of economies in which there is a medium of exchange.

It remains to be explained what this OG example has to do with the equilibrium approach. To understand the connection let us recall that an essential aspect of the approach is the development of policy-invariant relations. Now in dynamic settings, as Sargent (1982) has stressed, standard asset demand functions may not be policy-invariant; one must look "beyond decision rules to the objective functions that agents are maximizing and the constraints that they are facing" (p. 383). But the influence on agents' constraints of the store-of-value function of money is clear and simple to express analytically, while the influence of the medium-of-exchange function is just the opposite. Indeed, it is extremely difficult to devise a general equilibrium setting in which the medium-of-exchange role is both rigorously and convincingly depicted. The traditional method has of course been to include real money balances as an argument of agents' utility functions, but that is an unsatisfying practice which clearly must be proxying for something more fundamental. Together these considerations encourage analysts to shun the traditional approach and adopt ones that focus attention on money as a store of value. And because they are well-suited in important ways for the analysis of store-of-value issues, OG models provide an attractive vehicle. Thus it is not very surprising that an OG model without medium-of-exchange features

would be adopted by researchers striving to overcome the Lucas critique. But that attempt will nevertheless be unsuccessful if the model is used for certain monetary issues, for neglect of the medium-of-exchange function constitutes a potentially serious specification error. The Lucas critique itself amounts to a reminder (of an especially important type) that specification errors will keep a model from being policy invariant.

Turning to a substantive matter, it is interesting to note that an OG model of the type discussed above has recently been used by Sargent and Wallace (1982) in an attempted rehabilitation of the infamous "real bills" doctrine. Since one of Henry Thornton's important contributions to monetary economics was his criticism of that doctrine, a few brief remarks should be in order. In their recent paper, Sargent and Wallace argue that (among other things) the price level is determinate under a real-bills policy regime that pegs the interest rate at zero, a finding that contrasts sharply with the price-level indeterminacy result of their famous (1975) paper. Examination of the recent argument indicates, however, that determinacy is not actually established. What the paper shows is that each agent faces the same real budget constraint under the real-bills regime as under a "laissez-faire" regime in which the stock of fiat money is held fixed. But this implies only that the real aspects of the model's equilibria are the same under the two regimes; nothing is implied about nominal magnitudes. Furthermore, the interest rate in the Sargent-Wallace (1982) model does not, because of this model's neglect of the medium-of-exchange role of money, correspond to interest rates in actual economies. Thus pegging its real value at zero does not require a negative real return on money (i.e., positive inflation) as is the case in settings in which nonmonetary assets command higher rates of return than money because of the latter's transaction-facilitating properties. Consequently, the recent Sargent-Wallace paper does not provide a convincing reason for believing Thornton's analysis to be incorrect.

### **The VAR Challenge to the Lucas Critique**

The final topic to be discussed also concerns the Lucas critique. Previously I have claimed that its basic message—i.e., that traditional econometric models are poorly designed for policy evaluations because their basic relationships are unlikely to be policy invariant—has been very widely accepted, even

<sup>23</sup> Notable items in the literature in question are Bryant and Wallace (1979), Sargent and Wallace (1982), and Wallace (1980).

by economists who dispute other notions associated with the RE revolution (McCallum 1979, 1980). That situation still prevails, I believe, but within the past few months a notable challenge has arisen. More specifically, a number of prominent economists, who are certainly well aware of the critique, have authored papers in which so-called vector autoregression (VAR) models are used for policy analysis.<sup>24</sup> These VAR models are, as is well-known, constructed in a manner that involves no attempt to represent structural relationships; they consist of a set of reduced-form equations in which lagged values of the system's variables are used to explain current values, with all variables treated as endogenous. Consequently, VAR systems would seem to be even more vulnerable to the critique than the traditional econometric models that Lucas considered. One is naturally led, then, to ask: what is the justification given by those who have used VARs for policy analysis? In fact most users have provided no justification themselves, but have referred to a recent paper by Christopher Sims, the originator of VAR techniques. Let us then consider the argument put forth in that paper (Sims, 1982).

One important theme of Sims's discussion is that equilibrium-approach econometric techniques (exemplified by Hansen and Sargent (1980)) are unlikely to lead to accurate predictions of the effects of real-time *changes* in policy rules, as opposed to cross-regime steady-state comparisons. As it happens, that suggestion seems to me to be correct. But it also seems rather beside the point, since Lucas, Sargent, and other equilibrium-approach leaders have not claimed to be able to use their models in that way. Instead, they have expressed the aim of being able to make valid comparisons of the properties of stochastic steady states generated by alternative maintained policy regimes.

Another theme of Sims's paper is that genuine policy-rule or regime changes are extremely rare in actuality. Most policy *actions* involve instead the resetting of policy instruments in response to recent developments in the economy, a type of activity that Sims calls "normal policymaking." Again I would agree with the observation—but point out that it is in no way inconsistent with the Lucas critique.

In addition, however, Sims claims that VAR methods can be useful in the context of normal policy-

making. Since this claim appears to be inconsistent with the message of the critique, let us briefly examine the argument. Under a given policy regime, a policymaker's objectives are by definition unchanging through time. So if the structure of the economy were known and also unchanging, policy feedback rules would be unchanging and there would be no purpose for policy exercises using any kind of model. But of course the true structure of any actual economy is imperfectly known and probably changing, so there could often be some potential gain from re-estimation of models used to design policy. And with objectives constant, autoregressive representations of expectational variables may be changing only slowly and gently, so VAR models may not go badly astray in the way described by Lucas. Thus there could be some benefits from period-by-period re-estimation of VAR systems and their utilization in the selection of current instrument settings.

In this case, the argument seems plausible but not extremely consequential. What it suggests is that VARs can be helpful to policymakers, but only if the latter continue to behave in approximately the same way as in the past. There is no claim that VARs could be useful in evaluating the effects of substantially different sustained policies. Furthermore, the argument provides no compelling reason for believing that VAR methods would be superior, even in the context of normal policymaking, to Hansen-Sargent techniques.

Now let me turn to my outright disagreements with Sims's paper, of which there are two. The first involves an application of VAR methods in the context of an analysis of announced policy plans of the Reagan administration. I think it is fair to say that these plans, as announced, represent a substantial break with past policies. How, then, does Sims justify use of the VAR models? Apparently, his presumption is that the public does not believe that a genuine regime change will actually take place: "Precisely because those vying for control of policy will propose to make permanent changes in the rule much more often than they will succeed in doing so, the public is likely to discount their rhetoric and react to the actual course they set for policy as if it were a disturbance to the existing probabilistic structure" (1982, p. 139). Given this assumption that the public disbelieves in a regime change, there are two possibilities: either the public is correct in its disbelief or it is incorrect. But note that if Sims is assuming the former—that the "proposed paths of policy variables are . . . not attainable"—then he is evaluating the effects of a hypothetical change in

<sup>24</sup> Examples are provided by Friedman (1982), Gordon and King (1982), and Litterman (1982). Friedman does not carry out policy *simulations* but his "two-target" proposal for monetary policy is based in part on an assumption that VAR relationships are policy invariant.



policy under the assumption that there is no change in policy. This, clearly, involves a logical contradiction that negates any conclusion. The other possibility is that the public is incorrect in believing that there is no change in regime. In this case there is no logical contradiction, but the analysis presumes systematically incorrect expectations. To the extent that the public (correctly) believes in the policy change, Sims's predictions will be incorrect. And Sims shows no inclination to assume systematically incorrect expectations as a general matter. Thus his arguments concerning the Reagan plans are unsatisfactory.<sup>25</sup>

My other objection is that the general tone of Sims's discussion seems likely to encourage economists to conceive of policy in terms of isolated actions rather than sustained rules. Such encouragement is,

<sup>25</sup> This is not, of course, an endorsement of these plans.

of course, in direct opposition to the advice of Lucas, Sargent, and other RE advocates. Lucas (1976) (1978) has argued eloquently that economists should focus their attention on sustained rules, in part because understanding their effects is the most that there is any chance of doing well. This position seems to me correct. The profession hardly knows enough about *deterministic* steady states to evaluate their relative merits—consider the difficulties in conceptualizing the costs of anticipated inflation—much less, those of stochastic steady states or alternative sequences of arbitrary policy actions. Furthermore, actual policymakers are strongly inclined to focus attention on today's situation, to the neglect of both future and past. To me it seems undesirable for the economics profession to encourage them in this inclination, as it did during the period of time between the Keynesian and rational expectations revolutions.

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# OF HUME, THORNTON, THE QUANTITY THEORY, AND THE PHILLIPS CURVE

Thomas M. Humphrey

*Having shown that from a static point of view the quantity of money was unimportant, Hume went on to show that from a dynamic point of view changes in the quantity of money could have a very important influence.*

Lionel Robbins

**The Theory of Economic Development  
in the History of Economic Thought**

Many economists no doubt would agree that the strict classical *quantity theory of money* is logically incompatible with the concept of a stable, long-run *Phillips curve tradeoff* between output and inflation. For, according to the quantity theory, equilibrium changes in the money stock alter only prices and have no lasting impact on real variables. By contrast, the Phillips curve hypothesis holds that inflationary money growth can permanently raise the level of real output and employment. How could any economist simultaneously adhere to these two apparently contradictory views?

The foregoing question is central to a current controversy over the contributions of David Hume (1711-1776), the great eighteenth century Scottish philosopher-historian-economist, to monetary theory. Thomas Mayer, for example, argues that Hume implicitly rejected the Phillips curve tradeoff because it was inconsistent with his quantity theory. Thus Mayer states that whereas

The quantity theory was obviously central to Hume's economics. . . . The absence of a long-run tradeoff between unemployment and inflation was also central to Hume because, if such a tradeoff exists, it weakens . . . the quantity theory, since prices then do not rise in proportion to the increase in the quantity of money. [4; pp. 98, 99]

Similarly, Jacob Frenkel cites Hume's belief in the quantity theory's neutrality proposition (according to which a money stock change has no lasting effect on real variables) as evidence of Hume's rejection of the Phillips curve. Says Frenkel:

. . . there is evidence that Hume *did not* believe in a stable, long-run Phillips curve. . . . The overwhelming general tendency of Hume's writings and one of the most important characteristics of his monetary theory has been the proposition concerning the neutrality of money. . . . [which states that] monetary policy exerts no long-run real effects. [2; pp. 490, 492]

Frenkel and Mayer admit, of course, that Hume acknowledged that money could affect output and employment temporarily during a transitional adjustment period. But they contend that he felt that these temporary real effects would eventually vanish, leaving no long-run tradeoff. This view is disputed by Charles Nelson, who claims that Hume did indeed believe in a long-run tradeoff and that he was unique in doing so. Says Nelson,

Hume was clearly of the opinion that the *level* of activity would be raised *permanently* by a steady increase in the quantity of money, prices and wages. Hume was therefore a believer in stable, long-run Phillips curves and perhaps the only serious economist to have so committed himself in print! [5; p. 2. *Italics in original.*]

The purpose of this article is to show that both of the foregoing interpretations are at least partly mistaken: that, contrary to the contention of Frenkel and Mayer, Hume did believe in both the quantity theory and a stable long-run Phillips curve and saw no inconsistency in doing so; that, contrary to Nelson's suggestion, Hume was not alone in accepting the long-run Phillips curve but was joined by Henry Thornton (1760-1815), perhaps the leading monetary theorist of the nineteenth century British classical school; and finally, that neither Hume nor Thornton contended that the real effects of a steady, sustained rate of money growth were confined to a temporary transition period but thought those effects could persist indefinitely. More precisely, the article shows that both Hume and Thornton distinguished between *levels* and *rates of change* of the money stock, that they held the former to be neutral and the latter nonneutral with respect to real economic variables, and that this distinction reconciles their belief in both the quantity theory and the long-run Phillips curve. The article also shows that, although both

Hume and Thornton believed in the existence of a stable long-run Phillips curve, they differed regarding the desirability of exploiting that relationship for policy purposes—Hume favoring and Thornton opposing such a policy. The views of Hume and Thornton are important not only because they demonstrate that at least two leading classical quantity theorists accepted the Phillips curve, but also because they illustrate how opposing policy prescriptions can derive from the same underlying theoretical framework.

### David Hume (1711-1776)

To demonstrate that Hume and Thornton adhered to both the quantity theory and the long-run Phillips curve, it is sufficient to show

- (1) that they stated the quantity theory in terms of its neutrality and equiproportionality propositions, and
- (2) that they contrasted the short-run nonneutrality and the long-run neutrality of changes in money stock *levels* with the long-run nonneutrality of *rates of monetary change*.

That is, it must be shown that they distinguished between neutral alternative levels and nonneutral rates of change of the money stock and that they employed this distinction to reconcile their belief in the quantity theory with their belief in the Phillips curve.

First consider Hume, whose contributions to monetary theory are contained in his famous essays "Of Money," "Of Interest," and "Of the Balance of Trade," all originally published in 1752. He starts out by enunciating the quantity theory's equiproportionality and neutrality propositions according to which an equilibrium rise in the quantity of money causes an equiproportional rise in nominal prices and leaves all real variables unchanged. He says:

If we consider any one kingdom by itself, it is evident, that the greater or less plenty of money is of no consequence; since the prices of commodities are always proportioned to the plenty of money, and a crown in HARRY VII's time served the same purpose as a pound does at present. . . . Where coin is in greater plenty; as a greater quantity of it is required to represent the same quantity of goods; it can have no effect, either good or bad, taking a nation within itself; any more than it would make an alteration on a merchant's books, if, instead of the ARABIAN method of notation, which requires few characters, he should make use of the ROMAN, which requires a great many. [8; pp. 33, 37]

Hume realized that these comparative static propositions apply only to equilibrium levels of the money stock after all adjustments have occurred. In Eugene Rotwein's words, Hume "distinguishes between the ultimate effect of a higher absolute quantity of money as such and the effect of the process of change to a larger quantity of money. It is to the former alone that the quantity theory applies." [8; p. lxiii]

Hume notes, however, that during the adjustment from the old to the new equilibrium level, money can temporarily affect real variables. He attributes those real effects chiefly to the existence of two delays or time-lags in the adjustment process. The first is the lag of prices behind money, which, by raising the actual quantity of money relative to the amount required to purchase given output at existing prices, generates a rise in aggregate real demand as people attempt to get rid of the excess money by spending it on real goods and services. The second is the lag of money wages behind prices, which, by lowering real wages, leads to an increase in hiring and hence production. In Hume's view the first lag produces the increased real demand that justifies output expansion whereas the second lag produces the cost conditions that make that expansion profitable. Reinforcing the effect of these lags are several supplementary sources of nonneutrality, including (1) the nonneutral initial distribution of the new money, (2) the fact that the monetary injection and the corresponding new equilibrium price level are at first unperceived, and (3) shifts in the structure of relative prices owing to the failure of all product prices to adjust with equal speed to generalized inflationary pressure. Hume elaborates:

To account, then, for this phenomenon, we must consider, that though the high price of commodities be a necessary consequence of the encrease of gold and silver, yet it follows not immediately upon that encrease; but some time is required before the money circulates through the whole state, and makes its effect be felt on all ranks of people. At first, no alteration is perceived; by degrees the price rises, first of one commodity, then of another; till the whole at last reaches a just proportion with the new quantity of specie which is in the kingdom. In my opinion, it is only in this interval or intermediate situation, between the acquisition of money and rise of prices, that the encreasing quantity of gold and silver is favourable to industry.

Here are the temporary effects of (1) the lag in prices behind money, (2) the perception lag, and (3) the transitory shift in relative prices.

Hume continues, emphasizing the impact of the unequal initial distribution of the new money and especially the employment and output effects of the lag in money wages.

When any quantity of money is imported into a nation, it is not at first dispersed into many hands, but is confined to the coffers of a few persons, who immediately seek to employ it to advantage. Here are a set of manufacturers or merchants, we shall suppose, who have received returns of gold and silver for goods which they sent to CADIZ. They are thereby enabled to employ more workmen than formerly, who never dream of demanding higher wages, but are glad of employment from such good paymasters. If workmen become scarce, the manufacturer gives higher wages, but at first requires an increase of labour, and this is willingly submitted to by the artisan, who can now eat and drink better, to compensate his additional toil and fatigue. He carries his money to market, where he finds every thing at the same price as formerly, but returns with greater quantity and of better kinds, for the use of his family. The farmer and gardener, finding, that all their commodities are taken off, apply themselves with alacrity to the raising more; and at the same time can afford to take better and more cloths from their tradesmen, whose price is the same as formerly, and their industry only whetted by so much new gain. It is easy to trace the money in its progress through the whole commonwealth; where we shall find, that it must first quicken the diligence of every individual, before it encrease the price of labour. [3; pp. 37-38]

But these real effects are temporary and vanish once wages and prices fully adjust to the new higher level of the money stock. As Hume himself put it,

... it is only in this interval or intermediate situation, between the acquisition of money and rise of prices, that the encreasing quantity of gold and silver is favourable to industry. . . . but after the prices are settled, suitably to the new abundance of gold and silver, it has no manner of influence. [3; pp. 38, 48]

In short, a one-time increase in the level of the money stock has no permanent, lasting influence on real activity. That is, a one-time rise in the quantity of money generates only a temporary tradeoff.

### Long-Run Tradeoff

According to Hume, however, the same is not true of a steady succession of such monetary increases. He thought such increases would, if maintained over a continuous series of transitional adjustment periods, exert permanent real effects. That is, he asserted the real significance of a sustained monetary expansion, thereby prompting Adam Smith's remark that "Mr. Hume's reasoning is exceedingly ingenious. He seems, however, to have gone a little into the notion that public opulence consists in money." [9; p. 197 quoted in 7; p. 136] Implicitly assuming that expectations of future inflation would always be zero and therefore would never enter wage and price demands, Hume claimed that a continually expanding money stock would forever march ahead of wages and prices, perpetually frustrating their

attempts to catch up with it.<sup>1</sup> As a result, real cash balances would rise (thus stimulating spending) and real wages would fall (thus stimulating employment). These stimuli would persist indefinitely, allowing a permanent expansion in the level of real activity.

To illustrate how a sustained increase in the quantity of money can permanently raise activity, Hume refers to the observed real growth performance of the nations of Western Europe since the opening of the gold mines in America in the sixteenth century.

... it is certain [he says] that, since the discovery of the mines in AMERICA, industry has encreased in all the nations of EUROPE . . . and this may justly be ascribed, amongst other reasons, to the encrease of gold and silver. Accordingly we find, that, in every kingdom, into which money begins to flow in greater abundance than formerly, every thing takes a new face: labour and industry gain life; the merchant becomes more enterprising, the manufacturer more diligent and skilful, and even the farmer follows his plough with greater alacrity and attention. [3; p. 37]

Note that Hume is here referring to roughly 200 years of monetary expansion. This expansion, he says, has permanently raised the level of real activity. For Hume it is the rate of change of money and not its quantity that matters for real variables—a point he underscores in two additional passages. In the first passage he contrasts the economic performance of two hypothetical nations, the money stocks of which, though identical in size, are changing at different rates. He says, "A nation, whose money decreases, is actually, at that time, weaker and more miserable than another nation, who possesses no more money but is on the encreasing hand." [3; p. 40] That is, whether real activity is permanently raised or lowered depends upon whether the rate of

<sup>1</sup> Hume's omission of inflationary expectations may be explained on at least three grounds. First, he was describing a world metallic inflation rate quite low (1 to 3 percent per year on average) by modern standards, perhaps too low to reach the minimum perception threshold required for the generation of inflation expectations. Second, given a metallic monetary standard, one could argue on profitability grounds that the expected long-term inflation rate is zero. The reason, of course, is that if the stock of monetary metal were initially expanding at an inflationary pace so as to raise the metal price of goods and labor, the resulting fall in the purchasing power of metal combined with the rising labor cost of mining it would induce mineowners to cut back metallic production to noninflationary levels. In other words, the inflationary overproduction of gold would, by lowering its value relative to other goods, render the latter more profitable to produce than gold, thereby automatically checking gold's overproduction. Reinforcing this price-stabilizing production effect would be a shift in the demand for gold from monetary to nonmonetary uses as gold's value as money declines. Third, the discovery of gold and silver mines in the New World could be regarded as random, fortuitous events having an expected value of approximately zero. For these reasons, Hume's implicit assumption of zero expected inflation is perhaps understandable.

monetary change is positive or negative. That rate (and of course the wage/price lag associated with it) is "as pernicious to industry, when gold and silver are diminishing, as it is advantageous when these metals are encreasing." Specifically, in the deflationary case of negative money growth, "The workman has not the same employment from the manufacturer and merchant; though he pays the same price for everything in the market. The farmer cannot dispose of his corn and cattle; though he must pay the same rent to his landlord. The poverty, and beggary, and sloth, which must ensue are easily foreseen." [3; p. 40] Here is Hume's stress on the real significance and insignificance, respectively, of rates of change versus absolute quantities of money. This emphasis is also manifest in the second passage, in which he concludes

... that it is of no matter of consequence, with regard to the domestic happiness of a state, whether money be in a greater or less quantity. The good policy of the magistrate consists only in keeping it, if possible, still encreasing; because, by that means, he keeps alive a spirit of industry in the nation, and encreases the stock of labour, in which consists all real power and riches. [3; pp. 39-40]

Regarding this passage, Blaug notes that Hume's "demand for a continual inflow of precious metals amounts to a demand for a continuous series of transitional periods" during which inflationary money growth repeatedly and permanently stimulates trade. [1; p. 20] Here is Hume's adherence to the long-run Phillips curve. Here also is his reconciliation of that concept with his quantity theory. There is no conflict between the two theories, he thought, since the one refers to rates of change and the other to alternative levels of the money stock.

### Henry Thornton (1760-1815)

The preceding has argued that, in Nelson's words,

Hume was careful to distinguish between the effect of alternative *levels* of the quantity of money and the effect of a *changing* quantity of money. He clearly regarded the level to be of little or no real consequence. . . . Hume regarded only the rate of change of money as having real significance, but of such significance as to allow a long-run trade-off between output and inflation. There is nothing in Hume's analysis of monetary dynamics which implies that the trade-off cannot be sustained . . . [6; pp. 494-495]

This same description applies to Henry Thornton, whose analysis of the quantity theory and the output/inflation tradeoff are contained in his classic *An Enquiry into the Nature and Effects of the Paper Credit of Great Britain* (1802). Like Hume, he defines the tradeoff in terms that are at once succinct and unambiguous:

. . . additional industry will be one effect of an extraordinary emission of paper, a rise in the cost of articles will be another.

Probably no small part of that industry which is excited by new paper is produced through the very means of the enhancement of the cost of commodities. [10; p. 237]

Also like Hume, he distinguished between levels and rates of change of the money stock, holding that only the latter can affect real activity and sustain the tradeoff. This is epitomized in his remark that "it is the progressive augmentation of bank paper, and not the magnitude of its existing amount, which gives the relief." [10; p. 256] In other words, money stimulates activity only when it is continually increasing. For, says Thornton, "While paper is encreasing, and articles continue rising, mercantile speculations appear more than ordinarily profitable." But "as soon, . . . as the circulating medium ceases to encrease, the extra profit is at an end," and the stimulus vanishes. [10; p. 238] Thus a one-time rise in the money stock cannot sustain the tradeoff. Instead, a continuous increase or "progressive augmentation" is required. Here is the long-run non-neutrality of rates of change of the money stock that underlies Thornton's version of the Phillips curve tradeoff.

As for the tradeoff itself, Thornton, like Hume, attributed it chiefly to a tendency for money wages to consistently lag behind prices. He explicitly stated (1) that inflation stimulates activity, (2) that it does so by reducing real wages and raising real profits, (3) that this output-enhancing redistribution occurs because money wages lag behind prices, and (4) that this wage lag persists as long as inflation is sustained. Neither he nor Hume explained why the lag would persist nor did they allow for the formation of inflationary expectations. Because of this they did not attempt to explain why wages would not eventually catch up with prices once inflationary expectations had fully adjusted to actual inflation. In short, their analysis is largely silent about price anticipations; they did not incorporate inflationary expectations into the Phillips curve.<sup>2</sup>

<sup>2</sup> Thornton's omission of price expectations in his analysis of the labor market is hard to rationalize. After all, he explicitly recognized the role of exchange rate expectations in his discussion of the determination of foreign exchange rates. Moreover, he spelled out a fairly complete theory of the inflation-generating process in an inconvertible paper currency regime. A logical next step would have been to explain how people form expectations of future inflation consistent with that inflation-generating mechanism. Perhaps his omission of price expectations reflected his belief that Britain would return to the gold standard at pre-Napoleonic war prices such that the long-run expected rate of inflation was zero.

## Forced Saving

Thornton did, however, introduce one new element into the analysis, namely the celebrated *forced-saving doctrine*, which later played a leading role in the Austrian business cycle theory of von Mises and Hayek. As enunciated by Thornton, this doctrine refers to the potential rise in the rate of capital accumulation and hence long-term economic growth owing to the inflation-induced redistribution from wages to profit. This forced saving effect, which assumes for its existence that capitalists have a higher propensity to save and invest out of real income than do workers, is described by Thornton as follows:

It must be also admitted, that, provided we assume an excessive issue of paper to lift up, as it may for a time, the cost of goods though not the price of labour, some augmentation of [the capital] stock will be the consequence; for the labourer, according to this supposition, may be forced by his necessity to consume fewer articles, though he may exercise the same industry. [10; p. 239]

He then goes on to allude to the possibility of "a similar defalcation of the revenue of the unproductive members of the society," i.e., fixed-income recipients. [10; p. 239] In suggesting that the rate of monetary growth may influence the rate of capital accumulation, Thornton anticipated a key feature of those modern neoclassical monetary growth models that treat investment as a function of the monetary growth rate.

Finally, like Hume, Thornton also accepted the quantity theory which he reconciled with the Phillips curve by distinguishing between alternative levels and rates of change of the money stock. He states the quantity theory's neutrality and proportionality propositions as follows: Money, he says,

... is an article of such a nature ... that the vast encrease of it ... cannot possibly create such a new capital as shall furnish the new paper with employment. There remains, therefore, no other mode of accounting for the uses to which the additional supply of it can be turned, than that of supposing it to be occupied in carrying on the sales of the same, or nearly the same, quantity of articles as before, at an advanced price the cost of goods being made to bear the same, or nearly the same, proportion to their former cost, which the total quantity of paper at the one period bears to the total quantity at the other. [10; p. 241]

## Exploiting the Phillips Curve

The preceding has argued that both Hume and Thornton accepted the Phillips curve and that they reconciled it with their belief in the quantity theory by distinguishing between the nonneutrality of rates of monetary change and the neutrality of alternative levels of the money stock. In these respects at least, they were in perfect agreement.

They differed, however, over the desirability of exploiting the Phillips curve for policy purposes. Hume clearly believed that the policy authorities in the closed world economy should exploit the curve, using monetary gold inflation to stimulate output.<sup>3</sup> He says as much in his advice to the policymaker.

The good policy of the magistrate consists only in keeping it, if possible, still encreasing; because, by that means, he keeps alive a spirit of industry in the nation, and encreases the stock of labour, in which consists all real power and riches. [3; pp. 39-40]

In contrast to Hume, Thornton was much less enthusiastic about the desirability of exploiting the Phillips curve. Such exploitation involved inflation, which he saw as an unmitigated evil.<sup>4</sup> All inflationary policy, he said, is "attended with a proportionate hardship and injustice." [10; p. 239] Most important, he doubted that the output gains would be large enough to be worth the costs (uncertainty, injustice, social discontent) of higher inflation. In this connection he repeatedly stressed (1) that the economy normally tends to operate close to its full capacity constraint, (2) that the tradeoff is extremely unfavorable at this normal operating point, allowing at best only small increases in output per unit increase in inflation, and (3) that the tradeoff vanishes altogether at full employment. As a result, stimulative policy would almost immediately confront the full employment barrier where

... it is obvious, that the antecedently idle persons to whom we may suppose the new capital to give employ, are limited in number; and that, therefore, if the encreased issue is indefinite, it will set to work labourers, of whom a part will be drawn from other, and, perhaps, no less useful occupations. [10; p. 236]

<sup>3</sup> Note that Hume refers to the benefits of *metallic* inflation only. He strongly disapproved of paper money inflation which he believed would be excessive. This was on the grounds that there existed no natural forces to limit the rate of overissue of paper. "I scarcely know," he says, "any method of sinking money below its level, but those institutions of ... paper credit which are so much practiced in this kingdom." [3; pp. 67-68] By contrast, he felt that the rate of metallic inflation would be severely limited by the difficulty and expense of discovering precious metals and extracting them from the earth. For this reason, he believed that the metallic inflation rate would be low, corresponding to the slow, steady accretion of the world stock of monetary gold and silver. On this basis, he concluded that provided inflation was of the metallic kind, higher levels of real activity could be obtained at the cost of only moderate inflation.

<sup>4</sup> Thornton feared inflation more than did Hume because, at the time he was writing (1802), Britain was operating with an inconvertible paper currency and a ceiling bank loan rate of interest pegged below the prevailing profit rate on new capital investment. Such conditions, Thornton noted, created the potential for unlimited hyperinflation. Indeed, he pointed out that France had experienced such hyperinflation during the infamous Assignats episode.

In short, while it is true that "paper possesses the faculty of enlarging the quantity of commodities by giving life to some new industry," the full employment constraint ensures that "the encrease of industry will by no means keep pace with the augmentation of paper." [10; p. 239] On these grounds he concluded that there exist narrow "bounds to the benefit which is to be derived from an augmentation of paper; and, also, that a liberal, or, at most, a large encrease of it, will have all the advantageous effects of the most extravagant emission." [10; p. 236]

### Concluding Comments

This article has documented the following conclusions:

1. Contrary to Frenkel and Mayer, Hume accepted both the quantity theory and the long-run Phillips curve and saw no inconsistency in doing so.
2. Contrary to Nelson, Hume was not alone in accepting the Phillips curve but was joined by Henry Thornton.
3. Both Hume and Thornton reconciled the quantity theory with the Phillips curve by distinguishing between the neutrality of alternative levels and the nonneutrality of rates of change of the money stock.
4. Hume and Thornton differed over the desirability of exploiting the Phillips curve for policy purposes—Hume favoring and Thornton opposing its exploitation. Hume's preference, however, extended only to metallic inflation.
5. Neither Hume nor Thornton advocated paper money inflation. Both feared that such inflation could rapidly accelerate into hyperinflation since no natural forces existed to limit the over-issue of inconvertible paper.

Hume and Thornton attributed the existence of the Phillips curve to a lag of money wages behind prices. Neither, however, attempted to explain why the wage lag persists. Nor did they allow for the formation of price expectations or for the incorporation of those expectations into the Phillips curve. Instead, they implicitly assumed that the anticipated rate of inflation was always zero, thereby omitting a crucial feature of modern Phillips curve analysis, namely the assumption that expectations are formed consistent with the way actual inflation is generated so that those expectations are correct on average. Because of this omission, they could hardly be expected to explain how changes in inflationary expectations

alter the slope of the Phillips curve, rendering it vertical at the natural rate of unemployment. In these respects at least they differed from modern monetarist critics of the Phillips curve.

It should be noted, however, that Thornton's policy analysis was very much in the spirit of these critics. Like them, he did not believe in exploiting the Phillips curve for policy purposes. Like them, he stressed the costs rather than the benefits of inflation. And, though he did not think the curve was vertical at the economy's normal or standard level of output, he did think it was very steeply sloped, allowing little increase in output per unit rise in inflation. For these reasons, although he enunciated the concept of the long-run Phillips curve, he cannot be considered an enthusiastic proponent of it. Similarly, although Hume welcomed gradual metallic inflation, he was unalterably opposed to the kind of rapid paper money inflation that is unfortunately so common today. Thus, were both alive today, they undoubtedly would warn against using over-expansionist paper money policy to stimulate output and employment.

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