

# Marginal Utility Equilibrium between Money and Goods: A Reply to Professor Barnett's Criticism

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**W**illiam Barnett has offered what he purports to be a criticism to an appendical note that accompanied my article on the marginal utility and value of money in the previous volume of *The Review of Austrian Economics*.<sup>1</sup> Barnett's conclusion to the first section of his comment is that the equilibrium condition between marginal utilities of goods and money relative to their prices is a "legitimate neoclassical formulation *provided that . . . the nominal price of money, i.e., the price of money in terms of itself, [is] to wit: one (1)*. This condition is essential if the budget constraint is to be consistent." (Emphasis added. I presume that Barnett means by "consistent" "not violated.") He then remarks that my use of the price of money as the reciprocal of the price of the composite good ( $1/P(c)$ ) is "improper" and an "error."

The fact of the matter is that Barnett's "correction" of my "error" is his reformulation and not an error of mine at all. My economy has in it commodities ( $R$ ) and their prices ( $P_R$ ), which specify a monetary value of real goods ( $P_r \cdot R$ ). It also has in it a quantity of nominal money units ( $M$ ) and a real price for those units of money ( $pm$ ). The physical goods composing ( $R$ ) have real value, and so does the nominal money ( $M$ ) that exchanges them. If an exchange takes place between money and goods, the exchange itself is witness to the fact that the elements in the exchange are part of an equality. Thus, the price in terms of money that one pays for goods must be equal to the price in goods for which the other party to the exchange pays in money.

My inclusion of real money, as the theorists say, "in the budget constraint," was no error. Money in my theory is just as real as goods. If it were not, I would discard it with the rest of the waste paper.

Barnett asserts that the "price of money in terms of itself [is] one (1)," and, again, that money is "dimensionless." This contention for a generalized analysis of money exchanging for all goods in all markets is ridiculous. My question is: one WHAT?

Let me assert that I *want* real money in my utility function because nominal money by itself is meaningless. The modest analysis I made in my appendical note was to show how changes in nominal money accompanied by corresponding changes in money prices would lead to an adjustment in the marginal utility schedule for money. Within this changing framework, the real stocks of money and goods stayed constant, so the budget constraint was not violated. In Barnett's model, money has no real value, so his equation may be "consistent" in the way he has set it up, but it is also meaningless. Since when does a money-using economy have no real money in it, except during the final gasp of a hyperinflation?

Barnett's "correct" mathematics but flawed economics appears midway through his comment. He states that my budget constraint has the following dimension (I use his notation with brackets added for clarification):

$$Y = [P_c \cdot c] + [P_m \cdot M],$$

$$\text{\$} = [\$ / c \cdot c] + [c / \$ \cdot \$], \text{ or}$$

$$\text{\$} = \text{\$} + c.$$

Since  $\text{\$}$  cannot equal  $\text{\$} + C$ , "my" budget constraint is inconsistent.

Barnett's error here is that he has not included the services of real money-wealth in the original budget constraint. His  $Y$  is the real income of the composite good *without money*. Real money, it is true, is a real capital stock—wealth; in an "income model," this wealth must be converted into an income flow. In my grammatical model, I had no trouble making such an inclusion. In fact, I discussed this matter at length in my original article, which appeared in volume 1 of *The Review of Austrian Economics*.

Barnett concludes his criticism of my squared relationship with this statement:

In this latter form, Professor Timberlake's error and its source are clear. The rate [price] at which money and the composite good may be exchanged is given by the money price of the composite good, not by its square. It was the improper substitution of "the" real price of money (the reciprocal of the price of the composite good) for the nominal price of money in the budget constraint that caused the squared rule.

Barnett's observation in the next-to-the-last quoted sentence is a *reductio ad absurdum*. The price of goods in terms of money, believe it or not, is given by the money price of the composite good (!) "and not by its square." My "squared" conclusion for the marginal utility of money, however, does not argue that the *price* of money in terms of goods is squared. It only argues that the *utility* of the last unit of nominal money held in equilibrium will decline as the *square* of the increase in prices, due to the fact that the quantity of money

units has proportionately increased *and* that the marginal utility *schedule* of all money units has likewise fallen by this same magnitude. (The graphical expression of this change was given in the figure in my appendix.)

In his eagerness to frame my analysis in neoclassical mathematics, Barnett has violated the conditions I set forth in my model. I had economic man in equilibrium with money and goods. Then I promoted a formalized inflation by means of a specified increase in the quantity of nominal money units—the rate of inflation being in proportion to the increase in money. Throughout my analysis, the real quantity of money and its total utility remained statically constant. I therefore aimed at getting economic man into equilibrium with the nominal money units extant, but within the environmental framework of a constant real stock of money.

For Barnett to state that my “substitution of the ‘real’ price of money . . . for the nominal price of money” was “improper” is incorrect. It may be “improper” if one is mired down in mathematical assumptions, but it is not improper in the real world, and it most certainly is not a “substitution” in my model. Rather, it is a feature I want in the model. Nor does this inclusion violate any budget constraint. My model starts and ends with the same real quantities of money and goods. Why, indeed, would anyone hold any money if the money were only “nominal” as Barnett suggests? My analysis puts real goods and real money into the behavior function of economic man. When an economist states, as Barnett does, that the price of money is “one,” or that money is a “mere numeraire” (a common observation), he has abandoned the analysis of money altogether or he has never begun. If holdings of real goods are being analyzed in terms of money, the money must be real as well as nominal.

In the interest of brevity, and because Barnett’s criticism of my marginal utility analysis is trivial and confused, I do not treat this latter part of his comment.

## Note

1. “Reply to Murray N. Rothbard,” *The Review of Austrian Economics*, vol. 2 (1988), pp. 194–97.