The Impact of Fiscal Policy and Inflation on National Saving: A Reply

Four years after the publication of our article on fiscal policy and national saving in Italy on this *Review*, Prof. Rossi gives us the opportunity to return on the issues that we had raised in that contribution. Given the recent surge in the deficit figures, Prof. Rossi's acceptance of the proposition that higher private saving offsets public dissaving, leaving national saving unaffected, would be welcome news to our fellow-citizens. It is therefore with some regret that we have to confirm our previous finding that deficits have indeed reduced national saving in the last two decades.

In this reply, we make essentially two points: first, contrary to his assertions, Prof. Rossi's evidence is inconsistent with the Ricardian Equivalence Proposition (REP); second, this failure becomes even more apparent if one relies on the updated formulation and improved data underlying the more recent contribution of Modigliani and Jappelli (1987).[1]

1. On the basis of his equation (6), Prof. Rossi claims that "the evidence turns out to support, by and large, the Ricardian Equivalence Proposition" (p. 230). This conclusion seems unwarranted if one looks at the coefficients and standard error estimates of equation (6), and particularly at the coefficient of public debt. The latter should be significantly negative under the REP and is instead positive, large and significantly different from zero in equation (6). In fact, Prof. Rossi himself acknowledges that this result is inconsistent with the debt neutrality proposition.[2] He also acknowledges that a formal F-test rejects the constraints implied by

[1] This failure of the debt neutrality hypothesis on aggregate data accords with other recent work, where, using both microeconomic and aggregate data, we show that in Italy liquidity constraints are pervasive and have a large effect on consumption (JAPPELLI and PAGANO 1988, 1989). It is fair to say that liquidity constraints make the REP implausible, if not totally untenable: even if in some models liquidity constraints can coexist with the REP, BERNHEIM (1987) shows that this result crucially hinges on highly restrictive and implausible assumptions about the distribution of the future tax burden (p. 270).

[2] The excuse suggested by him for this failure, namely that income includes property income, is nothing but a plain *non sequitur*. The inclusion of property income might conceivably explain the low coefficient of wealth, but obviously could never account for the high positive coefficient of government debt.

the REP at the 5% standard and fails to do so only if one accepts the 1% standard. However, if the same restrictions are tested using a likelihood ratio test (that is asymptotically equivalent to the F-test), one finds that they are rejected even at the 1% standard. We can conclude therefore that, even employing the same data used by Prof. Rossi and accepting his partial adjustment model, the evidence hardly provides any support for the REP.

2. Prof. Rossi has based his criticism of our 1985 article on the proposition that the restrictions we have imposed on our model by using the AR1 correction are inconsistent with the data. In his opinion, the misspecification arises from the fact that we have failed to model the dynamic behavior of consumption according to his "good old" partial adjustment mechanism, that implies a very slow adjustment of consumption to shocks in the exogenous variables. We hold instead that the appropriate specification, consistent with the spirit of the Life-Cycle Hypothesis, is to suppose that consumption adjusts rapidly to changes in permanent income.[3] In our view, the misspecification in our 1985 article was in the modeling of permanent income.

We tried to improve on this and other shortcomings of our 1985 article in a subsequent contribution (Modigliani and Jappelli, 1987), that Prof. Rossi has simply chosen to ignore. Realizing that current income is an unsatisfactory proxy for the effect of permanent income on consumption, we included lagged disposable income among the regressors. At the same time, we revised and updated the data set. The new data set differs from the old one in two main respects, since it relies on:

(i) an improved series on public debt, published by Spaventa *et al.* (unavailable at the time of our 1985 article);[4]

(ii) a rolling forecast rather than actual inflation to proxy for expected inflation and to compute the expected real interest component of income.

We will now show that the estimates using the revised formulation and data are immune to Prof. Rossi's criticisms and unambiguously reject the REP. Relying on our revised approximation to permanent income and the AR1 specification, and using the revised data set spanning the 1950-85 period, we obtain the following results (t-statistics in parenthesis):

[3] This is certainly the view expressed in the voluminous modern literature on the subject, beginning with the classic paper by HALL (1978). Recent surveys are to be found in HALL (1987) and ABEL (1988).

[4] This has pervasive effects in the estimation, since it affects the time series behavior of all the regressors (via the real interest income component).

$$\underset{(4.7)}{0.047} + \underset{(10.9)}{0.48}\,YD + \underset{(3.2)}{0.17}\,YD_{-1} - \underset{(-2.0)}{0.17}\,DEF - \underset{(-1.7)}{0.15}\,DEF_{-1} + \underset{(3.2)}{0.022}\,W + \underset{(4.4)}{0.10}\,D \tag{1}$$

$$Rho = \underset{(4.16)}{0.57} \quad SE = 0.0063 \quad \text{Sample: 1950-85}$$

where C is consumption, YD is (inflation adjusted) disposable income, DEF is (inflation adjusted) government deficit, W is wealth inclusive of government debt and D is government debt. When we test the AR1 specification against a general first order version of (1) we find that there are no grounds for rejecting it: the likelihood ratio test statistic is 4.6, well below even the 5% value of a χ^2 with 4 degrees of freedom, namely 9.49. This result holds also if the sample period is restricted to be the same as that used in our 1985 article, *i.e.* 1952-82.[5]

The implications of the REP in this specification are twofolds:

(i) debt should not matter: since debt is included in wealth, its coefficient should be the opposite of that of wealth;

(ii) taxes should not matter: the sum of the coefficients of the deficit variables should be the opposite of the sum of the coefficients of the income variables.

It is easy to check that the restrictions implied by the REP are soundly rejected by the data even at the 1% level of significance. In order to anticipate possible objections by Prof. Rossi, we report that even his partial adjustment model soundly rejects the REP hypothesis when one replaces the obsolete data of our 1985 article with the revised data of our 1987 contribution. This is shown in equation (2), which replicates Prof. Rossi's equation (6) on the new data:

$$\underset{(5.2)}{0.025} + \underset{(11.7)}{0.40}\,YD - \underset{(-3.0)}{0.27}\,DEF + \underset{(0.2)}{0.001}\,W + \underset{(6.3)}{0.08}\,D + \underset{(6.8)}{0.44}\,C_{-1} \tag{2}$$

$$SE = 0.0055 \quad \text{Sample: 1950-85}$$

[5] Using this specification on the old data set, the AR1 correction is not rejected at the 1% level, although it is rejected at the 5% level.

This equation is not very different from (1) and, not surprisingly, it implies that the restrictions imposed by the REP are rejected again, and in this case even at the 1% level of significance.

As we have repeatedly stressed in our previous work, regressions such as (1) and (2) are likely to yield inconsistent estimates if one does not control for the endogeneity of income and the deficit (one important reason being the anticyclical nature of the deficit). An obvious problem in this respect is to find the appropriate set of instruments to purge the endogeneity bias: upon performing a series of Hausman (1978) specification tests, we have selected a wide set of instruments to estimate the following regression by an instrumental variables procedure.[6]

$$\begin{aligned}(3)\ C = {} & \underset{(4.6)}{0.051} + \underset{(9.1)}{0.49}\ YD + \underset{(2.4)}{0.15}\ YD_{-1} - \underset{(-0.2)}{0.02}\ DEF + \\ & - \underset{(1.2)}{0.12}\ DEF_{-1} + \underset{(3.4)}{0.025}\ W + \underset{(3.7)}{0.09}\ D\end{aligned}$$

$$\underset{(4.15)}{Rho = 0.57}\quad SE = 0.0067\quad \text{Sample: } 1950\text{-}85$$

It is again found that the AR1 specification is not rejected by the data. If nonetheless we reestimate the partial adjustment specification proposed by Prof. Rossi, using the same set of instruments as in equation (3), we find:

$$\begin{aligned}(4)\ C + {} & \underset{(5.2)}{0.029} + \underset{(10.4)}{0.41}\ YD - \underset{(-2.0)}{0.19}\ DEF + \underset{(0.4)}{0.002}\ W + \\ & + \underset{(6.2)}{0.08}\ D + \underset{(5.5)}{0.41}\ C_{-1}\end{aligned}$$

$$SE = 0.0057\quad \text{Sample: } 1950\text{-}85$$

In both equations the coefficients of the deficit are somewhat lower than in the corresponding equations without instrumental variables, which is consistent with the hypothesis of a downward simultaneity bias reflecting the cyclical behavior of consumption and the deficit. Upon correcting for this bias, the estimates are even less favorable to the REP than in equations

[6] The instruments chosen were three lags on consumption, income and the deficit, two lags on wealth and debt, one lag on taxes (net of interest transfers) and government expenditure, expected inflation, population and a time trend.

(1) and (2) above, as is clear from comparing the coefficients of the income and the deficit variables.

3. To summarize his main point, Prof. Rossi claims that his note should be regarded as nothing less but "a warning against the danger of testing microhypotheses and consequently deriving policy prescriptions on the ground of an incorrect application of econometric tools to (rather uninformative) aggregate data" (p. 230). This sounds like a rather confusing and confused statement, halfway between a trivial *caveat* about aggregation problems in performing tests on aggregate data, a positive statement about the uninformativeness of Italian data on the issue of debt neutrality and a serious methodological accusation of gross incompetence on our part.

On the *caveat* about aggregation problems, it is enough to remark that it has simply nothing to do with anything else in Prof. Rossi's note, except perhaps to fulfill an urge to warn innocent readers of such ever-present danger. On the more mundane level of what Italian data have to say on the issues at hand, we hope to leave the reader convinced that, far from being "rather uninformative", the data provide very strong evidence against debt neutrality, even when the specification proposed by Prof. Rossi is implemented.[7] Finally, on the accusations levelled against us for "incorrect application of econometric tools" and "careless use of the [econometric] methodology", we suggest that before indicting us for such faults Prof. Rossi should at least have read the 1987 paper and checked whether his accusations and conclusions would stand when applied to that reformulation and revised data. We suggest that if he had done so, he would have found that his criticism had been made obsolete by that revision, and would have spared himself the trouble of writing his comment.

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[7] It should be noticed that even these data will possibly become obsolete rather soon, as ISTAT proceeds further in the reconstruction of Italian national accounts that is currently under way. At the moment, the data points already available from the revised national accounts are too few to draw any statistical inference of interest.

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