Comments on Bayoumi, Laxton and Pesenti by Patrick Minford*

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This paper sets out the IMF's GEM Model and draws out some key implications for the supply-side in Europe and also for macroeconomic policy stabilization. The Model can be described as 'stretched (second generation) workhorse' (following the terminaology of Canzoneri, Cumby and Diba) rather like one used to describe the final evolution of first generation models as 'stretched Mundell-Fleming'. The 'workhorse model' of Corsetti and Pesenti has therefore here been added to in a variety of ways: on the macro dynamics side mainly with habit persistence and adjustment costs- and on the imperfect-competition supply side mainly with capital and a capital-goods sector.

The work is a tour de force and I would like to congratulate the authors on this massive effort. My comments are intended to be constructive as I very much like the intention and much of the architecture of the model, besides agreeing with the thrust of their supply-side conclusions. It is true that supply-side problems might well be addressed using more elaborate static Computable General Equilibrium models (with sectoral product distinctions as in Heckscher-Ohlin); however I would argue, along with these authors, that the essential 'natural rate' problem is readily and more parsimoniously addressed via a macro-model approach such as theirs.

The authors make three claims in this paper about the effects of greater competition in Europe. The first two relate to the supply-side, that is the long-run equilibrium of the model for real variables:

1) It will raise Europe's output and employment

2) By worsening Europe's terms of trade it will also raise that of the Rest of the World.

The third claim relates to the short-run macro dynamic properties of the model:

3) It will make the European model more easily stabilized by demand policy (they summarise this as 'reducing the sacrifice ratio').

The supply-side claims:

Let me begin with the two long-run claims.

The way that GEM works in the long run is as follows. Since there are constant returns to scale and capital adjusts in the long run to whatever is required we can write the home production function as $Q_H = L_H \bullet f$(productivity, relative cost of capital goods). Holding constant productivity and the cost of capital, the relationships on the following two slides then hold where I have used fairly obvious notation:

IT'S ALL IN]

IN L/R K_H ADJUSTS SO WITH CRTS $Q_H = L_H$ (PROD, COST OF K)

$$L_H^S = \left(\frac{W_H}{\pi_H}\right)^{]^{-1}}$$ SUPPLYING FN.

$$W_H = (1+\lambda_H) V_H$$

$$\pi_H = \left(P_H^{\beta} P_F^{1-\beta}\right) = P_H \left(\frac{1}{e}\right)^{1-\beta} \qquad e = \frac{P_F}{P_H} = \text{TOT}$$

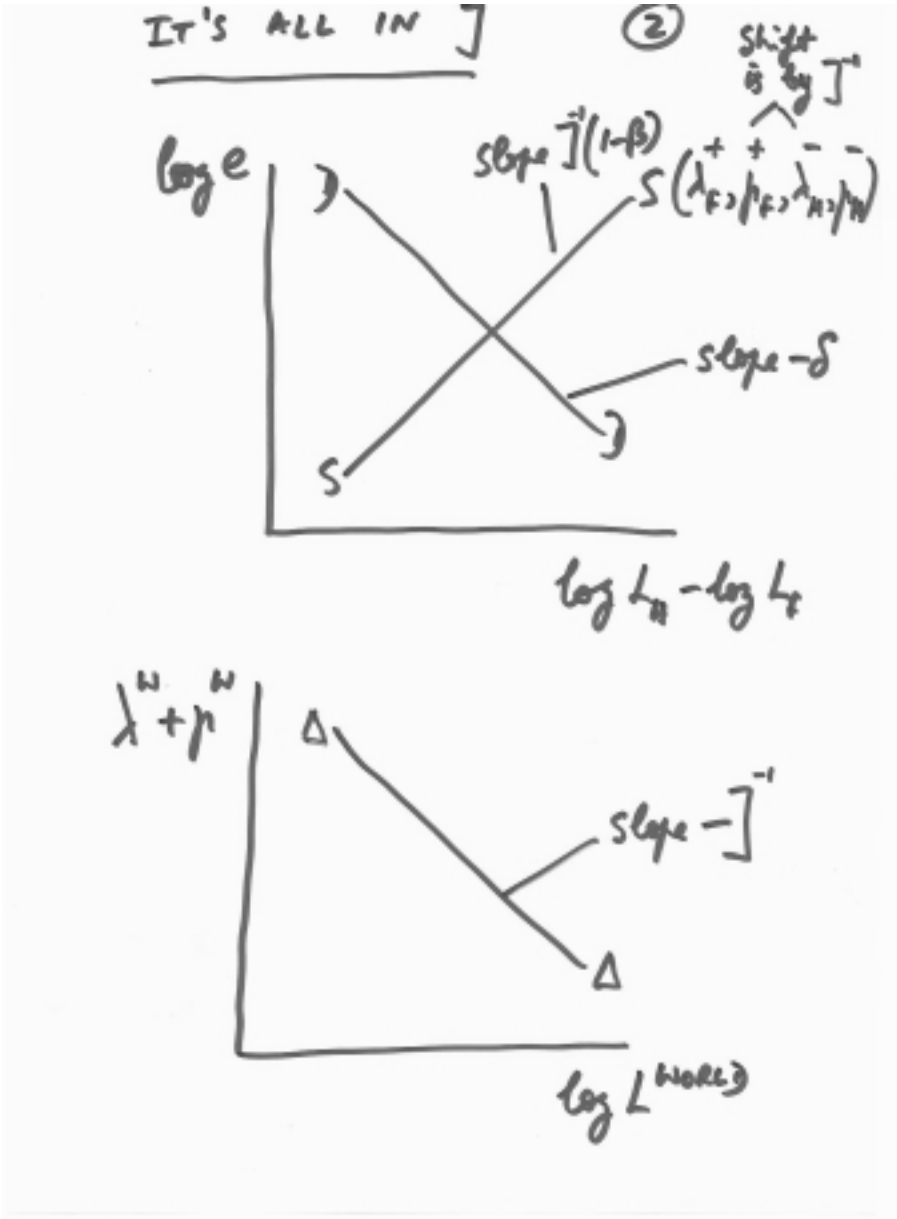
$$P_H = W_H (1+\mu_F)$$

$$\therefore L_H^S = \left[\frac{e^{(1-\beta)}}{(1+\lambda_H)(1+\mu_H)}\right]^{]^{-1}} \Rightarrow L_F^S = \left[\frac{e^{-(1-\beta)}}{(1+\lambda_F)(1+\mu_F)}\right]^{]}$$

$$\frac{L_H^D}{L_F^D} = (e)^{-\delta}$$

ASSUMING SYMMETRY

$\log L^{WORLD} = -]^{-1}(\lambda^{WORLD} + \mu^{WORLD})$



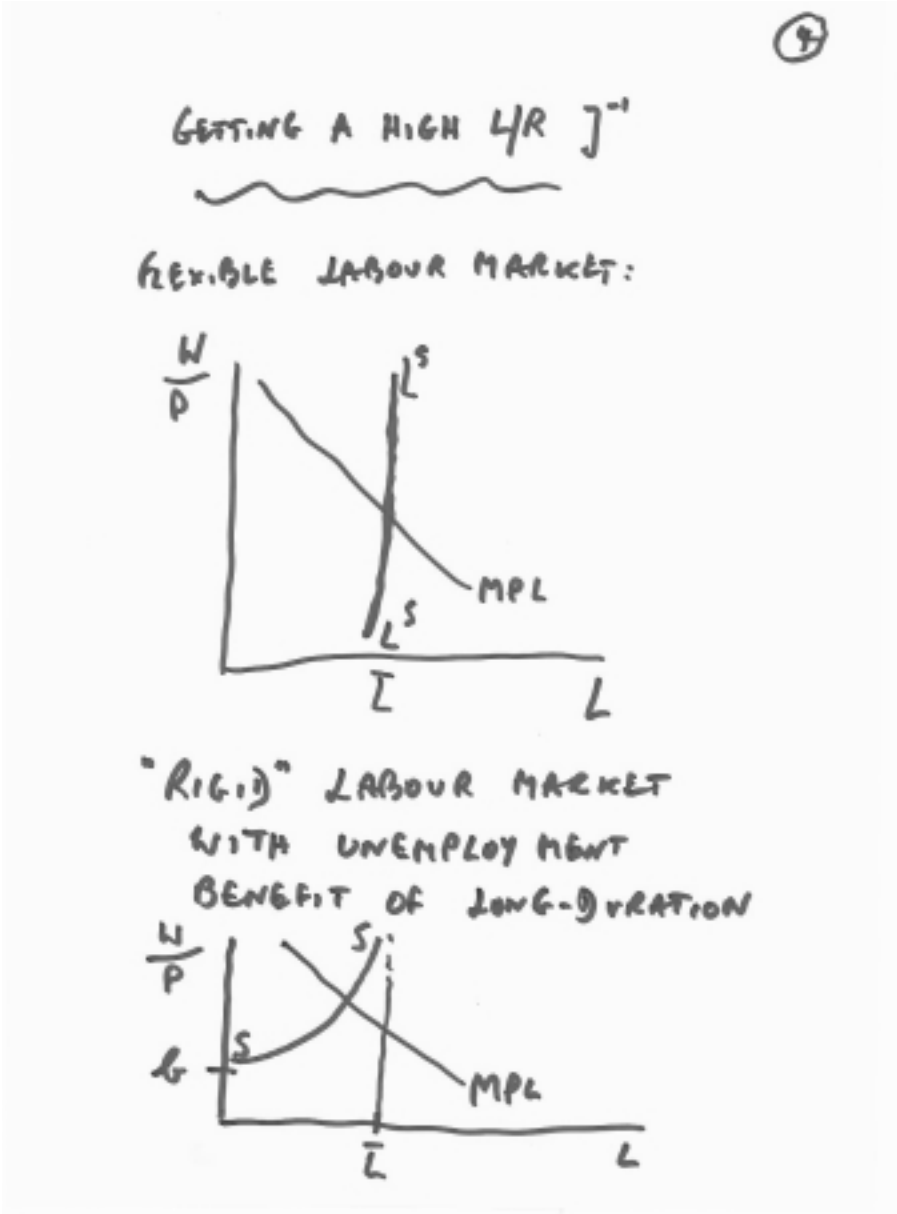
Hence the parameter ζ^{-1} is the key one. This is the Frisch (ie substitution) elasticity of labour supply- the model has utility additive in leisure so there is no income effect. Notice that ζ^{-1} determines for each economy both the slope of the labour supply curve (and hence of output too) and the shift effect of the degree of competition both in the labour and product markets; for the world economy it determines the slope of the competition effect on world output/employment.

I have drawn these curves for a given relative cost of capital. One should also note that the relative cost of the capital good is also important; this relativity to the cost of intermediate inputs and of labour implies that if the distortion in the capital goods industry falls the capital/other-inputs ratio will rise, increasing output for a given labour supply. One may gauge the importance of this quantitatively in this exercise by seeing that the rise in European labour is roughly 55% and 75% of the rise in output (Figs. 5 and 6) respectively for the lower and the higher values of ζ^{-1}; thus the effects of competition in lowering the relative costs of the capital good contribute the rest by raising the output/labour ratio. (This tallies roughly with the authors' own assessment as reported at the conference.)

Let me focus first on the crucial labour supply effect and the elasticity. There is, I

believe, a problem with the model's micro-foundations here. The authors appeal to micro studies of the Frisch elasticity. However, the point is that the empirical evidence on the total long-run elasticity indicates pretty clearly that it is close to zero: over decades real wages have been driven up in developed economies by large amounts and yet labour supply has changed rather little in most economies (if one discounts changing participation such as that of women- hence we are focusing on hours worked per participant). The total elasticity includes the income effect which essentially offsets the substitution effect. Of course in this model the income effect is ignored but while one may wish to defend this modelling choice for macroeconomic dynamics it cannot be right for the long-run steady state. Thus in practice one needs to think of zeta-inverse here as doubling up for the total elasticity if we are to get reliable results. In terms of the micro-foundations of this model it has to be treated as zero or very close to zero, thus removing a key prop of the results.

This issue has a long history in the literature dealing with European supply-side problems (see for example Layard, Nickell and Jackman, 1991, for a fairly complete survey of this, my own early efforts focused on the UK, Minford, 1983,
and those of Burda, 1988, on Europe). Eventually this work converged on the view that the root of the 'real wage rigidity' (high labour supply elasticity) of Europe lay in the importance of long-duration unemployment benefits. These have the effect of flattening the supply curve as a significant number of (mainly low-paid) workers found falling wages bringing them close to the levels of these benefits. These workers would in turn withdraw from the labour market for a long periods (either until their benefit entitlement expired which it rarely did to a large degree or until wages for their labour type improved)- hence the reduction of labour supply would take the form of long-term unemployment among lower-paid workers, a particularly awkward form from a social viewpoint. A typical estimate of the total elasticity from this literature would be rather high (for example mine for the UK is 20 times the unemployment fraction- thus unity at an unemplyment rate of 5%, about three times the authors' preferred value for ζ^{-1}) and Blanchflower and Oswald (1995) have a wide set which can reasonably be interpreted in these terms; their 'wage curve' relates the log of unemployment to the log real wage, implying that the labour supply elasticity is the unemployment rate times this unemployment elasticity- mirroring the shape of the labour supply curve in my graph below, with the elasticity rising as the real wage falls.



I see no reason why the GEM model should not reinterpret ζ^{-1} in these terms, treating the parameter as a convenient parable for the European institutions creating labour-market rigidity. The authors should not feel compelled to work within their paradigm in a literalist way since plainly they wish to address actual European problems. Furthermore this reinterpretation would be useful for measuring the degree of labour-market imperfection; this is a complex of the generosity of the benefit system, union power and labour taxes. Finally, it sheds important light on why these problems are so intractable from a political viewpoint.

Product mark-up versus labour mark-up: which is the more serious problem?

As noted above a quarter to a half of the total effects on output are coming from the rise in the capital intensity of production as capital-goods distortions fall; furthermore distortions of product market competition are working through the labour supply effect to

reduce output. Thus it is that labour market distortions in GEM's estimates account for around four fifths of the total distortion-created reduction in European output.

I find this highly implausible. First of all we know that Europe has much higher capital/output ratios than the USA. These have widely been interpreted as a sign of inefficient use of capital, largely encouraged by the tax system's generosity in capital allowances and the cosy relationship between the banks and industry. Thus reduced distortions in product markets should be expected to reduce, not increase, European capital/output ratios.

Secondly the theory of dynamic gains from competition stresses the importance of product innovation within differentiated products. Differentiation and innovation produces high quasi-rents ('mark-ups'); these in turn raise returns to new entry and innovation. Hence it seems entirely wrong to treat mark-ups as a source of damage to output and growth- the contrary is probably correct even if a high (temporary) mark-up does create a temporary static loss of surplus (much as a patent does).

Here, in line with a number of other conference participants, I would suggest distinguishing between distorting taxes (and other elements in the product 'wedge') and mark-ups per se. In particular, trade protection via anti-dumping and (non-negligible) tariffs is serious: studies of electrical goods for example indicate that retail prices (including for imports) in the UK and the euro-zone are some 50% higher than in the US. More costly distribution seems unlikely to account for more than a fraction of this large differential.

This distinction between mark-ups and the tax wedge will surely downgrade sharply the importance of the contribution of product market distortions to Europe's supply-side problem, in favour of labour market distortions.

We may also note that terms of trade effects from greater competition in the labour market would drive down Europe's wage costs and so relative prices, as the model implies. However, we have observed both for the US and the UK rising terms of trade in recent years apparently reflecting the result of a more deregulated economy which has led to the shift of activity towards services (their shifting comparative advantage); within the terms of this model one can think of this as rising productivity (reflected in higher prices) due to the better exploitation of comparative advantage permitted by deregulation. Thus quite often deregulation has the effect of raising productivity and price rather than lowering costs and prices. Studies of the UK (e.g. Bean and Crafts, 1996) showed that the reduction of union power did not lower wages, prices or the terms of trade; rather it raised productivity and quality both directly in the industries involved and indirectly by permitting resources to be reallocated away from older sectors like coal-mining and steel.

Macro dynamics and stability

I turn finally to the macro dynamic aspects of GEM where in fact the greatest intellectual effort has plainly gone. As my fellow-discussant, Gunther Coenen, has remarked, it is not helpful to describe greater ease of stabilization in terms of the sacrifice ratio; rather they are claiming that the trade-off frontier between inflation and output variance shifts inward which it indeed does in the GEM model. The reason is that more competition makes prices vary more directly with costs, hence keep closer to the flex-price solution (as shown in equation 25, p. 8, of their paper). In other words their Phillips Curve slope is steeper and its adjustment coefficient is faster- thus in sum the short-run Phillips Curve slope is steeper.

Notice that they derive their Phillips Curve from a set-up with costs of adjusting nominal wages and prices. This gives rise to a relationship of the form:

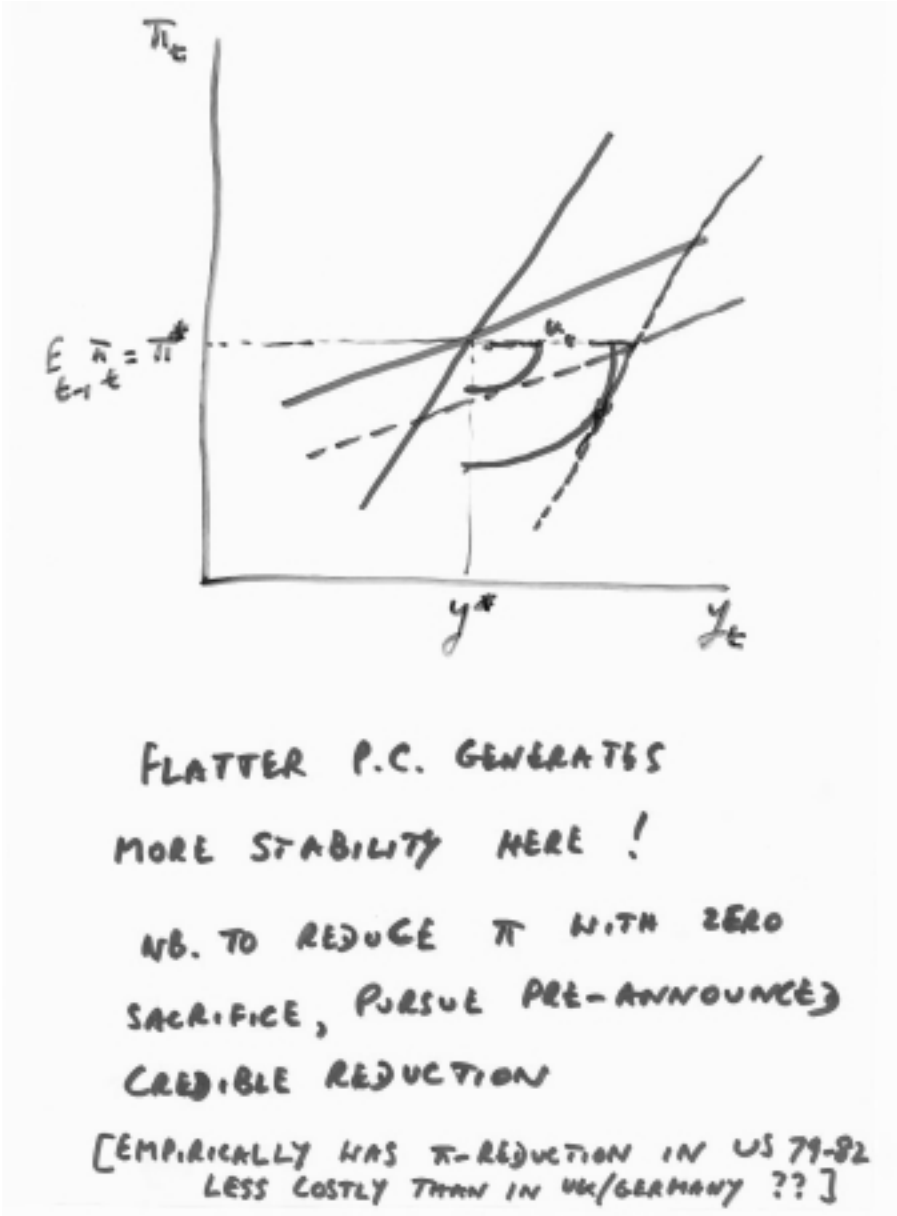
$\pi_t = f(y_t - y_t^*, \pi_{t-1}, u_t ...)$

However, I would argue (for some details see Minford and Peel, 2002, 2003) that this

formulation has reintroduced money illusion into the model: agents would set intended real wages and relative prices and that any adjustment costs would relate to such real variables; having done so they would then set nominal wages or prices by computing the rationally-expected price and then adding on the necessary real wage or relative price change. In this case one obtains a Phillips Curve of the form:

$$y_t - y_t^* = f(\pi_t - E_{t-1}\pi_t, y_{t-1} - y_{t-1}^*, u_t ...)$$

It is likely (as illustrated in the graph below- which juxtaposes the social indifference curves with Phillips Curves of different slopes shifted by a u_t shock) that were more competition to steepen the slope of this Phillips Curve it would have the opposite effect on stability- reducing it rather than increasing it. However it must be pointed out that the main determinant of the slope of this Phillips Curve will be the degree of indexation and the length of overlapping contracts, with the degree of competition a further but relatively minor or even negligible determinant. Thus for example in the extreme case where both wages and prices were totally set in nominal terms for the period of an overlapping contract then the Phillips Curve would be entirely flat regardless of the degree of competition. (We may also note in passing that the sacrifice ratio would be a function in such a set-up purely of the credibility of the (pre-announced) policy to reduce inflation.) These ideas seem reinforced by the empirical comparison of the US and Germany or the UK; the latter, with much less flexibility in labour and product markets, appear not to have had a worse sacrifice ratio if one adjusts for their high natural rates of unmployment when their disinflationary policies were pursued in the early 1980s..



Estimation:

The discussion of the macro-dynamic parameters is fairly typical of current methods in macro modelling in that it is dominated by the attempt to mimic the economy's response to a monetary shock as found in VARs. The deep supply-side parameters have been taken from micro studies as discussed above. The authors wish to pursue Bayesian estimation using strong priors. Such methods are uninformative about the confidence limits around the parameters: the VARs have already been identified in line with the authors' prejudices and so the fact that the model fits the response function with some standard error does not reflect the (true) errors of the model faced with the raw data. Given the IMF's computer resources, there is no reason why the authors should not estimate the model by a simultaneous estimator and use the bootstrapped raw errors to compute parameter confidence intervals.

Conclusions:

This model is an admirable piece of work whose results on the supply-side point in the

right direction. I believe that some modifications of it could improve the estimates and that we could usefully discover the implied confidence intervals from proper estimation. I am however rather unsympathetic to the macro-dynamic side of the model because of what I see as the money illusion embedded in the mechanisms designed to create nominal rigidity.

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