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by Stefano Siviero and Daniele Terlizzese

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MACROECONOMIC FORECASTING: DEBUNKING A FEW OLD WIVES' TALES

by Stefano Siviero[*] and Daniele Terlizzese[*]

Abstract

The forecasting profession, especially when producing forecasts intended to support economic policy, does not currently enjoy a good reputation. Complaints are sometimes voiced about its lack of scientific discipline, which in turn implies that the forecast results may be viewed as arbitrary. At other times, it is the excessively mechanical nature of the forecasting process which is criticised, on the grounds that it prevents a proper evaluation of any information concerning changes that alter the functioning of the economic system. Moreover, the use of structural models is often deemed superfluous, or even dangerous, and reduced forms are suggested as a preferable alternative. Drawing on the actual forecasting experience at the Bank of Italy, this paper argues that these views stem largely from a biased perception of how forecasting works, what it consists of and which goals it pursues. In particular, forecasting does not simply amount to producing a set of figures: rather, it aims at assembling a fully-fledged view — one may call it a "story behind the figures" — of what could happen: a story that has to be internally consistent, whose logical plausibility can be assessed, whose structure is sufficiently articulated to allow one to make a systematic comparison with the wealth of information that accumulates as time goes by. This implies that the forecasts are not the result of a black-box process that completely lacks discipline; neither are they the outcome of a purely mechanical process that cannot take new information into account. This paper tries to show that forecasting can be rigorous, not mechanical, informative, and useful even in the face of unprecedented situations.

JEL classification: C53.

Keywords: forecasting, policy-making.

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Contents

All predictions are impostures, the result of fraud, folly, or fanaticism.
Napoleon
From: "Collection of Napoleon's Maxims", by A. G. de Liancourt, translated by J. A. Manning; London, Arthur L. Humphreys, 1903

1. Introduction[1]

To paraphrase Leamer (1983), there are two things you are better off not watching in the making: sausages and macroeconomic forecasts.[2]

In this paper, while acknowledging that Leamer's punchline is indeed remarkably effective from a strictly rhetorical viewpoint, we support the view that, at least as far as macroeconomic forecasting goes, there are good reasons why one should feel less sceptical about the forecast production process.[3]

While a few of the arguments put forward in this paper may apply to other kinds of forecasts, we will focus exclusively on the particular kind of forecasting that aims to provide background analysis and support for economic policy-making; in particular, this paper will try to disprove some of the criticisms that are often voiced concerning macroeconomic forecasting:

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[1] We are greatly indebted to Carlo Giannini for his many and very stimulating suggestions and criticisms. Helpful comments from Filippo Altissimo, Albert Ando, Fabrizio Balassone, Andrea Brandolini, Riccardo Cristadoro, Olivier De Bandt, Alberto Locarno and Marco Magnani are gratefully acknowledged. The usual disclaimer applies. The views expressed in this paper are the authors' own and do not necessarily reflect those of the Bank of Italy.

[2] Diebold (1998, p. 175) is even sharper in his rebuttal of the "traditional" approach to forecasting: "The reports of the death of large-scale macroeconomic forecasting models are *not* exaggerated" (emphasis in the original).

[3] As to sausages, the law of comparative advantages suggests that the burden of disproving the first half of the statement above should be left to more competent experts than we feel we are.

(a) no structural model is needed to produce economic forecasts; it is generally sufficient, if not altogether preferable, to use a reduced-form model as long as the latter's statistical properties are "good";

(b) it is doubtful whether forecasting is at all respectable from an academic viewpoint, as it lacks scientific discipline. At the opposite end of the spectrum, there is the viewpoint that forecasting is not respectable because it is a purely mechanical activity;

(c) forecasts are useless precisely when they are most badly needed, i.e., whenever the structure of the economy undergoes relevant changes.

In this paper we claim that all of these views are, to a large extent, old wives' tales, and as such should be debunked. In particular, these views stem from a biased perception of how forecasting — especially when aimed at supporting the policy-making process — works, what it consists of and which goals it pursues.

To put in a nutshell, this paper aims to show that the forecasting process does not simply result in the production of a set of figures. Rather, it assembles a fully-fledged view — one may call it a "story behind the figures" — of what could happen: a story that must be internally consistent, whose logical plausibility can be assessed and whose structure is sufficiently articulated and multi-faceted to allow one to make a systematic comparison with the wealth of information that accumulates as time goes by. In short, the forecasting process does not produce a take-it-or-leave-it figure. Rather, it creates a story derived from sound analyses that may be of help when discussing the economy.

It follows that:

(a) projections derived from reduced-form models offer inadequate support to policy-making, since they cannot really provide a "story behind the figures";

(b) forecasters are tightly constrained, when making arbitrary adjustments to their forecasts, by the need to be explicit about the economic reasoning used to support their results;

(c) forecasters cannot simply "push the button", because they must be able to argue that the (possibly only qualitative) information that does not directly enter as an input to

the model's equations was properly taken into account in the course of the forecasting process;

(d) forecasters are not always clueless when facing a structural break, as pre-break models can still be of use in defining which mechanisms are likely to be affected by the break, which will presumably remain unaffected, and in which directions the changes can be expected to move.

In the following sections of this paper we will produce evidence to support these claims, drawing on hypothetical or actual examples, the latter being taken from our experience in producing forecasts for monetary policy-making using the quarterly macroeconometric model of the Bank of Italy. By their very nature, none of the propositions put forward in this paper can be inconfutably proved to be correct in a deductive sense. However, the extensive personal experience acquired by the authors suggests that the process of producing forecasts adopted in the Bank of Italy is not unlike that of other institutions in which projections are needed as an input for the policy-making process; and thus that the issues addressed and the solutions adopted are not very different from those described here. Hence, the paper aims to generate in the reader, possibly on behalf of much of the forecasting profession, a spark of intellectual curiosity about the activity of forecasting, while challenging the ungenerous slanderers of the macroeconomic forecasts' production process who maintain that it closely resembles sausage-making.[4]

While Sections 2 to 4 will address the three criticisms of macroeconomic forecasting listed above in a sequential fashion, the foregoing shows that there is in fact just one line of reasoning behind all the arguments put forward in this paper, resulting in frequent cross references among the different sections. In an effort to give a fair assessment of the forecasting process as we know it, in Section 5 we briefly discuss two of the main vices that often affect macroeconomic forecasters. Section 6 contains a few brief conclusions.

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[4] Provided that one holds the view, to which we do not necessarily subscribe, that the latter should better not be watched in the making.

2. Plain numbers? Or "stories"?

A forecast produced using a structural model: $Ay_t = By_{t-1} + Cx_t + u_t$ (using the standard notation) is obviously numerically equivalent to the forecast that would be delivered by the corresponding reduced-from model: $y_t = A^{-1}By_{t-1} + A^{-1}Cx_t + A^{-1}u_t$ (for non-linear models this statement needs to be qualified in a number of ways, all of which are well beyond the scope of this paper). Supplementing this rather obvious proposition with the scepticism that has been increasingly shown concerning the possibility of correctly identifying the matrix A — a scepticism that dates at least as far back as Sims (1980) — one may easily understand why there is widespread belief that all one needs (and actually all one should wish to have) for forecasting purposes is a reduced-form model (VAR with or without exogenous components),[5] which does not need to identify the contemporaneous relations among endogenous variables.[6]

This view of the forecasting process presents an obvious weakness in that it implicitly makes the positivistic and to a large extent presumptuous assumption that the figures produced by the model are unfailingly trusted. In other words, it does not take into account the fact that the policy-maker will autonomously decide if and to what extent s/he wishes to rely, in making policy choices, on the projections presented. It is clearly the case that, in the course of this process, the policy-maker must (and must want to) be convinced that the figures s/he is being shown are both plausible and reliable.

It would be erroneous to believe that, should one be able to claim that one's past forecasting errors were satisfactorily "small",[7] then one's forecasts will always be deemed

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[5] The need to identify a structure is, on the contrary, acknowledged whenever it is necessary to assess the reactions of the model to shocks (for a review of the literature see Christiano, Eichenbaum and Evans (1998)).

[6] Note, however, that the variables included in a VAR are themselves selected *a priori* in an arbitrary fashion. It follows that the impulse responses of a VAR, and hence also the projections obtained, are conditional on that preliminary selection. More generally, a reduced-size model may provide incorrect indications as to the properties of the larger system from which the model is implicitly derived. For instance, Lütkepohl (1982) shows that this may lead one to draw false conclusions as to the causal relationships among the variables of a VAR.

[7] Note, however, that a-theoretical, openly over-parameterised models such as VARs tend to result in relatively large confidence intervals around the central projections.

reliable by the policy-maker. First, forecasts produced for policy purposes are almost always conditional (e.g., they assume that economic policies will not change, they hinge on a postulated scenario for the rest of the world, etc.). Thus, it is very difficult to assess *ex-post* whether any given discrepancy between projected and actual values points to genuine forecasting errors or rather to differences between the exogenous assumptions on which the forecast was based and the actual exogenous outlook.[8] Second, even the best models are, by definition, false and incomplete. It is therefore both reasonable and desirable that each policy-maker who acts on the basis of the model outcomes should not limit her/himself to simply reviewing past performance, but should always assess each new forecast to determine whether the figures it contains are *reliable in the present circumstances*.

Examining past forecasting errors cannot therefore be the main (or, worse, the only) tool used to assess the reliability of a forecast. It is therefore necessary to find other arguments to convince policy-makers that the forecasts presented are in fact trustworthy.

The most natural way to tackle this issue, given the particular viewpoint from which it is being examined, would be to adopt a sort of "revealed preferences" approach: everything that can convince policy-makers of the reliability of the forecasts they wish to use can usefully and suitably accompany the forecast figures.

Our experience shows that, when adopting this viewpoint, the most important criterion to assess the reliability of a given forecast is the availability of an explicit and convincing economic "story" supporting the figures. In other words, it is essential that a logical, consistent causal chain can be traced from the assumptions to the results and that well-known economic mechanisms are evoked at each step.

The second criterion (which, as will be shown shortly, is not independent of the first) is whether or not the forecast takes stock of all available information.

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[8] The *post-mortem* analysis of forecasting errors is a particularly tricky business, in that possible (inevitable, actually) errors in the assumptions concerning the exogenous variables are often counterbalanced by conveniently setting the add-factors, if the results produced by the model are at variance with the available information.

But, if these are the relevant criteria against which to assess the reliability of a forecast, i.e., if the "story behind the figures" is a (possibly *the*) fundamental component of each forecast, it must necessarily follow that a reduced-form model is insufficient. The "story", i.e., the description of the causal chain that justifies a given result, is in fact contained in the matrix A, which is the part of the structure that describes the simultaneous links among the endogenous variables. If these links cannot be explicitly traced, i.e., if they are inextricably hidden within the coefficients of the predetermined variables, then the forecast will inevitably appear to be the result of obscure mechanisms (the so-called black box). One may or may not trust the forecast. One may not, however, argue about the rationale behind it, as it cannot be made explicit. This is not — nor should it ever be — acceptable to policy-makers.

2.1 The usefulness of a "story behind the figures"

The main benefit of using a structural model as a forecasting tool is the possibility of showing that any given projection produced by the model — "the figure" — can be traced back to the (possibly very complicated) interaction, in accordance with explicit and economically relevant conceptual schemes, of a given set of variables, each of which contributes to determining the final result according to known elasticities and with known lags — i.e., the "story behind the figures". It is thus possible to compare the forecast figures against three information sets: an assessment of the plausibility of the economic mechanisms from which the forecast stems; an appraisal of whether the size and timing of the effects associated with the different model variables are consistent with past experience; finally, an assessment of whether independent, possibly only indirect, evidence can confirm that those mechanisms are indeed at work with the same intensity and in the same direction as indicated in the forecast.

An example may serve to make those points more explicit. Let us assume that an inflation forecast has been formulated for the next two years. The sequence of inflation forecast values may be broken down into a number of separate contributions:

(a) given the projected path of unit wages, productivity and social contributions, the dynamics of unit labour costs can be estimated; (b) by combining the latter with the predicted evolution of foreign prices, and hence of competitiveness, and with the effects of aggregate demand pressure, one can compute the impulse associated with the dynamics of the private sector value added deflator; (c) given exchange rates, the prices of imported commodities and the pricing behaviour of exporters of manufactured goods to the domestic market, the contribution of the import deflator can be assessed; (d) the latter, together with the impact of the evolution of the private sector value added deflator, gives an estimate of consumer inflation net of indirect taxation; (e) given the assumptions formulated as to the latter, and the projected path for some non-core components of consumer inflation (rents, administered prices, etc.), the figure for headline inflation can be fully recovered.

Table 1 presents an example of such a breakdown.[9]

This way of decomposing the overall inflation figure puts the “real” determinants of inflation at the centre of the stage, in that the dynamics of consumer prices are governed exclusively by the evolution of production costs, labour productivity, aggregate demand pressures and competitiveness. Monetary policy remains in the background, its influence being partly reflected in the behaviour of the exchange rate; further back along the causal chain, monetary policy may be thought of as affecting inflation expectations (and hence wages and eventually prices), the degree of capacity utilisation (for given potential output) and the rate of unemployment (which in turn influences wages and profit margins). It is worth emphasising that the “story behind the figure” has a basically recursive nature: the breakdown of inflation outlined above is conditional on a given path of unit wages and hence neglects any simultaneity stemming from the interaction between inflation and the level of economic activity. In this particular instance, the choice of neglecting the inherent simultaneity in the relation between inflation and economic growth may also be justified on the ground of an informational efficiency argument: in the near future, wage dynamics are largely pre-determined, and it would thus be inefficient to overlook the wealth of information that is available from the knowledge of existing wage contracts. More generally, a high degree of simultaneity makes it difficult to interpret and “disentangle” the forecast results: on the basis of our personal experience, we tend to favour building partial “stories”, each of which can be

[9] A similar breakdown of the forecasts is routinely computed for a few other macroeconomic variables: consumption, investment, foreign trade.

interpreted separately as being recursive or quasi-recursive, although in a broader perspective they may interact with each other.

Table 1

A BREAK-DOWN OF THE ITALIAN INFLATION RATE
(households' final domestic consumption deflator; yearly average rates of change)

	Year 1			Year 2		
	Effect with endogenous mark-up	Effect with exogenous mark-up	Contribution through the mark-up	Effect with endogenous mark-up	Effect with exogenous mark-up	Contribution through the mark-up
Forecast	4.5	4.5	-	5.6	5.6	-
"Inertia"	2.3	1.0	1.3	-0.9	-0.8	-0.1
Unit labour cost	0.4	-0.8	1.2	1.7	0.3	1.4
of which:						
Compensation per employee	0.7	2.7	-2.0	2.4	3.9	-1.5
Productivity	-0.1	-2.7	2.6	-0.5	-4.2	3.7
Social security contributions	-0.2	-0.8	0.6	-0.2	0.6	-0.8
Import deflator	0.8	0.3	0.5	2.3	1.0	1.3
of which:						
Foreign prices	0.2	0.0	0.2	0.5	0.3	0.2
Exchange rate	0.6	0.2	0.4	1.9	0.7	1.2
Pass-through	0.0	0.1	-0.1	-0.1	0.0	-0.1
Taxes	-0.1	-0.1	0.0	0.2	0.2	0.0
Degree of capacity utilisation	0.1	0.0	0.1	0.5	0.0	0.5
Add-factors in equations of demand components deflators	0.3	0.3	0.0	0.2	0.2	0.0
Add-factor in equation of private sect. value added deflator	0.1	-	0.1	0.0	-	0.0
Other (rents, admin. prices, …)	0.5	0.3	0.2	1.5	1.0	0.5
Effect of mark-up behaviour	-	3.5	-	-	3.7	-
Discrepancies	0.1	0.0	0.1	0.1	0.0	0.1

Methodological note: In the case of the endogenous mark-up (first column), the inertial effect is given by the result of a simulation in which all the determinants of price dynamics were constrained to remain unchanged, starting in the first quarter of Year 1, at the same value as the average for Year 0. In the case of the exogenous mark-up (second column), the additional constraint was imposed that the mark-up (computed on the basis of trend productivity) remain unchanged at the same average level as in Year 0. The difference between the two effects (third column) thus provides an estimate of the inflationary impulse stemming from the inertial dynamics of the mark-up. The effect associated with each of the factors considered was computed as the difference between the result of a simulation in which only that factor is assumed to take its historical values and the inertial simulation. In the first column, the private sector value added deflator equation included in the Bank of Italy's quarterly model (also called, for short, mark-up equation) was allowed to react to the impulses stemming from the factor being considered. In the second column, on the contrary, the effect associated with the mark-up behaviour is separately identified. For any year, the sum of the effects reported in the third column coincides with the figure in the cell labelled "Effect of mark-up behaviour" in the second column. The figures shown in column three thus provide an estimate of how much of the overall impulse associated with each of the factors that underlie consumer price inflation is transmitted through firms' pricing behaviour.

To return to the "story" underlying a given inflation forecast, one may question it "globally". For instance, one may criticise the fact that money has no direct role in it. Perhaps more interestingly, the "story" may be questioned "locally". For instance, one may wonder whether the estimated impact of productivity on prices is in line with what one knows about the current evolution of labour demand; or whether the pricing behaviour of foreign exporters to the domestic market is consistent with what was observed in similar phases of the cycle. One may ponder (or question) either the path projected for each of the variables on which the final result hinges, or the elasticities of the latter with respect to the former (e.g., should a long-run elasticity of the private sector value added deflator with respect to competitiveness of 0.15 be deemed excessively high?), or the lags with which the various effects unfold.

The point is, then, that the forecast figures are not to be viewed from a take-it-or-leave-it perspective: one can appraise each component of the "story", and thus selectively compare and contrast the forecast figures with all the available information, whether the latter is "historical" (in that it concerns similar episodes from the past) or cyclical. Thus the reliability of a forecast hinges both on the economic reasoning behind it (the plausibility of the mechanisms that are invoked in order to justify a given outcome) and on the available empirical evidence (see below). Thus, its reliability can be assessed selectively and separately for each facet of a given forecast, distinguishing between aspects that appear relatively robust and those of more pronounced uncertainty.

The "story" is thus an essential ingredient to make the figures reliable and, therefore, trustworthy. Carrying this line of reasoning one small step further, one may claim that, at least in some instances, the real product of the forecast is the "story" itself: the more influential, longer-lasting message of a forecast exercise depends less on the precision of the figures (which, in any event, will prove inaccurate *ex post*, at least to some extent) than on the qualitative mechanisms it highlights as being particularly relevant in that particular instance.

Let us assume, for instance, that one is being presented with a forecast which predicts that households' consumption will increase considerably, although there is no sign of significant acceleration in either disposable income or wealth. The natural, initial scepticism towards this forecast might be considerably abated if the forecast figures were supplemented with the information that the acceleration of private consumption largely stems from the demand for durable goods, and that the latter is driven by the need to reconstitute stocks after the drop observed in the recent negative phase of the cycle. If no "structural" cause for the drop is apparent, one may deem it plausible that stocks will return to their desired level, as happened in similar phases in the past. The size and speed of the recovery may still be questioned, especially when compared with the past: however, the mere fact that a plausible explanation for the forecast figures is available may well be sufficient *per se* to revise upwards the probability assigned to the event "households' consumption accelerates". Moreover, this line of reasoning may lead the forecaster to consider other cyclical information that might otherwise go unnoticed, such as surveys of consumers' intentions; should these broadly confirm, albeit only qualitatively, the forecast figures, the reliability of the initial forecast would be greatly enhanced.

In other words, if the projected acceleration of consumption can be justified, if that reason is found to be plausible *a priori,* if it is supported by being consistent with both past cycles and independently available information, then the increase in private consumption may well be viewed as a robust feature of the forecast, albeit only qualitatively.

2.2 Was any information out there ignored?

Forecasts concern the future, about which by definition we have very little information.[10] This does not however exempt forecasters making efficient use of all available information. In fact, when a forecast is produced in time period t, it is usual for the variables in the forecasting model to be known only up to a period t-h, with h varying over time and across variables — thus giving a "ragged edge". The forecast must therefore at least also partly cover the present and the recent past: as these provide the initial conditions on which the predictions are based, they may heavily impact on the nearest segment of the outlook. However, a wealth of information is usually available concerning both the recent past and the present; in particular, partial information may be available concerning some variables, making it possible to reduce substantially or even to cancel completely the information lag about the

[10] The term "information" is here meant to denote certain knowledge of some event; in other words, a conjecture, no matter how plausible, does not constitute information in the sense given here to the term.

starting conditions of the forecasts. Information may also be available that allows one to form a reasonably precise, though necessarily approximate, projection of the future path of some of the variables (an obvious example, already mentioned above, is the information provided by settled wage contracts, which may significantly reduce the uncertainty surrounding future wage dynamics).

For a forecast to be credible, it is essential that it demonstrably take all available information into account. One of the most distasteful experiences for a forecaster is the realisation that a painstakingly assembled forecast scenario is deemed unreliable and hence discarded because some recently released information has not been taken into account. A forecaster is thus always acutely concerned lest some juicy nugget in the constant generous flow of news about the state of the economy pass unnoticed. Naturally, one of a forecaster's main tasks is precisely that of resisting the temptation to chase after every single item of high-frequency news. Rather, s/he should filter all available information, i.e., decide what is really relevant and what is not. To be able to reassure the policy-maker that some item that was in the end ignored is arguably irrelevant is a key ingredient of any reliable forecast.

Information keeps flowing in various forms. It may be quantitative, in which case it may directly concern exogenous or endogenous variables that explicitly appear in the forecast model being used, or it may relate to other macroeconomic variables not in the model (for instance, information may be available at a higher frequency, or may refer to a more disaggregate sectoral breakdown, than is contemplated in the forecast model); another instance of quantitative information is polychotomous variables, such as answers to surveys, or microeconomic variables that are somehow related to the macroeconomic phenomena that the model describes; there is also purely qualitative information which may be either anecdotal or the final outcome of fairly sophisticated analyses. This kind of information may also contribute to formulating the *a-prioris* against which the forecast must be cast.

The first of these information categories is, at least conceptually, easy to take into account (things are more complicated in practice, as the most recent information will probably be significantly revised in the near future, and thus must be appropriately "discounted"). The other types of information are, on the contrary, much more difficult to incorporate into the forecast.

However, the above discussion has already implicitly showed that, once a “story” is available to support the forecast figures, the “story” itself will enable the forecaster to explore the “information space” systematically and identify which information really is relevant. Once identified, the economic mechanisms most significantly responsible for a given forecast figure can be used to determine which checks on the model’s results are likely to be most useful and which information is most relevant. The effectiveness of a model as a “sieve” of the available information depends, among other things, on how detailed it is. A relatively aggregate model will highlight only a handful of mechanisms underlying a given result, and will involve only a limited set of variables to explain the forecast figures. Thus, a relatively aggregate model will, in general, provide fewer points of contact with the available information.

In any event, as the “distance” increases between the variables included in the model and those for which new information is available so it becomes proportionately more challenging to ensure that the forecast takes all available information into account. A partial solution to this problem is to build “bridge” models, linking the model variables with the miscellaneous information that gradually becomes available (Parigi and Schlitzer (1995)), thereby considerably shortening the information lag h mentioned above. By bridge models, we mean prevalently (if not exclusively) statistical models,[11] that do not generally allow for a structural interpretation and are intended to produce estimates for the recent past and the current period.[12] In other words, they are a tool to guarantee that the starting conditions of the forecast are updated, taking all recent cyclical information into account. These starting conditions will in turn require the model to be put “in line” by means of appropriate add-factors (see paragraph 3.1 below).

[11] The possibly purely statistical nature of bridge models may seem to clash against the emphasis that has so far been put on the need for a “story behind the figures”, and hence for a “structure behind the figures”. However, models of this kind are meant to exploit all available information for a very short horizon: given this limited task, a structural approach is arguably less acutely needed and possibly inferior to purely statistical methods.

[12] In some cases, if cyclical information is particularly timely and of exceptionally good quality, bridge models can also be used to produce projections for the immediate future (one-two quarters ahead).

2.3 When it comes to dynamics, anything goes

A corollary of the proposition that it is sufficient (and actually preferable) to use a reduced-form model, is that the dynamic structure of such a model should be totally unconstrained. Thus, in specifying the model dynamics, the only criterion should be the requirement that the resulting estimation error displays the usual, desirable properties. Implicit in this prescription is the argument that the dynamics of a reduced-form model can neither be interpreted *per se*, nor pose any problem when it comes to interpreting the results. It follows that a model's dynamics can be complicated at will, as they cost nothing.

Obviously, the importance of statistical tests of correct specification cannot be questioned.[13] However, as in reduced-form vs. structural-form forecasting models, allowing the dynamic structure of a forecasting model to be overly complicated (if this is what the statistical criteria call for) may not square with the reality of forecasting, particularly with the fact that the policy-maker must be convinced that the forecast is reliable.

A dynamic specification requires the introduction of a further component of the "story behind the figures", and specifically an "inertial" effect (this effect — shown explicitly in Table 1 — is often significant in the first few simulation periods and tends to vanish as the forecast horizon lengthens). In a structural model, this component can also be given an acceptable interpretation, since it represents the gradual absorption of the (possible) discrepancy between the actual values observed just before the forecast period and the long-run equilibria implied by the model equations. However, it may be difficult to provide a clear interpretation of the inertial effect, as it usually summarises a (possibly large) number of different adjustment processes.[14] It is also true that one can comfortably invoke equilibrium behaviour to justify this or that aspect of the forecasts, whereas it is more problematical to reason in terms of adjustment-toward-equilibrium processes, which are less clear both theoretically and empirically speaking. Complicated dynamics tend to produce a larger,

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[13] Note, however, the possibility that particularly complicated dynamics are a sign of mis-specification. The lagged values of either endogenous or exogenous variables may be statistically significant only in that they are correlated with omitted variables.

[14] Several adjustment mechanisms may be at work for the same equilibrium condition and/or for different equilibrium conditions.

longer-lasting, and — more importantly — non-monotonous inertial component, which makes it more difficult to explain the results and to convince the policy-maker that they are reliable. Experience thus suggests that there is a trade-off, when it comes to the dynamic specification of a model, between statistical properties and interpretability of the model results.

In may also be the case that strictly economic arguments call for the imposition of constraints on the dynamic specification of an estimated equation. For instance, foreign trade relationships should presumably allow for a faster response to demand than to relative prices, as producers will find it relatively easy to react promptly to changes in the former by modifying inventories and capacity utilisation. On the contrary, in the latter instance firms will probably re-organise their production processes so as to meet the shift in demand as soon as it is set in motion by the change in relative prices. As a further example, an investment function based on the assumption of putty-clay technology must satisfy some restrictions concerning the dynamic response of capital accumulation to desired changes in capacity output and to the relative factor cost. The lag structure of the investment function will in turn imply a well-defined dynamic structure of the relationship linking potential to actual output (Parigi and Siviero (2001)).

3. Better not to know?

The layman's perception of the forecast "production process" tends to polarise around two opposite, extreme positions.

On the one hand are those who emphasise the role of the econometric tools used to produce the forecasts, considering forecasting as an eminently mechanical and automatic process, in which the forecaster's sole task is to formulate the assumptions concerning the exogenous variables (if any) and "push the button" for the algorithms to solve the set of simultaneous equations that make up the model, and hence deliver the forecast results.

On the other hand are those who deny the possibility that econometric tools can provide enough scientific discipline, holding that the forecaster's own judgement is inevitably allowed to prevail and that the "neutral" projections supplied by econometric tools are always "bent" to satisfy the needs and whims of the moment. The forecaster's "nose" is probably given too much freedom, the argument continues, to adjust the projections to meet loosely-defined criteria such as "plausibility" and "realism" (the latter being unavoidably subjectively perceived). Given these conditions, the forecasting process would lack any scientific discipline and its results would be arbitrary and non-replicable.

Whether the forecasts lack discipline or, on the contrary, are so tightly constrained as to leave no room for intellectual speculation or insight, one would be better off not to watch the process of producing them, limiting oneself to observing the bottom-line results (just as in the case of sausages[15]).

The process of producing the forecasts used to support policy decision-making does not actually fit with any of the extreme viewpoints described above.

The foregoing discussion should have illustrated the non-mechanical nature of forecasting: the need to sift the available information is *per se* enough evidence to conclude that the forecasting process is more than simply "pushing a button". Judgement, intuition and ingenuity are also required when it comes to selecting the most appropriate scenarios to be explored and which phenomena deserve most attention. To elaborate further along these lines, investigating the risks inherent in a particular forecast is increasingly becoming a fundamental ingredient of the kind of forecast needed for policy-making purposes. Such an investigation usually calls for a complicated, multi-staged exchange of views with the policy-maker concerned, to define which aspects of the forecasts could usefully be subject to sensitivity analyses (covering the assumed path of exogenous variables, the plausibility of the mechanisms at work, the possibility that structural breaks have taken place) or, more ambitiously, to construct probability distributions. The practice of building subjective confidence bounds around the forecast figures was initiated and made popular by the Bank of England,[16] but other forecasters have subsequently adopted similar approaches. A

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[15] The authors, in a spirit of political correctness, wish to reiterate that they do not necessarily support this opinion of the sausage-making process.

[16] For a description of the asymmetric confidence interval, or fan-charts, published in the Bank of England's Quarterly Bulletin see Britton *et al.* (1998).

methodology aimed at pursuing the same objectives as the Bank of England's was developed at the Bank of Italy towards the end of 1997[17] and has since been occasionally[18] used to construct confidence intervals for consumer price inflation and GDP growth forecasts in order to reflect the judgmental assessments formulated by the management as to a set of potential risk sources. A distinctive feature of the methodology that evaluates forecast risks by means of fan-charts is the fact that the policy-maker is required to spell out her/his opinions concerning: (i) the probability that some of the forecast variables — chosen from a given set[19] — will turn out differently from the predictions based on the econometric model; (ii) the sign of those differences.[20] To the extent that the management's assessments identify asymmetric risks, the approach delivers confidence intervals that are not centred around the baseline simulation, as is the case with Figure 1 (in particular, that Figure shows the inflation rate forecasts produced in February 1998).

As to the claim that forecasters are not subject to any form of discipline, the need to build arguments that show the results to be sound and mutually consistent sets binding limitations to the possibility of arbitrary interventions on the forecast figures.[21] In particular, no intervention should preclude the possibility of justifying the projections by means of

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[17] The approach adopted at the Bank of Italy (for a description see Altissimo and Siviero (1998)), while broadly similar to that developed at the Bank of England, nonetheless differs in several technical aspects; specifically and most prominently, the Bank of Italy's procedure favours a non-parametric approach.

[18] Confidence bounds reflecting a judgmental assessment of the forecast risks are preferred to the more standard analysis of alternative scenarios whenever it is felt that those risks are spread over a wide spectrum around the baseline scenario. When, on the contrary, it is perceived that uncertainty clusters around a limited number of factors, and that the (subjectively) most likely alternative scenario(s) can be (tentatively) identified, it is deemed preferable to strengthen the main message of the forecast, and thus no fan-charts accompany the forecast report.

[19] The factors in relation to which one may formulate subjective assessments that are different from the baseline assumptions or results may be either exogenous variables (world demand, foreign prices, exchange rates) or behavioural mechanisms (pricing and wage-setting behaviour, demand for production factors, consumers' behaviour, foreign trade developments).

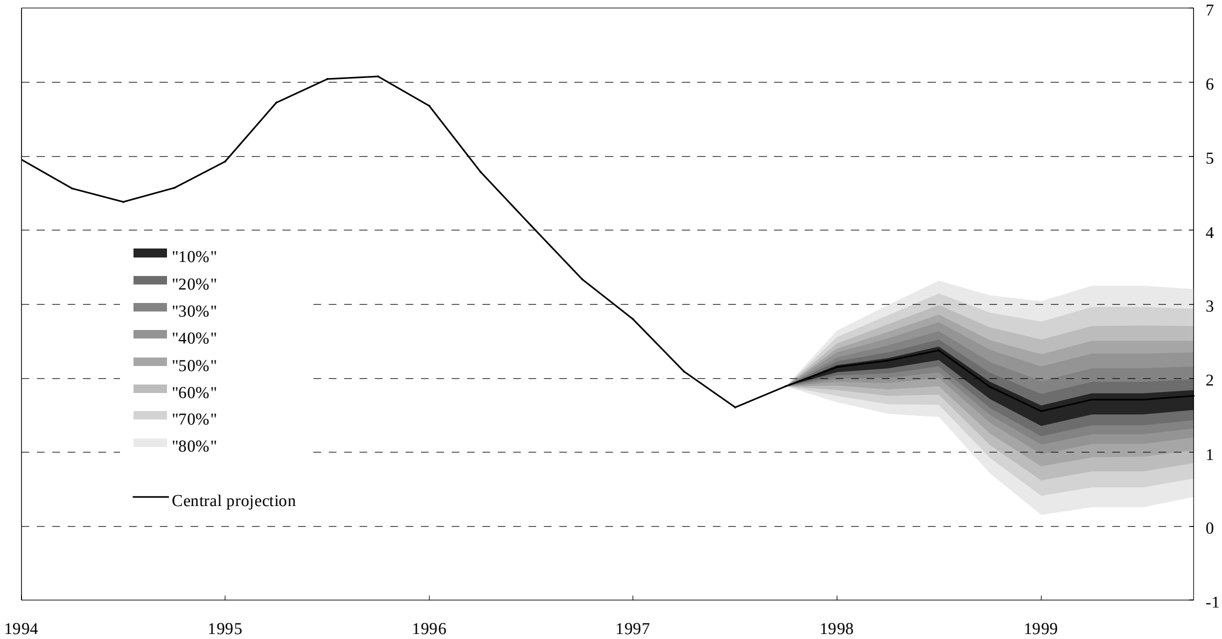
[20] The policy-maker is asked not only to formulate an opinion as to the probability and sign of deviations from the baseline, but also to indicate whether the degree of subjectively perceived risk in relation to a given aspect of the forecast is higher or lower than the historical average. This information is used to calibrate the width of the confidence bounds.

[21] If the main model features are, at least to some extent, known also to the end-users of the forecasts, more stringent constraints are limiting the possibility of "bending" the model forecasts.

appropriate economic arguments. This puts considerable constraints on the adjustments that can be made to the forecasts, to which we now turn.

Figure 1

HOUSEHOLDS' FINAL DOMESTIC CONSUMPTION DEFLATOR



Note: The figure presents the central forecast of inflation and the associated asymmetric subjective confidence intervals obtained in the course of the forecast exercise carried out in early 1998. The baseline projection is given by the solid line. The two central intervals (in darker colour) correspond to the values of the inflation rate that include the two central deciles of the subjective probability density function; the widest fan comprises 80 per cent of the probability density function.

3.1 The discipline imposed by the need for economically sensible results

When simulating an econometric model one can always produce whatever results one desires by appropriately introducing a (time-varying) correction to the constant term of each equation. This indeterminacy is presumably the source of the widely-held belief that forecasters are not subject to any kind of discipline. The adjustment of an equation's constant term (the adjustment is referred to with the following, perfectly equivalent, expressions: add-factor, intercept correction or constant adjustment) in this impudent way is neither widespread nor, in the light of the foregoing discussion, desirable: when it comes to de-composing the forecast results, the add-factor component (in the terminology of Table 1, the lines labelled "Add-factor in equations of demand components' deflators" and "Add-factor in equation of private sect. value added deflator"[22]) would be of overwhelming relevance in relation to the other elements of the forecast. It would therefore become nearly impossible to trace a "story behind the figures".

In fact constant adjustments are used parsimoniously, and, moreover, according to a limited set of sufficiently codified rules. In general, the corrections are used in order to make up for a known or suspected model failure, without completely re-estimating the model itself but, on the contrary, intervening only in the intercept term of the equation, thus not "dirtying" all other coefficients.[23]

The model failure one may want to compensate may be a systematic pattern of the static simulation residual in the very last periods before the beginning of the simulation, or may be only suspected to arise as a consequence of new phenomena known to occur in the future and for which the model is known to be inadequate. Whatever the case, a complete re-estimation of the model is probably not the most appropriate course of action. In the first instance, this is because the most recent data are usually only preliminary and will probably be considerably revised in the future, so that the information delivered by the latest available static simulation errors is prone to be affected by subsequent revisions. In these circumstances, it is undesirable to let the latest data impact on all the coefficients of a given equation: an adjustment of the sole constant term may in fact suffice to guarantee that the model is "in line" with the latest observations. In the second instance, it is simply that data for the "new regime" are not available, and it is again preferable to preserve the relationships among variables unchanged with respect to what emerged from the analysis of past experience, by only adjusting the constant term of the equation.[24]

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[22] It is worth noting that, if one limits oneself to using the add-factors in order to "put the model in line" with the available information, resisting the temptation to manipulate the error correction term of future periods with the objective of delivering forecast results that are consistent with one's *a-prioris*, then the contribution associated with the add-factor components tends to vanish in the course of the forecast horizon.

[23] Correcting the model results by means of add-factors does in general yield substantial improvements in the forecasting performance (see Haitovsky and Treyz (1972), Haitovsky, Treyz and Su (1974), Wallis and Whitley (1991), Clements and Hendry (1994), Hendry and Clements (1994)).

[24] The fact that one usually limits oneself to correcting the constant term of an equation is justified in the text on the ground of a strictly pragmatic argument: as not enough information is available for a full re-estimation of the model, it is preferable to keep the "key" coefficients (i.e., those that reflect the relationships

The setting of the numerical value of the add-factor depends on the interpretation given to the model failure one wants to obviate. Let us for the time being leave aside the case in which the constant adjustment is intended to compensate for a presumed future change in the model (see paragraph 4.4), and focus on that in which the pattern of recent out-of-sample static simulation errors unequivocally signals that the model is no longer adequate.

An analysis of static simulation errors often makes it possible to formulate an assumption as to the nature of the difficulties that the model is facing.[25] Given this assumption, a specific way to set the numerical value of the add-factor naturally follows. Symmetrically, any specific setting of the numerical value of the constant adjustment implicitly entails a well-defined (tentative) interpretative assumption as to the nature of observed static simulation errors. This assumption thus becomes a crucial component of the "story behind the figures". In other words, the need to build a story that supports the forecast figures provides reasonably clear indications as to how one should correct for a recently observed discrepancy between the model outcome and the actual data.

To illustrate this point, let us consider a single-equation model; let us also assume that its static simulation error is observable for only one period, but that an interpretation of the nature of the error may be nevertheless tentatively formulated.

Let us in particular consider the following model:

$$\Delta y_t = a + b\Delta x_t - c(y_{t-1} - x_{t-1}) + \varepsilon_t \tag{1}$$

or, equivalently:

$$y_t = \alpha + \beta x_t + \gamma x_{t-1} + \delta y_{t-1} + \varepsilon_t \tag{2}$$

with $\alpha = a, \beta = b, \gamma = c - b, \delta = 1 - c$, where y_t and x_t are logarithms. Let us assume to be in period 1 and that the model has coincided with the data generating process up to and

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among variables) unchanged, concentrating all adjustment in the level of the relationship. A more ambitious justification might hinge on the fact that the correlative structure among macroeconomic variables more frequently shifts than rotates.

[25] A full taxonomy of all possible patterns of static simulation errors and of their corresponding interpretations is beyond the scope of this work.

including period 0. Let us further assume, for the sake of simplicity, that the predetermined variable has been growing for a sufficiently long period of time at a constant rate g and that no disturbances have affected the endogenous variable. It follows that the latter must have reached its equilibrium growth path, along which the following expressions both hold:

$$\begin{aligned} y_t &= x_t + \frac{a+(b-1)g}{c} \\ &= x_t + \frac{\alpha-(\gamma+\delta)g}{1-\delta}. \end{aligned} \tag{3}$$

Under these assumption, the static simulation of the model, setting the error term equal to zero, results in a zero forecast error: $\Delta y_t - \Delta\hat{y}_t = y_t - \hat{y}_t = 0$, where $\hat{y}_t$ denotes the result that one obtains from the static simulation of the model with zero residuals, i.e.:

$$\begin{aligned} \hat{y}_t &= y_{t-1} + a + b\Delta x_t - c(y_{t-1} - x_{t-1}) \\ &= \alpha + \beta x_t + \gamma x_{t-1} + \delta y_{t-1}. \end{aligned} \tag{4}$$

Let us now assume that the static simulation error for period $t = 1$ turns out to amount to some given value k:

$$\Delta y_1 - \Delta\hat{y}_1 = y_1 - \hat{y}_1 = k\ . \tag{5}$$

Several alternative interpretations of this error may be formulated. In particular, one may speculate that the static simulation error is the consequence of:

1. a temporary error in the level of variable y : the equilibrium level is therefore unchanged, and the variable reverts to the previous equilibrium growth path from period 2;

2. a permanent error in the level of the variable, but only temporary in its growth rate: y instantaneously jumps on the new equilibrium growth path and resumes growing at the same pace as before starting from period 2;

3. a first step toward gradually reaching a new equilibrium growth path; on reaching the latter the variable will resume growing at the same rate as before. It is assumed that the adjustment toward the new equilibrium unfolds dynamically in accordance with the estimated coefficients. It is worth noting that this amounts to assuming a persistent (though not permanent) error in the rate of growth;

4. an unlooked-for removal from the equilibrium, that is likely to be made up for in the next period; the equilibrium level of y is unaffected, as in the first case above, but in this case also the integral of y does not change;

5. a persistent, but not permanent, error in the level of the variable, whose dynamic reversion to the previous equilibrium growth path takes place at a pace consistent with the estimated coefficients. Also in this case the error in the growth rate turns out to be persistent.

These alternatives basically cover the whole set of possible interpretations of static simulation errors (under the maintained assumption that they only affect the constant term of the equation to hand). Any of those alternatives is plausible in principle. The selection of the most appropriate one depends on the specific features of the case in hand, bearing in mind any additional information that may be available. Depending on which assumption one selects as the most appropriate, the add-factor will take different values, as shown in Table 2 (for a more detailed discussion see the Appendix).

If, for instance, one has reason to believe that the observed static simulation error is associated with a structural adjustment that has completely unfolded — in the sense that it is reasonable to expect that the dynamics of the endogenous variable will fall back in line with those implied by its determinants according to the coefficients estimated on the basis of the historical data — then one should set the add-factor as shown by line 2 of Table 2. The mirror-implication of this proposition is that, having somehow selected a numerical value for the add-factor, a well-defined interpretation of the contribution of this choice on the forecast will follow: the important point here is that, while this interpretation may be deemed more or less plausible, it may always be discussed and analysed in an explicit way.[26]

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[26] Whitley (1997) makes a similar point when, in listing the conditions that are needed for models to play any role at all in the policy-making processes, he states that “The judgmental part of the [forecasting or policy analysis] process [should be] made explicit”.

Table 2

ALTERNATIVE WAYS OF PROJECTING THE ADD-FACTOR AND CONSEQUENCES ON THE SIMULATION RESULTS

Ways of projecting the add-factor

Interpretation of static simulation residual	ε_1	ε_2	ε_3	ε_4	...	ε_j	...	ε_∞
1. Outlier	k	$-\delta k$	0	0	...	0	...	0
2. Err. dynamics	k	$(1-\delta)k$	$(1-\delta)k$	$(1-\delta)k$	...	$(1-\delta)k$	...	$(1-\delta)k$
3. Err. equilibr.	k	k	k	k	...	k	...	k
4. Compensat.	k	$-(1-\delta)k$	$\delta(1-\delta)k$	0	...	0	...	0
5. Grad. vanish.	k	0	0	0	...	0	...	0

Simulation results (levels)

Interpretation of static simulation residual	$\tilde{y}_1$	$\tilde{y}_2$	$\tilde{y}_3$	$\tilde{y}_4$	...	$\tilde{y}_j$	...	$\tilde{y}_\infty$
1. Outlier	$\hat{y}_1 + k$	$\hat{y}_2$	$\hat{y}_3$	$\hat{y}_4$	...	$\hat{y}_j$	...	$\hat{y}_\infty$
2. Err. dynamics	$\hat{y}_1 + k$	$\hat{y}_2 + k$	$\hat{y}_3 + k$	$\hat{y}_4 + k$	...	$\hat{y}_j + k$	...	$\hat{y}_\infty + k$
3. Err. equilibr.	$\hat{y}_1 + k$	$\hat{y}_2 + (1+\delta)k$	$\hat{y}_3 + k\sum_{i=0}^{2}\delta^i$	$\hat{y}_4 + k\sum_{i=0}^{3}\delta^i$	...	$\hat{y}_j + k\sum_{i=0}^{j-1}\delta^i$	...	$\hat{y}_\infty + \frac{k}{1-\delta}$
4. Compensat.	$\hat{y}_1 + k$	$\hat{y}_2 - k$	$\hat{y}_3$	$\hat{y}_4$	...	$\hat{y}_j$	...	$\hat{y}_\infty$
5. Grad. vanish.	$\hat{y}_1 + k$	$\hat{y}_2 + \delta k$	$\hat{y}_3 + \delta^2 k$	$\hat{y}_4 + \delta^3 k$	...	$\hat{y}_j + \delta^{j-1} k$	...	$\hat{y}_\infty$

Simulation results (changes)

Interpretation of static simulation residual	$\Delta\tilde{y}_1$	$\Delta\tilde{y}_2$	$\Delta\tilde{y}_3$	$\Delta\tilde{y}_4$	...	$\Delta\tilde{y}_j$	...	$\Delta\tilde{y}_\infty$
1. Outlier	$g+k$	$g-k$	g	g	...	g	...	g
2. Err. dynamics	$g+k$	g	g	g	...	g	...	g
3. Err. equilibr.	$g+k$	$g+\delta k$	$g+\delta^2 k$	$g+\delta^3 k$	...	$g+\delta^{j-1}k$	...	g
4. Compensat.	$g+k$	$g-2k$	$g+k$	g	...	g	...	g
5. Grad. vanish.	$g+k$	$g-(1-\delta)k$	$g-\delta(1-\delta)k$	$g-\delta^2(1-\delta)k$	...	$g-\delta^{j-2}(1-\delta)k$	...	g

Note: For a detailed description of the symbols used and possible alternative interpretations of static simulation residuals (first column of the table) see both the main text and the Appendix.

4. Predicting the unpredictable?

While one may tend to agree broadly with Sir Winston Churchill's celebrated remark that "The further backward you look, the further forward you can see", no one can dispute that, whenever something genuinely new takes place, the past may no longer provide reliable guidance. Similarly, the view is widely held that an econometric model (whose estimated coefficients, partly because of the very definition of "model", reflect historical experience) becomes quite useless in all those instances in which there are reasons to believe — because of recently observed data or some information concerning the future, such as already planned, significant changes in some institutional mechanisms — that a structural break[27] (i.e., a permanent or temporary change in the numerical value of the estimated parameters) has intervened or is about to do so.

In other words, it can be argued that models tend to "turn their backs" on their users precisely when they most need to see into the future. Certainly, a correct forecast is of most benefit just when considerable uncertainty — often associated with possible breaks with respect to the past — casts doubts on the reliability of the tool that was used. In short, model-based forecasts are unusable precisely when they would be most useful.

One can hardly question the rationale behind this proposition. Its conclusion, however, may be too hasty. A structural forecasting model may retain an effective and relevant role even in the face of structural breaks.

First, one may separately trace the causal paths that contribute to a given result for a variable of interest. One is thus not confined only to recognising that there has been a structural shift: it is in fact possible to identify which links in the causal chain, i.e., which behavioural relationship of the model, effectively show symptoms of a discontinuity. It is then possible to circumscribe the "range of influence" of the break: the break may thus be

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[27] In this context by structural break (or shift) we mean a generic inadequacy of the model to mimic a certain evolution of the observed data, independently of the cause of that inadequacy: we therefore do not distinguish between forecast failures due to changes in the data-generating process and those due to a mis-specification of the model.

"isolated", perhaps thereby considerably reducing the degree of uncertainty of the forecast, to the extent that the other behavioural relationships are deemed to be stable and hence reliable.

Further, once the nature of the structural break has been identified, the structure of the model itself may suggest the best way to try to assess the consequences of the discontinuity on the forecasts, taking advantage of other relationships which are known to be unaffected. In the following we will give examples of how the model may be used for this purposes.

4.1 All cats are not grey in the dark

Any forecaster's experience always contains significant discrepancies between the macroeconomic developments observed *ex post* and the predictions *ex ante*. This does not *per se* imply that the tool being used is inadequate satisfactorily to describe the mechanisms of an economy: as discussed in Section 2, forecasts are in fact almost unfailingly conditional — something that is even more true for projections intended for use in policy-making — so that a failure may sometimes be the consequence of specific conditioning assumptions that were adopted. But is also sometimes true that a *post mortem* analysis of the model results may indeed reveal inadequacies. In Siviero and Terlizzese (1997), for instance, the significant forecast errors observed for the inflation rate in the aftermath of the Lira's depreciation in September 1992 are systematically associated with the various behavioural relationships that determine consumer prices in the quarterly model. In the process, four possible causes of the forecast errors are identified: (*i*) the Phillips Curve; (*ii*) the equation that describes firms' pricing behaviour; (*iii*) the pricing-to-market policies of foreign firms; (*iv*) the formation of inflation expectations. The results of the analysis show that the mechanism that described the pricing-to-market policies of foreign firms in the version of the model available in the fall of 1992 was inadequate to capture the reduction in the pass-through that followed the Lira's depreciation. The other three behavioural mechanisms, on the contrary, do not present significant signs of structural break. The possibility to decompose the model's outcome makes it possible to identify the relationships responsible for a given forecast error. This has one very relevant consequence: it is possible to set an upper limit to the uncertainty associated with both the dimensions and ramifications of the forecasts. The effects of stable, and hence reliable, mechanisms allow one to quantify which parts of the forecast can be comfortably “believed”; the size of the effects that stem from the relationships that are known to be affected by structural instability may allow one to set an upper bound to the degree of uncertainty. In particular, one may build a sort of “confidence interval” around the forecast, the upper and lower bounds of which may be defined on the basis of “extreme” assumptions concerning the unstable relationships.

4.2 Does the past offer guidance into the unknown?

A scrutiny of forecast errors may sometimes give some clue as to how best to avoid making similar errors in the future, even when using the same model.

The forecasts produced in 1993 — i.e., in the wake of the extraordinary fiscal consolidation effort imposed by the Amato Government in the autumn of 1992 and the Lira’s suspension from the ERM in September 1992 — for some time systematically overestimated households’ consumption. It was thus necessary to ascertain whether the remarkably modest actual growth rates for private consumption were the result of an increase in precautionary saving (associated with increasing uncertainties about the future of disposable income) or an inadequate modelling of the process underlying households’ estimation of their perceived lifecycle income (which at the time was described as a distributed lag of current incomes). Had it been possible to discriminate between these two competing hypotheses, useful implications could have been drawn as to the way to correct the model outcomes during the following forecast rounds. In particular, if the parameters of the consumption function had been found to have undergone a structural change (the “precautionary saving” hypothesis), little, if any, indication would have been available to help to explore future consumer behaviour using the model. In the second case (the “lifecycle income mis-measurement” hypothesis), by contrast, it would have been appropriate to maintain the numerical values of the parameters estimated from the historical data, under new, suitable assumptions concerning the process of forming estimates for households’ lifecycle income and wealth.

In the Bank of Italy’s quarterly model, non-durable and durable consumption decisions are modelled separately. The latter do not depend directly on disposable income, but are the outcome of a process of adjusting to the desired stock of durable goods, which in turn is a function of the flow of non-durable consumption, the real interest rate, a measure of relative prices and some demographic factors, consistently with first order conditions of an appropriately defined utility maximisation problem. In the case mentioned above a close scrutiny of forecast errors showed no apparent instabilities affecting durable consumption choices. However, for non-durables standard statistical tests clearly rejected the hypothesis of parameter stability. The conjecture was thus formulated that the observed discontinuity reflected a revision of lifecycle income and wealth estimates rather than a rise in precautionary saving in response to increased uncertainty.[28] Had the latter been the case, it would have been reasonable to expect significant signs of forecast failure for the durable consumption equation as well.

In this particular instance the diagnosis also suggested the most promising therapy to minimise future forecast errors: since the source of income most heavily hit by the policy measure then taken was the expected future flow of pension payments, the impact of the "Amato reform" on pension wealth was estimated, and its consequences for consumption behaviour were then assessed using the numerical values of the parameters estimated over the historical period.[29]

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[28] This conclusion is consistent with the microeconomic evidence presented in Miniaci and Weber (1999): the fall of consumption in 1993 is due to the households' response to the permanent negative shock stemming from the major pension reform of 1992, rather than to a shift in their microeconomic behaviour. Their results exemplify how microeconomic analysis may be used to support, or possibly reject, the results that one obtains with a macroeconomic model. At the Bank of Italy occasional use is made, for this purpose, of a microeconomic model of households' behaviour based on data from the Survey of Household Income and Wealth (see Ando and Nicoletti Altimari, 2001).

[29] This example actually takes us on to very slippery ground, given the well-known difficulties associated with the evaluation of policy measures on the basis of behavioural relationships found to hold under a different policy set-up (Lucas (1976)). There are, however, several reasons to believe that the Lucas Critique may in practice be less disruptive than one may tend to believe in the first instance. First, the behaviour of economic agents may be backward-looking rather than forward-looking. The latter is a key ingredient in Lucas-type non-structurality. It is thus possible to test empirically which of the two behavioural schemes is indeed appropriate (Hendry (1988), Favero and Hendry (1992)). Second, even if the agents' expectation formation process is assumed to be forward-looking, the possibility exists that, because of the indeterminacy of the equilibrium, one may still specify rational and "Lucas-proof" decisional rules (Farmer (1991)). Third, the institutional changes or policy measures in question could not be the "regime shifts" necessary for the Lucas Critique to apply (Sims (1982)). Finally, even if each individual agent were to modify her/his decisional rule as a consequence of a policy regime shift, the aggregation of heterogeneous reactions may result in an aggregate response that is much less pronounced than each of the underlying individual reactions, so that the actual, aggregate macroeconomic effects of a policy change may be better approximated by an approach that disregards the inherent non-structurality (Altissimo, Siviero and Terlizzese (1999)).

A somewhat different circumstance in which the effects of a structural break can be at least partly assessed using historical relationships arises when information is available concerning future changes in some institutional mechanisms. As in the previous example, the Lucas Critique lurks dangerously in the background. It is nevertheless possible to identify some instances in which a relationship estimated on the basis of historical data can still help to quantify the effects of institutional changes. For instance, in 1994 a law was issued that established fiscal incentives to boost private capital accumulation, by means of tax cuts in proportion to the amount invested over a limited, pre-determined period (so-called "Tremonti Law"). The difficulty of assessing the impact of these incentives stemmed from the fact that, while the incentives were only temporary, the model's investment function lacked any explicit forward-looking component.[30] Thus, even a temporary reduction in the cost of capital — covering only the period during which the "Tremonti Law" was to be in force — would have implied lagged effects for a significantly longer period of time, thereby continuing to boost capital accumulation when fiscal benefits were non longer available.

In order to assess the impact of fiscal incentives it was tentatively assumed that the model could adequately estimate the overall effects, but that, given the temporary nature of the fiscal measures, the additional investment would be "front-loaded" in comparison with the model. The model was therefore simulated without changing the fiscal factors that affect capital accumulation decisions. An auxiliary simulation was then run in which the effects of the policy move were allowed to unfold gradually, in accordance with the lag structure of the investment equation. Finally, the cumulated differences between the two simulations over a long enough period of time were taken as an estimate of the overall effect of the fiscal incentives, which was then spread — unavoidably, in arbitrary fashion — over the quarters for which the incentives were available. The level of investment for these quarters was artificially boosted by add-factors; for the periods after the expiry of the incentives, add-

[30] The quarterly model investment function is characterised by complicated dynamics, due to the interaction of several factors: delivery lags, which result in installed investment being, in any time period, the outcome of decisions made in previous periods; the formation of expectations for both the increase in capacity output that firms project will be needed over the lifetime of the investment good, and the relative factor cost over the same time span (in both cases the expectations are implicitly assumed to be of the backward-looking type); and the aggregation of independent decisions made by different firms at different times.

factors were used to bring investment back to the level of the first of the two simulations described above.[31] This "trick" resulted in a basically correct assessment of the effects of the fiscal measures on capital accumulation (in light of the emphasis of this paper on the relevance of the "story behind the figures", as opposed to forecasting accuracy, when it comes to assessing the reliability of a forecast, this is purely incidental).

5. The vices of forecasters

This paper argues that forecasting activity is more interesting, disciplined, creative, ingenuous and honest than its critics would have us believe. This, of course, does not imply that forecasters have no skeletons in their closet. In the interest of fairness, two of the most disquieting vices of forecasters will be addressed: their marked preference for smooth forecasts; and the tendency to excuse failures by invoking the presence of structural breaks as a kind of blanket excuse.

It cannot be denied that the profession is markedly partial to smooth, jump-free patterns, perhaps because of one of the distinctive features of the "demand for forecasts": a regular, even path is much less likely to arouse suspicion and doubt in the final user, who is consequently less likely to ask the forecaster for additional checks of the robustness of the results. Sharp accelerations and decelerations in forecast results, on the contrary, often generate a sense of puzzled mistrust, producing calls for close and detailed scrutiny before they are deemed plausible. This attitude may lead the forecaster to eliminate all "irregular" features as a precaution.[32] It is clear that this may contrast with the need to trace a story behind the figures. In the example discussed in Section 2, the increase in durable consumption

[31] Further, minor adjustments were made to take into account the possibility that firms would anticipate part of the investment already planned for the following years.

[32] A variant of this first vice is that forecasters may tend to "iron out" any major differences with respect to projections by other forecasters, showing a sort of "herd instinct". The literature, however, shows that the opposite phenomenon also exists: in particular, forecasters whose reputation is sufficiently solid tend to produce projections that are markedly, perhaps intentionally, divergent (Lamont (1995)). In the authors' experience, while large discrepancies with respect to assessments by other forecasters arouse doubts concerning reliability and hence prompt a number of additional checks, this seldom results in actual modifications to the original projections.

— correctly projected by the model on the basis of mechanisms to restore the stock of durables to its desired level — could (and in the case mentioned initially did) foster doubts, precisely because it violated the requirement that forecasts be "regular" and smooth.

The second vice of forecasters is probably the result of the natural, psychologically gratifying tendency to believe that one's experience is a collection of unprecedented and unrepeatable events, rather than a quiet succession of uneventful episodes along a largely predetermined, surprise-free path. This attitude offers the not inconsiderable advantage of allowing one to invoke the *deus-ex-machina* "exceptional event" (either temporary or permanent) as a justification for observed forecasting errors. However, the impact of institutional changes on the model's behavioural equations can easily be overestimated. This in turn may induce one to replace some of the old model relationships with new, often *ad-hoc* ones, deemed to be more reliable in the face of the probable structural break. *Ex-post*, it is disappointing to realise that the former relationships could have mimicked actual developments more closely than the new ones. In Siviero and Terlizzese (1997), the case of the wage dynamics equation in the Bank of Italy's quarterly econometric model is recalled. When the effects of the Lira's depreciation in September 1992 were first assessed, it was felt that, as wage indexation had recently been discontinued, the Phillips curve of the quarterly model was inherently obsolete. In the course of the simulation rounds in autumn 1992, the Phillips curve was thus replaced with a mechanism indexing wages to the private sector value-added deflator. An *ex-post* analysis of forecasting errors revealed that a Phillips curve equation similar to the one used up to September 1992 would have resulted in a forecast of wage dynamics significantly closer to that actually observed.[33]

[33] This result could prompt one to speculate that the formal wage indexation in force up to 1992 was in fact a "veil" masking the real balance of power between the social partners, which is mirrored by the Phillips curve in a basically correct way. Similar conclusions are reached by Ginebri (1997) and Coquet and Le Bihan (1997); for a partially different opinion see Fabiani *et al.* (1998).

6. Conclusions

Given the lags with which the impact of economic policy-making unfolds, a forward-looking assessment of probable future conditions is usually required, both to decide whether immediate intervention is needed and, if it is, to design such intervention. The availability of reliable forecasts is thus an essential component of policy-making. This paper has moved beyond this unquestionable proposition to argue that the reliability of a forecast stems from the plausibility of the economic mechanisms that justify a given set of projections; from certainty that the "forecast production process" properly and fully exploits all available information; from the possibility to assess the results selectively and separately and to test their robustness in relation to — more or less limited — changes in underlying behavioural assumptions. This has deep and far-reaching implications for the way in which forecasts are produced and the design of the tools to be used in their production.

It is not, however, simply a matter of putting together arguments to support the forecasts effectively from a rhetorical viewpoint — although this aspect is also of some relevance. More importantly, it is the partial, fuzzy and ultimately speculative knowledge of an ever-changing world that suggests an approach based on cross-checks, which may also suggest the safest way to proceed in the event that the reliability of any result is questioned by the checks themselves.

Appendix

In addition to the symbols in the main text, let $\tilde{y}_2$ be the outcome of simulating eq. (1) (or, equivalently, eq. (2)), with appropriate non-zero values for the intercept correction of that equation.

Before going any further, note that the value of the endogenous variable observed in period $t=1$ may be written in the following equivalent ways, depending on whether one expresses it as a function of the outcome of the static simulation of eq. (1) or eq. (2):

$$\begin{aligned} y_1 &= x_1 + \frac{\alpha-(\gamma+\delta)g}{1-\delta} + k = \hat{y}_1 + k \\ &= y_0 + \alpha + \beta\Delta x_1 - (1-\delta)(y_0 - x_0 - \frac{k}{1-\delta}) = y_0 + \Delta\hat{y}_1 + k. \end{aligned} \tag{A.1}$$

As discussed in the main text, the appropriate way of projecting the add-factor depends on how one interprets the most recently observed static simulation errors. Let us distinguish among the following different cases:

1. *Temporary error in the level of the variable (outlier)*

If the observation of the endogenous variable for period $t=1$ is thought to be an outlier, it is sensible to set the value of the add-factor in the following period in such a way that the variable immediately reverts to the previous steady-state growth path. The intercept correction must thus be such as to compensate any lagged effects of the discrepancy between the value observed in period $t=1$ and the static simulation result.

Simulating eq. (2) for period $t=2$, with an add-factor given by ε_2, delivers:

$$\begin{aligned} \tilde{y}_2 &= \alpha + \beta x_2 + \gamma x_1 + \delta y_1 + \varepsilon_2 \\ &= x_2 + \frac{\alpha-(\gamma+\delta)g}{1-\delta} + (\varepsilon_2 + \delta k). \end{aligned} \tag{A.2}$$

It follows that, if the equation is required to settle back to the earlier steady-state growth path, the add-factor must be given by:

$$\varepsilon_2 = -\delta k \tag{A.3}$$

so that $\tilde{y}_2 = \hat{y}_2$. The rate of growth of the endogenous variable will thus be given by:

$$\begin{aligned} \Delta\tilde{y}_2 &= \alpha + \beta\Delta x_2 - (1-\delta)(y_1 - x_1) - \delta k \\ &= g - k. \end{aligned} \tag{A.4}$$

The average growth rate of periods 1 and 2 thus amounts to g.

In the following periods, given that $\tilde{y}_2 = \hat{y}_2$, it is obvious that the add-factors needed to keep the endogenous variable on the steady-state growth path are all nil.

2. Permanent error in the level of the variable, temporary error in its growth rate

Let us assume that the anomalous growth observed in period $t=1$ (g+k, rather than the equilibrium steady-state growth g) may have taken the variable to a new equilibrium level. This is tantamount to assuming that the observed discrepancy between the historical data and the long-run growth rate implied by the equation is temporary, and that the endogenous variable resumes growing at the rate g starting from period $t = 2$.

Eq. (1) gives:

(A.5)
$$\begin{aligned}\Delta\tilde{y}_2 &= \alpha + \beta\Delta x_t - (1-\delta)(y_1 - x_1) + \varepsilon_2 \\ &= g + (\varepsilon_2 - k(1-\delta)).\end{aligned}$$

Thus, if growth is to revert to the previously observed value, one must have:

(A.6)
$$\varepsilon_2 = k(1-\delta).$$

It is worth noting that, if the add-factor is set according to eq. (A.6), then the term in parentheses in the second line of eq. (A.5) vanishes; as this term corresponds to the disequilibrium in the observed value of the endogenous variable in period 1, by setting the add-factor as prescribed by eq. (A.6) one is indeed assuming that, in period 1, the endogenous variable has already reached a new equilibrium level, the latter being different from the previous one by exactly the amount k:

(A.7)
$$\tilde{y}_2 = x_2 + \frac{a-(\gamma+\delta)g}{1-\delta} + k = \hat{y}_2 + k.$$

To ensure that the variable remains on this new equilibrium path (permanently divergent by an amount k from the path followed up to period 0), all add-factors in the following periods must take the value given by eq. (A.6).

3. A first step towards a new equilibrium growth path

Let us consider the following expression (see eq. (A.1) above):

(A.8)
$$y_1 = y_0 + \alpha + \beta\Delta x_1 - (1-\delta)(y_0 - x_0 - \frac{k}{1-\delta}).$$

Eq. (A.8) may suggest that the discrepancy between the historical data and the static simulation result observed in period 1 could stem from the fact that the long-run equilibrium has changed by the amount $\frac{k}{1-\delta}$ with respect to the value implied by eq. (3), and that the endogenous variable is approaching this new equilibrium level following the dynamic path implied in the estimated coefficients. The rate of growth will thus revert to the value g only after the variable has reached the new equilibrium level:

(A.9)
$$y_t = x_t + \frac{\alpha + k - (\gamma+\delta)g}{1-\delta}.$$

If the ECM term that reflects the discrepancy of the endogenous variable with respect to its own equilibrium level must remain unchanged, in period 2, with respect to that in eq. (A.8), then the add-factor must be kept constant:

(A.10) $$\varepsilon_2 = k$$

and must retain that value in the following periods as well. In this case we will have:

(A.11) $$\tilde{y}_\infty = \hat{y}_\infty + \frac{k}{1-\delta}.$$

The rate of growth is thus bound to remain (decreasingly) higher than g for some time in the future; specifically:

(A.12) $$\Delta\tilde{y}_j = g + \delta^{j-1}k = \Delta\hat{y}_j + \delta^{j-1}k.$$

4. *Unlooked-for divergence from equilibrium*

Let us now suppose that the value observed in period 1 is the result of an undesired divergence from the long-run growth path, to be compensated in the following period. The add-factor must then be such that:

(A.13) $$\tilde{y}_1 - \hat{y}_1 + \tilde{y}_2 - \hat{y}_2 = 0$$

where $\tilde{y}_1 = y_1$. Since $y_1 = \hat{y}_1 + k$, the add-factor must produce the following result: $\tilde{y}_2 = \hat{y}_2 - k$. Using eq. (A.2) we get:

(A.14) $$\varepsilon_2 = -(1+\delta)k.$$

In order that the simulation remain on its previous steady-state growth path in the following period, the add-factor must be such that:

(A.15) $$\varepsilon_3 = \delta(1+\delta)k.$$

From period 4 onwards all add-factors must be nil.

5. *Persistent, but not permanent, error in the level of the variable*

Let us finally assume that the error observed in period 1 is interpreted as the first symptom of a phase in which the variable remains persistently, albeit not permanently, divergent from its previous equilibrium growth path, gradually reverting to the latter as implied by the estimated coefficients. If this is the case, then the add-factor must be nil from period 2 onwards, so that:

(A.16)
$$\tilde{y}_2 = \alpha + \beta x_2 + \gamma x_1 + \delta y_1 = x_2 + \frac{\alpha - (\gamma + \delta) g}{1 - \delta} + \delta k = \hat{y}_2 + \delta k \,.$$

In the following periods, the discrepancy between the simulation results and the steady-state growth path of the equation gradually shrinks:

(A.17)
$$\tilde{y}_j - \hat{y}_j = \delta^{j-1} k$$

eventually becoming nil in the long-run.

In the real forecasting business, the prescriptions briefly outlined in this appendix and in Table 1 are further complicated by the interaction of a number of factors, such as: the not inconsiderable task of identifying the “own” error of an equation, given the preliminary nature of the latest data; the fact that the equation’s dynamics may be considerably more complex than assumed here; the need to allow for violations of the assumption (here formulated for the sake of simplicity) that the variable has grown, in the previous periods, in exact accordance with the long-run equilibrium path implied by the equation.

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