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Conditional and structural error correction models Reply

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In his careful discussion of my paper, Ericsson (1995) stresses the need for structural modelling of cointegrated systems. At the same time, he shows that my concept of structural error correction model (SECM) may be too narrow, since it appears to exclude a number of error correction models (ECMs) which deserve the adjective 'structural' as much as any member of my class of models. Ericsson focuses on the possibility of having more endogenous variables than cointegrating vectors ($g > r$) and more than one cointegrating relationship in a single structural equation. There is no disagreement on this point; in fact, in my introduction I define an SECM as a transformation of a conditional ECM, formulated in terms of parameters that allow an economic interpretation. This leads to the very general class of models represented by (5) in Ericsson (1995), which includes closed systems ($g = n$), as analyzed by Hendry and Mizon (1993) *inter alia*, and which embeds any of the examples given by Ericsson. In my paper I only analyze a specific subset of this model class, defined by a diagonal matrix of adjustment coefficients. However, this should not be construed to imply that I would deny any other ECM the qualification 'structural'. This might have been more conspicuous if I had labelled my specific class of models 'diagonal ECMs', to distinguish it from a generic SECM.

It appears that the role of weak exogeneity in my paper should be clarified. I do not consider weak exogeneity of the conditioning variables (for the cointegration parameters) to be a necessary condition for a model to be structural; otherwise the material in Section 3.2 would have been redundant. Throughout the paper I do, however, stress the importance of explicitly analyzing this issue. Note that a violation of weak exogeneity does not imply that (3) and (5) in

Ericsson (1995) are no longer valid conditional models; it only implies that these models should be augmented with the marginal system (4) for efficient inference on cointegration. Therefore, it seems that some of Ericsson's examples could be modified to fit in my framework by redefining the endogenous and the conditioning variables (which might not be weakly exogenous). This still leaves open the question whether the resulting model should be called structural.

The debate on the use and meaning of 'structure' and 'structural' in econometrics is a very lively one, and my paper will not (and does not purport to) provide the definitive resolution to this discussion. Even without my specific assumptions, one could argue against calling Eq. (5) a structural model. As observed by Sims (1991) in his discussion of Clements and Mizon (1991), the sole fact that a system has the mathematical format of a simultaneous equations model does not make it structural. Without any explicit economic theory to interpret the equations and their parameters, such a system is merely a (potentially useful) reparametrization of a statistical (reduced form) model. Ericsson follows Hendry's (1993) definition of structural, viz. being 'invariant over extensions of the information set in time, interventions, or variables'. While invariance is a necessary requirement for a model to be structural, I would not consider it sufficient. Indeed, Hendry adds the requirement that a structural parameter vector 'directly characterizes the relations of the economy under analysis (i.e., is not derived from more basic parameters)' (Hendry, 1993, p. 12). This appears to correspond to the classical meaning of structural as opposed to reduced form. One might question whether all of Ericsson's empirical examples pass this test, i.e., whether they are not (implicitly) derived from more fundamental economic relations. In practice our aim might be somewhat less ambitious, in that we seek for well-specified, invariant, and parsimonious models, which may be given an *a posteriori* economic interpretation.

The question that remains is whether the class of diagonal ECMs provide a useful starting point for finding such empirical models. The proof of the pudding is in the eating; this question can only be answered by applying the methods in empirical practice. One of such applications is given in Boswijk (1992), who analyzes a two-equation model of real money and inflation in the UK. Another one is given in Urbain (1995), who formulates a diagonal ECM for imports demand and the exchange rate in Belgium, and compares it with a closed SECM, inspired by Clements and Mizon (1991) and Hendry and Mizon (1993). As might be expected, I fully endorse Ericsson's anticipation of more applications to follow.

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