Discussion

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Fung and Remolona have set out to identify the market's inflation and real-return expectations in Canada and the United States via a two-country extension of the equilibrium term-structure model introduced by Longstaff and Schwartz (1992). This paper is part of a larger research agenda that attempts to link empirically the structure of equilibrium term-structure models to macroeconomic fundamentals in both the United States and Canada. If they can accurately measure ex ante real interest rates and expected inflation from the term structure, the usefulness of such a project for monetary policy should be readily apparent. I commend the organizers of this conference and Fung and Remolona for choosing a subject that is topical—even long overdue.

Researchers and practitioners, driven by the demands of those on the receiving end of the information firehose called financial markets, have accumulated an intimate knowledge of the empirical properties of the term structure and its movements. Many of the subtleties in term-structure movements reveal themselves in the area of derivatives pricing, since interest-rate derivatives, unlike ordinary bonds, can be sensitive to narrowly isolated aspects of the term structure.

Much of this information has gone unappreciated by macroeconomists in general and by central bankers in particular. I have been around long enough to offer an anecdote that provides some perspective on the development of the profession. When I left graduate school in 1990 with a Ph.D. in finance, my first professional job was at the Federal Reserve Bank of St. Louis, where I was surrounded by economists. I was told specifically that I should work on finance, an area in which the research department professed a desire for additional expertise. I naturally found this to be an attractive assignment, since finance was what I had been trained to do. At least it seemed attractive until I learned that “finance” meant putting Tobin’s q on the right-hand side of one’s money-demand regressions. The gulf between academic macroeconomics and finance was wide.

The present paper attempts to marry an equilibrium model of the term structure with some macroeconomic fundamentals. Equilibrium term-structure models come clearly out of the finance tradition, as they typically assume an exogenous stochastic factor process that drives all interest rates. In this sense, they are partial-equilibrium models. Their basic aim is to formalize and parameterize the intricate empirical facts of term-structure evolution. Macroeconomics, on the other hand, is typically interested in a general equilibrium—one in which interest rates are both the cause *and* the effect of real and financial activity. Of course, this approach drills down a bit further, identifying as exogenous such “fundamentals” as preferences, initial wealth allocations, and learning rules.

This paper is thus an attempt to narrow the gulf between traditional macroeconomics and equilibrium term-structure models. The link here is made by identifying the term-structure model’s abstract factors with concrete macroeconomic variables, namely real returns and expected inflation. There are, ultimately, some significant flaws in the specification and identification of the model as estimated here; most of the rest of this discussion focuses on these limitations. Nonetheless, the basic structure of their approach is, I believe, of considerable interest, and should be strongly encouraged. A critique of the paper should not focus so narrowly on these specification problems that it overlooks the advance inherent in the general approach.

Modelling the Term Structure and the Problem of Specification

At the core of the model is the presumption that securities prices—the source of information to be filtered here—are determined by a stochastic discount-factor relation. Under this relation, the price, P_{nt}, of an n-period bond in period t is given as the expected present value of its one-step-ahead forecast:

$$P_{nt} = E_t[P_{n-1,t+1}M_{t+1}], \tag{1}$$

where M_{t+1} is the stochastic discount factor. For fixed-income securities such as zero-coupon bonds, the value at maturity, $P_{0,\,t+n}$, is known exactly. This fact can be manipulated recursively, so that the question of the stochastic evolution of the zero-coupon term structure ultimately reduces to a question of the evolution of the stochastic discount factor—see Chapter 11 of Campbell, Lo, and MacKinlay (1997) for a fuller derivation.

To develop the intuition in a simple setting, consider a finite state space over a one-step-ahead forecasting problem, defining the expectation $E_t(\bullet)$ over the S states as:

$$P_{nt} = \sum_{s=1}^{S} [Prob(s))P_{n-1,t+1,s}M_{t+1,s}] . \tag{2}$$

Note that both the future bond price and the discount factor may depend on the particular state that obtains. The discount factor is thus denoted as a vector, $(M_{t+1,1}, M_{t+1,2}, \ldots, M_{t+1,S})$. This same discount factor should be applied to all securities, lest arbitrage be available. Nonetheless, regardless of the total number of securities, the total number of states for any plausible financial market is much larger. Since S is much larger than the number of securities, there is a large number of valid discount factors, M_{t+1}, that would satisfy the summation equation above. For reasons of tractability, the authors here choose a parametric stochastic process—an “affine-yield” model of the term structure—to describe the evolution of M_{t+1}.

In this context, the key specification issue can be posed as a question of how to invalidate possible choices for the stochastic discount factor, to narrow the field to a stochastic discount factor model that is at least approximately or frequently correct. A wide range of candidate processes has been proposed, including the models of Vasicek (1977), Cox, Ingersoll, and Ross (1985), and Longstaff and Schwartz (1992).

One criterion for winnowing the field has already been mentioned: tractability. Fung and Remolona consider a closed-form, affine-yield model, essentially that of Longstaff and Schwartz (1992). Affine-yield models are tractable because they are log-linear in their underlying-state variables. The Longstaff and Schwartz model has two such underlying-state variables (labelled in the paper as x_1 and x_2 for the U.S., and $x_1{}^*$ and x_2 for Canada).

A second technique is to apply some simple economic common sense. That is, M_{t+1} is stochastic, and will therefore evolve randomly, but it should do so in a stable and sensible fashion. For example, the Vasicek model can allow nominal interest rates to drift into the negative range, a property often regarded as unsatisfactory. The model used here satisfies most such rules of thumb, including avoiding negative rates, and having sufficient flexibility to produce most general types of observed term structures, including humped, inverted, and inverted-humped shapes.

Financial market practitioners, who must quote prices on interest-rate derivatives, typically apply a third technique when selecting a term-structure model: a no-arbitrage condition. (Note that this differs from the no-arbitrage condition that Fung and Remolona refer to in Section 1.1.1 of their paper; this latter is simply the theoretical requirement that the same stochastic discount factor be applied to all securities.) For practitioners, a no-arbitrage model is one that correctly prices observed bonds. That is, the model must not only be able to reproduce the *general* shape of the current term structure (e.g., humped or inverted), it should be able to reproduce the current term structure exactly. This logic has also been extended to exact pricing of the term structure of volatilities as well. Closed-form term-structure models, while tractable, are not sufficiently flexible to reproduce actual term structures exactly.

There is a convenient and well-known mapping between the number of underlying factors in a closed-form term-structure model and the number of zero-coupon bonds that it can price exactly. Thus the Longstaff and Schwartz model, with its two factors, can, in general, plot exactly only two points on the yield curve; it must approximate the rest of the term structure. We know therefore that there must be some specification error in the model to be estimated. The question is thus whether this specification error is large enough to be troubling when the model is fitted to the data. As discussed below, there is reason to believe that this specification error is indeed significant.

Empirical Specification and Identification Issues

What remains is a list of (surmountable) concerns I have with the empirical results. I feel there are reasons to be suspicious of the empirical specification used here; most of these concerns are related to the statistical specification of the model and to the identification of the macroeconomic factors.

I will outline the methodology in the paper. First, the authors choose stochastic-difference equations to model the evolution of discount factors in Canada and the United States. In particular, they choose a discrete-time alias of Longstaff and Schwartz's (1992) two-factor model. They then solve the difference equation and rearrange the solution to get a parameterized yield curve, which is estimated for both the United States and Canada. One of the underlying factors, x_2, is common to both countries by assumption; it is identified as the ex ante real interest rate. Finally, yields are decomposed via the structure of the estimated model into four components (the ex ante real rate, expected inflation, an inflation-risk premium, and a real-rate-risk premium) whose properties are investigated in a series of graphs.

The limitation, noted above, that the Longstaff–Schwartz model can price only two bonds exactly, while the other parts of the term structure must be approximated, becomes a practical issue. In this regard, it is worth emphasizing (as the authors do) that Gong and Remolona (1997) need a third factor to achieve an accurate fit for the U.S. yield and the volatility curves. The present paper uses only the 2- to 10-year portion of the yield curve, precisely to accommodate the poor fit of the two-factor Longstaff–Schwartz model on both the short and the long ends of the yield curve. Econometrically, this methodology produces biased parameter estimates due to an omitted variable problem, and attempts to resolve the problem by narrowing the sample space. Without further diagnostics, including perhaps a comparison with a three-factor model, it is difficult to assess the full effect of this bias.

For the 2- to 10-year section that they use, the authors fit a cubic spline to generate the full zero-coupon yield curve. They restrict themselves to off-the-run Treasuries, to avoid measurement errors arising from differential liquidity premiums for different bonds. A priori, I would not have expected large liquidity premiums for Treasury issues, and questions are now raised in my mind. How large are such liquidity premiums? To what extent is the problem resolved by restricting attention to off-the-run Treasuries? Both of these questions should be easy to answer by comparing cubic splines with and without the off-the-run constraint.

The results in the paper split the sample period into 1984–1991 and 1991–1997 subsamples, effectively conceding that there are time-varying parameters. Indeed, some of the parameter estimates in Table 3—notably μ_1 in the three-risk model and $\mu_1{}^*$ in the four-risk model—are strikingly different across subsamples. The stated rationale for the break in the sample at February 1991 is the announcement of inflation-reduction targets in Canada. However, this break point coincides roughly with several other important contaminating factors, such as the dramatic steepening at the short end of the U.S. yield curve in the early 1990s, and the beginning of the Clinton presidency. Ultimately, it is impossible to tell what is causing the parameter shifts. As well, one is left to wonder whether February 1991 is the only appropriate break point to capture such shifts. Equation (26) in the paper presumes that the regression error terms v_{it} are mean-zero with constant variance. Some evidence on the presence of time variation in the estimated equation might be gained by examining the estimated residuals from equation (26) to verify that the white-noise assumption is indeed valid.

Another bit of evidence that the model is sensitive to specification arises when comparing results from the three-risk and four-risk models. The three-risk model restricts the Canadian factor loading on x_2 to be equal to the U.S. factor loading on that same variable, although there is no a priori reason to expect such a restriction to hold. Estimations of the two models produce noticeably different results. For example, in Figure 2, expected inflation for the three-risk model is roughly 2 per cent for most of the sample; for the four-risk model it is less than 1 per cent. For practical purposes, e.g., in monetary policy, a difference of more than 100 basis points in expected inflation is significant.

The final, and perhaps most important, concern I will raise regards the interpretation given to the factors x_1, x_2, and $x_1{}^*$. The model assumes no correlation between inflation expectations and the real interest rate. Offhand, this assumption seems ill-advised, as it is inconsistent with much of the empirical literature. Nor is this sort of money neutrality generally required by equilibrium term-structure models; Pennacchi (1991) and Sun (1992), for example, model inflationary expectations in the term structure without assuming money neutrality. Ultimately, the assumption is necessitated by the need to identify inflationary expectations and the real rate. Recall that x_2 is common to both countries, and that each country has its own idiosyncratic factor (x_1 and $x_1{}^*$). As I understand the Kalman filtering procedure, this estimation loads all of the common variation onto the x_2 factor, thus forcing x_1 and $x_1{}^*$ to be independent of x_2.

At this point, "identification" is effectively achieved by simply labeling x_1 as U.S. inflation expectations, $x_1{}^*$ as Canadian inflation expectations, and x_2 as the real rate factor. Some theoretical arguments might justify assuming that expected inflation is strictly domestic, but it is more difficult to argue that real rates are strictly international. One symptom of this "identification by fiat" may be the apparently poor performance of the estimated U.S. inflationary expectations depicted in Figure 2. After 1987, U.S. inflationary expectations appear to be utterly unresponsive to the actual inflationary experience. A critical reading must question whether the U.S. domestic factor x_1 is measuring something besides just inflationary expectations.

In sum, there are several spots in which the empirics in the present paper might be better implemented or better validated. Nonetheless, I strongly support the broader research program undertaken here.

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