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CORRESPONDENCE

From: Professor A. D. Wilkie

I was interested in the recent paper, 'The need for theory in actuarial economic models' by P. P. Huber & R. J. Verrall in *B.A.J.* **5**, Part II, pp 377-395, 1999. In it the authors characterise my model as data-based. This seems to me to be a misunderstanding of my methodology. I have seen data-based approaches to time series modelling of financial series, where an author takes the data, calls the observations X_{1t}, X_{2t}, X_{3t}, etc., plugs them into a standard time series package, and records the results. This methodology pays no attention to the underlying economic realities that the observations represent. I cannot quote references, because such papers seldom get as far as publication.

My method is not like that at all. It is very strongly theory-based, though it is different theory from that of some financial economists. Further, my approach has changed to some extent over the years, from the Report of the Maturity Guarantees Working Party (Ford *et al.*, 1980) through Wilkie (1981) and Wilkie (1986) to Wilkie (1995). As I have learned more, my method has become even more theory-based.

My theory, however, is based substantially on observation, not just observation of one particular data-set, but on observation of similar data-sets over very long periods and in many countries. It is also based on what practitioners in the investment markets and elsewhere say about how they look at things. This is different from the 'armchair' or introspective theory of the proponents of pure random walk models, who start from the hypothesis that prices, originally of commodities, ought to take into account all known facts about the commodity, so that prices only alter when there is new 'news'. Thus prices, perhaps deflated by some risk free accumulator, can be treated as martingales.

Allow me to explain some aspects of my theory: in my model for inflation I make the rate of inflation in one year depend on the rate of inflation in the preceding year. This has a much bigger effect when the rate of inflation is a long way away from its assumed mean level, as it was particularly in the late 1970s, than when inflation is near its mean level, as, perhaps, it is now. When the rate of inflation was high, one could observe those negotiating wages arguing that the particular group of workers they represented should get: (a) a wage increase somewhat above last year's price inflation; and (b) at least as much as 'the going rate' for wage increases for other workers. In both cases the annual rate of price or wage inflation would be quoted. Companies considering price increases for their products looked at the rate of price inflation in the preceding year, and the price increase of their competitors' products, year on year. This sort of behaviour gives a good reason for price inflation (looked at on its own) to behave in the way I represent in my model, and for prices and wages to behave either as a VAR (vector autoregressive) model or as a transfer function model, both of which I describe in Wilkie (1995).

Huber (1997) points out that, when one takes the data from 1923 to, say, 1970, the autoregressive parameter is smaller than the 0.6 or so that I found using also the more recent data. But again, one needs to consider the environment within which people were working at that time. Prices rose sharply during the 1914-18 war because of shortages, and fell again in the years after the war. Prices rose, but to a lesser extent, during the 1939-45 war, but did not fall again afterwards, although many people at the time expected them to. Then, during the 1950s and 1960s, the rate of inflation fluctuated rather little. 'The going rate' was not a concept that applied strongly at that time.

Further, in an environment when inflation has happened to be reasonably close to some mean value, the scatter of observations is much more likely to appear independent from year to year than to look highly correlated. It is only when more extreme values occur that the autoregressive pull back to the mean is observed.

Consider now my model for share prices, which I split into a model for dividends and a model for dividend yields. I suggest, in Wilkie (1995), that earnings and price/earnings (P/E) ratios would provide another approach. I observe that many investors assess shares of individual companies in terms of P/E ratios. I am not the only one at present who considers that the extremely high P/E ratios on United Kingdom and more especially United States shares indicate that prices are probably 'too high', and might well fall. I also observe that property (real estate) prices are often assessed in terms of so many 'years' purchase', i.e. as a multiple of the (rack) rental on the property. The prices paid for small businesses and professional firms are also often assessed on a years' purchase basis.

On the other hand, the prices of the shares of some companies, such as investment trusts and property companies, are clearly assessed in terms of the net asset value of the company. I understand that mineral resource companies are also often assessed in this way.

All of these observations make it reasonable to assume that the prices of shares of individual companies are based on 'fundamentals' like the dividends, earnings and net asset value per share; and it is also reasonable to assume that the prices of shares collectively also pay attention to these fundamentals. My use of dividend yield is a proxy for a more elaborate assessment, bringing in the other elements. However, when I started work on the model in the late 1970s there were no figures for earnings in the U.K. except on the then 500 Share Index (now the Non-Financials Index) from 1962 onwards. Now there is a much longer run of values for earnings, and there is a much longer run in the U.S.A., but there are still no figures for net asset value per share on any share indices that I know of.

Dividends, like earnings and net asset values, clearly depend, in some way, on price inflation, but can also drift away from strict dependence on a retail prices index. Little & Raynor (1966) discussed 'higgledy-piggledy growth', the concept that the earnings (and hence dividends) of individual companies changed from year to year in a random walk style. This is quite plausible, and it is precisely what I represent in my dividend model, with a dependence on past inflation (observe that dividends are paid with a delay from the time profits are earned), and an underlying modified random walk.

Consider now interest rates: Homer & Sylla (1996) demonstrate that interest rates have fluctuated around the same sort of levels for the past 4,000 years. Fisher (1907) observed that interest rates were higher when inflation was higher (even though it was not nearly so high in the period that he was observing as it has been more recently), and the existence of index-linked government stocks has emphasised to all those in the gilt market the concepts of nominal yields, real yields and an implied inflation rate. My model is based on these concepts.

I said, above, that my views had changed slightly over the years. I now consider it important that the model should have the possibility of representing short-term fluctuations as well as annual ones, and that it is nice if it can have a continuous form representation which is consistent with the discrete one. For this reason I like AR(1) models, which are consistent with the continuous Ornstein-Uhlenbeck model. In Wilkie (1995) I dropped the AR(3) representation for Consols yields that appeared in Wilkie (1986). However, the inflation model, necessarily, in my view, must look at inflation over the preceding year (and not over some shorter period), so it cannot be made into a simple continuous time Markov model; and, in any case, values of the Retail Prices Index in the U.K. are known only monthly, so a continuous model is not necessary.

Huber & Verrall (¶1.1.4) suggest that my treatment of economic theory has been inconsistent. They say: "The efficient market hypothesis was rejected on empirical grounds (see Ford *et al.*, 1980), but the purchasing power parity hypothesis was incorporated in the model despite the empirical evidence (see Wilkie, 1995)." This is misleading in both cases.

The pure random walk model for share prices, with no regard to the level of dividends (or other fundamentals) can be rejected on theoretical grounds, because it is: (a) unreasonable to imagine that shares in aggregate would (in normal circumstances) trade at extremely low or extremely high multiples of dividends (or earnings); and (b) one can observe that many investors do take the fundamentals into account. What is interesting is that dividend yields appear to fluctuate more than can be justified by changing expectations about dividends, but some authors have started to look at this problem, and also the way in which 'bubbles' (rational or irrational) may be modelled (see e.g. Campbell, Lo & McKinlay, 1997, Chapter 7).

The purchasing power parity model (PPP) for exchange rates is also much more plausible than the idea of a random walk, which disregards the levels of prices in the countries involved. It is possible to observe that prices of goods and services in developed countries are at similar levels, though, at any one time, some countries may seem relatively dear and others relatively cheap, but the notion that prices in one developed country could be many times those in another country is unimaginable. It would be better, perhaps, to use an index of the prices of traded goods and services, but the consumer price index is available and is a reasonable substitute. I speak of developed countries. It seems to me quite possible that retail prices in, say, Russia might behave quite differently from the prices of their exports, and that the exchange rate might behave quite differently from either; but that is a special case.

It is not true that the empirical evidence contradicts the purchasing power parity hypothesis. This depends on how, for example, unit root tests are interpreted. A common way in which such tests are used is to set up the hypothesis H_0, that a series has a unit root, i.e. is an $I(1)$ or integrated series, or, in effect, a modified random walk. A test such as the Dicky-Fuller test can say whether, for the data in question, this hypothesis is likely to be false, but it cannot say that the hypothesis is true. If there are good reasons, from first principles, to assume that the series is, in fact, stationary (or an $I(0)$ series), then the Dicky-Fuller test is of no use. It is impossible to distinguish between for example a random walk:

$$X(t) = X(t-1) + e(t)$$

and an autoregressive AR(1) model:

$$X(t) = aX(t-1) + e(t)$$

if the value of a is sufficiently close to 1. It does not matter how many observations you have; the more observations you have the narrower the likely range of values of a, but, if that range includes $a = 1$, you still cannot distinguish between $a = 1$ (the random walk) and $a < 1$ (a stationary series). And, however close a is to 1, the resulting model is qualitatively different from a random walk.

Actuaries will appreciate an analogy. If the rate of interest is positive the value of a perpetuity is finite; if the rate of interest is very small the value may be large, but it is still finite. Only when the rate of interest is taken as zero does the value of a perpetuity become infinite.

In fact, I find that, for the exchange rate model, the value of a is about 0.7 (per year) for many exchange rates. I point out (Wilkie, 1995, ¶10.3.4) that a value of 0.7 per year is equivalent to $0.7^{\frac{1}{12}} = 0.97$ per month, and one can add that $0.7^{\frac{1}{365}} = 0.9990$ per day, which is very close to 1, so that those who have looked at exchange rates on a daily basis and accepted the random walk hypothesis may have been misleading themselves. However, my PPP model is both theoretically reasonable and also empirically justified.

Huber & Verrall (¶2.3.3) are rightly critical of data-mining, i.e. searching for some model that fits the data well, however implausible the result in economic terms. However, Ford *et al*. (1980) came to the same conclusion. It is misleading of Huber & Verrall to say that the Maturity Guarantees Working Party considered over 400 models, without adding that these were applied to 95 different series, i.e. only 4 or 5 models per series. After these many investigations, the Maturity Guarantees Working Party nevertheless decided against this approach, rejected the work done by Godolphin (see Appendix C), and chose what was the simplest economically sensible model to represent the data that they were dealing with, *viz.* a random walk for dividends and an AR(1) model for dividend yields.

I am not clear whether Huber & Verrall consider that data-based and theory-based models are equally good or equally poor. My view is the latter; only a model that is both consistent with a suitable theoretical approach and is also justified by the data is likely to be satisfactory; a model that fails one of these requirements is unsatisfactory. Also, some models fail both tests, being driven apparently by mathematical convenience and little else. Such models may teach us something, perhaps quite a lot, but they are only a step along the road, not an end point.

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