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The work of science is to substitute facts for appearances and demonstrations for impressions.—RUSKIN

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my comments and any subsequent remarks that Professor Milgrom might add will help clarify the points already presented.

Again, consider a yield curve $\delta(t) = \delta + \Delta(t)$, where $\Delta(t)$ is a shape component and δ establishes the overall level of interest rates. Now, let $v(t) = \exp\{-\int_0^t \delta(s)\,ds\}$, the discount factor generated by such a yield curve.

If it is assumed that only δ varies in the short run while $\Delta(t)$ remains fixed, then arbitrage opportunities will always exist. To see this, consider the following construction.

Given $L = (L_1, L_2, \ldots, L_n)$, structure $A = (A_1, A_2, \ldots, A_n)$ in the following manner. For some arbitrary k such that $1 < k < n$, define

$$A_i = \begin{cases} L_i\,, & i \neq k-1, k, k+1 \\ L_i - 1, & i = k \\ L_i + v(k)/2v(k-1), & i = k-1 \\ L_i + v(k)/2v(k+1), & i = k+1. \end{cases}$$

In other words, use exact matching to support all but \$1 of the liability stream. For the remaining \$1, purchase assets of equal present value maturing one period prior and one period after the duration at which this \$1 is due to be paid. It is easily shown that the asset and liability structures have equal present values and durations. Furthermore, the second derivative of the assets with regard to changes in δ is greater than that for the liabilities.

$$\begin{aligned} PV(A-L,\delta) &= \{v(k)/2v(k-1)\}v(k-1) - \{1\}v(k) \\ &\quad + \{v(k)/2v(k+1)\}v(k+1) \\ &= 0 \\ -dPV(A-L,\delta)/d\delta &= \{v(k)/2v(k-1)\}(k-1)v(k-1) - \{1\}kv(k) \\ &\quad + \{v(k)/2v(k+1)\}(k+1)v(k+1) \\ &= 0 \\ d^2PV(A-L,\delta)/d\delta^2 &= \{v(k)/2v(k-1)\}(k-1)^2v(k-1) - \{1\}k^2v(k) \\ &\quad + \{v(k)/2v(k+1)\}(k+1)^2v(k+1) \\ &= v(k)\,\{k^2 - 2k + 1 - 2k^2 + k^2 + 2k + 1\}/2 \\ &= v(k) > 0 \end{aligned}$$

Therefore, the present value of the assets will be greater than or equal to the present value of the liabilities for δ sufficiently close to the current value of δ. Since interest rates are almost certain to vary, an arbitrage opportunity ... That is, the preceding technique would seem to be a fool-proof recipe ...ing surplus.

...bitrage hypothesis argues that such a technique may not survive ...market. Consequently, there must be something wrong with ... Professor Milgrom contends in section III that flat yield ...) produce arbitrage potential. This is certainly sup... ...mple. However, Professor Milgrom goes on in a

footnote to state that a linear form for $\Delta(t)$ may be constructed which is consistent with the no-arbitrage hypothesis. I find this difficult to reconcile with the previous findings, which apply to any reasonable form for $\Delta(t)$. Perhaps the author could help explain this apparent inconsistency in his response to the discussions.

What is wrong with the previous analysis? The only significant assumption required is the constancy of $\Delta(t)$ as δ varies. At this point, I can only conclude that in a market in which arbitrage opportunities are nonexistent, $\Delta(t)$ must vary as well as δ. As Professor Milgrom points out, unless we have some knowledge about feasible variations in $\Delta(t)$ and δ, we cannot hope to develop a practical measure of the interest rate risk. The author's two-factor yield structure and the associated first order sensitivity indexes are certainly more palatable than the overly simplistic counterparts in the traditional Macaulay-Redington model. With these points in mind, I tend to agree with Professor Milgrom's contention that the various immunization factors presented in the paper are not reliable measures of the total exposure to the risk of varying future interest rates but are more appropriately used as sensitivity indexes for a variety of speculated types of interest variations.

(AUTHOR'S REVIEW OF DISCUSSION)

PAUL R. MILGROM:

I would like to thank all the discussants for their comments, which serve to expand, elucidate, and in one case correct the points made in my paper. It was especially gratifying to find so much agreement—or at least so little disagreement—with the economic approach to present value theory that I described,[1] even though that theory diverges importantly from the traditional actuarial approach.

The discussion by Messrs. Buff and Lord brings out practically some of the issues I explored abstractly. The discussants focus their comments on the kinds of errors that are frequently made in evaluating the status and vulnerability of financial security plans. Their concrete listing of the dangers of using book values in place of market values for evaluating C-3 risk are well worth studying. But this listing of dangers is not their only contribution. They also highlight the problems that arise when the cash flows to be immunized may themselves be sensitive to interest rates. That is a problem of enormous practical importance not only for managing portfolios and measuring the interest rate risk, but also for the valuation of liabilities. For valuations, the difficulty is that the cost of a portfolio that matches the cash-flow pattern of the liability will generally depend on the interest sensitivity of the cash flows.

[1] This approach was first developed more than half a century ago by the American economist Irving Fisher. It has since become the standard approach used in textbooks on microeconomics.

It is important to recognize that the difficulty in valuing and immunizing interest-sensitive cash flows is not one of principle. The V_f indexes can (in principle) be computed exactly as specified in my paper regardless of the interest-sensitivity of the cash flows. The real difficulty is that there is little precise information about how the cash flows of even such major classes of financial security plans as life insurance and pensions actually respond to shifting interest rates. Failure to account for interest sensitivities introduces biases into the index calculations. For example, if annuity holders delay making withdrawals in order to take advantage of interest guarantees when market interest rates are below the contractually guaranteed rates, then falling interest rates increase liabilities more than otherwise; the index *understates* the true sensitivity of the present value of annuity payments to changing interest rates.

General statements about the biases introduced by interest-sensitive cash flows are hard to make because so little is known about how people actually respond to changing interest rates. Research shedding light on this matter would be of great value.

Four of the discussants, Messrs. Levin, Shiu, Clancy, and Wurzburger, have spotted an error in my treatment of second derivatives of the present-value function. (They also provide a formal mathematical treatment, using the Ito stochastic calculus, of some of the same issues I treated.) I had mistakenly argued on the last page of section III that if "*A* and *L* have equal present values and equal vulnerabilities" then the second derivatives of the present-value functions $PV(A; I)$ and $PV(L; I)$ with respect to the interest rate indexes must also be equal. I relied on this statement to reach the incorrect conclusion that frequent portfolio rebalancing is not critical for approximate immunization.

The correct argument, rendered here in plain English, goes as follows. If *A* and *L* have equal present values and vulnerabilities (evaluated at market interest rates), then the random variations in their returns[2] over the next short interval of time will be equal—that is how the vulnerability indexes were constructed. Then, if the mean returns over the next short time interval for *A* and *L* are not equal, the stream with the higher mean will *always have the higher return* over the next short interval of time so that there is an arbitrage opportunity. Therefore, assuming the no-arbitrage hypothesis, equalizing vulnerability measures assures that the two streams have precisely equal returns over the next short interval.[3] This argument confirms that, as I had claimed, *only first derivative measures of vulnerability are necessary for immunization if portfolios can be rebalanced sufficiently frequently*. And, importantly, for nonimmunized portfolios, these same vulnerability measures correctly reflect the risk the portfolio faces in the near term due to shifting interest rates.

[2] Around, say, the mean return.

[3] Those facile with the Ito calculus can check the corresponding formal steps of this argument as follows. Equal vulnerabilities and present values imply that the diffusion coefficients of the values of *A* and *L* are equal. Then the no-arbitrage hypothesis implies that the drift coefficients are also equal. For, otherwise, the investment portfolio corresponding to $A - L$ would have zero net cost, non-zero drift, and zero diffusion coefficient; that is, it would be an arbitrage portfolio.

The foregoing argument is not inconsistent with the argument advanced by several discussants that convexity (second derivative based) measures have a role to play in immunization theory. To maintain effective immunization over time without frequent (and expensive) rebalancing, one needs to arrange the portfolio so that the vulnerabilities of *A* and *L* remain nearly equal as time passes and interest rates change. One does this by equating the derivatives of the vulnerability measures with respect to time and the interest rate indexes. The latter involves second derivatives of the present-value function. Mr. Levin makes precisely this point when he says that convexity can be viewed as the "extent to which duration changes with a change in yield."

The upshot of the foregoing analysis is that *using both first and second derivative based measures for immunization reduces the need for frequent portfolio rebalancing*. For portfolios that are not fully immunized because immunization is but one of several objectives and for which vulnerability measures are needed, the indexes derived from first derivatives of the present-value function are probably adequate.

Two of the discussions, those of Mr. Shiu and of Messrs. Buff and Lord, assess the success of the Macaulay-Redington index in practice. While Messrs. Buff and Lord are optimistic about the value of the Macaulay-Redington index (arguing that immunization using that index is, as a practical matter, effective), Mr. Shiu cites a study by my colleague, Jon Ingersoll, who finds in a study of managed portfolios that the immunization using the Macaulay-Redington duration measure is ineffectual for eliminating the interest rate risk.

Given the strength of the theoretical arguments against the Macaulay-Redington duration index, Ingersoll's empirical findings are as expected. Certainly, no well-trained actuary would expect a single mortality "level" index to capture the subtle shifts in age/sex-specific mortality changes that occur over time and are important to insurance pricing; that is why detailed mortality studies are continually conducted by the life insurance industry. Imagine how much less satisfactory a single mortality index would be if the highest mortality rates occurred sometimes at the young ages and sometimes at the old, and "overall" rates were sometimes rising and sometimes falling! That is precisely the case for interest rates: short-term rates are sometimes higher and sometimes lower than long-term rates, and the whole twisting structure rises and falls quite suddenly. Just as mortality rates are key for all life insurance pricing, interest rates are key to bond pricing and liability valuation. Any sensible person can see that a single index based on the overall "level" of interest rates cannot represent the shifts of this kind adequately for liability valuation.

As I have emphasized in my paper, measures of risk are based on theories of interest rate movements, that is, theories that impose some restrictions on the term structure of interest rates and how it may change over time. The Macaulay-Redington duration measure is based on the theory that interest rates at all durations move up and down together equally and in unison. That model errs substantially in its description of actual interest rate movements. Shifts in the shape of the yield curve can and sometimes do have large effects on the solvency of financial security plans, even when the Macaulay-Redington duration measure says the risk is zero. Empirical studies normally operate by averaging the performance of the Macaulay-Redington index over those times when it works well and those when it fails. For a decision maker, the most telling point to emerge from the data is that the Macaulay-Redington index is unreliable, and it is most unreliable precisely when it is most needed, which is when the cash-flow streams of assets and liabilities are far from being matched. The favorable empirical studies, which average the good performance of Macaulay-Redington-immunized portfolios when the streams are well matched with the bad performance of Macaulay-Redington-immunized portfolios which are not well matched, are cold comfort to a manager who needs a reliable guide to measuring risks.

Mr. Christian's main criticism of my article is founded on his view that the no-arbitrage hypothesis is just a first order approximation and that any worthy financial institution ought to be looking for arbitrage opportunities to exploit. He then hints that such an institution ought to be suspicious of a risk measure based on the no-arbitrage hypothesis, such as any of the measures I have mentioned. However, Mr. Christian's argument does not justify the hinted conclusion.

In saying that the no-arbitrage hypothesis is a "first approximation," one might sensibly mean that financial institutions, from time to time, can identify arbitrage opportunities but not on so large a scale as to limit the institution's ability to finance other investments. Then, surprisingly, the economic theory of market valuation I have described applies exactly; that *is*, the economic value of a cash-flow stream is its present value computed using the interest rates implicit in the bond prices of the arbitrage-free portion of the market. Indeed, nothing in the argument given in my paper is affected by the presence of the limited arbitrage opportunities described previously: It is still true that an asset is worth no more or less than the cost of duplicating its cash flows by trading in liquid bonds. If the asset is cheaper than the corresponding bonds, one would still want to sell the bonds and make the

investment. If the investment were dearer, it would still be better to buy the bonds.

This is not to say that the economic valuation theory described in my paper is exactly right for the world we actually live in, with its illiquidities, tax consequences of trading, and interest-sensitive cash flows. My point is much more limited in that the no-arbitrage hypothesis need not hold exactly in order for the theory and its associated risk measures to be useful. Indeed, as we have seen, the theory is unchanged if one weakens the no-arbitrage hypothesis to allow limited arbitrage opportunities to arise from time to time.

Mr. Eckley criticizes the use of immunization theories and measures in principle, since they depend on parametric assumptions about the yield curve—assumptions which are only approximations, and possibly poor ones. I have already emphasized that these theories and measures do depend on assumptions about the shape of the yield curve. There are both theoretical and empirical reasons to believe that the yield curve does have some structure, although that does not guarantee that we will be able to isolate the structure in a useful way. Mr. Eckley's cautions are well-warranted.

Nevertheless, the only proper reason to eschew the use of all parametric yield curves is that one can do as well without them. Indeed, as Mr. Eckley argues, there may be some situations in which matching is perfectly appropriate and immunization indexes are dispensable. I suspect, however, that actuaries more often find matching strategies to be inadequate. How should an actuary advise a client who rejects matching because immunization is but one of his several objectives? Should he eschew all vulnerability measures because they are imperfect? How should managers who would like to approach perfect immunization proceed when the cash flows to be immunized are interest sensitive? Matching in such a case may be impossible, and then immunization using measured vulnerabilities is *presently* the only alternative.

I have emphasized the word *presently*, because it is certainly conceivable that actuaries sharing Mr. Eckley's views could develop "theory-free" measures of vulnerability that expand the domain of matching ideas. For example, one might say that the *M*-vulnerability index (*M* for matching) is defined as the maximum, over all nonnegative, year-by-year interest rates, of the excess in present value of the liability stream over the asset stream. For perfectly matched streams, the measure is zero. For other streams, it represents the worst-case loss from shifting interest rates. It is my guess that worst-case indexes will never be as useful as the kinds of indexes described in my paper: The worst case described is quite extreme, and attempts to account for more realistic variations in interest rates amount to reintroducing an interest rate theory through the back door. However, the matter is still far from resolved.

Mr. Robinson's discussion proves the claim in my paper that the Macaulay-Redington flat curve theory of interest rates contradicts the no-arbitrage condition. He does this by showing how one can create a spread which, with certainty, performs better than a given original portfolio if the yield curve is always flat. He then further argues, mistakenly, that the same spread rules out all theories of the term structure of interest rates in which the continuously compounded rate of interest rate at date t has the rigid form:

$$\delta(t) = \hat{\delta} + \Delta(t),$$

where $\hat{\delta}$ is subject to stochastic changes. However, when $\Delta(t)$ is not flat, the original and spread portfolios have different yields, and the inferior convexity of the original portfolio may be compensated by its higher yield. When $\Delta(t)$ is chosen exactly to compensate for differences in convexity, there is no arbitrage opportunity here, contrary to Mr. Robinson's assertion.

I have tried to keep my review of the discussants' remarks reasonably brief and free of complicated mathematical arguments. I hope that readers fluent in the stochastic calculus will forgive me for the consequent lack of rigor, and that the discussants who used these techniques will forgive me for my necessarily incomplete review of their comments. My choices have been guided by a desire to communicate the principal, relevant, economic ideas to the widest possible actuarial audience. I will be most gratified if actuaries who have not mastered economics, finance, and the Ito stochastic calculus find my paper and review of the discussions to be illuminating or, at least, stimulating.

UNITED STATES LIFE TABLES FOR 1979–81

ROBERT J. MYERS AND FRANCISCO R. BAYO

ABSTRACT

This paper presents age-specific mortality rates and expectations of life for the official decennial United States Life Tables for 1979–81. Analysis of these data shows trends and relationships by age, sex, and color. As in the past, mortality rates for males were higher than for females at all ages, especially at the young-adult ages. Mortality rates for other-than-white individuals were significantly higher than for white persons at all ages except the very highest ages, with the differential being the largest in the 30s and 40s.

The paper examines mortality trends since the first decennial life tables were prepared at the beginning of this century. Considerable reductions in mortality are shown for all categories. Until the last decade, the reductions were much larger for the younger and middle-aged persons than for older persons, but in the last decade, all age groups have had about the same relative reductions. Females have always had lower mortality rates than males (with certain minor exceptions at the young-adult ages many years ago), and the relative differential has been steadily increasing. Mortality rates for other-than-white persons have always been significantly higher than those of white persons, except at the very highest ages. However, this differential is decreasing (as is the opposite differential that exists at the highest ages).

The paper briefly compares U.S. mortality with that in selected industrialized countries. Although infant mortality rates in the U.S. decreased significantly (by 37 percent) from 1970 to 1980, the infant mortality rate in the U.S. is still somewhat higher than in most other industrialized countries (it decreased in those countries also). Similarly, the expectation of life at birth in the U.S. is generally at or below (usually by no more than 2–3 percent) the level found in other industrialized countries. U.S. females have a similar life expectancy to women in other industrialized countries, while U.S. males have a slightly lower life expectancy than do other men. However, the expectation of life for males at age 65 is about as high in the U.S. as in any other country, and for females, it is as high or higher than in any other country.