Paradise Lost and Found? The Econometric Contributions
of Clive W.J. Granger and Robert F. Engle

by

Peter Hans Matthew

September 2004

MIDDLEBURY COLLEGE ECONOMICS DISCUSSION PAPER NO. 04-16

DEPARTMENT OF ECONOMICS
MIDDLEBURY COLLEGE
MIDDLEBURY, VERMONT 05753

http://www.middlebury.edu/~econ

Paradise Lost and Found? The Econometric Contributions of Clive W. J. Granger and Robert F. Engle

Peter Hans Matthews
Department of Economics
Munroe Hall
Middlebury College
Middlebury Vermont 05753
peter.h.matthews@middlebury.edu

September 12, 2004

Abstract

This paper provides a non-technical and illustrated introduction to the econometric contributions of the 2003 Nobel Prize winners, Robert Engle and Clive Granger, with special emphasis on their implications for heterodox economists.

Keywords: ARCH, GARCH, cointegration, error correction model, general-to-specific

JEL Codes: B23, B40, C22

1 Introduction[1]

The 2003 Bank of Sweden Nobel Prize in Economic Science was awarded to Clive W. J. Granger, Professor Emeritus of Economics at the University of California at San Diego, and Robert F. Engle, the Michael Armelinno Professor of Finance at the Stern School at New York University, for their contributions to time series econometrics. In its official announcement, the Royal Swedish Academy of Sciences cited Granger's work on "common trends" or cointegration and Engle's on "time-varying volatility," but this understates their influence, both on the broader profession and, in this particular case, on each other.

Much of the research that the Royal Academy cited, not least their co-authored papers on cointegration (Engle & Granger, 1987) and "long memory processes" (Ding, Granger & Engle,1993), has been published over the last two decades, but even before then, both laureates were well known for their other contributions to econometrics. Granger's (1969) eponymous causality test and the results of his Monte Carlo studies with Paul Newbold on spurious regression (Granger & Newbold, 1974) were already (and still are) part of most economists' tool kits, Engle's (1974a) research on urban economics was familiar to specialists, and both (Granger & Hatanaka, 1964, Engle, 1973) were pioneers in the application of spectral methods to economic data. And both have pursued other, if related, research avenues since then: Granger continues to build on his earlier research on nonlinear models (Granger & Anderson, 1978), forecasting (Granger & Bates, 1969) and long memory models (Granger & Joyeux, 1980), and in addition to his seminal paper on the concept of exogeneity (Engle, Hendry & Richard, 1983), Engle has become the preeminent practitioner of the "new financial econometrics" and its practical applications, like value at risk (Engle & Manganelli, 2001) and options pricing (Engle & Rosenberg, 1995).

For more than two decades from the mid-1970s until the late 1990s, Engle and Granger were colleagues at the University of California at San Diego, during

[1] I thank Carolyn Craven and one of the editors for their comments on an earlier draft. The usual disclaimers hold.

which time its Economics Department joined LSE and Yale as the most productive centers of econometric research in the world. The paths that took them to UCSD were quite different, however. Clive Granger was born in Swansea, Wales, in 1934, and completed both his undergraduate (B.A. 1955) and graduate (Ph.D. 1959) degrees in statistics at the University of Nottingham, where he remained to research and teach, as a member of the Mathematics Department, until 1973. In an interview with Peter C. B. Phillips (1997, p. 257), he recalls that when he first started to reach, "I knew all about Borel sets ... but I did not know how to form a variance from data ... so I had to learn real statistics as I went along." But as someone who was from the start interested in the application of statistical methods, he benefitted from his position as the lone "official statistician" on campus:

> Faculty from all kinds of areas would come to me with their statistical problems. I would have people from the History Department, the English Department, Chemistry, Psychology, and it was terrific training for a young statistician to be given data from all kinds of different places and be asked to help analyze it. I learned a lot, just from being forced to read things and think about ... diverse types of problems with different kinds of data sets. I think that now people, on the whole, do not get that kind of training. (Phillips, 1997, p. 258)

Some of his earliest publications reveal this breadth: in addition to his first papers in economics, there are papers on (real) sunspots (Granger, 1957), tidal river floods (Granger, 1959) and personality disorders (Granger, Craft & Stephenson, 1964)! By the time Granger had moved to southern California, in 1974, his reputation as an innovative econometrician with a preference for "empirical relevance" over "mathematical niceties" (Phillips, 1997, p. 254) was well-established.

Robert Engle was born almost a decade later, in 1942, in Syracuse, New York, and received a B.Sc. from Williams in physics in 1964. He continued these studies at a superconductivity lab at Cornell but switched to economics after one year, earning his Ph.D. in 1969. In a recent interview with Francis Diebold (2003), he re‡ected brie‡y on this transformation. When Diebold (2003, p. 1161) observes that Engle is one of several prominent econometricians - he mentions John Cochrane, Joel Horowitz, Glenn Rudebusch, James Stock and another Nobel laureate, Daniel McFadden - with a physics background, Engle responds that "physicists are continually worried about integrating theory and data, and that's why ... physicists tend to make good econometricians [since] that's what econometricians do." So far, so good. But when he later recounts his job interviews at Yale and MIT, he speculates that "one of things that impressed them was that I knew things from my physics background that had been useful in analyzing this time aggregation problem, like contour integrals and stu¤ like that, and they thought 'Oh, anyone who can do that can probably do something else!'" (Diebold 2003, p. 1164). To be fair, and as this remark hints, his dissertation, written under the supervision of T. C. Liu, tackled an important econometric problem, namely the relationship between the frequency of economic data and model speci…cation, and lead to his …rst professional publication (Engle & Liu, 1972). Furthermore, a number of the tools and ideas would later become relevant for his research on cointegration. After six years at MIT, where he found a climate that was "inhospitable in an intellectual sense for the time-series people" (Diebold 2003, p. 1166) - one immediate consequence of which was an impressive series of papers (Engle, 1974a, 1974b, 1976, and Engle, Bradbury, Irvine & Rothenberg, 1977, for example) in urban economics - he moved to UCSD in 1974.

The new econometricians at UCSD - another theorist, Halbert White, was hired soon after, for example - found themselves in a world where the presumptions of classical statistics - stationary (constant mean and …nite variance) random variables with "thin-tailed" (normal, for example) distributions - were often implausible and the usual remedies had become suspect. In short, paradise lost.

As the next section describes, the development of autoregressive conditional heteroscedastic (ARCH) models, and the extensions it soon inspired - GARCH, ARCH-M, IGARCH, EGARCH, FIGARCH and TARCH, to mention just a few! - allowed researchers to model the otherwise unexplained variability of some time series in a more systematic fashion and, in the process, to represent (at least) two common properties of numerous series, clustered volatility and the leptokurtosis (or fat tailedness) of unconditional distributions. The third section considers Granger's radical reformulation of the old nonsense correlation problem, a consequence of the unre‡ective use of non-stationary data, and his new solution of it - paradise found? - based on the concept of cointegration. Because Granger's name will (also) be forever linked to the problem of causality - even if this was not the reason he was awarded the Prize - the fourth section provides a brief but critical review of this literature, as well as some of the laureates' other contributions. The …fth section then provides the rationale for the question mark in the title: it re‡ects on several practical and methodological criticisms of the "revolution" in time series econometrics, with emphasis on the particular concerns of heterodox economists.

2 Paradise Lost and Found, Part I

Consider the annual rate of return on the American stock market from 1871 to 2002, as reported in Shiller (2003). As the histogram in Figure 1 reveals, the distribution of returns is fatter tailed than the (superimposed) normal distribution or, as Mandelbrot (1963) was one of the …rst to appreciate, "extreme values" are more common in economics than in nature. (Inasmuch as Mandelbrot and his intellectual heirs have been most interested in …nancial market data, this seems an appropriate point of departure.) The plot of the squares of these returns in Figure 2 also hints, however, that if one were to model these returns as independent draws from some leptokurtic distribution, important information would be lost. In particular, it seems that extreme returns were more common when returns in the previous period(s) were also extreme, a phenomenon known as volatility clustering. One reason for the immediate success of ARCH models, first introduced in Engle (1982), was their consistency with both features.

[Insert Figures 1 and 2 About Here]

To illustrate, consider another sort of time series, an important but understudied element of labor market behavior, the recipient rate for unemployment insurance (UI) in the United States between 1950 and 1997, as depicted in Figure 3. The recipient rate, the ratio of insured to total unemployment, is an imperfect measure of UI utilization: while the collection rate and fraction of insured unemployment (Blank & Card 1991) are perhaps more intuitive measures, both are more difficult to measure.[2] As Figure 3 shows, this rate peaked, at close to 100 percent, in the early 1950s, but has fallen, more or less steadily, ever since, with two sharp declines in the mid 1960s and early 1980s. At the end of the sample period, it stood at 39 percent, close to its nadir of 36 percent in 1984.

[Insert Figure 3 About Here]

The simplest dynamic model of the recipient rate would perhaps assume that its difference from one year to the next, ΔREC_t, was equal to:

$$\Delta REC_t = \beta_0 + \sum_{j=1}^{p} \beta_j \Delta REC_{t-j} + u_t \tag{1}$$

where u_t is some mean zero, constant variance error or "innovation." Under this specification, the expected annual change in the recipient rate will be $\beta_0/(1 - \sum_{j=1}^{p} \beta_j)$ over the long run, but the expected change in any one year, conditional on the history of past changes, can be higher or lower than this. The standard specification tests reveal $p = 3$ to be a reasonable choice, and least squares estimation of (1) produces:

[2] The insured unemployment rate is itself defined to be the ratio of UI claims to covered employment, but not all workers are covered and not all claims are honored. Inasmuch as both the proportion of covered workers and the disqualification rates have varied over time, fluctuations in the recipient rate must therefore be interpreted with some care.

$$\Delta REC_t = \underset{(1.02)}{-2.38} - \underset{(0.15)}{0.33}\,\Delta REC_{t-1} - \underset{(0.14)}{0.42}\,\Delta REC_{t-2} - \underset{(0.13)}{0.35}\,\Delta REC_{t-3} + \hat{u}_t \qquad (2)$$

$$R^2 = 0.27 \qquad BP(1) = 0.58 \qquad BP(2) = 1.80$$

where the numbers in parantheses are standard errors and $BP(x)$ are Breusch-Godfrey test statistics, which indicate that the null hypothesis of no serial correlation cannot be rejected at even the 10% level. The estimated annual decrease in the recipient rate in the long run is 1.13 percentage points, which exceeds the sample mean of 0.98, but as the comparison of actual differences and (one) step ahead forecasts in Figure 4 hints, the information contained in past differences could be useful to those who, for example, set UI premia.

[Insert Figure 4 About Here]

It is not clear, however, how confident policy-makers should be about these forecasts, even if the simple model that produced them is believed to be "sensible." The reason for this is that the standard confidence intervals for such forecasts assume that both the conditional and unconditional variances of the error term u_t are constant and equal. And while applied econometricians have known for decades how to incorporate some forms of heteroscedasticty into their models, none of the proposed variations systematically captured the intuition of most policy-makers that (a) some periods are more turbulent than others and (b) in such periods, forecast confidence is diminished.

To see whether UI claim behavior exhibits such turbulence, consider the behavior of the squared residuals $\hat{u}_t^2$ from this model over time, as depicted in Figure 5. If clusters were present, these squared residuals should persistently exceed their sample mean, the horizontal line in the same diagram, for sustained periods of time. With the exception of a run of three years in the late 1950s - which comes as a surprise inasmuch as this has never been considered a turbulent period from the perspective of the UI program - there is little evidence of this.

[Insert Figure 5 About Here]

A more formal test of whether or not the introduction of ARCH effects is warranted was first proposed in Engle (1982): in an auxiliary regression of the squared residuals on a constant and q lags of themselves, the product of the sample size T and R^2 will be distributed $\chi^2(q)$ under the null hypothesis of no ARCH effects. Consistent with Figure 5, the null cannot be rejected at the 10% level - or for that matter, the 50% level - in this model.

In this context, it is still useful to understand how such effects could be incorporated into the model. In the spirit of Engle (1982), suppose the error term u_t is equal to $v_t\sqrt{\delta_o + \delta_1 u_{t-1}^2}$, where v_t is an independent and identically distributed (iid) random variable with mean 0 and variance 1, and the parameters are restricted such that $\delta_o > 0$ and $0 \leq \delta_1 < 1$. It is not difficult to show that the unconditional variance of u_t will be constant and equal to $\delta_o/(1-\delta_1)$ - the reason for the restrictions on δ_o and δ_1 - but that, conditional on u_{t-1}^2, the variance of $u_{t+k},\ k = 0, 1, ..,$ is $\delta_o(1 + \delta_1 + \delta_1^2 + ... + \delta_1^{k-2}) + \delta_1^{k-1}u_{t-1}^2$. Furthermore, the kurtosis of the (unconditional) distribution of innovations is $3(1-\delta_1^2)/(1-3\delta_1^2)$ which exceeds that of the normal (which is 3) if $\delta_1 \geq 0$, and is therefore consistent with "fat tails." This is the $ARCH(1)$ model but in most applications with high(er) frequency data, a natural extension of this model, $ARCH(p)$, is needed, in which $u_t = v_t\sqrt{\delta_o + \delta_1 u_{t-1}^2 + \delta_2 u_{t-2}^2 + ... + \delta_p u_{t-p}^2}$.

In his first illustration of the concept, for example, Engle (1982) fit an $ARCH(4)$ to a similar (autoregressive, that is) model of quarterly inflation rates in the UK and soon after, Engle & Kraft (1983) would conclude that an $ARCH(8)$ model was needed to represent clustered volaility in quarterly US inflation rates. And while other macroeconomic applications soon followed, it had become clear, Diebold (2004) believes, that evidence of ARCH effects in macro data was mixed unlike, or so it soon seemed, financial market data. The "new financial econometrics" tidal wave that followed can be traced to a small number of papers: French, Schwert & Stambaugh (1987) were the first to estimate the relationship between stock market returns and "predictable volatility," Engle, Lilien & Robins (1987) introduced the ARCH-M model (about which below) to calculate time-varying risk premia in the term structure of interest rates, and Bollerslev, Engle & Wooldridge (1988) would reframe the empirical capital asset pricing model (CAPM) in terms of conditional variances. (Intellectual historians will perhaps wonder, with some cause, about the broader political and cultural determinants of this tidal wave.)

To return to the behavior of recipient rates, the maximum likelihood estimates for the previous model supplemented with $ARCH(1)$ errors are:

$$
\begin{aligned}
\Delta REC_t &= \underset{(0.92)}{-2.32} - \underset{(0.19)}{0.28}\,\Delta REC_{t-1} - \underset{(0.13)}{0.41}\,\Delta REC_{t-2} - \underset{(0.12)}{0.32}\,\Delta REC_{t-3} + \hat{u}_t \\
\sigma_t^2 &= \underset{(12.1)}{32.1} + \underset{(0.32)}{0.16}\,u_{t-1}^2
\end{aligned}
\tag{3}
$$

where σ_t^2 is the conditional variance of u_t. Given the results of the Engle (1982) test, it comes as no surprise that this has little effect: the estimated coefficients on the ΔREC_{t-k} variables, and their standard errors, are almost the same as in (2), and the coefficient on u_{t-1}^2, which drives the ARCH effect, is statistically insignificant. The size of the last coefficient calls for comment, however: if significant, it would mean that a squared residual of 100 in one year, which is substantial but not implausible in this context, would push the conditional variance in the next year to 48.1, or 25% above the unconditional variance of 38.2 and, as a result, diminish forecast confidence.

Two of ARCH's numerous descendants also deserve mention in this context. Generalized ARCH, or GARCH, models, the brainchild of Tim Bollerslev (1982), a student and later collaborator of Engle's, have become even more common than their ancestor. A $GARCH(q,r)$ specification, for example, assumes that the conditional variance of the error in period t, σ_t^2, is $\delta_o + \delta_1 u_{t-1}^2 + \delta_2 u_{t-2}^2 + \ldots + \delta_q u_{t-q}^2 + \tau_1 \sigma_{t-r}^2 + \ldots + \tau_r \sigma_{t-r}^2$, but this is more parsimonious than first seems: in practice, $GARCH(1,1)$ or $GARCH(2,1)$ are often sufficient.

If heterodox economists should be interested in GARCH models for practical reasons, the ARCH in mean or ARCH-M specification, first proposed in Engle, Lilien & Robins (1987), is important for substantive reasons. In the UI example, it could have been the case that the recipient rate was itself a function of labor market turbulence or, to be more precise, conditional variance both past and present:

$$\Delta REC_t = \beta_0 + \sum_{j=1}^{p} \beta_j \Delta REC_{t-j} + \sum_{k=0}^{w} \beta_k \sigma^2_{t-k} + u_t$$

where the conditional variance of u_t is modelled as an ARCH or GARCH process. In a similar vein, the determination of wages, or even the "division of the working day," could reflect not just relative bargaining power, however measured, but also the conditional turbulence of this process. (As alluded to earlier, it was not Marx that inspired Engle, Lilien & Robins (1987), however, but William Sharpe, one of the founders of modern finance!)

It should be noted, however, that in small samples like this one - and sometimes in much more substantial ones - the algorithms that econometric software programs use to calculate the maxima of likelihood functions often fail to converge. (Over some of its domain, the likelihood function is so "flat" that local maxima are difficult to find.) Indeed, in this particular case, where the evidence of ARCH effects is far from decisive, it would not surprise experienced practitioners that neither $GARCH(1,1)$ nor $ARCH-M(1)$ converged reliably.)

Finally, the development of ARCH owes at least little to the influence of David Hendry and the "LSE approach" to econometrics: in his interview with Diebold (2003), Engle recalls that the first ARCH paper (Engle 1982) was started, and then finished, while on leave at LSE, and that Hendry's influence even extended to the choice of name and memorable acronym. Because cointegration is also consistent with, perhaps even a hallmark of, this approach, a more detailed discussion will be postponed until the fifth section.

3 Paradise Lost and Found, Part II

Cointegration theory is perhaps best understood as a new approach to the identi…cation of long-run (equilibrium) relationships in data with stochastic trends. Few recent contributions to economics have in‡uenced its actual practice more: a keyword search of EconLit shows almost three and a half thousand papers on "cointegration," most of them published since 1990, a number that is more than four …fths that for "Keynes" or more than double that for "Marx." The two most common sorts of application have been the reconsideration of traditional (that is, orthodox) macroeconomic relationships - the list of canonical examples includes the consumption function (Banarjee & Hendry, 1992), the demand for money (Dickey, Jansen & Thornton, 1991) and purchasing power parity (Taylor, l995) - and the behavior of asset prices (Campbell & Shiller, 1987).

Interest in cointegration has not been limited to the mainstream, however. Zacharias' (2001) reevaluation of the evidence for pro…t rate equalization in the United States, for example, has a distinct classical, even Marxian, ‡avor, and Loranger (2001) speci…es a "regulationist" model of the Canadian economy in these terms. And despite the concerns of Davidson (1991) about reliance on probabilistic methods in an uncertain world, a substantial number of post Keynesians have used these methods: in a series of papers, Atesoglu (2000, 2002) …nds support for what he calls the Keynes-Weintraub-Davidson (!) model of employment, while Lopez & Alberto (2000) discern the operation of Thirlwall's Law in several Latin American economies. Cliometricians have sometimes adopted this framework as well: Rappoport & White (1993), for example, conclude that it rea¢rms the once traditional view that a "bubble" in‡ated the pre-Depression stock market.

To understand better both its appeal and possible limitations, consider the data on recipient rates once more. The simple and self-referential model of the previous section cannot meet the expectations of those heterodox (and other) economists interested in structure: it is almost silent on the possible causes of claim behavior.

In contrast, Blank & Card (1991) conclude that much of the decline in the recipient rate over the last …ve decades can be attributed to a decrease in collection rates, which prompts the question why so many eligible workers do not, or are somehow unable to, claim UI bene…ts. To answer it, Blank & Card (1991) exploit the substantial interstate variation in recipient rates, and …nd that a combination of economic and social factors - from the replacement rate, the ratio of bene…ts to wages, and the mean duration of jobless spells to the shares of non-white, female and unionized workers - can explain a substantial fraction of this variation, consistent with Matthews, Kandilov & Maxwell (2002) who also discerm evidence of a "political culture" e¤ect.

For purposes of exposition, the in‡uence of a "consolidated" explanatory variable, denoted Z, is considered …rst. A scatter plot of REC and Z, depicted in Figure 6, holds considerable promise, and the results of a simple bivariate regression seems to con…rm this:

$$\begin{aligned} REC &= \underset{(2.68)}{102.8} - \underset{(0.29)}{0.51}Z + \hat{u}_t \\ \bar{R}^2 &= 0.86 \qquad DW = 1.09 \end{aligned} \tag{4}$$

Under the usual (if unfortunate) rhetorical conventions, this streamlined model "explains" 86% of the variation in recipient rates over time, and the in‡uence of Z is statistically signi…cant at the 1% (0.1%, in fact) level. More impressive, it passes a battery of diagnostic checks, including the so-called RESET test for omitted variables and/or misspeci…ed functional form, the CUSUM test for structural change and Breusch-Pagan test for non-spherical (heteroscedastic) errors.[3]

[Insert Figure 6 About Here]

[3] In fact, except for Ramsey's RESET test, it passes all of these tests with ‡ying colors. In the RESET case, the null of no omitted variables can just be rejected at the 5% level, but not at the 1% level. Some econometricians would see this as a "red ‡ag," but most, I suspect, would not.

The one obvious cause for concern, in fact, is the smallish Durbin-Watson statistic, consistent with the presence of serial correlation. The usual (first order) correction appears to solve the problem, however - the Durbin-Watson statistic of the transformed model increases, to 1.81 - and has almost no effect on the estimated coefficients and some, but not much, effect on their standard errors, so is not reported here. The step ahead forecasts based on this pattern of serial correlation seem to track actual recipient rates well, as evidenced in Figure 7.

[Insert Figure 7 About Here]

As a final check, a narrow(er) variable, the replacement rate, REP_t, was added to the model. After correction for serial correlation, the results are:

$$
\begin{aligned}
REC &= -30.0 - 0.53Z + 3.85REP + \hat{u}_t \qquad (5)\\
&\quad\;\; (33.9) \quad (0.09) \quad (0.97)\\
\bar{R}^2 &= 0.65 \qquad DW = 1.67 \qquad \rho = 0.76
\end{aligned}
$$

The importance of the consolidated variable Z seems to be confirmed: its estimated coefficient is close to its previous value and remains significant at the 1% level. On the other hand, consistent with the borderline results for the RESET test, it appears that Z does not capture the effects of the replacement rate on claim behavior. Both the sign and size of the REP coefficient are plausible - a one percent increase in the (relative) value of UI benefits is associated with an almost four percent increase in the recipient rate - and it, too, is significant at the 1% level.

So what, if anything, is wrong with (5), and where does Granger fit in? The immediate problem is that what Z "consolidates" isn't data on UI claim behavior but rather the weather where I live: it is the cumulative snowfall in Burlington, Vermont, in meters and with normalized initial value, over the same period![4] This is the sort of "structure" that econometricians can do without.

[4] Readers familiar with Hendry's (1980) influential "Alchemy or Science?" paper will have

There are two natural responses to the "snowfall model." First, unlike civil unions or universal health care for children, this is not a case of Vermont leading the nation: whatever the econometrics seem to tell us, the weather in Vermont is neither the cause nor the effect of UI utilization in New Mexico or Louisiana. Second, it would be a mistake to dismiss this exercise because no sensible economist would ever propose such a model: the real lesson here is that someone could have substituted another, more "plausible," trended series, with results that seem reasonable but are no more correct.

This doesn't explain, however, how such econometric calamaties occur. The venerable Yule (1926) is often considered the first to have identified (and named) the nonsense correlation problem - his own example concerned the positive correlation between the ratio of Church of England to all British marriages and the mortality rates - but Frain (1995) quotes the lesser known Hooker (1901), who observed that:

> The application of the theory of correlation to economic phenomena frequently presents many difficulties, more especially where the element of time is involved; and it by no means follows as a matter of course that a high correlation coefficient is a proof of a causal connection between any two variables, or that a low coefficient is to be interpreted as demonstrating the absence of such a connection. (Frain 1995: 13)

(As Hendry (2004) reminds us, it was the "Yule critique" that Keynes (1940) would later invoke in his assessment of Jan Tinbergen's work and, in broader terms, macroeconometrics.) Hooker's (1901) intuition, later formalized in Granger & Newbold's (1974) seminal paper on spurious regression, is that the use of non-stationary data - in particular, "trended" time series - undermines classical inference: there is no reson to suppose, for example, that the distribution of the t statistic will be the familiar one, so that one cannot conclude, on

seen this "trick" before. Stock & Watson's (1988) "tale of two econometricians" is a more recent variation. To write it off as no more than a rhetorical trick, however, is to underestimate the challenge to applied econometricians.

the basis of (4), that Z/snowfall matters. (It was not until much later, however, that the critical t values could be calculated.) In heuristic terms, when two random variables rise or fall over time, correlation is inevitable, whether or not there is a relationship between them.

The obvious solution to the nonsense correlation problem is to first detrend the data and then (re)estimate the model with deviations from trend, but there are two problems with this. First, it is seldom obvious how, or even if, a particular time series should be detrended. And second, information about possible relationships between levels is lost in the process, and economists are often more interested in these. In the UI example, the secular relationship between the recipient and replacement rates is no less important than the short(er) run effects of fluctuations in one on the other. Or, to use a more conventional example, data on the deviations from trend in consumption and disposable income cannot tell us much about autonomous consumption or its level.

To consider the first problem in more detail, it should first be noted that for decades, econometricians who were sensitive to the "Yule critique" allowed for deterministic trends. In practice, this meant that trend deviations were calculated as the residuals of a regression of the relevant series on some simple polynomial in time. For the UI data, the recipient rate and cumulative snowfall were each regressed on a quadratic time function and when the deviations from trend in the former, denoted $REC_t - TRENDREC_t$, are then regressed on deviations from trend in the latter, $Z_t - TRENDZ_t$, the results are:

$$\begin{aligned} REC - TRENDREC &= \underset{(1.35)}{0.35} + \underset{(0.82)}{0.05}(Z - TRENDZ) + \mathbf{b} \qquad (6) \\ \bar{R}^2 &= -0.02 \qquad DW = 1.83 \qquad \rho = 0.35 \end{aligned}$$

after correction for first order serial correlation. And with these results comes a small, and perhaps premature, sense of relief: someone with the presence of mind to detrend the data would have decisively rejected the hypothesis that snowfall is a determinant of UI recipient rates. Better, perhaps, if the detrended replacement rate is added to the model, the results are:

$$
\begin{aligned}
REC - TRENDREC &= \underset{(1.29)}{-0.10} - \underset{(0.79)}{0.97}(Z - TRENDZ) + \underset{(1.04)}{3.76}(REP - TRENDREP) \\
\bar{R}^2 &= 0.19 \qquad DW = 1.48 \qquad \rho = 0.40
\end{aligned}
$$

Snowfall still doesn't matter - even if the rejection is much less emphatic than it was in (6) - but replacement rates do.

To situate Granger & Newbold's (1974) contribution, reconsider the fundamental premise of this exercise, that all of the variables are stationary around some deterministic trend or, as this property is sometimes called, trend stationary. This means that each time series X_t can be decomposed into the sum of (at least) two parts, $TREND_t + CYCLE_t$, where $TREND_t$ is some non-random function, more often than not of time, and $CYCLE_t$ is an autocorrelated series with mean zero and finite and constant variance. In the case of cumulative snowfall, for example, $TREND_t$ was estimated to be $25.7 + 2.55t - 0.002t^2$ for $t = YEAR - 1950$. As a consequence, the best forecast of X_{t+k} in period t tends to $TREND_{t+k}$, its trend value, as k increases. In other wrods, the effects of $CYCLE_t$ are assumed to wear out over time, a formulation that reinforces the (too) sharp distinction between the short and long runs that is characteristic of much empirical research.

But as some Vermonters would put it, no one but a "flatlander" would model cumulative snowfall as a trend stationary process: last winter's substantial snows have increased forecasts of cumulative snowfall one, five, ten or even fifty years from now the same amount. Conventional wisdom about winter is better represented as:

$$Z_t - Z_{t-1} = \theta_Z + u_t \tag{7}$$

where θ_Z can be viewed as "normal" annual snowfall and u_t as the annual surplus or deficit relative to this norm, an example of a random walk with (no pun intended) drift. This can be rewritten, after repeated substitution, as $Z_t = Z_0 + t\theta_Z + \sum_{j=0}^{t-1} u_{t+j}$ or even $TREND_t + CYCLE_t$, where $TREND_t = Z_0 + t\theta_Z$ and $CYCLE_t = \sum_{j=0}^{t-1} u_{t+j}$. The similarities to the previous decomposition are more cosmetic than real, however. The trend component is not deterministic, but random, since the value of Z_0 must itself be random. And the remainder, $\sum_{j=0}^{t-1} u_{t+j}$, cannot be stationary, since the mean is zero but the variance is neither constant nor, for that matter, even bounded: $\lim_{t \to \infty} E(\sum_{j=0}^{t-1} u_{t+j})^2 = \lim_{t \to \infty} t\, E(u^2) = \infty$ as $t \to \infty$.

Under these conditions, cumulative snowfall will be difference stationary, however: ΔZ_t will be stationary, with mean θ_Z and variance $E(u_t^2)$. In broader terms, it is said that a series X_t is integrated of order d, written $X_t \sim I(d)$, if its d^{th} difference is stationary: in this case, $Z_t \sim I(1)$. Linear functions of $I(1)$ variables will of course be $I(1)$ and, with an important class of exceptions to be discussed later, a linear combination of $I(1)$ variables will also be $I(1)$. (In fact, if $X_t \sim I(d)$ and $Y_t \sim I(e)$, then $\alpha X_t + \beta Y_t \sim I(f)$, where $f = \max[d, e]$.)

This has two important implications. First, if REC_t is also $I(1)$, the researcher must contend with the modern version of the Yule critique, the problem of spurious regression. In what would soon become "one of the most influential Monte Carlo studies of all time" (Phillips 1997, p. 273), Granger & Newbold (1974) constructed 100 pairs of independent $I(1)$ variables and found that in simple bivariate regressions, the hypothesis that the slope coefficient was zero was rejected (at the 5% level) almost 80 percent of the time for conventional critical values of the t statistic. In addition, they found that in most cases, the R^2 was surprisingly high, but the Durbin-Watson statistic, the usual measure of serial correlation, was low, and this combination is still considered an important, if informal, diagnostic. (It should be noted, therefore, that the first results for (3) should have sounded the alarm: the R^2 is 0.86 but the DW is about 1.) Granger recalls that when he first presented the results in a seminar at LSE, they were "met with total disbelief ...[t]heir reaction was that we must have gottten the Monte Carlo wrong, [that] we must have done the programming wrong" so that if "I had been part of the LSE group, they might well have persuaded me not to have done that research at that point" (Phillips, 1997, p. 262).[5]

A little more than a decade later, Phillips (1986), whose later contributions to time series econometrics would rival Granger's, provided the requisite theoretical foundations for these results. In particular, he showed that as sample size increased, so, too, would the value of the t statistic, so that rejection of the null of a zero slope coefficient was, under the usual conventions, inevitable, but that the value of the Durbin Watson statistic tended to 0.

The second implication is an immediate consequence of the properties of integrated time series: if snowfall is indeed an $I(1)$ process, there is no reason to believe that the measured deviations from an assumed deterministic trend would be stationary, which undermines the conclusions drawn from the modified deviations-from-trend model. In other words, the subsequent rejection of the null that snowfall matters was not as decisive as first seemed and, perhaps worse, if snowfall did matter, this transformation could well have obscured its role! From the perspective of most practitioners, then, the classification of individual series as trend or difference stationary had become a critical first step in model specification.

(This is perhaps not the place for a detailed discussion of the various unit root tests available to researchers, except to note that the most popular of these - the augmented Dickey-Fuller (1979) and Phillips-Perron (1987) tests, for example - were derived under the null hypothesis of difference stationarity, so that failure to reject is interpreted as evidence in favor of a unit root. The so-called KPSS test proposed in Kwiatkowski, Phillips, Schmidt & Shin (1992) is the best known exception. For the series considered here, the augmented Dickey-Fuller test indicates that the null cannot be rejected for REC, REP or, consistent with the intuition of Vermonters, Z, and the KPSS results are consistent with this: the null of trend stationarity is rejected at the 10% level for REC, REP and Z. It should be noted, however, that the power of these

[5] On the other hand, the intuition that some economic relationships were better estimated with differenced data was not novel. Hendry (2004) notes that Fisher (1962) and others had recommended the practice.

tests in small samples and against "local alternatives" if often poor, an issue that will be revisited later.)

If, on this basis, the relationship between recipient rates and cumulative snowfall is estimated in first differences, the results are:

$$
\begin{aligned}
\Delta REC &= \underset{(4.01)}{-0.52} \; \underset{(1.61)}{-\,0.19}\,\Delta Z + \hat{u}_t \qquad (8)\\
\bar{R}^2 &= 0.00 \qquad DW = 2.09 \qquad \rho = -0.14
\end{aligned}
$$

after correction for serial correlation. The rejection of the pure snowfall model is no less decisive than it was when deviations from some deterministic trend were used. If the first differences in replacement rates and the duration of unemployment are then added to the model, the results become:

$$
\begin{aligned}
\Delta REC &= \underset{(3.62)}{-0.41} \; \underset{(1.44)}{-\,0.18}\,\Delta Z + \underset{(0.93)}{4.67}\,\Delta REP + \hat{u} \qquad (9)\\
\bar{R}^2 &= 0.33 \qquad DW = 1.89 \qquad \rho = 0.09
\end{aligned}
$$

Snowfall is still statistically insignificant at any level and the influence of the replacement rate is, in a limited sense, confirmed. (The caveat would lose some of its force if the second result survived the addition of other plausible determinants of UI claim behavior.)

As mentioned earlier, the second problem with models based on transformed data, whether deviations from deterministic trend or first differences, is that information about relationships between levels is lost. If the data are trend stationary, the problem is easily solved: it is an implication of the Frisch-Waugh (1933) Theorem, perhaps better known as the partialling out result, that the addition of a time trend to a model is equivalent to the transformed model in which each variable is instead detrended.

If the data are difference stationary, the problem is more complicated, and it is Granger's solution, and his appreciation for its broad(er) implications, rather than his other in‡uential contributions to econometrics, that are featured in his Nobel citation. What this solution shares with other recent econometric innovations is that it was both di¢cult to see but almost "obvious" once seen. Granger's own description of his epiphany is instructive:

> I do not remember where it took place now, but [David Hendry] was saying that he had a case where had two $I(1)$ variables, but their di¤erence was $I(0)$, and I said that is not possible, speaking as a theorist ... So I went away to prove I was right, and I managed to prove that he was right. Once I realized this was possible, then I immediately saw how it was related to ... [the] error correction model and their balancing. So, in a sense, all the main results of cointegration came together in a few minutes. I mean, without any proof, at least not any deep proof, I just sort of saw what was needed ... [and] I could see immediately what the main results were going to be ... It is one of those things that, once pointed out to people, all kinds of other things made sense, and I think that is why it was accepted so readily. (Phillips, 1997, pp. 274-27)

Hendry's (2004) own account of this exchange provides some additional context. The members of the "LSE school" had wrestled with the representation of equilibrium relationships and disequilibrium dynamics in non-stationary data for some time. By the late 1970s, there was a consensus of sorts that error correction models - which relied on precise combinations of data in di¤erences and levels, and about which more below - were often adequate. (The intuition for such models, Hendry (2004) believes, can be traced back to Klein's (1953) emphasis on the "great ratios" of economics: total wages and national income are both trended, for example, but their ratio or, expressed in natural logarithms, their di¤erence, did not seem to be.) The problem was that the solution was based on "ocular econometrics" (Hendry, 2004, p. 198) and the imposition of a priori structure.

To cultivate some intuition for this solution, recall that linear combinations of nonstationary $I(1)$ variables are, as a rule, also $I(1)$, so that Granger's suspicions about Hendry's claim were, in some sense, well-founded. This means, for example, that if Y_t and X_t are both $I(1)$, the linear combination $Y_t - \beta X_t = \alpha + u_t$ - that is, the combination embedded in the classical linear model - will also be $I(1)$, as will (therefore) the error term u_t. In the absence of some sort of "equilibrium relationship" between Y_t and X_t, the two series will therefore tend to drift apart from one another over time. But if there is such a relationship, some sort of mechanism that prevents such drift, there will exist one (and, in this case, no more than one) combination such that $Y_t - \beta X_t$, and therefore u_t, will be stationary or $I(0)$. In this special case, the two variables are said to be cointegrated, the term that Granger (1981) coined almost two decades ago. To be more precise, the vector of N random variables $\mathbf{X}_t = (X_t^1, X_t^2, ..., X_t^N)$ will be cointegrated of order d,b, denoted $\mathbf{X}_t \sim CI(d, b)$, if each $X_t^i \sim I(d)$ and there exists some cointegrating vector $\beta = (\beta^1, \beta^2, ..., \beta^N)$ such that $Z_t = \sum_{i=1}^{N} \beta^i X_t^i \sim I(d - b)$. There can be at most $N - 1$ such vectors, each of them unique up to a scale factor, but there will often be fewer. The most common procedures to determine this number, and therefore the number of equilibrium relationships, are those described in Johansen (1988), another pre-eminent contributor to the literature. So in the simple bivariate case, if Y_t and X_t are both $I(1)$, then each will be non-stationary in levels but stationary in first differences, but if, in addition, (Y_t, X_t) is $CI(1, 1)$, then some linear combination $Y_t - \beta X_t$ will also be stationary.

Murray's (1994) variation on the drunkard's walk metaphor, the standard illustration of the most famous $I(1)$ process, the random walk, provides some homespun intuition. In the parable of the "drunk and the dog," it is impossible to predict where the next steps of either will take them - in which case the best forecast of whether each will be a few hours from now is where each started, which isn't much of a prediction - unless the dog is hers, in which case the two never drift far apart. It will still be difficult to predict where the pair will be, but the distance between them will be stationary. Or, in other words, their paths will be cointegrated.

From this intuition comes the almost "obvious" test for cointegration first proposed in Engle & Granger (1987), perhaps the most cited paper in either laureate's vita. In terms of UI claim behavior, if REC_t and REP_t are $CI(1,1)$, it should be that $REC_t - \alpha - \beta REP_t = u_t$ is not $I(1)$ but $I(0)$ - that is, stationary - for some β. We do not know the "true" value of β or observe the "true" errors u_t, but there are natural proxies for both, namely the least squares estimates and the associated residuals. The Engle-Granger test is, in effect, a unit root test of the residuals, where rejection of the null (difference stationarity) is interpreted as (indirect, to be sure) evidence of cointegration. In this case, it is difficult to reject - for some specifications of the test, it can be rejected at the 10% level, but no lower - so that the evidence of an equilibirum relationship between REC and REP is, at best, mixed. (This shouldn't come as much of a surprise: this is, after all, a "toy model.")

The Granger Representation Theorem, first articulated in Granger and Weiss (1983), then provides the desired relationship between levels and (first or other) differences of cointegrated time series. As the previous quotation and the subsequent discussion both hinted, the connection can be described in terms of the error correction models or ECMs that predated this work. (The most familiar ECM is perhaps the DHSY model of consumption (Davidson, Hendry, Srba & Yeo, 1978) but (the other) Phillips' (1957) much earlier work on stabilization is another example.)

To illustrate, consider a dynamic version of the simple bivariate model, $Y_t = \gamma_0 + \gamma_1 X_t + \gamma_2 Y_{t-1} + \gamma_3 X_{t-1} + e_t$, which has the steady state equilibrium $Y = \alpha + \beta X$, where $\alpha = \gamma_0/(1 - \gamma_2)$ and $\beta = (\gamma_1 + \gamma_3)/(1 - \gamma_2)$. It is not difficult to show that the model can be rewritten as:

$$\Delta Y_t = \gamma_1 \Delta X_t + (\gamma_2 - 1)(Y_{t-1} - \alpha - \beta X_{t-1}) + e_t \qquad (10)$$

This is the so-called error correction form: period-to-period fluctuations in Y_t depend both on fluctuations in X_t and the extent to which the model was in disequilibrium the previous period, $Y_{t-1} - \alpha - \beta X_{t-1}$. If $(Y_t, X_t) \sim CI(1,1)$, then all of the variables in the ECM are $I(0)$ and its coefficients can be estimated with standard (classical) methods. If Y_t and X_t are not cointegrated, on the other hand, then $Y_{t-1} - \alpha - \beta X_{t-1}$ will not be stationary, and classical methods are inappropriate.

To provide a more formal statement of the theorem, recall that if the N random variables $\mathbf{X}_t \sim CI(d,b)$, there will exist some $r \times N$ matrix $\mathbf{B}$ of rank r such that the r random variables $\mathbf{Z}_t = \mathbf{B}'\mathbf{X}_t \sim I(0)$. (This is just a restatement of the previous definition, where the rows of $\mathbf{B}'$ are the cointegrating vectors.) Now suppose that the evolution of $\mathbf{X}_t$ can be modelled as a vector autoregression of order k, denoted $VAR(k)$, a generalization of the dynamic model in the previous paragraph:

$$\mathbf{X}_t = \boldsymbol{\alpha} + \boldsymbol{\Phi}_1 \mathbf{X}_{t-1} + \boldsymbol{\Phi}_2 \mathbf{X}_{t-2} + \ldots + \boldsymbol{\Phi}_k \mathbf{X}_{t-k} + \boldsymbol{\epsilon}_t \tag{11}$$

where $\boldsymbol{\alpha}$ is a column vector, and $\boldsymbol{\Phi}_1, \ldots, \boldsymbol{\Phi}_k$ square matrices, of coefficients. The theorem asserts that there will exist an $N \times r$ matrix $\mathbf{A}$ such that:

$$\mathbf{I} - \boldsymbol{\Phi}_1 - \boldsymbol{\Phi}_2 - \ldots - \boldsymbol{\Phi}_k = \mathbf{A}\mathbf{B}' \tag{12}$$

and $k - 1$ square matrices $\boldsymbol{\Gamma}_1, \boldsymbol{\Gamma}_2, \ldots, \boldsymbol{\Gamma}_{k-1}$ such that:

$$\Delta \mathbf{X}_t = \boldsymbol{\Gamma}_1 \Delta \mathbf{X}_{t-1} + \boldsymbol{\Gamma}_2 \Delta \mathbf{X}_{t-2} + \ldots + \boldsymbol{\Gamma}_{k-1} \Delta \mathbf{X}_{t-k+1} + \boldsymbol{\alpha} - \mathbf{A}\mathbf{B}'\mathbf{X}_{t-1} + \boldsymbol{\epsilon}_t \tag{13}$$

which is the multivariate version of the error correction model.[6]

For the UI data, the equation we are most interested in is:

$$\begin{aligned} \Delta REC_t &= \alpha_1 + \lambda_1(\beta_1 REC_{t-1} + \beta_2 REP_{t-1}) + \sum_{i=1}^{k-1} \gamma_1^i \Delta REC_{t-i} \\ &\quad + \sum_{i=1}^{k-1} \gamma_2^i \Delta REP_{t-i} + \epsilon_{1t} \end{aligned} \tag{14}$$

where $\beta_1 REC_{t-1} + \beta_2 REP_{t-1}$ or, after normalization, $REC_{t-1} - \beta_2 REP_{t-1}$, is often understood as last period's deviation from equilibrium, and the parameter λ_1 measures the speed of adjustment.

[6] This is the version of the theorem, or at least part of it, presented in Hamilton's (1994) useful text.

Engle & Granger's (1987) two step estimator for (14) was the first, and remains the simplest, available to time series econometricians. Building on the work of Stock (1987), who showed that least squares estimates of the cointegrating vector $[\hat{\beta}_1, \hat{\beta}_2]$ were superconsistent - that is, converged more rapidly than usual - they demonstrated that if these estimates were substituted into the ECM and the values of the other paramaters $\alpha_1, \lambda_1, \gamma_1^1, ..., \gamma_1^{k-1}, \gamma_2^1, ..., \gamma_2^{k-1}$ estimated via maximum likelihood, the results would be both consistent and asymptotically normal.

The Johansen (1988) test reveals, however, that the evidence that REC_t and REP_t are cointegrated is at best mixed. Nevertheless, for $k = 4$, the maximum likelihood estimate of the (standardized) cointegrating vector is $[1, 5.67]$, while the estimate of λ_1 is -0.12. So if an equilibrium relationship does exist, the speed of adjustment is quite slow, consistent with the view that in the short run, fluctuations in the replacement rate can drive claim behavior far from its eventual "equilibrium." This said, the fitted values of the estimated ECM track the observed first differences remarkably well, as illustrated in Figure 8.

[Insert Figure 8 About Here]

Three extensions of the CI framework deserve special mention in this context. Hylleberg, Engle, Granger & Yoo (1990) introduced the notion of seasonal cointegration. Granger & Lee (1989) developed multicointegration to model stock-flow relationships. In their example, if sales and output are $CI(1,1)$ then the difference, or investment in inventories, will be stationary, but the stock of inventories will not be. If the stock of inventories and sales are also cointegrated, however, then sales and output will be multicointegrated. And Granger & Swanson (1995) have considered nonlinear cointegration.

4 A Few Words About Causality and Prediction

Granger (1986) was also the first to notice an important connection between CI models and his own much earlier work (Granger, 1969) on causality. In particular, he perceived that if a pair of economic time series was cointegrated, then one of them must Granger cause the other. Most readers will recall that the definition of Granger causality embodies two crucial axioms (Granger, 1987): uniqueness, which is the principle that the "cause" contain unique information about the "effect," and strict temporal priority. In operational terms, this amounts to a condition on prediction variance: in crude terms, if one stationary time series is better predicted with a second series than without it, the latter is said to cause the former. It was not Granger's (1969) paper that launched a thousand cauality test ships, however, but rather Sims' (1972): regressing the logarithm of current nominal GNP on a number of its own lags and lags of the logarithm of either "broad money" or the monetary base, and then vice versa, he famously concluded that "money causes income" but that "income does not cause money." A little more three decates later, empirical macroeconomists continue to publish variations on this simple exercise, among them a number of post Keynesians who believe - mistakenly, it seems to me - that the debate with the mainstream about the endogeneity of money, or for that matter the internal debate between structuralist and accommodationist explanations of endogeneity, will be resolved on this basis.[7] Granger himself remains ambivalent about Sims' (1972) paper. Much later, he would observe that "part of the defense of the people who did not like the conclusion of [this] paper was that this was not real causality, this was only 'Granger causality' [and] they kept using the phrase ... everywhere in their writings, which I thought was very inefficient, but ... made my name very prominent" (Phillips, 1997, p. 272). More important, he believes that the most common form of the test, based on a comparison of in sample fits, violates the spirit, and perhaps the letter, of his own definition, which emphasizes the importance of (out of sample) predictability.

[7] The most impressive example of this literature is perhaps Palley (1994), who uses Granger causality tests to evaluate the orthodox, structuralist and accommodationist approaches, concluding that the evidence favors the last of these.

If Granger's (1969) causality paper is the most visible of his other contributions to econometric theory, it remains his most controversial. Tobin's (1970) sharp reminder of the perils of post hoc ergo propted hoc reasoning in economics was written in response to Friedman and Schwartz (1963), but it can also be read as a critique of the later Sims (1972). It was for this reason that Leamer (1985) would recommend that econometricians substitute "precedence" for "causality." In a similar vein, Zellner (1979) dismisses the notion that causal relations can be de…ned, or detected, without respect to economic laws.

Granger himself remains unrepentant. He believes that the test - the intuition for which is found outside economics, in the work of the mathematician Norbert Weiner - is "still the best pragmatic de…nition [or] operational de…nition" (Phillips, 1997, p. 272). And he believes that the philosphers have started to come around:

> The philosophers ... initially did not like this de…nition very much, but in recent years several books on philosophy have discussed it in a much more positive way, not saying that it is right, but also saying that it is not wrong. I view that as supporting my position that it is probably a component of what eventually will be a de…nition of causation that is sound. (Phillips, 1997, p. 272)

At the least, Granger's paper remains the point of departure for most "pragmatic" discussions of causality in economics, a number of which (Hoover, 2001, for example) have produced viable extensions or alternatives. It is also embedded in other econometric concepts: it is part, for example, of the de…nition of strong exogeneity (Engle, Hendry & Richard, 1983).

In some sense, Granger's research on causality can be viewed as a particular manifestation of his interest in prediction or forecasting, one that he shares with co-laureate Engle. Published at the same time as the (now, if not then) better known causality paper, for example, Bates & Granger (1969) were the …rst to prove the once counterintuitive result that pooled forecasts tend to perform better than individual ones. For those familiar with James Surowiecki's recent

The Wisdom of Crowds (2004), the intuition is similar: pooled forecasts allow di¤erential biases to o¤set one another. And in the last few years, Granger (2001) has returned to the problem of forecast evaluation.

Diebold (2004, p. 169) believes that "Engle's preference for parametric, parsimonious models [also re‡ects a] long-standing concern with forecasting ...[that] guard against in-sample over…tting or data mining," but adds that he is also responsible for several more speci…c contributions to the literature. Engle & Yoo (1987), for example, was one of the …rst papers to explore the properties of forecasts made on the basis of cointegrated systems. From a broader perspective, the whole ARCH framework can be understood as an attempt to produce better forecasts, or at least better estimates of forecast error variances.

5 Trouble In Paradise?

It seems reasonable to suppose that with the increased attention that ARCH and CI models have received since the Royal Academy's announcement, more economists outside the mainstream will be tempted to experiment with one or both. There is reason to be cautious, however. The most important operational criticism of the common trends framework, for example, is that testing for unit roots or estimating cointegrating vectors is problematic in small(ish) samples. For the researcher armed with annual, or even quarterly, data, for example, the bene…ts are uncertain. Miron (1991, p. 212), himself a contributor to the technical literature, believes that "since we can never know whether the data are trend stationary or di¤erence stationary [in …nite samples], any result that relies on the distinction is inherently uninteresting." The implicit advice, that researchers should …t both sorts of models to time series data, even in those cases where test results seem decisive, strikes (at least) me as sound. For the UI claim data, for example, the conclusion that recipient and replacement rates share a common trend, or that short run ‡uctuations in replacement rates are associated with substantial movements in claims, is a more robust one than it would have been if both alternatives had not been considered.

One of the most persuasive illustrations of this problem is Perron's (1989) paper on unit root tests in a world which, if trend stationary, is trend stationary with structural breaks. It is important to appreciate that at the time the paper was published, the conventional wisdom held that almost all macroeconomic time series were either $I(1)$ or $I(2)$: Nelson & Plosser's (1982) often cited article, for example, found that for 13 of 14 series - the one exception was the unemployment rate - the null hypothesis of a unit root could not be rejected. The immediate and widespread success of Engle and Granger (1987), which provided researchers with a tractable framework for the analysis of relationships between such series, owed at least a little to this near consensus. What Perron (1991) showed, however, was that if one allowed for just two structural breaks - what he describes as the "intercept shift" of the Great Crash of 1929 and the "slope shift" of the oil price shock of 1973 - around an otherwise stable, trend stationary, process, the unit root hypothesis could now be rejected for 11 of the same 14 series! While later studies have qualified Perron's reversal - Zivot & Andrews (1992), for example, endogenized the selection of breakpoints and found that 6 of the 14 series were not difference stationary - the lesson for empirical researchers, heterodox or otherwise, is clear: there is still much to be learned from old fashioned, if more flexible, decompositions of economic time series.

ARCH models suffer from their own small sample woes, as Engle himself understood from the beginning (Engle, Hendry & Richard, 1985). On the basis of their own Monte Carlo studies, for example, Hwang and Periera (2003) find that at least 250 observations are required to offset the bias problem in ARCH(1) models, and more than 500(!) in GARCH(1,1) models. This is seldom a constraint with daily, or even weekly, financial market data, but it represents a very long time indeed when the data source is annual state UI reports!

(At this point, little is known about the interaction of these small sample problems. Mantalos (2001) finds, however, that $I(1)$ series with $GARCH(1,1)$ errors, the null hypothesis tends to be overrejected or, in other words, that cointegration is detected more often than it should be.)

A second practical concern about ARCH/GARCH models is related to their common, and sometimes lucrative, application to financial market data. The most important of these involve the use of conditional variances to improve estimates of value at risk, as embodied in J. P. Morgan's influential RiskMetrics paradigm. In this context, it is important to ask how well this class of models capture the stylized facts of asset markets. It has already been observed, for example, that clustered volatility and the information release effect can be represented, and it can be shown that another variant, Nelson's (1991) EGARCH, can accommodate a leverage effect.[8] But there is some evidence (Brock & Potter, 1993, Brock & de Lima, 1996) that there is otherwise unmodelled nonlinear structure in financial data.

The contribution of ARCH/GARCH models to the "new financial econometrics" raises broader questions, however, about the existence of such structure. In a recent critique, Mayer (1999), for example, observes that "[Basle's] abdication of supervisory function followed J. P. Morgan's publication of its RiskMetrics methodology, which supposedly enabled banks to measure the extent of their risks looking forward into the realm where uncertainty, not of probability, reigns" (emphasis added).

Concerns about statistical inference in a world where outcomes are uncertain, rather than probabilistic, extend to CI models, too. Davidson (1994) is perhaps the most prominent of the post Keynesians to assert that economic processes are nonergodic and thus nonstationary, and that this difference cannot be "undone" with the use of differenced data. If true, the structure uncovered in such models will often be an artifact. Lawrence Klein, the Nobel laureate who for decades embodied the older Cowles Commission approach to macroeconometrics, shares a practical concern about the (over)use of differences, if not the post Keynesian premise:

> I do not think economic data are necessarily stationary or that

[8] EGARCH models were one of the first asymmetric conditional variance models, and allow the volatilties of positive and negative shocks to differ.

> economic processes are stationary. The technique of cointegration, to keep differencing the data until stationarity is obtained and then relate the stationary series, I think can do damage. It does damage in the sense that it almost always done on a bivariate or maybe trivariate basis. That keeps the analysis simple ... but the world is not simple.
>
> [I] would conclude that one should accept the fact that economic data are not stationary, relate the non-stationary data, but include the explicit [deterministic] trends and think about what could cause the trends in the relationship. One then uses that more complicated relationship with trend variables. Successive differencing, as it is done in cointegration techniques, may introduce new relationationships, some of which we do not want to have in our analysis (Klein, 1994, p. 34, emphasis added)

Klein (1994, p. 34) also expresses some unease that the "youngest ... generation of econometricians simply do [this] mechanically ... and do not understand the original data series as well as they should."

The heterodox economist who is still tempted to use the CI framework must also consider its unfortunate - but I believe inessential - link to the "real business cycle" or RBC paradigm. The near consensus that once existed that almost all macro time series were $I(1)$ or $I(2)$ but that subsets of these formed cointegrated systems was, and to some extent still is, linked to the rise of RBC models. (I am tempted to add that if, as some believe, the term "heterodox" is too broad to be useful, the widespread rejection of such models could be the exception that proves the rule.) The reason is not difficult to infer: if real output has a "strong unit root," for example, then one could interpret its evolution over time in terms of a series of permanent shocks or, as Shapiro & Watson (1988) concluded, as random increases in potential income, rather than a sequence of transitory shocks around some deterministic trend/potential. Furthermore, as King, Plosser, Stock & Watson (1988, p. 320) reminded their readers, in the presence of technology shocks, "balanced growth under uncertainty implies that consumption, investment and output [will be] integrated."

There are at least two reasons to suppose that the connection is more tenuous than …rst seems, however. First, as De Long & Summers (1988) have observed, the strength of the unit root in real output is in some measure an artifact, a consequence of macroeconomists' preoccupation with post World War II data. (Of course, to the extent that earlier data is either unavailable or unreliable, the preoccupation is an understandable one.) Viewed from this perspective, the diminished importance of "transitory ‡uctuations" in this period has less to do with RBCs that with the success of old fashioned Keynesianism at stabilization, a point also made, albeit in a di¤erent context, by Tobin (1982).

The second is that whether or not balanced growth is su¢ cient for cointegration, it does not follow that it is also necessary. It is not clear that old fashioned Keynesianism, or any other heterodox approach, requires that the evolution of critical time series be limited to stationary ‡uctuations around some deterministic trend. It is not clear to me, for example, that the existence of a stochastic trend is incompatible with some sort of Harrodian, or demand driven, model of economic growth.

There is another, more subtle, explanation for this connection, one that is related to current methodological debates in econometrics. The recent concern over the use of incredible restrictions as a means of identi…cation, the fashion for "unrestricted" vector autoregressions, the interest in cointegrating vectors and ECMs and, to a lesser extent, the use of ARCH models can all be understood as manifestations of the LSE or general-to-speci…c approach to empirical research. Like "rational" expectations, however, general-to-speci…c is not always the unalloyed virtue it …rst seems. For each new variable added to an ECM, for example, there is a substantial increase in the number of new parameters to estimate, a mild version of the curse of dimensionality. (To assume a priori that some of these coe¢ cients are zero is, of course, to impose the sort of restrictions that "old fashioned" econometricians have relied on for decades.) As a result, most CI or ECM models tend to be parsimonious and while there are no doubt exceptions to the rule, the models of orthodox economists tend to be "lower dimensional" than those of their heterodox counterparts (Pandit 1999). If in‡ation is "always and everywhere a monetary phenomenon," for example, then a bivariate VAR is su¢cient for many purposes! Pandit (1999) also cites Klein (1999) as a source of examples of cases in which parsimonious models lead to incorrect conclusions.

6 Conclusion

It is not uncommon for some outside the mainstream to remind us that the Bank of Sweden's prize for "economic science" is newer than, and di¤erent from, the other Nobels, or to protest that it resembles a "beauty contest" that some kinds of contestants will never win. There is some truth to this, of course: the failure to award the prize to Joan Robinson, for example, is still incomprehensible. To dismiss the achievements of those who have won, however, because of those who haven't is facile. More than a few laureates have challenged, and sometimes informed, various heterodox traditions. It would be naive to claim, for example, that the in‡uence of Arrow, who shared the 1972 prize, Leontief (1973), Myrdal (1974), Lewis (1979), Tobin (1981), Nash (1994), Sen (1998), Akerlof (2001) or Stiglitz (2001) were limited to the mainstream.

Should Engle and/or Granger be added to this illustrious list? However durable their technical accomplishments prove, there is little doubt that each has in‡uenced how we think about and sometimes organize economic data.

7 References

Atesoglu, H. S. (2000) Income, employment, in‡ation and money in the United States, Journal of Post Keynesian Economics, 22, pp. 639-646.

Atesoglu, H. S. (2002) Stock prices and employment, Journal of Post Keynesian Economics, 24, pp. 493-498.

Banarjee, A. & Hendry, D. F. (1992) Testing integration and cointegration: an overview, Oxford Economic Papers, 54, pp. 225-255.

Bates, J. M. & Granger, C. W. J. (1969) The combination of forecasts, Operations Research Quarterly, 20, pp. 451-468.

Blank, R. & Card, D. (1991) Recent trends in insured and uninsured unemployment: is there an explanation? Quarterly Journal of Economics, 106, pp. 1157-1189.

Bollerslev, T., Engle, R. F. & Wooldridge, J. M. (1986) A capital asset pricing model with time-varying covariances, Journal of Political Economy, 96, pp. 116-131.

Bollerslev, T. (1986) Generalized autoregressive conditional heteroscedasticity, Journal of Econometrics, 31, pp. 307-327.

Brock, W. & Potter, S. (1993) Nonlinear time series and macroeconometrics, in: G. S. Maddala, C. R. Rao & H. D. Vinod (Eds) Handbook of Statistics, 11, pp. 195-229.

Brock, W. & De Lima, P. (1995) Nonlinear time series, complexity theory and ...nance, in: G. S. Maddala & C. R. Rao (Eds) Handbook of Statistics, 14.

Campbell, J. Y. & Shiller, R. J. (1987) Cointegration and tests of present value models, Journal of Political Economy, 95, pp. 1062-1088.

Davidson, J. E. H., Hendry, D. F., Srba, F. & Yeo, S. (1978) Econometric modelling of the aggregate time-series relationship between consumers' expenditure and income in the United Kingdom, Economic Journal, 88, pp. 661-692.

Davidson, P. (1991) Is probability theory relevant for uncertainty: a post Keyensian perspective, Journal of Economic Perspectives, 5, pp. 129-143.

De Long, J. B. & Summers, L. (1988) On the existence and interpretation of a "unit root" in US GDP, NBER Working Paper, 2716.

Dickey, D. A. & Fuller, W. A. (1979) Distribution of the estimators for autoregressive time series with a unit root, Journal of the American Statistical Association, 74, pp. 427-431.

Dickey, D. A., Jansen, D. W. & Thornton, D. L. (1992) A primer on cointegration with an application to money and income, Federal Reserve Bank of St. Louis Review, 73, pp. 58-78.

Diebold, F. X. (2003) The ET interview: Professor Robert F. Engle, Econometric Theory, 19, pp. 1159-1193.

Diebold, F. X. (2004) The Nobel Memorial Prize for Robert F. Engle, Scandinavian Journal of Economics, 106, pp. 165-185.

Ding, Z., Granger, C. W. J. & Engle, R. F. (1993) A long memory property of stock market returns and a new model, Journal of Empirical Finance, 1, pp. 83-106.

Engle, R. F., Bradbury, K., Irvine, O. & Rothenberg, J. (1977) Simultaneous estimation of the supply and demand for household location in a multizoned metropolitan area, in: G. K. Ingram (Ed) Residential Location and Urban Housing Markets (Cambridge, Ballinger).

Engle, R. F. & Granger, C. W. J. (1987) Cointegration and error correction: representation, estimation and testing, Econometrica, 55, pp. 251-276.

Engle, R.F., Granger, C. W. J. & Robins, R. (1986) Wholesale and retail prices: bivariate modeling with forecastable variances, in: D. Belsey and E. Kuh (Eds) Model Reliability (Cambridge, MIT Press).

Engle, R. F., Hendry, D. F. & Richard, J.-F. (1983) Exogeneity, Econometrica, 51, pp. 277-304.

Engle, R. F., Hendry, D. F. & Trumble, D. (1985) Small-sample properties of ARCH estimators are tests, Canadian Journal of Economics, 18, pp. 66-93.

Engle, R. F. & Kraft, D. (1983) Multiperiod forecast error variances of in‡ation estimated from ARCH models, in A. Zellner (Ed) Applied Time Series Analysis of Economic Data (Washington, Bureau of the Census).

Engle, R. F., Lilien, D. M. & Robins, R. P. (1987) Estimating time varying risk premia in the term structure: the ARCH-M model, Econometrica, 55, pp. 391-407.

Engle, R. F. & Liu, T. C. (1972) E¤ects of aggregation over time on dynamic characteristics of an economic model, in: B. G. Hickman (Ed) Econometric Models of Cyclical Behavior: Studies in Income and Wealth (New York, National Bureau of Economic Research).

Engle, R. F. & Manganelli, S. (2001) CAViaR: conditional autoregressive value at risk by regression quantiles, NBER Working Paper, W7341.

Engle, R. F. & Rosenberg (1995) GARCH gammas, Journal of Derivatives, 2, pp. 47-59.

Engle, R. F. & Yoo, B. S. (1987) Forecasting and testing in co-integrated systems, Journal of Econometrics, 35, pp. 143-159.

Engle, R. F. (1973) Band spectrum regression, International Economic Review, 15, pp. 1-11.

Engle, R. F. (1974a) Issues in the speci…cation of an econometric model of metropolitan growth, Journal of Urban Economics, 1, pp. 250-267.

Engle, R. F. (1974b) A disequilibrium model of regional investment, Journal of Regional Science, 14, pp. 367-376.

Engle, R. F. (1976) Policy pills for a metropolitan economy, Papers and Proceedings of the Regional Science Association, 35, pp. 191-205.

Engle, R. F. (1982) Autoregressive conditional heteroscedasticity with estimates of the variance of United Kingdom in‡ation, Econometrica, 50, pp. 987-1007.

Fisher, F. M. (1962) A Priori Information and Time Series Analysis (Amsterdam, North-Holland).

Frain, J. (1995) Econometrics and truth, Bank of Ireland Technical Paper, 2/RT/95.

French, K. R., Schwert, W. G. & Stambaugh, R. F. (1987) Expected stock returns and volatility, Journal of Finance, 19, pp. 3-29.

Friedman, M. & Schwartz, A. (1963) Monetary History of the United States, 1870-1960 (Princeton, Princeton University Press).

Frisch, R. & Waugh, F. V. (1933) Partial time regression as compared with individual trends, Econometrica, 1, pp. 221-223.

Granger, C. W. J. (1957) A statistical model for sunspot activity, Astrophysical Journal, 126, pp. 152-158.

Granger, C. W. J. (1959) Estimating the probability of ‡ooding on a tidal river, Journal of the Institution of Water Engineers, 13, pp. 165-174.

Granger. C. W. J. (1969) Investigating causal relations by econometric models and cross-spectral methods, Econometrica, 37, pp. 424-438.

Granger, C. W. J. (1981) Some properties of time series data and their use in econometric model speci…cation, Journal of Econometrics, 16, pp. 121-130.

Granger, C. W. J. (1986) Developments in the study of cointegrated economic variables, Oxford Bulletin of Economics and Statistics, 48, pp. 213-228.

Granger, C. W. J. (1987) Causal inference, in: J. Eatwell, M. Milgate and P. Newman (Eds) The New Palgrave: Econometrics (New York, Norton).

Granger, C. W. J. (2001) Evaluation of forecasts, in: D. F. Hendry & N. R. Ericsson (Eds) Understanding Economic Forecasts (Cambridge, MIT Press).

Granger, C. W. J. & Anderson, A. P. (1978) Introduction to Bilinear Time Series Models (Gottingen, Vandenhoeck & Ruprecht).

Granger, C. W. J. & Bates, J. (1969) The combination of forecats, Operations Research Quarterly, 20, pp. 451-468.

Granger, C. W. J., Craft, M. & Stephenson, G. (1964) The relationship between severity of personality disorder and certain adverse childhood in‡uences, British Journal of Psychiatry, 110, pp. 392-396.

Granger, C. W. J. & Hatanaka, M. (1964) Spectral Analysis of Economic Time Series (Princeton, Princeton University Press).

Granger, C. W. J. & Joyeux, R. (1980) An introduction to long-memory time series models and fractional di¤erencing, Journal of Time Series Analysis, 1, pp. 15-30.

Granger, C. W. J. & Lee, T.H. (1989) Investigation of production, sales and inventory relationships using multicointegration and non-symmetric error correction models, Journal of Applied Econometrics, 4, pp. 145-159.

Granger, C. W. J. & Newbold, P. (1974) Spurious regression in econometrics, Journal of Econometrics, 2, pp. 111-120.

Granger, C. W. J. & Swanson, N. R. (1995) Further developments in the study of cointegrated variables, Oxford Bulletin of Economics and Statistics, 58, pp. 537-553.

Granger, C. W. J. & Weiss, A. A. (1983) Time series analysis of error correction models, in: S. Karlin, T. Amemiya & L. A. Goodman (Eds) Studies in Econometrics, Time Series and Multivariate Statistics in Honor of T. W. Anderson (San Diego, Academic Press).

Hamilton, J. D. (1994) Time Series Analysis (Princeton, Princeton University Press).

Hendry, D. F. (1977) On the time series approach to econometric model building, in: C. A. Sims (Ed) New Methods in Business Cycle Research (Minneapolis, Federal Reserve Bank).

Hendry, D. F. (2004) The Nobel Memorial Prize for Clive W. J. Granger, Scandinavian Journal of Economics, 106, pp. 187-213.

Hooker, R. H. (1901) Correlation of the marriage rate with trade, Journal of the Royal Statistical Society, 64, pp. 485-492.

Hoover, K. (2001) Causality in Macroeconomics (Cambridge, Cambridge University Press).

Hwang, S. & Periera, P. L. V. (2003) Small sample properties of GARCH estimates and persistence, Imbec Finance Lab Working Paper.

Hylleberg, S., Engle, R. F., Granger, C. W. J. & Yoo, B. S. (1990) Seasonal cointegration, Journal of Econometrics, 44, pp. 215-238.

Johansen, S. (1988) Statistical analysis of cointegrating vectors, Journal of Economic Dynamics and Control, 12, pp. 231-254.

Keynes, J. M. (1940) Comment, Economics Journal, 50, pp. 154-156.

King, R. G., Plosser, C. I., Stock, J. H. & Watson, M. W. (1991) Stochastic trends and economic ‡uctuations, American Economic Review, 81, pp. 819-840.

Klein, L. R. (1954) A Textbook of Econometrics (Evanston, Row, Peterson & Company).

Klein, L. R. (1994) Problems with modern economics, Atlantic Economic Journal, 22, pp. 34-39.

Klein, L.R. (1999) Economic stabilization policy: pitfalls of parsimonious modelling, Journal of Quantitative Economics,15.

Kwiatkowski, D., Phillips, P. C. B., Schmidt, P. & Shin, T. (1992) Testing the null hypothesis of stationarity against the alternative of a unit root: how sure are we that economic series have a unit root? Journal of Econometrics, 54, pp. 159-178.

Leamer, E. E. (1985) Vector autoregressions for causal inference? Carnegie Rochester Conference Series on Public Policy, 22, pp. 255-303.

Lopez G. J. & Cruz, A. B. (2000) Thirlwall's Law and beyond: the Latin American experience, Journal of Post Keynesian Economics, 22, pp. 477-495.

Loranger, J-G. (2001) Structural form of an accumulation model: an analysis of cointegration of the long term growth of the Canadian economy, University of Montreal Working Paper.

Mantalos, P. (2001) ECM-cointegration tests with GARCH(1,1) errors, Interstat, March 2001. Online at http://interstat.stat.vt.edu/InterStat/.

Matthews, P. H., Kandilov, I. T. & Maxwell, B. (2002) Interstate di¤erences in insured unemployment: some recent evidence, Applied Economics Letters, 9, pp. 945-948.

Mayer, M. (1999) Somebody turn on the lights, Derivatives Strategy, November.

Miron, J. (1991) Comment, NBER Macroeconomics Annual, pp. 211-219.

Mizon, G. E. (1995) Progressive modelling of macroeconomic time series, in: K. D. Hoover (Ed) Macroeconometrics: Developments, Tensions and Prospects (Boston, Kluwer).

Murray, M. (1994) A drunk and her dog: an illustration of cointegration and error correction, The American Statistician, 48, pp. 37-39.

Nelson, C. R. & Plosser, C. I. (1982) Trends and random walks in macroeconomic time series, Journal of Monetary Economics, 10, pp. 139-162.

Nelson, D. B. (1991) Conditional heteroscedasticity in asset returns: a new approach, Econometrica, 59, pp. 347-390.

Palley, T. (1994) Competing views of the money supply process: theory and evidence, Metroeconomica, 45, pp. 67-88.

Pandit, V. (1999) Structural modelling under challenge, Center for Development Economics/Delhi School of Economics Working Paper, 98.

Perron, P. (1989) The great crash, the oil price shock, and the unit root hypothesis, Econometrica, 57, pp. 1361-1401.

Phelps, E. (1988) Comment on recent developments in macroeconomics, Journal of Money, Credit and Banking, 20, pp. 456-458.

Phillips, A. W. (1957) Stabilization policy and the time forms of lagged responses, Economic Journal, 67, pp. 265-277.

Phillips, P. C. B. (1986) Understanding spurious regressions in econometrics, Journal of Econometrics, 33, pp. 311-340.

Phillips, P. C. B. (1997) The ET interview: Professor Clive Granger, Econometric Theory,13, pp. 253-303.

Phillips, P. C. B. & Perron, P. (1987) Testing for a unit root in a time series regression, Biometrika, 75, pp. 335-346.

Rappaport, P. & White, E. N. (1993) Was there a bubble in the 1929 stock market? Journal of Economic History, 53, pp. 549-574.

Shapiro, M. D. & Watson, M. W. (1988) Sources of business cycle fluctuations, NBER Macroeconomics Annual, pp. 111-148.

Shiller, R. J. (2003) From efficienct markets theory to behavioral finance, Journal of Economic Perspectives, 17, pp. 83-104.

Sims, C. A. (1972) Money, income and causality, American Economic Review, 62: 540-552.

Stock, J. H. (1987) Asymptotic properties of least squares estimators of cointegrating vectors, Econometrica, 55, pp. 1035-1056.

Stock, J. H. & Watson, M. W. (1988) Variable trends in economic time series, Journal of Economic Perspectives, 2, pp. 141-174.

Surowiecki, J. (2004) The Wisdom of Crowds (New York, Random House).

Taylor, A. (1995) International capital mobility in history: purchasing-power parity in the long run, NBER Working Paper, 5742.

Tobin, J. (1970) Money and income: post hoc ergo propter hoc? Quarterly Journal of Economics, 94, pp. 301-317.

Tobin, J. (1982) Asset Accumulation and Economic Activity: Reflections on Contemporary Macroeconomic Theory (Chicago, University of Chicago Press).

Yule, G. U. (1926) Why do we sometimes get nonsense correlations betweeen time series? A study in the sampling and nature of time series, Journal of the Royal Statistical Society, 89, pp. 1-6.

Zacharias, A. (2001) Testing profit rate equalization in the US manufacturing sector: 1947-1998, Jerome Levy Institute Working Paper.

Zellner, A. (1979) Causality and econometrics, Carnegie Rochester Series on Public Policy, 10, pp. 9-54.

Zivot, E. and Andrews, D. W. K. (1992) Further evidence on the Great Crash, oil price shock and the unit root hypothesis, Journal of Business and Economic Statistics, 10, pp. 251-270.

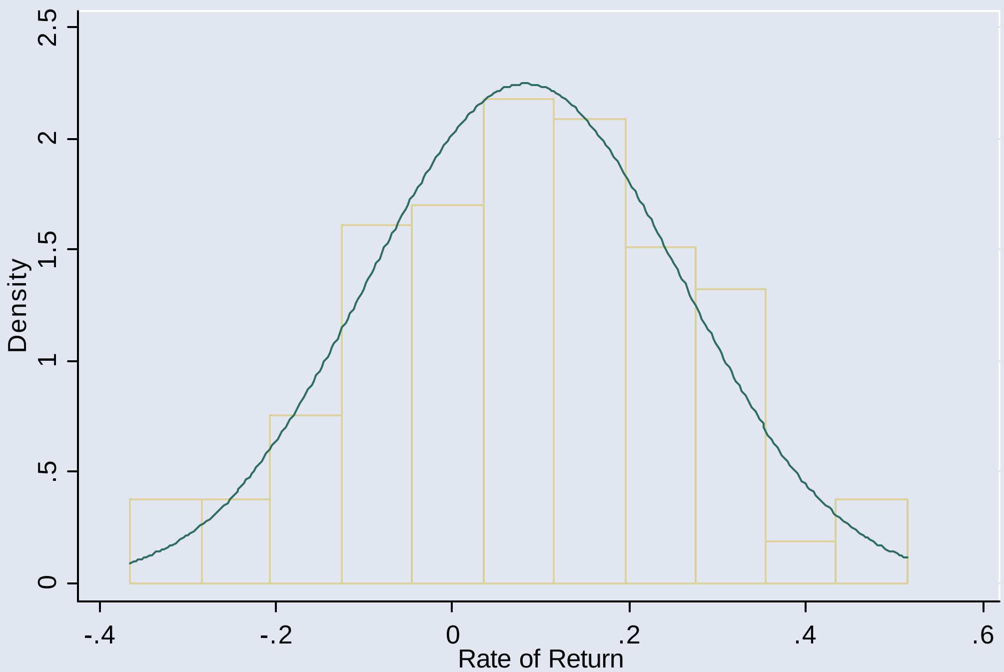


Figure 1. Rates of Return in the Stock Market 1871-2002

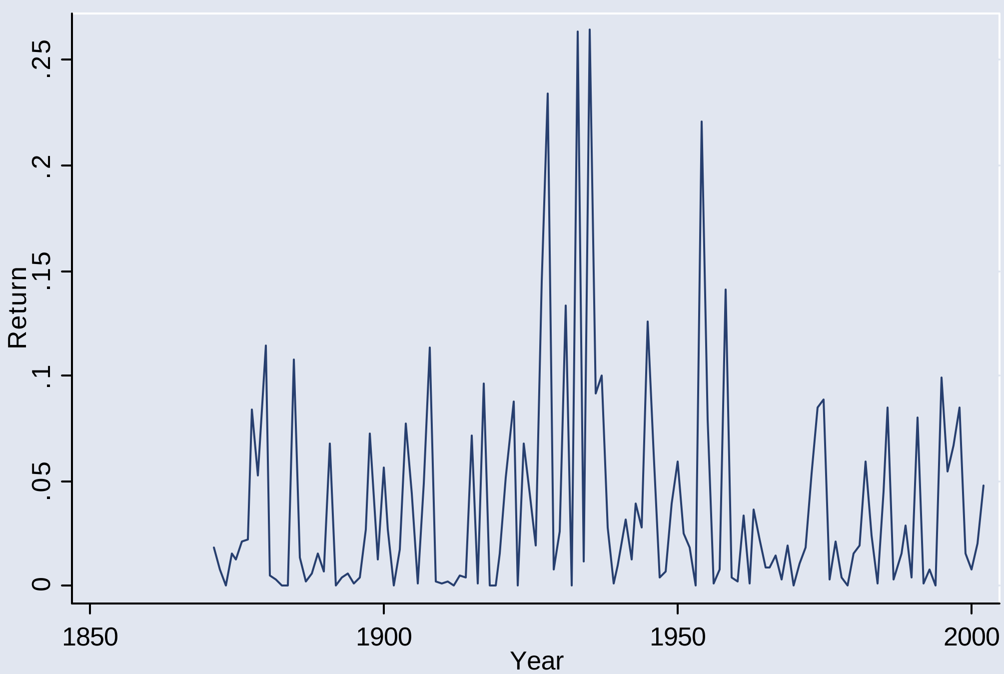


Figure 2. Squared Returns in the Stock Market 1871-2002

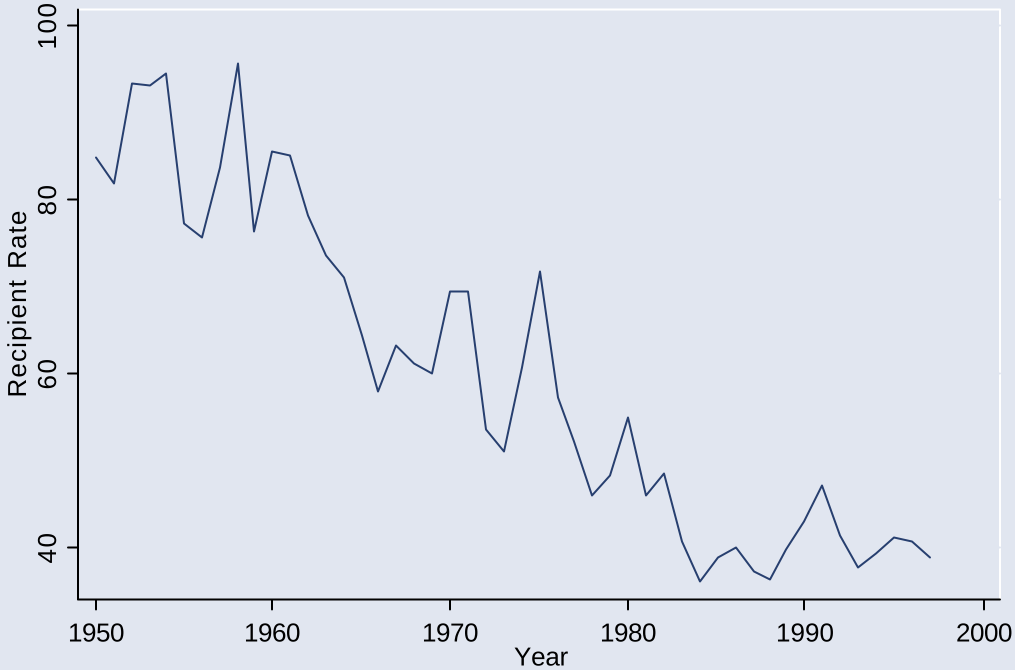


Figure 3. The UI Recipient Rate in the United States

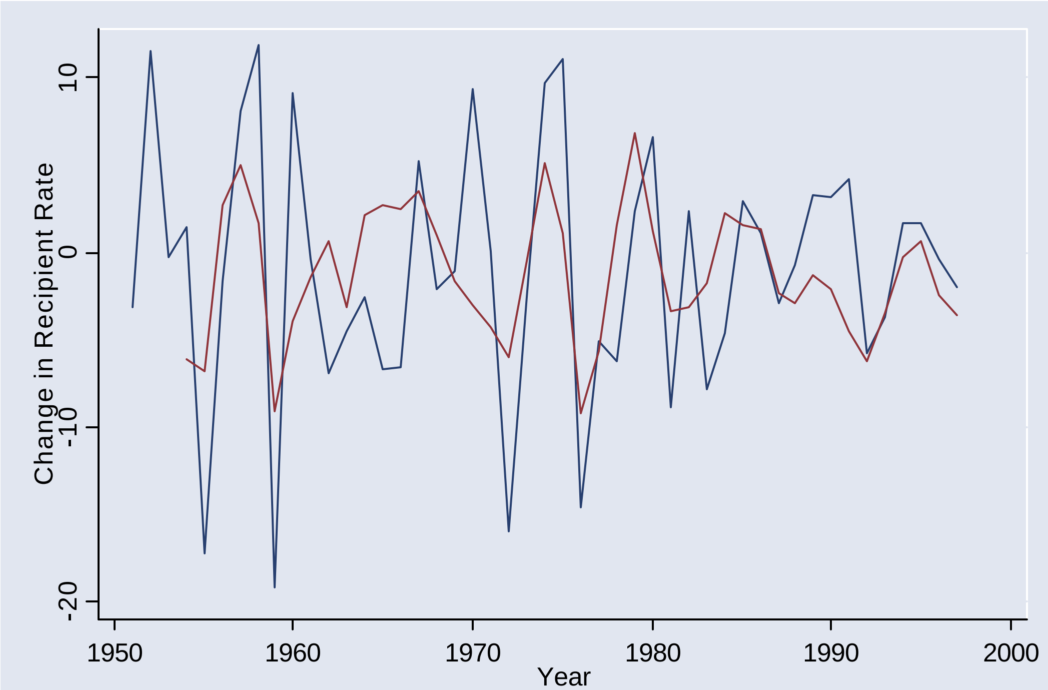


Figure 4. Actual and Predicted Changes in Recipient Rates

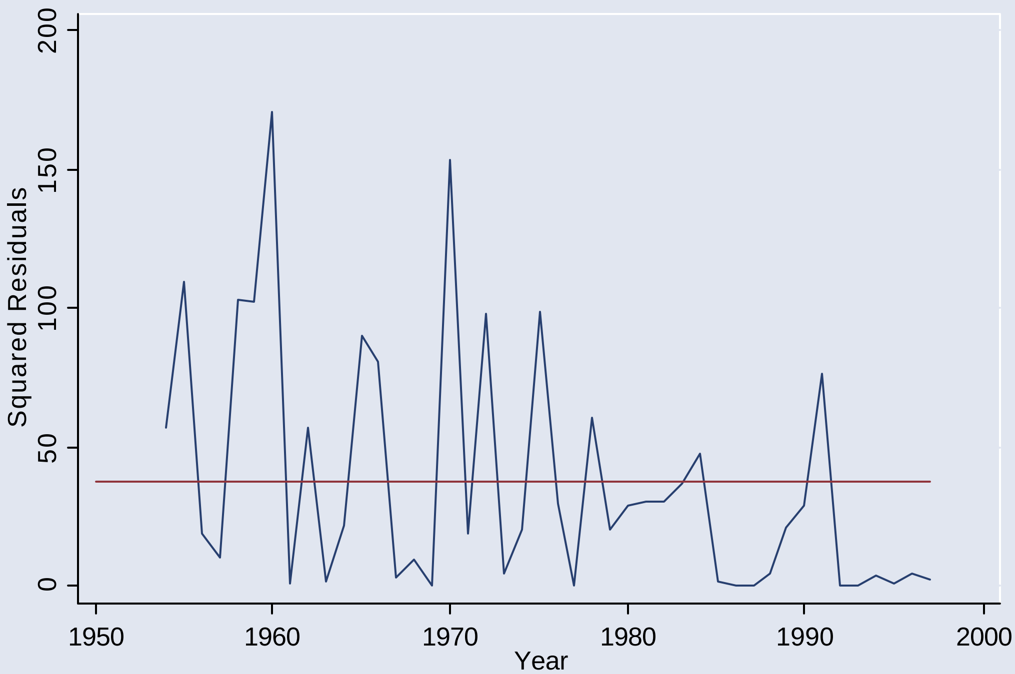


Figure 5. Squared Residuals From Simple Model

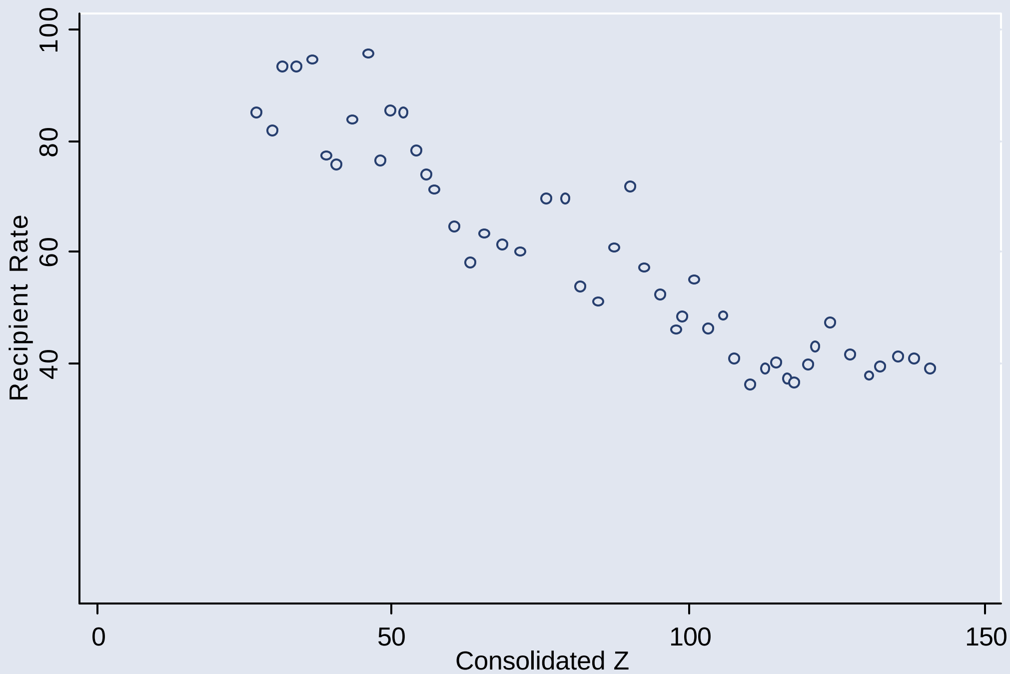
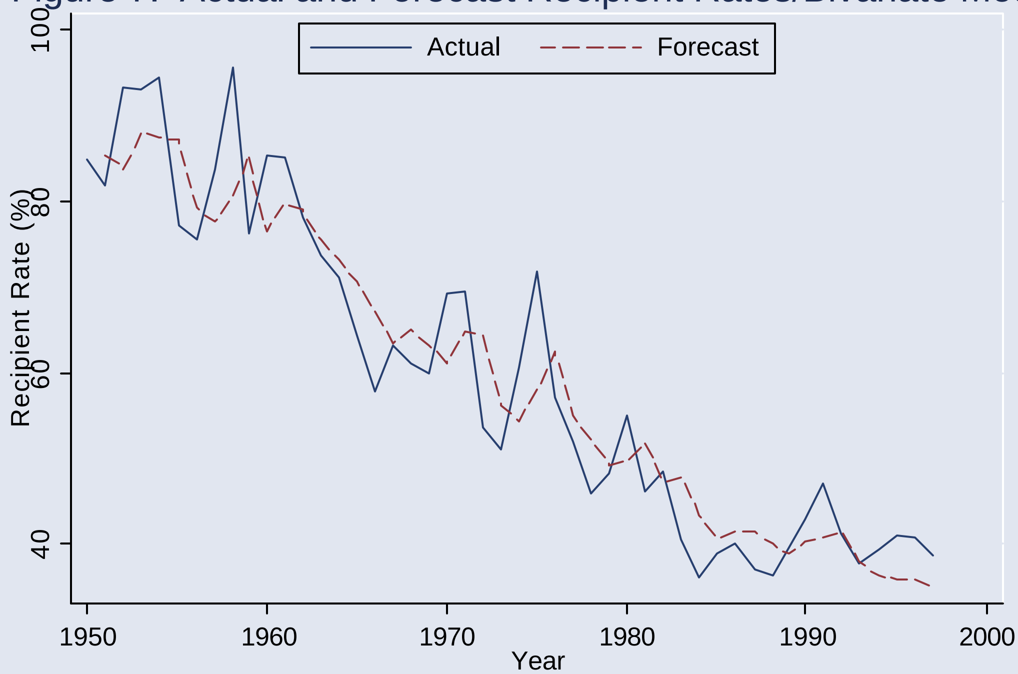


Figure 6. Recipient Rate and Consolidated Variable Z

Figure 7. Actual and Forecast Recipient Rates/Bivariate Model



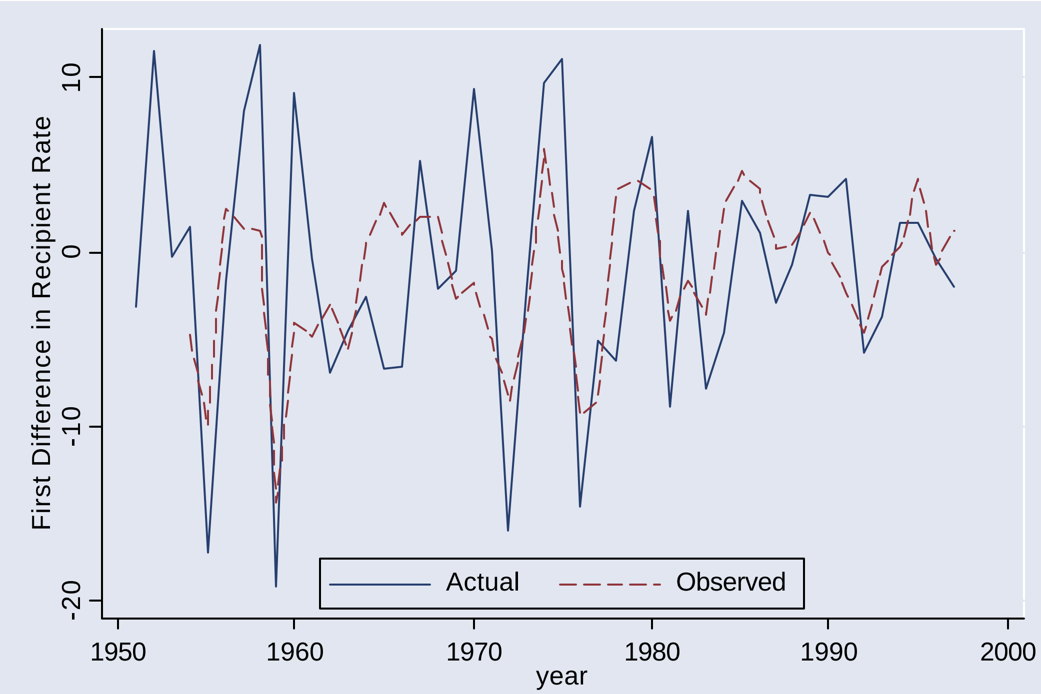


Figure 8. Observed and Predicted Differences in ECM