ment in the column vector U_i. Again, the terms r_b and U_{jb} are similarly defined.

Since r is independent of U and $Er_a r_b = 0$, $a \neq b$ and $Er_a r_b = 1$, $a = b$, it follows that

$$\Sigma_{S_{ij}} = T^{-1} E \sum_{a=1}^{T} r_a^2 U_{ia} U_{ja}$$

$$= T^{-1} E \sum_{a=1}^{T} U_{ia} U_{ja}$$

$$= \Sigma_{ij}$$

Thus, $\Sigma_S = \Sigma$.

This property holds as long as U is derived from a known structural form. This result suggests that if we are given a sample set of errors, $\hat{U}$, derived from an estimated model, we might use as pseudo-disturbances the following:

$$\hat{S} = T^{-1/2} r \hat{U} \tag{2}$$

For models estimated by consistent methods, the covariance matrix of $\hat{S}$ will be asymptotically equal to the true covariance matrix of the system.

For small samples, if we take the $\hat{U}$ as given, it will be true that the conditional expected value of $\hat{S}$ will be equal to the sample variance-covariance matrix of the system, calculated as

$$\hat{\Sigma} = T^{-1} \hat{U}' \hat{U}$$

To obtain pseudo-errors that have suitable auto-regressive properties, we might define

$$S_t = T^{-1/2} \bar{r}_t U, \text{ and } S_{t-1} = T^{-1/2} \bar{r}_{t-1} U$$

where $\bar{r}_t = [r_t r_{t-1} r_{t-2} \ldots r_{t-T+1}]$

and $\bar{r}_{t-1} = [r_{t-1} r_{t-2} r_{t-3} \ldots r_{t-T}]$

and t is a time subscript.

In what follows, we examine the first-order auto-regressive properties of S. Form

$$ES_t' S_{t-1} = T^{-1} E U' \bar{r}_t' \bar{r}_{t-1} U \tag{3}$$

The typical element in (3) is given by

$$Z_{ij} = T^{-1}EU_i\bar{r}_t'\bar{r}_{t-1}U_j$$

$$= T^{-1}EU_i'\left[\begin{bmatrix} r_t \\ r_{t-1} \\ r_{t-2} \\ \cdot \\ \cdot \\ \cdot \\ r_{t-T+1} \end{bmatrix} [r_{t-1}r_{t-2} \quad \cdots \quad r_{t-T}] \; U_j\right]$$

$$= T^{-1}EU_i'\begin{bmatrix} r_tr_{t-1}r_tr_{t-2} & & r_tr_{t-T} \\ (r_{t-1})^2r_{t-1}r_{t-2} & & r_{t-1}r_{t-T} \\ r_{t-2}r_{t-1}(r_{t-2})^2 & & r_{t-2}r_{t-T} \\ \cdot & & \\ \cdot & & \\ \cdot & & \\ r_{t-T+1}r_{t-1}r_{t-T+1}r_{t-2} & \cdots & (r_{t-T+1})^2r_{t-T+1}r_{t-T} \end{bmatrix} U_j$$

Since $\bar{r}$ is independent of U, and $Er_ar_b = 0$, $a \neq b$, and finally $Er_a^2 = 1$, we have:

$$Z_{ij} = T^{-1}E\sum_{k=t-T+1}^{t-1} U_{ik}U_{jk+1}$$

$$= \frac{T-1}{T}EU_{jt}U_{it-1}$$

where U_{jt} is the tth error of the jth equation, and $U_{i_{t-1}}$ is the $t-1$th error for the ith equation.

Returning to equation (3), we have:

$$Z = ES_t'S_{t-1} = \frac{T-1}{T}EU^{t'}U^{t-1}$$

where $U^{t'}$ is the transpose of the tth row of U, and U^{t-1} is similarly defined.

In the case of kth-order auto-regression, we would find

$$ES_t'S_{t-k} = \frac{T-k}{T} EU^{t'}U^{t-k}$$

If T is sufficiently large, this suggests that we use as pseudo-structural errors:

$$\hat{S}_t = T^{-1/2}\bar{r}_t\hat{U} \text{ and } \hat{S}_{t-1} = T^{-1/2}\bar{r}_{t-1}\hat{U}$$

where $\hat{U}$ is a set of sample structural errors and $\bar{r}_t$ and $\bar{r}_{t-1}$ are defined as above. Hopefully, $\hat{U}$ will be derived from a model estimated by consistent methods.

In what follows, we examine the asymptotic distribution of S in the absence of auto-regression. It will be shown that if r is a vector of random numbers distributed $N(0,1)$, and if the structural disturbances are jointly normally distributed, the asymptotic distribution of S will be the same as the distribution of the structural errors.

PROOF: Let U^i be the ith observation of the structural disturbances. We assume that U^i is distributed $N(0,V)$ for all i; that is, the frequency function is given by

$$F(U^i) = (2_\pi)^{-M/2}|V|^{-1/2}e^{-1/2U^iV^{-1}U^{i'}}$$

for all i. (Note that U^i is a row vector containing M elements.)

The joint moment generating function of U^i is

(4) $$M(U^i) = e^{1/2t'Vt}$$

where t is now a $M \times 1$ column vector of constants, with elements t_i, $i = 1, 2, \ldots, M$.

In the following demonstration we show that as the number of observations approaches ∞, the joint moment generating function of S is the same as (4).

The moment generating function of S is given by

$$M(S) = (2_\pi)^{-MT/2}|V|^{-T/2}\int \cdots \int e^{T^{-1/2}\sum_{j=1}^{M} t_jrU_j - \frac{1}{2}\sum_{i=1}^{T} U^iV^{-1}U^{i'} - \frac{1}{2}\sum_{i=1}^{T} r_i^2} \cdot \prod_i \prod_j dU_j^i \cdot \prod_i dr_i$$

where U_j is the jth column of U.

This may be rewritten

$$M(S) = (2_\pi)^{-MT/2}|V|^{-T/2} \int \cdots \int e^{T^{-1/2} \sum_{i=1}^{T} r_i U^i t - \frac{1}{2} \sum_{i=1}^{T} U^i V^{-1} U^{i'}}$$

$$-\frac{1}{2} \sum_{i=1}^{T} r_i^2 \cdot \prod_i \prod_j dU_j^i \prod_i dr_i$$

The term $T^{-1/2} r_i U^i t$ can be interpreted as a linear combination of the terms in U^i. It can be verified that $M(S)$ may be written

$$M(S) = (2_\pi)^{-MT/2}|V|^{-T/2} \int \cdots \int e^{1/2} \sum_{i=1}^{T} r_i^2/T \; t'Vt$$

$$\cdot\, e^{-1/2} \sum_{i=1}^{T} (U^i - r_i T^{-1/2} t'V) V^{-1} (U^i - r_i T^{-1/2} t'V)'$$

$$-\frac{1}{2} \sum_{i=1}^{T} r_i^2 \cdot \prod_i \prod_j dU_j^i \prod dr_i$$

$$= (2_\pi)^{-MT/2}|V|^{-T/2} \int \cdots \int e^{1/2} \sum_{i=1}^{T} r_i^2/T \; t'Vt - \frac{1}{2} \sum_{i=1}^{T} Z^i V^{-1} Z^{i'}$$

$$\cdot\, e^{-1/2} \sum_{i=1}^{T} r_i^2 \cdot \prod_i \prod_j dZ_j^i \prod_i dr_i$$

where $Z^i = U^i - r_i T^{-1/2} t'V$. The Jacobian of the transformation equals one.

We first integrate $M(S)$ with respect to Z^i, taking the r_i as given. The result is

$$(5) \quad M(S) = (2_\pi)^{-T/2} \int \cdots \int e^{1/2} \sum_{i=1}^{T} r_i^2/T \; t'Vt - \frac{1}{2} \sum_{i=1}^{T} r_i^2 \cdot \prod_i dr_i$$

$$= (2_\pi)^{-T/2} \int \cdots \int e^{1/2t'Vt} \sum_{i=1}^{T} r_i^2/T - \frac{1}{2} \sum_{i=1}^{T} r_i^2 \cdot \prod_i dr_i$$

Next, integrate (5), taking T as given, and then take the limit. The result is

$$M(S) = \left(1 - \frac{t'Vt}{T}\right)^{-T/2}, \text{ and}$$

$$\lim_{T \to \infty} M(S) = e^{1/2t'Vt}$$

In the case of consistently estimated models, the pseudo-structural errors

$$\hat{S} = T^{-1/2} r \hat{U}$$

will have the same asymptotic distribution as U^i, if r and U^i are distributed in the manner assumed above. Generalization of the proof about asymptotic distribution to the case where errors exhibit autocorrelation is not difficult.

DISCUSSION

FRANK DE LEEUW

URBAN INSTITUTE

One way to classify theories of economic fluctuations is by which forces they identify as the underlying ones setting an economy's dynamic responses in motion. The Slutsky-Frisch theory emphasizes stochastic disturbances—forces of the kind we often cannot measure directly or pinpoint in time. Many textbooks on macroeconomics emphasize measurable and relatively autonomous injections, or withdrawals, from the income-expenditure process: government spending, exports, tax laws, and so on. Monetarists emphasize exogenous disturbances affecting the quantity of money: changes in monetary policy, gold discoveries, bank runs, and so on. The theories have to do with which one, or ones, of these forces have been the prime movers in the past, or are likely to be dominant in the future.

The classification is useful here, since the Conference appears to have been designed with the stochastic-disturbance view exclusively in mind. There has been much attention as to how the models behave under different short-run error-term adjustments, and how they respond to simulated "typical" stochastic shocks. These are characteristics concerning which the Slutsky-Frisch theory has strong implications. In contrast, no attention has been given to which kinds of exogenous variables the models are sensitive, or to what kinds of fluctuations "typical" patterns of change in exogenous variables produce.[1] Outside of this Conference, the emphasis is largely reversed; current discussion of economic fluctuations centers mainly around which exogenous forces matter, and how they work, to the virtual exclusion of discussion of the role of stochastic disturbances.

What I would like to do here is look at the Evans-Klein-Saito paper, and some of the other papers, from the point of view of the light they shed on these alternative explanations of economic fluctuations. First, I shall discuss the question of whether the simulation results tell us anything about the validity or strength of the stochastic-disturbance theory. My conclusion is that the simulation results tell us very little, since we would expect the same kind of results if: (*a*) the stochastic-disturbance theory is valid; or (*b*) the theory isn't valid, but the models are mis-specified. The next section takes up the question of whether the models—mainly, the Wharton and OBE Models—are mis-specified, and attempts to use some comparisons of model results with single-equation "reduced-form" explanations of *GNP* fluctuations to make headway on this question. The conclusion stresses the likelihood that there are important mis-specifications in the models.

1. THE SIMULATION RESULTS

From the many simulations of the models under investigation at this Conference, three generalizations find support (though not unanimous support by each investigator of each model). First, both within and beyond the sample period, forecasts one quarter ahead are better than forecasts two or more quarters ahead. Second, forecasts which take some account of serial correlation in error terms are better than forecasts which do not. Third, simulations of the model when it is subjected to serially correlated stochastic shocks whose distribution is based on equation errors during the sample period, succeed in reproducing mild fluctuations; exactly how mild seems to be a matter of controversy. This third generalization is somewhat ambiguous in the case of the Wharton Model simulations, because the exogenous variables are not set at perfectly smooth growth rates for the stochastic simulation runs but include at least half a cycle due to an assumed

[1] In his oral presentation, George Green referred to some simulation results produced by "typical" patterns of change in exogenous variables in the OBE Model. I hope that these interesting results will be made available in written form.

Vietnam War settlement. For the OBE Model simulations, in which exogenous variables *are* set at perfectly smooth growth rates, the cycles generated by stochastic simulations are very mild. The first two generalizations hold for the OBE Model, and also for a version of the FRB-MIT Model that I worked with in late 1968.

Now, if economic fluctuations are largely the economy's response to stochastic disturbances which interact over time, then we would expect all of these generalizations to hold true for a well-specified econometric model. Predictions should improve, the shorter the prediction period; because a short prediction-period enables us to capture, in the initial conditions, the effect of past stochastic forces. Predictions should improve when account is taken of intercorrelations of error terms, because the error terms, including their intercorrelation properties, are the basic force driving the economy. Finally, stochastic simulation should reproduce the characteristics of historical fluctuations, because they reproduce the kind of impulses which, in fact, give rise to historical fluctuations. Thus, the reported simulation results might seem at first glance to provide strong support for the stochastic-disturbance theory—at least, as far as mild cycles are concerned.

Unfortunately, another set of conditions can just as easily account for these reported simulation results. Suppose that an economy responds to the forces which drive it by effects which develop gradually over time; but suppose, further, that a particular model mis-specifies the path by which these effects develop. This mis-specification could result from incorrect lag distributions, from bias in estimating the initial impact effect of some exogenous variable, or from other problems. Whatever the cause, one result ought to be better predictions one quarter ahead than two or more quarters ahead, since much of the effect of the mis-specification ought to be undone by inserting actual initial conditions of the endogenous variables in one-quarter forecasts. Another result ought to be serially correlated residuals in the sample period; taking account of this correlation ought to correct for some of the model's error and improve forecasts.

As for stochastic simulation, it might, on first thought, seem unlikely that random shocks could result in anything like historical fluctuations unless the stochastic-disturbance theory has validity. But when we remember that the simulations are based on the model's actual errors during the historical sample-period, I think success in matching the characteristics of historical fluctuations is not so surprising. The actual errors from a poorly specified model will tend to be larger than the true magnitude of stochastic forces. These large actual errors would, in themselves, lead to exaggeration of the power of stochastic forces to generate fluctuations. But if a model happens to understate the economy's response to stochastic forces, this understatement will work in the opposite direction. It is, therefore, hard to form any clear expectation of how stochastic simulation results should turn out if a model is mis-specified.

In summary, the principal simulation results reported in the Evans-Klein-Saito paper and in some of the other Conference papers could, it seems to me, just as easily result from mis-specification as from the historical validity of the Slutsky-Frisch theory.

2. COMPARISONS WITH REDUCED-FORM *GNP* EQUATIONS

Another way of stating the conclusion of the previous analysis is to remark that much of what we can say about the simulations prepared for this Conference depends on what we are willing to assume about the specification accuracy of the models. One way to form an impression of the specification accuracy of the models is to compare them with simpler, reduced-form relationships between *GNP* and major exogenous variables, such as the recently publicized relationship of *GNP* to monetary and fiscal policy variables. If these simple relationships catch certain features of historical fluctuations that a model misses, then, I think, there is some support for the proposition that the model is mis-specified. These reduced-form relationships are crude in their fixed-weight distributed lags, and in their ignoring of initial conditions; but it seems to me that these and other shortcomings put the reduced-form method under a handicap, making any indication of superiority over models all the more impressive.

The reduced-form equations I will use are those estimated by Edward Gramlich and reported in his paper, "The Usefulness of Monetary and Fiscal Policy as Discretionary Stabilization Tools."[2] He has

[2] Gramlich's paper was presented at the Conference of University Professors, sponsored by the American Bankers' Association, Milwaukee, September, 1969.

improved on earlier reduced-form equations: (1) by separating Federal government transactions into those which affect final demand directly (purchases plus grants-in-aid); those which affect household income directly; and those which operate through other channels (representing these by dummy variables) – instead of employing the usual classification into disbursements and receipts; (2) by adding exports to Federal purchases and grants-in-aid; (3) by adjusting defense expenditures to a value-added basis (that is, adding in changes in inventories of defense products); and (4) by adding a variable to represent major strikes.

Like previous investigators, Gramlich has estimated his equations in first-difference form, using the Almon polynomial-weight technique to reduce the lag problem to manageable proportions. He has tried out several monetary policy variables; I shall report on results using the monetary base, and on those using unborrowed reserves – the residuals of which he has kindly made available to me.

Since these equations take no account of initial conditions – that is, initial capacity utilization, initial unemployment rates, recent residuals, and so forth – they are most directly comparable to the "long-period" historical simulations for the Wharton and OBE Models, which also make *GNP* depend only on current and lagged exogenous variables. However, I will, in addition, compare the equations with the shorter-span simulations for the OBE Model. Unfortunately, the shorter-period simulations of the Wharton Model have not been reported in detail, so I cannot comment on them. The summary statistics presented indicate that the Wharton Model is less satisfactory than the OBE Model in these short-period simulations.

In the two recession-recovery periods of the 1950's – the 1953–54 cycle and the 1957–58 cycle – reduced-form equations do no better than the models. In fact, in the 1953–54 cycle, the models mirror the actual *GNP* path quite closely, proving slightly better than the reduced-form equations. The Wharton Model misses the 1957–58 cycle completely, and the OBE Model (both long-period and short-period simulations) indicates only the mildest of slowdowns; the reduced-form equations likewise indicate a very mild slowdown for this period.

In the 1960's, however, the reduced-form equations do capture some fluctuations that the models tend to miss. The models do not capture the 1960 decline, apart from slight declines in the second quarter of 1960, which followed the post-steel-strike inventory jump. The reduced-form equations do better—especially the one using unborrowed reserves, which correctly estimates practically no change in *GNP* from the second through the fourth quarter of 1960. The Wharton Model completely misses the 1967 slowdown; the reduced-form equations indicate, at least, a reduction in *GNP* growth, though not as much of a reduction as actually took place. Model calculations for 1968, as reported in the Evans-Haitovsky-Treyz paper, indicate a severe understatement in the second half of that year. Reduced-form equations also understate that period, but I would guess that their understatement of about $2 billion per quarterly change in *GNP* is not so severe. (Perhaps, however, the inclusion of 1968 in the reduced-form sample period heavily affects these results.)

Thus, there does seem to be some evidence in the 1960's supporting the view that the models are mis-specified. The evidence is not dramatic but, I think, it is enough to raise serious doubts as to how to interpret the simulations prepared for this Conference. Furthermore, the periods in which the reduced-form equations do better than the models suggest that it may be in the representation of the effects of monetary policy that these models are weak.

As a final point, let me note that these comments bear on only one possible contribution of this Conference; namely, its assistance in understanding the underlying causes of economic fluctuations. There are other contributions, as well; not the least of which is simple tabulation of the average errors models make when unaided by forecaster judgment. Any negative remarks about the area I have chosen to discuss are not meant to belittle these additional contributions.

BRIDGER M. MITCHELL

STANFORD UNIVERSITY

For purposes of long-run simulation, the structure of the Wharton Model can be considered as a set of 47 stochastic equations

$$y_i = G(y,x) + \epsilon_i, \quad i = 1, \ldots, 47$$

where the vectors of the endogenous variables y and exogenous variables x include both current and lagged values.

If G is a set of linear difference equations in the endogenous variables, the model can be solved to yield the values of the endogenous variables as rational lag functions of both the exogenous variables and the disturbances. The stochastic behavior of current y, given the values of the exogenous x, is then determined by the distribution of the errors ϵ_i and the estimated lag structure. In particular, for fixed exogenous values, such a model can be viewed as a linear filter operating on the random errors. In the frequency domain, the spectral densities of the output of this filter completely characterize the model's response to random shocks, obeying the assumed probability distribution, and the presence of peaks in the spectra of the outputs indicates the tendency of endogenous variables to reflect a cycle at the corresponding frequencies.

When the model contains *nonlinear* functions of the endogenous variables, this approach is not directly available. Since virtually all interesting macroeconomic models will include both real and monetary variables with endogenously determined prices, nonlinearity is the general case. One line of attack has been taken in another paper prepared for this Conference by Howrey; namely, to linearize the model around sample means and to evaluate the spectra of the endogenous variables as computed from this linear approximation.

The present paper by Evans, Klein, and Saito preserves the nonlinear features of the model by fixing the exogenous variables at the values of a control solution, drawing random numbers for values of the disturbances, and solving the model for 100 successive quarters. Fifty such experiments yield an equal number of time series for each of the endogenous variables. The authors then *estimate* the spectra of the nonlinear filter applied to the random process, treating each experiment as a random sample from that process.

Unfortunately, the paper is marred by a technical deficiency at this point. The authors have produced 50 separate estimates of the power spectrum of each of the important endogenous variables. However, as the experiments are designed to generate independent drawings from the same stochastic process with fixed initial conditions, it will be efficient to average the estimated auto-covariances from the 50 experiments, each of about 100 observations, and then compute a single estimate of the spectrum. For estimation purposes this must approach the ideal large sample in economics, and will certainly allow asymptotic confidence limits to be used to test hypotheses regarding spectral peaks at business-cycle frequencies.[1]

In lieu of this, the authors have tabulated the relative frequency of occurrence in the 50 experiments of various characteristics of the estimated spectra, such as the number of experiments having one, two, or three peaks. It is difficult to use these statistics to draw inferences about the probability of occurrence of periodic fluctuations of different frequencies in the model, since the spectrum of a particular endogenous variable has a determinate number of peaks, whereas the occurrence of estimated spectra with different numbers of peaks, or peaks at different frequencies, is an indication of the sampling variation of the estimate. In any event, the probability of a cycle of a given period occurring in a 100-quarter run of the model is not related in a simple way to the existence of significant power at the corresponding frequency. A direct estimate, of course, would be to count the number of occurrences in the time domain of cycles of the given period over the 50 experimental series.

Turning to the evidence which is tabulated for these runs, there appears to be substantial variance in the *GNP* series at a three-and-one-half- to seven-year band of frequencies. This is in striking contrast to the results from Howrey's linearized condensed version of the model, since in the latter, the longest period is only a year and a half (apart from a very low frequency component).[2]

The makings of an equally interesting difference between the linearized and simulated results appear when one is considering the stability of the model for constant values of the exogenous variables. The simulated nonlinear model's stability—in the range of appropriate values of the variables—is somewhat uncertain and calls for further investigation. To achieve a desirable base line, or nonstochastic path, for the endogenous variables required that equations relating to labor markets be more-or-less continually adjusted to keep the control solu-

[1] In estimating the spectra, the particular trend removal technique can have a major effect on the final results. In stochastic simulations of this sort, a natural procedure would be to take deviations from the nonstochastic control solution.

[2] The quantitative results from Howrey's study are based on his first-circulated computations, whose numerical accuracy he later questioned.

tion close to the target values for long-run steady growth.[3] On the other hand, the solutions as plotted on the charts, which show only one of the 50 stochastic simulation runs, do not appear to exhibit increasing variance with time. Not readjusting the labor equations for the different shocks in different experiments may account for the rather large variability seen in the chart, as compared with the nonstochastic run. The fact that this does not lead to explosive behavior may be due to damping effects of some nonlinear relations, when values depart sufficiently far from the base-line solution.

By way of comparison, Howrey has analyzed the linearized model both with, and without, the monetary sector. Both versions display explosive behavior, but the presence of the monetary equations has a decided effect in making the model more nearly stable. This, too, suggests that the stabilizing role of prices and interest rates at values away from their sample means may be stronger than one measured by purely linear equations.

I have suggested the possible role of nonlinearities as the key to the different dynamics found by these two papers, for uncovering such a structural explanation would be most interesting. However, other differences between the papers obscure such a finding. First, the linearized model is a somewhat reduced version of the full Wharton Model. Secondly, Howrey appears to assume that the shocks are independent both over time and across equations, whereas the Evans-Klein-Saito results I have referred to are based on disturbances independent over time, but having the contemporary covariances estimated from the sample. The effects of this relatively greater interdependence of the model on the cyclical behavior of the endogenous variables are uncertain a priori but could be established.

I would urge the authors of these two papers to collaborate in order to obtain comparative results from their different approaches. By use of the same model and parameter estimates, constant values for all exogenous variables, and identical assumptions about the distribution of disturbances, any differences due to nonlinearities can be isolated, and the adequacy of linear approximations in analyzing dynamic responses assessed.

[3] The emphasis on the short-run nature of the model suggests that the authors would not draw policy implications from this behavior.

McCarthy's suggested method of calculating Monte Carlo shocks, having the covariance structure estimated from the sample, provides a useful addition to our computational techniques, as one simply uses disturbances calculated as linear combinations of independent random variables, with the sample residuals as weights. I question, however, whether one wants to employ this method for generating auto-correlated disturbances in the manner that Evans, Klein, and Saito have adopted in their second set of stochastic experiments. In those runs, the generating mechanism reproduces auto-correlations up to the forty-fourth order. Most of the smoothing effects and longer cyclical periods are likely to be obtained from merely first- or second-order auto-correlation, and it is straining the data rather fine to estimate auto-correlation coefficients of all orders, as well as the structural parameters of the model.

The authors found no clear gain from using first-order auto-correlation corrections in making their short-run forecasts of the historical period. However, in view of the abundant evidence of auto-correlated data in quarterly models, it would be interesting to experiment further with this approach, employing asymptotically more efficient estimators of both the structural and auto-correlation parameters.