Ta-Chung Liu's Exploratory Econometrics

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Introducing Ta-Chung Liu

A lauded Chinese economist, Ta-Chung Liu (1914–1975) is remembered only as a remote historical figure in his home country, and few people nowadays read him. But his legacies to Taiwanese economics and the Taiwanese economy still remain. Liu helped establish the first PhD program in economics at National Taiwan University and persuaded the Taiwanese government to build macroeconometric models for policy analysis, which are still used today. During the Cold War period, while he was working at Cornell University, Liu was President Chiang Kai-shek's chief economic adviser in the 1960s. Fully trusted by Chiang, he implemented a radical tax reform that in many ways can be regarded as one of the foundations of Taiwan's economic progress. At that time, even the public had high hopes for his efforts to accelerate the modernization of Taiwan's economy. A household name in Taiwan, Liu's every arrival and departure was reported in local newspapers.

For econometricians, Liu is famously known as the precursor of two later developments in econometrics: the inquiry into ultra–high frequency data mastered by his former PhD student and Nobel laureate Robert F.

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Engle (cf. Engle 2000), and Christopher A. Sims's vector autoregressive (VAR) approach to time-series analysis. Yet Liu is regarded as little more than an interesting footnote to the history of econometrics. This is especially so because postwar econometrics is generally portrayed as the rise of the great debates between the Cowles Commission and Herman Wold's camp over interpreting structural models, and between Cowles and the National Bureau of Economic Research (NBER) on theory without measurement. Liu's works, to some extent, are thus overshadowed by those debates. His later adoption of Wold's recursive methodology might also imply that Liu is a Woldian, and his role in the history of econometrics would then be better understood in the context of Woldian econometrics versus the Cowles Commission: recursive versus structural systems in modeling, and the least-squares method versus the maximum likelihood methods in estimation.[1] However, as this essay demonstrates, a careful study of the literature reveals that even though Liu might have known of such debates in the 1950s and 1960s, he did not deliberately or intentionally mean to associate himself with the Wold camp to take a stand against the Cowles Commission approach.

From the 1950s until his tragic suicide with his wife in 1975, Liu vehemently criticized the Cowles Commission method of structural macroeconomic modeling. His criticism was based on his belief that econometric models needed to be specified as a realistic miniature of the complex economic world. In particular, as today's econometricians know, Liu believed that models were neither simultaneous nor able to be specified by using a priori theories to exclude variables. These concerns led him not only to oppose the Cowles Commission approach but also to establish his own approach to econometrics.

Much of Liu's work on empirical econometrics can be seen, on the one hand, as being derived from his early empirical practices, including measurements of national income in the 1940s. Liu first addressed the problem of econometric modeling in a 1950 article on measuring consumption and investment propensities of the United States (Liu and Chang 1950). His earliest attempt to model the macroeconomy with monthly data was subsequently published (Chang and Liu 1951).[2] During the 1950s Liu's published econometric works were purely empirical, including

1. For discussion on such debates in the history of econometrics, see Morgan 1991 and Qin 1993 (esp. pp. 164–74).

2. Perhaps the intellectual environment at Johns Hopkins University played a role in Liu's research. During his stay at Johns Hopkins University, his quantitative-minded colleagues included Carl F. Christ, Evsey D. Domar, Arnold C. Harberger, and Simon Kuznets.

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computations of measures of variables, and uses of these measures to construct models to account for functional relationships among economic variables. On the other hand, Liu's own empirical econometric models arose in the process of exploring possible ways to model the macroeconomy. His econometric approach can be described as "exploratory." He used such an adjective in several ways. While it usually meant the preliminary and "experimental" nature of the processes and findings of econometric models subject to modification in future trials, Liu first used the term in 1955 to refer specifically to the use of structural models in an initial stage of forecasting model construction. As the present essay demonstrates, *exploratory* well describes the evolution of Liu's econometric methodology and practices concerning methods and models for quantitatively approaching macroeconomic reality, especially for the problem of identification.

The present essay studies Liu's contribution to econometrics by focusing on the methods and models of his econometric practices from the 1950s to the 1970s. We document Liu's search that led him to accept the recursive methodology represented by Wold. Liu "discovered" the Woldian recursive system in his long search for a "satisfactory empirical model" for time-series data.[3] And he did so, as we discuss in more detail below, by experimenting with tools and elements in the model-building process—"exploratory" in a methodological sense. We also discuss Liu's theoretical and empirical concerns for econometric models that spurred his involvement in a series of debates in the 1960s and 1970s. We briefly compare Liu's approach to VAR approaches. Finally, we methodologically explicate Liu's econometric approach.

Early Econometric Practices and the Problem of Identification

Although a true econometrician, Liu was not trained as such. He first entered Cornell University and received his master's degree in civil engineering in 1937, with a thesis on the stress that railroad cars put on railroad tracks. He then switched to pursue a PhD degree in economics, also at Cornell, and graduated in 1940. His thesis, "A Study in the Theory of Planning by the Individual Firm under Dynamic Conditions," deals with the microtheoretical modeling of a firm's behavior. One chapter of his

3. In the sense of Morgan 1988.

thesis, "The Bearing of Risk and Uncertainty," is an early evaluation of Ronald Coase's (1937) static analysis. His thesis was supervised by Donald English, and the thesis committee members consisted of Fred A. Barnes, Paul T. Homan, and Paul M. O'Leary, while Fritz Machlup and Frank Knight were acknowledged in the preface.

Whereas there was nothing empirical in Liu's dissertation, he soon devoted himself to the quantitative study of the economic situations of contemporary China during World War II and after, while working at the Chinese embassy in the United States and later serving as an official of the Chinese mission to Bretton Woods in 1944. Liu published two articles on national income measurement in the 1940s (Liu 1946; Liu and Fong 1946) before he returned to China to teach at Tsinghua University in Peiping (Beijing) in 1946. But not until 1948 did he start to teach himself econometrics, when he moved to the International Monetary Fund in Washington, D.C., because of the outbreak of the Chinese Civil War.[4] From 1949 to 1958 Liu was also a visiting lecturer of mathematical economics and taught econometrics at Johns Hopkins University, where he was reunited with Machlup and Simon Kuznets. The latter was Liu's old acquaintance; they met as early as 1946 during Kuznets's advisory mission to China about plans for postwar economic reconstruction.

Liu first encountered the issue of identification in his 1950 article on measuring the consumption and investment propensities of the United States by using annual data (Liu and Chang 1950). His model was a simple Keynesian system consisting of a consumption function, an investment function, and a goods market equilibrium condition. But he treated the consumption and investment functions as a single equation estimated by the ordinary least squares (OLS) method. Liu did realize, however, that his model was an oversimplification, and any application of it to the real world was "necessarily experimental in nature" (576). The identification problem was considered (579–80), as Liu also derived a reduced-form model from a just-identified structural model. This finding can be regarded in the history of econometrics as one of the very early studies on the insignificance of the OLS bias in small samples, as the income coefficients in both the consumption and the investment functions—representing the marginal propensity to consume and the marginal propensity to invest—estimated by a system of single equations were the same as those estimated by reduced-form equations. Liu concluded that the single-equation approach

4. Based on the reminiscence of Sho-Chieh Tsiang (1976).

was superior, since it was simpler than the methods applied to the structural model (582).

Several comments on this paper were published in 1953.[5] One interesting comment, that the Liu-Chang model contains no theoretical consideration, came from Francis M. Bator (1953, 141). He criticized the model as a "'Baconian' inductive-statistical approach," indicating that Liu and Chang were "running correlations between aggregate magnitudes with no attempt at a theoretical foundation, looking for higher and higher correlation coefficients." To take the consumption function as an example, it is because Liu and Chang used gross national product (GNP), instead of disposable income, as the determinant of the aggregate consumption expenditure. They did so for the reasons that the GNP and personal income were highly correlated: using the former would simplify the model. But Bator argued that a high correlation between GNP and disposable income did not necessarily guarantee that the consumption function was invariant with respect to the change in the gross national income–disposable income relationships. Hence their consumption function was useless for predicting behavior and testing hypotheses.

"Baconian" may be inaccurate philosophically to describe the Liu-Chang model, but Bator's criticism demonstrates that over the entire time Liu explored econometrics, economic theories played no significant role in his econometric models. And to many extents, the Liu-Chang paper set the tone for Liu's later econometric approach. In addition to the identification problem, Liu and Chang pointed out a crucial point in time-series model specification, that is, whether the relations were dynamic, as lagged variables were included in the regressions. They complained that existing studies did not seriously take lagged variables into account. They argued that, first, numerous experiments were required to introduce significant lagged variables, and second, most important lags were shorter than a year (Liu and Chang 1950, 569–70). As a result, Liu and Chang subsequently published an article on monthly estimates of certain macroeconomic variables for 1946–49 (Chang and Liu 1951). In this article they argued that only a few published models, such as Lawrence R. Klein's Model II, included lagged annual variables (Chang and Liu 1951, 225). However, without further justification, such an inclusion was only superficial. They went on to state that the reason for introducing lagged variables was due to the consideration of nonsimultaneity in light of asymmetrical temporal

5. By Francis M. Bator, Thomas Mayer, and Gardiner C. Means in *American Economic Review* in 1953.

relations between causes and effects. For instance, when considering economic relations such as the consumption function, they argued as follows: "By the nature of the economic process involved, all of the consumption expenditures out of a given income cannot occur simultaneously with the receipt of that income. Some lagged relation between income and consumption expenditures is bound to exist" (225). Hence their monthly estimates could serve as a basis for studying a consumption function consisting of lagged variables. In fact, Chang and Liu used their monthly data to run several regressions, but they were cautious about the results obtained by the least-squares method: "Most economic time series are highly autoregressive, especially those with a short unit time-period (in the present case, one month). Results obtained by correlating such time series by the classical method could be very misleading" (225). This led Liu to seek alternative methods.

In 1955 Liu published a paper on forecasting the U.S. economy, representing his first systematic treatment of underidentification and the objection to the Cowles Commission approach.[6] In practice, this article suggested a two-stage estimation process. At the first and "exploratory" stage, structural equations consisting of all relevant variables were established to find predetermined variables that are statistically significant. Liu thought economic theories dealt only with structural forms rather than reduced forms, so theories were imported to provide useful information for specifying the model. He called them the "exploratory structural relationships" (Liu 1955, 436). Liu also realized that exploratory structural relationships oversimplified the economy, but they remained useful for singling out the predetermined variables. At the second stage, these predetermined variables were used as regressors in a single-equation least-squares regression model—Liu called it a "least square reduced-form equation"—for forecasting the future values of endogenous variables.

One important point in this paper was multicollinearity.[7] In any model containing a vast number of variables, some variables would be highly correlated. Such multicollinearity harmed structural estimation. But since Liu's purpose was to forecast, he turned multicollinearity into an advantage. He also claimed that his method relied entirely on multicollinearity (Liu 1955, 437). The reason is, in the case of the consumption function,

6. In his three appendixes attached to the article.

7. The term that Liu used is "collinearity." But what he really meant, in modern terminology, is multicollinearity, referring to correlation among the explanatory variables in multiple regression.

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> there must be scores, or even hundreds, of variables that have a bearing on consumption expenditures; but they tend to move in a certain manner with disposable income, or cash holdings, or defense expenditures, or some combinations of these variables, so that their influences have been attributed to the three variables through the mechanism of statistical calculation. As a result, *the three variables successfully explained practically all the variations in consumption*. (457; emphasis added)

By then, Liu had formulated his main idea on econometric modeling and the identification problem. Two most important concerns had emerged. One was the need for a large number of variables included in a typical macroeconometric model; the other was the existence of asymmetrical causal relations between variables that econometric models were deemed to capture. Liu had realized in his study of monthly time-series data that by adding lagged variables measured by a shorter unit of time, both concerns could be resolved. But empirically Liu had yet to build a realistic model for explanation; only models for forecasting were concerned.

Liu's "Disturbing Argument"

Liu had decided to attack the Cowles Commission approach theoretically as early as 1957.[8] One attack appeared in his famous 1960 *Econometrica* article, which was originally presented at the Econometric Society meeting at Chicago in 1958 in the symposium "Simultaneous Equation Estimation—Any Verdict Yet?" The panel consisted of Carl F. Christ, Clifford G. Hildreth, Lawrence R. Klein, and Liu. In a sense, this symposium can be seen as Liu versus three Cowles people.[9] Yet the symposium also reflected the uncertainty among econometricians over the empirical advantages of the Cowles Commission approach compared with the single-equation least-squares method at the time (Morgan 1991, 246). Another article in which Liu attacked the Cowles approach (Liu 1963a) is less known to Western econometricians, as it was published by Taiwan's National Tsing Hua University (the Chinese Nationalist government's reinstatement of the original Tsinghua University in Taiwan in 1956); it can be regarded as the full version of Liu 1960, with a more detailed account of his criticism of the Cowles Commission approach.

8. See Liu 1963a, 157.

9. Despite the fact that Christ's early work on large-scale macroeconometric models had been harshly criticized by Klein. See Christ 1951 and Klein's subsequent criticism.

Both articles started with stating the conditions for identifiability, and both took Klein's models as representative of the Cowles Commission approach. Liu (1960, 855) in the first place stated a *rank condition*, but then discussed the problem of identifiability in terms of *order condition*. It was known to econometricians that the order condition is only a *necessary condition* for identification, while the rank condition is a *sufficient condition*. But in Liu 1963a he did not consider the rank condition; he used only the order condition to analyze the situations of just-, over-, and underidentification.[10] Furthermore, in Liu 1963a there was a simple Keynesian model that contains a consumption function, an investment function, and a total expenditure function, for illustration. Through this model, Liu demonstrated that when more relevant variables were introduced subsequently into the model, the equation, say, the consumption function, turned from being overidentified to being just-identified, and finally to being underidentified. He then asserted, "All structural relationships, if their true form can ever be revealed, tend to be underidentified" (Liu 1963a, 160).

The condition for identification in Liu 1963a can be best illustrated using Christ's (1966, 320–27) notation. In a model with G structural equations, G endogenous variables, and K exogenous variables, for a structural equation containing H endogenous variables and J exogenous variables to be identified, the number of exogenous variables excluded from this equation ($K - J$ in number) should be at least as great as the number of endogenous variables less one:

$$K - J \geq H - 1. \tag{1}$$

This is known to econometricians as the order condition for identification and is what Liu had in mind. Liu stated (also in Christ's notations) that

A structural equation is overidentified if $K - J > H - 1$.
A structural equation is just-identified if $K - J = H - 1$.
A structural equation is underidentified if $K - J < H - 1$.

Liu's argument for the ubiquity of unidentification and the rareness of just- or overidentification followed straightforwardly from the identification conditions: it is more likely that the right-hand sides of the conditions are greater than the left-hand sides. To make his case, Liu needed to demonstrate that either H or J is greater or K is smaller than commonly

10. Liu (1963a) referred to just-identification as "exact-identification."

perceived. Liu's criticisms can be summarized as follows. First, Liu argued that the economy was complex; there were more rather than less variables that had influences on the variable to be explained (hence *H* should be large). He surveyed the studies of investment in the United States and found that these investment equations have different explanatory variables. These investment functions were those in Tinbergen 1939, Ezekiel 1942, Clark 1949, Klein 1950, and Klein and Goldberger 1955. Liu (1963a, 226) also found that, among these studies, the most common explanatory variable was profit (in various forms), which was included in all but Clark 1949. The existence of nonconsensus on which set of explanatory variables should be included in the investment function reflected the fact that a great number of explanatory variables, which should have been included in the equation, were excluded to make the equation overidentified. Moreover, the economic world should be recognized as a system in which only a few variables could be regarded as truly exogenous (hence *H* should be large and *K* should be small). Usual exogenous variables in empirical studies, such as tax rates and populations, were only "relatively so," compared with other variables (Liu 1963a, 227). As Liu claimed, "Except for weather conditions and natural calamities, one can think of very few factors that are not more or less influenced by the economic system" (227).

Second, the Cowles Commission approach justified a priori restrictions by resorting to "economic theory." But, for Liu, while the Cowles Commission thought that economic theory provided more information for identifying the model, he believed the contrary was true: "One would be disregarding relevant economic theory and *a priori* information and over-simplifying economic reality if one should introduce the so-called '*a priori* restrictions' to preclude variables which obviously would have important influences on the variables being explained" (226). He famously stated,

> It is important to realize that unreasonable magnitudes (or signs) of the structural coefficients of the included variables can be removed by *adding* relevant explanatory variables as well as by *dropping* variables. When a "reasonable" structural relationship could be obtained either by dropping variables from, or adding variables to, an over-simplified relationship, the complexity of the modern economy ensures that the "enlarged" estimate is a closer approximation to reality than the two simpler ones. (Liu 1960, 858–59; original emphasis)

But note that Liu did not want to make the "ridiculous" suggestion that every variable should depend on every other variable so that all variables should be included in the model. He pointed out that in *all* the existing empirical equations, the margins of overidentification were small so that it was necessary to include only a tiny set of variables to make the model become unidentified (Liu 1963a, 227).[11]

Third, Liu regarded certain ways of satisfying the identification condition as rather superficial, that is, they did not help solve the identification problem. Liu did not think the process of disaggregation that reduces the number of joint determined variables necessarily helps, because, as Hildreth and F. G. Jarrett (1955, 24–25) pointed out in their book on the statistical estimation of livestock production and demand, treating consumer income and general price level as exogenous would contrast with the fact that the income generated by livestock production was part of total income. Hence the latter (total income) was not independent of the relations in the livestock model. All they could do was to "hope" the bias would be small to make their assumption of independence harmless.

Similarly, neither would the use of higher-frequency data necessarily help. This seems to contradict Liu's well-known position on using higher-frequency data to resolve the simultaneity problem, but the point was really to counter Klein and Harold Barger's (1954) quarterly model of the U.S. economy. Recall that Liu constructed monthly data for certain variables in Chang and Liu 1951 based on the idea that using high-frequency data would introduce more lagged variables and hence add more predetermined variables to the model, since long-run endogenous variables would become predetermined in the short run. Yet he argued that so doing would not necessarily solve the problem because, as Liu contended, there would be serial correlation in the disturbance terms so that the lagged endogenous variables cannot be treated as exogenous variables. While Klein and Barger (1954, 416) were in fact trying to solve the problem of serial correlation of disturbances by imposing an autoregressive process of order three on disturbances, Liu (1960, 861) thought Klein and Barger's attempt was not satisfactory, since it resorted to "a confession that an economic explanation for a systematic (nonrandom) part of the movement in the variable to be explained has not been found." Moreover, he cited Guy Orcutt's (1948) work to demonstrate that it was the omission of relevant variables that caused the existence of serial correlation in disturbances (Liu 1960, 861): "Economic variables tend to be serially correlated. When relevant explanatory variables are omitted from a

11. Liu further stated this point in Liu [1974] 1976. See below.

structural equation, their effects on the dependent variables are left with the unexplained residuals. As a result, the residuals are serially correlated." Following Marc Nerlove and William Addison (1958), Liu (1960, 863) suggested including more lagged, but not current, variables to reduce serial correlation in residuals.

Finally, as discussed above, Liu (1955) argued that we should include more variables on a single equation despite the problem of multicollinearity. Liu (1960) argued similarly that we should accept the least-squares biases (first discovered in Haavelmo 1943), because Richard Foote and Frederick Waugh's (1958) Monte Carlo simulation had demonstrated that adding more jointly determined variables to the model would improve the efficiency (Liu 1960, 861). In the terminology used in measurement literature, this means that Liu traded accuracy (low bias) for precision (low variance).

Consequently, Liu's argument was described by Franklin M. Fisher (1961, 139) as "disturbing" because "its premises apparently cannot be doubted" and because it implies that "the hope of structural estimation by any techniques whatsoever is forlorn indeed."

Disturbing as it was, Liu's criticism inspired a few responses in the 1960s. For instance, in the 1970s, Denis Sargan (2001, 167) thought that Liu's criticism was only important for small models because "in models of any size, there are usually sufficiently large blocks of variables excluded from each equation to make the equation identified with any specification that the economist would consider reasonable." Apart from the objection from prominent Cowles econometricians such as Klein, and the papers by Wilford L. L'Esperance (1964, 1967), who tried to evaluate the predictive powers of econometric models formulated by Cowles's and Liu's approach, only Fisher took Liu's criticism seriously. Fisher not only provided constructive criticism but also formulated his own econometric method of block-recursive models that was claimed as a go-between of Liu and the Cowles Commission. They are discussed in more detail in the next two sections.[12]

Criticism 1: Lawrence Klein

As we illustrated above, by the mid-1950s Liu started to consider the problems of simultaneity and underidentification, and constructed a

12. The block-recursive models are also central to the program of Albert Ando, Fisher, and Herbert Simon (1963).

simple forecast model (Liu 1955) as an attempt to seek an alternative to the Cowles Commission models. This directly confronted Klein, who was well known as a representative of the Cowles Commission methodology in the applied realm and had already established empirical models, including the Klein I–III models (Klein 1950), the Klein-Goldberger (1955) model, and, particularly, the Klein-Barger (1954) quarterly model of the United States. Klein's empirical models, consisting of interpretation of interdependent structural relations in terms of simultaneity as well as the application of the maximum likelihood methods of estimation as opposed to the least-squares method that Liu preferred.

Subsequently, Klein (1956) argued that both can also apply to the problem of forecasting. It is not only because a forecasting model needed to be a reduced-form model derived from a system of structural equations but also because the property of efficiency in the maximum likelihood estimates of structural parameters was unchanged when being transformed to reduced-form coefficient, while the least-squares estimates were not. These ideas led Klein to object to Liu's (1955) simple forecasting model. Klein demonstrated that Liu's claimed trade-off between efficiency and bias by applying the least-squares method on single equations did not exist, because Klein formally proved that the more a priori restrictions imposed on the system, the more efficient the estimates of reduced-form parameters are (the variances become smaller). As a result, Klein (1956, 8) opposed Liu (1955) by criticizing him, posing the "seemingly paradoxical proposition" that estimating the reduced-form parameters by the least-squares method without a priori restrictions would lead to a smaller forecasting error.

Later, in the same 1958 symposium in the Econometric Society's Chicago meeting where Liu presented his famous critique (mentioned above), Klein and Liu had a chance to meet face-to-face to debate the nature of econometric models and a priori restrictions. Klein (1960) opposed Liu to argue that a priori restrictions were regarded as the vehicle for the progress of econometric modeling. He judged that building better knowledge of economic institutions into a priori restrictions resulted in "fifty per cent" of improvement in precision in econometric judgment, while only "five or ten per cent" results from advanced methods of statistical inferences (876). Klein stated: "The adoption of more powerful methods of mathematical statistics is no panacea" (896). Consequently, Klein argued that the econometric reality was overidentification: "Contrary to Professor Liu's contention, I believe that the general rule in realistic econometric models is heavy overidentification" (870).

It seems that Liu was not persuaded. His work on identification (discussed above) can be seen as he stood firm by his methodological position of underidentification and replied to Klein's criticism. Although Liu applied the Cowles Commission method in his (1963b) article (discussed below), he never really regarded the Cowles Commission method of simultaneous-equations model as a right way for econometric modeling, perhaps because of the difference in methodology and worldview. Klein, representing the Cowles Commission approach, is concerned with the mathematical model problem as he investigated the rank and order conditions of identification and sought to logically recover parameters from the simultaneous-equations model. Liu, in contrast, was more interested in the statistical data problem of the model's correspondence with reality discussed above, which is more elaborated on in the final section.

Criticism 2: Franklin Fisher

Fisher (1961) accepted Liu's criticism of the Cowles Commission method, yet he showed that an equation system in block-recursive formation was usually "good enough" because specification errors resulting from false a priori restrictions were often negligible, as they would close to zero. Hence Liu's criticism, though generally true, would not undermine the validity of structural models.

Fisher presented the block-recursive system as the following. Let the whole system of equations be represented by

$$Ax = u, \tag{2}$$

where A is an $m \times n$ coefficient matrix with $m < n$, x is an $n \times 1$ vector of variables, and u is an $m \times 1$ vector of disturbances.[13] The system can be rewritten as

$$A = [B \mid G],$$

and

$$x = \left[\frac{y}{z}\right],$$

where B is a square nonsingular matrix of rank m, y is an $m \times 1$ vector of endogenous variables, and z is an $(n - m) \times 1$ vector of exogenous variable. Now, when the matrix B takes the following "block triangular" form,

13. It is assumed that there are no lagged terms and all variables in vector x are for the same period t.

$$B = \begin{bmatrix} R^1 & S^1 & & & & & \\ 0^{21} & R^2 & S^2 & & & & \\ 0^{31} & 0^{32} & R^3 & S^3 & & & \\ . & . & . & . & & & \\ . & . & . & . & . & & . \\ . & . & . & . & . & & . \\ 0^{K1} & 0^{K2} & 0^{K3} & . & . & & R^K \end{bmatrix},$$

the system of equations in (2) are of block-recursive structure. The B matrix above has the following properties: the R^i are nonsingular (and hence square matrices) and the S^i are matrices that may or may not be zero. The 0^{ij} are zero matrices with $r(0^{ij}) = r(R^i)$ and $c(0^{ij}) = c(R^j) = r(R^j)$, where $r(R^i)$ denotes the number of rows in matrix R^i and $c(R^i)$ denotes the number of columns in R^i. Note that each of the submatrices of B,

$$R^k, \begin{bmatrix} R^{k-1} & S^{k-1} \\ 0^{kk-1} & R^k \end{bmatrix}, \begin{bmatrix} R^{k-2} & S^{k-1} & . \\ 0^{k-1\,k-2} & R^{k-1} & S^{k-1} \\ 0^{kk-2} & 0^{kk-1} & R^k \end{bmatrix}, \ldots, B$$

is square and nonsingular and is block triangular. To see the causal properties of a block-recursive system, we partition u and y into k corresponding blocks:

$$u = \begin{bmatrix} u^1 \\ u^2 \\ u^3 \\ . \\ . \\ . \\ u^k \end{bmatrix}; \; y = \begin{bmatrix} y^1 \\ y^2 \\ y^3 \\ . \\ . \\ . \\ y^k \end{bmatrix}.$$

For a block-recursive system as above, the variables in y^k are determined solely by the block of equations corresponding to R^k and the exogenous variables in z; the variables in y^{k-1} are determined by the variables in y^k, the exogenous variables in z, and the block of equations corresponding to R^{k-1}; and so forth. Within such a system, the variables in any y^i are exogenous to the jth subset of equations (corresponding to y^j) provided that $i > j$, or to any union of such subsets. Accordingly, if all the equations that involve the variables have been included in the system and the system itself correctly specified, the parameters of the subset of equations with the jth subset may be estimated with regard only for the equations in that subset and without regard for the existence of the remaining equations.

For Fisher (1961), this block-recursive system might well approximate the true world. That is, it might be acceptable to break down a complete system into subsets of equations by assuming certain variables exogenous that are only approximately so and assuming certain variables absent that truly appear with very small coefficients. As a result, the structural estimation might be entirely possible in general, so that discussion and criticism must be directed toward the goodness or badness of the approximate assumptions in a particular case and not toward the truth or falsity of them. For good-enough approximations, the use of a priori restrictions leads only to negligible inconsistencies in the estimates. More specifically, "the restricted estimates of the reduced form obtained from structural equation estimates converge more rapidly to probability limits that differ slightly or negligibly from the true reduced form coefficients than the unrestricted least squares reduced form estimates converge to the true reduced form parameters" (Fisher 1961, 149). Thus, provided that the approximations are good enough, the efficiency properties of simultaneous-equations estimators will more than compensate for their inconsistency.

In sum, even though Fisher admitted the validity and the importance of Liu's criticism on the Cowles Commission approach, by invoking the possible "near block recursive" structure of the contemporaneous relations among the variables in the whole sociophysical universe, he posited that the simultaneous-equations estimation of subsets of equations was permitted and should produce reasonable results in many circumstances.

The Road to Recursive Models

If any hope of structural estimation was forlorn, then what did Liu propose? His first step was rather surprising, since he retreated to a Cowles

Commission structural-equations model, which he referred to as the Tinbergen-Klein tradition.[14]

In Liu 1963b—which was written in 1961—the modeling process proceeded with a simple form of investment function with five explanatory variables. It was followed by a basic form of the investment function, which was derived by adding more variables into the function by considering the "complications" caused by such elements as "past commitments," "expectations," and "nonlinearity and asymmetry." Using this basic form as the foundation to other functions, Liu went on to build a thirty-six-equation structural model. The U.S. time-series data from the third quarter of 1947 to the fourth quarter of 1959 (fifty observations) were used to estimate the model by applying the least-squares method on each reduced-form equation. Liu found that the equations fitted well as the R^2 values were very high; the results were close to the two-stage least-squares (2SLS) estimates, except that the 2SLS estimate of the marginal propensity to consume was unsatisfactorily smaller than the least-squares estimate. Liu continued in the second part of the paper by providing eight simulations to investigate the influences of government policy on certain variables under different assumptions. Liu (1963b, 335) stated that his simulation results were "crude and experimental," just as his empirical models were "crude and exploratory" (301), meaning that such empirical model specifications and simulation results are intrinsic to involving many trials, and the conclusions were hypothetical.

But it should be forcefully noted that, though the model in this article was nonrecursive, he vehemently objected to this type of model, as stated in appendix C of Liu 1963b, "Methodological Issues": "While the exploratory model presented here is constructed in the Tinbergen-Klein tradition, the present author has a strong reservation about this type of structural estimation" (346). Such a reservation is understandable, as we have shown in our previous discussion of Liu's position on the structural models and the problem of identification. By now, Liu discovered and embraced Wold's recursive approach wholeheartedly, as if he adopted the way suggested by Fisher (1961). He claimed, "In its true nature, . . . an economic system is necessarily recursive" (Liu 1963b, 345). And Liu

14. Liu, however, did not realize the methodological difference between Tinbergen's disequilibrium process analysis and Klein's simultaneous-equations analysis, which is an equilibrium approach. Wold's recursive system that Liu proposed is more similar to Tinbergen's than Klein's. See Morgan 1991.

was ready to build recursive models for the macroeconomy based on the methodology of Wold and Robert H. Strotz, especially Wold's conception of causation.[15]

Liu finally built his own recursive model and published it in 1969. He saw his monthly recursive model as superior not only to the simultaneous-equations model built on annual and quarterly data but also to Strotz's and Wold's recursive systems. This is because, as he argued, his model had the least specification error on causal direction, as it involved the smallest time unit available, and his model might not have greater serial correlation in the disturbance terms than models with longer units (Liu 1969, 1–2).[16]

In practice, the central issue of estimating a monthly model was to determine the lag structures of (1) the lagged responses of dependent variables to the explanatory variables and (2) serial correlations in the disturbance terms. Liu's monthly recursive model contains thirty-three equations in which sixteen were estimated (what Liu called "recursive structural relationships" [7]) and seventeen were definitional and behavioral. Unlike a typical recursive model consisting of the triangular coefficient matrix, Liu set the coefficient matrix attached to dependent variables as diagonal. This indicates that variables were not contemporaneously causally related to each other. Hence his model is recursive in the specific sense that all variables on the right-hand side of the equations are one-period time lagged, except for time trend. For estimation, Liu first applied the OLS method to all equations and applied the Durbin-Watson statistic to test first-order serial correlations in disturbances in this two-stage process. If first-order serial correlations in disturbances existed, then it was necessary to transform the variables and test again. The U.S. monthly data for 1948–69 were used for estimation.

Liu's final effort to construct a monthly recursive model was published in 1974 in a joint paper with his student Erh-Cheng Hwa (Liu and Hwa 1974).[17] This is perhaps Liu's first and last application of the maximum likelihood (ML) method. It should be noted that early versions of the Liu-Hwa monthly recursive model had been circulated among econo-

15. See also Gilbert and Qin 2006.

16. For Liu (1969, 1), weekly data were not available.

17. One of Liu's unfinished projects is the application of his approach to regional econometric models. The work was carried on posthumously by his coinvestigators Sidney Saltzman and Hua-Shan Chi and published in 1977 as "An Exploratory Monthly Integrated Regional/National Econometric Model."

metricians, and one of them was included in Gary Fromm and Klein's (1973) comparison of eleven U.S. macroeconometric models. As revealed in Liu's comment on Fromm's (1974) survey, the version of the Liu-Hwa model that Fromm reviewed used the OLS method, and Liu still showed his contempt for the ML method of estimation (417).

Fromm and Klein compared these models' predictive power (both within and outside the sample period) for the four important macro variables: GNP in current dollars, GNP in constant dollars (1958 dollars), GNP deflator, and unemployment rate. According to the root mean square errors (RMSEs) reported in their tables 1–4, the Liu-Hwa model performed admirably well in the within-sample forecasts (1–8 quarter forecasts for the 1961:1–1967:4 simulation period), usually among the top three performers in the predictions of nominal GNP, real GNP, and GNP deflator. The Liu-Hwa model also produced very good outside-sample forecasts, as revealed by the generally low values of RMSE.

In the published version of the Liu-Hwa model (1974), there were improvements from their previous version, one notable difference being to abandon the OLS method adopted in Liu's (1969) recursive model and turning to David Hendry's (1970) computer program and the ML method (Liu and Hwa 1974, 330). The computer program is Hendry's GIVE; its 1970 manual used by Liu and Hwa is the precursor to a later version prepared by Hali Edison, Mary Morgan, and Frank Srba.[18] One reason for Liu and Hwa to adopt Hendry's approach was to take care of the problem of serial correlations in disturbances. In Liu's (1969) two-stage method, it required performing Durbin-Watson tests iteratively on the residuals. But Hendry's method allowed Liu not only to estimate both the regression coefficients and the serial correlation coefficients at the same time (Liu and Hwa 1974, 30) but also, as an advantage of the ML method, to reach more efficient estimates of both types of coefficients.[19]

Liu versus Sims

Liu [1974] 1976 delivered two consistent attacks on the Cowles Commission in his comment on Fromm's (1974) survey article on U.S.

18. Personal communication with Hendry, 19 October 2010; personal communication with Morgan, 20 October 2010.

19. Hendry's own research using GIVE was not published until 1974 in *Econometrica* (personal communication with Hendry, 19 October 2010). The 1974 paper deals with the issue of simultaneity and serial correlation in an aggregate demand model.

macroeconometric models.[20] To start with, unlike the Cowles Commission people, Liu was skeptical about the effective role that economic theories would play in model identification and specification. He listed several reasons. The first was that theories were usually developed without prior testing of them against empirical data, or lacked the properties that are statistical testable. Second, time-series data were not obtained from controlled experiment, and thus "the procedures used in estimating the equations can never fulfill all the assumptions required for rigorous statistical testing of a theory" (Liu [1974] 1976, 417). Third, the size of time-series data was usually not large enough to determine the correct theory among empirically equivalent ones. Finally, as he observed from Fromm's article, none of the surveyed models used "the Cowles Foundation maximum likelihood methods." The majority of the models used the OLS method, while only two models used the two-stage least-squares methods (Liu [1974] 1976, 417).

Liu also briefly clarified his 1960 criticism of identification. He thought his argument was misconstrued by Fromm and others as a claim that a true structural relationship was not identifiable because they must not contain a finite number of explanatory variables. Yet Liu stated that he argued only for the fact that the rank and order conditions would not be fulfilled whenever one tries to model such relationships, so that the true structural relationships are usually underidentified.

These points proposed by Liu are closely related to Sims's VAR approach.[21] The comparison and contrast of Liu's and Sim's approaches, as Robert Engle recalled in his "*ET* Interview" with Francis Diebold (Engle and Diebold 2003, 1162), were that

> T.C. [Liu] wanted to get into higher frequency modeling: he wanted to build recursive models. I suppose this is in fact the vector autoregression (VAR) idea in another guise. . . . I don't remember him complaining about the need to find new instruments, and so forth. He was concerned about what is the best collection of instruments, and that sort of thing, but it wasn't like the way it's presented in the VAR literature, in which nothing is assumed exogenous. I never remember him saying that.

The VAR models, in which all variables in the system are treated as endogenous, were regarded by econometricians as atheoretical. However, Sims's (1980) VAR model is essentially a small-scale reduced-form model,

20. Fromm's paper was first presented in 1972.

21. For the history of the VAR approach, see also Qin 2011.

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which contrasts drastically with Liu's empirical models. The number of variables (equations) in a VAR model is usually small, for example, six variables (equations) in Sims 1980. Since the focus of VAR analysis is on the dynamic relationships among the variables, not the parameter estimates, Sims claimed that the problem of incredible identifying restrictions plaguing the Cowles Commission had been avoided. However, to be able to provide empirical evidence about the response of macroeconomic variables of interest (e.g., real output and price level) to various autonomous shocks (e.g., monetary shocks and technology shocks), a main purpose of VAR analysis, the identification of various autonomous shocks is essential. To this end, some identifying restrictions on the model are still required. Here, to avoid employing incredible a priori restrictions, Sims (1980) joined Liu in invoking Wold's contention of unilateral causation by assuming that the contemporaneous correlations among the variables in the model are with recursive structure. However, the invocation of the recursive structure per se does not guarantee the complete solution of the identification problem. In particular, if the data sampling interval is not fine enough to reveal the true recursive causation structure of the system, the problem of simultaneity and underidentification still remains. This is the reason that the early VARs were criticized as inadequate for policy analysis and for Sims's (1982, 1986) switch to structural VARs. In structural VARs, theoretically based restrictions, which may not exhibit any recursive structure, are required for identifying various autonomous shocks. The structural VAR models thus face the similar criticism raised by Liu as that confronting the Cowles Commission approach.[22]

The Exploratory Approach to Econometrics

From our illustration of the development of Liu's econometric thought, we can see that his final econometric model (Liu and Hwa 1974) differs drastically from his original ideas of applying OLS method and an approach rooted in the single-equation approach. In the history of science, it is natural to see a scientist develop his or her theory gradually. But the evolution of Liu's methods may contain something extraordinary to contemporary econometric approaches. Indeed, Liu shared with other econometricians the idea of admitting the impossibility of conducting controlled experiments in economics, and thus data analysis was

22. See, for example, Cooley and Dwyer 1998.

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used as an alternative.[23] For Liu, econometrics required experimentation with many if not all methods and models, as Engle's quote above (Engle and Diebold 2003, 1162) describes: it is the best collection of instruments that is concerned. To wit, Liu differs from his contemporaries in regarding economics as an observational science (Sims 1987) and econometric techniques as observational tools.[24] In Liu's exploratory econometrics, econometricians are scientists: in contrast to the image of working in an observatory, they work in an *exploratory* in which econometricians learn about the scientific research by performing tasks and using tools. As the real world is so complex that econometric models cannot be identified by, for instance, imposing rank and order conditions, one can gain better understanding by practicing with tools and methods under difference circumstances. This is perhaps why Liu, while rejecting the concept of simultaneity, still used Cowles modeling approaches in his early works.

Liu's approach also demonstrates that econometrics is advanced by practicing: one can reach the best model only by continuously experimenting with concepts, methods, procedures, and ingredients until the results are in conformity to the real-world phenomena. Conversely, their degree of fitness would lead to trial and error in selecting experimental devices. Wold's recursive method of modeling and Hendry's ML method of estimation were adopted for such a reason. For so doing, Liu needed to explore possibilities to find satisfactory empirical models. Thus Liu would have objected to such activities as armchair economics, thought experiments, and observations because they do not involve "hands-on" operations.

Liu's exploratory thinking can also be observed in an interesting attempt to study the important problem of time aggregation in Engle and Liu 1972. Many years after using higher frequency time-series data for model building, Engle and Liu (1972) theoretically investigated the specification biases in the problem of time aggregations. This paper contains parts of Engle's PhD thesis supervised by Liu:

> My dissertation was very much along the lines of T. C. [Liu]'s research, which was on temporal aggregation, basically asking, "What's the relationship between macro models estimated at different data frequencies?" T. C. had already built an annual model and a quarterly model, and he was working on a monthly model, and so that was what I was

23. For models as experiments, see Morgan 2001, 2002.

24. For critical appraisals of econometrics as observation, see Hoover 1994 and Boumans 2010.

> trying to analyze and reconcile, from both theoretical and empirical viewpoints. The key issue was, if you started out with a certain high-frequency (say, monthly) dynamic model and assumed it to be true, and you aggregated to a lower frequency (say, annual), then what would the lower frequency model look like? You ended up being able to talk about the time aggregation problem in the frequency domain, and work out moments of aggregated data when the whole thing was dynamic, and it had to do with integrating over the spectrum, stuff like that, and the answer was messy. But what T. C. had observed, I think, was that the lag lengths were affected by aggregation; they got shorter, and that's what I was trying to characterize rigorously. (Engle and Diebold 2003, 1163)

One of the theses concerned in Engle and Liu 1972 is that more highly aggregated models will have a specification bias. To examine the biases, Engle and Liu estimated the average lag and long-run multiplier in distributed lag models with data aggregated over time. Because of the difficulty of developing statistical models of time aggregation, Engle and Liu postulated a model with the highest frequency (i.e., Liu's 1969 monthly model) as the true model, and aggregated into lower-frequency models. They found that the bias in the estimated average lag depended on whether the serial correlation coefficient was positive or negative. However, they also found that the long-run multipliers were unchanged by the frequency of observation, which supports Engle's assumption of cointegration in his later works.[25]

Furthermore, as previously discussed, Liu used the term *exploratory* as an adjective, which, by accident perhaps, is much similar to the usage as in "exploratory data analysis," which became an accepted term in statistics no earlier than the 1970s. Exploratory data analysis is in contrast to "confirmatory data analysis" that often relates to a structural-equations model in statistics and other social sciences and indicates an approach to postulate the pattern of data with knowledge of theory and then test ("confirm") the hypothesis or theory. Exploratory data analysis aims to explore the underlying pattern of data without imposing a presupposed structure.[26]

One constant throughout Liu's career was his view on the minimal role of economic theory in model specification. As we have illustrated, Liu held this view in his early empirical works containing no economic theory.

25. Personal communication with Engle, 5 October 2010.

26. We thank Duo Qin for pointing this out to us. For exploratory data analysis, see Tukey 1977.

Later in the 1960s his criticism on the a priori restrictions imposed in the Cowles Commission method objected to the idea that practice precedes theories. Finally, in the 1970s the view was maintained in his comments on Fromm's survey articles published one year before his passing away. For this, Liu was an antirealist about theories (see Hacking 1983).

Liu searched for useful tools and components for his models to accomplish the purposed end. When facing the complex economy, Liu chose to build a model for forecast when he did not have enough tools at hand. Liu built a model whose recursive structure was thought to capture the causal relationships of the macroeconomy. In this sense, Liu's approach could be seen as consistent with a "pragmatic" approach in econometrics, the label used by Clive Granger (2009) on Hendry's econometric practice and methodology.[27] In the context of a complex world, for Liu, econometric models were approximations, as Fisher (1961) pointed out. Liu's case shows that to construct econometric models that were approximations of a complex world, one has to explore, to practice long, and to practice hard.

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27. For a philosophical appraisal of Hendry's econometric methodology, see Chao 2009.

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