THE ECONOMETRIC PRACTICE OF TA-CHUNG LIU

[ALTERNATIVE TITLE: **TA-CHUNG LIU'S EXPLORATORY***]

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1. Introducing Ta-Chung Liu

A legendary Chinese economist, Ta-Chung Liu (1914-1975) is almost forgotten. Few people in his home country read him. But his legacies to the Taiwanese economics and economy still remain. Liu helped to 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 to day. Moreover, during the Cold War period, Liu was President Chiang Kai-shek's chief economic advisor in the 1960s, while working at Cornell University. Fully entrusted by Chiang, he implemented a radical tax reform that in many ways can be regarded as one of the foundations for Taiwan's economic progress. At that time, even the public put high hope on his endeavors to accelerate the modernization of

* Exploratory: "1982 *Times* 17 May 3/2 [Professor Gregory's] scheme proposes the establishment of an 'exploratory', a centre at which visitors would learn about science not by looking at exhibits but by actually performing experiments, operating computers and using information banks". *Ibid.* 3/3 "Professor Gregory, professor of psychology at Bristol University, got the name exploratory by analogy with observatory" (*Oxford English Dictionary*).

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Taiwan's economy. His every arrival and departure was reported in local newspapers. Ta-Chung Liu was a household name in Taiwan.

For econometricians, Liu is nowadays only remembered as the one of the intellectual predecessors of Christopher A. Sims's vector autoregressive approach for time-series analysis. But looking back, Liu's works are in fact valuable contributions to econometrics. Though he was a true econometrician, he was not trained as an econometrician. Liu first entered Cornell University and received his master degree in civil engineering in 1937, with a thesis dealing with the stresses in railway track. He then switched to pursue a PhD degree in economics at Cornell, and graduated in 1940. His thesis, entitled *A Study in the Theory of Planning by the Individual Firm under Dynamic Conditions*, deals with the topic on micro-theoretical modeling of firm's behavior. One chapter of his thesis entitled "The Bearing of Risk and Uncertainty" is an early evaluation of 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 of the thesis.

While there was nothing empirical in Liu's dissertation, he soon devoted to quantitatively studying the economic situations of contemporary China during the period of World War II and after the War, 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 did publish 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 self-learn econometrics, when he moved to the International Monetary Fund in Washington, D.C., due to the outbreak of the Chinese Civil

War.[1] From 1949 to 1958, Liu served as a visiting lecturer of mathematical economics and taught econometrics at the Johns Hopkins University, where he reunited with Machlup and Simon Kuznets, who was Liu's old acquaintance since they met as early as 1946 during Kuznets's advisory mission to China regarding the plans for postwar economic reconstruction.

From the 1950s until his tragic suicide with his wife in 1975, Liu was well known in the econometrics community as a chronic criticizer of the Cowles Commission method of macroeconomic modeling. His criticism is based on his belief that econometric models are in need of being specified as a miniature of the economic world in a realistic way. Much of his works 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. He first addressed the problem of econometric modeling in a 1950 article on measuring the propensities of consumption and investment of the United States (Liu and Chang, 1950).[2] This was also his first attempt to model the macroeconomy with monthly data. Subsequently, during the period of 1950s, Liu's published works were purely empirical, including computations of measures of variables, and uses of these measures on the construction of models to account for functional relationships among economic variables.

On the other hand, Liu's belief led him to explore possible and suitable ways of modeling macroeconomy. Liu's econometric approach can be described as "exploratory". He

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[1] Based on the reminiscence of Sho-Chieh Tsiang. In *Liu Dazhong Xiansheng Kangli Zhuisilu Xupian* (*Memoirs of Mr. and Mrs. Ta-Chung Liu, Volume 2*), (Taipei: Liu Dazhong Xiansheng Kangli Zhuisilu Bianji Weiyuanhui, n.d.), p. 87.

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

used such an adjective in several ways. In most places it means the preliminary and "experimental" nature of the processes and findings of his econometric models that are subject to modifications of future trials, while he first used this term in 1955 (Liu, 1955) to refer specifically to an approach to econometric modeling in which structural models were used in an initial stage of forecasting model construction. Furthermore, the term well describes the evolution of Liu's econometric methodology and practices, in searching for satisfactory methods and models for quantitatively approaching the macroeconomic reality, especially when the problem of identification was concerned.

This paper focuses on the models and methods of Liu's econometric practices in 1950s-70s. We will document Liu's search that led him to conclude to accept the recursive model represented by Wold. We will also discuss Liu's theoretical and empirical concerns for econometric models that made him to get involved in series of debates in the 1960-70s. Finally, we will briefly elaborate Liu's legacy to the VAR approach and provide a comparative study.

2. Early Econometric Practices and the Problem of Identification

Liu first encountered the issue of identification in his 1950 article (Liu and Chang, 1950) on measuring the consumption and investment propensities of the United States by using annual data. His model is a simple Keynesian system consisting of a consumption function, an investment function, and a goods market equilibrium condition. But when measuring, each of the consumption and investment functions was treated as a single equation estimated by the ordinary least squares (OLS) method. However, Liu did realize that his model is oversimplified and any application of his model to the real world was "necessarily experimental in nature" (Liu and Chang, 1950, p. 576). The identification problem was considered (Liu and Chang, 1950, pp. 579-80) as Liu also derived a reduced-form model from an exact-identified structural model. However, Liu found that the income coefficients in both the consumption and investment functions—representing the marginal propensity to consume and the marginal propensity to invest, respectively— estimated by the single-equation approach are the same as those by the Cowles Commission structural approach. Liu concluded that the OLS method is superior since it is simpler than the methods applied to the structural model (Liu and Chang, 1950, p. 582).

Several comments on this paper were published in 1953.[3] One interesting comments is that the Liu-Chang model contains no theoretical consideration. Francis M. Bator criticizes it as a "'Baconian' inductive-statistical approach" indicating they were "running correlations between aggregate magnitudes with no attempt at a theoretical foundation, looking for higher and higher correlation coefficients" (Bator, 1953, p.141). It is because, take the consumption function as an example, Liu and Chang used gross national product, instead of disposable income, as the determinant of the aggregate consumption expenditure. They did so for the reasons that the GDP and personal income are highly correlated, using the former would simplify the model. But Bator argued that, a high correlation between gross national product and disposable income does not necessarily guarantee that the consumption function is invariant with respect to the change in the gross national income-disposable income relationships. Hence their consumption function is useless for predicting behavior and testing hypotheses.

[3] By Clarence L. Barber, Francis M. Bator, Thomas Mayer, and Gardiner C. Means in *American Economic Review* in 1953.

Nevertheless, this paper can be seen as the point of departure for Liu's later journey of exploration in econometrics. In addition to Liu's awareness of identification problem, Liu and Chang pointed out a crucial point in time-series model specification, that is, whether to include lagged variables in the regressions. They complained that existing studies did not seriously take lagged variables into account. They argued that, first, it required numerous experiments to introduce significant lagged variables, and second, most important lags are shorter than a year (Liu and Chang, 1950, pp. 569-70). As a result, they subsequently published an article on monthly estimates of certain macroeconomic variables for the period of 1946-49 (Chang and Liu, 1951). In this article they pointed out that only few published models, such as Klein's Model II, included lagged annual variables (Chang and Liu, 1951, p. 225). However, without further justification, such an inclusion is only superficial. They went on to state that the reason for introducing lagged variables is due to the consideration of non-simultaneity in light of asymmetrical temporal relations between causes and effects. For instance, when consider of economic relations such as the consumption function, they argued, "by nature of the economic process involved, all of the consumption expenditures out of a given income cannot occur simultaneously with the receipts of that income. Some lagged relation between income and consumption expenditures is bound to exist" (Chang and Liu, 1951, p. 225). Hence their monthly estimates can be served as a basis for studying the consumption function consisting of lagged variables. In fact Chang and Liu did use their monthly data to run several regressions, but they have been cautious about the results obtained by the least-square method (Chang and Liu, 1951, p. 225): "Most economic time series are highly auto-regressive, 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." This led Liu to seek for alternative methods.

In 1955 Liu published a paper on forecasting the U.S. economy (Liu 1955), representing his first systematic treatment on the nature of underidentification and the objection to the Cowles Commission approach.[4] In practice, this article suggests a two-stage estimation process. At the first and "exploratory" stage, structural equations consisting of all relevant variables were established to find predetermined variables which are statistically significant. Liu thought economic theories deal only with structural forms than reduced forms, so theories were brought into in this process to provide useful information for specifying the model. He called them the "exploratory structural relationships" (Liu, 1955, p. 436). Liu also realized that exploratory structural relationships represent the economy only in an oversimplifying way, but they remain useful in the sense of singling out the predetermined variables. At the second stage, these predetermined variables are used as regressors in a single-equation least square regression model—Liu called it a "least square reduced-from equation"—for the purpose of forecasting the future values of endogenous variables.

One important point in this paper is multicollinearity. In an underidentified model containing a vast number of variables, there would be variables that are highly correlated. Such an existence harms structural estimation. But since Liu's purpose was to forecast, he turned collinearity into an advantage. He also claimed that his method relied entirely on the collinearity (Liu, 1955, p. 437). The reason is, in the case of consumption function, in his own words, (Liu, 1955, p. 457, emphasis added),

[4] In his three appendices attached to the article.

> 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*.

By then, Liu had formulated his main idea on econometric modeling and the identification problem. The two most important concerns had emerged. The 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 was deemed to capture. Liu had realized in his study of monthly time-series data that by adding lagged variables measured by shorter unit of time could help to resolve both concerns. But empirically Liu had yet to build a realistic model for explanation; only models for forecasting model were concerned.

3. Liu's "Disturbing Argument"

Liu then had decided to attack the Cowles Commission approach theoretically since 1957.[5] Two pieces of works subsequently published in the early 1960s. The one is 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?".

[5] See Liu (1963a, p. 157).

The panel discussants consist of Carl F. Christ, Clifford G. Hildreth, Lawrence R. Klein, and Liu. So in a sense this symposium can be seen as an occasion with Liu versus three Cowles people. The other article (Liu 1963a), which is less known to the 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), can be seen as the full version of Liu (1960), with a more detailed account for his criticism on the Cowles Commission approach.

Both articles start with stating the conditions for the identifiability, and both took Klein's models as the representative to the Cowles Commission approach. Liu (1960) in the first place stated a *rank condition* but then argued the problem of identifiability in terms of *order condition* (Liu 1960, p. 855). It is known to econometricians that the order condition is only a *necessary condition* for identification, while the rank condition is a *sufficient condition*. But in his (1963a) paper Liu did not give thought of the rank condition; he only used the order condition to analyze the situations of exact-, over-, and under-identification. Furthermore, in the (1963a) paper 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 are introduced subsequently into the model, the equation, say, the consumption function, turns from being overidentified, to being exact-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, p. 225).

The condition for identification in Liu (1963a) can be best illustrated using Christ's (1966, pp. 320-7) notations. 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 exact identified if $K - J = H - 1$.
- A structural equation is underidentified if $K - J < H - 1$.

By looking at the identification conditions represented above, it should be obvious to see that Liu's strategy of defending the nature of underidentification and guarantee the falsity of exact-and over-identification is by straightforwardly arguing the RHS of the condition is greater than the LHS. In doing so he needed to prove that there are either more H or J, or fewer K than perceived.

Based on this condition for identification, Liu's criticisms can be summarized as the following:

Firstly, Liu argued that the economy is complex; there are more rather than less variables which have 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 include Tinbergen

(1939), Ezekiel (1942), Clark (1949), Klein (1950), and Klein and Godberger (1955). Liu (1963a, p. 226) also found that, among these studies, the most common explanatory variable is profit (in various forms), though it is not included in Clark (1949). This non-consensus reflected the fact that there could be a vast amount of explanatory variables to be included in the investment function. Hence a number of variables, which should have been included in an equation, were in fact excluded to make the equation overidentified. Moreover, the economic world should be recognized as a system in which only few variables can 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, are only "relatively so", comparing to other variables (Liu, 1963a, p.227). As Liu claims, "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" (Liu 1963a, p. 227).

Secondly, the Cowles Commission approach justifies the application of *a priori restrictions* by resorting to "economic theory". But, for Liu, while the Cowles Commission thought economic theory provides more information for identifying the model, he believes the contrary is 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" (Liu, 1963a, p.226). He famously stated that (Liu, 1960, pp. 858-9; original emphasis),

> 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 one.

But note that Liu did not want to make the "ridiculous" suggestion that every variable should depend on every other variables so that all variables should be included in the model. He pointed out that in *all* the existing empirical equations, the margins of overidentification are small so that it is only needed to include a tiny set of variables to make the model become unidentified (Liu, 1963a, p.227).[6]

Thirdly, Liu regarded certain ways of satisfying the identification condition as rather superficial, that is, they do not help to solve the identification problem. Liu did not think the process of disaggregation that reduces the number of joint determined variables help, because, as Hildreth and Jarrett (1955: pp.24-5) pointed out in their book on 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 is part of total income. Hence the latter (total income) is not independent of the relations in the livestock model. All they can do is to "hope" the bias would be small to make their assumption of independence harmless.

Similarly, using higher frequency data would not necessarily help. Liu targeted on Klein and Barger's (1954) model, which is estimated using quarterly data for the US economy.

[6] Liu further stated this point in Liu (1974). See Section 6.

Recall that Liu has 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 the so doing would not necessarily solve the problem of identification. This is because, as Liu contended, that 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, p. 416) were in fact trying to solve the problem of serial correlation of disturbances by imposing an autoregressive process of order 3 on disturbances, Liu thought Klein and Barger's attempt is not satisfactory, since it resorts 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" (Liu, -1960, p. 861). Moreover, he cited Orcutt's (1948) work to demonstrate that it is the omission of relevant variables that causes the existence of serial correlation in disturbances (ibid.): "Economic variables tend to be serially correlated. When relevant explanatory variables are omitted from a structural equation, their effects on the dependent variables are left with the unexplained residuals. As a result, the residuals are serially correlated." Liu suggested to adopt Nerlove and Addison's (1958) finding to include more lagged, but not current, variables to reduce serial correlation in residuals (Liu, 1960, p. 863).

Finally, in Liu (1955), as discussed above, he argued that we should include more variables on a single equation at the tradeoff of problem of collinearity. Liu (1960) furthermore argued, similarly, that we should accept the least squares biases, that was first discovered in Haavelmo (1947), because, according to Foote and Waugh's (1958) finding by a Monte Carlo simulation, adding more jointly determined variables into the model would improve the efficiency (Liu, 1960, p. 861). In the terminology used in measurement literature, this means that Liu traded accuracy (low bias) for precision (low variance).

Consequently, Liu's argument is described by Fisher (1961, p. 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" (Fisher, 1961, p. 139).

Disturbing as it was, Liu's criticism inspired few responses in the 1960s. For instance, in the 1970s, Denis Sargan thought that Liu's criticism 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" (Sargan, 2001, p. 167). Apart from the objection from prominent Cowles econometricians such as Klein, and the papers by L'Esperance (1964, 1967) who tried to evaluate the predictive powers of econometric models formulated by Cowles's and Liu's approach, only Franklin M. Fisher took Liu's criticism seriously. Fisher not only provided a 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 will be discussed in more detail in the next two sections.

4. Criticism 1: Lawrence Klein

Liu and Klein have been in a tangle over econometric modeling since the publication of Liu (1955). Klein spent a small portion of space on his (1956) article to reply to Liu (1955). Later Klein (1960) directly rejected Liu's methodology of the single equation approach.

It is Klein's position that one should not be interested in measuring individual parameters in a system of equations—a "partial" analysis, but the solution (reduced forms) of the whole system—a "general" analysis (Klein, 1956, p.1). Therefore, with regard to the problem of forecasting, it requires to transform a system of structural equations into its reduced form. Accordingly, Klein advocated the maximum likelihood method because the property of efficiency in the maximum likelihood estimates of structural parameters are unchanged when being transformed to reduced-form coefficients, while the least-square estimates are not. With regard to Liu's claim to trade off efficiency against bias by applying least squares method on single equations, Klein demonstrated that such a tradeoff does not exist, because he formally proved that more a priori restrictions imposed on the system, more efficient the estimates of reduced form parameters are (the variances become smaller). As a result, Klein (1956, p. 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 smaller forecasting error.

Moreover, in Klein (1960), a priori restrictions are regarded as the vehicle for the progress of econometric modeling. He (Klein 1960, p. 876) regarded that building better knowledge of economic institution into a priori restrictions results 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. Klein (1960, p. 896) stated: "The adoption of more powerful methods of mathematical statistics is no panacea." Consequently, Klein argued that the econometric reality is overidentification (Klien, 1960, p.870): "Contrary to Professor Liu's contention, I believe that the general rule in realistic econometric models is heavy overidentification."

It seems that Liu was not persuaded. In a sense that Liu's work on identification (discussed above) can be seen as he stood firm and replied to the criticism from Klein, the representative of the empirical modelers of the Cowles Commission. Although Liu applied the Cowles Commission method in his (1963b) article, he never regarded the Cowles Commission method of simultaneous equations model as a right way for econometric modeling.

5. 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 is usually "good enough" because specification errors resulting from false a priori restrictions are 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.[7] We assume that the system can be re-written as

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

and

$$x = \begin{bmatrix} y \\ \hline z \end{bmatrix},$$

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:

[7] 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 is with the following properties: (2) the R^i are nonsingular (and hence square matrices) and the S^i are matrices which 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:

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

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:

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

It is easy to see that, 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 $y^{k\text{-}1}$ are determined by the variables in y^k, the exogenous variables in z, and the block of equations corresponding to $R^{k\text{-}1}$; 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), a block recursive system as above 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 to assume certain variables absent that truly appear with very small coefficients. As a result, 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, p.149). Thus, provided that the approximations are good enough, the efficiency properties of simultaneous equation estimators will more than compensate for their inconsistency.

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

6. The Road to Recursive Models

If any hope of structural estimation was forlorn, then what did Liu propose? His first step was rather surprisingsince he retreat to a Cowles Commission structural equations model, which he referred to as the Tinbergen-Klein tradition.

In Liu (1963b)—which is written in 1961—the modeling process proceeds with a simple form of investment function with five explanatory variables, then a basic form of the investment function is reached by adding more variables into the function by considering the "complications" caused by the elements such as "past commitments", "expectations", and "nonlinearity and asymmetry". Using this basic form as the foundation to other functions, Liu went on to build a 36-equation structural model. The U.S. time-series data from the third quarter of 1947 to the fourth quarter of 1959 (50 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 are very high; the results are very close to the two-stage least squares (2SLS) estimates, except that the 2SLS estimate of the marginal propensity to consume is unsatisfactorily smaller than the least squares estimate. Liu continued in the second part of the paper by providing eight simulations in order to investigate the influences of government policy on certain variables under different assumptions. He (Liu 1963b, p. 335) stated that his simulation results are "crude and experimental", just as his empirical models are "crude and exploratory" (Liu 1963b, p. 301), meaning that such empirical model specifications and simulation results are in nature involves many trials and the conclusions are hypothetical.

But it should be forcefully noted that, though the model in this article is nonrecursive, he vehemently objected this type of model, as stated in the Appendix C of Liu (1963b), entitled "Methodological Issues", "[w]hile the exploratory model presented here is constructed in the Tinbergen-Klein tradition, the present author has strong reservation about this type of structural estimation" (Liu 1963b, p.346). Such a reservation is understandable, as we have seen in our previous discussion on 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 necessary recursive" (Liu, 1963b, p. 345). And Liu was ready to build recursive models for the macroeconomy based on the methodology of Wold and Strotz, especially Wold's conceptions of causation.[8]

Liu finally built his own first recursive model and published it in 1969. He saw his monthly recursive model superior to not only the simultaneous equations model built on annual and quarterly data, but also to Strotz's and Wold's recursive systems. This is because his model has the least specification error on causal direction as it involves the smallest time unit available, and his model may not have greater serial correlation in the disturbance terms.[9]

In practice, Liu's monthly recursive model contains 33 equations in which 16 equations are estimated (what Liu called "recursive structural relationships" (p.7)) and 17 definitional and behavioral equations. Liu specifically set the coefficient matrix attached to

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[8] See also Gilbert and Qin (2006).

[9] For Liu weekly data were not available (Liu, 1969, p.1).

dependent variables as diagonal, indicating that contemporary variables are not causally relation to each other. For estimation, he first applied the OLS method to all equations, and apply Durbin-Watson statistic to test the first order serial correlation in residuals in a two-stage process. If the first order serial correlation in residuals exists, then transform the variables and test again. The US monthly data for the period 1948-1969 was used for estimation. His model is recursive in the sense that all variables on the right-hand side of the final equations are one-period time lagged, except for time trend.

Liu's final effort to on monthly recursive model was published in 1974 in a joint paper with his student Erh-Cheng Hwa (Liu and Hwa 1974). It should be noted that early versions of the Liu-Hwa monthly recursive model had been circulated among econometricians, and one of them was included in Fromm and Klein's (1973) comparison of 11 U.S. macroeconometric models. Fromm and Klein compared these models' predictive power (both within and outside sample period) regarding the four important macro variables: GNP in current dollar, GNP in constant dollar (1958 dollars), GNP deflator, and unemployment rate. According to the root mean square errors (RMSE) reported in their Tables 1-4, the Liu-Hwa model performs 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 produces 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 are improvements from their previous version, one notable difference is to abandon the OLS method adopted in Liu's (1969) recursive model and turn to David Hendry's computer program and the method of maximum likelihood (Hendry 1970).[10] This is perhaps Liu's first and last application of the ML method. Even in the early version of the Liu-Hwa model, as revealed in Liu's comment on Fromm's (1974) survey, he still showed his contempt for the ML method of estimation (p. 417) and pointed out that the version of the Liu-Hwa model that Fromm reviewed used the OLS method.

7. Liu versus Sims

In Liu (1974), he delivered two consistent attacks on the Cowles Commission in his comment on Fromm's survey article on the U.S. macroeconometric models.[11] To start with, unlike the Cowles Commission, Liu was skeptical about the effective role that economic theories would play in model identification and specification. He listed three reasons. The first is that theories are usually developed without prior testing them against empirical data, or lack of the properties which are statistical testable. Secondly, time-series data are not obtained from controlled experiment, thus "the procedures used in estimating the equations can never fulfill all the assumptions required for rigorous statistical testing of a theory." Thirdly, the size of time-series data is usually not large enough to determine the correct theory among empirically equivalent ones. Moreover, observed from Fromm's article, none of the surveyed models used "the Cowles Commission" maximum likelihood method. The majority of the models used OLS method, while the two-stage least squares methods were employed by only two models (Liu [1974] 1976, p. 417).

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[10] Possibly an early version of Hendry (1971).

[11] Fromm's paper was first presented in 1972.

In addition, Liu also briefly clarifies his 1963 criticism, whereas he thought that his argument was misconstrued by Fromm and others as a claim that a true structural relationship is not identifiable because they must not contain finite number of explanatory variables. Yet Liu stated that he only argued 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.[12] Actually, Liu's early attempts is seen as bearing much similarity to Sims's. As Robert Engle, a student of Liu, recalled that (Engle and Diebold, 2003, p. 1162)

> 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.

At the outset, the VAR approach is well known to econometricians as *a*theoretical. Furthermore, despite that all variables in the system are treated as endogenous, Sims's (1980) VAR model is essentially a small-scale reduced form model: the number of variables (equations) in the model is usually small, e.g., six variables (equations) in Sims (1980). Since

[12] See also Qin (2006) for the history of the VAR approach.

the focus of VAR analysis is on the dynamic relationships among the variables, not the parameter estimates, the "incredible identification" problem plaguing the Cowles commission has been claimed to be avoided. However, to be able to provide empirical evidence on the stylized facts regarding 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 improperly imposed 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.

In practice, Sims (1980) VAR approach actually echoed an earlier effort of Liu (1969) and Liu and Hwa (1974). In Liu (1969) and Liu and Hwa (1974), recursive yet structural monthly U.S. macro econometric models were constructed and estimated. According to Liu and Hwa (1974), in a monthly model, there would be much less simultaneity involved in the system of economic relationships than in quarterly or annual models. Accordingly, Liu and Hwa (1974) argued that the estimation of monthly recursive relationships by ordinary least squares would produce better results than those from quarterly Cowles commission models. Thus, it appears that in Liu's view, the resolution of the identification problem hinges on whether or not the sampling interval of the available data is fine enough to be able to discern the true recursive causation structure of the systems.

Accordingly, although Sims (1980) VAR approach appears to largely avoid the identification problem of Klein-Goldberger type structural models, it may still be hampered by the identification problem to a certain extent as long as the data sampling interval is not fine enough to reveal the true recursive causation structure of the system. Under such circumstance, theoretically based restrictions, which may not exhibit any recursive structure, are required for the identification of various autonomous shocks. The "structural VAR" model as such faces the similar criticism as that confronting the Cowles commission approach; see, for example, Cooley and Dwyer (1998).

8. Ta-Chung Liu's Exploratory

From our illustration of the development of Liu's econometric thought, it can be observed how his methods and models evolved. 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 in a gradual way. But Liu's way is perhaps having both similar and dissimilar ways to contemporary econometric approaches. The similarity lies in their experimental minds. Econometricians are widely aware of that in econometrics controlled experiments are impossible. The dissimilarity exists because, unlike the reasoning involves viewing econometrics as "passive observation" such as Haavelmo (1944), Liu's way is to experiment with many if not all methods and models in his "exploratory"—in contrast to "observatory" for passive observers.[13] Econometrics is learned and advanced by actual practices. For Liu, attempts are made to modify and improve "experimental" results in order to meet the real world phenomena. Conversely, their degree of fitness would lead to the process of trials and errors in selecting experimental devices. Wold's recursive method of

[13] For econometrics as observation, see Hoover (1994). See also Boumans (2010) for a critical appraisal. For models as experiments, see Morgan (2001, 2002).

modeling and Hendry's ML method of estimation were adopted for such a reason. For so doing, he needed to explore possibilities in order to find satisfactory empirical models, even though those are rejected in heart—such as his employment of the Cowles Commission method in his (1963b). Liu's journey of econometrics as we illustrated above demonstrates this. Liu's empirical models are summarized in Table 1.

Table 1 The comparison of Liu's empirical models

Paper	Data Type	Data Period	Model Type	Method of Estimation
Liu and Chang (1950)	Annual	1930-1948	Structural, 3 equations, exact-identified	OLS
Chang and Liu (1951)	Monthly	1946/01-1949/12	Single equations	OLS
Liu (1954)	Annual	1923-1951	Single equations	OLS
Liu (1955)	Annual	1929-1952	"exploratory"	LIML then OLS
Liu (1963b)	Quarterly	1947(3)-1959(4)	Structural, 36 equations	OLS
Liu (1969)	Monthly	1948-1964	Recursive, 33 equations	OLS
Liu and Hwa (1974)	Monthly	1954/1-1971/12	Recursive, 131 equations	Hendry's ML method (early versions: OLS)

Liu's experimental thinking can explain why an article published by Robert Engel and Liu in 1972 (Engle and Liu 1972) goes the other way to investigate the time aggregation problem, while the need for shorter time lag data were well demonstrated in Liu's previous works as he started with low-frequency model (quarterly model in Liu 1963b) to a higher-frequency model (monthly model in Liu 1969).[14] In Engle's "*ET* Interview" with Francis Diebold, he said, to quote in length, that (Engle and Diebold, 2003, p. 1163)

> 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 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.

[14] Engle and Liu (1972) was derived from Engle's PhD dissertation under supervision of Liu at Cornell University.

Since in Liu's previous works he practically said very few about the time aggregation problem in theory. The Engle-Liu article can be seen as they "experiment" with models with different time unit.

Another example that deserves more detail is Liu's long-standing fondness of the OLS method. It can be traced to his criticism on Orcutt's (1950) objection to the OLS method. This is because Orcutt realized that, when there are simultaneous shifts to both demand and supply (see Chart 3 of Figure 1), the elasticity measured by observed data (*EE* in Chart 3) would be underestimated. Orcutt's diagram means that supply and demand cannot be identified when there are changes in both supply and demand. His other diagrams (Charts 4 and 5 in Figure 1) represent possible observation errors to a single relation (Chart 4) or a system of equation (Chart 5).

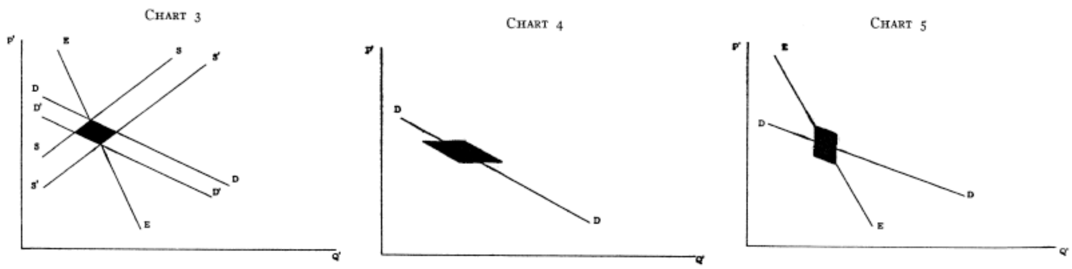

Figure 1 Orcutt's (1950) diagrams on identification.

Back then, Liu thought Orcutt's objection does not obtain (Liu 1954). He argued that in real world simultaneous shifts in both supply and demand are rare to non-existent, and certain variables could be legitimately regarded as predetermined, so that the problem of identification doe not arise.[15] Nevertheless, he used diagrams similar to those of Orcutt's in Liu (1963a) for illustration, with the consumption function as an example (see Figure 2).

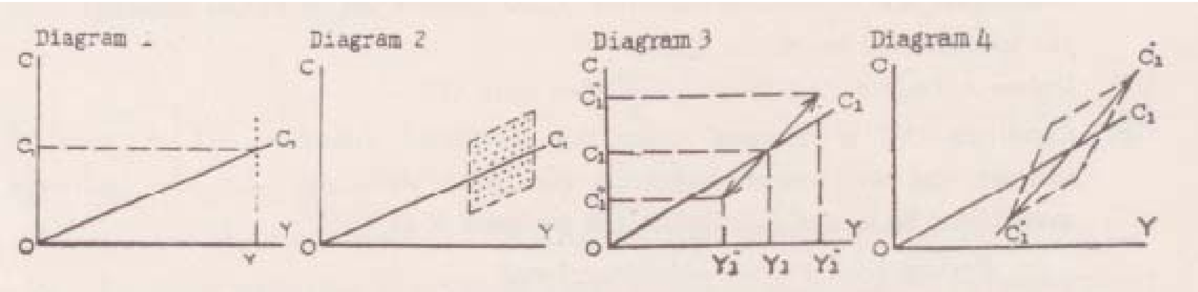

Figure 2 Liu's (1963a) diagrams on identification.

This is perhaps because for Liu, despite having a different view on the least square method, Orcutt's diagrams are satisfactory tools to illustrate the problem of identification.

Yet, one thing remaining unchanged throughout his career is his view on the role of economic theory playing in model specification. As we have illustrated, Liu held this view from the 1950s, in his early empirical works containing "measurement without theory" (see Section 2), his criticism on the a priori restrictions imposed in the Cowles Commission method in the 1960s, to his comments on Fromm's survey articles published one year before his passing away. For this, Liu was an "empiricist" (philosophically speaking), in the sense that he regards practices precedes theories.

9. Concluding Remarks

Liu searched useful tools and components for his models to accomplish the purposed end. When facing a complex economy, he chose to build a model for forecast. When there

[15] See Liu (1954, pp. 421-427).

were enough tools at hand, he was able to build a model containing a recursive structure that was thought to capture the causal relationships of the macroeconomy. In this sense, Liu's approach may be labeled as "pragmatic". In the context of a complex world, for Liu, econometric models are approximations, as Fisher (1961) pointed out. To reach this, Liu's example shows that it is by practicing with models in a long, hard exploration in search of satisfactory empirical models.

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