Inforum Models: Origin, Evolution and Byways Avoided*

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Inforum models have been built and used in a number of countries including the USA, Russia, Germany, Poland, Latvia, Austria, Italy, Spain, the United Kingdom[1], South Africa, Mexico, Colombia, China, Japan, Taiwan, Thailand, and Vietnam and perhaps others. They make extensive use of econometrics and input-output analysis to describe the functioning of an economy not only at a macroeconomic level but also at the level of individual products and industries. I want to record – somewhat autobiographically-- the beginnings of these models, outline their evolution, defend them and other econometric models against the sweeping Lucas critique, and mention some of the fads in economics and modeling which have been avoided for good reason.

Origin of the Inforum Models

The first of this family of models was the one built for my Ph.D. thesis at Harvard in 1961. My motivations for building it reached back into my childhood in the Great Depression. I was born near the bottom of the Depression; among my earliest memories are those of young men coming to our house and asking to be allowed to do a day's work for a day's food. I remember watching freight trains go by on winter days with men in box cars huddled over kerosene heaters. I asked my father why they did not ride the passenger trains. “They have no money,” he replied. “They had hoped to find work here, but the didn't, so they will go on to Decatur (the next town) and look there.” These memories left the deep impression that the economy could go seriously wrong. Then came World War II. Everyone was employed, even many women who had not previously wanted jobs outside the home.

World War I ended in 1918; the Great Crash and beginning of the Depression came in 1929, eleven years later. Eleven years after the end of World War II was 1956, and I was graduating from college. I had earnestly wanted to understand how the same economy that had produced the postwar booms could also produce the Great Depression. I wanted to know how another crash could be prevented, so I had studied especially economics and mathematics.[2] Math was important because I was convinced that a satisfactory understanding had to be quantitative and not just qualitative. In my college years, I had read Keynes in the original and in various digested forms. I had read carefully Wassily Leontief's *Structure of the American Economy,* and I felt, correctly I think, that I understood as much as my teachers did, and that wasn't much. Still hoping to find answers, I went on to graduate study at Harvard. Surely, I thought, if anyone understood it would be the professors there. I studied with Gottfried Habeler, author of *Prosperity and Depression,* and James Dusenberry, who was at that time writing *Business Cycles* for the Economic Handbooks series. With Dusenberry, I read John Hicks's, *The Trade Cycle* and the business cycle chapters of R.G.D. Allen's *Mathematical Economics* (not to be confused with his *Mathematics for Economists*). With Robert Dorfman, I read Paul Samuelson's *Foundations of Economic Analysis.* With Robert Mosteller, I had a comprehensive course

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1 The reference here is to the U.K. model built by Robert Shackleton at the University of Maryland. The models built by Cambridge Econometrics in England, though not built with Inform software, are closely kin in spirit and method.

2 In addition to French and German, I had also studied Russian, largely because I wanted something remote and exotic to escape the somewhat humdrum world of my other studies. I never thought I might ever actually go to Russia!

in mathematical statistics. It was a great year, but now I was sure that *no one* understood at the level I felt necessary how the economy worked.

The summer after that first year at Harvard, I worked in the capital budgeting office of Westinghouse Electric Corporation. There I could see all of economic forecasting services to which the company subscribed, and I think that was nearly all there were. There were as yet no regularly functioning econometric models, and the forecasts being offered were hunches based on ideas about cycles. They were pretty gloomy. The director of the office felt we should cut back our investments because we were surely going into a recession if not another depression. It occurred to me that if his views were widely shared, *we could forecast ourselves into a depression.*

Through this experience, the importance of forecasting for the functioning of the economy was impressed on me. Depressions and recessions had invariably been preceded by booms fueled by unreasonable expectations. It would be as important to moderate the booms as to lift sights in a recession. These ideas were quite close to those motivating the indicative planning then evolving in France.

To be safe from being drafted into the Army, I had been in the Reserve Officer Training Corps during college, and in the fall of 1957, I began my required two years of active duty. The Army knew that I knew some Russian, so after basic officer school I was detailed to the Central Intelligence Agency where I worked on piecing together a quantitative picture of the USSR Economy. For the first time since the 1920's, the USSR had just begun publishing some statistics, and all of our useful sources were open publications. There were, of course, many secret statistics which were not published in the carefully censored statistical handbooks. But Khruschev knew the secret numbers and was fond of enlivening his long speeches with secret tidbits. The speeches were published verbatim, apparently without censorship, so they had to be read, and I spent many an hour scanning them. I was much more interested in what was happening with central planning, but the limitations and censorship under which the Soviet economists worked made reading their articles depressing. I had no interest in continuing in Sovietology; I wanted to study the economy I was part of, an economy whose functioning I could hope to improve.

Back at Harvard in the fall of 1959, two important things happened for me. The first was that I was finally able to take a course in econometrics. Harvard offered no course in the subject. I had to catch the subway down to MIT, where a course was being offered by Edwin Kuh. There were all of four students in the course, two MIT graduate students, one undergraduate and me. Ed worked very hard on creating an interesting course, and through it I became acquainted with the work on construction of econometric models which Lawrence Klein was starting at the University of Pennsylvania.

The second, and even more important thing that happened was that I got a job as a computer programmer for Wassily Leontief. It was on-the-job training, as I tackled programming the Univac I in machine language. Leontief himself knew nothing of computer programming, but he had a very able assistant who was my guide. Leontief was seeking a way of getting sensible results from his dynamic model. This model can be written as

$$(1) \quad x(t) = Ax(t) + f(t) + B\dot{x}(t)$$

where *x(t)* is the vector of outputs at time *t*, A is the input-output matrix, *f(t)* is the vector of final demands excluding expansion investment, $\dot{x}(t)$ is the derivative of output with respect to time, and B is a matrix of incremental capital/output ratios for various types of capital goods. The growth path for the economy should satisfy, at least approximately, such an equation. A mathematician looking at the formula would say, “Yes, a system of linear ordinary differential equations. Easy. Solve for $\dot{x}(t)$

given *x(t)* and use any of a number of numerical methods or calculate the characteristic roots and vectors of $B^{-1}(I - A)$, $\lambda_1, \lambda_2, \ldots \lambda_n$ and $V_1, V_2, \ldots V_n$, write the general solution as

$$x(t) = c_1 V_1 e^{\lambda_1 t} + c_2 V_2 e^{\lambda_2 t} + \ldots + c_n V_n e^{\lambda_n t} + p(t)$$

where p*(t)* is some particular solution, and then determine the *c*'s to fit the initial conditions." Leontief was thinking of this equation in this mathematical way, and getting nowhere. In the first place, the B matrix had some all-zero rows, so it was singular, so B^{-1} did not exist. We aggregated the matrices until B was non-singular. But the lambdas were large and complex, so the solutions were explosive – all except the particular solution, *p(t)*.

It finally occurred to me to pay attention to *p(t)*. I knew that if *f(t)* was a polynomial of degree *m*, there would be a particular solution as a polynomial of the same degree. So, start with the simplest case where f(t) is linear: $f(t) = a_0 + a_1 t$ where a_0 and a_1 are constant vectors. Then there must be a particular solution of the form $p(t) = b_0 + b_1 t$. Substituting into (1) and equating the coefficients of equal powers of *t* to solve for the b's and then substituting into the formula for *p(t)* gives,

$$(2) \quad p(t) = (I - A)^{-1}(a_0 + B(I - A)^{-1}a_1) + (I - A)^{-1}a_1 t.$$

This formula made perfect sense economically! The first term is the outputs required, directly and indirectly to produce the final demands in the base year, including the investment needed for the straight-line growth of final demands. The second term provides the outputs required, directly and indirectly for the growth in final demand since the base year. There is no B^{-1} involved; all the calculations are easily made. The solution could be easily generalized to higher degree polynomials and exponential terms in the final demands. In doing so, the particular solution continued to make sense and be easily computed.

There was just one problem: it might not go through the initial values of *x(t)*. It was clear that adding in terms from the general solution to (1) was not the way to go. I had finally realized that using (1) to determine $\dot{x}(t)$ made no sense at all economically. There is no way and no reason that managers in various industries could or should set the rates of growth of their output so that the investment they generate would exactly use up whatever differences there might be between current levels of output and those generated by the first two terms of the right side of (1). Yet that is exactly what is required for (1) to be used to determine $\dot{x}(t)$. I thought of some devices to adjust for differences between observed initial values of *x* and those of the particular solution. On the page where I described them in my Ph.D. dissertation, Leontief wrote, "You have cut the Gordian knot, not untied it." I, on the other hand, think that by realizing that (1) could not be used to determine $\dot{x}(t)$ the knot was untied.

This sort of solution to (1) was used in an article in the *Review of Economics and Statistics.* An improved solution method using moving polynomial interpolation to approximate the evolution of outputs in a way which could be differentiated and the Neuman expansion[3] of the operator $(I - (A+BD))^{-1}$ – where D is the differentiation operator – was used in an *Econometria* article and was the basis of the calculations in the book *The American Economy to 1975,* which was published in 1966. In the model for the book, econometrically estimated consumption functions played an important role. Their primary driver was disposable income. Technically, it was an exogenous variable, but in fact it was adjusted so that, given exogenous government and export

3 The Neuman expansion of $(I - A)^{-1}$ is $I + A + A^2 + A^3 + \ldots$. That this sort of expansion of the $(I - (A+BD))^{-1}$ operator would give the same result as my equating of like powers of *t* was pointed out to me, rather reproachfully, by Robert Dorfman. He implied, correctly, that I should have seen it for myself.

demands, the model would generate high and stable rates of employment relative to an exogenously forecasted labor force. Thus, disposable income became, in effect, a policy variable. Otto Eckstein referred to that as an "oddity" of the model. To me, it was not odd at all but a shortcut. A fully developed model would generate personal income and use exogenous tax rates as the policy variable to achieve the desired level of employment. That would be better, but the lack of that mechanism did not invalidate the forecasts as a *vision of the future* consistent with (a) their being believed and used by those making capital investment decisions and (b) tax policy being adapted to yield full employment.

I came to the University of Maryland in the fall of 1966 and immediately set to work to build a new model with an important difference: an investment function for each industry would be econometrically estimated, based on replacement and **past** values of increases in output. This decision was based on two factors: (1) the need to have a good forecasting performance in the early years of the forecast, and (2) the studies of my wife, Shirley M. Almon, on investment decisions. She found these decisions to be based almost entirely on replacement and past growth in output. Future growth, even if made available with perfect foresight to her regression equations, had no predictive power. Apparently American firms like to operate with excess capacity; only when output rises up too close to – but still below – capacity do they invest. There are exceptions to this view, but not many.

Another early development of crucial importance was the interest taken in the work by Leonard Silk, an editor of *Business Week.* Indeed, it is from a telephone call from him in early January of 1967 that I date the beginning of Inforum. He then organized the initial business support for our work. This form of support has had the great advantage over government grant support that our forecasts are looked at carefully by users. On the other hand, the fact that some companies paid for the forecast limited our ability to publish them as broadly as I would have liked.

Initially, all of our support came from private companies using the forecasts for their long-range planning. Gradually, government agencies began to use our services. Sadly, most of the private companies dropped out saying that our service was fine but that they were no longer engaged in long-term strategic planning. Management was instead concerned rather with short-term results which affected the stock price. Thus the financial supporters of our work have come to be primarily government agencies. They want access to the model to run alternative scenarios more than they care about the basic forecasts. Maybe we could now release them freely without disturbing the supporters.

Some of the further developments at Maryland included:

(a) The creation and use of product-to-product input-output tables made by the method published years later in *Economic Systems Research*.

(b) Three Ph.D. theses over a thirty-year period exploiting the household data in the *Survey of Consumer Expenditures* for forecasting personal consumption expenditures by product. Mostly these theses were looking for help in predicting the pattern of expenditures as income increased. The last also looked for predictable effects of age cohort. For example, Do the differences between the expenditure patterns of today's 20-30 year-olds from those of 20-30 year-olds 20 years ago tell us anything about how the expenditure patterns of 40-50 year olds 20 years from now will differ from those who are today in that age bracket?

(c) Several theses on investment. The last of them, by Daniel Wilson, used firm-level data to try to determine to what extent technological improvement was embodied in fixed investment.

(d) Development of a complete national income accounting part of the model so that it could be run either as a business cycle model with given tax rates or, as before, with a tax rate designed to give a specified level of employment. Whereas earlier models had concentrated on producing a full-employment vision of the future, models with this new feature – which we called the National Income

Accountant, or just the Accountant – should be able to show what would happen if tax policy was not correctly set. They could be used as multisectoral business cycle models, just as most macroeconomic econometric models were business cycle models.

(e) Development of software to make construction of Inforum models as easy as possible. One part of this software is the G7 regression package which includes, besides many standard techniques, estimation with soft constraints to allow the user to tell the program what values of parameters make sense economically. Macro econometric models are easily built with G7. It will also do basic matrix computations and display matrices. Another part is the Interdyme system which enables the model builder to use matrix notation to write some equations of the model, while standard C++ is used for parts not easily written with matrices. Programs for display of model results are also part of the collection of programs.

(f) The development and application of the Perhaps Adequate Demand System (PADS) for consumer expenditures. The name was a reaction against the so-called Almost Ideal Demand System, which in my view might better have called the Absolutely Unusable Demand System because, in it, if the demand for one product rises faster than income, then the demand for some other product must turn negative with a sufficient rise in income. That other product is usually food, and the rise in income necessary to turn it negative may be fairly small, like 30 to 50 percent. Such demand functions in a model aimed at forecasting in a 10 to 20 year horizon would make for utterly implausible results. PADS preserves the two empirically useful results of the theory of the consumer, namely, (1) increasing all prices and income in the same proportion has no effect on demand, and (2) income-compensated cross price partial derivatives are equal. The second result is not necessarily preserved as we go from the demands of a single consumer to market demand functions, but assuming that it is preserved cuts down by a factor of 2 the number of parameters we have to estimate. Estimating PADS requires non-linear least squares. Symcon, a program for estimating PADS, has been developed and is part of the standard Inforum set of programs.

(g) The ability to optimize the forecast of the model with respect to policy variables, such as the personal income tax rate, was added to the software and applied to a model of Thailand by Somprawin Manpraesert.

(h) The development of a Bilateral Trade Model (BTM) for linking models of a number of countries. As a number of countries began to develop Inforum-type models, it became increasingly important to link them to together. The first linking was done by Douglas Nyhus in his Ph.D. thesis. Some years later, it was thoroughly updated and extended by Qiang Ma. Current updating and extension is being conducted by Leonardo Ghezzi and Rossella Bardazzi in Florence.

(i) Solution of a perennial problem for forecasting in industry detail when there is a long lag in appearance of detailed data. In December of 2015, we will produce forecasts of industry outputs for 2016 and later years. Our industrial clients already know their output for 2015, but we will not get the data on 2015 from our Census sources for another year and a half or more. If it looks like we don't know what happened in 2015, how can we expect anyone to have any confidence in what we say about 2016 and later years? Largely through the Ph.D thesis of San Sampattavanija we now have a systematic way of dealing with this problem. Basically, the annual industry-level data is regressed on high-frequency data that is available with a short lag, such as industrial production indexes. These may, in turn, be regressed on variables forecasted in our quarterly econometric model. Thus, we can have in December of 2015 a good idea not only of detailed product outputs in 2015 but also of what they will be in 2016.

This list includes only work done at the University of Maryland, and only some of that. There is much other work around the world, but I am not capable of giving a proper summary of it. All of this

work was done with the serious purpose of improving the forecasting or simulation ability of the model. None of it was done to be fashionable or to use some sophisticated technique.[4]

Meanwhile, however, the whole econometric modeling approach came in for heavy – but greatly exaggerated – criticism, and new approaches to modeling became popular among academic economists. Here we need to look quickly at the criticism and explain why we have been uninterested in following several types of modeling which have become fashionable in the academic world.

The Lucas Critique

In 1976 Robert Lucas published in a rather obscure place an article[5] that put an end to almost all academic modeling in the econometric tradition.[6] It was entitled "Econometric Policy Evaluation: A Critique" and announced on the first page that econometric models could not *in principle* be used for policy analysis, though they might be quite useful for forecasting. The Inforum models were not the direct object of the Lucas criticism but they were close enough to the traditional econometric models that, if the criticism was valid, Inforum models would be in trouble for policy analysis though not for forecasting.

Anyone who took the Lucas critique seriously – and that seems to have been almost all of the academic economic profession – must not have read beyond the first page, for the rest of the article shows nothing *in principle.* Rather it examines three specific models applied to three specific policy questions and finds the specific theory used in the models inappropriate.

The first model was used to study the effects of a temporary personal income tax cut. The model used adaptive expectations of income whereas Lucas – being from the University of Chicago – asserted that permanent income was what mattered. Since permanent income was hardly affected at all by the temporary tax cut, Lucas knew without any formal model that consumption would not be affected. Curiously, he pointed out that the model was correctable, that is, it could have used permanent income. But if it were correctable, then it was not *in principle* impossible to use an econometric model to study this question. The future, incidentally, was not kind to the permanent income theory. The Reagan tax cuts of the early 1980s put it to the test. As soon as Reagan's proposal was announced, it was certain that taxes would be cut in three future steps, so permanent income rose immediately. Consumption did not. Consumers waited until the money was in their pockets.

The second model examined by Lucas studied the effect on investment of a temporary investment tax credit, but the formula used was for a permanent investment tax credit. Lucas observed, correctly I think, that the short-term response to a temporary credit would be much larger, because firms would try to cram into the period when the credit was in effect investment they would otherwise have undertaken over the next several years. Lucas was correct and perceptive, but the problem had nothing to do with econometric part of the model. It was an error in what was essentially an exogenous

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4 While Inforum modeling was taken up at a number of universities abroad, it did not spread in the USA academic world. That was not surprising, because it requires a large investment in human capital and constant updating of data but does not offer much opportunity for academic publishing. Not having close competitors was nice for the commercial side of our work, but not becoming “mainstream” also meant that there were no new Ph.D.'s coming from other schools with the ability to build and improve Inforum models. Since the Maryland Economics Department did not hire any of its own graduates, when I retired there was no one to continue teaching Inforum modeling in the graduate program at Maryland or anywhere else in the USA.

5 Lucas, Robert (1976). "Econometric Policy Evaluation: A Critique". In Brunner, K.; Meltzer, A. *The Phillips Curve and Labor Markets. Carnegie-Rochester Conference Series on Public Policy 1.* New York: American Elsevier. pp. 19–46.

6 Ray Fair at Yale and the Inforum group at Maryland were the main holdouts.

variable. The fact that errors in exogenous variables could cause errors in the forecast was neither new nor unknown nor particularly disturbing.

The third model was used to study inflation, but used a Phillips curve without acceleration, that is, without a shifting of the curve in response to current rates of inflation. Today it would be hard to find an econometric model that does not include acceleration. Lucas's criticism of that particular model was justified, and it was a good example of what he wanted to claim was generally true, namely that the parameters of the model were dependent on policies followed. But the fact that the model was so easily modified to eliminate that dependence left Lucas without a single valid example of his point. Thus, he by no means showed that econometric models could not, in principle, be used for policy analysis.

Indeed, the Phillips curve example invites a counter-Lucas generalization: *any parameter which depends upon a policy can be replaced by a* ***variable*** *which is a function of that policy*. That is exactly what modern econometric models do with the Phillips curve. A model in which there are no policy-dependent parameters can be said to have a policy-invariant structure. Building such models by conventional econometric methods need not be especially difficult; it just requires a little thought.

Interestingly, it was not the models' econometric part proper – the estimation of parameters by some sort of least squares – which Lucas criticized. It was the economic theory built into the models. He could have made a different and universally valid criticism: no study of past data alone is going to tell us – without some theory – how the economy will react if pushed into territory unlike any it has been in before. In such a case, we must rely upon theory, not the past alone. And the model's validity will depend upon that theory.

Of course, there is a positive lesson to be learned from the Lucas critique: think carefully about the equations, especially those most relevant to the policy you intend to study.

But did Lucas establish that econometric models cannot *in principle* be used for policy analysis? Not in the least. Instead, the critique seems to have been politically motivated. Later work was to show that Lucas, typical of the Chicago school, was skeptical of any sort of active fiscal policy. The first step was to show that econometric models could not be used to design such policy. In fact, the effort failed, but much of the economics profession seems to have thought it succeeded.

The Trailer Input-Output Model

Sometimes builders of elaborate but aggregate econometric models have added industry information to them by attaching an input-output model as sort of a trailer pulled by the aggregate model. The aggregate model provides column totals for the final demands of the input-output model, which is then solved for product or industry outputs. If these calculations are done with constant input-output coefficient matrices over a historical period, the calculated product outputs will often steadily diverge from the historical outputs, so the builders of the trailer model add what they call rowscalars to correct for the systematic divergence. The rowscalars are projected into the future and used to scale each row of the input-output coefficient matrix and the corresponding row of the final demand share matrices. In this way, one can get product projections which are mostly not unreasonable. BUT, after applying the rowscalars to the final demand share matrices, the columns no longer necessarily add to 1.0, so the sum of all the final demands may not be equal to the GDP of the aggregate model.

This problem can, of course, be fixed by re-scaling the columns of the final demand share matrices, but then the rowscalars may be messed up.

A more fundamental criticism of the Trailer approach is that it fails to use information it

generates. If a model generates outputs by industry, those outputs should be used to generate investment and employment by industry. A Trailer model ignores the information is supposedly generates. When a leading commercial forecasting company introduced its trailer model it described the model as state-of-the-art. In fact, it could hardly have been more primitive. If you want product outputs, get them the right way; build an Inforum model which uses the product outputs to calculate investment and employment by each industry.

The Computable General Equilibrium Fashion

I should preface my remarks on Computable General Equilibrium (CGE) models by admitting that

> I never built a CGE,
> I never hope to build one;
> But this I'll say, 'tween you and me,
> I'd sooner build than b'lieve one.

Perhaps I should simply pass over CGE's in silence, but their proponents have been proud and loud, and rather look down on the lowly and modest builders of Inforum type models. So let me try to justify the last line of the above parody of the Purple Cow.

There is potentially a great deal of variety in CGE models. Possibly Inforum models are a special, very atypical kind of CGE. Almost anything specific that one says about them invites the retort, "not necessarily." But in practice most of those I have read about are pretty simple, static affairs, with parameters taken "from the literature" – that is, taken out of the context in which they were estimated. The econometrics tends to be very casual; and the estimates, second hand. The models referred to one period only.

A CGE is usually built in the framework of a Social Accounting Matrix (SAM) which combines *make* and *use* input-output tables with various other elements of national accounts. The identities of the national accounts are expressed through the rule that the sum of any row is equal to the sum of the corresponding column. This way of writing the identities means that the SAMs are usually very large, contain mainly 0 elements, are hard to read but are, of course, mathematically correct. There is presumed to be a representative consumer who has specified demand functions – often of the AIDS type – and each industry has a production function with substitution between capital and labor. A total labor supply is specified. The way of handling the input-output make and use tables make the models use the industry-technology assumption rather than the more plausible product-technology assumption.

The "general equilibrium" term is justified by the solution method. For any specified vector of prices and wages, the consumer demands can be computed and the output that each industry is willing to supply can be calculated. A very ingenious algorithm devised by Herbert Scarf and implemented in the GAMS program and other software with which CGE's are usually built is used to find the equilibrium prices, outputs, and consumer purchases. The sum of employment in all industries equals the labor force or some specified fraction of it defined as "full employment." It is all just like economics textbooks and theory courses say the economy works – though they leave out the Scarf algorithm. Moreover, with GAMS, it is all pretty easy once the SAM is in place. The production functions are usually Cobb-Douglas, so they can be estimated with data from one year. (In the Cobb-Douglas function, $Q = AL^{\alpha}K^{1-\alpha}$ where Q is output, L is labor input, and K is capital input. Assuming perfect competition, it is easily shown that α is labor's share of the product, so no econometrics is necessary to estimate it.) The model is then *calibrated*, that is, its parameters are set

so that the data for one year is fit by the model. There is seldom if ever any testing of the model to see if it fits other years.

In short, the usual CGE is a pretty casual affair, a SAM and some parameter estimates taken from here and there. The glory of the CGE is the Scarf algorithm. But how necessary is it in reality? In the typical CGE, given a set of prices, there will be a unique output which each industry is willing to produce and a unique amount consumers want to buy of each product. And if the prices change, the amounts the firms are willing to produce change. This is the world where the Scarf algorithm thrives.

But it is a world very different from the one I live in. I am typing on a computer for which I paid $400. If I ask the maker how many he would make at $400 each, he will say, “How many do you want?” In other words, his supply curve is horizontal. That puts us back into the world of Inforum models where the producer sets the price and the consumers decide how much to buy. All the fancy market-clearing price calculation is irrelevant. What matters are the consumption functions, the investment functions, the import functions, and so on. In the typical CGE, they were probably made up pretty informally, whereas in the typical Inforum model they have been carefully estimated.

When the North American Free Trade Agreement was being discussed there were more that twenty studies using models to determine its effects. All but two of them were CGEs and assumed full employment. But the effects of the Agreement on employment and the dynamics of adjustment over time were the very heart of the debate! No wonder that the Inforum analysis which linked an Inforum model of the USA with a similar one for Mexico was the only one used by the administration in its presentations to Congress.

For me, dynamics are essential. The model has to be able to show growth to be interesting, Down turns and crises should not be ruled out by the very structure of the model – though it may contain policy variables that can be used to mitigate them. Unemployment is a very real phenomenon, and a model that excludes it is of little interest. I wish I had a model that would produce the subprime boom of 2001-2007 and the crash of 2008, not one that pretends such things don't exist.

The Dynamic Stochastic General Equilibrium (DSGE) Byway

The exaggerated message of the Lucas critique was not unwelcome in the academic world. In fact, much of its influence was due, I believe, to how welcome it was. Building and constantly updating empirically rich econometric models is a lot of work and is not easily published in academic journals. Academic economists who did not like having to build large, data-rich models and having to keep up with what was happening in the real world in order to have their views on economic policy respected were only too happy to be told that those models were no good for policy analysis anyway.

It was said that the Lucas critique demanded models that had “deep” economic roots. What “deep” turned out to mean was that the models had to involve explicit maximization subject to constraints of some utility function. The utility function should be maximized by varying policies; any influence of the policies on the constraints had to be explicit. The Dynamic Stochastic General Equilibrium (DSGE) models[7] were born. They all had a maximizer who had some objective function usually based on his consumption in various periods and a production function with links from one period to the next. Typically, saving in one period would create capital for the next, which would improve the production function in that later period. The maximizer, however, would be hit by random “shocks” which might benefit or hurt him. He could know the probability of various events,

7 A good introduction to these models may be found in *Dynamic Economics – Quantitative Methods and Application* by Jerome Adda and Russell Cooper, (MIT Press, 2003). The applications there are mostly of the sort that I find quite valid. Sadly, the book is dedicated to Lance Armstrong, “notre maitre à tous” and the cover picture shows him winning a race.

but he did not know until the future period how the event would actually turn out. Numerical methods could be used to find optimal policies for the maximizer. A main point was that no constant parameter of the constraints should depend upon the actions of the maximizer.

Such a model would seem to me to make a lot of sense in operations research or in the theory of the firm or of the individual consumer. But they were applied immediately to the whole economy in what were called Real Business Cycle models.[8] It was found, apparently to the surprise of the model builders, that if the production function was an aggregate function for the whole economy, some imaginary maximizer was hypothesized, and the random events hit the aggregate production function, then "business cycles" appeared. Because the models had no financial sector, they were called "real" business cycles. The models were elaborated, run many times with randomly generated shocks, and their results subjected to analysis to describe the nature of the cycles. With proper adjustment of the parameters of the model, they could produce cycles similar on average with those of the real economy.

As mentioned, the origin of the cycles in the Real Business Cycle models is variations in what is rather strangely called "total factor productivity" or TFP. If the aggregate production function is written

$$Q(t) = A(t)f(K(t), L(t)) \quad ,$$

where Q, K, and L are output, capital, and labor respectively, then $A(t)$ is called total factor productivity – "factor independent productivity" would have been a better name. There are indeed, fluctuations in $A(t)$ The reason is that, in recessions, firms "hoard labor," that is, they do not cut back employment as much as output has fallen. They expect to need the labor again as output recovers. If a laid-off worker finds a job elsewhere, the firm will have the expense of training a new employee. Thus, the observable fluctuations in A(t) are not, in fact, the primary cause of business cycles but a secondary effect of the fluctuations. This well-known fact seems never to have bothered the builders of Real Business Cycle models, who confidently put the cart before the horse.

Fluctuations in TFP, which had been quite considerable in the USA during the 1960s and 1970s, lessened after about 1981, a development called the Great Moderation by the Real Business Cycle school and taken as the exogenous explanatory factor for the moderation of business cycles after that date. In fact, however, it was the change in Regulation Q of the Federal Reserve Board that moderated the business cycle, which then moderated the fluctuations in TFP. In other words, the Real Business Cycle proponents got it exactly backwards.

This school has, however, become so influential that I find today's graduate students actually believe it, despite the fact the two recessions they should be able to remember – the dot-com crisis of 2000 and the subprime crisis and Great Recession of 2008 clearly had nothing whatever to with a productivity shock. In both cases, the root problem was that some financial assets had become greatly over-valued. In the second case, having to face the true value of the tranches of packages of subprime mortgages which it held led to the failure of the Wall Street firm of Lehman Brothers. That failure, in turn, set off a paralysis of the financial system that produced The Great Recession. This crash, like all I can think of, originated in the financial sector and spread to the real sector, not the other way around.

A further fundamental problem with the DSGE models is deciding who the maximizer is and specifying his objective function. This is no doubt the basis of Robert Solow's stinging criticism voiced at Congressional hearings on July 20, 2010 held to learn why macroeconomists failed to foresee the financial crisis of 2007- 2008.[9] He said:

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8 Kydland, F.E.; Prescott, E.C. (1982). "Time to Build and Aggregate Fluctuations". *Econometrica* 50 (6): 1345–1370

9 It is off the subject of this paper, but in case anyone would like my answer to this question, I'll give it briefly. The problem arose in the subprime mortgage market, that is, in mortgages that did not meet the traditional standards for

> I do not think that the currently popular DSGE models pass the smell test. They take it for granted that the whole economy can be thought about as if it were a single, consistent person or dynasty carrying out a rationally designed, long-term plan, occasionally disturbed by unexpected shocks, but adapting to them in a rational, consistent way... The protagonists of this idea make a claim to respectability by asserting that it is founded on what we know about microeconomic behavior, but I think that this claim is generally phony. The advocates no doubt believe what they say, but they seem to have stopped sniffing or to have lost their sense of smell altogether.

These models are said to offer "insights" into how the economy worked. How a model which has so little relation to how the economy works could offer "insights" into its functioning totally escapes me. When the nation was in bad need of good economic advice during the 2008 crisis, I heard of none coming from the DSGE or Real Business Cycle school, perhaps because it was so patently obvious that the crisis did not result from a productivity shock.

Nevertheless, some central banks are now maintaining DSGE models in addition to other tools. Perhaps they think of themselves as the maximizer. But in that case, the DSGE is little different from a traditional econometric model with policy-invariant parameters and capable of being run with random shocks and of optimizing a specified objective function with respect to various policies. Models built with the Inforum software have those capabilities.

In short, I think the DSGE models may be a useful tool in operations research, but they add little if anything to our ability to model the whole economy or design good policy. They have, however, absorbed the attention of many bright young economists.

Conclusion

Well-built Inforum and Inforum-type models are data-rich, realistic descriptions of economies that provide sufficient product detail to speak to business users as well as to government policy makers. If thoughtfully constructed, they avoid the Lucas criticism. They are capable of being run in a business-cycle mode or, by proper choice of policies, in a balanced, steady growth mode. The work done on them has been motivated by a desire to make them better descriptions of the real economy, never just to use an ingenious mathematical device. They are more flexible and realistic than the CGEs, more detailed and reasonable than the DSBEs. But building and maintaining them is a lot of work of a kind that is not much rewarded in the academic world. Moreover, they do not guarantee that dangerous developments – such as the subprime boom – will be detected and eliminated before it is too late. But then neither do any of the present alternatives. Somehow, to follow Solow's metaphor, economists need to develop better tools for sniffing out trouble. When they do, I hope the tools will be built into Inforum models.

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investor-grade mortgages. The first problem was – and is – that there are no *regularly released, free* statistics on subprime mortgages. Statistics are compiled, copyrighted, and sold at hefty prices by the Mortgage Bankers Association. From time to time, the Association publishes articles which show the national totals, so the data are not secret but neither are they readily and regularly accessible to academic economists The second problem was that no one suspected that firms with such financial acumen as Bear Stearns and Lehman Brothers would get themselves into such a situation. Even if one suspected trouble, there was no way to prove it from outside the firm. Indeed, it is now clear that only a handful of the top management at Lehman knew how serious the situation was. Thirdly, no one – especially not the President of the New York Federal Reserve Bank – realized how strongly Lehman was tied in with the rest of the financial system. When Lehman failed, no one wanted to lend to anybody for fear that the borrower might in some way be dependent on Lehman for its financial soundness. Thus panic, the greatest danger in the financial world, greatly magnified the effect of the Lehman failure. While it was easy enough to see, as I had done, that the stock market was – and had been for several years – at levels out of all proportion to corporate earnings, it was quite a different matter to pinpoint the time when the crash would come or to forecast its depth.