

A chronological survey of the Friedman–Meiselman / Andersen–Jordan single equation debate

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ABSTRACT

Celebrating the fiftieth anniversary of Friedman – Meiselman’s (1963) empirical answer to the efficacy of monetary and fiscal policies, this paper chronologically surveys twenty-four papers that have been an integral part of the debate over the reduced form, single equation approach that was started by Friedman and Meiselman’s 1963 study. Many of the arguments and criticisms related to the Friedman – Meiselman equation and its famous progeny, the Andersen – Jordan (1968) St. Louis equation are reviewed. Indeed, nowadays few economists even speak of the Friedman and Meiselman approach, it having been long ago usurped by Andersen – Jordan; however, Friedman and Meiselman were the pioneers of the single-equation test and it is their seminal approach and anniversary that is acknowledged here.

Keywords: St. Louis equation, single-equation approach, monetary policy, fiscal policy

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INTRODUCTION

In the third edition of his very popular monetary economics textbook, Carl Walsh (2010) introduces his examination of monetary economics with a brief synopsis of a rather stylized version of Friedman – Meiselman’s (1963) equation, which was the seminal empirical estimate of monetary and fiscal policy comparisons. It is fitting that Walsh would begin with the Friedman - Meiselman equation because it was the starting point for all of the heated debate and empirical testing of monetary and fiscal policies that since transpired.

Below is a chronological review of the debate over the reduced form, single equation approach that was started by Friedman and Meiselman’s 1963 study, beginning with the seminal paper itself. Many papers have been written regarding the subject, and even more are tangential, covering simultaneous equations, fiscal multipliers, the efficacy of monetary policies, etc.; however, the debate Friedman and Meiselman started was about a particular question: can a single-equation regression model answer the most profound questions in macroeconomics, i.e. do fiscal and monetary policies have empirically determinable real effects? This paper stays on that non-tangential path and focuses on the papers that stem solely from, and refer back to, either their seminal work or the paper that ultimately replaced the Friedman – Meiselman approach, Andersen and Jordan (1968) and the St. Louis equation.

When Friedman and Meiselman began their empirical quest, not only was econometrics in its infancy but so was the computer. Indeed much of what they worked on was done by hand. Econometric sophistication and computing power has unequivocally enabled us to do much more since their time, yet the question remains open whether a single-equation approach (or any other approach) can unlock one of the deepest mysteries of macroeconomics.

FRIEDMAN AND MEISELMAN (1963)

Fifty years ago Milton Friedman and David Meiselman (1963) used a simple reduced form ordinary least squares regression equation to compare the effectiveness of monetary and fiscal policies – but even more so to compare Keynesian and monetarist theories. Their brash equation (See equation 1 below) was designed to “prove” whether monetarism was ultimately superseding Keynesianism as the correct macroeconomic theory.

$$C_t = a + VM_t + KA_t \quad (1)$$

In the equation C is induced private consumption, M is roughly $M2$ (currency in the hands of the public plus adjusted demand deposits plus time deposits in commercial banks), and A is autonomous expenditures (actually a combination of deficit spending fiscal policy and net private investment and net exports) and where V represents a special money velocity (i.e. monetarism) and K represents a special expenditure multiplier (i.e. Keynesianism). What Friedman and Meiselman found was that whether using annual data from 1897 to 1958 or quarterly data from 1946.1 to 1958.4 and whether solely contemporaneous or experimenting with various lags, private consumption was not a statistically significantly impacted by

discretionary fiscal policy, but it was by monetary policy. They found that their monetary variables were highly correlated with consumption whereas fiscal policy variables were not.

Of course, as would be expected, there were several criticisms of Friedman and Meiselman's study and specifically about the reduced form approach that they used. These are outlined below.

ANDERSEN AND JORDAN AND THE ST. LOUIS EQUATION

The answers to their critics that Friedman and Meiselman came up with are presented later in this paper, but the many criticisms were also being addressed by others. Indeed the other very famous paper spawned by the seminal Friedman and Meiselman study, and which was to become one of the classics in monetary literature, was published in 1968 by Federal Reserve Bank of St. Louis economists Leonall C. Andersen and Jerry L. Jordan (Andersen and Jordan, 1968). Their study fully supported the Friedman and Meiselman single-equation approach but expanded it to answer several of the criticisms that had befallen that seminal paper. See equation 2.

$$\Delta Y_t = a + \sum_{i=0}^4 m_i \Delta M_{t-i} + \sum_{i=0}^4 e_i \Delta E_{t-i} + \sum_{i=0}^4 z_i \Delta Z_{t-i} \quad (2)$$

In their new equation all variables are in first difference form as denoted by Δ , a is a constant, Y is nominal domestic spending; M represents monetary policy, which was defined either by monetary base or money stock; E represents variously high-employment expenditures, high-employment receipts, or high-employment surplus; and Z represents a catch-all variable they define as "a variable summarizing all other forces that influence total spending." Those forces include weather, international trade, preferences, technology, resources, infrastructure, war, and the like. Using an Almon lag technique with fourth degree polynomials and a four period lag, they combined various measures of monetary and fiscal policies to determine whether changes in those policy variables had a significant impact on the economy's nominal spending. Using quarterly data from 1953.1 to 1969.4 they concluded that, just as Friedman and Meiselman found, monetary policy seemed to have an impact on whatever measure was used for spending, while fiscal policy did not.

REVIEW OF THE SINGLE EQUATION APPROACH LITERATURE

The debate and evolution of the St. Louis equation from the beginnings as the FM model and the first real St. Louis equation by AJ has been significant and has taken place because of the criticisms that have been brought forward by many others. A list of most of the major criticisms is given below. Early on much criticism took place regarding the use of particular consumption functions or gross national (or domestic) product, but another question was: what is the proper form of the variable being studied? Over time the models have become differenced and/or log-linear so as to ensure the similar trends does not create spurious collinearity. But most criticisms have ultimately revolved around three things: 1) the various data do not measure what the theory truly specifies; 2) it is difficult to untangle the

exogenous from the endogenous policy behaviors; or 3) there is inherent coefficient bias (toward zero) when using any variables that are used counter-cyclically.

Still, there were and have been several supporting studies and papers that conceded to a few of the criticisms but held fast to the concept of the single equation approach as well as the broad empirical outcome that fiscal policy is ineffective, while monetary policy is effective. The very interesting and important debate, in chronological order, is followed below.

Friedman and Meiselman 1963

See Section 2 above for the Friedman - Meiselman contribution and the attendant equation (1). But in review, with one broad sweeping stroke Friedman and Meiselman (1963; hereafter FM) shook up the established Keynesian, fiscal consensus by claiming that aggregate consumption expenditures were closely tied to money supply but apparently not strongly tied to autonomous expenditures (read: fiscal policy). Their approach was simple, but perhaps *profoundly* simple in Nobel physicist Frank Wilczek's (2008) use of the term. Using a reduced form equations approach, they modeled the economy in several different versions of the simple single equation form shown earlier (equation 1) and reproduced here.

$$C_t = a + VM_t + KA_t \quad (1)$$

It was this paper that set off a firestorm of protests and counter-articles. Those countering papers made claims such as: the model was misspecified in that important, statistically relevant variables were omitted; the data used were not actually coincident with the theory behind them; there was no correction for the thermostat effect – that discretionary fiscal policy is used because spending and output are down – so that even if fiscal policy is effective it will seem to have a neutral or even negative relationship with spending rather than the positive effect it is theorized to have; and that the results were time-specific, etc. Many of those critical and supporting papers that followed over the last half century are reviewed below.

Hester 1964

Donald D. Hester (1964) was perhaps the first to respond to the FM paper but the types of criticisms he presented were to become a common theme in the years to come. His main criticism was that FM had stacked the deck against a 'Keynesian' outcome. By using the wrong income values by leaving out tax-financed government expenditures (because, importantly, taxes are a function of income) and the wrong 'autonomous' expenditures by using net instead of gross investment, FM had not allowed the empirical model to measure the right things. He argued that government deficits are endogenously determined, not exogenously, thus no single-equation approach could properly capture government spending and deficits. And the same could be said for short-run private investment. Lastly, Hester emphasized that the actual data should have been empirically tested in first difference form so as to de-trend both explanatory variables and show only the endogenously generated

growth. When Hester tried his ‘improved’ data and empirical methods he found that if the data were run in this corrected way, then “the autonomous expenditure theory outperformed the quantity theory” i.e. Keynesian economics again triumphed over monetarist economics.

Friedman and Meiselman – Reply to Hester 1964

In response FM (1964) did a pretty good job of trying to embarrass Hester with their reply. FM argued that their interpretations of income and autonomous expenditures are relevant despite Hester’s misgivings, and proceeded to explain that there was any number of reasonable means to define the two and that theirs had been on sound footing. They showed, too, that Hester’s use of correlation coefficients with his newly defined autonomous expenditures is an unsound argument. Finally, in a strongly worded conclusion they maintained that:

“We remain of the opinion that there is a striking division among students of economic affairs about the role of money in determining the course of economic events. One view is that the quantity of money matters little; the other, that it is a key factor in understanding, and even more, controlling economic change. Our paper tried to present some evidence relevant to deciding between these views. The kind of evidence we gave is not the only kind that is relevant and may not be the most important or significant. And, of course, much other evidence is available from other work by us and by many others. This other evidence needs to be added to and brought to bear on the main issue that divides economists into two groups. Hester does not quarrel with the relevance of our evidence but with the particular form of the income-expenditure theory we use. His criticism of our procedure rests primarily on a misunderstanding of the theoretical basis of our approach. He offers neither theoretical argument nor empirical evidence in support of his alternative formulation. Hence his criticism is largely beside the point. That is unfortunate. We badly need work on these problems that will clarify the issues involved. We can ill afford to waste the energy, interest, and ability that Hester displays in his paper on frivolous quibbling.”

Still, FM did concede elsewhere in their reply that Hester’s suggestion of using first differences was correct and that it is the better method for their single equation approach.

Ando and Modigliani 1965

Albert Ando and Franco Modigliani (1965; hereafter AM) are the classically best-known “first responders” to FM. Their paper is still felt to be the quintessential argument against most of the conclusions FM made and was succinctly synopsised by their opening paragraph: FM has shortcomings in procedures that if repaired changes the result, but moreover, the single-equation approach coupled with the equally single independent variable approach and the corresponding correlations cannot shed light on macro-policy.

AM's first criticism was that FM had misspecified the consumption function (which is interesting in that Friedman (1957) had recently written a book on the subject – a book that he many years later ultimately considered his most important professional contribution) with their particular use of autonomous expenditures. AM claimed that the variable that FM had derived was actually saving, not autonomous expenditures and that the data that were used by FM would need to be modified by including corporate retained earnings, transfer payments made by the government to foreigners, and “wage accruals over disbursement.” A second complaint was that the ordinary least squares equation was biased because of induced influence on the independent variable by the dependent variable, i.e. the independent variable is not really independent. AM manufactured a theoretical model to remove the independent part from the induced part to show that the model would be quite different.

A very different AM criticism of FM was that there was no attempt to determine exogenous and endogenous components to monetary policy in the same manner as there was with fiscal policy. Thus they introduced M^* , rather than a standard money supply variable, which is meant to represent what the money stock would be if high powered money were fully utilized – a high usage variable somewhat akin to a high-employment fiscal variable. The idea is to show that money is not exogenously determined because people can choose to hold money in different amounts as situations warrant and lenders need not lend out all of their excess reserves if they so desire. To AM, this “levels the playing field” for the comparisons of policy variables. If fiscal policy is pulled apart to determine the exogenous components, monetary policy should be done in the same manner. Moreover, they found that the error variance in predicting NNP was much higher when using money than any of the fiscal variables that they introduced, although they never claimed, therefore, that fiscal policy “wins.”

Their conclusions instead were that Friedman and Meiselman's results favored monetary policy because FM had, in effect, stacked the deck. AM concluded that, if both policy variables are given a similarly balanced approach, the end result is that both policies will have real, statistically significant impacts on the economy.

Friedman and Meiselman – Reply to Ando and Modigliani 1965

Having been dressed down by AM's very long, exhaustive analysis, FM (1965) came back with some reasoned answers to AM and to others who had made critical analyses of their original work. First, FM claimed that the autonomous expenditure variable was certainly possible to criticize, but so were any of the alternatives that had been put forward by others – and there had been many. Indeed, in just the three papers, twice as many definitions were put forward as papers themselves. In addition, in particular, because AM used nominal data rather than real data (as did FM), they argued that the empirical AM results were not correctly comparable to their FM results.

As would be expected, FM defended the use of their consumption function and explained why it is the right method to use. However, FM did agree in theory with AM that M^* is a valid means to determine the exogeneity versus the endogeneity of the policy variable; however, they still disagreed with the actual methodology to determine M^* that AM used in their paper.

All in all, FM conceded that if they had built their model to favor monetary policy over fiscal policy, it was because the theory comes out that way, not because they were trying to do so. Indeed FM claimed complete unbiasedness in determining the theoretical and empirical processes they used.

Andersen and Jordan 1968 and the introduction of the St. Louis Equation

See Section 3 above for the Andersen – Jordan contribution and the attendant equation (2), the St. Louis equation.

De Leeuw and Kalchbrenner 1969

Frank De Leeuw and J. Kalchbrenner (1969; hereafter DK) were one of the first to take issue with the new AJ approach, arguing that exogenous fiscal policy cannot be properly measured by using any of the AJ fiscal policy definitions, nor can any single equation approach pull out the particular influences of such a policy variable. DK claimed that because the effects are lost in the complex machinations of the entire economy, there can be no proper means to disentangle the endogenous from the exogenous policy behaviors. They cited, particularly, that the tax and monetary base variables are hopelessly entangled with the endogeneity-exogeneity problem. Moreover, they claimed that the AJ method leaves out any convoluting influences introduced by inflation. And, finally, they contended that causality cannot be shown by the single equation approach and that it is just as likely that GNP is driving fiscal spending as the other way around.

DK then recast the original AJ model using their own modifications, which they claimed was a “clear improvement.” Their changes were to use high employment receipts adjusted for inflation as the fiscal variable and two different versions of the monetary base: an adjusted monetary base (adjusted for changes in reserve requirements) and an adjusted monetary base less currency for the monetary variable.

Using their altered data set cast from 1952.1 to 1968.2 (they did not explain why they did not use the original AJ dates, 1953.1 to 1969.4) they re-ran the St. Louis equation and found that fiscal expenditures were statistically significant and positively correlated to changes in GNP in the long run as was also true for changes in monetary policy. They concluded that AJ were wrong to find fiscal policy as statistically insignificant adding indeed that a closer examination of Fed behavior might be worth pursuing so as to disentangle the endogenous from the exogenous components of monetary policy.

Silber 1971

William Silber (1971) determined that altering the equations to fit what a particular researcher was looking for was enough to alter the findings in the researcher’s favor, hence the political title “The St. Louis Equation: ‘Democratic’ and ‘Republican’ Version and other experiments.” He determined this by looking at four particular elements of the studies: 1) Do changes in lag lengths make a difference? No. When running the St. Louis equation with extended lag lengths Silber was unable to find much difference from that found by AJ. 2)

Have structural changes in the economy changed the results? Yes. By running the St. Louis equation in various time periods that were deemed to have the same underlying structural form, some periods appeared to show fiscal policy as quite significant (e.g. 1960s) while others did not. 3) Do better/other definitions of fiscal policy change the results? Yes. Attempting to answer E. G. Corrigan's (1970) quarrel with how fiscal policy was measured, Silber used what Corrigan called "initial stimulus" and determined that any periods where fiscal policy had shown previously shown effectiveness were enhanced by the new measure; however, other time periods were still not impacted. And 4) are there any exogenous variables left out such that the equations are immediately misspecified? No. Silber made an attempt to determine the possibility of government borrowing crowding out the private sector, but found no impact. He also tried to answer the claim that the failure to use clearly exogenous and theoretically important variables such as consumption and international trade meant that the reduced form equation had to be misspecified. He made attempts to introduce all of the theoretically important variables, but was unable to find statistical significance. Silber took that result as a reason to question the validity of the overall methodology behind the St. Louis equation approach.

Gramlich 1971

Edward Gramlich (1971) gave a very nice synopsis of the *AM – FM* "radio" debate that had occurred up to 1971. He reiterated the three main arguments against FM that had been levied: time periods matter (anticipating Lucas perhaps?), wrong variables for fiscal and for monetary policies, and no correction for co-movements of all variables concerned. Gramlich then explained why he felt that, although AJ had fixed each of the *AM – FM* arguments with their model, AJ was still not the final say in the matter. Indeed, perhaps the most interesting part of his paper (aside from his own attempts at the question) is his Table 1 comparison of multiplier and elasticity estimates for monetary and fiscal policies among several different models and different types of non-single equation models: FM, AM, AJ, Deprano-Mayer, Wharton School Model, Brookings Model, Ando-Goldfeld Model, and FRB-MIT Model. All of the models (except AM) showed monetary policy with a multiplier above one, and usually substantially above one. And in every case save AM and Deprano-Mayer, the money multiplier was larger than the fiscal multiplier.

Gramlich took an empirical stab at it himself by trying three new definitions of monetary policy variables, each of which was intended to remove the endogeneity components of older definitions: adjusted monetary base, free reserves, and adjusted monetary base less borrowed reserves less currency. He defined fiscal variables differently too. Notably, he tried to alter the autonomous expenditures approach by budget aspects that either directly or indirectly affect aggregate demand (rather than the expenditures and tax receipts approach). This led him to use government purchases plus grants as one fiscal variable, and full employment taxes plus full employment social security payments minus unemployment benefits minus interest payments.

Gramlich also introduced two other modifications to the independent variables as well as the main modification already described above. One was to ensure defense spending data were allocated to the correct period, and the other was to account for labor strikes,

believing that they could significantly influence the final results. He also took a novel approach in that he tried several different variables as his dependent variable (and this is important to the modifications done for this paper). He tried variously: real GNP, nominal GNP, Moody's triple-A bond interest rate, nominal personal consumption expenditures, nominal plant and equipment investment, nominal residential construction, nominal inventory investment, and nominal savings and loan deposits. He ran his new equations using an Almon lag technique for either 8 or 10 quarters from 1953.1 – 1968.4 in the form (equation 3) below:

$$\Delta Y_t = a + \sum_{i=0}^{8,10} m_i \Delta M_{t-i} + \sum_{i=0}^8 e_i \Delta E_{t-i} + \sum_{i=0}^8 t_i \Delta T_{t-i} + s_i \Delta S_t \quad (3)$$

where a is a constant, which he described as the coefficient of time when data are in the first difference form; Y is nominal domestic spending; M represents monetary policy in one of the three forms described above; and E represents “government purchases plus exports plus grants-in-aid plus defense inventory adjustment”; T represents taxes and the rest as described above; and S represents man-hours of striking workers per quarter.

Alas, Gramlich ended up with some more or less similar results for GNP measures compared to previous studies. When using monetary base as the monetary policy variable, monetary policy was nicely correlated with GNP and more so than fiscal policy, although both were statistically significant this time. What was more interesting was his results with inventory investment as the dependent variable. His inventory investment results were counter-expected when monetary policy was defined by the monetary base. When free reserves were used the inventory investment results came in line and fiscal policy still showed as a significant variable. When adjusted non-borrowed reserves were used as the monetary variable both monetary and fiscal policy variables were statistically significant and a better fit than the other two cases. Ultimately, Gramlich's study supported that monetary policy is strongly correlated with spending but also found that fiscal policy is correlated as well.

Goldfeld, Blinder, Kareken and Poole 1972

Stephen M. Goldfeld, Alan S. Blinder, John Kareken and William Poole (1972; hereafter GBKP) attacked the AJ approach from an econometric standpoint, and one that has held up to this day: without a reaction function, how can you determine the nature of the “exogenous” from the “endogenous”? Indeed, just as is seen with any monetary rules or automatic fiscal stabilizers, if the rules or stabilizers are done to counter-cyclical perfection, the correlations will not show up with the comparative static sign we would expect. GBKP starkly pointed out the problems with the single equation empirical approach when they showed that simulations that they performed were showing the “wrong” results, much like what would occur using the AJ approach. Their conclusion: The Andersen – Jordan single-equation approach to empirically determining the efficacy of monetary and fiscal policies was without merit.

Poole and Kornblith 1973

William Poole and Elinda B. F. Kornblith (1973; hereafter PK) did a very interesting study of both the FM model and the AJ model in a paper that included AM and Hester's models as well. PK ran each of the single equation models using the same data so that they were able to compare all of the models on 'equal footing.' They extended the data from 1959 up to 1970 for their analysis but used the FM, Hester, and AM models precisely as those authors had formulated. PK then used root mean squared error as a criterion to compare each model's predictions versus the actual data, where they truncated the model and allowed it to forecast data that had already existed. What they found was that the models all tended to underpredict. PK put forth several plausible explanations to try to explain the unexpected negative bias: they suggested that the original equations didn't use a long enough time period; that you can't find short-run effects using long-run models; that the models all failed to take interest rates into account; and that there was a failure to include income taxes, which had changed considerably over the time periods investigated. Finally, when they did a similar analysis of the original AJ model, the opposite occurred and they found that there was an overprediction bias rather than a negative one.

Given all of the results, Poole and Kornblith's conclusion regarding the efficacy of the models was that they had determined that the "decision (about which models were correct or supported monetary or fiscal policies) must still be rated a draw."

Elliot 1975)

J. W. Elliot (1975) entered the fray with an empirical analysis of his own, while using the same variables as AJ. Elliot pointed out that it is difficult to compare the regression coefficients as "multipliers" because their corresponding variables are money, a stock, and fiscal spending, a flow. Still, he acknowledged that the "multipliers" found in his analysis favored the original AJ result by as much as five or six times the effect. Elliot's technique was to compare the original Almon lag approach to other possible approaches to see if there were model-specific reasons for AJ's results. Elliot concluded that irrespective of that technique, the results still fully supported Andersen and Jordan's 1968 results.

Modigliani and Ando 1976

Nine years after their first attack, Ando and Modigliani (1976) teamed up again at a conference at Brown University in 1974 to try to finally bury monetarism. Their corresponding paper was published in a book about the conference two years later (Stein, 1976). Their approach this time was to actually put together a simulated economy of their own making and then to analyze it in an AJ method. By knowing what was actually impacting each variable in their model – and it wasn't monetary policy – but then finding that the AJ model still favored monetarism, they felt that they had hard evidence that the decks were stacked in favor of monetary policy by the AJ approach. This paper was thought to be devastating to monetarism at the time it was published.

B. Friedman 1977

Nine years after AJ, Benjamin Friedman (1977) took a shot at the AJ model, which by then had been made even more famous when used in the Andersen – Carlson (1970) text and which by that time had already been dubbed the St. Louis equation. B. Friedman found that, in using the same AJ model but by simply extending the data set out to 1976.2, fiscal policy was now statistically significant and important in the determination of expenditures although serious heteroscedasticity problems had crept in that had not been there earlier. And he also found that if he used data starting at 1960.1, the statistics were even more favorable to discretionary fiscal policy. Friedman reiterated that Goldfield, Blinder, et al (1972) had already uncovered the coefficient bias inherent in the AJ model and that it was due to the hopelessly entangled endogeneity of fiscal and monetary policy. Thus Friedman warned that it should not have been surprising that a reversal could occur with new data, nor a counter-reversal later on. Ultimately, to B. Friedman, Goldfield, Blinder, and many others, the AJ methodology was unsalvageable.

Carlson 1978

In answer to B. Friedman’s insistence that the AJ model was misspecified, Keith Carlson (1978) made an important empirical modification to the original Andersen-Jordan model. Whereas AJ had used a first difference approach for their model, Carlson determined that a rate of change approach eliminated the heteroscedasticity problems that B. Friedman had uncovered. As can be seen in the new formulation below, Carlson’s single-equation model is similar to the original AJ model but with the significant exception that the rates of change are being used rather than first differences. Carlson’s model is seen in equation 4 below

$$\dot{Y}_t = a + \sum_{i=0}^4 m_i \dot{M}_{t-i} + \sum_{i=0}^4 e_i \dot{E}_{t-i} \quad (4)$$

where the variables are the same as in the AJ model but where the dots over the variables denote growth rates for those variables. Carlson ran the regression model using data from 1953.1 – 1976.4 and also re-ran the model using the original AJ dates from 1953.1 to 1969.4 in order to see if the specific dates made a difference as had been found to be the case using the AJ methodology. Using the new methodology he determined that the model once again supported the original Andersen – Jordan conclusion of significant monetary effects but insignificant fiscal effects.

Van Order 1978

In a short note Robert Van Order (1978) disagreed with the arguments against the AJ model that had been made by Goldfeld, Blinder et al, i.e. that fiscal policy is endogenously determined by the machinations of the economy such that automatic stabilizers and even

discretionary spending are not as “automatic” as they might appear. If true, then the AJ fiscal policy result might again be correct; however, Van Order agreed with the argument against AJ that there is inherent coefficient bias in a monetary authority variable when interest rates are the target and money supplies are thus endogenously determined.

Stein 1980

Sheldon Stein (1980) broke the main criticisms of the AM – FM debate into four categories: 1) policy variables are not truly exogenous; 2) there are omitted variables in the equation; 3) monetary policy should be represented by variables other than money supply; and 4) fiscal and monetary policies follow counter-cyclical behavior thus empirically hiding their true impacts. But the focus of his paper is really on the second problem. Indeed, Stein put together an interesting simulation model to test the problem of omitted variable bias and determined that the AJ model is subject to this problem. He found that by the nature of the St. Louis equation construction there was an upward bias on monetary coefficients and downward bias on fiscal coefficients. Stein therefore concluded that “the St. Louis equation is incapable of yielding accurate estimates of the true ceteris paribus monetary and fiscal multipliers.”

Batten and Hafer 1983

Dallas Batten and R. W. Hafer (1983; hereafter BH) joined ‘the battle of St. Louis’ with an improvement to the empirical analysis by answering another criticism that had been levied: the AJ model does not directly include the influence of international trade. Thus, BH used data from five other countries, Canada, France Germany, Japan, and the UK, to compare to the US results. Their model was (equation 5)

$$\dot{Y}_t = a + \sum_{i=0}^j m_i \dot{M}_{t-i} + \sum_{i=0}^k \gamma_i \dot{E}_{t-i} + \sum_{i=0}^l d_i \dot{X}_{t-i} \quad (5)$$

where all of the variables are the same as has been seen earlier and where \dot{X} represents net exports rates of growth (what BH call “merchandise exports”) and, significantly, where the ‘appropriate’ lag lengths are determined by an orthogonal regression procedure that utilizes a sequential hypothesis testing approach. When BH ran their data from roughly 1960.1 – 1982.1 (there were slight differences in each of the countries, presumably due to data problems) for all six countries they found that the AJ result still held: as they put it “monetary actions have a significant, permanent effect on nominal GNP growth, while fiscal actions exert no statistically significant, lasting influence.” Only the UK was found to have a statistically significant fiscal policy impact by which a one percent increase in the growth rate of government expenditures appeared to generate a half percent permanent increase in income growth rate. All others were statistically insignificant or negative in the long run.

It is interesting that the \bar{R}^2 gradually go down for the US as new data have been added to the original AJ version, and this despite the addition of presumed improvements to the equation over time. It is also telling that the coefficients for determination for the other

countries were all significantly lower than had been the case through the 1960s with the exception of France, which still had a coefficient of determination above 0.80.

Another improvement that HB introduced was to test each country's equation for stability using the dummy variable approach advocated by Gujarati (1970). As they mention, it is important to know that a policy action will not de-stabilize the economy, and they determined that fiscal policy is stable but ineffective in most countries, but where it was unstable was the UK. Thus, where they had found fiscal policy to have an impact was the same place they found instability in the equation.

Layson and Seaks 1984

In a somewhat tangential paper, because their paper was econometrically themed about general testing for proper functional forms, Stephen K. Layson and Terry G. Seaks (1984) used the AJ St. Louis equation as an example of the ability to determine whether a first difference form or a percentage change form is econometrically viable. They performed a maximum likelihood test on both versions of the AJ model for the period 1953.1 – 1969.4 and determined that the first difference approach is the econometrically correct functional form while the percentage rate of change is not, thus supporting the AJ and Gramlich approach, while not supporting the BH approach.

Ahmed and Johannes 1984

In a “Note” published in 1984 Ehsan Ahmed and James M. Johannes (1984) attempted to reduce the criticisms to three main arguments against the Andersen – Jordan model: “the regressors...are not statistically exogenous”; “there may be other relevant regressors”; and “the constrained Almon lag procedure imposed” may lead to improper estimates in the OLS equation. Thus, they set out to determine the validity of the three claims. Using data from 1959.1 to 1979.3, all variables as recommended by AM's criticisms, they run the model below that follows the BH approach (equation 5, the one panned by Layson and Seaks) (equation 6):

$$\dot{Y}_t = a + \sum_{i=0}^j m_i \dot{M}_{t-i} + \sum_{i=0}^k \gamma_i \dot{E}_{t-i} + \sum_{i=0}^l d_i \dot{X}_{t-i} + e_t \quad (6)$$

and which places endpoint restrictions on the coefficients such that the summation of the lagged coefficients from $i = 0$ to l are zero. The paper attempted to simultaneously test the validity of all of the restrictions placed on the St. Louis equation: for exogeneity of the independent variables, for the imposed restrictions, and for the endpoint restrictions shown above. They concluded that the St. Louis equation passes the test of exogeneity and other restrictions and that, in confirmation with AJ, only money matters, in that money was statistically significant while government spending was not.

Batten and Thornton 1986

The sole purpose of the Dallas S. Batten – Daniel L. Thornton paper (1986; hereafter BT) was to defend the AJ methodology. BT broke the criticisms into three main types and proceeded to explain why the criticisms were off the mark. First they dealt with the misspecification criticism. They showed that consistent single equation models could be developed and that the Ando – Modigliani 1976 paper was not the nail in the coffin after all. To Batten and Thornton, the AM 1976 model was a “statement about Keynesian versus monetarist views” more than an indictment of the AJ model and proceeded to prove this by subjecting their model to a number of econometrics tests for misspecification, perhaps the most significant being the Ramsey – Schmidt (1976) RESET test. The Andersen – Jordan model “passed.”

The second criticism they tackled was the exogeneity/endogeneity problem or simultaneous equation bias. After Chow, Granger causality, and Wu tests were performed on the AJ model, it again “passed.” Finally, the third major criticism was that AJ used the wrong variables to test for monetary and fiscal policies. As has been indicated in this paper, for every reviewer there seems to be a better variable to use. Batten and Thornton come to the conclusion that no matter what variables AJ had chosen, because the results were in favor of monetarism, there would have been a sincere backlash. Ultimately, BT found the AJ paper to be one of the most important macroeconomic papers by giving us “one of the most stable, lasting and robust equations in applied economics.”

McCallum 1986

Bennett T. McCallum (1986) published a paper that reviewed the monetary versus fiscal policy debate that had ensued up to that time and included a review of the St. Louis, single-equation approach from an econometric perspective. He enumerated the “methodological objections” to Friedman and Meiselman that have been seen elsewhere (1) How do you properly measure exogenous fiscal and monetary policies? and 2) Shouldn’t the analysis be done by simultaneous equations?). But his emphasis was on refuting the econometric criticisms of the AJ model that had been put forward by Blinder and Solow (1974), Meyer and Rasche (1980), and Ando and Modigliani (1976). McCallum first reminded us that Carlson (1978) had already shown that using rates of change rather than first differences took care of the heteroscedasticity problem, and then he addressed the criticisms pointed out by Blinder and Solow. Their paper had made the claim that the AJ equation was misspecified, that fiscal policy was improperly measured causing a coefficient bias toward zero, and that AJ were unable to untangle exogenous from endogenous policy behaviors.

McCallum answered the criticisms thusly: First, he showed that the AJ equation is not misspecified because it is not actually a reduced-form equation. By the interesting argument given below, McCallum was able to show that the equation is instead a final-form equation if policy variables are truly exogenous. He argued that a single equation model of the form used by Carlson and others (using McCallum’s notation), i.e.

$$\Delta y_t = \alpha + \beta(L)\Delta m_t + \gamma(L)\Delta g_t + \mu_t$$

would instead look like

$$\Delta y_t = \alpha + \beta(L)\Delta m_t + \gamma(L)\Delta g_t + \delta(L)\Delta z_t + \mu_t$$

if there were an omitted variable, ΔZ_t . And if ΔZ_t is endogenously impacted by the other variables such as the form

$$\Delta z_t = a_0 + a_1\Delta y_{t-1} + a_2\Delta m_{t-1} + a_3\Delta g_{t-1} + \xi_t$$

then by substitution the relationship arrived at is

$$\Delta y_t = \alpha' + \beta'(L)\Delta m_t + \gamma'(L)\Delta g_t + \mu'_t$$

which, although it looks like Carlson's form above, is actually crucially different in that the coefficients represent final forms rather than reduced form coefficients. The single equation approach is completely correct as long as the changes in policy variables are truly exogenous. Of course, as McCallum pointed out, no macroeconomic variable can be considered entirely exogenous, even including population growth and technological change. Charges that the AJ equation cannot verify endogeneity in its variables is therefore actually true for *any* econometric model. Only the degrees of exogeneity are different.

Second, McCallum dismissed Blinder and Solow's (1974) fiscal policy measurement problem, that AJ did not use proper weighting in their fiscal policy variables, by showing that Blinder and Solow's argument is true only when assuming a Keynesian multiplier model. Given that AJ's model was trying to show that the Keynesian multiplier models are wrong, McCallum claimed that the Blinder and Solow argument was not germane to the AJ model, which has a completely different axiomatic foundation.

Additionally, the problem of variable endogeneity was tackled by an empirical analysis comparing instrumental variables to those of OLS estimators. What he found was that this last criticism is to some extent valid, but even under his new analysis, monetary policy seemed to be significant in the long run while fiscal policy was not.

McCallum then turned to the Ando – Modigliani (1976) paper that, through a simulation model, was purported to show a favorable monetary policy bias and an unfavorable fiscal policy bias when applied to a St. Louis equation. McCallum claimed that their simulation approach using the MPS (M.I.T. – Penn – Social Science Research) model was misleading in that the empirically determined multipliers were being compared to fiscal and monetary multipliers "known" from the MPS model. McCallum argued that the MPS model was subject to the very same objections because it too treats many variables as exogenous and "incorrectly omits a large number of behavioral relationships." This strong argument is much like the Lucas (1976) critique, which changed the face of macroeconomics.

Finally, McCallum addressed the Sims (1982) argument that vector autoregression (VAR) analysis shows that money is nearly statistically insignificant when interest rates are

simultaneously used as a second measure of monetary policy. McCallum countered simply that changes in the money stock will not show monetary policy if interest rates are included. Much like the AM argument, McCallum contended that changes in money supply do not necessarily reflect monetary policy changes, particularly when interest rates are the monetary policy variable used by monetary authorities.

Jordan 1986

Almost twenty years later, Jerry L. Jordan (1986) revisited his Andersen – Jordan paper and addressed the controversy that the paper had stirred. He still backed the original analysis and remained staunchly monetarist, arguing that the original paper had been attacked for two main reasons: because it seemed to eliminate fiscal policy as a viable macroeconomic option and because it appeared to mitigate the need for large scale econometric models – and thus put a lot of econometricians out of business – not for the truthfulness of the results. He recognized that new data, different structural underpinnings in the economy and newer techniques had and would later come into existence. Still, he maintained that “the enduring contribution of the AJ approach is the methodology employed to assess the differential impacts of policy action on the economy, not the specific results offered at that time” – and this despite the widespread use of vector autoregression techniques in the interim. Significantly, Jordan lamented not having been more specific in their paper’s conclusion. To him, policymakers mistakenly took the AJ paper as a support for using monetary policy as a discretionary tool, given that the correlations were always high, but that was not AJ’s intent. To Jordan, ΔZ (the “variable summarizing all the other forces that influence total spending in the economy”) was an important catch-all variable in their paper, but policymakers were conveniently leaving out its importance. Jordan was still pushing the idea that fiscal policy was ineffective, but felt that monetary policy should take a monetarist stance: rule-based predictable rates of monetary growth, not as a discretionary tool as he felt the AJ paper had led policymakers to use.

Belliveau 2011

In a recent working paper, Stefan Belliveau (2011) rehashed a stereotypical view of the entire monetary policy versus fiscal policy debate by breaking the debate down to three what he called “interpretations”: Real Business Cycle theory says that neither policy is very effective; Keynesian theory suggests that government expenditures can influence economic output while monetary policy is not as effective; and monetarist theory says that monetary policy is effective while fiscal policy is not.

This view of “interpretations” is more political than it is economic; however, there is a grain of truth to the view he presents, despite the old-school approach. To settle the matter Belliveau tried yet another attempt at the Andersen – Jordan equation by including Gross Value Added by Sector as his output dependent variable, considering it necessary to look at these data if policymakers are attempting to stabilize economic fluctuations. His main model is given in equation 7 below, although he performed several differing versions of it including versions that used a lagged dependent variable approach, which is new to the literature.

$$\Delta y_t = a + \sum_{i=0}^1 m_i \Delta b_{t-i} + \sum_{i=0}^1 e_i \Delta r_{t-i} + \sum_{i=0}^1 d_i \Delta o_{t-i} + e_t \quad (7)$$

where Δy_t is the first difference of the natural log of value added in the business sector (found in the National Income and Product Accounts Table 1.3.5 Gross Value Added by Sector), Δb_{t-i} is the first difference of the natural log of the adjusted monetary base, Δr_{t-i} is the first difference of the natural log of cyclically-adjusted government revenues, and Δo_{t-i} is the first difference of the natural log of cyclically-adjusted government outlays. The data were annual and due to that the lags were of one year only. Belliveau did not try to fight any serious econometric battles that had been raised earlier; he simply sidestepped those problems and went to the meat of the questions raised about the efficacy of monetary and fiscal policies with a standard OLS regression analysis. Using annual data from 1956 to 2007, and music to the ears of policymakers everywhere, Belliveau found empirical support that both monetary and fiscal policy seem to help stabilize the US economy and considers the use of both policies reasonable over the recent recession.

A Reflection and Summary

It has been fifty years since Milton Friedman and David Meiselman asked the simple question: what is the efficacy of monetary policy compared to that of fiscal policy? Their paper, its model, its results, and its meaning, set off a storm of controversy and indeed helped send macroeconomists on a quest for new modeling and econometric techniques. In the half-century interim the importance of monetary policy has certainly gained acknowledgement; to the extent that it can now be said that monetary policy has often superseded fiscal policy as the demand management policy of choice, something unheard of during the halcyon days of the Old Keynesian era. Still, the numerous papers, models, results, and interpretations, that transpired have left us with no true consensus in the profession and monetary policy was not the sole policy of choice during the Great Recession.

Of the papers that were spawned by Friedman and Meiselman, there were several important and also some minor criticisms that were levied against the Friedman – Meiselman and/or Andersen – Jordan results. The main criticisms are summarized below, but without the responses from Friedman and Meiselman or any of their supporters.

1. The first major criticism was that, in representing discretionary fiscal policy, Friedman and Meiselman had used a peculiar deficit spending variable rather than using any tax-financed government spending (Hester, 1964; Ando and Modigliani, 1965; De Leeuw and Kalchbrenner, 1969; Silber, 1971; Gramlich, 1971).
2. The second critique was that, because government budget deficits are at least in part endogenous, it was incorrect to use those data because they do not reflect true discretionary fiscal policy (Hester, 1964; Ando and Modigliani, 1965; De Leeuw and Kalchbrenner, 1969; Silber, 1971; Gramlich, 1971).

3. It was inappropriate to use non-differenced data because there can arise spurious correlations simply due to the commonality of growth over time by all variables involved (Hester, 1964).
4. The Friedman and Meiselman consumption function itself (i.e. equation 1) was said to be misspecified (Ando and Modigliani, 1965). The claim was that, given the way Friedman and Meiselman had defined their variables, what was being used was actually saving rather than the mislabeled autonomous expenditures, A .
5. It was argued that even if the variables in the function were correct, the two explanatory policy variables were not given the same test. That is, discretionary fiscal policy was not properly compared to discretionary monetary policy (Ando and Modigliani, 1965). Ando and Modigliani suggested breaking any monetary variables into discretionary and non-discretionary components as well, famously introducing M^* as the discretionary component.
6. The argument was made that even if Friedman and Meiselman had done everything correct econometrically, there still was no way to show causality and thus it was just as possible that the correlations revealed a causal relationship between consumption and money and not the other way around (Ando and Modigliani, 1965; De Leeuw and Kalchbrenner, 1969).
7. As is well known, if a regression equation is missing a relevant variable, then there is subsequent coefficient bias in the remaining explanatory variables. Thus, the criticism that there are other omitted variables (Stein) intimated that none of the results that had been found by either Friedman and Meiselman or Andersen and Jordan were usable.
8. Some authors used data from differing time periods and found that their results were time period specific (Gramlich 1971; Silber, 1971).
9. Growth rate forms for the variables may be incorrect (Layson and Seaks, 1984)
10. Independent variables are really not independent (Van Order, 1978; Goldfeld et al, 1972; Ando and Modigliani, 1965; De Leeuw and Kalchbrenner, 1969; B. Friedman, 1977)

This paper has laid out a chronological survey of the famous debate spawned by Andersen and Jordan's 1968 St. Louis equation. Much criticism and support for their view has transpired over the last forty-five years, but to no definite conclusion. However, it seems clear from these studies that altering the dependent and independent variables has a large impact on the final results, irrespective of the timing or lags. The survey points to the importance of clearly defining precise objective functions or theories and using the appropriate variables and methodologies to empirically test those theories.

The single equation approach now competes with various vector autoregression methods, just as it competed with large simultaneous equation models in the past, but it still appears to be breathing.

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