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GEORGE SHULTZ, ARTHUR LAFFER, AND THE ECONOMIC DEBATE OF THE NIXON YEARS

By Brian Domitrovic, Ph.D.¹

On the death of legendary public servant George P. Shultz several days ago, the *New Republic* ran an extraordinary piece of historical reflection, Bruce Bartlett's "George Shultz, the Godfather of the Discredited Laffer Curve."²

Bartlett's piece uses Shultz's death as the merest of pretexts for its principal purpose, which is to ridicule Arthur Laffer's turn as Shultz's chief economist at the Office of Management and Budget (OMB) from 1970-72. I take particular interest in this topic because I have been researching it intensively for the last three years, across all the relevant archives including the Richard M. Nixon Presidential Library in California, in preparation of a [book](#) I have now completed on the first years of Laffer's career in economics. This book, *The Emergence of Arthur Laffer: The Foundations of Supply-Side Economics in Chicago and Washington, 1966-1976*, will be out next month from Palgrave Macmillan.

Bruce Bartlett has an impeccable sense of timing, in that this very moment, right before my book comes out, is the last possible occasion on which one can get away with making utterly untenable statements about Laffer's time in his OMB position. Bartlett's piece is a collection of such untenable statements, and they echo similar ones from over the years from source-deprived historical commentators who have tried to trash the work Laffer did at OMB. Let us now put to rest the claims. And for the full story, there are two full chapters in my book, numbers 5 and 6.

Bartlett's claims against the evidence

1. The 1065 forecast number

For starters, Bartlett writes that:

"During this time," in the early 1970s at OMB, "Laffer was involved in a highly contentious dispute between the OMB and the CEA. Briefly, Laffer forecast a level for the gross national product of \$1,065 billion for 1971. This was about \$20 billion above the consensus view among forecasters."

The archival evidence from the Nixon library indicates without exception that the \$1,065 billion number forecast for 1971 GNP—a distinctly high number representing nearly 10 percent nominal economic growth in one year—came from the Council of Economic Advisers (CEA), not OMB, and therefore not Laffer, beginning in June 1970, four months before Laffer joined OMB. Laffer did not forecast "1065," as everyone called the number then. CEA forecast it, and then asked OMB if its own model would find this estimate reasonable. Laffer built the OMB model in the fall of 1970, ran 1065 through it, and confirmed that the forecast was reasonable. See [this](#) series of documents for CEA memos from June-November 1970 on its deriving of 1065. OMB played a spectator's role (Laffer joined the agency in October 1970) as CEA concocted the forecast number in the summer and fall of 1970.

As for "a highly contentious dispute between the OMB and CEA," Bartlett never tells us what it was. He implies that it was over the number 1065, that presumably it was too high. There was, in the first place, no contentious debate over 1065 through almost the whole of the latter part of 1970, as the White House agencies prepared their unified forecast for 1971. CEA came up with 1065 and OMB okayed it. This made for no dispute.

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² Bruce Bartlett, "George Shultz, Godfather of the Discredited Laffer Curve," *The New Republic*, Feb. 8, 2021. <https://newrepublic.com/article/161293/george-shultz-monetarism-laffer-curve>

As for a dispute that did arise, in late December 1970 and January 1971, a disagreement emerged between CEA and OMB about whether monetary policy had to be loose to get GNP as high as 1065. CEA felt so, and Shultz and Laffer felt not. Nixon opted for the OMB position. When the president did this, CEA lost confidence in its own forecast of 1065. Various members of the CEA, above all chair Paul McCracken, tried to distance themselves from the forecast because Nixon would not allow loose money.

Among the papers in the McCracken collection in the Nixon library is a memorandum McCracken wrote to Shultz in December 1970. It proceeds:

“I propose that for the purpose of settling on budget receipts estimates we use the following projection of the path of the economy which is desirable and feasible.”

He named 1064 as the GNP number for 1971 GNP, derived as it had been at CEA. McCracken went on:

“I further propose that we do not settle on an Administration view of the rate of monetary growth needed to achieve this path until we have discussed together the estimates available from a variety of sources.”

The dispute between CEA and OMB had nothing to do with 1065 (or 1064) per se, or OMB's okaying it through Laffer's model. It had only to do with the rate of money growth needed to get there. Importantly, McCracken told Shultz, in this memo, that no matter what monetary-policy view would be the final one, he accepted 1064 as the joint administration forecast. 1064/5 did not arise from Laffer's model. It came to Laffer's model from CEA.³

2. Laffer's money doctrine

Bartlett's next point concerns the nature of the econometric model Laffer constructed at OMB in the fall of 1970:

“Furthermore, the method by which Laffer derived his forecast was a bit cockamamie. He thought increases in the money supply translated almost instantaneously into increases in nominal GNP—a view far outside the economics mainstream.”

The “endogeneity” of the money supply—that the money supply and GNP activity are one and the same—had earned Laffer a *Journal of Political Economy* (the top in the field) publication on the topic in 1970, Laffer's Stanford professor Edward Shaw was an eminent proponent of this doctrine throughout the 1960s, and Fischer Black at the University of Chicago in the early 1970s prevailed with this argument in both University of Chicago workshops dominated by Milton Friedman and in organs as powerful as the *Journal of Finance* and the *Journal of Economic Literature*. In 1970, Yale's James Tobin wrote a *Quarterly Journal of Economics* article calling into question the view that money is not felt immediately in GNP, putting it this way:

“A great deal of the popular and semiprofessional appeal of the modern quantity theory can be attributed to these often repeated facts. However, the relevance of timing evidence has been seriously questioned.”

That Laffer's money views as embedded in the OMB model, as Bartlett put it, were “far outside the economics mainstream” is entirely incorrect.⁴

3. The simplicity of the model

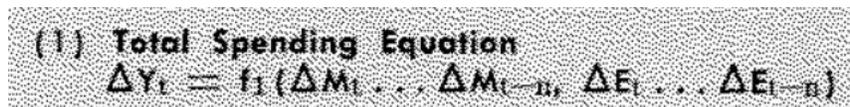
Bartlett's next point:

“Basically, Laffer ignored everything economists normally look at when making a macroeconomic forecast, such as personal income and spending, corporate investment, the trade deficit, and so on. He looked only at the money supply.”

The celebrated new macroeconomic model of the day in 1970 was that of the St. Louis Federal Reserve. This was that model's GNP change equation in its entirety:

³ The sources for these and other quotations may be found in Brian Domitrovic, *The Emergence of Arthur Laffer: The Foundations of Supply-Side Economics in Chicago and Washington, 1966-1976*, Palgrave Macmillan, forthcoming.

⁴ James Tobin, “Money and Income: Post Hoc Ergo Propter Hoc?” *Quarterly Journal of Economics* 84, no. 2 (May 1970), 302.



(1) Total Spending Equation
 $\Delta Y_t = f_1(\Delta M_t, \dots, \Delta M_{t-n}, \Delta E_t, \dots, \Delta E_{t-n})$

There were two variables, the change in the money supply (M) and that of certain government expenditures (E), inclusive of lagged values of these variables. The St. Louis model included nothing that Bartlett said econometric models have. Yet as of 1970, this was the premier example of “what economists normally look at” as they strove to model the relationship of major economic variables to macroeconomic outcomes such as GNP.⁵

The Laffer model, made shortly thereafter, was the St. Louis model with several intriguing additions. The purpose of the St. Louis model was to test Keynesianism against monetarism. If federal expenditures tracked GNP better than the lagged money supply did, the model would vindicate Keynesianism over monetarism. The opposite result would vindicate monetarism over Keynesianism. The purpose of the model was to test these comprehensive hypotheses against each other.

Laffer added one other tradition, that of efficient markets. He had been collaborating with Eugene Fama at the University of Chicago in developing this theory that would become synonymous with Fama’s name in upcoming years. To the M’s and the E’s Laffer added stock market and treasury yield variables (and hours lost to strikes), on the assumption that market prices track GNP levels. As Laffer fed in the data, he found that the efficient market variables captured GNP change levels better than the two variable sets of the St. Louis model. There was no assumption in the model that changes in the money supply were immediately felt in GNP. The model tested for this, among other things, as hypotheses. The lack of a money-supply lag in GNP was a result, not a premise.

This in sum was Laffer’s contribution in the historical event to which Bartlett addresses himself. To say that the OMB model was “far outside the mainstream” is to know nothing about the model and its context. Laffer’s model was so close to the mainstream that it was virtually indistinguishable from the St. Louis model, but for its addition of another macroeconomic tradition to be tested, that of efficient markets, as well as (in a point Bartlett does not make) its inclusion of non-seasonally adjusted data.

To say, furthermore, that “Laffer looked only at the money supply” is again to get the history exactly wrong. Laffer looked at three things: the money supply; government purchases and transfer payments (both together and separately); and the efficient markets variables. Each one represented a macroeconomic tradition, and the point of the model was to test the traditions against each other. It is to have no regard for the facts to say that Laffer’s OMB model “looked only at the money supply.”

4. Friedman’s view

Bartlett then speculated:

“It must have been especially painful when the economist Milton Friedman—the dean of monetarism—dismissed the Laffer model as utterly implausible.”

The purpose of the Laffer model was to test monetarism (along with Keynesianism and efficient markets), which as it happened was Friedman’s life work. Of course Friedman was going to be fully defensive regarding his own position if the model did not vindicate it. Laffer and Friedman were close friends at the University of Chicago and stayed close friends through Friedman’s death years later. A key part of that friendship was intensive debate on points of near fundamental disagreement. To say that Friedman’s critique “must have been especially painful” suggests, again, a lack of understanding of the context in which these events took place.

Moreover, it is well known in economic history circles that Friedman, as of the early 1970s, was taking on increasingly confident advocates of the money view supported by Laffer’s OMB model. As I write in my forthcoming book:

“Fischer Black’s math indicated that it was impossible not to profit from exogenous characteristics of the money supply. Unless there were some mass arbitrage failure in the markets, an economy creates money endogenously. If there were lags, there was money to be made on the knowledge. Milton Friedman was cool to the idea.”

⁵ Leonall C. Andersen and Keith M. Carlson, “A Monetarist Model for Economic Stabilization,” *Federal Reserve Bank of St. Louis Review*, April 1970, p. 9.

I then quote historian Perry Mehrling on Black's presentation of these views in Friedman's money workshop in 1972:

"Into the lion's den went Fischer....[Business professor] Jim Lorie recalls, "It was like an infidel going to St. Peter's and announcing that all this stuff about Jesus was wrong." Friedman led off the discussion: 'Fischer Black will be presenting his paper today on money in a two-sector model. We all know that the paper is wrong. We have two hours to work out why it is wrong.' And so it began. But after two hours of defending the indefensible, Fischer emerged bloodied but unbowed. As one participant remembers, the final score was Fischer Black 10, Monetary Workshop 0."

Friedman's criticism of Laffer must have been especially painful? Laffer was the one who was starting the tidal wave of criticism of the Friedman model.

5. Rivalry at Chicago.

Bartlett's next point is another imponderable evidentiary challenge:

"It should be noted that relations between the university's economics department and the business school had long been chilly, with those in the business school feeling that the economics department treated them as members of the junior varsity."

Friedman having been in the economics department and Laffer at the business school. On that business school faculty were such lions of economics as George Stigler, Eugene Fama, Fischer Black, Arnold Zellner, Merton Miller, and George Shultz himself. What Bartlett is referring to in his remark about "the junior varsity" is beyond comprehension.

The OMB model and the arc of history

Bartlett continues with more mistakes. A controversy at Chicago regarding whether Laffer had completed his graduate program "caused him to depart for the University of Southern California when his government service ended." In point of fact, Laffer's government service ended in the spring of 1972, and he left Chicago in the fall of 1976, a period of more than four years. About the presidential Medal of Freedom, Dolly Parton (unlike Laffer who accepted) "had the class to turn it down—twice," even though the article that Bartlett cites on the matter is clear that serious health matters compelled the singer to beg off. Bartlett even says that:

"During the Trump era, Laffer jettisoned his long-held view on the critical importance of free trade, in order to curry favor with the protectionist president."

Laffer has been a steadfast and reasoned advocate of free-trade, and Bartlett has no idea what Laffer said in his private consultations with President Trump.

Bartlett does regain some form in his observation that 1065 turned out to be a perfectly reasonable figure, just as the OMB model had had it in 1970. He notes:

"Subsequent revisions" of 1971 GNP came in "far above the forecast that was so widely ridiculed for being absurdly high."

Bruce Bartlett has written the most authoritative historical essays on the concept of the Laffer curve—his remarkable 2004 and 2012 series on the topic. His 2004 essay on "Supply-Side Economics: Voodoo Economics or Lasting Contribution?" is the greatest historical essay on the Laffer curve ever produced. His voice on the epic tax history matters of the fomenting supply-side years of the 1970s is essential and invaluable. Bartlett's talents in the Shultz case here, however, were put to poor use.

In *The Emergence of Arthur Laffer*, I comment on the costs we have incurred, in terms of historical understanding, in being careless about—and supercilious toward—supply-side history. The academic establishment (and now via the Bartlett *The New Republic*) makes us all less informed on account of its condescending Keynesian proclivities. We have next to no legitimate historical commentary, that actually reads sources and gets things right, about such episodes as the 1065 matter (Allen J. Matusow's work of the 1990s being an exception). This is a central reason I pushed on with researching and getting my book now to print. One conclusion Bartlett's piece does make inescapable is the centrality of Arthur Laffer's presence in the highest circles of American political-economic debate, policy, and decision-making over the last five decades.

George Shultz was one of the premier American statesmen of the twentieth century in two realms, those of diplomacy and political economy as well. On so many occasions in the latter capacity, he chose to bring in Arthur Laffer as an advisor. He called on Laffer to be the chief economist at OMB in 1970. When Shultz became Treasury secretary in 1972, he hired Laffer as a consultant. And in 1981, Shultz arranged for Laffer's membership on Ronald Reagan's Presidential Economic Policy Advisory Board. And before all this public service, Shultz was dean when Laffer joined the University of Chicago School of Business in 1967. Bartlett's piece therefore has clarified an important fact about the remarkable public servant George Shultz, namely that one of his closest policy confidantes over the decades was Arthur Laffer.