# Stock/Flow Ratios with Money and Debt: What Can Be Learned From the Breakup of Past Relationships in the United States?

By George M. von Furstenberg,* Bloomington/Indiana

In recent years, the behavior of the GNP velocities of money (M1) and total domestic nonfinancial debt (NFD) have served as reminders that relations between economic stocks and flows may change unpredictably. Although laws linking flows to stocks inevitably remind one of physical or technological processes, the looseness and changeability of such laws in economics is quite different from the stability encountered in down-to-earth uses of physics. For instance, the flow of electricity that can be derived from a stock of gravitational energy, estimated from the weight and height of water in a hydroelectric reservoir, is closely related to the kinetic energy imparted to the turbines. If turbine efficiency and the relevant settings are approximately known and constant over periods such as days or weeks, there are covering laws (general laws that need not be proved for each individual case) that apply for deducing electricity output. Indeed, the functions that deduce flows from stocks via the generating process can be fitted almost as precisely as one cares to measure and calculate. Empirically, one would find that past relations are predictive. While some unexplained statistical variation in the stock to flow relation will remain even under the most tightly controlled operating conditions, that variation is presumably stationary around the mean rate of (electricity) output.

Contrary to this example from physics, stocks of assets and liabilities that are widely used in economics are pastiches of convenience and not tightly linked to flows by physical laws or engineering controls. The definitional boundaries of such stocks are largely arbitrary; *Milton Friedman* (1961, p. 208) has written eloquently about this. Furthermore, a change in one component of a stock aggregate may well have a different effect on a flow variable than the same amount of change in another component. Even successive changes in the same component, equal in all respects except their dates, may have different implications if costs and benefits, or the purposes served,

---

* Rudy Professor of Economics, Indiana University. *Bang Nam Jeon* assisted with this research. My colleague *Elmus R. Wicker,* provided useful comments.

have changed over time. In economics, past relations between stocks and flows thus need not be predictive.

Another difficulty is that the setting of the controls may not be immediately observable or entirely governable by reliable mechanisms laid down in advance. In that case, finding a close simultaneous relationship between a financial quantity variable and nonfinancial activity ex post would be of little help to controlling the latter by operating on the former. *Benjamin Friedman* (1983 b) has explored this and related points.

Notwithstanding these familiar cautions, the appearance of empirical regularities in ratios of particular stocks, constructed as simple dollar aggregates, to flows tempted eminent economists to make patterns of the past data record on which policy designs could be based. Until quite recently, they issued advice in which money and debt were treated as a solid means of support for policy goals. This paper attempts to give an account of what their confidence rested on, how its empirical basis was shattered, and what might be learned from the most recent experience about prognosticating stock-flow relations. Proceeding in order of seniority, this will be done first for *Milton Friedman* and then for *Benjamin Friedman*. In both cases we will not explore the extent to which the intermediate policy targets, money and credit, can be controlled, but what might be achieved predictably if they could be controlled. Achievements relate, of course, to contributing to some of the ultimate objectives of economic policy, or at least to attaining the path desired for nominal GNP.

## I. Milton Friedman on the Velocity of Money

In September of 1983, *Friedman*, counting on continuation of the past pattern of velocity, published a widely noticed article in which he complained of excessive monetary growth over the preceding year. Pointing in particular to the first two quarters of 1983 during which M 1 was then reported to have grown at an annual rate of 13.8 percent, Friedman (1983, p. 18) concluded that the damage was done because undesirable consequences were bound to follow. He saw no middle course left that could avoid both the near-certainty of overheating, followed by higher inflation "probably in middle or late 1984," and a premature termination, or at least decided slowing, of the expansion should money supply growth be reduced sharply from the levels previously found excessive.

A passage between *Scylla* and *Charybdis* nevertheless was found. The money stock grew, for a time, by less than half its previous rate, rising only

6 percent from the third quarter of 1983 to that of 1984. However, over the same period, real GNP grew at exactly the same rate as money, while inflation in the GNP deflator continued unchanged at 4 percent. Velocity growth thus quickened temporarily. Thereafter, it slowed as monetary growth increased progressively, finally topping 15 percent from fourth quarter 1985 to 1986, but inflation stayed low and the expansion continued at a slackening pace. Both did not pick up again until early 1987. Hence, for at least three years running, ending in 1986, nothing had gone the way *Friedman* would have predicted from the vantage of 1983: during this time, velocity appeared to trample randomly over the past pattern. At the very least, timing was off by several years.

What is the pattern that *Friedman* fitted perfectly to the expansion starting in the first quarter of 1975 and ending, as he saw it, six years later? First of all, Friedman used "leading" velocity, GNP divided by the seasonally adjusted average stock of M1 two quarters earlier, in all his deductions. He took this velocity to grow at a trend rate of 3.1 percent per year plus 3.0 percentage points extra for the velocity-raising cyclical effect of expansion from trough to peak. A two percentage point rise in the 6-month commercial paper rate contributes part of his normal cyclical effect in expansions[1] but anything beyond, multiplied by 0.38, the ratio of the monetary base to M1, gives the change in the opportunity cost of holding money. For each one percentage point addition to the latter, leading velocity increases by 2.2 percentage points.

This last coefficient, 2.2, was chosen to fit the data available to *Friedman* (1983) perfectly in replicating the change in leading velocity between start and end of the 1975:I to 1981:I "expansion." The entire assignment of effects was then "tested" for the "recession" from 1981:I to 1982:IV. Using the current data shown in Table 1, this "test" still comes out very favorably: it yields little more than the 0.5 percentage point error in the behavior of trend-adjusted leading velocity Friedman originally found over this period.[2] Specifically, from the "peak" in the first quarter of 1981 to the trough in the fourth quarter of 1982, leading velocity declined by 3.0 percentage points. This translates into a trend-adjusted reduction of 8.5 percent, given the previously noted tendency of M1 velocity to rise 3.1 percent a year. The 6-

[1] Perhaps to account for the upward ratcheting of nominal interest rates up to the present decade, *Friedman* treats cyclical effects on the 6-month commercial paper rate asymmetrically. In his figures he assumes that a normal 2 percentage point rise in expansions, but no corresponding interest rate decline in recessions, is included in the cyclical effect allowed for separately. Only the latter is treated symmetrically.

[2] *Friedman* (1983) attributes the small unexplained decline in velocity to "higher instability plus other factors" raising the demand for cash balances.

*Table 1:* **The Record of U.S. Business Cycles and Financial Data Since OPEC I***

| Peak (P) to Trough (T) or Trough to Peak (Month or Quarter) | Length of Period (Months) | Percentage Change from P to T (T to P) Quarter in: Real GNP | M. Velocity ($GNP/M_{-2}$) | D. Velocity (NFD/GNP) | Debt/Money ($NFD/M_{-2}$) | Change in Percentage: Inflation (GNP) Rate | Commercial Paper Rate |
|---|---|---|---|---|---|---|---|
| Nov. 73 P - Mar. 75 T | 16 | –4.3 | 1.7 | 2.4 | 4.2 | 1.0 | –2.4 |
| Mar. 75 T - Jan. 80 P | 58 | 22.4 | 24.8 | –1.0 | 23.6 | –1.7 | 7.7 |
| Jan. 80 P - July 80 T | 6 | –2.3 | –0.3 | 1.8 | 1.5 | 0.5 | –4.6 |
| July 80 T - July 81 P | 12 | 3.3 | 6.2 | –2.9 | 3.1 | 0.1 | 6.6 |
| July 81 P - Nov. 82 T | 16 | –3.2 | –3.5 | 7.4 | 3.7 | –5.8 | –7.4 |
| Nov. 82 T - Nov. 86 "P" | 48 | 17.0 | –9.0 | 23.8 | 12.7 | –3.0 | –3.1 |
| Memorandum | | | | | | | |
| 1975 : I T - 1981 : I "P" | – | 23.4 | 31.6 | –2.8 | 28.0 | 0.3 | 8.0 |
| 1981 : I "P" - 1982 : IV T | – | –3.1 | –3.0 | 8.2 | 4.9 | –7.2 | –5.7 |

* Data status is March 1987. Money, M, is calculated as a quarterly average of seasonally adjusted weekly figures of M1 and lagged two quarters. Deposit components of M1 reported weekly to the authorities by banks are averages of daily figures. Total domestic nonfinancial debt, NFD, also known as total credit market debt owed by domestic nonfinancial sectors in the flow-of-funds accounts, is reported unadjusted at end of quarter containing the month shown, as in *Benjamin Friedman* (1983a). The commercial paper rate, 6 months to maturity, is reported on a bank-discount basis and not seasonally adjusted. Quarterly rates are obtained by averaging the published monthly rates which are averages of business day data. All these data are provided by the Board of Governors of the Federal Reserve System. The inflation rate, calculated from the GNP deflator, and real GNP (in constant 1982 dollars) are obtained from the (U.S.) Bureau of Economic Analysis. The monthly dating of the cycle is from *Eckstein* and *Sinai* (1986), pp. 44 - 45. The first quarterly dating, recognizing a peak "P" in 1981:I rather than 1981:III, is used in *Milton Friedman* (1983).

month commercial paper rate fell 5.7 percentage points over this period, giving a predicted 4.8 percentage point decline in velocity (after multiplying 5.7 by 0.38 and 2.2 as explained before). Together with the 3.0 percentage point decline attributable to the cyclical effect of recession, a 7.8 percentage point fall in trend-adjusted leading velocity is predicted. Since the actual fall was 8.5 percentage points, only a change of 0.7 percentage point remains unexplained by these factors.

To see in what sense an error of this size can be viewed as small, the standard deviation was used as comparator that is obtained after converting leading velocity into a stationary series. Instead of fitting a constant exponential time trend, the conversion was handled more flexibly by using an atheoretical, univariate method that involved constructing a centered moving average with the benefit of hindsight. Specifically, having constructed leading velocity for all quarters from 1972:I through 1986:IV (with M1 data reaching back into 1971), I took the geometric mean of 12 adjoining quarters starting each quarter from 1972:I to 1984:I. The geometric average of two successive mean values was used to obtain a weighted moving average that is centered precisely on each quarter from 1973:III through 1985:II.[3] Dividing leading velocity in these quarters by the corresponding centered moving average values yields a series whose mean is indistinguishable from unity for all practical purposes. Hence, the standard error of that stationary (moving-average adjusted) ratio series, which turns out to be 1.2 percent,[4] can be compared with the "unexplained" error of 0.7 percentage point in trend-adjusted velocity change previously left over after applying *Friedman*'s method to the period 1981:I to 1982:IV. Up to the last date, therefore, trend adjustment alone, no matter how flexible and prescient, may not be doing as well as applying the pattern Friedman calibrated from earlier history, particulary the 1975:I to 1981:I "expansion."

Because all this changed after 1982 or even earlier, it may be useful to ask what, other than past data, supported *Friedman*'s construction of the evidence. Friedman (1953, pp. 28 - 30) has long held with the hypothetico-deductive method. This method ranges over hypotheses, formed with

---

[3] This may be explained by example of the first such average constructed. The geometric average of the 12-quarter means constructed for 1972:I - 1974:IV and 1972:II - 1975:I, centered on 1973:III, involves weighting the outer quarters, 1972:I and 1975:I, half as much as each of the inner quarters in between.

[4] The flexible detrending procedure implies that the standard deviation of the series detrended by division by its centered moving average is comparable ot the coefficient of variation of velocity detrended by a constant exponential factor. Using such a factor, *B. Friedman* (1983b, p. 121) reports a higher coefficient of variation of 0.02 for M1 with quarterly data for 1959 - 80.

DOI https://doi.org/10.3790/ccm.20.4.415 | Generated on 2025-02-02 06:16:09

assumptions and implications that can in part be tested for empirical suitability, until a hypothesis is found that works satisfactorily, for the phenomena it purports to explain. Hence, it is certainly desirable to have past data patterns on the side of the hypothesis currently favored although one would also need a reason for believing that those patterns will continue. In a social discipline there is no equivalent of *Newton*'s First Law so that simply appealing to inertial continuation would be unpersuasive.

*Kenneth Arrow* (1986, pp. S386 - 87) was recently observed that he knows of no serious derivation of the demand for money from a rational optimization. Further, in his view, the loose arguments that substitute for a true derivation would not suggest a very stable relation but rather one that would change quickly with any of the considerable changes in the structure and technology of finance.

Before getting to the changes in velocity, it is useful to take a look at what "theory" there is that bears directly on the various elements of *Friedman*'s (1983) method. There is first the use of leading velocity, GNP divided by M1 two quarters earlier. This lag has no particular theory behind it but is abidingly opportune empirically. Friedman (1969, p. 249) simply observed monetary policy actions that produce a peak in the rate of change in the stock of money on average being followed by a peak in general business some 16 months later and by a peak in the deviation of the money stock from trend 11 months later. Hence if the money stock "11 months later" is associated with economic activity "16 months later" in ratio form, this timing difference of almost two quarters between money and GNP will reduce the deviation. This shift remains useful. For instance, the standard error of the moving-average adjusted M1 velocity series is 1.5 percent without leading, compared with 1.2 percent found for leading velocity before with 48 quarterly observations. In fact, the convenience continues to be rediscovered. For instance, *Phillip Braun* and *Stefan Mittnik* (1985, p. 19) have reported that "money innovations cause GNP initially to decrease through the second quarter and then to rise through the eighth." This shows that difficulties can be avoided by lagging money two quarters in deriving the velocity measure used.

There is slightly more theory, that of competitive supply of financial services, behind multiplying a loan rate by the ratio of high-powered money to money. This is done to obtain the opportunity cost of the latter given that no interest is paid on the former. *Friedman* and *Anna Schwartz* (1982, p. 260) attribute this method to *Benjamin Klein*. Although they called for "a compact way to describe the whole structure of yields – the 'general' level; the 'tilt' of the yield structure to maturity; and the 'difference' between real and nominal yields" – Friedman (1983) settled for the 6-month commercial paper rate. The survey by *John Judd* and *John Scadding* (1982, pp. 1017 - 19) shows several who had used that rate before in empirical specifications of U.S. money demand as an alternative to the Treasury bill rate favored by others.

The remaining building blocks, trend and cycle, again have little explicit theory behind them. Conclusions reported on the subject of velocity trends are mostly empirical. Deviations of the permanent real income elasticity of money demand from unity, and money-economizing innovations may be among the factors contributing to a positive trend. *Friedman* (1969, p. 207) accounts for cyclical effects on velocity mainly with the hypothesis that the demand for money is linked to permanent income, and the lifetime wealth it represents, more closely than to current income. Useful as it is, this hypothesis fails to settle the place of money in actual portfolios, particularly if the value of portfolios in the aggregate tends to fluctuate systematically with the cycle. Hence as a theory of cyclical effects on velocity, the permanent income hypothesis is, at the very least, incomplete. If nominal shocks, i.e., changes in money demand unaccompanied by changes in supply, cause income to change in the first place, the resulting change in velocity explains something about the cycle, not vice versa. Hence, whether or not one would observe the procyclical pattern of velocity change identified by Friedman might very well depend on the source of the disturbances propelling cyclical change. It might also depend on the size of the disturbances. Which way is uncertain because unusually severe recessions could raise precautionary money demand through heightening uncertainty and lowering credit quality as *Bharat Trehan* (1985) has stressed, but the same loss of confidence could also trigger a major downward revision in expected future incomes and hence in money demand.

All considered, a number of hypotheses about factors affecting velocity behavior have been formulated and considered by *Friedman* (1983). However, the size of these effects and the aggregates they are supposed to work on need not be unchanging, and there is no theory that says they should be. The data suggests that the stability of the money demand function which Friedman (1969, p. 155) identified as "the important consideration for monetary theory and policy" has been absent for some years, although it could return. One way of showing this – informally at this stage – is to apply Friedman's (1983) method to the first four years of the expansion that started in 1982: IV. Using the data in Tab. 1 and allowing for the full cyclical effect[5] yields a predicted increase in leading velocity of 11.4 percentage points compared with an actual decline of 9.0 percent. The formula-predicted increase is composed of 13.0 points for cumulative trend, 3.0 points for the cyclical effect of expansion, and −4.3 points for the decline in the cyclically-adjusted commercial paper rate.[6] It stands in direct contrast to the velocity decline of almost equal size that actually occurred. Referring back to the unexplained error of only 0.7 (originally 0.5) percentage point found in the earlier expansion on which Friedman (1983) based his confident prognostications of economic difficulties in 1984 shows an almost 30-fold increase in error for which neither Friedman nor anyone else I know of[7] was at all prepared.

## II. Monitoring Milton Friedman's Regime

Since the end of 1981, velocity has fallen increasingly below earlier trendlines. Thus, it is useful to allow the annualized rate of velocity change to shift after that date. In what follows, the dependent variable, leading velocity rate change, is abbreviated LVRC for money. It is constructed by first dividing successive quarterly data on leading velocity, then taking this quotient to the power four, and finally converting the result to natural logarithms to obtain the annualized rate of velocity change for each quarter from 1972: II through 1986: IV. A dummy variable, D 8286, which is 1 for the 20 quarters starting in 1982: I and ending in 1986: IV, allows for a change in

---

[5] This expansion reached at least into 1987 and can become the second-longest postwar U.S. expansion by lasting beyond September of this year. Although that achievement is entirely possible, using 1986: IV, the last quarter for which data were available at the time of writing, as a hypothetical endpoint of the expansion may not involve any more important timing errors than *Friedman*'s (1983) use of 1981: I, rather than 1981: III, as a provisional peak. As can be inferred from Table 1, the difference in real GNP between 1981: I and 1981: III was small, but the commercial paper rate was 1.7 percentage points higher in the latter quarter.

[6] The adjusted rate decline, which amounted to −(3.1 +2.0) percentage points, was multiplied by 0.38 and 2.2 as explained before to get the interest rate effect of −4.3. The 13 percent trend change together with the 3 percent cyclical and −4.3 percent interest rate effect yield a predicted increase of 11.4 (rather than 11.7) percent in leading velocity because of successive multiplication ($1.13 \times 1.03 \times 0.957 = 1.114$). This predicted increase can be compared with an actual decline of 9.0 percent (Table 1). An almost equally large error is measured if the predicted fall in trend-adjusted leading velocity, of 1.4 percent, is compared with the actual decline of 19.5 percent trend-adjusted.

[7] Since writers critical of the forecasts of others can easily be misunderstood to imply that they knew better, let me hasten to add that I did not. Rather, I wrote in 1984 (about Germany and the rest of the industrial world) that "it may be too late to do much about the return of stagflation in 1985 or, more likely, in 1986 - 87." See *von Furstenberg* (1984), p. 373. Subsequent events have shown that just as it was not too late in 1983, it was not yet too late in 1984.

intercept.[8] The cyclical effect is represented by another dummy variable, DC, which is one in each quarter of expansion, excluding the trough but including the peak quarter, and zero otherwise. The reason for trying this variable was that velocity might grow systematically faster in expansion than contractions, as *Friedman* had indicated. However, no statistically significant cyclical effect was found during the period considered here, so that regression results with DC will not be shown. This leaves the effect of the commercial paper rate change, CPRC, expressed in the same natural units as LVRC rather than in percent.[9] Multiplying CPRC by D8286 to obtain the variable CPRC8286, which is zero prior to 1982:I and the same as CPRC from that quarter on, allows for the coefficient on CPRC, and not just the intercept of the LVRC equation, to change after 1981.[10]

The first column of results in Tab. 2 shows that the intercept for 1972 - 81, 0.033, is very close to, and statistically indistinguishable from, the 3.1 percent annualized trend rate of growth assumed by *Friedman* (1983). However, the coefficient on D8286 of $-0.054$ turns this positive trend into the opposite direction of $-0.021$[11] after 1981, with the new intercept becoming almost negative significant at the 5 percent level with 18 degrees of freedom. For 1972 - 81, the coefficient on the change in the commercial paper rate, CPRC, is almost twice as large as the 2.2 Friedman had assumed. The reason is that cyclical effects, that could not be reliably separated out in the present estimate, are picked up in interest rates which are somewhat cyclical. According to Friedman (1983), if interest rates rise by the 2 percentage points normal for expansions, their influence would be subsumed under the normal cyclical effect of 3 percent on velocity. By contrast, according to the present estimate, there would be a rise of 8.1 percent through the interest rate channel, although an effect as weak as 3 percent could not be excluded statistically at the 5 percent level of significance. After 1981, the coefficient

[8] The *Quandt* test confirmed that a regime break occurred late in 1981 for debt velocity, with a further break indicated in 1984:II. However, a regime break for money velocity was signalled already for 1979:IV, with the log of likelihood function reaching only a local maximum in 1981:IV. Nevertheless, for expositional convenience, we keep year-end 1981 as the breakpoint here, leaving the details of how soon a breakpoint could have been discovered to Section IV.

[9] To make the coefficient on CPRC comparable to *Friedman*'s, CPRC was expressed in natural units (with, say, a 2 percentage point change in the rate written as 0.02) to match the dimension of LVRC. Furthermore, CPRC was multiplied by the ratio of the monetary base (high-powered money) to money which averaged 0.35 over the period covered here compared with Friedman's earlier choice of 0.38.

[10] This procedure is recommended in *Jan Kmenta* (1971), p. 421.

[11] Because of rounding error, the correct value is actually -0.022, as can be seen from equation (4) in Table 1.

*Table 2*

**The Behavior of the Annualized Quarterly Rate of Change of Leading Velocity (LVRC) (Absolute t-values in parentheses)**

| | 1972 - 86 | | 1972 - 81 | 1982 - 86 |
|---|---|---|---|---|
| Equation Number | (1) | (2) | (3) | (4) |
| *Independent Variable* | | | | |
| Constant | 0.033<br>(4.37) | 0.016<br>(2.32) | 0.033<br>(4.29) | –0.022<br>(2.05) |
| D8286 | –0.054<br>(4.08) | – | – | – |
| CPRC | 4.043<br>(2.93) | 4.476<br>(3.24) | 4.043<br>(2.88) | 0.625<br>(0.21) |
| CPRC8286 | –3.417<br>(1.01) | – | – | – |
| *Regression Statistics* | | | | |
| $\bar{R}^2$ | 0.316 | 0.140 | 0.161 | neg. |
| SSE | 0.117 | 0.152 | 0.081 | 0.035 |
| N | 59 | 59 | 39 | 20 |
| D.W. | 1.75 | 1.36 | 1.95 | 1.23 |

on CPRC is no longer significantly different from zero, with the coefficient on CPRC8286 almost as large as that on CPRC, with sign reversed. Hence, through 1981, Friedman's (1983) parameterization is validated most closely for trend and not rejected for other factors explaining the behavior of velocity over the cycle. After 1981, however, the method becomes invalid in every respect.

Another way to show this is to estimate equations for velocity change, LVRC, separately for the entire period and two subperiods: 1971:II - 1981:IV and 1982:I - 1986:IV. One can then test whether the two ($K = 2$) parameters of the velocity function, in this case the intercept and the coefficient on CPRC, changed so as to be significantly different after 1981 than before. With $n = 39$ observations in the first sample, $m = 20$ observations in the second sample, and the corresponding sum of squared errors $SSE_1$ and $SSE_2$, as well as $SSE_c$ for the combined sample the relevant *Chow* test statistic is:

$$F_{K,\,n+m-2K} \sim [(\mathrm{SSE}_c - \mathrm{SSE}_1 - \mathrm{SSE}_2) / K] / [(\mathrm{SSE}_1 + \mathrm{SSE}_2) / (n + m - 2K)]$$

Substituting from columns 2 - 4 of Table 2 yields an $F$-value of 8.6. At the 5 percent significance level with 2 numerator and 55 denominator degrees of freedom, an $F$-value in excess of 3.2 would indicate rejection of the null hypothesis that the parameters of the function do not differ significantly between 1972 - 81 and 1982 - 86. Clearly, therefore, the null is soundly rejected and there was a lasting change in regime, probably around the end of 1981.

While this is easy to prove more than five years later, it is interesting to see how soon after the end of 1981 one could have detected the regime change if one had monitored velocity behavior continuously. One way to approach this is to assume that, as in quality control, monitoring is done in a binary mode: either the old regime is judged to continue with the usual flutters or a new regime has begun and an alarm will go off when that is first recognized. In quality control, this new regime might be precipitated by some internal malfunction, device or system failure. In economics, other not self-reversing developments might be the cause which, unlike in quality control, can and need not be corrected through external intervention. The problem then is to recognize whether and when the switch to a new regime has been made and what are its features.[12] For the purpose of issuing responsible policy advice it is obviously useful to be alert to regime change. Such monitoring, applied in Section IV, can keep one from continuing to base prognostications on historical parameter values, which are unsupported by theoretical necessity in the first place, as soon as the application of standard statistical techniques shows these values to be no longer apt empirically.

## III. Benjamin Friedman to the Rescue?

With the GNP velocity of M1 in obvious difficulty and opportunistic switching of emphasis between different monetary aggregates discredited, perhaps the GNP velocity of debt can be relied upon to a greater degree for control purposes. Starting in 1981, *B. Friedman* suggested an alternative to macroeconomists' almost exclusive reliance on money for exploring the

[12] This formulation of the inference problem is not as restrictive as it might, at first, appear. In particular, it does not imply that the number of regimes encountered over time can not be more than two. Rather two is the maximum to choose from at a point in time. Whenever a new regime has been established, another alternative to the old regime can be considered if that regime is judged to have become irrelevant. Hence, a number of regimes can emerge over time, with tests applied for successive bifurcations, although we will attempt to identify only one such juncture, the first after 1981.

links between economic activity and quantity measures of what is happening in the financial markets. Through the diversification of such measures, additional powers of discrimination might be obtained.

For instance, if firms increase credit demand because they wish to expand capacity and output, and there is some positive interest elasticity of demand deposits and of loans supplied by banks as *Stephen King* (1986, p. 298) has found, then a current rise in money, credit, and interest rates will signal a rise in future output. In this case one would be observing the consequences of a planned outward shift in the "IS" schedule, with both banks and the households increasing their holdings of nonfinancial debt. Interest rates, money supply, and loans supplied by banks could, however, rise just as well because the money demand of households or nonfinancial businesses has increased spontaneously, as *Trehan* (1985) has pointed out. If we continue to assume that actual GNP, as opposed to final sales, is determined by momentum over a short run of, say, one or two quarters, the GNP velocity of money would again decline initially. However, this would not be the case for the velocity of debt as the nonbank sector would be reducing its claims on nonfinancial entities as banks increase theirs. Hence a rise in debt velocity (NFD/GNP) accompanied by a decline in money velocity (GNP/$M1_{-2}$) could be taken as bullish, while the same decline in money velocity without a rise in debt velocity could be bearish for future output. All this requires, of course, that the relation between money and debt (NFD/$M1_{-2}$) be sufficiently variable to allow such discrimination.

For the choice of the financial quantity aggregates to be worth debating, there must be some appreciable difference in the information provided by each. Assuming this to be so, *B. Friedman* found that there were no theoretical reasons for expecting a role for the nonbank public's money holdings but not for its credit liabilities. Rather, the evidence should decide what emphasis should be placed on each. Provided financial quantity aggregates are taken to be under about equally close control of the authorities, there may be numerous contenders for the best fit to economic objectives. Indeed, if "money" is defined simply as the asset that bears the closest relationship to economic activity, as B. Friedman (1983a, p. 162) has characterized the practice, then a credit aggregate may be found that has an even closer fit. Friedman (1983a, pp. 165 - 67) reported:

> Results based on a variety of methodological approaches consistently indicate that the aggregate outstanding indebtedness of all nonfinancial borrowers in the United States bears a close and as stable a relationship to U.S. nonfinancial economic activity as do the more familiar asset aggregates like the money stock (however defined) or the monetary base. Moreover, in contrast to the familiar asset aggregates, among

which there seems little basis for choice from this perspective, total nonfinancial debt appears to be unique in this regard among major liability aggregates.[13]

*Friedman* added (1983a, p. 167) that "the strong stability of the total nonfinancial debt ratio" was shown by the ratio having remained within a few percentage points of its 1960 level (of 143.9 percent) during the entire two decades that followed (the 1980 level was 142.9 percent[14]). In fact, trend stability in (the inverse of) debt velocity (NFD/GNP), like in the leading velocity of money (GNP/$M1_{-2}$) continued through 1981, with the former ratio stationary and the latter smoothly rising up to that time, as evident from Fig. 1. With data reflecting revisions through March 1987, the mean of NFD/GNP was 1.363 for the period 1972:I through 1982:IV with a standard deviation (with 39 degrees of freedom) of only 0.0137, implying a coefficient of variation of 0.010.[15] This is even lower than the coefficient of variation for the leading velocity of money previously derived from detrended data.[16] To avoid fussing over small differences, one can think of both types of

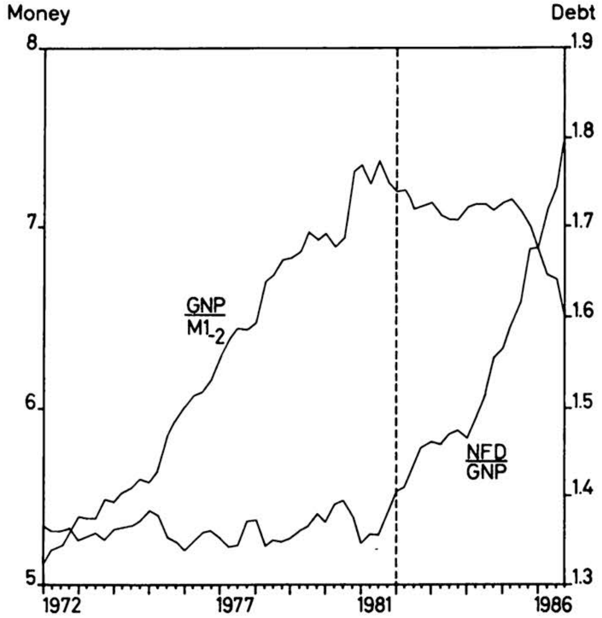
[13] Total nonfinancial debt is the sum of federal, state, and local government debt, household debt, and the debt of nonfinancial business corporations and other nonfinancial business. The concept used by *Friedman* (1983a) has since been called "total credit market debt owed by domestic nonfinancial sectors" since it excludes "foreign credit market debt" held in the United States, which is the debt of foreign financial and nonfinancial entities to U.S. sources only. It thus consists of (a) net Treasury and (nonfinancial) agency issues of all levels of government in the United States owed to holders, foreign and domestic, (b) private domestic debt capital instruments such as tax-exempt obligations and corporate bonds, (c) mortgages, and (d) other domestic debt instruments including bank loans, consumer credit, and commercial paper. Trade debt, loans for the purposes of carrying securities, and funds raised from equity sources are excluded. As a result "leveraged buy-outs" or other means of substituting debt for equity finance increase the relevant debt total.

[14] Given the status of the data as of March 1987, the ratio was 137.8 percent, rather than the 142.9 percent calculated by *B. Friedman* from figures reported about 6 years earlier.

[15] The corresponding coefficient for leading debt velocity, $NFD_{-2}$/GNP, of 0.015 was much higher. Hence debt velocity, as in *B. Friedman* (1983a), is constructed on a nearly simultaneous basis involving end-of-quarter stocks divided by that quarter's GNP flow. *Porter / Offenbacher* (1983) have pointed out that results of vector autoregressions are sensitive to whether quarterly average stocks, as for money and related statistics, or end of quarter stocks, as in the flow of funds statistics, are used in the construction of debt velocity.

[16] For the period 1973:III - 1981:IV, the stationary (moving average adjusted) ratio series with a mean of unity have a coefficient of variation (= standard deviation) of 0.0115 for debt velocity and 0.0129 for leading money velocity, implying quarterly fluctuations in detrended velocity of little more than 1 percent in both cases. Since the raw series of debt velocity appeared quite stationary through 1981, it is "detrended" through division by its centered moving average only to obtain a methodologically precise comparison with money velocity which needs to be detrended.

Figure 1
*The GNP Velocity of Money and Debt over the Period 1972 - 1986*




detrended velocity data as having a coefficient of variation of 1.0 percent for debt and 1.3 percent for money for the decade ending in 1981.

The closer the velocity data once hewed to the indicated path, the farther they ranged from it thereafter. Already before the end of 1983, *Richard Porter* and *Edward Offenbacher* noticed that debt velocity was straying well beyond its past range of variation. By the end of 1986 it had reached 1.803, a figure 32 percent and 32 standard deviations above the 1972 - 81 mean of 1.363. Similarly if the leading velocity of money, which was still on track in the fourth quarter of 1981, had continued to grow by 3.1 percent a year, it would have been 8.45 in the fourth quarter of 1986 when 6.48 was actually recorded. The error is −23 percent, with the actual value 18 standard deviations below the value predicted by means of M. *Friedman*'s trend alone. While consideration of the fall in the 6-month commercial paper rate from 12.9 percent in 1981:IV to 5.7 percent in 1986:IV would reduce the size of the unexplained velocity decline by about one-fourth with M. Friedman's (1983) coefficient estimates, overwhelming evidence of a break in the past pattern around the end of 1981 remains.

The similarity of the experience with the GNP velocity of money and of debt in this regard is evident from the significance of the change in the intercept, allowed for through D8286, in the first equation of Tab. 3. It is equally evident from the *Chow* test that utilizes results from equations (2) through (4) of that table.[17] The format corresponds to that of Tab. 2 because the cyclical dummy variable, DC, was again not significant, while the change in the commercial paper rate was (negative) significant in at least some runs. That rate has also been used by *Porter* and *Offenbacher* (1983) in this context. Since its effect was positive on (the rate of change in) the leading velocity of money, where $M1_{-2}$ appears in the denominator, and negative for debt velocity with NFD in the numerator, its net effect on (the rate of change in) the money velocity of debt, $NFD/M1_{-2}$, was small and insignificant. This is shown in column (5) of Tab. 3.

*Table 3*

**The Behavior of the Annualized Quarterly Rate of Change in the Velocity of Dept in Relation to GNP (NFD/GNP, eqs. 1 - 4) and Money (NFD/M1$_{-2}$, eq. 5) (Absolute t-values in parentheses)**

| | 1972 - 86 | | 1972 - 81 | 1982 - 86 | 1972 - 86 |
|---|---|---|---|---|---|
| Equation Number | (1) | (2) | (3) | (4) | (5) |
| *Independent Variable* | | | | | |
| Constant | 0.003<br>(0.42) | 0.019<br>(3.22) | 0.003<br>(0.45) | 0.050<br>(4.61) | 0.036<br>(5.88) |
| D8286 | 0.047<br>(4.06) | – | – | – | –0.004<br>(0.33) |
| CPRC | –2.156<br>(1.79) | –3.202<br>(2.63) | –2.156<br>(1.92) | –2.854<br>(0.94) | 1.206<br>(1.17) |
| CPRC8286 | –0.698<br>(0.24) | – | – | – | – |
| *Regression Statistics* | | | | | |
| $\bar{R}^2$ | 0.291 | 0.093 | 0.066 | neg. | neg. |
| SSE | 0.089 | 0.118 | 0.052 | 0.037 | 0.080 |
| N | 59 | 59 | 39 | 20 | 59 |
| D.W. | 1.99 | 1.55 | 2.10 | 1.83 | 2.11 |

[17] The F-value is 9.0, well above the critical value of 3.2 given in the previous section.

Also from that equation, specifically its intercept, it can be seen that the trend in the ratio of debt to money two quarters earlier was strongly rising throughout the entire period, at a rate of over 3 percent per annum, with no significant difference between 1972 - 81 and 1982 - 86. In other words, the GNP velocities of money and debt went off track together around the end of 1981, but the relation of money and debt to each other remained about the same as before 1981. Year after year debt rose faster than M1, and when M1 stopped falling in relation to GNP, debt started growing faster than GNP. *B. Friedman*'s (1983a, 1983b, 1983c) "diversification" argument for targeting the quantity of debt as well as money has therefore found no recent support: what the history of money velocity did not help with, the history of debt velocity could not help explain either. The quotation below (1983c, p. 31) contains an expectation no longer fulfilled:

> A credit aggregate, by drawing on the liability side of the economy's balance sheet supplements the information about the economy's asset holding contained in the monetary aggregates, and therefore usefully diversifies the information base underlying the signals that presumably matter for monetary policy.

Instead, money and credit have been emitting similar signals that were wrong for several years running in predicting faster growth of real GNP and inflation than actually occurred.

## IV. Continuous Monitoring

When could one have known with significance of at least 0.05 that velocity regimes have changed? Looked at with the naked eye, the data suggested a break in 1981 or 1982 (see Figure 1). Others, like *Kopcke* (1987), have seen it in the same way mainly on account of the depository institutions deregulation legislation passed in March 1980 for future years. For these reasons, year-end 1981 was chosen before as the common breakpoint in the velocity series stretching to the end of 1986. Assuming, therefore, that the first quarter under the new regime, $t^*$, was 1982: I, the *Chow* tests reported in the previous sections showed significant differences in the two regression equations estimated with data before and after year-end 1981. The *Quandt* tests supported this choice of $t^*$ more closely for the (rate of change in the) velocity of debt than the velocity of money, where a break in regime appears to have occurred two years earlier, approximately coinciding with the adoption of new operating procedures by the Federal Reserve in October 1979.

In this section, the breakpoint will be treated as unknown as we monitor the data series that start in 1972: II from 1979: IV on to determine the earliest quarter by which a regime change could have been observed. The F-test recommended by *John Farley, Melvin Hinich,* and *Timothy McGuire* (1975) is appropriate for this situation because it dominates the *Chow* test when the shift is not close to the middle of the data record but close to its end.

The null of no significant shift in the equations involving only an intercept and the interest rate change, CPRC, (like equations (2) of Tables 2 and 3) could first be rejected for money velocity in 1980:II and for debt velocity in 1981:IV. Tab. 4 shows that the F-values rise continuously and steeply thereafter. Hence unless subsequent revisions to the data contributed greatly to making the F-values so high, continuous monitoring should have provided strong indications of a break. By the end of 1982 at the latest it would have alerted those involved in analyzing or planning economic policy that extrapolating the stock to flow pattern of the 1970s was no longer appropriate.

*Table 4*

**Farley / Hinich / McGuire F-Test Statistics for Regime Break**

| Observations | | | Velocity | | Critical Value[a)] |
|---|---|---|---|---|---|
| From | to | n | Money | Debt | |
| 1972:II | 1980:I | 32 | 1.99 | | 3.35 |
| | 1980:II | 33 | 3.66 | | 3.34 |
| | 1980:IV | 35 | 15.20 | | 3.31 |
| | 1981:III | 38 | | 1.51 | 3.28 |
| | 1981:IV | 39 | 21.00 | 5.16 | 3.27 |
| | 1982:IV | 43 | 20.36 | 11.84 | 3.24 |
| | 1983:IV | 47 | 25.19 | 12.15 | 3.22 |

a) *Farley / Hinich / McGuire* (1975, p. 301) report that for testing the null that no regime change has occurred the test statistic has a central $F_{P,\,n-2P}$ distribution, where $p$ is the number (here 2) of regression parameters that could change, and $n$ is the number of observations. The $F$ values reported are for the significance level of 0.05. For debt velocity, significant $F$-values first appear in 1980 (the 1980 : IV value is 4.23), but these values then again become insignificant before 1981 : IV. For this reason we recognize a "confirmed" new regime governing the rate of change in debt velocity only from that date on.

Such indications were picked up soon after 1982 for NFD/GNP not just by those, like *Porter* and *Offenbacher* (1983) and *R. Hafer* (1984), who may have been averse to working with the debt velocity concept to start with. Even its inventor, *Benjamin Friedman* (1986, p. 437), in a 1984 conference paper published in 1986 that analyzed data through 1982, issued a strong caution:

The paper's main message, therefore, is a warning against accepting too readily – either as a matter of positive economics or for policy purposes – the appearance of simple and eternal verities in much of the existing literature of monetary and financial aspects of business fluctuations.

Monetarists, by comparison, clung to cherished beliefs about simple log-linear relations between money and GNP, and hence their rates of growth, much longer. *Hafer* (1984, p. 24), for instance, while first noting that out-of-sample 1982 - 83 debt velocity behavior "appears as equally at odds with historical patterns ... as does M1 velocity behavior", then finds a special factor, "financial innovation", to make adjusted M1 appear a better predictor of GNP than unadjusted debt. *Milton Friedman* (1984, p. 399), at a conference held in December 1983 whose proceedings were published in 1984, offered another special effect to "explain" the decline in the velocity of money relative to past expectations. He attributed it to the exceptional volatility of monetary growth, which increased the degree of perceived uncertainty and thereby increased the demand for money. Since the volatility of money growth did not continue to grow,[18] M. Friedman's (1984) rationalization for 1981 - 83 is unable to explain why past rates of velocity growth were not resumed in 1984 - 86. As one would expect from decision theory, those with strongly held (or "precise") prior beliefs were most reluctant to revise their estimation procedures so as to avoid the long series of positively correlated errors that is inevitable with reluctant learning if a regime change has, in fact, occurred.

## V. What Is to be Done?

The extreme reductionism that supported faith in the predictability, indeed near-constancy, of the trend in ratios of financial stocks to current and prospective economic flows has come close to wit's end. Updates on velocity behavior, such as that by *Robert Rasche* (1987), continue to appear to keep up with the "shift in the drift" of velocity generally identified as beginning in late 1981. Parameters are reestimated to allow for increased interest elasticity after 1981 or anything in addition to falling interest rates that can

[18] Indirect evidence on this point comes from analyzing the volatility of the leading velocity of money detrended by division by its centered moving average, as explained in Section I. Calculating the rolling standard deviations (coefficients of variation) of 12 adjoining values of this stationary series (identified by the earliest quarter included in each) shows a pattern that is first rising and then declining. Specifically, the standard deviation more than doubled during the second half of the 1970s, reaching a value of 0.017 (1.7 percent) in 1979:IV (covering the period 1979:IV - 1982:IV). It then declined to a low of 0.006 in 1982:I (covering the period 1982:I - 1985:I). Changes in the volatility of "detrended" debt velocity are quite similar to those of money velocity over this period.

explain why the GNP velocity of M1 did not rise as it used to.[19] Others, including *Trehan* (1985), attempt to resolve velocity puzzles by defining them away, for instance, by lobbing the fastest-growing component, government debt, off the total domestic nonfinancial debt aggregate that is related to GNP.[20]

Quite obviously, therefore, in recent years, new rhymes could be made of the data ex post, but there was no convincing reason beforehand. Since prognostications based on past patterns regularly proved wrong, should we go on extrapolating recent developments as if they constituted a new pattern? Or should we fear worse even than *Goodhart*'s Law that any strong empirical relationships between money or debt on the one hand, and real GNP and inflation on the other, vanish as soon as they appear as a settled feature of the data?

I share the skepticism expressed in the last question more than the attitude of "carry on regardless". *Richard Kopcke* (1987) has taken a similar conviction to the point of rejecting monetary targeting entirely. He argued against those who believe that such targets may be a useful "governor" for monetary policy by calling the analogy false because this regulatory device is no more effective than the relationships on which it rests. At a higher level, multiple equilibria and strange attractors, perceived by *Peter Diamond* (1987) and *William Barnett* and *Ping Chen* (1986), respectively, may require active steering by the monetary authorities in an environment of uncertainty to avoid gravitating toward undesirable or chaotic (non-predictive) outcomes or regimes.

However, there is a cost to discretion and to giving the appearance of lacking policy principles that can be demonstrated by means of announced operating targets. *Hermann-Josef Dudler* (1986, p. 485) has drawn attention to this. He pointed out correctly that the return to trial and error, unchecked

[19] *Richard Kopcke* (1987) has recognized the superficial attraction of exploiting the interest rate channel for explaining away the M1 velocity puzzle. Since the complete deregulation of financial yields in the United States that did not begin in earnest until 1981, M1 has increased rapidly, with money velocity tending to fall, rather than rise year after year as before. This coincidence, *Kopcke* (1987, p. 28) continues, suggests that deregulation has been responsible for the surge in M1 and that, because of deregulation, the demand for M1 has become more responsive to short-term interest rates. However, he rejects this suggestion because the relation between monetary assets and total liquid assets, just like the relation between monetary assets and total domestic nonfinancial debt touched on in Section III of this paper, did *not* change after 1981.

[20] Federal Reserve Board Chairman *Paul Volcker*'s February 1987 testimony that the Fed has (once again) dropped M1, watching M2 and M3 instead, probably belongs into the same category of evasion. His testimony was reprinted in the April 1987 issue of the Federal Reserve Bulletin.

by preannounced money growth targets, lays the monetary authorities open to political pressures from all sides. He mentions, in particular, the low-interest lobby in the United States that has yet to mind the counterproductive long-run consequences of faster money growth. Hence even if adhering to smooth and inflexible monetary targeting may be dominated by other policies in wonderlands where the monetary authorities share unquestioned power with the "benevolent dictator", targeting may not be dominated in the real world in which monetary authorities may be put upon. Of course, there is some irony in defending monetary targeting as a means of protecting the independence of the monetary authorities, affording them the political freedom to exercise their second best political judgment when attempting to apply their best judgment would deprive them of that freedom. Nevertheless, continued investments in myth and practice of targeting, provided they are politically effective in generating credibility and respect, may be entirely justifiable even when close predictability of velocities of money and debt has slipped away.

What, if anything, should be targeted? *Joshua Aizenman* and *Jacob Frenkel* (1986) have shown succinctly how nominal GNP, CPI, interest-rate, and preannounced money-growth targeting compare with the money-supply rule that is optimal in their small model with nominal and real shocks. *Bennett McCallum* (1985) has emphasized that all of these, except inflation reflected in the CPI, are intermediate targets and *Manfred Neumann* (1986) has noted interesting open-economy considerations in the authorities' reaction function. In general, which of the targeting alternatives comes closest to avoiding as much welfare loss as the optimal discretionary rule depends on the uncertain value of parameters and the variance of the shocks.

There is, however, an additional, political consideration for choosing what to target that has been raised, for instance, by *Paul Atkinson* and *Jean-Claude Chouraqui* (1986, p. 29). They argue that by formulating their policies in terms of an intermediate (quantity) target, central banks can increase the likelihood that they will be held responsible for developments they are in a strong position to control and not for developments they are either poorly placed to influence or cannot responsibly do anything to change. Central banks should then concentrate on identifying rates of money growth which they believe to be non-inflationary and draw public attention to those rates. While the ability to pinpoint rate levels deserving of such belief, and hence the strength of convictions about them, have declined, there are quite a few "solutions" offered in the domestic political markt,[21] or pushed by for-

[21] Thus *Marc Miles* (1984, pp. 234 - 42) has offered a "supply-side" proposal requiring the Fed to peg both the interest rate on long-term bonds by increasing the money

eign governments, that have exceedingly low probability of being found non-inflationary in the end. Strengthening the hand of the second best against those "solutions" would not be bad. Thus we need not join *Robert Gordon*'s (1986, p. 9) celebration of the drowning of monetarism in "the waves of velocity instability ... crashing so hard", even if we acknowledge that just basing monetary targets on a forecast of the trend rate of growth of the income velocity of money is no longer very helpful, or indeed practical advice. At least in the United States, that trend rate has become inconstant and quite uncertain.

## Literature

*Aizenman / Frenkel* (1986): "Targeting Rules for Monetary Policy", Economics Letters 21 1986. – *Arrow,* K. (1986): "Rationality of Self and Others in an Economic System", Journal of Business 59 1986. – *Atkinson / Chouraqui* (1986): "The Formulation of Monetary Policy: A Reassessment of Recent Experience", OECD Department of Economics and Statistics Working Papers No. 32 March 1986. – *Barnett / Chen* (1986): "The Aggregation-Theoretic Monetary Aggregates are Chaotic and Have Strange Attractors: An Econometric Application of Mathematical Chaos", Center for Economic Research Discussion Paper No. 86-5, University of Texas, Austin 1986. – *Braun / Mittnik* (1985): "Structural Analysis with Vector Autoregressive Models: Some Experimental Evidence", Washington University Department of Economics Working Paper No. 84, St. Louis 1985. – *Diamond,* P. (1987): "Multiple Equilibria in Models of Credit", American Economic Review 77 May 1987. – *Dudler,* H.-J. (1986): „Geldmengenpolitik und Finanzinnovationen“, Kredit and Kapital 19 1986. – *Eckstein / Sinai* (1986): "The Mechanisms of the Business Cycle in the Postwar Era," in R. *Gordon* (Hrsg.): "The American Business Cycle: Continuity and Change", Chicago 1986. – *Farley / Hinich / McGuire* (1975): "Some Comparisons of Tests for a Shift in the Slopes of a Multivariate Linear Time Series Model", Journal of Econometrics 3 1975. – *Friedman,* B. (1983a): "The Roles of Money and Credit in Macroeconomic Analysis", in J. *Tobin* (Hrsg.): "Macroeconomics, Prices and Quantities", Washington 1983. – *Friedman,* B. (1983b): "Monetary Policy with a Credit Aggregate Target", in K. *Brunner* and A. H. *Meltzer* (Hrsg.): "Money, Monetary Policy, and Financial Institutions", Carnegie-Rochester Conference Series on Public Policy 18 1983. – *Friedman,* B. (1983c): "Money, Credit, and Federal Reserve Policy: Reply to Porter and Offenbacher", Federal Reserve Bank of Richmond Economic Review 69 1983. – *Friedman,* B. (1986): "Money, Credit, and Interest Rates in the Business Cycle", in R. *Gordon* (Hrsg.): "The American Business Cycle: Continuity and Change", Chicago 1986. – *Friedman,* M. (1953): "Essays in Positive Economics", Chicago 1953. – *Friedman,* M. (1969): "The Optimum Quantity of Money and Other Essays", Chicago 1969. – *Friedman,* M. (1983): "Why a Surge of Inflation is Likely Next Year", Wall Street Journal, 1. September 1983. – *Friedman,* M. (1984): "Lessons from the 1979 - 82 Monetary Policy Experiment", American Economic Review 74 May 1984. – *Friedman /*

supply whenever that rate has moved above its "appropriate" target level of "say, 3.5 or 4 percent" and the future price of gold or other commodities one year ahead ("or two years, or both").

*Schwartz* (1982): "Monetary Trends in the United States and the United Kingdom", Chicago 1982. – *Gordon,* R. (1986): "What Future for Monetarism?" Comments prepared for Allied Social Science Associations Meetings panel discussion session, New Orleans 28 December 1986. – *Hafer,* R. (1984): "Money, Debt and Economic Activity", Federal Reserve Bank of St. Louis Review 66 1984. – *Judd / Scadding* (1982): "The Search for a Stable Money Demand Function", Journal of Economic Literature 20 1982. – *King,* S. (1986): "Monetary Transmission: Through Bank Loans or Bank Liabilities?" Journal of Money, Credit and Banking 18 1986. – *Kmenta,* J. (1971): "Elements of Econometrics", New York 1971. – *Kopcke,* R. (1987): "Financial Assets, Interest Rates, and Money Growth", New England Economic Review March / April 1987. – *McCallum,* B. (1985): "On Consequences and Criticisms of Monetary Targeting", Journal of Money, Credit and Banking 17 1985. – *Miles,* M. (1984): "Beyond Monetarism: Finding the Road to Stable Money", New York 1984. – *Neumann,* M. (1986): "Intervention in the Mark/Dollar Market: The Authorities' Reaction Function", in *Neumann* (Hrsg.): "Monetary Policy and Uncertainty", Baden-Baden 1986. – *Porter / Offenbacher* (1983): "Empirical Comparisons of Credit and Monetary Aggregates Using Vector Autoregressive Methods", Federal Reserve Bank of Richmond Economic Review 69 1983. – *Rasche,* R. (1987): "An Update on Velocity Behavior", Shadow Open Market Committee Policy Statements and Position Papers No. 87-01, Center for Research in Government Policy and Business, University of Rochester, Rochester March 1987. – *Trehan,* B. (1985): "The Information Content of Credit Aggregates", Federal Reserve Bank of San Francisco Economic Review Spring 1985. – *von Furstenberg,* G. (1984): "A Step Ahead, (A Review of the) 1983/84 Report of the German Council of Economic Exports", Zeitschrift für die gesamte Staatswissenschaft 140 1984. –

## Zusammenfassung

### Bestands-Strom-Beziehungen von Geld und Verschuldung: Was kann aus dem Zusammenbruch vergangener empirischer Beziehungen in den Vereinigten Staaten gelernt werden?

Wegen der in der Vergangenheit offenbar stabilen Umlaufgeschwindigkeitsrelationen machten namhafte Wirtschaftswissenschaftler zu Beginn dieses Jahrzehnts starke Aussagen über die Kontrollierbarkeit des nominalen Bruttosozialprodukts (BSP) mit Hilfe von Finanzaktiva-Beständen. Erst 1983 popularisierte *Milton Friedman* die Funktion, die das Verhalten der Umlaufgeschwindigkeit von M 1 über die Phasen des Konjunkturzyklus zu erklären schien (wobei M 1 zwei Quartale früher als das BSP gemessen wurde). Er benutzte diese Funktion, um sich mit großem Selbstvertrauen über die Politik und die Aussichten aufzuregen. Etwa zur selben Zeit begann *Benjamin Friedman,* ein Aggregat von Verbindlichkeiten vorzustellen, das definiert war als gesamte Kreditmarktverschuldung aller heimischen (privaten und staatlichen) nichtfinanziellen Sektoren. Er zeigte, daß dieses Schuldenkonzept mindestens in so enger Relation zum BSP stand wie die Geldmenge, die üblicherweise in Umlaufgeschwindigkeitsbeziehungen berücksichtigt wird. Tatsächlich hatte das Verhältnis zwischen Schulden und BSP während früherer Jahrzehnte nahezu keinen Trend aufgewiesen und zeigte nur geringe Schwankungen. Diese sowie weitere Vorzüge überzeugten

Benjamin Friedman, die Gesamtverschuldung als zusätzliches Zwischenziel zur Geldmenge zu empfehlen.

Während sich Geldmenge und Verschuldung so eng zusammen entwickelt haben wie schon in früheren Jahrzehnten, hat sich demgegenüber alles andere verändert. Ende 1986 wichen die BSP-Geschwindigkeiten der Geldmenge und der Verschuldung um 18 bis 32 Standardabweichungen von dem Pfad ab, den die beiden *Friedmans* extrapoliert hätten, wenn sie von den Relationen der Jahre 1972 - 1982 ausgegangen wären, auf die man sich noch vor wenigen Jahren verlassen hatte. Dieser Artikel versucht zu bestimmen, was denn eigentlich das Vertrauen in die Gültigkeit dieser Bestands-Strom-Beziehungen für Prognosen verursacht hat, wenn man von dem „empirischen" Argument absieht, daß „sofern eine Regel (oder Verhaltensweise) in der Vergangenheit immer gestimmt hat, es sicherlich vernünftig ist, anzunehmen, daß dies auch in der Zukunft so sein wird."

## Summary

### Stock/Flow Ratios With Money and Debt: What Can Be Learned From the Breakup of Past Relationships in the United States?

Because of the apparent stability of velocity relations in the past, noted economists made strong statements earlier this decade about the controllability of nominal GNP by means of financial stock aggregates. As late as 1983, *Milton Friedman* popularized the function that seemed to explain M1 velocity behavior (with M1 taken from 2 quarters earlier than GNP) over the phases of the business cycle. He used this function to expostulate on prospects and policies with great assurance. At about the same time, *Benjamin Friedman* began to promote a liability aggregate, defined as the total credit market debt of all (private and governmental) domestic nonfinancial sectors. He showed that this concept of debt had been at least as closely related to GNP as the monetary aggregate normally used in velocity relations. In fact, the ratio between debt and GNP had been very nearly trendless over previous decades, showing only very small variations. This and other virtues persuaded Benjamin Friedman to recommend the debt total as an intermediate target in addition to money.

While money and debt have moved about as closely together as in earlier decades, everything else changed since. By the end of 1986, the GNP velocities of money and debt had moved between 18 and 32 standard deviations away from the path the different *Friedmans* might have extrapolated on the basis of (1972 - 82) relations relied upon only a few years earlier. The paper seeks to determine what, beyond the "empirical" argument that "if a rule (or pattern) has always been true in the past it is surely reasonable to suppose that it will continue to hold in the future," had caused confidence in the predictive significance of these stock-flow relations in the first place. It then analyzes how soon one could have discovered through continuous monitoring of the data that this confidence was misplaced. The remaining question is what to do about targeting once close predictability of the velocities has slipped away.

## Résumé

### Rapport fonds/flux de la monnaie et de la dette. Quelle leçon peut-on tirer de la rupture des relations passées aux Etats-Unis?

Vu la stabilité apparente des relations de vitesse dans le passé, des économistes renommés firent au début de cette décennie des rapports concluants sur la contrôlabilité du PNB par l'ensemble des fonds financiers (financial stock aggregate). En 1983, *Milton Friedman* popularisa la fonction qui paraissait expliquer le comportement de vitesse de M1 (en prenant le M1 de deux trimestres avant le PNB) pendant les phases du cycle économique. Il utilisa cette fonction pour expostuler des prévisions et des politiques, et ceci avec une grande assurance. A peu près au même moment, *Benjamin Friedman* commença à avancer un ensemble d'obligations (liability aggregate), défini comme la dette totale du marché du crédit de tous les secteurs intérieurs non-financiers, privés comme gouvernementaux. Il montra que ce concept de dette se rapporte du moins tout autant au PNB que l'ensemble monétaire, générallement utilisé dans les relations de vitesse. En réalité, le rapport entre dette et PNB n'a que très faiblement varié au cours des précédentes décennies. C'est ce qui a entre autres persuadé Benjamin Friedman qu'il fallait considérer la dette totale comme objectif intermédiaire, en plus de la monnaie.

Alors que les mouvements de la monnaie et de la dette étaient fort liés l'un à l'autre au cours des décennies précédentes, tout le reste a changé depuis lors. Vers la fin de 1986, les vitesses du PNB de la monnaie et de la dette avaient atteint entre 18 et 32 déviations standards par rapport à ce que les deux *Friedman* auraient pu extrapoler sur la base des relations de 1972 - 82. L'auteur de cet article cherche à déterminer ce qui explique la confiance en la plausibilité des relations fonds/flux en premier lieu. Il tient compte pour ce faire de l'argument «empirique» suivant: «si une règle (ou un modèle) s'est toujours avérée vraie dans le passé, il est certainement raisonnable de supposer qu'elle continuera à l'être dans le futur». L'auteur analyse ensuite à quel moment on s'est rendu compte que cette confiance n'était pas justifiée. La question qui reste encore à poser est de savoir quel objectif prendre, depuis qu'il n'est plus possible de prévoir les vitesses exactes.