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Temporarily suspending indexation of the personal income-tax code is often suggested as a means for raising federal revenues. Here, the authors argue that this method of taxation is inefficient in that it is inferior to direct increases in marginal tax rates. They conclude that attempts to use bracket creep in future deficit-reduction efforts should be viewed with appropriate skepticism.

The standard measure of the labor input in aggregate production is the sum of employment hours over all individuals. The validity of this measure for cyclical purposes requires that the composition of the work force by skill and ability remain approximately unchanged over the cycle. Here, the authors investigate the accuracy of aggregate hours as a cyclical measure of the labor force and find that it is much more cyclically volatile than the labor input. Using data for almost 5,000 men and women in the Panel Study of Income Dynamics, they conclude that the labor input's real wage is strongly procyclical, but that average compensation per hour is not.

This study examines the short-run dynamic relationships between nonborrowed reserves, the federal funds rate, and transaction accounts using daily data from 1979 through 1982. Separate models are estimated for each day of the week, and simulation experiments are performed. The results suggest that the funds rate responded quite rapidly to a change in nonborrowed reserves, but that the short-run nonborrowed reserves multiplier for transaction accounts was only about 18 percent of its theoretical maximum. In addition, the Federal Reserve appeared to accommodate about 65 percent of a permanent shock to money, and lagged reserve requirements seemed to delay depository institutions' response to a money shock.

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Using Bracket Creep to Raise Revenue: A Bad Idea Whose Time Has Passed

by David Altig and
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Introduction

The Clinton administration, seconded by a majority in Congress as well as many economic experts, has made the adoption of new revenue sources a central element of its deficit reduction efforts. According to Congressional Budget Office (CBO) projections, over fiscal years (FY) 1994 to 1998, the federal government would borrow about $355 billion less under the Clinton budget proposals than if it merely continued with the tax and spending programs in place, or planned, at the beginning of FY1993. Almost 75 percent of this difference is accounted for by new federal receipts.[1]

However, even if the Clinton budgets unfold as envisioned, the federal deficit relative to gross domestic product (GDP) in FY1998 will differ little from the level realized in FY1989, the year just prior to the confluence of a protracted economic slowdown and the significant, but unusual, outlays associated with the savings and loan crisis. Worse yet, virtually all forecasts suggest that after FY1998, deficits will again begin to climb dramatically. Given these circumstances—and the probability that some of the tax changes currently on the table will be scaled back or rejected—it is likely that many of the revenue alternatives the administration has opted against will find their way back into policy deliberations in the near future.

One of the alternatives reportedly considered in the early stages of the Clinton team's budget deliberations was the suspension of adjustments to income-tax rate brackets that automatically occur when the Consumer Price Index (CPI) rises. In fact, this alternative has been periodically discussed ever since inflation indexation was introduced by the Economic Recovery Tax Act of 1981 (ERTA).[2]

The fact that monetary policy can influence government revenues is at the heart of traditional concerns about central-bank independence. Indeed, recognizing the temptation for governments to compromise long-run price stability in the service of short-run fiscal pressures is the key to understanding the history and

■ 1 See Altig and Gokhale (1993) for a detailed explanation of these estimates.

■ 2 See Tatom (1985) and Altig and Carlstrom (1991a) for more-detailed descriptions of these provisions.

evolution of centralized monetary institutions.[3] For those who, like us, believe that great skepticism should accompany any policy that introduces an inflationary bias into the economic environment, making government receipts positively related to the inflation rate is sufficient reason to be wary of abandoning indexation.

Quite apart from these considerations, however, is the simpler issue of efficiency. Suspension of inflation indexing raises revenues by permanently increasing the income base to which tax rates are applied. A straightforward alternative would be to continue adjusting the tax base for price-level changes while simultaneously increasing the applicable rates. The former approach is preferable to the latter only if the economic costs of allowing inflation to expand the tax base are less than the costs of increasing tax rates to levels sufficient to raise an equivalent amount of revenue.

In this article, we formally address this issue, asking whether, in the long run, the utility of an average consumer is higher when a given amount of revenue is raised by temporarily abandoning inflation indexation, as opposed to adopting a comparable, but explicit, change in the rate structure. Our analysis employs the well-known quantitative framework pioneered by Alan Auerbach and Laurence Kotlikoff (an extensive discussion of which can be found in their 1987 book *Dynamic Fiscal Policy*), and a rate structure similar to the one found in the current U.S. federal tax code. In all of the cases we consider, direct changes in tax rates are superior to the strategy of raising revenue by forgoing inflation adjustments.

Although a rising price level can affect tax liabilities in many ways, the channel relevant for our discussion is associated with bracket creep, or the tendency for taxpayers to be pushed into higher rate brackets as a result of inflation-induced increases in nominal income. In section I, we briefly review the specifics of indexation in the U.S. tax code, emphasizing the bracket-creep issue and its relation to another important effect of inflation, capital-income mismeasurement. Section II then illustrates the effect of suspending inflation indexation and contrasts this approach with one involving structural changes in the tax code. In section III, we lay out the basic model from which we calculate the welfare costs of bracket creep as a revenue source. The balance of the article contains our results.

I. What Does Indexation Really Index?

Indexation of the personal tax code formally commenced in 1985 under provisions of ERTA. Ad hoc indexation, in the form of infrequent adjustments of nominal tax brackets, personal exemption levels, and so on, was periodically legislated prior to 1985, but ERTA represented the first time that regular, ongoing inflation adjustments were codified in the tax laws.

Under ERTA, indexation required annual adjustments in the dollar value of tax-bracket limits and personal exemption levels based on a cost-of-living index derived from the Bureau of Labor Statistics' CPI for urban wage earners (CPIU). The indexing provisions of ERTA were in effect for only two years before being superseded by the Tax Reform Act of 1986 (TRA86). However, TRA86 extended ERTA's indexing scheme with only minor modifications.

The particulars of ERTA and TRA86 are such that inflation adjustments are made with a lag of approximately one year. For example, to implement inflation adjustments for tax year 1986, a cost-of-living index was calculated by dividing the average CPIU for 1985 by the average for 1984. The adjusted tax-bracket limits and personal exemption levels were then obtained by multiplying those in effect for the 1984 tax year by the resulting cost-of-living index. Thus, the procedure effectively adjusts the tax code in a given year using realized rates of inflation through the prior year.[4]

Because inflation adjustments are not contemporaneous, the accumulated effects of bracket creep might not be zero in any given year. However, if indexation is otherwise perfect, this is not an issue in the long run, which is the focus of our analysis. To clarify, suppose that real income is constant and equal to y, and that nominal income in year t grows by $1 + \pi_t$, where π_t is the annual rate of inflation. Ignoring exemptions, deductions, and other adjustments to gross income, the ERTA and TRA86 indexation schemes can be thought of as procedures that effectively deflate nominal income in each year by one plus the rate of inflation

3 A more complete articulation of this position is contained in the Federal Reserve Bank of Cleveland's 1990 *Annual Report*.

4 This statement is not precisely correct, since ERTA provided formulas for annual cost-of-living indexes that used October through September data.

BOX 1

The Effects of Bracket Creep: A Simple Hypothetical Example

Let the marginal tax-rate schedule be given by

Marginal Tax Rate (Percent)	Tax Bracket (Dollars)
0	0 – 1,000
25	> 1,000

If an individual has a constant real income of $1,000, the annual inflation rate is 3 percent, and indexation is suspended for two years, then the sequence of taxable income levels, marginal tax rates, and average tax rates is given by

Time	Nominal Income (Dollars)	Real Income (Dollars)	Nominal Tax-Bracket Limit (Dollars)	Marginal Tax Rate (Percent)	Average Tax Rate (Percent)
0	1,000	1,000	1,000	0	0
1	1,030	1,000	1,000	25	0.7
2	1,061	1,000	1,000	25	1.4
3	1,093	1,000	1,030	25	1.4
4	1,126	1,000	1,061	25	1.4
5	1,159	1,000	1,093	25	1.4
6	1,194	1,000	1,126	25	1.4
•	•	•	•	•	•
•	•	•	•	•	•

SOURCE: Authors' calculations.

for the *previous year*.[5] Thus, taxable income in year t is given by

$$y^* = y \prod_{j=1}^{t} \frac{(1+\pi_j)}{(1+\pi_{j-1})},$$

where we have designated the year in which indexing commences as time period 1. Because the long run, or steady state, is characterized by the condition $\pi_j = \pi_{j-1}$ (for all j), it is clear from the above expression that long-run taxable income just equals real income y if timing lags are the only flaws in the adjustment provisions.

Unfortunately, timing lags are not the only flaw; problems with our current indexation methods would exist even if all adjustments were contemporaneous. To see this, note that nominal income in year t, relative to year $t-1$ (which for simplicity we will henceforth assume is the base year), is given by

$$y_t = w_t(1+\pi_t) + R_t A_{t-1},$$

where w is real wage income, A is the household stock of assets (from the previous period), and R is the nominal rate of return on these assets. Real income, on the other hand, is given by

$$y_t = w_t + A_{t-1}(R_t - \pi_t)/(1+\pi_t).$$

Although deflating nominal income by $1+\pi_t$ is fine for obtaining real wage income, this adjustment is not appropriate for capital income. Specifically, dividing nominal asset income $(R_t \cdot A_{t-1})$ by one plus the inflation rate would result in an overstatement of capital income by an amount equal to $\pi_t A_{t-1}/(1+\pi_t)$.

This capital-income mismeasurement problem is logically distinct from the bracket-creep problem per se: Although distortions from bracket creep would vanish under a flat-tax regime, distortions from capital-income mismeasurement would remain. Furthermore, as shown, indexation as currently implemented does not address the problem of overstating real capital income in inflationary environments.

On the other hand, because capital-income mismeasurement does result in an overstatement of real income, it contributes to bracket-creep effects. In what follows, we provide calculations that examine the effects of suspending indexation with and without the capital-income mismeasurement problem.

II. Raising Revenue with Bracket Creep: A Simple Example

Bracket creep effectively raises the income tax base by an amount equal to the inflation rate realized for the period over which indexation is suspended. Although this point is fairly obvious, we provide a simple example to make the discussion a bit more concrete.

Suppose that the marginal tax-rate schedule is as described in box 1, that a representative taxpayer has a constant real income of $1,000, and that the price level increases by 3 percent every year. Treating time 0 as the base year, assume that indexation is forgone in years 1 and

5 Formally, the law requires that the income limits to which particular rates apply in year t be inflated by the factor P_{t-1}/P_b, where P is the appropriately defined CPIU and b refers to the base year used in the adjustment. However, because P_{t-1}/P_b just equals $\Pi_{j=b+1}^{t-1}(1+\pi_j)$, the indexing procedures are equivalent to holding the rate limits constant and adjusting nominal income as described.

2. As shown in the box, in the long run (after period 2) this policy causes nominal income to exceed the 0-percent rate bracket by about 6 percent in every period. As a consequence, the marginal tax rate faced by our average taxpayer is higher from time 1 onward, even though real income is unchanged.

Reflecting the simple two-bracket rate structure proposed in this example, temporarily shelving inflation adjustments for two years (or longer) has exactly the same effect on *marginal* tax rates as would a one-year suspension: In both cases, the marginal tax rate rises from 0 to 25 percent. However, as seen in the last column of the second table in box 1, the *average* tax rate increases as long as indexation is suspended. This reflects the fact that inflation expands the amount of income subject to the 25 percent rate, even when the marginal rate itself does not change.[6]

III. A Quantitative Framework

In subsequent sections, we quantitatively compare the long-run effects of raising revenue through bracket creep with those that arise from raising the same amount of revenue by proportionately increasing statutory marginal tax rates. The analysis uses a general-equilibrium overlapping-generations model, similar to that of Auerbach and Kotlikoff (1987), in which individuals face a tax-rate schedule and indexing scheme much like those legislated by TRA86. In this section, we outline the model's structure and discuss its parameterization. More-detailed discussions of similar frameworks can be found in Auerbach and Kotlikoff and in Altig and Carlstrom (1991b, 1992).

Preferences and the Budget Constraint

Assuming that productive life starts at age 1, a representative member of each generation in the model's steady state maximizes a time-separable utility function of the form

$$(1) \quad U = \sum_{s=1}^{55} \beta^{s-1} \left(\frac{c_s^{1-\sigma_c}}{1-\sigma_c} + \alpha \frac{l_s^{1-\sigma_l}}{1-\sigma_l} \right)$$

subject to

$$(2) \quad A_s = 1 + r_s(1-\tau_s) A_{s-1} + \varepsilon_s \omega (1-\tau_s)(1-l_s) - c_s + T_s,$$

where c_s is real consumption expenditure at age s, l_s is leisure (where the total time endowment has been normalized to one), r is the pre-tax real interest rate $(R-\pi)/(1+\pi)$, ω is the pre-tax real market wage, ε_s is an exogenous human capital productivity endowment, τ_s is the individual's marginal tax rate, and T_s is a lump-sum transfer payment equaling the individual's total tax payment.[7] The parameters σ_c and σ_l represent, respectively, the inverse of the intertemporal elasticities of substitution in consumption and in leisure. The parameter β is the subjective time-discount factor, given by $1/(1+\rho)$, where ρ is the rate of time preference.

Capital and the Production Technology

The aggregate production technology is of the standard Cobb–Douglas form

$$(3) \quad Y = Ak^{\theta},$$

where Y is aggregate output per unit of labor, A is an arbitrary scale variable, k is the aggregate capital–labor ratio, and θ is capital's share of production. The steady-state value of the capital stock satisfies

$$(4) \quad k = \frac{Y-C}{n+\delta},$$

where C is aggregate consumption per labor unit, n is the rate of population growth, and δ is the rate of depreciation on physical capital.[8]

■ 6 Simulations in this paper consider the effect of bracket creep only on marginal tax rates. Because tax revenues are returned to agents in a lump sum, increases in tax revenues that are independent of marginal tax-rate hikes have no effect in equilibrium.

■ 7 The basis for the human capital productivity profile is the labor efficiency estimates reported by Hansen (1986). We transformed Hansen's discrete function into a continuous function by linear extrapolation. Because we will be focusing exclusively on steady states, we have dropped time subscripts for expositional convenience.

■ 8 Equation (4) is obtained from the goods-market-clearing condition

$$Y_t = C_t + (1+n)k_{t+1} - (1-\delta)k_t$$

and the requirement that $k_{t+1} = k_t$ in a steady state.

TABLE 1

Benchmark Parameters

Parameter	Description	Value
$\frac{1}{\sigma_c}$	Intertemporal elasticity of substitution in consumption	1.000
$\frac{1}{\sigma_l}$	Intertemporal elasticity of substitution in leisure	0.200
ρ	Subjective rate of time preference	0.010
α	Utility weight of leisure	0.500
n	Population growth rate	0.013
θ	Capital share of output	0.360
δ	Capital depreciation rate	0.100

SOURCE: Authors.

Model Calibration

As a benchmark, the parameters in equations (1) through (4) are set at the values shown in table 1. Given the tax code described below and interpreting a time period as one year, these parameters yield a steady-state capital/output ratio of 2.59, compared to 2.68 for the U.S. economy over the post–World War II period.[9] With respect to the labor supply, our benchmark parameterization implies that, on average, individuals spend about 28 percent of their total time endowment in market-wage-generating activities. How this matches the actual data depends on the total hours that individuals have available for leisure. If we assume that an average of six hours per day is required for sleeping, over the postwar period U.S. workers have devoted approximately 31 percent of their available time to market-labor activities.[10]

The Benchmark Tax Code

Throughout the remainder of this paper, we focus specifically on the pure distortionary effects of the different tax regimes considered. Accordingly, we assume that all revenues raised through income taxes are rebated to the affected cohort via lump-sum transfers, so that both income-tax payments and lump-sum transfers are given by

$$(5) \qquad T_s = \begin{cases} \tau^L y^* & \text{for } y^* \leq \bar{y} \\ \tau^L \bar{y} + \tau^H (y^* - \bar{y}) & \text{for } y^* > \bar{y}, \end{cases}$$

where y^* is taxable income and $\bar{y}$ defines the maximum level of taxable income for which the lower *marginal* tax rate, τ^L, is applicable.

In the benchmark case, we choose $\tau^L = 0.15$, $\tau^H = 0.28$, and set $\bar{y}$ using the 1989 tax schedule for married persons filing jointly. Appendix 1 explains how we calibrated the model to this tax schedule.

Solving the Model

The model is solved using numerical techniques. Our procedures involve conjecturing values for the aggregate capital stock and labor supply, calculating steady-state consumption and leisure paths conditional on the factor prices (wages and interest rates) implied by those conjectures, and iterating on updates of the aggregate variables until individual choices are consistent with all relevant market-clearing conditions. More-detailed discussions are contained in appendix 2 and in Altig and Carlstrom (1992).

IV. The Welfare Costs of Raising Revenue through Bracket Creep

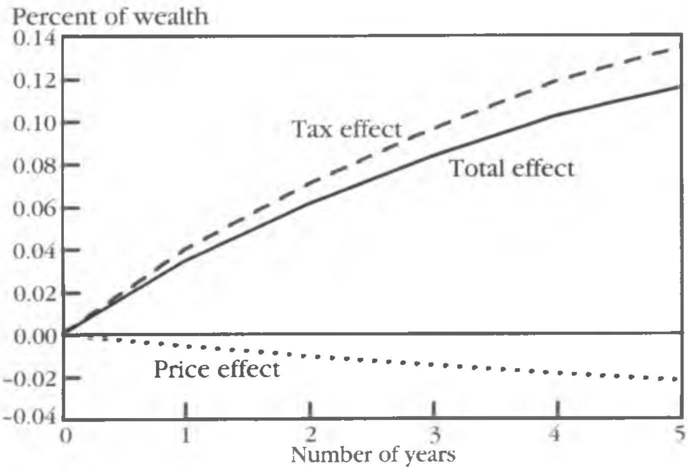
The policy in question involves temporarily forgoing indexation of the tax code. As discussed in section II, this is equivalent to raising the tax base by an amount equal to the rate of inflation prevailing over the period when inflation adjustments are suspended. In this section, we focus on the pure bracket-creep case, meaning that we abstract from problems associated with capital-income mismeasurement. Accordingly, for each age s individual, the new steady-state tax base obtained after repealing indexation for T periods is

9 We use the constant-cost net stock of fixed reproducible tangible wealth reported in the January 1992 *Survey of Current Business* as our measure of the U.S. capital stock. This measure includes consumer durables and government capital.

10 The Bureau of Labor Statistics' survey of payroll establishments implies that nonfarm employees worked an average of 36.5 hours per week over the 1959–92 period.

FIGURE 1

Welfare Losses from Suspending Indexation



NOTE: The model parameters are set to their benchmark values (see table 1).
SOURCE: Authors' calculations.

$$(6) \quad w_s \prod_{t=t'}^{t'+T-1} (1+\pi_t) + A_{s-1}(R-\bar{\pi})/(1+\bar{\pi}) - d_s,$$

where t' is the time at which inflation adjustments are repealed and $t' + T$ is the time at which they are reinstated. The term $\bar{\pi}$ represents the steady-state rate of inflation. Note that this definition of taxable income assumes that deductions are eventually adjusted for inflation and incorporates the assumption that inflation causes no further overstatement of real income once indexation commences.

Our experiments contrast the welfare effects of raising revenue through bracket creep, which we will refer to as the inflation-revenue regime, with an alternative strategy of directly increasing marginal tax rates, which we will refer to as the structural-revenue regime. All adjustments to the statutory rate structure involve proportionate increases in both τ^L and τ^H.[11]

Our welfare measure is the amount of wealth that must be given to a representative individual to compensate for utility losses resulting from raising revenue by suspending inflation indexation. Specifically, if we let $U_{\pi R}$ be the lifetime utility level of each member of a generation living in a steady state under the inflation-revenue regime, and U_{SR} be that of an individual under the structural-revenue regime, then welfare losses are measured as the share of full wealth that must be transferred to individuals in the inflation regime in order to equate $U_{\pi R}$ and U_{SR}.[12]

The solid line in figure 1 plots welfare losses under our benchmark parameterization for T equals 1–5 years, assuming an annual inflation rate of 2.7 percent.[13] Suspending indexation for one year would result in a loss equivalent to about 0.07 percent of wealth. This number grows, although at a decreasing rate, as the number of years over which indexation is suspended (and hence the cumulative rate of inflation) increases. Eliminating inflation adjustments for five years, which in our examples corresponds to price-level growth of about 14 percent, results in welfare losses of roughly 0.14 percent of wealth.[14]

The welfare losses indicated by our benchmark simulations indicate that the exploitation of bracket creep is a relatively inefficient method of taxation. The source of these losses can be more fully understood by examining the broken lines in figure 1. These experiments decompose welfare changes into a "tax effect" and a "price effect." The tax effect is the welfare loss due to changes in the marginal tax rate alone, absent any general-equilibrium price effect. That is, the tax effect is determined by setting prices r and ω at their steady-state levels from the structural-revenue regime, and by setting the age-specific marginal tax rates at the levels determined from the inflation-revenue regime. The welfare loss is the share of full wealth that must be transferred under these circumstances in order to maintain the utility level

11 We have also attempted experiments in which only the top marginal tax rate is increased. Interestingly, "Laffer curve" effects render this alternative infeasible. That is, tax receipts begin to decrease as τ^H rises before revenues in the structural-revenue regime can be equated to those in the inflation-revenue regime.

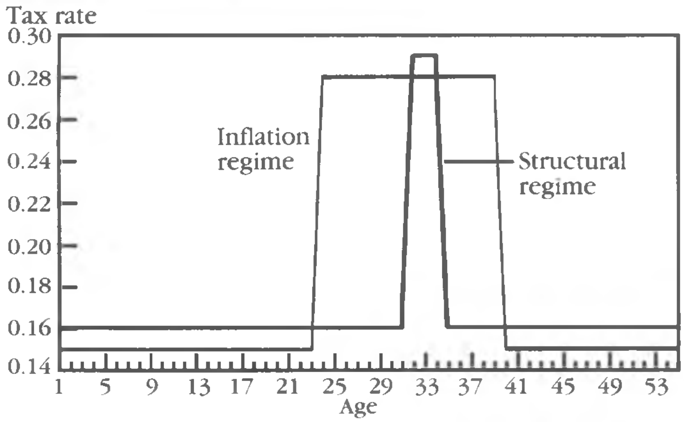
12 Full wealth is defined as the present value of maximum labor income, that is, the amount of market wealth that could be generated if individuals allocated their entire time endowment to working. If welfare losses are negative, then the inflation-revenue regime generates higher utility than does the structural-revenue regime. In this case, the welfare measure would be the share of wealth that must be taken away in order to lower $U_{\pi R}$ to the appropriate level.

13 This corresponds to the inflation rate assumed by the CBO in its most recent estimates of the revenue effects of suspending indexation. See CBO (1993).

14 When indexation is suspended for five years, the equal-revenue structural alternative to the inflation-revenue regime implies marginal tax rates of 16.5 and 30.8 percent.

FIGURE 2

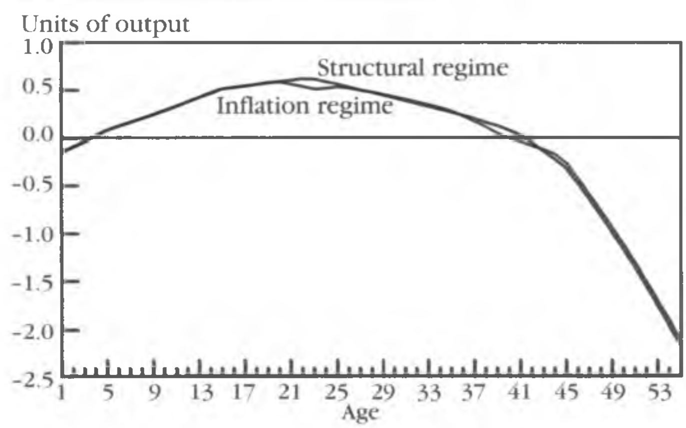
Life-Cycle Paths of Marginal Tax Rates



NOTE: The model parameters are set to their benchmark values (see table 1). Indexing is suspended for two years in the inflation regime.
SOURCE: Authors' calculations.

FIGURE 3

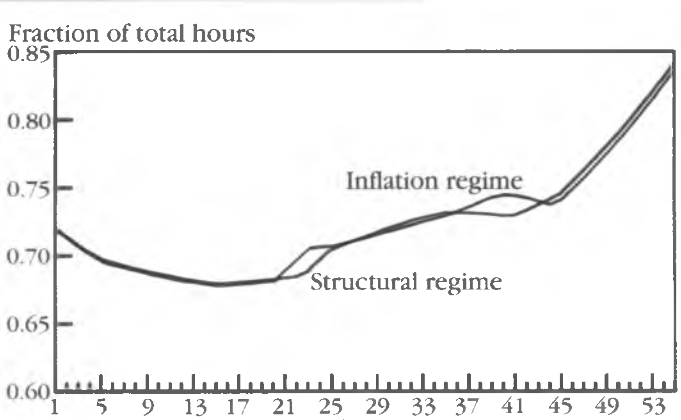
Life-Cycle Saving Profiles



NOTE: The model parameters are set to their benchmark values (see table 1). Indexing is suspended for two years in the inflation regime.
SOURCE: Authors' calculations.

FIGURE 4

Life-Cycle Leisure Profiles



NOTE: The model parameters are set to their benchmark values (see table 1). Indexing is suspended for two years in the inflation regime.
SOURCE: Authors' calculations.

$U_{\pi R}$.[15] Analogously, the price effect is then determined by setting taxes at their steady-state levels from the structural-revenue regime and by setting prices at the levels obtained in the inflation regime.[16]

These partial-equilibrium experiments clearly indicate that the welfare losses from suspending indexation are driven by the direct effects of taxation: The differences in interest rates and wages between the two regimes actually dampen the inefficiency of the inflation-revenue case relative to the structural-revenue case, which is apparent from the fact that price effects are negative.

The reasons for a strong tax effect are suggested by examining the life-cycle paths of marginal tax rates, which are shown in figure 2 for the case where indexation is suspended for two years. Although the rates are marginally higher in the structural-revenue regime over much of the life cycle, they are substantially higher in the inflation-revenue case at some critical ages, specifically, 23–30 and 37–40.[17]

These effects show up clearly in figures 3 and 4, which depict life-cycle saving and leisure profiles in steady states under the two tax regimes, again assuming that inflation adjustments are forgone for two years. In both cases, saving and work effort are depressed near the "kinks" in household budget constraints—the points at which taxable income equals $\bar{y}$—induced by jumps in marginal tax rates. Because the equilibrium outcomes are such that distortions are more severe in the inflation-revenue regime, welfare is lower relative to the structural-revenue case.

Table 2 reports results from various sensitivity experiments in which welfare losses are calculated under alternative settings for the model's parameters. Suspending indexation creates welfare losses relative to our structural alternatives in all cases considered. Although these alternatives are clearly not exhaustive, we conclude from this evidence that our basic finding is robust to plausible changes in the model's parameterization.

■ **15** Thus, for purposes of calculating the tax effect, the modified inflation-revenue regime involves solving for the consumption and leisure profiles given r_{SR}, ω_{SR}, and the marginal tax rates obtained from the regime's original steady-state solutions.

■ **16** Thus, for purposes of calculating the price effect, the modified inflation-revenue regime involves solving for the consumption and leisure profiles given $r_{\pi R}$, $\omega_{\pi R}$, and the marginal tax rates obtained from the general-equilibrium steady-state solutions for the structural-revenue case.

■ **17** Age here refers to a period of adult life. If we assume that adult economic activity begins at biological age 20, then ages 23–30 and 37–40 in the model correspond to biological ages 43–50 and 57–60.

TABLE 2

Welfare Losses under Alternative Parameterizations

	Indexing Suspended for Two Years	Indexing Suspended for Four Years
Benchmark	0.0611	0.1023
Utility weight of leisure		
$\alpha = 0.25$	0.0743	0.1126
$\alpha = 1.0$	0.0465	0.0811
Rate of time preference		
$\rho = 0$	0.0582	0.1001
$\rho = 0.04$	0.1021	0.1271
Elasticity of substitution in consumption		
$1/\sigma_c = 0.33$	0.0601	0.0988
$1/\sigma_c = 0.2$	0.0793	0.1282
Elasticity of substitution in leisure		
$1/\sigma_l = 0.14$	0.0468	0.0759
$1/\sigma_l = 0.33$	0.0719	0.1219
Capital share of output		
$\theta = 0.3$	0.0515	0.0850
$\theta = 0.45$	0.0690	0.1111
Capital depreciation rate		
$\delta = 0.07$	0.0623	0.1031
$\delta = 0.13$	0.0522	0.0857
Population growth rate		
$n = 0$	0.0665	0.1096
$n = 0.03$	0.0552	0.0924

SOURCE: Authors' calculations.

V. Welfare Costs with Capital-Income Mismeasurement

Implicitly, the experiments conducted in the previous section assume that taxable income is calculated as follows: First, an individual's *real* income is determined, then it is multiplied by the appropriate inflation adjustment to obtain nominal income. It is this measure of nominal income to which inflation adjustments are applied.

The actual procedure, of course, omits the first step: Nominal taxable income is obtained directly and then deflated according to the relevant inflation index in order to determine the appropriate tax liability. As noted in section I, while the difference in these two procedures is not critical for calculating real wage income, the second approach overstates real asset income by $\pi \cdot A/(1+\pi)$.

In table 3, we provide a comparison of the welfare losses with and without capital-income mismeasurement. Results are reported for several different parameterizations of the model and pertain to experiments in which indexation is suspended for one year. Not surprisingly, the added, but realistic, complication of capital-income mismeasurement serves only to reinforce the welfare losses associated with the bracket-creep strategy of taxation.

VI. Concluding Remarks

In its recent analysis of alternative deficit-reduction options, the CBO argues that increasing revenue by suspending indexation is inappropriate because it amounts to "unlegislated tax increases." However, because such a suspension is possible only by a vote of Congress and the signature of the President, it is unclear why taxes raised through this approach should be considered unlegislated. Although it is true that the additional amount of revenue obtained over the course of several years would be determined by the inflation outcomes associated with Federal Reserve policy, Congress has ample scope to express itself on the issue of an appropriate inflation trend.

We suggest a more straightforward objection: Raising revenue by temporarily suspending indexation is inefficient relative to the more direct approach of raising marginal tax rates. This inefficiency arises because distortions of private work effort and saving decisions associated with rising marginal tax rates are more severe when revenues are raised through bracket creep. The net result is that the utility of the average individual is higher in the long run if inflation indexation is maintained and if tax revenues are raised by permanently adjusting structural tax rates.

Of course, a multitude of additional factors are ignored in the type of highly stylized model we have employed here. For instance, there is no lifetime heterogeneity and therefore no distributional issues of which to speak. Despite this caveat—which, after all, applies to any model—our analysis suggests that the decision to abandon the bracket-creep tax strategy is a wise one. As the public debate on deficit reduction inevitably continues into the future, taxation through suspending inflation indexation is probably one option we should keep off the table.

TABLE 3

Welfare Losses from Capital-Income Mismeasurement

	Without Capital-Income Mismeasurement	With Capital-Income Mismeasurement
Benchmark	0.0343	0.0718
Utility weight of leisure		
$\alpha = 0.25$	0.0506	0.0776
$\alpha = 1.0$	0.0256	0.0655
Rate of time preference		
$\rho = 0$	0.0318	0.0639
$\rho = 0.04$	0.0624	0.0652
Elasticity of substitution in consumption		
$1/\sigma_c = 0.33$	0.0344	0.0774
$1/\sigma_c = 0.2$	0.0488	0.0793
Elasticity of substitution in leisure		
$1/\sigma_l = 0.14$	0.0277	0.0578
$1/\sigma_l = 0.33$	0.0396	0.0975
Capital share of output		
$\theta = 0.3$	0.0285	0.0684
$\theta = 0.45$	0.0438	0.0840
Capital depreciation rate		
$\delta = 0.07$	0.0378	0.0812
$\delta = 0.13$	0.0291	0.0678
Population growth rate		
$n = 0$	0.0373	0.0868
$n = 0.03$	0.0318	0.0550

NOTE: Simulations assume indexation is suspended for one year.
SOURCE: Authors' calculations.

Appendix 1–Calibration of the Tax Code

Because our simulation model is geared toward capturing the average effects of life-cycle behavior, we calibrate gross income so that the highest level of steady-state cohort income matches the highest median income in the data. Taking 1988 as the reference year, this value was \$42,192, associated with families headed by individuals aged 45–54. This number was obtained from the *Current Population Reports* (series P-60, No. 166, published by the Bureau of the Census) and was converted to 1989 dollars according to the CPIU inflation rate from 1988 to 1989 (4.8 percent). This yields a value for high income in 1989 dollars of \$44,217. The scale of incomes in the model is chosen so that the highest steady-state income generated with the chosen tax code and 4 percent inflation is equal to this value.

Taxable income levels are obtained by adjusting gross income levels for deductions and personal exemptions. In the benchmark case, we assume that all taxpayers take the 1989 standard deduction of \$5,200. The personal exemption level in 1989 was \$2,000. Multiplying by 3.13, the average family size in 1988, yields total personal exemptions of \$6,260. Thus, our simulations assume that $d =$ \$11,460 per household at every age.

Appendix 2–Outline of Solution Strategy

Given a marginal tax-rate structure that is a continuous function of taxable income, the model can be solved using the following algorithm:

(i) Conjecture values for K and L (and hence for r and ω).

(ii) Conjecture a sequence of marginal tax rates, τ_t, for $t =$ 1–55.

(iii) Let u_{it}, $i = c,l$, denote the age t marginal utilities of consumption and leisure, respectively, and let λ_t denote the LaGrange multiplier associated with the time t budget constraint in equation (2). Given the conjectured net prices, use equation (2) and the first-order conditions

(A1) $$u_{c,t} - \lambda_t = 0,$$

(A2) $$u_{lt} - \lambda_t \varepsilon_t \omega_t (1 - \tau_t) = 0,$$

and

(A3) $$-\lambda_{t-1} + \lambda_t \beta[1 + r(1 - \tau_t)] = 0$$

to solve for the optimal consumption and leisure plans for members of each generation.

(iv) Apply the implied path of wage and asset income to the tax code and update the path for marginal tax rates. Updates can be obtained using the Gauss–Seidel algorithm.

(v) Repeat steps (iii) and (iv) until the optimal paths of consumption and leisure are consistent with the marginal tax rates they imply.

(vi) Aggregate individual labor and asset supplies to obtain updates for K and L.

(vii) Repeat steps (ii) through (vi) until aggregate labor and asset supplies are consistent with individual consumption and leisure decisions.

Altig and Carlstrom (1992) demonstrate how a simple change-of-variables strategy can be used to apply this algorithm to the case where marginal tax rates are a step function of taxable income.

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Cyclical Movements of the Labor Input and Its Implicit Real Wage

by Finn E. Kydland and Edward C. Prescott

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Introduction

The standard measure of the labor input is the sum of market-sector employment hours over all individuals. Validity of this measure requires that the composition by skills and ability of those working at each point in time be approximately the same. Over the long term, the experience and educational achievements of the work force have changed markedly, and various methods have been devised to correct for this transformation in quality (Jorgenson, Gollop, and Fraumeni [1987]) or in composition (Dean, Kunze, and Rosenblum [1988]). From a secular point of view, these corrections are large, with the size of the correction being sensitive to the method employed. But from a cyclical accounting point of view, as we show in section IV, it makes little difference whether the standard measure or these alternative measures are used.

The question addressed here is whether, on a cyclical basis, aggregate hours is a good measure of the labor input. It could very well be an adequate cyclical measure despite being a poor secular measure. In particular, if the composition changes are slow relative to cyclical variations in the labor input, it would be a good yardstick from the point of view of cyclical accounting.

Prior to this study, the evidence on this question was mixed. Clark and Summers (1981) find considerable differences in cyclical employment variability across age and sex groups. Hansen (1986), using Current Population Survey data, aggregates the labor input by weighing the hours worked for different age and sex groups by their relative wages. He reports that his measure of the labor input is only slightly more stable than is aggregate hours. Thus, if differences in cyclical variability within such groups were small, as he implicitly assumes, then composition changes would not hinder evaluation of the cyclical variability of the labor input. Kydland (1984), however, maintains that there are in fact strong systematic differences for males (ages 30 and over) in the Panel Study of Income Dynamics (PSID). He states that for this group, the more-educated workers had higher average compensation per hour and less variability in annual hours. Furthermore, the empirical sensitivity of this group's hours with respect to the unemployment rate decreased with the level of education. Using Kydland's estimates, Prescott (1986b) finds that if one adjusts a group's average hours for quality by multiplying it by that group's average wage, quality-adjusted hours worked are only half as sensitive to the unemployment rate as are the quality-unadjusted hours.

In this paper, we systematically examine the issue for all individuals in the PSID sample. We treat each person's time as being a different type of labor input. The rental prices used to construct the sample's aggregate labor input for each of the 14 years from 1969 to 1982 are that person's total labor compensation divided by his total number of hours for the entire period. Because each person's human capital weight is constant over time, these weights are orthogonal to the cycle.

Our measurement procedure is in the national income and product accounting tradition of Kuznets (1946) and Stone (1947). With this approach, aggregate real time series are obtained by evaluating output in different periods using the same set of prices. This is precisely what we do with respect to the labor input.

We determine that, cyclically, our measure of the labor input varies by about one-third less than the measure obtained by the standard method for the PSID sample in the 1969–82 period. Such a large correction, if it held for the entire population, would dramatically change the business cycle facts. If the labor input accounts for a lesser share of the cyclical variation in output, then the residual (the Solow technology parameter) must account for more.

To see this point more clearly, suppose that output, y, is determined by a standard aggregate production function, $y = zk^{\alpha} n^{1-\alpha}$, where z is the level of technology, k and n are the capital and labor inputs, and α is a parameter whose value generally is determined from the respective income shares of GNP. To undertake growth accounting, as proposed by Solow (1957), one then proceeds to take logarithms of the production function and rewrite as follows:

$$\log z = \log y - \alpha \log k - (1 - \alpha) \log n.$$

With time series for y, k, and n, a time series for z is computed as a residual. More recently, this relation has been used with quarterly data as the basis for evaluating the statistical properties of cyclical technological change.[1]

Cyclical movements in the real wage have been the subject of numerous empirical investigations. In an early study, Dunlop (1938) examines British real-wage movements from 1860 to 1913. He finds that real wages tended to increase in most expansions and decline in contractions. Tarshis (1939) corroborates these findings for the U.S. economy in the 1932–38 period, also noting that changes in both the real wage and hours worked were slightly negatively correlated. Fischer (1988, p. 310) reviews these and subsequent studies and concludes "...the weight of the evidence by now is that the real wage is slightly procyclical." This is consistent with Lucas's (1981, p. 226) assessment that "...observed real wages are not constant over the cycle, but neither do they exhibit consistent pro- or countercyclical tendencies."

These findings are problematic for any business cycle theory that assigns an important role to real-wage movements. As Phelps (1970) points out, this is a concern for theories with nominal-wage rigidities because they imply countercyclical movements of the real wage. It is also a problem for theories in which technology shocks induce fluctuations. Unless leisure is highly intertemporally substitutable, as it is in the Hansen (1985) economy, the real wage is strongly procyclical for this class of theories. As emphasized by Christiano and Eichenbaum (1992), and implicitly also by McCallum (1988), the essentially zero correlation of cyclical hours and compensation per hour is especially bothersome for these theories.

Panel studies have examined the sensitivity of individuals' real compensation per hour to the aggregate unemployment rate, with Bils (1985) and Solon and Barsky (1989) finding very strong procyclical movements and Keane, Moffitt, and Runkle (1988) much weaker procyclical movements.[2] For some theoretical frameworks, this is an appropriate procedure for determining how the real wage moves cyclically, but for others it is not. This method assumes that the nature of the employment contract is such that the worker chooses hours and is compensated in proportion to the number of hours worked. This contractual arrangement is the exception rather than the rule, however. Because it is usually the employer who chooses hours given some explicit or implicit compensation schedule, we did not adopt the real-wage definition implicit in the cited panel studies and in the implied measurement procedure. Rather, we employed the approach used

■ 1 For more details, see Prescott (1986a).

■ 2 The samples used in these studies are much narrower than that used in this paper. Bils and Keane, Moffitt, and Runkle use the National Longitudinal Survey of Young Men (ages 14–24) as of the beginning of the sample period. In the section of the Solon and Barsky paper that uses PSID data, the sample is restricted to 357 men who worked in every year of the sample period.

by Kuznets for other series, with the real wage being defined implicitly as total labor compensation divided by the aggregate labor input using a fixed set of wages to value the many different types of labor inputs.

Our finding of strongly procyclical labor-input compensation contrasts sharply with most previous findings. The reason for the difference does not appear to be the special nature of the PSID sample. For this sample in this period, real compensation per hour is weakly procyclical and, cyclically, real compensation per hour and hours are only weakly correlated, as they are for U.S. aggregate data. The disparity arises because we use an alternate definition of the labor input.

I. Measuring the Labor Input and Its Rental Price

The standard measure of the labor input is simply aggregate hours. Let h_{it} be individual-i hours of work in year t. Aggregate hours per person is

$$H_t = \sum_i h_{it} / N_t,$$

where N_t is the number of individuals in the population in year t. The real rental price in period t is

$$W_t^H = \sum_i e_{it} / \sum_i h_{it},$$

where e_{it} is real earnings of individual i in period t.

Our measure of the labor input is

$$L_t = \sum_i \varphi_i h_{it} / N_t,$$

where φ_t is the "normal" price of individual-i labor services. For the sample period, there was little long-term change in real compensation per hour. This led us simply to use as weights real compensation per hour for the entire period. Thus,

$$\varphi_i = \sum_t e_{it} / \sum_t h_{it},$$

where the summations are over years for which individual hours and earnings are available.

Following standard procedures, the implicit real wage of labor services is

$$W_t^L = \sum_i e_{it} / \sum_i \varphi_i h_{it},$$

where, as before, the summation is over those in the sample at date t.

II. Data

The PSID data covered the years 1969–82. Included in the study were individuals in the Survey Research Center's representative national sample of families; those in the additional sample of low-income families drawn from the Survey of Economic Opportunity were not included. Family information was used to construct individual data for the head of the household (defined in the PSID as the male, if present) and, in the case of a married couple, for the wife as well. All people with at least four years of positive annual work hours were included, resulting in a sample of 4,863 individuals.

We obtained labor incomes for heads of household by summing reported income for regular labor, overtime, the labor portion of unincorporated family business, professional practice or trade, and farm activity. Annual hours worked is the sum of hours devoted to these activities. We did not include 1967 and 1968 in the sample because some of the income series went unreported in these years.

Dollar figures were posted for regular income in all years and for the other income categories after 1974. In the 1969–74 period, only an income bracket was reported for each of the other categories, so these observations were assigned income numbers based on the respective bracket. The rule we use for that assignment is specified in appendix 1. Typically, the head of the household reported his or her labor hours and various incomes in the interview. If this person was a married male, he also reported his wife's income and hours. These were the figures used for the married females.

In some cases, the interviewers made major assignments because of insufficient data; we treated these years as missing observations. For some people in some years, the reported annual hours are substantial. We treated figures larger than 365 x 12 = 4,380 hours per year as missing observations.

The tables in appendix 2 present aggregate statistics for the entire sample population as well as separately for males, married females,

TABLE 1

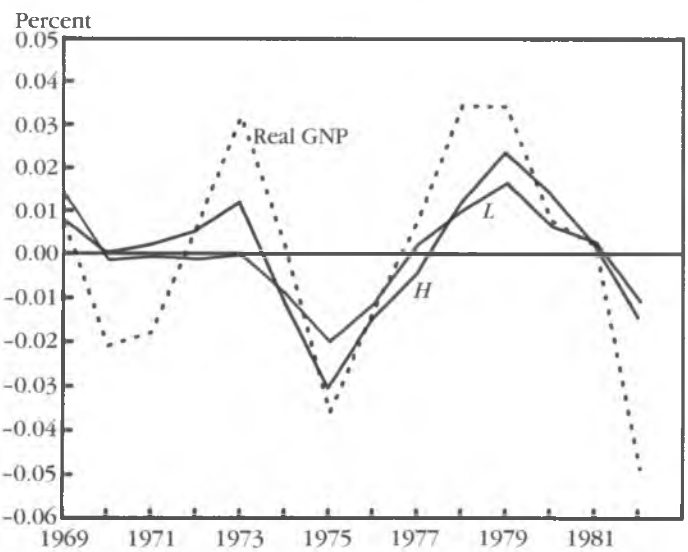
Cyclical Labor Input, Real Wage, and Real GNP: PSID Sample, 1969–82

	Percentage Standard Deviation	Correlations with		
		L	W^L	GNP
Labor input (L)	1.02	—	0.52	0.75
Real wage (W^L)	0.84	0.52	—	0.51
Real GNP	2.50	0.75	0.51	—
		H	W^H	GNP
Hours (H)	1.42	—	0.25	0.80
Compensation per hour (W^H)	0.51	0.25	—	0.12
Real GNP	2.50	0.80	0.12	—
			Empirical Elasticities with Respect to GNP[a]	
Labor input (L)			0.30 (0.08)	
Hours (H)			0.45 (0.10)	

a. Standard errors are in parentheses.
SOURCE: Authors' calculations.

FIGURE 1

Labor Input (L), Aggregate Hours (H), and Real GNP: Full Sample, 1969–82



SOURCE: Authors' calculations.

and single females. The marital status of some women changed over the sample period, so that they appear in the married female group in some years and in the unmarried female group in the other years. The men are not subdivided by marital status because of the small number of unmarried males in the sample.

III. Findings

For purposes of this study, the cyclical component of a time series is defined as the deviation from the time trend.[3] Because it is the percentage variation of each series that is of interest, the logarithms of the various aggregates are the time series whose properties are examined. Key statistics for the full sample are presented in table 1.

Cyclical Behavior of Aggregate Hours and Labor Input

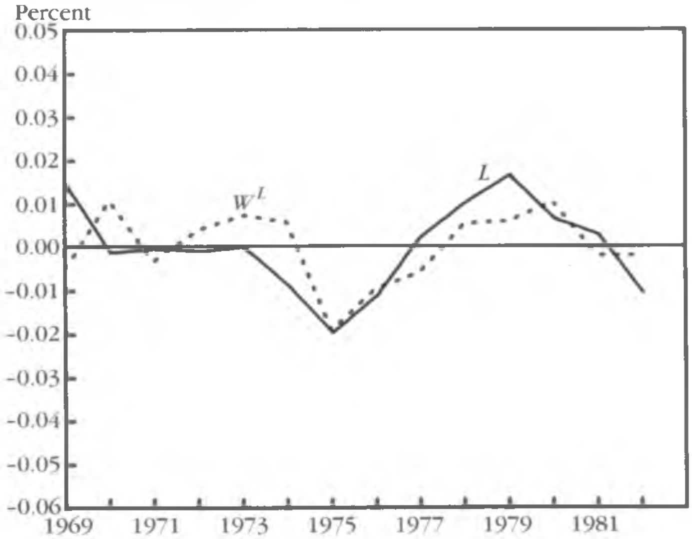
Figure 1 plots the cyclical component of aggregate hours, the aggregate labor input, and real GNP versus time. Clearly, both hours and the labor input vary with GNP, but the hours component varies much more. As shown in table 1, the percentage standard deviations are 1.42 for hours and 1.02 for the labor input, yielding a ratio of the two volatility figures of 1.39.

Insofar as the behavior of the PSID sample is similar to that of the entire population, the use of aggregate hours as a proxy for the labor input gives a highly distorted picture of the cyclical movement of the labor input and therefore of productivity as well. The empirical elasticity of hours with respect to GNP is 0.45, while the empirical elasticity of the labor input with respect to GNP is 0.30—only two-thirds as large.

3 Some view aggregate time series as the sum of a cyclical and a growth component. We do not (see Kydland and Prescott [1982]). One should think of these elements as well-defined statistics that adequately capture for this sample period what are commonly referred to as business cycle fluctuations.

FIGURE 2

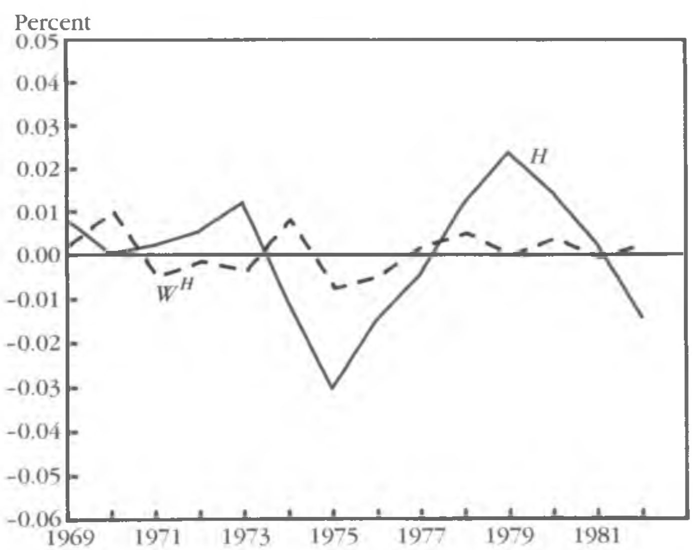
Labor Input (L) and Real Wage (W^L): Full Sample, 1969–82



SOURCE: Authors' calculations.

FIGURE 3

Aggregate Hours (H) and Real Compensation (W^H): Full Sample, 1969–82



SOURCE: Authors' calculations.

Cyclical Behavior of the Labor Input and Its Implicit Real Wage

Part of the conventional wisdom is that real wages and the labor input do not move together cyclically. This reflects the Tarshis (1939) findings for the 1930s and the Christiano–Eichenbaum (1992) results for the postwar period. For our sample in the 1969–82 period, aggregate hours and average compensation did not move together much: The correlation is only 0.25. Thus, the Tarshis findings hold for this period as well if the measure of the labor input is aggregate hours. But the labor input and its implicit real wage—that is, real aggregate compensation divided by the labor input —are strongly and positively associated, with a correlation of 0.52. Figure 2 plots the labor input and real wage versus time, which can be contrasted with aggregate hours and hourly compensation in figure 3.

Clearly, for the human-capital-weighted labor input, the Tarshis result does not hold. The real wage and the labor input move together cyclically. Both average compensation per hour, W^H, and the real wage, W^L, are positively associated with GNP. The correlation for the real wage is 0.51, but it is only 0.12 for average compensation per hour.

Behavior of Growth Rates

The more traditional (and, given current computational capabilities, we think inferior) method of deducing the cyclical behavior of real wages, hours, and employment is to examine relations between changes in variables. This is the methodology employed by Dunlop (1938) and Tarshis (1939) in their pioneering studies. A question that naturally arises is whether our disparate findings are due in part to the difference in methodology. To answer this question, the statistics calculated for cyclical components and reported in table 1 were also calculated for growth rates and are shown in table 2.

We find that growth rates of hours are much more variable than those of labor inputs, with the difference exceeding that for the cyclical components. Growth rates of the labor input and its real wage are positively correlated, while those of hours and compensation per hour are nearly uncorrelated. Similar relations hold for the empirical elasticities of growth rates of the labor input and hours with respect

TABLE 2

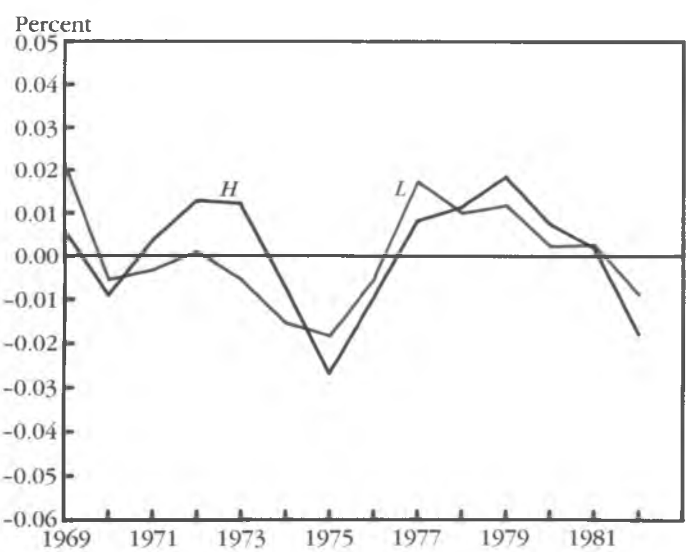
Growth Rates of Labor Input, Real Wage, and Real GNP: PSID Sample, 1969–82

	Percentage Standard Deviation	Correlations with L	Correlations with W^L	Correlations with GNP
Labor input (L)	0.94	—	0.21	0.87
Real wage (W^L)	1.13	0.21	—	0.36
Real GNP	2.75	0.87	0.36	—
		H	W^H	GNP
Hours (H)	1.37	—	0.01	0.88
Compensation per hour (W^H)	0.83	0.01	—	0.02
Real GNP	2.75	0.88	0.02	—
			Empirical Elasticities with Respect to GNP[a]	
Labor input (L)			0.30 (0.05)	
Hours (H)			0.44 (0.07)	

a. Standard errors are in parentheses.
SOURCE: Authors' calculations.

FIGURE 4

Labor Input (L) and Aggregate Hours (H): Males, 1969–82



SOURCE: Authors' calculations.

to GNP. Thus, examining cyclical components versus growth rates does not account for the difference in our findings. The conclusions are the same independent of the method.

Robustness of the Findings

These results strongly support the view that, cyclically, the labor input varies significantly less than does aggregate hours and consequently that productivity fluctuates much more. There would be a problem if the human capital weights were systematically too low for individuals with the most cyclically variable hours of employment. We can think of no reason for such a pattern. On the contrary, a study by Kotlikoff and Gokhale (1992) suggests why the opposite may be the case. Measuring life-cycle compensation and productivity profiles, they find that for highly skilled workers, compensation is lower than productivity in the first half of the life cycle, usually until individuals reach their mid-forties. Especially in the early part of the life cycle, this difference is substantial. Because our sample period includes years in which the baby boomers had just entered the work force (the average age is under 40 in all years before 1980), our sample may include an unusually large number of such highly skilled workers whose measured quality weights understate their productivity.

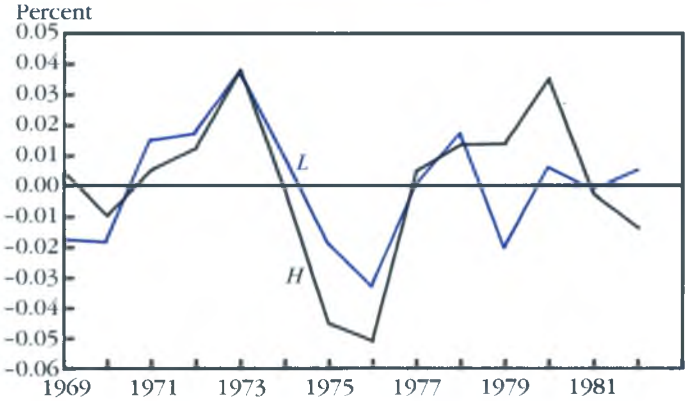
Neither do we believe that cyclical variations in human capital are a concern. The stock of human capital is several times larger than annual output. The variations in the human capital investment would have to be huge to induce significant cyclical variation in the human capital stocks. If they were, cyclical GNP would be a poor measure of cyclical output, for it would not include this large and highly volatile investment component. Other capital stocks are roughly orthogonal to cyclical output, and we can think of no plausible reason for the human capital stock to differ.[4]

We multiplied the weights by identically and independently distributed log-normal random variables with a mean of 1 and a standard deviation of 0.1. This did not affect any of the findings, which indicates that errors in measuring the weights that are not systematically related to the cyclical variability of individuals' hours are not a problem.

4 See Kydland and Prescott (1982).

FIGURE 5

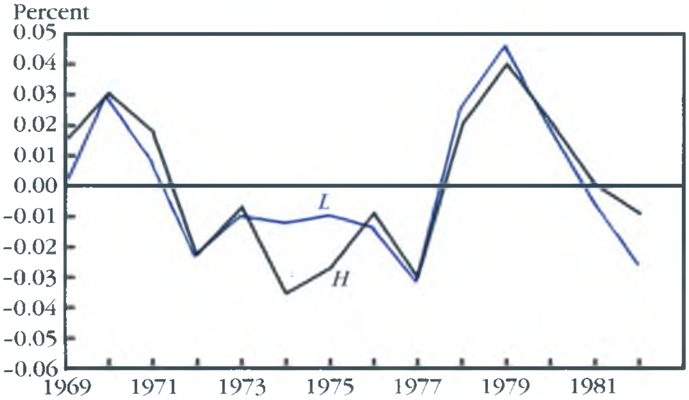
Labor Input (L) and Aggregate Hours (H): Single Females, 1969–82



SOURCE: Authors' calculations.

FIGURE 6

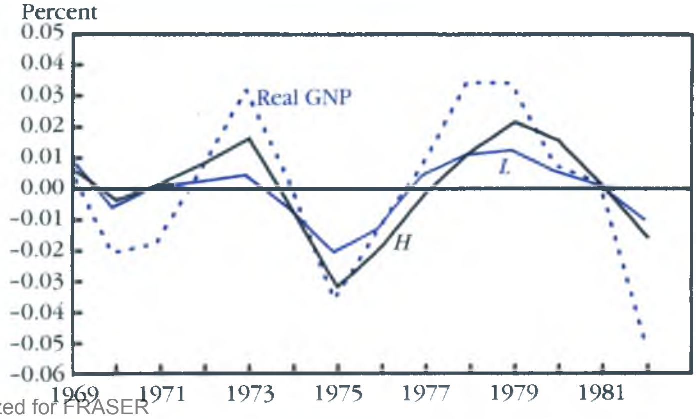
Labor Input (L) and Aggregate Hours (H): Married Females, 1969–82



SOURCE: Authors' calculations.

FIGURE 7

Labor Input (L), Aggregate Hours (H), and Real GNP: Weighted Sample, 1969–82



SOURCE: Authors' calculations.

Over time, people enter and exit the PSID sample and there are missing observations. The number of people in the sample varied smoothly over time and did not fluctuate with GNP. Consequently, our surprising findings do not appear to be the result of missing observations being systematically related to the business cycle.

Some wage observations are sufficiently extreme that they are almost certainly errors. To see how our measurements could be affected by such observations, we omitted year t for person i if e_{it}/h_{it} exceeded both three times φ_i and \$15 in 1969 dollars. The findings were essentially the same with these extreme observations deleted.

Cyclical Behavior of Variables by Demographic Groups

The sample was subdivided into males, single females, and married females. Men were not classified separately by marital status because in half of the years, the number of single males in the sample was less than 200, which is far too small for our purposes. Men accounted for approximately two-thirds of the hours supplied by the total sample and for four-fifths of the labor input. Given this, it would be surprising if the aggregate statistics for males and those for the entire sample were dissimilar. Figures 1 and 4 show only a slight difference in the aggregate behavior of males and that of the entire sample.

An interesting finding is the disparity in the behavior of single and married women. Figures 5 and 6 present plots of their hours and labor input versus time. Given the small size of the samples (less than 600 single and 1,700 married women) and the fact that coefficients of variation are about 0.6 for singles and 0.8 for marrieds, random sampling variability is not small. The empirical elasticities of hours and the labor input with respect to GNP are reported in table 3 along with the standard errors. We find that the labor inputs for males and single females are much less responsive to real GNP than are their hours of work in the business sector. The estimated elasticities are largest for single women.

TABLE 3

Empirical Elasticity of Hours and Labor Input with Respect to Real GNP for Demographic Groups, 1969–82

	Empirical Elasticity with Respect to Real GNP[a]	
	Hours	Labor Input
All	0.45 (0.10)	0.30 (0.08)
Males	0.47 (0.06)	0.27 (0.11)
Single females	0.69 (0.21)	0.26 (0.21)
Married females	0.28 (0.27)	0.35 (0.24)

a. Standard errors are in parentheses.
SOURCE: Authors' calculations.

TABLE 4

Cyclical Labor Input, Real Wage, and Real GNP: Weighted Sample, 1969–82

	Percentage Standard Deviation	Correlations with		
		L	W^L	GNP
Labor input (L)	0.98	—	0.55	0.82
Real wage (W^L)	0.80	0.55	—	0.51
Real GNP	2.50	0.82	0.51	—
		H	W^H	GNP
Hours (H)	1.50	—	0.01	0.83
Compensation per hour (W^H)	0.43	0.01	—	−0.09
Real GNP	2.50	0.83	−0.09	—

	Empirical Elasticities with Respect to GNP[a]
Labor input (L)	0.32 (0.06)
Hours (H)	0.50 (0.10)

a. Standard errors are in parentheses.
SOURCE: Authors' calculations.

IV. Implications of the Findings

The results for the PSID sample indicate that, cyclically, workers' aggregate hours are not a good measure of their aggregate labor input. To make the PSID sample more representative of the U.S. population, we weighted the three demographic groups by their relative numbers in the U.S. population. Figure 7 plots both the weighted-sample hours and labor input along with real GNP. These hours move in closer conformity with GNP than do the unweighted figures. Table 4 presents some summary statistics, which are essentially the same as those for the unweighted sample as reported in table 1.

Bias of Measures of Relative Volatility

The statistics reported are nonlinear functions of sample moments. A question is how close they are to the statistics for the population from which the sample was drawn. For a random sample of a given size, there is generally a sampling distribution, which is a function of the distribution of characteristics in the sampled population. This sampling distribution is a continuous function of the distribution of population characteristics.

We used Monte Carlo techniques to determine the sampling distribution for the population for which the PSID sample is representative. If this distribution is close to the actual population distribution, continuity implies that the distribution of sampling errors for the actual population will be close to the computed one. Insofar as it is sufficiently close (which is true asymptotically), the sampling-error distribution for the ratio of the standard deviations of hours and the labor input has a negative bias of 0.13 and a standard deviation of 0.16. If the true value were 1.25, which is a large number from the point of view of business cycle accounting, in only one of the 100 random samples was the statistic as much as 0.28 above its true value. This, we think, indicates that the difference in volatilities is most likely greater than 25 percent for the actual population in this period.

Comparison with Other Measures of the Labor Input

The standard measure of the labor input is aggregate hours. When it is adjusted using the composition adjustment factor of Dean, Kunze, and Rosenblum (1988), the measure's cyclical variability is reduced from 2.34 to 2.07 percent, implying that hours are 13 percent more volatile than their adjusted hours in the 1969–82 period. Similarly, for the Jorgenson, Gollop, and Fraumeni (1987) adjustment, the variability of hours is 2.20, while it is 1.84 for their labor input in the 1969–79 period. Thus, hours are 19 percent more variable than is their labor input. Finally, comparing Darby's (1984) total hours and quality-adjusted hours for the same period, the cyclical variability of the former is 3.02 percent, while that of the latter is 3.06.

In all three studies, there is considerable aggregation within each group, and quality weights are computed on this basis. From a cyclical accounting point of view, these adjustments are somewhat significant, but are dwarfed by the adjustments suggested by our study. We, of course, use separate weights for each individual. The weighted-sample hours variability is 1.50 percent—a full 53 percent larger than the labor-input variability, which is only 0.98 percent.

Implications for Accounting for Cyclical Variations in Output

To the extent that the relative variabilities of hours and the labor input found for the weighted PSID sample hold for the entire U.S. population, our findings call for major revision of the traditional view of the nature of business cycles. Rather than productivity and the labor input being slightly negatively correlated, they become strongly positively associated. The importance of variations in the labor input in accounting for fluctuations in aggregate output is substantially reduced. Given that cyclical components of capital stocks and output are roughly orthogonal, variation in the Solow technology coefficient must account for much more of business cycle fluctuations in output. This factor, then, is nearly as important as are variations in the labor input.

APPENDIX 1

Figures Used for Bracketed Income Variables, 1969–74

Income Bracket (Annual Dollars)	Value Used
1–499	250
500–999	750
1,000–1,999	1,500
2,000–2,999	2,500
3,000–4,999	4,000
5,000–7,499	6,000
7,500–9,999	8,500
10,000 and over	14,000

SOURCE: Authors.

APPENDIX 2

Sample Averages: Full Sample and Males, 1969–82

	Full Sample						Males				
Year	H	L	E	Age	No.	Year	H	L	E	Age	No.
1969	1,657	1,754	5,981	38.8	2,710	1969	2,210	2,647	8,999	40.4	1,425
	(962)	(1,502)	(5,568)	(13.1)			(659)	(1,427)	(5,745)	(13.1)	
1970	1,634	1,704	5,979	38.6	2,914	1970	2,149	2,530	8,863	40.1	1,534
	(951)	(1,440)	(5,542)	(13.4)			(699)	(1,387)	(5,782)	(13.5)	
1971	1,628	1,681	5,900	38.5	3,161	1971	2,135	2,491	8,682	40.0	1,666
	(954)	(1,436)	(5,507)	(13.7)			(715)	(1,403)	(5,814)	(13.9)	
1972	1,623	1,657	5,939	38.3	3,374	1972	2,142	2,456	8,799	39.7	1,779
	(968)	(1,387)	(5,549)	(14.0)			(733)	(1,314)	(5,793)	(14.2)	
1973	1,625	1,637	5,965	38.4	3,596	1973	2,114	2,396	8,744	39.7	1,905
	(963)	(1,390)	(5,569)	(14.1)			(767)	(1,366)	(5,892)	(14.4)	
1974	1,580	1,600	5,901	38.6	3,757	1974	2,048	2,330	8,607	39.8	1,990
	(961)	(1,351)	(5,711)	(14.3)			(796)	(1,322)	(6,200)	(14.5)	
1975	1,540	1,561	5,694	38.4	3,889	1975	1,983	2,281	8,290	39.6	2,026
	(950)	(1,386)	(5,956)	(14.3)			(819)	(1,432)	(6,803)	(14.6)	
1976	1,555	1,553	5,798	38.9	4,037	1976	1,992	2,269	8,445	40.1	2,102
	(957)	(1,367)	(5,929)	(14.5)			(843)	(1,414)	(6,714)	(14.8)	
1977	1,562	1,553	5,899	39.2	4,149	1977	2,002	2,280	8,648	40.2	2,143
	(963)	(1,348)	(5,884)	(14.5)			(843)	(1,374)	(6,549)	(14.8)	
1978	1,578	1,544	6,012	39.4	4,287	1978	1,983	2,222	8,707	40.5	2,221
	(946)	(1,336)	(5,891)	(14.7)			(846)	(1,392)	(6,570)	(14.9)	
1979	1,588	1,532	6,053	39.7	4,474	1979	1,972	2,186	8,680	40.6	2,330
	(939)	(1,339)	(5,827)	(14.7)			(852)	(1,422)	(6,483)	(14.9)	
1980	1,564	1,496	6,019	40.7	4,401	1980	1,926	2,127	8,611	41.7	2,286
	(947)	(1,315)	(5,964)	(14.8)			(875)	(1,406)	(6,724)	(15.1)	
1981	1,537	1,470	5,922	41.8	4,376	1981	1,892	2,089	8,467	42.6	2,271
	(949)	(1,287)	(5,888)	(14.7)			(881)	(1,361)	(6,631)	(14.9)	
1982	1,502	1,431	5,841	42.7	4,309	1982	1,832	2,028	8,319	43.6	2,223
	(968)	(1,316)	(6,627)	(14.7)			(928)	(1,439)	(7,872)	(14.9)	

NOTES: H = annual hours of work; L = annual labor input; and E = annual real labor earnings in 1969 dollars. Standard deviations are in parentheses.

SOURCE: Authors' calculations based on PSID data.

APPENDIX 2 (CONT.)

Sample Averages: Single and Married Females, 1969–82

Single Females						Married Females					
Year	H	L	E	Age	No.	Year	H	L	E	Age	No.
1969	1,527 (794)	1,174 (917)	4,068 (3,139)	41.7 (15.5)	263	1969	919 (844)	659 (726)	2,264 (2,495)	35.9 (11.7)	1,022
1970	1,482 (796)	1,142 (897)	4,037 (3,213)	41.2 (15.7)	290	1970	950 (845)	690 (743)	2,439 (2,663)	35.7 (12.0)	1,090
1971	1,483 (811)	1,150 (901)	4,106 (3,226)	42.3 (15.8)	306	1971	954 (845)	686 (732)	2,466 (2,664)	35.4 (12.2)	1,189
1972	1,472 (815)	1,122 (907)	3,943 (3,316)	42.1 (16.1)	333	1972	932 (842)	674 (739)	2,435 (2,694)	35.4 (12.4)	1,262
1973	1,488 (793)	1,115 (878)	4,009 (3,255)	42.4 (16.5)	358	1973	963 (841)	692 (738)	2,520 (2,693)	35.4 (12.4)	1,333
1974	1,409 (831)	1,056 (879)	3,813 (3,117)	42.6 (16.7)	388	1974	953 (831)	701 (755)	2,587 (2,804)	35.7 (12.5)	1,379
1975	1,329 (831)	999 (834)	3,738 (3,217)	42.0 (16.8)	429	1975	977 (829)	713 (745)	2,613 (2,749)	35.7 (12.6)	1,434
1976	1,302 (832)	958 (817)	3,657 (3,203)	42.7 (17.3)	454	1976	1,012 (831)	720 (723)	2,700 (2,785)	36.0 (12.6)	1,481
1977	1,357 (867)	964 (812)	3,626 (3,180)	42.9 (17.4)	478	1977	1,008 (833)	718 (731)	2,756 (2,931)	36.5 (12.7)	1,528
1978	1,348 (883)	955 (812)	3,645 (3,217)	43.3 (17.8)	493	1978	1,078 (830)	771 (750)	2,951 (2,996)	36.8 (12.6)	1,573
1979	1,329 (872)	895 (747)	3,547 (3,081)	43.0 (18.1)	527	1979	1,118 (833)	799 (761)	3,087 (3,083)	37.4 (12.7)	1,617
1980	1,338 (906)	895 (769)	3,527 (3,107)	44.4 (18.0)	533	1980	1,116 (840)	789 (743)	3,115 (3,182)	38.1 (12.6)	1,582
1981	1,269 (931)	865 (778)	3,477 (3,216)	45.5 (17.9)	552	1981	1,112 (838)	781 (757)	3,070 (3,157)	39.2 (12.7)	1,553
1982	1,237 (938)	848 (788)	3,414 (3,334)	46.7 (17.8)	551	1982	1,120 (861)	776 (756)	3,126 (3,345)	40.0 (12.6)	1,535

NOTES: H = annual hours of work; L = annual labor input; and E = annual real labor earnings in 1969 dollars. Standard deviations are in parentheses.
SOURCE: Authors' calculations based on PSID data.

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Money and Interest Rates under a Reserves Operating Target

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Introduction

Between October 1979 and mid-summer 1982, the Federal Reserve focused its attention on controlling a narrow monetary aggregate (M1) and relied primarily on nonborrowed reserves as the short-run instrument for achieving its monetary target. This brief but important period provides a unique opportunity to examine the dynamic effects of the short-run monetary supply process.

Although many interesting issues could be examined, we concentrate on two that have received little empirical attention: 1) the speed and dynamic response patterns of both money and short-term interest rates to changes in nonborrowed reserves, and 2) the extent of "feedback" effects between short-run shocks to money and the central bank's provision of nonborrowed reserves.

We provide unique estimates of liquidity effects following changes in the supply of nonborrowed reserves. This work originated over a decade ago in the midst of heated debate about Federal Reserve operating procedures and monetary control. Although the debate has cooled, there is renewed interest in understanding liquidity effects because they are central to the monetary policy transmission mechanism.[1] Using monthly data, Leeper and Gordon (1992) show that evidence about the existence of liquidity effects is ambiguous. They conclude that a successful characterization of such effects requires the identification of private and public behavior. This paper identifies liquidity effects through temporal disaggregation and a structural specification based on the mechanisms of monetary control.

Our approach is to disaggregate the time dimension of the analysis into the shortest period of practical concern for most monetary policy decisions. Thus, our estimation procedures exploit daily data collected by the Federal Reserve. A simplified structural model of the short-run money supply process is developed that, because of the paucity of variables available on a daily basis, is estimated using lagged endogenous variables. We further emphasize the short-run nature of the model by estimating separate statistical models, and therefore separate effects, for each day of the week. Each model includes controls for the Federal Open Market Committee's (FOMC) M1 target growth paths and is estimated in first-difference form. Thus, the models focus only on very short-term

1 Bernanke and Blinder (1992) make a persuasive case for the presence of liquidity effects in the U.S. economy. Christiano and Eichenbaum (1992), Christiano (1991), and Coleman, Labadie, and Gilles (1993) build explicit models of liquidity effects.

behavior and abstract from longer-term relationships and policymaking. In essence, we examine the reaction of money and interest rates to deviations of nonborrowed reserves from weekly and longer-term target growth paths. Thus, we control for those factors that are generally the focus of monthly or quarterly models and examine the variability that is averaged out in such analyses.

Statistical tests of the importance of day-by-day effects are performed. We then use the estimation results to simulate the short-run dynamic relationships between nonborrowed reserves, the federal funds rate, and a measure of transaction accounts. These experiments provide insights regarding the short-run money supply process that are not accessible using the more time-aggregated data of previous studies.[2]

I. The Model

On October 6, 1979, the Federal Reserve announced that it was switching its short-run operating target from the federal funds rate to nonborrowed reserves in an effort to better control the money supply. By the latter part of 1982, the Fed had begun to deemphasize M1 as its monetary target, with a resulting decline in the use of nonborrowed reserves as its operating target.[3] During this brief period, however, nonborrowed reserves appear to have been the primary short-term instrument of central bank policy, while the federal funds rate was determined primarily by market factors.[4]

Stevens (1981) characterizes Federal Reserve nonborrowed reserves policy during this period as consisting of five steps. Steps one and two occurred at the FOMC meetings and involved the setting of yearly and short-run (inter-FOMC meeting) paths for M1. In the third step, the staff derived the target growth paths for borrowed and nonborrowed reserves on the basis of the short-run path for M1. Step four was repeated each week. Incoming information about the money multiplier, unexpected changes in the mix of deposits, and unexpected changes in the demands for currency, excess reserves, and borrowed reserves were used to translate the inter-meeting objective for nonborrowed reserves into a target for the reserve maintenance week. "On Friday," Stevens argues, "...objectives ... can be set, reflecting any technical corrections and judgmental adjustments to the inter-meeting reserve objectives ...." Step five translated the weekly objective into a daily program. At this level, changes in nonborrowed reserves were primarily reactive to very short-run changes in the market factors absorbing and supplying reserve funds. These factors included such items as Treasury operations, Federal Reserve float, and unexpected discount window borrowing. Although federal funds rate targeting was not explicitly used, funds rate changes were sometimes read by policymakers as indicators of changes in these underlying factors, which would have prompted daily adjustments in nonborrowed reserves.

We focus on the last two steps, examining how unexpected shocks to money and the federal funds rate influenced weekly and daily reserve operations. We also examine how reserve changes affected money and the funds rate. Open market operations have a direct effect on money via the creation or destruction of bank deposits, while indirect effects may work through the funds rate. The use of daily data allows us to study feedback effects. That is, changes in reserves induce changes in money and the federal funds rate, which may ultimately cause additional changes in reserves because policymakers cannot distinguish them from other money or interest-rate shocks.[5]

The general lack of daily data and the analytical complexity of combining five daily models into an empirically tractable system forces us to restrict our description of the daily money supply process to a straightforward structure.[6] In this spirit, a reasonably accurate—but admittedly simplified—model of the bank reserves

■ 2 Other studies have used weekly and often monthly or quarterly data to examine what are frequently very short-run issues. See, for example, Spindt and Tarhan (1983, 1987), Tinsley et al. (1982), Jones (1981), Johannes and Rasche (1981), Feige and McGee (1977), and Gavin and Karamouzis (1985).

■ 3 See Axilrod (1982, 1985).

■ 4 Poole (1982) disputes this claim, arguing that the Fed actually used borrowed reserves as its short-run target and that under the lagged reserve requirements in effect at the time, it should have been using free reserves. Notice, however, that since free reserves equal nonborrowed reserves minus required reserves, which are fixed in any given week under lagged reserve requirements, over any weekly period nonborrowed reserves and free-reserves targeting are functionally equivalent. Spindt and Tarhan (1987) present evidence supporting the view that over an inter-FOMC operating horizon, the Federal Reserve, from fall 1979 through fall 1982, followed a nonborrowed reserves operating target. Over shorter (weekly) intervals, however, their results are ambiguous.

■ 5 Avery (1979) models monetary policy as an endogenous variable. His results suggest that over the 1955–75 period, feedback effects occurred within a month.

■ 6 A more complex model of short-run money supply over the period studied here is provided in Goodfriend et al. (1986). Other authors, including Judd and Scadding (1982), have suggested linkages between the federal funds rate and money demand, working through interest-rate term structures. Since the current model and subsequent empirical work ignore interest rates other than the federal funds rate, the maintained assumption is that the term structure shifts proportionately with changes in the funds rate. Spindt and Tarhan (1987) provide results that support this assumption for our sample period.

market during the November 1979 to mid-summer 1982 period would focus on three key variables: 1) nonborrowed reserves (*NBR*), 2) the federal funds rate (*FFRT*), and 3) the equilibrium quantity of transaction money (*TRAN*). Such a model may be written as

$$(1) \quad NBR_t = a_{11}\, FFRT_t + a'_{12} X_t + e_{1t},$$

$$(2) \quad FFRT_t = a_{21}\, NBR_t + a'_{22} X_t + e_{2t},$$

$$(3) \quad TRAN_t = a_{31}\, NBR_t + a_{32}\, FFRT_t + a'_{33} X_t + e_{3t},$$

where

X_t is a vector of relevant exogenous variables, a_{ij} represents behavioral parameters (or vector a' of parameters), t is time measured in days, and e_{it} is normally distributed random disturbances, which are serially uncorrelated (though they may be correlated with each other).

This is a block recursive model in which nonborrowed reserves and the funds rate are determined simultaneously in the federal funds market (equation [1] represents *NBR* supply and equation [2] *NBR* demand), and the equilibrium quantities of *NBR* and *FFRT* help to contemporaneously determine *TRAN*. Thus, feedback is allowed between *FFRT* and *NBR*, but not between *TRAN* and *NBR*. The rationale for this is based both on the institutional fact of lagged reserve requirements, under which required reserves held in week three were based on transaction accounts held in week one, and on the view that neither the Federal Reserve nor the market observed changes in aggregate money during the day.

Analysis of the dynamic relationships among the endogenous variables is facilitated by considering the reduced form of the model:

$$(4) \quad NBR_t = P'_1 X_t + v_{1t},$$

$$(5) \quad FFRT_t = P'_2 X_t + v_{2t},$$

$$(6) \quad TRAN_t = P'_3 X_t + v_{3t},$$

where the P's are reduced-form coefficients, and

$$v_{1t} = \frac{a_{11} e_{2t} + e_{1t}}{1 - a_{11} a_{21}}$$

$$v_{2t} = \frac{a_{21} e_{1t} + e_{2t}}{1 - a_{11} a_{21}}$$

$$v_{3t} = a_{31} v_{1t} + a_{32} v_{2t} + e_{3t}.$$

The functional relationships among the reduced-form errors (the v's) are identical to the contemporaneous relationships that exist among the endogenous variables in the structural model. That is,

$$(7) \quad v_{1t} = a_{11} v_{2t} + e_{1t},$$

$$(8) \quad v_{2t} = a_{21} v_{1t} + e_{2t},$$

$$(9) \quad v_{3t} = a_{31} v_{1t} + a_{32} v_{2t} + e_{3t}.$$

Thus, analysis of the reduced-form errors in equations (7)-(9) will provide impulse-response functions identical to those obtained by analyzing the structural model directly.[7]

The reduced-form equations (4)-(6) are estimated as a set of vector autoregressions (VARs), where the X's in each equation are a set of lagged endogenous variables (with some minor additions). This particular choice of exogenous variables implies that the v's in (7)-(9) will be one-step-ahead forecast errors.

The VAR methodology, pioneered by Sims (1980, 1982), was adopted primarily because of the difficulty of collecting more-traditional exogenous variables on a daily basis.[8] Many exogenous variables that have been used in weekly or monthly money-demand models are simply not available on a daily basis. Use of the VAR methodology allows us to get around this problem by thinking of the lagged endogenous variables as instruments for a more complex set of X's. An additional advantage of using lagged endogenous variables is that it allows us to perform a relatively simple calculation of the dynamic

7 Briefly, this can be seen as follows. Consider the simultaneous equation system

$$Y_t = BY_t + PX_t + e_t,$$

where Y_t, X_t, and e_t are vectors of endogenous, exogenous, and random errors, respectively, and B and P are matrices of coefficients. The reduced form of this system is

$$Y_t = (I - B)^{-1} PX_t + u_t,$$

with

$$u_t = (I - B)^{-1}\, e_t.$$

From the latter relationship, it is clear that

$$u_t = Bu_t + e_t,$$

of which equations (7) - (9) are but a special case.

8 A few examples will give the flavor of the types of variables that might be used in a more explicit model. Under the system of lagged reserve requirements in existence during the study period, required reserves were fixed within each reserve maintenance week (Thursday–Wednesday) and were determined by required reserve ratios and the two-week lagged values of reservable liabilities. The demand for excess reserves is affected by a number of factors, including the volume of reserve account transactions and the risk preferences of individual banks. The supply of reserves is influenced by the demand for borrowed reserves, which depends in part on the spread between the federal funds rate and the discount rate, and the degree of "moral suasion" exerted at the discount window.

reaction of the system to shocks without having to specify or estimate the dynamic behavior of the exogenous variables separately.

Even with the assumption that money is determined recursively, the structural parameters of equations (7)-(9) cannot be calculated from the reduced-form equations without further restriction because of the simultaneous determination of *NBR* and *FFRT.* A traditional identifying assumption would be that the reserve supply is set during the previous period and thus is exogenous. However, we decided that this assumption is inappropriate in the daily model, since we are focusing on the reaction of the Federal Reserve Open Market Desk to unforeseen changes in the economic environment. Moreover, as shown in the next section, it also turns out to be inconsistent with the positive contemporaneous relationship observed empirically between the two variables (or between their one-step-ahead forecast errors, v_1 and v_2). An alternative, albeit arbitrary, restriction was therefore imposed.

We assumed that the structural coefficient representing the effect of a contemporaneous change in reserves on the federal funds rate, a_{21}, was identical to the structural coefficient of the previous day's reserve change on the funds rate (an element of a'_{22}). This additional assumption, which identifies the entire system, centers around the belief that banks trading in the federal funds market smooth the price of reserves from day to day. This may occur for two reasons. First, because of lags in the system, it isn't clear that traders can actually detect "new" reserves within a day. Second, during the study period, reserve accounting took place on a weekly rather than daily basis. Reserves on any one day of the maintenance period were almost perfect substitutes for reserves on another day. Thus, the relevant reserve quantity in determining the funds rate was an estimate of "weekly" reserves, which would be equally affected by contemporaneous and one-day-lagged shocks.[9]

II. Data

Our empirical analysis is based on the dynamic relationships of the three variables discussed above—nonborrowed reserves, the federal funds rate, and money as measured by transaction accounts. Data on each of these variables were collected for the five working days (Thursday through Wednesday) of the 139 reserve maintenance weeks from November 7, 1979 through June 30, 1982. This represents a relatively homogeneous period with respect to the operating procedures of monetary policy following the Federal Reserve's October 1979 adoption of reserve targeting. The only likely deviation occurred during the April–August 1980 period of credit controls. Our calculations for this interval are characterized by an intercept shift in all estimated equations.

Data were collected from several sources. Systemwide nonborrowed reserves were taken from the Federal Reserve's daily balance sheet and then corrected for "as-of" adjustments and overdrafts.[10] Transaction accounts were measured as the sum of gross demand deposits, automatic transfers from savings accounts, telephone and preauthorized transfer accounts, and NOW accounts and share drafts, minus demand balances due from depository institutions in the United States, less cash items in the process of collection. These data, designated *TRAN,* were gathered at the individual bank level and then aggregated daily across all Federal Reserve member banks.[11] The federal funds rate was measured as the daily weighted average computed by the Federal Reserve.

Sample means and standard deviations for the variables used in this study are presented in table 1. Average levels for each variable are given, as are average changes by day of the week. The data show substantial variation in the day-by-day change in every variable. For example, Friday and Monday appear to have been especially atypical for member-bank transaction accounts. Each Friday, an average of $9.8 billion flowed out of these accounts, and on Monday $13.4 billion flowed in. The Friday outflow may have resulted either from the weekend migration of transaction accounts to higher yields, from Eurodollar arbitrage behavior by the big banks that was common

■ 9 The robustness of this assumption was tested in estimating the impulse-response functions (presented in the next section) by resetting the coefficient a_{21} to one-half and to two times the structural coefficient of the previous day's reserve change. In neither case were the substantive conclusions drawn from calculating the impulse-response functions changed.

■ 10 As-of adjustments are corrections made up to three weeks later to reflect errors in the original accounting. Overdrafts are negative balances not reflected in the original accounting.

■ 11 *TRAN* is taken from the Report of Deposits submitted to the Federal Reserve for the purpose of computing required reserves. The data used are final "hard" numbers subject to little revision. It should be noted that the money supply data were released each Friday and were computed from different sources than those used for *TRAN.* However, the *TRAN* definition was chosen after extensive conversations with the Federal Reserve staff responsible for computing the monetary aggregates over the sample period. They indicated that *TRAN* would be extremely highly correlated with the aggregate measures of transaction money used at the time.

TABLE 1

Sample Means[a]

Variable	Average Daily Level	Average Daily Change: Thursday	Friday	Monday	Tuesday	Wednesday
NBR	40,361	149	–126	–39	–726	690
		(3,549)	(2,055)	(3,079)	(2,599)	(3,115)
FFRT	1,467	40	–9	3	–10	–25
		(88)	(52)	(62)	(67)	(110)
TRAN	213,329	570	–9,849	13,432	–4,394	421
		(3,751)	(7,308)	(8,753)	(9,109)	(3,972)
Total system excess reserves	531	–873	116	–220	–778	1,721
		(3,839)	(2,037)	(3,087)	(2,662)	(3,543)
Total system reserve borrowings	1,426	–1,052	247	–181	–52	1,031
		(1,298)	(431)	(395)	(332)	(1,433)

a. The federal funds rate is measured in basis points. All other variables are measured in millions of dollars. All data are nonseasonally adjusted.
NOTE: Standard deviations are in parentheses.
SOURCE: Board of Governors of the Federal Reserve System.

over part of the estimation period, or from banks' attempts to reduce reservable liabilities over the Friday to Sunday period.

The more-than-compensating Monday inflow may reflect the return of Friday's funds and the Monday posting of weekend transactions.[12] In addition, it is interesting to observe that the average federal funds rate fell on both Tuesday (10 basis points) and Wednesday (25 basis points). However, these declines were more than offset by the 40-basis-point average increase on Thursday. This may reflect either a falloff in reserve demand toward the end of the reserve maintenance week (because risk-averse banks obtained their reserves earlier) or an expansion of reserve supply. It may also indicate risk-averse actions on the part of the Federal Reserve Board to supply reserves and thus avoid a large swing in the funds rate.

Prior to use in the regression analysis, the raw data had to be adjusted. To control for trends, we converted each variable to a daily first difference. Because data were not otherwise seasonally adjusted, variables were further transformed to deviations around seasonal (monthly) means. We computed values for the 10 bankers' holidays per year using predicted values from auxiliary regressions similar in form to those ultimately used in the analysis, but employing only those observations with complete data.

The basic VARs were estimated utilizing all three variables: *NBR*, *TRAN*, and *FFRT*. Results for these regressions are available in Avery and Kwast (1986). We regressed the daily change in each variable against both the lagged daily changes in the current and previous reserve week and the weekly changes of the second through fourth lagged reserve weeks for each of the three series.[13] In addition, we used variables representing the FOMC's short-run path for M1, an intercept shift for the April–August 1980 credit control period, and binary variables for the day, the day after, and two days after a Social Security payment (generally the third of each month), as well as for the end of a quarter. These variables were designed to capture what are commonly recognized as the most important seasonal effects not accounted for by the transformation using monthly means. Each regression was fit separately for all five days in the reserve week, utilizing the 139 sample weeks of data.

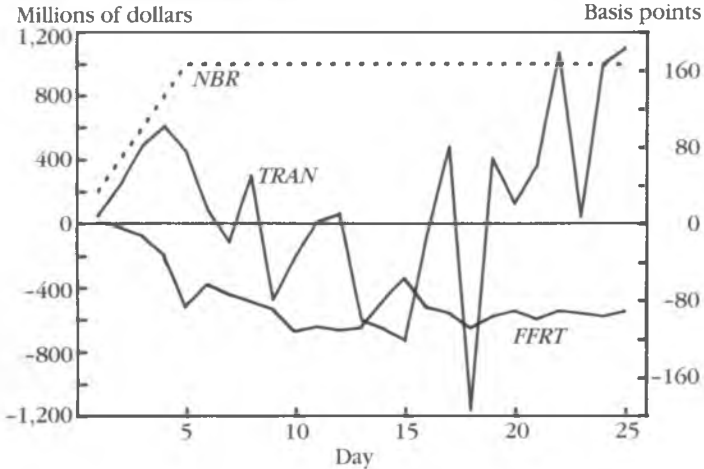
The first-differencing and VAR forms of the regressions appear to have removed most first-

12 Such transactions include deposits by retail stores and automated teller machine activity.

13 The choice of lag structure reflects a trade-off between nonrestrictive completeness and estimation parsimony. Once daily observations for the previous week were included, the model results were not particularly sensitive to the lag specification of the endogenous variables.

FIGURE 1

Cumulative Response to a $200-Million-per-Day Net Shock to *NBR* Maintained for Five Days



NOTE: Cumulative net change.
SOURCE: Authors' calculations.

TABLE 2

Cumulative Response to a $200-Million-per-Day Net Shock to *NBR* Maintained for Five Days

Variable	One Week	Two Weeks	Three Weeks	Four Weeks	Five Weeks
NBR change[a]	1,000	1,000	1,000	1,000	1,000
	(–)	(–)	(–)	(–)	(–)
Percent change[b]	2.48	2.48	2.48	2.48	2.48
FFRT change[c]	–87.7	–113.0	–56.9	–90.9	–91.0
	(15.5)	(31.5)	(26.6)	(27.3)	(40.9)
Percent change[b]	–5.98	–7.70	–3.88	–6.20	–6.20
TRAN change[a]	447.8	–209.4	–734.2	122.8	1,098.7
	(170.0)	(601.0)	(822.3)	(788.4)	(896.7)
Percent change[b]	0.21	–0.10	–0.34	0.06	0.52

a. Cumulative net change, millions of dollars.
b. Based on average values over the estimation period.
c. Cumulative net change, basis points.
NOTE: Standard errors are in parentheses.
SOURCE: Authors' calculations.

order serial correlation from the equation residuals, with estimated first-order serial correlation coefficients of 0.02, -0.02, and 0.06 for *NBR*, *TRAN*, and *FFRT*, respectively.[14] The average contemporaneous residual correlation is 0.27 between *NBR* and *TRAN*, 0.10 between *FFRT* and *TRAN*, and 0.15 between *NBR* and *FFRT*. As mentioned earlier, the positive correlation between *NBR* and *FFRT* led us to adopt our somewhat arbitrary identifying restriction for the model. When structural parameters were determined using the lagged identifying restriction, the imposed coefficient was the "right" sign and "a reasonable order of magnitude" in all five cases (there is a separate model for each day of the week).[15]

III. Dynamic Behavior

We are concerned here with the magnitude, sign, and significance of the impulse-response functions of each endogenous variable with respect to an exogenous shock to both itself and the other endogenous variables. The contemporaneous effects follow directly from estimates of the a_{ij}'s computed by solving the sample analog of equations (7)-(9). The effect of exogenous shocks on future values of the endogenous variables does not follow as straightforwardly. However, given a solution to the contemporaneous relationships of (7)-(9), future effects could be computed by solving for the moving-average representation of the VAR structure in equations (4)-(6).

Below, we examine the reactions of the system to two different shocks. The first is the estimated impact of an unexpected change in nonborrowed reserves, and the second is the reaction of policymakers to a shock to money demand.

Response to a Change in Nonborrowed Reserves

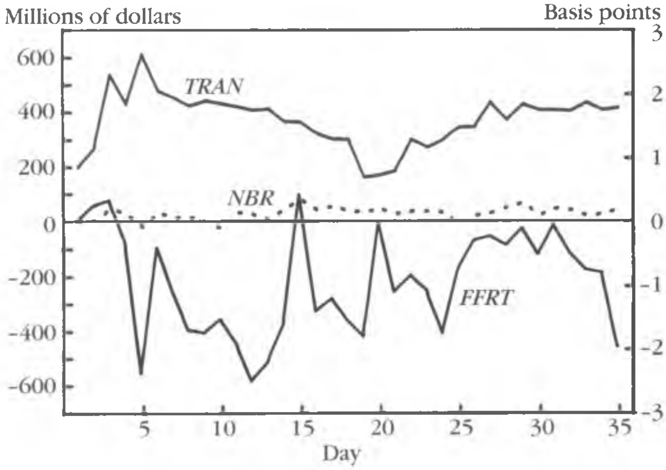
Figure 1 displays calculated responses of each variable to a *net* $1.0 billion positive shock to

■ 14 Higher-order serial coefficients were smaller in absolute magnitude than these, and none was statistically significant.

■ 15 The structural parameter estimates are not reported here because the impulse-response functions derived from them are, for purposes of this paper, more meaningful. The impulse-response functions are presented in the next section.

FIGURE 2

Cumulative Response to a $200-Million-per-Day Gross Shock to *TRAN* Maintained for Five Days



NOTE: Cumulative net change.
SOURCE: Authors' calculations.

NBR ($200 million per day for five days).[16] A net change was simulated to represent a shift in the weekly objective for *NBR* growth, which presumably would be represented by a net rather than a gross change in *NBR*. Because the model is first-differenced, the figures presented are cumulative moving averages. Approximate standard errors at intervals of one through five weeks are displayed in table 2. These were calculated using bootstrap methods, since analytic derivation would have been extremely difficult.[17]

As seen in the figure, *FFRT* responds quite *rapidly* and inversely to the change in *NBR*. In fact, the two-week response is greater than the five-week response. By five weeks, *FFRT* has declined more than 6 percent. Given the 2.4 percent change in *NBR*, this implies a short-run elasticity with respect to nonborrowed reserves of -2.5.

Initially, *TRAN* rises, fueled primarily by the contemporaneous correlation that is probably due to the open market operation itself. During weeks two and three, cumulative *TRAN* changes are actually negative, and it is not until week five that there is any appreciable increase. Even then, the implied "money multiplier" (ignoring currency and other transaction accounts) is only slightly larger than one. This suggests that most of the effects of an unexpected change in nonborrowed reserves were absorbed by changes in borrowing, excess reserves, or cash—not by changes in transaction accounts. To investigate this possibility, reduced-form models identical to those of *NBR, FFRT,* and *TRAN* (same independent variables and lagged endogenous variables) were run for excess and borrowed reserves. Assuming a recursive contemporaneous ordering, response functions similar to those shown in figure 1 were calculated for both variables. After three weeks, declines in borrowing and increases in excess reserves were estimated to total $1,187 million more than the injection of reserves. By week five, decreases in borrowing totaled $423 million and increases in excess reserves were estimated at $323 million. Together, these results imply that almost three-quarters of the reserve injection was absorbed by these short-term "buffers."

Feedback Effects: Response of Nonborrowed Reserves to a Change in Money Demand

The short-run reaction of the money market, particularly nonborrowed reserves, to a change in money may be examined with calculations similar to those utilized in the previous subsection. The difference is that in this case, a shock is exerted on *TRAN*, and feedback (on *TRAN*) is allowed. Figure 2 presents the results of a $200-million-per-day gross positive shock to *TRAN* maintained for five days.[18] For reasons that will become apparent below, the results of this simulation are displayed out to seven

16 The linearity of the model makes the size of the shock unimportant, since the values of the multipliers are independent of the shock's size. The shock simulated is one that offsets any implied feedback (even in the near future) and that provides an additional $200 million injection per day for five days. Thus, the shock is equivalent to a net injection.

17 The estimated covariance of the VARs was calculated assuming a three-equation system with contemporaneously correlated, but serially uncorrelated, errors. The resulting coefficient covariance matrix was used to generate 50 random multivariate normal coefficient vectors centered on the estimated parameter vector. Simulated moving-average responses were then derived for each of the random coefficient vectors (contemporaneous coefficients were also adjusted), and the sample standard deviation of the cumulative responses was calculated at each point in time. These estimates suggest that the cumulative moving averages are significantly different from zero.

18 Ceteris paribus, this is also a large shock and would increase *TRAN* by 25 percent if continued for one year with no feedback. With feedback, the shock would increase *TRAN* by 8.5 percent. We handle contemporaneous correlations the same way as in the *NBR* simulations. Neither *NBR* nor *FFRT* is assumed to react to contemporaneous *TRAN;* thus, there is no intraday feedback.

TABLE 3

Cumulative Response to a $200-Million-per-Day Gross Shock to *TRAN* Maintained for 5 Days

Variable	One Week	Two Weeks	Three Weeks	Four Weeks	Five Weeks	Six Weeks	Seven Weeks
NBR change[a]	−21.8	−27.0	80.2	37.1	−11.4	21.0	43.0
	(23.2)	(30.7)	(29.3)	(32.6)	(32.6)	(37.4)	(32.7)
Percent change[b]	−0.05	−0.07	0.20	0.09	−0.03	0.05	0.11
FFRT change[c]	−2.38	−1.52	0.43	−0.01	−0.74	−0.51	−1.96
	(1.27)	(1.67)	(1.81)	(1.83)	(2.16)	(1.82)	(2.12)
Percent change[b]	−0.16	−0.10	−0.03	−0.001	−0.051	−0.03	−0.13
TRAN change[a]	607.9	427.5	360.3	168.2	342.9	404.7	413.0
	(40.4)	(73.6)	(78.9)	(72.0)	(76.0)	(74.2)	(80.8)
Percent change[b]	0.28	0.20	0.17	0.08	0.16	0.19	0.19

a. Cumulative net change, millions of dollars.
b. Based on average values over the estimation period.
c. Cumulative net change, basis points.
NOTE: Standard errors are in parentheses.
SOURCE: Authors' calculations.

weeks. Approximate standard errors are given in table 3.

Without feedback, the gross change in *TRAN* would have been $1.0 billion. The data plotted in figure 2, however, show that at the end of five weeks the net increase is only $343 million. Thus, only about 35 percent of the gross increase in *TRAN* persists for five or more weeks. The path of this change is also of interest. After an initial increase, *TRAN* declines through week four and then begins to rise. The time path of *FFRT* is similar (though reversed in sign) to that of *TRAN*. After an initial decline, *FFRT* rises through week three and then starts to fall again. The decline in *FFRT* at the end of five weeks is somewhat surprising, although small and, as judged by estimated standard errors, apparently insignificant.

The most interesting results of this simulation are suggested by the *NBR* data. During the first two weeks of the positive money shock, the Federal Reserve withdraws reserves, perhaps in response to the initial decline in the funds rate. By the end of three weeks, however, $80 million of *NBR* has been injected. After five weeks, $11 million has been withdrawn, while a net addition of $43 million is observed at the end of seven weeks. This pattern of withdrawals and injections is roughly consistent with the changes in reserve demand that occur under lagged reserve requirements. In that case, an increase in money translates into greater reserve demand in the third week of these calculations (the second week after the monetary shock). Thus, the simulated pattern for *NBR*, reflecting the timing required by lagged reserve requirements, strongly suggests that under this system, the Federal Reserve did accommodate at least some of the increase in money.

An estimate of the extent of central bank accommodation may be computed as follows. Consider the $43 million net increase in *NBR* supplied by the end of week seven. Clearly, this would not support the total $1.0 billion shock to *TRAN*. However, the Federal Reserve never really observes the $1.0 billion increase, but sees only the net changes shown in figure 2. The appropriate procedure is to compare the permanent increase in *NBR* with the increase in required reserves resulting from the permanent increase in *TRAN*. Assuming that the shock to *TRAN* is the only shock to money, that all of the net increase in nonborrowed reserves goes to member banks, and that the marginal reserve requirement is the transaction account limit in effect over the estimation period (16.25 percent), then the data underlying figure 2 imply that the Federal Reserve

TABLE 4

Cumulative Response after Five Weeks to a One-Day, $1.0 Billion Net Shock to *NBR* Administered on Different Days

Variable	Thursday	Friday	Monday	Tuesday	Wednesday
TRAN change[a]	1,091.1	1,445.0	1,030.3	1,133.8	692.3
	(941.0)	(984.0)	(953.3)	(977.5)	(797.2)
Percent change[b]	0.51	0.68	0.48	0.53	0.32
FFRT change[c]	–74.0	–81.2	–95.3	–119.3	–89.4
	(42.3)	(43.9)	(43.9)	(45.1)	(37.3)
Percent change[b]	–5.0	–5.5	–6.5	–8.1	–6.1

a. Cumulative net change, millions of dollars.
b. Based on average values over the estimation period.
c. Cumulative net change, basis points.
NOTE: Standard errors are in parentheses.
SOURCE: Authors' calculations.

accommodated about 65 percent of the increase in required reserves during the sample period.[19]

As an additional test, a shock was simulated that was identical to that of figure 2 in week one, but negative and offsetting in week two (so that the cumulative change in *TRAN* was close to zero after two weeks). In this case, we estimated that the Federal Reserve would supply more than 100 percent of the reserves required (assuming a 16.25 percent reserve requirement) for the week-one shock. This suggests that the Fed may have been even more willing to accommodate money shocks when they appeared to be temporary.

Day-of-the-Week Effects

One of the premises underlying the use of the particular model forms employed in this paper is the view that causal relationships might have differed by the day of the week. The estimated model system allows us to test this premise.

To examine the importance of a given day, we performed calculations identical to those presented in figure 1, but with the shock applied to only one day of the week. Results are displayed in table 4. The five-week multiplier for *TRAN* ranges from a high of $1.45 per dollar of *NBR* for a Friday shock to a low of $0.69 for a Wednesday shock. The five-week interest-rate multiplier ranges from a Tuesday high of 1.119 basis points per million dollars of *NBR* to a low of -0.074 for a Thursday shock. To examine whether these differences are statistically significant, we performed "Wald-type" chi-square tests using the approximate covariance matrix of the five-week multipliers. Chi-squares testing the equality of daily coefficients were 3.46 for *TRAN* and 8.49 for *FFRT*, with only the latter significant at the 10 percent level. Thus, while the quantitative variation is large, it is difficult to tell decisively whether the daily variations are important.

IV. Conclusion

The short-run money multiplier for nonborrowed reserves appears, at least over the period considered here, to be quite small relative to its potential long-run value. The estimated short-run multiplier for total transaction accounts of 1.1 is only 18 percent of the long-run value of 6.2 implied by the highest reserve ratio in effect over the estimation period. In the short run, banks appear to accommodate almost three-quarters of a change in nonborrowed reserves by altering their holdings of excess reserves and borrowings. Thus, the size of the open market operation needed to achieve a desired change in money appears to be much larger in the short run than that needed to effect the same change in the long run.

19 Using weekly data for the October 1979 – October 1982 period and a somewhat different methodology, Spindt and Tarhan (1987) estimate virtually the same degree of accommodation in nonborrowed reserves. They suggest that "... of an increase in required reserves caused by an increase in money almost 2/3 were supplied in non-borrowed form and about 1/3 in borrowed form." (p. 113)

The Federal Reserve's short-run influence over the funds rate is considerably greater than that over money. The estimated short-run elasticity of the funds rate with respect to nonborrowed reserves is -2.5. This contrasts with an estimated short-run elasticity of transaction accounts with respect to nonborrowed reserves of 0.2. Taken together, these results suggest that (again over the time period considered) although a short-run change in nonborrowed reserves could quickly and substantially affect the federal funds rate, the induced change in money in the short run was much smaller. Thus, the Fed may have had to accept substantial interest-rate volatility in counteracting short-term shocks to money. Viewed from this perspective, the apparent Federal Reserve policy in 1979–82 of supplying about 65 percent of the increase in reserves needed to accommodate a short-run increase in money may have been prudent, since it helped to avoid an even larger increase in short-term interest rates.

Finally, over much of the period covered here, there was considerable debate about whether, given its reserves operating procedure, the Federal Reserve should have substituted a system of contemporaneous reserve requirements for the extant lagged reserves system. A contemporaneous system, it was argued, could have substantially improved short-run money control.

The results presented here suggest that, during the period under examination, depository institutions at least partially delayed their response to a money shock by two weeks—the exact timing implied by lagged reserve requirements. Specifically, a positive shock to money was estimated to lower the funds rate initially and then to put upward pressure on it in the second week after the shock. This suggests that contemporaneous reserve requirements would likely have accelerated the response of the funds rate to a change in money demand, since reserve demand would have responded contemporaneously rather than with a lag. However, the modest short-run interest elasticity of money estimated in this study suggests that the quicker response of the funds rate would not, ceteris paribus, have resulted in a substantial short-run reversal of the shock to money. Thus, it appears that while contemporaneous reserve requirements would likely have resulted in a modest improvement to short-run monetary control, the Federal Reserve would still have faced a rather sharp short-run trade-off between interest-rate volatility and monetary control.[20]

■ 20 Indeed, the Fed implemented a contemporaneous reserves system in February 1984, but only after switching to a borrowed reserves (interest-rate-smoothing) procedure.

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