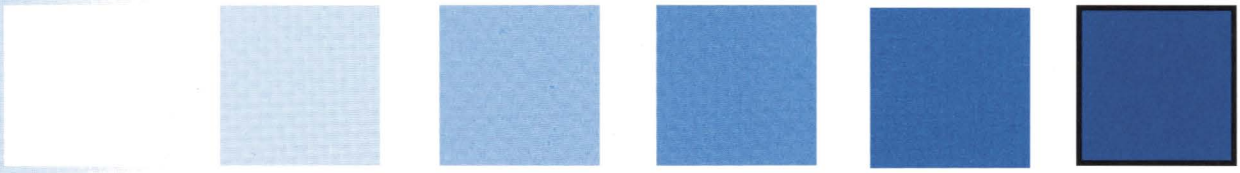
Federal Reserve Bank of St. Louis

REVIEW

NOVEMBER/DECEMBER 2007 VOLUME 89, NUMBER 6



Review is published six times per year by the Research Division of the Federal Reserve Bank of St. Louis and may be accessed through our web site: **research.stlouisfed.org/publications/review**. All non-proprietary and nonconfidential data and programs for the articles written by Federal Reserve Bank of St. Louis staff and published in *Review* also are available to our readers on this web site. These data and programs are also available through Inter-university Consortium for Political and Social Research (ICPSR) via their FTP site: www.icpsr.umich.edu/pra/index.html. Or contact the ICPSR at P.O. Box 1248, Ann Arbor, MI 48106-1248; 734-647-5000; **netmail@icpsr.umich.edu**.

Single-copy subscriptions are available free of charge. Send requests to: Federal Reserve Bank of St. Louis, Public Affairs Department, P.O. Box 442, St. Louis, MO 63166-0442, or call (314) 444-8809.

General data can be obtained through FRED (Federal Reserve Economic Data), a database providing U.S. economic and financial data and regional data for the Eighth Federal Reserve District. You may access FRED through our web site: **research.stlouisfed.org/fred**.

ISSN 0014-9187

The Decline in the U.S. Personal Saving Rate: Is It Real and Is It a Puzzle?

Massimo Guidolin and Elizabeth A. La Jeunesse

Since the mid-1990s, the national income and product accounts personal saving rate for the United States has been trending down, dropping into negative territory for three months during the past two years. This paper examines measurement problems surrounding two of the standard definitions of the personal saving rate. The authors conclude that, despite these measurement problems, the recent decline of the U.S. personal saving rate to low levels seems to be a real economic phenomenon and may be a cause for concern for several reasons. After examining several possible explanations for the trend advanced in the recent literature, the authors conclude that none of them provides a compelling explanation for the steep decline and negative levels of the U.S. personal saving rate. (JEL D10, E21)

Federal Reserve Bank of St. Louis *Review*, November/December 2007, *89*(6), pp. 491-514.

The national income and product accounts (NIPA) personal saving rate computed by the Bureau of Economic Analysis (BEA) includes households and other nonprofit institutions and entities (such as charities and churches), and it is calculated simply by taking the difference between disposable personal income (essentially, incomes of all kinds minus taxes) and personal consumption expenditures (outlays including non-mortgage interest payments), then dividing this quantity (i.e., personal saving) by disposable personal income (see Figure 1).[1]

In the past two decades, the widely reported NIPA personal saving rate for the United States has been trending down, dropping from averages of around 9 percent in the 1980s, to approximately 5 percent in the 1990s, to almost zero in the first years of the new century. Recent reports in the media have alerted the public that the U.S. saving rate, as currently measured, is at its lowest level since 1933, the bleakest year of the Great Depression. Of course, this historical comparison is disturbing at a minimum. Moreover, monthly data on household debt service payments as a percent of personal income have reached all time highs (see Poole, 2007).

The strongly declining trend in Figure 1 poses a number of problems. Taken at face value, a negative personal saving rate simply means that U.S. households are consuming more than their after-tax income allows them to. This tendency seems to be structural: For instance, the U.S. personal saving rate has remained persistently non-positive since April 2005. One naturally wonders whether it really can be true that the United States has become a spendthrift nation.

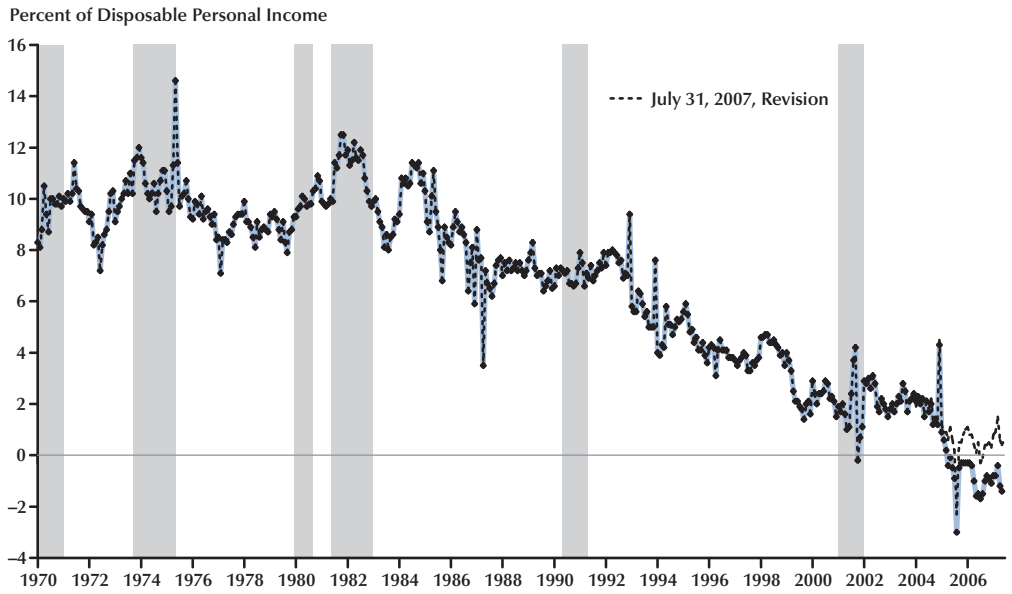
On a deeper level, many researchers and commentators have expressed a concern that the recent down-trending behavior of the U.S. personal saving rate may pave the way to a structural and persistent dependence of the U.S. economy on savings coming from foreign individuals and

[1] In Figure 1, the dotted curve represents the NIPA personal saving rate reported by the BEA after the revision of July 31, 2007.

Massimo Guidolin is an assistant vice president and Elizabeth A. La Jeunesse was a senior research associate at the Federal Reserve Bank of St. Louis. The authors thank Bill Gavin, Bill Poole, and Bob Rasche for comments and encouragement on previous drafts of this manuscript.

Figure 1

NIPA: Personal Saving as a Percent of Disposable Personal Income (monthly, SA)



NOTE: Shaded bars indicate recessions.

SOURCE: Bureau of Economic Analysis.

firms, in the form of structural current account deficits.[2] As argued by a number of authors (see Poole, 2005, for a review of the basic arguments), a situation in which the U.S. net international investment position keeps growing more negative as a percentage of gross domestic product (GDP) is inconsistent with long-run equilibrium: In such a situation, no debtor in the international financial market would be allowed to expand his position (as a percentage of output) without bounds. Because an adjustment is eventually inevitable, running a large current account deficit then becomes a risky strategy; hard landings—reductions of the international net debt position based on painful and disruptive adjustments in the domestic economy—may not be ruled out ex ante.

From simple macroeconomic principles, it is well known that the following accounting identity must hold at all times:

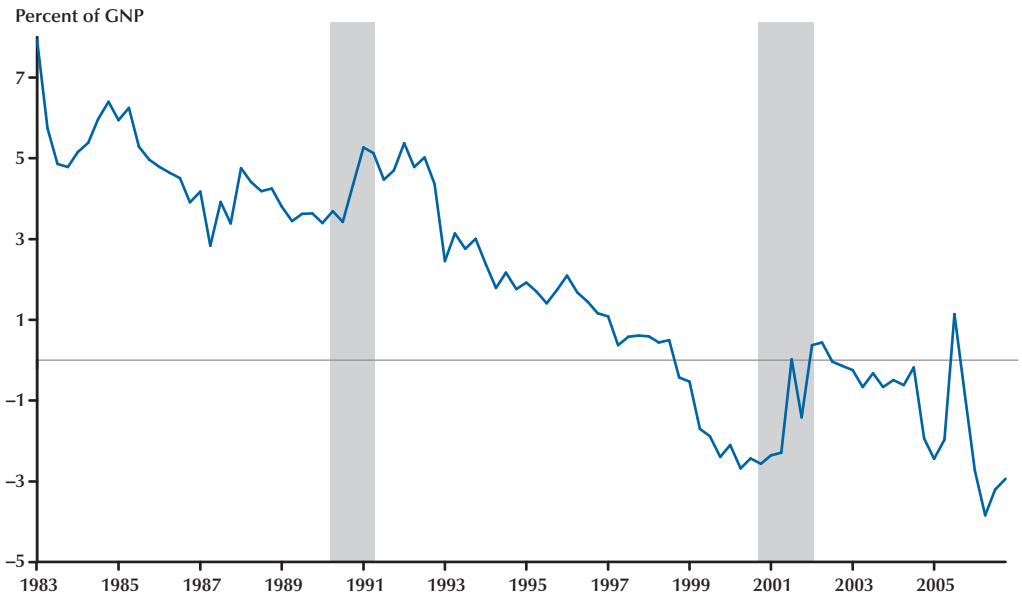
private gross investment = personal saving
+ business saving + net saving of the public sector
+ borrowing from foreigners (current account deficit)

In other words, a given level of investments (mostly by firms) may be financed by household savings, by public sector surpluses (when it collects more taxes than current expenditures and

[2] From Poole (2007): "Reports in the financial press have discussed the rapid accumulation of foreign exchange reserves by China, held mostly in U.S. dollars, and speculated on the impact on U.S. interest rates and the dollar exchange rate should the Chinese choose to diversify a significant fraction of such holdings out of dollars." According to economic theory, some uncertainty surrounds the relationship between running a large, persistently negative net international investment position and the future standard of living of the citizens of a country. In complete and frictionless markets, capital should simply flow toward the most productive uses, i.e. to projects with positive net present value and with the highest marginal return. Assuming that these projects systematically happen "to appear" within the U.S. borders, capital should keep flowing without any limits and this would raise the standard of living both in the United States and abroad. Of course, in reality, international capital markets are segmented and far from frictionless, and "states" (events) exist that—because large national economies are involved—are hardly insurable. All of these factors corroborate the contention that there are limits to the current account deficits that the United States may incur. For recent examples of papers that have discussed the notion of an optimal external debt ratio on the basis of frictions and market incompleteness, see, e.g., Fleming and Stein (2004) and Guimaraes (2007).

Figure 2

Private Saving Less Gross Investment as a Percent of GNP (quarterly, SA)



SOURCE: Bureau of Economic Analysis, Federal Reserve Board.

transfers), and by foreign investment. Of course, firms themselves may elect to retain some of their current earnings (profits) to finance future investments. Usually we assume that the public sector will not be able to set aside consistent savings, whereas according to simple logic, firms ought to be investing more resources than simply retained earnings.[3] This leaves only two other possible sources of funds to finance gross investments: personal saving and borrowing from abroad. Consequently, because we have argued that it is sensible to think that a country would want to avoid large current account (external) deficits for protracted periods (to avoid building up massive international debt positions),[4] it is usually considered healthy (sustainable) that, at least in the long-run,

$$\text{private gross investment} \leq (\text{personal saving} + \text{business saving}) \equiv \text{private saving},$$

i.e., that total private saving should at least cover total gross investment, or

$$\text{(1)} \qquad \text{private gross investment} - \text{business saving} \leq \text{personal saving}.$$

Given the presumption that the left-hand side will be positive most of the time, it is obvious that this inequality cannot be satisfied when personal saving turns negative for long periods of time. In fact, Figure 2 shows that, since 1999, private gross investment has systematically exceeded private saving. Moreover, at the end of 2005, the U.S. net international investment position was reported to be over –20 percent of out-

[3] This does not mean that the saving of the public sector cannot be positive, although it usually tends to be limited. For instance, between 1980 and 2006 the average ratio between public sector savings (budget surpluses) and GNP has been –2.2 percent. Additionally, the recent debate on the future of the Medicare and Social Security programs implies that most experts predict large and growing federal budget deficits (negative savings of the public sector) for a few decades to come.

[4] Using Gale and Sabelhaus's (1999, p. 182) wording, this "breeds increasing dependence on fickle foreign capital."

put, another all-time low that has attracted some further concerns and a heated debate (see Poole, 2005).

As recently stressed by Garner (2006), the risk of an increased dependence from foreign savings is not the only reason for concern. Although the aging trend of the U.S. population is a long-range one, all recent projections tell us that the share of the population 65 and older is destined to rise at a much faster pace than in the past, as the postwar Baby Boom generation ages. This trend, together with increasing medical costs in real terms, is likely to produce increasing liabilities for Social Security and Medicare programs (see Hakkio and Wiseman, 2006). This means that exactly when the United States will most need portions of its population to rely on their own personal savings to relieve the pressure on the federally funded programs, a likely saving crisis may make resources for financing investments dramatically scarce.[5]

Finally, especially during 2005, the financial press has often called attention to the existence of retrenchment risk in consumer spending, which might suddenly lead the U.S. economy into a recession. The concern is that—should the current personal saving rate be too low to be consistent with sound long-run household plans—a *sudden* correction of consumption habits may translate into a substantial reduction in consumption expenditure and therefore aggregated demand. This may impose an undesirable uncertainty for the optimal course of monetary policy.[6]

In this article we ask three separate questions. In the first section we ask whether the decline in the U.S. personal saving rate is real or a simple statistical artifact due to measurement problems. In particular, we review and discuss pros and cons of two standard definitions of the personal saving rate. Because the decline manifests itself in all standard measures and cannot be easily explained by measurement issues, our conclusion is that yes, the decline of the U.S. personal saving rate seems to be a real phenomenon worthy of further attention. In the second section, we ask whether one should worry about the recent downward-trending U.S. saving rate. Our results are ambiguous. We find there are potentially legitimate reasons for concern: For instance, after the mid-1990s, the tendency of non-financial corporations to retain a growing fraction of their earnings has failed to fully compensate the decline in household savings. We also find reasons to suspend an immediate judgment: For instance, similar declines have been recorded in a number of other countries, such as Canada and Australia. In the third section we ask whether economic research has developed any solid understanding of the recent dynamics of the U.S. saving rate. After reviewing a number of arguments and theories that have been proposed, we conclude that the recent decline and negative values of the U.S. private saving rate remain a puzzle.

IS THE DECLINE REAL? MEASUREMENT ISSUES

There are two basic sources of calculated values for the personal saving rate: the NIPA estimates from the BEA and the estimates of the changes in personal net wealth that can be computed from the flow of funds (FoF) accounts maintained at the Board of Governors of the Federal Reserve System (BOG). Although both measures tend to receive some press coverage and are routinely cited in the economic debate, the NIPA estimates have recently enjoyed a great deal of attention in the financial press because—as shown in Figure 1—they turned negative during 2005. In what follows, we describe both measures, stressing their advantages and disadvantages. Generally, there are a number of reasons to think that both the NIPA and FoF measures provide an often-biased or, at best, incomplete representation of the saving behavior of U.S. households.

[5] Standard life-cycle consumption models imply a declining saving rate over an agent's lifetime; i.e., youngsters should display high saving rates used to cumulate savings that go to finance negative saving rates (dis-saving) after retirement. As a result, as the overall population ages, the aggregate saving rate is likely to decline.

[6] Garner (2006) reports some back-of-the-envelope calculations by which a simple 1-percentage-point increase in personal savings would cause an annualized, same-quarter decline of 2.8 percent in real output. A word of caution is in order: Empirical research has so far failed to provide clear results on the causal links between saving rate dynamics and economic recessions. See Steindel (2007) for empirical evidence on this tenuous link.

One way to accurately define some of the criteria used and assumptions made by the BEA and the BOG when they compute the personal saving rate is to start from the basic budget constraint of a standard representative consumer:

$$(2)\quad W_{t+1} = (1+r_{t+1})W_t + L_{t+1} - C_{t+1} - \tau\left[r_{t+1}W_t + L_{t+1}\right],$$

where W_t is tradable net wealth (financial and real, e.g., including stocks, bonds, check deposits, and housing); r_{t+1} is the overall (before tax) rate of return on wealth (e.g., capital gains, dividends, coupons, rents received from owned houses) over the interval; L_t is labor income; C_t is current consumption (personal outlays); and τ is the (average) tax rate, assumed to be constant for simplicity. $\tau r_{t+1}W_t$ corresponds, then, to the taxes paid on capital gains (notice, both realized and unrealized), while τL_{t+1} are the taxes paid on labor income.[7] Notice that W_t is wealth net of debt and obligations (also called net worth). Equation (2) implies

$$(3)\quad W_{t+1} - W_t = r_{t+1}(1-\tau)W_t + (1-\tau)L_{t+1} - C_{t+1},$$

That is, changes in wealth must equal the difference between net disposable (after tax) income and consumption. Crucially, the left-hand side of (3) corresponds to a FoF definition of personal saving, while the right-hand side corresponds to a definition based on the difference between income and demand flows (disposable income and personal outlays). In an ideal, frictionless world with no measurement errors or problems with accounting definitions, the NIPA and FoF definitions would perfectly agree, just because the left- and right-hand sides of (3) coincide by construction. In the following, we discuss what in reality may cause the two definitions to differ, as well as the pros and cons of each definition.

[7] This equation, which transforms differences of flows into stocks, is obviously a simplified description that abstracts from many important practical details. For instance, it is clear that capital gains may be taxed at a rate different from labor income; in reality, only realized capital gains (besides dividends, coupons, and rents) are taxed; households may receive and/or pay transfers to the public sector, etc. However, for the purpose of describing differences between NIPA and FoF definitions, this equation will do. Many of the simplifying assumptions will be removed later on.

The BEA defines the personal saving rate as the ratio of (i) the difference between disposable personal income and current consumption and (ii) disposable personal income (the right-hand side of (3)) divided by disposable personal income. Note that this focus on flows of personal income and outlays has the potential to create a number of accounting discrepancies: Disposable income and personal outlays are two series that are collected from distinct bodies of data. Income data are collected from payroll data, Internal Revenue Service income tax filings, and corporate profit reports. Personal outlays derive almost entirely from personal consumption expenditures, i.e., the data that come from the revenues of retailers and service suppliers (such as hospitals and hotels. The more complete and reliable data are those concerning the demand (consumption) side, whereas income data are notoriously imprecise, for instance, typically failing to add up to aggregate GNP by as much as 2 to 3 percent (the so-called statistical discrepancy). This means that income is usually underestimated, which suggests that NIPA saving rates may be subject to (i) substantial measurement error and (ii) frequent, major revisions as income data are progressively revised.

Besides these general limitations of the standard BEA and NIPA saving rate measures, a number of statistical and measurement issues have been debated in the literature on the U.S. saving rate evolution. The literature on the subject is rather voluminous. We choose to focus on at least four distinct aspects that may cause the measured NIPA personal saving rate to substantially differ from the true, unobserved personal saving rate.[8]

NIPA Measures of the Personal Saving Rate and (Realized) Capital Gains

Distortions are likely to be caused because NIPA conventions exclude (realized and unrealized) capital gains from disposable income but include taxes on the realized capital gains in the same definition of disposable income. Using the notations in (3), this means that the BEA measures

[8] See, for instance, Garner (2006), Peach and Steindel (2000), and Reinsdorf (2004).

disposable income as $(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t$ and not as $(1-\tau)L_{t+1} - r_{t+1}(1-\tau)W_t$, where $\rho_{t+1} \neq r_{t+1}$ is the *realized* rate of return on wealth (i.e., inclusive only of realized, actual capital gains that have been transformed into cash). The difference is given by the term

$$r_{t+1}W_t + (\rho_{t+1} - r_{t+1})\tau W_t, \tag{4}$$

which may be sometimes substantial and—even when $\rho_{t+1} = r_{t+1}$, that is, all capital gains are realized—never disappears as long as $r_{t+1} \neq 0$. Formally, this means that while the NIPA personal saving rate is measured to be

$$s_{t+1}^{NIPA} = \frac{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t - C_{t+1}}{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t},$$

the true (but unobserved) rate should be

$$\hat{s}_{t+1} = \frac{(1-\tau)L_{t+1} - r_{t+1}(1-\tau)W_t - C_t}{(1-\tau)L_{t+1} - r_{t+1}(1-\tau)W_t}.$$

A few straightforward manipulations show that the unobservable personal saving rate can be written as

$$\hat{s}_{t+1} = \underbrace{\frac{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t}{(1-\tau)L_{t+1} + r_{t+1}(1-\tau)W_t}}_{\kappa_{t+1}^0} \left(s_{t+1}^{NIPA} + \underbrace{\frac{r_{t+1}W_t + (\rho_{t+1} - r_{t+1})\tau W_t}{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t}}_{\kappa_{t+1}^1} \right), \tag{5}$$

$$\hat{s}_{t+1} \simeq s_{t+1}^{NIPA} + \kappa_{t+1}^1.$$

For reasonable values of the quantities involved—essentially, when labor income represents a non-negligible fraction of total initial net worth for households and for plausible tax rates because the coefficient $\kappa_{t+1}^0 < 1$ will be relatively close to 1, but less than 1, while κ_{t+1}^1 will be positive—$\hat{s}_{t+1} > s_{t+1}^{NIPA}$ follows.[9] This means that, provided that the average tax rate and (realized and total) rates of return on assets are not too large, the NIPA personal saving rate is bound to systematically underestimate the true personal saving rate. Approximating (5) for the simple case in which $\rho_{t+1} = r_{t+1}$ and $\rho_{t+1}\tau \simeq 0$, we obtain

$$\hat{s}_{t+1} - s_{t+1}^{NIPA} \simeq \frac{r_{t+1}W_t}{(1-\tau)L_{t+1}},$$

which shows that the difference between the "true" saving rate and the NIPA estimate is proportional to the total capital gains of the economy. Figure 3 provides a description of the behavior of this quantity over time (as a percentage of disposable personal income) and illustrates the potential for substantial underestimation of the saving rate using NIPA accounts.

A few economists have taken issue with this broad definition of a "true" saving rate, $\hat{s}_{t+1}$, arguing that only *realized* capital gains should be considered. Three motivations are offered. Unrealized capital gains should not be included in the definition of saving as they simply represent returns on past saving activity, which has already been accounted for. In many cases, simple appreciation of existing assets (e.g., houses) fails to create new productive assets. The fact that unrealized gains fail (by definition) to be transformed into cash resources that allow households (or other agents that borrow from households) to acquire physical, productive capital stock should (consistent with current BEA practices) dissuade analysts from using capital gains altogether. Furthermore, it has been observed that a large portion of unrealized capital gains tends to arise in the presence of volatile "bubbling" conditions (e.g., the stock market boom of the late 1990s and possibly the housing price surge of 2002-05); as such, these gains have to remain unrealized almost by definition—if households tried to cash them in, they would cause the bubble to burst, causing the capital gains to vanish.[10] Therefore it is debatable whether such unstable components should be

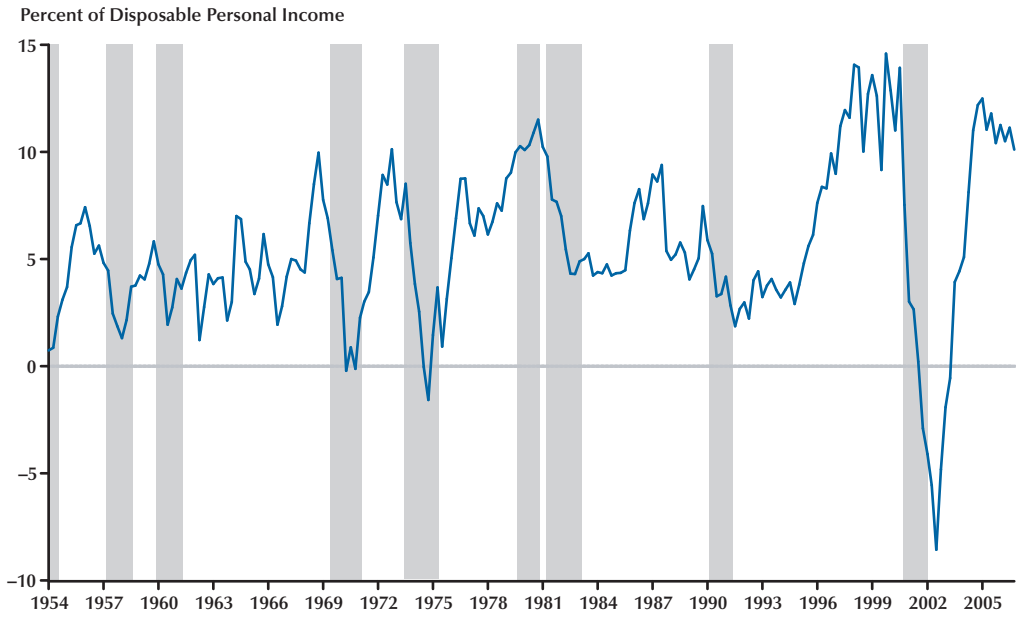
[9] This happens because r_{t+1} will normally exceed $(\rho_{t+1} - r_{t+1})\tau$. For instance, for plausible values such as $\tau = 0.25$, $r_{t+1} = 0.1$, $\rho_{t+1} = 0.05$, and $\kappa_{t+1}^1 > 0$ reduces to

$$\frac{0.1W_t + 0.0125W_t}{0.75L_{t+1} - 0.0125W_t} = \frac{0.1125}{0.75\frac{L_{t+1}}{W_t} - 0.0125} > 0 \Rightarrow \frac{L_{t+1}}{W_t} > 0.017.$$

[10] Notice, however, that—assuming efficient credit markets with modest transaction costs—capital gains do not need to have been realized to cause an increase in personal outlays: Unrealized capital gains may be used as collateral to support additional borrowing.

Figure 3

Total Capital Gains (Losses) as a Percent of Disposable Personal Income (eight-quarter moving average)



SOURCE: Federal Reserve Board.

considered as part of private saving. Third, in the empirical literature, considerable debate persists as to what fraction of such unrealized capital gains might be actually increasing saving (the complement of the so-called "wealth effect" on consumption).[11] When only realized capital gains are considered, the true (but unobserved) personal saving rate is defined as

$$\tilde{s}_{t+1} = \frac{(1-\tau)L_{t+1} + \rho_{t+1}(1-\tau)W_t - C_t}{(1-\tau)L_{t+1} + \rho_{t+1}(1-\tau)W_t},$$

with the implication that

(6)
$$\tilde{s}_{t+1} = \underbrace{\frac{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t}{(1-\tau)L_{t+1} + \rho_{t+1}(1-\tau)W_t}}_{\kappa^0_{t+1}} \left(s^{NIPA}_{t+1} + \frac{\rho_{t+1}W_t}{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t} \right).$$

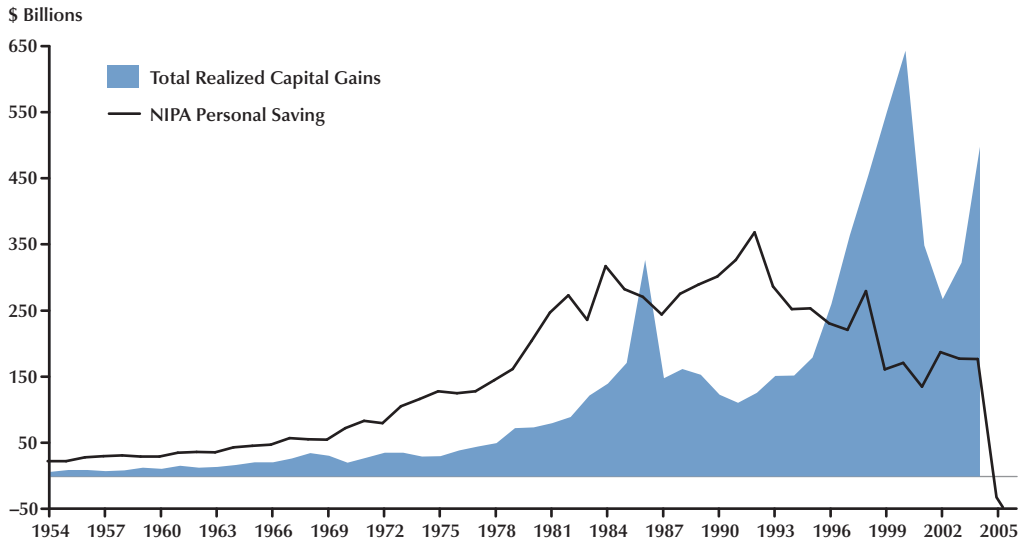
For reasonable values of the quantities involved, one can show that $\tilde{s}_{t+1} > s^{NIPA}_{t+1}$. Once more, the NIPA personal saving rate will systematically underestimate the true personal saving rate. As a first approximation, the amount of the bias is increasing in (proportional to) both the amount of realized capital gains, $\rho_{t+1}W_t$, and in the amount of taxes paid on the realized capital gains.[12] Figures 4 and 5 show that the recent decline in the measured NIPA saving rate occurred simulta-

[11] In the empirical literature, estimates are rather heterogeneous. Among many others, Poterba (2000) reports a tiny 3 percent elasticity of consumption to wealth, while Parker (2000) finds 4 percent. Ludvigson and Steindel (1999) report that the elasticity is small and the effect quickly dies out after one quarter. Such low estimates of the elasticity of consumption to wealth imply that most of the unrealized capital gains might be converted into savings. On the other hand, Juster et al. (2006) found a massive 19 percent elasticity for stock price increases, although the overall effect of wealth increases is consistent with the standard 3 percent in the literature. Therefore the impact of capital gains on saving might be much higher for housing (and other assets) than it is for equities.

[12] Formal differentiation shows that the derivatives versus the tax rate and the realized capital gain rate of the term that is added to s^{NIPA}_t inside the parenthesis are both positive.

Figure 4

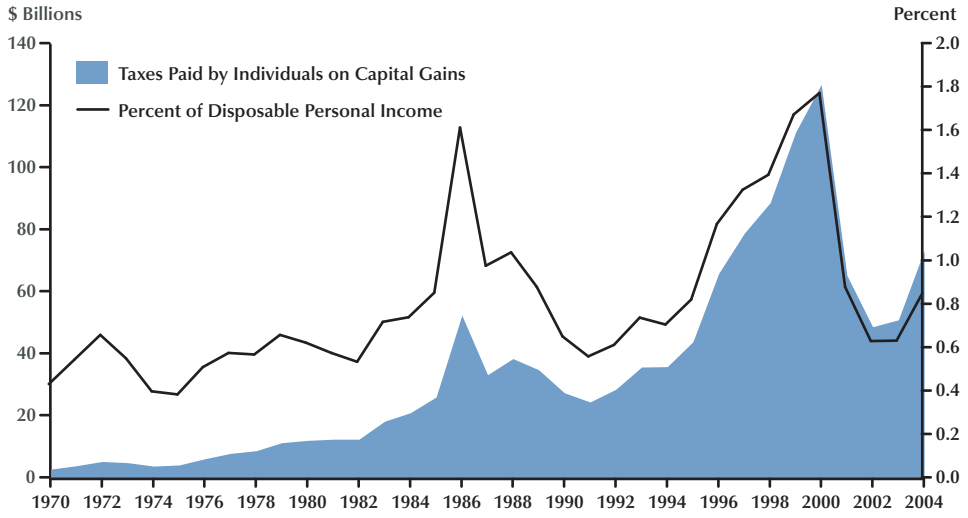
NIPA Personal Saving and Total Realized Capital Gains (annual)



SOURCE: Bureau of Economic Analysis, U.S. Treasury.

Figure 5

Capital Gains Tax Receipts Excluded from NIPA Disposable Personal Income (annual)



SOURCE: Bureau of Economic Analysis, U.S. Treasury.

neously with high realized capital gains and high capital gains taxes. Therefore a possibility exists that a substantial portion of the recent decline may be simply imputed to increasing biases (underestimation) in NIPA measures. Moreover, the capital gains issue is likely to become increasingly important not just because stock market gains have been substantial in recent years, but also because companies are using more and more share repurchases (and not cash dividends) to distribute profits to the shareholders. Share repurchases tend to increase stock prices, yielding capital gains to shareholders that do not appear in personal income. If companies increasingly use share repurchases instead of dividends—which seems to characterize recent data—the result would be to create a growing downward bias in the measured NIPA saving rate.

Notice, however, that the most recent dramatic dip in the measured NIPA saving rate (during 2005) corresponds to a decline in the taxes paid on realized capital gains and—absent any major fiscal reform—in the realized capital gains themselves. In summary, although the NIPA measure of the personal saving rate is likely to underestimate the true, unobservable rate by a few percentage points, and some logical inconsistencies exist in the NIPA treatment of capital gains, it is difficult to conclude that these discrepancies entirely explain the declining trend in the NIPA measure or—especially—the negative saving rates that have been reported during 2005.

NIPA Measures of the Personal Saving Rate and Pension Schemes

A second, obvious flaw of NIPA measures of the personal saving rate is that the methodological criteria of the BEA exclude pension benefits received as disposable income, but deduct from personal disposable income the contributions paid into pension funds. Call the net pension benefits npb_{t+1}, defined as the difference between gross benefits (transfers) received (pb_{t+1}) and contributions (pc_{t+1}), $npb_{t+1} \equiv pb_{t+1} - pc_{t+1}$. Then calculations similar to those performed above show that, although the NIPA personal saving rate is calculated as

$$s_{t+1}^{NIPA} = \frac{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t - pc_t - C_{t+1}}{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t - pc_t},$$

the true but unobserved personal saving rate is

$$\tilde{s}_{t+1} = \frac{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t - pc_t}{(1-\tau)L_{t+1} + \rho_{t+1}(1-\tau)W_t + npb_{t+1} - C_t} \left(s_{t+1}^{NIPA} + \frac{\rho_{t+1}W_t + pb_{t+1}}{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t - pc_t} \right).$$

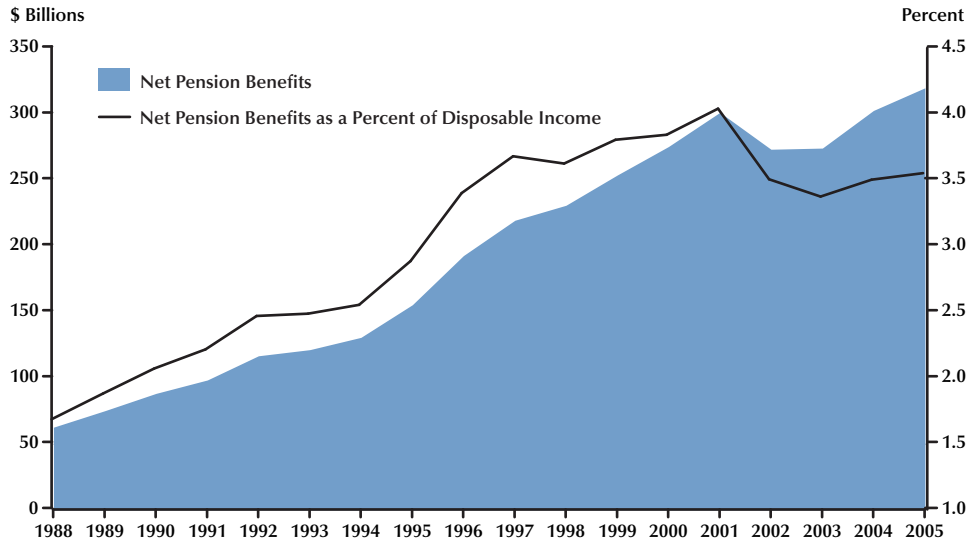
Once more, if the ratio that precedes the sum in parenthesis is approximately 1, then, because $\tilde{s}_{t+1} > s_{t+1}^{NIPA}$, the NIPA rate will systematically underestimate the actual saving rate. Figure 6 shows that the amount of net pension benefits received by U.S. households has substantially increased (as a percentage of the NIPA personal disposable income) since the mid-1990s, peaking at roughly 4 percent in 2001. As a result, it is likely that a portion of the downward-trending NIPA estimate of s_{t+1} may be due to omitting pension benefits, although the quantitative relevance of the bias is probably of second-order importance. For instance, a quantitative estimate of the term $pb_{t+1}/(1-\tau)L_{t+1}$ as of the end of 2005 was approximately 14 percent.[13]

Another, different issue concerns the way in which the BEA treats defined *benefits* (DB) pension plans when computing the personal saving rate. NIPA estimates treat defined *contribution* (DC) plans in a perfectly consistent way: Because the employee directly owns the assets and retains a substantial amount of control, it seems correct for NIPA to include employers' contributions and capital gains and income as personal income and to consider the plan's administrative expenses as personal outlays. With DB plans, however, employers make the investment decisions and bear the investment risks. Moreover, DB plans can be a source of cash flows only upon retire-

[13] Notice that NIPA's treatment of IRAs and 401(k) plan contributions, for example, is perfectly consistent: Because these defined contributions are not part of personal outlays (and, therefore, must be included in the difference between personal income and personal outlays), they are correctly included in national saving computations.

Figure 6

Net Pension Benefits (annual)



SOURCE: Bureau of Economic Analysis, U.S. Treasury.

ment and potential plan surpluses generally fail to be passed on to the employees to increase their pension benefits. These latter two features would suggest that DB plans should be considered in NIPA estimates of the personal saving rate, yet they are. In principle, if one thinks that in recent years DB plans have generated large net losses to households (i.e., that the employers' contributions have been modest relative to capital losses and administrative expenses), excluding DB pension plans from NIPA calculations may increase the measured personal saving rate over the actual (unknown) rate. A further issue is that, although investment income on DC plans is treated as personal income, payments out of both DC and DB plans are not. However, such payments are subject to income taxes and these taxes reduce measured personal disposable income—and hence the saving rate—at the time the retirement benefits are paid.

We therefore compute a modified NIPA saving rate that excludes DB pension plan-implied income and outlay components. First, we remove from personal income the employer contributions to DB plans as well as rental income, dividends, and interest; second, we add to personal income the benefits paid by DB plans net of employee contributions; and third, we remove from personal consumption expenditures the administrative expenses of DB pension. Figure 7 shows the results. There are two obvious implications. First, excluding DB plans generates quantitative implications of second-order importance. Second (and more important), when DB incomes and outlays are excluded, the implied personal saving rate is actually even *lower* than the official rate reported by the BEA.[14]

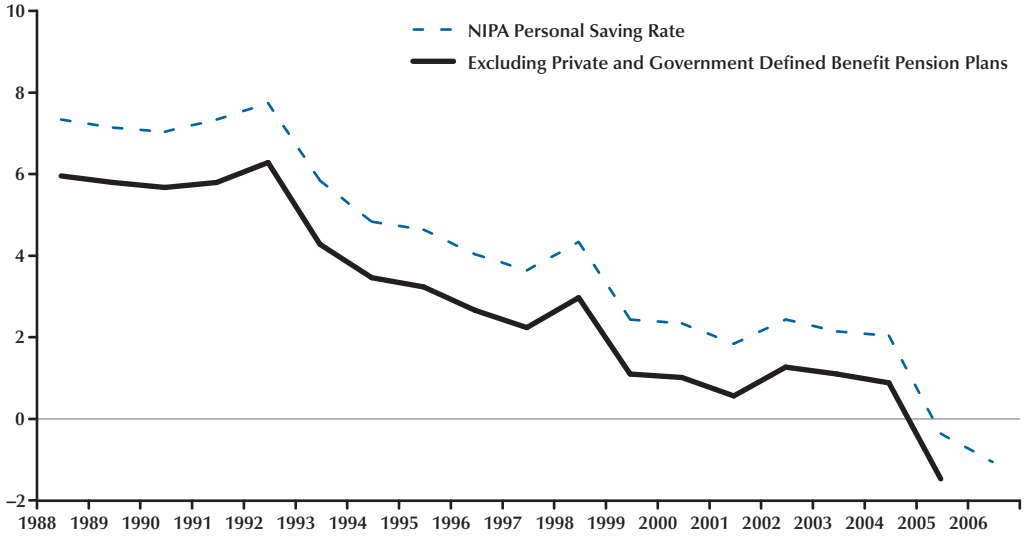
Other Issues with the NIPA Measures of the Personal Saving Rate

Economists and the financial press have focused on a few other accounting issues in their attempt to make sense of the recent decline (to negative territory) of the U.S. personal saving rate. First, the BEA's choice to consider net acquisitions

[14] Reinsdorf (2007, p. 9) reaches similar conclusions with data up to 2005.

Figure 7

Alternative Personal Saving Rate with Defined Benefit Pensions Excluded (annual)



SOURCE: Bureau of Economic Analysis, Federal Reserve Board, author's calculations.

of consumer durable goods by households as personal consumption expenditures has been a cause of dissatisfaction: At least a portion of household purchases of durable goods (e.g., cars) have many features of an investment decision and increase the stock of physical capital that produces services over time. Of course, if we define the true personal saving rate as (notice that this ignores many issues already discussed)

$$\breve{s}_{t+1} = \frac{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t - \left(C_{t+1} - C_{t+1}^{DUR}\right)}{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t},$$

where C_{t+1}^{DUR} is durable consumption, it is clear that

$$\breve{s}_{t+1} = s_{t+1}^{NIPA} + \frac{C_{t+1}^{DUR}}{(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t},$$

which implies $s_{t+1}^{NIPA} > \breve{s}_{t+1}$. The amount by which the true saving rate is underestimated depends on the ratio between consumption of durables and personal disposable income. Figure 8 shows the behavior of such a ratio over time.

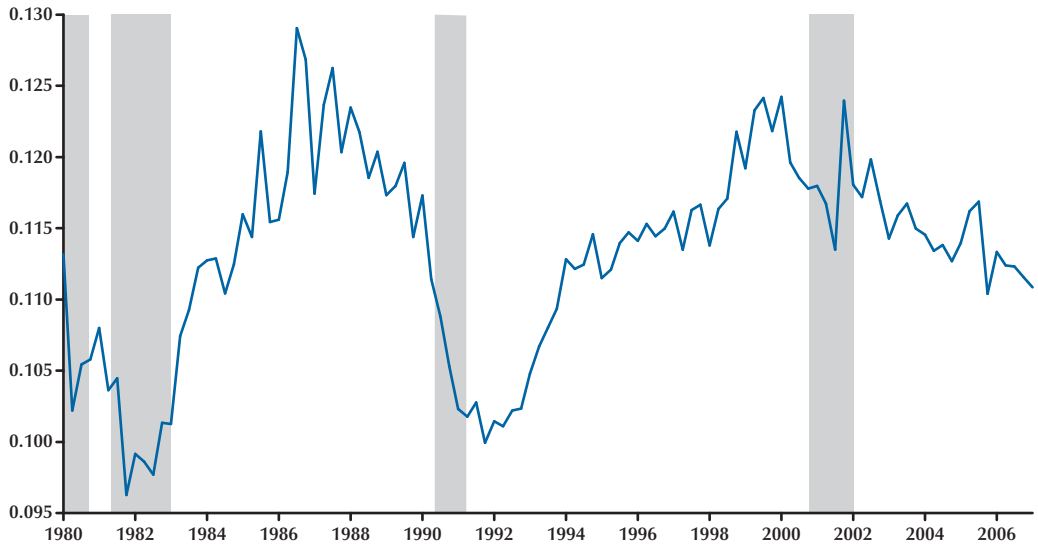
On the one hand, Figure 8 stresses that the ratio

$$C_{t+1}^{DUR} / \left[(1-\tau)L_{t+1} - \rho_{t+1}\tau W_t\right]$$

is quantitatively important. In fact, if computing the personal saving rate on the basis of durables only were the correct choice, then the reported personal saving rate could be at least 10 percent higher. On the other hand, Figure 8 reveals that the ratio between durables and personal disposable income has not changed much over time—it has constantly oscillated between 9.5 and 13 percent—and as such it cannot be responsible for the recent downward trend in the measured personal saving rate (see also Parker, 2000, for similar conclusions). With multiple possibilities, it's unclear what the "victory" would be. Notice, too, that if this treatment of durable consumption goods has the ability to shift up measured saving behavior of U.S. households by approximately 10 percent, then personal expenditures on durables should then be considered as a form of private, gross investment. If, however, as noted earlier, we believe that there is evidence that private gross saving might be currently insufficient in the United States, moving some items from con-

Figure 8

Ratio of Personal Consumption Expenditure of Durable Goods to Disposable Personal Income (quarterly, SAAR)



SOURCE: Bureau of Economic Analysis.

sumption to investments cannot solve the problem because the intervention raises both the left- and right-hand sides of the basic accounting identity in (1).

Commentators have also taken issue with the way in which the BEA defines the notion of "personal" sector. In principle, such a sector ought to include households and nonprofit institutions serving households (e.g., churches and charities, also called NPISH). NIPA methodological guidelines, however, do not consistently use this definition. For instance, bequests or gifts to charities are considered as personal outlays (and therefore reduce the reported saving rate) in standard NIPA accounts, although they should not be. The opposite happens when households receive transfers from NPISH. Obviously, as long as transfers by households to and from NPISH approximately balance out, no relevant bias will affect the reported saving rate. In fact, for a long time this has been approximately the case. Even though recent years have seen households increasing their transfers to NPISHs, Reinsdorf (2007) shows that the effect on the personal saving rate is marginal (0.6 percent between 1997 and 2002 and 0.2 after 2007).

Finally, other measurement issues that have been discussed (see, e.g., Reinsdorf, 2004, for additional details) are the use of nominal versus real interest rates in NIPA calculations of net interest payments by households, the treatment of real estate "closing" costs, and the nature of education expenditures. Perozek and Reinsdorf (2002) recalculate personal disposable income by replacing nominal personal interest income with real interest income (i.e., excluding the inflation premium, which purely compensates for the loss in value of existing assets). The idea is that saving should allow financing of capital accumulation in real terms and not simply serve as protection from inflation. However, this adjustment implies an overall downward adjustment of the personal saving rate (e.g., between 0.5 and 1.2 percent between 1993 and 2000) and fails to explain the recent, puzzling trend. It is also uncertain whether real estate closing costs (to purchase

or mortgage a residence) should be considered as current personal outlays or as investments. Reinsdorf (2004) concludes that the impact is quantitatively marginal. Education expenditures by households are treated by NIPA as current consumption expenditures, even though they obviously increase the stock of human capital. Their exclusion from personal outlays would increase measured personal saving but also increase gross private investments, which also does not seem to solve the puzzle under investigation.

Recently, NIPA revised its policies concerning the way in which stock options are treated. Currently, stock options are treated in a manner consistent with IRS practices: When exercised, options generate wage incomes to recipients and expenses to corporations; however, holdings of stock options fail to generate (non-realized) capital gains before the option is exercised. This clearly creates a potential for understating saving relative to the perceptions of option holders. Yet, the NIPA measure of business profits usually fails to include stock options as a potential expenditure before expiration, and this also leads to the systematic inflation of the estimates of business saving, with compensating effects.[15] In any event, the NIPA accounts show that total deferred compensations to workers (of which stock options are just one example) accounts for at most 0.3 percent of personal income, and therefore hardly explains the recent, major swings in the saving rate.

The FoF Measure of the Personal Saving Rate

Estimates of the assets and liabilities of the personal sector are available in the FoF accounts of the Federal Reserve BOG. These accounts also provide estimates of holding gains and losses for assets such as real estate and corporate equities, including assets held indirectly through mutual funds, pension funds, and life insurance contracts. The main competing method to estimate the saving rate can be derived from the FoF accounts published by the BOG. In essence, we use FoF data to estimate the left-hand side of (3). According to this FoF definition, the personal saving rate is simply given by the ratio between the change in the *net* wealth (net worth) of U.S. households and their disposable income. Ideally, the change in the value of the assets owned by U.S. households (net of their debt and obligations) should be measured applying current market prices.[16] Figure 9 compares this alternative notion of the personal saving rate with the standard NIPA estimate.[17]

Although over the sample period 1954-2006 the two alternative measures provide rather different averages (11.3 percent for FoF statistics vs. 7.3 percent for NIPA), their time-series behavior is quite similar; by the end of 2005, the FoF personal saving rate also dips below 2 percent.[18] Therefore it is clear that, although one feels compelled to provide an explanation for the recent dynamics of the saving rate when the FoF definition is also adopted, it does not appear that a difference between –1 percent (based on NIPA calculations) and +1.3 percent (based on FoF calculations) at the end of 2006 is economically meaningful: The saving rate of U.S. households appears to be currently low and to have quickly trended down after the mid-1990s.

What is the intuition for the finding that the FoF and NIPA estimates of the personal saving rate have been approximately identical (and small) after the turn of the century? In principle, this is a moot question because (3) tells us that the two measures should in principle give identical results. In practice, this is an interesting question because it should be obvious that, when calculating the quantities involved, both the BEA

[15] After 2003, the BEA began to incorporate stock-option adjustments in corporate profit estimates for the periods that are treated using public financial reports. For example, the extrapolated corporate profits estimates for 2002 and 2003 have been revised and—because the gains on exercised stock options declined from 2001 to 2002—the result has been an increase in the BEA's estimate of corporate profits for 2002.

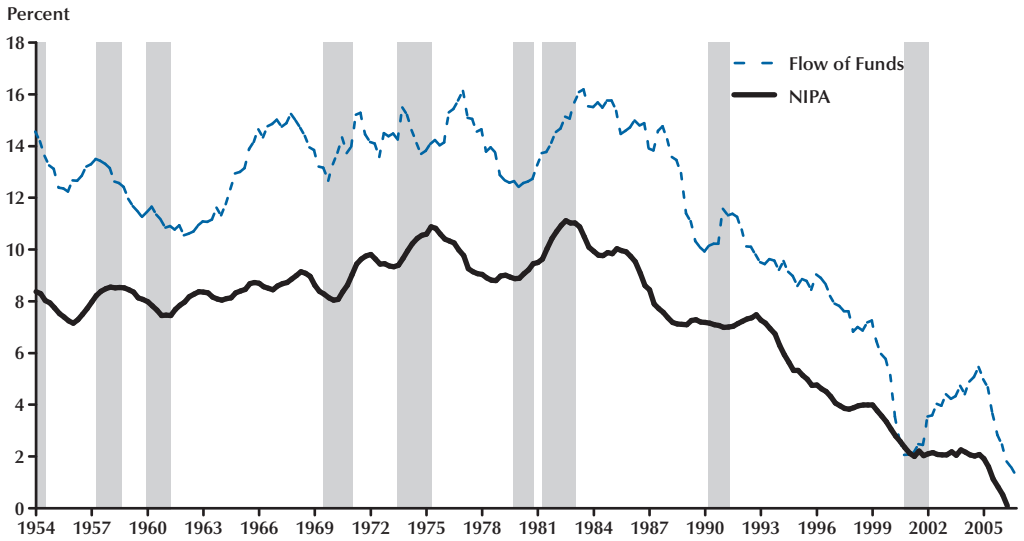
[16] However, debt instruments, such as bonds, are carried at book value in the FoF accounts, so they are excluded from the calculations of holding gains and losses.

[17] To avoid devoting too much attention to high-frequency movements (induced by asset prices) that lack much economic meaning, we report eight-quarter moving averages of the seasonally adjusted FoF quarterly series.

[18] The correlation between the two series is in fact almost perfect, 0.96.

Figure 9

Personal Saving as a Percent of Disposable Personal Income (eight-quarter moving average, quarterly, SA)



SOURCE: Bureau of Economic Analysis, Federal Reserve Board, Haver Analytics.

and the BOG need to make a number of working assumptions. It so happens, however, that many methodological practices are shared by the BEA and the BOG. For instance, both exclude capital gains from disposable income, both exclude pension benefits received from disposable income, and both deduct from personal disposable income the contributions paid into pension funds.[19]

FURTHER DISCUSSION: SHOULD WE BE CONCERNED ABOUT THE DECLINE OF THE PERSONAL SAVING RATE?

We have shown that, even after taking into account a number of methodological and accounting issues, the recent decline in the U.S. personal saving rate is likely to correspond to a key economic phenomenon. Even if we may concede that the current personal saving rate is probably not negative, it seems unquestionable that this rate has declined precipitously since the mid-1990s.[20] However, four arguments have appeared in the literature that may imply that there are (so far at least) no strong reasons to be concerned.

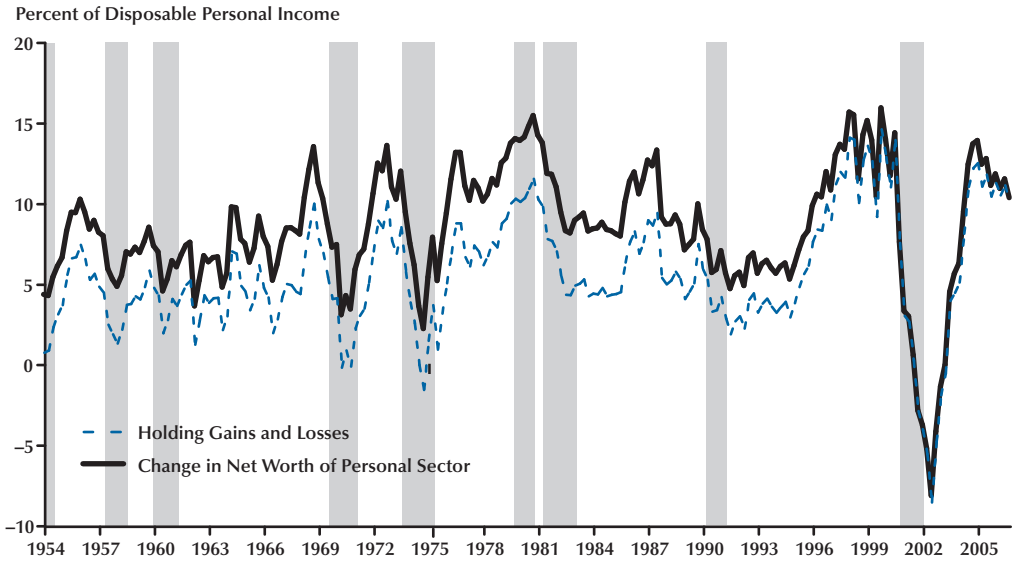
The first argument proposes that personal savings should be measured not from aggregate income and demand NIPA accounts (as routinely done by the BEA), but from data on the changes in the net worth (assets) of U.S. households.

[19] However the BOG considers the consumption of durable goods as part of gross private investment.

[20] Note that BEA/NIPA estimates of the saving rate have been frequently revised up. In fact, the NIPA saving rate has approached zero at several points in recent history. In one of his speeches, former Philadelphia Fed President Santomero (2005) noted that a perception of a near-zero personal saving rate is far from new in economic history and seems to have occurred rather regularly if one looks at real-time data. For instance, 1980 now appears to have been characterized ex post by a relatively high personal saving rate; however, the reported, real-time 1980 NIPA personal saving rate was negative. See Nakamura and Stark (2005) for a discussion. Garner (2006, p. 16) anticipated an upward revision of the NIPA saving rate because the U.S. Census Bureau has revised downward its estimates of food services sales for recent years; his projected revision is on the order of 1.5 percent. Figure 1 reports the effects of the recent revision of July 31, 2007, using a dotted curve.

Figure 10

Measures of Wealth Accumulation as a Percent of Disposable Personal Income (eight-quarter moving average)



SOURCE: Federal Reserve Board.

Additionally, and differently from the current FoF practices of the BOG, this measure of savings should capture not only the acquisition cost of new assets, but also the sum of the acquisition costs and of the capital gains cumulated on the stock of existing wealth. For some types of applications (and policy analysis) this seems to be an appropriate notion. For instance, if policymakers are concerned that a re-entrenchment effect may be caused by retired households that need to cut their consumption because they are unable to support it, then there is little doubt that such households would/could finance their standards of living by selling assets in their net wealth, thus "cashing out" from their cumulated capital gains (see, e.g., Lusardi, 2000, p. 378).[21] Many commentators have stressed that when capital gains are included in the picture, the U.S. personal saving rate either stops showing any trending tendency (see e.g., Poole, 2007) or if any trend appears, it is an upward one; that is, U.S. households appear to have saved more in the recent decade than previously. Figure 10 shows one such measure, the ratio between total net wealth accumulation and disposable income.

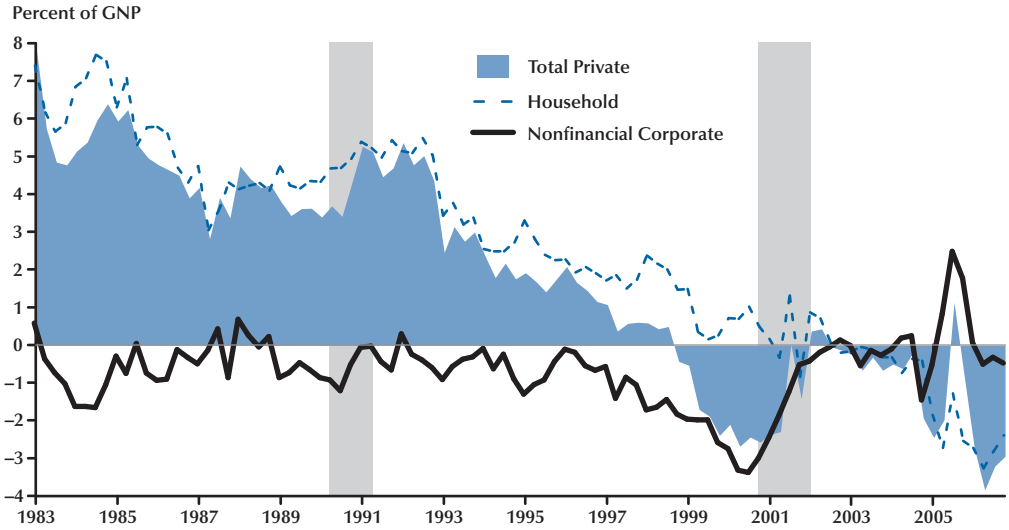
The dotted line shows why such a notion of the personal saving rate differs so much from the FoF estimate: In most of the years, the *holding* (as opposed to the realized) gains or losses represent most of the change in net wealth. This estimate of the personal saving rate is, in practice, below the FoF estimate (on average 5.6 percent vs. 11.3 percent) and similar to the standard NIPA average. However, it actually fails to trend down: For instance, between 1954 and 1994, the average saving rate would have been 5.1 percent versus 7.4 percent between 1995 and 2006.

However, to many commentators, it is not clear whether Figure 10 may actually represent an alternative definition of the personal saving rate, as opposed to a simple adjustment to the

[21] There are other issues with the way FoF savings rates are computed. For instance, Reinsdorf (2004, p. 23) stresses that the BOG FoF accounts fail to give a complete picture of the changes in wealth because debt instruments (such as bonds) appear at book (not market) value.

Figure 11

Private Sector Financial Balances as a Percent of GNP (quarterly, SA)



SOURCE: Bureau of Economic Analysis, Federal Reserve Board.

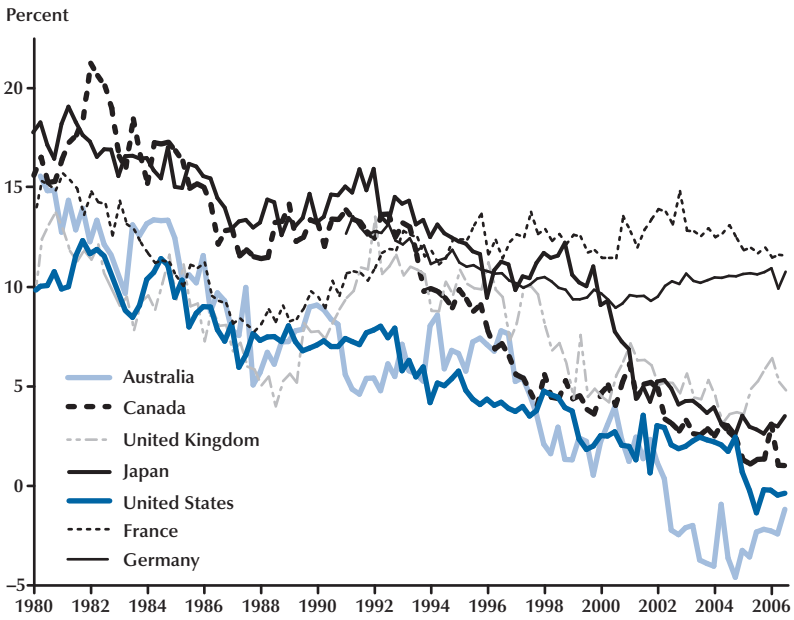
standard FoF definition. Clearly (and even after applying eight-quarter moving-average smoothing!) the wealth accumulation measure remains extremely volatile. This is natural because the numerator mostly reflects the dynamics of asset prices—mainly stocks, bonds, and real estate—which easily manifest annualized volatilities exceeding 20 percent. Moreover, although 2005-06 turns out to have been a "thrifty" period (with average saving rates in excess of 12 percent), one wonders about the actual meaning of the –9 percent rate reached during 2002, in correspondence with the burst of the tech stock bubble of the late 1990s.

A second argument stresses that personal (household) savings cannot simply be assigned the role of the main, dominant component of *private* gross saving; (nonfinancial) businesses also can and do retain a portion of their profits to finance their investment activities. Earlier, we stressed that what really matters for healthy growth is that private saving exceeds private investment. This argument implies that the recent behavior of U.S. households may not be a reason for concern if, at the same time, U.S. nonfinancial firms have happened to increase their saving. This proposition—that net private saving would be roughly constant as a percentage of national income as a result of a strong negative correlation between personal and business saving—is famous in economics (it is often called Denison's law, from Denison, 1958). Unfortunately, it does not successfully withstand serious empirical scrutiny.[22] Figure 11 shows the recent movement of household and business savings as a percentage of output. As previously stressed, gross private savings became negative for long periods at the end of the 1990s and between 2004 and 2005. This means that, although starting in the late 1990s there has been a tendency for nonfinancial corporations to retain a growing fraction of their earnings, such a trend does not fully compensate for the apparent

[22] Hendershott and Peek (1989) were the first to notice that such an inverse relationship between personal and business savings was largely an artifact of measurement problems. Parker (2000, p. 322) stresses that NIPA accounts do a very ambiguous job at separating household savings from business savings. This may justify why different researchers have reached a range of conclusions on the validity of Denison's law after the mid-1990s.

Figure 12

International Household Saving Ratios (quarterly)



SOURCE: Organisation for Economic Co-operation and Development.

"consumption boom" that has at the same time characterized the behavior of U.S. households.

A third argument refers mainly to FoF estimates of the personal saving rate and leads to conclude that such a measure—certainly to be considered superior to NIPA measures, the argument goes—could be grossly underestimated at present. For instance, Hall (2000) has estimated that a large part of the increase in the net worth of U.S. households during the 1990s would have taken the form of what he calls "e-capital," a body of information-processing methods and organizational knowledge that has strongly increased the productivity of labor. Hall has argued that the accumulation of such e-capital by households would have created a new, intangible type of asset that should legitimately enter saving rate calculations. Obviously, a similar phenomenon would have involved U.S. firms that therefore would have a much higher net saving rate than recorded by the BEA. From this perspective, the recent decline in the U.S. personal saving rate would simply hide a shift from savings in the form of accumulation of traditional assets (stocks, bonds, houses) to what we could call "e-assets." In parallel, the net saving of U.S. businesses also might be substantially underestimated. Given the growing importance of information technology in a globalized world, the decline in the personal saving rate would actually reflect an encouraging development, likely to predict sustained productivity growth. Although some of these innovative notions of what constitutes an asset and what constitutes saving behavior are of key importance, at this point the estimates of the amount of annual investments as a percentage of GNP remain fairly uncertain and probably insufficient to explain the decline in the personal saving rate.

One final argument exploits the fact that the recent U.S. experience is not very different from the recent historical record of a number of developed countries. Figure 12 shows the personal

saving rates for seven different countries. These comparisons should be accompanied by a word of caution because different countries are known to follow rather different national income accounting procedures. For instance, while the BEA has a rather complicated set of rules concerning imputations—that is, when a market value is placed on transactions that do not occur in the market economy or that are not observable in its records—many other countries (e.g., Germany) are known to mostly rely on market transactions for all that concerns their calculations of the saving rate.[23]

In spite of these qualifications, the recent downward trend in the personal saving rate clearly has failed to involve only the United States: Similar dynamics also characterize, for instance, Canada and Australia.[24] In particular, the Australian saving rate has been negative since 2002. Furthermore, the Canadian personal saving rate appears now close to zero (it is 1.4 percent), which is remarkable because between 1970 and 1989 the Canadian rate had been 14 percent against 9 percent for the U.S. Thus, a gap of 5 percentage points appears to have almost disappeared in the past 17 years. In contrast, the evolution of both the U.K. and the German personal saving rates have been markedly different from that in the United States. The German rate does not appear to be drifting down over time and in the third quarter of 2005 was still exceeding 10 percent. Of course, differences in the accounting methodologies might explain a relevant portion of these differences. However, absent further evidence to explain the different behavior of U.S., Canadian, and Australian personal saving rates, the safest conclusion is that the recent level and evolution of the U.S. personal saving rate represents a puzzle in search of a convincing economic explanation, which is the subject of the following section.

THE DECLINE OF THE PERSONAL SAVING RATE: IS IT A PUZZLE?

In economics, a phenomenon is said to represent a puzzle when standard and generally accepted economic principles and theories fail to provide a quantitatively satisfactory explanation for a set of empirical regularities. In this case, the empirical "stylized fact" consists of the low and declining U.S. personal saving rate. As shown in Figure 1, such a trend manifested itself as early as in 1993. Therefore, economists and policymakers alike have had more than a decade to develop theories and models that might somehow explain the recent, anomalous behavior of the U.S. personal saving rate. Additionally, it seems now to be received wisdom that the drop in the U.S. saving rate is just a reflection of a contemporaneous "consumption boom" that has swept through the United States since the mid-1990s (see Figure 13). At least six different theories/explanations for the recent dynamics of the personal U.S. saving rate have been put forth. We review them here.[25]

Wealth Effects

This theory is fairly simple and can be traced back to early theories that stressed that a household's net worth ought to influence its consumption/saving patterns: The occurrence of price run-ups in equity (during the late 1990s) and real estate (especially after 2001) markets have created bubble-like conditions in which high and growing capital gains (both realized and unrealized) together increase the current outlays by U.S. households. Lusardi, Skinner, and Venti (2001) conclude that on the basis of the bulk of

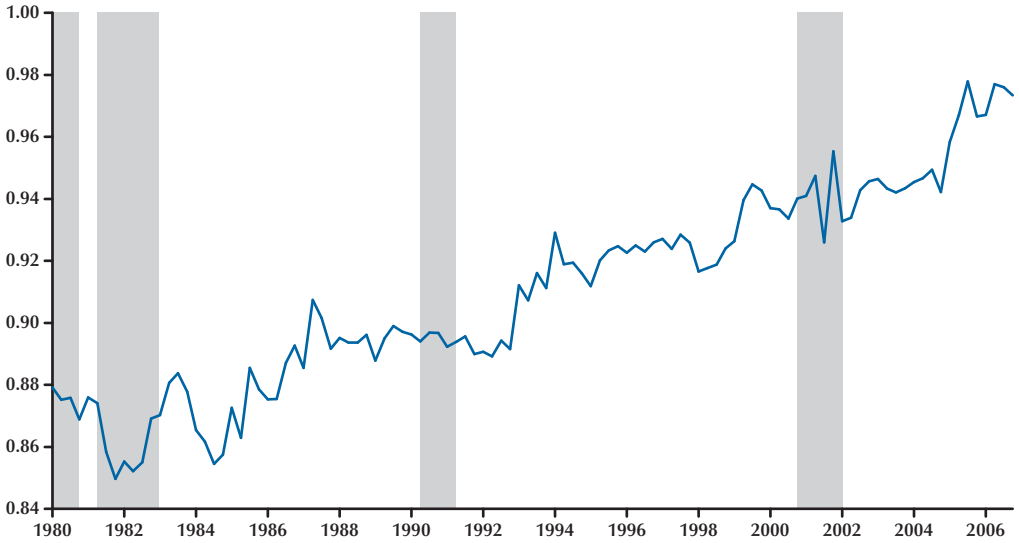
[23] Despite the general principle driving BEA practices that NIPA measures should reflect only market transactions in goods and services, imputations are included in personal income and in other NIPA aggregates, generally to keep the NIPA aggregates invariant to how certain activities are carried out. Specifically, six imputations are included in the estimates of personal income: imputed pay-in-kind, employer-paid health and life insurance premiums, the net rental value of owner-occupied farms and the value of food and fuel produced and consumed on farms, the net rental value of owner–occupied nonfarm housing, the net margins on owner-built housing, and the imputed interest paid by financial intermediaries except life insurance carriers. These imputations accounted for about 8 percent of personal income at the national level in 2001.

[24] The declining Japanese personal saving rate has received some distinct attention in the academic literature (see, e.g., Horioka and Watanabe, 1997).

[25] Our review of the literature is necessarily incomplete. The milestones of the debate on the declining U.S. saving rate seem to be Bosworth, Burtless, and Sabelhaus (1991), Browning and Lusardi (1996), Gale and Sabelhaus (1999), and Parker (2000).

Figure 13

Rate of Personal Consumption Expenditure to Disposable Personal Income (quarterly, SAAR)



SOURCE: Bureau of Economic Analysis.

the econometric evidence, the appreciating stock market wealth since 1988 may have reduced the personal saving rate between 3 and 5 percentage points. Dynan and Maki (2001) and Juster et al. (2006) have reported estimates from micro-level data that are consistent with this conclusion. Although this explanation is intuitively (and quantitatively) appealing, a number of researchers have expressed doubts. For instance, Parker (2000, p. 330) objects that the timing of recent bubbles seems to follow the decline of the U.S. saving rate. The rate kept declining even during 2001 and 2002, when the stock market bubble burst and billion of dollars of unrealized (paper) capital gains were lost (see, e.g., Figure 4). Because the recent U.S. saving rate data imply an elasticity of about one-sixth, this means that the stock market bull periods of the late 1990s should have generated (but did not) a large response of consumption, whereas the strongest dip in the saving rate seems to have occurred after 2002. Lettau and Ludvigson (2004) find that a vast majority of variation in asset wealth is purely transitory and as such tends to have no impact whatsoever on consumer spending, implying that wealth effects represent a plausible explanation only if we believe that most stock and housing market booms in the past two decades were largely due to permanent, structural shifts in the way assets are evaluated. Additionally, Poterba and Samwick (1995) and Ludivgson and Steindel (1999) have shown that the structure of lagged effects connecting consumption to wealth changes are rather complicated and generally support only short-lived and weak effects. Finally, Lusardi, Skinner, and Venti (2001) correctly stress that although the decline in personal saving seems to have involved most cohorts/types of households, only roughly half of the U.S. population holds stocks. The fraction holding housing properties is only slightly higher.

Permanent Income Hypothesis (the "New Economy" Effect)

According to this theory, recent technological advances and enormous increases in labor productivity would have led U.S. households to apply vigorous upward revisions to their permanent-income estimates (see, e.g., Greenwood and

Jovanovic, 1999). In this case, the unobservable effective saving rate perceived by households is

$$\hat{s}_{t+1} = \frac{Y^p_{t+1} - C_{t+1}}{Y^p_{t+1}},$$

where Y^p_{t+1} is some present discounted value of the stream of future incomes (after taxes),

$$Y^p_{t+1} \equiv Y_{t+1} + \sum_{j=1}^{\infty} \beta^j Y_{t+1+j}.$$

In words, The permanent income can be described as a smoothly growing measure whose value corresponds to the present value of all real resources available to a consumer. When $Y^p_{t+1} > Y_{t+1}$, it is possible for agents to perceive (and act upon)

$$\hat{s}_{t+1} > s^{NIPA}_{t+1} \equiv \left(Y_{t+1} - C_{t+1}\right) / Y_{t+1}.$$

In practice, the wealth-effects explanation stresses the effects of the increases in the net worth of households, whereas the permanent-income theory relies more on revisions of the expectations of future incomes. Although many researchers have noticed that this latter explanation is consistent with the fact that the high rate of growth of productivity has survived the recession of 2002 (see, e.g., Parker, 2000, p. 319), most recent research has concluded that productivity effects may explain, at most, 20 percent of the recent changes in the saving rate.

Financial Innovation

This model stresses that improvements in the credit markets have made it possible to transform *unrealized* capital gains and future incomes into current purchasing power (see, e.g., Carroll, 1997). Examples are "exotic" (interest-only) mortgages and subprime rate loans and revolving debt with flexible payment features (e.g., credit cards and overdraft plans on checking accounts). In this case, households do not need to perceive a higher stream of current incomes to increase consumption; a given level of permanent income becomes easy to convert into current consumption as the financial innovation process progresses. According to this model, U.S. households would have plunged into increasing debt. For instance, Kennickell and Starr-McCluer (2000) show that the median amount of outstanding household debt has almost doubled between the end of the 1980s and the turn of the new millennium. Empirically, this explanation has been remarkably successful. For instance, Parker (2000) concludes that the increase in the debt/GDP ratio can explain a remarkable one-third of the observed decline in U.S. personal saving. Gokhale, Kotlikoff, and Sabelhaus (1996) have noticed also that the increasing annuitization of retirement income in the United States may exert some downward pressure on the NIPA saving rate. Yet, a few commentators have expressed reservations about the effects of financial innovations. For instance, Lusardi et al. (2001) remark that the FoF accounts show that the drop in the saving rate has much more to do with households' failure to purchase sufficient financial assets than with their propensity to increase their financial liabilities.

Social Security Programs and Macroeconomic Stability

This explanation relies once more on the mechanism of expectation formation. It stresses that U.S. households, faced with the evidence that Social Security, Medicare, and other government transfer programs work, have increased their consumption levels, feeling that their own personal saving might not be needed as much as they age or experience other debilitating events. For instance, Lusardi et al. (2001) update original calculations in Gokhale, Kotlikoff, and Sabelhaus (1996) and observe that the entire growth in the ratio of consumption to GDP between 1988 and 2000 (roughly 2 percentage points) can be explained by increases in medical care expenditures. This may reveal that consumption has increased simply because social programs are in fact assumed to be paying for the additional expenditure. Huggett and Ventura (2000) and Gustman and Steinmeier (1999) have argued that especially households in the lowest wealth-distribution brackets, which also tend to be relatively young, may rationally expect generous relative (post-"Baby Boom") retirement benefits, either from Social Security or from other pension plans.

Another take on these expectations is that, as households and firms perceive declining macroeconomic risks (e.g., of inflation) as a result of sound economic policies, they might progressively reduce their "precautionary saving" that is supposed to work as a buffer during "bad times." In this respect (and paradoxically), a successful Fed policy might have contributed to long-run instability through a progressive reduction of private saving rates.[26]

Demographics

According to the life-cycle hypothesis of consumption, individuals save when young and dissave when old. If the American population is aging, a decline in the personal saving rate is to be expected. Although there is now some empirical evidence that this explanation might provide a good fit for the Japanese saving decline (see, e.g., Horioka and Watanabe, 1997, and more recently Chen, Imrohoroglu, and Imrohoroglu, 2007), the evidence for the United States is rather weak (see, e.g., Parker, 2000). Browning and Lusardi (1996) offer a rather compelling explanation for why aging cannot work as a main explanation: Aging happens too slowly to generate sufficient variation to explain the U.S. case. Moreover, Lusardi, Skinner, and Venti (2001) do find that the demographic structure of the U.S. population is shifting and that a significant group of households have saving rates too low to be explained by conventional life-cycle models. Notice, however, that this is the opposite of a sensible explanation of the puzzle, because economists so far have not been able to explain why exactly such a cluster of households has difficulty recognizing the need to save and calculating the amount of savings they need.[27]

[26] This explanation can also be read as suggesting that U.S. households are applying a higher level of (effective) subjective discount factors when deciding optimal consumption patterns. Parker (2000, p. 331) observes that this is consistent with the recent evidence of high real interest rates in the United States.

[27] A number of behavioral models have been proposed to interpret this behavior. For instance, Laibson, Repetto, and Tobacman (1998) suggest that people may display hyperbolic rather than exponential discount functions, which implies that short-run discount rates are higher than long-term rates, so that decisionmaking appears to be time inconsistent.

Ricardian Equivalence

Consider a world in which Ricardian equivalence applies: Unless taxes are distortionary, higher taxes should induce households to save less, given a steady level of public expenditures and hence higher public saving. As we noted earlier, a net increase in public sector savings has taken place only between 1993 and 1999, while private saving has kept sliding. Hall (1999) argues that most of the changes in the composition of total national saving between the 1980s and 1998 may be explained by an application of Ricardian neutrality, which is consistent with the empirical findings in Tanzi and Zee (1998) for saving rates and tax data for a panel of countries in the Organisation for Economic Co-operation and Development. However, in quantitative terms, Parker (2000) also rejects that households might simply be acting on the basis of expected, future reductions of budget deficits, as the reductions to be anticipated would have to be implausibly high and historically unprecedented.

Trends in the Way Companies Compensate Shareholders

Financial economists have for decades alerted the economics profession that—for a variety of reasons, related to both institutions (corporate governance mechanisms) and taxes—U.S. corporations have become less and less inclined to pay cash flows to stockholders in the form of dividends. The standard motto is that "dividends are disappearing." From this perspective, the preferred way of compensating stockholders would increasingly be stock repurchases (both directly and as a part of tender offers) and swaps of stocks with bonds and other liquid securities. With a complete shift away from dividends, the amount of stock repurchases by all U.S. non-financial corporations has increased from $42 billion in 2003 to $602 in 2006, an increase by a factor of 14! Currently, cash dividend payments are included in the NIPA definition of disposable income, yet share repurchases are not. Measurement of the saving rate is further complicated because possible taxes paid on the repurchase gains are taken into account, reducing personal disposable

income. Simple math shows that, with the saving rate defined as $s_{t+1} = (Y_{t+1} - C_{t+1})/Y_{t+1}$, if Y_{t+1} gets underestimated, then s_{t+1} will be unduly underestimated. Recent estimates by Steindel (2007) show that almost one-third of the recent saving rate decline may be explained away by this structural change in the way stockholders are compensated. However, the trend is rather recent and, although the saving rate has been falling at least since the early 1990s, these developments in the ratio between cash dividends and stock repurchases have assumed large proportions only in recent years. Additionally, it may be argued that only a portion of a share repurchase actually represents a permanent income component.

CONCLUSIONS

Many economists have stressed that a number of flaws characterize the most widely known estimates of the U.S. personal saving rate. However, none of the problems of the measures currently used (NIPA and FoF rates) seems to fully account for the steep decline and the negative levels reached by the U.S. saving rate after the mid-1990s. Moreover, even when the recent dynamics of households' net wealth, the saving of nonfinancial firms, and the declining saving rates in a number of developed countries are taken into account, there is reason to be concerned about the low level reached by the U.S. personal saving rate after the mid-1990s. These concerns are spurred by the possibility that U.S. households may soon re-entrench and reduce their consumption expenditures. There are also long-term worries that the United States might either be prevented from financing all of the available, positive net present value investment opportunities or forced to accept a high and increasing dependence on foreign lending. Although we have reviewed a number of concurring explanations that have been proposed for the declining propensity of U.S. households to save, it seems that (sometimes on logical grounds, in other occasions on an empirical level) such theories remain insufficient to explain the entire magnitude of the recent transformation of the United States into a nation of spendthrifts. In this sense, the U.S. personal saving rate remains a puzzle.

REFERENCES

Bosworth, Barry; Burtless, Gary and Sabelhaus, John. "The Decline in Saving: Evidence from Household Surveys." *Brooking Papers on Economic Activity*, 1991, No. 1, pp. 183-241.

Browning, Martin and Lusardi, Annamaria. "Household Saving: Micro Theories and Micro Facts." *Journal of Economic Literature*, December 1996, *34*, pp. 1797-855.

Carroll, Christopher D. "Buffer-Stock Saving and the Life Cycle/Permanent Income Hypothesis." *The Quarterly Journal of Economics*, February 1997, *112*(1), pp. 1-55.

Chen, Kaiji; Imrohoroglu, Ayse and Imrohoroglu, Selahattin. "The Japanese Saving Rate between 1960 and 2000: Productivity, Policy Changes, and Demographics." *Economic Theory*, July 2007, *32*(1), pp. 87-104.

Denison, Edward F. "A Note on Private Saving." *Review of Economics and Statistics*, August 1958, *40*(3), pp. 261-67.

Dynan, Karen E. and Maki, Dean M. "Does Stock Market Wealth Matter for Consumption?" Finance and Economics Discussion Series No. 2001-23, Board of Governors of the Federal Reserve System, 2001.

Fleming, Wendell H. and Stein, Jerome L. "Stochastic Optimal Control, International Finance and Debt." *Journal of Banking and Finance*, May 2004, *28*(5), pp. 979-96.

Gale, William, G. and Sabelhaus, John. "Perspectives on the Household Saving Rate." *Brooking Papers on Economic Activity*, 1999, No. 1, pp. 181-222.

Garner, Alan C. "Should the Decline in the Personal Saving Rate Be a Cause for Concern?" Federal Reserve Bank of Kansas City *Economic Review*, Second Quarter 2006, pp. 5-28.

Gokhale, Jagadeesh; Kotlikoff, Laurence, J. and Sabelhaus, John. "Understanding the Postwar Decline in U.S. Saving: A Cohort Analysis." *Brooking Papers on Economic Activity*, 1996, No. 1, pp. 315-407.

Guimaraes, Bernardo. "Optimal External Debt and Default." Discussion Paper No. 6035, Center for Economic Policy Research, January 2007.

Gustman, Alan L. and Steinmeier, Thomas L. "Effects of Pensions on Savings: Analysis with Data from the Health and Retirement Study." *Carnegie-Rochester Conference Series on Public Policy*, June 1999, *50*, pp. 271-324.

Greenwood, Jeremy and Jovanovic, Boyan. "The Information-Technology Revolution and the Stock Market." *American Economic Review*, May 1999, *89*(2), pp. 116-22.

Hakkio, Craig S. and Wiseman, Elisha J. "Social Security and Medicare: The Impending Fiscal Challenge." Federal Reserve Bank of Kansas City *Economic Review*, First Quarter 2006, pp. 7-41.

Hall, Robert E. Comment on "Perspectives on the Household Saving Rate" by Gale and Sabelhaus, *Brooking Papers on Economic Activity*, 1999, No. 1, pp. 215-20.

Hall, Robert E. "E-Capital: The Link Between the Stock Market and the Labor Market in the 1990s." *Brooking Papers on Economic Activity*, 2000, No. 2, pp. 73-118.

Hendershott, Patric H. and Peek, Joe. "Aggregate U.S. Private Saving: Conceptual Measures and Empirical Tests," in Robert E. Lipsey and Helen Stone Tice, eds., *The Measurement of Saving, Investment and Wealth*. Chicago: University of Chicago Press, 1989, pp. 185-226.

Horioka, Charles Yuji and Watanabe, Wakō. "Why Do People Save? A Micro-Analysis of Motives for Household Saving in Japan." *The Economic Journal*, May 1997, *107*(442), pp. 537-52.

Huggett, Mark and Ventura, Gustavo. "Understanding Why High Income Households Save More than Low Income Households." *Journal of Monetary Economics*, April 2000, *45*(2), pp. 361-97.

Juster, Thomas F.; Lupton, Joseph P., Smith, James P. and Stafford, Frank. "The Decline in Household Saving and the Wealth Effect." *The Review of Economics and Statistics*, November 2006, *88*(1), pp. 20-27.

Kennickell, Ann and Starr-McCluer, Martha. "Recent Changes in U.S. Family Finances: Results from the 1998 Survey of Consumer Finances." *Federal Reserve Bulletin*, January 2000, pp. 1-29.

Laibson, David; Repetto, Andrea and Tobacman, Jeremy. "Self-Control and Saving for Retirement." *Brooking Papers on Economic Activity*, 1998, No. 1, pp. 91-172.

Lettau, Martin and Ludvigson, Sydney C. "Understanding Trend and Cycle in Asset Values: Reevaluating the Wealth Effect on Consumption." *American Economic Review*, March 2004, *94*(1), pp. 276-99.

Ludvigson, Sydney and Steindel, Charles. "How Important Is the Stock Market Effect on Consumption?" Federal Reserve Bank of New York *Economic Policy Review*, July 1999, *5*(2), pp. 29-51.

Lusardi, Annamaria. "Spendthrift in America? On Two Decades of Decline in the U.S. Saving Rate: Comment," in Ben S. Bernanke and Julio J. Rotemberg, eds., *NBER Macroeconomics Annual 1999*. Cambridge, MA: MIT Press, 2000, pp. 374-86.

Lusardi, Annamaria; Skinner, Jonathan and Venti, Steven. "Saving Puzzles and Saving Policies in the United States." *Oxford Review of Economic Policy*, Spring 2001, *17*(1), pp. 95-115.

Nakamura, Leonard I. and Stark, Tom. "Benchmark Revisions and the U.S. Personal Saving Rate." Working Paper No. 05-6, Federal Reserve Bank of Philadelphia, April 2005.

Parker, Jonathan A. "Spendthrift in America? On Two Decades of Decline in the U.S. Saving Rate," in Ben S. Bernanke and Julio J. Rotemberg, eds.,

NBER Macroeconomics Annual 1999. Cambridge, MA: MIT Press, 2000, pp. 317-70.

Peach, Richard W. and Steindel, Charles. "A Nation of Spendthrifts? An Analysis of Trends in Personal and Gross Saving." Federal Reserve Bank of New York *Current Issues in Economics and Finance*, September 2000, *6*(10), pp. 1-6.

Perozek, Maria G. and Reinsdorf, Marshall B. "Alternative Measures of Personal Saving." *Survey of Current Business*, April 2002, *82*(4), pp. 13-24.

Poole, William. "How Dangerous Is the U.S. Current Account Deficit?" Economic Policy Lecture Series, Lindenwood University, St. Charles, MO, November 9, 2005; http://stlouisfed.org/news/speeches/2005/11_09_05.htm.

Poole, William. "U.S. Saving." Presented at the CFA Society of Nebraska, Omaha, NE, February 15, 2007; http://stlouisfed.org/news/speeches/2007/02_15_07.html.

Poterba, James M. "Stock Market Wealth and Consumption." *Journal of Economic Perspectives*, Spring 2000, *14*(2), pp. 99-118.

Poterba, James M. and Samwick, Andrew A. "Stock Ownership Patterns, Stock Market Fluctuations, and Consumption." *Brooking Papers on Economic Activity*, 1995, No. 2, pp. 295-357.

Reinsdorf, Marshall B. "Alternative Measures of Personal Saving." *Survey of Current Business*, September 2004, *84*(9), pp. 17-26.

Reinsdorf, Marshall B. "Alternative Measures of Personal Saving." *Survey of Current Business*, February 2007, *87*(2), pp. 7-13.

Santomero, Anthony M. "Making Monetary Policy: What Do We Know and When Do We Know It?" Presented at the National Economists Club, April 7, 2005; www.philadelphiafed.org/publicaffairs/speeches/santomero/2005/04-07-05_natl-economists-club.cfm.

Steindel, Charles. "How Worrisome Is a Negative Saving Rate?" Federal Reserve Bank of New York *Current Issues in Economics and Finance*, May 2007, *13*(4), pp. 1-7.

Tanzi, Vito and Zee, Howell H. "Taxation and the Household Saving Rate: Evidence from OECD Countries." IMF Working Paper No. 98/36, International Monetary Fund, March 1998.

Measuring Commercial Bank Profitability: Proceed with Caution

R. Alton Gilbert and David C. Wheelock

The federal tax code creates challenges for comparing the profit rates of different banks on a consistent basis. The earnings of banks that elect to operate under subchapter S of the federal tax code are not subject to federal corporate income tax, but shareholders of these "S-banks" are taxed on their pro rata share of the entire earnings of the bank. The number of banks electing subchapter S tax treatment has increased rapidly, especially among small banks. The authors use estimates of the federal corporate income tax that S-banks would pay if they were subject to the tax to show that the difference in the tax treatment of S-banks and other banks has a large impact on measures of U.S. banking system profitability. Further, the article shows that adjustment of S-bank earnings by estimates of federal income taxes to make them comparable with the earnings of other banks can markedly affect conclusions of studies that use net income as a measure of performance. Finally, the article shows that S-banks (even after their earnings are reduced by estimated federal taxes) tend to out-earn their peers; S-banks also tend to have higher earnings rates than their peers in the year before they elect S-bank status. (JEL G21, G28, H25)

Federal Reserve Bank of St. Louis *Review*, November/December 2007, *89*(6), pp. 515-32.

Measures of after-tax rates of return, such as the return on average total assets (ROA) and the return on total equity (ROE), are widely used to assess the performance of firms, including commercial banks. Bank regulators and analysts have used ROA and ROE to assess industry performance and forecast trends in market structure—as inputs in statistical models to predict bank failures and mergers—and for a variety of other purposes where a measure of profitability is desired.

The usefulness of standard profit measures can be affected by tax laws and regulations, which are subject to occasional amendment and revision. Subchapter S of the federal tax code, for example, was established to benefit small businesses by granting them relief from the double taxation of corporate dividends. The dividends paid by most corporations are taxed twice—once at the firm level under the corporate income tax and again at the shareholder level under the personal income tax. However, the earnings of banks and other firms that elect subchapter S tax treatment (S-corporations) are not subject to the federal corporate income tax. (However, shareholders of S-corporations are subject to personal income taxes on their pro rata share of the firm's entire earnings, including nondistributed retained earnings. Corporations not electing subchapter S status operate under subchapter C of the federal tax code—hereafter, C-corporations.)

Because the earnings of S-corporations are taxed differently from those of C-corporations, the profit rates of S- and C-corporations are not directly comparable on the basis of standard measures of after-tax rates of return.

R. Alton Gilbert is a former vice president and banking economics advisor and David C. Wheelock is an assistant vice president and economist at the Federal Reserve Bank of St. Louis. The authors thank Andrew Meyer and Rajdeep Sengupta for helpful comments. Craig Aubuchon and Daniel McDonald provided research assistance.

Recent growth in the number of banks that elect to operate under subchapter S of the federal tax code has complicated the use of after-tax profit measures to assess trends in industry profitability and to compare rates of return across banks and over time. This article examines the consequences of the proliferation of S-banks for assessing the profitability of the U.S. banking industry. The quarterly Uniform Bank Performance Report (UBPR) produced by the Federal Financial Institutions Examination Council (FFIEC) provides hypothetical after-tax rate of return data for individual S-banks: That is, S-banks' rates of return are adjusted by an estimate of the federal corporate income tax that those banks *would have had to pay* if they were subject to the tax.[1] The adjustment is quantitatively large for many banks, indicating that comparisons of S- and C-banks using standard after-tax profit measures can lead to erroneous conclusions. Because S-banks are more prevalent among smaller banks, comparison of average after-tax profit rates across groups of banks delineated by size is especially problematic unless differences in the tax treatment of S- and C-bank earnings are taken into account. This article shows quantitatively the impact of the differences in the tax treatment of S- and C-banks on measures of U.S. banking system profitability.

We find that the net profit rates of S-banks tend to exceed those of similarly sized C-banks, even after S-bank earnings are adjusted by the UBPR estimate of federal income taxes that they would have had to pay if they were subject to the corporate income tax. The UBPR adjustment does not account for any differences in how S- and C-banks are taxed by states, however, nor does it capture differences in how S- and C-banks are managed in response to the incentives they face because of how their earnings are taxed. We find that S-banks consistently have higher pre-tax earnings rates and net interest margins than C-banks and tend to be more cost efficient. Further, we find that C-banks that became S-banks tended to have higher profit rates in the year before they changed status than other C-banks, suggesting that S-bank status alone cannot fully account for the higher average adjusted profit rates of S-banks.

Because one cannot meaningfully compare the earnings of S- and C-banks on the basis of standard after-tax profit rates, some analysts use *pre-tax* profit measures to evaluate the performance of banks and in statistical models that include a profit measure. Presumably banks seek to maximize after-tax profits rather than pre-tax profits, however, and some strategies for maximizing after-tax profits can result in relatively low pre-tax earnings rates. For example, some banks hold large amounts of securities whose interest payments are exempt from taxation at the federal, state, and/or local levels. All else equal, a bank that holds a large amount of tax-advantaged securities may have a relatively low pre-tax rate of return but a relatively high after-tax rate of return. Hence, comparison of pre-tax profit rates can give a misleading view of bank performance. The UBPR includes an adjustment to banks' pre-tax income for tax-exempt earnings. This article investigates how large an impact this adjustment has on pre-tax bank earnings rates.

In summary, the federal tax code creates challenges for measuring the profit rates of banks on a consistent basis across banks and across time. The UBPR, however, provides two measures of bank profits designed to permit such comparisons: (i) pre-tax income adjusted for earnings on tax-advantaged securities and (ii) after-tax income adjusted for the federal corporate income tax that S-banks would have had to pay if they were C-banks. While these measures can be useful, this article suggests that analysts should proceed with caution when using any measure of bank profitability.

The following section illustrates the implications of subchapter S tax treatment for after-tax measures of bank earnings and for shareholder income. Subsequently, we examine the growth in the number of S-banks across different groups sorted by asset size and show how the proliferation of S-banks has affected measures of banking industry profitability. We then examine how conclusions about the viability of small, community

[1] Regulators use the UBPR for offsite surveillance of banks. Private-sector bank analysts also frequently use the report, which can be accessed at the web site of the FFIEC, an interagency body comprising the federal regulators of bank and thrift institutions. See www.ffiec.gov.

Table 1

Illustration of the Effects of Taxation as an S-Bank on Bank Profit Rates and Shareholder Returns

	C-bank	S-bank
Total assets	$50,000,000	$50,000,000
Pre-tax income	1,000,000	1,000,000
Federal corporate income tax	300,000	0
Net income after tax	700,000	1,000,000
Adjustment to the net income of the S-bank for taxes it would pay if taxed as a C-bank		–300,000
UBPR tax-adjusted net income of bank	700,000	700,000
Dividends to shareholders	210,000	300,000
Taxes paid by shareholders	63,000	300,000
Returns to shareholders		
Retained earnings	490,000	700,000
Plus dividends	210,000	300,000
Minus taxes on dividends	63,000	300,000
Increase in the net worth of shareholders	637,000	700,000

banks can be substantially affected by whether or not one adjusts S-bank earnings for estimated federal taxes. Further, we examine differences in the financial characteristics of S- and C-banks and explore the implications of using pre-tax earnings as an alternative to after-tax profits.

HOW THE TAXATION OF S-BANK PROFITS AFFECTS BANK RETURNS AND SHAREHOLDER INCOME

Subchapter S enables small firms to avoid double taxation on distributed earnings without sacrificing the advantages of limited liability. Although the earnings of ordinary (subchapter C) corporations are subject to the federal corporate income tax, the earnings of subchapter S corporations are exempt from the tax. However, shareholders of subchapter S corporations are subject to personal income tax on their pro rata share of the entire earnings of the corporation, not just on dividends. The example below illustrates how the shareholders of S-banks benefit from the elimination of double taxation of dividends.

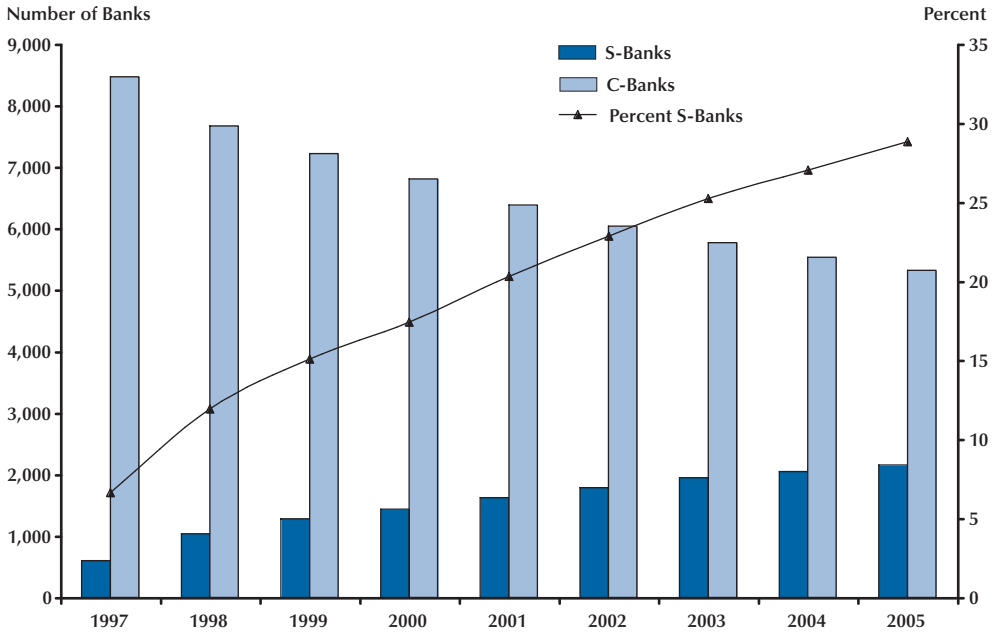
Consider the hypothetical C- and S-banks with financial data given in Table 1. Each bank has total assets of $50 million and pre-tax income of $1 million. In addition, each bank pays 30 percent of its net after-tax income as dividends to its shareholders.[2] To simplify the illustration, we assume that the state corporate income tax is zero for these banks. Further, we assume that the shareholders of each bank have a marginal tax rate of 30 percent and that the federal income tax rate for corporations is also 30 percent.

The C-bank pays federal income tax of $300,000, whereas the S-bank pays no federal income tax. The C-bank reports net after-tax income of $700,000, and the S-bank reports net after-tax income of $1,000,000. Thus, the standard ROA of the C-bank is 1.4 percent, whereas the standard ROA of the S-bank is 2 percent. This difference in ROA is due entirely to the difference in how the earnings of the two banks are taxed, because their pre-tax earnings and their total assets are the same. The UBPR would report the *adjusted* net income of the S-bank as $700,000, the

[2] In practice, S-banks tend to have higher dividend payout rates than C-banks. We assume equal payout rates in our example for simplicity and to focus on the implications of the different federal corporate income tax rates for S- and C-banks.

Figure 1

Number of Banks Electing Subchapter-S and -C Status



same as the net income of the C-bank, and the *adjusted* ROA of each bank would be 1.4 percent.[3]

The C-bank pays dividends of $210,000, whereas the S-bank pays dividends of $300,000. With a marginal tax rate of 30 percent, the shareholders of the C-bank pay income tax of $63,000 on their dividends, whereas the shareholders of the S-bank pay income tax of $300,000 because they are taxed on the full earnings of the bank, not just on the dividends they receive.

Positive profits in the current year increase the net worth of the shareholders of both the C-bank and S-bank. The increase in net worth is higher for the shareholders of the S-bank by $63,000, which is the amount of tax that the shareholders of the C-bank pay on their dividends. Of course, these magnitudes would differ under other possible assumptions.

The Proliferation of S-Banks

Congress created subchapter S of the federal tax code in 1958, but commercial banks have been permitted to elect subchapter S status only since January 1997. The number of commercial banks electing subchapter S tax treatment has since risen rapidly. Figure 1 illustrates the growth in the number and percentage of banks electing S-status over time. The number of S-banks increased from 601 banks (representing 6.6 percent of the industry) at year-end 1997 to 2,155 banks (representing 28.8 percent of the industry) at year-end 2005.

Subchapter S corporations are limited to a maximum of 100 shareholders, which precludes many larger banks from electing S-status.[4] Hence, S-banks are concentrated among smaller banks. Table 2 reports the relative number and asset

[3] UBPR adjusts an S-banks' net income by subtracting from pre-tax income the UBPR estimate of the federal corporate income tax that the S-bank would have had to pay if it were taxed as a C-bank, which creates a measure comparable to C-banks' adjusted net income, which equals after-tax net income.

[4] See Landau (2005) and www.s-corp.org/asp/products/product_3_4.asp for information about the history of subchapter S and current requirements for election of S status.

Table 2

S-Bank Presence By Bank Size Group, 2005

	Number of banks			Assets (in thousands)		
Bank size group	All banks	S-banks	Percent S-banks	All banks	S-banks	Percent S-banks
Greater than $1 billion	460	26	5.7	$7,190,934,374	$37,148,074	0.5
$300 million to $1 billion	1,094	186	17.0	525,041,331	85,106,377	16.2
$100 million to $300 million	2,279	654	28.7	380,418,078	105,267,783	27.7
$50 million to $100 million	1,585	616	38.9	112,932,985	43,362,442	38.4
Less than $50 million	1,480	597	40.3	44,274,181	19,095,383	43.1
All groups	6,898	2,079	30.1	8,253,600,949	289,980,059	3.5

NOTE: Data include only those banks in peer groups 1 through 15 of the Uniform Bank Performance Report. Size groups are based on total end-of-year assets.

holdings of S-banks for five size groups, as well as across all groups, as of December 31, 2005.[5] For example, S-banks accounted for less than 6 percent of banks with $1 billion or more of assets and just 0.5 percent of the total assets of banks with more than $1 billion of assets. By contrast, S-banks accounted for over 40 percent of banks and over 43 percent of the total assets of all banks with less than $50 million of assets.

EFFECTS OF THE TAX TREATMENT OF S-BANKS ON MEASURES OF BANK INCOME

This section examines how the proliferation of banks electing S status has affected aggregate measures of banking industry profitability.[6] Figures 2 and 3 plot annual data from 1996 to 2005 on median after-tax ROA and ROE, respectively, for large and small banks; here, large banks are those with more than $1 billion of assets and small banks are those with less than $1 billion of assets. The median after-tax profit rates of large banks exceeded those of small banks throughout the period and increased relative to those of small banks after 2000.[7]

In addition to the standard ROA and ROE measures, the dashed lines in Figures 2 and 3 also show median earnings rates based on the alternative measure in which the earnings rates of S-banks are reduced by the UBPR estimates of the tax that they would have had to pay if subject to the federal corporate income tax. The median values of ROA adjusted and ROE adjusted shown in the figures are calculated using the standard ROA and ROE measures for C-banks and the measures that are adjusted for estimated federal corporate income taxes for S-banks.[8] Because few large banks are S-banks, the S-bank adjustment for estimated taxes has only a small effect on the median profit rates of banks with assets of at least $1 billion. However, for small banks, the impact of the adjustment is large and has been growing

[5] The data reported in Table 2 are for all banks in peer groups 1 through 15 of the UBPR. These peer groups include all U.S. commercial banks except those chartered during the most recent five years. Including such banks would raise the total number of S-banks to 2,155. Peer groups 1 through 15 also exclude credit card specialty banks, bankers' banks, and thrifts. See the March 2006 UBPR user's guide (FFIEC, 2006, Section II: Technical Information).

[6] Hein, Koch, and MacDonald (2005) present similar information through 2002.

[7] Figures 2 and 3 report median profit rates because extreme values distort mean profit rates. Comparisons such as those in Figures 2 and 3 can be sensitive to how one distinguishes "large" and "small" banks. For example, Bassett and Brady (2001) find that between 1985 and 2000, small banks (defined as those outside the largest 1,000 banks) consistently had higher average earnings rates than the largest 100 U.S. banks. Bassett and Brady do not use the UBPR data on net income adjusted for the tax treatment of S-banks.

[8] See FFIEC (2006, Section II, "Technical Information," p. 4) for information about the adjusted measure of after-tax earnings of S-banks for estimated income taxes. This document is available at www.ffiec.gov/ubprguide.htm.

Figure 2

ROA With and Without S-Bank Adjustment

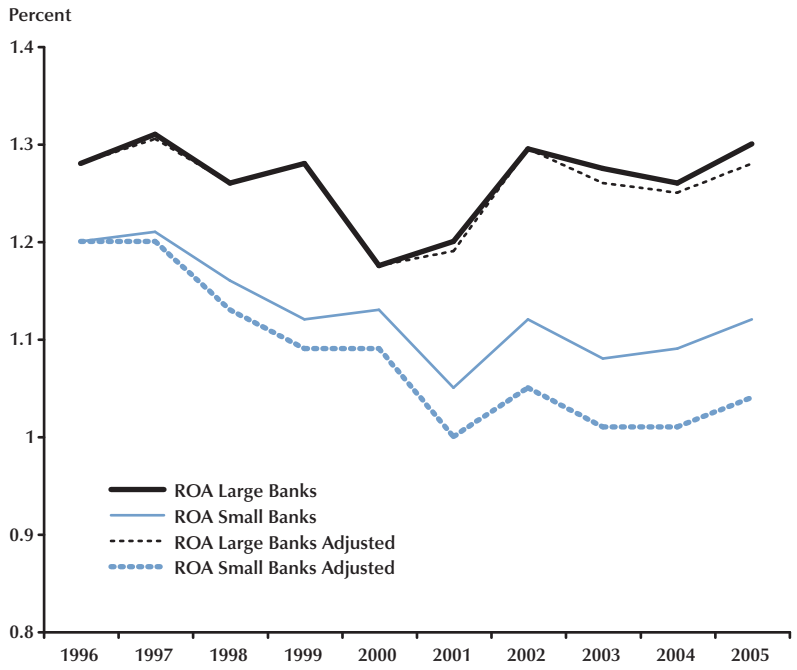


Table 3

Median Return on Assets (ROA) and Return on Equity (ROE) for Banks Grouped by Size and Tax Status, 2005

	All banks				
Bank size group	Number	ROA	ROA adjusted	ROE	ROE adjusted
More than $1 billion	450	1.30	1.28	13.87	13.62
$300 million to $1 billion	1,071	1.21	1.16	13.27	12.84
$100 to $300 million	2,250	1.17	1.08	11.77	11.01
$50 to $100 million	1,560	1.10	0.99	10.55	9.52
Less than $50 million	1,429	0.99	0.89	8.51	7.64

NOTE: Includes only those banks in peer groups 1 through 15 of the Uniform Bank Performance Report. ROA adjusted: ROA with adjustment for imputed taxes; for all banks, this is the median ROA across all banks, where ROA for S-banks is adjusted for imputed taxes. ROE adjusted: ROE with adjustment for imputed taxes; for all banks, ROE adjusted is the median ROE across all banks, where ROE for S-banks is adjusted for imputed taxes. Bank size groups are based on total end-of-year assets.

Figure 3

ROE With and Without S-Bank Adjustment

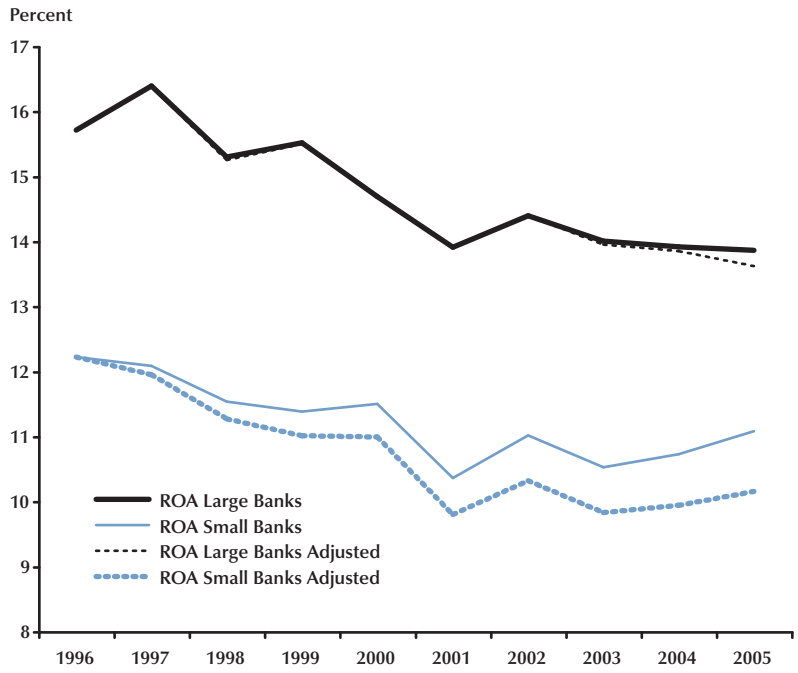


Table 3, cont'd

C-banks			S-banks				
Number	ROA	ROE	Number	ROA	ROA adjusted	ROE	ROE adjusted
426	1.28	13.62	24	1.78	1.34	19.07	13.78
896	1.14	12.55	175	1.74	1.25	19.64	14.25
1,607	1.05	10.41	643	1.63	1.18	17.47	12.49
952	0.94	8.80	608	1.49	1.08	15.15	10.91
848	0.82	6.94	581	1.37	0.97	12.41	8.88

over time as the number of S-banks has risen. Moreover, the earnings gap between large and small banks based on the adjusted earnings measures has been getting wider over time.

Table 3 presents information on the median after-tax profit rates (ROA and ROE) of commercial banks of various size groups for 2005. The table also reports median adjusted ROA and ROE (calculated as in Figures 2 and 3).

Only 24 banks with $1 billion or more of assets elected S-bank status in 2005; accordingly, for all banks with total assets greater than $1 billion, the differences between median ROA and median adjusted ROA and between median ROE and median adjusted ROE are small. The median ROA of commercial banks with at least $1 billion of assets is 1.30 percent and median ROE is 13.87 percent, whereas median adjusted ROA is 1.28 percent and median adjusted ROE is 13.62 percent.

Tax adjustment of S-bank earnings has a larger impact on group median earnings rates for smaller banks. For the smallest banks—those with no more than $50 million of assets—median ROA drops from 0.99 percent to 0.89 percent and median ROE drops from 8.51 percent to 7.64 percent when S-bank profit rates are adjusted to include imputed taxes. Hence, the exemption of S-banks from the corporate income tax has an especially large impact on median after-tax earnings rates for groups consisting of small banks.

In addition to showing median profit rates across all banks in each size group, Table 3 reports data for C- and S-banks separately. For S-banks, we report median values of both unadjusted and adjusted ROA and ROE. The median values of ROA and ROE for S-banks are considerably larger than those for C-banks, with much of the differences accounted for by the different tax treatment of S- and C-banks. Adjusting ROA and ROE to include the UBPR estimate of federal income taxes has a large impact on median earnings rates for S-banks across all size ranges. For example, for the S-banks with less than $50 million of assets, the adjustment reduces median ROA from 1.37 percent to 0.97 percent and median ROE from 12.41 percent to 8.88 percent. Clearly, the absence of federal corporate income taxes on S-bank earnings has a large impact on their measured after-tax rates of return, indicating that caution is warranted when comparing after-tax rates of return of S- and C-banks—or of groups of banks that include both S- and C-banks.[9]

IMPLICATIONS OF THE ADJUSTMENT OF S-BANK PROFITS FOR ECONOMIC RESEARCH: AN EXAMPLE INVOLVING THE VIABILITY OF SMALL BANKS

The total number of small banks and their share of industry assets have been falling in recent years. This trend has led many analysts to question whether small, "community" banks remain viable in today's banking environment. Advances in communications and information-processing technology have eroded the benefits of close proximity and local ties that traditionally enabled community banks to provide financial services profitably to small firms and other local borrowers. In addition, the removal of state and federal restrictions on branch banking has put further strain on many community banks by exposing them to increased competition.

Conclusions about the viability of community banks have often been based on comparisons of the profit rates of small and large banks. For example, DeYoung, Hunter, and Udell (2004) compare after-tax rates of return (ROA and ROE) of community and rural banks with those of mid-size banks (defined as banks with assets between $1 billion and $10 billion of assets) and large banks (defined as banks with at least $10 billion of assets). Their data on bank profits are not adjusted for the corporate income tax that S-banks would pay if they were taxed like C-banks.

DeYoung, Hunter, and Udell (2004) show that in 2001, the average ROA of "best practice"

[9] Hein, Koch, and MacDonald (2005) and Keeton, Harvey, and Willis (2003) also note that the growing number of banks electing S status distorts comparison of after-tax rates of return across banks and especially comparisons between groups of large and small banks.

Table 4

Implication of S-bank Adjustment for Mean ROA and ROE of Best- and Worst-Practice Banks, 2005

	ROA				ROE			
	Above median ROE		Below median ROE		Above median ROE		Below median ROE	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Large community banks	1.55	1.43	0.94	0.92	17.95	16.55	9.36	9.24
Medium community banks	1.60	1.36	0.89	0.87	17.87	15.30	8.37	8.18
Small community banks	1.52	1.22	0.71	0.67	15.39	12.36	5.68	5.45
Rural community banks	1.45	1.19	0.60	0.57	15.10	12.37	4.65	4.46
Mean value for large banks	1.32				14.14			

NOTE: Includes only those banks in peer groups 1 through 15 of the Uniform Bank Performance Report. Data exclude banks with ROA among the largest or smallest 1 percent of observations.

community banks exceeded the average ROA of mid-size and large banks, where "best-practice" banks are defined as those with an ROE exceeding the median for their asset-size group. In addition, these authors show that the average ROE of best-practice community banks with at least $100 million of assets also exceeded average ROE for mid-size and large banks. The authors conclude that these and other comparisons strongly suggest that the "community bank business model is economically viable," though they also note that many community banks are not operating profitably or at an efficient scale (p. 122).

Table 4 updates and extends the analysis of DeYoung, Hunter, and Udell (2004) using data for 2005. The table reports mean values of ROA and ROE for three groups of community banks based on asset size and for all community banks headquartered in rural areas (i.e., outside of metropolitan statistical areas). As in DeYoung, Hunter, and Udell (2004), we define large community banks as those with assets between $500 million and $1 billion of assets, medium community banks as those with assets between $100 million and $500 million of assets, and small community banks as those with assets less than $100 million. We identify large banks as those with total assets in excess of $1 billion. For each group, we report separate means for banks with ROE exceeding the group median and for those with ROE below the group median. Further, we report means based on the standard after-tax ROA and ROE measures and for data using the tax-adjusted S-bank measure.[10]

As shown in Table 4, for each group of community banks the mean values of unadjusted ROA and ROE for the best-practice banks exceed those for large banks—where, again, best-practice banks are defined as those with ROE above the median for their group and large banks are defined as those with assets in excess of $1 billion. Among large community banks, for example, best-practice banks have a mean ROA of 1.55 percent, compared with a mean of 1.32 percent for large banks. Among rural community banks, best-practice banks have a mean ROA of 1.45 percent. However, the group means are substantially reduced if one adjusts S-bank earnings rates to include estimates of their hypothetical federal tax liability. For example, among large community banks, the mean adjusted ROA of best-practice banks is 1.43 percent, whereas among rural community banks, the mean adjusted ROA of best-practice

[10] Our data are from the UBPR and include all banks in peer groups 1 through 15. However, we omitted banks with extreme values of ROA (those in the upper-most or smallest 1 percent tails of the distribution) to eliminate outliers and some banks that appear to have been misclassified in the UBPR.

banks is 1.19 percent. Further, among both small and rural community banks, the mean values of adjusted ROA and adjusted ROE for best-practice banks are lower than the overall means for large banks. Of course, these results do not necessarily imply that small community banks and rural banks are not viable. A definitive answer to the viability question would require a full accounting of the costs and benefits of electing S-bank tax treatment, which include not only the corporate and personal income tax issues, but also the implications for growth associated with legal limits on the number of shareholders an S-bank may have. However, the analysis here does show that conclusions about the profitability of banks of different sizes, and hence about the viability of small banks, can be markedly affected by whether or not one adjusts rate of return measures to include estimates of the federal corporate income taxes that S-banks would pay if subject to that tax.

A COMPARISON OF S- AND C-BANK CHARACTERISTICS

The UBPR tax adjustment of S-bank profits closes much of the gap between the after-tax profit rates of S- and C-banks of similar asset size. However, for most size groups it does not close the gap entirely. For all years from 1997 to 2005, we find that even with the imputation for federal corporate income taxes, S-banks tend to have higher adjusted rates of return than do C-banks. Table 5 presents information for 2005. For banks in the same asset-size group, the means of adjusted ROA and adjusted ROE of S-banks are higher than those of C-banks. The *p*-values shown below the differences in the mean profit rates of S-banks and C-banks in the bottom panel of Table 5 indicate that these differences are statistically significant for banks with assets of less than $1 billion.[11]

[11] The information reported in Table 5 is based on data for all commercial banks assigned to peer groups 1 through 15 in the UBPR except those with values for ROA among the upper or lower 1 percent in a given year. By dropping banks with extreme values of ROA, we avoided including observations with implausible values, some of which were for banks that appeared to be misclassified in the UBPR.

We made similar comparisons for other years and obtained results that are similar to those for 2005, except as noted below.[12]

There are several possible explanations for why the tax-adjusted earnings rates of S-banks tend to exceed the earnings rates of C-banks. The UBPR adjustment to the net income of S-banks does not take into account any differences in the applicability of state corporate income or other taxes between S- and C-banks. In addition, this report makes no attempt to adjust profit measures for differences in the incentives that S- and C-banks face in the management of their revenues and expenses because of the differences in how their income is taxed. The adequacy of the UBPR net income adjustment has implications for studies involving bank profit rates, such as those addressing the viability of community banks. For example, if the adjustment is too small, then the differences between the adjusted and unadjusted profit measures for small banks shown in Table 4 understate the true differences.

Comparison of Mean Values of Various Financial Ratios Across S- and C-Banks

Aside from the possibility that the UBPR tax adjustment of S-bank earnings is incomplete, S-banks might have higher average earnings rates than similar-size C-banks because of superior operating efficiency. This section compares S- and C-banks on the basis of various financial characteristics in an effort to understand better why S-bank earnings rates tend to exceed those of C-banks.

We compare S- and C-bank performance on measures of pre-tax net operating income (as a percentage of average total assets), net interest income, net non-interest income, and cost efficiency.[13] As shown in Table 5, we find that S-banks consistently have higher pre-tax profit rates

[12] For banks with assets between $300 million and $1 billion, the difference is statically significant in some years between 1997 and 2004. For banks with less than $300 million, the difference is statically significant in every year.

[13] See Harvey and Padget (2000) for additional discussion of the implications of S-status election for commercial banks and evidence on differences in the characteristics and performance of S- and C-banks during 1997-99.

Table 5

Mean Values of Various Performance Measures, 2005

S-Banks

Bank size group	ROA adjusted	ROE adjusted	Pre-tax op. profit/assets	Op. profit + personnel/ assets	Net interest margin	Net non-interest margin	Cost efficiency
More than $1 billion	1.35	15.20	2.02	3.74	4.31	–1.80	56.80
$300 million to $1 billion	1.24	14.06	1.79	3.65	4.45	–2.03	58.27
$100 to $300 million	1.18	12.80	1.69	3.51	4.44	–2.17	60.15
$50 to $100 million	1.09	11.20	1.56	3.37	4.37	–2.29	62.65
Less than $50 million	0.99	9.31	1.42	3.32	4.40	–2.47	66.32

C-Banks

Bank size group	ROA	ROE	Pre-tax op. profit/assets	Op. profit + personnel/ assets	Net interest margin	Net non-interest margin	Cost efficiency
More than $1 billion	1.27	13.76	1.93	3.40	3.89	–1.42	54.98
$300 million to $1 billion	1.17	12.70	1.73	3.41	4.21	–1.92	58.56
$100 to $300 million	1.06	10.85	1.53	3.28	4.33	–2.20	62.01
$50 to $100 million	0.94	8.95	1.33	3.18	4.34	–2.46	66.91
Less than $50 million	0.82	6.96	1.11	3.18	4.35	–2.67	76.62

Difference Between C-Banks and S-Banks (mean and *p*-value for hypothesis tests)

Bank size group	ROA*	ROE*	Pre-tax op. profit/assets	Op. profit + personnel/ assets	Net interest margin	Net non-interest margin	Cost efficiency
More than $1 billion	–0.07	–1.45	–0.09	–0.34	–0.42	0.38	–1.82
	0.32	0.20	0.33	0.07	0.04	0.02	0.34
$300 million to $1 billion	–0.07	–1.34	–0.06	–0.24	–0.24	0.11	0.29
	0.05	0.00	0.19	0.02	0.01	0.06	0.38
$100 to $300 million	–0.12	–1.97	–0.16	–0.23	–0.11	–0.03	1.85
	0.00	0.00	0.00	0.00	0.02	0.27	0.00
$50 to $100 million	–0.16	–2.24	–0.23	–0.19	–0.03	–0.18	4.26
	0.00	0.00	0.00	0.00	0.30	0.00	0.00
Less than $50 million	–0.18	–2.48	–0.31	–0.14	–0.05	–0.19	10.30
	0.00	0.00	0.00	0.04	0.23	0.00	0.04

NOTE: ROA* (ROE*): Difference between mean ROA (ROE) of C-banks and mean adjusted ROA (ROE) for S-banks; *p*-values for the hypothesis test are below the differences. Sample includes only those banks in peer groups 1 through 15 of the Uniform Bank Performance Report. Data exclude banks with ROA among the largest or smallest 1 percent of observations. Bank size groups are based on total end-of-period assets.

than C-banks of similar size, and the differences are statistically significant for banks with less than $300 million of assets.[14] S-banks also tend to have higher net interest margins (i.e., net interest income divided by average earning assets) than C-banks, as reflected in higher mean values across all size groups.[15] For 2005, the differences in the means are statistically significant for banks in the three largest size groups. However, for banks with less than $100 million of assets, we cannot reject the hypothesis that mean values of net interest margins of S- and C-banks are equal. Although for other years we also find that S-banks tend to have higher mean net interest margins than C-banks, the differences in the means are often not statistically significant, especially for the smallest banks.

We also compare non-interest margins (i.e., net non-interest income divided by average total assets) across S- and C-banks. For banks with less than $100 million of assets, S-banks consistently have higher mean non-interest margins than C-banks. However, for larger banks, especially those with more than $300 million of assets, we find that S-banks tend to have lower mean non-interest margins than C-banks, and the difference is statistically significant in some years.[16]

Finally, we compare the cost efficiency of S- and C-banks using the efficiency ratio (i.e., total overhead expenses as a percentage of net interest income plus non-interest income). Except for banks with at least $1 billion of assets, we find that S-banks consistently have lower efficiency ratios than C-banks (implying that S-banks are more cost efficient). Mean values are significantly smaller for S-banks with less than $300 million of assets than for C-banks of similar size. We also find that S-banks tend to have smaller mean efficiency ratios than C-banks in other years, though the differences are consistently statistically significant only for banks with less than $100 million of assets. Hence, it appears that relatively low overhead expenses can account for at least part of the higher profit rates of smaller S-banks as compared with C-banks. For S-banks with between $100 million and $300 million of assets, we find that both lower overhead expenses and higher net interest margins may play some role; whereas, for S-banks with between $300 million and $1 billion of assets, a higher net interest margin is more important for explaining the higher profit rates of S-banks.[17]

Taxes may account for some of the tendency for S-banks to have lower overhead expenses than C-banks of similar size, which further suggests caution when comparing either pre-tax or adjusted after-tax profit rates across S- and C-banks. S-banks are closely held corporations, and their senior managers often own a high percentage of the outstanding stock of the banks they manage. Owner/managers generally prefer to receive income in the form of earnings distributions rather than salary because salary is subject to employment taxes but other distributions are not. S-banks are required to pay reasonable compensation to shareholder-employees,[18] but the differential tax treatment of salary income and other distributions of S-bank earnings might help explain the tendency for S-banks to have relatively lower overhead expenses, and hence higher pre-tax operating profit rates, than C-banks.

Unfortunately, data on the salaries of shareholder-employees of banks are not available to test for differences in the compensation of owner/managers of S- and C-banks. The UBPR does provide data on total personnel expenses, however. We test whether lower personnel expenses can explain the higher mean pre-tax operating profit rates of S-banks. Table 5 reports mean values of

[14] For banks with assets between $300 million and $1 billion, this difference is statistically significant in some years between 1997 and 2004. For banks with less than $300 million, the difference is statistically significant in every year.

[15] The UBPR makes a tax-equivalent adjustment to net interest income and, hence, to net interest margin, to account for differences in the tax treatment of different assets that banks hold without regard to whether a bank is an S- or C-bank. The implications of this adjustment are examined in a later section.

[16] Because there were very few S-banks with more than $1 billion of assets, especially before 2001, differences in the mean values for S- and C-banks in this size range are not especially interesting.

[17] The UBPR does not include data on the efficiency ratio before 2000. In addition, for banks with between $300 million and $1 billion of assets, in some years, the differences between mean values for S- and C-banks of net interest margin, and of net pre-tax operating profit, are not statistically significant.

[18] See Hritz (2005).

Table 6

2004 Performance of C-Banks that Became S-Banks in 2005

Bank size group	ROA	ROE	Pre-tax operating profit/assets	Net interest margin	Cost efficiency	Number of banks
Performance of C-banks that became S-banks in 2005 (mean values of various performance measures in 2004)						
More than $1 billion	1.22	13.15	1.91	4.17	62.07	3
$300 million to $1 billion	1.23	14.62	1.83	4.44	58.37	9
$100 to $300 million	1.06	11.06	1.55	4.38	62.79	40
$50 to $100 million	1.06	10.58	1.57	4.43	62.96	40
Less than $50 million	0.91	8.19	1.24	4.32	70.18	35
Performance of C-banks that did not become S-banks in 2005 (mean values of various performance measures in 2004)						
More than $1 billion	1.22	13.53	1.81	3.82	57.77	398
$300 million to $1 billion	1.13	12.28	1.65	4.10	60.19	850
$100 to $300 million	1.03	10.63	1.48	4.25	63.11	1,701
$50 to $100 million	0.91	8.69	1.27	4.27	67.11	1,056
Less than $50 million	0.78	6.90	1.08	4.30	71.57	923
Means of non-converting banks minus means of converting banks (*p*-values for hypothesis tests of equal means)						
More than $1 billion	0.00	0.38	–0.10	–0.35	–4.29	
	0.35	0.35	0.33	0.14	0.30	
$300 million to $1 billion	–0.10	–2.34	–0.18	–0.34	1.83	
	0.26	0.14	0.23	0.06	0.30	
$100 to $300 million	–0.03	–0.43	–0.07	–0.13	0.32	
	0.36	0.32	0.30	0.16	0.39	
$50 to $100 million	–0.16	–1.89	–0.30	–0.16	4.15	
	0.02	0.01	0.00	0.10	0.03	
Less than $50 million	–0.12	–1.28	–0.16	–0.02	1.39	
	0.08	0.09	0.12	0.39	0.32	

NOTE: Includes only those banks in peer groups 1 through 15 of the Uniform Bank Performance Report. Data exclude banks with ROA among the largest or smallest 1 percent of observations.

the sum of pre-tax net operating profit (as a percentage of average total assets) plus personnel expenses (also as a percentage of average total assets) for banks in the five size groups. If lower personnel expenses account for the higher pre-tax operating profit of S-banks, we would expect to fail to reject the hypotheses that the mean values of the sum of personnel expenses and pre-tax net operating profit are equal for S- and C-banks. However, we reject the hypothesis at standard significance levels for banks in all size groups, indicating that lower personnel expenses cannot account fully for the higher mean pre-tax operating profit rates of S-banks.[19]

Ex Ante Performance of S-Banks

We have been unable to identify definitively why S-banks tend to earn more than C-banks of similar size. Therefore, we next investigate the C-banks that have become S-banks and whether

[19] For banks with $300 million or more of assets, we cannot reject the hypothesis in some years. However, we always reject the hypothesis for banks with less than $300 million of assets.

Table 7

Means of Non-Converting Banks Minus Means of Converting Banks (*p*-values for hypothesis tests of equal means)

	2005			2004			2003			2002		
Bank size group	ROA	ROE	N*	ROA	ROE	N*	ROA	ROE	N*	ROA	ROE	N*
More than $1 billion	0.00	0.38	3	n/a	n/a	0	n/a	n/a	0	n/a	n/a	0
	0.35	0.35		n/a	n/a		n/a	n/a		n/a	n/a	
$300 million to $1 billion	–0.10	–2.34	9	–0.06	1.47	7	0.02	0.47	14	–0.07	–2.23	15
	0.26	0.14		0.29	0.15		0.38	0.33		0.28	0.09	
$100 to $300 million	-0.03	–0.43	40	–0.15	–1.87	39	–0.11	–1.39	59	–0.02	–0.29	42
	0.36	0.32		0.01	0.02		0.09	0.06		0.36	0.36	
$50 to $100 million	–0.16	–1.89	40	–0.12	–1.53	40	–0.23	–2.82	43	–0.23	–2.96	66
	0.02	0.01		0.10	0.09		0.00	0.00		0.00	0.00	
Less than $50 million	–0.12	–1.28	35	–0.27	–3.03	45	–0.17	–1.80	54	–0.18	–1.99	78
	0.08	0.09		0.00	0.00		0.03	0.02		0.00	0.00	

NOTE: N*: Number of C-banks converting to S-banks in given year. Includes only those banks in peer groups 1 through 15 of the Uniform Bank Performance Report. Data exclude banks with ROA among the largest or smallest 1 percent of observations. Bank size groups are based on total end-of-period assets.

they had higher rates of return than other C-banks *before* they became S-banks. If so, it would suggest that at least some of the tendency for S-banks to have higher rates of return than C-banks might be due to inherent characteristics rather than their status as S-banks.

Table 6 presents summary data on several financial ratios for banks that converted to S-banks during 2005. The table reports mean values of various performance measures as of year-end 2004 for C-banks that converted to S-bank status during 2005, as well as for C-banks that remained C-banks in 2005. The table also reports the differences in the mean values for converting and non-converting banks and *p*-values for tests of the hypothesis that the means of converting and non-converting banks are equal. Only three banks with more than $1 billion of assets became S-banks in 2005. Among smaller banks we find a tendency for converting banks to have had higher rates of return during 2004 than non-converting banks. Converting banks with less than $100 million of assets had significantly higher ROA, ROE, and pre-tax operating-profit rates during 2004 than did non-converting banks. Converting S-banks with between $300 million and $1 billion of assets had significantly higher net interest margins than similar-sized non-converting banks; converting banks with between $50 million and $100 million of assets had significantly lower cost efficiency ratios (i.e., they were more cost efficient).

Table 7 reports data for other years; specifically, the table shows the differences in the mean values of ROA and ROE between non-converting and converting banks in the indicated years. As in Table 6, the mean values used to prepare Table 7 are as of December 31 of the year prior to conversion, and the differences shown are the mean values for non-converting banks less the mean values for converting banks. As shown in the table, the banks that converted to S-bank status in a given year tended to have higher ROA and ROE in the year before they converted to S-bank status than the banks that did not convert; in several cases the differences in the means are statistically significant. Hence, it appears that characteristics other than S-bank status explain at least some of the tendency for S-banks to outearn C-banks of similar size. Banks that choose to switch to S-bank status appear to be systematically different from those of similar size that do not elect S-status.

Table 7, cont'd

2001			2000			1999			1998			1997		
ROA	ROE	N*	ROA	ROE	N*	ROA	ROE	N*	ROA	ROE	N*	ROA	ROE	N*
n/a	n/a	0	n/a	n/a	0	–0.22	–1.82	1	–0.42	–1.19	1	–0.25	–1.59	1
n/a	n/a		n/a	n/a		n/a	n/a		n/a	n/a		n/a	n/a	
–0.08	–0.73	9	–0.23	–2.56	3	0.27	1.51	5	–0.25	–2.96	12	0.07	1.66	6
0.28	0.34		0.14	0.15		0.04	0.19		0.04	0.11		0.37	0.33	
–0.14	–1.05	41	–0.04	–0.31	37	–0.05	–0.58	58	–0.09	–0.54	75	–0.19	–1.92	86
0.06	0.20		0.29	0.35		0.25	0.26		0.07	0.24		0.00	0.00	
–0.22	–2.17	58	–0.10	–0.69	55	–0.10	–2.06	90	–0.17	–1.76	131	–0.17	–2.68	174
0.00	0.00		0.16	0.27		0.02	0.00		0.00	0.00		0.00	0.00	
–0.18	–2.27	96	–0.21	–1.72	93	–0.18	–1.78	109	–0.14	–1.61	224	–0.18	–1.93	299
0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00	

PRE-TAX EARNINGS AS AN ALTERNATIVE EARNINGS MEASURE

The pitfalls of comparing banks on the basis of after-tax measures of return caused by the proliferation of S-banks have led some analysts and regulators to use pre-tax profit measures. For example, the FDIC uses income before taxes and extraordinary charges (as a percentage of total assets) in its statistical model designed to identify banks whose financial condition has deteriorated significantly since its last on-site examination (Collier et al., 2003).[20] Presumably, however, banks seek to maximize after-tax profit, and pre-tax profit is not necessarily a good measure of a bank's performance. Many banks invest substantial proportions of their assets in securities that yield tax-exempt income. By holding large amounts of tax-advantaged securities, a bank could appear relatively unprofitable on a pre-tax basis but highly profitable on an after-tax basis.

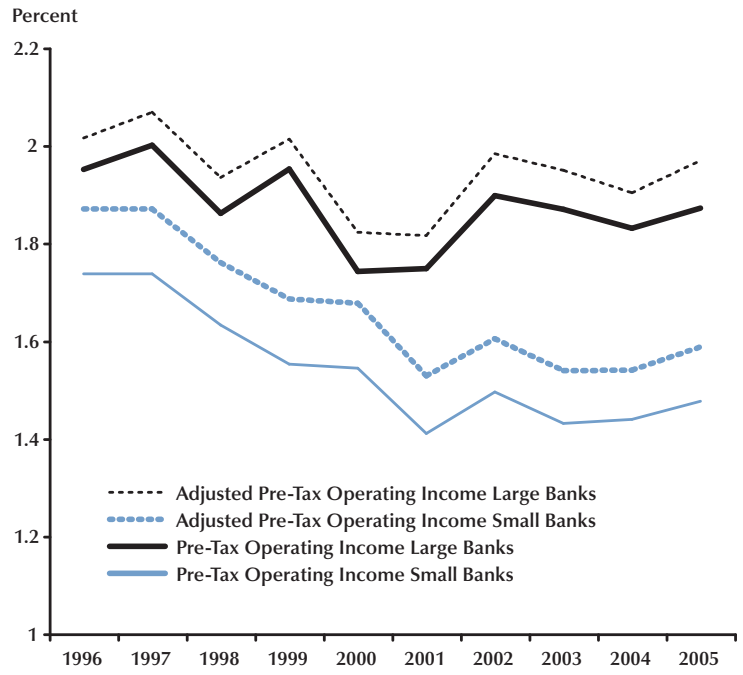
The UBPR includes an adjustment to make pre-tax operating profits more comparable across banks with different mixes of taxable and tax-exempt securities.[21] Figure 4 shows the impact of this adjustment on median pre-tax net operating income divided by average total assets for large and small banks, where, as before, large banks are defined as those with $1 billion or more of assets and small banks are those with less than $1 billion of assets. The figure shows that over the 10-year period from 1996 to 2005, the median pre-tax net operating-income rate of large banks consistently exceeded that of small banks. Further, the figure shows the impact of the adjustment of pre-tax operating income rates for tax-exempt income. The dotted lines show median pre-tax net operating income rates with the adjustment for tax-exempt income. Over the 10-year period, the adjustment contributed between 0.06 and 0.10 percentage points to the median rate for large banks and between 0.10 and 0.13 percentage

[20] The Federal Reserve uses a similar model (Board of Governors of the Federal Reserve System, 2006). See also Whalen (2005).

[21] Pre-tax net operating income (TE) equals net interest income (on a tax-equivalent basis) plus non-interest income and realized gains (or losses) on securities, less non-interest expenses, provisions for loan and lease–financing receivables losses, and provisions for allocated transfer risk. See FFIEC (2006, Section III, p. 4).

Figure 4

Impact of Adjustment for Tax-Exempt Income on Pre-Tax Net Operating Income/Assets



points to the median rate for small banks. Although the impact of the adjustment on pre-tax net operating-income rates has typically been somewhat larger for small banks than for large banks, the adjustment added approximately 0.10 percentage points to the median pre-tax net operating-income rates of both large and small banks in 2005.

CONCLUSIONS

The proliferation of banks that elect subchapter S tax treatment has greatly complicated the meaningful comparison of banks on the basis of after-tax rates of return. Because S-bank earnings are not subject to the federal corporate income tax, S-banks generally have higher after-tax rates of return than other commercial banks (i.e., C-banks). However, S-bank shareholders face a personal income tax liability for their pro rata share of the bank's entire earnings—not just the portion distributed as dividends. S-banks have proliferated, however, because the dividends that they pay to shareholders are not taxed twice. S-banks are permitted to have no more than 100 shareholders, which generally restricts the election of S-status to small banks that do not anticipate rapid growth and whose shares do not trade publicly.

In an attempt to make after-tax earnings rates of S-banks comparable with those of C-banks, the Uniform Bank Performance Report produced by the Federal Financial Institutions Examination Council includes estimates of the federal corporate income taxes that S-banks would pay if subject to that tax. Using these estimates, this article shows that the different federal tax treatments of S- and C-banks has a quantitatively large impact on comparisons of mean after-tax profit rates across banks. Because most S-banks are smaller institutions, comparisons of mean after-tax rates of return across groups of different-size banks are especially problematic. If S-bank earnings are *not*

adjusted to make them comparable with C-bank earnings, we find that mean earnings rates of groups of best-practice small banks compare favorably with mean earnings rates of large banks, similar to the results of DeYoung, Hunter, and Udell (2004). However, we also find that mean earnings rates of best-practice small banks are considerably lower if S-bank earnings are adjusted by estimates of federal income taxes, indicating that conclusions of studies that use net after-tax income as a measure of performance can be affected markedly by whether or not S-bank earnings rates are adjusted for taxes.

Our research also finds that S-banks tend to have higher rates of return than C-banks of similar size even when S-bank earnings rates are adjusted by the UBPR estimates of their hypothetical federal corporate income taxes. Smaller S-banks also tend to have higher pre-tax net operating income rates than similar-sized C-banks, mainly because of lower expenses and higher ratios of net noninterest income to assets, whereas larger S-banks tend to have higher net interest margins than C-banks of similar size. Owner/managers of S-banks generally prefer to receive income in the form of distributed earnings, rather than salary, to limit employment taxes. However, we find that lower personnel expenses do not explain fully the tendency for S-banks to have higher pre-tax net operating income rates than C-banks. Finally, we find that C-banks that became S-banks in a given year tended to have higher after-tax rates of return than other C-banks in the year *before* they became an S-bank. This result suggests that characteristics other than election of subchapter S tax status account for some of the tendency for S-banks to out-earn C-banks. The banks that choose S-bank tax status appear to be systematically different from other banks of similar asset size.

The growth in the number of banks electing subchapter S tax treatment has seriously compromised the usefulness of standard after-tax return measures, such as ROA and ROE, for comparing profit rates across banks, and undoubtedly explains the increasing use of pre-tax earnings measures in studies of bank performance. Our study does not show that any particular measure of return is superior for comparing the profit rates of different banks, as the ideal measure largely depends on the question at hand. The evidence reported here indicates that researchers and other analysts should exercise caution when using any profit measure to evaluate bank performance, however, particularly in light of the proliferation of S-banks.

REFERENCES

Bassett, William F. and Brady, Thomas F. "The Economic Performance of Small Banks, 1985-2000." *Federal Reserve Bulletin*, November 2001, *87*(11), pp. 719-28.

Board of Governors of the Federal Reserve System. *Commercial Bank Examination Manual*. Washington, DC: 2006; www.federalreserve.gov/boarddocs/SupManual.

Collier, Charles; Forbush, Sean; Nuxoll, Daniel A. and O'Keefe, John. "The SCOR System of Off-Site Monitoring: Its Objectives, Functioning, and Performance." *FDIC Banking Review*, 2003, *15*(3), pp. 17-32.

DeYoung, Robert; Hunter, William C. and Udell, Gregory F. "The Past, Present, and Probable Future for Community Banks." *Journal of Financial Services Research*, April/June 2004, *25*(2/3), pp. 85-133.

Federal Financial Institutions Examination Council. *A User's Guide for the Uniform Bank Performance Report*. Arlington, VA: March 2006.

Harvey, James and Padget, Jane. "Subchapter S—A New Tool for Enhancing the Value of Community Banks." Federal Reserve Bank of Kansas City *Financial Industry Perspectives*, December 2000, pp. 17-31.

Hein, Scott E.; Koch, Timothy W. and MacDonald, S. Scott. "On the Uniqueness of Community Banks." Federal Reserve Bank of Atlanta *Economic Review*, First Quarter 2005, *90*(1), pp. 15-36.

Hritz, Steven R. "Reasonable Compensation and SE Taxes." *Tax Advisor*, October 2005, pp. 608-10.

Keeton, William; Harvey, James and Willis, Paul. "The Role of Community Banks in the U.S. Economy." Federal Reserve Bank of Kansas City *Economic Review*, Second Quarter 2003, *88*(2), pp. 15-43.

Landau, Zev. "Recent Reform and Simplifications for S Corporations." *CPA Journal*, November 2005, *75*(11), pp. 46-50.

Whalen, Gary. "A Hazard Model of CAMELS Downgrades of Low-Risk Community Banks." Working Paper No. 2005-1, Office of the Comptroller of the Currency, May 2005.

The Determinants of Aid in the Post-Cold War Era

Subhayu Bandyopadhyay and Howard J. Wall

The authors estimate the responsiveness of aid to recipient countries' economic and physical needs, civil/political rights, and government effectiveness. They look exclusively at the post-Cold War era and use fixed effects to control for the political, strategic, and other considerations of donors. They find that aid and per capita income have been negatively related, while aid has been positively related to infant mortality, rights, and government effectiveness. (JEL F35)

Federal Reserve Bank of St. Louis *Review*, November/December 2007, *89*(6), pp. 533-47.

This paper estimates the extent to which aid, or official development assistance, is related to measures of recipient countries' physical and economic needs, civil/political rights, and government effectiveness. We examine the post-Cold War era, which thus far has not been the focus of substantial research, although there are fairly obvious reasons to believe that the differences in the geopolitics between the pre- and post-Cold War eras amount to a structural difference in terms of aid allocation.

There are many reasons why we should be interested in the determinants of aid levels. First, because aid is an important means by which donor countries and agencies try to alleviate poverty, we should care about whether aid is being directed towards those most in need of it. Similarly, we should also be interested in whether aid tends to go more towards where it might be most effective, as measured by the effectiveness of the recipient government in making use of the aid or in fostering economic growth.[1]

Early studies of aid allocation tend to apply some version of the McKinlay and Little (1979) dichotomy—recipient needs versus donor interests—to models of aid allocation. As laid out by Maizels and Nissanke (1984, p. 879), in the recipient-needs model, "aid is given to compensate for the shortfalls in domestic resources," whereas in the donor-interests model, aid serves donors' "political/security, investment, and trade interests." Maizels and Nissanke found that multilateral aid tended to follow the recipient-needs model, while bilateral aid tended to follow the donor-interests model, although there were elements of each model in both types of aid.[2]

Subsequent research has added two other categories—civil/political rights and recipient-country institutions—to the McKinlay and Little dichotomy, although not all papers deal with all four categories simultaneously.[3] For example,

[1] See Boone (1996) and Kosack (2003) for discussions of the links between institutions and aid effectiveness. Also, in Burnside and Dollar (2000 and 2004) the impact of aid on growth depends on the quality of recipient-state institutions and policies; although Easterly, Levine, and Roodman (2004) and Rajan and Subramanian (2005) found little or no evidence of this.

[2] See also Dowling and Hiemenz (1985).

[3] Neumayer (2003b) provided an excellent survey of the literature.

Subhayu Bandyopadhyay was an associate professor at West Virginia University and a research fellow at IZA (Institute for the Study of Labor) at the time this paper was written; he is now a research officer at the Federal Reserve Bank of St. Louis. Howard J. Wall is an assistant vice president and economist at the Federal Reserve Bank of St. Louis. This paper was written while Howard Wall was a visiting scholar at the Institute for Economic and Monetary Studies at the Bank of Japan; he is grateful for their resources and hospitality. The authors also thank Kristie Engemann for her research assistance. This article was published as chapter 17 in *Theory and Practice of Foreign Aid*, volume 1 (edited by Sajal Lahiri and published by Elsevier in May 2007). It is reprinted here with permission from the publisher.

Wall (1995) found that countries with lower per capita incomes tended to receive higher levels of aid per capita, although aid was not related to infant mortality or to civil/political rights. On the other hand, Trumbull and Wall (1994) found that, when recipient-country fixed effects are included to control for donor interests, aid levels respond to changes in infant mortality and rights, but not to changes in per capita income.

Alesina and Dollar (2000) included a variety of variables, such as trade openness, colonial history, and friendliness at the United Nations (UN), to capture the effects of donor interests. They concluded that, although aid is related to per capita income and democracy (but not to civil rights), it is as much directed by political and strategic considerations. A pair of recent studies focus on the institutions of the recipient countries: Alesina and Weder (2002) found that corrupt governments do not tend to receive less aid than "clean" governments, and Dollar and Levin (2004) found that, over time, aid has become directed more towards countries with sound institutions and policies, although there were differences across bilateral donors and multilateral agencies.

In a series of papers, Eric Neumayer provided a detailed analysis of the relationship between aid and civil/political rights.[4] In Neumayer (2003a), UN agencies were found to respond to economic and possibly civil/political-development needs, but not necessarily to political freedom and corruption. There is some evidence in Neumayer (2003b) that high levels of rights or improvements in rights mean higher bilateral aid, but Neumayer concluded that the role of rights is limited and did not increase after the end of the Cold War. Finally, Neumayer (2003c) found that although respect for rights tends to play a role at the selection stage, there is significant inconsistency in the application of rights to the determination of the levels of bilateral aid.

This paper focuses on three of the four categories of aid determinants—recipient needs, civil/political rights, and recipient-government effectiveness—while following Trumbull and Wall (1994) in using fixed effects to control for the fourth category, the strategic and political interests of donor countries. The advantage of this approach is that, because we do not have to choose strategic/political variables explicitly, we avoid the problems that can arise if there are excluded variables that determine both the level of aid and one or more of our other explanatory variables. This means that we do not run the risk of heterogeneity bias because of omitted time-invariant factors related to history, geography, culture, etc. If these factors, which are primarily the sort of factors that are used to measure donor interests, are not completely specified and they are correlated with aid and one or more of the included explanatory variables, then heterogeneity bias is the result. The relative shortness of our sample provides comfort that fixed effects provide a useful control for donor interests.

While our fixed-effects approach follows Trumbull and Wall (1994), there are two main differences between our analysis and theirs. The first and more obvious difference is that we are able to look at a more recent time period, so our results should be more relevant for understanding the present situation. Second, because we use a quadratic rather than a log-linear functional form, we are able to provide a richer analysis of the functional relationship between aid and the variables of interest.

EMPIRICAL MODEL AND DATA

Our dependent variable, Aid_{it}, is real net official development assistance from all sources for country i in year t. Data are taken from the World Bank and are denominated in constant 2000 U.S. dollars. We estimate the following reduced-form regression, in which i denotes the recipient country and t denotes time:

$$
\begin{aligned}
Aid_{it} &= \alpha_0 + \alpha_i + \gamma_t \\
&+ \beta_1 GDPpercapita_{it} + \beta_2 GDPpercapita_{it}^2 \\
&+ \delta_1 InfantMortality_{it} + \delta_2 InfantMortality_{it}^2 \\
&+ \lambda Civil\,/\,PoliticalRights \\
&+ \omega GovernmentEffectiveness \\
&+ \theta_1 Population_{it} + \theta_2 Population_{it}^2 + \varepsilon_{it}.
\end{aligned}
$$

[4] See also Neumayer (2003d).

The intercept includes a component, α_0, that is common to all recipient countries, and a recipient-country fixed effect, α_i, that is specific to each recipient country but fixed over the sample period. We also include a period effect, γ_t, that is common to all countries in the sample but varies over time. Our two recipient-needs variables are real gross domestic product (GDP) per capita and infant mortality, both of which are from the World Bank.[5] We think it is important to include both of these variables because each captures a different element of recipient need: Per capita income captures economic need while infant mortality represents physical need. Although clearly correlated in the long run, economic and physical needs do not necessarily move in the same direction over shorter periods of time, and aid is clearly meant to respond to both.

For our rights variable, we use the sum of the civil liberties and political rights indices produced by Freedom House. For each category, the Freedom House index scores countries from 1 to 7, with 1 being the most free and 7 being the most restrictive. For the regression here, we have reversed the order, so that the level of rights increases with the index. Our measure of recipient-government effectiveness is from the World Bank's Governance Indicators (see Kaufmann, Kraay, and Mastruzzi, 2006), which scores governments between –2.5 and 2.5 on the basis of the competence of their bureaucracy and the quality of public service delivery. Finally, we include recipient-country population to capture differences in recipient-country size. The quadratic specification enables us to consider the extent of population bias, by which the per capita aid allocation falls with country size: A concave relationship between the level of aid and population is consistent with a population bias.

We have three years of data, 1995, 2000, and 2003. After eliminating observations for which data are incomplete and countries for which there are fewer than two useful observations, we are left with 135 recipient countries and 395 observations. The sample statistics for all variables are provided in Table 1, and the country averages of the variables are provided in the data appendix.

Table 1

Sample Statistics

	Mean	Standard deviation
Real aid ($ millions)	356.93	439.26
Real GDP per capita ($ thousands)	4.96	4.54
Infant mortality	52.33	39.56
Civil/political rights	8.29	3.39
Government effectiveness	–0.30	0.67
Population (millions)	36.25	139.51

The distribution of average aid to countries in our sample is illustrated by Figure 1. The mean country in our sample received $357 million per year in aid, although the median country, Yemen, received only $226 million, indicating that aid was skewed toward a few countries. Specifically, there were 13 countries that received more than $1 billion in aid per year, the top five of which were China, Poland, Congo, Indonesia, and Russia. At the other extreme, four countries in our sample—Singapore, the Bahamas, St. Kitts, and Kuwait—averaged less than $10 million in aid receipts per year.

Figure 2 provides a different angle on the distribution of aid across countries by showing the shares of total aid received. The three countries receiving the most aid—China, Poland, and Congo—alone accounted for 13 percent of the total. These countries plus the 10 countries that received between $900 million and $1,800 million per year accounted for a larger share of aid (40 percent) than did the 102 countries that received less than $450 million per year.

To get a clear picture of how aid is distributed, we need to control for the sizes of the recipient countries, so Figure 3 plots the within-country averages of our explanatory variables against per capita aid. These plots serve to illustrate the simple correlations between the dependent and independent variables as well as the distribution of the values of our independent variables.

[5] Per capita GDP is converted into U.S. dollars using purchasing-power-parity exchange rates.

Figure 1

Distribution of Average Aid

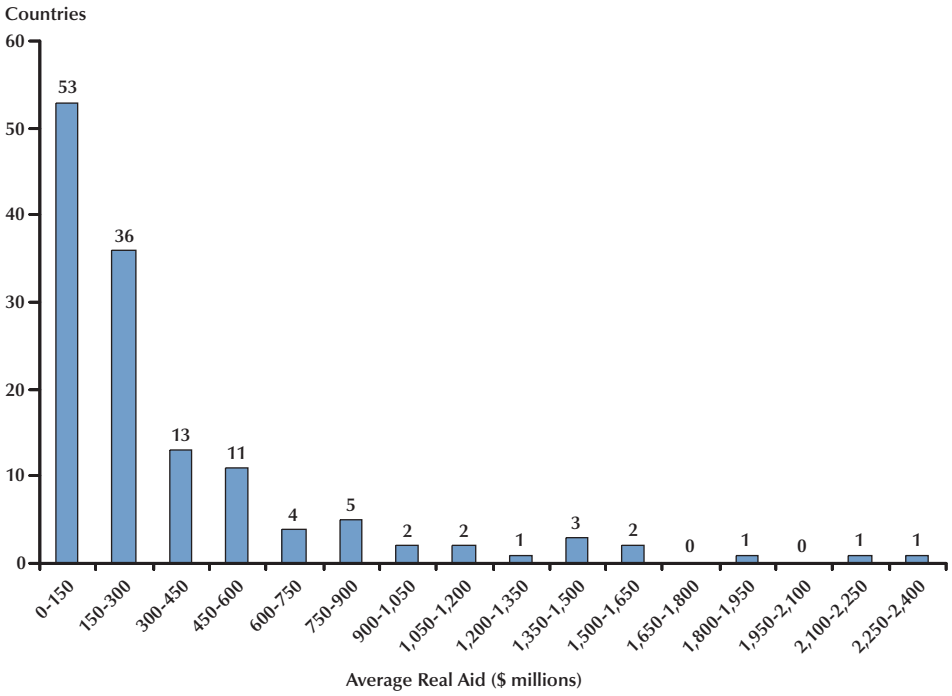


Figure 2

Shares of World Aid

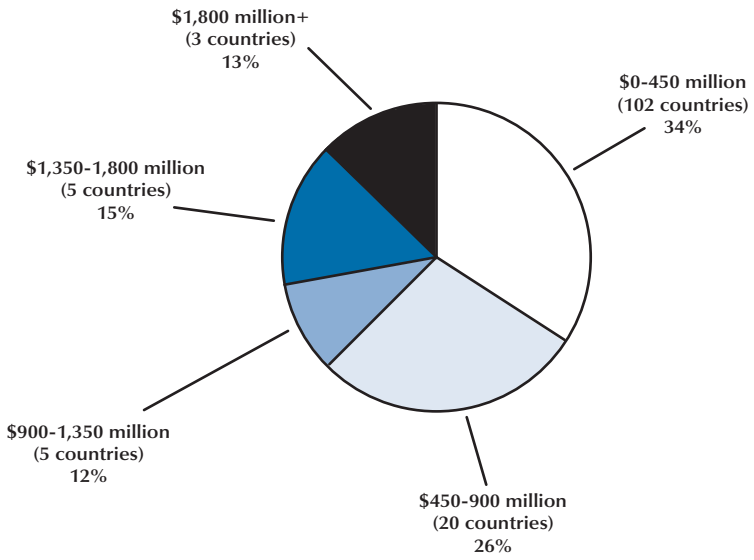
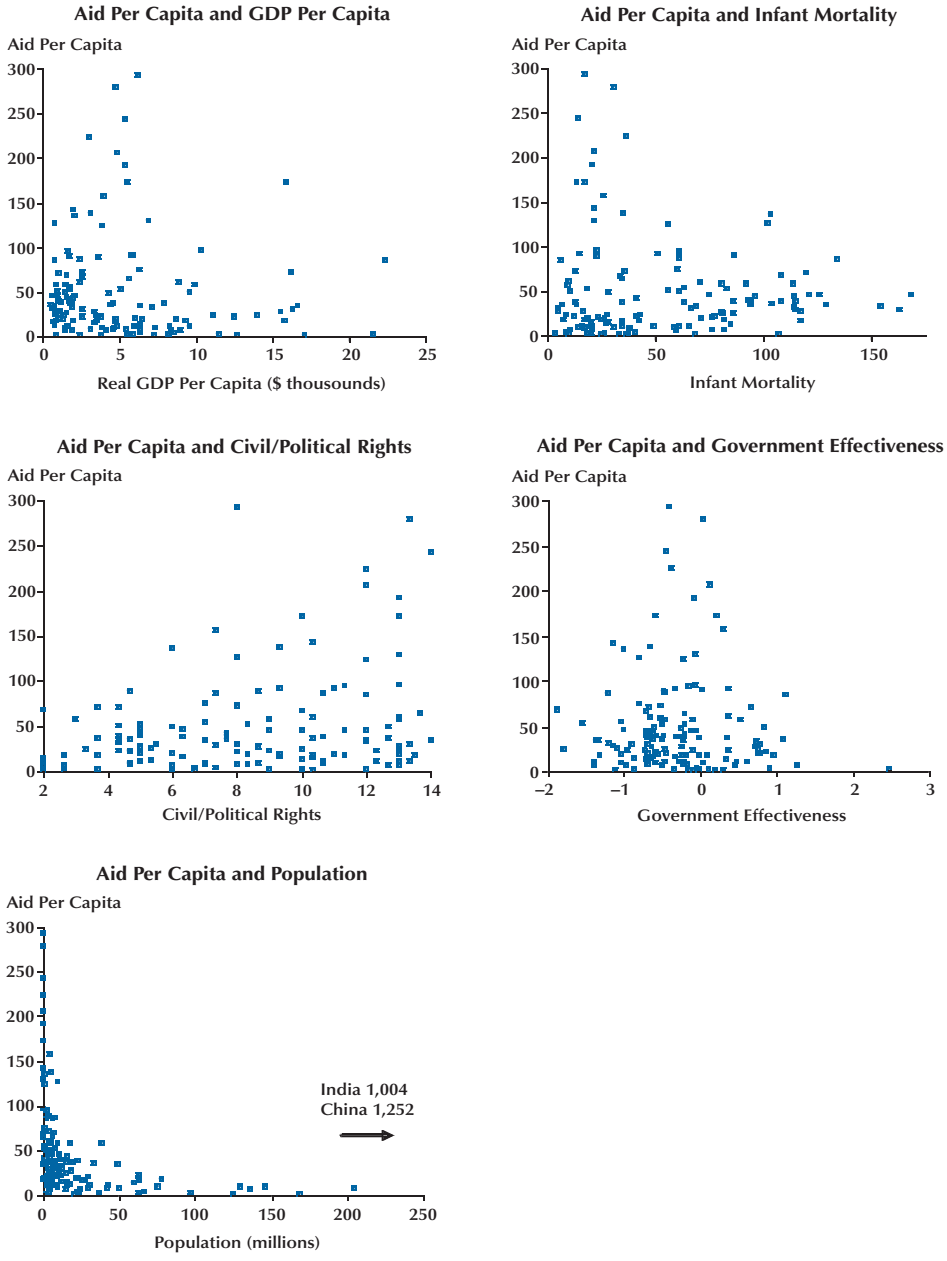


Figure 3

Aid Per Capita and the Explanatory Variables (country averages)



Note that the vast majority of our recipient countries had average per capita incomes around or below $10,000, although there were nine countries with average incomes above $15,000: Israel and Singapore were the richest of these countries, followed by Kuwait, Malta, Slovenia, Bahrain, Seychelles, the Bahamas, and the Czech Republic. There was a general tendency for relatively poor countries to receive more aid per capita, but some countries' receipts were well in excess of the sample average. For example, eight countries—Tonga, Cape Verde, Dominica, Vanuatu, Samoa, St. Vincent, St. Lucia, and Seychelles—saw average per capita aid that was more than two standard deviations above the mean. At the other extreme, six countries—Nigeria, China, Brazil, Kuwait, India, and Saudi Arabia—received less than $2 per capita.

From the second panel in Figure 3, it is clear that the eight countries listed above as having the highest per capita aid allocation also tended to have relatively low rates of infant mortality. Also note from this panel that there was a negative correlation between average per capita aid and infant mortality; the three countries with the highest average infant mortality rates—Sierra Leone, Niger, and Angola—received only about the average level of aid per capita.

As the third panel of Figure 3 shows, our civil/political rights variable was pretty evenly distributed across the countries in our sample, and there was a general positive correlation between per capita aid allocation and rights. In fact, of the eight countries listed above as receiving the most aid per person, only two—Tonga and Seychelles—had civil/political rights scores below 12. According to the fourth panel, there was no apparent correlation between aid per capita and the effectiveness of recipient-country governments. Also, the governments were clustered below the mediocre score of +1, with Singapore as the lone really effective government. Still, there is significant variation among countries, with many scoring worse than –1. Finally, consistent with the notion of population bias, the fifth panel illustrates the tendency for the smallest (largest) countries to receive the highest (lowest) levels of aid per capita.

EMPIRICAL RESULTS

While the distributions and correlations discussed above are suggestive, they are, of course, inadequate for addressing whether aid is responsive to needs, rights, government effectiveness, and/or donor interests. Therefore, we need to control for all four categories of variables simultaneously, as in our regression equation above, to determine the influence of each category individually on aid.

We first estimate the model under the restriction that fixed effects, which we use to control for donor interests and other omitted factors, do not matter ($\alpha_i = 0 \forall i$), and then without these restrictions. So that we can control for recipient-specific heteroskedasticity, we estimate both models with feasible generalized least squares. Table 2 provides the regression results for both models, while Table 3 provides the Wald tests for the joint significance of those explanatory variables with quadratic specifications. For each estimation, we have produced a set of figures (Figures 4 and 5) to illustrate the shapes of the estimated relationships between aid and the five explanatory variables. Table 4 reports for the two models the effect on aid of one-standard-deviation increases in each of the five explanatory variables for the average country.

Model without Fixed Effects

In the estimation without fixed effects, the effects of all of our explanatory variables except for the civil/political rights variable are statistically different from zero. This is according to the *t*-statistics for the coefficients on the variables with linear specifications and according to the Wald tests in Table 3 for the variables with quadratic specifications. Thus, according to this model, the level of aid is responsive to recipient needs (as measured by per capita income and by infant mortality), the effectiveness of recipient-country governments, and population, but not to civil/political rights.

For the nature of these relationships, refer to Figure 4, which illustrates the U-shapes of the relationships between aid and both needs variables; i.e., from high levels of need (low income

Table 2

Regression Results: Dependent Variable = Level of Real Aid

	No fixed effects			With fixed effects		
	Coefficient	Standard error	*t*-Statistic	Coefficient	Standard error	*t*-Statistic
Common intercept	564.693*	48.850	11.56	400.684*	126.088	3.18
Recipient fixed effects	No			Yes		
2000 dummy	–56.913*	12.688	–4.49	–82.195*	6.984	–11.77
2003 dummy	–18.343	12.985	–1.41	–11.714	10.667	–1.10
Real GDP per capita	–78.178*	5.955	–13.13	–116.490*	8.848	–13.17
Real GDP per capita squared	2.646*	0.268	9.86	3.927*	0.387	10.14
Infant mortality	–3.053*	0.693	–4.41	3.632*	1.291	2.81
Infant mortality squared	0.022*	0.004	5.75	–0.015*	0.008	–1.95
Civil/political rights	0.212	1.841	0.12	8.940*	2.486	3.60
Government effectiveness	114.432*	13.934	8.21	82.453*	12.856	6.41
Population (millions)	7.497*	0.394	19.01	13.419*	2.815	4.77
Population squared	–0.005*	0.000	–10.78	–0.012*	0.002	–6.95
Log likelihood		–2,563.56			–2,264.07	
Number of observations		395			395	
Number of recipient countries		135			135	
Estimated coefficients		11			145	

NOTE: Estimated using feasible generalized least squares, allowing for recipient-specific heteroskedasticity; *indicates statistical significance at the 10 percent level.

Table 3

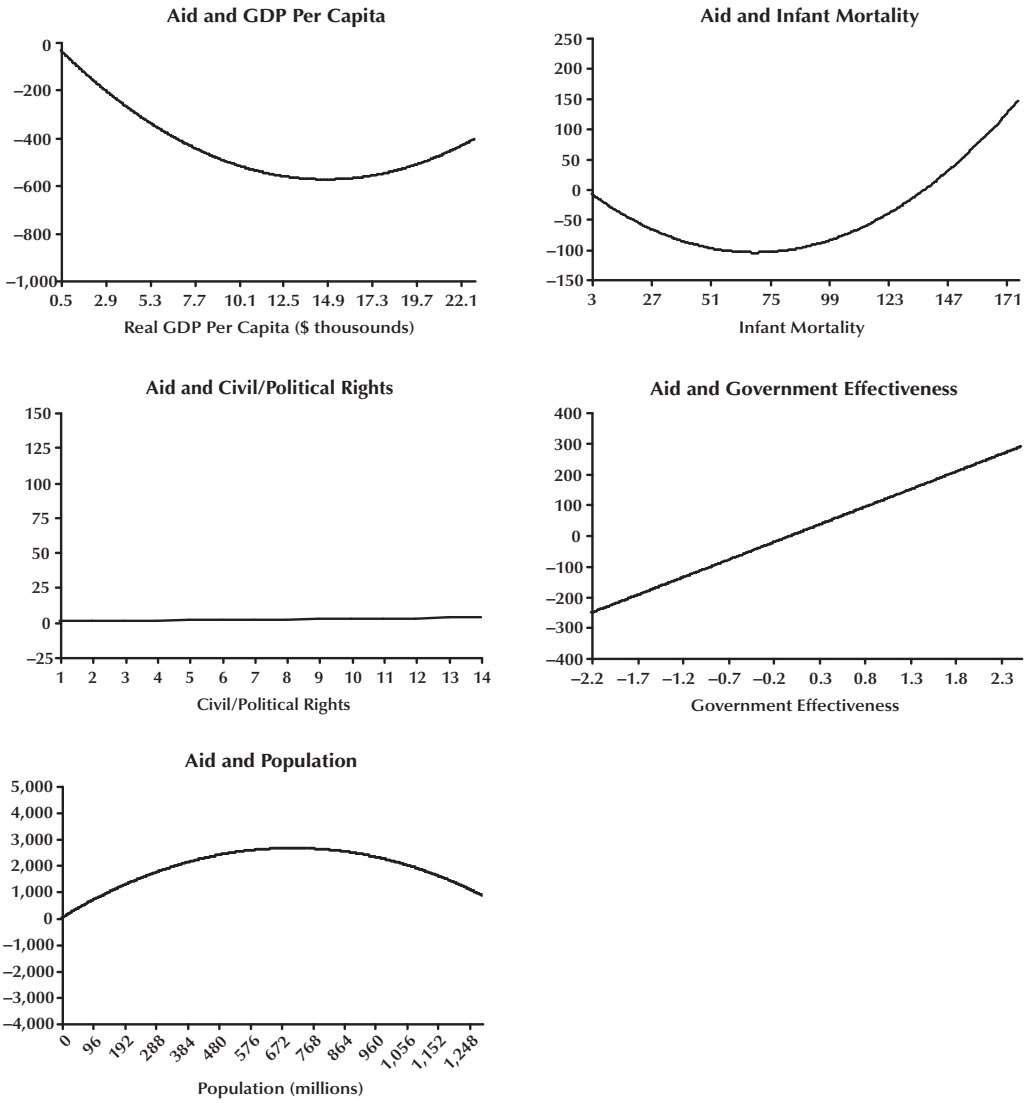
Wald Tests of Joint Significance

	No fixed effects		With fixed effects	
	χ^2	Probability > χ^2	χ^2	Probability > χ^2
Real GDP per capita	202.53	0.000	174.00	0.000
Infant mortality	46.40	0.000	8.37	0.015
Population	388.91	0.000	49.34	0.000

and high infant mortality), an increase in need brings an increase in aid. On the other hand, at low levels of need, an increase in need brings a decrease in aid. This rather peculiar result is not much of a concern when looking at per capita income, however, because there are very few countries with incomes on the upward-sloping portion of the relationship. As reported in Table 4, a one-standard-deviation increase in per capita GDP (about $4,500) from the average (about $5,000) means a decrease in aid of $90 million. The U-shape of the relationship for infant mortality is more troubling because the majority of countries have infant mortality levels that would place them on the downward-sloping portion of the relationship (see Figure 3). For example, for

Figure 4

Relationships without Fixed Effects

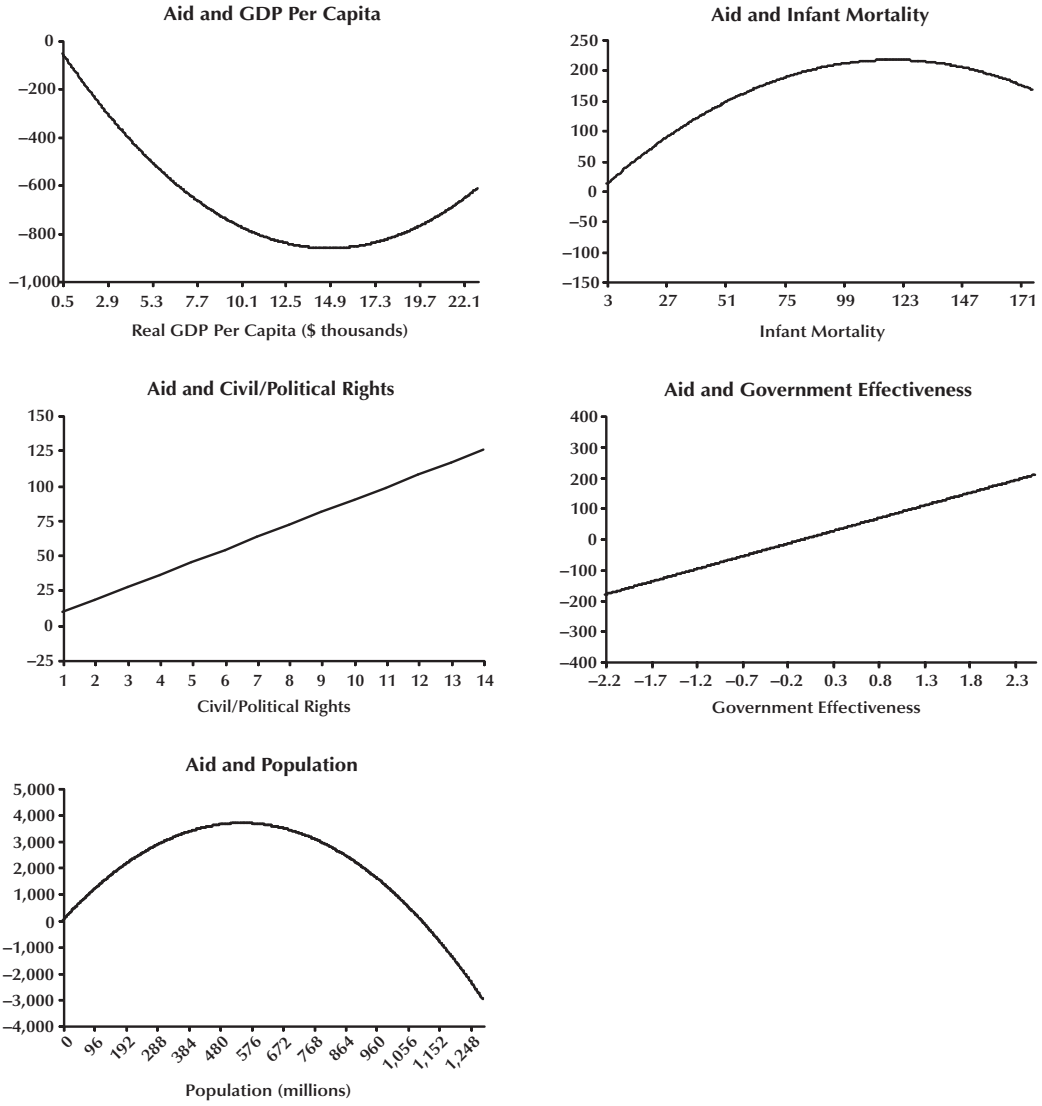


a country with the sample average rate of infant mortality (about 52), a one-standard-deviation increase in infant mortality (about 40) means a *decrease* in aid of $19 million.

The two other statistically significant explanatory variables are worth noting. First, in this model, aid is fairly responsive to government effectiveness: The difference between the least-effective government and the most-effective government is close to $550 million. Put another way, a one-standard-deviation increase from the average level of government effectiveness (–0.30) to the still-mediocre level of 0.37 means a $75 million increase in aid. And, finally, the hill shape of the relationship between aid and population confirms the oft-observed population bias; i.e., per capita aid falls with population. In fact, the bias is strong enough that for countries with

Figure 5

Relationships with Fixed Effects



populations above around 700 million (just India and China), an increase in population means a decrease in the level of aid, not just per capita aid.

Model with Fixed Effects

When we do not impose the restrictions that the fixed effects are all zero (i.e., the intercepts are the same for all recipients), we find that all five explanatory variables are statistically significant in explaining levels of aid. Further, a likelihood-ratio test easily rejects the null hypothesis that the fixed effects are all zero, meaning that this is the statistically preferred model. Because there are no theory-based reasons to impose these restrictions, it is also the preferred model in terms of theory. The rejection of these restrictions on the fixed effects has important implications for our interpretation of the relationships between

Table 4

Responsiveness of Aid to Explanatory Variables

	No fixed effects	With fixed effects
Real GDP per capita	–90	–135
Infant mortality	–19	27
Civil/political rights	1	29
Government effectiveness	75	54
Population	1,013	1,734

NOTE: Change in aid ($ millions) for the average country from a one-standard-deviation increase in the explanatory variable.

aid and the explanatory variables and highlights the importance of controlling for donor interests.

Comparing Figures 4 and 5, for which axes in corresponding figures have the same scale, it is clear that the estimated relationships between aid and each of the variables differ importantly between the two models. Even though per capita income, infant mortality, government effectiveness, and population are statistically significant in both, the actual responsive of aid differs between models.

The relationship between aid and per capita GDP has the same U-shape as in the previous model, with the upward-sloping portion having very few recipient countries. In this model, however, aid is more responsive to per capita income: A one-standard-deviation increase in per capita GDP means a $135 million decrease in aid for the average country, which is 50 percent higher than with the previous model (see Table 4).

The relationship between aid and infant mortality differs a great deal between the two models. Recall that in the first model, the relationship was U-shaped and most countries' levels of infant mortality put them on the downward-sloping portion of the curve. But in the preferred model, the relationship is hill-shaped and is upward-sloping for all but a handful of countries. For the average country, a one-standard-deviation increase in infant mortality means a $27 million increase in aid. One might expect that the relationship between aid and infant mortality, if positive, would be convex rather than concave as we have found. One reason for the concavity is that, while higher levels of infant mortality indicate greater need, they might also indicate health care systems that are less effective at making use of any money that they receive. If so, donors might then be allocating more of their limited aid budgets to countries with better health care systems, where each dollar of aid might have a larger impact on well-being. At the extreme, for those countries with the very highest levels of infant mortality and least effective health care systems, this concavity might make the relationship between aid and infant mortality a negative one.

An increase in the civil/political rights variable means an increase in aid according to the preferred model, in contrast with the no-fixed-effects model, for which it was statistically insignificant. A one-standard-deviation increase in civil/political rights means an increase in aid of $29 million. Recipient-government effectiveness matters in both models, although it matters somewhat less in the model with fixed effects. A one-standard-deviation increase in government effectiveness means a $54 million increase in aid, which is $21 million less than from the first model. Finally, because the estimated relationship between aid and population is concave, we find a population bias, which is somewhat larger than in the first model. Per capita aid falls more than twice as fast in this model, and the peak of the relationship is at a lower population level.

CONCLUSIONS

In this paper, we have estimated the responsiveness of total aid in the post-Cold War era to the needs, civil/political rights, and government effectiveness of recipient countries. To do so, we used the approach espoused in Trumbull and Wall (1994): to use fixed effects to control for donor interests. We have found that aid in this era generally responded negatively to per capita GDP and positively to infant mortality, rights, and government effectiveness. This is in contrast with much of the existing literature, which, while

tending to find a negative link between aid and per capita income, has been decidedly more mixed in terms of the other variables.

REFERENCES

Alesina, Alberto and Dollar, David. "Who Gives Foreign Aid to Whom and Why?" *Journal of Economic Growth*, March 2000, *5*(1), pp. 33-63.

Alesina, Alberto and Weder, Beatrice. "Do Corrupt Governments Receive Less Foreign Aid?" *American Economic Review*, September 2002, *92*(4), pp. 1126-37.

Boone, Peter. "Politics and the Effectiveness of Foreign Aid." *European Economic Review*, February 1996, *40*(2), pp. 289-329.

Burnside, Craig and Dollar, David. "Aid, Policies, and Growth." *American Economic Review*, September 2000, *90*(4), pp. 847-68.

Burnside, Craig and Dollar, David. "Aid, Policies, and Growth: Revisiting the Evidence." Policy Research Working Paper No. 3251, World Bank, 2004.

Dollar, David and Levin, Victoria. "The Increasing Selectivity of Foreign Aid, 1984-2002." Policy Research Working Paper No. 3299, World Bank, 2004.

Dowling, J.M. and Hiemenz, Ulrich. "Biases in the Allocation of Foreign Aid: Some New Evidence." *World Development*, April 1985, *13*(4), pp. 535-41.

Easterly, William; Levine, Ross and Roodman, David. "Aid, Policies, and Growth: Comment." *American Economic Review*, June 2004, *94*(3), pp. 774-80.

Kaufmann, Daniel; Kraay, Aart and Mastruzzi, Massimo. "Governance Matters V: Aggregate and Individual Governance Indicators for 1996-2005." Policy Research Working Paper No. 4012, World Bank, 2006.

Kosack, Stephen. "Effective Aid: How Democracy Allows Development Aid to Improve the Quality of Life." *World Development*, January 2003, *31*(1), pp. 1-22.

Maizels, Alfred and Nissanke, Machinko K. "Motivations for Aid to Developing Countries." *World Development*, September 1984, *12*(9), pp. 879-900.

McKinlay, Robert D. and Little, Richard. "The U.S. Aid Relationship: A Test of the Recipient Need and the Donor Interest Models." *Political Studies*, 1979, *27*(2), pp. 236-50.

Neumayer, Eric. "The Determinants of Aid Allocation by Regional Multilateral Development Banks and United Nations Agencies." *International Studies Quarterly*, 2003a, *1*(47), pp. 101-22.

Neumayer, Eric. "Is Respect for Human Rights Rewarded? An Analysis of Total Bilateral and Multilateral Aid Flows." *Human Rights Quarterly*, 2003b, *25*(2), pp. 510-27.

Neumayer, Eric. "Do Human Rights Matter in Bilateral Aid Allocation? A Quantitative Analysis of 21 Donor Countries." *Social Science Quarterly*, September 2003c, *84*(3), pp. 650-66.

Neumayer, Eric. *The Pattern of Aid Giving: The Impact of Good Governance on Development Assistance*. London: Routledge, 2003d.

Rajan, Raghuran G. and Subramanian, Arvind. "Aid and Growth: What Does the Cross-Country Evidence Really Show?" NBER Working Paper No. 11513, National Bureau of Economic Research, 2005.

Trumbull, William N. and Wall, Howard J. "Estimating Aid-Allocation Criteria with Panel Data." *Economic Journal*, July 1994, *104*(425), pp. 876-82.

Wall, Howard J. "The Allocation of Official Development Assistance." *Journal of Policy Modeling*, June 1995, *17*(3), pp. 307-14.

DATA APPENDIX

Country Averages

Country	Real aid per capita ($)	Real aid ($ millions)	Real GDP per capita ($ thousands)	Infant mortality	Civil/ political rights	Government effectiveness	Population
Albania	88.9	280.0	3.6	23.0	8.7	–0.49	3.2
Algeria	8.3	247.9	5.4	38.3	4.7	–0.69	30.1
Angola	33.8	410.6	1.9	154.0	4.3	–1.33	12.3
Argentina	3.1	111.8	11.5	18.7	12.0	0.11	36.5
Armenia	72.3	228.7	2.6	35.7	8.0	–0.52	3.2
Azerbaijan	22.7	182.9	2.6	77.3	4.7	–0.96	8.0
Bahamas	17.9	5.2	15.8	17.0	13.5	0.96	0.3
Bahrain	71.9	46.0	16.2	13.0	4.3	0.66	0.7
Bangladesh	10.1	1,297.0	1.5	58.3	8.7	–0.59	129.7
Belarus	10.3	104.0	4.7	14.0	4.7	–1.04	10.0
Belize	64.4	15.4	5.6	34.3	13.7	–0.20	0.2
Benin	45.1	273.5	1.0	96.0	12.0	–0.12	6.1
Bolivia	87.0	711.1	2.4	60.7	10.7	–0.47	8.2
Botswana	33.4	52.3	7.2	68.7	12.0	0.73	1.6
Brazil	1.8	299.5	7.2	36.3	10.3	–0.14	168.7
Bulgaria	34.4	275.4	6.4	13.3	12.0	–0.22	8.1
Burkina Faso	39.5	431.8	1.0	108.0	7.7	–0.49	11.1
Burundi	31.3	205.6	0.6	114.0	4.3	–1.20	6.7
Cambodia	40.3	494.0	1.8	93.3	4.3	–0.57	12.4
Cameroon	37.7	565.6	1.8	94.0	3.7	–0.70	14.9
Cape Verde	279.6	119.1	4.7	31.0	13.3	0.04	0.4
Central African Republic	29.0	101.9	1.1	115.0	7.3	–1.15	3.7
Chad	27.3	206.9	1.0	117.0	5.0	–0.64	7.7
Chile	6.6	97.2	9.0	10.3	12.7	1.27	15.1
China	1.8	2,252.6	3.8	33.0	2.7	0.19	1,252.0
Colombia	8.7	376.8	6.4	20.7	8.0	–0.18	41.8
Comoros	54.8	29.1	1.7	63.0	7.0	–1.04	0.6
Congo, Democratic Republic	34.7	1,826.4	0.8	129.0	7.0	–1.38	48.7
Congo, Republic	24.5	78.5	1.0	81.0	3.3	–1.79	3.4
Costa Rica	6.9	25.2	8.4	10.7	13.0	0.46	3.7
Cote d'Ivoire	43.8	635.7	1.5	114.0	5.0	–0.65	15.5
Croatia	17.7	79.4	9.3	7.7	10.3	0.09	4.5
Czech Republic	27.5	282.4	15.5	5.0	13.0	0.72	10.3
Djibouti	136.1	86.3	2.1	103.3	6.0	–1.00	0.7
Dominica	243.9	17.6	5.3	14.3	14.0	–0.45	0.1
Dominican Republic	10.6	85.9	6.0	34.7	10.7	–0.28	8.3
Ecuador	15.4	186.6	3.4	28.3	10.3	–0.86	12.3

DATA APPENDIX, cont'd

Country Averages

Country	Real aid per capita ($)	Real aid ($ millions)	Real GDP per capita ($ thousands)	Infant mortality	Civil/ political rights	Government effectiveness	Population
Egypt, Arab Republic	23.6	1,454.0	3.4	43.0	4.3	–0.10	63.2
El Salvador	37.9	228.0	4.6	34.7	10.7	–0.28	6.1
Equatorial Guinea	68.7	28.9	1.5	108.0	2.0	–1.89	0.4
Eritrea	51.5	209.4	1.0	56.0	4.3	–0.50	4.0
Estonia	49.9	69.0	9.6	10.3	12.7	0.84	1.4
Ethiopia	16.2	1,024.4	0.7	117.0	6.3	–0.63	63.1
Fiji	52.3	42.0	5.1	18.3	8.3	–0.20	0.8
Gabon	75.2	84.0	6.3	60.0	7.0	–0.79	1.2
Gambia, The	40.9	52.1	1.7	92.7	5.0	–0.20	1.3
Georgia	42.2	201.5	2.0	41.0	7.7	–0.62	4.8
Ghana	37.5	721.2	1.9	63.3	10.3	–0.02	19.3
Grenada	129.6	13.1	6.9	21.7	13.0	–0.07	0.1
Guatemala	21.7	241.6	3.8	41.0	8.0	–0.60	11.2
Guinea	39.2	276.7	1.9	115.0	5.0	–0.71	7.3
Guinea-Bissau	86.4	115.4	0.8	133.7	7.3	–1.21	1.3
Guyana	124.5	94.2	3.9	56.0	12.0	–0.23	0.8
Haiti	52.8	395.1	1.7	82.7	5.0	–1.54	7.9
Honduras	66.9	419.3	2.5	34.0	10.0	–0.71	6.4
Hungary	24.2	243.4	13.9	8.5	13.0	0.76	10.1
India	1.4	1,421.4	2.4	68.3	10.0	–0.09	1,004.2
Indonesia	7.7	1,573.4	3.1	37.3	7.0	–0.23	204.6
Iran, Islamic Republic	2.5	154.5	5.8	37.3	3.7	–0.34	63.0
Israel	85.0	526.7	22.3	6.0	12.0	1.12	6.2
Jamaica	17.5	43.7	3.6	17.0	11.3	–0.19	2.6
Jordan	157.5	768.3	4.0	25.7	7.3	0.30	4.8
Kazakhstan	11.3	171.3	4.8	61.0	5.0	–0.70	15.3
Kenya	20.4	588.6	1.0	76.3	6.0	–0.70	29.6
Kuwait	1.7	3.5	17.1	9.7	6.7	0.29	2.1
Kyrgyz Republic	49.3	236.9	1.5	60.7	6.0	–0.61	4.9
Lao PDR	58.2	299.7	1.5	92.3	3.0	–0.52	5.2
Latvia	37.4	89.1	7.9	13.0	12.7	0.35	2.4
Lebanon	48.3	206.2	4.3	28.3	5.0	–0.25	4.3
Lesotho	45.7	78.3	2.1	74.7	9.0	–0.05	1.7
Lithuania	61.2	215.0	8.9	10.0	13.0	0.37	3.5
Macedonia, FYR	91.8	186.0	5.8	15.0	9.3	–0.33	2.0
Madagascar	25.2	386.3	0.8	85.7	10.0	–0.46	15.2
Malawi	45.8	463.0	0.6	120.7	10.0	–0.69	10.2
Malaysia	3.9	88.9	8.5	8.7	6.7	0.91	22.9

DATA APPENDIX, cont'd

Country Averages

Country	Real aid per capita ($)	Real aid ($ millions)	Real GDP per capita ($ thousands)	Infant mortality	Civil/ political rights	Government effectiveness	Population
Mali	45.7	482.0	0.8	125.7	11.3	–0.70	10.7
Malta	35.0	13.6	16.7	7.0	14.0	1.08	0.4
Mauritania	89.8	230.5	1.7	86.7	4.7	0.02	2.6
Mauritius	19.9	22.9	8.7	18.5	13.0	0.75	1.2
Mexico	2.8	257.8	8.2	26.5	10.0	–0.01	96.7
Moldova	23.7	101.4	1.4	27.3	9.0	–0.73	4.3
Mongolia	95.1	226.6	1.6	61.0	11.3	–0.15	2.4
Morocco	17.1	483.6	3.5	42.7	6.3	–0.01	28.4
Mozambique	58.2	1,002.7	0.9	113.3	9.0	–0.47	17.4
Namibia	91.9	166.3	5.8	51.0	11.0	0.37	1.9
Nepal	19.3	433.4	1.3	71.3	8.3	–0.56	22.7
Nicaragua	138.2	685.9	3.1	35.0	9.3	–0.65	5.0
Niger	29.7	312.4	0.8	163.0	8.0	–0.90	10.5
Nigeria	1.9	238.3	0.9	106.7	6.0	–1.11	124.9
Oman	21.7	50.5	12.5	12.3	4.7	0.86	2.4
Pakistan	6.4	868.9	1.9	81.7	6.0	–0.50	136.3
Panama	10.6	29.5	6.0	20.3	12.3	–0.21	2.8
Papua New Guinea	60.2	295.6	2.5	70.3	10.3	–0.66	5.1
Paraguay	18.8	93.7	4.7	26.3	9.3	–1.04	5.2
Peru	16.6	426.2	4.8	34.7	9.3	–0.32	25.6
Philippines	10.2	752.6	3.9	31.0	10.7	0.04	75.5
Poland	57.4	2,212.6	9.9	9.3	13.0	0.52	38.5
Romania	19.9	441.8	6.5	19.3	11.0	–0.46	22.3
Russian Federation	10.3	1,500.2	7.3	17.3	7.0	–0.47	145.7
Rwanda	71.0	465.9	1.1	120.0	3.7	–0.67	7.3
Samoa	206.6	35.2	4.8	21.3	12.0	0.13	0.2
Saudi Arabia	1.1	23.5	12.7	24.3	2.0	–0.04	20.5
Senegal	57.7	523.7	1.5	80.7	9.0	–0.09	9.4
Seychelles	172.2	13.7	15.9	13.3	10.0	–0.59	0.1
Sierra Leone	46.2	229.1	0.6	168.0	6.3	–1.01	5.0
Singapore	2.7	9.6	21.5	3.5	6.0	2.47	3.8
Slovak Republic	23.0	123.6	11.1	8.7	12.3	0.37	5.4
Slovenia	30.2	60.1	16.3	5.0	13.3	0.79	2.0
Solomon Islands	143.1	59.1	2.0	21.7	10.3	–1.15	0.4
South Africa	11.6	500.1	9.6	49.3	13.0	0.48	43.0
Sri Lanka	27.6	504.6	3.3	16.7	8.7	–0.25	18.3
St. Kitts and Nevis	96.2	4.1	10.4	23.0	13.0	–0.06	0.0
St. Lucia	172.6	25.8	5.5	17.0	13.0	0.21	0.2

DATA APPENDIX, cont'd

Country Averages

Country	Real aid per capita ($)	Real aid ($ millions)	Real GDP per capita ($ thousands)	Infant mortality	Civil/ political rights	Government effectiveness	Population
St. Vincent and the Grenadines	192.0	21.3	5.3	20.7	13.0	–0.09	0.1
Sudan	11.3	358.5	1.7	65.7	2.0	–1.39	31.0
Swaziland	35.3	34.0	4.4	93.7	4.7	–0.50	1.0
Syrian Arab Republic	15.3	233.3	3.3	20.3	2.0	–0.64	15.9
Tajikistan	17.9	110.6	0.9	81.7	3.7	–1.32	6.1
Tanzania	35.5	1,183.9	0.5	103.7	7.7	–0.63	33.1
Thailand	13.5	805.0	6.4	27.0	10.0	0.33	59.7
Togo	26.0	107.0	1.6	80.3	5.3	–1.10	4.4
Tonga	292.9	29.0	6.2	17.3	8.0	–0.42	0.1
Tunisia	20.5	197.7	6.0	23.3	5.0	0.78	9.5
Turkey	4.2	274.8	6.3	40.3	7.3	–0.04	66.6
Turkmenistan	6.4	29.2	4.1	76.0	2.0	–1.39	4.6
Uganda	38.6	877.5	1.2	86.0	6.3	–0.31	22.9
Ukraine	8.0	397.8	4.5	17.3	8.3	–0.70	49.8
Uruguay	10.9	35.6	8.3	15.7	13.3	0.61	3.3
Uzbekistan	6.2	153.5	1.5	59.3	2.7	–0.96	24.3
Vanuatu	224.2	41.9	3.0	36.3	12.0	–0.38	0.2
Venezuela, RB	2.8	67.3	5.5	20.0	9.0	–0.87	24.0
Vietnam	18.1	1,420.6	2.0	24.7	2.7	–0.23	77.6
Yemen, Republic	13.1	226.0	0.8	85.0	5.3	–0.70	17.3
Zambia	126.7	1,177.6	0.8	102.0	8.0	–0.80	9.7
Zimbabwe	30.3	355.9	2.6	66.5	5.5	–0.69	12.1

Open Market Operations and the Federal Funds Rate

Daniel L. Thornton

It is commonly believed that the Fed's ability to control the federal funds rate stems from its ability to alter the supply of liquidity in the overnight market through open market operations. This paper uses daily data compiled by the author from the records of the Trading Desk of the Federal Reserve Bank of New York over the period March 1, 1984, through December 31, 1996: He analyzes the Desk's use of its operating procedure in implementing monetary policy and the extent to which open market operations affect the federal funds rate—the liquidity effect. The author finds that the operating procedure was used to guide daily open market operations; however, there is little evidence of a liquidity effect at the daily frequency and even less evidence at lower frequencies. Consistent with the absence of a liquidity effect, open market operations appear to be a relatively unimportant source of liquidity to the federal funds market. (JEL E43, E52)

Federal Reserve Bank of St. Louis *Review*, November/December 2007, *89*(6), pp. 549-70.

The conventional view is that the Fed controls the federal funds rate by altering the supply of liquidity in the overnight market by changing the supply of reserves relative to demand through open market operations (e.g., Taylor, 2001, Friedman, 1999). Open market operations are conducted by the Trading Desk of the Federal Reserve Bank of New York (the Desk). Although the procedure that the Desk follows has evolved and continues to do so, the fundamental procedure has remained largely the same since at least the mid-to-late 1970s. Specifically, the Desk estimates (i) the demand for reserves that are required to achieve the Federal Open Market Committee (FOMC)'s operating objective and (ii) the quantity of reserves that would be available if the Desk did nothing. If (i) exceeds (ii), the procedure indicates that reserves should be added through an open market purchase of government securities. If (i) is less than (ii), the procedure suggests that the Desk drain reserves through an open market sale.

It is important to note that the operating procedure is intended only to provide the Desk with guidance in conducting daily open market operations. It was never intended to be strictly adhered to. Specifically, frequent, yet informal, adjustments to the estimate of excess reserves were made.[1] Moreover, the Desk's behavior is also guided by other factors, such as its estimate of free reserves, in determining the day's open market operations.

This paper uses daily data compiled by the author from the records of the Desk to analyze the effect of open market operations.[2] The paper addresses two issues: the use of the operating

[1] These informal adjustments were stated in the morning call and depended on estimates of the distribution of cumulative excess reserves holding to date. These informal adjustments were particularly important on the last two days of the maintenance period.

[2] The Federal Reserve Bank of New York and the Board of Governors (BOG) of the Federal Reserve System jointly control the access to and the use of these data. I thank Jonathan Albrecht and Joanna Barnish for their valuable assistance in gathering these data and John Partlan for helping me understand the nuances of the Desk's operating procedure.

Daniel L. Thornton is a vice president and economic policy advisor at the Federal Reserve Bank of St. Louis. He thanks John Partlan and Sherry Edwards for comments and John McAdams for research assistance. An earlier version of this article was published in D.G. Mayes and J. Toporowski, eds., *Open Market Operations and Financial Markets.* Abingdon and New York: Routledge, 2007, pp. 178-202.

procedure in implementing monetary policy and the extent to which open market operations affect the federal funds rate—the liquidity effect. In so doing, it provides some evidence on the relative importance of Fed operations in supplying liquidity to the federal funds market.

The next section presents a detailed analysis of the Desk's operating procedure and its use of the procedure. The following sections investigate the relationship between open market operations and the federal funds rate and analyze these findings.

THE DESK'S OPERATING PROCEDURE

The equilibrium federal funds rate is determined by the demand for and supply of total reserves. Hence, the Desk's operating procedure under a federal funds targeting procedure is simply to equate the supply of reserves with the expected demand, conditional on the target for the federal funds rate. To illustrate the procedure, assume that the demand for total reserves (TR^d) is given by

$$TR_t^d = f(ff_t, x_t) + \eta_t, \tag{1}$$

where ff_t is the federal funds rate, x_t is a vector of other variables that determine reserve demand, and η_t is a random i.i.d. demand shock. Implicitly, the demand for reserves includes the demand for excess reserves—reserves in excess of those needed to satisfy Federal Reserve–imposed reserve requirements.

The quantity of total reserves supplied if the Desk conducts no open market operations is determined by the Fed's holding of government securities, B_t, borrowing by depository institutions, BR_t, and what the Desk refers to as autonomous factors that affect reserve supply, F_t (e.g., currency in circulation, the Treasury's balance at the Fed, and float).[3] That is,

$$TR_t^s = B_t + BR_t + F_t. \tag{2}$$

In practice, the Desk knows the magnitude of none of the variables on the right-hand side of (2) at the time that it conducts open market operations; however, because the errors are very small for B_t, for the sake of this analysis B_t is assumed to be known exactly.[4] The Desk makes an estimate of the autonomous factors that affect reserve supply, i.e., $E_{t-1}F_t = F_t + v_t$, where E_{t-1} denotes the expectation operator conditional on information available before that day's open market operation and v_t denotes the forecast miss. The Desk does not estimate borrowing, but rather applies the FOMC-determined borrowing assumption, called the initial borrowing assumption (IBA_t).[5,6] Given these assumptions and definitions, the estimate of reserve supply if the Desk conducts no open market operations is given by

$$E_{t-1}TR_t^s = B_t + E_{t-1}F_t + IBA_t. \tag{3}$$

The amount of the open market operations suggested by the Desk's operating procedure, which I call the operating procedure–determined open market operation ($OPDOMO_t$), is given by

$$OPDOMO_t = E_{t-1}f(ff_t^*, x_t) - (E_{t-1}NBR_t + IBA_t), \tag{4}$$

[3] Borrowing (and later, the initial borrowing assumption) refers to seasonal plus adjustment borrowing. Extended credit borrowing was treated separately, as one of the autonomous factors affecting reserve supply.

[4] The reason is that the Desk assumes that there would be no purchases or sales on foreign accounts that day. The foreign desk, however, has permission to make sales during the day up to some specified amount. The foreign desk is not permitted to make purchases on the System account, however. Purchases are executed in the secondary market to neutralize their impact on reserves.

[5] Thornton (2006) shows that borrowed reserves targeting was a euphemism for federal funds rate targeting. He also notes that the IBA was last mentioned in discussing monetary policy during a conference call on January 9, 1991. Despite this fact, the FOMC never formally announced it was no longer targeting borrowed reserves and a borrowing assumption remained part of the Desk's formal operating procedure until at least the end of our sample period. However, it is no longer used today. Also, compare the discussion of "operating procedures" in Sternlight (1991) with Sternlight (1992).

[6] The IBA is changed relatively infrequently and most often when the funds rate target is changed (see Thornton, 2001b, for an analysis of the connection between the IBA and changes in the funds rate target). Separate estimates of the demand for required and excess reserves are made. Like the IBA, the estimate of the demand for excess reserves is changed infrequently. In contrast, the estimate of the demand for required reserves is typically changed six times during each maintenance period.

where ff_t^* denotes the Fed's target for the federal funds rate and $E_{t-1}NBR_t = B_t + E_{t-1}F_t$ is the expected level of nonborrowed reserves.[7] If $OPDOMO_t$ is positive, the procedure directs the Desk to purchase government securities to keep the funds rate at the targeted level. If it is negative, the procedure indicates government securities should be sold.

An Evaluation of the Desk's Operating Procedure

The Desk's use of its operating procedure is analyzed using daily estimates of $OPDOMO_t$ during the period March 1, 1984, through December 31, 1996. In practice, the staffs of the New York Fed and the BOG made separate estimates of the maintenance-period demand for reserves and the supply of nonborrowed reserves. Hence, there are two separate estimates of procedure-determined open market operations for the day. Because there are more observations available for the BOG estimates, only the BOG's estimates are used here.[8] However, the qualitative conclusions are essentially unchanged when the New York Fed estimates are used. This is not surprising because the correlations between these alternative estimates of reserve supply and demand are 0.9986 and 0.9996, respectively.

Reserve Requirement Changes. There were two major changes in reserve requirements during the sample period. The first occurred on December 13, 1990, when reserve requirements on non-personal time and saving deposits and net eurocurrency liabilities were reduced from 3 percent to zero over two maintenance periods. The second occurred on April 2, 1992, when the reserve requirement on transactions deposits was reduced from 12 percent to 10 percent. The first of these was a surprise move. It took time for banks to adjust to the lower level of operating balances, and the funds rate became more volatile for a period of time. Consistent with the New York Fed's assessment of the impact of these changes, preliminary analysis indicated that the Desk did not follow the operating procedure closely during maintenance periods affected by these reserve requirement changes.[9] Consequently, these maintenance periods were deleted to avoid biasing the results. Finally, there are days when some of the observations are missing because of incomplete records. These observations also have been deleted. The final number of daily observations is 3,176.

Table 1 summarizes, by day of the maintenance period, whether the procedure suggested the Desk add or drain reserves and what the Desk actually did. The reserve maintenance period ends on every other Wednesday. This is called settlement Wednesday and is denoted by SW. There were four instances in the sample period when the maintenance period effectively ended on Tuesday because the normal reserve settlement day was a holiday. In these instances, the preceding Tuesday was designated SW because banks settled their reserve accounts on that day.[10] Hence, all but four settlement days are Wednesdays. All other days in the maintenance period are recorded on their corresponding calendar day.

Table 1 shows that, for all days, the procedure indicated that reserves be added more often than drained. This is due in large part to the fact that the primary government security dealers, with whom the Desk conducts daily open market operations, prefer to sell rather than purchase securities from the Desk. Hence, the operating procedure is designed so that, more often than not, there is a need to add rather than drain reserves. It is also due to the fact that the currency grew at a fairly constant rate over most of this period. Hence, reserves needed to be added more often than drained to accommodate currency growth.

[7] This terminology stems from the fact that, before June 1995, the borrowed reserves assumption was presented in each of the policy alternatives voted on by the FOMC. The borrowing assumption was frequently stated in terms of a range for borrowed reserves, rather than a specific level. The level used by the Desk was often (but not always) the midpoint of the range voted on by the FOMC. Moreover, the borrowing assumption was often changed during the intermeeting period without a specific vote of the FOMC. Beginning with the June 30, 1995, meeting, the FOMC dropped the explicit reference to the level of seasonal plus adjustment borrowing that it believed was consistent with the policy alternatives being considered.

[8] There are 19 missing observations for the BOG and 586 missing observations for the New York Fed. Also, there are seven days when daily open market operations are missing.

[9] See Sternlight (1991).

[10] Reserve balances held on that day counted for two days.

Table 1

Distribution of $OPDOMO_t$, $OMOMPA_t$, and $OMOD_t$ by Day of the Maintenance Period

Day of maintenance period	No. of observations	$OPDOMO_t$		$OMOMPA_t$			$OMOD_t$			Percent positive		
		Positive	Negative	Zero	Positive	Negative	Zero	Positive	Negative	$OPDOMO_t$	$OMOMPA_t$	$OMOD_t$
D1	318	254	64	41	233	44	59	228	31	0.80	0.73	0.72
D2	326	259	67	83	173	70	97	165	64	0.79	0.53	0.51
D3	285	219	66	45	197	43	51	203	31	0.77	0.69	0.71
D4	327	254	73	70	182	75	78	186	63	0.78	0.56	0.57
D5	325	258	67	84	169	72	98	153	74	0.79	0.52	0.47
D6	321	256	65	54	205	62	65	196	60	0.80	0.64	0.61
D7	327	264	61	84	167	76	101	157	69	0.81	0.51	0.48
D8	296	231	65	38	214	44	45	214	37	0.71	0.72	0.72
D9	323	229	94	51	187	85	54	189	80	0.71	0.58	0.59
SW	328	221	107	36	219	73	37	219	72	0.67	0.67	0.67
Totals	3,176	2,445	729	586	1,946	644	685	1,910	581	0.76	0.61	0.60

The need to add reserves is particularly acute on the first day of the maintenance period: Estimates of reserve demand and reserve supply are estimates of the maintenance period average; that is, they are daily estimates of the demand for or supply of reserves on average over the maintenance period. Consequently, the procedure automatically accounts for repurchase agreements (RPs) that were executed during previous maintenance periods but are scheduled to mature sometime during the current maintenance period.

Table 1 compares $OPDOMO_t$ with two measures of actual daily open market operations, $OMOD_t$ and $OMOMPA_t$. $OMOD_t$ is the net of open market purchases and sales of government securities on the day. This is likely what most people think of when discussing open market operations. In contrast, $OMOMPA_t$ reflects the effect of the net operation on the supply of reserves over the maintenance period. For example, assume that the Desk purchases exactly as much as it sold on the day but sold overnight and purchased with a multiple-day term. In this instance, $OMOD_t$ would be zero but $OMOMPA_t$ would be positive. $OMOMPA_t$ reflects the net effect of the day's open market operation on reserves over the maintenance period, while $OMOD_t$ indicates the net amount of purchases and sales on the day. Consequently, one measure may indicate a purchase and the other a sale. Indeed, there are 102 days when this occurred. There are another 102 days when $OMOD_t$ is zero but $OMOMPA_t$ is not. There are only three instances when the reverse is true, however. Despite these differences, these measures are highly correlated (0.75).

Both measures indicate that Desk actions frequently had no impact on the supply of reserves. On nearly 22 percent of the days $OMOD_t$ was zero, while on nearly 19 percent of the days $OMOMPA_t$ was zero. The decision not to affect the supply of reserves either on the day or over the maintenance period appears to be influenced, in part, by the magnitude of $OPDOMO_t$. $OMOD_t$ and $OMOMPA_t$ are more likely to be zero when $OPDOMO_t$ is relatively small and are almost never zero when $OPDOMO_t$ is relatively large.

While the data in Table 1 suggest that the Desk follows the operating procedure relatively closely, it did not follow the procedure mechanically. The correlation between $OPDOMO_t$ and $OMOMPA_t$ is 0.61.[11] Figure 1 presents a scatter plot of these variables with $OPDOMO_t$ on the horizontal axis and $OMOMPA_t$ on the vertical axis. These data indicate that the Desk's actions were not exactly as prescribed by the operating procedure: They generally added less than indicated when the procedure called for adding reserves and drained less than indicated when the procedure called for draining reserves. This behavior is due in part to the fact that the Desk often does nothing when the procedure suggests a relatively small need to add or drain reserves.

The discrepancy is also due, in part, to the fact that the Desk underestimated reserve demand on average. The average forecast error is \$0.07 billion, with a standard deviation of \$0.37 billion. The forecast errors are slightly skewed upward, as the median is \$0.06 billion, and are highly serially correlated (0.83).[12] Although the mean and median forecast errors are both significantly different from zero at the 5 percent significance level, they are small relative to the mean (\$54.6 billion) and median (\$56.8 billion) levels of total reserves. Hence, the Desk did a good job of forecasting reserve demand.

How Well Did the Desk Follow Its Operating Procedure?

I estimate the following equation to formally investigate the extent to which the Desk followed its operating procedure and the extent to which the Desk responded to other factors in conducting daily open market operations:

$$(5) \qquad OMOMPA_t - OPDOMO_t = \alpha + \beta z_t + \varepsilon_t,$$

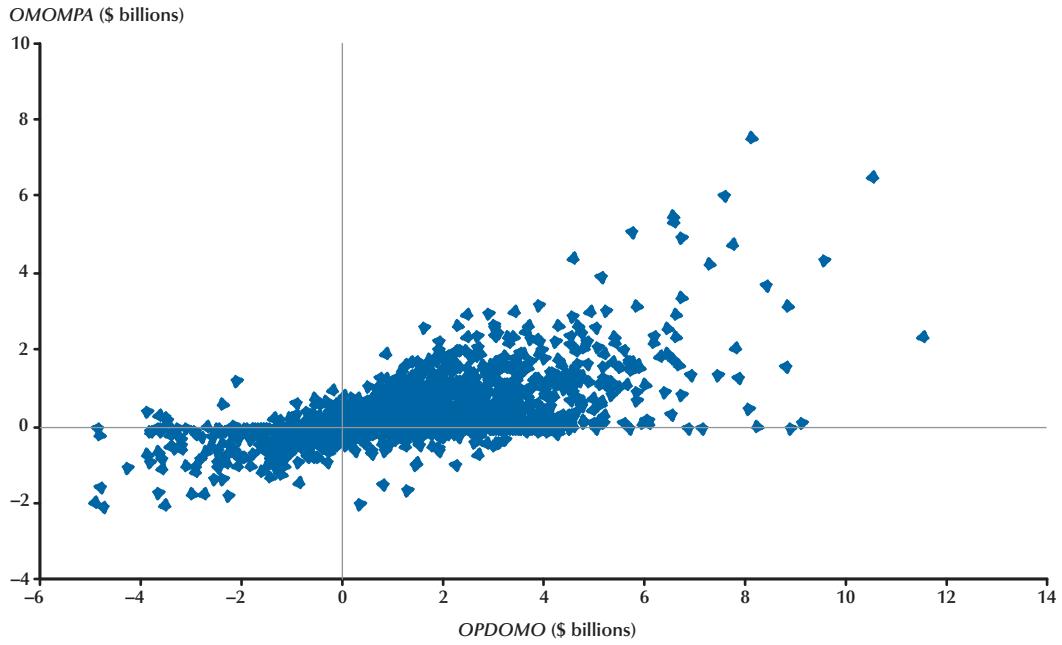
where z_t denotes a vector of factors that might cause the Desk to deviate from its operating procedure and ε_t denotes the effect of all factors not

[11] Because the operating procedure is directed at the quantity of reserves over the maintenance period, it is not surprising that the correlation between $OPDOMO_t$ and $OMOD_t$ is considerably lower, 0.46.

[12] Daily total reserves are available only for the period January 2, 1986–December 31, 1996. These statistics are based on the official measures of required and excess reserves for the period.

Figure 1

Comparison of *OPDOMO* and *OMOMPA*, March 1, 1984–December 31, 1996



reflected in z_t. If the Desk followed the operating procedure perfectly, then $\alpha = \beta = \varepsilon_t = 0$.

Factors That May Have Caused the Desk To Respond Differently. There are a number of factors that might cause the Desk to deviate from its operating procedure. For example, demand for reserves is determined by banks' reserve requirements over a two-week period ending on the Monday two days before settlement Wednesday. Hence, on the last two days of the maintenance period, the demand for reserves is perfectly interest inelastic. Because the demand for reserves is fixed on these days, the Desk might behave somewhat differently on these days. The Desk may also behave differently on various days of the year, such as the first and last days of the month, quarter, or year and particular days of the maintenance period. Indeed, Hamilton (1997), Thornton (2001a), Carpenter and Demiralp (2006), and Demiralp and Farley (2005) report statistically significant day-of-the-maintenance-period and day-of-the-year effects for various aspects of open market operations. These possibilities are investigated by including dummy variables for the beginning of month, *bom*; end of month, *eom*; beginning of quarter, *boq*; end of quarter, *eoq*; beginning of year, *boy*; end of year, *eoy*; and for each day of the maintenance period.

Table 1 suggests that the Desk may follow the operating procedure more closely when it indicates that reserves should be added than when it indicates that reserves should be drained. To investigate this formally, the day-of-the-maintenance-period dummy variables are partitioned according to whether $OPDOMO_t$ is positive or negative.

Because of the difficulty in estimating reserve demand, the Desk might look to the recent behavior of the funds rate or other signals of current market conditions in conducting daily open market operations. The Desk takes a reading on the funds rate just prior to the *morning call*. The morning call is a telephone conference among the staffs of the Board of Governors, the Desk,

and one of Federal Reserve Bank presidents. All parties have access to the reserve projections, and the Desk outlines its intentions for that day's open market operation. One element of the call is where the funds rate is trading "at the time of the call." There are no transcripts of these calls; however, Thornton (2006) documents that the rate at the time of the call was used as a check on the Desk's estimates of reserve demand. Hence, it is reasonable to conjecture that the Desk might respond differently depending on the difference between the funds rate at the time of the call and the funds rate target, *call – fftar*.

It seems likely that the Desk does not follow its procedure on days when the funds rate target is adjusted. Conceptually, the Desk's operating procedure is conditional on the funds rate target. Consequently, a change in the target should have an effect on the estimate of the quantity of reserves demanded; however, it may be difficult to estimate the effect of a target change on the quantity of reserves demanded. Moreover, because the demand for reserves is fixed on the last two days of the maintenance period, exactly how the Desk would behave relative to the operating procedure on those days is uncertain.

Finally, Hamilton (1997) has argued that the Fed responds to forecast misses in one of the components of v_t—the Treasury's balance with the Fed. Specifically, Hamilton suggests that if the Treasury's balance were $400 million lower than expected, the Desk would add x for each of the n remaining days in the maintenance period to make up for that day's error in forecasting the Treasury's balance. If the forecast errors are serially correlated, this information could be used in making today's estimate of F_t. To my knowledge the forecast errors were never saved and analyzed. Consequently, it seems unlikely that the Desk engaged in the explicit error-correction behavior Hamilton describes. In any event, if it did, it should have also responded to the previous day's difference between actual bank borrowing and the *IBA* because borrowing is highly serially correlated and the *IBA* was changed relatively infrequently.

Empirical Results. Equation 5 was estimated accounting for the factors noted above. Estimates of v_t are those used by Carpenter and Demiralp (2006) and were provided by the authors. These data are available only beginning in January 1986; consequently, the estimation period is January 2, 1986, through December 31, 1996. There is only an estimate of the net forecast error for all components. There is a separate estimate for the Treasury's balance at the Fed. Hence, the BOG's forecast error for Treasury balances on the previous day ($FE(Tbal)_{t-1}$) is also included. With this addition, the coefficient on v_{t-1} should reflect the explicit error correction behavior of the Desk for the remaining factors, whereas the coefficient on $FE(Tbal)_{t-1}$ reflects the explicit error-correction behavior with respect to Treasury balance forecast errors.

Finally, at its first meeting in 1994, the FOMC began announcing policy actions upon taking them. Because of this, and because banks began implementing deposit sweep programs that reduced the demand for reserves at about the same time, estimates of (5) are presented for periods both before and after 1994. Also, the announcement came later in the day, after the Desk had conducted that day's open market operations. Consequently, for analyses of the effect of changes in the funds rate target on Desk operations, the changes in the funds rate target are aligned to the first day that the Desk could have responded to the FOMC's action.

The estimates are presented in Table 2. The equation was estimated using a Newey-West estimator of the covariance matrix. The coefficient estimates are reported in one column, and the significance level associated with the null hypothesis that the coefficient is zero is reported in the adjacent column. Although a formal test of the null hypothesis of temporal stability is easily rejected, the results for the two periods are remarkably similar. Consistent with Table 1, during both periods the Desk adds less reserves than the procedure indicates should be added and drains less reserves than the procedure indicates should be drained. Moreover, during both periods, the absolute values of the coefficients on the day-of-the-maintenance-period dummy variables decline

Table 2

The Desk's Use of the Operating Procedure: February 2, 1986–December 31, 1996

	Pre-1994		Post-1994	
Variable	Coefficient	Significance level	Coefficient	Significance level
bom	0.272	0.014	0.313	0.051
eom	0.277	0.050	0.182	0.242
boq	–0.198	0.365	–0.308	0.446
eoq	–0.150	0.533	–0.504	0.188
boy	–0.401	0.229	0.314	0.447
eoy	–0.244	0.559	0.538	0.214
1st Thursday positive	–2.135	0.000	–3.087	0.000
1st Friday positive	–1.943	0.000	–2.905	0.000
1st Monday positive	–1.590	0.000	–2.477	0.000
1st Tuesday positive	–1.503	0.000	–2.212	0.000
1st Wednesday positive	–1.434	0.000	–1.941	0.000
2nd Thursday positive	–0.923	0.000	–1.188	0.000
2nd Friday positive	–0.837	0.000	–0.943	0.000
2nd Monday positive	–0.345	0.000	–0.365	0.000
2nd Tuesday positive	–0.297	0.000	–0.329	0.000
SW positive	–0.223	0.000	–0.148	0.002
1st Thursday negative	1.532	0.000	1.576	0.000
1st Friday negative	1.501	0.000	1.118	0.000
1st Monday negative	1.081	0.000	1.007	0.000
1st Tuesday negative	0.904	0.000	1.096	0.001
1st Wednesday negative	0.757	0.000	1.103	0.001
2nd Thursday negative	0.609	0.000	0.360	0.008
2nd Friday negative	0.374	0.000	0.330	0.041
2nd Monday negative	0.187	0.002	0.182	0.249
2nd Tuesday negative	0.208	0.000	0.149	0.087
SW negative	0.198	0.000	0.194	0.002
Δ*fftar* 2nd Tuesday and SW	–0.393	0.446	–0.084	0.786
Δ*fftar* all other days	–1.166	0.043	1.575	0.113
$Call_t - fftar_t$	0.237	0.094	0.532	0.028
v_{t-1}	–0.016	0.585	0.000	0.996
$FE(Tbal)_{t-1}$	–0.016	0.080	0.014	0.823
$BR_{t-1} - IBA_{t-1}$	–0.016	0.124	–0.249	0.313
No. of observations	1,680	—	743	—
Standard error	0.972	—	0.956	—
$\bar{R}^2$	0.515	—	0.635	—

nearly monotonically from the first to the last day of the maintenance period. Moreover, the Desk does not systematically deviate from its operating procedure at the beginning or end of the quarter, or year, during either period. The Desk's response on the first and last days of the month are similar during both periods; however, the response at the end of the month is clearly not statistically significant for the post-1994 period.

There are some differences in the Desk's response to other information. Specifically, during the pre-1994 period the Desk deviated from the operating procedure on days when the funds rate was changed—except on the last two days of the maintenance period, when reserve demand was fixed. In contrast, after 1994, there is no statistically significant deviation from the operating procedure when the funds rate target is changed. This finding is consistent with Taylor (2001) and Thornton (2001a). There was no attempt to alter the supply of reserves immediately after the FOMC began the practice of announcing policy actions.[13]

The estimates also suggest that the Desk relied more on the behavior of the funds rate at the time of the call after 1994 than it did before 1994. The point estimate indicates that on average the Desk added about $0.5 billion more than the operating procedure suggested for every percentage point deviation of the funds rate from the target at the time of the call. Although the estimate is small given the size of the daily market for federal funds, it nevertheless indicates that the Desk behaved in a manner consistent with keeping the funds rate close to the target after 1994.

Finally, there is no evidence of explicit error correction by the Desk during either period. The coefficient on v_{t-1} is negative but not statistically significant at the 5 percent level for either period. The coefficient on $FE(Tbal)_{t-1}$ is negative for the pre-1994 period, but again not statistically significant. Likewise, the coefficients on $BR_{t-1} - IBA_{t-1}$ are negative but not significantly different from zero in either period.

[13] I do not say "announcing changes in the funds rate target" because the FOMC had not formally acknowledged that it was targeting the funds rate at this time. See Thornton (2005) for details.

THE LIQUIDITY EFFECT

The liquidity effect—the decline in nominal interest rates associated with an exogenous, central bank–engineered increase in the monetary base—has received relatively little empirical support historically (e.g., Pagan and Robertson, 1995; and Thornton, 1988; 2001a,b; 2006). The Desk's open market data provide a unique opportunity to investigate the extent to which Fed actions influence the federal funds rate. If the Fed acts to change the equilibrium funds rate through open market operations, there should be a marked change in open market operations on days when the funds rate target is changed.

The estimates reported in Table 2 suggest that the Desk behaved in a manner consistent with the liquidity effect prior to 1994, but not after. Specifically, the Desk added about $0.3 billion fewer reserves than the procedure suggested when the funds rate target was increased by 25 basis points before 1994.[14] This estimate suggests that the demand for federal funds is very interest inelastic: That is, a very small exogenous change in reserves generates a relatively large change in the funds rate. If the demand for reserves is this inelastic, however, one has to wonder why the liquidity effect has been so elusive. Hence, the remainder of this section investigates the liquidity effect in a variety of ways.

Changes in Estimates of Reserve Demand

Consistent with the conventional view, the results in Table 2 suggest that, before 1994 but not after, the Desk drained more reserves than the operating procedure suggested when the funds rate target was increased and added more when

[14] Demiralp and Jorda (2002) investigate the liquidity effect using a similar methodology. Specifically, they estimate the response of open market transactions of various types to surprise changes in the funds rate target for a subperiod of the period April 25, 1984, through August 14, 2000. They find evidence that they interpret as being "broadly consistent with the traditional liquidity effect" prior to 1994 but not after. Recently, however, de Jong and Herrera (2004) have re-evaluated Demiralp and Jorda's work. Consistent with the findings presented here, they find no evidence consistent with a liquidity effect over the entire sample period, but find evidence consistent with a liquidity effect for a sub-period after August 18, 1998, when lagged reserve accounting was reintroduced.

Table 3

The Desk's Behavior When the Target Is Changed

	90% Coverage interval	Sample results
Mean		
43	–0.397 to –1.079	–0.454
45	–0.414 to –1.081	–0.056
Median		
43	–0.168 to –0.727	–0.490
45	–0.171 to –0.733	–0.231
Standard deviation		
43	0.990 to 1.776	1.436
45	1.006 to 1.770	1.234

the target was reduced. The size of the estimated coefficient for the pre-1994 period is relatively small, however. A potential explanation for the small coefficient is that, because the Desk's estimates of reserve demand are conditional on the funds rate target, the Desk reduces its estimate of the quantity of reserves demanded on days when the target is increased and increases its estimate on days when the target is reduced. There were 88 changes in the funds rate target during the sample period (43 increases and 45 decreases). Of these, 78 occurred prior to 1994 and 10 after. Figures 2 and 3 present the revisions to reserve demand when the funds rate target was increased or decreased, respectively. These data are not consistent with the idea that the Desk revises its estimate of reserve demand systematically in response to a change in the target. Figure 2 shows that there were only six occasions when reserve demand was revised down by $0.5 billion or more when the target was increased, whereas there were four days when it was revised up by a corresponding amount. Likewise, Figure 3 shows that estimates of reserve demand were not systematically revised up in response to a decrease in the target. Indeed, most often the estimates were essentially unrevised, despite the change in the target. Hence, the relatively small estimated coefficient in Table 2 is not the consequence of systematic revisions of reserve demand.

The Desk's Behavior When the Funds Rate Target Is Changed

The results in the previous section indicate that the Desk deviated significantly from its operating procedure when the target was changed, at least prior to 1994. This result is investigated more fully in Figures 4 and 5, which show scatter plots of *OPDOMO* vs. *OMOMPA* on days when the funds rate target was decreased and increased, respectively. If the Desk causes the funds rate to fall, there should be many more observations above the 45-degree line than below in Figure 4. This is not the case, however. Likewise, if the Desk causes the funds rate to rise, there should be many more observations below the 45-degree line than above in Figure 5. Although this is the case, as I have already noted, the procedure was skewed toward adding rather than draining reserves. Moreover, Figure 1 shows that the Desk generally added significantly less than the procedure suggested on all days when the procedure indicated reserves should be added. Consequently, it is not clear whether Figure 5 represent a significant change in the Desk's behavior on days when the target was increased.

To investigate whether the Desk behaved significantly differently when the funds rate target was changed, 10,000 samples (sizes 43 and 45) were obtained by bootstrapping the 3,088 observations of *OMOMPA* – *OPDOMO* on days when the target was not changed. Table 3 reports the 90 percent coverage intervals for the mean, median, and standard deviation of these samples along with the same sample statistics for days when the funds rate target was changed. The results suggest that the Desk did not change its behavior significantly when the funds rate target was increased. Five of the six sample statistics are well within the corresponding 90 percent coverage intervals. The sample mean of the 45 days when the target was decreased lies outside of the 90 percent coverage interval, suggesting that the Desk added significantly more reserves on average than the operating procedure indicated when the target was decreased. Because the distributions of *OMOMPA* – *OPDOMO* are skewed, the median is a better measure of central tendency. The sample

Figure 2

Revisions to Reserve Demand When the Target Was Increased

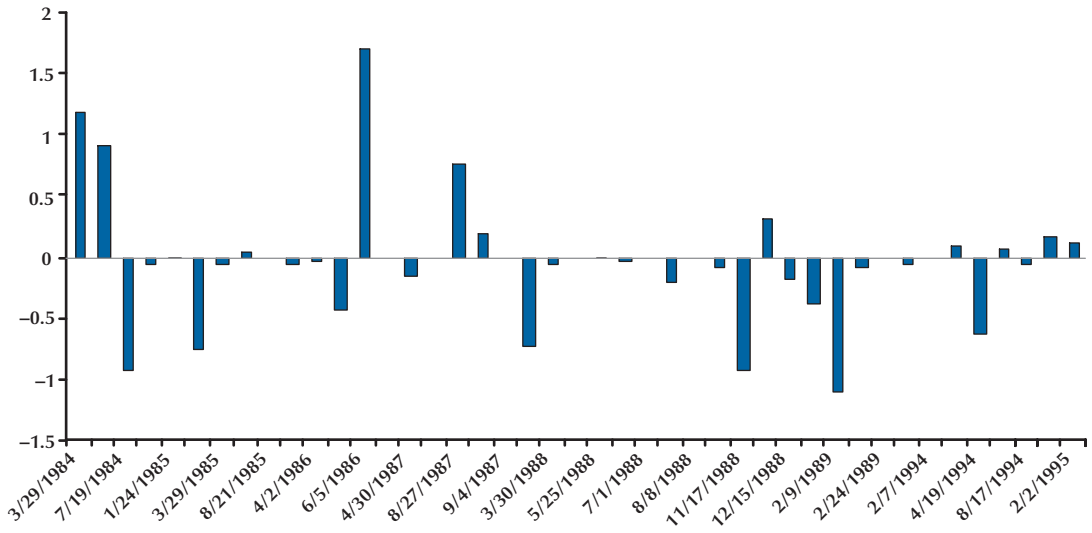


Figure 3

Revisions to Reserve Demand When the Target Was Decreased

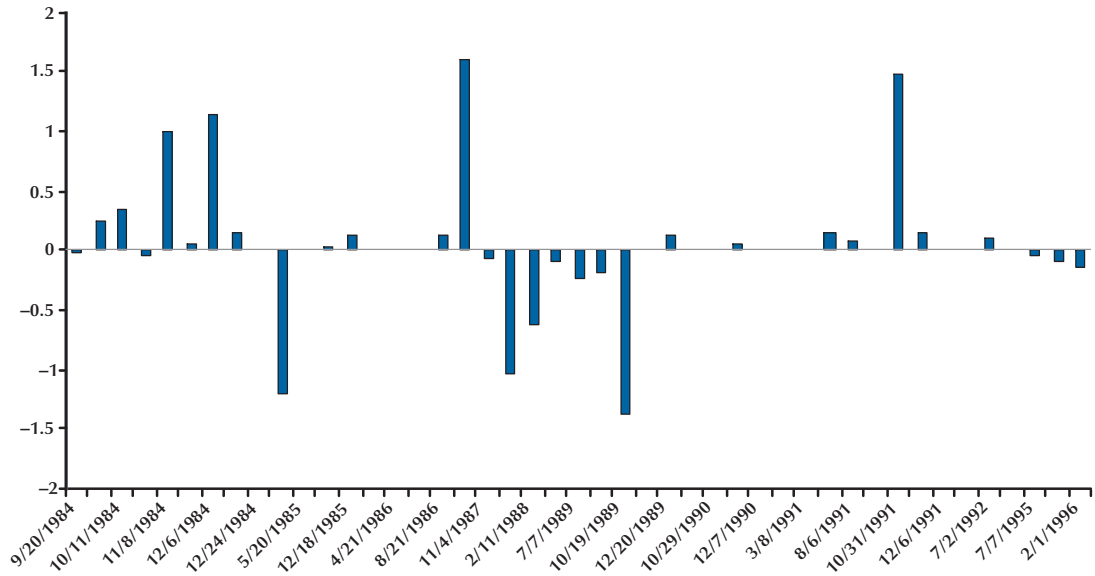


Figure 4

OPDOMO and *OMOMPA* on Days When the Target Was Decreased

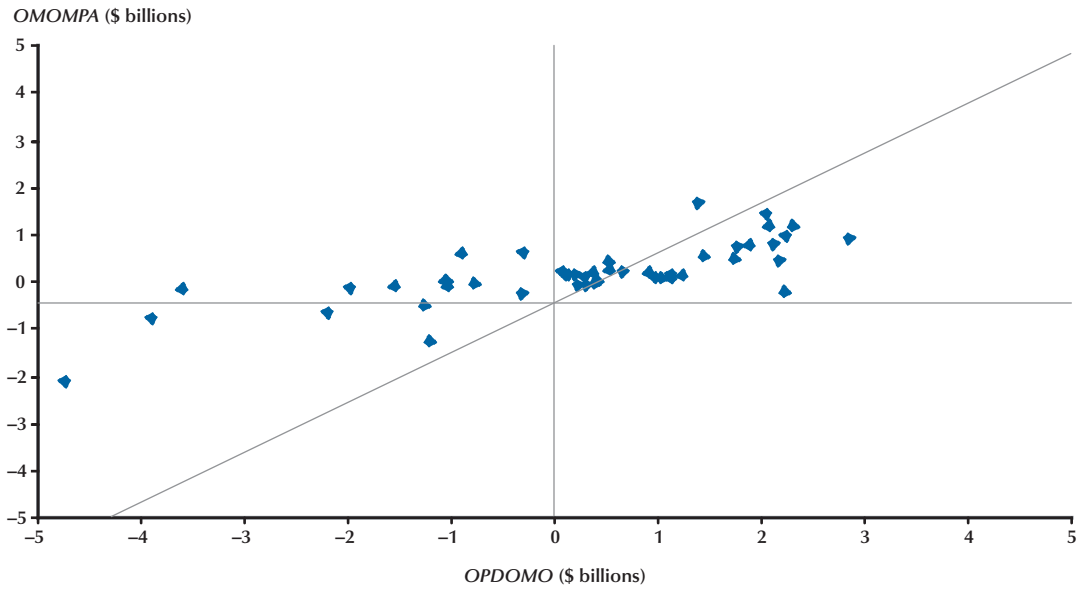
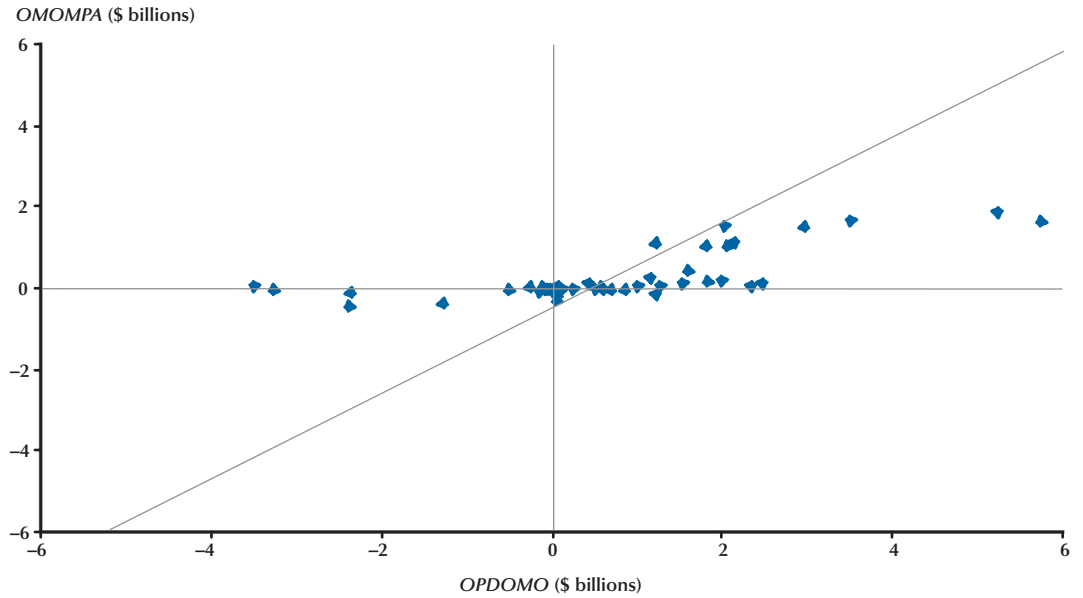


Figure 5

OPDOMO and *OMOMPA* on Days When the Target Was Increased



statistic for the median is well within the coverage interval, suggesting that the Desk did not behave differently when the target was decreased. Hence, there is weak evidence that suggests the Desk attempted to engineer decreases in the funds rate.

Implementing a Target Change Over Time

It might be the case that the Desk does not take all the operations necessary to change the funds rate on the day the target is changed. Instead, the Desk may add or drain reserves over several days to bring about the change in reserves necessary to sustain the funds rate at the new target level (e.g., Taylor, 2001).

This possibility is investigated by comparing the five-day averages of *OMOMPA – OPDOMO* for five days before each target change and for the day of the target change and four days after the change. The five-day averages are plotted in Figures 6 and 7 for increases and decreases in the funds rate target, respectively.[15] If the Desk pursued the increase in the funds rate, there should be more observations below the 45-degree line than above in Figure 6. Similarly, if the Desk pursued the decrease in the funds rate, there should be more observations above the 45-degree line than below in Figure 7. This is not the case. In both instances, the number of observations above and below the 45-degree line is nearly equal. Moreover, simple tests of the equality of the means, medians, and variances of the distributions before and after target changes cannot reject the null hypothesis of equality at even the 10 percent significance level for either positive or negative target changes. Consequently, there is no evidence that the Desk implemented target changes over a period of five days. It is important to note that the conclusion is the same for both increases and decreases in the target. Hence, if the Desk engineered increases in the funds rate target, it completed the operations necessary to effect these changes quickly.

[15] There were 14 occasions (8 for positive and 6 for negative changes in the target) when there were fewer than five days between successive target changes. These changes were deleted so as not to bias the results.

Estimating the Liquidity Effect Directly

The conventional way to estimate the liquidity effect is to regress changes in the interest rate on a variable that represents an exogenous change in reserves or monetary policy. Hamilton (1997) used this approach and found evidence of a statistically significant liquidity effect of exogenous changes in reserves on the federal funds rate. His measure of a supply shock was his estimate of the forecast error the Desk makes in forecasting the Treasury's balance with the Fed. Hamilton found the liquidity effect to be statistically significant, but only on settlement Wednesdays. Thornton (2001a) notes three problems with this analysis. First, the slope of the reserve demand function (and, therefore, the liquidity effect) cannot be estimated on settlement Wednesdays because of the two-day lag in the Fed's reserve accounting system. Second, what matters on the last day of the maintenance period is the imbalance of reserve supply and demand on average over the maintenance period. Because a one-day error in forecasting the Treasury's balance contributes only one-fourteenth of the average error, it would take a very large shock to the Treasury's balance on the last day of the maintenance period to generate a large maintenance-period-average reserve imbalance. Finally, Thornton notes that Hamilton used an estimate of the Desk's forecast error, not the actual forecast error.[16] Thornton (2001a) goes on to show that Hamilton's settlement-Wednesday liquidity effect was idiosyncratic to his sample period, and, even during Hamilton's sample period, it is attributable to just six observations when the funds rate changed by a large amount on settlement Wednesdays.

Carpenter and Demiralp (2006) attempt to overcome some of the data shortcomings of Hamilton's analysis by using a more comprehensive measure of a reserve supply shock. Specifically, they use an estimate of v_t based on the Board of Governors' estimate of F_t.[17] They find a

[16] See Thornton (2004) for analysis of the Desk's forecast error and comparison of those errors with Hamilton's estimates.

[17] The Board kindly provided me with these forecast errors, which cover the period January 2, 1986–June 30, 2000, for the Board of Governors' estimates and December 23, 1993–June 30, 2000, for the New York Fed's estimates.

Figure 6

Scatter Plot of *OPDOMO* – *OMOMPA* Five Days Before and Five Days After Target Increases

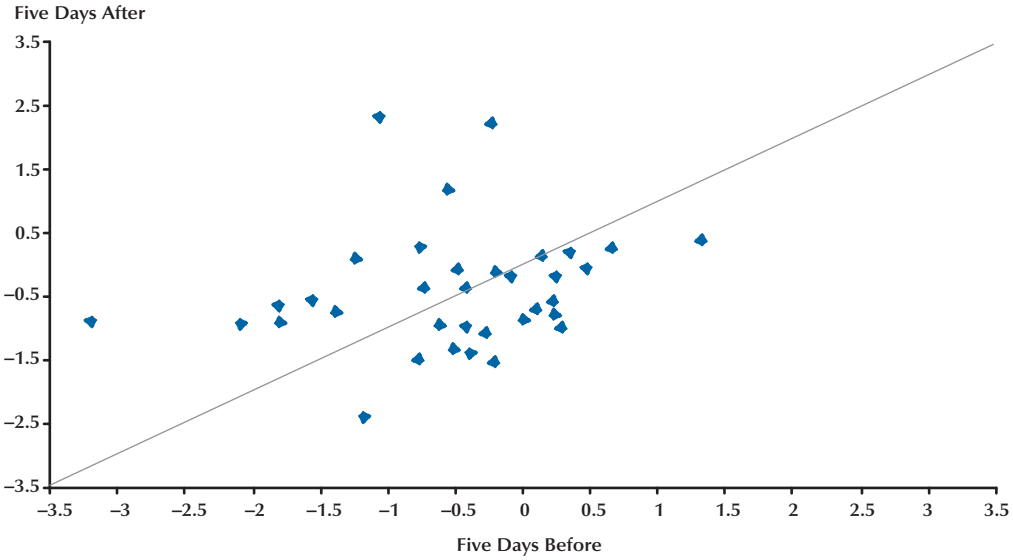


Figure 7

Scatter Plot of *OPDOMO* – *OMOMPA* Five Days Before and Five Days After Target Decreases

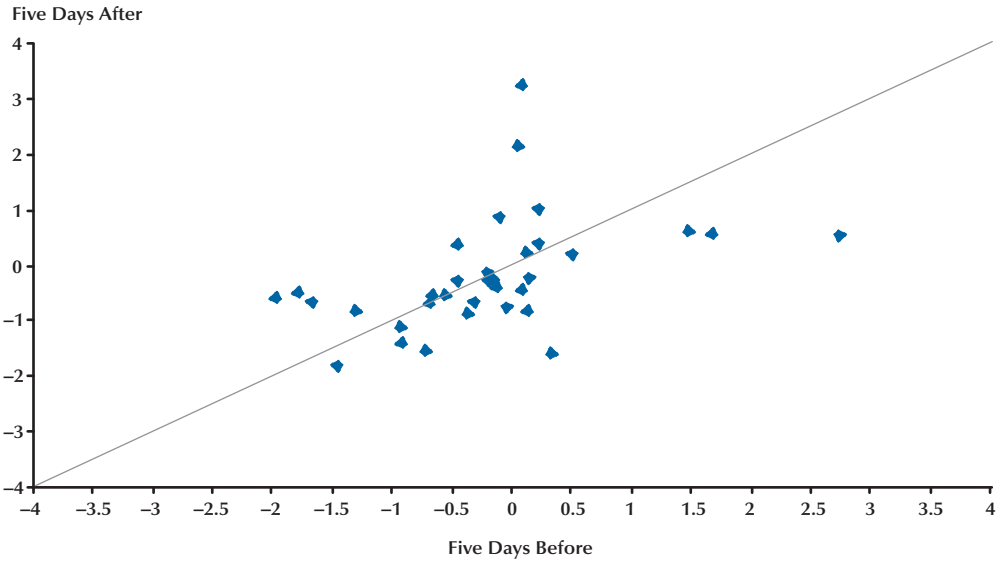
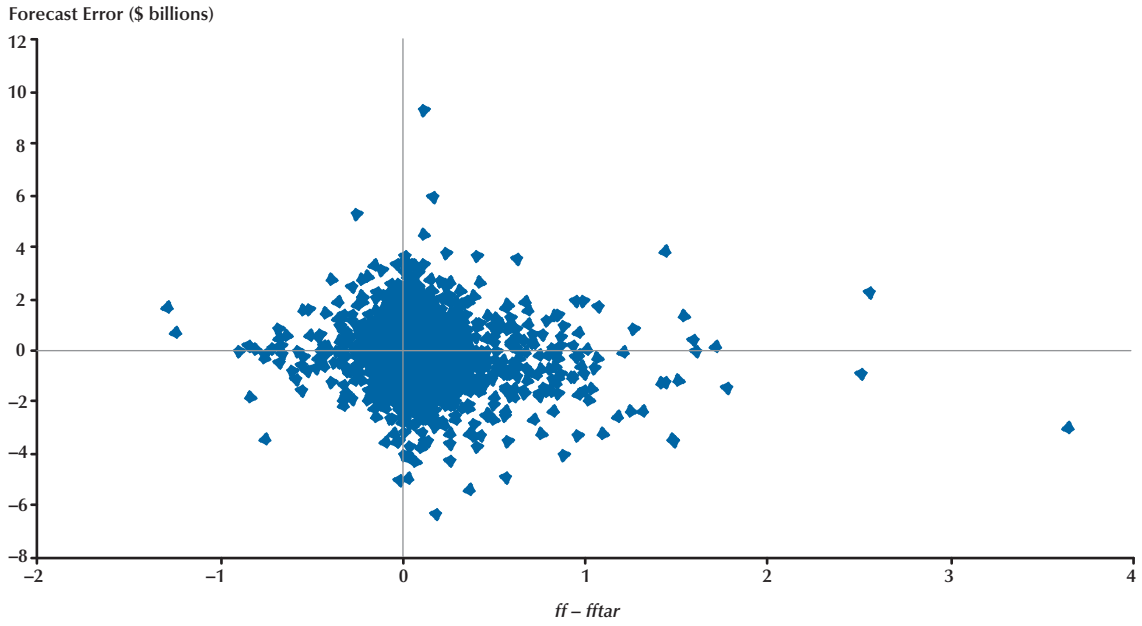


Figure 8

Scatter Plot of *ff – fftar* and Errors in Forecasting Autonomous Factors (January 2, 1986–December 31,1996)



statistically significant liquidity effect on six of the ten days during the maintenance period over the period May 18, 1989, through January 30, 2004. As with Hamilton's findings, the estimated liquidity effect is largest on settlement Wednesdays when, contrary to Carpenter and Demiralp's assertion, the slope of the demand for reserves cannot be estimated.[18]

The effects of shocks to reserves on the funds rate is investigated here using Carpenter and Demiralp's data. Figure 8 presents a scatter plot of the $(ff - fftar)_t$ and the BOG's estimate of v_t over the period January 2, 1986, through December 31, 1996. Days when v_t was not available and the last two days of 1986, when $(ff - fftar)_t$ was more than 8 percentage points, are deleted, leaving 2,676 daily observations. Although not obvious from Figure 8, there is a weak negative relationship between v_t and $(ff - fftar)_t$. The correlation is –0.124. Carpenter and Demiralp (2006) suggest that the relationship between supply shocks and the funds rate is nonlinear, finding that their statistically significant liquidity effect is due to large supply shocks (≥$1 billion). Hence, the relatively low correlation could be due to the fact that most often supply shocks are relatively small. There is some evidence of this. When only days for which the absolute value of the supply shock is greater than $2 billion (180 observations) are considered, the correlation nearly doubles to –0.215. Nevertheless, even for large reserve supply shocks the relationship between reserve supply shocks and the funds rate appears weak.

I investigate this possibility further: Table 4 presents the results for a regression of $(ff - fftar)_t$ on day-of-the-year and day-of-the-maintenance-period dummy variables, $(OMOMPA - OPDOMO)_t$, and v_t over the period January 2, 1986–December 31, 1996. One might expect that, as with shocks to reserve supply, if the Desk adds

[18] The slope of the demand curve cannot be estimated during any of the days of the maintenance period after August 1998, when the Fed returned to lagged reserve accounting.

Table 4

Estimate of $ff_t - fftar_t$, February 2, 1986–December 31, 1996

Variable	Coefficient	Significance level	Coefficient	Significance level
lg	—	—	0.492	0.000
med	—	—	0.379	0.000
sm	—	—	0.361	0.000
eom	0.165	0.000	0.160	0.000
bom	0.084	0.003	0.077	0.006
eoq	0.287	0.001	0.283	0.002
boq	0.093	0.326	0.099	0.278
eoy	0.178	0.803	0.161	0.818
boy	0.253	0.027	0.200	0.087
1st Thursday	0.106	0.000	–0.266	0.000
1st Friday	–0.013	0.252	–0.376	0.000
1st Monday	0.080	0.000	–0.289	0.000
1st Tuesday	0.040	0.000	–0.331	0.000
1st Wednesday	0.000	0.964	–0.370	0.000
2nd Thursday	0.013	0.244	–0.358	0.000
2nd Friday	–0.069	0.000	–0.439	0.000
2nd Monday	0.080	0.000	–0.292	0.000
2nd Tuesday	0.045	0.237	–0.330	0.000
SW	0.245	0.000	–0.131	0.000
$(OMOMPA - OPDOMO)_t^{\Delta fftar}$	–0.058	0.021	–0.025	0.106
$(OMOMPA - OPDOMO)_t^{No\,\Delta fftar}$	–0.002	0.673	0.000	0.926
$v_t^{\Delta fftar}$	–0.077	0.012	—	—
$v_t^{No\,\Delta fftar}$	–0.032	0.005	—	—
$v_t^{l,\Delta fftar}$	—	—	–0.102	0.020
$v_t^{l,No\,\Delta fftar}$	—	—	–0.045	0.031
$v_t^{m,\Delta fftar}$	—	—	–0.083	0.055
$v_t^{m,No\,\Delta fftar}$	—	—	–0.010	0.255
$v_t^{s,\Delta fftar}$	—	—	0.008	0.847
$v_t^{s,No\,\Delta fftar}$	—	—	–0.034	0.076
No. of observations	2,678	—	2,678	—
Standard error	0.344	—	0.342	—
$\bar{R}^2$	0.102	—	0.111	—

more reserves than the operating procedure indicates, the funds rate might fall, and vice versa. Given the previous results, $(OMOMPA - OPDOMO)_t$ and v_t are partitioned into days when the funds rate target was and was not changed. Consistent with Carpenter and Demiralp's finding, there is a negative and statistically significant relationship between $(ff - fftar)_t$ and v_t. Surprisingly, the absolute value of the estimate is nearly twice as large on days when the funds rate target was changed than when it was not.[19]

The results also suggest that the funds rate will decline if the Desk adds or drains more reserves than the operating procedure indicates is necessary. The coefficients are not statistically significant, however.

Following up on Carpenter and Demiralp's finding of non-linearity in the effect of supply shocks on the funds rate, I partition v_t into days when the corresponding shocks are large ($l \geq \$2$ billion), medium ($m >$ \$1 billion but $<$ \$2 billion), and small ($s \leq \$1$ billion). To guarantee that the effect is due to non-linearity and not to an intercept shift, I include dummy variables for each of these partitions. The estimates, also presented in Table 4, confirm Carpenter and Demiralp's finding.[20] Specifically, although the effect of v_t on the funds rate is nearly always negative, it is statistically significant only for large supply shocks. Moreover, it is only on days when the target is not changed. The coefficient is larger for days when the target was changed, but not statistically significant at the 5 percent level. It is important to note that it takes a relatively large supply shock to have a statistically significant impact on the funds rate. Consequently, in contrast with the implications of the estimates from Table 2, these estimates suggest that the demand for reserves is relatively interest elastic. As noted above, shocks this large are relatively rare events. However, it is worth noting that when v_t is partitioned by size, with the exception of settlement Wednesday, day-of-the-maintenance-period differences in the behavior of the funds rate are significantly reduced and become statistically significant. Hence, there appears to be some relationship between large supply shocks and days of the maintenance period.

There are two reasons these findings do not support Carpenter and Demiralp's assertion that the response of the funds rate to supply shocks provides "strong evidence of a liquidity effect at the daily frequency." First, consistent with Figure 8, reserve supply shocks account for very little of the daily variability of the funds rate from the target. Indeed, if v_t is omitted from the equation, $\bar{R}^2$ declines by less than 0.01 of a percentage point. Second, and most important, while the estimates suggest that large shocks to reserves are associated with changes in the equilibrium funds rate, such estimates provide no evidence for the more interesting and policy-relevant question of whether the Fed brings about permanent changes in the funds rate through open market operations. Indeed, the estimates suggest that it is unlikely that the Fed does this. There were only 554 days in the entire sample of 3,176 daily observations when the Desk deviated from its operating procedure by \$2.0 billion or more. Moreover, the estimates suggest that the largest deviation (–\$9.19 billion) would have generated about a 42-basis-point rise in the funds rate. Hence, these estimates suggest that it would take a series of relatively large open market operations in one direction to bring about the kind of changes in the equilibrium funds rate that the Fed is often credited with engineering. As I have already noted, there is no evidence that the Desk engaged in such open market operations upon changing the funds rate target.

The Liquidity Effect and the Federal Funds Market

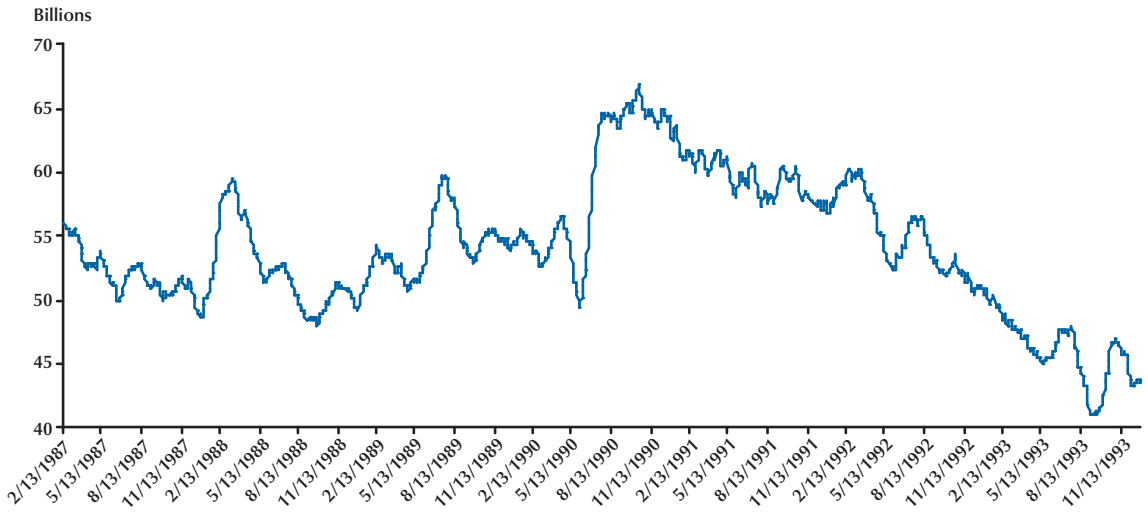
As a general rule, the larger a single market participant's activities are in the market, the larger should be the effect of such activities on equilibrium price. Indeed, the hypothesis of atomistic market participants is a cornerstone of the com-

[19] The results are very similar if the sample ends on December 31, 1993; hence, the results for the shorter sample are not presented here.

[20] The equation was also estimated to allow for corresponding shifts in the intercept. The qualitative results were unchanged, so only the results that do not include corresponding shifts in the intercept are presented here.

Figure 9

Thirty-Day Moving Average of Federal Funds Trading Volume



NOTE: Data are plotted on the last day of the moving average.

petitive market model. As a general rule, one would expect the Fed's ability to influence the federal funds rate to be positively related to the relative importance of its activities in the federal funds market—the more liquidity the Fed provides to the market, the larger should be its ability to affect the equilibrium federal funds rate. Hence, some additional evidence on the potential for a liquidity effect can be obtained by investigating the relative importance of open market operations in the federal funds market.

Despite the importance of the federal funds rate in the conduct of monetary policy, surprisingly little is known about it. Federal funds transactions involve the purchase or sale of deposit balances at the Fed. Hence, direct market participation is limited to entities that hold deposits at the Fed. For the federal funds market, this means banks, Fannie Mae, Freddie Mac, and Federal Home Loan Banks.[21] There are both brokered and non-brokered transactions in the market.[22] Until recently, relatively little was known about the overall size of the market. Using estimated data from Fedwire funds transfers during the first quarter of 1998, Furfine (1999) estimates the average daily volume of federal funds transactions to be $144 billion. Recently, Demiralp, Preslopsky, and Whitesell (2006) have used a modification of Furfine's methodology to estimate the size of the funds market over the period 1998-2003. They find that the average daily volume of transactions in the funds market in the first quarter of 1998 was $145 billion and that the daily volume of federal funds transactions increased until 2001 and then declined slightly.

Knowledge of the division of the market between brokered and non-brokered trading is less well known. Stigum (1990) suggested that the brokered funds market was about $70 billion per day in the late 1980s; however, Furfine (1999) found that about 83 percent of the identified federal funds transactions were brokered.

The published federal funds rate is a quantity-weighted average of transactions of a group of

[21] Fannie Mae, Freddie Mac, and Federal Home Loan Banks are major players in the federal funds market and often have had zero or near zero balances with the Fed at the end of the day.

[22] See Stigum (1990), Furfine (1999), and Demiralp, Preslopsky, and Whitesell (2006) for discussions of various aspects of the federal funds market.

brokers that report their transactions daily to the Federal Reserve Bank of New York. The 30-day moving average of the total volume of federal funds transactions reported by these brokers for the period January 1, 1987, through December 31, 1993, is presented in Figure 9. The trading volume hovered around $53 billion from the beginning of 1987 to mid-1990 and then increased dramatically by about $10 billion. Trading volume peaked in October 1990 and then began to decline. The initial decline in trading volume coincides with the elimination of reserve requirements on nonpersonal time and savings deposits, which reduced reserve demand by about $13.5 billion. The sharp decline in 1992 also coincides reasonably well with the reduction in percentage reserve requirements from 12 to 10 percent.[23] Why trading volume trends down beginning in 1991 is unclear, however.

In any event, these volume figures suggest that the brokers who report daily to the Federal Reserve Bank of New York account for a relatively small share of the brokered market—and an even smaller share of the total market. Indeed, based on Furfine's and Demiralp, Preslopsky, and Whitesell's estimates, the brokers that report daily to the Fed account for roughly about a third of the federal funds market.

Despite the possibility that the brokered transactions appear to represent a relatively small share of the federal funds market, these are the correct data for analyzing the relative importance of open market operations because these data are used to calculate the effective federal funds rate—the rate used in virtually all analyses of monetary policy.

The day-to-day variation in the volume of trading among these brokers is relatively large. There are only four days in this sample when the daily change in the trading volume is $5 billion or less. In contrast, there were only 267 days (of the 3,176 days) where the absolute value of *OMOD* was larger than $5.0 billion. It is hardly surprising, therefore, that *OMOD* accounts for almost none of the daily variation in the volume of federal funds transactions.

The relatively small size of open market operations alone may account for the results presented above. But there are other reasons for suspecting that the impact of open market operations on the funds rate is small. While seldom discussed in analyses of open market operations and the federal funds rate, in reality the link between open market operations and the funds rate is second-order. Open market operations do not directly affect the supply of federal funds. Rather, they directly affect the supply of reserves available to banks. Banks need not automatically increase or decrease federal funds trading when open market operations alter the availability of reserves. Nevertheless, because the initial effect of open market operations is on the reserves of large banks, some of whom may act as brokers in the federal funds market, simultaneously buying and selling funds (e.g., Furfine, 1999), it is reasonable to assume that open market operations will likely affect the availability of funds in the market.

Nevertheless, it is important to remember that the volume of federal funds trading is determined by a variety of factors that are independent of daily open market operations. For example, Meulendyke (1998) notes that beginning in the 1960s, when short-term rates rose above Regulation Q interest rate ceilings, large banks began financing their longer-term lending in the overnight market. It is now recognized that many banks finance a significant part of their loan portfolio in the overnight markets. It is also well known that large banks tend to be net demanders of funds, while small banks tend to be net suppliers. Hence, daily changes in the volume of federal funds transactions are likely to be affected by changes in the distribution of deposit and reserve flows unrelated to daily open market operations.

Not only is the daily volume of federal funds transactions large relative to daily open market operations, it is many times larger than the overnight reserve balance at the Fed—the commodity being traded (e.g., Taylor, 2001). Although the exact source of the disparity between the flow of federal funds transactions and the stock of the

[23] It is also the case that the number of brokers has changed over time. Unfortunately, there is no precise dating of changes in the number of participating brokers.

Table 5

The Daily Change in Total Reserves: January 2, 1986–December 31, 1996

Variable	Coefficient	Significance level
Constant	–0.461	0.000
$\Delta E_{t-1} f(ff_t^*, x_t)$	0.688	0.000
$\Delta E_{t-1} NBR_t$	0.558	0.000
v_t	0.672	0.000
ΔBR_t	0.846	0.000
$OMOD_t$	0.373	0.000
No. of observations	2,677	—
Standard error	2.7542	—
$\bar{R}^2$	0.2602	—

commodity being traded is unclear, there can be little doubt that the flow of federal funds transactions is only weakly linked to the stock of the commodity being traded.[24]

Finally, since the early 1980s the Desk has followed the practice of entering the market once per day—before January 1987 this occurred at about 11:30 EST. Federal funds transactions occur continuously throughout the day. Indeed, spikes in the funds rate that are often associated with settlement Wednesdays are thought to be due to trading that occurs later in the day. In any event, if open market operations were to have a significant effect on the funds rate, one might expect the effect to occur around the time that the Desk is in the market. Hence, the extent to which these activities would affect the transactions-weighted-average of transactions rates over the day is difficult to say.

Although the effect of open market operations on the funds market (and, consequently, the funds rate) is indirect and uncertain, their effect on total reserves is not. Moreover, conceptually, open market operations affect the funds rate by causing banks to buy or sell funds when the supply of reserves is decreased or increased, respectively, through open market operations. Hence, the relative importance of open market operations can be gauged by seeing how much of the variation in daily changes in total reserves they account for. To this end, changes in total reserves are regressed on changes in the Desk's estimate of reserve demand and reserve supply, changes in borrowing, errors in forecasting autonomous factors that affect reserves, and daily open market operations. The results, reported in Table 5, show that changes in total reserves are positively and significantly related to daily open market operations. Indeed, when $OMOD_t$ is deleted from the equation, $\bar{R}^2$ decreases from 0.2602 to 0.1736, suggesting that $OMOD_t$ accounts for nearly 10 percent of the daily changes in total reserves. This simple analysis suggests that, while important, $OMOD_t$'s contribution to changes in total reserves is quantitatively small. Given their relatively small effect on total reserves, it is not surprising that open market operations have an even smaller effect on federal funds.

ANALYSIS AND CONCLUSIONS

My analysis of the Desk's use of its operating procedure over the period March 1, 1984, through December 31, 1996, indicates that the Desk relied on the operating procedure in conducting daily open market operations. Indeed, the operating procedure alone accounts for nearly 40 percent of open market operations conducted during this period. The operating procedure and other factors—such as day-of-the-maintenance-period and day-of-the-year effects, differences between the funds rate and the funds rate target just prior to open market operations, and changes in the funds rate target—account for more than 50 percent of the variation in daily open market operations. Although large, these estimates indicate that there are other important factors that cause the Desk to deviate from its operating procedure.

Contrary to conventional wisdom—that the Fed controls the federal funds rate through open

[24] The large flow of federal funds relative to the daily volume of balances at the Federal Reserve would appear to be inconsistent with Demiralp and Farley's (2005, p. 1132) characterization of open market operations and the equilibrium federal funds rate. They suggest that open market operations "are used to bring the supply of balances at the Federal Reserve in line with the demand for them at an interest rate (the federal funds rate) near the level specified by the Federal Open Market Committee (FOMC)."

market operations—I find little support of an important liquidity effect at the daily frequency. While there is some evidence of a statistically significant negative relationship between reserve supply shocks and the funds rate, the relationship is weak. Consequently, to move the funds rate by 25 basis points or more, it appears that the Desk would have to conduct considerably larger open market operations than it has, in fact, conducted.

One possible reason for this finding is that changes in the funds rate target were anticipated. However, after conducting an extensive analysis of press reports, Poole, Rasche, and Thornton (2002, p. 73) found "little indication that the market was aware that the Fed was setting an explicit objective for the federal funds rate before 1989." This is not surprising in that Thornton (2006) shows that the FOMC was reluctant to acknowledge that it was targeting the funds rate. Moreover, Poole, Rasche, and Thornton (2002) show that the market frequently did not know that policy had changed when the Fed changed the target during 1989 and 1990 and that the target changes prior to 1994 were generally not predicted. Furthermore, prior to 1994, most funds rate target changes occurred during the intermeeting period (the period between consecutive FOMC meetings) and, hence, would have been difficult to predict exactly even if the market knew the Fed was targeting the funds rate and was expecting a target change. Consequently, it is extremely unlikely that rational expectations accounts for the lack of evidence of a liquidity effect.

Another possible explanation for the lack of evidence of a liquidity effect is that target changes are implemented over a period of several days, not immediately (e.g., Taylor, 2001). The analysis presented here finds no support for this explanation, however.

Yet another explanation for this finding is that open market operations account for a very small proportion of the variation in the equilibrium quantities in the reserves and federal funds markets. This explanation is supported by the fact that open market operations explain relatively little of the maintenance-period variation in total reserves and an extremely small amount of the daily variation in daily volume of federal funds transactions.

One explanation not investigated here is that some, and perhaps many, changes in the funds rate target are endogenous. Economic theory suggests that the Fed cannot control the natural rate of interest. Hence, when market forces bring about changes in inflation expectations or the real rate, the Fed can either change its target or permit policy to become inadvertently tighter or easier, depending on whether market forces are driving interest rates down or up. In any event, if target changes represent a response of the Fed to changing conditions that affect nominal interest rates rather than an exogenous change engineered to achieve some policy objective, the Desk would not necessarily have an incentive to add or drain reserves aggressively when the target is changed. Elsewhere (Thornton, 2004), I have presented evidence that many of the target changes identified in an influential paper by Cook and Hahn (1989) were endogenous. A proper investigation of this possibility during this period is left for future research.

Finally, I would note that evidence that the liquidity effect is small and statistically unimportant does not mean that the Fed could not move interest rates if it desired. It merely suggests that the Fed has not done so. Given their direct effect on reserves and the corresponding effect of changes in reserves on banks, one can understand why the Fed might be reluctant to engage in large open market operations. This reluctance would be particularly strong if the Fed is a small enough player in the credit market that it would take very large open market operations to generate significant changes in the equilibrium short-term rates.

REFERENCES

Carpenter, Seth and Demiralp, Selva. "The Liquidity Effect in the Federal Funds Market: Evidence from Daily Open Market Operations." *Journal of Money, Credit, and Banking*, June 2006, *38*(4), pp. 901-20.

Cook, Timothy and Hahn, Thomas. "The Effect of Changes in the Federal Funds Rate Target on Market

Interest Rates in the 1970s." *Journal of Monetary Economics*, November 1989, *24*(3), pp. 331-51.

de Jong, Robert and Herrera, Ana Maria. "Dynamic Censored Regression and the Open Market Desk Reaction Function." Unpublished manuscript, 2004.

Demiralp, Selva and Farley, Dennis. "Declining Required Reserves, Funds Rate Volatility, and Open Market Operations." *Journal of Banking and Finance*, May 2005, *29*(5), pp. 1131-52.

Demiralp, Selva and Jorda, Oscar. "The Announcement Effect: Evidence from Open Market Desk Data." *Economic Policy Review*, May 2002, *8*(1), pp. 29-48.

Demiralp, Selva; Preslopsky, Brian and Whitesell, William. "Overnight Interbank Loan Markets." *Journal of Economics and Business*, January/February 2006, *58*(1), pp. 67-83.

Friedman, Benjamin M. "The Future of Monetary Policy: The Central Bank as an Army with Only a Signal Corps?" *International Finance*, November 1999, *2*(3), pp. 321-38.

Furfine, Craig H. "The Microstructure of the Federal Funds Market." *Financial Markets, Institutions & Instruments*, December 1999, *8*(5), pp. 24-44.

Hamilton, James D. "Measuring the Liquidity Effect." *American Economic Review*, March 1997, *87*(1), pp. 80-97.

Meulendyke, Ann-Marie. *U.S. Monetary Policy & Financial Markets*. Federal Reserve Bank of New York, 1998.

Pagan, Adrian R. and Robertson, John C. "Resolving the Liquidity Effect." Federal Reserve Bank of St. Louis *Review*, May/June 1995, *77*(3), pp. 33-54.

Poole, William; Rasche, Robert H. and Thornton, Daniel L. "Market Anticipations of Monetary Policy Actions." Federal Reserve Bank of St. Louis *Review*, July/August 2002, *84*(4), pp. 65-93.

Sternlight, Peter D. "Monetary Policy and Open Market Operations during 1990." Federal Reserve Bank of New York *Quarterly Review*, Spring 1991, *16*(1), pp. 52-78.

Sternlight, Peter D. "Monetary Policy and Open Market Operations during 1991." Federal Reserve Bank of New York *Quarterly Review*, Spring 1992, *17*(1), pp. 72-95.

Stigum, Marcia. *The Money Market*. Third Edition. New York: McGraw-Hill, 1990.

Taylor, John B. "Expectations, Open Market Operations, and Changes in the Federal Funds Rate." Federal Reserve Bank of St. Louis *Review*, July/August 2001, *83*(4), pp. 33-47.

Thornton, Daniel L. "The Effect of Monetary Policy on Short-Term Interest Rates." Federal Reserve Bank of St. Louis *Review*, May/June 1988, *70*(3), pp. 53-72.

Thornton, Daniel L. "Identifying the Liquidity Effect at the Daily Frequency." Federal Reserve Bank of St. Louis *Review*, July/August 2001a, *83*(4), pp. 59-78.

Thornton, Daniel L. "The Federal Reserve's Operating Procedure, Nonborrowed Reserves, Borrowed Reserves and the Liquidity Effect." *Journal of Banking and Finance*, September 2001b, *25*(9), pp. 1717-39.

Thornton, Daniel L. "The Fed and Short-Term Rates: Is It Open Market Operations, Open Mouth Operations or Interest Rate Smoothing?" *Journal of Banking and Finance*, March 2004, *28*(3), pp. 475-98.

Thornton, Daniel L. "A New Federal Funds Rate Target Series: September 27, 1982–December 31, 1993." Working Paper 2005-032A, Federal Reserve Bank of St. Louis, 2005.

Thornton, Daniel L. "When Did the FOMC Begin Targeting the Federal Funds Rate? What the Verbatim Transcripts Tell Us." *Journal of Money, Credit, and Banking*, December 2006, *38*(8), pp. 2039-71.

Federal Reserve Bank of St. Louis *Review*, Annual Index, 2007

JANUARY/FEBRUARY

William Poole, "Milton Friedman, 1912-2006: Some Personal Reflections."

William Poole, "Understanding the Fed."

Thomas A. Garrett, "The Rise in Personal Bankruptcies: The Eighth Federal Reserve District and Beyond."

Giang Ho and Anthony Pennington-Cross, "The Varying Effects of Predatory Lending Laws on High-Cost Mortgage Applications."

Howard J. Wall, "Regional Business Cycle Phases in Japan."

MARCH/APRIL

William Poole, "Data Dependence."

William Poole, "Data, Data, and Yet More Data."

Michael D. Bordo and David C. Wheelock, "Stock Market Booms and Monetary Policy in the Twentieth Century."

Christopher H. Wheeler, "Trends in Neighborhood-Level Unemployment in the United States: 1980 to 2000."

MAY/JUNE

William Poole, "The GSEs: Where Do We Stand?"

Edward Nelson, "Milton Friedman and U.S. Monetary History: 1961-2006."

Daniel L. Thornton, "The Lower and Upper Bounds of the Federal Open Market Committee's Long-Run Inflation Objective."

Yi Wen, "Granger Causality and Equilibrium Business Cycle Theory."

JULY/AUGUST

Frontiers in Monetary Policy Research

Riccardo DiCecio and Edward Nelson, "An Estimated DSGE Model for the United Kingdom."

Glenn D. Rudebusch, Brian P. Sack, and Eric T. Swanson, "Macroeconomic Implications of Changes in the Term Premium."

Ravi Bansal, "Long-Run Risks and Financial Markets."

Michael F. Gallmeyer, Burton Hollifield, Francisco J. Palomino, and Stanley E. Zin, "Arbitrage-Free Bond Pricing with Dynamic Macroenomic Models."

Gaetano Antinolfi, Costas Azariadis, and James B. Bullard, "Monetary Policy as Equilibrium Selection."

Narayana R. Kocherlakota, "Model Fit and Model Selection."

Commentaries by:

Martin Fukač
Adrian Pagan
John H. Cochrane
Thomas J. Sargent
Pamela A. Labadie
Peter N. Ireland
Lee E. Ohanian

SEPTEMBER/OCTOBER

Marcela M. Williams and Richard G. Anderson, "Currency Design in the United States and Abroad: Counterfeit Deterrence and Visual Accessibility."

Rubén Hernández-Murillo, "Experiments in Financial Liberalization: The Mexican Banking Sector."

Kevin L. Kliesen, "How Well Does Employment Predict Output?"

Robert H. Rasche and Marcela M. Williams, "The Effectiveness of Monetary Policy."

NOVEMBER/DECEMBER

Massimo Guidolin and Elizabeth A. La Jeunesse, "The Decline in the U.S. Personal Saving Rate: Is It Real and Is It a Puzzle?"

R. Alton Gilbert and David C. Wheelock, "Measuring Commercial Bank Profitability: Proceed with Caution."

Subhayu Bandyopadhyay and Howard J. Wall, "The Determinants of Aid in the Post-Cold War Era."

Daniel L. Thornton, "Open Market Operations and the Federal Funds Rate."