

EMPIRICAL ESSAYS ON MACRO-FINANCIAL LINKAGES

Ola Melander



EFI THE ECONOMIC RESEARCH INSTITUTE



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To Nilla



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Tegucigalpa, April 2009

Ola Melander

Summary of Thesis

Introduction

This Ph.D. thesis consists of four self-contained papers in the field of empirical macroeconomics. The common theme of the thesis is empirical investigation of linkages between macroeconomics and financial markets. All four papers investigate issues which are important in the field of economics as well as relevant for policy-makers, especially in the current international financial crisis.

The first three papers investigate the macroeconomic implications of financial-market imperfections. Imperfect information between borrowers and lenders makes it more costly for firms to finance investments with external funds than with internal funds. The external finance risk premium depends on the strength of firm balance sheets, which hence affects firm investment. The first paper examines the importance of financial constraints for investment using a large Swedish firm-level data set which includes many smaller firms (where balance sheet effects are likely to be especially important). I study the effect of cash flow on investment, controlling for fundamental determinants of investment and any information in cash flow about investment opportunities.

The second paper estimates the impact of an adverse shock to bank capital on credit availability and spending in the United States, allowing for feedback from spending and income through the balance sheets of banks, firms and households. The purpose is to help predict the depth and duration of the current economic downturn.

Finally, the third paper studies the effects of real exchange rate depreciations in Bolivia, which is an economy with extreme liability dollarization. A currency depreciation increases the domestic-currency value of dollar liabilities and the debt-service burden. Thus, there is an adverse effect on firms' balance sheet position and investment, which could potentially cause depreciations to have contractionary effects.

The fourth paper studies another aspect of macro-financial linkages. The so-called uncovered interest parity (UIP) condition states that interest rate differentials compensate for expected exchange rate changes, equalizing the expected returns from holding assets which only differ in terms of currency denomination. This theoretical relationship between financial-market variables is a key assumption in open-economy macroeconomic models. Because of data availability problems, there is a lack of empirical

tests of UIP for developing countries. The paper aims to fill this gap in the literature by studying the case of Bolivia, where there are bank accounts which only differ in terms of currency denomination (bolivianos or U.S. dollars).

Summary of Papers

Paper 1: The Effect of Cash Flow on Investment: An Empirical Test of the Balance Sheet Channel

Does the availability of financing matter for the quantity of firms' investment? In a world of perfect information, investment is only determined by economic fundamentals. But when there is imperfect information between borrowers and lenders, external financing is more expensive than internal financing. Moreover, the external finance premium depends on the strength of firm balance sheets, which hence affects firm investment.

This paper empirically tests the balance sheet theory, where balance sheet status affects the economy's response to monetary and other shocks. The theory predicts a positive effect of cash flow on investment, given fundamental determinants of investment. I use an empirical method developed by Gilchrist and Himmelberg (1995, 1999), which has previously only been used to study very large, publicly traded firms. In contrast, this paper uses a large Swedish data set with many smaller firms, where balance sheet effects are likely to be especially important.

I find that a firm's cash flow has a positive impact on its investment, controlling for economic fundamentals and any information in cash flow about investment opportunities. As predicted by the balance sheet channel, the estimated effect of cash flow on investment is especially large for firms which, a priori, are more likely to be financially constrained (low-dividend, small and non-group firms). Moreover, the investment-cash flow sensitivity is significantly larger and more persistent during the first half of the sample period, which includes a severe banking crisis and recession, than during the second half.

Paper 2: Credit Matters: Empirical Evidence on U.S. Macro-Financial Linkages (with Tamim Bayoumi)

How deep and protracted will the current economic downturn in the United States be? The crisis originated in the financial system and financial-sector developments

will continue to be of crucial importance. In particular, there are risks of an adverse feedback loop from economic activity to balance sheets and credit availability.

This paper develops a framework for analyzing U.S. macro-financial linkages and uses a survey measure of bank lending standards as a proxy for credit availability. We estimate the effects of a negative shock to banks' capital/asset ratio on lending standards which, in turn, affect consumer credit, mortgages, and corporate loans, and the corresponding components of private spending (consumption, residential investment and business investment). In addition, our empirical model allows for feedback from spending and income to bank capital adequacy and credit. Hence, we trace the full credit cycle.

We find that an exogenous fall in the bank capital/asset ratio by one percentage point reduces real GDP by some $1\frac{1}{2}$ percent through its effects on credit availability, while an exogenous fall in demand of 1 percent of GDP is gradually magnified to around 2 percent through financial feedback effects. These quantitative results are similar to those obtained by Lown and Morgan (2006) and Swiston (2008) using different methods.

Paper 3: The Effects of Real Exchange Rate Shocks in an Economy with Extreme Liability Dollarization

In standard, small-open economy models, a real exchange rate depreciation has an expansionary effect on aggregate demand and output by reducing the relative price of domestically produced goods. In contrast, in an economy with substantial liability dollarization, the impact of depreciation on output could be reversed. Currency depreciation increases the domestic-currency value of foreign-currency liabilities, thus adversely affecting firm balance sheets. Such a balance sheet deterioration could have adverse effects on investment. In the current international financial crisis, a key issue for policymakers in countries with widespread foreign-currency borrowing is whether a real exchange rate depreciation would have the standard, expansionary effect, or if an adverse balance-sheet effect would dominate.

This paper studies the effects of real exchange rate shocks in an economy with extreme liability dollarization using vector autoregression (VAR) methods. Bolivia's extreme liability dollarization makes it an interesting case for empirical testing of the contractionary-depreciations hypothesis. In contrast to the previous contractionary-depreciations literature, which follows the approach in a paper by Kamin and Rogers (2000), this paper uses identification assumptions which are inspired by modern macro-economic theory and are common in the empirical VAR literature on the effects of monetary policy. I find that a real exchange rate depreciation has negligible effects on

output, since a contractionary balance-sheet effect on investment is counteracted by the standard expansionary effect on net exports. Furthermore, I find that depreciation has inflationary effects.

Paper 4: Uncovered Interest Parity in a Partially Dollarized Developing Country: Does UIP Hold in Bolivia? (And If Not, Why Not?)

According to the Uncovered Interest Parity (UIP) condition, interest rate differentials compensate for expected exchange rate changes, equalizing the expected returns from holding assets which only differ in terms of currency denomination. In the previous literature, there are many tests of UIP for industrialized countries and, more recently, some tests for emerging economies. In almost every study, the tests have found substantial deviations from UIP. However, due to data availability constraints, poorer developing countries have not been studied.

This paper tests UIP in a partially dollarized developing country, Bolivia, where bank accounts only differ in terms of currency denomination (bolivianos or U.S. dollars). The approach is similar to that used in a recent paper by Poghosyan, Kocenda and Zemcik (2008). I find that UIP does not hold in Bolivia, but that the deviations are smaller than in most other studies of developed and emerging economies. Interestingly, similar results are obtained irrespective of which dollar interest rate is used (dollar deposit rate in Bolivia or dollar deposit rate in the United States). Another finding is that several factors seem to contribute to the deviations from UIP. The so-called peso problem arises when domestic interest rates are high to compensate for the small possibility of a major currency crash, but no such large depreciation occurs during the sample period. The peso problem could possibly account for the observed data, but there is also evidence of a time-varying risk premium, as well as deviations from rational expectations.

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Papers

The Effect of Cash Flow on Investment: An Empirical Test of the Balance Sheet Channel

Ola Melander

ABSTRACT. This paper tests the balance sheet theory, where the status of balance sheets affects the economy's response to monetary and other shocks. The theory predicts a positive effect of cash flow on investment, given fundamental determinants of investment. I use an empirical method developed by Gilchrist and Himmelberg (1995, 1999), which has previously only been used to study very large, publicly traded firms. In contrast, this paper uses a large Swedish data set with many smaller firms, where balance sheet effects are likely to be especially important. I find that a firm's cash flow has a positive impact on its investment, controlling for any information in cash flow about investment opportunities. As predicted by the balance sheet channel, the estimated effect of cash flow on investment is especially large for firms which, a priori, are more likely to be financially constrained (low-dividend, small and non-group firms). Moreover, the investment-cash flow sensitivity is significantly larger and more persistent during the first half of the sample period, which includes a severe banking crisis and recession, than during the second half.

1. Introduction

In the current international financial crisis, the impact of financial shocks on real variables is clearly a key issue for economists and policymakers. According to the neoclassical theory of investment, firm investment is only determined by economic fundamentals, and it is not affected by financial variables such as cash flow. But in the presence of financial frictions due to imperfect information between borrowers and lenders, financial variables can have an effect on investment.

The purpose of this paper is to test for a balance sheet channel in the monetary transmission mechanism by studying the effect of cash flow on investment. However,

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the empirical results are of more general interest, not least in the current international financial crisis, when the impact of financial constraints on investment is one of the most important macroeconomic issues. According to the balance sheet theory, monetary policy causes changes in firm investment not only directly by affecting the level of interest rates, but also indirectly through its impact on firms' balance sheets. For example, Bernanke, Gertler and Gilchrist (1999) have developed a dynamic macroeconomic "financial accelerator" model, where financial frictions amplify the economy's response to monetary and other shocks. In the presence of financial frictions, it is more difficult and costly for firms to finance investments with external funds than with internal funds. In particular, the so-called external finance premium depends on the strength of a firm's balance sheet, which hence affects firm investment.

The standard empirical method which is used to investigate the importance of financial frictions for investment is to estimate the effect of cash flow (a proxy for balance sheet strength) on investment, controlling for fundamental determinants of investment. Schiantarelli (1995) and Hubbard (1998) provide excellent surveys of the empirical literature.¹ Most papers find a positive impact of cash flow on investment, which indicates that financial frictions influence investment decisions and that a balance sheet channel exists in the monetary transmission mechanism. A well-known potential problem with the standard method is that cash flow may not only be correlated with liquidity, but also with investment opportunities, which would cause estimates to be biased. In the early literature, a common solution to this problem was to include Tobin's Q in the regression to control for investment opportunities.

However, even in the absence of financial frictions, measured Tobin's Q may not be a sufficient control variable for investment opportunities, for example due to excess stock market volatility. A common approach in the more recent literature is to estimate separate regressions for groups of firms which, a priori, are more or less likely to be credit constrained, for example small vs. large firms. The purpose is to investigate if cash flow has a larger impact on investment for the more constrained firms (as predicted by the balance sheet theory), which is also the typical empirical finding. An underlying assumption is that measurement problems related to Tobin's Q are equally important for all firms. However, the method may give misleading results if Tobin's Q is relatively less informative about investment opportunities (and cash flow more informative) for small, young firms than for large, established firms. A larger coefficient on cash flow for small firms than for large firms may be a result of variation across firms in the explanatory power of Tobin's Q , rather than in the importance of liquidity constraints.

¹ For examples of more recent work, see Chatelain et al. (2003), Carpenter and Guariglia (2008) and Martinez-Carrascal and Ferrando (2008).

This paper uses a method developed by Gilchrist and Himmelberg (1995, 1999), which is specifically designed to deal with potential differences across firms in the information content of cash flow. Investment opportunities are summarized by a sales-based measure of the marginal product of capital, MPK . Cash flow is divided into two parts: one fundamental part which may contain information about investment opportunities, and one financial part which is orthogonal to investment opportunities. The authors estimate a vector autoregression (VAR) model with investment, MPK and cash flow, and investigate the impulse response of investment to a cash flow shock. By construction, the cash flow shock does not affect *current* MPK . To control for any predictive value of cash flow for *future* MPK , the impulse response of MPK is also studied. Separate VAR models are estimated for constrained and unconstrained firms. Thus, the method controls for any differences in the informational content of cash flow across the two groups of firms. If the financial part of cash flow (which does not contain any information about investment opportunities) still affects investment, then the availability of internal funds matters for firm investment, which constitutes evidence in favor of the balance sheet channel. Gilchrist and Himmelberg (henceforth GH) also study differences in investment-cash flow sensitivity across firms. The balance sheet theory predicts that the effect of cash flow on investment is especially large for firms which are likely to be financially constrained.

GH use firm-level panel data on large, publicly traded U.S. manufacturing firms. The key contribution of this paper is to extend their analysis by studying a much broader set of firms. I apply the GH methodology to a large, Swedish firm-level panel data set covering the period 1989-2005. Importantly, the data set includes many smaller firms where financial frictions are likely to be especially important. The GH methodology is particularly useful when studying smaller, non-publicly traded firms, since it does not require any data on the stock market value of a firm (which, in contrast, is needed when using Tobin's Q to control for fundamentals).

Another contribution of this paper is that the sample period includes the Swedish banking crisis in the early 1990's. This crisis was followed by a severe recession, during which GDP contracted by around 2% per year. Thus, it is possible to divide the sample into two parts and test whether the effect of cash flow is larger during a recession, when more firms are likely to be financially constrained.

I find that a positive cash flow shock has a positive effect on investment, even using the entire sample of firms. As expected, the effect is especially strong for financially constrained firms and, in particular, during the recession period. There are only two previous papers using similar firm-level data from Sweden. One paper is by Hansen (1999) who uses Euler equation methods with data from the period 1979-1995 and finds

evidence in favor of a balance sheet channel. My paper uses a larger and more recent data set, as well as an alternative method. Another related study by Jacobson, Lindé and Roszbach (2005) uses the aggregate default frequency as a measure of firm-level finances and finds substantial spillover effects on macroeconomic variables. However, the paper does not focus specifically on testing for the presence of a balance sheet channel.

The rest of this paper is organized as follows. Section 2 provides a theoretical background and discusses different empirical methods, in particular the Gilchrist-Himmelberg method. Section 3 describes the data set and Section 4 presents the empirical analysis, including robustness checks. Section 5 concludes the paper.

2. Testing for financial frictions: theory and empirical methods

Before discussing empirical tests for financial frictions, it is useful to briefly outline a benchmark model without any financial frictions. In the neoclassical investment model, investment is only determined by real factors. The model can be used as a basis for the empirical specifications.

2.1. Benchmark neoclassical investment model. In the standard neoclassical model, a firm maximizes the expected discounted value of future dividend payments:²

$$(2.1) \quad V_{i,t} = E_t \left[\sum_{s=0}^{\infty} \beta_{t+s} d_{i,t+s} \right]$$

where $V_{i,t}$ is the expected present discounted value of future dividends of firm i in period t , $d_{i,t+s}$ denotes the dividend payment in period $t+s$, β_{t+s} is the discount factor used for payments occurring in period $t+s$ and E_t is the standard expectations operator.

The dividend payout function is:

$$(2.2) \quad d_{i,t}(K_{i,t}, I_{i,t}) = p_t [F(K_{i,t}) - G(I_{i,t}, K_{i,t})] - p_t^k I_{i,t}$$

where $K_{i,t}$ is the capital stock, $I_{i,t}$ is gross investment, p_t is the price of output, p_t^k is the price of capital goods, $F(K_{i,t})$ is the production function, and $G(I_{i,t}, K_{i,t})$ is an adjustment cost function. Both functions $F(K_{i,t})$ and $G(I_{i,t}, K_{i,t})$ are assumed to exhibit constant returns to scale and there is perfect competition. The adjustment costs are quadratic and subject to technology shocks $\varepsilon_{i,t}$:

$$(2.3) \quad G(I_{i,t}, K_{i,t}) = \frac{b}{2} \left(\frac{I_{i,t}}{K_{i,t}} - a - \varepsilon_{i,t} \right)^2 K_{i,t}.$$

² This presentation follows Cummins, Hassett and Oliner (2006). The model was originally developed by Hayashi (1982).

Given these standard assumptions, investment is described by the following regression equation:

$$(2.4) \quad \left(\frac{I}{K}\right)_{i,t} = a + \frac{1}{b} \left[\frac{V_{i,t}}{p_t^k (1 - \delta) K_{i,t-1}} - 1 \right] \frac{p_t^k}{p_t} + \varepsilon_{i,t} = a + \frac{1}{b} Q_{i,t} + \varepsilon_{i,t}$$

where Q denotes average q , which is the total value of the firm relative to the replacement cost of its capital. Naturally, investment decisions are not based on the average value of capital, but rather on the marginal value of capital. Marginal q is defined as the shadow value of capital (the expected marginal contribution of an additional unit of capital to future profits). However, marginal q is unobservable, and hence empirical studies need to use some measure of average q , usually based on the stock market value of the firm. Fortunately, under the above assumptions, marginal and average q are equal.

Under the “null hypothesis” of perfect capital markets (no financial frictions), equation (2.4) perfectly describes a firm’s investment behavior. In this special case, there is no theoretical reason for including any additional explanatory variables. Most empirical research uses equation (2.4) as a point of departure and tests the neoclassical theory by investigating whether financial factors do, in fact, add explanatory value in empirical investment equations.

2.2. Empirical tests of financial frictions. There are several different ways of introducing financial frictions in theoretical models.³ A general result in the theoretical literature is that asymmetric information in one form or another—adverse selection, moral hazard or costly state verification—gives rise to an external finance premium. External finance is more expensive than internal finance, and the premium is larger when the borrowing firm’s balance sheet is in poor condition and the required loan is large. Thus, in the presence of financial frictions, a firm’s access to internal funds affects its investment decisions.

A standard approach in the empirical literature is to augment equation (2.4) with cash flow (a measure of changes in the firm’s liquidity position):

$$(2.5) \quad \left(\frac{I}{K}\right)_{i,t} = a + \frac{1}{b} Q_{i,t} + \gamma \left(\frac{CF}{K}\right)_{i,t} + \varepsilon_{i,t}.$$

Under the null hypothesis of perfect capital markets, the estimated coefficient on cash flow, γ , should be insignificantly different from zero. In contrast, under the alternative hypothesis of financial frictions, the estimated γ should be positive and significant.

³ However, the purpose of this paper is to empirically test for financial frictions rather than theoretical modeling. See Gertler (1988) for a broad survey with a focus on theoretical models, and Bernanke, Gertler and Gilchrist (1999) for a representative model.

Schiantarelli (1995) and Hubbard (1998) provide excellent surveys of the empirical literature.

A potential problem when estimating equation (2.5) is that there may be measurement error in stock-market based measures of Q , so that measured Q is an imperfect control for fundamentals. Such measurement error could, for example, be due to excess stock-market volatility, as discussed by Blanchard, Rhee and Summers (1993) and Shiller (2000). Intuitively, if non-fundamental factors such as bubbles may influence equity prices, stock-market based control variables for fundamental investment opportunities are imperfect. Moreover, cash flow is likely to not only be correlated with a firm's liquidity position, but also with its investment opportunities. Thus, the estimated coefficient on cash flow may turn out to be positive and significant even if, in fact, firms are not financially constrained and there are no deviations from the benchmark model in subsection 2.1.

In an attempt to solve this problem, Fazzari, Hubbard and Petersen (1988) and many subsequent papers investigate the effect of cash flow on investment for different categories of firms. If the importance of financial frictions varies across firms, the impact of cash flow on investment should also vary. Firms are divided into groups which, a priori, are more or less likely to be financially constrained. Specifically, Fazzari et al. divide firms into different groups based on firm dividend policy. A high dividend signals that a firm is not credit constrained—if it were, dividends would be cut. Therefore, the investment of high-dividend firms should not be sensitive to cash flow. Conversely, a low dividend signals that a firm is credit constrained, which causes cash flow to be a determinant of investment. In the presence of financial frictions, the sensitivity of investment to cash flow should be larger for credit-constrained (low-dividend) firms, which is also a common finding in the empirical literature. Other variables which have been used to divide firms into groups according to the importance of financial frictions are firm size, the existence (or not) of a bond rating and membership in a company group. The prediction of the balance sheet theory is that cash flow has a larger effect on investment for firms which are small and/or do not have a bond rating, since they are less monitored by external analysts. Moreover, firms which are independent of company groups do not have access to a group's internal capital market to alleviate financing constraints, which makes their investment more sensitive to cash flow.

However, there is a potential problem with the sample-split method when applied to equation (2.5). As pointed out by Poterba (1988), the method assumes that the amount of measurement error in Q is the same for small, young companies as for larger, established companies (and that cash flow is equally informative about investment opportunities for both groups of firms). However, it is likely that measurement error

is more severe for small, young firms (and that cash flow is more informative about investment opportunities), whose valuation is subject to more uncertainty and is more dependent on current profitability. If so, a finding that cash flow has an especially large effect on investment for small companies is only to be expected and does not constitute any evidence in favor of a balance sheet channel.⁴

An alternative empirical method which has been used in the literature is to estimate the firm's first-order condition for the capital stock (the Euler equation), derived under the null hypothesis of perfect capital markets. Some early papers using this approach are Whited (1992) and Bond and Meghir (1994). A rejection of the Euler equation model (using a test of overidentifying restrictions) is interpreted as evidence in favor of financial frictions. However, there are some drawbacks with this approach. First, as shown by, for example, Oliner, Rudebusch and Sichel (1996), the estimates suffer from parameter instability, thus making the results sensitive to model specification. Moreover, as shown in the consumption literature by Zeldes (1989), the method may fail to detect financial frictions which are approximately constant over time.⁵ Against this background, Gilchrist and Himmelberg developed yet another empirical method which is described in the following subsection.

2.3. The Gilchrist-Himmelberg empirical method. The papers by Gilchrist and Himmelberg (1995, 1999) study large, publicly traded U.S. manufacturing firms from the Compustat database for the periods 1979-1989 and 1980-1993, respectively. A recent paper by Love and Zicchino (2006) uses the same methodology to investigate how cross-country differences in the level of financial development affect investment-cash flow sensitivities. They use firm-level panel data on large publicly traded firms in 36 countries from the Worldscope database for the period 1988-1998. The main finding is that the importance of financial frictions for investment behavior is larger in countries with low financial development. The same methods are also used by Gilchrist, Himmelberg and Huberman (2005) who study the effect of stock price bubbles on corporate investment.

The GH method divides cash flow into two parts: one part which may contain information about investment opportunities (as summarized by the marginal product of capital, MPK), and another part which is orthogonal to investment opportunities.

⁴ Some other criticisms of the investment-cash flow sensitivity literature are that: (i) it is not necessarily true that investment-cash flow sensitivities measure the degree of financing constraints (see Kaplan and Zingales, 1997 and 2000, and Gomes (2001)), and (ii) the positive coefficient on cash flow disappears when the earnings forecasts of equity analysts are used to construct Q (see Cummins, Hassett and Oliner (2006)).

⁵ See Gilchrist and Himmelberg (1995) and Schiantarelli (1995) for further discussion and additional references.

The idea is to first estimate a VAR model with investment, MPK and cash flow, and then investigate the impulse response of investment to a cash flow shock. By construction, the cash flow shock is orthogonal to *current* MPK . To control for any predictive value of cash flow for *future* MPK , the impulse response of MPK is also studied.

Separate VAR systems are estimated for firms which are likely to be constrained vs. unconstrained. Thus, the method controls for any differences in the informational content of cash flow across the two groups of firms. If the part of cash flow which does not contain any information about investment opportunities still affects investment, the availability of internal funds matters for investment, which constitutes evidence in favor of the balance sheet channel. A larger effect for constrained than unconstrained firms would provide additional supportive evidence.⁶

The GH method is particularly useful for data sets (such as that used in this paper) with many smaller, non-quoted firms, since it does not require a stock-market based measure of Q to control for fundamentals in the investment regressions. Instead, GH (1999) use a sales-based measure of MPK to control for fundamentals. Assuming a Cobb-Douglas production function and profit-maximizing behavior, the following expression can be derived for MPK :

$$(2.6) \quad MPK \equiv \frac{\partial \pi}{\partial k} = \theta \frac{s}{k}$$

where π denotes profits, θ is a parameter and s denotes sales. The parameter θ , which can differ across industries, is related to the capital share of output and the (firm-level) price elasticity of demand. Hence, up to a scale parameter, the sales-to-capital ratio measures MPK .⁷

GH also assume that, on average, firms are at their equilibrium capital stocks, which implies that the marginal benefit of an additional unit of capital is equal to the marginal cost of capital:

$$(2.7) \quad MPK = r + \delta$$

where r is the risk-adjusted discount rate and δ is the depreciation rate of capital.

⁶ GH also develop a second, more structural method to control for possible information in cash flow about investment opportunities (current and future MPK). Following Love and Zicchino (2006), I do not use this alternative method, which has been criticized for not properly identifying the effect of cash flow on investment (see, for example, footnote 11 in Cummins, Hassett and Oliner (2006)).

⁷ Another possible measure of MPK , which is used by GH in their earlier paper, is based on operating income rather than sales. As discussed in GH (1999), the operating-income based measure requires the possibly unrealistic assumptions of zero fixed costs and perfect competition, which makes the measure less reliable.

To compute MPK from equation (2.6), the parameter θ must first be estimated for each industry. Substituting equation (2.6) into equation (2.7), and taking the average over all firms $i \in I(j)$ and years $t \in T(i)$ in industry j , and solving for θ gives the estimator:

$$(2.8) \quad \hat{\theta}_j = \left(\frac{1}{N_j} \sum_{i \in I(j)} \sum_{t \in T(i)} \left(\frac{s}{k} \right)_{i,t} \right)^{-1} \frac{1}{N_j} \sum_{i \in I(j)} \sum_{t \in T(i)} (r_{i,t} + \delta_{i,t})$$

where N_j is the number of observations for industry j . While GH assume that the depreciation rates $\delta_{i,t}$ are the same for all industries, I allow for industry-specific depreciation rates (which are reported in Table 2 in the appendix).

Finally, we can use the estimated $\hat{\theta}_j$ from equation (2.8) in equation (2.6), which gives an estimated MPK for each firm and year:

$$(2.9) \quad \widehat{MPK}_{i,t} = \hat{\theta}_j \left(\frac{s_{i,t}}{k_{i,t}} \right).$$

The empirical model is a reduced-form panel data VAR with the assumed Cholesky ordering investment, MPK and cash flow:

$$(2.10) \quad y_{i,t} = Ay_{i,t-1} + f_i + e_t + v_{i,t}$$

$$(2.11) \quad E(v_{i,t} \mid y_{i,t-1}, f_i, e_t) = 0 \Rightarrow E(y_{i,t-1}v_{i,t+s}) = 0 \quad \forall s \geq 0$$

and with the following definitions:

$$(2.12) \quad y_{i,t} = \left(\frac{I_{i,t}}{K_{i,t}}, MPK_{i,t}, \frac{CF_{i,t}}{K_{i,t}} \right)$$

$$(2.13) \quad f_i = \text{firm effect}$$

$$(2.14) \quad e_t = \text{time effect}$$

$$(2.15) \quad v_{i,t}^{I/K} = \eta_{i,t}^{I/K}$$

$$(2.16) \quad v_{i,t}^{MPK} = \rho_1 \eta_{i,t}^{I/K} + \eta_{i,t}^{MPK}$$

$$(2.17) \quad v_{i,t}^{CF/K} = \rho_2 \eta_{i,t}^{I/K} + \rho_3 \eta_{i,t}^{MPK} + \eta_{i,t}^{CF/K}.$$

The $v_{i,t}$ terms are the reduced-form errors, which are combinations of the underlying structural errors $\eta_{i,t}$, as determined by the Cholesky ordering. The assumed ordering implies that investment shocks may affect MPK and cash flow contemporaneously, and that MPK shocks are allowed to affect cash flow in the same period. In contrast, there is no contemporaneous effect of MPK shocks on investment or of cash flow shocks on any of the other variables. Intuitively, given the time lags involved in investment decisions, it is reasonable to assume that other shocks do not have any contemporaneous

effect on investment. The reduced-form errors $v_{i,t}$ are assumed to be orthogonal to lags of $y_{i,t}$ (see equation (2.11)).

To control for aggregate shocks, time effects are removed by using deviations from year-specific means (an alternative method would be to use year dummies). Furthermore, firm effects are removed by using deviations from forward means (Helmert transformation or forward orthogonal deviations). Arellano and Bover (1995) developed this method to improve the efficiency of estimators for models with predetermined (but not strictly exogenous) variables, for example lagged dependent variables. The methodology is standard in the panel VAR literature, and it is described in more detail in Appendix 8.1 in GH (1999).

3. The data set

The firm-level data set used in this paper is the result of merging two separate data sets, which were provided by Sveriges Riksbank. The first data set is from Upplysningscentralen AB (UC), a major Swedish credit bureau, and contains balance-sheet and income statement data for the period 1989-2005. The second data set is from Statistics Sweden (SCB) and contains investment data for the period 1985-2005. From 1996, all Swedish firms are included, but many smaller firms were excluded during the earlier period, and for many observations the data are incomplete. Around 200,000 firms are observed each year from 1996, and the original sample consists of 2.4 million firm-year observations (before any data cleaning and sample restrictions).

SCB provided identification numbers to make it possible to identify the same firm in both data sets. However, the accounting years in the UC data did not always coincide with the calendar years in the SCB investment data, so the time periods were not the same for a given firm and “year”. This issue needed to be dealt with before merging the two data sets. The calendar year variable in the SCB data was constructed from the underlying accounting periods according to specific rules. Using the same rules, I created a calendar year variable in the UC data based on the available accounting periods. Finally, I could use the calendar year variables, along with the firm identification number, to merge the two data sets.⁸

My benchmark sample is an unbalanced panel of firms in the manufacturing sector with at least 20 employees for the period 1989-2005. I do not require firms to have existed during the entire sample period, which makes the panel unbalanced. This is in order to get a representative sample which includes small firms (which may have

⁸ Details on this procedure and other data issues are available in the appendix.

been started during the sample period) and firms in financial distress (which may have disappeared during the sample period).

During the pre-1996 period, data availability is severely limited for manufacturing firms with fewer than 20 employees (only a small random sample is observed each year). Therefore, I focus on manufacturing firms with at least 20 employees. There are three reasons for restricting the benchmark sample to the manufacturing sector. First, it facilitates the comparison of results with GH (1999) and most other papers in the literature, which only study manufacturing firms. Second, the calculation of the capital stock at replacement cost is more reliable.⁹ Finally, data availability is better for the manufacturing sector than for other industries. During the pre-1996 period, only a small random sample of non-manufacturing firms with fewer than 50 employees is observed each year.

Regarding the benchmark definition of capital and investment, both machines and buildings are included. There are two reasons for not only including machines but also buildings. First, it facilitates the comparison of results with GH (1999) and most other papers in the literature, which use the broader definition of capital and investment. Second, only information on total investment is available for the entire sample period.¹⁰

It is well known that the book value of capital is an imperfect measure of the replacement value of a firm's capital stock. To get a better measure, I estimate the capital stock using the perpetual inventory method:

$$(3.1) \quad K_{i,t} = (1 - \delta_{i,t}) K_{i,t-1} \frac{p_t^k}{p_{t-1}^k} + I_{i,t}$$

where $K_{i,t}$ is the capital stock of firm i at the end of period t , $\delta_{i,t}$ is the depreciation rate, p^k is the price of capital and $I_{i,t}$ is the investment during period t . The recursive formula requires an initial value for capital, and I use the initial book value of capital.

The variables which are needed for the empirical analysis are I/K , MPK and CF/K . I denotes investment, and the definition of K is clear from the perpetual inventory formula above (equation (3.1)). The estimated MPK has also been defined (see equation (2.9)). The definition of cash flow is similar to that used by GH (1999) who define cash flow as the sum of net income before extraordinary items and depreciation (Compustat data items 18 and 14, respectively). I define cash flow as profits after financial income and expense (a measure of net income from which taxes have not been deducted), minus taxes, plus depreciation.

⁹ See, for example, footnote 11 in Chatelain et al. (2003).

¹⁰ For 1996, Statistics Sweden had data-collection problems and the separate variables for investments in machines and buildings are missing.

Before proceeding to the empirical estimation, it is necessary to use observable firm characteristics to classify all firms as either “constrained” or “unconstrained”. In their original paper, Fazzari, Hubbard and Petersen (1988) used firms’ dividend policy to make this classification, and several other criteria have been used in the subsequent literature. The GH papers use dividend payout, firm size and the presence (or not) of a bond rating. My data set includes information on dividend payout, firm size and membership in a company group. As a robustness check, I use all three indicators separately to produce three alternative sample splits between constrained and unconstrained firms.

For the first indicator, dividend payout, I calculate the fraction of time during a firm’s existence when the firm pays out a positive dividend. I classify firms with a dividend-payment fraction value below the 90th percentile as constrained (DIV=0), and firms with a dividend-payment fraction value above the 90th percentile as unconstrained (DIV=1).¹¹ The second sample-split indicator is firm size, as measured by the number of employees. Small firms are classified as constrained (SIZE=0) and large firms as unconstrained (SIZE=1), using the 90th percentile for firms’ average number of employees as the cut-off value. The third indicator is membership in a company group. For each firm, I calculate the fraction of time that the firm belongs to a company group. Then, I use the 90th percentile of the group-membership fraction value as the cut-off point: firms with a lower value are classified as constrained (GROUP=0) and firms with a higher value are classified as unconstrained (GROUP=1). For the benchmark sample, this procedure results in the following three alternative sample splits. First, firms who pay dividends at least (less than) 75% of the time are unconstrained (constrained). Second, firms with at least (less than) 277 employees on average are unconstrained (constrained). Finally, firms which always belong to a company group are unconstrained, and firms which are independent of company groups at least some of the time are constrained.

Table 1 presents summary statistics for the variables used in the empirical analysis. After the data cleaning procedures described in the appendix, a total of 35,396 firm-year observations remains in the benchmark sample (denoted “all firms” in Table 1). The main reasons for the large decrease in the number of observations are missing data and the fact that most firms have fewer than 20 employees.

¹¹ I use the 90th percentile as the cut-off point in all three sample splits. In contrast, GH use the 66th percentile as the cut-off for dividend payout and size. The reason is that the firms in my sample are, on average, much smaller than those in the GH sample. If I used the 66th percentile, many small and constrained firms would be misclassified as unconstrained, thus leading to incorrect inference regarding any differences between sub-samples.

Table 1
Summary statistics for different samples

		Mean	Std dev	Percentile		
				25%	Median	75%
CF/K	(all firms)	0.40	0.94	0.10	0.25	0.49
I/K	(all firms)	0.21	0.43	0.04	0.11	0.23
MPK	(all firms)	0.05	0.08	0.02	0.03	0.06
CF/K	(DIV=1)	0.54	0.95	0.20	0.33	0.57
	(DIV=0)	0.38	0.93	0.09	0.24	0.48
I/K	(DIV=1)	0.22	0.42	0.05	0.12	0.24
	(DIV=0)	0.20	0.43	0.04	0.10	0.22
MPK	(DIV=1)	0.04	0.07	0.02	0.03	0.04
	(DIV=0)	0.05	0.08	0.02	0.03	0.06
CF/K	(SIZE=1)	0.37	0.72	0.12	0.28	0.52
	(SIZE=0)	0.40	0.96	0.10	0.25	0.49
I/K	(SIZE=1)	0.17	0.27	0.06	0.12	0.20
	(SIZE=0)	0.21	0.44	0.04	0.10	0.23
MPK	(SIZE=1)	0.04	0.06	0.02	0.03	0.05
	(SIZE=0)	0.05	0.08	0.02	0.03	0.06
CF/K	(GROUP=1)	0.47	1.08	0.13	0.30	0.59
	(GROUP=0)	0.39	0.92	0.10	0.25	0.48
I/K	(GROUP=1)	0.20	0.42	0.05	0.11	0.21
	(GROUP=0)	0.21	0.43	0.04	0.11	0.23
MPK	(GROUP=1)	0.05	0.09	0.02	0.03	0.05
	(GROUP=0)	0.05	0.08	0.02	0.03	0.06
		Firm-year observations	Median # of employees/firm			
All firms		35,396	53			
DIV=1		4,077	40			
DIV=0		31,319	55			
SIZE=1		3,555	560			
SIZE=0		31,841	46			
GROUP=1		3,765	107			
GROUP=0		31,631	49			

Note: the table presents summary statistics for the ratio of cash flow to capital (CF/K), the ratio of investment to capital (I/K) and a sales-based measure of the marginal product of capital (MPK). More details on variable definitions are given in Section 3. The variables DIV, SIZE and GROUP take the value 1 for unconstrained firms and the value 0 for constrained firms.

4. Empirical analysis

To identify shocks to current cash flow which are orthogonal to current MPK , a recursive ordering of contemporaneous shocks must be assumed. Following GH, I use the Cholesky ordering I/K , MPK and CF/K in the main specifications, but also check for robustness by using alternative orderings. In the empirical analysis, I first estimate the VAR model, and then I investigate the impulse responses of investment and MPK to cash flow shocks.

4.1. Impulse responses for the benchmark sample. The benchmark sample consists of manufacturing firms with at least 20 employees during the period 1989-2005. The impulse responses for the benchmark sample are presented in Figure 1.

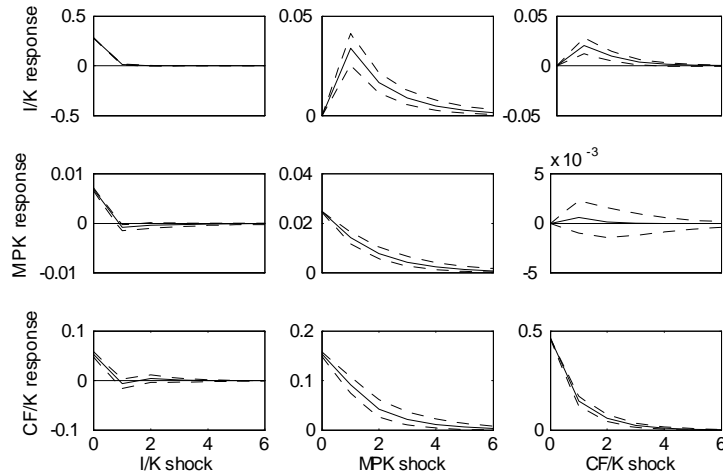


FIGURE 1. Impulse responses for the benchmark sample. Horizontal axis shows response horizon (years). Dashed lines denote 90-percent confidence intervals generated by Monte Carlo with 1000 draws.

The top right-hand graph in Figure 1 shows how investment responds to a one-standard-deviation cash flow shock. The effect is positive, statistically significant and substantial in economic terms. The peak effect on I/K is 0.02, which can be compared to an average I/K ratio of 0.21 for all firms in Table 1. Thus, the impact corresponds to around 10% of the average investment-capital ratio.

In contrast, the response of MPK to a cash flow shock is weak and insignificant. If the positive response of investment to cash flow had been due to a positive effect of cash flow on future fundamentals (i.e. future MPK), we would have found a positive response of MPK . Hence, there is no evidence that the positive effect of cash flow on investment is a spurious result of any predictive value of cash flow for future fundamentals.

Most of the remaining impulse responses in Figure 1 are less central for the purposes of this paper, but there are some interesting exceptions. For example, the top graph in the middle column shows that investment increases following a positive MPK shock, as would be expected. It is also interesting to note that a positive MPK shock causes an increase in cash flow. Hence, it is important to control for MPK when studying the effect of cash flow on investment. To sum up, the key result for the benchmark sample is that cash flow affects investment, which constitutes preliminary evidence in favor of the balance sheet channel. The next subsection studies different categories of firms and different time periods separately.

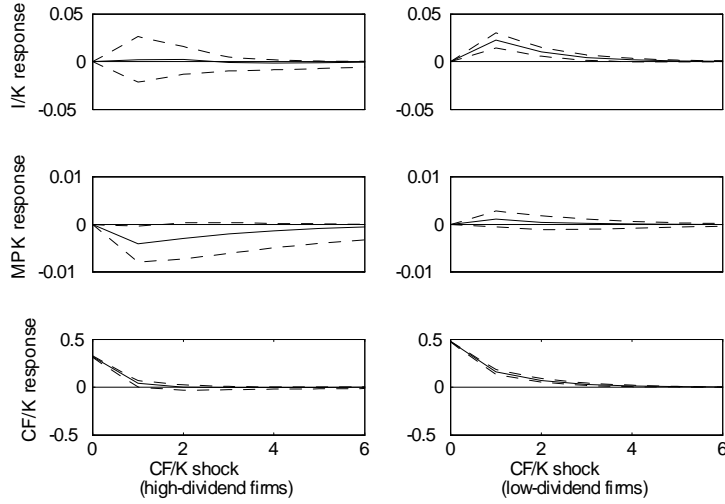


FIGURE 2. Impulse responses for high-dividend firms (left column) and low-dividend firms (right column). Horizontal axis shows response horizon (years). Dashed lines denote 90-percent confidence intervals generated by Monte Carlo with 1000 draws.

4.2. Impulse responses for sub-samples of constrained vs. unconstrained firms, and recession vs. non-recession periods. As discussed in Section 3, I classify firms as financially unconstrained or financially constrained in three different ways. For each classification, I estimate separate panel VAR models for the unconstrained and constrained sub-samples. This is followed by separate estimation for the early, recession part of the sample period, and for the late, non-recession part.

Figure 2 presents impulse responses to cash flow shocks for the sub-samples of high-dividend, unconstrained firms and low-dividend, constrained firms. For the high-dividend, unconstrained sample of firms, there is hardly any investment response following a cash flow shock. MPK actually falls, but the effect is barely significant. In contrast, for the low-dividend, constrained firms, there is a significant and long-lasting effect of cash flow on investment. The impact of cash flow on MPK is positive, but not significant. Thus, as predicted by the balance sheet theory, investment by constrained firms is more sensitive to changes in cash flow than investment by unconstrained firms.

The corresponding impulse response functions for large, unconstrained and small, constrained firms are presented in Figure 3. MPK increases in response to a positive cash flow shock, but not significantly. For both categories of firms, investment responds positively to a cash flow shock, but the effect is larger and more persistent for small,

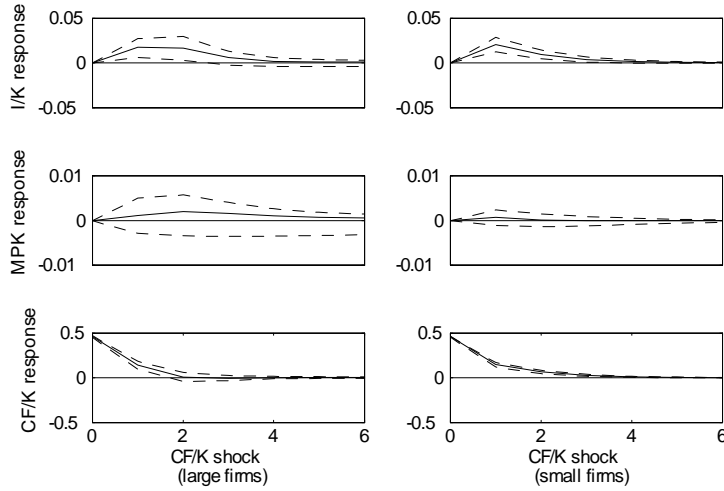


FIGURE 3. Impulse responses for large firms (left column) and small firms (right column). Horizontal axis shows response horizon (years). Dashed lines denote 90-percent confidence intervals generated by Monte Carlo with 1000 draws.

constrained firms. However, the difference between constrained and unconstrained firms is not as clear as for the dividend policy classification.

The third division between unconstrained and constrained firms is based on group membership, and the results are similar to the large-small firm division discussed above. Figure 4 shows the impulse responses for group, unconstrained firms, and for non-group, constrained firms. Once more, the impact of cash flow on investment is somewhat larger and more longer-lasting for constrained firms, and there are no significant increases in *MPK*.

The final division is based on time rather than firm characteristics. I estimate separate panel VARs for the early, recession period, during which a larger fraction of firms is likely to be constrained, and for the late, non-recession period. The impulse responses are shown in Figure 5. The effect of cash flow on investment is much larger and much more persistent during the recession. Moreover, there is hardly any response of *MPK* to cash flow shocks during either of the two sub-periods.

To summarize, using several different sample splits, the investment of constrained firms is consistently more sensitive to cash flow than the investment of unconstrained firms. In particular, the investment-cash flow sensitivity is larger during the 1989-1996 period, which includes a severe recession.

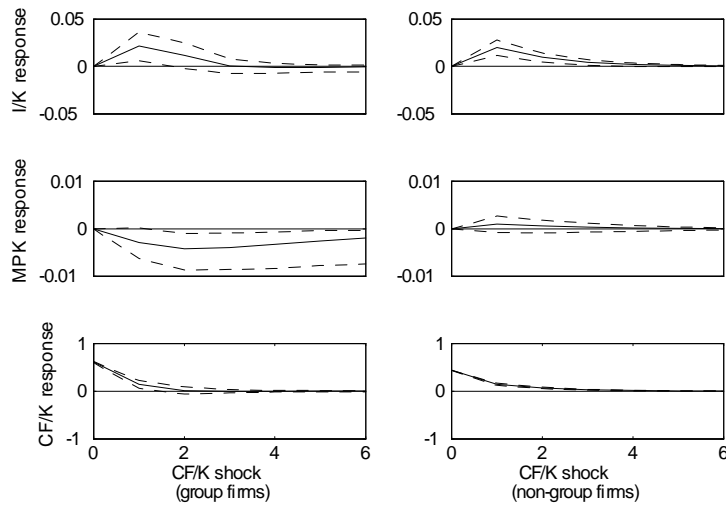


FIGURE 4. Impulse responses for group firms (left column) and non-group firms (right column). Horizontal axis shows response horizon (years). Dashed lines denote 90-percent confidence intervals generated by Monte Carlo with 1000 draws.

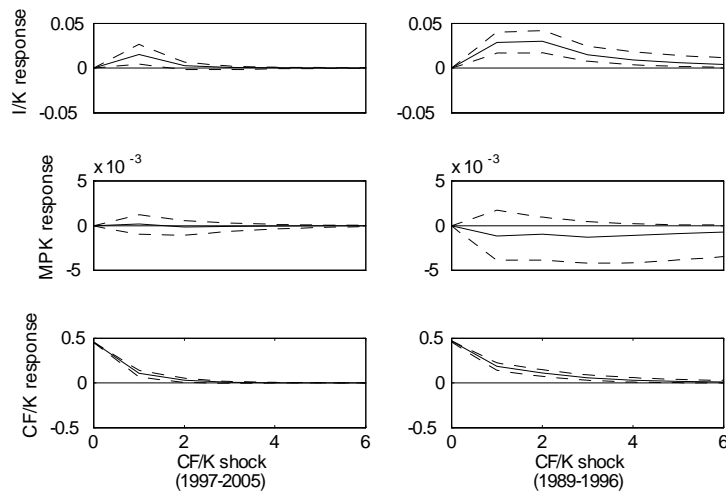


FIGURE 5. Impulse responses for the late, non-recession period (left column) and the early, recession period (right column). Horizontal axis shows response horizon (years). Dashed lines denote 90-percent confidence intervals generated by Monte Carlo with 1000 draws.

4.3. Robustness tests. As seen above, the main empirical results are at least qualitatively similar for the different sample splits, which is reassuring from a robustness perspective. In this section, I discuss some additional robustness tests. The main results are qualitatively robust to the choice of lag length, Cholesky ordering, definition of capital/investment and the inclusion of smaller and/or non-manufacturing firms. However, when using a balanced panel of firms, the estimated response of investment to cash flow is weak. The key impulse response functions, showing the response of investment to cash-flow shocks, are presented in the appendix (Figures 6-11).

The choice of lag length in the panel VAR does not matter for the results. Estimation with 1 lag (rather than 2 lags) produces very similar results, both qualitatively and quantitatively, as shown in Figure 6.

It is well known that different Cholesky orderings can give different results. The results reported above are based on the identification assumptions of Gilchrist and Himmelberg (1999), but the alternative ordering used by Love and Zicchino (2006)—*MPK*, cash flow and investment—gives qualitatively similar results in most cases (see Figure 7). However, for several sample splits, there is a large and immediate response of investment for the constrained firms.

Another choice which may affect the results is the definition of capital and investment, where both machines and buildings are included. As can be seen in Figure 8, very similar results are obtained by only including machines (which is only possible for the period 1996-2005 because of data availability constraints).

The benchmark sample only includes manufacturing firms with at least 20 employees. When I include even smaller manufacturing firms (which is only possible for the period 1996-2005 because of data availability constraints), the differences between constrained and unconstrained firms are somewhat less clear (see Figure 9). However, cash flow shocks have positive effects on investment in all cases. When also including all non-manufacturing firms, there are substantial investment responses for constrained firms as well (see Figure 10).¹²

Finally, an exception to the general robustness of the results occurs for a balanced panel of firms. The response of investment to cash flow is weak and insignificant (see Figure 11). One possible explanation is that data availability constraints necessarily limits the sample period to 1997-2005, when the investment-cash flow sensitivity is weaker than in the earlier period, which includes a severe recession. Another explanation could be that the small sample of firms for which all necessary data are available in each year consists of established firms, which are less affected by financial constraints.

¹² There are many firms with fewer than 20 employees, all of which are likely to be more financially constrained than larger firms. In order to avoid misclassifying small, constrained firms as unconstrained, I use the 97th percentile as the cutoff between constrained and unconstrained.

5. Conclusions

This paper uses reduced-form VAR methods on firm-level panel data from the period 1989-2005 to investigate whether there exists empirical evidence of a balance sheet channel in Sweden. The main empirical results are that: i) cash flow has a significant effect on investment and ii) the effect is especially strong for constrained firms and, in particular, during recessions. Cash flow shocks do not have any predictive value for future MPK , neither for constrained nor for unconstrained firms. Hence, the difference in investment-cash flow sensitivity across firms is not due to any difference in the information content of cash flow for investment opportunities. Moreover, a positive MPK shock causes both investment and cash flow to increase, which shows the importance of controlling for MPK when investigating investment-cash flow sensitivities. The results are generally robust to different procedures for the classification of firms as constrained or unconstrained, as well as different specification choices, variable definitions and samples. Thus, the empirical results provide clear evidence in favor of a balance sheet channel in the monetary transmission mechanism in Sweden.

The results in this paper provide micro-level support for the introduction of financial frictions in macro-level empirical models, which are needed to study the quantitative importance of financial frictions for monetary transmission. In a recent paper, Christiano, Trabandt and Walentin (2007) add financial frictions to a general-equilibrium macro model of the Swedish economy. They find that the presence of financial frictions causes monetary policy to have an increased effect on investment.

A possible extension of the analysis in this paper would be to study differences across firms in the dynamics of employment and inventories in response to cash flow shocks. As discussed by, for example, Gilchrist and Himmelberg (1999), firms do not only use external financing for investment, but also to finance labor inputs and inventories, which should cause cash flow to matter for the cyclical dynamics of these other variables as well.

Appendix

The calendar year variable in the data from Statistics Sweden (SCB) was constructed from the underlying accounting periods according to the following specific rules (which I also use to create a corresponding calendar year variable in the UC data):

For the period 1985-1995, if the accounting-period end date is May 1 or later during year x , the observation is assigned to year x . If the accounting-period end date is April 30 or earlier during year x , the observation is assigned to year $x-1$.

For the period 1996-2002, firms with more than 50 employees were treated according to the above rule. For firms with 50 or fewer employees, if the accounting-period end date occurs during year x (regardless of month), the observation is assigned to year x .

For the period 2003-2005, firms with more than 500 employees were treated according to the rule for 1985-1995. For firms with 500 or fewer employees, if the accounting-period end date occurs during year x (regardless of month), the observation is assigned to year x .¹³

This procedure for creating a calendar year variable in the UC data may cause duplicates when a company has two reports during the same year, for example due to a change of reporting period. To deal with duplicate observations, I follow the rule used by SCB, which is to keep the one observation per firm and year with the latest reporting period end date. Very few observations are lost in this procedure.

Following Gilchrist and Himmelberg, I remove the time effects by using deviations from year-specific means and the firm effects by using deviations from forward means. It should be noted that there is a minor problem with the use of deviations from year-specific means because of differences between calendar and accounting years. For example, the calendar year 1997 does not correspond to the same accounting year for all firms, but I use deviation from calendar-year means.

In the SCB data, all variables are scaled in order to correspond to 12-month values even for firms with an accounting period of more or less than 12 months. I scale all variables in the UC data in the same way.

Another scaling issue is that the SCB variables are defined in thousands of Swedish kronor and the UC variables in Swedish kronor. To have all variables defined in the same units, I divide the UC variables by 1000.

¹³ To be precise, SCB only uses this rule for the manufacturing sector. The definition of a “large” company is somewhat different for the non-manufacturing sector. Since the benchmark sample only includes firms from the manufacturing sector, this is not a major problem.

From the initial sample, I remove all observations for which there is not sufficient data to calculate the variables needed or which have unreasonable values for some variables, for example a negative capital stock.

In my benchmark sample, I only include manufacturing firms with at least 20 employees. One reason is data availability. During the period 1985-1995 the SCB data does not cover all smaller firms, and I want my sample to be comparable over time. For the non-manufacturing sector, data availability is even more limited. During the period 1985-1995 the SCB data includes all non-manufacturing firms with at least 50 employees, but not all firms with 20-49 employees.

Equation (3.1) in the text describes the perpetual inventory method used to calculate the capital stock. I calculate industry-specific depreciation rates for total capital (machines and buildings) by taking an average of industry-specific depreciation rates for machines and buildings, respectively, weighted by the relative shares of machines and buildings in the industry's capital. To define an industry, I use two-digit SNI codes (SNI69 for the period 1985-1989 and SNI92 for the period 1990-2005).

The industry-specific depreciation rates for machines and buildings are taken from a publication by the U.S. Bureau of Economic Analysis (2003). For buildings, I use the depreciation rate 0.0314 for all sectors. This number is taken from "Private nonresidential structures, industrial buildings" on page 31, but there are only minor differences compared to other sectors. The depreciation rates for machines are taken from the same source, and are presented in Table 2 below.

The price of capital in the perpetual inventory formula is calculated from gross fixed capital formation in current and fixed prices, respectively (from national accounts data available on the web page of Statistics Sweden).

Following GH (1999), I first calculate the ratios needed for the analysis (see Table 2 in their paper), and then I remove outliers (observations with ratios below the 1st or above the 99th percentile). I also remove firms with fewer than four observations, and I require that all observations for a firm are consecutive.

Table 2

Industry-specific depreciation rates for machines					
Depreciation rates for two-digit SNI69 code industries for 1985-1989		Depreciation rates for two-digit SNI92 code industries for 1990-2005			
Industry	Depr. Rate	Industry	Depr. Rate	Industry	Depr. Rate
11	0.1179	1	0.1179	32	0.1225
12	0.1179	2	0.1179	33	0.1225
13	0.1179	5	0.1179	34	0.1225
21	0.15	10	0.15	35	0.1225
22	0.15	11	0.15	36	0.1072
23	0.15	12	0.15	37	0.1072
29	0.15	13	0.15	40	0.05
31	0.1072	14	0.15	41	0.05
32	0.1072	15	0.1072	45	0.155
33	0.1072	16	0.1072	50	0.165
34	0.1072	17	0.1072	51	0.165
35	0.1072	18	0.1072	52	0.165
36	0.1225	19	0.1072	55	0.15
37	0.1225	20	0.1072	60	0.1232
38	0.1225	21	0.1072	61	0.1232
39	0.1072	22	0.1072	62	0.1232
41	0.05	23	0.1072	63	0.1232
42	0.05	24	0.1072	64	0.11
50	0.155	25	0.1072	70	0.1072
61	0.165	26	0.1225	71	0.1232
62	0.165	27	0.1225	72	0.3119
63	0.15	28	0.1225	73	0.1350
71	0.1232	29	0.1225	74	0.15
72	0.11	30	0.1225		
83	0.1072	31	0.1225		

Note: the table presents the assumed industry-specific depreciation rates for machines in Sweden at the two-digit SNI code level. For each Swedish industry, I use the closest possible U.S. industry-specific depreciation rate from the U.S. Bureau of Economic Analysis (2003).

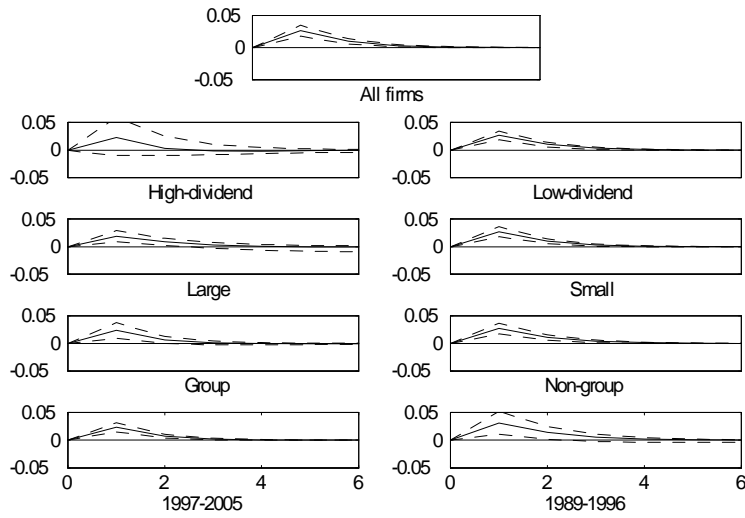


FIGURE 6. Impulse responses of investment (I/K) to cash flow shock with 1-lag VAR for the period 1989-2005.

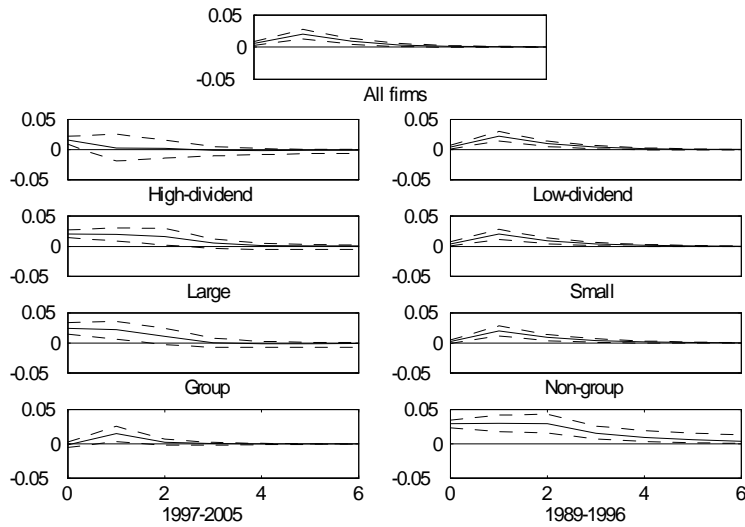


FIGURE 7. Impulse responses of investment (I/K) to cash flow shock with Love-Zicchino Cholesky ordering assumption for the period 1989-2005.

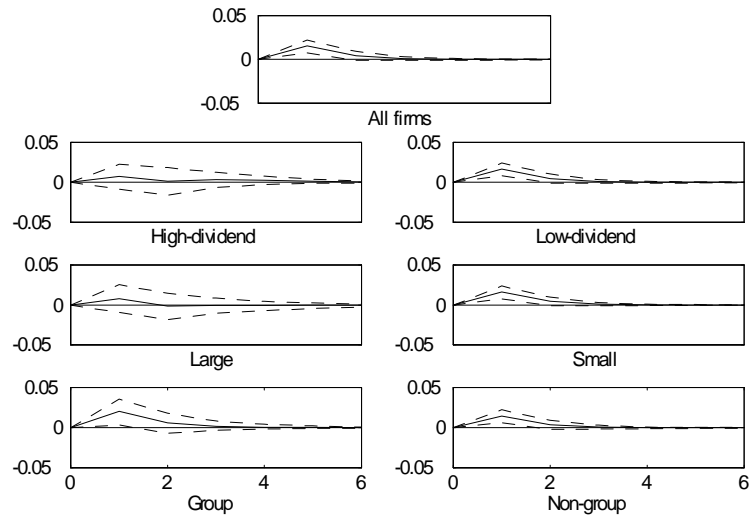


FIGURE 8. Impulse responses of investment (I/K) to cash flow shock with only machines (not buildings) included in definition of capital for the period 1996-2005.

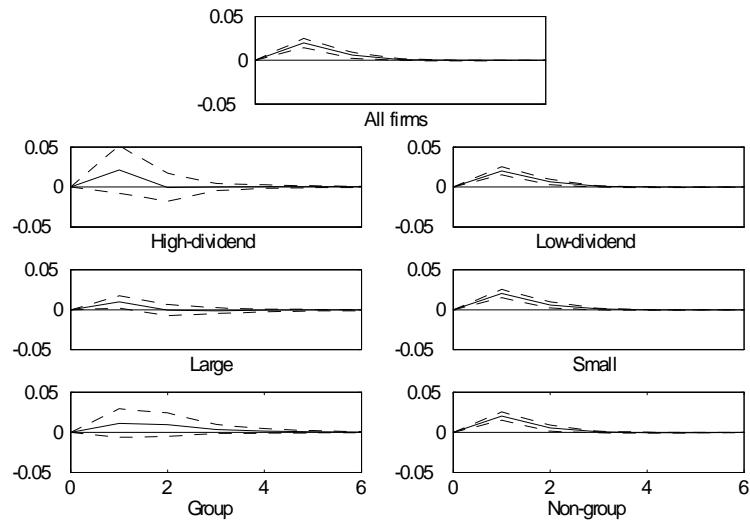


FIGURE 9. Impulse responses of investment (I/K) to cash flow shock for sample of all manufacturing firms for the period 1996-2005.

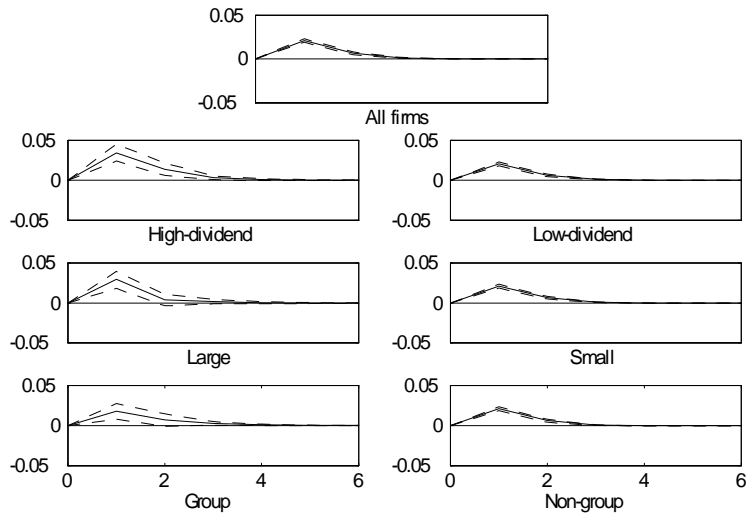


FIGURE 10. Impulse responses of investment (I/K) to cash flow shock for sample of all manufacturing and non-manufacturing firms for the period 1996-2005.

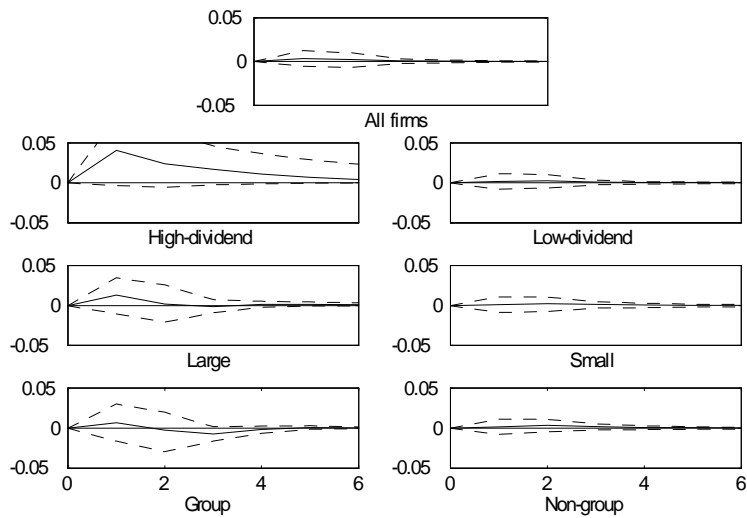


FIGURE 11. Impulse responses of investment (I/K) to cash flow shock with a balanced panel of firms for the period 1997-2005.

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Credit Matters: Empirical Evidence on U.S. Macro-Financial Linkages

Tamim Bayoumi and Ola Melander

ABSTRACT. This paper develops a framework for analyzing macro-financial linkages in the United States. We estimate the effects of a negative shock to banks' capital/asset ratio on lending standards, which in turn affect consumer credit, mortgages, and corporate loans, and the corresponding components of private spending (consumption, residential investment and business investment). In addition, our empirical model allows for feedback from spending and income to bank capital adequacy and credit. Hence, we trace the full credit cycle. An exogenous fall in the bank capital/asset ratio by one percentage point reduces real GDP by some $1\frac{1}{2}$ percent through its effects on credit availability, while an exogenous fall in demand of 1 percent of GDP is gradually magnified to around 2 percent through financial feedback effects.

1. Introduction

For any analyst of the global economy, the million-dollar question is: how deep and protracted will the current U.S. economic downturn be? One of the main determinants will be how balance sheet deterioration for banks and other leveraged lenders affects credit and spending. A particular concern is the possibility of an adverse feedback loop from economic activity to the financial system, with second-round effects on the macroeconomy through reduced credit availability. U.S. policy-makers are aware of this potential feedback, as indicated for example by the minutes of the March 18, 2008, meeting of the Federal Open Market Committee of the Federal Reserve:

“Evidence that an adverse feedback loop was under way, in which a restriction in credit availability prompts a deterioration in the economic outlook that, in turn, spurs

⁰ The authors gratefully acknowledge helpful comments and suggestions by Ravi Balakrishnan, Rupa Duttagupta, our discussant Kristoffer Nimark and—in particular—Andrew Swiston, and seminar participants at the 2008 annual meeting of the Canadian Economics Association, the June 2008 Quarterly Outlook meeting of Macroeconomic Advisers, the EABCN/CEPR conference Business Cycle Developments, Financial Fragility, Housing and Commodity Prices, and the IMF. Andrew Swiston also provided outstanding research assistance. All remaining errors are ours. Melander is grateful to Jan Wallander's and Tom Hedelius' Research Foundation for financial support.

additional tightening in credit conditions, was discussed. Several participants noted that the problems of declining asset values, credit losses, and strained financial market conditions could be quite persistent, restraining credit availability and thus economic activity for a time and having the potential subsequently to delay and damp economic recovery.”

This paper develops a practical framework for policy analysis of macro-financial linkages. The purpose is to complement the IMF staff’s Financial Conditions Index, which uses vector autoregressions to examine the interaction of financial and macro-economic conditions (Swiston, 2008). In contrast to the reduced-form approach used in that work, this paper examines the individual linkages using more structural methods. To our knowledge, this is the first paper to fully trace out these linkages for different components of private spending, although earlier papers have studied parts of the chain.

More specifically, we estimate the effects of a negative financial shock on consumption and investment through credit availability in the United States. We start the process by assuming an exogenous negative shock to the bank capital/asset ratio (CAR), for example from a rise in bank loan losses. In response, banks tighten their lending standards, which reduces credit availability. A credit tightening causes spending to fall, both directly through credit constraints and indirectly through the effects of an economic slowdown on balance sheets of banks, households, and firms. The linkages are described in more detail in Section 2.

All else equal, we find that a one-percentage-point reduction in the CAR causes a fall in overall credit of some $2\frac{1}{2}$ percent of GDP, and a reduction in the level of GDP by around $1\frac{1}{2}$ percent. We can also use the model to see how demand shocks are amplified through macro-financial linkages. An exogenous one-percent decline in demand is gradually magnified and reduces GDP by around 2 percent.

It is interesting to compare our estimate of the effect of a financial shock with the findings of other recent studies. In general, while different assumptions regarding the initial shock makes direct comparisons difficult, our quantitative results are similar to other estimates. Lown and Morgan (2006) find that a 16 percentage-point increase in the net fraction tightening loan standards in the Fed’s senior loan officer survey (similar to the impact from our CAR shock) causes GDP to decline by 1 percent, while Swiston (2008) finds that a tightening in standards of 20 percentage points lowers GDP by around $1\frac{1}{4}$ percent in a paper that includes a wide range of other financial variables. The Spring 2008 IMF Global Financial Stability Report uses a vector autoregression (VAR) model to investigate the effect of aggregate credit on growth. The variables included are real GDP growth, inflation, private borrowing and the prime loan rate.

There is a significant effect of lower credit growth on GDP. Quantitatively, a reduction of credit growth from the U.S. post-war average of 9 percent to 4 percent (“credit squeeze”) or 1 percent (“credit crunch”) reduces year-on-year GDP growth by 0.8–1.4 percentage points. Finally, Greenlaw and others (2008) estimate (i) the losses by U.S. financial institutions due to their exposure to mortgage securities, (ii) the credit contraction from the resulting deleveraging and (iii) the effects on GDP growth. The authors find that a 3 percentage-point decrease in credit growth causes a decline in GDP growth of 1.3 percentage points over the coming year.

Thus there exists recent empirical evidence that financial shocks have real effects. But from a theoretical point of view, why should credit matter in the first place? After all, in a Modigliani-Miller world with perfect information and no credit constraints, real decisions are made independently of financial factors. Spending is determined by intertemporal optimization given preferences and technology. However, in the presence of financial frictions and information imperfections, the availability of financing is also an important consideration for consumption and investment decisions. There is an extensive theoretical literature which has shown that alternative ways to model imperfect information between borrowers and lenders (moral hazard, adverse selection or costly state verification) have similar implications for the importance of credit.¹ External financing is more expensive than internal financing, and credit rationing can occur. The effects are especially large when balance sheet positions are weak. In sum, according to theories of imperfect information, financial factors such as credit availability have real economic effects.

Much of the previous empirical literature on the effects of credit aims to distinguish between different transmission mechanisms, such as the balance sheet channel, the bank lending channel and the bank capital channel. Since these different channels have similar predictions for aggregate quantities, many empirical studies use micro-level data from banks and/or firms rather than aggregate data.²

In contrast, the focus of this paper is on calculating the quantitative importance of credit at a macroeconomic level. Therefore, we use aggregate data across different types of lending—consumer, mortgage, and corporate credit—to study the determinants and aggregate importance of credit availability, without necessarily distinguishing between specific transmission channels. Our approach to estimating the effect of credit on spending is similar in spirit to, but more comprehensive than, that of Bacchetta and Gerlach (1997) and Ludvigson (1998), who study the effects of predictable changes in

¹ For a survey, see Gertler (1988), and for a financial-accelerator model, see Bernanke, Gertler and Gilchrist (1999).

² See e.g. Oliner and Rudebusch (1996) (balance sheet channel vs. bank lending channel) and Van den Heuvel (2007b) (bank capital channel vs. bank lending channel).

credit on consumption. In turn, these papers are inspired by a Campbell and Mankiw (1990) paper on consumer liquidity constraints, which tests for effects of predictable changes in income on consumption.

While the previous papers only study the effect of credit on consumption, we widen the types of spending analyzed to include consumption, residential investment, and business investment. In addition, our approach is more holistic, since we also study the links from bank capital adequacy through lending standards (survey measures of credit availability) to credit and spending, and allow for feedback effects from spending and income back to the credit market. Another related paper by Lown and Morgan (2006) uses vector autoregression (VAR) methodology to study the effect of lending standards on bank loans and output. Our paper uses less reduced-form methods, includes also non-bank loans, and studies separately all the main components of private spending, while Lown and Morgan only examine inventories and aggregate GDP.

The rest of this paper is organized as follows. Section 2 outlines our analytical framework for macro-financial linkages. The subsequent three sections present empirical estimates of the links in the chain from the bank capital-asset ratio to spending. We estimate the effects of capital adequacy on lending standards (Section 3), lending standards on credit (Section 4) and credit on spending (Section 5). Then we estimate how spending affects income (Section 6) and finally the feedback from income to banks' capital position (Section 7). In Section 8 we present the "bottom line", i.e. our quantitative estimates of how macro-financial linkages affect the propagation of financial and macroeconomic shocks. Section 9 concludes.

2. A framework for analyzing macro-financial linkages

Figure 1 presents a simple graphical framework for thinking about macro-financial linkages. Each link in the chain is described in more detail in the corresponding section of the paper, so what follows is just a brief outline of the structure of the paper and a summary of the links. We start with a negative exogenous shock to bank capital, which causes the Capital/Asset Ratio (CAR) to decline. Of course, the underlying motivation is subprime-related losses. Then we use our framework to trace out the macroeconomic effects, taking macro-financial feedback channels into account.

The first link is from the CAR to lending standards. Capital requirements on banks are imposed by regulators and/or market discipline, so a negative shock constrains the capacity for lending. Thus banks are induced to tighten their lending standards in order to reduce the quantity of credit and restore the CAR. Lending standards are non-price loan terms, which reflect credit availability. We use the standard measure of

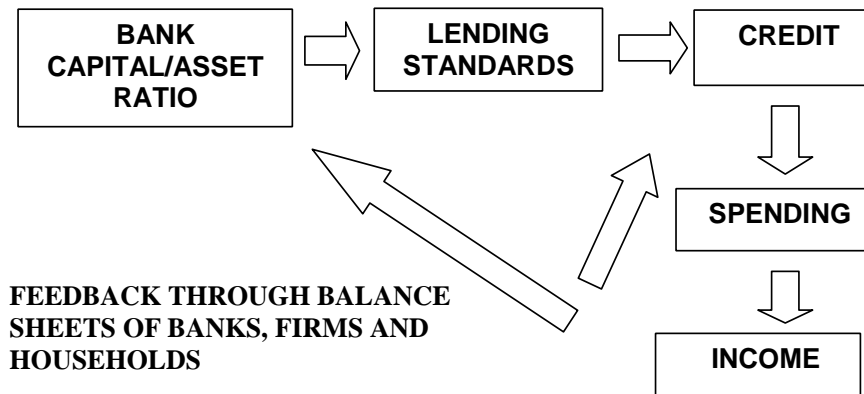


FIGURE 1. A framework for macro-financial linkages.

lending standards used in the literature, which is based on answers from the quarterly Federal Reserve survey of bank loan officers.

A tightening of loan standards causes a decrease in the quantity of credit, as shown in the second link. We investigate separately the impact on consumer credit, mortgage credit and business credit. In the estimation, we also include other variables which affect credit, such as income and interest rates.

A key link in the chain goes from credit to spending. The credit data is from the Flow of Funds and the income data from the National Income and Product Accounts, so we integrate information from several different sources. When credit availability falls, there is a direct effect on spending due to credit constraints. For each of the credit categories, we estimate the effect of credit on the corresponding measure of spending (consumption, residential investment, and business fixed and inventory investment, respectively). A positive correlation between credit and spending does not necessarily reflect causality from credit to spending. Instead, it could be due to reverse causality from spending to credit. If households and firms choose to borrow in order to finance their spending, then the variables will move together even in the absence of credit constraints. To avoid an upward bias in the estimated effect of credit on spending due to reverse causality, we use instrumental variables with lagged variables as instruments.

Changes in spending cause changes in income through standard multiplier effects. For each of our different measures of income (personal disposable income, GDP and business profits), we estimate how income is affected when spending changes. We also allow for an impact of spending on home equity.

The final link is the feedback loop from income through balance sheets of banks, firms and households. The feedback takes place through two different channels. The first channel works through the effect of an economic slowdown on bank balance sheets.

As spending and income fall, loan losses gradually increase and the CAR deteriorates further. In Figure 1, this channel is represented by the arrow from INCOME to BANK CAPITAL/ASSET RATIO. The second feedback channel is due to deterioration of incomes and balance sheets for households and firms, which has a further adverse financial-accelerator effect on credit availability. In Figure 1, this channel is represented by the arrow from INCOME to CREDIT. Taking these feedback mechanisms into account, the final effect of a CAR shock on aggregate economic activity is larger than the direct effect. Eventually, as bank credit declines the capital/asset ratio starts to improve. Bank deleveraging causes a decrease in the denominator of the capital/asset ratio, which increases the ratio.

3. The effect of the bank capital/asset ratio on lending standards

The first link is from the bank Capital/Asset Ratio (CAR) to lending standards. While the interest rate is the price of a loan, standards reflect non-price terms associated with a loan, such as collateral requirements and loan limits.³ It is useful to think of standards as measuring credit supply given borrower characteristics. As a proxy for bank lending standards, we use answers from the Federal Reserve's quarterly Senior Loan Officer Opinion Survey on Bank Lending Practices. Following the previous literature we define the tightness of lending standards as the percent of respondents reporting a tightening of standards minus the percent of respondents reporting an easing of standards.

Capital adequacy is the main determinant of banks' lending capacity. In Figure 2 we can see a clear negative relationship between changes in the CAR and standards. During periods when the capital/asset ratio is increasing, standards are typically negative, which means that there is a net easing of standards. It seems that the effect occurs with a lag. Conversely, when the CAR has been falling, as in the second half of the 1990's and in the recent past, there has been a subsequent net tightening of standards.

A negative shock to the capital/asset ratio constrains the capacity for lending and forces banks to tighten lending standards in order to restore their capital adequacy. By reducing both assets (loans) and liabilities (short-term debt), banks can increase their capital/asset ratio. Another way for banks to increase the (risk-based) CAR is to substitute safe securities for riskier loans. Van den Heuvel (2007a) develops a model where a combination of (i) risk-based capital adequacy requirements and (ii) an imperfect market for bank equity causes a bank capital channel of monetary policy.

³ See Lown and Morgan (2006) for a discussion of the meaning and measurement of standards.

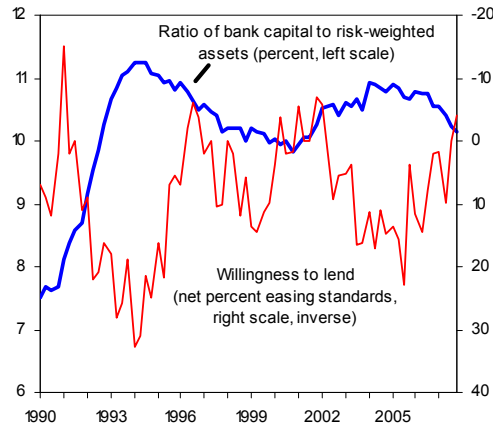


FIGURE 2. Bank capital adequacy and lending standards.

More generally, the model implies that capital adequacy affects banks' willingness to lend.⁴

There is also empirical evidence that loan standards depend on bank balance sheets. In a recent paper using VAR methods and aggregate data, Lown and Morgan (2006) find that a negative shock to the CAR causes a tightening of standards. The peak effect occurs 1–2 years after the initial shock. Other empirical studies use micro-level data and find evidence that differences in capital positions across banks causes differences in the response to shocks. Kishan and Opiela (2006) and Van den Heuvel (2007b) use U.S. bank-level panel data and find that the lending of less-capitalized banks reacts more strongly to monetary policy shocks. Peek and Rosengren (1995) study a cross-section of banks in New England during the 1990–1991 recession. They find evidence in favor of a “capital crunch”; in response to a negative capital shock, less-capitalized banks shrink their balance sheets to a larger extent than more-capitalized banks.

Other factors beyond capital adequacy may have an effect on loan standards. Gorton and He (2008) develop a theoretical model where asymmetric information between competing banks cause lending standards to change over time. In empirical tests they find that relative bank performance affects subsequent credit card and C&I (Commercial and Industrial) lending. Another recent paper by Dell’Ariccia, Igan and Laeven (2008) finds empirical evidence that increased competition and securitization caused an easing of lending standards in the U.S. subprime mortgage market. Nevertheless,

⁴ The bank capital channel differs from the bank lending channel studied in earlier literature (see for example Bernanke and Blinder (1988)). Bernanke (2007) argues that the traditional bank lending channel is currently unlikely to be quantitatively important in the United States (because of financial deregulation), but that financial intermediaries are still important for the transmission of shocks, in particular through the bank capital channel.

banks' balance sheet position is a key factor driving loan standards, which has also been shown in the literature. For example, Lown and Morgan (2006) find that loan standards are affected by the CAR, but not by GDP. Therefore we use a simple specification with the lagged change in the CAR as the only explanatory variable. However, in Section 7 of the paper we introduce feedback from the aggregate economy to capital adequacy, which allows for an *indirect* effect of GDP on standards through capital adequacy.

We use an aggregate bank capital/asset ratio from the Federal Deposit Insurance Corporation. The definition of CAR is Tier 1 bank capital divided by risk-weighted assets. In the Federal Reserve survey of bank lending standards, loan officers are asked how their standards have changed over the past three months. There are separate questions for different types of loans (mortgages etc). For mortgage credit we use survey responses for residential mortgage loans, and for business credit we use the average of small-firm and large-firm responses for C&I (Commercial and Industrial) loans. The survey answers for consumer loans are reported as the net percent of respondents more willing to make loans. We change the sign of this variable so that an increase means that banks are less willing to make loans, i.e. a tightening of credit. Finally, the series are seasonally adjusted using the X12-ARIMA method.

For all three loan standards, we regress standards on the lagged four-quarter change in the bank capital/asset ratio. The Federal Reserve survey asks about changes in loan standards over the past three months, so the standards variable is already expressed in differences, which is why we use the change in the CAR. A lagged dependent variable is included to capture dynamic effects. The results are reported in Table 1. The specification used is:

$$LOAN\ STANDARDS = \alpha + \beta \cdot \Delta(BANK\ CAPITAL/ASSET\ RATIO) \\ + \gamma \cdot (LAGGED\ LOAN\ STANDARDS) + \varepsilon.$$

The parsimonious specification works well and the coefficient on capital adequacy is always negative and highly significant. Quantitatively, the estimates imply relatively similar short-run and long-run effects of a one-percentage-point reduction in the CAR across loan categories. In the short run, a percentage-point decrease in the capital ratio causes the balance of responses on loan standards to tighten by 2–5 percentage points. Taking into account the lagged dependent variable, the long-run effect is a tightening of our measure of standards by 10-30 percentage points.

In order to get a better sense for the size of a one-percentage-point CAR shock, it is useful to compare it with the standard deviation of CAR (0.72). Thus the assumed

Table 1: The Bank Capital/Asset Ratio and Loan Standards			
	Consumer loan standards (t)	Mortgage loan standards (t)	Business loan standards (t)
CAR (t-1) - CAR (t-5)	-4.73 *** (0.77)	-2.37 ** (1.16)	-3.99 *** (1.44)
Loan standards (t-1)	0.67 *** (0.06)	0.80 *** (0.10)	0.87 *** (0.07)
Adjusted R-squared	0.67	0.63	0.81

Source: IMF staff calculations.
Sample period: 1991q2 – 2007q3. Standard errors in parentheses (adjusted for heteroscedasticity and autocorrelation). ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

shock is somewhat larger than a typical change in CAR. Note also that we assume a permanent rather than a temporary shock.

It can be seen in Figure 2 that the capital/asset ratio increased rapidly during the early 1990's, which could potentially distort the empirical results. As a robustness check, we also estimate the model for the period 1995-2007, after the capital asset ratio stabilized. We find that the average effect of capital on standards is relatively unchanged, with falls in the impact on consumer and mortgage credit offset by a large increase in the impact on corporates.

The estimates confirm the expected negative relationship between the CAR and loan standards. In the next step, we investigate the effect of loan standards on credit.

4. The effect of lending standards and balance sheets on credit

There already exists some empirical evidence on the effect of lending standards on credit. The papers by Lown and Morgan (2006) and Lown, Morgan and Rohatgi (2000) find that a tightening of standards causes the quantity of bank credit to decline. However, while only bank lending is covered by the Federal Reserve loan officer survey, there are reasons to believe that the banks' responses contain some information about more general credit availability. The lending capacity of banks and other, non-bank credit providers (e.g. insurance companies, finance companies and pension funds) is likely to be positively correlated.

This hypothesis has received some support in the literature. Friedman (1991) discusses the generalized fall in credit during the credit crunch in 1990, and he advocates the supply interpretation: "the credit crunch of 1990 resulted from the impact on bank balance sheets of the credit excesses of the 1980s, and just as banks were not alone in participating in those excesses, they are not alone in suffering the consequences. The

same problems that have impaired some banks' capital have also shrunk the "surpluses" of insurance companies, have caused profitability problems for finance companies, and have led to the collapse of the junk-bond market." More recently, Bernanke (2007) argues that banks and non-banks are subject to similar forces in the sense that they all have to raise external funds in order to lend, and that their cost of external funds depends on balance-sheet variables such as net worth, liquidity and leverage.

Since non-bank lenders are likely to behave in a similar way as banks, we use broader measures of credit than only bank credit. More precisely, we use consumer credit, home mortgages, and nonfinancial corporations' credit market instruments.⁵ For example, consumer credit is provided also by finance companies, and credit market instruments include commercial paper and corporate bonds. A specification with only bank credit works well (results are reported in Appendix I), but the quantitative impact of a change in loan standards on credit is underestimated. Since bank and non-bank credit move in the same direction following a change in loan standards, bank credit changes by less than total credit. Results for the impact of only bank credit on spending (not reported for the sake of brevity but available on request) find that this underestimation of the impact on credit also leads to a smaller effect on spending. Therefore, for macroeconomic analysis it is important to use a wider definition of credit.

In addition to loan standards, we include income and home equity as explanatory variables in order to allow for effects of borrower balance sheet conditions on credit provision. Loan standards reflect credit availability given borrower characteristics, while balance sheet variables have additional effects on credit availability because of changes in borrower characteristics.

We use Flow of Funds data on credit flows and balance sheet stocks for various sectors in the U.S. economy. Since our measure of loan standards is the change in standards over the last three months, the credit variables are defined using changes in the flow of credit. Income is defined as personal disposable income in the consumer credit equation, home equity for mortgage lending, and business profits in the business credit equation. To reduce potential heteroscedasticity problems and to facilitate the interpretation of coefficients, we divide changes in credit and income by lagged GDP and multiply by 100, so that changes are expressed as a percent of GDP. We use a long-term real interest rate for mortgage credit and a short-term rate for the other two sectors.⁶ As in the previous section the loan standards variables are defined so that

⁵ In the Flow of Funds, the variables "consumer credit" and "home mortgages" are taken from Table F.100 (lines 41 and 40, respectively), and the variable "credit market instruments" is taken from Table F.102 (line 39).

⁶ The long-term rate is the 10-year Treasury note minus the 10-year ahead average expected inflation, and the short-term rate is the 3-month LIBOR minus the 1-year ahead expected inflation.

Table 2: Loan Standards, Balance Sheet Variables, and Credit			
	Change in credit at time t		
	Consumer	Mortgage	Business
Change in income 1/	0.19 ** (0.09)	0.23 *** (0.15)	0.62 (0.53)
Loan standards (t) 2/	-0.011 *** (0.003)	-0.006 (0.007)	-0.013 ** (0.006)
Change in interest rate (t-1)	-0.07 (0.07)	-0.71 *** (0.25)	-0.04 (0.21)
MA(1) term 3/	-0.57 *** (0.13)	-0.59 *** (0.15)	-0.39 *** (0.08)
Adjusted R-squared	0.31	0.29	0.18

Source: IMF staff calculations.
Sample period: 1990q4 – 2007q2/q3. See Table 1 for additional notes.
1/ At time t-1 for consumer and mortgage credit, and time t-2 for business credit.
2/ The first lead of standards is used in the mortgage credit regression.
3/ Business credit uses AR(1) term instead of MA(1) term.

an increase means further tightening of credit conditions. As the changes in flow of funds variables appear to exhibit significant negative serial correlation, we include an MA(1) term in the error (for credit to firms the estimated coefficient on the MA(1) error was unreasonably large, so we substituted an AR(1) error). Given that we use a specification in changes, it is not surprising that the R-squared's are relatively low. The regression results are presented in Table 2. The general specification is thus:

$$\begin{aligned} \Delta(CREDIT)/GDP_{-1} = & \alpha + \beta(L) \cdot \Delta(INCOME)/GDP_{-1} \\ & + \gamma(L) \cdot (LOAN\ STANDARDS) + \theta(L) \cdot \Delta(INTEREST\ RATE) \\ & + \varepsilon + \tau \cdot \varepsilon_{-1}. \end{aligned}$$

The main result is that a tightening of loan standards causes the quantity of credit to decline. The effect is significant at the 1 percent level for consumer credit and at the 5 percent level for business credit. The estimated effect of standards on mortgage credit is somewhat smaller and not statistically significant. Mortgage loan standards did not tighten in the 2001 recession and there is little movement in the series during most of the sample period, which makes it difficult to find any significant effects.

For all three credit categories, the estimated effect of standards on credit is relatively similar. A one-percentage-point tightening of standards causes each separate measure of credit to fall by around 0.01 percent of GDP in the same quarter as the

The main source of inflation expectations is the Philadelphia Fed's Survey of Professional Forecasters (before 1991Q4 the sources are the Livingston Survey, also from the Philadelphia Fed, and the Blue Chip survey).

tightening is reported. However, a typical change in standards is much larger than one percentage point. The standard deviation of standards is in the range 9-18 depending on credit category. A larger tightening of 30 percentage points, which is similar to what we have seen in the recent past, would cause the flow of credit in each of the three categories to decline contemporaneously by around 0.25 percent of GDP, or around \$35 billion in current dollar terms. For comparison, the flow of consumer, mortgage and business credit in the last quarter of 2007 was around 85, 430 and 640 billion dollars, respectively.⁷ The estimated direct effects are thus large in relative terms, especially for consumer loans. The coefficient on lagged income is positive for both consumer and business credit, but only significant (at the 5 percent level) for consumer credit. Home equity has a positive impact on mortgage credit (significant at the 1 percent level).

As a robustness check, we re-estimated these regressions over the first and second halves of our relatively short sample (1990:4-1999:4 and 2000:1-2007:3). The results show a similar pattern to those found when estimating the link between the capital/asset ratio and standards. Namely, the results were weaker for consumer credit (where the estimates for the second period were incorrectly signed) as well as the relatively unimportant mortgage channel, but the links to corporate credit were stronger in the later part of the sample.

5. The effect of credit on spending

One possible way to test for financial frictions/credit constraints is to investigate if income or cash flow has an effect on spending after controlling for fundamental determinants of spending according to benchmark theories without any frictions or constraints. Early examples of this approach are the papers by Campbell and Mankiw (1990) (for consumption) and Fazzari, Hubbard and Petersen (1988) (for investment). Campbell and Mankiw test the permanent-income hypothesis by estimating the effect of predictable changes in income on consumption. According to the permanent-income hypothesis, only unpredictable changes in income should affect consumption, so the theory predicts a coefficient of zero. In fact, the effect is found to be positive, indicating that at least some consumers are credit constrained.

Similarly, a seminal paper by Fazzari, Hubbard and Petersen (1988) tests the neoclassical theory of investment by including cash flow in a regression of investment on Tobin's Q. According to neoclassical theory, only fundamentals (in their approach, Tobin's Q) should matter for investment, and financial factors such as cash flow should be irrelevant. However, cash flow often has a positive and significant effect on investment,

⁷ All credit flows are expressed in annual terms.

even when controlling for fundamentals. Hence for both consumption and investment there exists evidence that the availability of financing matters for spending. Recent evidence is provided by Dynan, Elmendorf and Sichel (2006) who investigate changes over time in the responsiveness of consumption and investment to income, interest rates and cash flow.

However, the purpose of this section is not to show that “credit matters” by demonstrating that income or cash flow affects spending. Instead, the aim is to estimate the quantitative effect of credit on spending, which is an important link in our chain of macro-financial linkages. For our purposes, a more useful empirical strategy is the one developed by Bacchetta and Gerlach (1997) and Ludvigson (1998). These authors study the effects of predictable changes in credit on consumption. The approach is closely related to and inspired by the Campbell and Mankiw (1990) paper which tests for effects of predictable changes in income on consumption. We apply the method to study the effects of credit on both consumption and investment.

The econometric method used is two-stage least squares. The following brief description of the Campbell-Mankiw model illustrates why OLS is not appropriate. A fraction of total income is earned by “rule-of-thumb” consumers who consume their current income rather than their permanent income. It follows that the change in consumption can be written as:

$$\Delta C_t = \mu + \lambda \cdot \Delta Y_t + (1 - \lambda) \cdot \varepsilon_t$$

where the parameter λ is the fraction of income earned by rule-of-thumb consumers and the shock ε_t is the innovation between time $t - 1$ and t in agents’ forecast of permanent income. The innovation is orthogonal to any variable known at time $t - 1$. However, the change in income ΔY_t , and the permanent-income revision ε_t , are likely to be positively correlated. Such a correlation between the error term and an explanatory variable causes OLS estimates to be biased and inconsistent. The natural solution is to use instrumental-variable methods, using lagged variables (such as income and consumption) as instruments.⁸

More generally, in a regression of current spending on current income, there is an endogeneity problem, since spending affects income (reverse causality). For example,

⁸ There is a minor technical problem when estimating the model for consumption. If the Permanent Income Hypothesis holds in continuous time, then measured consumption is a (quarterly) time average of a random walk and follows an MA(1) process even under the null hypothesis that the PIH is true. Campbell and Mankiw solve the problem by lagging instruments at least twice. A disadvantage of this procedure is that it throws away the best instruments, i.e. variables lagged only once. We use an alternative method proposed by Carroll, Fuhrer and Wilcox (1994), who include an MA(1) error term, $\nu_t + \theta\nu_{t-1}$, in the estimation. Then variables dated $t - 1$ are valid instruments, since they are uncorrelated with ν_t .

consumption and investment (spending) clearly affect GDP (income). There is a similar endogeneity issue in a regression of current spending on current credit. If spending is financed by credit, but without any credit constraints, then the variables will have a positive correlation even if the causality runs from spending to credit rather than from credit to spending. By using lagged variables as instruments for current income and credit, it is possible to solve the endogeneity issue.

Greenlaw and others (2008) also use an IV approach to study the effect of credit on spending. There are two main differences between their approach and ours. First, they study aggregate credit and aggregate GDP, while we use more disaggregated measures of credit and spending, which allows us get more accurate estimates. Second, in their first-stage regression of credit on lagged variables, they include variables associated with credit supply (e.g. loan standards) as instruments, but they exclude income. The idea is to identify movements in credit which are caused by credit supply. We use a broader set of instruments, including in particular lagged income, in the first-stage regression, and we also include income in the second-stage regression of spending on credit and other variables. The motivation is that if income has an effect on credit through its impact on the balance sheets of households and firms, it is important to include income both as an instrument and in the spending equation. Otherwise, the estimates do not capture financial-accelerator effects, which are likely to be important for macro-financial linkages.

Changes in spending are defined as quarterly changes divided by lagged GDP, i.e. in the same way as changes in income and credit are defined. To increase the number of observations, we start the sample in 1983. This choice restricts the availability of data on loan standards for some categories of credit. Following Greenlaw et al (2008) we use standards for consumer loans as a proxy instrument for general credit availability. The correlation between consumer and other loan standards is high (0.63 for business loans and 0.46 for mortgage loans). To test if the use of consumer loan standards as a proxy for general loan standards affects the results, we also estimate equations with the “correct” standards since 1991 for residential, business, and inventory investment. The results are basically unchanged, except in the case of business fixed investment where the use of “incorrect” standards leads to a slight underestimation of credit effects. In the equation for business investment, we include a proxy for Tobin’s Q ratio to control for fundamental determinants of investment. The Q ratio is defined as the market value of equities divided by net worth (for nonfarm nonfinancial corporate business). In Table

3 we report the two-stage least squares regression results. The general specification is:

$$\begin{aligned} \Delta(SPENDING)/GDP_{-1} = & \alpha + \beta(L) \cdot \Delta(INCOME)/GDP_{-1} \\ & + \gamma(L) \cdot \Delta(CREDIT)/GDP_{-1} + \theta(L) \cdot \Delta(SPENDING)/GDP_{-1} \\ & + \varphi(L) \cdot (OTHER FUNDAMENTALS) + \varepsilon + \tau \cdot \varepsilon_{-1}. \end{aligned}$$

For all three components of spending (consumption, residential investment and business investment) we find a positive and significant effect of credit on spending. Most credit coefficients are significant at the 1% level. In quantitative terms, the effect of credit is particularly rapid and strong for consumption. An increase in consumer credit of 1 dollar causes spending to increase by 64 cents contemporaneously and an additional 26 cents—for a total of 90 cents—after one quarter. It is interesting to note that credit seems to be a more important direct determinant of spending than income (recall, however, that credit is itself a function of income). As a further check on the robustness of our specification, we estimate the equations using subcomponents of consumption, reported in Appendix II. As expected, the impact of consumer credit on spending is largest for durable goods and smallest for services (large parts of which are imputed, such as owner occupied housing) with aggregate effects similar to those from total consumption.

For business investment, the effects are smaller and more delayed, but they are still substantial. The effect of a one-dollar increase in business credit is to raise investment by 12 cents contemporaneously and almost 50 cents in the long run. The impact of mortgage credit on residential investment is quantitatively much smaller. After one quarter only 2 cents of a one-dollar increase in mortgage credit is reflected in higher residential investment, rising to around 10 cents in the longer run. Mortgage loans are generally made to finance purchases of old, existing houses or after the construction of new houses, so the absence of substantial effects of loans on housing investment is not surprising.

For each type of spending, we also estimate equations (not reported) which allow for asymmetric effects of positive and negative changes in credit, but without finding any significant asymmetries. To the extent that credit constraints bite more in downturns than upturns, it appears to occur through the impact of banks on credit, not the impact of credit on spending. As a robustness check, we also investigate the stability of estimates over time by comparing results for the first and second halves of the sample (1983-1994 and 1995-2007). The results accept stability for all of the series except mortgage spending, which is not important for our results. This again suggests our results are not being biased by the particular sample period.

Table 3: The Effect of Credit on Spending (Two-stage least squares regressions)				
	Change in spending at time (t)			
	Consumption	Residential investment	Business fixed investment	Inventory accumulation
Change in income (t) 1/	0.004 (0.10)	0.09 *** (0.03)	...	0.68 *** (0.25)
Change in income (t-1) 1/	0.12 * (0.06)	0.41 *** (0.10)
Q ratio (t-1)	0.10 ** (0.05)	...
Change in credit (t) 2/	0.64 *** (0.15)	...	0.12 *** (0.04)	0.05 ** (0.02)
Change in credit (t-1) 2/	0.26 ** (0.11)	0.02 *** (0.01)	0.09 *** (0.02)	...
Change in credit (t-2) 2/	0.08 *** (0.02)	...
Change in credit (t-3) 2/	0.04 ** (0.02)	...
Change in credit (t-4) 2/	0.01 (0.02)	...
Change in spending (t-1)	...	0.72 *** (0.05)	0.23 ** (0.11)	-0.31 *** (0.09)
MA(1) term	0.04 (0.13)
Adjusted R-squared	0.08	0.65	0.29	0.05

Source: IMF staff calculations.
Sample period: 1983q1 – 2007q3. See Table 1 for additional notes.

Instruments in consumption regression: lags one to four of change in personal disposable income, change in consumer credit, change in mortgage credit, change in interest rate, and change in consumption; current and lags one to four of loan standards; and lagged consumption-income ratio.

Instruments in residential investment regression: lags one to four of changes in GDP, mortgage credit, residential investment, and population of home-buying age; current and lags one to four of loan standards; and lagged Q ratio.

Instruments in business investment regression: lags one to four of change in profits, change in business credit, and change in business investment; current and lags one to four of loan standards; and lagged Q ratio.

Instruments in inventory accumulation regression: lags one to four of change in profits, change in business credit, and change in inventory accumulation; and current and lags one to four of loan standards.

1/ Income is defined as personal disposable income, GDP, and profits, respectively.
2/ Credit is defined as consumer, mortgage, and business credit, respectively.

6. The effect of spending on income

When private spending changes, there are multiplier effects on income. A given increase in spending causes an equivalent increase in income, part of which further

	Change in income at time (t)			
	Personal disposable income	GDP	Profits	Home equity
Change in consumption	0.44 *	0.97 ***	0.10	0.05
	(0.24)	(0.09)	(0.10)	(0.30)
Change in residential investment	0.36	0.65 ***	0.39 **	0.66
	(0.38)	(0.16)	(0.20)	(0.53)
Change in business fixed investment	0.81 ***	0.87 ***	0.005	-0.03
	(0.29)	(0.13)	(0.15)	(0.31)
Change in rate of inventory accumulation	0.40 **	0.91 ***	0.13 *	0.04
	(0.18)	(0.09)	(0.07)	(0.27)
Lagged dependent variable	-0.31 **	-0.09 *	-0.15	0.54 ***
	(0.13)	(0.05)	(0.11)	(0.08)
Adjusted R-squared	0.19	0.80	0.07	0.29

Source: IMF staff calculations.
Sample period: 1983q1 – 2007q3. See Table 1 for additional notes.

increases spending and so on. To estimate these multiplier effects, we run OLS regressions of income on components of spending and a lagged dependent variable. Similarly, increases in spending have positive effects on house prices and hence on home equity, so we also regress home equity on spending. It should be noted that our empirical specifications are very simple. The purpose of this section is to provide rough estimates of the links from spending to income rather than to carry out an in-depth study of multiplier effects.⁹ The results are reported in Table 4. The general specification is:

$$\Delta(INCOME)/GDP_{-1} = \alpha + \beta(L) \cdot \Delta(SPENDING)/GDP_{-1} + \gamma(L) \cdot \Delta(INCOME)/GDP_{-1} + \varepsilon.$$

In almost all cases the estimated coefficients are positive, and in many cases they are highly significant. Since the variables are scaled by lagged GDP, the coefficients should be interpreted in the same way as in the previous section. For example, in the regression of personal disposable income on spending, the coefficient 0.44 on consumption means that if consumption increases by 1 dollar, then personal disposable income increases by 44 cents.

⁹ One potential concern is endogeneity problems. We tried other specifications with lagged explanatory variables or instrumental variables, but the estimated coefficients were often economically unreasonable (e.g. negatively signed).

Table 5: Feedback Effects of GDP Growth on Bank Capital	
Bank Capital/Asset Ratio (t)	
Change in GDP (t-1)	0.54 *
Adjusted R-squared	0.08
Source: IMF staff calculations. Sample period: 1990q1 – 2007q3. See Table 1 for additional notes.	

7. Feedback loop through balance sheets of banks, firms and households

In Sections 3-6 we estimated the effects of a bank capital adequacy shock on spending and income through its impact on lending standards and credit. Now we allow for feedback from spending and income through the balance sheets of banks, firms and households. Taking these feedback channels into account, the final effect of a bank capital shock on aggregate economic activity is larger than the direct effect.

There are two distinct and mutually reinforcing feedback channels. The first channel is that as spending and income fall, loan losses increase and thus there are further negative effects on bank capital in addition to the initial negative bank capital shock. We model this feedback from the real economy to bank balance sheets by regressing the capital adequacy ratio on changes in lagged GDP growth. This specification is a simplification in the sense that GDP does not have any *direct* causal effect on CAR. There is only an *indirect* effect through increased loan losses. In Table 5 we present estimates of the feedback from GDP growth to bank capital. A one-percent decrease in GDP growth is estimated to be associated with loan losses which decrease the capital ratio by around $\frac{1}{2}$ percentage point. The empirical specification is:

$$BANK\ CAPITAL/ASSET\ RATIO = \alpha + \beta(L) \cdot \Delta(GDP)/GDP_{-1} + \varepsilon.$$

The second feedback channel is that a deterioration of incomes and balance sheets for households and firms has a further negative financial-accelerator effect on credit and spending. In the regressions in Section 4 we allowed credit to be affected not only by loan standards, but also by lagged income and home equity. When calculating the feedback from a real economic slowdown to the credit market, we take the model's predicted decreases in income and home equity, and allow them to have further negative effects on credit. There is also a direct impact of lower incomes on spending, estimated in Section 5. Hence there is no need for any further estimation in order to capture these feedback effects. It is sufficient to allow the model's predicted fall in income to have second-round effects on credit and spending according to our previous estimates.

In addition to the two feedback channels from income to the CAR and credit, there is another mechanism which eventually reverses the adverse macro-financial cycle. All else equal, the gradual decline in bank credit improves the capital/asset ratio by shrinking the asset side of bank balance sheets (deleveraging), possibly supported by recapitalization. We attempted to estimate the empirical size of this effect directly, but with poor results possibly reflecting the impact of the cycle on risk-adjusted assets.

Instead, we impose a realistic impact of credit on the CAR, based on the regressions reported in Appendix I for the impact of tightening standards on banks' provision of consumer credit, mortgages, and corporate loans. The ratio of the coefficient on bank loans to the coefficient on overall loans allows us to calculate the reduction in bank assets implied by lower overall lending. For consumer loans, the ratio is around one-half. Assuming that bank capital is \$1100 billion and bank assets \$11000 billion, which gives a realistic CAR of 10%, we can then calculate the impact of lower assets on the CAR. For example, a \$200 billion reduction in overall consumer credit implies a \$100 billion decline in the flow of bank credit; i.e. a reduction in assets to \$10900. The new CAR would be $1100/10900$ or around 10.1%. One caveat to this calculation is that the CAR is risk-adjusted, so the boost to bank capital may be overestimated. On the other hand, no allowance is made for active recapitalization through issuing additional equity. Overall, we regard our model as a reasonable estimate of the support to CAR from reductions in loans.

8. Bottom line: quantitative importance of macro-financial linkages

Having developed and estimated a model of macro-financial linkages, we can now study the model's implications for the quantitative effects of financial or macroeconomic shocks.

Our first experiment is to study the effects of a financial shock. Given a hypothetical one-percentage-point decrease in the bank capital/asset ratio, what is the effect on GDP, taking into account macro-financial feedback effects? We assume that the capital/asset ratio falls exogenously by one percentage point. Then we use our estimated links between the variables to investigate the dynamic response of the economy, allowing for macro-financial feedback. There is a gradual slowdown of economic activity as lending standards tighten over time. As a result, the CAR declines by more than the initial shock. The negative effect on GDP grows gradually over time, peaking at 1.4 percent of GDP three years after the initial CAR shock and two years after the maximum impact on bank lending standards. Figure 3 presents the results within our graphical framework, and Figures 4 and 5 present responses of the level and annualized

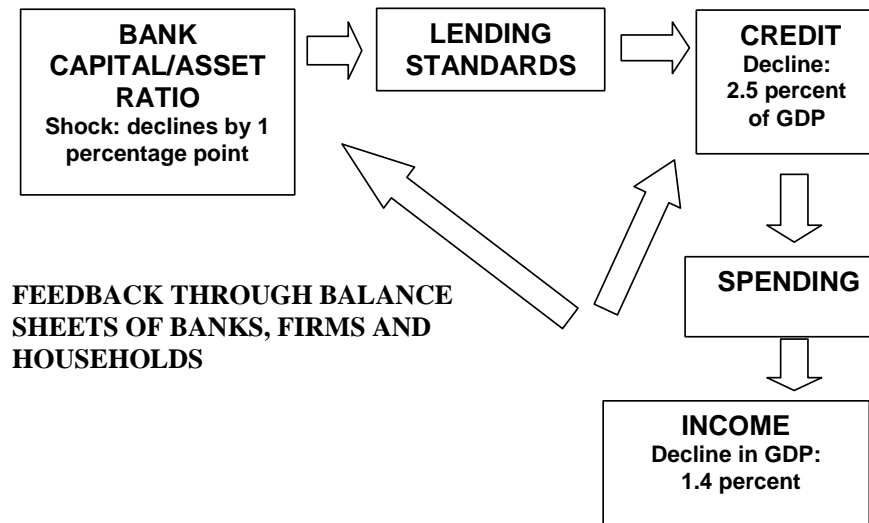


FIGURE 3. The effects of an adverse bank capital shock.

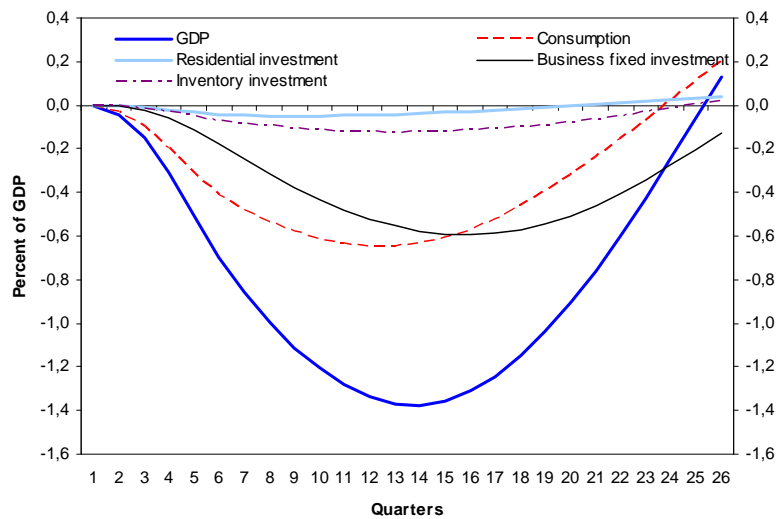


FIGURE 4. The impact of an adverse capital shock on the level of GDP and its components.

growth rate of GDP and its components (the impact on growth peaks at $\frac{3}{4}$ percent just over a year after the initial CAR shock).

There are some interesting results when looking at the sub-components of spending. It is clear that changes in consumption and business fixed investment are the main factors behind the impact on GDP. Also, consumption responds more rapidly than business fixed investment. This is to be expected, given the longer planning horizons

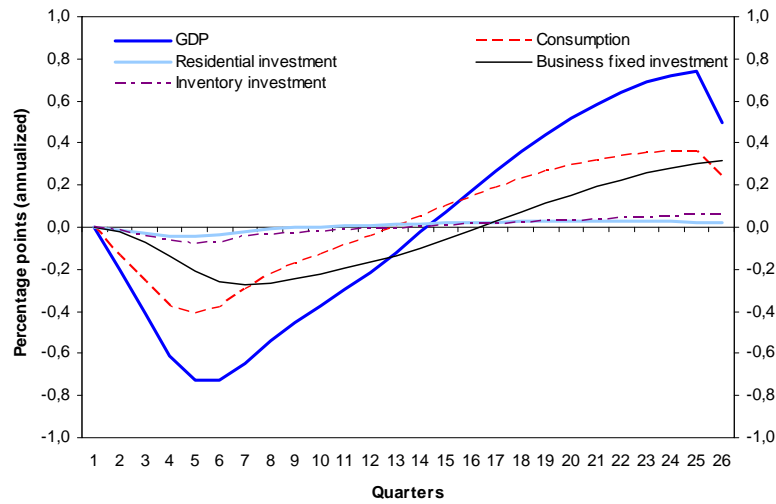


FIGURE 5. The impact of an adverse capital shock on GDP growth and the contribution of GDP components.

involved in investment decisions. The response of residential investment is very minor, which is not surprising given the small estimated effects of mortgage credit, and the contribution of inventory investment is only somewhat larger.

Another interesting experiment is to study the effects of a macroeconomic shock. We investigate how a real demand shock is propagated and amplified through the model's macro-financial linkages. The hypothetical negative demand shock is an exogenous decline in consumption and investment of 1 percent of GDP (in total). A weaker macroeconomic environment causes the CAR to decline, which causes a tightening of credit and makes GDP fall by more than the initial shock. Unsurprisingly, the response to a macroeconomic shock has similar time lags and dynamics for sub-components of spending as a financial shock. The additional impact on the level of GDP gradually grows to 1.2 percent after three years. Thus the initial demand shock is approximately doubled through macro-financial linkages. These results are summarized in Figures 6 and 7. Similar overall patterns are evident when (possibly more realistic) temporary shocks to spending are used.

We also investigate the effect of a demand shock in a restricted version of the model where the link from GDP to the CAR (“bank capital effects”) is cut off, so that the only macro-financial link is the traditional financial accelerator channel through household/firm credit constraints (“financial accelerator effects”). The additional impact on GDP is reduced to 0.5 percent, compared with 1.2 percent in the full model. This result

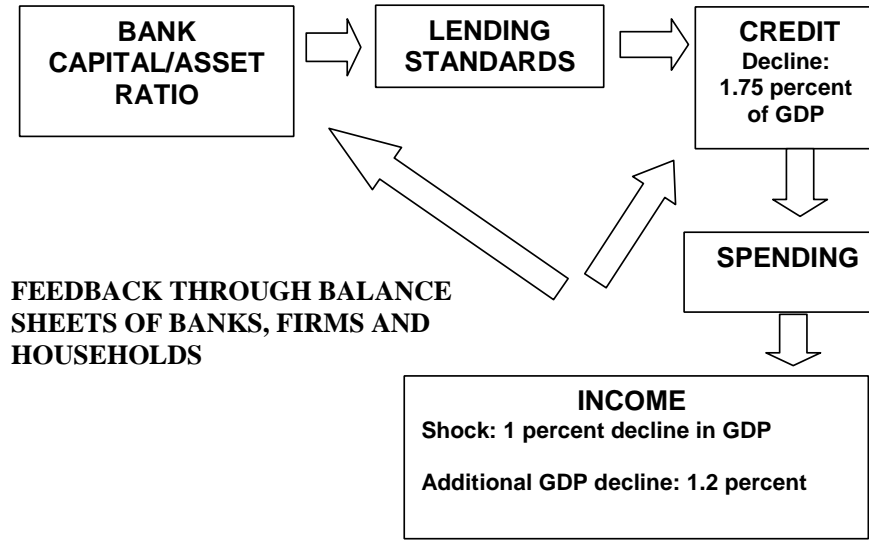


FIGURE 6. The effects of an adverse demand shock.

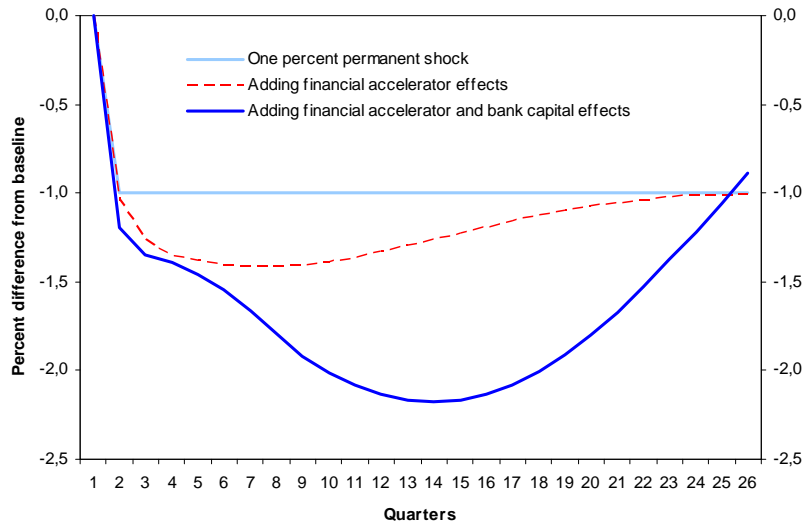


FIGURE 7. The impact of an adverse demand shock on the level of GDP.

implies that bank capital effects and financial accelerator effects have approximately the same quantitative importance. A striking difference in the restricted model is that the peak effect on GDP occurs much sooner than in the full model (after 6 rather than 13 quarters). Thus the link from GDP to the CAR both deepens and lengthens the economy’s response to shocks.

Naturally, all of these quantitative predictions of the estimated model have wide confidence intervals, due to potential model misspecification and uncertain parameter estimates and the simplifications necessary to obtain a tractable model. In particular, the following two caveats apply.

First, the model does not fully capture the likely dynamics of banks' adjustment to a deterioration of credit quality. Our empirical specifications in Section 3 assume that lending standard contractions occur only after capital losses are visible on bank balance sheets, which is a simplification, since banks are likely to tighten lending standards already in anticipation of capital losses.

Second, we do not directly model any monetary or fiscal policy response even to a substantial negative financial shock.

9. Conclusions

This paper studies U.S. macro-financial linkages in a clear empirical framework which allows for feedback effects from the real economy to the credit market. The policy purpose is to investigate the likely effects of a negative shock to bank capital on the macroeconomy. Each separate link could be studied in more detail, using a larger number of explanatory variables and more sophisticated econometric techniques. Yet the main results are similar to what other studies have found using alternative (but not necessarily more sophisticated) methods. In particular, the estimated effect of an adverse credit market shock is similar to results using a Financial Conditions Index developed by the IMF staff.

The key findings are that (i) banks' balance sheet conditions have substantial effects on credit availability, (ii) credit conditions significantly affect real spending decisions of consumers and firms, and (iii) there are important feedback effects from the real economy to the credit market, which amplifies and prolongs the response to shocks.

By necessity the empirical estimates are based on historical patterns in the data. But the specific circumstances in each financial crisis are different, and it is possible that the current downturn may be more or less severe than implied by previous experience. For example, a significant increase in mortgage defaults due to widespread negative equity positions could potentially have larger effects than predicted by empirical models. On the other hand, banks appear to have recognized losses and recapitalized rapidly.

The topic of macro-financial linkages clearly needs to be studied further. One interesting issue is the impact of changes in short-term interest rates on bank profitability. A decrease in short-term rates should improve banks' profitability by making the yield

curve steeper. Banks can pay lower interest rates on short-term deposits, while long-term lending rates are relatively unchanged. An empirical study by Robinson (1995) finds that U.S. banks' exposure to interest rate risk has increased since the implementation of the Basle agreement (which puts limits on credit risk, but not interest rate risk), which makes bank profits more sensitive to changes in interest rates. However, the result is only preliminary, since the sample period used only includes few observations from the post-Basle era. Hence it would be useful to study the effects of monetary policy on bank balance sheets using more recent data.

Another possible approach for future research would be to apply the methodology used in a paper by Hartelius, Kashiwase and Kodres (2008). They study the impact of changes in the sovereign credit rating outlook on emerging market bond spreads. Similar methods could be used to study the impact of changes in the bank credit rating outlook on bank financing costs.

Appendix I: Credit regressions for bank lending

Our measures of consumer, mortgage, and corporate sector credit include a significant share of financing from non-bank lenders. In this Appendix, we report the results for regressions using only bank credit. As expected, the impact of loan standards on bank credit is important but significantly smaller than overall credit. Hence, focusing only on bank loans would underestimate the shock to credit available to households and firms.

	Change in credit from banks at time (t)		
	Consumer	Mortgage	Business
Change in income 1/	0.11 ** (0.05)	0.03 (0.04)	0.44 ** (0.20)
Loan standards (t) 2/	-0.005 ** (0.002)	-0.004 (0.005)	-0.005 * (0.002)
Change in interest rate (t-1)	-0.08 (0.05)	0.03 (0.17)	-0.03 (0.09)
MA(1) term 3/	-0.68 *** (0.09)	-1.18 *** (0.27)	-0.33 *** (0.09)
Adjusted R-squared	0.29	0.63	0.16

Source: IMF staff calculations.
Sample period: 1990q4 – 2007q2/q3. See Table 1 for additional notes.
1/ At time t-1 for consumer and mortgage credit, and time t-2 for business credit.
2/ The first lead of standards is used in the mortgage credit regression.
3/ Business credit uses AR(1) term instead of MA(1) term.

Appendix II: Spending regressions for sub-components of consumption

For the various sub-components of consumption, we find significant effects in most cases. As expected, the impact of consumer credit on the consumption of durable goods is large and significant. For non-durable goods the effect is smaller but still significant, and for services the impact is not significant. One reason for the insignificant impact on services could be that many services are not paid directly by the consumer, and are thus not subject to credit constraints. In particular, owner-occupied housing and health care paid for by insurance companies are two large components of services consumption.

Table 7: The Effect of Credit on Personal Consumption Expenditure (Two-stage least squares regressions)			
	Change in spending at time (t)		
	Durable goods	Nondurable goods	Services
Change in income (t)	-0.02 (0.06)	0.04 (0.03)	-0.06 * (0.04)
Change in credit (t)	0.32 *** (0.09)	0.12 ** (0.05)	0.06 (0.06)
Change in credit (t-1)	0.10 * (0.06)	0.03 (0.04)	0.06 (0.05)
MA(1) term	-0.19 (0.12)	-0.11 (0.14)	0.33 *** (0.09)
Adjusted R-squared	0.07	0.10	-0.02

Source: IMF staff calculations.
Sample period: 1983q1 – 2007q3. See Table 1 for additional notes.
Instruments in consumption regression: lags one to four of change in personal disposable income, change in consumer credit, change in mortgage credit, change in interest rate, and change in dependent variable; current and lags one to four of loan standards; and lagged consumption-income ratio.

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The Effects of Real Exchange Rate Shocks in an Economy with Extreme Liability Dollarization

Ola Melander

ABSTRACT. This paper studies the effects of real exchange rate (RXR) shocks in an economy with extreme liability dollarization using vector autoregression (VAR) methods. Bolivia's extreme liability dollarization makes it an interesting case for empirical testing of the contractionary-depreciations hypothesis. In contrast to the previous contractionary-depreciations literature, the paper uses identification assumptions which are inspired by modern macroeconomic theory and common in the empirical VAR literature on the effects of monetary policy. I find that a RXR depreciation has negligible effects on output, since a contractionary balance-sheet effect on investment is counteracted by the standard expansionary effect on net exports. Furthermore, I find that a RXR depreciation has inflationary effects.

1. Introduction

In standard small-open economy models, such as, for example, the model by Svensson (2000), a real exchange rate (RXR) depreciation has an expansionary effect on aggregate demand and output. Depreciation increases the demand for domestically produced goods by reducing their relative price. Such expenditure-switching effects are familiar from the traditional Mundell-Fleming-Dornbusch models and generally remain valid in more modern New Open Economy Macro (NOEM) models.¹

However, the impact of depreciation on output could be reversed in an economy with substantial liability dollarization. When liabilities are denominated in foreign currency, but revenues in domestic currency, the possibility of contractionary depreciations arises. Currency depreciation increases the domestic-currency value of foreign-currency

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¹ See Dornbusch (1976) for the original model and Lane (2001) for a survey of the modern NOEM literature.

liabilities and the debt service burden, while firm revenues are denominated in domestic currency. Thus, there is an adverse effect on firms' balance sheet position. In the presence of financial frictions of the Bernanke-Gertler type, a balance sheet deterioration causes the external finance premium to increase and, consequently, investment to decrease. If this negative effect of depreciation on investment outweighs the positive effect on net exports, then a real depreciation has contractionary effects rather than the standard expansionary effects.

This possibility has long been recognized in the literature, but the Asian crisis in the late 1990's created a renewed interest in the possible negative balance sheet effects of depreciation.² An indication that the topic is perceived as relevant by both researchers and policymakers is the title of a recent IMF Mundell-Fleming lecture by Jeffrey Frankel (2005): "Contractionary Currency Crashes in Developing Countries". Possible adverse balance-sheet effects of depreciation in countries with debts denominated in foreign currency is also a key topic in the current international financial crisis. For example, the case of Latvia, whose IMF program does not require an abandonment of the currency peg, has been much discussed. According to the IMF report, the risk of large adverse balance sheet effects was one of the main arguments for keeping the peg (see International Monetary Fund (2009)).

There is an extensive empirical literature which investigates whether real depreciations are expansionary or contractionary (see Bahmani-Oskooee and Miteza (2003) for a survey). However, no paper has studied the Bolivian economy, which is a particularly interesting case given its extreme liability dollarization. Around 96 percent of the outstanding bank loans to households and firms were denominated in dollars during the period 1990-2003. In contrast, only around one quarter of the debt was denominated in foreign currency in the 1997-98 Korean crisis.³ Bolivia's financial dollarization is also extreme in comparison with other countries. However, most goods and services are priced in the local currency, the boliviano, so real dollarization is limited (see Morales (2003)). This is a key difference compared to fully dollarized countries such as Ecuador.

The purpose of this paper is to empirically test the contractionary depreciations hypothesis using Bolivian data and vector autoregression (VAR) methods. As succinctly stated by Bagliano and Favero (1998), VAR models are estimated to "provide empirical evidence on the response of macroeconomic variables to monetary policy impulses in order to discriminate between alternative theoretical models of the economy." Accordingly, I estimate VAR models and investigate the impulse response functions to RXR depreciation shocks to test the contractionary-depreciations hypothesis, which states

² See Cooper (1971) for an early discussion. Krugman (1999) and Aghion, Bacchetta and Banjee (2000) are examples of papers inspired by the Asian crisis.

³ See Escóbar (2003) and Gertler, Gilchrist and Natalucci (2007), respectively.

that a real depreciation shock causes an increase in net exports, but an even larger decrease in investment, and, hence, a fall in output. The main finding is that the two opposite effects on output approximately cancel out, which constitutes evidence against the contractionary-depreciations hypothesis. I also find that a RXR depreciation has significant inflationary effects, regardless of the definition of inflation.

This paper makes the following three contributions to the literature on contractionary depreciations. First, I suggest a method for identification of RXR shocks which is more closely related to modern macroeconomic theory and the previous empirical VAR literature than an alternative method by Kamin and Rogers (2000) which is often used in the contractionary-depreciations literature. Second, I study the Bolivian case, which is particularly interesting given the country's extreme liability dollarization. If liability dollarization makes depreciations contractionary in developing countries, this effect should be particularly strong in Bolivia. Third, while most other papers on contractionary depreciations only study the response of aggregate output, I also investigate the response of various sub-components of output. This helps to distinguish between the benchmark theoretical model with no financial frictions or liability dollarization, on the one hand, and the alternative model where RXR depreciations have contractionary effects, on the other hand.

The rest of this paper is organized as follows. Section 2 provides a theoretical background and Section 3 discusses the previous empirical evidence. In Section 4, I present some cross-country data showing that the Bolivian case is particularly interesting for empirical testing of the contractionary-depreciations hypothesis. Section 5 suggests an improved method for identification of real exchange rate shocks. Section 6 presents the empirical analysis, including robustness checks, while Section 7 concludes the paper.

2. Contractionary depreciations: theoretical background

Before describing the theory behind contractionary depreciations, in subsection 2.1 I first present a modern macroeconomic model where a real exchange rate depreciation has the standard expansionary effect. Then, in subsection 2.2, I discuss possible deviations from the standard case due to the balance sheet effects arising from liability dollarization. There are other, alternative models of contractionary depreciations but the balance sheet channel has received most attention in the academic literature and policy debate. For example, Frankel (2005) argues that the balance sheet channel is the most important. Partly for this reason, and partly since liability dollarization is the motivation for studying the Bolivian case, I do not discuss other possible reasons for contractionary depreciations (see the survey by Bahmani-Oskooee and Miteza (2003)).

2.1. Benchmark model where a real exchange rate depreciation has expansionary effects. This section presents a model by Svensson (2000) where a real exchange rate depreciation has expansionary effects. The framework is a standard small-open economy model with microfoundations and forward-looking expectations. I present the main equations and focus on the economic intuition (see the paper and especially the working paper version for further details). For simplicity, I only discuss the case where monetary policy follows a Taylor rule. The RXR q_t is defined such that an increase denotes a real depreciation.

There are four main equations in the model. First, there is an aggregate supply equation (Phillips curve) for inflation (equation (1) in Svensson (2000)):

$$(2.1) \quad \begin{aligned} \pi_{t+2} = & \alpha_\pi \pi_{t+1} + (1 - \alpha_\pi) E_t \pi_{t+3} + \alpha_y [E_t y_{t+2} + \beta_y (y_{t+1} - E_t y_{t+1})] \\ & + \alpha_q E_t q_{t+2} + \varepsilon_{t+2}^{CP} \end{aligned}$$

where π_t denotes domestic inflation in period t , y_t is the output gap, q_t is the real exchange rate, ε_t^{CP} is a cost-push shock and, for any variable x , $E_t x_{t+\tau}$ is the rational expectation of $x_{t+\tau}$, given the information available in period t . Thus, domestic inflation depends on lagged inflation and previous expectations of output and inflation. Inflation is predetermined two periods in advance—that is, the desired prices for period $t + 2$ are determined at time t , but the actual inflation for the period is also affected by output at time $t + 1$ and the cost-push shock which is realized at time $t + 2$.

Second, there is an aggregate demand equation (IS curve) for output (equation (7) in the paper):

$$(2.2) \quad y_{t+1} = \beta_y y_t - \beta_\rho E_t \rho_{t+1} + \beta_y^* E_t y_{t+1}^* + \beta_q E_t q_{t+1} - (\gamma_y^n - \beta_y) y_t^n + \varepsilon_{t+1}^{AD}$$

where $\rho_t \equiv \sum_{\tau=0}^{\infty} E_t r_{t+\tau}$ summarizes current and future real interest rates, y_t^* is the foreign output gap and ε_t^{AD} is a combination of aggregate demand and productivity shocks. Thus, output depends on previous expectations of the real interest rate path, foreign output and the real exchange rate. Output is predetermined one period in advance—that is, the desired output quantity for period $t + 1$ is determined at time t , before the shocks are realized at time $t + 1$.

The third equation is the Taylor rule for the instrument of monetary policy, i.e. the nominal interest rate i_t :

$$(2.3) \quad i_t = \gamma_\pi \pi_t + \gamma_y y_t + \varepsilon_t^{MP}$$

where ε_t^{MP} is a monetary policy shock which arises since the instrument rule is not followed perfectly. Interest rates can react contemporaneously to the observed values of output and inflation.

Fourth and finally, there is also an uncovered interest parity condition:

$$(2.4) \quad i_t - i_t^* = E_t s_{t+1} - s_t + \varphi_t$$

where i_t^* is the foreign nominal interest rate, s_t is the nominal exchange rate and φ_t is the foreign-exchange risk premium.

The model can be used as a basis for the necessary identification assumptions regarding the timing of relationships between variables. Monetary policy has a contemporaneous effect on real interest rates which affect output with a one-period lag (as shown in equation (2.2)). In turn, output affects inflation with another one-period lag, as can be seen in equation (2.1). The real exchange rate affects output with a one period-lag (see equation (2.2)). By making domestic goods relatively cheaper, a real depreciation stimulates net exports and output. Naturally, this has an indirect effect on inflation with a further one-period lag. In addition to this indirect, delayed effect of the RXR on domestic inflation, there is also a direct, contemporaneous effect on CPI inflation. A real depreciation increases the domestic-currency price of imports, which affects CPI inflation contemporaneously (but not domestic inflation).⁴

In sum, the model suggests that the effect of output shocks on inflation occurs with a shorter lag than the effect of inflation shocks on output. Moreover, interest rates can react to contemporaneous values of output and inflation. Finally, the real exchange rate is an asset price and should be allowed to respond to the other variables within the period.

2.2. Liability dollarization, balance sheet effects and contractionary depreciations. Balance sheet effects have been extensively studied in a closed-economy context. An overview of the literature, as well as a modern general-equilibrium macroeconomic model with financial frictions, is given by Bernanke, Gertler and Gilchrist (1999). The key assumption is imperfect information between borrowers and lenders, which gives rise to an external finance risk premium for borrowing firms. External financing is more expensive than internal financing and the premium is particularly high when firms' balance sheets are in poor condition. The status of balance sheets affects the required rate of return for investment, and, hence, the quantity of investment.

In many emerging and developing countries, liabilities are to a large extent denominated in foreign currency. As is well known in the literature, financial frictions may have larger effects in open economies with extensive liability dollarization than in closed economies. Examples of early papers on adverse balance sheet effects of currency depreciation in countries with foreign-currency liabilities are Cooper (1971),

⁴ CPI inflation is given by $\pi_t^c = \pi_t + \omega(q_t - q_{t-1})$ where ω is the share of imports in the CPI.

Gylfason and Risager (1984), van Wijnbergen (1986) and Lizondo and Montiel (1989). The Asian crisis caused a renewed interest in the role of balance sheet effects in currency crises. Some examples are the papers by Krugman (1999) and Aghion, Bacchetta and Banjee (2000). However, these papers only presented simple one- or two period models which were not empirically evaluated.

Cespedes, Chang and Velasco (2004) develop a dynamic general-equilibrium model with liability dollarization where the country risk premium depends on the value of investment relative to net worth. Holding income constant, a real depreciation increases the debt burden, which has a negative effect on net worth and thereby increases the risk premium. However, a real depreciation also causes an expansion of net exports and output through the standard expenditure-switching mechanism. This has the opposite effects on net worth and the risk premium. Whether the risk premium goes up or down depends on the steady-state ratio of foreign debt to net worth. Real depreciations only have contractionary effects in a “theoretically possible but empirically implausible” case (when an adverse foreign interest rate shock causes a domestic appreciation and an expansion of domestic output). Similarly, Chang and Velasco (2001) study contractionary depreciations in a simplified version of the model in Cespedes, Chang and Velasco (2004). They also find that contractionary balance sheet effects are not sufficiently large to offset the standard expansionary effects of a real depreciation.

In these papers, liability dollarization does not reverse the standard expansionary effect of real exchange rate changes. In contrast, Cook (2004) finds that a real depreciation causes a persistent contraction in output, and conjectures that the difference in results is due to differences in the modeling of nominal rigidities. Thus, it is not unambiguously clear from economic theory whether depreciations are expansionary or contractionary in the presence of financial frictions and liability dollarization.

3. Previous empirical evidence

3.1. Previous international evidence. An important paper in the literature on contractionary depreciations is by Kamin and Rogers (2000). They estimate a number of different VAR models using Mexican quarterly data for the period 1980-1996. The real exchange rate, inflation and real GDP are included in all models, and other control variables are the nominal US interest rate, government spending, money, the capital account and oil prices. Even when control variables are included, real depreciations still make output decrease and inflation increase. Thus, real depreciations are found to be contractionary and inflationary.

A number of recent papers have applied the Kamin-Rogers (henceforth KR) methodology to other developing countries. Some examples are the papers by Ahmed, Ara and Hyder (2006) for Pakistan, Berument and Pasaogullari (2003) for Turkey, Shi (2006) for China, and Vinh and Fujita (2007) for Vietnam. The estimated effects on output are mixed. Real depreciations are expansionary in China and Vietnam, but contractionary in Pakistan and Turkey. As for the effects on inflation, real depreciations are inflationary in Vietnam, Pakistan and Turkey (no evidence is reported for China).

Ahmed (2003) extends the KR methodology to a panel setting. He estimates a panel VAR model using annual data from Argentina, Brazil, Chile, Colombia and Mexico for the period 1983-1999, and finds that real depreciations are contractionary. The effect on prices is negative, but not statistically significant.

There also exist some panel studies of both developed and developing countries. Kamin and Klau (2003) use pooled annual data from 27 countries for the period 1970-1996. They find that real depreciations have contractionary effects in the short run, but insignificantly expansionary effects in the long run. A puzzling result is that real depreciations are (weakly) contractionary for *developed* countries, both in the short and long run. Another paper by Ahmed, Gust, Kamin and Huntley (2002) finds more intuitively plausible results. The authors estimate panel VAR's for different groups of developed and developing countries using similar methods as those in Ahmed (2003). They find depreciations to be contractionary in developing countries, but expansionary in developed countries (as would be expected). In both cases, depreciations cause inflation. A paper by Kamin (1998) specifically focuses on the short-run effect of real depreciations on inflation. He uses a panel with annual data from 38 countries. Real depreciations are found to be inflationary in all cases, but the effect is stronger in Asia and especially Latin America than in developed countries.

To sum up, real depreciations are often found to have contractionary effects on output in developing countries, but there are some cross-country differences. In almost all cases, real depreciations are found to cause higher inflation.

3.2. Previous evidence from Bolivia. Some progress in understanding the effects of real exchange rate changes in Bolivia has already been made by central bank economists. Mendieta and Escóbar (2006) estimate a Vector Error Correction model using quarterly data for the period 1990-2005. They find that a real depreciation has an expansionary effect on output in the short run, but a contractionary effect in the long run. However, they do not investigate the effects on inflation. Other studies focus on the effects of nominal, rather than real, depreciations. Orellana, Lora, Mendoza

and Boyán (2000) estimate VAR models using monthly data for the period 1990-1999 and study the effects of nominal depreciations. They find that a nominal depreciation does not affect output, but makes inflation increase. A similar IMF study by Jaramillo (2007) reaches the same conclusions.

Another IMF paper by Leiderman, Maino and Parrado (2006) finds evidence of real depreciations having negative effects on company balance sheets in financially dollarized countries. Specifically, the authors show that the real exchange rate Granger causes nonperforming loans in Peru (where dollarization is high) but not in Chile (where dollarization is low), which is consistent with adverse balance sheet effects due to real depreciation. The authors also estimate monetary policy reaction functions for a number of countries. When discussing the results for Bolivia, they claim that “In view of its expansionary impact, an [real] exchange rate depreciation leads to . . . a slowing down of the rate of crawl [depreciation] in Bolivia” (p. 17). However, the paper does not present any evidence of real depreciations indeed being expansionary in Bolivia.

4. Why is the Bolivian case especially interesting?

This section discusses in some more detail why we should be especially interested in the Bolivian case, by comparing Bolivia to a number of other countries in Latin America. There are three key results. Bolivia has (i) an extreme degree of liability dollarization, (ii) an above-average level of financial development and (iii) a below-average level of openness.

Table 1 presents data from a paper by Barajas and Morales (2003) which empirically studies the determinants of liability dollarization in a sample of Latin American countries. The first column shows dollar-denominated bank loans as a percentage of total bank loans. As previously discussed, the Bolivian economy exhibits extreme liability dollarization; 97 percent of the bank loans are denominated in dollars, as compared to an average across countries of 40 percent. The measure of financial development is the outstanding credit to the private sector relative to GDP, which is presented in the second column. If financial development had been very low for Bolivia, there would only have been minor balance sheet effects of depreciations, regardless of the currency composition of private sector liabilities. In an economy with few loans, the currency denomination of loans is of little importance. In fact, Bolivia has an above-average credit-to-GDP ratio: 57 percent as compared to an average of 35 percent. For example, the ratio is higher than Argentina’s (22 percent) and Mexico’s (18 percent). Finally, the third column in Table 3 shows economic openness, defined as the sum of exports and imports relative to GDP. Bolivia is somewhat less open than the average

Table 1
Liability dollarization, financial development and openness
in 14 Latin American countries

Country	Liability dollarization	Financial development	Openness
	Dollar loans (percent of total)	Credit to private sector (percent of GDP)	Exports+imports (percent of GDP)
Argentina	59	22	17
Bolivia	97	57	34
Chile	19	64	47
Costa Rica	16	19	73
Dominican Republic	12	31	76
El Salvador	7	38	52
Haiti	26	15	24
Honduras	26	35	81
Jamaica	20	29	65
Mexico	24	18	56
Nicaragua	70	42	90
Paraguay	31	26	82
Peru	62	24	25
Uruguay	83	38	27
Average	40	35	55

Note: the data are from Tables 1 and 6 in a paper by Barajas and Morales (2003). The sample periods vary somewhat across countries and variables. The data for Bolivia are from 1989-2001 (column 1) and 1995-2001 (columns 2 and 3), respectively.

economy (34 percent as compared to 55 percent). However, it is not an extreme outlier, which makes it reasonable to assume that the standard, expansionary effect of a real depreciation on the economy works in similar ways as in other economies.

If anything, Table 1 probably underestimates the extreme nature of liability dollarization in Bolivia. A recent study by Kamil and Sutton (2008) uses firm-level data from more advanced Latin American economies and finds that firms' foreign-currency exposure has been reduced over the past 10 years. One of the reasons behind the reduction has been a rapid development of currency-derivative markets. In contrast, liability dollarization in Bolivia only started to decrease in 2006, which does not affect the results in this paper (see Jaramillo (2007)). Furthermore, there is no currency-derivative market in Bolivia, so it is not possible to hedge currency-risk exposure.

5. Identification of real exchange rate shocks

It is well known that different identification assumptions may produce different results (see, for example, the discussion in Christiano, Eichenbaum and Evans (1999)). Kamin and Rogers (2000) identify real exchange rate shocks by a standard Cholesky decomposition. They assume the following recursive ordering for the main variables: RXR, inflation and output. This implies that the real exchange rate may affect both

inflation and output contemporaneously, but not vice versa, and that inflation may affect output within the period, but not vice versa.⁵ The assumed recursive ordering is inspired by a simple model where a real exchange rate adjustment alters the nominal price level which, in turn, causes changes in output. However, it is not clear that the model is appropriate for imposing contemporaneous restrictions, since all variables are simultaneously determined in a static environment.

A more serious cause for concern is that the assumed Cholesky ordering (RXR, inflation, output) departs from the standard theoretical small-open economy model, as well as the standard recursive ordering in the empirical VAR literature on the effects of monetary policy shocks (output, inflation, RXR). The theoretical model by Svensson (2000), which is outlined in subsection 2.1, suggests the latter ordering.⁶ Intuitively, the RXR is an asset price and should therefore be allowed to respond contemporaneously to other variables. Examples of empirical VAR studies using the standard recursive ordering are those by Eichenbaum and Evans (1995) and Peersman and Smets (2003). Based on modern open-economy macro models and following standard practice in the empirical VAR literature on the effects of monetary policy, this paper uses the standard recursive ordering (output, inflation, RXR) rather than the reverse KR ordering (RXR, inflation, output).

Bolivia has a fixed nominal exchange rate against the U.S. dollar which is gradually adjusted by the central bank in response to economic conditions (crawling peg). In fact, Banco Central de Bolivia uses the nominal boliviano-dollar exchange rate as the main instrument of monetary policy. McCallum (2006) argues that “use of [the nominal exchange rate] as the policy-rule instrument rather than the more standard [interest rate], is perfectly sensible and coherent. Which of the two instrument/indicator variables would be more desirable will be determined by quantitative aspects of the economy under consideration” (pp. 7-8). Parrado (2004) and Leiderman, Maino and Parrado (2006) estimate monetary policy reaction functions with the nominal exchange rate as the policy instrument. Moreover, Jaramillo (2007) finds that the interest rate controlled by Banco Central de Bolivia has insignificant effects on output and inflation.

In the Bolivian case, the economic meaning of the RXR being ordered last is that Banco Central de Bolivia may change the nominal exchange rate contemporaneously in response to observed output and inflation. In a sticky-price environment, changes in the nominal exchange rate have short-run effects on the real exchange rate. However,

⁵ When studying the effects of RXR shocks, it is only the ordering of the RXR relative to the other variables which may affect the impulse responses of output and inflation.

⁶ A minor difference between the theoretical model and the empirical VAR models is that inflation is predetermined two periods in advance in the theoretical model, but only one period in advance in the empirical models.

the RXR should not be interpreted as a policy instrument. It is also assumed that the RXR does not have any contemporaneous effects on output or inflation.

An alternative empirical specification would be to use the nominal boliviano-dollar exchange rate instead of the RXR, which would allow a clearer interpretation of the residuals in the exchange rate equation as deviations from the central bank's instrument rule. A problem with such an approach is that the trade-weighted RXR may change even when the bilateral boliviano-dollar exchange rate is constant. This paper uses the trade-weighted RXR, which also facilitates a comparison with the rest of the literature, in particular the contractionary-depreciations literature.

As discussed above, the recursive-ordering identification method imposes zero-restrictions on the contemporaneous relationships between variables. In fact, several alternative methods have been suggested in the literature. For example, a recent paper by Bjornland (2008) allows contemporaneous two-way effects between the interest rate and the exchange rate, but adds the restriction that interest rate shocks have no effect on the long-run level of the real exchange rate. While alternatives to the recursive-ordering method are useful, the recursive method remains widely used, not least because of its simplicity. Among all possible recursive orderings, the standard ordering is preferable to alternative recursive orderings, such as that used by Kamin and Rogers (2000) and subsequent papers in the contractionary-depreciations literature.

6. Empirical analysis of the effects of real exchange rate shocks

The main empirical relationships of interest are those between the RXR and output and between the RXR and inflation. Figure 1 depicts the relationships graphically for the sample period 1990:Q1-2006:Q3. The trade-weighted RXR is defined such that an increase signifies a real depreciation. Output is the output gap in percent, which is calculated using an HP filter to remove the trend from the log real GDP. Inflation is the annualized log difference in the quarterly CPI. As can be seen in Figure 1, there is no clear relationship between the RXR and output, while RXR depreciations seem to be associated with increases in inflation.

In addition to the modifications of the Kamin-Rogers approach discussed in Section 5, this paper also differs in two other respects. First, while KR estimate a VAR in first differences, I estimate a VAR in levels. As pointed out by Sims, Stock and Watson (1990) and Hamilton (1994), if the true process is not a VAR in first differences, then estimates from a VAR in first differences will be inconsistent. My procedure avoids inconsistent estimates, but at the cost of reduced efficiency. Second, KR estimate a single VAR over four different exchange rate regimes. This may be problematic since

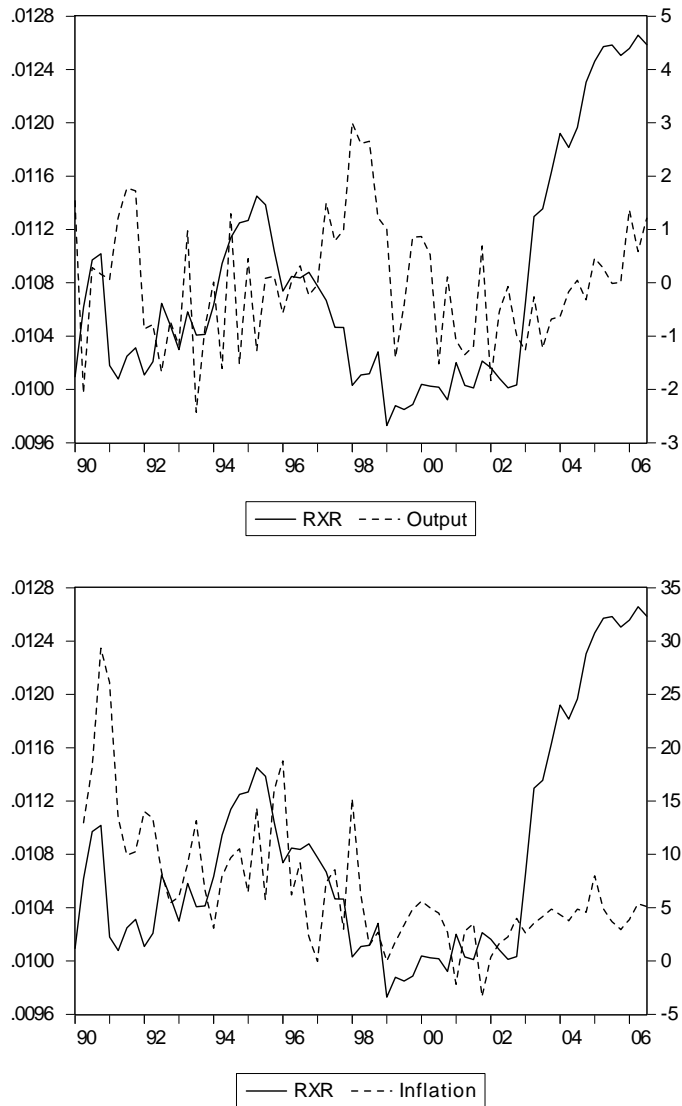


FIGURE 1. Upper graph: RXR (left scale, inverse index, increase=depreciation) and output (right scale, percent output gap using HP-trend). Lower graph: RXR (left scale, inverse index, increase=depreciation) and inflation (right scale, percent).

their Granger causality tests show that the relationship between the RXR and real GDP is different in different parts of the sample. In contrast, I use a sample with only one exchange rate regime. As demonstrated by Bagliano and Favero (1998) using US data, unless the VAR is estimated over a sample with a single monetary regime, the estimates may suffer from parameter instability.

Another issue is which measure of output to include in the VAR. Most empirical papers use the level of output but as argued by Giordani (2004), it is more consistent with the theoretical models to use the output gap. However, the output gap is difficult to measure since the level of potential output is unobservable. Following Lindé (2003) and Bjornland (2006), I use the level of output but include an exogenous linear trend. As a robustness check, I also estimate a model with a measure of the output gap.

Given the limited number of observations, it is necessary to limit the number of variables included in each VAR model. Following Kamin and Rogers, I first estimate a baseline model with the main variables and then estimate a number of alternative models with one additional control variable for each model. Detailed variable definitions and sources are given in the appendix. The exogenous variables which are included in all specifications are a U.S. interest rate and the trade-weighted external GDP. The main endogenous variables are output, CPI inflation and the trade-weighted RXR. The additional control variables are the terms of trade, the capital account balance and dummy variables for periods affected by social unrest and the weather phenomenon El Niño. I also check for robustness using a measure of GDP which excludes the mining and hydrocarbons sectors. While KR only investigate the effects of the exchange rate on aggregate GDP, I also study the effects on exports, imports, investment and consumption (inspired by Mojon and Peersman (2003)). All the following VAR models are estimated with two lags, as suggested by standard lag length criteria, and using the sample period 1990:Q1-2006:Q3.

Figure 2 presents the baseline impulse responses of output and inflation to a real exchange rate depreciation shock. The upper row uses CPI inflation and the lower row uses GDP deflator inflation. However, the results do not depend on the definition of inflation. There is a minor and gradual increase in output, but it is small and not statistically significant. In contrast, there is a significant and persistent increase in inflation, regardless of the definition of inflation. The peak effect on inflation is reached two quarters after the shock, and the increase in inflation remains significant during 1.5-2 years.

Figure 3 presents the impulse responses of output and CPI inflation to a real exchange rate depreciation shock when including a number of additional control variables (one for each row). The main results are very robust. There is no significant change in output for any specification, and the response of inflation is always positive, significant and persistent.

Figure 4 shows the impulse responses of GDP components: exports, imports, investment and consumption. There is a significant increase in exports and imports remain relatively unchanged. This creates a significant increase in net exports (not

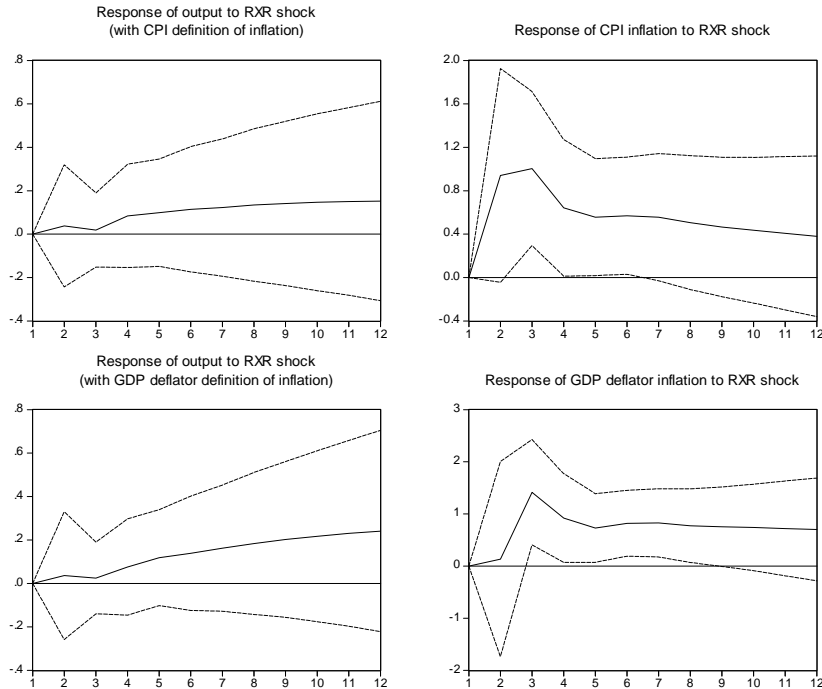


FIGURE 2. Impulse responses of output (percent) and inflation (percentage points) to RXR shock. Confidence intervals: plus/minus two Monte Carlo standard errors (500 repetitions).

shown). Thus, there is evidence of a standard expansionary effect of real depreciations. Furthermore, there is a significant decrease in investment, as predicted by the balance sheet channel. Consumption remains relatively unchanged. Combined with the evidence for exports and imports, the decrease in investment indicates that the standard positive effect and the negative balance-sheet effect tend to cancel each other out, which helps explain the lack of a significant response of aggregate GDP.

Figure 5 investigates how the baseline results change when using the output gap rather than the level of output. In general, the effects of RXR shocks are similar but less significant.

Finally, Figure 6 presents impulse responses for estimates based on the Kamin-Rogers recursive ordering. As compared to the results presented above, the responses of output and its components are similar. However, there are notable differences in the response of inflation. In all models presented above, inflation increases significantly in the short term. In contrast, with the Kamin-Rogers ordering of variables, there is an immediate *decrease* in inflation which is counter-intuitive. A possible justification for

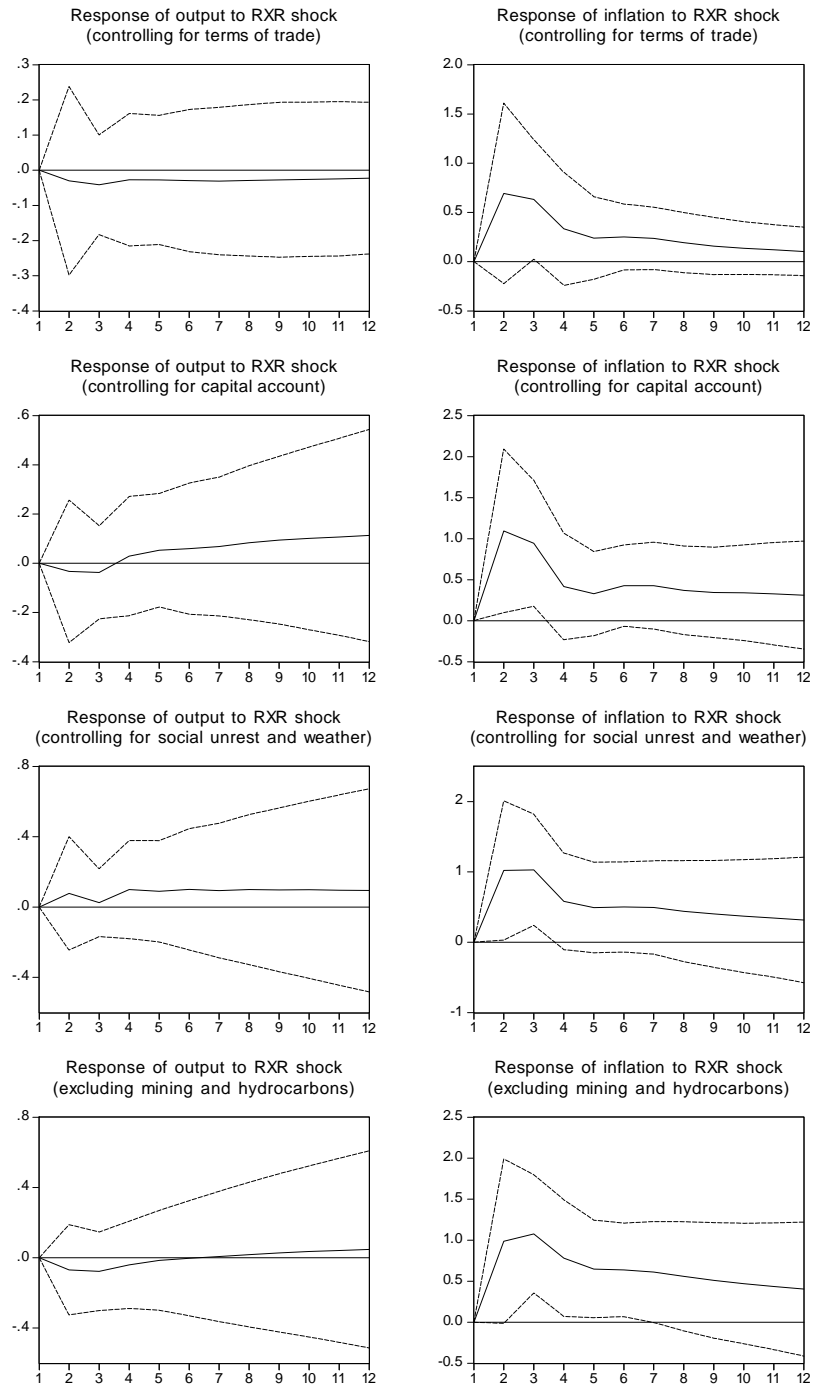


FIGURE 3. Impulse responses of output (percent) and inflation (percentage points) to RXR shock. Confidence intervals: plus/minus two Monte Carlo standard errors (500 repetitions).

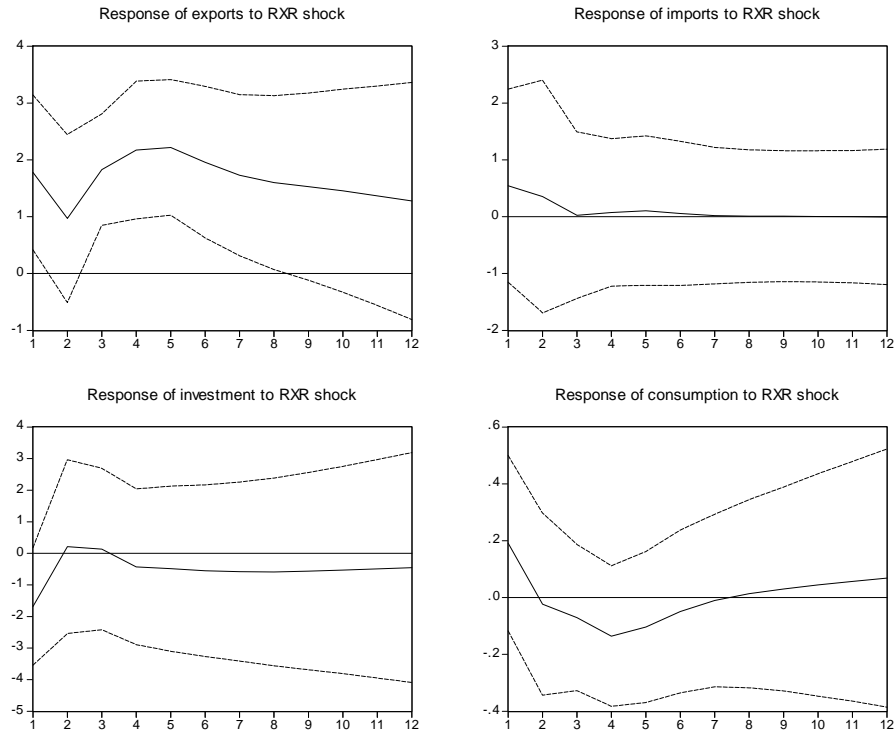


FIGURE 4. Impulse responses of exports, imports, investment and consumption (percent) to RXR shock. Confidence intervals: plus/minus two Monte Carlo standard errors (500 repetitions).

ordering the real exchange rate before inflation would be to allow for an immediate inflationary impact of depreciation. The observed deflationary impact of depreciation is puzzling and suggests model misspecification. Thus, the empirical results are different for different variable orderings, which shows the importance of using appropriate identification assumptions. However, the more general arguments in favor of the ordering used in this paper remain valid irrespective of how the results depend on identification assumptions in the specific case of Bolivia.

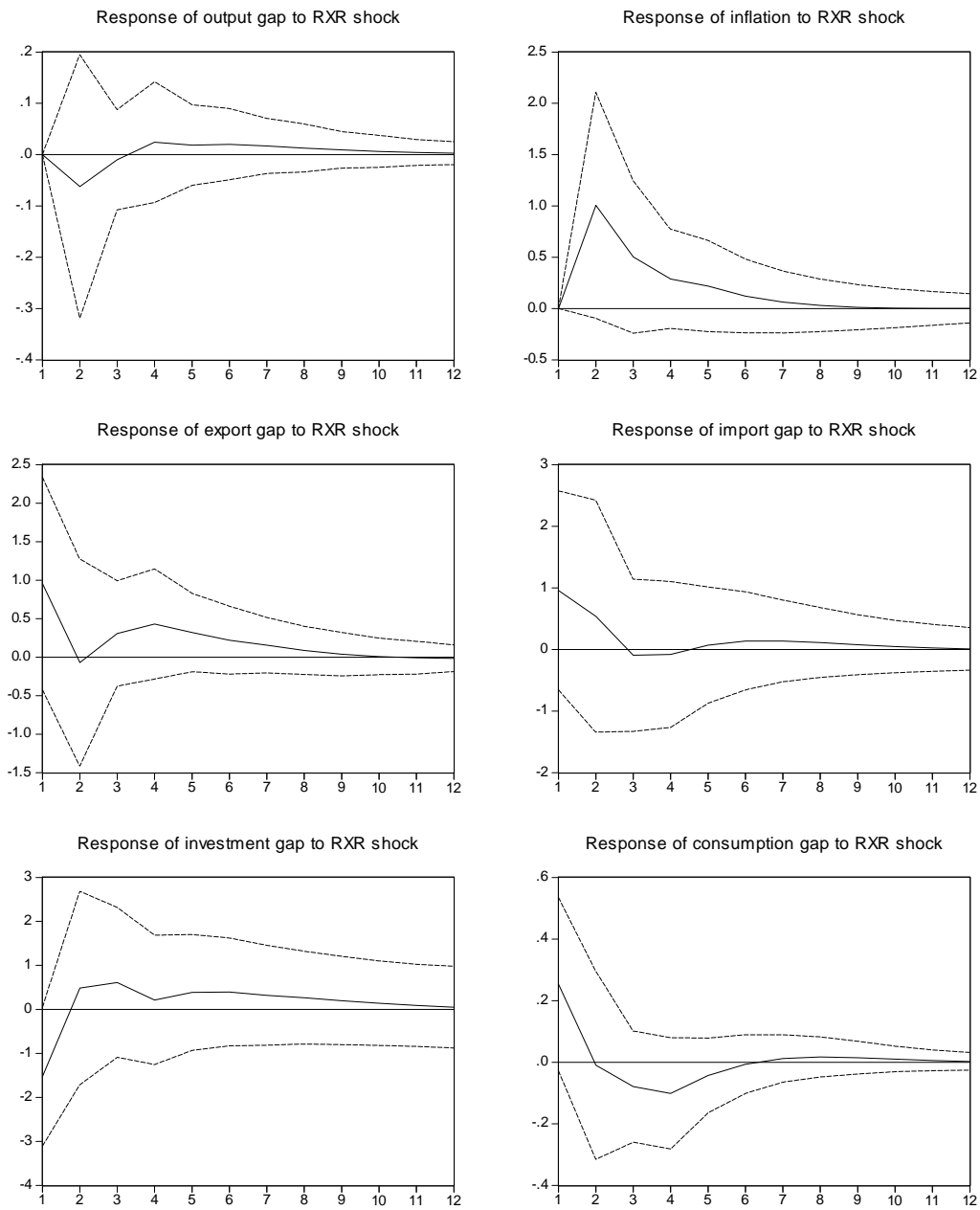


FIGURE 5. Impulse responses when using output gaps rather than output levels to RXR shock. Confidence intervals: plus/minus two Monte Carlo standard errors (500 repetitions).

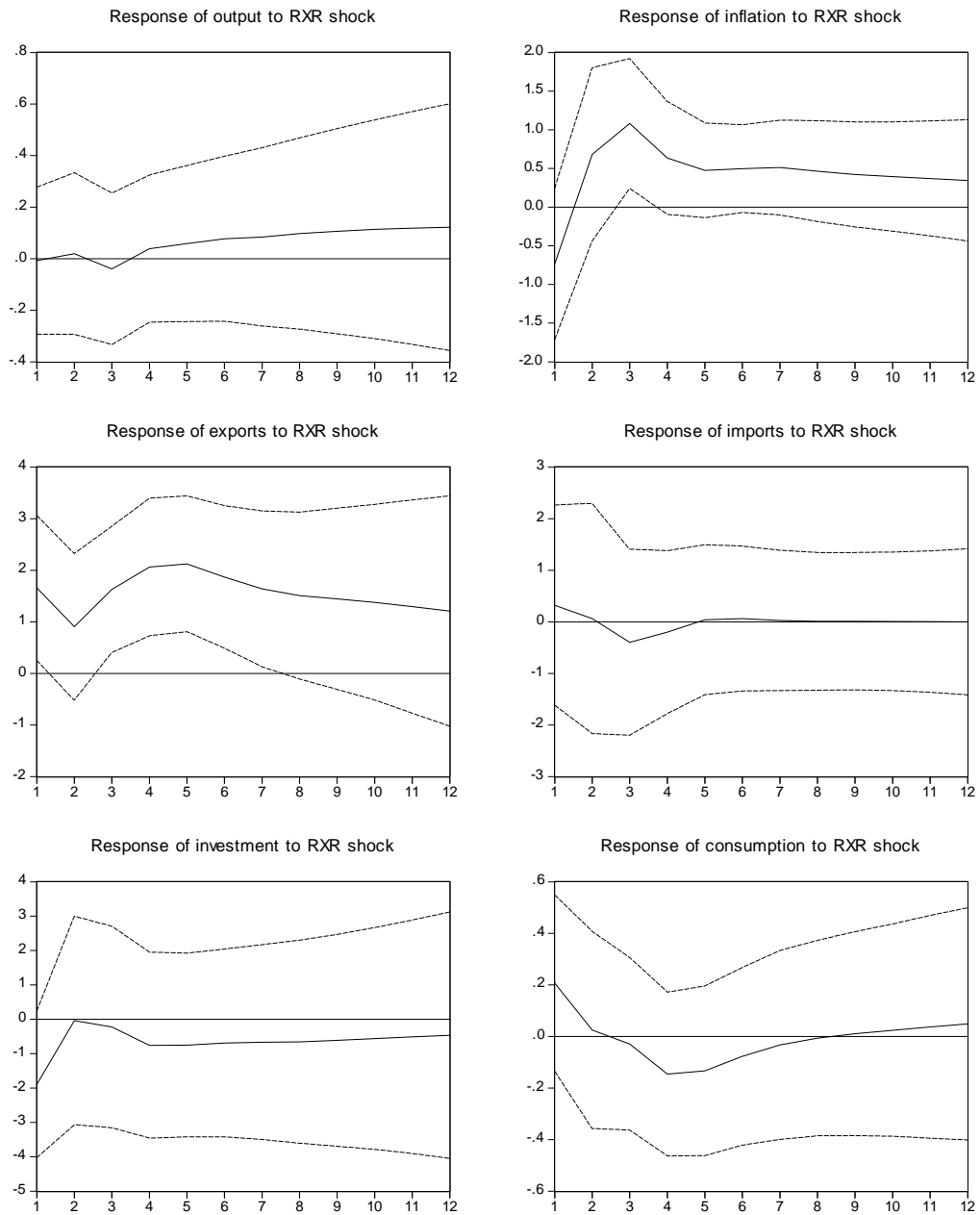


FIGURE 6. Impulse responses to RXR shock when using the Kamin-Rogers recursive ordering (RXR, inflation, output). Confidence intervals: plus/minus two Monte Carlo standard errors (500 repetitions).

7. Conclusions

As in most of the empirical literature, real exchange rate depreciations are found to be inflationary in Bolivia. However, depreciations are not contractionary, since the negative balance-sheet effects are not sufficiently large to outweigh the standard positive effects on international competitiveness. Thus, the adverse balance-sheet effects of currency depreciation are of limited size, even in an economy with extreme liability dollarization. Another result is that the identification assumptions affect the results, and that the recursive ordering used in this paper produces more reasonable results than the alternative ordering used in the previous contractionary-depreciations literature.

An interesting extension of the analysis in this paper would be to study countries with different degrees of liability dollarization to investigate if the strength of adverse balance-sheet effects varies with liability dollarization. It would also be interesting to study the importance of recursive-ordering assumptions for the results in a broader set of countries.

Appendix

The following variables were seasonally adjusted using the X-12 method (multiplicatively): GDP, consumption, investment, government spending, exports, imports, CPI and GDP deflator.

The variables used in the paper are defined as follows.

Output: real gross domestic product (GDP). Source: Banco Central de Bolivia (BCB).

Consumption: real private consumption. Source: BCB.

Investment: real gross fixed capital formation. Source: BCB.

Government spending: real government spending. Source: BCB.

Exports: real exports. Source: BCB.

Imports: real imports. Source: BCB.

The consumer price index (CPI) and the GDP deflator are also from BCB.

U.S. interest rate: nominal interest rate on 3-month Treasury Bills. A quarterly series was constructed by averaging monthly data. Source: Federal Reserve Bank of St. Louis.

Terms of trade: unit value of exports divided by the unit value of imports. Source: Instituto Nacional de Estadísticas.

Real exchange rate (RXR): multilateral trade-weighted RXR based on relative CPI. The original series is defined such that an increase in the series signifies a real appreciation. For pedagogical purposes, I inverted the series so that an increase means a real depreciation. Source: IMF International Financial Statistics.

Foreign output: export-weighted average of real GDP in the ten most important Bolivian export markets (Argentina, Belgium, Brazil, Chile, Colombia, Peru, Switzerland, Venezuela, the United Kingdom and the United States). The Bolivian export weights are from 2000. In the few cases where only annual real GDP was available, I used the same annual index value for all quarters of the year. Source: IMF International Financial Statistics (foreign GDP) and BCB (Bolivian export weights).

Capital account: net capital and financial account. Source: IMF Balance of Payments Statistics.

Dummy variable for social unrest: during quarters with substantial economic effects of social unrest, the variable takes the value 1, otherwise it takes the value 0. Source: Mendieta and Escóbar (2006).

Dummy variable for the weather phenomenon El Niño: during quarters with substantial economic effects of El Niño, the variable takes the value 1, otherwise it takes the value 0. Source: Mendieta and Escóbar (2006).

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Uncovered Interest Parity in a Partially Dollarized Developing Country: Does UIP Hold in Bolivia? (And If Not, Why Not?)

Ola Melander

ABSTRACT. According to the Uncovered Interest Parity (UIP) condition, interest rate differentials compensate for expected exchange rate changes, equalizing the expected returns from holding assets which only differ in terms of currency denomination. In the previous literature, there are many tests of UIP for industrialized countries, and, more recently, some tests for emerging economies. However, due to data availability problems, poorer developing countries have not been studied. This paper tests UIP in a partially dollarized economy, Bolivia, where bank accounts only differ in terms of currency denomination (U.S. dollars or bolivianos). I find that UIP does not hold in Bolivia, but that the deviations are smaller than in most other studies of developed and emerging economies. Moreover, several factors seem to contribute to the deviations from UIP. The so-called peso problem could possibly account for the observed data, but there is also evidence of a time-varying risk premium, as well as deviations from rational expectations.

1. Introduction

The Uncovered Interest Parity (UIP) condition states that interest rate differentials compensate for expected exchange rate changes, thereby equalizing the expected returns from holding any two currencies. It is a cornerstone assumption in open-economy macroeconomic models. Moreover, as shown in a recent paper by Adolfson, Laséen, Lindé and Villani (2008), imposing UIP matters for the quantitative results in dynamic stochastic general equilibrium (DSGE) models. The aim of this paper is to answer two questions: are there any deviations from UIP in Bolivia, and, if so, what explains these deviations?

⁰ The author gratefully acknowledges helpful comments and suggestions by Fernando Escóbar, Martin Flodén, Jesper Lindé, Pablo Mendieta, Juan Antonio Morales, and seminar participants at the Stockholm School of Economics and Banco Central de Bolivia. All remaining errors are mine. I am also grateful to Banco Central de Bolivia for data and hospitality, and Jan Wallander's and Tom Hedelius' Research Foundation for financial support.

There is an extensive empirical literature which tests UIP.¹ Ideally, empirical tests of UIP should use interest rates on assets which are identical in every respect except currency denomination. Otherwise, deviations from UIP can be due to “political risk”, i.e. deviations from Covered Interest Parity due to the presence or possible imposition of capital controls.² Data availability is not a problem for developed countries and emerging markets with euro-currency interest rates and/or forward exchange rates. Therefore, almost all previous papers study industrialized countries (and to some extent emerging markets) rather than poorer, developing countries.

The contribution of this paper is to fill this gap in the literature by studying a partially dollarized developing country, Bolivia, where deposits in different currencies only differ in terms of currency denomination and not in terms of political risk. The idea is to test UIP by using interest rates on assets in the same location (Bolivia) which only differ in terms of currency denomination (bolivianos and US dollars). This approach is analogous to the method used by Asplund and Friberg (2001) who test the Law of One Price by using prices on goods in the same location (Scandinavian duty-free stores) which only differ in terms of currency nomination (Swedish kronor and Finnish markka). In both cases, the idea is to use good data to construct a clean test of theoretical predictions.

A recent, similar paper by Poghosyan, Kocenda and Zemcik (2008) uses data from another partially dollarized developing economy (Armenia) to test UIP. My paper is different in a number of ways. First, I have a 50% larger data set with 12 years of weekly data. This diminishes the risk of small-sample bias. Second, Bolivia has had a fixed exchange rate regime (crawling peg) rather than a freely floating exchange rate. Previous papers have found differences in the extent of UIP deviations between countries with fixed and floating exchange rates, which makes it interesting to study different regimes. Third, I study the ability of several possible factors to explain the deviations from UIP (time-varying risk premia, the so-called peso problem and deviations from rational expectations).

The main finding of this paper is that UIP is rejected in the case of Bolivia. However, the rejection is less clear than in most previous studies of developed countries or emerging markets. In particular, there is no sign of any “forward premium puzzle”. Another finding is that several different factors seem to contribute to the deviations from UIP.

The rest of this paper is organized as follows. Section 2 presents the theory behind UIP as well as the previous empirical evidence. In section 3, I describe the most

¹ For literature surveys, see Froot and Thaler (1990), Engel (1996), Isard (2006) and Chinn (2006).

² See Aliber (1973) and Dooley and Isard (1980).

important candidate explanations for empirical deviations from UIP, with a special focus on methods for empirical testing. Section 4 presents the data set and the main empirical analysis, while section 5 empirically investigates alternative explanations for deviations from UIP in Bolivia. Finally, section 6 concludes the paper.

2. The UIP hypothesis and previous empirical tests

In subsection 2.1, I describe the UIP hypothesis and derive a regression equation which is often used for empirical testing. Then, in subsection 2.2, I summarize the previous empirical literature.

2.1. The UIP hypothesis. What is the relationship between interest rate differentials and expected currency depreciations? Perhaps the most well-known theoretical relationship is known as Uncovered Interest Parity (UIP). If investors are risk-neutral and have rational expectations, interest rate differentials should compensate for expected depreciations so that the expected returns from holding any two currencies are equal. More formally, the UIP hypothesis can be expressed as:

$$(2.1) \quad 1 + i_t = (1 + i_t^*) E_t \left(\frac{S_{t+k}}{S_t} \right)$$

where i_t and i_t^* are domestic and foreign interest rates with maturity k at time t , and S_t is the exchange rate at time t (expressed as domestic currency units per unit of foreign currency, so that an increase means a depreciation of the domestic currency). Taking logs of both sides of equation (2.1) gives :

$$(2.2) \quad \ln(1 + i_t) = \ln(1 + i_t^*) + \ln \left(E_t \left(\frac{S_{t+k}}{S_t} \right) \right).$$

Expected exchange rates are not directly observable, so it is not possible to use equation (2.2) as a basis for empirical testing. Assuming rational expectations, using the approximation that $\ln(1 + x)$ is close to x for small x and rearranging gives:

$$(2.3) \quad s_{t+k} - s_t = i_t - i_t^* + \varepsilon_{t+k}$$

where $s_t \equiv \ln(S_t)$ and ε_{t+k} is a rational expectations forecast error.³

Hence, in the regression:

$$(2.4) \quad s_{t+k} - s_t = \alpha + \beta(i_t - i_t^*) + \varepsilon_{t+k}$$

the coefficient values are $\alpha = 0$ and, in particular, $\beta = 1$, under the UIP hypothesis. In other words, a positive interest rate differential in favor of the domestic currency

³ The derivation ignores a Jensen's inequality term, which is very small empirically (see Engel (1996) for a discussion and references).

should, on average, be associated with a future depreciation of the domestic currency of equal magnitude. Ex-post returns from holding different currencies will differ, but only because of random rational expectations errors in the exchange rate forecasts.

An equivalent empirical specification could be obtained by replacing the interest differential on the right-hand side of equation (2.4) by the forward premium, i.e. the percentage difference between the forward and spot exchange rates. A forward premium means that the foreign currency is more expensive (sells at a premium) on the forward market than on today's spot market, i.e. the k -period log forward exchange rate, $f_{t,t+k}$, is higher than the current log spot exchange rate, s_t . The equivalence between the interest differential and the forward premium comes from Covered Interest Parity (CIP), which holds very well empirically (except perhaps during periods of financial market turmoil, as shown by, for example, Baba and Packer (2008)). CIP is an arbitrage condition and thus holds even if investors are not risk neutral.⁴

2.2. Previous empirical tests of UIP. Equation (2.4) has been estimated in a large number of studies, for different countries and time periods.⁵ In almost every study, the estimated β coefficient is significantly smaller than one, which is the value predicted by UIP. In fact, β is often estimated to be negative. Froot (1990) reports an average estimate across a large number of studies of -0.88, which is strong evidence against UIP. A negative β coefficient has a surprising economic interpretation. When the domestic interest rate is higher than the foreign interest rate, the domestic currency on average *appreciates* (rather than depreciates by enough to exactly offset the interest rate differential, as predicted by UIP). The common finding of a negative β coefficient is known as the “forward premium puzzle” or “forward discount bias”, since it implies that the forward market systematically mispredicts the direction of currency movements.⁶

⁴ Assume that CIP did not hold, for example that a positive forward premium ($f_{t,t+k} - s_t$) is smaller than a positive interest differential ($i_t - i_t^*$). In other words, the positive interest differential more than compensates for the expected depreciation of the domestic currency in the forward market. Then it would be profitable to borrow abroad at the low foreign interest rate, exchange the foreign currency into domestic currency, invest the money at the high domestic interest rate and sell forward the returns in domestic currency. The interest rate gain would be larger than the currency loss, thus making a riskless profit possible. Such riskless profit opportunities will be arbitrated away, regardless of investor risk preferences.

⁵ For surveys of the empirical literature, see Froot and Thaler (1990), Engel (1996), Isard (2006) and Chinn (2006).

⁶ In equation (2.4), let us replace the interest differential by the forward premium. Suppose that the forward market predicts that the domestic currency will *appreciate*. The forward premium term ($f_{t,t+k} - s_t$) is negative, so there is a forward discount. Then, a negative β coefficient implies that the (average) actual currency movement will be the opposite of the prediction of the forward market; the domestic currency will *depreciate*.

Chinn (2006) runs UIP regressions using G7 data for the period 1980-2000 and does not find any evidence that the puzzle is becoming less pronounced over time. However, UIP seems to hold better at longer horizons. Most early studies which rejected the UIP hypothesis used data from industrial countries with floating exchange rates. More recent work, for example that by Flood and Rose (1996) and Bansal and Dahlquist (2000), also studies countries with fixed exchange rates and emerging economies. Flood and Rose (1996) find that UIP holds better for fixed exchange rates than for floating exchange rates. The authors point out that there is no *theoretical* reason to expect any difference across exchange rate regimes, but that the potential *empirical* peso problem is only present in fixed exchange rate countries. The problem arises when the sample includes periods when the interest rate is high to compensate for a small-probability, major depreciation, but is too short to include such depreciations. Thus, to the extent that the peso problem is a significant cause of UIP deviations in countries with fixed exchange rates, we should expect UIP to hold better empirically in countries with floating exchange rates. The evidence to the contrary presented in their paper is therefore puzzling. In contrast, Flood and Rose (2001) find that UIP, if anything, holds better for floating exchange rate countries, and they do not find any significant differences between countries with different income levels. Bansal and Dahlquist (2000) find that the forward premium puzzle is only present in industrial countries where the interest rates are lower than the U.S. interest rate, and not in emerging markets. Frankel and Poonawala (2006) also find smaller deviations from UIP for emerging markets.

A recent paper by Poghosyan, Kocenda and Zemcik (2008) investigates whether UIP holds in Armenia. The authors use the fact that Armenian banks offer households a choice between domestic and foreign currency accounts. Such deposit accounts only differ in currency denomination and therefore provide useful interest rate data for tests of UIP. The authors find that UIP holds better than in many other studies, but there is evidence of positive and time-varying deviations from UIP, such that holders of domestic currency earn a higher return. The paper investigates whether an affine term structure framework and a GARCH-M methodology can produce such time-varying risk premia and obtain mixed results.

Some similar empirical work has been carried out at Banco Central de Bolivia. However, there are certain problems with the data and the empirical methodology. Morales (2003) estimates a version of the standard UIP regression with a yearly horizon using monthly data on domestic deposit rates for the period 1990-2003. He finds significant deviations from UIP. The main problem with the study is that Morales uses an average of interest rates across all available maturities, which is inconsistent

with the yearly horizon used in the empirical specification. Another problem is that the combination of a yearly horizon and monthly observations induces moving-average serial correlation into the residuals, which is not taken into account in the estimation (Newey-West standard errors must be used). Finally, both the interest rate and the exchange rate data are monthly averages. Daily data or at least weekly averages would be preferable.

Morales also investigates the ability of the Ize and Levy Yeyati (2003) model to account for dollarization in Bolivia since the early 1990's. The model assumes that UIP holds and that depositors' choice of currency portfolio depends on the volatilities of real returns in dollars and bolivianos. The model does poorly and Morales views this as evidence against the time-varying risk premium explanation of UIP deviations. Therefore, he interprets all deviations from UIP as stemming from peso problems.

Escóbar (2003) uses interbank interest rates and finds evidence of non-cointegration between the interest differential and depreciation, which is naturally very problematic for the UIP hypothesis. However, the empirical methods suffer from the same problems as those in Morales' paper.

3. Different explanations for the empirical deviations from UIP

As discussed in the previous section, there is abundant empirical evidence against UIP. But what are the underlying causes of the empirical deviations from UIP? There are three main explanations in the literature: the peso problem, time-varying risk premia and deviations from rational expectations.⁷

Before discussing these different explanations in more detail, it should be noted that the economic importance of deviations from UIP has been questioned in some recent papers. Burnside, Eichenbaum, Kleshchelski and Rebelo (2006) argue that deviations from UIP do not necessarily imply unexploited profit opportunities, and that statistically significant deviations are of little economic significance. Similarly, Sarno, Valente and Leon (2006) find evidence of nonlinear deviations from UIP, which is consistent with limits to speculation. Nevertheless, UIP is a key assumption in many macroeconomic models and a better understanding of empirical deviations from parity remains an important research challenge.

⁷ Other explanations are monetary policy responses to exchange rate changes (McCallum (1994)), endogenous asset market segmentation (Alvarez, Atkeson and Kehoe (2009)) and infrequent portfolio decisions (Bacchetta and van Wincoop (2008)).

3.1. The peso problem. The peso problem (named after the Mexican currency) has been prominent in policy discussions in Bolivia and is viewed as a likely explanation for UIP deviations. If the sample is short and includes periods when investors put a small, but positive, probability on a large depreciation, but does not include periods when such large depreciations actually occur, then the estimated beta coefficient will have a downward bias. Domestic interest rates will tend to be high to compensate for an expected depreciation, but an actual depreciation does not occur in the sample.⁸ Flood and Rose (1996) try to quantify the peso problem bias by testing UIP for two different samples of fixed exchange rate data: one full sample including realignment periods and one smaller sample excluding such periods. The estimated β coefficient for the full sample should not suffer from any bias, while the estimate using the smaller sample should be biased downwards due to the peso problem. By comparing the two estimated β coefficients, the authors estimate the peso problem bias to be -0.5.

Another possible explanation for UIP deviations is also related to expectations: gradual investor learning of exchange rate regime shifts, for example from fixed to floating. Lewis (1989) finds some evidence of such expectational errors, but notes that they do not seem to decrease over time, which contradicts the learning hypothesis.

3.2. Time-varying risk premia. A second possible explanation for UIP deviations is a time-varying risk premium which is correlated with the expected depreciation and thus with the interest differential. Any time-varying risk premium is part of the residual in the UIP regression and its correlation with the regressor causes the estimated beta coefficient to be biased. The issue is best understood by decomposing the interest rate differential into an expected depreciation and a risk premium.⁹ The decomposition can be expressed as $i - i^* = E(depr) + rp$, where $i - i^*$ is the interest rate differential, $E(depr)$ is the expected depreciation and rp denotes the risk premium. Using the expression to substitute for the interest differential in equation (2.4), the β coefficient can be expressed as follows:

$$(3.1) \quad \beta = \frac{Cov(depr, i - i^*)}{Var(i - i^*)} = \frac{Var(E(depr)) + Cov(rp, E(depr))}{Var(rp) + Var(E(depr)) + 2Cov(rp, E(depr))}.$$

First, suppose that the risk premium is constant ($Var(rp) = 0$), which implies that the covariance between the risk premium and expected depreciation is zero ($Cov(rp, E(depr)) = 0$). Then we have $\beta = 1$ and thus UIP holds. In contrast, suppose that $Cov(rp, E(depr)) < 0$ and $Var(rp) > |Cov(rp, E(depr))| > Var(E(depr))$. Under these conditions β is

⁸ See Rogoff (1980) and Krasker (1980) for early discussions of the peso problem and Lewis (1995) for a survey.

⁹ See Fama (1984) and Hodrick and Srivastava (1986) for more details.

negative and thus there is a forward premium puzzle. Investors demand such a large risk premium for holding risky high-interest rate currencies that those currencies are expected to appreciate rather than depreciate. Holders of the risky currencies are compensated both by higher interest rates and by currency appreciation.

There are three different methods that have been used to test the risk premium explanation. One approach for testing the risk premium explanation is to examine whether the predictable excess returns caused by the forward discount bias can be explained by the expected variance of future returns. Domowitz and Hakkio (1985) use an autoregressive conditional heteroscedasticity (ARCH) model to obtain a measure of the expected variance. A second approach is to use a fundamentals-based model of time-varying risk premia, as in, for example, Giovannini and Jorion (1989). Finally, a third method for testing the risk premium explanation is to obtain a survey-based measure of expected depreciation rather than to rely on ex-post depreciation as a proxy. Froot and Frankel (1990) use a survey measure of expected depreciation to decompose the estimation bias into a risk premium bias and an expectational bias. They find that although the risk premium varies over time, it is not correlated with the expected depreciation and hence it cannot cause a downward bias in β . Instead, there is evidence of systematic prediction errors.

3.3. Deviations from rational expectations. The third and final possible explanation for the deviations from UIP is deviations from rational expectations. Froot and Thaler (1990) discuss the hypothesis that at least some investors respond to interest rate differentials with a lag. The hypothesis is testable, since it predicts that not only current but also lagged interest differentials affect the exchange rate in a UIP regression. Froot (1990) presents supportive empirical evidence. Similarly, Chinn (2006) finds that UIP holds better when using survey-based measures of expected depreciation.

4. An empirical test of UIP in Bolivia - data and results

4.1. The data set. The data set has been compiled from the Monthly Bulletins of Banco Central de Bolivia. It consists of weekly averages for nominal deposit interest rates in Bolivia for the period April 1994 – November 2006 and daily observations on the boliviano-dollar exchange rate for the same period. There is a number of different deposit rates which differ in terms of currency denomination (bolivianos or U.S. dollars) and maturity range (1-30, 31-60, 61-90, 91-180, 181-360 or 361-720 days). Data is also available on interbank rates and sight deposit rates. However, as pointed out by Escóbar (2003), interbank and sight deposit transactions are primarily made for

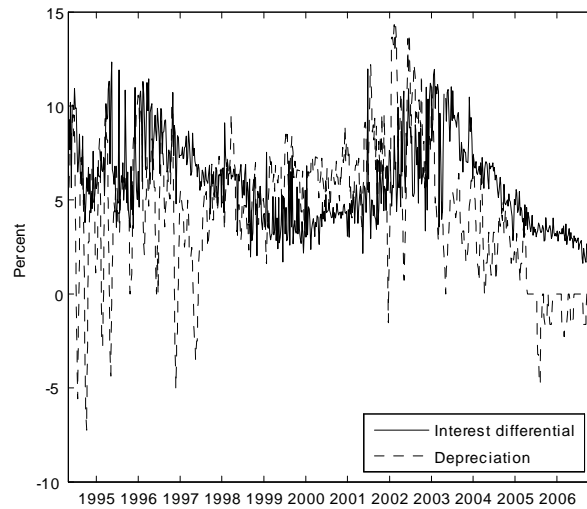


FIGURE 1. Deposit interest rate differential (boliviano-dollar) and depreciation of boliviano for the interest rate maturity range 1-30 days.

transaction rather than investment purposes, which makes such rates less relevant for testing UIP. In Bolivia, there is no forward market, so I use the interest differential specification rather than the equivalent forward discount specification.

Figure 1 presents the boliviano-dollar interest differential and depreciation for the interest rate maturity range 1-30 days. The interest differential between bolivianos and dollars is always positive, i.e. deposits denominated in bolivianos yield a higher interest rate. Moreover, depreciation is generally positive, so the value of the boliviano against the dollar falls over time.

Some data transformations are necessary before proceeding with the empirical testing. The interest rate data are weekly; each observation is a weekly average of the interest rates registered for all deposit transactions during the week. In contrast, the exchange rate data are daily. To get the same frequency for all series, I convert the daily exchange rate data to a weekly frequency by calculating the average exchange rate for each week.

Another issue is that the exact interest rate maturity for each maturity range is not observable in the data. For example, should we match the interest rate with a 31-60 day maturity with the depreciation for, say, 33, 48 or 56 days? I deal with this problem by examining individual deposit transaction data for 12 specific dates during the period 2001-2006 (end-May and end-November). First, for each date, I calculated the volume-weighted average maturity within each range (separately for boliviano and dollar deposits). Second, I calculated the average maturity within each

Table 1
Average interest rate maturity

		Maturity range (days)					
		1-30	31-60	61-90	91-180	181-360	361-720
BOB	Mean	30	52	85	160	294	414
	(Std dev)	(0)	(9)	(7)	(21)	(53)	(88)
USD	Mean	30	40	86	155	245	398
	(Std dev)	(0)	(10)	(8)	(27)	(73)	(1)
P-value for t-test		N/A	0.03**	0.85	0.87	0.97	0.04**
Average maturity (days)		30	46	85	158	265	406
Average maturity (weeks)		4	7	12	23	38	58

Note: the table presents average interest maturities for the different maturity ranges. The t-test p-value presented in the table is the p-value for a two-sided t-test for pairs of average maturities (weighted by transaction amount) for each currency, maturity range and date. The null hypothesis is that the pair of average maturities is generated from populations with the same mean. ** denotes significance at the 5% level.

range across the 12 dates (separately for bolivianos and dollars). Finally, for each range, I took the average of the boliviano and dollar maturities to get one single maturity for each range. The results are presented in Table 1. The data in the 1-30 day range are particularly reliable, since the transactions have exactly a 30-day maturity in all cases examined. In most other cases, the difference between the boliviano and dollar maturities is insignificant. The only exceptions are the maturity ranges 31-60 and 361-720 days, where the difference in maturity between boliviano and dollar interest rates is statistically significant at the 5% level. However, the estimated difference in maturity is only 10-15 days, so in quantitative terms the problem is not important.

Before proceeding to the estimation, I carry out a number of unit root tests for the following variables (for each maturity range): interest rate in bolivianos, interest rate in dollars and depreciation. The results are presented in Table 2. With the exception of the depreciation variables for the longer maturity ranges, the null hypothesis of a unit root is always rejected by at least some of the tests. It is possible that the non-rejection cases can be explained by a structural break in monetary policy, which would bias the results towards finding unit roots. Moreover, unit root tests have low power and often fail to reject a false null hypothesis of a unit root. In the following, I treat all variables as stationary.

4.2. Empirical results: does UIP hold in Bolivia? I do not use the standard UIP regression equation (2.4) since the approximation that x is close to $\ln(1 + x)$ only holds for small x , and interest rates in Bolivia have at times been quite high. Without the approximation and lagging by k periods, the following regression equation

Table 2
Unit root tests for interest rate and depreciation variables

Variable	P-values for unit root test					
	ADF CT	ADF C	ADF N	PP CT	PP C	PP N
BOB 1-30	0.00***	0.59	0.08*	0.00***	0.00***	0.06*
BOB 31-60	0.21	0.88	0.04**	0.00***	0.00***	0.20
BOB 61-90	0.05*	0.66	0.00***	0.00***	0.00***	0.10*
BOB 91-180	0.05*	0.08*	0.00***	0.00***	0.00***	0.06*
BOB 181-360	0.00***	0.31	0.03**	0.00***	0.11	0.28
BOB 361-720	0.29	0.15	0.08*	0.00***	0.10	0.31
USD 1-30	0.55	0.87	0.04**	0.07*	0.81	0.07*
USD 31-60	0.81	0.83	0.04**	0.00***	0.73	0.10
USD 61-90	0.42	0.75	0.10	0.02**	0.72	0.09*
USD 91-180	0.87	0.78	0.07*	0.43	0.67	0.11
USD 181-360	0.66	0.71	0.14	0.00***	0.50	0.14
USD 361-720	0.77	0.77	0.12	0.07*	0.60	0.19
Depr 1-30	0.53	0.37	0.19	0.00***	0.00***	0.00***
Depr 31-60	0.73	0.57	0.23	0.01**	0.00***	0.03**
Depr 61-90	0.83	0.74	0.32	0.17	0.07*	0.05*
Depr 91-180	0.86	0.75	0.29	0.88	0.75	0.25
Depr 181-360	0.95	0.92	0.39	0.96	0.92	0.38
Depr 361-720	1.00	1.00	0.43	0.99	0.98	0.42

Note: the table presents p-values for various unit root tests for the interest rate and depreciation variables for different maturities. BOB denotes the boliviano interest rate in Bolivia, USD is the US dollar interest rate in Bolivia and Depr is depreciation. The unit root tests are ADF (Augmented Dickey-Fuller) and PP (Phillips-Perron) using different assumption regarding the deterministic regressor (CT = constant and trend, C = constant and N = no constant or trend). ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

is obtained:

$$(4.1) \quad s_t - s_{t-k} = \alpha + \beta[\ln(1 + i_{t-k}) - \ln(1 + i_{t-k}^*)] + \varepsilon_t.$$

I present the OLS estimation results in Table 3. The estimated β is much lower than 1 for each maturity range and the UIP restriction $\beta = 1$ is always rejected. However, the estimated β coefficients are larger than those obtained in many other studies, where the coefficients are often negative (for developed countries) or close to zero (for emerging markets). The point estimates of β are positive in all cases. For the three shortest maturity ranges, the coefficients are significant at the 5% or 10% level, with point estimates of 0.31, 0.20 and 0.21, respectively. For the longer maturity ranges, the coefficients are closer to zero and insignificant. One possible explanation could be measurement error in the longer-maturity interest rate data (which would cause a downward bias).¹⁰

¹⁰ In contrast, Chinn (2006) finds that UIP fails less clearly at longer horizons (using data from developed countries).

Table 3
OLS estimation of standard UIP regression

	Maturity range (days)					
	1-30	31-60	61-90	91-180	181-360	361-720
α	2.76*** (0.84)	3.32*** (0.73)	3.37*** (0.72)	3.77*** (0.66)	3.59*** (0.65)	3.37*** (0.82)
β	0.31** (0.14)	0.20* (0.11)	0.21* (0.12)	0.13 (0.11)	0.10 (0.09)	0.02 (0.11)
P-value	0.00	0.00	0.00	0.00	0.00	0.00

Note: the estimated equation is $s(t)-s(t-k) = \alpha + \beta[\ln(1+i(t-k)) - \ln(1+i^*(t-k))] + \epsilon(t)$ where s is the log exchange rate (defined as bolivianos per dollar) and k is the interest rate maturity in weeks for each range (from Table 1). The depreciation variable was multiplied by $52/k$ to get an annualized measure corresponding to the annual interest rates. Newey-West standard errors (robust to heteroscedasticity and autocorrelation) are reported in parenthesis. The reported p-value is associated with the chi-square statistic for a Wald test of the restriction $\beta=1$ (which holds under UIP). ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

These results are clearly more favorable to UIP than those obtained for developed countries, where the coefficients are often negative. They are also more favorable than previous results for emerging markets. Bansal and Dahlquist (2000) pool data from a number of emerging market countries and estimate a coefficient of 0.19, but with a standard error of 0.19. In sum, UIP does not hold in Bolivia, but the deviations are smaller than in previous studies.

The question is whether the relative success of UIP in Bolivia is due to the high-quality data set – with better dollar interest rate data than in existing studies for emerging markets – or whether the results would be similar using more standard data for the dollar interest rate. In particular, would the results differ substantially if we used interest rates on certificates of deposit (CODs) in the United States instead of interest rates on dollar-denominated deposits in Bolivia? First of all, let us graphically compare the interest rate series in Figure 2. During most of the sample period, the Bolivian dollar-deposit interest rate is higher than the US COD interest rate, but since 2004 the opposite has been true. The correlations between the series are high, ranging from 0.73 for the shortest maturity to 0.80 for the longest maturity.

I re-estimate equation (4.1) using the US COD interest rate instead of the Bolivian dollar deposit rate. The results are presented in Table 4. The estimates are strikingly similar to those reported above, which indicates that political risk was not a significant cause of UIP deviations during the sample period. There is almost no difference in results for the shortest maturity range. For the longer maturity ranges, the coefficients on the interest rate variables are, if anything, somewhat more positive and significant with US COD rates. In contrast, Poghosyan, Kocenda and Zemcik (2008) find larger deviations from UIP in the cross-country case. The restriction $\beta = 1$ is rejected at

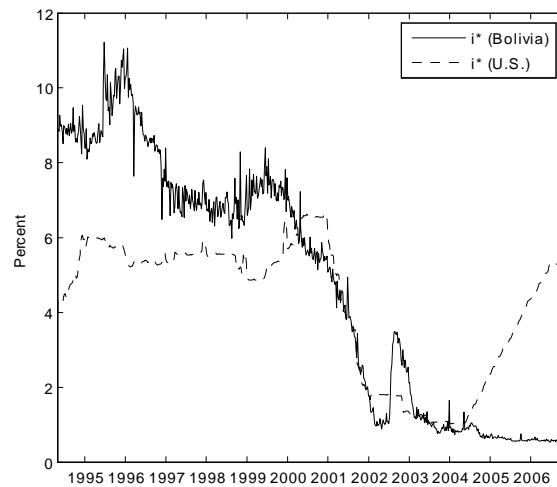


FIGURE 2. Interest rates on dollar-denominated deposits in Bolivia and Certificates of Deposit (CODs) in the United States (30-day maturities).

Table 4

**OLS estimation of standard UIP regression
with US COD dollar interest rate**

	Maturity range (days)		
	1-30	61-90	91-180
α	2.50*** (0.57)	2.47*** (0.67)	2.89*** (0.76)
β	0.32*** (0.08)	0.30*** (0.09)	0.23** (0.09)

Note: the estimated equation is $s(t)-s(t-k) = \alpha + \beta[\ln(1+i(t-k)) - \ln(1+i^*(t-k))] + \varepsilon(t)$ where s is the log exchange rate (defined as bolivianos per dollar) and k is the interest rate maturity in weeks for each range. See Table 3 for additional notes. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

the 1% level for all maturity ranges. The similarity of the results in Tables 3 and 4 suggests that it may be worthwhile to test UIP even in developing countries which are not partially dollarized and where it would be necessary to use US COD rates rather than local dollar-deposit rates.

5. Explaining the deviations from UIP in Bolivia

Previous tests of UIP in Bolivia do not investigate the underlying reasons for the empirical deviations from UIP. Morales (2003) attributes any deviations from UIP to the peso problem, but he does not test the explanatory value of different theories. This section studies the following three candidate explanations: the peso problem, time-varying risk premia and deviations from rational expectations.

5.1. How much can the peso problem explain? Suppose that investors put a small, but positive, probability on a major depreciation of the domestic currency and, therefore, require high interest rates as compensation, but suppose that no such major depreciations actually occur in the sample. Then, the estimated beta coefficient will have a downward bias due to a “peso problem” (as discussed in subsection 3.1).

In the Bolivian case, the interest rate differentials presented in Figure 1 are almost always positive, and no major depreciation occurs in the sample. On average, the interest differential in favor of the boliviano was more than sufficiently large to compensate for depreciation. Thus, on average, the deviations from UIP follow the prediction of the peso problem explanation.

Froot and Thaler (1990) carry out some simple calculations to informally evaluate the peso problem as an explanation for the dollar appreciation in the early 1980’s. It is instructive to perform a similar calculation for Bolivia during the sample period. In this period, the average depreciation of the boliviano was 4.5% per year. Suppose that the depositors expected this to be the “normal” depreciation rate, given no major depreciation, but that depositors put some probability on a very large depreciation of, say, 50%. The expected depreciation would be equal to the yearly probability π of a major depreciation times 50%, plus the yearly probability $(1 - \pi)$ of a small depreciation times 4.5%. Moreover, suppose that, in fact, UIP held perfectly during the sample period (i.e. any empirical deviations are only due to empirical peso problems). Then, the expected depreciation must be equal to the interest rate differential of 6%.

Given these assumptions, it is possible to solve for the yearly probability of a major depreciation by solving the equation:

$$(5.1) \quad 0.06 = \pi \cdot 0.5 + (1 - \pi) \cdot 0.045$$

which gives $\pi = 0.033$. The yearly probability of a major depreciation would be 3.3%, so the probability of not observing such an event in the sample would be $(1 - \pi)^{12} = 67\%$. This “p-value” is obviously not sufficiently low to reject the “null hypothesis” of the peso problem being responsible for all empirical deviations from UIP in Bolivia.¹¹ These calculations show that the peso problem alone might possibly account for the observed data. However, the calculation only shows that this possibility cannot be excluded, so it is important to also investigate the ability of other factors to explain deviations from UIP.

¹¹ Varying the size of the hypothetical depreciation between 30% and 70%, the “p-value” only varies between around 50% and 75%. Thus, the result is robust to the assumed size of the major depreciation.

Table 5
ML estimation of GARCH(1,1)-M model for 1-30 days

Coefficient (standard errors in parenthesis)					
α	β	γ	c	a	b
6.19***	-0.10***	-0.03***	0.85***	0.78***	0.22***
(0.14)	(0.03)	(0.00)	(0.17)	(0.09)	(0.07)

Note: the estimated mean equation is $s(t) - s(t-k) = \alpha + \beta[\ln(1+i(t-k)) - \ln(1+i^*(t-k))] + \gamma\sigma^2(t-k) + \varepsilon(t)$ where s is the log exchange rate (defined as bolivianos per dollar) and k is the interest rate maturity in weeks for each range (from Table 1). The variance equation is $\sigma^2(t-k) = c + a\varepsilon^2(t-k-1) + b\varepsilon^2(t-k-1)$. Bollerslev-Wooldridge standard errors (robust to non-normality in the residuals) are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

5.2. How much can time-varying risk premia explain? I use generalized autoregressive conditional heteroscedasticity in mean (GARCH-M) modeling to examine the explanatory value of a time-varying risk premium, modeled as the expected variance of future returns. This approach follows Domowitz and Hakkio (1985), Tai (2001) and Poghosyan, Kocenda and Zemcik (2008). The methodology can also be motivated empirically; for all maturity ranges, OLS regressions show clear signs of ARCH (not reported).

Specifically, I estimate a GARCH (1, 1)-M model.¹² I present the results for the maturity range 1-30 days in Table 5. All coefficients are highly significant and there are clear deviations from UIP. The restriction $\beta = 1$ is rejected at the 1% level for all maturity ranges. Moreover, the coefficient for the conditional variance in the mean equation (γ) is significantly negative, indicating the presence of a time-varying risk premium. Intuitively, when uncertainty increases (σ goes up), the depreciation of the boliviano decreases, thus causing the expected returns from holding bolivianos to increase. The increase in expected returns is required by agents to compensate for the additional risk involved in holding bolivianos when uncertainty is high.

The estimated risk premium consists of a constant part, α , and a time-varying part, $\gamma \cdot \sigma^2$. Wald tests reject the null hypothesis of no risk premium at the 1% level. The estimated premium is presented in Figure 3 and it is large, positive and time-varying throughout the sample period. Thus, there is clear empirical evidence of a time-varying risk premium, which could account for at least part of the deviations from UIP.

However, a puzzling result is that the estimated coefficient on the interest rate differential goes from significantly positive (in the standard UIP regression) to significantly negative (in the GARCH-M regression). If the deviation from UIP were caused by a downward bias in β due to an omitted risk premium, the estimated coefficient would be expected to increase rather than decrease when explicitly modeling the risk premium.

¹² Before making the estimation, missing values in the interest rate series were replaced by the average value of adjacent observations.

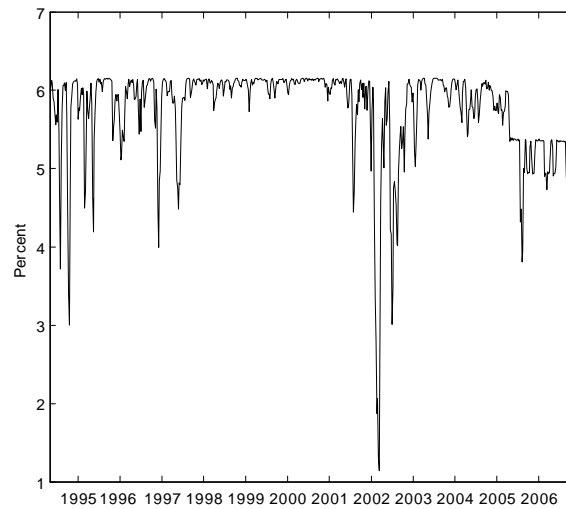


FIGURE 3. Estimated risk premium ($\alpha + \gamma \cdot \sigma^2$) for the interest rate maturity range 1-30 days.

Interestingly, Domowitz and Hakkio (1985) obtain the same result for the two countries (United Kingdom and Japan) for which they find a significant risk premium.

Another approach for dealing with possible endogeneity bias due to an omitted risk premium is to use instrumental variables (IV) methods rather than OLS. Instrumental-variables estimation of equation (4.1), using lagged interest differentials for the past 26 weeks as instruments for the current interest differential, gives similar results as OLS, but the β coefficients are generally somewhat higher (not reported). UIP is still rejected, but the deviations are smaller. Thus, the IV results indicate that time-varying risk premia may be partially responsible for the empirical deviations from UIP in Bolivia.¹³

5.3. How much can deviations from rational expectations explain? I test the non-rational expectations hypothesis presented in Froot and Thaler (1990). The hypothesis is that at least some investors respond with a lag to interest rate differentials. Table 6 presents the estimation results. There are no substantial changes in the estimated β coefficient for any maturity range. The coefficients on the lagged interest rate differentials are jointly significant at the 1% level in two cases (for maturity ranges

¹³ Another interpretation would be that the OLS estimates have a downward bias due to measurement error in the interest rate data. Interest rates are measured as weekly averages and interest maturity is not measured perfectly.

Table 6
OLS estimation of standard UIP regression with
lagged interest rate terms on the RHS
(test for deviations from rational expectations)

	Maturity range (days)				
	1-30	31-60	61-90	91-180	181-360
α	2.89** (1.23)	1.71 (1.22)	2.82*** (1.05)	-1.02 (1.09)	-0.21 (0.15)
β	0.20 (0.14)	0.33** (0.15)	0.30*** (0.10)	0.24 (0.16)	0.01 (0.03)
P-value	0.74	0.92	0.01	0.11	0.00

Note: the estimated equation is $s(t)-s(t-k) = \alpha + \beta[\ln(1+i(t-k)) - \ln(1+i^*(t-k))] + c(1)[\ln(1+i(t-k-1)) - \ln(1+i^*(t-k-1))] + \dots + c(26)[\ln(1+i(t-k-26)) - \ln(1+i^*(t-k-26))] + \varepsilon(t)$ where s is the log exchange rate (defined as bolivianos per dollar) and k is the interest rate maturity in weeks for each range (from Table 1). The depreciation variable was multiplied by $52/k$ to get an annualized measure corresponding to the annual interest rates. Newey-West standard errors (robust to heteroscedasticity and autocorrelation) are reported in parenthesis. The reported p-value is associated with the chi-square statistic for a Wald test of the significance of the coefficients for the lagged interest rate terms. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

61-90 and 181-360 days). Hence, there is also some evidence in favor of non-rational expectations. However, the estimated β coefficients are very similar.

6. Conclusions

The main finding of this paper is that while UIP does not hold in Bolivia, the deviations are smaller than in previous studies using data from developed or emerging economies. Moreover, several factors seem to have contributed to the observed deviations from UIP. The peso problem could possibly account for the observed data, but there is also evidence of a time-varying risk premium, as well as deviations from rational expectations.

Future research could test UIP with similar data from other partially dollarized developing economies. It would be particularly interesting to investigate whether a surprising result in this paper—that the UIP tests gave similar results irrespective of which dollar interest rate was used—holds more generally.

Another possibility would be to use pooled data from several partially dollarized countries rather than time-series data for separate countries. The drawback of the single-country approach used in this paper is that the estimates may be imprecise in small samples. Baillie and Bollerslev (2000) show that even when UIP holds, persistent exchange rate volatility might cause a wide dispersion of estimated β coefficients around the true value of one. However, the Bolivian data set consists of weekly data for a 12-year period, which gives a relatively large number of observations. Moreover, Flood and

Rose (2001) test for UIP in individual countries using large samples and still obtain estimation results which vary considerably between countries. They argue that this makes pooling – i.e. estimating a single β for all countries – a somewhat dubious procedure. There are also data availability problems; it is likely to be difficult to find high-frequency interest rate data of sufficiently similar maturity for a large number of countries.

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