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CROSS-COUNTRY VARIATION IN THE LIQUIDITY EFFECT: THE ROLE OF FINANCIAL MARKETS*

William D. Lastrapes and W. Douglas McMillin

This paper examines cross-country variation in the liquidity effect – the negative response of interest rates to money supply shocks – focusing on the role of financial factors in explaining this variation. We estimate the liquidity effect for each of 21 countries using VAR models in which money supply shocks are restricted to be neutral in the long-run, then regress the estimated liquidity effect on financial market variables across countries. We find that financial factors play an important role in determining the magnitude of the liquidity effect, and that this evidence is most consistent with generalised versions of limited-participation models.

Over the past decade, empirical macroeconomists have accumulated substantial time-series evidence supporting the existence of a 'liquidity effect' – the temporary, but persistent, negative response of interest rates to nominal money supply shocks.[1] This evidence is inconsistent with classical assumptions of price flexibility, perfect information and insignificant transactions costs – money supply shocks will be matched in this case by proportional price level responses, money will be neutral, and there will be no liquidity effect. However, the specific type of market rigidity that generates the observed liquidity effect is not yet well understood. Understanding the sources of the liquidity effect is important in distinguishing among alternative explanations of the monetary transmission mechanism, and thus helping determine the types of models best suited for the analysis of monetary policy.

Time series evidence alone is possibly not rich enough to explain the existence and magnitude of the liquidity effect completely. Cross-country variation in the liquidity effect is potentially informative in this regard but has not been fully examined. To our knowledge, no studies have systematically documented or compared the magnitude of the liquidity effect across countries. At best, extant comparisons in the literature are informal and qualitative in nature, and are made over only a handful of countries (e.g., the G-7 countries).

In this paper, we attempt to begin filling this void in the literature. We do so by estimating the liquidity effect for a larger country sample (21 countries) than other studies, using a common time-series technique and a careful empirical strategy for

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[1] A partial list of studies that find a liquidity effect in US data includes Christiano *et al.* (1999), Bernanke and Mihov (1998), Hamilton (1997), Strongin (1995), Lastrapes and Selgin (1995), Gordon and Leeper (1994) and Bernanke and Blinder (1992). Pagan and Robertson (1995) survey some of the earlier literature. Evidence from countries besides the US can be found in Kim (1999), Fung and Kasumovich (1998), Lastrapes (1998), Cushman and Zha (1997), Grilli and Roubini (1996), and Sims (1992).

appropriately identifying the liquidity effect. We then focus on the role of financial factors in explaining the observed cross-country variation in our measures of the liquidity effect. While our approach is limited by the availability of consistent interest rate data and the extent to which good proxies for financial market factors can be found, we believe that this paper takes a step forward in bringing international evidence to bear on the source of liquidity effects and the monetary transmission mechanism.

Institutional aspects of financial markets – transactions costs, the prominence and health of financial intermediaries, the efficiency of secondary markets, and so on – are likely to be important in any explanation of the existence and magnitude of the liquidity effect, regardless of the ultimate source of short-run monetary non-neutrality. We have in mind three broad classes of models that would predict a role for such factors in explaining the liquidity effect.

First, the state of credit markets and institutions are likely to have an important effect on the elasticity of money demand. For example, a highly developed banking system might allow a large number of substitutes for money, and thus promote a high interest rate elasticity. In this case, and in the presence of nominal rigidities in goods or labour markets, a given change in the supply of money would be associated with a relatively small liquidity effect. Thus, characteristics of financial markets can influence the liquidity effect even when they are not the ultimate cause of monetary non-neutralities.

Second, financial market factors can influence the liquidity effect in models of credit-market imperfections such as the bank-lending channel of monetary transmission (e.g. Bernanke and Gertler 1995; Bernanke and Blinder 1988). According to these models, which are based upon asymmetric information, changes in the money supply, by affecting the supply of bank loans, affect the yield spread between bank loans and bonds and thus the cost of funds for bank-dependent borrowers. Because spending will respond to changes in bank loan rates for a given bond rate, the bond rate response to a money supply shock (the liquidity effect) will be smaller when the bank-lending channel is operative than when it is not.

Finally, generalised versions of limited participation models imply a direct role for financial factors in explaining variation in the liquidity effect. The original versions of these dynamic general equilibrium models introduce rigidities on the saving behaviour of households. In particular, households can rebalance their portfolios only with a lag when money shocks occur – in effect they face infinite transactions costs of (immediate) portfolio adjustment. This rigidity limits the ability of some agents to participate in financial markets, thereby causing a liquidity effect when money is injected exogenously into those markets.[2] In Dotsey and Ireland's (1995) generalisation of the limited participation model of Christiano and Eichenbaum (1995), households have at their disposal a transactions technology that allows portfolio rebalancing at *finite* cost. Their model implies that

[2] Fuerst (1992), Lucas (1990) and Christiano (1991), based on the work of Rotemberg (1984) and Grossman and Weiss (1983), are the seminal studies in this literature. Christiano *et al.* (1997) finds weak and qualified support for these models.

the smaller the transactions costs in financial markets, the greater the opportunity household's have to react or adjust to money shocks, and the smaller is the magnitude of the liquidity effect required to clear the loanable funds market.[3]

The estimation procedure in our paper proceeds in two steps. First, using a time-series sample for each country, we employ standard vector autoregression (VAR) methods to estimate the liquidity effect, as described in Section 1 of the paper. To identify money supply shocks from the reduced form VAR, we rely on identifying restrictions implied by long-run monetary neutrality, a well-accepted stylised fact. In the second step in Section 2, we treat the liquidity effect as the dependent variable in a cross-country regression analysis. Financial market variables are the primary independent variables of interest, based on the motivation above, although we control for size of the money shock and other types of rigidities that may have explanatory power. We find that variables associated with financial intermediaries have substantial explanatory power for the cross-country variation in the liquidity effect. As argued later, the finding that the variables associated with financial intermediaries are important while broader financial market variables are not is more consistent with a limited participation model explanation of cross-country variation in the liquidity effect than with a bank-credit channel explanation. These results are robust to changes in the statistical model, nonparametric rank tests, and the inclusion of other type of rigidities and measures of the interest elasticity of money demand in the cross-country regressions.

1. Identifying and Estimating the Liquidity Effect

1.1. *The Empirical Model*

The first step of the empirical strategy is to use time series data to obtain estimates of the liquidity effect for each country in the sample. These estimates must be reasonable and convincing measures of the response of interest rates to exogenous money supply shocks, and must be consistently obtained across countries. This is a difficult, but not impossible task.

The VAR framework has been the most common means in the literature for estimating the liquidity effect, and we use this approach here. Although the approach has drawbacks, such as a lack of economic restrictions on the dynamics of the system (Cooley and Dwyer, 1998) and sensitivity to identifying restrictions (Pagan and Robertson, 1998; Faust and Leeper, 1997; Faust, 1998), it has the advantage of being able to capture general dynamic relationships and identifying economic interactions without the imposition of too much structure. And, as noted earlier, a large number of studies using VARs with alternative identification schemes have found a significant liquidity effect.

To identify the liquidity effect, we impose long-run monetary neutrality as the key identifying restriction. Long-run monetary neutrality is consistent with most

[3] Walsh (1998, p. 189) also notes that the magnitude of the liquidity effect should diminish as transactions costs in financial markets fall. In his comments on Dotsey and Ireland (1995), Kydland (1995, p. 1459), very much in line with our objectives, suggests that international evidence may be useful in understanding the role of transactions costs in driving liquidity effects.

macroeconomic models, and is generally accepted as being a good description of long-run behaviour. It is also general enough to accommodate monetary policies that differ greatly across the countries in our sample. As reported below, our estimated responses for all variables in the system generally and reasonably match prior expectations for these effects, which we interpret as lending support to the plausibility of the identifying restrictions and statistical specification. However, we also consider the robustness of our estimates to alternative identification assumptions and VAR specifications.

A general VAR model would contain all macro variables from all countries. Clearly, such a model would be over-parameterised given the available time series data. To make the VAR analysis tractable, we assume that the domestic variables of country i have no direct impact, either contemporaneously or lagged, on the economy of country j, for $i \neq j$. This restriction implies that the reduced form covariance matrix for the 'world-wide' VAR is block-diagonal in each country block. This block diagonal structure allows us to collapse the general VAR with variables from m countries into m separate, country-specific VARs. However, these restrictions do not rule out cross-country correlations among the variables in the system since we include a set of exogenous world aggregates in each VAR. Including these aggregates can pick up cross-country correlations through joint dependence on these variables. We also allow cross-country interactions by including a measure of the real exchange rate in each system.[4]

Thus, let $\mathbf{z}_{it}$ be an $n \times 1$ vector of country-specific macro variables (in first differences), including a nominal interest rate, for country i ($i = 1 \cdots m$), and $\mathbf{w}_t$ be an $h \times 1$ vector of exogenous world aggregates, presumably unaffected by economic activity in any particular country. Suppose the country-specific variables for country i are generated by the following structural model:

$$\mathbf{A}_{0i}\mathbf{z}_{it} = \mathbf{A}_{1i}\mathbf{z}_{it-1} + \cdots + \mathbf{A}_{pi}\mathbf{z}_{it-p} + \mathbf{B}_{0i}\mathbf{w}_t + \mathbf{B}_{1i}\mathbf{w}_{t-1} + \cdots + \mathbf{B}_{qi}\mathbf{w}_{t-q} + \mathbf{u}_{it}, \qquad (1)$$

for all i, in which $\mathbf{u}_{it}$ is an $n \times 1$ vector of country specific structural shocks, with E $\mathbf{u}_{it}\mathbf{u}'_{it}$ normalised to equal the identity matrix.[5]

For notational convenience, let the lag orders p and q be one. The reduced form of this structural model is then

$$\begin{aligned} \mathbf{z}_{it} &= \mathbf{A}_{0i}^{-1}\mathbf{A}_{1i}\mathbf{z}_{it-1} + \mathbf{A}_{0i}^{-1}(\mathbf{B}_{0i} + \mathbf{B}_{1i}\mathrm{L})\mathbf{w}_t + \mathbf{A}_{0i}^{-1}\mathbf{u}_{it} \\ &= \mathbf{\Pi}_{1i}\mathbf{z}_{it-1} + \mathbf{\Pi}_{2i}(\mathrm{L})\mathbf{w}_t + \boldsymbol{\varepsilon}_{it}, \end{aligned} \qquad (2)$$

where $\mathrm{E}\,\boldsymbol{\varepsilon}_{it}\boldsymbol{\varepsilon}_{it}' = \mathbf{\Sigma}_i = \mathbf{A}_{0i}^{-1}\mathbf{A}_{0i}^{-1'}$. This reduced form is also the VAR representation of the $\mathbf{z}_{it}$ process. Conditional on the block-diagonal restrictions, the coefficients in

[4] Not only does the block-diagonal structure preserve degrees of freedom, it eliminates passing misspecification errors from one country to all others. Note that any model of a 'closed' economy implicitly makes such a set of restrictions.

[5] In effect, we assume that the joint system $[w_t\ z_t]$ is block exogenous in w (Hamilton 1994, pp. 311–3). This is a standard assumption in most empirical studies of small, open economies, e.g. (Ahmed and Park, 1994; Cushman and Zha, 1997). Since in this study we are interested only in the response of domestic variables to money supply shocks, but not world shocks, we do not compute the response of z to shocks in w_t.

(2) are efficiently estimated by ordinary least squares applied to each equation in each country-block.[6]

Our objective is to use the estimated VAR in (2) to identify the responses of the macro variables in each domestic economy, interest rates in particular, to the economy's own money supply shocks. As noted, we achieve this identification by invoking long-run monetary neutrality; to wit, permanent shocks to the nominal money supply in each country have no impact on real variables in that country at the infinite horizon.

Suppose that each vector $\mathbf{z}_{it}$ contains real variables in the first $n - 1$ elements and the nominal money stock as the final variable. Furthermore, define the final element in $\mathbf{u}_{it}$ vector as an unpredictable shock to money supply behaviour. Solving the difference equation system in (2), and dropping the country notation for convenience, yields two interpretations of the moving average representation of $\mathbf{z}_t$:

$$\begin{aligned}\mathbf{z}_t &= (\mathbf{I} - \mathbf{\Pi}_1\mathrm{L})\mathbf{\Pi}_2(\mathrm{L})\mathbf{w}_t + (\mathbf{I} - \mathbf{\Pi}_1\mathrm{L})^{-1}\boldsymbol{\varepsilon}_t \equiv \mathbf{G}(\mathrm{L})\mathbf{w}_t + \mathbf{C}(\mathrm{L})\boldsymbol{\varepsilon}_t \\ &= (\mathbf{I} - \mathbf{\Pi}_1\mathrm{L})\mathbf{\Pi}_2(\mathrm{L})\mathbf{w}_t + \mathbf{C}(\mathrm{L})\mathbf{A}_0^{-1}\mathbf{u}_t \equiv \mathbf{G}(\mathrm{L})\mathbf{w}_t + \mathbf{D}(\mathrm{L})\mathbf{u}_t,\end{aligned} \tag{3}$$

where $\mathbf{C}(\mathrm{L}) = (\mathbf{I} + \mathbf{C}_1\mathrm{L} + \mathbf{C}_2\mathrm{L}^2 + \cdots)$, and $\mathbf{D}(\mathrm{L}) = (\mathbf{D}_0 + \mathbf{D}_1\mathrm{L} + \mathbf{D}_2\mathrm{L}^2 + \cdots)$. The reduced form dynamic multipliers, $\mathbf{C}(\mathrm{L})$, are obtained directly from estimation of the VAR. However, the parameters of interest – the dynamic responses to money supply shocks – are contained in final columns of $\mathbf{D}(\mathrm{L})$.

Long-run monetary neutrality allows us to identify the parameters of interest from the estimated reduced form coefficients $\mathbf{C}(\mathrm{L})$ and $\mathbf{\Sigma}$. The restriction sets the elements of the final column of $\mathbf{D}(1) \equiv \sum_{i=0}^{\infty}\mathbf{D}_i$ (the set of infinite-horizon multipliers on the *levels* of the endogenous variables) to zero, except for the final element. Thus, a money supply shock is defined to have a permanent effect on the nominal money stock but no permanent effect on the other (real) variables in the system. Under these restrictions, $\mathbf{D}(1)$ is uniquely identified as the Cholesky factor of $\mathbf{C}(1)\mathbf{\Sigma}\mathbf{C}(1)'$, and $\mathbf{D}(\mathrm{L}) = \mathbf{C}(\mathrm{L})\mathbf{C}(1)^{-1}\mathbf{D}(1)$.[7] We take the identified dynamic response of the interest rate, after accumulating the response functions to measure the level response, and net of the effects on anticipated inflation, as our measure of the liquidity effect of a money supply shock.

1.2. *Estimating the Dynamic Responses*

Our sample comprises quarterly data for 21 developed countries, all but one of which are members of the OECD, over the period 1970:1 to 1998:4.[8] The sample period begins roughly after the post-war fixed exchange rate period, and ends prior to the

[6] The 'world-wide' VAR is a system of seemingly unrelated regressions with different right-hand-side variables in each country-block. However, because we restrict the covariance matrix of this system to be block diagonal, OLS is an efficient estimation technique; see Theil (1971, p. 309).

[7] Because the Cholesky decomposition imposes a lower triangular structure on $\mathbf{D}(1)$, it appears that more than just long-run neutrality has been imposed. However, Lastrapes (1998) shows that the identified money supply response coefficients are independent of these additional restrictions. Blanchard and Quah (1989) and Shapiro and Watson (1988) pioneered the long-run restriction approach to identifying VAR models.

[8] The sample period used in estimation differs for some countries due to data availability, as noted below.

introduction of the euro. The number of countries included in our sample is limited by the availability of reliable and comparable interest rate data over the period.

We consider the following four world variables to include in w : aggregate world output (total gross domestic product in constant prices, seasonally adjusted: OECD *Main Economic Indicators,* OCDRGDPS), the aggregate world price level (consumer price index, all items, OECD total: OECD MEI, OCDCPILT), the nominal price of oil (PPI, crude petroleum: DRI/Citibase, PW561), and a nominal commodity price index (CRB spot market index, all commodities: DRI/citibase, PSCCOM). The vector of domestic variables, $\mathbf{z}_t$, contains a nominal interest rate, output, the real exchange rate, real money balances, and the nominal stock of money. With certain exceptions, most of these data come from the *International Financial Statistics* (IFS) database. In most cases, output is proxied by the industrial production index, the price level (used to compute real money balances and the real exchange rate) is the CPI, the real exchange rate is the domestic-currency value of SDRs times the world CPI divided by domestic CPI, and nominal money is M1. This choice of variables is dictated primarily by data availability but is reasonable in light of our objectives and the need for consistency across countries. We provide complete definitions and sources of the country-specific data in an Appendix that is separately available (http://www.terry.uga.edu/people/last/personal/research.html).

Given our focus on the liquidity effect, we use a measure of short-term yields (1-month maturity or less) for the nominal interest rate.[9] Over the sample period, Switzerland has on average had the lowest short-term rates (3.33%), while Korea has had the highest (14.09%). Interest rates have been most variable in South Africa, with a standard deviation of 5.18%, and least variable in Austria, with a standard deviation of 2.09%. The overall mean short-term rate is 9.16%, with standard deviation of 2.81%.[10]

For each country, we estimate the VAR in (2) as described in the previous subsection, over the sample period given in the third and fourth columns of Table 1. The actual estimation period begins six periods after the first available observation to account for differencing and lags. All variables but interest rates are transformed into natural logs, and all variables are first-differenced prior to estimation.[11] In the interest of parsimony, we initially estimated systems excluding world variables. For most countries, the world aggregates were not needed to generate impulse response functions consistent with prior beliefs about the dynamic effects of positive money supply shocks – short-run increases in output and real money balances, and a permanent increase in the price level. For some countries, however, the world aggregates were ultimately included because the

[9] Our inclusion of the nominal rate of interest in the model is not inconsistent with our method of imposing long-run monetary neutrality. In the face of one-time changes in the *level* of the money supply, not its growth rate, the nominal rate will mimic the real rate at the infinite horizon since expected inflation will be unaffected at that horizon.

[10] The interest rate series for Ireland and Sweden exhibit large spikes in 1992; however, dropping these potential outliers alters none of our main findings below. Plots of the interest rate data we use are available in the separate Appendix.

[11] We deal below with the possibility of model misspecification due to cointegration.

Table 1
Estimated Liquidity Effects

	Country	Begin	End	r_1	rank	r_m	rank	r_c	rank
1	Australia	70:1	96:2	−33.98	17	−33.98	20	−22.06	17
2	Austria	70:1	98:1	−29.45	19	−67.18	15	−8.41	21
3	Belgium	70:1	98:1	−186.61	1	−186.61	1	−114.55	1
4	Canada	75:1	98:1	−131.44	2	−131.44	5	−74.00	5
5	Denmark	72:1	98:1	−93.78	9	−143.82	3	−64.11	7
6	France	70:1	98:1	−122.81	4	−122.81	7	−62.70	8
7	Germany	70:1	98:1	−45.55	16	−59.95	17	−14.26	19
8	Ireland	73:1	98:1	−81.76	10	−123.30	6	−65.80	6
9	Italy	71:1	98:1	−106.13	6	−106.85	10	−88.50	2
10	Japan	70:1	98:1	−74.72	11	−74.72	14	−31.91	12
11	Korea	76:4	97:4	−63.40	13	−134.63	4	−46.80	10
12	Netherlands	70:1	97:4	−50.89	15	−61.02	16	−29.51	14
13	New Zealand	70:1	98:1	−97.89	7	−97.89	11	−12.50	20
14	Norway	70:1	98:1	−96.30	8	−96.30	12	−78.83	3
15	Portugal	81:1	98:1	−3.85	20	−32.95	21	−17.59	18
16	South Africa	70:1	98:1	−73.23	12	−81.02	13	−51.19	9
17	Spain	74:1	98:1	−121.54	5	−121.54	8	−45.34	11
18	Sweden	70:1	98:1	−59.21	14	−59.21	18	−27.17	15
19	Switzerland	75:4	98:1	55.84	21	−116.45	9	−26.45	16
20	United Kingdom	70:1	98:1	−123.57	3	−157.66	2	−74.74	4
21	United States	70:1	98:1	−31.12	18	−37.73	19	−30.01	13
	μ			−74.83		−97.48		−46.97	
	σ			52.20		42.58		28.80	

Notes: Dependent variables in the cross-country regressions, estimated over given sample period. r_1 is the response of the real interest rate at horizon 1, r_m is the maximum (absolute) response, and r_c is the average response over eight quarters, all to a unit money supply shock. μ is the mean and σ is the standard deviation.

estimated responses from the parsimonious systems (without the aggregates) indicated that money supply shocks were likely misidentified.[12]

Our baseline impulse response function estimates are from VARs with five lags of the country-specific variables (i.e. $p = 5$), and a constant and seasonal dummies as deterministic variables. For Japan, New Zealand, Portugal and Spain, contemporaneous and lagged values of the world aggregates are included as exogenous variables. France requires, in addition, a linear trend term to generate a positive output response. The VARs for the remaining countries contain no world aggregates. Of the 105 equations estimated (5 variables for 21 countries), the Q-test for residual serial correlation is significantly different from zero at a 5% level for only three equations. We consider how sensitive the response functions are to alternative specifications below.

Figures 1, 2, and 3 report for the baseline model the estimated (accumulated) dynamic responses of output, the price level and real money balances to a positive money supply shock, according to the long-run identification scheme. The responses are plotted up to a horizon of 40 quarters, and are shown with standard

[12] For example, in a few cases for the initial systems we found negative responses of output and real money, and positive responses of the price level, which suggested a negative aggregate supply shock rather than a positive money supply shock.

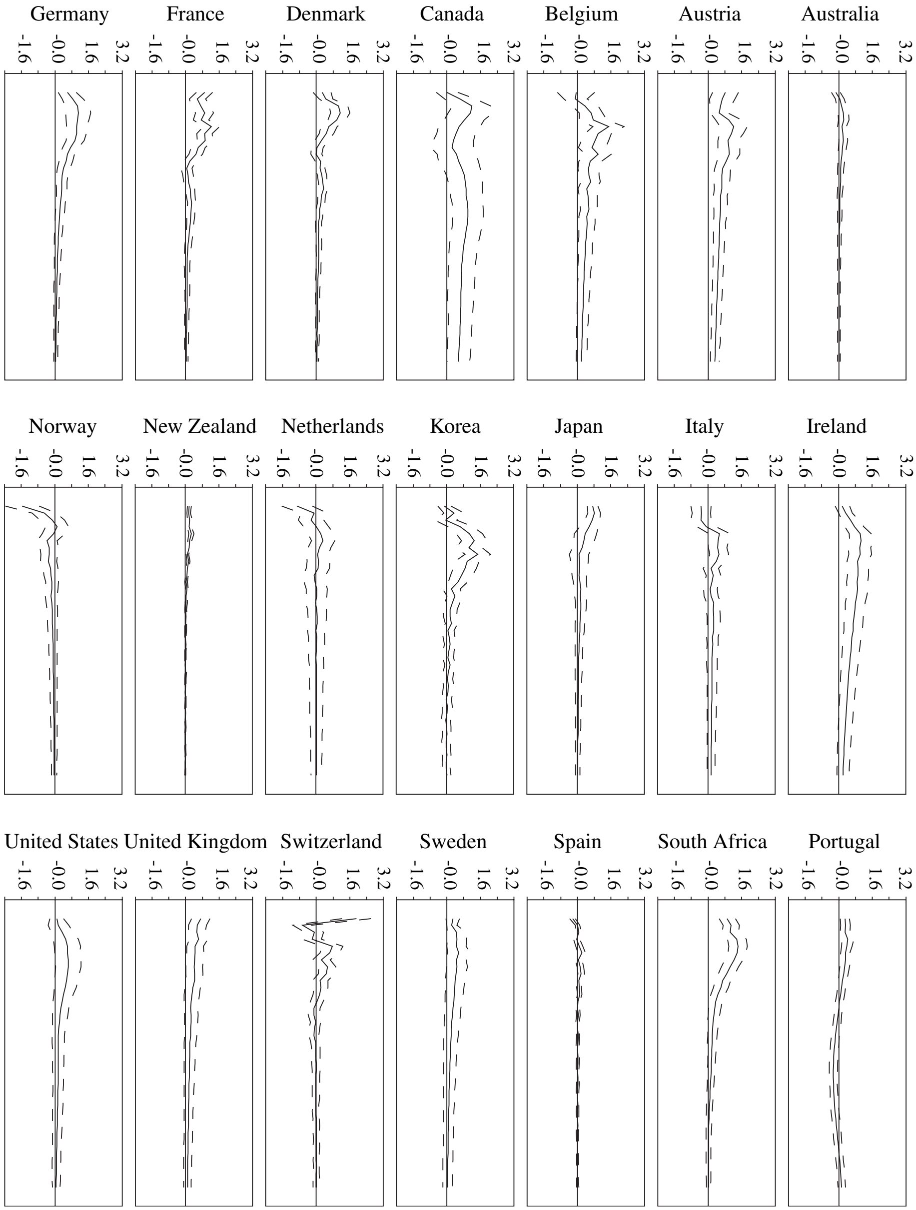

Fig. 1. *Response of Output to Money Supply Shocks*

error bands computed from a standard antithetically-accelerated Monte Carlo integration with 5,000 replications (dashes). All responses are normalised on the estimated standard deviation of the country-specific money supply shock; i.e. the coefficients reflect the dynamic response to a unit money supply shock, rather than a standard deviation shock as is conventional, to standardise the size of the shock across countries.

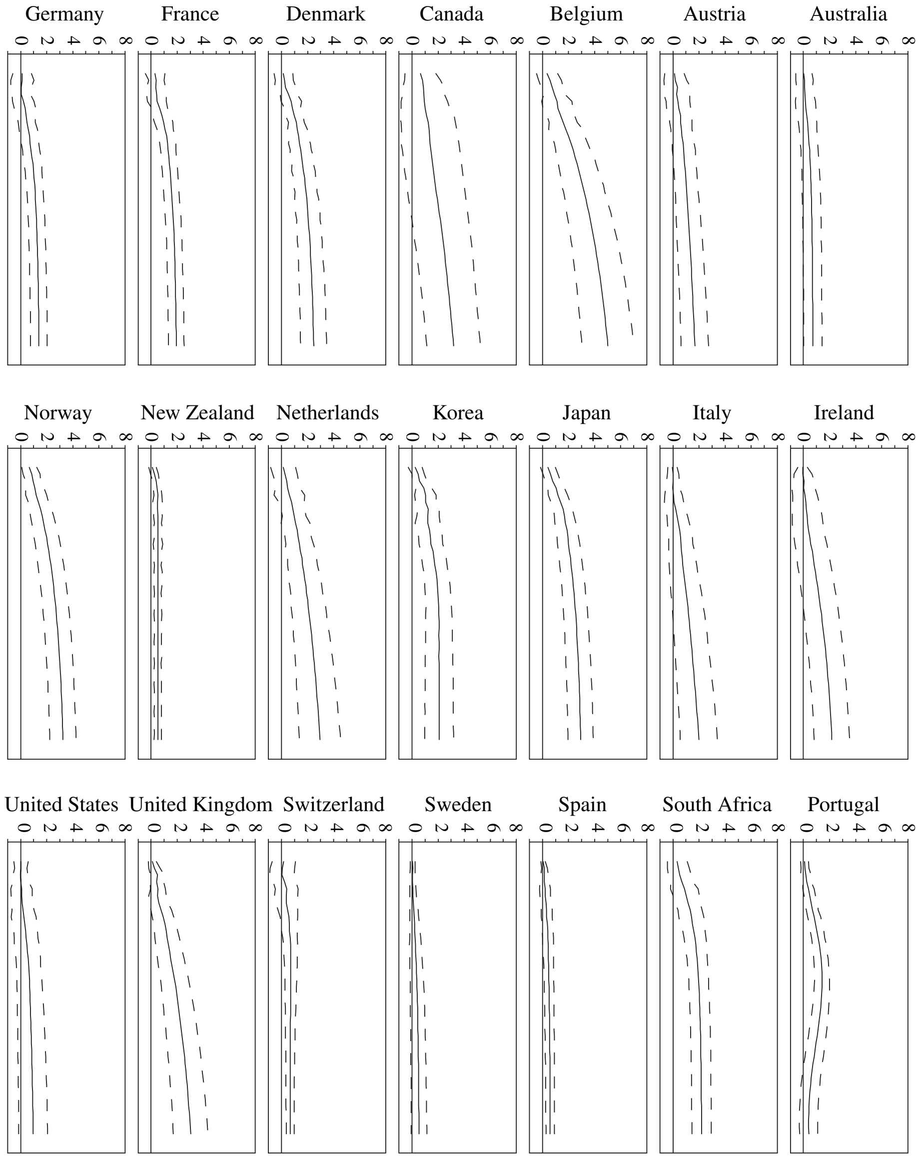

Fig. 2. *Response of Price Level to Money Supply Shocks*

The estimated dynamic responses are qualitatively similar across the countries in the sample. The price level shows a small response on impact, then gradually increases to a new, higher steady-state value. Output generally rises in the short run but returns to its steady-state value in the long run (by the assumption of monetary neutrality). Only in the Netherlands and Norway do the point estimates provide no convincing evidence of a temporary positive output response. In general, real money balances rise in the short run, which indicates that the price level response is smaller than the nominal money stock response in the short run. Only in

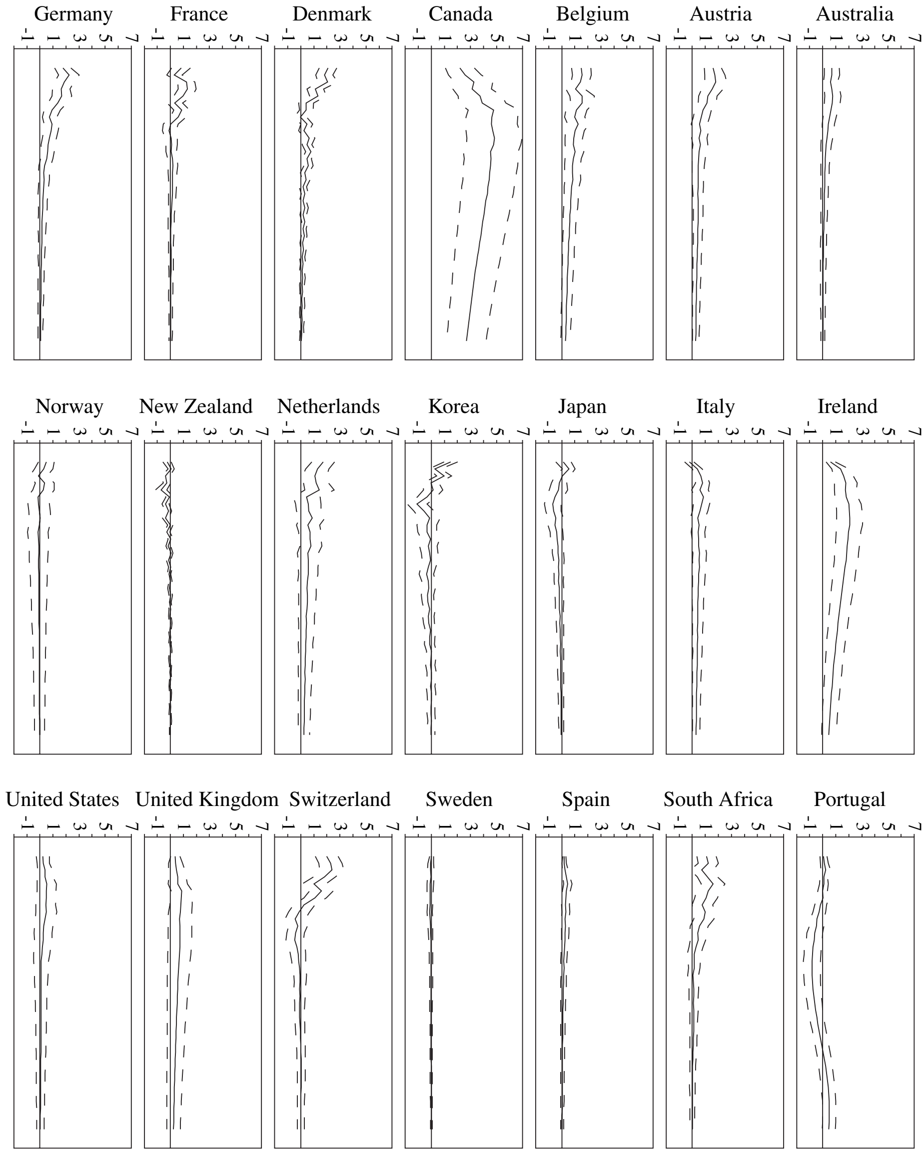

Fig. 3. *Response of Real Money to Money Supply Shocks*

New Zealand do the results suggest the possibility of a short-run negative impact on real money balances.

As noted earlier, we consider the response of the real interest rate to money supply shocks as our measure of the liquidity effect. The real interest rate response cannot be estimated directly from our model, since only the nominal rate is observable and included in the VAR. However, it can be inferred from the nominal rate response and the price level (P) response (the latter of which is simply the difference between the nominal money and real money responses), as in Gali (1992) and Lastrapes (1998). Let k denote the forecast horizon of the dynamic response functions and $\pi_{h,t+k}$ denote the rate of inflation at time $t+k$ over the following h quarters; i.e. $\pi_{h,t+k} \equiv (1/h)(\ln P_{t+k+h} - \ln P_{t+k})$. Then,

$$\frac{\partial \pi_{h,t+k}}{\partial u_{mt}} = \left(\frac{1}{h}\right)\left(\frac{\partial \ln P_{t+k+h}}{\partial u_{mt}} - \frac{\partial \ln P_{t+k}}{\partial u_{mt}}\right), \tag{4}$$

where u_{mt} is the exogenous shock to the money supply. This equation gives the response of the per period inflation rate to the exogenous money impulse. But if agents use the VAR to form expectations, then (4) shows how the path of inflationary expectations will be revised in light of the money shock. Hence, (4) can be interpreted as the response of *expected* inflation under this assumption of expectation formation. If r is the (continuously-compounded) nominal yield-to-maturity on h-period bonds and R the corresponding real yield, then

$$\frac{\partial R_{t+k}}{\partial u_{mt}} = \frac{\partial r_{t+k}}{\partial u_{mt}} - \frac{\partial \pi_{h,t+k}}{\partial u_{mt}}. \tag{5}$$

That is, the real rate response is the difference between the nominal rate response (directly estimated from the identified VAR) and the response of expected inflation as computed in (4). We set $h = 1$ since our interest rate measures have a maturity of one month or less. We assume that the short run behaviour of the real rate, as measured by response functions derived in (5), reflects the liquidity effect. It is from this response function that we compute measures of the liquidity effect used in the cross-country regressions.

Figure 4 reports the dynamic responses of real interest rates to money supply shocks based on (5), along with the standard error confidence bands. As with the previous figures, the magnitude of the coefficients are relative to a unit money supply shock. For each country in the sample, there is evidence of a liquidity effect: over the short-run, short-term real rates tend to fall in response to a money supply shock that temporarily raises output and real money balances, and permanently raises prices and nominal money. The only country for which the impact response is positive is Switzerland, but the coefficients become negative immediately after impact.

1.3. *Interpretation and Robustness*

In the next Section, we analyse the cross-country variation in the liquidity effect by regressing the estimated interest rate responses on potential explanatory variables, with a focus on financial factors. It is therefore important that we have properly identified money supply shocks so that errors in the time series estimation do not bias the cross-sectional results. It is also important that the magnitude of the estimates of the liquidity effect be robust to reasonable variation in the statistical models.

It is possible that we have confused a positive money supply shock with a temporary negative money demand shock – for each of these shocks, interest rates will

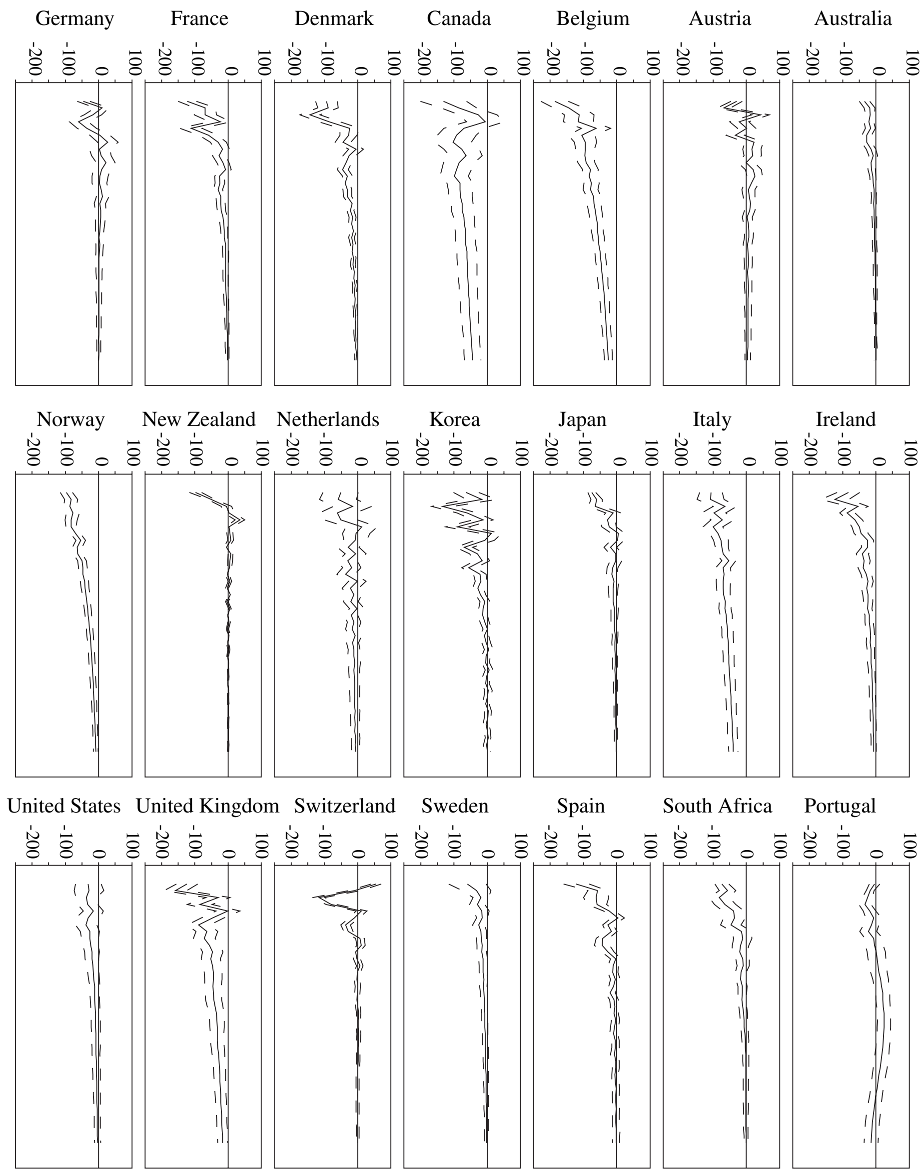

Fig. 4. *Response of Short-term Real Interest Rates to Money Supply Shocks*

fall, and prices and output will rise, as we find. However, real money balances would fall initially for the money demand shock and rise initially for the money supply shock. As seen in Figure 3, in almost every case, real money balances rise in the short-run, lending credibility to our interpretation of the shocks as due to unpredictable changes in money supply, given our modelling assumptions.

The question remains – have we misinterpreted money supply shocks and found pervasive liquidity effects because of mis-specification of the statistical model and inappropriate identifying restrictions?[13] We consider this question along many dimensions.

We first make straightforward changes to the VAR model. Reducing the common lag length of the endogenous variables from five to four generally yields smoother response functions but does not alter the shapes or magnitudes of the dynamic responses. Likewise, adding contemporaneous and lagged world aggregates to the baseline specifications generally does not alter our interpretation of money supply shocks and has little effect on the cross-sectional inference.[14] We also perform CUSUM tests on the residuals of the baseline VARs, which provide no evidence for substantial or important structural breaks over the sample period.

Most significantly, we considered the possibility of model misspecification due to first-differencing the data. This restriction rules out the possibility of cointegrating relations among the variables in the system, and requires the matrix of long-run multipliers, $\mathbf{D}(1)$, to be of full-rank (under the assumption that each variable in the system has a unit root). Because we rely on $\mathbf{D}(1)$ under our identification scheme, this restriction could have important consequences for our estimates.

Johansen's maximum eigenvalue and trace tests for cointegration (not reported) imply that a reasonable case can be made for a single cointegrating vector for at most half of the countries in the sample. We therefore re-estimate the VAR models for each country in vector error correction form, imposing one cointegrating vector. As in Fung and Kasumovich (1998), we maintain the primary identifying restriction that money is neutral in the long run.[15] Generalising the statistical model to allow for cointegration has essentially no effect on our estimates of the liquidity effect and no important effects on the cross-country inference below. The cointegration tests, response functions and cross-country regressions verifying this claim are available separately from the authors.

It is noteworthy that our estimates of the liquidity effect are qualitatively similar to those of Fung and Kasumovich (1998), who use an identification strategy similar to ours for the G-6 countries. But our results are also consistent with those for the G-7 countries found using very different – contemporaneous – identification strategies, such as in Kim (1999) and Grilli and Roubini (1996). While we cannot claim that our estimates of the liquidity effect are precisely correct, they are plausible given conventional views of the macroeconomy, consistent with other recent VAR studies, and robust to changes in the statistical specification.[16]

[13] The need for careful attention to identification and model specification has been re-emphasised recently by Wickens and Motto (2001).

[14] In fact, when the price of oil was included separately, and when both the price of oil and world output were included in the VARs, we found an increase in the importance and statistical significance of financial factors in explaining the liquidity effect.

[15] Under this approach, $\mathbf{D}(1)$ is restricted to be of rank $n - 1$ by containing all zeroes in its final column, but the coefficients corresponding to nominal money and the price level are constrained to be equal. See Fung and Kasumovich (1998) and Fisher *et al.* (1995).

[16] The fact that our results are consistent with those generated from short-run restrictions mitigates to some extent the Faust and Leeper (1997) criticisms of infinite-horizon restrictions. The robustness of the response functions to alternative statistical models also helps, since the Faust-Leeper critique focuses on the sensitivity of the estimated response to the the statistical specification under infinite-horizon restrictions.

One further note – because our estimates of the liquidity effect are used as the dependent variables in the cross-sectional regressions to follow, specification and identification errors in the first stage VAR estimation will bias cross-sectional inference only if these errors are correlated across countries with the regressors in those regressions. If such errors exist but are random, they will most likely only add independent noise to the regression error, making it more difficult to uncover both economically and statistically significant results. As will be seen below, we find strong and regular patterns in the cross-section regressions that require explanation. Thus, despite the real possibility of measurement error in estimating the liquidity effect, our results reported in the following section are of interest and, though our preferred explanation is plausible, should stimulate further research into the validity of our claims.

2. The Liquidity Effect Across Countries

2.1. *Characterising the Cross-country Variation*

To perform the cross-country analysis, we must devise a precise measure of the liquidity effect based on the dynamic response coefficients estimated above, given that the 'short run' is not definitively identified. Based on the real interest rate responses of the previous section, we consider three measures of the liquidity effect: r_1, the dynamic response at impact (the one-quarter horizon), r_m, the maximum (absolute) real rate response (which is negative in all countries), and r_c, the average response over the first eight quarters. The different measures give different weights to the particular timing and persistence of the estimated liquidity effect.

Table 1 reports these measures of the liquidity effect for each of the countries, from the baseline VAR estimates. As in the figures, all measures of interest rate responses are in basis points and in relation to a unit money supply shock. The Table also reports the country rank for each measure, where 1 denotes the largest (absolute) value across the 21 countries in the sample.

The Table indicates that there is ample variation across countries to be explained. Across countries, the mean liquidity effect on impact (r_1) is 75 basis points, and the standard deviation is 52 basis points. The mean is 97 basis points for the maximum effect (r_m) and 47 basis points for the average effect (r_c), with corresponding standard deviations of 43 and 29. The measures range from −187 basis points (Belgium) to 56 basis points (Switzerland) on impact, −187 (Belgium) to −33 (Portugal) for the maximum, and −115 (Belgium) to −8 (Austria) for the average effect. The ranks are relatively stable across measures; for example, Belgium has the largest liquidity effect for each proxy, the UK ranks either second, third, or fourth, and Portugal generally ranks toward the back of the pack. On the other hand, Switzerland and New Zealand have rank changes larger than 10.

2.2. *The Role of Financial Market Variables*

Our primary objective is to examine the extent to which financial factors explain the observed cross-country variation in the liquidity effect. To this end, we estimate regressions of the following form:

$$y_i = \beta_0 + \beta_1 \sigma_m + \gamma x_i + \epsilon_i, i = 1, \cdots, 21, \tag{6}$$

where y_i is the estimated liquidity effect (r_1, r_m, or r_c), σ_m is the standard deviation of the money supply shock estimated from the VAR, and x_i is a financial market variable. The money supply standard error is included as a control variable to account for potential nonlinearities in scale effects and to allow for the possible effects of liquidity risk.[17] Regressions of this sort have been used before; for example, Cecchetti (1999) performs similar regressions in his analysis on the relationship between financial structure and the impact of monetary policy on output and prices across the members of the European Monetary Union.[18]

We use the following financial variables for x in the regressions: the ratios of bank assets to GDP, private credit to GDP, liquid liabilities to GDP, bank credit to GDP, commercial bank credit to the sum of commercial and central bank credit, privately issued debt to GDP, national stock market capitalisation to GDP, the turnover rate in the stock market, an index of financial market depth, the ratio of aggregate bank reserves to demand deposits and the ratio of bank reserves to total deposits. These variables are presumably related to the level of financial services, the functioning of financial markets and the importance of financial intermediaries in the economy. They are also likely to be related to transactions costs in financial markets and therefore the degree to which households can participate in these markets.

The first five variables, which are closely related to the financial intermediary sector, have been constructed and used to measure the provision of financial services by Levine *et al.* (2000) in their study of the sources of economic growth. In particular, these variables attempt to measure the relative size (bank asset, bank credit to total credit, and liquid liabilities ratios) and activity (private and bank credit ratios) of banks in the economy. In addition, we consider the reserve ratio proxies as possibly related to the degree of financial intermediary services. These variables are averages over the 1960–95 period (1974–98 for the reserve ratios), a sample similar to the one used to estimate the VAR models.

The next three variables are frequently used to capture the depth, liquidity, and sophistication of all financial markets (Cecchetti 1999), and are thus broader than the previous five proxies. The financial market depth variable is a summary index of privately issued debt, stock market capitalisation and the turnover rate, and is similar in construction to Cecchetti's (1999) index of alternative finance. These last four variables are measured at a single point in time (1996) near the end of

[17] For example, Fuerst's model (1992, p. 15) implies that the higher is money supply variance, the greater is the liquidity risk faced by households. They respond by increasing bank deposits for precautionary purposes, making money injections less important in the loanable funds market. This would tend to reduce the liquidity effect, that is, lead to a *positive* β_1 coefficient. Since households respond to increases in money supply variance by increasing their holdings of bank deposits, and since intermediaries expand asset holdings as deposits rise, money supply variance is expected to be positively correlated with many of the variables we use for x, especially the bank ratios noted below. Thus, it is essential to control for money supply volatility in the cross-country regressions, given our focus on estimating the effects of the financial market variables on the liquidity effect.

[18] Karras (1999), in studying how openness alters monetary policy effects across countries, uses a panel approach. The nature of his strategy, however, does not allow him to identify money supply shocks as we do.

our sample.[19] Table 2 contains the data for the financial market proxies along with summary statistics, where the countries are identified by number as in Table 1. Sources and complete descriptions of these variables are contained in the separate appendix.

Table 3 reports the regression results for each of the three measures of the liquidity effect, and each of the financial market proxies. We do not have sufficient degrees of freedom to include all financial variables in a single regression, so we consider the alternative measures in separate regressions. The Table reports conventional OLS t-statistics for inference; however, these statistics are almost identical to those computed from White's consistent estimator of the covariance matrix allowing for heteroscedasticity.

In each regression, the constant term is significantly less than zero at very small levels of significance. The estimated coefficient on money supply variability is significant in almost all of the regressions, and is positive – the higher the variance, the smaller is the liquidity effect. This result is in line with the liquidity risk story of Fuerst mentioned in footnote 17.

Table 2

Financial Market Variables

	D/Y	*MC*	*MD*	*TO*	*R/D*	*R/T*	*BA*	*PC*	*LL*	*BC*	*CB*
1	28.40	76.40	2.00	52.20	22.40	4.60	47.80	54.82	51.73	34.01	92.67
2	45.68	14.60	2.00	61.80	46.40	5.10	81.36	65.30	67.50	62.30	98.44
3	60.19	44.70	2.00	23.20	5.60	1.50	58.12	25.65	49.02	25.39	92.01
4	18.44	79.50	2.00	62.20	14.30	3.10	42.35	60.86	56.50	35.51	89.00
5	100.65	39.20	3.00	54.20	8.20	3.80	49.87	42.45	49.48	42.13	88.10
6	48.44	38.00	2.00	49.80	6.70	2.40	62.50	75.47	63.37	55.36	96.54
7	57.06	28.10	3.00	123.20	34.40	6.90	88.89	76.46	57.46	71.00	97.57
8	12.18	16.80	1.00	24.50	38.50	11.10	36.77	49.14	54.74	28.14	94.73
9	36.81	21.00	1.00	43.80	29.30	14.80	73.13	59.09	77.48	58.13	87.77
10	39.14	67.10	2.00	37.10	8.20	2.10	98.94	128.38	125.94	88.63	96.72
11	43.03	26.70	2.00	110.50	58.80	11.00	42.24	65.48	41.02	40.09	83.95
12	47.80	96.40	3.00	92.40	2.60	0.60	70.88	86.69	71.41	52.35	98.10
13	7.97	58.60	1.00	28.10	5.70	1.90	30.55	37.59	49.63	25.44	82.43
14	24.97	36.30	2.00	70.30	5.40	1.70	57.31	81.62	54.04	40.76	90.02
15	18.81	22.70	1.00	33.20	80.40	17.70	74.85	55.01	78.02	60.66	90.35
16	6.05	172.00	1.00	10.40	18.60	4.60	56.91	71.94	51.44	49.22	94.77
17	11.02	41.70	1.00	113.10	48.50	12.10	74.81	65.05	70.31	58.37	92.74
18	70.69	94.40	3.00	64.40	2.60	2.60	49.43	89.11	53.49	42.28	88.94
19	62.03	136.00	3.00	94.00	25.50	5.70	133.08	141.29	123.41	119.13	98.99
20	43.68	147.00	3.00	36.80	1.90	1.90	54.78	46.31	48.63	45.55	83.55
21	62.58	109.00	3.00	92.80	11.60	4.30	70.42	113.07	62.12	58.42	93.11
μ	40.27	65.06	2.05	60.86	22.65	5.69	64.52	70.99	64.61	52.04	91.93
σ	24.06	45.81	0.80	32.48	21.57	4.81	23.46	28.88	22.31	21.92	5.02

Notes: D/Y is debt/GDP, *MC* is market capitalisation, *MD* is the index of market depth, *TO* is the turnover rate, *R/D* is the ratio of reserves to demand deposits, *R/T* is the ratio of reserves to total deposits, *BA* are bank assets, *PC* is private credit, *LL* are liquid liabilities, *BC* is bank credit, and *CB* is the ratio of commercial credit to central bank credit. Countries are identified by number, as in Table 1.

[19] Using sample averages for these variables rather than point-in-time estimates does not alter the regression results discussed below.

Table 3

Cross-country Regressions and Rank Correlations

y	x	β_0 (tstat)	β_1 (tstat)	γ (tstat)	R^2	ρ
r_1	*D/Y*	−113.16 (−3.65)	1.99 (1.28)	0.35 (0.71)	0.10	0.24
r_m	*D/Y*	−123.89 (−5.27)	2.56 (2.18)	−0.12 (−0.33)	0.22	−0.01
r_c	*D/Y*	−67.77 (−4.17)	1.65 (2.03)	0.01 (0.06)	0.19	0.06
r_1	*MC*	−113.23 (−4.07)	1.92 (1.26)	0.23 (0.90)	0.11	−0.01
r_m	*MC*	−134.39 (−6.31)	2.62 (2.25)	0.07 (0.38)	0.22	0.05
r_c	*MC*	−70.73 (−4.83)	1.66 (2.06)	0.05 (0.40)	0.19	0.00
r_1	*MD*	−142.31 (−3.70)	2.21 (1.47)	19.73 (1.39)	0.16	0.27
r_m	*MD*	−143.38 (−4.76)	2.71 (2.30)	6.17 (0.56)	0.23	0.05
r_c	*MD*	−84.12 (−4.13)	1.77 (2.23)	7.52 (1.00)	0.23	0.12
r_1	*TO*	−133.36 (−4.45)	1.98 (1.36)	0.56 (1.66)	0.20	0.33
r_m	*TO*	−145.06 (−6.15)	2.66 (2.32)	0.25 (0.92)	0.25	0.18
r_c	*TO*	−87.10 (−5.70)	1.71 (2.30)	0.32 (1.82)	0.31	0.28
r_1	*R/D*	−107.75 (−4.87)	1.19 (0.77)	0.81 (1.50)	0.18	0.40
r_m	*R/D*	−131.52 (−7.50)	2.47 (2.02)	0.17 (0.39)	0.22	0.15
r_c	*R/D*	−70.37 (−5.93)	1.42 (1.73)	0.26 (0.90)	0.22	0.30
r_1	*R/T*	−103.61 (−4.40)	1.50 (0.93)	1.83 (0.71)	0.10	0.38
r_m	*R/T*	−128.88 (−7.19)	2.64 (2.14)	−0.17 (−0.09)	0.22	0.17
r_c	*R/T*	−65.66 (−5.34)	1.73 (2.04)	−0.45 (−0.33)	0.19	0.22
r_1	*BA*	−227.74 (−6.93)	3.71 (3.22)	1.66 (4.48)	0.56	0.39
r_m	*BA*	−191.12 (−6.00)	3.48 (3.12)	0.79 (2.19)	0.38	0.34
r_c	*BA*	−116.94 (−5.62)	2.35 (3.23)	0.64 (2.71)	0.42	0.27
r_1	*PC*	−191.22 (−6.62)	2.56 (2.21)	1.20 (3.93)	0.50	0.49
r_m	*PC*	−185.16 (−7.44)	3.02 (3.03)	0.71 (2.72)	0.44	0.46
r_c	*PC*	−103.98 (−5.98)	1.92 (2.75)	0.47 (2.57)	0.41	0.27
r_1	*LL*	−197.69 (−5.23)	2.90 (2.20)	1.35 (3.02)	0.39	0.38
r_m	*LL*	−178.12 (−5.52)	3.11 (2.76)	0.66 (1.72)	0.33	0.45
r_c	*LL*	−106.17 (−4.93)	2.05 (2.72)	0.53 (2.07)	0.34	0.30
r_1	*BC*	−214.37 (−8.10)	3.48 (3.36)	1.86 (5.21)	0.63	0.52
r_m	*BC*	−180.29 (−6.42)	3.31 (3.02)	0.81 (2.14)	0.37	0.43
r_c	*BC*	−113.67 (−6.55)	2.29 (3.37)	0.74 (3.17)	0.48	0.41
r_1	*CB*	−621.23 (−3.12)	2.97 (2.14)	5.55 (2.64)	0.33	0.49
r_m	*CB*	−552.81 (−3.82)	3.50 (3.47)	4.49 (2.94)	0.47	0.35
r_c	*CB*	−316.25 (−2.99)	2.17 (2.94)	2.64 (2.37)	0.38	0.37

Notes: The regression is $y_i = \beta_0 + \beta_1\sigma_{mi} + \gamma x_i + \epsilon_i$, where y is the liquidity effect, σ_{mi} is the estimated standard deviation of the money supply shock, and x is a measure of financial market transactions costs. ρ is Spearman's rank correlation coefficient, which we obtain by regressing the rank of y on the rank of x. According to Table P in Siegel (1956), the 5% critical value under the null of no correspondence is 0.368 for 21 observations. See notes to Tables 1 and 2 for variable definitions.

Of primary interest is the coefficient on financial market variables, γ. The estimated γ for the debt ratio, market capitalisation, turnover, and the depth index, as well the reserve ratios, are generally small and insignificantly different from zero. Only for the turnover ratio in the r_1 and r_c regressions does the marginal significance level approach a reasonably small value. However, the proxies based primarily on the size and activity of financial intermediaries (excluding the reserve ratios) – bank assets, private credit, liquid liabilities, bank credit, and the bank credit to central bank credit ratio – all have statistically significant explanatory power at typical test size for the liquidity effect, and are consistently positive. The positive coefficient means that as the bank ratio increases, the liquidity effect becomes a smaller negative number. This inference is robust across the different measures of the liquidity effect. The overall fit of the regressions including these variables is good: the lowest R^2 is 33% and the highest 63%.[20]

While statistical significance is important in understanding the extent to which sampling error affects inference, a coefficient estimate that is statistically significant (for an arbitrary test size) need not be economically important. One way to determine the importance of the effect is to consider how much the liquidity effect would change for a typical change in x. For example, the average country in our sample has a liquidity effect on impact (r_1) of −75 basis points (Table 1) and a bank asset ratio (BA) of 64.5% (Table 2). According to the estimates in Table 3, a country having a bank asset ratio of 88%, or one standard deviation above the mean, will have a liquidity effect of −36 basis points, 39 basis points *smaller* than average ($39 = 1.67 \times 23.46$). This quantity is 75% of r_1's cross-country standard deviation of 52 basis points (Table 1).

The other financial market variables have similar quantitative effects on r_1: private credit – 35 basis points; liquid liabilities – 34 basis points; bank credit – 41 basis points; share of commercial credit – 28 basis points. The latter creates the largest reduction in the maximum liquidity effect (r_m) – 22.5 basis points, which is 54% of the standard deviation in r_m. Liquid liabilities (LL) has the largest effect on r_c – 16.2 basis points, or 56% of its cross-country variation. Overall, these results suggest that the explanatory power of the financial market factors for cross-country variation in the liquidity effect is plausible, non-trivial, and not likely to be affected by sampling error.[21]

2.3. *Robustness*

We discussed earlier that these results are not sensitive to a number of changes in the statistical model generating the liquidity effect estimates. Here, we provide further evidence of the robustness of these results by computing non-parametric rank-order correlations, and including other variables in the regressions that could explain cross-country variation in the liquidity effect.

The final column of Table 3 reports Spearman's rank-correlation coefficient (Siegel, 1956) for the liquidity effect measures and the financial market variables. Correlations based on rank are less sensitive to extreme point estimates than correlations estimated from the regression model. The Table shows that at a 5% significance level, the null hypothesis of no correlation can be rejected in favour a positive correlation for the same variables that are significantly non-zero in the regression analysis.

[20] Informal inspection of Table 2 suggests that Switzerland (country 19) may be an outlier for bank assets, private credit, liquid liabilities and bank credit. To see if Switzerland drives the results, we dropped it from the sample; the only effect is to render liquid liabilities marginally insignificant for r_1 and r_c, even though the quantitative effect is the same.

[21] Our approach exploits variation in the time-averages of the liquidity effect and financial market factors across countries. An alternative approach, as suggested by a referee, is to exploit variation *over time* in the liquidity effect and financial market structure *within countries*, perhaps due to discrete changes in regulations or innovations in financial markets. While our CUSUM tests indicated little significant structural change in the estimated VAR models, this line of research is clearly of potential interest. However, such an analysis, done carefully and correctly, lies well beyond the scope of our paper.

To the extent that our measures of financial market factors are correlated with variables representing other causes of variation in the liquidity effect, our previous estimates will be biased and our inferences may be incorrect. We consider the robustness of the results to three other potential alternative explanations of variation in the liquidity effect.

The first is variation in the extent of capital mobility across open economies. The fewer the restrictions on international capital flows, the lower the variability in interest rate responses to country-specific monetary shocks, which could explain differences in the magnitude of liquidity effects across countries independently of financial market channels. Consequently, we have estimated regressions that add a capital mobility proxy to the basic regression model. We use as our capital mobility proxy the index of capital account openness developed by Quinn (1997). He constructs the index for a variety of industrialised and developing economies based on careful consideration of capital account restrictions imposed by each country. The only countries in our sample not considered by Quinn are Korea and South Africa, so the regressions reported in Table 4 exclude both of these countries from the sample. The index ranges from 0 to 4, with higher values implying greater capital mobility; thus, the expected sign of the coefficient on this variable is positive. The specific values of the index used in our regressions are the average values over the period 1974–97.[22] The results in Table 4 indicate essentially no effect of capital mobility on the magnitude of the liquidity effect. Only in one case is the coefficient on the capital mobility proxy statistically significant at the 10% level or better but the pattern of significant coefficients on the financial proxies is similar to that in Table 3. The only noteworthy difference is that the coefficients on the turnover proxy are now statistically significant.[23]

Differences in the extent of wage and price rigidity in an economy are another possible reason for differences in the magnitude of the liquidity effect, and we have estimated equations that add rigidity proxies to the base model. Other things equal, we expect that the more rigid are wages and prices, the bigger the change in real money balances following a change in nominal money, and, consequently, the bigger the liquidity effect. Measuring the extent of wage and/or price rigidity is difficult but Grubb *et al.* (1983) provide measures of nominal and real wage stickiness for 18 of the 21 countries in our sample.[24] Measures are not available for Korea, Portugal, and South Africa. The higher the value of these measures, the stickier are wages. Consequently, a negative coefficient is expected on the wage rigidity proxies since the greater the extent of stickiness, the greater the liquidity

[22] Time series of his index for each of the countries in our cross-section (except, as noted earlier, for Korea and South Africa) were kindly provided by Professor Dennis Quinn of Georgetown University.

[23] An alternative capital mobility proxy has been suggested by Obstfeld and Taylor (1997) who use relative patterns of dispersion of the real interest rate to proxy for variation in capital mobility across countries. If a country's capital market is well-integrated with world capital markets, a shock to the real rate should be mitigated quickly if capital mobility is high, and the standard deviation of the real rate should be low. Thus, standard deviations of ex post real interest rates might be used as a proxy for differences in capital mobility. Unfortunately, this type of real interest rate based measure is inappropriate given our focus on the liquidity effect.

[24] These measures are based on estimates of wage and price equations using annual data for 1957–80 and are provided in Table 3, p. 25 of Grubb *et al* (1983). The nominal rigidity measure has recently been employed by Fischer (1997) in a study of the institutional determinants of the speed of disinflation.

Table 4

Cross-country Regressions Including Capital Mobility

y	x	γ (tstat)	β_2 (tstat)
r_1	*D/Y*	0.191 (0.30)	21.384 (0.66)
r_m	*D/Y*	−0.051 (−0.11)	−0.162 (−0.01)
r_c	*D/Y*	−0.056 (−0.16)	6.740 (0.39)
r_1	*MC*	0.215 (0.55)	16.546 (0.50)
r_m	*MC*	−0.030 (−0.10)	−0.094 (−0.00)
r_c	*MC*	0.059 (0.28)	2.652 (0.15)
r_1	*MD*	21.227 (0.99)	2.811 (0.08)
r_m	*MD*	16.463 (1.05)	−19.638 (−0.74)
r_c	*MD*	10.716 (0.94)	−6.524 (−0.34)
r_1	*TO*	0.687 (1.55)	11.350 (0.41)
r_m	*TO*	0.631 (2.04)	−15.154 (−0.78)
r_c	*TO*	0.460 (2.07)	−4.669 (−0.33)
r_1	*R/D*	1.174 (1.92)	39.997 (1.53)
r_m	*R/D*	0.449 (0.92)	3.795 (0.18)
r_c	*R/D*	0.396 (1.14)	9.951 (0.67)
r_1	*R/T*	2.458 (0.87)	30.765 (1.10)
r_m	*R/T*	0.415 (0.20)	−0.692 (−0.03)
r_c	*R/T*	−0.310 (−0.20)	4.745 (0.31)
r_1	*BA*	1.745 (4.57)	23.035 (1.27)
r_m	*BA*	0.755 (1.95)	−2.848 (−0.15)
r_c	*BA*	0.652 (2.48)	4.109 (0.33)
r_1	*PC*	1.166 (3.55)	17.017 (0.82)
r_m	*PC*	0.718 (2.67)	−7.146 (−0.42)
r_c	*PC*	0.468 (2.31)	1.605 (0.12)
r_1	*LL*	1.704 (3.84)	44.341 (2.16)
r_m	*LL*	0.654 (1.53)	5.491 (0.28)
r_c	*LL*	0.611 (2.10)	11.799 (0.88)
r_1	*BC*	1.862 (5.01)	20.221 (1.17)
r_m	*BC*	0.788 (1.99)	−4.009 (−0.22)
r_c	*BC*	0.740 (2.87)	2.913 (0.24)
r_1	*CB*	6.577 (2.91)	29.201 (1.30)
r_m	*CB*	4.152 (2.33)	0.405 (0.02)
r_c	*CB*	2.963 (2.31)	6.639 (0.52)

Notes: The regression is $y_i = \beta_0 + \beta_1\sigma_{mi} + \beta_2 h_i + \gamma x_i + \epsilon_i$, where h is the proxy for capital mobility. See notes to Tables 2 and 3 for variable definitions.

effect. The coefficient estimates for the transaction costs proxies and nominal and real wage rigidity proxies are reported in Table 5. There is essentially no evidence that the wage rigidity proxies explain a significant amount of the cross-country variation in the liquidity effect; in only two cases is the coefficient on the wage rigidity proxy significant. In the case of the financial variables, the pattern of significance is essentially identical to that in Table 3.

Finally, as noted in the introduction, financial market variables may simply capture differences in the interest rate elasticity of money demand across countries. Indeed, if large bank ratios are associated with high money demand elasticity, then the positive coefficients found in the cross-country regressions are consistent with this idea. To determine if the bank ratios have an independent effect on the liquidity effect, we include a direct measure of the interest rate semi-elasticity of demand in the regressions. We use, as a measure of this semi-elasticity, the coefficient on the interest rate in the cointegration relationship estimated above as part of our robustness check, which in general can be reasonably interpreted as a

Table 5

Cross-country Regressions Including Nominal and Real Wage Rigidities

y	x	γ(nom)	(tstat)	β_2(nom)	(tstat)	γ(real)	(tstat)	β_2(real)	(tstat)
r_1	*D/Y*	0.58	(1.03)	−11.60	(−0.59)	0.52	(0.95)	−24.78	(−1.18)
r_m	*D/Y*	0.01	(0.01)	4.70	(0.32)	0.00	(0.01)	−18.20	(−1.17)
r_c	*D/Y*	0.07	(0.22)	−8.74	(−0.83)	0.02	(0.07)	−18.15	(−1.65)
r_1	*MC*	0.48	(1.44)	−14.88	(−0.78)	0.41	(1.28)	−24.19	(−1.18)
r_m	*MC*	0.01	(0.03)	4.63	(0.31)	0.01	(0.04)	−18.18	(−1.17)
r_c	*MC*	0.14	(0.74)	−10.04	(−0.95)	0.09	(0.50)	−17.89	(−1.64)
r_1	*MD*	31.69	(1.98)	−14.73	(−0.82)	30.32	(2.00)	−26.71	(−1.40)
r_m	*MD*	12.69	(1.01)	2.54	(0.18)	13.48	(1.14)	−18.71	(−1.26)
r_c	*MD*	11.51	(1.29)	−10.45	(−1.04)	10.53	(1.27)	−18.58	(−1.79)
r_1	*TO*	0.93	(2.31)	−15.56	(−0.90)	0.91	(2.40)	−28.23	(−1.55)
r_m	*TO*	0.63	(2.12)	0.47	(0.04)	0.66	(2.41)	−20.13	(−1.54)
r_c	*TO*	0.52	(2.52)	−11.96	(−1.35)	0.50	(2.67)	−19.64	(−2.19)
r_1	*R/D*	0.81	(0.92)	−6.04	(−0.30)	1.02	(1.24)	−29.27	(−1.41)
r_m	*R/D*	0.30	(0.46)	5.92	(0.40)	0.37	(0.59)	−19.53	(−1.26)
r_c	*R/D*	0.36	(0.78)	−7.02	(−0.67)	0.53	(1.28)	−20.11	(−1.91)
r_1	*R/T*	0.33	(0.09)	−9.15	(−0.45)	1.88	(0.53)	−29.41	(−1.30)
r_m	*R/T*	−0.84	(−0.33)	4.43	(0.31)	0.02	(0.01)	−18.25	(−1.11)
r_c	*R/T*	−1.19	(−0.66)	−8.88	(−0.86)	−0.22	(−0.12)	−17.74	(−1.53)
r_1	*BA*	1.71	(4.01)	−2.54	(−0.18)	1.66	(3.75)	−6.56	(−0.41)
r_m	*BA*	0.73	(1.80)	7.60	(0.58)	0.60	(1.43)	−11.30	(−0.74)
r_c	*BA*	0.64	(2.29)	−5.96	(−0.66)	0.56	(1.97)	−11.81	(−1.15)
r_1	*PC*	1.26	(4.15)	−12.49	(−0.92)	1.25	(3.63)	0.94	(0.06)
r_m	*PC*	0.73	(2.78)	2.86	(0.24)	0.71	(2.43)	−3.29	(−0.23)
r_c	*PC*	0.50	(2.52)	−9.73	(−1.11)	0.40	(1.81)	−9.75	(−0.89)
r_1	*LL*	1.44	(2.67)	2.28	(0.13)	1.43	(2.34)	0.50	(0.02)
r_m	*LL*	0.65	(1.46)	9.93	(0.71)	0.41	(0.81)	−10.73	(−0.60)
r_c	*LL*	0.51	(1.63)	−4.32	(−0.43)	0.39	(1.10)	−11.13	(−0.90)
r_1	*BC*	1.85	(4.51)	0.36	(0.03)	1.80	(4.24)	−5.16	(−0.34)
r_m	*BC*	0.78	(1.89)	8.78	(0.67)	0.63	(1.48)	−11.01	(−0.72)
r_c	*BC*	0.72	(2.65)	−4.69	(−0.54)	0.65	(2.36)	−10.80	(−1.10)
r_1	*CB*	6.45	(2.69)	0.03	(0.00)	6.08	(2.41)	−7.59	(−0.39)
r_m	*CB*	4.49	(2.55)	11.19	(0.91)	3.81	(2.01)	−6.92	(−0.47)
r_c	*CB*	2.80	(2.08)	−4.42	(−0.47)	2.39	(1.73)	−11.11	(−1.03)

Notes: Results for two regressions are reported, both of the form $y_i = \beta_0 + \beta_1\sigma_{mi} + \beta_2 h_i + \gamma x_i + \epsilon_i$. In the first, h is the proxy for nominal wage rigidities; in the second, h is the proxy for real wage rigidities. See notes to Tables 1, 2 and 3 for variable definitions.

money demand relationship. We set up the regression so that a high money demand elasticity corresponds to a large value for the semi-elasticity measure; hence, we expect a positive coefficient on the semi-elasticity variable. Table 6 shows that indeed this is the case, with a high degree of statistical confidence.[25] Quantitatively, a standard deviation increase in semi-elasticity (1.65%) reduces the liquidity effect by between 12 and 20 basis points. But most importantly, while magnitudes are generally reduced, the financial intermediary variables remain for the most part both statistically and economically significant.

[25] We have adjusted the standard errors on the estimate of γ to account for the fact that the semi-elasticity regressors have been generated from a first step regression, using Murphy and Topel (1985). In making this adjustment, we assumed independence across the cointegration regressions. Since the t-statistics for β_2 test the null hypothesis that $\beta_2 = 0$, no adjustment to these standard errors is required. See Murphy and Topel (1985, equation 9).

Table 6

Cross-country Regressions Including Semi-elasticity of Money Demand

y	x	γ (tstat)	β_2 (tstat)
r_1	D/Y	0.508 (1.14)	17.636 (2.96)
r_m	D/Y	−0.002 (−0.01)	13.426 (2.98)
r_c	D/Y	0.096 (0.41)	9.035 (2.87)
r_1	MC	0.078 (0.31)	16.114 (2.54)
r_m	MC	−0.056 (−0.30)	13.833 (3.00)
r_c	MC	−0.032 (−0.24)	9.079 (2.80)
r_1	MD	15.500 (1.17)	15.711 (2.64)
r_m	MD	2.602 (0.25)	13.268 (2.95)
r_c	MD	5.227 (0.75)	8.528 (2.74)
r_1	TO	0.436 (1.36)	15.272 (2.59)
r_m	TO	0.139 (0.56)	12.984 (2.90)
r_c	TO	0.249 (1.53)	8.055 (2.72)
r_1	R/D	0.827 (1.75)	16.802 (2.99)
r_m	R/D	0.183 (0.49)	13.458 (3.04)
r_c	R/D	0.271 (1.07)	8.895 (2.93)
r_1	R/T	2.867 (1.29)	17.942 (3.02)
r_m	R/T	0.629 (0.37)	13.708 (3.04)
r_c	R/T	0.070 (0.06)	8.883 (2.79)
r_1	BA	1.389 (3.65)	9.836 (2.00)
r_m	BA	0.479 (1.34)	11.072 (2.43)
r_c	BA	0.452 (1.90)	6.629 (2.17)
r_1	PC	0.985 (3.31)	11.043 (2.18)
r_m	PC	0.512 (2.05)	10.506 (2.49)
r_c	PC	0.339 (1.91)	6.915 (2.29)
r_1	LL	1.014 (2.21)	11.694 (2.01)
r_m	LL	0.315 (0.83)	11.884 (2.52)
r_c	LL	0.317 (1.23)	7.295 (2.25)
r_1	BC	1.614 (4.77)	10.589 (2.56)
r_m	BC	0.544 (1.52)	11.379 (2.60)
r_c	BC	0.587 (2.61)	6.640 (2.41)
r_1	CB	3.552 (1.52)	11.513 (1.73)
r_m	CB	2.885 (1.73)	9.241 (1.95)
r_c	CB	1.478 (1.21)	6.707 (1.93)

Notes: The regression is $y_i = \beta_0 + \beta_1\sigma_{mi} + \beta_2 h_i + \gamma x_i + \epsilon_i$, where h is an estimate of the interest rate semi−elasticity of money demand generated from cointegrating regressions. The t−statistics for γ are adjusted to account for the randomness of the first step estimation. See Murphy and Topel (1985). See notes to Tables 1, 2 and 3 for variable definitions.

2.4. *Interpretation and Assessment*

To this point, we have documented the existence of a liquidity effect and a statistically and economically significant positive effect of financial market factors in explaining this liquidity effect. We now assess this evidence in light of the three potential explanations discussed in the introduction. Specifically, we consider whether our results shed light on or distinguish among the possible explanations of the liquidity effect and its magnitude.

As noted above, financial variables could reflect variation in money demand elasticities. However, we find that such variation cannot provide a complete explanation of the liquidity effect – financial variables maintain independent effects on the estimated cross-country variation even after including proxies for money demand elasticity. In addition, we find no evidence that cross-country differences in nominal or real wage rigidities can explain the magnitude of the liquidity effect. Hence, rigidities in labour markets coupled with different money demand elasticities are not likely to be a complete explanation of the cross-country variation. Our robustness tests also imply that the cross-country explanatory power of financial factors is not due to correlations with the openness of the economies.

The fact that the financial market proxies most related to the size and activity of financial intermediaries have the strongest and most certain impact on the liquidity effect seems to provide ample support for bank-lending channel theories. If we assume that the greater the bank asset and credit ratios the more important is bank financing to the economy, and hence the more likely the importance of a bank lending channel (as in Cecchetti, 1999, pp. 14–5), then the positive coefficients we find in the cross-country regressions indeed support this view.

However, these aggregate financial intermediary ratios may be ambiguous proxies for the bank-lending channel. Indeed, Cecchetti (1999) also argues that firms with good access to capital markets are more likely to be found in countries with extensively developed bond and stock markets. In this case, the bank lending channel should decrease in importance with increases in our more general financial market ratios, such as market capitalisation, implying a *negative* coefficient on these broader ratios in the cross-country regressions. In general, our estimates for these coefficients in Tables 3 to 6 are positive, though small and statistically insignificant.

A stronger test for the bank lending channel is to regress the liquidity effect on the bank ratios and market capitalisation, thereby controlling for the possibility that the non-negative coefficients on market capitalisation are primarily due to omitted variable bias in the cross-sectional regressions. In light of the discussion in the previous paragraph, the bank lending channel would imply a positive coefficient on the bank ratios but a negative coefficient on market capitalisation. The results, reported in Table 7, reveal that the coefficient estimates on market capitalisation remain mostly positive and are always statistically insignificant. Thus, we interpret our results as providing at best only limited and qualified support for the bank-lending channel as a determinant of variation in the liquidity effect.

Finally, Dotsey and Ireland's (1995) generalised version of limited participation models implies that countries with low transactions costs will exhibit small liquidity effects and *vice versa*. In this model, households have access to cash-management technologies that allow portfolio adjustment with positive but finite costs in the form of time and resources. In this case, households can at least partially reduce their savings through bank deposits in reaction to a monetary injection. The smaller the costs of adjustment, the larger the adjustment made and the smaller is the effect of the injection on the supply of loanable funds.

It is reasonable to assume that high values for the bank size and activity variables, as well as the measures of overall financial market depth and liquidity, reflect low costs of financial transactions. Thus, these models generally imply a positive relationship between these financial market variables and the magnitude of the liquidity effect. The coefficient on the reserve ratios, however, is ambiguous under limited participation models. In Li (2000), liquidity effects occur through a household credit channel, and the reserve-deposit ratio is one measure for the supply of household credit by the credit service industry. In this case, the lower the reserve ratio, the

Table 7

Cross-country Regressions with Bank Ratios and Market Capitalisation

y	x	γ (tstat)	β_2 (tstat)
r_1	*BA*	1.630 (4.26)	0.116 (0.63)
r_m	*BA*	0.784 (2.09)	0.019 (0.11)
r_c	*BA*	0.635 (2.59)	0.009 (0.08)
r_1	*PC*	1.231 (3.66)	0.057 (−0.27)
r_m	*PC*	0.777 (2.70)	−0.107 (−0.60)
r_c	*PC*	0.511 (2.53)	−0.065 (−0.52)
r_1	*LL*	1.314 (2.88)	0.158 (0.73)
r_m	*LL*	0.649 (1.64)	0.038 (0.21)
r_c	*LL*	0.522 (1.98)	0.025 (0.20)
r_1	*BC*	1.841 (4.90)	0.047 (0.27)
r_m	*BC*	0.816 (2.04)	−0.007 (−0.04)
r_c	*BC*	0.753 (3.05)	−0.021 (−0.19)
r_1	*CB*	5.480 (2.60)	0.209 (0.95)
r_m	*CB*	4.468 (2.85)	0.057 (0.35)
r_c	*CB*	2.626 (2.29)	0.044 (0.37)

Notes: The regression is $y_i = \beta_0 + \beta_1\sigma_{mi} + \beta_2 h_i + \gamma x_i + \epsilon_i$, where h is market capitalisation (MC). See notes to Tables 1, 2 and 3 for variable definitions.

greater the supply of loanable funds for any given monetary injection, and the greater the liquidity effect. On the other hand, high bank reserve ratios may reflect low availability of financial intermediation services, high transactions costs, and thus imply a negative coefficient in the cross-country regressions.

We interpret our results as providing strong support for the generalised limited participation models, at least relative to the alternatives considered. We find robust, statistically significant, and economically non-trivial positive coefficients on the proxies representing bank size and activity. The link between these proxies and financial transactions costs is intuitively strong, and is consistent with findings in studies linking these variables to other measures of economic activity. And as seen in Table 3, we even pick up the ambiguity in the reserve ratio variables.

Finally, the difference between the strength of the cross-sectional explanatory power across the two types of financial market factors (banking size and activity versus the broader measures), even after controlling for the possibility of correlation between these different types, lends support to limited participation models. While it is possible that the bank measures are simply better proxies for transactions costs, one could argue that it is the presence of banks and financial intermediaries *per se* that determines the extent of household participation in financial markets.

3. Conclusion

Our systematic analysis of the cross-country variation in the liquidity effect uncovers an important role for financial market factors and provides support for limited participation models as an explanation for the liquidity effect. This conclusion is conditional on our identification strategy but is robust to alternate statistical specifications for the time-series model generating the estimates of the liquidity effect and to different control variables in the cross-country regressions.

Overall, the findings imply that rigidities in financial markets, as envisaged in the generalised versions of the limited participation model of Dotsey and Ireland (1995), are a potentially important part of the monetary transmission mechanism.

We acknowledge that there are many potential problems with our empirical strategy and implementation. In particular, our inference relies heavily on proper identification of money supply shocks and the liquidity effect in the time-series analysis. Because such identification is inherently difficult in VAR models, it is certainly possible that our estimates of the liquidity effect are mis-measured, with unknown consequences for the second step regressions. Other models may yield different results. But many of these problems would tend to hide the sort of cross-sectional patterns we find in the international data. Thus, our approach has most likely captured something relevant and systematic about the cross-country variation in the liquidity effect.

University of Georgia
Louisiana State University

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