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One cost of the Chilean capital controls: Increased financial constraints for smaller traded firms

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Abstract

There is growing support for taxes on short-term capital inflows in emerging markets, such as the *encaje* adopted by Chile from 1991 to 1998. This paper assesses whether the Chilean capital controls increased financial constraints for different-sized, publicly-traded firms. It uses an Euler-equation framework and shows that during the *encaje*, smaller traded firms experienced significant financial constraints. These constraints decreased as firm size increased. Both before and after the *encaje*, however, smaller firms did not experience significant financial constraints, and there is no relationship between firm size and financial constraints. Although Chilean-style capital controls may yield some benefits, any such benefits should be weighed against this cost of increasing financial constraints for small and mid-sized firms.

Keywords: Capital controls; *Encaje*; Chile; Firm-financing constraints

JEL classification: F3; F21; G1; G32; O16; O54

1. Introduction

In the early and mid-1990s, most international economists and Washington-based policy-makers supported rapid capital account liberalization for emerging markets. Many countries followed this advice. The initial results were generally positive — increased capital inflows, investment booms, and impressive growth performance. In the last decade, however, several countries with recently liberalized capital accounts experienced severe financial crises, such as Mexico, Thailand, Korea, Russia, and Argentina. These experiences, especially when combined

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with the recent backlash against globalization, have caused many people to question the benefits of unrestricted capital flows.[1] Could controls on capital flows have prevented these crises, or at least reduced their virulence? This question gained renewed attention when the U.S. government insisted that free-trade agreements with Singapore and Chile include strict restrictions on their ability to use capital controls in the future.

Although there continues to be widespread disagreement on the desirability and feasibility of certain types of capital controls, such as a Tobin-style tax on currency transactions or limits on capital outflows during crises, there is fairly widespread support for market-based taxes on short-term capital inflows. For example, *The Economist* concluded a survey on global finance with the statement: "…some kinds of restriction on inflows (not outflows) of capital will make sense for many developing countries."[2] The most well known example of a market-based tax on capital inflows is the *encaje* adopted by Chile from 1991 to 1998. Even the IMF, formerly the bastion of capital market liberalization, has cautiously supported these sorts of controls. For example, Stanley Fischer, former First Deputy Managing Director of the IMF writes: "The IMF has cautiously supported the use of market-based capital inflow controls, Chilean style."[3] IMF officials have even suggested that other emerging markets, such as Russia, could benefit from adopting similar capital controls in certain circumstances.[4]

A series of empirical studies have supported this sea-change in attitudes by providing fairly positive or neutral assessments of the Chilean capital controls. Although there is some variation in the results, most studies conclude that the capital controls shifted the composition of capital inflows to a longer maturity and provided a small increase in monetary policy flexibility, but had minimal effect on other variables (such as the total volume of capital inflows or exchange rate). These studies suggest that the only costs of the controls were relatively minor, such as any deadweight loss from the government establishing and monitoring the system, or from firms attempting to evade the controls. In other words, the general interpretation of this body of empirical work is that the Chilean capital controls generated some small economic benefits, but no significant economic costs.

Managers of small and medium-sized companies in Chile, however, have a different interpretation. They claim that the capital controls made it substantially more difficult to obtain external financing. Edwards (1999) reports that between 1996 and 1997 (during the *encaje*) the costs of dollar borrowing for smaller firms exceeded 20% per year, while larger firms could access international markets at a cost of only 7–8%. It is not surprising, however, that smaller firms in Chile faced a higher cost of external capital than larger firms. A large body of theoretical literature explains why asymmetric information problems, which tend to be greater in smaller and younger firms, will raise the cost of external capital relative to that for internal capital. Moreover, a large body of empirical literature documents that firms' investment decisions tend to be affected by their internal sources of funds in a range of countries, especially for smaller firms.

There are a number of reasons, however, why the Chilean capital controls may have increased financial constraints for smaller firms. First, many Chilean firms responded to the controls by adopting alternate forms of financing that were not subject to the tax (such as issuing ADRs or obtaining direct credit from foreign suppliers). These alternative financing sources were not only

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[1] For an excellent study on the effects of financial globalizations, see Prasad et al. (2003).

[2] *The Economist,* "A Cruel Sea of Capital: A Survey of Global Finance." 05/03/98, pg 24.

[3] Fischer (2002).

[4] For example, see the interview with John Odling-Smee, Director of the IMF's European II Department, in the March 17, 2003, *IMF Survey.*

more costly than those used before the *encaje*, but often unavailable to smaller firms with less established reputations. Second, and closely related, there were a number of loopholes to the capital controls, but finding and developing the mechanisms to utilize these loopholes required an up-front fixed cost. For larger firms, this fixed cost could be spread across a greater volume of financing, thereby increasing the return to finding the loopholes. As a result, larger firms were more likely than smaller firms to search for, find, and utilize these exceptions to the *encaje*, and therefore obtain a relatively lower cost of capital.

Third, banks tended to have less flexibility avoiding the *encaje*, since banks are more closely monitored by the government than other firms. As a result, the cost of borrowing from banks may have risen more than the cost of borrowing from other institutions that could more easily evade the capital controls. Since smaller firms tend to be more reliant on bank loans for financing than larger firms, this additional cost would have fallen disproportionately on smaller firms, especially since they were less likely to be able to find alternate financing sources. Fourth and finally, there is some evidence that the capital controls shifted capital inflows to longer maturities. Since smaller firms have a harder time borrowing long-term than larger firms, any increase in lending maturities could have disproportionately affected the ability of small firms to obtain financing. This effect could have occurred whether the small firms received capital inflows directly, or whether they borrowed from banks (which experienced a lengthening of their maturities and attempted to match the maturities of their assets and liabilities).

Several studies of the *encaje* mention that the capital controls might have made it relatively more difficult and expensive for smaller companies to raise financing.[5] None of these studies, however, has made any attempt to assess whether these effects were significant or economically important. If the capital controls substantially increased the costs and/or constrained the ability of smaller firms to obtain capital for productive investment, this inefficient allocation of capital and resources may have reduced growth and productivity in Chile. Although the *encaje* may also have had important benefits, countries considering the adoption of Chilean-style capital controls should carefully evaluate this potential cost. This could be important for emerging markets in which small and new firms can be important sources of job creation and economic growth.[6]

Therefore, this paper assesses whether the Chilean capital controls had this cost of increasing financial constraints for smaller, publicly-traded firms. It builds on the extensive literature on firm-financing constraints to test whether investment decisions in smaller and medium-sized traded firms are more dependent on internal finance than in large firms, and if any such differences increased during the period that the *encaje* was in place. It focuses on an Euler-equation framework, but also performs a series of tests using other models. The implementation of the *encaje* in 1991 and its removal in 1998 provide a natural experiment by which to examine any time-series variation in financing constraints for different types of Chilean firms.

The results indicate that investment in smaller, publicly-traded firms was significantly affected by internal finances during the period of the *encaje*, and that these financial constraints decreased as firm size increased. During the periods before and after the *encaje*, however, neither small nor large traded firms appear to have been financially constrained, and there is no significant relationship between firm size and financial constraints. These results are highly robust to an extensive series of sensitivity tests. Although this effect of the Chilean *encaje* is only one factor

[5] See Gallego et al. (1999) and Edwards (1999).

[6] McMillan and Woodruff (2002) provide evidence of the importance of small firms and startups in promoting job creation and economic growth in the transition economies.

that countries should consider when evaluating whether or not to adopt controls on capital inflows, it does suggest an important cost of these capital controls.

The remainder of the paper is as follows. Section 2 provides background information on the Chilean capital controls and briefly surveys the empirical literature assessing the macroeconomic impact of these controls. Section 3 discusses the previous literature testing for the presence of firm-financing constraints and then develops the Euler-equation framework that is used as the baseline in the remainder of the paper. Section 4 discusses the data set and examines trends in several variables for evidence of any impact of the *encaje* on different-sized, publicly-traded firms. Section 5 discusses several econometric issues and then estimates the base model. It also reports an extensive series of sensitivity tests. Section 6 concludes.

2. Background on the Chilean capital controls

Chile, as well as many other emerging markets, experienced a surge of capital inflows between 1988 and 1991. Largely in response, the Chilean government enacted a series of capital account restrictions in 1991. The *encaje*, or unremunerated reserve requirement (URR), was a key component of these restrictions. It required that a fraction of certain types of capital inflows must be deposited at the central bank in a non-interest bearing account for a fixed term. The *encaje* was initially set at a rate of 20% and only applied to fixed-income securities and foreign loans, excluding trade credits (as long as the shipment occurred within 6 months). The tax did not initially include portfolio flows or FDI. The holding period at the central bank was initially equal to the loan's maturity, with a minimum of 90 days and maximum of 1 year. Investors were also given the option of either making the deposit at the central bank, as described above, or paying an up-front fee equivalent to the interest cost of the URR.

The primary goal of the *encaje* was to moderate the appreciation of the Chilean peso in order to maintain competitive export prices. A secondary goal was to regulate short-term capital inflows, especially from banks and institutional investors, and therefore moderate the buildup of speculative short-term liabilities. A final goal was to increase the ability of the central bank to effectively use monetary policy by creating a wedge between domestic and foreign interest rates. This concern was particularly important because the government sought to reduce inflation, which was becoming increasingly difficult as any attempt to reduce demand by raising interest rates was often overwhelmed by the expansionary effect of capital inflows. Reflecting this combination of goals, the capital controls initially only covered short-term borrowing and debt, and only later were expanded to cover equities and other types of portfolio flows. A primary focus of the *encaje* were not initially to reduce Chile's vulnerability to contagion or global financial turmoil — a central motivation for other countries considering these controls.

During the 7-year period after the *encaje* was enacted, it was continually modified.[7] These changes were aimed mainly at closing loopholes frequently discovered by investors and borrowers, although occasionally were designed to adapt to changes in the economic environment. These modifications are summarized in Appendix A and include virtually all aspects of the *encaje*.

[7] During this period, there were also modifications to a variety of capital account restrictions in addition to the *encaje*. For example, there were changes to maturity requirements, minimum size restrictions, and minimum solvency limitations for the issuance of bonds or equity abroad. There was also a continual liberalization of controls on capital outflows. See Gallego et al. (1999), Simone and Sorsa (1999), and Ariyoshi et al. (2000) for detailed information on this evolution of capital account restrictions.

Table 1
Cost of the unremunerated reserve requirement (URR)

	Cost in % per year[a]		
	3-month borrowing	6-month borrowing	1-year borrowing
1991	1.5	1.5	1.5
1992 (Jan–Apr)[b]	1.1	1.1	1.1
1992 (May–Dec)[b]	7.7	3.9	1.9
1993	6.9	3.4	1.7
1994	9.4	4.7	2.4
1995	10.3	5.1	2.6
1996	9.4	4.7	2.4
1997	9.4	4.7	2.4

Source: From Ariyoshi et al. (2000), box 4 in Appendix I.

[a] In each year, the approximate cost of the URR tax rate (as a percent of loanable funds) is calculated using the equation: $t=\frac{r(i^* + s)T/(1+r)}{D}$, where t is the implied tax rate, r is the URR rate, i^* is the nominal interest rate for the currency in which the URR is constituted, s is the premium applied to the investor when borrowing funds to cover the URR (i.e. the country risk premium plus specific credit risks for the investor), T is the duration of the URR, and D is the duration of the foreign investment.

[b] The division for 1992 represents changes in the duration of the URR from between 3 months and 1 year (based on the maturity of the loan) to a minimum of 1 year.

In 1998, as capital flows to emerging markets dwindled, the *encaje* was suspended. Some of the most noteworthy changes to the *encaje* between 1991 and 1998 were:

- 1992: Fraction of the capital inflow deposited in the central bank was increased from 20% to 30%. The minimum holding period was extended to 1 year.
- 1995: Coverage was extended to include secondary ADRs. Also, the deposit in the central bank (and calculation of the corresponding fee) had to be made in U.S. dollars. This substantially raised the cost of the *encaje*.[8]
- 1996: Coverage was extended to include "speculative investment from FDI" (defined as FDI which "does not increase productive capacity").
- 1998: Fraction of the capital inflow deposited in the central bank was decreased to 0%.

During the period from 1991 to 1998, the actual cost of the *encaje* depended on the maturity and/or permanence of the capital inflow, as well as the opportunity cost of the funds, with shorter-term inflows subject to a higher implicit tax. Table 1 reports estimates of the actual cost of the *encaje* for borrowings of different maturities between 1991 and 1998. It shows that although the cost fell quickly over time, it was still substantial for longer-term borrowing. For example, in 1997 the cost of the URR was 9.4% per year for 3-month borrowing and 2.4% for 1-year borrowing. The table also shows that the changes in the *encaje* lead to substantial fluctuations in its cost over time. The cost increased in the first few years after the *encaje* was initially implemented, reaching a peak of 10.3% for 3-month borrowing in 1995. The cost fell slightly after 1995, but remained high throughout the entire *encaje* period.

[8] Before 1995, investors could choose the currency of the deposit. Many chose the yen, which reduced the implicit cost of the *encaje* as the yen depreciated against the dollar. Simone and Sorsa (1999) estimate that changing the currency requirement raised the implicit cost of the URR by 50–100%.

There is some evidence, however, that although the tax rate was highest between 1994 and 1997, the actual impact on capital inflows may have declined over time. The central bank's attempts to continually close loopholes and tighten the *encaje* were generally effective in the short term, but over time investors and firms found new ways to evade the controls and any new restrictions.[9] One of the most common strategies was to shift capital inflows to sources that were not currently subject to the *encaje* (such as redefining capital inflows as trade credit). Central bank data show that in 1992, the URR covered about half of total gross inflows, but in subsequent years coverage declined to 24%. Despite this evasion, the large revenues collected by the Chilean government suggest that the *encaje* still affected a substantial volume of capital inflows. Gallego et al. (1999) report that between June 1991 and September 1998, collection of the URR (including the money in reserves as well as the equivalent fees) increased central bank reserves by an average of 2.0% of GDP, or 40% of the average capital account surplus.

But what was the impact of this tax? Measuring the impact of the *encaje* is complicated by a number of factors, such as: accurately measuring short-term inflows (since firms shifted financing to trade credit, which is generally not included in short-term inflows); controlling for changes in the macro-environment (and especially the general increase in capital flows to emerging markets during this period); and controlling for simultaneous changes in other Chilean policies that could affect capital flows (such as the liberalization of capital outflows and improvements in banking system supervision and regulation). Despite these methodological challenges, a number of empirical papers have attempted to measure how the *encaje* impacted a series of macroeconomic variables.[10] The studies use a range of modeling strategies, definitions, and econometric methodologies, and reach several general conclusions. First, there is no evidence that the *encaje* affected the exchange rate. Second, there is little evidence that the capital controls protected Chile from the shocks emanating from other emerging markets during the Mexican, Asian, Russian, and Brazilian crises. Third, there is some evidence that the *encaje* had no significant effect on the total volume of capital inflows (although this result is subject to the caveat that it is extremely difficult to construct the counterfactual of what the volume of capital inflows would have been without the *encaje*). Fourth, there is some evidence that the capital controls shifted the composition of capital inflows to longer maturities. Fifth and finally, there is some evidence that the *encaje* raised domestic interest rates by creating a wedge between domestic and foreign interest rates (although there is no agreement on whether this was a short- or long-run effect). Therefore, this series of results can be summarized as suggesting that the Chilean capital controls may have had a positive, albeit weak, impact on Chile's macroeconomy.

Despite the attention paid to the impact of the *encaje* on Chile's macroeconomy, there has been almost no analysis of how the capital controls impacted micro-level variables, such as individual firms and their cost of capital.[11] The only paper that partially addresses this question, albeit indirectly, is Gallego and Loayza (2001). They provide a detailed review of the rapid growth in

[9] See Simone and Sorsa (1999) for discussion of the effectiveness of the capital controls and evidence on their avoidance. Some authors, such as Cowan and De Gregorio (1998), argue that their "power" declined between 1995 and 1997 as evasion increased. Other authors, such as Gallego et al. (1999), however, argue that their "power" increased steadily over time until the controls were removed in 1998. Valdés-Prieto and Soto (1998) argue that the *encaje* was most effective after 1995.

[10] Simone and Sorsa (1999) provide an excellent survey of the empirical work on this subject, as well as a detailed discussion of the methodological and econometric problems. Noteworthy examples of these empirical studies are: De Gregorio et al. (2000), Edwards (2000), Gallego et al. (1999), and Valdés-Prieto and Soto (1998).

[11] Work done at the same time as this paper by Gallego and Hernández (2003) is examining how the Chilean capital controls affected financial expenditures and balance sheets of Chilean firms.

Chilean financial markets over the past 3 decades, and then examine how this growth affected financial development for publicly-listed firms between 1985 and 1995. In the section most closely related to this paper, Gallego and Loayza use a Tobin's q model and find evidence that although firms were financially constrained during the late 1980s, these constraints were reduced in the first half of the 1990s. Their paper does not explicitly discuss the impact of the *encaje* on financial constraints, and they focus on measuring average financing constraints in Chile, without testing for differences between small and large firms. Therefore, the impact of the *encaje* on the financing constraints of small versus large Chilean firms remains an open question.

3. Testing framework and methodology

3.1. Background

Modigliani and Miller (1958) show that under certain conditions, including frictionless capital markets, firms should face the same cost for internal and external finance. As a result, a firm's liquidity and capital structure should not affect its investment decisions. Since this seminal article, however, there has been an extensive theoretical literature explaining why informational asymmetries and incentive problems can make external financing more expensive than internal financing, as well as an extensive empirical literature testing these propositions.[12] Although results vary somewhat across studies, the majority of evidence suggests that firms predicted to have greater asymmetric information and incentive problems, and therefore be more financially constrained, tend to have a greater sensitivity of investment to fluctuations in internal funds.

Most closely related to this paper, studies of both developed countries and emerging markets have found extensive evidence that smaller firms tend to be more financially constrained than larger firms.[13] Other studies, such as Love (2003) and Laeven (2003), provide cross-country evidence that firms are less financially constrained in countries with more developed or liberalized financial markets. Harrison et al. (2004) is the only study which directly considers the impact of capital controls on firm-financing constraints, although this is only a short extension of their more detailed analysis of capital flows and financing constraints. They use an Euler-equation model and find that "restrictions on payments for capital account transactions" increase firm-financial constraints. Their measures of capital controls are based on dummy variables constructed by the IMF, however, which are highly problematic.[14] In fact, these IMF dummy variables do not show an increase in the Chilean capital controls when the *encaje* was enacted. Using a very different approach and methodology, Desai et al. (in press) show that capital controls affect the asset allocation, financing, transfer pricing, and dividend policy of U.S. multinational affiliates. Although they do not directly test for the existence of firm-financing constraints, their results suggest that capital controls distort investment decisions.

Although this empirical literature testing for the microeconomic effects of capital controls and existence of financial constraints is extensive, there continues to be disagreement on the appropriate testing framework. The earliest work on this subject estimated investment equations based on Tobin's q. This methodology, however, has been subject to rather extensive criticism

[12] Stein (2001) and Hubbard (1998) provide excellent surveys of this theoretical and empirical literature.

[13] See Schiantarelli (1995) for a more detailed review of relevant studies.

[14] Problems with these IMF measures of capital account restrictions have been well documented in other work. For example, see Edison et al. (2002) or Kaminsky and Schmukler (2003).

and has a number of weaknesses.[15] For example, since it is virtually impossible to measure marginal q (as required by the theoretical models), most studies use average q, which will only equal marginal q under fairly restrictive assumptions. Also, if stock markets are inefficient, observed stock market valuations (which are a component of q) may diverge from the manager's valuation of the marginal return on capital — an issue which is especially problematic for emerging markets where stock markets are less developed. If marginal q is mismeasured for any reason, then the estimated coefficients on the financial constraints' variable (or other explanatory variables) could capture shocks to investment opportunities instead of financial constraints.[16] Also, the assumptions underlying the basic q-theory of investment, such as convex adjustment costs or value maximization, may not hold.

Instead of using a Tobin's q framework to test for firm-financing constraints, more recent work has focused on direct estimation of Euler equations. This paper will follow this recent trend in the literature and focus on an Euler-equation based model of financing constraints. A section in the sensitivity analysis, however, will also report results based on a Tobin's q framework. The results based on the Tobin's q framework are extremely close to those obtained using an Euler-equation framework. The remainder of this section derives the central estimating equation.

3.2. The Euler-equation methodology

The following derivation closely follows that in Harrison et al. (2004), Love (2003), and Laeven (2003), all of which build on Gilchrist and Himmelberg (1999). Assume that each firm maximizes its present value, which is the expected discounted value of dividends, subject to a capital accumulation constraint. The optimization problem is:

$$V_t(K_t, \xi_t) = \max_{\{I_{t+s}\}_{s=0}^{\infty}} D_t + E_t\left[\sum_{s=1}^{\infty} \beta_{t+s-1} D_{t+s}\right] \tag{1}$$

subject to:

$$D_t = \Pi(K_t, \xi_t) - C(I_t, K_t) - I_t \tag{2}$$

$$K_{t+1} = (1-\delta)K_t + I_t. \tag{3}$$

$$D_t \geq 0. \tag{4}$$

Variables are defined as: t is the current time period; s represents increments to t; K_t is the capital stock at the start of period t; ξ_t is a productivity shock; D_t is the dividend paid to shareholders over the period t; $E_t[]$ is the expectations operator conditional on information available at time t; β is a discount factor; Π is the profit function (already maximized with respect to variable costs); C is the adjustment cost function; I_t is investment over the period t; and δ is the depreciation rate for capital.

[15] See Schiantarelli (1995) or Hubbard (1998) for a more detailed description of these issues.

[16] See Erickson and Whited (2000) for a discussion of problems with measurement error in q.

Next, define marginal q_t (measured at the end of period t or the start of $t+1$) as the increase in the firm's value (over period $t+1$) from one additional unit of capital (in place at the start of $t+1$):

$$q_t = \left(\frac{\partial V}{\partial K}\right)_{t+1}. \tag{5}$$

If λ is the multiplier for the constraint in Eq. (4), then the first-order condition combined with the definition in Eq. (5) is:

$$\left(\frac{\partial V}{\partial I}\right)_t = -(1+\lambda_t)\left[\left(\frac{\partial C}{\partial I}\right)_t + 1\right] + \beta_{t+1}E_t q_t = 0 \tag{6}$$

and the envelope condition is:

$$\left(\frac{\partial V}{\partial K}\right)_t = q_{t-1} = (1+\lambda_t)\left(\frac{\partial D}{\partial K}\right)_t + (1-\delta)\beta_{t+1}E_t q_t. \tag{7}$$

Combining Eqs. (6) and (7) to eliminate q_t and q_{t-1} yields the Euler equation:[17]

$$1 + \frac{\partial C(I_t, K_t)}{\partial I_t} = \beta_t E_t\left[\frac{1+\lambda_{t+1}}{1+\lambda_t}\left\{\frac{\partial \Pi(K_{t+1}, \xi_{t+1})}{\partial K_{t+1}} + (1-\delta)\left(1 + \frac{\partial C(I_{t+1}, K_{t+1})}{\partial I_{t+1}}\right)\right\}\right]. \tag{8}$$

The key variable of interest in the following analysis is $(1+\lambda_{t+1}/1+\lambda_t)$, which is the relative shadow cost of external financing in period $t+1$ versus period t (i.e., the measure of financial constraints). More specifically, in perfect capital markets, $\lambda_t = \lambda_{t+1}$. If the shadow cost of external funds is higher today (at t) than tomorrow (at $t+1$), however, then $(1+\lambda_{t+1}/1+\lambda_t) < 1$ and the firm is "financially constrained."

Next, to estimate Eq. (8), assume that the term measuring financial constraints can be written as a function of firm-specific financing constraints and the firm's cash stock at the start of the period, with the impact of the firm's cash stock allowed to vary with firm size:[18]

$$\frac{1+\lambda_{t+1}}{1+\lambda_t} = \psi_{0i} + (\psi_1 + \psi_2 \text{Size}_{it})\left(\frac{\text{Cash}}{K}\right)_{it}. \tag{9}$$

Also, define MPK_t as the marginal profit of capital, net of adjustment costs and financing costs (which is the term in {} in Eq. (8)). If production follows a Cobb–Douglas function, which can have fixed costs and quasi-fixed factors of production (other than K), then MPK_t can be expressed as:

$$\text{MPK}_{it} = \vartheta_i + \vartheta_{1,t}\left(\frac{\text{Sales}}{K}\right)_{it}, \tag{10}$$

where ϑ_i is a firm-fixed effect, $\vartheta_{1,t}$ is the ratio of capital's share in production to the markup, and Sales is total sales.[19]

[17] I also follow previous literature and set $(\partial C/\partial K)_t = 0$, since this is a second-order effect. See Love (2003) for a detailed explanation and evidence supporting this assumption.

[18] The sensitivity analysis also shows that redefining financial constraints as a function of cash flow in the current (or last period) has no significant impact on the results.

[19] Gilchrist and Himmelberg (1999) provide a proof of this derivation. I have added the firm-fixed effect.

In order to obtain a closed-form solution, it is necessary to specify the adjustment cost function. I make the standard assumptions of linear homogeneity in capital and investment. I also extend this standard function slightly to allow for persistence in adjustment costs, period-specific shocks, and a firm-specific level of investment that minimizes adjustment costs:

$$C(I_t, K_t) = \frac{1}{2\alpha_1}\left[\left(\frac{I}{K}\right)_t - \alpha_2\left(\frac{I}{K}\right)_{t-1} - \alpha_i + \alpha_t\right]^2 K_t, \tag{11}$$

where α_1 and α_2 are constants, α_i is a fixed effect for each firm i, and α_t are period-specific effects. Therefore, the marginal adjustment cost of investment is:

$$\left(\frac{\partial C}{\partial I}\right)_t = \frac{1}{\alpha_1}\left[\left(\frac{I}{K}\right)_t - \alpha_2\left(\frac{I}{K}\right)_{t-1} - \alpha_i + \alpha_t\right]. \tag{12}$$

Finally, since $E(1+\lambda_{t+1}/1+\lambda_t)\cong 1$, it is possible to use a first-order Taylor approximation around the means to rewrite Eq. (8). Continuing to assume rational expectations, and inserting the terms defined in Eqs. (9), (10) and (12) into Eq. (8) yields the central estimating equation for this paper:

$$\left(\frac{I}{K}\right)_{it} = \theta_0 + \theta_1\left(\frac{I}{K}\right)_{i,t-1} + \theta_2\left(\frac{\text{Sales}}{K}\right)_{it} + \theta_3\left(\frac{\text{Cash}}{K}\right)_{it} + \theta_4\left(\frac{\text{Cash}}{K} * \text{Size}\right)_{it} + f_i + d_t + \varepsilon_{it}. \tag{13}$$

Therefore, if Size is a dummy variable equal to 1 for large firms, then a test if firm-financing constraints are significantly lower for large firms than for small firms is a test of the null hypothesis that $\theta_4=0$ against the alternative hypothesis that $\theta_4<0$. Or, if Size is a continuous measure of firm size, then a test if financial constraints exist for small firms and decrease with firm size is a test of the null hypothesis that $\theta_3=0$ and $\theta_4=0$ (against the alternative hypothesis that $\theta_3>0$ and $\theta_4<0$).

This testing strategy based on the Euler equation has important advantages over other methodologies, such as the q-based methodology. It circumvents the numerous problems related to measuring marginal q. The model should therefore better control for the impact of shocks to future profitability on investment decisions, reducing any bias to the estimated coefficients on financial constraints. Also, the Euler-equation framework permits the explicit modeling of the shadow cost of financing as a function of a firm's cash stock (or other proxies for financial constraints).

Despite these important advantages, this Euler-equation methodology also has several shortcomings. First, it requires imposing a high degree of structure on the estimating equation, such as the normalization technique and form of the financial constraints. Results can be extremely sensitive to model specification, and a rejection of the "no financial constraints" hypothesis could occur for a number of reasons other than capital market imperfections. Second, tests based on the Euler-equation methodology have poor small sample properties. Third, this methodology relies on period-by-period restrictions derived from the firm's first-order conditions, so that it may not capture financial constraints that exist across periods. Fourth, the model assumes one discount rate for all firms and time periods, and although some of this effect may be captured in the firm- and period-specific effects, any residual correlation between discount rates and cash stocks or flows could bias estimates. Finally, this approach does not yield structural

Table 2
Sample information for Chilean firms

	1988	1990	1992	1994	1996	1998	2000
Number of firms [a]							
Food/tobacco	5	5	10	12	16	23	27
Petroleum	0	0	0	0	0	0	0
Consumer durables	1	2	3	4	4	6	7
Textiles/trade	1	1	1	2	2	9	14
Basic industry/capital goods	3	5	10	13	14	18	29
Construction	2	3	4	5	9	9	11
Transportation	1	1	4	4	6	6	7
Utilities	6	8	14	16	18	24	29
Services	0	0	0	0	0	1	7
Leisure	0	0	0	0	0	3	7
This paper's sample	19	25	46	56	69	99	138
Financial and excluded companies	2	2	9	12	19	29	38
Total firms in Worldscope	21	27	55	68	88	128	176
Listed firms in Chile [b]	205	215	245	279	283	287	258
Worldscope as % of listed firms	10%	13%	22%	24%	31%	45%	68%
Total market capitalization (in billions of pesos) [c]							
This paper's sample	0.8	2.6	8.7	19.5	19.3	17.2	26.6
Full Worldscope sample	0.8	2.7	8.8	20.4	21.6	20.4	33.7
Total Chilean market [b]	1.7	4.6	11.3	27.3	28.0	24.5	34.7
Worldscope as % of total market	45%	58%	78%	75%	77%	83%	97%

[a] SIC industry definitions are based on two-digit SIC groups defined in Campbell (1996). More specifically, SIC codes for each group are: Food/tobacco (1, 2, 7, 9, 20, 21, 54); Petroleum (13, 29); Consumer durables (25, 30, 36, 37, 39, 50, 55, 57); Textiles/trade (22, 23, 31, 51, 53, 56, 59); Basic industry/capital goods (8, 10, 12, 14, 24, 26, 28, 33–35, 38); Construction (15–17, 32, 52); Transportation (40–42, 44, 45, 47); Utilities (46, 48, 49); Services (72, 73, 75, 76, 80–82, 87, 89); Leisure (27, 58, 70, 78, 79, 83–86, 88); and Financial (60–69).

[b] All companies listed on the Bolsa de Comercio de Santiago. Source: *Emerging Stock Markets Factbook 1996* and *Emerging Stock Markets Factbook 2000*.

[c] Market capitalization calculated as of 12/31 in current year.

estimates of the key variables, so it is necessary to make assumptions about additional parameters to obtain certain coefficient estimates.

4. Dataset

The dataset used to estimate the model developed in Section 3 is based on the Worldscope, September 2002 CD-ROM.[20] I include all non-financial firms located in Chile that reported any information in the database between 1988 and 2002. Since the database only reports financial data for the last 10 years, I augment the data with information from the Worldscope, May 1999 CD-ROM, excluding any firms for which the time series between the two sources is inconsistent. I also exclude extreme outliers and unrealistic observations for the key variables used to estimate Eq. (13). Each of these steps is described in detail in Appendix B.

[20] The Worldscope-Disclosure database is produced by Thomson Financial and includes detailed profiles, financial data, and market information for about 32,000 companies, representing about 96% of the world's market capitalization. See Forbes (2004) for more detailed information on the database.

Table 3
Sample statistics across periods

	Pre-*encaje*	*Encaje*	Post-*encaje*
Tradable firms as % of sample [a]	57.7%	57.9%	57.2%
Cash stock/capital stock [b]	0.124	0.181	0.188
Leverage [b]	0.228	0.215	0.242
Net income (inflation adjusted, in millions of pesos)	30,100	30,900	33,400
Sales (inflation adjusted, in millions of pesos)	14,100	12,600	12,700
Short-term debt/total debt [b]	0.436	0.431	0.449
Tobin's q [b]	1.370	1.445	0.876

Notes: All variables are sample medians. Pre-*encaje* period is 1988–91; *encaje* period is 1992–97; post-*encaje* period is 1998–2001.

[a] Nontradable firms defined as firms whose primary SIC code is in the following industries: construction, transportation, utilities, services or leisure (firms in the financial sector and public administration were already excluded). All other firms are defined as tradable.

[b] See Table 4 for variable definitions.

The Worldscope database only reports information that is publicly available, so most of the sample consists of publicly-traded companies. This has the obvious disadvantage that since many smaller firms are not publicly listed, they are underrepresented in the dataset. As a result, the analysis in this paper can only be interpreted as evidence of how the *encaje* affects different-sized, publicly-traded firms, and not necessarily as evidence on how the *encaje* affected small, private firms. Focusing mainly on publicly-listed companies, however, has the advantage that disclosure requirements for public listing are fairly stringent in Chile (especially compared to other emerging markets) so that the financial statistics in the dataset are more comprehensive and reliable than for most non-public firms.

Table 2 reports the number of firms by broad industry group for every other year in the sample. The number of firms is substantially lower at the beginning of the period — largely because Worldscope is a fairly new data set and coverage has steadily improved over time. Therefore, although firms continuously "enter" the sample, many of these entrants are not newly incorporated firms. The middle of the table reports the number of financial firms included in Worldscope but excluded from the dataset used for this paper, as well as the number of firms included in Worldscope but excluded from this paper due to data availability. The next set of rows reports the total number of firms listed on the Chilean stock exchange, as well as the percent of listed Chilean firms covered by Worldscope in each year of the sample. In 1988, Worldscope covered only 10% of listed firms in Chile, but by 2000 it covered 68% of listed firms. The bottom of the table also reports the total market capitalization of firms included in the sample, firms covered by Worldscope, and firms in the entire Chilean market. Worldscope coverage has increased from 45% of Chilean market capitalization in 1988 to 97% in 2000. These comparisons indicate that although the Worldscope database covers most of the Chilean market when judged by size, it under-represents smaller, publicly-listed firms.

Since the sample size changes across time, Table 3 examines how key statistics used in the paper's analysis change between the period before the *encaje* was enacted, the period that the *encaje* was in place, and the period after the *encaje* was lifted.[21] The table suggests that most variables are fairly stable across the different periods. The one exception is Tobin's q (which falls

[21] The sensitivity analysis also includes tests using the same sample of companies across each time period in order to correct for changes in sample composition across time.

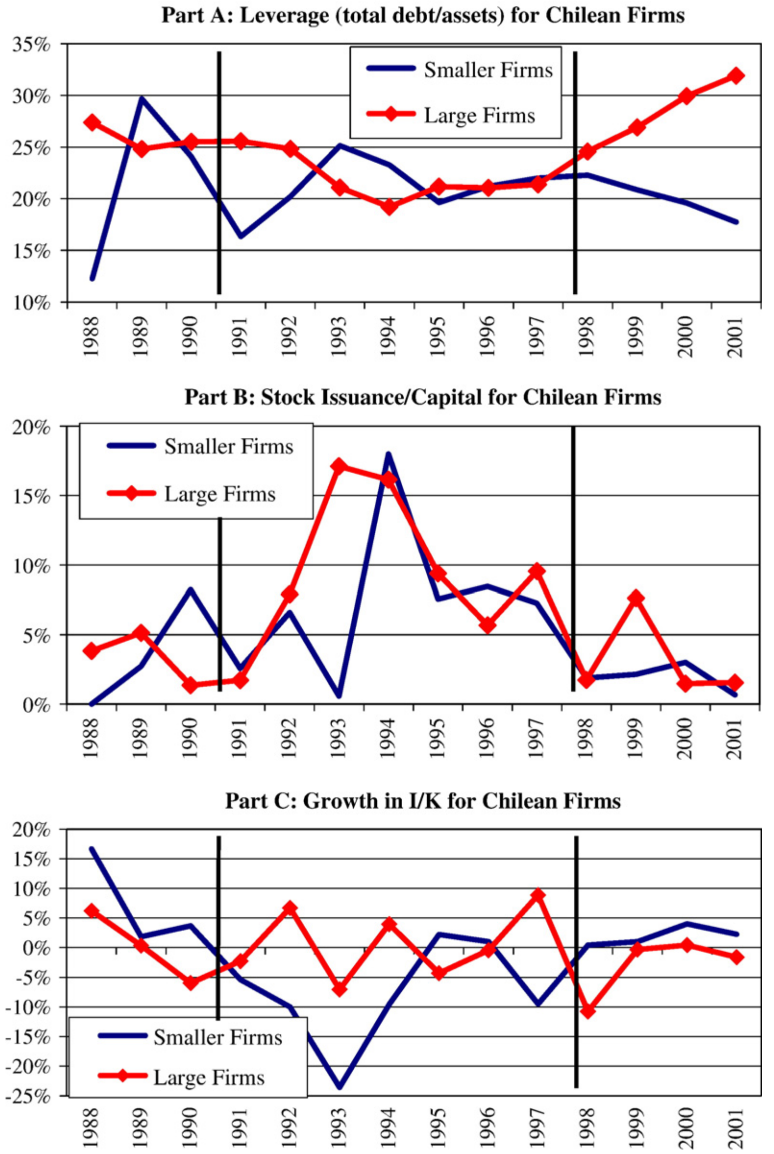

Fig. 1. Chilean firms before, during and after the *encaje*.

during the post-*encaje* period). This undoubtedly reflects a decline in the Chilean stock market (as well as many emerging markets) in the 1998–2001 period.

As a preliminary analysis of this dataset and the impact of the *encaje* on firm-financing constraints in Chile, Fig. 1 graphs several relevant variables between 1988 and 2001.[22] Variable definitions are provided in Table 4. I divide the sample into smaller and large firms, based on a cutoff of inflation-adjusted assets of 100 billion pesos (equivalent to approximately

[22] See Forbes (2003) for more detailed analysis of graphical trends in this dataset.

Table 4
Variable definitions

Variable	Code	Definition
Assets (total)		Sum of total current assets, long-term receivables, investment in unconsolidated subsidiaries, other investments, and net property, plant, and equipment. Adjusted for inflation. Calculated at start-of-period. Expressed in logarithmic form in regressions.
Capital stock	K	Property, plant, and equipment (net of depreciation) at the end of the period, less investment (defined below), plus depreciation and amortization expenses. Calculated at start-of-period.
Cash flow		Operating income, which is defined as total sales and revenues less operating expenses. Weighted by K in regressions.
Cash stock	Cash	Cash and equivalents, which is money available for use in normal operations, including short-term investments. Weighted by K in regressions. Calculated at start-of-period.
Debt (total)		All interest bearing and capitalized lease obligations. The book value of the sum of long- and short-term debt. Calculated at start-of-period.
Investment (capital expenditure)	I	Funds used to acquire fixed assets, other than those associated with acquisitions. Weighted by K in regressions.
Leverage		The ratio of total debt (defined above) to total assets (defined above). Calculated at start-of-period.
Market value (or market capitalization)		Market price at year-end multiplied by common shares outstanding. For companies with more than one type of common/ordinary share, market value represents the total market value of the company.
Sales (net)	Sales	Gross sales and other operating revenues less discounts, returns and allowances. Weighted by K in regressions.
Short-term debt		The portion of debt payable within one year, including the current portion of long-term debt and sinking fund requirements of preferred stock or debentures. Calculated at start-of-period.
Stock issuance		Proceeds from the sale or issuance of stock over the past year.
Tobin's q	q	Defined as total debt (defined above) plus total market value (defined above) divided by the book value of total assets (defined above). Calculated at start-of-period.

Notes: All variables calculated in Chilean pesos.

\$250 million).[23] Part A graphs leverage (total debt to assets) and shows that smaller public firms had leverage ratios comparable to larger firms during the later part of the *encaje*. Leverage ratios for large firms, however, fell during the initial years of the *encaje* and then increased substantially after the *encaje* was removed. This could indicate that large firms shifted to non-traditional financing sources during the *encaje* (i.e. financing not included in leverage ratios and excluded from the tax), and then returned to traditional debt financing after the *encaje* was removed. Small firms do not show evidence of similar patterns, and actually show a decline in leverage ratios after the *encaje*.

Part B of Fig. 1 graphs firm stock issuance as a share of beginning-of-period capital. Stock issuance increased dramatically for both small and large public firms during the initial stages of the *encaje* from 1991 to 1994, but then declined by a comparable amount during the later half of the *encaje* from 1994 to 1998. These trends are driven by changes in the regulations governing secondary ADRs. Secondary ADRs were not subject to the URR between 1991 and 1994, thereby providing a relatively cheap and legal method of raising external financing. From 1995 until the *encaje* was lifted, however, secondary ADRs were subject to the same tax as other capital inflows, so that they were no longer more attractive sources of external financing. Patterns of stock issuance for small and large firms, however, are fairly similar and dominated by these aggregate trends.

[23] Inflation adjustment based on the consumer price index as reported in line 22864…zf in IMF (2002) with 1995 as the base year. The assets-based division of 100 billion pesos is chosen because it is close to the sample median of 104 billion pesos. Firms are allowed to switch between small and large each year.

Part C of Fig. 1 graphs investment growth. Investment growth for smaller firms was higher than for large firms both before 1991 and after 1998, but rarely during the period of the *encaje*. In fact, investment growth for smaller firms plummeted after the *encaje* was enacted, while simultaneously increasing for large firms. Moreover, around the time that the *encaje* was removed, investment growth fell for large firms and increased for smaller firms. Although none of the relationships depicted in the graphs controls for changes in investment opportunities or other factors that could affect firm-level investment, they suggest that the *encaje* may have differentially affected investment growth in small and large firms.

5. Estimation and results

This section performs more formal tests than the graphical trends of how the *encaje* affected financing constraints for different-sized, publicly-traded firms in Chile. It begins by briefly discussing several econometric issues in estimating the model developed in Section 3. The middle section presents the base results. The final part of this section summarizes an extensive series of sensitivity tests.

5.1. Econometric issues and estimation methodology

In order to estimate Eq. (13), there are two problems with a simple OLS, fixed-effects, or random-effects estimator.[24] First, many of the variables in each of the estimating equations are likely to be jointly endogenous — i.e. either simultaneously determined with the dependent variable or subject to two-way causality. Second, the presence of the lagged endogenous variable for investment will bias coefficient estimates.

To address these problems, I use a GMM-difference estimator developed by Holtz-Eakin et al. (1990), Arellano and Bond (1991), and Arellano and Bover (1995). This estimator first-differences each of the variables in order to eliminate the firm-specific effects, and then uses lagged levels of the variables as instruments.[25] Two critical assumptions must be satisfied for this estimator to be consistent and efficient. First, the explanatory variables must be predetermined by at least one period. Second, the error terms cannot be serially correlated (or at least must follow a moving average of finite order). More specifically, if $\boldsymbol{X}'_{it}$ is the vector of explanatory variables in Eq. (13) and ε_{it} is the error term, then the two conditions are:

$$E(\boldsymbol{X}'_{it}\varepsilon_{is}) = 0 \text{ for all } s>t, \text{ and} \tag{14}$$

$$E(\varepsilon'_{it}\varepsilon_{i,t-s}) = 0 \text{ for all } s\geq 1. \tag{15}$$

Arellano and Bond (1991) propose two tests for the accuracy of this estimator. First, a Sargan test of over-identifying restrictions, which tests the null hypothesis of no correlation between the

[24] See Beck and Levine (2004) for an excellent discussion of these econometric issues and the advantages and disadvantages of different panel-GMM estimators.

[25] A variant of this estimator is a "GMM-system" estimator developed in Arellano and Bover (1995). This technique estimates the equation in levels using first differences as instruments, as well as the equation in differences using lagged levels as instruments. This system estimator requires not only the two assumptions in Eqs. (14) and (15), but also the additional assumption that there is no correlation between the differenced explanatory variables and the firm-specific effects. This additional assumption, however, is rejected in this paper's dataset and therefore, the GMM-system estimator is not valid for this paper's tests.

Table 5
Base results: no controls for firm size

	Full period: 1988–2001	Pre-*encaje*: 1988–1991	*Encaje*: 1992–1997	Post-*encaje*: 1998–2001
	(1)	(2)	(3)	(4)
$Investment_{t-1}$	0.139**	−0.022	0.148	0.144**
	(0.033)	(0.105)	(0.130)	(0.033)
Sales	0.189**	0.099	0.374**	0.177**
	(0.012)	(0.084)	(0.164)	(0.011)
Cash	0.022	−0.330	0.215	−0.002
	(0.025)	(0.231)	(0.179)	(0.008)
Period dummies[1]	14.8	2.5	3.8	6.5
# observations	594	38	262	294
# firms	114	20	61	113
Sargan test[2]	75.2	14.2	58.1	47.2
Serial correlation[3]	−2.4**	0.5	−1.4	−2.4**

Notes: * is significant at the 10% level and ** is significant at the 5% level. All estimates are based on Eq. (13) without the interaction between Cash and Size.
[1]Period dummies is the Wald statistic of a test of the null hypothesis that the period dummy variables are jointly insignificant.
[2]Sargan test is the χ^2 statistic of a test of the null hypothesis that the over-identifying restrictions are valid. Statistics are based on the two-step estimator, so that the test is adjusted for heteroscedasticity.
[3]Serial correlation is the Z-statistic from a test of the null hypothesis of no second-order serial correlation in the residuals.

instruments and the residuals. Second, a test for different-order serial correlation in the residuals. If this test is unable to reject the null hypothesis of no second-order serial correlation in the differenced equation, then the level variables lagged by one period are valid instruments. If there is evidence of second-order serial correlation, but not third-order (or higher) serial correlation, then the level variables lagged by two periods are valid instruments.

One potential weakness of this GMM-difference estimator, discussed in Griliches and Hausman (1986), is that first differencing can exacerbate any bias resulting from measurement error. An additional potential problem is that the lagged levels of the variables can be weak instruments for the regression estimated in differences. This could not only increase the variance of the coefficient estimates, but in small samples, could also generate biased estimates. This problem is aggravated if the explanatory variables are persistent over time. Staiger and Stock (1997), however, suggest a method of testing if the lagged levels are weak instruments. They recommend regressing the first differences of each of the relevant variables on their instruments, with an F-statistic less than 5 indicating that the instruments are unreliable.

An additional concern with this GMM estimator is that if there is no evidence of firm-specific effects, it is more efficient to estimate the equation in levels (using lagged levels as instruments) than in first differences. There are two methods to test if the firm-specific effects should be included in the specification. First, estimate the model in levels and test for the presence of first-order serial correlation, which would indicate the presence of unobserved firm-specific effects. Second, estimate the model in levels with firm-dummy variables and then test for the joint significance of the firm-dummy variables.

A final consideration with the GMM-difference estimator is whether to focus on first-stage or second-stage robust estimates. The two-stage estimates are more efficient asymptotically, but often generate standard errors that are biased downward (especially in small samples). As a result, the two-stage estimates are less accurate for hypothesis testing, and Arellano and Bond (1991) recommend focusing on the first-stage robust estimates. Therefore, in the analysis below I focus on the first-stage robust estimates, although I continue to estimate each of the specifications using the two-stage robust estimates. As shown in the sensitivity analysis, using the two-stage robust estimates has no impact on the key results and conclusions, although it does tend to reduce the estimated standard errors as predicted. Therefore, focusing on the first-stage estimates is the more conservative set of hypothesis tests and should make it more difficult to find evidence of increased financing constraints for smaller firms during the *encaje*.

5.2. Baseline results for Chile

Table 5 presents a series of initial results on the impact of the *encaje* on firm-financing constraints in Chile without controlling for firm size. I focus on one-stage robust estimates that have been corrected for heteroscedasticity, using a maximum of 3 lags for each of the explanatory variables as instruments.[26] Each of the explanatory variables is assumed to be predetermined instead of strictly exogenous (except the period dummy variables). Column (1) reports estimates of Eq. (13) for the full sample of Chilean firms during the entire sample period from 1988 through 2001. Columns (2) through (4) present results during the period before the *encaje* was in place (from 1988 to 1991), for the period of the *encaje* (from 1992 to 1997), and after the *encaje* was lifted (from 1998 to 2001), respectively.[27] Variable definitions are the same as in Section 4 and are described in detail in Table 4.[28] The period dummy variables are not reported, but the row labeled "Period dummies" reports the Wald statistic of a test of the null hypothesis that the period dummy variables are jointly insignificant. The lower section of the table reports test statistics for the tests discussed in Section 5.1.

Focusing first on the coefficient estimates, the estimates for the financial constraints' variables in Table 5 fluctuate between positive and negative and are never significant at the 5% level. These results suggest that financing constraints were not important for the full sample of public Chilean firms during the periods before, during, and after the *encaje*. The other coefficient estimates generally support theoretical predictions. The coefficients on lagged Investment and Sales are usually positive and significant (with the only exceptions for the pre-*encaje* period when the small sample size limits the power of the estimation.) The period dummy variables also have fluctuating significance.

The test statistics reported in Table 5 support the validity of the model specification and estimation technique. The Sargan test statistics indicate that in each case, it is impossible to reject the null hypothesis that the over-identifying restrictions are valid.[29] Although the test for serial

[26] I focus on estimates using only 3 lags, instead of the full possible instrument matrix, because adding additional lags adds little explanatory power but weakens the power of the instruments. As shown in the sensitivity analysis, using fewer or additional lags has no significant impact on the results.

[27] The *encaje* was enacted in mid-1991 and removed in mid-1998. As shown in Appendix A, however, the *encaje* was implemented in phases and slowly phased out, so that it was substantially less stringent in 1991 and 1998. Adopting this narrower definition of the *encaje* period also leads to a more balanced sample distribution across the periods. Moreover, if the *encaje* had strong effects in 1991 or 1998, then these narrower period definitions should bias results against finding any impact of the *encaje* on financing constraints. Finally, the sensitivity analysis explores the impact of using different period definitions.

[28] There is an active debate on whether cash stock or cash flow is the preferred measure of a firm's liquidity. To be consistent with the most recent work in this literature (such as Harrison et al., 2004; Laeven, 2003; Love, 2003), I focus on cash stock for the base estimates. As discussed in the sensitivity analysis, the central results are unchanged if I use a range of different measures of cash flow. See Love (2003) for an excellent discussion of why cash stock is a better proxy for Cash than cash flow.

[29] I focus on Sargan test statistics based on the two-stage estimates, since these are adjusted for heteroscedasticity. Sargan test statistics based on one-step estimates (which assume homoscedasticity in the errors) yield similar results. The null hypothesis is never rejected in either set of tests.

Table 6
Base results: cash stock interacted with firm size

	Full period: 1988–2001	Pre-*encaje*: 1988–1991	*Encaje* period: 1992–1997	Post-*encaje*: 1998–2001
	(1)	(2)	(3)	(4)
Interaction with large firm dummy				
$Investment_{t-1}$	0.135**	−0.088	0.128	0.145**
	(0.033)	(0.041)	(0.148)	(0.032)
Sales	0.180**	0.050	0.343**	0.170**
	(0.015)	(0.092)	(0.124)	(0.018)
Cash	0.027	−0.149	0.797**	−0.008
	(0.034)	(0.179)	(0.300)	(0.015)
Cash* size dummy	−0.022	−0.357**	−0.713**	0.023
	(0.046)	(0.087)	(0.282)	(0.038)
Period dummies[1]	14.1	5.4	3.4	7.1
# observations	594	38	262	294
# firms	114	20	61	113
Sargan test[2]	75.2	12.4	50.6	66.2
Serial correlation[3]	−2.4**	0.4	−0.7	−2.1**
Interaction with ln(Assets)				
$Investment_{t-1}$	0.135**	−0.010	0.120	0.141**
	(0.035)	(0.088)	(0.127)	(0.034)
Sales	0.180*	0.089	0.285**	0.169**
	(0.016)	(0.086)	(0.115)	(0.018)
Cash	−0.021	0.841	5.851**	−0.098
	(0.184)	(3.429)	(2.780)	(0.154)
Cash* size	0.004	−0.095	−0.444**	0.008
	(0.016)	(0.303)	(0.217)	(0.014)
Period dummies[1]	13.9	2.7	5.3	7.1
# observations	594	38	262	294
# firms	114	20	61	113
Sargan test[2]	78.5	14.6	54.4	62.9
Serial correlation[3]	−2.4**	0.4	−0.8	−2.1**

Notes: Estimates of Eq. (13). Top of table includes an interaction term between Cash and a size dummy variable equal to 1 for large firms. Large firms defined as having inflation-adjusted assets greater than 100 billion pesos. Bottom of table includes an interaction between Cash and ln(Assets). * is significant at the 10% level and ** is significant at the 5% level. See notes to Table 5 for numbered footnotes.

correlation suggests that second-serial correlation may be a concern for estimates based on the full period, the tests are unable to reject the null hypothesis of no second-order serial correlation for most of the sub-periods. The only exception is the estimates for the post-*encaje* period, but as discussed in the sensitivity analysis, addressing this by only using the appropriate higher-order lags as instruments has no significant impact on the key results. I also estimate regressions and the corresponding F-statistics for the explanatory variables to test the validity of the instruments. In each case, the F-statistic is greater than 5, and usually greater than 10, suggesting that the instruments are valid.[30] Finally, I estimate each model in levels (instead of first differences) to test if the firm-specific effects are significant. When I exclude the firm-specific effects, tests indicate

[30] For example, for the *encaje* period, the F-statistic from the regression of the first difference of Cash on its instruments is 19.5.

that there is first-order serial correlation for the equation in levels. When I include the firm-dummy variables, an F-test indicates that the firm-dummy variables are jointly significant.[31] Both results confirm that there are unobserved firm-specific effects and therefore the model should be estimated in differences instead of levels.

Next, Table 6 repeats the same analysis as Table 5, but allows financial constraints to vary for different-sized firms, as specified in Eq. (13). More specifically, the top of the table includes an interaction term between Cash and a size dummy variable, with Size = 1 for large firms.[32] The bottom of the table includes an interaction term between Cash and a continuous measure of firm size (Size), which is defined as ln(Assets). Therefore, the top of the table captures different financial constraints, on average, between small and large firms, while the bottom of the table tests for any linear relationship between firm size and financial constraints.

In both the top and bottom of Table 6, coefficient estimates for the financial constraints' variables continue to be insignificant for the full period, suggesting that on average between 1988 and 2001, neither small nor large publicly-traded firms were financially constrained. Estimates from the different sub-periods, however, suggest that this result masks important differences across time. During the period when the *encaje* was in place, the coefficients on *Cash* are positive and highly significant in both parts of the table. Moreover, the coefficients on the interaction between Cash and Size (measured as either the dummy variable or continuous variable) are negative and highly significant. These results indicate that during the *encaje* smaller public firms were significantly more financially constrained than larger firms on average, and that as firm size increased, these financial constraints decreased.

As mentioned in Section 3, however, extensive evidence suggests that smaller firms tend to be more financially constrained than larger firms in a range of countries. The evidence presented in column (3) only shows that smaller, public firms were more financially constrained than larger firms in Chile between 1992 and 1997, and does not necessarily indicate that the *encaje* aggravated these constraints. The results in the rest of Table 6, however, suggest that these differences in financial constraints between smaller and large firms did not exist in the periods before nor after the *encaje*. More specifically, in column (4) there is no evidence of financial constraints for either group of firms for the period after the *encaje* was lifted, nor any evidence of a significant relationship between firm size and financial constraints. The coefficient estimates from the period before the *encaje* (in column (2)) should be interpreted more cautiously due to the small sample sizes. With this caveat, the coefficient estimates at the top of the table suggest that before the *encaje*, financial constraints for large firms may have been lower than for smaller firms, but that these constraints for smaller firms were insignificant. The bottom of the table finds no significant relationship between firm size and financial constraints before the *encaje*. Moreover, the coefficients on Cash are significantly greater, and the coefficients on the interaction term are significantly less, during the *encaje* than either before or after.[33] This series of results

[31] For example, for the *encaje* period, the F-statistic from a test of the joint significance of the firm-dummy variables is $F(65,235) = 222.3$.

[32] Large firms continue to be defined as having inflation-adjusted assets greater than 100 billion pesos.

[33] These results are based on two different testing strategies. First, I use a one-sided t-test to evaluate if the estimated coefficient is significantly greater (or less) during the *encaje* than the other period. Second, I estimate the model including both the *encaje* and the other relevant period, allowing all of the coefficients to vary across periods. Then I use a Wald test to compare the restricted model (when the coefficients on Cash and the interaction are restricted to be equal across periods) versus an unrestricted model. The key conclusion−that the constraints are significantly greater during the *encaje*−are identical using either test.

suggests not only that smaller firms were more financially constrained than larger firms during the *encaje*, but also that smaller firms were more constrained during the *encaje* than either before or after the capital controls were in place.

Not only are the estimated financial constraints for smaller firms during the *encaje* significant, but also the magnitudes of the coefficient estimates suggest that this effect is economically important. Focusing first on the top of the table with the estimates based on the Size dummy variables, an increase in a smaller firm's cash–stock ratio by one standard deviation (0.426) during the *encaje* is correlated with an increase of about 34% in the firm's investment ratio.[34] The same increase in a large firm's cash–stock ratio during the *encaje* is correlated with a 4% increase in the firm's investment ratio. The results at the bottom of Table 6, with Size measured by ln (Assets), support these large effects. For example, for a firm with assets of 133 million pesos (the sample mean less one-third the sample standard deviation), the same one standard deviation increase in the firm's cash stock ratio would be correlated with a 26% increase in the firm's investment ratio. If the firm was twice as large (based on assets), than the same increase in the firm's cash stock ratio is correlated with a 13% increase in its investment ratio. Therefore, the estimates from both the top and bottom of Table 6 suggest that the impact of financial constraints on investment may have been economically important during the period of the *encaje*, and that these effects decreased rapidly as firm size increased.

The other coefficient estimates in Table 6 follow the same general patterns as reported for the full sample of firms in Table 5. The test statistics also indicate that the model continues to satisfy each of the assumptions outlined in Section 5.1. The only exception is the test for second-order serial correlation for the estimates based on the entire period and for the post-*encaje* period. Estimating this model using only higher-order lags as instruments (in order to satisfy the assumptions for serial correlation), however, has no significant impact on the results.

In order to further explore this section's central result that financial constraints are significant for smaller, traded firms and decrease with firm size during the *encaje*, I perform an extension to this base analysis. I divide the sample of firms into 5 approximately equally-sized groups based on firm size (as measured by total assets) and then create a new size dummy variable for each quintile.[35] Then I estimate the model including interaction terms between Cash and each of these size-quintile dummies. Although the degrees of freedom available for estimation during the pre-*encaje* period are limited, this approach provides more concrete evidence on whether financial constraints decrease linearly with firm size. This approach also provides an informal test of the validity of the testing framework used in this paper and addresses some of the concerns with this methodology discussed in Section 3.[36]

The resulting coefficient estimates are reported in Table 7. The estimates for the *encaje* period clearly suggest that as firm size increases, financial constraints decrease. In fact, the estimated coefficient on Cash is largest for the quintile of smallest firms, decreases but is still positive and significant for the medium-small quintile, and then is close to zero (and insignificant) for medium size and larger firms. In sharp contrast to these results during the *encaje*, there is no pattern between financial constraints and firm size either before or after the *encaje*. The estimates during

[34] For comparison, the sample mean and standard deviation for the investment ratio are 17% and 20%, respectively.

[35] I divide the sample into equally-sized quintiles during the *encaje* period, and then use these divisions (inflation-adjusted) for each period in order to maintain consistent sizes for each quintile across periods.

[36] For example, if the estimated financial constraints increase with firm size based on these finer gradations, it is difficult to explain these results based on results being driven by firm characteristics other than size, or results being driven by shocks to profit opportunities that are not adequately captured in Sales.

Table 7
Financial constraints for quintiles of different-sized firms

	Pre-*encaje*[1]: 1988–1993	*Encaje*: 1992–1997	Post-*encaje*: 1998–2001	Is coefficient significantly greater during *encaje* than[2]:	
	(1)	(2)	(3)	Pre-*encaje*	Post-*encaje*
I_{t-1}	0.151	0.090	0.118**		
	(0.105)	(0.138)	(0.048)		
Sales	0.186**	0.304**	0.163**		
	(0.053)	(0.124)	(0.022)		
Smallest dummy* cash	−1.399	0.959**	−0.016	Y**	Y**
	(0.978)	(0.216)	(0.024)		
Medium-small dummy* cash	0.143	0.549**	0.011	Y**	Y**
	(0.378)	(0.280)	(0.133)		
Medium dummy* cash	−0.726**	0.001	0.007	Y**	N
	(0.138)	(0.122)	(0.026)		
Medium-large dummy* cash	1.009	−0.130	0.164	N	N
	(0.720)	(0.167)	(0.191)		
Largest dummy* cash	1.429**	0.093	0.043	N	N
	(0.534)	(0.099)	(0.083)		
# observations	58	262	294		
# firms	21	61	113		

Notes: Estimates based on Eq. (13) except Cash is interacted with dummy variables for firm size quintiles. Firm size is measured by total assets in billions of inflation-adjusted pesos. Quintile divisions are based on sample distribution during the *encaje*, and then divisions are held constant across periods. Regression includes period dummy variables (not reported). Standard errors in parentheses. * is significant at the 10% level and ** is significant at the 5% level.
(1) Pre-*encaje* period extended through 1993 in order to have sufficient degrees of freedom for meaningful estimation.
(2) Summary result of both a one-sided *t*-test and a Wald test if the estimated coefficient on the cash interaction is significantly greater during the *encaje* than the other period. "Y" denotes yes (according to both tests) and "N" denotes no. Stars continue to indicate degree of significance.

the pre-*encaje* period indicate that larger firms may actually have been more financially constrained than smaller firms, although it is impossible to draw any strong conclusions due to the limited sample size. More striking, however, during the post-*encaje* period the coefficient on Cash is not significant for any of the size quintiles, and there is no trend between size quintiles and financial constraints. These results are supported by the formal tests summarized in the columns on the far right of the table. According to both one-sided *t*-tests and Wald tests, the coefficient on Cash is significantly greater during the *encaje* period than either before or after for the smallest and medium-small firms, but not for the medium-large and large firms (with mixed results for the middle quintile).

These results provide support for the testing framework used in this paper. Table 7 suggests that the estimates are capturing relationships between internal finances and investment for different-sized firms, and not spurious relationships caused by correlations between Cash and other firm characteristics. These results also clearly indicate that any financial constraints resulting from the *encaje* decreased with firm size.

5.3. Sensitivity analysis

This section discusses an extensive analysis of the robustness of the key results reported above. Given the large number of tests, I only report a small subset of these results, focusing on concerns that have been raised most often in the literature. Table 8 summarizes several tests. I focus on

Table 8
Sensitivity analysis: with size dummies

	Base[1]	Tobin's q[2]	Add leverage[3]	Add accelerator[4]	Add bus. cycle[5]	*Encaje*: 1991–94[6]	*Encaje*: 1995–97[7]	Cash flow[8]	Fixed effects[9]	Add instruments[10]	Two-stage[11]	Balanced panel[12]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pre-encaje												
Cash	−0.149	0.074	−0.055	−0.146	−0.260	0.728**	0.565*	0.381	−0.132	−0.134	−0.348**	−0.149
	(0.179)	(0.287)	(0.181)	(0.179)	(0.328)	(0.270)	(0.321)	(0.242)	(0.208)	(0.186)	(0.174)	(0.179)
Interaction	−0.357**	−0.348*	−0.307**	−0.382**	−0.300**	−0.352**	−0.757**	−0.072	−0.090	−0.287**	−0.372**	−0.357*
	(0.087)	(0.188)	(0.147)	(0.089)	(0.110)	(0.122)	(0.225)	(0.064)	(0.175)	(0.117)	(0.050)	(0.087)
Encaje												
Cash	0.797**	1.039**	0.808**	0.740**	0.528*	0.969**	0.350*	0.642**	0.316**	0.828**	0.808**	0.944**
	(0.300)	(0.501)	(0.299)	(0.286)	(0.307)	(0.441)	(0.192)	(0.317)	(0.093)	(0.287)	(0.009)	(0.354)
Interaction	−0.713**	−0.732*	−0.707**	−0.717**	−0.757**	−1.172**	−0.271**	−0.621**	−0.170*	−0.730**	−0.725**	−0.776**
	(0.282)	(0.424)	(0.279)	(0.260)	(0.281)	(0.404)	(0.134)	(0.317)	(0.096)	(0.273)	(0.007)	(0.343)
Post-encaje												
Cash	−0.008	0.010	−0.011	−0.203	−0.058	−0.003	−0.008	0.003	−0.095	−0.013	0.002	−0.011
	(0.015)	(0.008)	(0.017)	(0.162)	(0.052)	(0.013)	(0.015)	(0.005)	(0.058)	(0.020)	(0.008)	(0.055)
Interaction	0.023	0.023	0.052	0.209	0.082	0.000	0.023	−0.006	0.102	0.009	0.010	0.012
	(0.038)	(0.021)	(0.054)	(0.154)	(0.078)	(0.026)	(0.038)	(0.010)	(0.062)	(0.025)	(0.009)	(0.056)

Notes: Estimates from Eq. (13). The interaction term is the coefficient on the interaction between Cash and Size, with Size equal to 1 for large firms. Standard errors are in parentheses. Throughout the table, * denotes significance at the 10% level and ** denotes significance at the 5% level.
(1) Base estimates, with full results reported at the top of Table 6.
(2) Replaces Sales with Tobin's q. Tobin's q is defined in Table 4.
(3) Adds an additional variable controlling for leverage to each of the specifications. Leverage defined in Table 4.
(4) Includes accelerator variable, measured by sales growth (over the start-of-period capital stock).
(5) To control for business cycle effects, adds a control variable that is an interaction between GDP growth and Cash.
(6) Redefines *encaje* as period from 1991 to 1994. Years from 1995 to 1997 included in the post-*encaje* period. 1991 no longer included in pre-*encaje* period.
(7) Redefines *encaje* period to focus on the later period from 1995 to 1997. Years from 1991 to 1994 included in the pre-*encaje* period.
(8) Financial constraints redefined as lagged cash flow instead of cash stock. Cash flow defined in Table 4.
(9) Model estimated using fixed effects (with no instruments). Lagged term for investment is excluded.
(10) Model estimated with additional instruments: cash flow (weighted by capital stock), leverage, and the operating profit definition of MPK.
(11) Model estimated using the two-stage robust estimator.
(12) Sample restricted to a balanced panel including only companies with data for every year from 1990−1998 (so the sample is constant across periods).

results when Cash is interacted with the dummy variable equal to 1 for large firms (as shown on the top of Table 6), although results when Cash is interacted with the continuous measure of firm size are equally robust. The base results from Section 5.2. are reported in column (1), and the sensitivity tests are divided into five groups: model specification, variable definitions, estimation methodology, sample selection, and global effects. Variable definitions continue to be reported in Table 4.

I begin by testing for the impact of altering the model specification. First, I use a Tobin's q framework to specify a model of firm-financing constraints instead of using the model formulated in Eqs. (1) through (13). Although the Tobin's q-based methodology has a number of shortcomings (as discussed in Section III), it continues to be used in this literature. This approach is not only intuitive, straightforward to implement, and based on strong theoretical foundations, but some authors argue that under certain assumptions, the q-based methodology may be better than more complicated testing frameworks. Full details of the model derivation are not reported here due to space constraints, but are available in Forbes (2003). The estimating equation using a Tobin's q framework is:

$$\left(\frac{I}{K}\right)_{it} = \theta_0 + \theta_1\left(\frac{I}{K}\right)_{i,t-1} + \theta_2 q_{it} + \theta_3\left(\frac{\text{Cash}}{K}\right)_{it} + \theta_4\left(\frac{\text{Cash}}{K}{*}\text{Size}\right)_{it} + f_i + d_t + \varepsilon_{it}. \tag{16}$$

Even though the Tobin's q modeling framework is based on a number of different assumptions than the Euler-based framework used throughout this paper, Eq. (16) is identical to the Euler-based Eq. (13), except that each uses a different measure of the marginal product of capital. Results based on this Tobin's q framework are reported in column (2) of Table 8.[37]

In addition to the Tobin's q model, I also estimate several additional model specifications. First, I follow a number of papers by adding different measures of leverage to Eq. (13). Column (3) of Table 8 reports results when leverage is measured by the ratio of total debt to total assets. Second, since the significance of the period-dummy variables fluctuates across specifications, I exclude the period dummies. Third, since there is no reason to expect the relationship between financial constraints and firm size to be linear, I experiment with squared and cubed interaction terms. In each case, the additional terms are insignificant and do not improve the equation's fit. Fourth, I follow several papers in this literature by including an accelerator term for investment. More specifically, column (4) reports results when I add a term measuring sales growth (weighted by capital). Fifth, I respecify the models using the growth in the investment ratio instead of the level of the investment ratio as the dependent variable. Finally, in order to test for any effect of business cycle fluctuations on financial constraints, I add a variable interacting GDP growth with Cash.[38] Results are reported in column (5).

The next series of sensitivity tests examines if modifying the variable definitions has any significant impact on results. First, I use different measures of firm size, such as total sales or market value, and/or express each firm size variable in levels instead of logarithmic form. Second, I use different time periods to define the pre-*encaje*, *encaje*, and post-*encaje* periods. Column (6) only includes 1991–94 as the "*encaje*", while column (7) only includes the later period from 1995 to 1997. I also use a less stringent definition of the *encaje*, including 1991 and 1998 (years in which the *encaje* was either enacted or removed). Third, instead of using cash stock at the start of

[37] For an extensive set of results using a Tobin's q framework, see Forbes (2003).

[38] GDP growth is calculated as log differences of annual inflation-adjusted GDP, using data from IMF (2002).

the period to measure financial constraints, I use several different definitions of cash flow. For example, I use operating income, net income before preferred dividends, net income before extraordinary items, and net income (all weighted by capital stock and either for the current period or lagged by one year). Column (8) reports estimates using lagged operating income to measure Cash. Fourth, instead of weighting each of the variables (except q) by the start-of-period capital stock, I weight each of the variables by the start-of-period assets. Finally, Gilchrist and Himmelberg (1999) suggest that the marginal profit of capital (MPK) can be defined based on operating profits instead of sales (as in Eq. (10)). I estimate the model using this alternative definition.

As an additional series of sensitivity tests, I examine the impact of using alternate estimation methodologies and assumptions. First, I re-estimate the model using fixed effects or random effects, both with and without the lagged term for investment. Column (9) reports results using fixed effects (without the lagged term for investment, which causes biased and inconsistent coefficient estimates under fixed effects). Second, I use different lag structures. Instead of using a maximum of 3 lags for each of the explanatory variables, I use only 1 lag, a maximum of 2, 4, or 5 lags, or as many lags as are available. I also re-estimate the model using only variables that have been lagged by two periods or more (instead of 1 period or more) in order to avoid any bias resulting from any second-order serial correlation. Third, I try using a broader set of variables as instruments. For example, I follow Love (2003) and use lagged values of the operating-profit definition of MPK and cash flow (weighted by the capital stock) as additional instruments. I also include leverage as an additional instrument. Column (10) reports results using the full set of additional instruments. Fourth, column (11) reports estimates of the base model using the two-stage robust estimator, which tends to understate standard errors and therefore overstate coefficient significance. Finally, I alter my assumptions about whether the explanatory variables other than Cash and its interactions are constrained to remain constant across periods and size groups. For example, I estimate separate equations for each period and size group, thereby allowing all coefficients to vary across periods and small/large firms. I also estimate a model constraining all of the explanatory variables other than Cash and its interaction to remain constant across periods and size groups.

For the next series of sensitivity tests, I explore the impact of sample selection and removing outliers. I remove one industry group at a time (based on Table 2), one firm at a time, and one year at a time.[39] I also include the observations that were excluded as outliers or "unrealistic" variables (as described in Appendix B) as well as exclude the five extreme outliers for each differenced variable. Next, I exclude all "non-traditional companies", such as several sporting clubs, hospitals, and educational facilities, for which the standard accounting variables may be less informative. Finally, I create several more balanced panels. More specifically, I only include firms with data for every year from 1988 to 2001, from 1991 to 2000, or from 1990 to 1998. The later division is designed to maximize the number of observations but include the same set of firms in each of the time periods. These final results are reported in column (12) of Table 8.

As a final series of sensitivity tests, I evaluate whether the evidence of greater financial constraints for smaller Chilean firms during the mid-1990's could be driven by changes in the global environment that affected emerging markets. To perform this analysis, I use the

[39] To test if firm size may actually be proxying for whether a firm is in the tradable or nontradable sector, I replace the size dummy variables with tradable dummy variables. Firms in the nontradable sector do not have significant financial constraints during the *encaje* period, suggesting that the different results for large and small firms are not capturing differences between tradable and nontradable firms.

Worldscope database to compile a dataset of firms in emerging markets.[40] Coverage is extremely limited for many of these countries, especially before 1995, but the resulting dataset includes information for several Latin American countries (Argentina, Brazil and Mexico), several Asian countries (Korea, Malaysia, Singapore, and Taiwan), and several Eastern European countries (Czech Republic, Hungary, and Poland). Then I estimate Eq. (13) for several individual countries, for regional groups of emerging markets, and for the entire group of emerging markets. Although coefficient estimates vary significantly across countries/indices and periods, there is no evidence of patterns similar to those in Chile. More specifically, during the *encaje* period, there is not a single case in which the coefficient on Cash is positive and significant (at the 5% level) or the interaction term is negative and significant. Although these results are only preliminary, they suggest that increased financial constraints for smaller, publicly-traded Chilean firms during the *encaje* can not be explained by any global shocks or changes in the external environment that affected comparable emerging markets over this period. The reduction in financial constraints for smaller firms in Chile that occurs after the *encaje* was lifted is also not evident in other emerging markets.

This series of sensitivity tests reported in Table 8 suggests that the key results reported in Section 5.2 are highly robust to a range of variable definitions, model specifications, estimation methodologies, and sample selection. In every single test for Chile during the *encaje*, the coefficient estimates on Cash are positive and significant (always at the 10% level and usually at the 5% level), while the coefficients on the interaction term are negative and significant. In sharp contrast, during the pre-*encaje* and post-*encaje* period the coefficients on Cash are rarely positive and significant, and the coefficients on the interaction term have fluctuating signs and significance (and are always insignificant in the post-*encaje* period). One of the few exceptions to these patterns is when the *encaje* period is only defined to include 1991–94 or 1995–97. Although the coefficient signs still follow the above patterns, the estimates in columns (6) and (7) suggest that the impact of the *encaje* on financing constraints for smaller firms may have been greater during the first half of this period (a result supported by the graphical evidence on investment growth in Part C of Fig. 1). Finally, one-sided *t*-tests and Wald tests indicate that in every specification (except when the *encaje* is defined as lasting from 1995 to 1997), the coefficient on Cash is significantly greater, and on the coefficient on the interaction is significantly less, during the *encaje* than in the other periods. None of these patterns are apparent for identical regressions in other emerging markets.

6. Conclusions

Managers of small and medium-sized firms in Chile argued that the *encaje* made it more difficult to obtain capital to fund productive investment. Despite the attention paid to the macroeconomic impact of the *encaje*, there was no empirical evidence supporting or refuting these arguments. The results in this paper, however, suggest that these complaints were justified. Smaller, publicly-traded Chilean firms appear to have experienced significant financial constraints during the period that the *encaje* was in place, but not before or after. Large Chilean firms do not appear to have experienced significant financial constraints before, during or after the *encaje*. Moreover, during the period of the *encaje*, there appears to be a strong, linear, inverse relationship between a firm's size and its degree of financial constraints.

[40] More detailed information on this data set is available in Forbes (2003). Results are not reported due to space constraints, but are also available in Forbes (2003).

These conclusions, however, are subject to several caveats. The sample of firms for the period before the *encaje* is small, so that the corresponding coefficients are imprecisely estimated. The dataset only includes publicly-listed firms, so that the results may not apply to a broader set of private companies. The estimates are based on linear normalizations of economic relationships that may be nonlinear. Many of the variables are imperfect measures of the underlying economic concept (such as sales/capital to capture the marginal profit of capital). The GMM estimation technique may not fully account for endogeneity. Most important, firms are defined as being financially constrained if their investment is significantly affected by their cash stocks (or cash flows), after controlling for future expected profitability. Although this strategy to test for financial constraints has been widely used, there is still an active debate on the accuracy of this definition and testing strategy.

In order to address as many of these caveats as possible, this paper performs an extensive series of sensitivity tests. It examines the impact of modifying model specification, variable definitions, estimation methodology, and sample selection. The main results are highly robust to each of these variations. The paper also repeats this analysis for a sample of additional emerging markets to see if the results are driven by changes in the global environment instead of domestic events in Chile. None of the other emerging market countries shows evidence of increased financial constraints for smaller, publicly-traded firms during the period from 1992 to 1997, indicating that the Chilean results were not driven by external factors.

The consistency and strength of these results is particularly striking given three factors that could either bias the empirical tests against finding evidence of financial constraints for smaller Chilean firms during the *encaje*, or at least counteract any such impact. First, the database used for this analysis does not include the smallest firms in the Chilean economy, and even under-represents the smallest publicly-traded firms. The coefficient estimates from the paper indicate a strong negative relationship between financial constraints and firm size. These results are supported by the estimates based on size quintiles in Table 7. Therefore, a more representative sample including more of the smallest firms in Chile–either public or private–could yield even greater estimates of financial constraints for “smaller” firms.[41]

A second factor that would be expected to reduce estimates of the impact of the *encaje* on smaller firms’ financial constraints is the liberalization and growth of Chilean financial markets. As described in Gallego and Loayza (2001), financial market reforms led to the development and deepening of Chilean financial markets in the first half of the 1990s (as compared to the later half of the 1980s). This development of domestic capital markets would be expected to not only decrease financial constraints for all firms, but to decrease financial constraints for smaller firms relatively more than for large firms (since small firms tend to be more financially constrained than larger firms in less developed financial markets).[42] Although the period dummy variables may capture any reduction in financing constraints for firms on average, this development of Chilean financial markets during the 1990s could counteract any increase in financial constraints for smaller firms relative to large firms from the *encaje*.

A final factor that could counteract any impact of the *encaje* on financial constraints for smaller firms is the contraction in lending to emerging markets that occurred during 1998 as a result of the Russian crisis and problems in the U.S. hedge fund Long-Term Capital Management (LTCM).

[41] On the other hand, the smallest private firms may be entirely reliant on internal finances or local banks for external financing during all periods, and therefore be less affected by the *encaje* than small publicly-traded firms or medium-sized private firms.

[42] For evidence, see Love (2003) or Laeven (2003).

Capital flows into Chile declined dramatically in 1998, and peso-borrowing rates rose. These factors would be expected to increase financial constraints for Chilean firms, especially for smaller firms. Since 1998 is included in the post-*encaje* period, these effects could counteract any reduction in financing constraints for smaller Chilean firms that resulted from the removal of the *encaje*.

Despite these three factors that would reduce estimates of any impact of the *encaje* on smaller firms' financial constraints, this paper still finds strong and robust evidence that the Chilean capital controls significantly increased financial constraints for smaller, publicly-traded firms, and that these constraints decreased with firm size. These results have important implications for the debate on the reform of the international financial architecture and the desirability of implementing taxes on capital inflows. Although other work suggests that the Chilean *encaje* might have generated some macroeconomic benefits, there has been no empirical evidence of any substantial cost to this policy. This paper suggests, however, that the *encaje* had the negative effect of increasing financial constraints for small and medium-sized, publicly-traded firms. This cost could be important for emerging economies in which small firms can be valuable engines of job creation and economic growth. As result, before countries adopt any Chilean-style tax on capital inflows, they should carefully weigh the potential benefits against the negative effects on investment and growth of smaller firms.

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Appendix A. A history of the *encaje*[43]

June 1991: The unremunerated reserve requirement (URR) was established. 20% of capital inflows were to be deposited at the central bank in a non-interest bearing, unremunerated account denominated in the currency of the credit. The holding period was equal to the loan maturity with a minimum of 90 days and a maximum of 1 year. Borrowers could alternatively opt to pay an up-front fee (a promissory note at a discounted repurchase priced at LIBOR). The URR applied to all new foreign borrowing (by banks as well as non-financial institutions) and excluded trade credit (as long as shipment occurred within the next 6 months).

July 1991: The URR was extended to include the renewals of all borrowings (still excluding trade credits), as well as any credits linked to FDI projects.

January 1992: The URR was extended to include foreign currency deposits in commercial banks (held by domestic or foreign residents). The length of the URR for banks was also extended to equal the length of time that a deposit was in place.

May 1992: The URR was raised to 30% and extended to a minimum of 1 year (regardless of the maturity of the loan). The increase of the rate to 30% did not initially apply to direct borrowing by corporations, for which the URR stayed at 20%.

[43] *Sources*: Based on Ariyoshi et al. (2000, Table 1 in Appendix I), Gallego et al. (1999, Annex 2), and Simone and Sorsa (1999, Table 1).

August 1992: The above exception for direct borrowing by corporations was removed (so that the URR for this type of borrowing also rose to 30%). The discount rate on the paid up-front fee was raised to LIBOR + 2.5%.

October 1992: The discount rate on the paid up-front fee was raised to LIBOR + 4.0%.

October 1993: The trade-credit exemption from the URR is extended to include trade credit if the merchandise is shipped within 300 days. This enlarged the trade-credit loophole.

November 1994: An announcement that starting in January 1995, the URR deposits and equivalent up-front fee can only be paid in dollars. This substantially increased the implicit cost of the URR.

July 1995: The URR was extended to include secondary ADRs (since these were no longer considered as FDI) and other inward financial instruments. Primary ADRs were still excluded from the URR (since they were viewed as capital additions).

September 1995: The period to purchase foreign exchange after selling ADRs in the domestic market was shortened to 5 days. (There was previously no regulation of foreigners selling stock holdings in the domestic market and then taking the proceeds out of the country.)

December 1995: New foreign borrowing to repay old debt (when the new loan is of equal or shorter maturity than the outstanding loan) was exempted from the URR.

May 1996: The URR was extended to cover "speculative FDI". FDI is considered non-speculative, and therefore exempt from the URR, if it "increases productive capacity". An FDI committee decides whether FDI is speculative.

June 1996: Foreign credits can no longer be rolled over more than once a year.

December 1996: Small foreign borrowing (less than US$ 200,000 per loan or less than US$ 500,000 per year) became exempt from the URR.

March 1997: The above exemption for small foreign borrowing was reduced to less than US$ 100,000 per year (cumulative).

April 1997: The remittance of funds (principal and profits) from investments by Chileans abroad were exempt from the URR.

September 1997: Proceeds from closing positions in derivatives/options in foreign markets by Chileans were exempt from the URR.

June 1998: The URR was reduced to 10%, except for short-term credit lines and foreign-currency denominated deposits, which remained at existing rates.

August 1998: The URR was eliminated for secondary ADRs.

September 1998: The URR was reduced to 0% for all capital inflows. Foreign investors were still required to keep money in Chile for at least 1 year.

Appendix B. Creation of the data set

The data set used in this paper was created in several steps:

1. Compile information for all Chilean firms included on the September 2002 Worldscope CD-ROM. Data reported for a maximum of 10 years ending with the latest annual report.
2. Augment dataset with historical information from the May 1999 CD-ROM. Additional data is only included if the historical time series is consistent across the two sources.
3. Exclude all financial companies, defined as having a 1-digit SIC code of 6.
4. Exclude any companies that do not have information for at least 1 year for the 3 key variables necessary to estimate Eq. (13) — investment, cash stock, and capital stock. This leads to 1 additional exclusion (Marbella Country Club).

5. Exclude outliers and unrealistic observations for the variables used to estimate the base specifications for Eq. (13). More specifically, exclude individual observations where:

- $K \leq 0$ (10 observations)
- $I/K<0$ or $I/K>3$ (6 observations)
- Cash$/K<0$ or Cash$/K>10$ (7 observations)
- Sales$/K<0$ or Sales$/K>10$ (9 observations)

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