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INDEX

ABSTRACT

1. INTRODUCTION 1

2. MOTIVATING THEORY 4

3. DATA AND EMPIRICAL APPROACH 5
3.1 Data Description 5
3.1.1 Labor Flows 5
3.1.2 Sudden Stops 7
3.1.3 Sectoral Financial Characteristics 7
3.1.4 Country Level Variables 8
3.2 Empirical Strategy 9
3.3 Identification 11

4.RESULTS 12
4.1 Sudden Stops And Labor Flows 12
4.2 Sectoral Effects 13
4.2.1 Financial Dependence 13
4.2.2 Liquidity Needs 14
4.2.3 Financial Factors Overall 14
4.3 Sudden Stops, Financial Characteristics, And Net Labor Growth 15
4.4 Robustness Checks 16
4.4.1 Falsification Exercises 16
4.4.2 Sudden Stops and Financial Crises 16
4.4.3 Additional Control Variables 18
4.4.4 Different Sample Coverage 19
4.4.5 Different Measures Of Sectoral Financial Characteristics 19

5 CONCLUDING COMMENTS 20

REFERENCES 22

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Francisco A. Gallego† José A. Tessada‡

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Abstract

Sudden stops and international financial crises have been a main feature of developing countries in the last three decades. While their aggregate effects are well known, the disaggregated channels through which they work are not well explored yet. In this paper, we study the sectoral responses that take place over episodes of sudden stops. Using job flows from a sectoral panel dataset for four Latin American countries, we find that sudden stops are characterized as periods of lower job creation and increased job destruction. Moreover, these effects are heterogeneous across sectors: we find that when a sudden stop occurs, sectors with higher dependence on external financing experience lower job creation. In turn, sectors with higher liquidity needs experience significantly larger job destruction. This evidence is consistent with the idea that dependence on external financing affects mainly the creation margin and that exposure to liquidity conditions affects mainly the destruction margin. Overall, our results confirm the large labor market effects of sudden stops, and provide evidence of financial conditions being an important transmission channel of sudden stops within a country, highlighting the role of financial frictions in the restructuring process in general.

Keywords: sudden stops, gross job flows, adjustment, financial frictions.

JEL Codes: E24, F3, G21, J63

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1 Introduction

Many emerging economies have suffered sudden stops of capital flows in the last three decades.[1] These sudden stops have shown to have significant impact on most macroeconomic aggregates, including output growth, domestic credit and unemployment among others.[2] However, little is known about the sectoral effects of sudden stops in developing countries. Most of our knowledge on the reaction of gross job flows to shocks comes from the study of the effects of (smoother) macroeconomic shocks –such as recessions– on job creation and destruction in developed countries (see Caballero , 2007 and the references therein). Sudden stops are clear big shocks to emerging economies that likely provide an extreme experiment to study the effects of negative shocks on job flows. Moreover, it is reasonable to think that the effects of sudden stops on job flows should be heterogeneous, depending on sector- and country-specific variables.

This paper extends our knowledge in this respect by looking at the effects of sudden stops on sector level job creation and destruction in a sample of Latin American countries. By doing this, our work also expands the current understanding of sudden stops and their effects on countries that suffer them. It also provides additional evidence on the effects of macroeconomic shocks and international financial crises in emerging markets. This paper also highlights how big macroeconomic shocks are transmitted to the labor market and shows how much creation and destruction in this market change when hit by a large financial crises and how those effects differ across production sectors.[3]

We use a panel dataset on job creation and destruction in manufacturing sectors, at the 2-digit sector level, for four Latin American countries (Brazil, Chile, Colombia, and Mexico) that covers various time periods from 1978 to 2001. We identify sudden stops following previous definitions in the macroeconomic literature (Calvo et al , 2006, 2008; Cavallo and Frankel , 2008; Joyce and Nabar , 2009). Using these data, we find that sudden stops are periods during which job creation decreases and job destruction increases.[4] In particular, we find the effect of sudden stops on job destruction to be larger and more robust. In the case of job creation we find (weaker) evidence of a negative effect of sudden stops only in the case of data coming from all plants sampled; when data for continuing plants only is considered we find little evidence of an effect of sudden stops on job creation at the 2-digit sector level in manufacturing.

Furthermore, we might expect some of the sectoral effects of sudden stops to be linked to financial channels. One can hypothesize that sectors where firms depend more on external finance, to the firm, to suffer more from a negative external shock. Likewise, the same argument is true for firms that face larger liquidity needs, and hence may need to have access to liquid resources from financial institutions more often, or in larger amounts. Motivated by this argument, we relate the sector level gross job flows to the interaction of sudden stops with proxies for external dependence and liquidity

[1] For example, Rothenberg and Warnock (2006) document that between 1989 and 2005 most of the time there was at least one country experienced a *sudden stop* episode.

[2] See for example Calvo et al (2006).

[3] For example, see Pratap and Quintin (2008) for a description of the labor markets effects during the Tequila crisis in Mexico.

[4] Job destruction takes only positive values, thus an increase in its values implies that more jobs are destroyed.

needs of each sector.

We find evidence that the negative effect of sudden stops on job creation is stronger in sectors with stronger dependence on external finance, as originally measured by Rajan and Zingales (1998).[5] Similarly, the positive effects of sudden stops on job destruction are stronger in sectors with higher indicators of financial needs (measured as the ratio of inventories over sales and the cash conversion cycle, as suggested by Raddatz (2006)). We thus provide evidence that financial conditions are an important determinant of the extent of the impact of shocks on sectoral job flows in a country. Moreover, these variables are meant to capture two different aspects of the financial characteristics of a firm, and our empirical results seem to highlight that these two facets are indeed related to different margins of adjustment by firms when subject to a sudden stop.

These results are mostly robust to controlling for two-way fixed effects, a falsification exercise, alternative proxies for financial characteristics, adding additional controls, adding data for Argentina and Uruguay, and using a different definition of our crisis variable.

The results in this paper relate to four strands of the literature. First, we draw from the existing literature on the characteristics of sudden stops and their aggregate effects. Dornbusch et al (1995) were the first to refer to reversals in financial flows as *sudden stops*; shortly thereafter, Calvo (1998) explored the basic mechanism and the implications of these reversals. More recently, Guidotti et al (2004) and Calvo et al (2006) have documented the aggregate effects of sudden stops; in particular, Calvo et al (2006) show that sudden stops are associated with a decline in GDP, TFP, investment, and domestic credit. Related to our approach, Guidotti et al (2004) decompose the adjustment in current account into adjustment in exports and imports and find that countries that are more open and have lower financial dollarization adjust their current account mostly through exports, which they argue are less costly than an imports-based adjustment. This connection between export-import responses and financial dollarization is related to our approach, but they do not look at the particular factors driving the differences across sectors.[6] Using a general equilibrium approach, Kehoe and Ruhl (2009) and Pratap and Urrutia (2007) study the Mexican 1994-5 crisis and Gertler et al (2006) study the Korean performance around the Asian crisis. These papers find that labor or financial market frictions improve the ability to match some stylized facts of the two sudden stops they study.[7]

Second, our results are related to the literature on job and worker flows, labor market dynamics and restructuring.[8] We borrow from this literature the insight that the microeconomic channels behind the aggregate picture gives us information on the mechanisms and the effects of particular shocks and changes in economic conditions. One conclusion from this literature that is highly related to our work is that firms' reactions to (negative) shocks depend on (i) financial aspects related to the

[5]We also use updated measurs of sectoral external dependence computed by Raddatz (2006), and Micco and Pagés-Serra (2006).

[6]The connection between sudden stops and domestic lending by banks has been documented in Brei (2007) among others.

[7]Also related to the literature is the work by Chari et al (2005) that presents a very suggestive result. They show how in a relatively standard model of a small open economy, a sudden stop modeled as a tightening of a collateral constraint can, under certain assumptions, generate an increase rather than a decrease in output. An important conclusion for our paper is that other economic frictions might be needed to generate the usual output drops that accompany sudden stops. See also the work by Mendoza and Smith (2006).

[8]See, for example, Caballero (2007) and Shimer (2010).

ability of entrepreneurs to raise external funds to keep the firm running, and (ii) labor regulations that determine the costs of destroying a job and the relative bargaining power of entrepreneurs. There is, however, a difference in the focus between our paper and the main work in this literature; we deal with a shock that is larger and that, at least at the country level, corresponds more to a financial shock, instead of business cycle variation or productivity innovations.

Third, our paper is related to a literature that deals with the effects of real exchange rates in sectoral flows in open economies. Using firm level data for France, Gourinchas (1998) and Gourinchas (1999) find that following a real exchange depreciation, job creation and destruction decrease. Klein et al (2003) use sectoral data for US manufacturing firms and find that job destruction decreases and net employment growth increases after a depreciation of the dollar.[9] Finally, Haltiwanger et al (2004) use the same dataset we use in this paper and confirm previous results in that real exchange rate appreciations are periods of increased job reallocation. While our methodology is related to this literature, we exploit an extreme case of an external shock, which (i) reflects countries' external financial conditions (and probably much better than the real exchange rate) and (ii) is also more exogenous to sector-specific situations across countries. In addition, it is worth emphasizing that the results in our paper are robust to controlling for interactions of the real exchange rate and sectoral dummies.

Finally, our empirical approach is also related to the literature on finance and sector level outcomes, largely started by Rajan and Zingales (1998). Braun and Larraín (2005) show, using a cross-country sample of manufacturing industries over forty years, that industries that are more dependent on external finance are hit harder during recessions. In a related way, Larraín (2006) shows that output volatility is dampened in countries with more developed bank systems, as they provide firms with more access to countercyclical borrowing. Raddatz (2006) studies the relation between output volatility, country financial development and liquidity needs at the sector level and finds that lower financial development magnifies the effects of liquidity needs on sector level volatility. Therefore, all these three papers suggest a possible role for financial frictions in the transmission of sudden stops to sectors; either because there is a reduction in external funds as a whole or because particular sources of financing, i.e. bank lending, are affected.[10]

The paper is organized as follows. Section 2 presents a motivating theory on the determinants of creation and destruction under the presence of financial constraints as a framework for the interpretation of the empirical strategy and results. Section 3 discusses the data and describes the empirical strategy. Section 4 presents the main results of the paper together with a number of complementary and robustness checks and section 5 briefly concludes.

[9] See also Goldberg et al (1999) and Campa and Goldberg (2001) for related work on the effects of international factors on employment and labor markets.

[10] Although not related to restructuring, Aghion et al (2007) present a model where financial frictions induce entrepreneurs to choose some projects that generate liquid resources; this misallocation lies behind the connection between volatility and growth they study.

2 Motivating Theory

Our empirical strategy aims at linking responses to sudden stops with exposure to financial conditions. Let us assume that each plant p in sector i has access to resources

$$w_{ipt} = \phi_i + \omega_i G_t + \varepsilon_{ipt}, \tag{1}$$

where G is an indicator of aggregate conditions in the financial markets. A sudden stop reduces the availability of funds in the market, in particular consider the case where G is 0 in normal times and 1 when there is a sudden stop. The coefficient $\omega_i < 0$ represents a sector-wide sensitivity to financial conditions. This implies that the effect of sudden stops on financial resources available to plants (or firms) is larger for sectors with larger ω_i. Our variables for financial characteristics should then be interpreted as proxies for the *ranking* of sectors according to (different dimensions of) ω_i.

We assume that firms (or plants) need financial resources both to expand their production, which implies *creating* new jobs, **and** to *maintain* their production levels from previous periods. Whenever financial resources are scarce firms adjust to the conditions through reduced expansion, thus *decreasing* job creation, and/or by downsizing their current operations, thus *increasing* job destruction.

Although we can talk about financial resources in general, the sources of funds for creating more jobs and for maintaining the scale of production need not be the same. Bond issuance or large loans from banks might be the most common source of funds for starting new projects or growing current operations, particularly in emerging markets. However, simple credit lines, trade credit, or credit from suppliers are also sources of short-term/more liquid funds used by firms while in production. Although we expect both sets of measures to be correlated, there is no reason a priori to believe that the correlation should be perfect and that the effects should be the same.[11] Given this, it is important for us to use measures that capture separately the "average" exposure to external financing and the "average" exposure to liquidity needs for each sector. When financial markets dry up during a sudden stop, we should expect both sources of financing to be reduced, but the differential exposure of sectors to each type of funding should then be reflected in a differentiated way on the job creation and job destruction flows.

In spite of the importance financial conditions have on the investment and size adjustment decisions by plants, we know that other variables can affect the investment decision. More importantly for the purpose of our study, some of those variables are likely to be affected by the occurrence of a sudden stop. Consider for example aggregate demand, which can be affected by sudden stops through various possible channels (e.g. real exchange rate in the case of tradable sectors like manufacturing); in turn it can affect demand for the goods produced in each sector differently. In our empirical analysis we perform robustness checks, using the real exchange rate and fixed effects to control for some of these additional channels. As we explain in more detail later we observe that although these channels play a role, they do not eliminate the direct effect of sudden stops on gross flows and the role of sectoral financial characteristics.

[11] In fact, as we discuss in Section 3.1 there exists a significant literature that has proposed different variables that capture these differences across sectors. See for example Rajan and Zingales (1998) and Raddatz (2006), among many others.

3 Data and empirical approach

3.1 Data Description

3.1.1 Labor Flows

Data on sectoral gross flows comes from Haltiwanger et al (2004). The dataset is an unbalanced panel at the 2-digit sector level for 6 Latin American countries from 1978 to 2001. The database was originally constructed by the Inter-American Development Bank (IADB) using firm level data from Argentina, Brazil, Chile, Colombia, Mexico and Uruguay. The original surveys record flows in workers or jobs, not in hours, hence our study captures only the extensive margin on workers. The original country-specific data contained employment at the firm level and it was aggregated by sectors.[12]

Consider a given sector and country, let p index the plants and t the period, then $E_{p,t}$ represents employment in plant (firm) p at time t. Net employment growth is given by

$$Net_{p,t} = 2\left(\frac{E_{p,t} - E_{p,t-1}}{E_{p,t} + E_{p,t-1}}\right). \tag{2}$$

Notice that, by construction, this measure goes between -2, in the case of a plant that was created between t and $t-1$, and 2, in the case of a plant that was closed during the same period.

Job creation corresponds to the sum of net employment growth over all plants with positive net employment growth (for a given country-sector pair) between period $t-1$ and t,

$$Creation_t = \sum_p \phi_{p,t} \max\left(Net_{p,t}, 0\right), \tag{3}$$

where $\phi_{p,t}$ is employment share of plant p.

Job destruction is then the sum of the absolute value of net employment growth over all plants with negative employment growth between period $t-1$ and t,

$$Destruction_t = \sum_p \phi_{p,t} \left|\min\left(Net_{p,t}, 0\right)\right|. \tag{4}$$

We use data for manufacturing sectors, as it is the only data available for all countries in the original sample available from the IADB. Each job flows series is provided for 2 sets of firms: *continuing* and *all* plants.[13] Job creation data for continuing plants include information from continuing plants in t, alive in $t-1$ and t; all plants/firms include all plants in t and $t-1$. Job destruction data for continuing plants includes information from continuing plants as reported in period t; for all plants/firms it again includes information on job destruction from all plants in t and $t-1$. As previously mentioned, for Argentina and Uruguay, only data for continuing plants is available, and we use it in a robustness exercise.

Panel (a) in Table 1 presents the time periods for which we have information for each country

[12] See Appendix Table A.3 for a description of the dataset.

[13] The dataset includes data on plants and firms, but for simplicity we refer only to plants.

and the average values of creation and destruction for the whole set of six countries and for the main group of countries in our estimation (Brazil, Chile, Colombia and Mexico). Average job creation of all plants is 12.3%, significantly higher than job creation of continuing plants (9.1% for the main countries and 7.5% considering also Argentina and Uruguay). Job destruction rate of all plants is very close to the value of job creation (11.8%), implying that net job creation is just 0.5%. We do not observe big differences in terms of job destruction rates among all and continuing plants. Finally, in our sample, net job creation for continuing plants is slightly negative in average (with a rate of -1.6%).

We can see that there is large variation both in creation and destruction across countries. Mexico presents both the highest average job creation rate (17.4%) and one of the lowest destruction rates (10.5%) for all and continuing plants–thus, having the highest net job creation rate of all the countries. The country with the lowest net job creation is Colombia with -0.8% for all plants and one of the lowest (-3.7%) for continuing plants, reflecting a relatively low rate of creation and a relatively high rate of destruction (especially in the case of continuing firms). In addition, if we look at the detailed data (see Table Appendix A.1), we can observe that Mexico has the highest rates of creation (in 1996), but Chile shows the highest destruction rates for all plants (in 1982) and Colombia for continuing plants (in 1992). Chile also presents the largest differences between the maximum and minimum values for creation and destruction in the sample.

These differences in gross and net labor flows may be related to the dissimilar time periods for which we have information for the different countries and also to different conditions under which labor markets in these countries operate over these periods (including macroeconomic shocks, regulations, and other country-specific variables). In the empirical analysis of this paper we study whether sudden stops and the interaction of sudden stops with sectoral financial characteristics may affect these differences in creation and destruction rates.

Unfortunately, there are two dimensions that our dataset misses. First, we do not observe plant turnover data, i.e., we have no information on flows associated with closing down plants, nor the plant flows by sector. The latter dimension is important when studying the effects of financial shocks, as liquidity needs may drive firms out of the market if they cannot borrow to maintain operation. It is also relevant to observe firms that change property, either because of bankruptcy procedures or because of fire-sales when in sudden stops.

Second, our dataset only includes data for formal plants/firms. This may certainly be an issue in Latin America given the extent of informality in some countries and sectors (IADB (2004)) and implies that we may be missing some movements along the informal-formal employment margin. However, it is important to notice that our sample includes data just from manufacturing sectors. IADB (2004) documents that informality –measured using coverage in social security of waged workers as a proxy– in the manufacturing sector is significantly lower than in the rest of the economy. For example, accordingly to IADB (2004) informality rates are 21% for Brazil (in 1999), 19% for Mexico (in 2001), and 17% for Chile (in 2000)–as a reference, informality rates for the whole economy are 36% for Brazil, 20% for Chile, and 34% for Mexico. Moreover, average informality measured using this proxy for OECD countries —where arguably informality rates are quite small— is 7% (with the two countries with the highest informality rates being Greece with 16% and France with 14%). Thus, we think that even we may be losing some movements along the formal-informal employment margin,

this is much less relevant in our sample than in other sectors and Latin American countries.

3.1.2 Sudden Stops

We take the dates for sudden stop episodes directly from the episodes identified by Calvo et al (2006), Calvo et al (2008), and Cavallo and Frankel (2008). In addition, we extend their definition of sudden stops using data from the International Financial Statistics for Chile and Colombia in the late 1970s and 1980s (countries for which we have job flows data over periods that are not covered by the above-mentioned papers). Following Joyce and Nabar (2009), and given the fact that there is no unique empirical implementation of the definition of a sudden stop episode, we identify a country-year observation as a sudden stop if it is identified as such by any of three papers mentioned at the beginning of the paragraph (and by our extension of their methodologies to the 1970s and 1980s).[14]

Finally, we transform the monthly definition of sudden stops in the papers by Calvo et al. to annual frequency, to match with the information in Cavallo and Frankel (2008) and in our job flows dataset. To do this, we take the fraction of months of a year in which a sudden stop is identified by either of the papers by Calvo et al. Then the variable that combines the annual information from the three papers corresponds to our baseline definition of sudden stop (henceforth denoted by *SS*).[15]

Table 2 shows the periods for which we identify a sudden stop together with the years and months for which we have job flows data for each of the six countries. Panel B of Table 1 shows that there has been a sudden stop in about 20% of the periods included in our sample. Both tables show that we do not identify any sudden stop for Uruguay according to this definition. On the other hand, we find that Brazil, Chile, and Mexico have spent more than 20% of the sample period in sudden stops. Interestingly for our identifying assumptions, with the exception of Mexico 1994-1995, all the sudden stops identified in our sample correspond to periods of bunching of sudden stops as observed in the work by Rothenberg and Warnock (2006), which in turn correspond to periods during which credit conditions worsened due to exogenous reasons as documented in Gallego and Jones (2005).

3.1.3 Sectoral Financial Characteristics

We use two sets of financial characteristics:

1. *External financing dependence*: The first sector level characteristic we use corresponds to the Raddatz (2006) measure of external financing dependence (We denote it by *Fin*). It captures a sector's dependence on external financing by measuring the fraction of the assets that is financed with external funds (following the seminal paper by Rajan and Zingales (1998)). A sector with a higher external financing dependence measure should suffer more in the event of a financial crunch or any other reduction in the access to credit. Alternatively, we also use the Micco and

[14]The three empirical measures of sudden stops are correlated given the definitions are very similar among them (Joyce and Nabar (2009)). The correlation of the sudden stop dummies in the papers by Calvo et al. is 0.81. In turn, the correlation between Cavallo and Frankel (2008) and Calvo et al (2006), and between Cavallo and Frankel (2008) and Calvo et al (2008) are 0.55 and 0.52, respectively.

[15]In addition to this variable, we also constructed a second dummy variable that takes a value of 1 if there is a sudden stop in any month of the year. We do not present results using this variable to save space, but the results are qualitatively the same if we use this variable instead.

Pagés-Serra (2006)and Rajan and Zingales (1998) measures of external financing dependence as a robustness check (we denote them by *Fin1* and *Fin2*, respectively).[16]

2. *Liquidity "needs"*: Following Raddatz (2006) we use the median value of the ratio of total inventories to sales (denoted by *Inv/Sales*) across firms in each sector as our main proxy for the liquidity needs of firms. Alternatively, we also use the cash conversion cycle (denoted by *CCC*), which corresponds to an estimate of the length in days between the moment a firm pays for the raw materials and the moment it finally receives the payment for the sale of the final goods it produces.[17]

All the original external financing dependence and the liquidity needs variables were calculated for 3-digit sectors. Given that our data for labor flows contains information for 2-digit sectors, in our main specifications, we use the median value of each indicator across 3-digit sectors within a 2-digit sector.[18]

It is worth emphasizing that these measures capture different dimensions of the financial needs of firms, and, as we discussed in section 2, relate to different types of financial funds firms need. The first set, based on the initial Rajan-Zingales approach, measures dependence related to the use (in equilibrium) of external funds in asset acquisition, and hence it relates more to long-run and investment decisions. In turn, the liquidity needs measures explicitly capture financial needs arising from delays between production and sales revenue collection. This is obviously related to short-run liquidity needs and the dependence on financial markets to cope with them during the production process. This separation is in line with the evidence in the existing literature and with the discussion in section 2.

Interestingly, the Spearman (Kendall) rank correlation between both proxies for different margins of financial characteristics for the nine sectors we use in this paper is just 0.28 (0.17) and we cannot reject the null hypothesis that both series are independent among them, with a p-value of 0.46 (0.60). This indeed shows that both margins of financial frictions are different, which is key for the empirical analysis and the interpretation of the results of this paper.

3.1.4 Country Level Variables

We use a number of country-level characteristics in our regressions. We list them here.

1. We use a labor regulation proxy from Botero et al (2004). Following Micco and Pagés-Serra (2006), we focus on the sum of the cost of firing workers and the number of procedures required to dismiss a worker. The cost of firing workers is a measure of how expensive it is for a firm to fire 20% of the workers; it includes all the compensations and penalties needed to pay in this case. The dismissal procedures variable counts the number of measures a firm must undertake

[16] See Table 9.

[17] See Table 9.

[18] The two alternative measures of Financial Dependence (*Fin1* and *Fin2*) consider the mean of the same measure across sub-sectors in each 2-digit sector. Consequently, the different measures have different sensitivity to heterogeneity within each 2-digit sector. We chose to use the Raddatz (2006) measure of external dependence in our main specifications given that is computed using the same procedure as our proxies for liquidity needs.

in order to be able to dismiss a worker. The highest value of the labor regulation measure in our sample corresponds to Mexico with 1.28 out of a maximum of 2; the minimum is 0.24 in Uruguay (see Table 1 Panel B).

2. In addition, in order to have a proxy for labor market regulations that changes by country within our sample, we use a variable for labor market reforms from Heckman and Pagés-Serra (2004). They identify years in which the countries included in our dataset implemented reforms that either reduced or increased the legal protection to workers. Our proxy takes a value of 1 if the country implemented labor reforms that increased legal protection of workers, a value of -1 if the country implemented labor reforms that decreased legal protection of workers, and a value of 0 if the country did not implemented a labor reform.[19] In our sample, Brazil in 1985 and Chile in 1991 implemented reforms increasing legal protection to workers and Colombia in 1990 implemented reforms decreasing legal protection to workers.

3. We use a rule of law proxy from La Porta et al. (1998). The variable is constructed by the country-risk rating agency International Country Risk (ICR) and corresponds to the assessment of the law and order tradition in the country averaged over the 1982-1995 period. The index goes from 0 to 10, with lower scores for less tradition for law and order. The highest value of the rule of law measure in our sample corresponds to Chile with 7.02 and the minimum is 2.08 in Colombia (see Table 1 Panel B).

4. We use a measure of trade openness recently developed by Chang et al (2009) that corresponds to the residual of a regression of the log of the ratio of exports and imports (in 1995 US$) to GDP (in 1995 US$), on the logs of area and population, and dummies for oil exporting and for landlocked countries. We use the average of the measure over the period for which we have labor flows information for each country.[20] The highest value of the this measure in our sample corresponds to Mexico with 73.1% and the minimum is 17.5% in Brazil (see Table 1 Panel B).

5. Finally, we use a dummy for de facto fixed exchange rate regimes from Levy-Yeyati and Sturzenegger (2003) to study how differences in the exchange rate regime affects our results.[21] Using this proxy we find the following episodes of fixed exchange rate regimes in the years included in our sample: Argentina (1992-2001), Brazil (1998 and 2000), Chile(1980-1981), and Mexico (1994).

3.2 Empirical Strategy

Motivated by the discussion in section 2 and the existing literature, we proceed to study the case of sudden stops in Latin America. In particular, we look for evidence on the following hypotheses:

1. Firms in sectors depending more on external finance should be more affected during sudden

[19] We have experimented with alternative ways of constructing this variable –ie. using different dummies for increasing and decreasing the legal protection of workers–finding similar results. We chose this specification for parsimony.

[20] Using the time variant version of the variable yields very similar results, thus suggesting that most of the identification for the effects of trade openness in our sample occurs at cross-country and not at the within country level.

[21] We also experimented with other transformations of the Levy-Yeyati and Sturzenegger (2003) indicators of exchange rate regimes finding similar results, but for parsimony and easiness of interpretation we use this dummy.

stops. Thus, creation will be lower in these sectors. The effect on destruction is ambiguous as plants can adjust their sizes.

2. Firms in sectors more exposed to liquidity needs are likely to destroy more during a sudden stop.

We use our data on job flows from continuing versus all plants to contrast the effect of the financial shock on the complete sample of plants versus those that have survived between two consecutive periods to shed some light on the potential role of plant closing and opening in the process.

Then, we seek evidence along these lines using data on gross job flows in Latin America, over a sample period where these countries suffered significant sudden stops. We estimate the following equation:

$$y_{ijt} = \alpha S_{jt} + \delta m_j S_{jt} + \rho z_i S_{jt} + \mu + \varepsilon_{ijt}, \tag{5}$$

where y_{ijt} is some measure of job flows (mainly creation and destruction and in some specifications net employment growth) in sector i, country j, and time t, S is a measure of external shocks to financial conditions –sudden stops in this paper–, m_j is a vector of country specific institutional variables (e.g. labor market regulation and a proxy for the rule of law), z_i is a vector of sector specific characteristics (e.g. financial dependence and liquidity needs), and μ is a vector of fixed effects that includes country, year and sector fixed effects, and in some specifications it also includes interactions of (any two) of them. Finally, all sector and country variables are included as deviations with respect to their sample means to facilitate interpretation.

The interaction effects ($z_i S_{jt}$ and $m_j S_{jt}$) are the most important part of this regression for testing the main hypotheses of our paper. The sector specific characteristics are related to financial characteristics of the sectors, and we will follow the existing literature assuming that at least part of the observed differences across sectors in financial outcomes is associated with technological differences. Thus, the α coefficients reflect estimates of the effects of sudden stops on an average country and on the average sector, and hence gives an estimate of the baseline effect of the sudden stops on labor flows.

In many cases sudden stops are accompanied by abrupt changes in relative prices, particularly in the real exchange rate. Consequently our sudden stop variable may be capturing, partially at least, the effect of real exchange rate changes during the periods of current account reversals. We thus add the real exchange rate (in different specifications) to our baseline regression, and we estimate

$$y_{ijt} = \alpha S_{jt} + \delta m_j S_{jt} + \rho z_i S_{jt} + \sum_i \pi_i RER_{jt} + \mu + \nu_{ijt}, \tag{6}$$

where all variables are as defined in equation (5), and RER_{jt} is a measure of the real exchange rate and we allow π to be different for each sector in order to capture different sensitivities to relative prices, which might be due to different degrees of tradability, among other factors.

As has been noted before, our main analysis restricts the sample of countries to Brazil, Chile, Colombia and Mexico. There are two different reasons to drop Argentina and Uruguay. First, we do not identify any sudden stop in Uruguay during the years for which we have job flows data. Second, the nature of the original surveys from which data was collected in both countries differs from the

rest. For both countries there is no information on new plants, as only continuing plants are observed in their sampling. This lack of data makes it impossible to compare continuing and all plants data.[22]

We also implement some additional regressions adding additional controls and interactions (such as trade openness, the exchange rate regime, and labor market reforms) and implement some instrumental variable regressions. Given that these exercises are mostly additional checks, we discuss them in Section 4.

3.3 Identification

Sector level data allows us to control for unobserved country characteristics and rely on particular sector specific (but not country-sector specific) variables to identify sector specific effects of sudden stops. Part of this effect comes from interaction effects between sector characteristics and the prevalence of sudden stops, e.g. we expect sectors that rely more on external financing or have less access to collateral to suffer more during a sudden stop than sectors with better chances of self-financing its operations (or at least part of them). The same argument follows for the liquidity related variables, as the source of identification is the same.

Our identification of differentiated sectoral sensitivity to sudden stops relies on the assumption that any determinant of the sudden stop (or its size) may not to be systematically correlated with sector characteristics that determine the sensitivity of firms in each sector to the sudden stop, which in our case are financial dependence and liquidity needs (or any other sector characteristic that is correlated with any of these two characteristics). Notice that it does not require the sudden stop to be independent of country characteristics, but to be uncorrelated with determinants of the sector specific sensitivity to them. We believe this condition to be weaker than the one we would need to identify direct effects of sudden stops on creation and destruction.

Our discussion above implies that of the two sets of estimates we obtain, it is more plausible to give some structural or causal interpretation to the sector characteristics. Even if we were not able to interpret some of the coefficients as causal effects, our results can still be interpreted as stylized facts about correlations between financial characteristics and the extent of the equilibrium response of sectoral gross job flows to sudden stops.

We also implement a falsification exercise to check for our identification assumptions. We run our equations using *lagged* creation/destruction rates (ie., y_{ijt-1}) as the left-hand side variable. If we found a significant effect of sudden stops in *the future* and/or of interactions of sudden stops *in the future* with sectoral financial characteristics on job flows *today*, that would imply that there is either reverse causality or some omitted variable(s) is(are) driving our results. Albeit certainly imperfect, this procedure allows us to check our basic identification assumptions.

Finally, a small comment on our proxies for sectoral financial characteristics. The use of US-based measures has caused some controversy in the literature because of the assumption that we can extrapolate to different countries. There are two elements to consider in this respect. First, there is evidence that rankings based on the Rajan and Zingales (1998) measure of financial dependence per-

[22] We also present results using all countries in Table 8 and there we can observe that our main conclusions do not depend on this selection criteria.

forms well in other countries (Ilyina and Samaniego (2008)). Second, as we are interested in intrinsic (most likely technological) characteristics that make sectors differ in their financial decisions, we can think of equation (5) either as the reduced form of an IV estimation where the US-based measure is used as an instrument for the country specific variables or as an equation in which the interactions if sudden stops and financial sector characteristics are affected by attenuation bias (Raddatz (2006)). Therefore, we do not think it is a problem to use US-based measures of sector characteristics and, if anything, our estimates are biased towards 0 because of attenuation bias, so they are conservative estimates of the interaction effects related to sectoral financial characteristics.

4 Results

Following, our previous discussion, we start presenting our basic results for the estimated effects of sudden stops on job creation and job destruction, in Tables 3 and 4 respectively. In both tables we present two panels: in panel A we show results of job flows using data from all plants and in panel B we show estimates using job flows from continuing firms only. In addition, in each table we present eight different specifications. In the first three columns we present the direct effect of sudden stops and include interactions of sectoral financial characteristics and sudden stops (in columns 1 and 2 including separately each characteristic and in column 3 including both at the same time). Next, in column 4 we include interactions of sudden stops with two country level characteristics that may affect the reactions of the economy to sudden stops: proxies for labor market regulations and the rule of law. In columns 5 to 7 we include two-way fixed effects to check the robustness of results in column 4 to include: $country * year$ fixed effects (column 5), $sector * year$ fixed effects (column 6), and $country * sector$ fixed effects (column 7). Finally, in column 8 we include interactions of the (log of the) real exchange rate and sector dummies to see whether the estimated effects are not being confounded by the heterogeneous impact of real exchange rate fluctuations at the sectoral level.[23]

4.1 Sudden Stops and Labor Flows

The main results for the effects of sudden stops on creation and destruction by all firms can be observed in the top row of panel A in Tables 3 and 4. Table 3 shows the effects on job creation, where we estimate a consistently negative effect of between -2.3% and -3.1%, depending on the specification. Results in Panel B imply that the negative impacts on creation *by continuing firms* are also negative but smaller suggesting that the effects of sudden stops on the extensive margin of creation could be more relevant. The results for job destruction are in the first row of each panel in Table 4; there we observe that the negative impact of sudden stops on destruction are between -4.8 and -5.2%. In this case, we see that the effect of sudden stops is not sensitive to whether the sample is restricted to only continuing plants or not.

The estimated effects are also economically relevant. During sudden stops destruction is between 40% and 62% larger than in an average year (in the average sector and country), implying a very large effect of sudden stops on labor flows. Results for creation rates of all firms imply a negative

[23] Notice that when using $country * year$ fixed effects we cannot identify the direct effect of sudden stops since this fixed effect annihilates the effect of any other variable that varies at the $country * year$ level.

effect which is equivalent to about 20% of creation in the average year.

Although not the central results of our paper and not surprising, these results are important, particularly because they imply that labor market flows (and potentially frictions) are relevant in any model that wants to explain the economic effects of sudden stops on a developing economy. To study in more detail this point, we now move to sectoral effects.

4.2 Sectoral Effects

While the results on the average effect of sudden stops are important and highlight an aggregate pattern for the effects in manufacturing sectors, they also hide significant differences across sectors. In particular, we focus on financial fragility or exposure to financial market conditions. As previous literature and our motivation theory suggest, both dimensions are likely to affect hiring and firing decisions by firms: new projects may be delayed, some plants/firms may reduce their scale because of financing problems, etc.[24]

4.2.1 Financial Dependence

The rows labeled *Fin*SS* in Tables 3 and 4 correspond to the estimated effects of the interaction of the Raddatz (2006) measure of financial dependence by sector with the sudden stop variable. In all specifications including creation rates as dependent variables, the coefficient for *Fin*SS* is negative as can be seen in both panels of Table 3. Moreover, the estimates are statistically significant in 13 of the 16 columns. The cases in which this interaction is not statistically significant are: the specifications in which we control for $sector * year$ fixed effects in both panels (column 6) and the specification for all plants in which we control for $sector * country$ fixed effects (column 7 in panel A). Given that the introduction of these two-way fixed effects may be decreasing the efficiency of the estimates without affecting the consistency of them, we implement simple Hausman tests in which we compare our estimates for this interaction with estimates that do not include these two-way fixed effects (column 4 in both panels). Under the null, both estimates are consistent and the estimates that do not include two-way fixed effects are more efficient. Results imply that we cannot reject the null hypothesis and therefore we prefer the more efficient estimates (without two-way fixed effects).[25] Interestingly, the estimated interaction effects do not seem to be different in both panels.[26]

Our main results using *Fin*SS* (column 4) suggest that during a year long sudden stop, job creation in the sector with the highest financial exposure is approximately 2.1 percentage points smaller than in the sector with the smallest financial exposure, and approximately 1.7 percentage points smaller than in the average sector of our sample.[27]

In contrast to job creation, our estimates for the effect of *Fin*SS* on destruction rates are positive,

[24] Another margin refers to destruction of plants and the consequent separation of workers, unfortunately, as we mentioned before, due to lack of data, we cannot study this channel. Similarly, we cannot follow plants individually, thus we cannot track what fraction of the changes comes from reductions within a firm and how much comes from changes in the number and size of firms that enter and exit the market. We leave both aspects as topics for further research.

[25] The relevant p-values are the following: 0.34 for column 6 in panel A, 0.50 for column 6 in panel B, and 0.22 for column 7 in panel A.

[26] We also run a "pooled" specification with data from both continuing and all plants and find similar results.

[27] The same numbers are 2.0 and 1.6, respectively, for continuing plants only.

but never statistically different from 0 (except for the case of column 2 in panel A when we do not control for our measure of liquidity needs). This is consistent with our theoretical motivation and implies that for the destruction margin the effect of sudden stops on sectoral flows do not depend significantly from financial dependence of the firms.

4.2.2 Liquidity Needs

The results for short-run liquidity needs are in the rows labeled *(I/S)*SS* (for inventories over sales) in Tables 3 and 4. We observe that in Table 3 most of the coefficients for *(I/S)*SS* are negative, they are never statistically significant. The opposite picture arises in the case of job destruction in Table 4, where the coefficients are always positive and statistically different from 0 in most specifications (in 13 out of 16 estimates). As in the previous regressions for *Fin*SS*, the estimates are not statistically significant in columns 6 in both panels and in column 8 in panel A. As before, to check whether the inclusion of two-way fixed effects (in this case $sector * year$ fixed effects) are affecting the consistency or just the efficiency of the estimates, we performed Hausman tests and found that the estimates of columns 4 and 6 in both panels are not statistically different among them.[28] Therefore, we conclude that the lack of significance of estimates in column 6 in both panels is due to inefficiency and not to bias in the estimates effects without including $sector * year$ fixed effects.

In the case of estimates in column 8 in Table 4 (i.e. the effects of sudden stops on job destruction), the *(I/S)*SS* term is statistically significant only for continuing plants and decreases in magnitude implying that part of the effects of the financial characteristics of the sectors are more related to heterogenous effects of real exchange rate movements on destruction. The fact that the decrease in the point estimates is relatively small (it decreases by just about 15% from estimates in column 4), and that the coefficient is similar for both continuing and all plants makes us believe that overall *(I/S)*SS* has a negative effect on destruction rates.

The estimated results regarding the impact of *(I/S)*SS* are also economically relevant in magnitude. For the case of continuing plants, on average the sector with the highest value for *(I/S)*SS* exhibits a job destruction flow 2.3 percentage points higher than the sector with the lowest value (considering our most conservative estimate in column 8 Panel A). This difference represents approximately 50% of the effect of a sudden stop on job destruction in the average sector.[29] Overall, these results suggest that patterns of job flows across sectors during a sudden stop are related to the financial characteristics of the sectors.

4.2.3 Financial Factors Overall

It is important to emphasize that our results suggest that our measures of financial characteristics, financial dependence and liquidity needs, are related to different margins. First, this dichotomy is interesting from an empirical point of view and we believe it to be reasonable, given the way the proxy variables are constructed and what they are supposed to capture. Furthermore, these effects

[28] The relevant p-values are the following: 0.92 for column 6 in Panel A and 0.36 for column 6 in Panel B.

[29] Similar calculations for only continuing firms yield a 2.1 percentage point increase in job destruction for the sector with the highest value of *(I/S)* with respect to the sector with the lowest value, and an effect which is equivalent to 37% of the effect of a sudden stop on the average manufacturing sector (we also use estimates in column 8).

on separate margins are also robust to changes in the specification of the regressions. Second, this is a new result in the literature on financial frictions and sector outcomes; previous results have shown that both dimensions are correlated with sectoral variation and volatility at the sectoral level, but do not distinguish between creation and destruction margins –because of the lack of data.[30] Thus, our results are also consistent with the evidence in Raddatz (2006), who finds that liquidity variables, and not the external financing ones, explain growth volatility in a panel sample of manufacturing sectors; the magnitude of our results imply that liquidity variables produce a larger variation in the observed flows than external financing variables do.

Finally, analyzing two separate margins on gross flows allows us to depict a slightly more detailed picture of the mechanics behind some of the observed results regarding financial characteristics. We interpret our results as evidence that there is indeed a connection to both aspects of finance and that we are not capturing a more general idea of financial constraints, with each gross margin having a closer relation to one of the finance characteristics, with the extent of this relation partially hidden when looking at a more macro level. From the point of view of the effects of sudden stops, the point estimates also suggest that financial characteristics play a role in net job flows and total reallocation, defined as the sum of creation and destruction for a sector, during a sudden stop. We turn to this point in the next section.

4.3 Sudden Stops, Financial Characteristics, and Net Labor Growth

In this section we extend our previous analysis by estimating equation 5 using net labor flows as the left-hand side variable. Table 5 presents the results of estimating models similar to those in the previous section. We also present estimates for all plants and for continuing plants separately in each panel. Given the close relationship with results in the previous sections we focus on the main results in Table 5.

Consistent with the results in the previous section, sudden stops have a significantly robust and negative effect on net creation at the sector level. The estimates for the sample including all firms imply that net employment growth decreases between 7.5 and 8 percentage points in a year with a sudden stop. The results for the dataset including job flows of continuing firms are slightly lower in absolute value with point of between -5.6 and -5.9 percentage points.

Regarding the sectoral impact of sudden stops on net employment growth, point estimates confirm results of negative impact of both dimensions (external dependence and liquidity needs) on the effect of sudden stops on net labor flows. However, results in these cases are slightly less robust to the inclusion of both financial variables together, but in no case signs are overturned. In general, the interaction of sudden stops with our proxy for sectoral external dependence (Fin*SS) is always significant and with point estimates fairly robust (except for inefficient models in column 6 in both panels in which the inclusion of sector*year fixed effects affect the efficiency of the estimates without affecting point estimates, as discussed before). In turn, the interaction of sudden stops with liquidity needs *(I/S*SS)* presents the expected sign but is significant in just four specifications.

However, if we consider the size of the estimated impacts on net employment growth, both vari-

[30] See for example Braun and Larraín (2005), Raddatz (2006) and references therein.

ables have impacts of the same order of magnitude (using our preferred estimates in Panel A, column 4). The sector with the highest external dependence decreases net employment growth by 2.8 percentage points less than the sector with lowest value for external dependence when there is a year long sudden stop. A similar calculation regarding liquidity needs imply a differential net growth of 3.9 percentage points. However, these effects are not precisely estimated. Thus, our reading of these results is that probably the lack of significance of *(I/S*SS)* is more related with precision problems than with a zero impact on net employment growth.

4.4 Robustness Checks

In order to check the robustness of our results we perform four different groups of exercises. The first two exercises are related to study the identification strategy we use in the paper. The second set of exercises are related to include additional control and interaction variables. The third set of exercises are related to using alternative proxies for sectoral financial characteristics. The final set of exercises relate to changing the sample on which we are estimating.

4.4.1 Falsification exercises

In Table 6 we present a set of falsification exercises in which we run the same specifications of column 4 in Tables 3 and 4 but using the lag of our gross job flows measures as the left-hand side variables in each regression. Our aim is to study the endogeneity of our sudden stop variables to overall and sectoral job shocks. If we found a positive effect of sudden stops in the future and/or of interactions of sudden stops in the future with sectoral financial characteristics on job flows today, that would imply that there is either reverse causality or any other omitted variable is driving our results. Albeit certainly imperfect, this procedure allows us to check this basic identification assumption.

Table 6 presents the results. Interestingly, none of the variables has a significant impact on lagged creation and destruction rates. Moreover, the size of the estimated effects are clearly smaller than those estimated in previous tables suggesting that the lack of significance is not due to increases in the estimated standard errors. Thus, we conclude from this table that our results are not driven by reverse causality or other biases related to the potential endogeneity of sudden stops to domestic omitted variables affecting both job flows and sudden stops.

4.4.2 Sudden stops and financial crises

Next, we study how our results are related to the potential effects of sudden stops on financial crises. Our theoretical argument relates mainly to financial market conditions and, therefore, we could study how sudden stops affect job flows through their effects on banking crises. To implement this idea we follow the literature and use the financial crises identified in Caprio et al (2003) (and used by several papers, e.g. Cerra and Saxena (2008)). We identify a systematic financial crisis with a dummy that takes a value of 1 for all the years marked as crisis years in that paper (we denote this dummy variable by *FC*). Caprio et al (2003) identify financial systemic crises in our sample for the

following years: Argentina (1995, 2001), Brazil (1994-1999), Chile (1981-1986), Colombia (1982-1987), and Mexico (1994-1997).

Given that a share of these financial crises is domestic in nature, and therefore highly endogenous, we implement an IV procedure in which we use sudden stops as an instrumental variable for *FC*. This way, we identify the effect of *FC* on job flows that is due to the effect of sudden stops on a financial crisis. Our estimates in Tables 3 and 4 could be interpreted as reduced forms of these IV regressions. Due to collinearity problems, we can only identify the interaction effects.[31] This is not a fundamental problem, as the main focus of the paper is the identification of these interaction effects.

We report these results in Table 7. Before going to the instrumental variable estimates, we discuss the first stages.[32] In the first stage regression for *Fin*FC*, the interaction *Fin*SS* is statistically significant (with a coefficient of 0.29 and t-stat of 4.09) but the interaction *(I/S)*SS* is not different from 0 (with a coefficient of -0.08 and a t-stat of -0.17). Analogously, in the first stage for *(I/S*FC)*, the interaction the interaction *(I/S)*SS* is statistically significant (with a coefficient of 0.27 and t-stat of 3.68) but the interaction *Fin*FC* is not different from 0 (with a coefficient of 0.0001 and a t-stat of 0.01). In terms of diagnostic tests for underidentifcation, the Kleibergen-Paap LM statistic is 8.79 thus we reject the null hypothesis of under-identification (with a p-value of 0.003). In terms of weak identification, the Cragg-Donalds F statistic of weak identification–suggested by Stock and Yogo (2002)– is 4.51, very close to the 15% maximal IV size of 4.58 and, therefore, we do not seem to have a problem of weak instruments. Thus, these results suggest that the IVs have the expected signs in the first stage and are statistically significant and that the IV estimates do not suffer from a weak instruments or an under-identification problem.

We report IV results in columns 1 and 2 of Table 7. Estimates related to creation flows are mostly consistent with our estimates in Table 3: only the interaction between financial crises and *Fin* is positive and statistically significant. The point estimate is actually bigger than in Table 3 suggesting that, as expected, when a sudden stop creates a financial crisis its impacts are amplified by sectoral financial frictions.

Results for destruction rates are less precisely estimated. The point estimates for the interaction of *(I/S)* and *FC* are bigger than the point estimates for interaction of *Fin* and *FC*. This is consistent with our results in Table 4. However, the interaction of (I/S) and *FC* is only marginally significant (p-values of 0.12 and 0.11 in Panels A and B, respectively).[33] As in case of the point estimates of the interaction between *Fin* and *FC*, point estimates are bigger than the estimates in Table 4, suggesting that also in this case financial frictions amplify the effects of sudden stops that produce banking crises, as expected.

In all, results in this section give additional evidence that is consistent with our theoretical motivation emphasizing the potential effects of sudden stops through financial channels.

[31] The complete IV procedure to estimate an specification that is analogous to our preferred estimates in Tables 3 and 4 imply the estimation using 5 variables as instruments for 5 potentially endogenous variables (SS, SS*Fin, SS*(I/S), SS*labor, and SS*rule-of-law). This procedure yielded unreliable second stage estimates due to the collinearity in the five first stages.

[32] These results are available upon request.

[33] If we just include the interaction between *(I/S)* and *FC*, the estimate coefficient is statistically different from 0 with p-values of 0.06 in both panels.

4.4.3 Additional control variables

In the next group of exercises we add additional controls related to policy characteristics of the countries. Given the fact that we only have four countries in our main estimations, we do not have enough data variation to derive clear implications on the direct effects of these variables on gross flows. Thus, we take these exercises mostly as robustness checks to our initial estimates and focus on how our interactions of sudden stops and sectoral financial characteristics change. The three variables we use are: the degree of trade openness of the countries, the exchange rate regime, and a proxy for labor market reform.[34] In each case we present in Table 8 the coefficients on variables of interest (*Fin*SS* and *(I/S)*SS*) and triple-interactions with each variable.

We start with trade openness. Results in column 1 in both panels imply that for job creation the interaction *SS*Fin* is statistically significant an maintains the size even after controlling for the direct and interactive effects of trade openness. No triple interaction is statistically significant suggesting that the degree of trade openness does not affect how frictions affect sudden stop shocks. Regarding job destruction rates (see column 6), similar results appear: as in our basis case, only the interaction *(I/S)*SS* is statistically significant and the introduction of triple interactions does not affect the estimated coefficients in comparison to the results we obtain in Table 4 (even in the case of Panel B, the estimated coefficient is more precisely estimated).

Next, we consider the exchange rate regime using a dummy that takes a value of 1 if the de facto exchange rate regime is classified as fixed accordingly to Levy-Yeyati and Sturzenegger (2003). Results appear in columns 2 and 7 in Table 8. The estimated effect of *SS*Fin* on the creation margin for all firms is very similar to the estimated effects on Table 3 but is only marginally significant (p-value of 0.15). Interestingly, however, the triple interaction of this variable with the fixed exchange rate regime variable is negative and an F test of the sum the coefficients on SS*Fin and the triple interaction yields that the sum of the two coefficients is different from 0 (p-value of 0.06). In the case of the estimated effects for creation for continuing firms *SS*Fin* is statistically significant and the triple interaction is very close to 0.

Regarding effects on the destruction margin (in column 7), for both continuing and all firms, *(I/S)*SS* is statistically significant and presents a very similar value to the one we obtained in Table 4. The triple interaction in this case is negative and important in absolute value but not statistically significant. We take these results as suggestive that the results we find in Tables 3 and 4 are robust, but also as suggestive (weak) evidence that the exchange rate regime may interact in a differentiated way with financial frictions in the creation and destruction margins. We leave a detailed study of this point for future research.

The third policy dimension we study is labor market regulation. We use a variable identified using information from Heckman and Pagés-Serra (2004). As previously discussed our proxy takes a value of 1 if the country implemented labor reforms that increased legal protection of workers, a value of -1 if the country implemented labor reforms that decreased legal protection of workers, and a value of 0 if the country did not implemented a labor reform. Results are presented in columns 3 and 8 in Table 8. On the creation margin, *SS*Fin* is statistically significant and keeps a value similar to those

[34] We thank the referees for suggesting us to perform exercises using these variables.

estimated in Table 3 in both panels. The triple interactions are not statistically significant. In turn, on the destruction margin, results in Panels A and B show that *(I/S)*SS* is positive and statistically significant, even tough the point estimates decrease. No triple interaction is significant.

Summarizing, we interpret these results mostly as robustness checks of our main results in Tables 3 and 4, and they usually confirm our baseline results.[35]

4.4.4 Different sample coverage

In this set of robustness checks we study whether our main results are robust to two variations in the sample coverage on which we estimate. The first relates to excluding Mexico from the estimating sample. We perform this exercise because as discussed in Section 3.1, the sudden stop the literature identifies for Mexico is probably highly related to domestic conditions. Even tough we already have presented exercises that deal with the potential endogeneity of sudden stops, we present this exercise as an additional robustness check. Columns 4 and 9 in Table 8 present the results. Results are very similar to the ones presented in Tables 3 and 4: a positive and significant interaction *(I/S)*SS* for destruction rates, and a negative and significant interaction *Fin*SS* for creation rates. If anything the results in these columns are more precisely estimated than those in Tables 3 and 4, in spite of the decrease in the sample size.

Finally, we include Argentina and Uruguay in our sample for estimates using data for only continuing firms. In all the previous tables we include in both panels the same country coverage in order to allow us comparing between continuing and all plants while maintaining the same pool of countries. Here we expand the sample coverage to these two additional countries and present the results in columns 5 and 10 of Panel B of Table 8. Results for our two interactions remain significant and the estimated coefficients are very similar to those we present in Tables 3 and 4.

4.4.5 Different measures of sectoral financial characteristics

As we discussed in Section 3.1 there are different measures of financial sector characteristics available in the literature. In this section we present regressions in which we use two alternative proxies for external dependence (the Micco and Pagés-Serra (2006) and Rajan and Zingales (1998) proxies for external dependence, which we denote *Fin1* and *Fin2*, respectively) and one alternative proxy for liquidity needs (*CCC*). Table 9 presents the results. In columns 1 and 4 we present our main specification using *Fin1* as the proxy for external dependence at the sectoral level. The estimated effect of the *SS*Fin1* variable is positive and economically and statistically significant with point estimates of the same order of magnitude as the estimated effects for the *SS*Fin*. Regarding the destruction

[35] In another robustness exercise, we add interactions of the high yield spreads (HYS) in the US with the sectoral financial characteristics to equation (5). Previous research treats a big rise in HYS as a (common) exogenous negative shock to external financing conditions for emerging markets and, as such, as a determinant of a potential sudden stop, see for example Caballero and Panageas (2007), and Gallego and Jones (2005). Thus, when adding this continuous indicator of external conditions (such as HYS) to our proxies for SS, we are studying whether the assumption in the literature and our paper that the relation between the continuous indicators of external conditions and the occurrence of sudden stops and their impacts on the economy is highly non-linear. We find that the interaction effects of SS and sectoral financial characteristics are robust to these additional terms and that most of the interactions of HYS and financial sector characteristics are not statistically different from 0. Results available upon request from the authors.

margin, as with *SS*Fin*, *SS*Fin2* has a zero effect. Next, in columns 2 and 5, we substitute *SS*Fin2* for *SS*Fin1* and also find very similar effects. Positive and significant effects of this variable on the creation margin and insignificant effects on the destruction margin.

Finally in columns 3 and 6 we use *CCC* as our proxy for liquidity needs. As with the interaction *(I/S)*SS*, *CCC*SS* has a zero impact on the creation margin and a negative and statistically and economically significant on the destruction margin.[36] Our estimates imply that, for the case of continuing (all) plants, on average the sector with the highest value for *CCC* exhibits a job destruction flow 3.4 (4.1) percentage points higher than the sector with the lowest value.

Thus, we conclude that our results are main robust to using alternative proxies for sectoral financial characteristics.

5 Concluding Comments

This paper studies the effects of sudden stops on job creation and destruction in a sample of Latin American countries, as captured by a measure of gross job flows at the sector level. We find consistent evidence that sudden stops are associated with decreased job creation and, particularly, increased job destruction. Importantly, we also observe the magnitude of the sectoral effects of the sudden stops on job flows to be related to financial characteristics of the sector: job creation tends to decrease more during sudden stops in sectors with strong dependence on external finance. Similarly, the increasing effect of sudden stops on job destruction is larger in sectors with higher liquidity needs. Simple calculations show that the associated sector differences are economically significant.

Studying the connection between reallocation and restructuring, and financial characteristics in response to sudden stops moves us forward in two different, but related, areas. First, and central to the main interest of this paper, it provides us with a novel look at the mechanics of sudden stops within countries. Since differences in the creation and destruction flows can affect the speed of adjustment and recovery during and after shocks, our results also signal the relevance of further studying the dynamics of the flows in the labor markets before, during and after a sudden stop, something that we leave as a topic for further research. Moreover, to the extent that the responses of different sectors are correlated with financial characteristics, the empirical results also suggest that we should incorporate financial market frictions into our study of the effects of sudden stops and why these differ across countries. The results on the relation between external financial dependence (i.e., Rajan-Zingales type of measures), liquidity needs (e.g., cash conversion cycle and inventories over sales), and the response of gross job flows to a country level shock, a sudden stop in this case, also complement previous studies on the relation between financial frictions and sectoral outcomes, in particular with respect to the effects on volatility and sensitivity to shocks.

Finally, as sudden stops constitute large financial shocks for a country as a whole, we also contribute to the literature on job flows, reallocation/restructuring, and financial conditions by presenting additional evidence from this "extreme" shock in emerging economies, which complements the existing evidence drawn from the effects of recession and business cycles in developed economies.

[36] Actually the statistical significance of results using *CCC* as proxy for sectoral liquidity needs is higher than when we use *(I/S)*. We still choose to be conservative and to present results using *(I/S)* as our main estimates.

The relation between sectoral financial characteristics, sector responses to sudden stops and the financial nature of the shock lends support to the idea that financial conditions do matter for the process of restructuring. Moreover, these results are qualitatively relevant for other situations and relate to the existing evidence on the microeconomic responses to macroeconomic shocks, particularly about the different responses of job creation and destruction.

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Table 1. Descriptive Statistics: Job Creation and Destruction, main countries.

	Panel (a). Summary Statistics for Job Flows					
	Job Creation		Job Destruction			
	All firms	Continuing firms	All firms	Continuing firms	All firms	Continuing firms
ARGENTINA (1991–2001)	.	0.053	.	0.089	.	-0.036
BRAZIL (1992–2000)	0.158	0.088	0.164	0.108	-0.006	-0.020
CHILE (1980–1999)	0.119	0.082	0.119	0.074	-0.000	0.008
COLOMBIA (1978–1991, 1993–1999)	0.095	0.067	0.103	0.105	-0.008	-0.037
MEXICO (1994–2000)	0.174	0.126	0.105	0.078	0.068	0.048
URUGUAY (1989–1995)	.	0.050	.	0.088	.	-0.038
Total	0.123	0.075	0.118	0.091	0.005	-0.016

	Panel (b). Summary Statistics for Country-specific variables						
	Sudden Stop	Banking Crisis	Rule of Law	Labor Costs	Trade Openness	Fixed Exchange Rate Regime	Labor market reform
ARGENTINA	0.258	0.182	5.350	0.559	0.201	0.909	0.000
BRAZIL	0.306	0.667	6.320	1.180	0.175	0.222	1.000
CHILE	0.225	0.300	7.020	1.098	0.505	0.100	0.450
COLOMBIA	0.111	0.286	2.080	0.835	0.268	0.000	-0.429
MEXICO	0.286	0.571	5.350	1.283	0.731	0.143	0.000
URUGUAY	0.000	0.000	5.000	0.244	0.327	0.000	0.000
Total	0.190	0.316	4.881	0.882	0.357	0.215	0.098

	Panel (c). Summary Statistics for Sector-specific variables			
	Mean	St. Dev.	Max	Min
External Dependence (Raddatz Proxy, Fin)	-0.000	0.228	0.461	-0.260
External Dependence (Micco-Pages Proxy, Fin1)	-0.000	0.215	0.309	-0.203
External Dependence (Rajan-Zingales Proxy, Fin2)	-0.000	0.122	0.174	-0.126
Inventory to Sales	0.000	0.038	0.057	-0.053
Currency conversion cycle (CCC)	-0.000	0.291	0.416	-0.495

Notes: Column 1 in Panel A shows sample coverage in parenthesis.

Table 2. Sample Coverage and Months in Sudden Stop

Country	Sample	SS periods
Brazil	1992-2000	1995.1–1995.12
		1998.1–1999.99
Chile	1980-1999	1982.1–1984.1
		1995.10–1996.8
		1998.1–1999.12
Colombia	1978-1991, 1993-1999	1978.1–1978.3
		1997.12–1999.12
Mexico	1994-2000	1994.1–1995.12
Argentina	1991-2001	1994.10–1995.12
		1999.5–1999.11
		2001.1–2001.12
Uruguay	1989-1995	None

Source: See text.

Table 3. Job Creation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel (a). All plants series							
Sudden Stop (SS)	-0.0228***	-0.0227***	-0.0228***	-0.0302**		-0.0291*	-0.0308**	-0.0314**
	(0.00534)	(0.00531)	(0.00532)	(0.0148)		(0.0168)	(0.0130)	(0.0145)
I/S*SS	-0.160		-0.0278	-0.0359	-0.0226	-0.0382	-0.153	-0.0690
	(0.104)		(0.128)	(0.130)	(0.116)	(0.219)	(0.129)	(0.138)
Fin*SS		-0.0429***	-0.0407**	-0.0407**	-0.0407**	-0.0148	-0.0324	-0.0373*
		(0.0159)	(0.0198)	(0.0193)	(0.0181)	(0.0346)	(0.0201)	(0.0203)
Labor Regulation Costs*SS				0.0308		0.0268	0.0322	0.0298
				(0.0576)		(0.0633)	(0.0491)	(0.0579)
Rule of Law*SS				-0.00322		-0.00306	-0.00344	-0.00302
				(0.00396)		(0.00459)	(0.00356)	(0.00401)
N	484	484	484	484	484	484	484	466
Adjusted R^2	0.599	0.602	0.601	0.600	0.725	0.474	0.674	0.620
	Panel (b). Continuing plants series							
Sudden Stop (SS)	-0.0165***	-0.0166***	-0.0165***	0.000561		0.00144	-0.0000378	-0.000487
	(0.00398)	(0.00397)	(0.00395)	(0.0105)		(0.0126)	(0.0102)	(0.0103)
I/S*SS	-0.0868		0.0381	0.0299	0.0388	0.0630	-0.0796	0.0120
	(0.0807)		(0.0945)	(0.0938)	(0.0904)	(0.165)	(0.101)	(0.0985)
Fin*SS		-0.0355***	-0.0386***	-0.0385***	-0.0386***	-0.0287	-0.0327**	-0.0372***
		(0.0116)	(0.0138)	(0.0139)	(0.0141)	(0.0260)	(0.0158)	(0.0141)
Labor Regulation Costs*SS				-0.0695		-0.0722	-0.0682*	-0.0701*
				(0.0425)		(0.0476)	(0.0385)	(0.0424)
Rule of Law*SS				0.000794		0.000905	0.000573	0.000935
				(0.00291)		(0.00345)	(0.00280)	(0.00294)
Observations	484	484	484	484	484	484	484	466
Adjusted R^2	0.525	0.530	0.529	0.533	0.673	0.423	0.610	0.554
One-way fixed effects	C,S,Y	C,S,Y	C,S,Y	C,S,Y	S	C	Y	C,S,Y
Two-way fixed effects	No	No	No	No	CY	SY	CS	No
LRER*sector dummies	No	No	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis. Significance level: *** p<0.01, ** p<0.05, * p<0.1. C: country fixed effects, S: sector fixed effects, Y: year fixed effects

Table 4. Job Destruction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel (a). All plants series							
Sudden Stop (SS)	0.0522***	0.0516***	0.0522***	0.0478***		0.0504***	0.0481***	0.0485***
	(0.00569)	(0.00571)	(0.00570)	(0.0175)		(0.0176)	(0.0147)	(0.0170)
I/S*SS	0.283**		0.237*	0.247*	0.228*	0.229	0.275*	0.209
	(0.121)		(0.141)	(0.139)	(0.120)	(0.230)	(0.146)	(0.143)
Fin*SS		0.0331*	0.0142	0.0142	0.0143	0.0210	0.0206	0.0201
		(0.0193)	(0.0223)	(0.0224)	(0.0187)	(0.0364)	(0.0228)	(0.0228)
Labor Regulation Costs*SS				0.0175		0.00818	0.0168	0.0189
				(0.0614)		(0.0664)	(0.0558)	(0.0616)
Rule of Law*SS				0.00166		0.00199	0.00178	0.00141
				(0.00446)		(0.00482)	(0.00405)	(0.00450)
N	484	484	484	484	484	484	484	466
adj. R^2	0.585	0.583	0.585	0.584	0.729	0.468	0.613	0.605
	Panel (b). Continuing plants series							
Sudden Stop (SS)	0.0412***	0.0406***	0.0412***	0.0569***		0.0573***	0.0572***	0.0581***
	(0.00538)	(0.00534)	(0.00537)	(0.0157)		(0.0134)	(0.0116)	(0.0154)
I/S*SS	0.282**		0.241**	0.216**	0.208**	0.0881	0.257**	0.192*
	(0.109)		(0.103)	(0.105)	(0.0996)	(0.175)	(0.115)	(0.109)
Fin*SS		0.0317	0.0125	0.0127	0.0127	0.0280	0.0155	0.0156
		(0.0196)	(0.0194)	(0.0189)	(0.0156)	(0.0277)	(0.0179)	(0.0194)
Labor Regulation Costs*SS				-0.0624		-0.0645	-0.0631	-0.0626
				(0.0522)		(0.0506)	(0.0438)	(0.0523)
Rule of Law*SS				-0.00373		-0.00372	-0.00361	-0.00384
				(0.00375)		(0.00367)	(0.00318)	(0.00378)
Observations	484	484	484	484	484	484	484	466
Adjusted R^2	0.559	0.555	0.558	0.571	0.688	0.488	0.604	0.597
One-way fixed effects	C,S,Y	C,S,Y	C,S,Y	C,S,Y	S	C	Y	C,S,Y
Two-way fixed effects	No	No	No	No	CY	SY	CS	No
LRER*sector dummies	No	No	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis. Significance level: *** p<0.01, ** p<0.05, * p<0.1. C: country fixed effects, S: sector fixed effects, Y: year fixed effects

Table 5. Net Creation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel (a). All plants series							
Sudden Stop (SS)	-0.0750***	-0.0744***	-0.0750***	-0.0779***		-0.0794***	-0.0788***	-0.0800***
	(0.00848)	(0.00842)	(0.00848)	(0.0268)		(0.0263)	(0.0224)	(0.0266)
I/S*SS	-0.443**		-0.265	-0.283	-0.251	-0.268	-0.428*	-0.278
	(0.187)		(0.221)	(0.222)	(0.166)	(0.344)	(0.222)	(0.237)
Fin*SS		-0.0761***	-0.0549	-0.0549*	-0.0550**	-0.0358	-0.0530	-0.0575*
		(0.0281)	(0.0335)	(0.0331)	(0.0259)	(0.0543)	(0.0347)	(0.0343)
Labor Regulation Costs*SS				0.0133		0.0186	0.0153	0.0108
				(0.0985)		(0.0992)	(0.0849)	(0.102)
Rule of Law*SS				-0.00488		-0.00505	-0.00522	-0.00443
				(0.00671)		(0.00720)	(0.00616)	(0.00699)
N	484	484	484	484	484	484	484	466
adj. R^2	0.484	0.485	0.485	0.484	0.710	0.336	0.500	0.519
	Panel (b). Continuing plants series							
Sudden Stop (SS)	-0.0578***	-0.0572***	-0.0577***	-0.0563**		-0.0559***	-0.0572***	-0.0586***
	(0.00767)	(0.00756)	(0.00763)	(0.0229)		(0.0212)	(0.0183)	(0.0225)
I/S*SS	-0.369**		-0.203	-0.186	-0.169	-0.0250	-0.336*	-0.180
	(0.160)		(0.166)	(0.170)	(0.151)	(0.277)	(0.182)	(0.181)
Fin*SS		-0.0673***	-0.0511*	-0.0512*	-0.0513**	-0.0567	-0.0482*	-0.0528*
		(0.0254)	(0.0270)	(0.0272)	(0.0236)	(0.0438)	(0.0284)	(0.0278)
Labor Regulation Costs*SS				-0.00711		-0.00771	-0.00510	-0.00753
				(0.0819)		(0.0800)	(0.0694)	(0.0825)
Rule of Law*SS				0.00452		0.00463	0.00418	0.00477
				(0.00566)		(0.00581)	(0.00503)	(0.00577)
Observations	484	484	484	484	484	484	484	466
Adjusted R^2	0.539	0.541	0.541	0.541	0.690	0.446	0.571	0.575
One-way fixed effects	C,S,Y	C,S,Y	C,S,Y	C,S,Y	S	C	Y	C,S,Y
Two-way fixed effects	No	No	No	No	CY	SY	CS	No
LRER*sector dummies	No	No	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis. Significance level: *** p<0.01, ** p<0.05, * p<0.1. C: country fixed effects, S: sector fixed effects, Y: year fixed effects

Table 6. Falsification Exercises

	(1)	(2)	(3)	(4)
Sample:	All plants	Continuing plants	All plants	Continuing plants
Dependent Variable:	Job Creation		Job Destruction	
Sudden Stop (SS)	-0.0212	0.00904	-0.0184	0.0111
	(0.0176)	(0.0208)	(0.0141)	(0.0139)
Labor Regulation Costs*SS	0.129*	-0.0761	0.111*	-0.0778
	(0.0735)	(0.0743)	(0.0594)	(0.0508)
Fin*SS	-0.0160	0.0413	-0.0147	0.0331
	(0.0269)	(0.0308)	(0.0220)	(0.0220)
I/S*SS	0.00143	0.127	-0.0511	0.0880
	(0.174)	(0.169)	(0.138)	(0.126)
Rule of Law*SS	-0.00799	-0.000317	-0.00550	0.00165
	(0.00531)	(0.00561)	(0.00426)	(0.00390)
N	450	450	450	450
Adjusted R^2	0.483	0.259	0.363	0.228
One-way fixed effects	C,S,Y	C,S,Y	C,S,Y	C,S,Y

Notes: Standard errors in parenthesis. Significance level: *** p<0.01, ** p<0.05, * p<0.1. C: country fixed effects, S: sector fixed effects, Y: year fixed effects

Table 7. Sudden Stops and Financial Crises, Instrumental Variable estimations; all plants series

	(1)	(2)	(3)	(4)
Sample:	All plants	Continuing plants	All plants	Continuing plants
Dependent Variable:	Job Creation		Job Destruction	
Fin*FC	-0.138*	0.0482	-0.131**	0.0428
	(0.0738)	(0.0863)	(0.0620)	(0.0739)
(I/S)*FC	-0.126	0.866	0.105	0.789
	(0.480)	(0.556)	(0.351)	(0.492)
N	484	484	484	484
Adjusted R^2	0.637	0.611	0.554	0.522
One-way fixed effects	C,S,Y	C,S,Y	C,S,Y	C,S,Y

Notes: Standard errors in parenthesis. Significance level: *** p<0.01, ** p<0.05, * p<0.1. C: country fixed effects, S: sector fixed effects, Y: year fixed effects

Table 8. Robustness checks

Dependent variable:	Job Creation					Job Destruction				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel (a). All plants series									
Fin*SS	-0.0452**	-0.0312	-0.0419**	-0.0502***	–	0.0105	0.0130	0.0172	0.0187	–
	(0.0190)	(0.0214)	(0.0180)	(0.0190)		(0.0250)	(0.0259)	(0.0216)	(0.0280)	
I/S*SS	-0.0648	-0.146	-0.0199	-0.119	–	0.251*	0.329**	0.242*	0.319*	–
	(0.124)	(0.141)	(0.121)	(0.122)		(0.150)	(0.162)	(0.138)	(0.166)	
(I/S)*SS*Trade Openness	0.470					-0.155				
	(0.813)					(0.860)				
(Fin)*SS*Trade Openness	0.0513					0.0837				
	(0.124)					(0.141)				
(I/S)*SS*Fixed Exchange Rate Regime		0.542					-0.495			
		(0.334)					(0.309)			
(Fin)*SS* Fixed Exchange Rate Regime		-0.0549					0.0586			
		(0.0506)					(0.0572)			
(I/S)*SS*Labor market reform			-0.0366					-0.0172		
			(0.137)					(0.164)		
(Fin)*SS*Labor market reform			0.00547					-0.0142		
			(0.0211)					(0.0267)		
N	484	448	484	421		484	448	484	421	
Adjusted R^2	0.616	0.606	0.630	0.596		0.603	0.601	0.593	0.602	
	Panel (b). Continuing plants series									
Fin*SS	-0.0423***	-0.0372**	-0.0407***	-0.0457***	-0.0276**	0.0100	0.0130	0.0177	0.0130	0.0190
	(0.0133)	(0.0152)	(0.0134)	(0.0132)	(0.0115)	(0.0209)	(0.0212)	(0.0196)	(0.0224)	(0.0173)
I/S*SS	0.000373	-0.0627	0.0399	-0.0419	0.00143	0.237**	0.290**	0.193*	0.308**	0.190**
	(0.0894)	(0.103)	(0.0901)	(0.0884)	(0.0762)	(0.116)	(0.122)	(0.106)	(0.123)	(0.0895)
(I/S)*SS*Trade Openness	0.339					-0.296				
	(0.584)					(0.650)				
(Fin)*SS*Trade Openness	0.0560					0.0556				
	(0.0898)					(0.112)				
(I/S)*SS*Fixed Exchange Rate Regime		0.442**					-0.308			
		(0.222)					(0.220)			
(Fin)*SS* Fixed Exchange Rate Regime		-0.00240					0.00840			
		(0.0394)					(0.0485)			
(I/S)*SS*Labor market reform			-0.0429					0.0723		
			(0.0911)					(0.122)		
(Fin)*SS*Labor market reform			0.0102					-0.0241		
			(0.0132)					(0.0231)		
N	484	448	484	421	646	484	448	484	421	
Adjusted R^2	0.551	0.550	0.591	0.470	0.528	0.576	0.582	0.588	0.583	
One-way fixed effects	C,S,Y	C,S,Y	C,S,Y	C,S,Y	C,S,Y	C,S,Y	C,S,Y	C,S,Y	C,S,Y	C,S,Y
Countries incuded	BRA, CHL COL, MEX	BRA, CHL COL, MEX	BRA, CHL COL, MEX	BRA, CHL COL	ARG, BRA CHL, COL MEX, URU	BRA, CHL COL, MEX	BRA, CHL COL, MEX	BRA, CHL COL, MEX	BRA, CHL COL	ARG, BRA CHL, COL MEX, URU

Notes: Standard errors in parenthesis. Significance level: *** p<0.01, ** p<0.05, * p<0.1. C: country fixed effects, S: sector fixed effects, Y: year fixed effects. ARG: Argentina, BRA: Brazil, CHL: Chile, COL: Colombia, MEX: Mexico, URU: Uruguay.

Table 9. Alternative Definitions of Sectoral Financial Characteristics

Dependent variable	Job Creation			Job Destruction		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel (a). All plants series					
Sudden Stop (SS)	-0.0305**	-0.0304**	-0.0303**	0.0478***	0.0478***	0.0480***
	(0.0148)	(0.0147)	(0.0148)	(0.0175)	(0.0174)	(0.0174)
Labor Regulation Costs*SS	0.0316	0.0314	0.0312	0.0175	0.0174	0.0170
	(0.0578)	(0.0577)	(0.0579)	(0.0612)	(0.0613)	(0.0611)
Fin1*SS	-0.0415**			0.000758		
	(0.0186)			(0.0210)		
I/S*SS	-0.0674	-0.0112		0.291**	0.261*	
	(0.119)	(0.128)		(0.131)	(0.149)	
Rule of Law*SS	-0.00335	-0.00332	-0.00328	0.00167	0.00169	0.00176
	(0.00396)	(0.00395)	(0.00397)	(0.00444)	(0.00445)	(0.00442)
Fin2*SS		-0.0897**			0.0183	
		(0.0375)			(0.0468)	
Fin*SS			-0.0352			-0.000897
			(0.0220)			(0.0250)
CCC*SS			-0.0109			0.0456**
			(0.0198)			(0.0196)
N	484	484	484	484	484	484
Adjusted R^2	0.600	0.601	0.600	0.584	0.584	0.586
	Panel (b). Continuing plants series					
Sudden Stop (SS)	0.000243	0.000351	0.000367	0.0569***	0.0569***	0.0570***
	(0.0105)	(0.0105)	(0.0105)	(0.0157)	(0.0157)	(0.0156)
Labor Regulation Costs*SS	-0.0688	-0.0690	-0.0691	-0.0624	-0.0624	-0.0627
	(0.0427)	(0.0427)	(0.0427)	(0.0522)	(0.0523)	(0.0521)
Fin1*SS	-0.0358***			0.00243		
	(0.0135)			(0.0171)		
I/S*SS	-0.00857	0.0271		0.251**	0.250**	
	(0.0868)	(0.0931)		(0.105)	(0.119)	
Rule of Law*SS	0.000676	0.000716	0.000722	-0.00372	-0.00372	-0.00367
	(0.00291)	(0.00291)	(0.00293)	(0.00375)	(0.00375)	(0.00374)
Fin2*SS		-0.0700***			0.00388	
		(0.0271)			(0.0386)	
Fin*SS			-0.0351**			0.000959
			(0.0160)			(0.0208)
CCC*SS			-0.00142			0.0378**
			(0.0148)			(0.0153)
N	484	484	484	484	484	484
Adjusted R^2	0.532	0.532	0.533	0.571	0.571	0.573
One-way fixed effects	C,S,Y	C,S,Y	C,S,Y	C,S,Y	C,S,Y	C,S,Y

Notes: Standard errors in parenthesis. Significance level: *** p<0.01, ** p<0.05, * p<0.1. C: country fixed effects, S: sector fixed effects, Y: year fixed effects

Table A.1. Descriptive Statistics: Job Creation and Destruction, main countries.

	Obs	Mean	Std. Dev.	Max	Min	p5	p50	p95
			Brazil					
Creation (Continuing)	72	0.088	0.024	0.147	0.044	0.055	0.084	0.131
Creation (All)	72	0.158	0.035	0.245	0.085	0.101	0.154	0.218
Destruction (Continuing)	72	0.108	0.026	0.183	0.056	0.069	0.104	0.160
Destruction (All)	72	0.164	0.032	0.263	0.104	0.120	0.159	0.220
			Chile					
Creation (Continuing)	160	0.082	0.041	0.213	0.006	0.020	0.078	0.156
Creation (All)	160	0.119	0.055	0.267	0.010	0.034	0.116	0.221
Destruction (Continuing)	160	0.074	0.046	0.294	0.005	0.023	0.067	0.151
Destruction (All)	160	0.119	0.070	0.370	0.005	0.029	0.109	0.255
			Colombia					
Creation (Continuing)	189	0.067	0.026	0.135	0.011	0.027	0.067	0.116
Creation (All)	189	0.095	0.034	0.197	0.025	0.038	0.094	0.156
Destruction (Continuing)	189	0.105	0.043	0.316	0.029	0.047	0.099	0.173
Destruction (All)	189	0.103	0.042	0.310	0.029	0.047	0.098	0.170
			Mexico					
Creation (Continuing)	63	0.126	0.041	0.254	0.064	0.076	0.124	0.201
Creation (All)	63	0.174	0.055	0.310	0.098	0.105	0.174	0.296
Destruction (Continuing)	63	0.078	0.029	0.171	0.035	0.045	0.069	0.134
Destruction (All)	63	0.105	0.041	0.232	0.047	0.058	0.094	0.185
			Main Countries					
Creation (Continuing)	484	0.083	0.038	0.254	0.006	0.028	0.078	0.152
Creation (All)	484	0.123	0.053	0.310	0.010	0.040	0.116	0.215
Destruction (Continuing)	484	0.092	0.043	0.316	0.005	0.033	0.086	0.166
Destruction (All)	484	0.118	0.055	0.370	0.005	0.046	0.111	0.215
			All Countries					
Creation (Continuing)	646	0.075	0.038	0.254	0.006	0.025	0.071	0.145
Destruction (Continuing)	646	0.091	0.041	0.316	0.005	0.035	0.085	0.162

Table A.2. Description of the main variables used in the paper.

Variable	Source	Description
Creation	from Haltiwanger et al (2004)	Job creation by firms in a given sector, country and year; see equation (3).
Destruction	from Haltiwanger et al (2004)	Job destruction by firms in a given sector, country and year; see equation (4).
SS	own construction, based on Gallego and Jones (2005)	Fraction of the year that the country is in a sudden stop.
Fin	from Raddayz (2006)	Computation of the original Rajan and Zingales (1998) measure of (long-run) external finance dependence. Unlike our previous two measures, this corresponds to the median firm for the 2-digit sector, and not to the mean of the median firm of each subsector.
Fin1	own construction based on RZ (1998) data	Mean across subsectors of the original Rajan-Zingales measure of financial dependence.
Fin2	own construction based on Micco and Pages (2006) data	Mean across subsectors of the Micco and Pages (2006) computation of the Rajan-Zingales measure of financial dependence.
I/S	from Raddatz (2006)	Median ratio of inventories to sales in 1980-1989 in the US, using Compustat data.
CCC	from Raddatz (2006)	Median across firms of the cash conversion cycle variable. It estimates the length in days between a firm pays for its raw materials and it receives the payment for the final sales. We express this variable in hundreds of days.
Labor	own construction using data from La Porta et al (2004)	We consider the sum of *firing* and *dismiss*.
firing	from La Porta et al (2004)	It measures how expensive it is for a firm to fire 20% of the workers; it includes all the compensations and penalties needed to pay in this case.
dismiss	from La Porta et al (2004)	It counts the number of measures a firm must undertake in order to be able to dismiss a worker; the variable used is the ratio of procedures required as a fraction of the total number of procedures considered (seven).
Net	from Haltiwanger et al (2004)	Net employment growth by firms in a given sector, country and year, see equation (2)
RER	from IFS and local central banks	Effective real exchange rate, year average, 1995=1.

Note: The series *Inv/Sales*, *CCC* and *Fin* were generously provided by Claudio Raddatz.

Table A.3. Dataset Characteristics by Country

Country	Argentina	Brazil	Chile	Colombia	Mexico	Uruguay
Type data	Job	Job + Workers	Job	Job	Job + Workers	Job
Source	INDEC	RAI	ENIA	EAM DANE	IMSS	INE
Period	91-01	92-00	80-99	77-91 and 93-99	94-00	89-95
Coverage	Manuf	Private (Formal)	Manuf	Manuf	Private	Manuf
Unit	Firms	Plants	Plants	Plants	Firms	Plants