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Does Trade Credit Substitute for Bank Credit? Evidence from Firm-Level Data

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**Does Trade Credit Substitute for Bank Credit?
Evidence from Firm-Level Data**

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Abstract

The paper examines micro data on Italian manufacturing firms' inventory behavior to test the Meltzer (1960) hypothesis according to which firms substitute trade credit for bank credit during periods of monetary tightening. It finds that their inventory investment is constrained by the availability of trade credit. As for the magnitude of the substitution effect, however, this study finds that it is not sizable. This is in line with the micro theories of trade credit and the evidence on actual firm practices, according to which credit terms display modest variations over time.

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I. Introduction

Monetary policy affects the real economy by reducing the financial resources available to firms. In particular, during a monetary contraction, financially constrained firms cut back on their inventory holdings. The impact of a monetary restriction, however, may be softened by the availability of trade credit (TC). Financially constrained firms may raise working capital by taking on more TC (that is, further delaying payment of their bills) or extending less TC (that is, reducing the delays in payment they allow to their customers). Thus, the net impact of a monetary restriction depends on the extent to which the use of TC offsets banks' and other financial institutions' financing.

Italy represents an ideal setting for testing the TC-bank credit substitution hypothesis. First, Italian firms display very high levels of TC in their balance sheets, either as receivables or payables. The ratios of trade receivables and trade payables to assets amount, respectively, to more than 35 and 25 percent. While the volume of TC exceeds by far the volume of short-term bank credit in virtually all the developing and industrialized countries (Demirgüc-Kunt and Maksimovic (2001), Marotta (1992), Omiccioli (2002), and Rajan and Zingales (1995)), in Italy the amounts of TC both received and extended are the highest among them. Second, aside from TC, alternative sources of finance are mostly unavailable to both banks and firms: the development of the stock and bond markets is modest and the commercial paper market is nearly absent. Moreover, Italy's industrial structure is tilted toward small firms. To the extent that credit to small firms is more likely to be rationed by banks and other financial institutions, the role of a monetary policy tightening will be magnified.

This paper provides an empirical investigation of the TC-bank credit substitution hypothesis. It tests for a linkage between inventory investment and TC by estimating a standard inventory investment model augmented by measures of internal resources, which include both liquidity and TC. The data are taken from the 1982–99 Company Accounts Data Service (*Centrale dei Bilanci*) "accounting units" (*unità contabili*) files for manufacturing firms. There are three methodological advantages in using these data. First, with firm-level panel data, one can control for the many possible time-invariant determinants of TC (and inventory investment) that differ across firms.[2] Second, the panel is fairly extended over the time dimension: it covers 18 years, a period over which five monetary-restriction episodes took place. Thus, this study on the role of TC is not restricted to a single episode. Third, the data cover a major fraction of the aggregate economy. The sample accounts for almost one-half of aggregate manufacturing sales and valued added. Accordingly, this extent of coverage makes it possible to draw macroeconomic as well as microeconomic conclusions.

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[2] In their seminal paper, Kashyap, Stein, and Wilcox (1993) suggested that pursuing the empirical verification of the Meltzer (1960) hypothesis would have required firm-level data.

Turning to the study's findings, the results show that the TC-bank credit substitution hypothesis receives empirical support. The study finds that inventory investment of Italian firms is constrained by the availability of TC to them and that the magnitude of this effect more than doubles during monetary restrictions. Moreover, it shows that traditional measures of liquidity seem to be less important than TC during periods of monetary tightening. The study also tests some cross-sectional implications of the substitution hypotheses. We find that small firms and firms not paying dividends are more likely to substitute TC for bank credit during contractions. Moreover, firms with large shares of assets that cannot be used as collateral seem to be both liquidity– and TC– constrained during monetary tightening. As for the magnitude of the substitution effect, we find that it is not sizable. This is in line with the micro theories of TC and the evidence on actual firm practices, according to which credit terms display modest variations over time.

The remainder of the paper is structured as follows. Section II presents the relevant literature. Section III discusses some background macroeconomic facts for the Italian economy over the 1982–99 period. Section IV describes the sample and data. In Section V the baseline inventory specification is developed and estimated and then extensive robustness tests are performed. Section VI concludes.

II. Relevant Literature

The TC-bank credit substitution hypothesis comes from Meltzer (1960). He found that the conjecture by which credit rationing favors large firms was not established, since banks and financial institutions were not the only source of credit for small firms. He showed that during the mid–fifties money tightening in the U.S., firms with relatively large cash balances increased the average length of time for which trade credit was extended, thus favoring the firms against whom the credit restriction was said to discriminate. Meltzer's (1960) idea was soon followed by further empirical support: Brechling and Lipsey (1963), Jaffee and Modigliani (1969), Jaffee (1971) and Herbst (1974), Duca (1986) who observed in one way or another that credit constrained firms made larger use of TC when credit conditions were tighter. The substitution hypothesis, however, did not remain long undisputed. Oliner and Rudebush (1996) and Gertler and Gilchrist (1993) looked at the U.S. 1974-1991 period and concluded that there was no support for the Meltzer's idea. More recently, however, Nielsen (2002) looked at a longer period and argued that the use of TC as a substitute to bank credit was prominent in the U.S. for both small firms and large firms without access to open market credit. Similarly, Fisman and Love (2002) find cross-country evidence consistent with Meltzer's suggestion. As for the Italian case, Bianchi, Frasca e Micossi (1976) and Marotta (1997) provided some evidence according to which the substitution hypothesis seemed to work, even though the direction of redistribution was at odds with Meltzer's original idea. In particular, Bianchi, Frasca e Micossi (1976) found that during the 1970 and 1974 monetary contractions, the substitution effect operated in favor of larger firms. This was somewhat confirmed by Marotta (1997), who however failed to find any significant role for monetary policy proxies over the period 1982-1993[3].

The substitution hypothesis relies on TC being an alternative to bank credit as a source of finance. This idea however contrasts with the actual commercial practices that display small variations across time. In a thorough survey on the U.S. interfirm trade, Ng, Smith and Smith (1999) conclude that there is "wide variation across industries in credit terms, but little variation within industries; furthermore the data indicate that credit terms are stable over time. (…) Firms generally do not respond to fluctuations in market demand by adjusting trade-credit terms (…) and rarely alter credit terms in response to fluctuations in prevailing interest rates. Similarly, in a recent survey on Italian manufacturing firms, Cannari, Chiri, and Omiccioli (2002) confirm the high cross-section variability across industries and firms.

A limited time variability over time and high sector and firm heterogeneity would be what the micro theories of TC would predict. In different ways, these theories stress the role of time-invariant aspects of TC. Transaction cost theories (Ferris (1981)) suggest that TC reduce the transaction costs of paying bills. Financing advantage theories (Petersen and Rajan (1997)) emphasize that TC should be seen primarily as providing contractual solutions to information problems concerning product quality and buyer creditworthiness. In this vein, Smith (1987) suggests that delayed payment can facilitate exchange by allowing the buyer to verify product quality before paying. Schwartz and Whitcomb (1979), Emery (1987), Freixas (1993), Burkart and Ellingsen (2002) propose that the existence of TC is due to a monitoring advantage that suppliers have over banks. Finally, Frank and Maksimovic (1998) hint that the advantage for suppliers relies to salvaging value from existing assets.

The bottom line is that TC has both a finance and a transaction component. The former represents a source of financing alternative to bank credit, which may vary over time according to the conditions of the credit market. The latter serves to facilitate the exchange of goods and represents time-invariant aspects of TC. This twofold feature of TC might have complicated the empirical analysis of the substitution effect, which has been carried out so far by aggregate and semi-aggregate data that cannot get rid of sector– and firm– level heterogeneity. This is particularly relevant for the effects of monetary tightening, which are highly heterogeneous across industries and firms (Guiso *et al.* (2000) and Dedola and Lippi (2000))[4].

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[3] It should be also emphasized that these papers consider the effects of monetary contractions on credit aggregates and their composition, without dealing explicitly with a measure of firm's real activity. This could be a problem since TC and measures of real activity, like inventory investment are most likely being jointly determined.

[4] Moreover, as for the Italian case, the use of TC is correlated to the effectiveness of the judiciary, which is highly differentiated across areas (Carmignani (2002)). Again,

III. Background Macroeconomic Facts

A prerequisite for our test is a good indicator of the stance of monetary policy for the 1982-1999 period under scrutiny. As the U.S. case displays, there is no consensus on this topic and a whole host of different indicators have been proposed in the recent literature (Bernake and Mihov (1998); Christiano, Eichenbaum, Evans (2000)). The fact that we have yearly data, also complicates the identification of money tightening, since we need to characterize the "prevailing monetary stance" over the whole year. Fortunately, we can rely on the chronology of stylized facts proposed by Gaiotti and Generale (2001), which provides a yearly classification of both monetary and business cycle phases. This chronology is based on the narrative accounts of the main monetary restrictions in Italy (Caranza and Fazio (1983); Angeloni e Gaiotti (1990); Gaiotti (1999)) while for the real side it relies, in turn, on the Altissimo, Marchetti, and Onado (2000) analysis of the Italian business cycle.

According to the Gaiotti and Generale (2001) chronology (see Figure 1), over the 1982-99 period there are five years in which the prevailing monetary condition can be defined as restrictive: 1986, 1987, 1989, 1992, and 1995 (as well as three years in which the business cycle phase can be classified as recessive: 1992, 1993, 1996). In particular, there were two major episodes: i) the severe 1992 tightening, which was intended to countering the turbulence in the European Monetary System and, after the devaluation of the lira in September, the threat of imported inflation; ii) the 1995 contraction, which was implemented to react to inflationary pressures and depreciating exchange rate. It should be noted that, while there is no discussion about the prevailing monetary stance for the two major episodes of the nineties, some doubts remain with reference to the 1989 and, to a lesser extent, to the 1986-87 episodes. In particular, Gaiotti and Generale (2001) argue that the "instances of monetary tightening (…) in 1989 (…) are (…) less straightforward to interpret, often representing only a temporary reaction to the dynamics of the exchange rate or money growth"; in a similar vein, they argue that "the 1986-1987 restriction was aimed at countering tensions in the foreign exchange market. Despite a temporary tightening of liquidity conditions and a rise in very short-term rates, however, bank lending rates continued to come down through most of the 1987, reflecting the fall in inflation, the continued effect of the lifting of credit controls in the early 1980s and the increased competition in he banking market"[5].

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asymmetric territorial effects of monetary restrictions would confuse the identification of the substitution effect in aggregate data.

[5] This is why the robustness of our empirical results will be checked also relatively to the inclusion of the 1989 and 1986-87 as a monetary contraction years.

IV. DATA

We use annual balance sheet data at the firm level for the 1982-1999 Company Accounts Data Service (*Centrale dei Bilanci*) "accounting units" (unità contabili) files. Data are collected from a consortium of banks; the sample is not randomly drawn since firms enter only by borrowing from one of the banks in the consortium. The account units refer to firms whose balance sheets have been adjusted by the Company Accounts Data Service to consider all the M&A operations that occurred during the sample period. A new firm acquired by (born trough a breaking up from) a firm already in the sample is also aggregated to the acquiring (divesting) firm for the years before (after) the incorporation. Moreover, balance sheets are reclassified to ensure cross-section comparability (Centrale dei Bilanci (2001)).

There are three main advantages in using these data. First, with firm-level panel data, we can control for the many possible time-invariant determinants of TC and inventory investment that differ across firms. Second, the long time extension of the panel allows us to study the role of TC over five major contraction episodes. Third, the data cover a major fraction of the aggregate economy (almost an half of aggregate manufacturing sales and valued added). Accordingly, this extent of coverage enables us to draw conclusions relevant to macroeconomics as well as microeconomics, notwithstanding the fixed effects model would allow only inference with respect to the firms in the sample (that is, inference conditional on the particular set of realizations of the individual effects).

There are however also a couple of drawbacks with these data. First, as discussed by Guiso *et al.* (2000) the focus on the level of borrowing in the Company Accounts Data Service skews the sample toward larger firms. Second, the accounting units file could be affected by a survivorship bias since the firms in the sample have been continuously in activity for 17 year. All in all, the fact that our sample could be skewed towards larger and older firms might indicate that our results could provide a conservative estimate of the substitution effect, since typically larger and older firms are less likely to be credit-rationed[6].

From the original files, which include 5.249 firms belonging to the private sector, we selected a sub-sample of 3.877 manufacturing firms for a total of (3.877*17)=65.909 observations. This sub-sample covers all the 21 two-digit NACE-CLIO 1981 manufacturing sectors. The estimation sample is constructed as follows. First, the two initial years were lost by lagging the variables and constructing first differences. Next, we deleted firm-year data with missing or negative (when implausible) numbers for our crucial variables (inventories, sales, measures of liquidity, and TC). Finally, to correct for outliers we excluded firm-year data that fell in the first and 99th percentile of the relevant variables. Thus, our estimation

[6] An additional note of caution is also warranted. The fact that our test is based on annual data could not be without consequences. Inventory investment is an high-frequency phenomenon and thus important cyclical variation could be missed with yearly data. This implies that our estimates should be taken only as suggestive and indicative.

sample is made up of 3.862 firms with an average time observations per firm of 14.5 and a total number of observations equal to 55.832. Table 1 provides summary statistics.

V. Results

Our test is based on inventory accumulation. Its rationale goes as follows. In response to a monetary contraction, inventories of bank-dependent firms with insufficient internal resources – that is, liquidity and TC – are expected to fall more sharply than the inventories of bank-dependent firms with plenty of internal resources.

The role of inventories during money tightness cannot be overemphasized (see Hubbard (1998)). While all the components of investment should be affected by monetary tightening, one would expect relatively liquid assets with low adjustment costs, such as inventories, to bear the brunt of the adjustment. In particular, unlike largely irreversible investments in R&D and fixed capital, inventories constitute a relatively flexible part of firms' assets, providing potential liquidity to offset shocks to external finance. The idea of testing financial constrains in an inventory accumulation framework follows the original intuition of Kashyap, Lamont, and Stein (1994) and Carpenter, Fazzari and Petersen (1994). It has also been recently applied by Bagliano and Sembenelli (2001) to study financial factors in the recession of the early '90s in France, Italy, and the United Kingdom.

To identify the role of TC we proceed as follows.

We start from the Kashyap, Lamont, and Stein (1994) (hereinafter KLS) model of inventory accumulation, which is based on the idea that what really matters during money tightening for bank-dependent firms is the amount of liquidity (LIQ), internal cash reserves, firms have. That is, the KLS model assumes implicitly that TC cannot be used to substitute bank credit.

To test if TC can be assimilated to LIQ as a source of internal finance, we augment the KLS model with a measure of net TC, defined as the difference between trade receivables and trade payables. In our specification, both LIQ and TC are also interacted with a dummy variable MPR, which takes on the value one in the "monetary policy restriction" years. The intuition is that the LIQ and TC coefficient will capture the "average" degree of liquidity and TC constraints, while LIQ*MPR and TC*MPR will capture the added effects seen in a tight money year. Our focus will be on the interaction terms.

This specification allows us a straightforward assessment of the substitution effect. We distinguish four cases:

i) LIQ*MPR and TC*MPR do not enter with a positive coefficient: *no evidence of bank credit – rationing, at least in the KLS meaning.*

ii) LIQ*MPR takes on a positive sign while TC*MPR does not: *evidence contrary to the substitution hypothesis, since bank-dependent firms appear LIQ-rationed but not TC-rationed.*

iii) LIQ*MPR and TC*MPR both enter with positive coefficients: *evidence in favor of the substitution hypothesis.*

iv) TC*MPR takes on a positive value while LIQ*MPR does not: *strong evidence in favor of the substitution hypothesis, since what seems to count are only internal resources in form of TC*[7].

The KLS model augmented with TC provides our baseline specification. It can be represented as follows.

$$\Delta Log(INV_{i,t}) = \alpha_1 Log(INV_{i,t-1}/SALES_{i,t-1}) + \alpha_2\, \Delta Log(SALES_{i,t}) + \alpha_3 \Delta Log(SALES_{i,t-1}) + \\ + \alpha_4 LIQ_{i,t-1} + \alpha_5 LIQ_{i,t-1} * MPR + \alpha_6 TC_{i,t-1} + \alpha_7 TC_{i,t-1} * MPR + u_{i,t} \quad (1)$$

where the dependent variable is the first difference of (the log of) the end of period stock of real inventories[8]. The right-hand-side variables include, the log of the inventory-sales ratio at the beginning of the year, the change in the log of firm sales over both the current and the preceding year, our financial variables LIQ (firm's ratio of cash and marketable securities to total assets at the beginning of the period) and TC (firm's ratio of net TC to total assets at the beginning of the period[9]) as well as their interaction with MPR, and a idiosyncratic, serially uncorrected component. The relationship between inventories and sales intends to control for the nonfinancial determinants of inventories; its formalization in eq. (1) comes from the KLS model.

Baseline results are shown in Table 2. In order to control for the many possible time-invariant determinants of TC and inventory investment that differ across firms, we use a fixed-effects specification where time dummies have also been included. This is also the preferred specification, as suggested by the Hausman (1978) test.

We start by estimating the original KLS model, where only LIQ appears on the financial side. Column (2.1) displays results. Quite surprisingly LIQ*MPR does not enter significantly and neither it does its average effect. From the KLS model, one would conclude that liquidity constraints do not play any role for inventory accumulation. Column (2.2) shows the results from a KLS model in which LIQ and LIQ*MPR have been replaced by TC and TC*MPR. As can be seen, the TC variables are strongly significant. The average estimated TC effect equals 0.025. Crucially, in periods of duress this estimate increases of an additional 70%, bringing the total effect to 0.067.

[7] The coefficients for LIQ and TC might also take positive values during easy money periods. That is, even when bank lending is relatively unconstrained by monetary policy it may not be a perfect substitute for internal finance.

[8] Throughout the paper nominal variables are deflated with manufacturing producer prices.

[9] Because of trade relationships between firms in the same control group might reflect within-group finance redistribution needs, we exclude them in the definition of TC. However, within-group TC will be considered later.

Column (2.3) provides the results from the benchmark represented by the eq. (1). In this case, LIQ and TC enter with similar point estimates, both as time-average effects and tight money additions. However, the statistical significance of TC appears much higher than that of LIQ (the p-values for LIQ and LIQ*MPR are respectively 0.082 and 0.120; those for TC and TC*MPR are lower than 0.01).

Column (2.4) presents the results from estimating eq. (1) with a complete set of disaggregated industry time dummies. That is, a separate dummy for each possible year in each of the 21 two-digit NACE-CLIO 1981 industrial sectors. The disaggregated time dummies control for a wide range of alternative hypothesis that would be observationally-equivalent in tests based on aggregate time-series data. For example, cost or technological shocks might drive both internal finance and inventory investment. By including industry time dummies to control for these shocks, however, their influence can be disentangled from other variables[10]. As can be seen in column (2.4), the inclusion of these dummies strengthens our findings: TC and TC*MPR remain highly significant with only minor reductions in their point estimates, while both LIQ and LIQ*MPR do not differ significantly from zero.

The results above make a point in favor of the Meltzer (1960) hypothesis. In particular, in terms of the four cases discussed above the evidence in favor of the substitution hypothesis seems to take its strongest form. However, while the TC and TC*MPR coefficients are statistically significant, their magnitude is not that big. For comparison, in the KLS model, the estimated coefficient for the LIQ variable is equal to 0.41. In our estimate of column (2.3) if we add the LIQ and TC coefficients (as well as their interactions with MPR, when they enter significantly) our total effect for internal resources barely reaches 0.12. For the purpose of a back-of-the-envelop calculation it should be observed that during monetary tightening, the median firm in our sample increases its real inventories by 5.2% while the median value of the TC variable is 9.3%. Taking the (2.3), this means that if we start with a typical firm that is increasing its inventories by 5.1% and then double its TC, we get an additional boost in inventories of only 0.7%, admittedly not a sizeable effect[11].

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[10] As explained in Carpenter, Fazzari, and Petersen (1994), the inclusion of disaggregated industry time dummies is not without consequences. The dummies control for all time-varying effects at the industry level or at higher level of aggregation. But they also remove the common cyclical components of inventories, sales, and financial variables for any two-digit industry. Therefore, the estimates of eq. (1) with the inclusion of disaggregated time dummies should be interpreted as an extreme test of the substitution hypothesis, when idiosyncratic firm variation alone, independent of cyclical industry movements, is used to estimate the coefficients. The robustness of all the estimates presented in the remaining of the paper has also been checked relatively to the inclusion of disaggregated industry time dummies. The differences with the results shown in the text were always very limited.

[11] In our sample, the ratio of short-term bank debt over inventories for the median firm is equal to 0.58 and 0.54, respectively for easy and tight money periods. The ratio of TC over inventories remains approximately constant over the two periods around 0.46.

The fact that the quantitative importance of the substitution effect is not that great, is by no means surprising. As argued above, TC has a twofold role that encompasses both a (time-varying) financial motive, represented by financial resources that can be used to offset the effects of a monetary restriction, and a (time-invariant) transaction component, which derives from the fact that TC provides contractual solutions to minimize transaction costs and to overcome information problems concerning product quality and buyer creditworthiness. The larger the transaction enhancing role the smaller the amount of TC left to cushion against a money tightening.

We then turn to analyze the robustness of our estimates. We check the robustness with respect to 5 main issues:

1. Endogeneity of financial variables.
2. Specification of the inventory-sales relationship.
3. Potential pitfalls of the monetary policy chronology.
4. Appropriateness of the balance sheet variables.
5. Sensitivity to more restrictive definition of bank-dependent firms.

Endogeneity of financial variables. LIQ and TC might be endogenous and might be proxying for other factors that could affect inventory behavior. For example, it could be that LIQ and TC are proxies for innovations in firm profitability. Also, firms might be using TC as a price discrimination tool to buy market shares. That is, firms that have a high value of LIQ and TC are, for some reasons, devoting more resources to inventory investment, regardless of whether or not they are LIQ– or TC– constrained. It could also be, for example, that firms anticipate the money tightening and attempt to offset it by stockpiling LIQ or TC[12]. To mitigate any endogeneity bias, we use firm's lagged values of LIQ and TC as instruments respectively for LIQ and TC.

IV estimates are shown in the first three columns of Table 3.

We start (3.1) by instrumenting only LIQ (and LIQ*MPR) in the (2.3) model. The LIQ coefficient increases to 0.106 and becomes significant at the 5% level. Its interaction with the MPR dummy, remains however not statistically different from zero. TC variables are only marginally affected.

We then (3.2) instrument only for TC (and TC*MPR). The results are very similar to (2.3), with all coefficients for the financial variable increasing slightly.

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[12] In the first two examples there would be an upward bias in our estimates. In the third case, the bias would be downward.

Finally, we proceed (3.3) by treating both LIQ and TC variables as endogenous. In this case, again the LIQ*MPR coefficient does not enter significantly. Taken at face value, the specification (3.3) would suggest that the effect of LIQ is on average stronger than that of TC (0.111 versus 0.048). However, during money tightening years the TC effect more than doubles, while there is no additional effect for LIQ[13].

IV estimates suggest that treating LIQ and TC as exogenous variables could moderately bias downwards their estimated effects. They also underscore that the evidence on the substitution effect remain confirmed. The time-average effects seem to fall in case iii), while case iv) still appears as a good characterization of monetary restrictions.

Specification of the inventory-sales relationship. We analyze now the sensitivity of our results to alternative specification of the inventory-sales relationship. Inventory accumulation modeling choices could be critical for our results. While in principle one would like to control for all the fundamental firm-level determinants of inventories, there is no unique way to do so since there is no agreement on which model of inventories is preferable among the many proposed. Rather than try to come up with our 'right' specification, we follow the KLS sensitivity strategy and try a variety of alternative specifications of the inventory-sales model within the KLS model. Subsequently, we leave the KLS model altogether and try a specification in levels, which derives from a much more structured model of inventory investment. The aim is to show that the evidence on the role of TC during monetary contraction is relatively insensitive to how inventory accumulation is modeled.

We start from (3.4), in which we use the change in the log of the inventory-to-sales ratio as dependent variable and exclude all the sales term from the right-hand side of the equation. This amounts to constrain the coefficient for the contemporaneous change in sales to unity. This specification, which can be deemed as a poor representation of the real aspect of inventory accumulation, has the effect of magnifying the average LIQ and TC coefficients. The TC*MPR coefficient remains however roughly the same at 0.044 while the LIQ*MPR coefficient continues to be not significantly different from zero.

Next, (3.5) we instrument for the contemporaneous change in sales with the lagged change in inventories. This is supposed to matter if anticipated changes in sales influenced inventories differently from unanticipated changes. While this modification makes the average effect of LIQ and TC disappeared, there is only a minor impact on the LIQ*MPR and TC*MPR coefficients.

[13] We also tried instrumenting with twice-lagged LIQ and TC. Estimates differed only marginally.

Then, in (3.6) we add the lagged change in the log of inventories to the right-hand side. While this variation increases a little bit the LIQ*MPR coefficient to 0.05, it has no effect whatsoever on the TC*MPR coefficient[14].

Finally, we move from a specification in differences to a specification in levels (3.7). This replicates the exercise proposed for example by Carpenter, Fazzari, and Petersen (1994) and Bagliano and Sembenelli (2001). In this specification (the log of) real inventories is explained by its lagged value and contemporaneous and lagged values of (the log of) real sales. In contrast with the relatively unstructured KLS model, this specification has more structure since it assumes, on the one hand, that the desired level of inventories is a linear function of the level of sales expected at the beginning of the period and, on the other, that expected sales follow a first-order process[15]. What is relevant for our purposes, is that the specification in levels produces only minor modification to our results[16].

Our results seem not to rely on the way the inventory-sales relationship is modeled. The TC*MPR coefficient always enters significantly with a modest magnitude. On the contrary, the estimate for the LIQ*MPR coefficient is less robust, since it depends on the way one specifies the inventory accumulation process. In terms of the four cases above, our results keep oscillating between cases iii) and iv).

Potential pitfalls of the monetary policy chronology. The sensitivity of our results should be checked with respect to two issues that come from the Gaiotti and Generale (2001) chronology.

[14] We also added the log of real assets to the right-hand side (results not shown). This modification had a minimal impact on the TC coefficients. On the contrary, it made statistically insignificant, in addition to LIQ*MPR, also the average LIQ term. The sensitivity of our results to firm size will be investigated below.

[15] This specification derives from the Blinder and Maccini (1991) model, in which the change in the (log of) real inventories is explained in terms of the adjustment of the actual inventory stock to a target level and a term representing the buffer stock role of inventories. Then, the assumptions that - the desired level of inventories is a linear function of expected sales, and that – expected sales follow a first-order process, will deliver the model estimated in the text.

[16] Specification (3.6) and (3.7) contain a lagged dependent variable on the right-hand-side. In these cases, there could be a bias in the within estimator (Bond et al., (1997)) and a GMM estimator would be preferable. However, the inconsistency of the within estimator decreases as the number of periods becomes large, while depending on the instruments, GMM could lead to very large standard errors for the estimated coefficient. Hall, Mulkay and Mairesse (2000), for example, prefer the within estimator for a panel with 12 annual observation for each firm, which they deem as a time span long enough to minimize the inconsistency problem. In any case, we also estimate specifications (3.6) and (3.7) by GMM, with no major implications for our results.

First, while there is no discussion about the prevailing monetary stance in 1992 and 1995, some doubts remain with reference to 1989 and, to a lesser extent, 1986-87. Our concern here can be expressed as follows. Since it is debatable that these periods can be defined as money tightening years, we want to be sure that their potential erroneous inclusion into the tight money year sample does not affect the results. The first column of Table 4 shows the result when we drop the 1989 in the definition of MPR. In the second column we drop 1986, 1987 and 1989 altogether. The results are only marginally influenced.

Second, over the 1982-1999 period, two years are classified as recession years. In particular, the 1992 represents a year characterized by both a monetary contraction and a recession, while the 1996 is classified as a recession year with loose prevailing monetary conditions. Disentangling the effects of a monetary restriction from those deriving from real shocks is not an easy task, and it goes far beyond the scope of this paper. It could be however interesting to verify to what extent our results are driven by recession years. In this vein, we perform two experiments. Column (4.3) presents the estimate that leaves the 1992 out of the definition of the MPR dummy. That is, in this case MPR takes on the value of 1 only for the years: 1986, 1987, 1989, 1995. The results confirm the role of TC and its economic significance. As for LIQ, however, we find now a larger (close to 7%) and statistically significant coefficient for LIQ*MPR, suggesting that the "credit crunch/recession" year 1992 has much to do with the scant evidence on the LIQ effect. Next, we add (4.4) two more interaction terms: LIQ*REC and TC*REC, where the REC variable is now a dummy that takes on the value one in the 1992, 1993 and 1996. We find that the two interaction terms do not enter significantly and do not affect our results.

The evidence on the substitution effect does not seem to depend either on the potential erroneous classification of the 1989 and 1986-87 episodes or the effects of the 1992, 1993, and 1996 recessions.

Appropriateness of the balance sheet variables. We next try to evaluate the robustness of our estimates with respect to the balance sheet variables so far used.

First, TC has been defined so far as trade receivables less trade payables (over assets). One could wonder which one of the two components is more accountable for the substituting role of TC. This issue has received a lot of attention in the literature. For example, Nilsen (2002) argues that what really matters are payables, while Marotta (1997) takes the view that both payables and receivables should be accounted for the substitution hypothesis. In column (4.5) we replicate (2.3) splitting the TC variable in its two components TR and TP (and, similarly, we define two interaction terms TR*MPR and TP*MPR). The evidence suggests that while the average degree of TC constraints is mainly explained in terms of payables, during monetary restrictions firms are rationed by the availability of receivables. This would suggests that in periods of duress they move predominantly receivables; that is, reducing payment delays they allow to their customers would be an easier option than delaying payments for their bills.

Second, our measure of TC did not take into account so far TC relationships with firms belonging to the same control group. We left them out because trade relationships between firms in the same control group might reflect within-group finance redistribution needs, and might mask the underling TC determinants. We now consider also within-group TC and include it into our TC measure (4.6). We find that this inclusion has minor implications for our estimates.

Third, our proxy for LIQ so far has been the stock of cash and marketable securities, as in the KLS model. Measures of cash flow, defined as net profits plus depreciation, are alternatively used in the literature. We need to be sure that our results are not due to the specific proxy LIQ chosen. In column (4.7) we replicate specification (2.3) replacing LIQ with CF[17]. The results are invariant[18].

Our results seem robust to alternative definitions of the key balance sheet variables.

Sensitivity to more restrictive definition for bank-dependent firms. As a final test we verify some cross-sectional implications. In particular, we want to check what happens to our results when we consider only sub-samples of firms that are likely to be characterized by higher financial constraints. Following Gaiotti and Generale (2001), we consider the following three groups.

First, we take only small firms. Small firms are traditionally considered more subject to financial constraints, since they have weaker and more opaque balance sheet. They should be more affected by a monetary tightening. In column (5.1) we reply the regression (2.3) only for small firms (42,977 obs., 3,000 groups) defined by the Gaiotti and Generale (2001) threshold, that is firms with fewer than 200 employees. The TC*MPR coefficient increases from 0.046 to 0.057. Admittedly, however, this size threshold is somewhat too large for our sample, in which the 75 percentile in the distribution of employees equals to 184. Thus, we restrict the threshold to 90 employees (5.2), which represent the median value in the sample (26,968 obs., 1,913 groups). In this case however the increase for the TR*MPR coefficient is more limited. While surprising, this finding is consistent with the evidence provided by Angeloni *et al.* (1995) and Ferri and Pittaluga (1997), according to which because of thither bank-firm relationships, small firms are shielded from a monetary contraction by their banks.

Second, we consider the group of firms with a high proportion of intangible assets. Intangible assets include R&D expenditures, patents, development and advertising costs and

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[17] In this regression CF is defined as net profits plus the difference between gross and net margins.

[18] We present here only a sub-sample of the sensitivity checks performed. Robustness has also been checked with reference to the type of inventories (raw materials and finished goods), the accounting method for inventories (FIFO and LIFO), a more extensive definition of LIQ that includes cash and all short-term assets, a more narrow definition of CF that includes net profits plus fixed asset depreciation.

the like. The idea is that firms in this group are more subject to financial constraints, since intangible assets are relatively difficult to evaluate for an outside lender and cannot be used as collateral. To define this group we take the same definition proposed by Gaiotti and Generale (2001). Accordingly, we include only firms whose ratio of intangible assets to total assets is higher than 75% of the distribution in at least one year. For this group (39.274 obs, 2,711 groups) we find (5.3) a slightly smaller TC*MPR coefficient (0.040) along with a much higher LIQ*MPR coefficient (0.097). This type of firms appears severely liquidity-constrained.

Third, we take only firms paying dividends. Again we take the Gaiotti and Generale (2001) very conservative definition of firms with non-negative payout for the whole sample period. This would minimize the scope for the endogenous dividend reshuffling across years. Given the majority of unlisted firms in the Italian case, this group is intended to capture firms with excess cash. Restricting the sample to firms paying dividends over the whole sample period leaves us with only 784 observations (49 groups). The results (5.4) would indicate that financial variables are jointly not significant. These results are confirmed when we relax somewhat the criteria to include firms that distribute positive dividends for a number of years lower than the whole sample period. For example, when we consider firms that distribute dividends for 12 out of 16 periods (5.5), we still find that both TC*MPR and LIQ*MPR do not enter significantly (with the only statistically significant variable being TC).

Finally, as underscored by many (see, for example, Hubbard (1998)) it is doubtful that a single proxy for higher financial constraints will do the job of identifying those firms more likely to face capital-market imperfections. More appropriately, firms facing higher financial constraints are those who score high according to a host of financial constraint indicators. In this vein, in column (5.6) we restrict the sample to small (with less than 200 employees) firms with an high ratio of intangible assets who did not distribute positive dividends continuously for the whole sample period. Results show that the TC*MPR coefficient increases marginally, while the LIQ*MPR coefficient rises substantially.

While the evidence presented suggests that main cross-sectional implications appear to receive empirical support, the fact that our sample is likely to be skewed towards larger and older firms makes our data not the best place where such implications could be verified.

VI. Conclusions

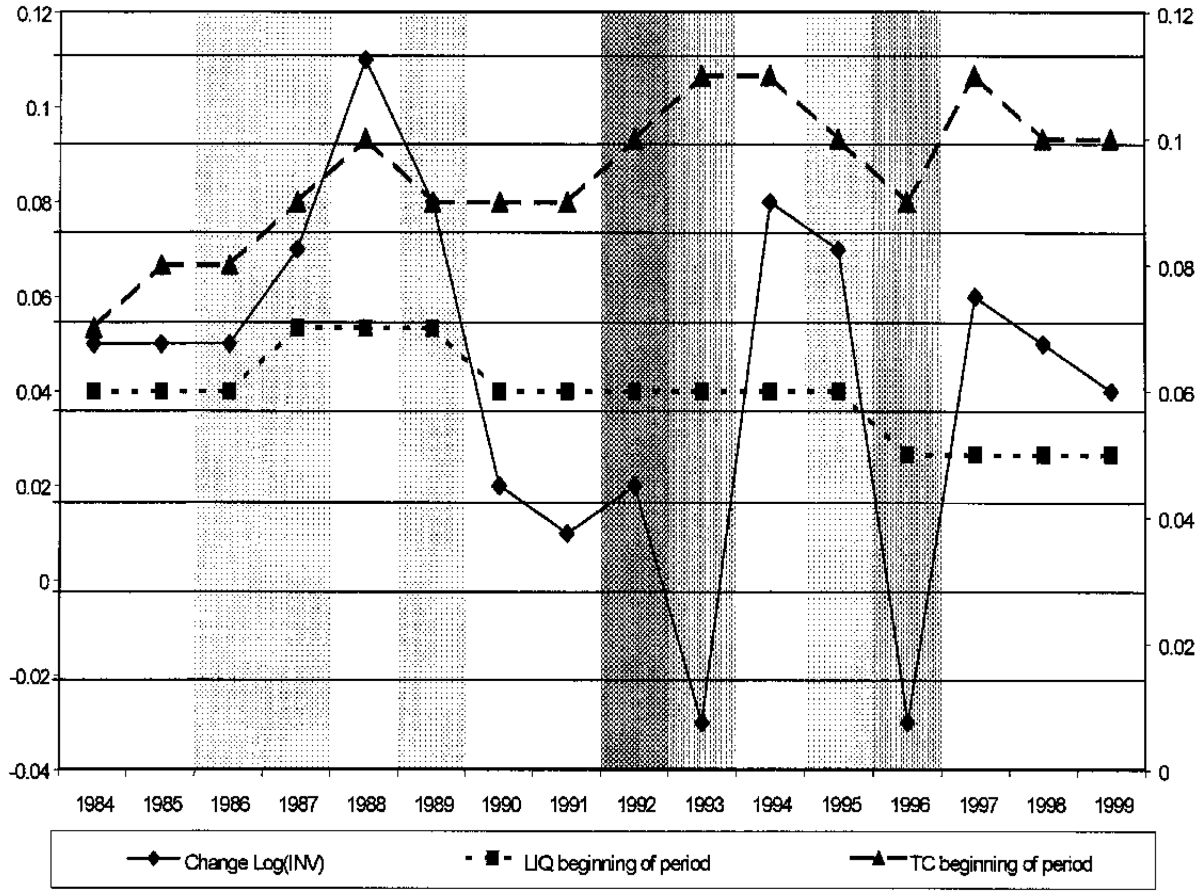
The existence of a TC-bank credit substitution effect has been under scrutiny for more than four decades. This paper presents new evidence that, on the one hand, confirms Meltzer's (1960) intuition, according to which firms substitute trade credit for bank credit during money tightening, and, on the other hand, shows that the magnitude of the substitution effect is quite modest – that is, that the substitution of trade credit for bank credit is unlikely to be very relevant in practice.[19]

Our empirical investigation is based on micro panel data of Italian manufacturing firms covering an extended period. The importance of our conclusions relies on two facts. First, Italy represents an ideal setting for testing the substitution hypothesis, because Italian firms display very high levels of TC in their balance sheets; alternative sources of finance are mostly unavailable; and the country's industrial structure is tilted toward small firms. Second, the structure of our data allows one to control for the many possible time-invariant determinants of TC (and inventory investment) that differ across firms and to study the role of TC over five monetary-restriction episodes.

Generally speaking, the study's results would indicate that TC in Italy should be mainly explained in terms of time-invariant factors. The limited variability over time (and the high sector and firm heterogeneity) would suggest that the high levels of TC in Italy should be seen primarily as consequences of information problems concerning product quality and buyer creditworthiness and as ways of minimizing transaction costs. It is also plausible that credit protection and other institutional factors might play a role.

[19] This conclusion has also implications for the debate on the potential asymmetric effects of the single monetary policy in the euro area.

Figure 1. Gaiotti and Generale (2001): Chronology and Selected Variables.



Restrictive Monetary Policy

Recession and Restrictive Monetary Policy

Recession

Table 1. Summary Statistics

	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999
Δ Log(INV)																
mean	0.05	0.05	0.05	0.07	0.11	0.08	0.02	0.01	0.02	-0.03	0.08	0.07	-0.03	0.06	0.05	0.04
standard deviation	*0.28*	*0.28*	*0.28*	*0.27*	*0.26*	*0.26*	*0.25*	*0.25*	*0.24*	*0.26*	*0.25*	*0.26*	*0.25*	*0.24*	*0.24*	*0.23*
Log(INV/SALES) beginning of period																
mean	-1.86	-1.90	-1.91	-1.94	-1.94	-1.93	-1.91	-1.90	-1.90	-1.90	-1.92	-1.94	-1.97	-1.97	-1.94	-1.9
standard deviation	*0.71*	*0.70*	*0.71*	*0.71*	*0.69*	*0.68*	*0.69*	*0.70*	*0.72*	*0.73*	*0.72*	*0.72*	*0.73*	*0.75*	*0.75*	*0.76*
Δ Log(SALES)																
mean	0.08	0.06	0.08	0.07	0.10	0.07	0.02	0.01	0.02	0.01	0.09	0.10	-0.02	0.02	0.03	0.00
standard deviation	*0.15*	*0.15*	*0.15*	*0.14*	*0.14*	*0.13*	*0.14*	*0.15*	*0.14*	*0.15*	*0.15*	*0.15*	*0.14*	*0.13*	*0.13*	*0.14*
LIQ beginning of period																
mean	0.06	0.06	0.06	0.07	0.07	0.07	0.06	0.06	0.06	0.06	0.06	0.06	0.05	0.05	0.05	0.05
standard deviation	*0.07*	*0.08*	*0.08*	*0.08*	*0.08*	*0.08*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*
TC beginning of period																
mean	0.07	0.08	0.08	0.09	0.10	0.09	0.09	0.09	0.10	0.11	0.11	0.10	0.09	0.11	0.10	0.10
standard deviation	*0.16*	*0.16*	*0.16*	*0.16*	*0.16*	*0.16*	*0.16*	*0.15*	*0.15*	*0.15*	*0.15*	*0.15*	*0.15*	*0.15*	*0.15*	*0.15*
MPR	0	0	1	1	0	1	0	0	1	0	0	1	0	0	0	0
REC	0	0	0	0	0	0	0	0	1	1	0	0	1	0	0	0
N. Obs.	*3,276*	*3,383*	*3,441*	*3,444*	*3,465*	*3,488*	*3,525*	*3,531*	*3,557*	*3,501*	*3,495*	*3,532*	*3,528*	*3,541*	*3,576*	*3,549*

Notes: The sample is selected following the procedure described in the text. MPR=1 denotes a year in which, according to the Gaiotti and Generale (2001) chronology, a monetary tightening occurred. REC=1 denotes a year in which a recession occurred, according to the same source.

Table 2. Baseline Specifications
Dependent variable: Δ Log(INV)

	(2.1)	(2.2)	(2.3)	(2.4)
$Log(INV/SALES)_{t-1}$	-0.266***	-0.265***	-0.263***	-0.265***
	(0.003)	*(0.003)*	*(0.003)*	*(0.003)*
Δ Log(SALES)	0.201***	0.203***	0.203***	0.193***
	(0.008)	*(0.008)*	*(0.008)*	*(0.008)*
Δ $Log(SALES)_{t-1}$	-0.075***	-0.074***	-0.073***	-0.068***
	(0.007)	*(0.007)*	*(0.007)*	*(0.008)*
LIQ_{t-1}	0.024	-	0.039*	0.034
	(0.022)	-	*(0.022)*	*(0.022)*
LIQ_{t-1}*MPR	0.033	-	0.047	0.034
	(0.030)	-	*(0.030)*	*(0.030)*
TC_{t-1}	-	0.025**	0.031***	0.026**
	-	*(0.012)*	*(0.012)*	*(0.012)*
TC_{t-1}*MPR	-	0.042***	0.046***	0.043***
	-	*(0.014)*	*(0.015)*	*(0.015)*
N. Obs.	55,832	55,832	55,832	55,832
N. Groups	3,862	3,862	3,862	3.862
R-sq (within)	0.15	0.15	0.15	0.16
Hausman Test	7,514.37	7,618.22	7,528.10	6,866.60

Notes: *** (**) (*) denotes 1% (5%) (10%) significance level. Standard errors in parenthesis. Year-effects included in (2.1), (2.2), and (2.3). Year by industry-effects included in (2.4)

Table 3. Robustness Checks for Issues 1 and 2

	(3.1)	(3.2)	(3.3)	(3.4)	(3.5)	(3.6)	(3.7)
Dependent variable:	Δ Log(INV)	Δ Log(INV)	Δ Log(INV)	Δ Log(INV/SALES)	Δ Log(INV)	Δ Log(INV)	Log(INV)
$Log(INV/SALES)_{t-1}$	-0.261***	-0.262***	-0.260***	-	-0.332***	-0.256***	-
	(0.004)	*(0.003)*	*(0.004)*	-	*(0.081)*	*(0.003)*	-
Δ Log(SALES)	0.203***	0.203***	0.203***	-	-0.227***	0.208***	-
	(0.008)	*(0.008)*	*(0.008)*	-	*(0.006)*	*(0.008)*	-
Δ $Log(SALES)_{t-1}$	-0.073***	-0.073***	-0.072***	-	-0.064***	-0.069***	-
	(0.007)	*(0.008)*	*(0.008)*	-	*(0.008)*	*(0.008)*	-
Δ $Log(INV)_{t-1}$	-	-	-	-	-	-0.027***	-
	-	-	-	-	-	*(0.004)*	-
$Log(INV)_{t-1}$	-	-	-	-	-	-	0.725***
	-	-	-	-	-	-	*(0.003)*
Log(SALES)	-	-	-	-	-	-	0.156***
	-	-	-	-	-	-	*(0.008)*
$Log(SALES)_{t-1}$	-	-	-	-	-	-	0.017***
	-	-	-	-	-	-	*(0.008)*
LIQ_{t-1}	0.106**	0.044*	0.111**	0.564***	0.035	0.038*	0.045**
	(0.048)	*(0.024)*	*(0.046)*	*(0.026)*	*(0.023)*	*(0.022)*	*(0.022)*
LIQ_{t-1}*MPR	0.024	0.051*	0.028	0.002	0.043	0.050*	0.050*
	(0.042)	*(0.030)*	*(0.042)*	*(0.036)*	*(0.032)*	*(0.030)*	*(0.030)*
TC_{t-1}	0.039***	0.043*	0.048**	0.325***	-0.003	0.024**	0.026**
	(0.013)	*(0.022)*	*(0.022)*	*(0.014)*	*(0.013)*	*(0.012)*	*(0.012)*
TC_{t-1}*MPR	0.044***	0.059***	0.058***	0.044**	0.050***	0.046***	0.054**
	(0.015)	*(0.018)*	*(0.018)*	*(0.017)*	*(0.015)*	*(0.015)*	*(0.015)*
Instrumented:	LIQ_{t-1}	TC_{t-1}	LIQ_{t-1}, TC_{t-1}	-	Δ Log(SALES)	-	-
Instruments:	LIQ_{t-2}	TC_{t-2}	LIQ_{t-2}, TC_{t-2}	-	Δ $Log(INV)_{t-1}$	-	-
N. Obs.	55,832	55,832	55,832	55,832	55,832	55,832	55,832
N. Groups	3,862	3,862	3,862	3,862	3,862	3,862	3,862
R-sq (within)	0.15	0.15	0.15	0.03	0.07	0.15	0.70
Hausman Test	7,563.17	7,532.64	7,575.00	789.93	n.a.	5,695.96	n.a.

Notes: *** (**) (*) denotes 1% (5%) (10%) significance level. Standard errors in parenthesis. Year-effects included. The Hausman test is not available for (3.5) and (3.7) because the model failed to meet the asymptotic assumption of the test.

Table 4. Robustness Checks for Issues 3 and 4
Dependent variable: Δ Log(INV)

	(4.1)	(4.2)	(4.3)	(4.4)	(4.5)	(4.6)	(4.7)
$Log(INV/SALES)_{t-1}$	-0.263***	-0.263***	-0.263***	-0.263***	-0.271***	-0.263***	-0.262***
	(0.003)	*(0.003)*	*(0.003)*	*(0.003)*	*(0.003)*	*(0.003)*	*(0.003)*
Δ Log(SALES)	0.203***	0.203***	0.203***	0.203***	0.207***	0.203***	0.203***
	(0.008)	*(0.008)*	*(0.008)*	*(0.008)*	*(0.008)*	*(0.008)*	*(0.008)*
Δ $Log(SALES)_{t-1}$	-0.074***	-0.074***	-0.073***	-0.073***	-0.062***	-0.073***	-0.077***
	(0.007)	*(0.007)*	*(0.007)*	*(0.007)*	*(0.008)*	*(0.007)*	*(0.008)*
LIQ_{t-1}	0.041*	0.058***	0.035	0.042*	0.006	0.041*	-
	(0.022)	*(0.020)*	*(0.022)*	*(0.023)*	*(0.023)*	*(0.022)*	-
LIQ_{t-1}*MPR	0.050	-0.028	0.069**	0.047	0.054*	0.048	-
	(0.032)	*(0.043)*	*(0.032)*	*(0.030)*	*(0.030)*	*(0.030)*	-
TC_{t-1}	0.032***	0.042***	0.031***	0.033***	-	0.034**	0.025**
	(0.012)	*(0.011)*	*(0.012)*	*(0.012)*	-	*(0.012)*	*(0.012)*
TC_{t-1}*MPR	0.052***	0.038*	0.054***	0.046***	-	0.047***	0.041***
	(0.016)	*(0.021)*	*(0.016)*	*(0.015)*	-	*(0.015)*	*(0.014)*
LIQ_{t-1}*REC	-	-	-	*-0.022*	-	-	-
	-	-	-	*(0.037)*	-	-	-
TC_{t-1}*REC	-	-	-	*-0.013*	-	-	-
	-	-	-	*(0.018)*	-	-	-
TR_{t-1}	-	-	-	-	-0.040***	-	-
	-	-	-	-	*(0.014)*	-	-
TR_{t-1}*MPR	-	-	-	-	0.065***	-	-
	-	-	-	-	*(0.017)*	-	-
TP_{t-1}	-	-	-	-	-0.139***	-	-
	-	-	-	-	*(0.017)*	-	-
TP_{t-1}*MPR	-	-	-	-	-0.010	-	-
	-	-	-	-	*(0.019)*	-	-
CF_{t-1}	-	-	-	-	-	-	0.075***
	-	-	-	-	-	-	*(0.029)*
CF_{t-1}*MPR	-	-	-	-	-	-	*0.037*
	-	-	-	-	-	-	*(0.038)*
Action Taken:	1989 out of MPR	1989 and 1986-87 out of MPR	1992 out of MPR	Dummies for recession years included	TC splitted in TR and TP	TC includes within-group credits	CASH FLOW replaces LIQ
N. Obs.	55,832	55,832	55,832	55,832	55,832	55,832	55,832
N. Groups	3,862	3,862	3,862	3,862	3,862	3,862	3,862
R-sq (within)	0.15	0.15	0.15	0.15	0.15	0.15	0.15
Hausman Test	7,530.58	10,872.26	7,526.68	7,516.18	7,610.16	7,530.65	7,369.66

Notes: *** (**) (*) denotes 1% (5%) (10%) significance level. Standard errors in parenthesis. Year-effects included.

Table 5. Robustness Checks for Issue 5
Dependent variable: Δ Log(INV)

	(5.1)	(5.2)	(5.3)	(5.4)	(5.5)	(5.6)
$\text{Log(INV/SALES)}_{t-1}$	-0.263***	-0,261***	-0.259***	-0.327***	-0.294***	-0.256***
	(0.003)	*(0.05)*	*(0.004)*	*(0.028)*	*(0.011)*	*(0.004)*
Δ Log(SALES)	0.164***	0.143***	0.208***	0.404***	0.337***	0.162***
	(0.009)	*(0.011)*	*(0.009)*	*(0.062)*	*(0.026)*	*(0.010)*
Δ Log(SALES)_{t-1}	-0.083***	-0.082***	-0.067***	0.005	-0.022	-0.077***
	(0.009)	*(0.011)*	*(0.009)*	*(0.061)*	*(0.025)*	*(0.010)*
LIQ_{t-1}	0.032	0.085**	0.012	-0.002	0.067	0.007
	(0.026)	*(0.034)*	*(0.027)*	*(0.147)*	*(0.057)*	*(0.032)*
LIQ_{t-1}*MPR	0.043	0.047	0.097***	0.068	-0.021	0.122***
	(0.035)	*(0.046)*	*(0.037)*	*(0.181)*	*(0.074)*	*(0.045)*
TC_{t-1}	0.028**	0.045***	0.018	0.056	0.090**	0.022
	(0.014)	*(0.017)*	*(0.014)*	*(0.100)*	*(0.038)*	*(0.016)*
TC_{t-1}*MPR	0.057***	0.049**	0.040**	0.119	0.005	0.050**
	(0.017)	*(0.021)*	*(0.018)*	*(0.103)*	*(0.046)*	*(0.021)*
Subsample of firms:	with less than 200 employees	with less than 90 employees	with high ratio of intangible assets	paying dividends for 16 periods	paying dividends at least for 12 periods	intersection of (5.1), (5.3) and (5.4)[1]
N. Obs.	42,977	26,968	39,274	784	5,084	29,335
N. Groups	3,000	1,913	2,711	49	322	2,046
R-sq (within)	0.15	0,15	0.15	0.25	0.18	0.14
Hausman Test	5,270.80	3,598.03	4,481.87	101.55	859.04	1,663.64

Notes: *** (**) (*) denotes 1% (5%) (10%) significance level. Standard errors in parenthesis. Year-effects included.

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