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Author: Ana Carla A. Costa, João M. P. De Mello

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# Judicial Risk and Credit Market Performance
## Micro Evidence from Brazilian Payroll Loans

Ana Carla A. Costa and João M. P. De Mello

### 5.1 Introduction

In recent years, the literature has built a near consensus that "sound" institutions are congenial to good economic performance (North 1994). Institutions, insofar as they determine the economic environment agents operate in, should be important for explaining economic outcomes. Quite often, the specific mechanism through which institutions influence economic performance is protection from expropriation. In environments in which expropriation is likely, agents underinvest (from a social perspective) relative to more secure ones. In the end, a plethora of suboptimal microeconomic decisions amount to a poorer aggregate economic performance.

Indeed, most of the empirical effort in associating institutional "soundness," however defined, and economic performance has been on the aggregate level. An observation on a typical study is a country (La Porta et al. [1998b] is a seminal example). Institutional measures are then linked to economic performance on various dimensions. La Porta et al. (1998b), for example, document that the origin of the legal system is associated with the

Ana Carla A. Costa is a researcher at Tendências, a consulting firm in São Paulo, Brazil. João M. P. De Mello is an assistant professor of economics at Pontifical Catholic University in Rio de Janeiro, Brazil.

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degree of creditor protection. La Porta et al. (1997) find that a lower degree of creditor protection implies smaller debt and equity markets (Djankov et al. 2003).[1] Another set of articles study the financial deepening-economic growth link (King and Levine 1993; Levine and Zervos 1998), finding a positive relationship. Taken all together, these papers seem to imply the following chain of causality. At the basic level, legal origin (institution) causes creditor protection (protection from expropriation). At the second stage, better creditor protection causes financial deepening. Finally, financial deepening causes economic growth.

This chain of causality would be more convincing were microeconomic evidence available. The missing link is due to the level of analysis, much broader than the relevant *locus* of economic decisions. There is, for example, an implicit assumption that agents do invest less if creditor protection is lower. For several reasons, it is hard to be completely convincing with such an aggregate level of analysis. One such reason is reverse causality. The following example, however farfetched, is illustrative. Assume investment is completely inelastic, and creditor protection is a superior good. Creditor protection, in this setting, has only distributive, not allocative, effects. For demand reasons, there is, however, a reverse causality running from income to creditor protection. Evidently, investment is not completely inelastic, but the demand driven story is still conceivable. Most of the studies do recognize this possibility, and try to find sufficiently exogenous variation to relate institutions and economic performance. Acemoglu, Johnson, and Robinson (2001) and Levine (1998) are good examples of careful searches for such variation.

Another problem stems from the fact that legal procedures are "chosen" by society and, hence, may be endogenously designed to tackle the issues often put as the dependent variables in the regressions. La Porta et al. (2004) face this difficulty. They argue that legal formalism reduces the quality of the judicial system. But formalism, as they recognize it, could also be a response to "weaker law and order environment." Their strategy is to use the fact that most countries inherit their legal tradition (and that French civil law is more "formalistic"), which makes the legal tradition a source of exogenous variation. Again, the story is compelling insofar as it is prohibitively costly for countries to "change" their legal tradition because otherwise "maintenance" of tradition would itself be endogenous.

However well argued (as it is the case in all papers cited), identification

1. Pinheiro and Cabral (1998) follow this tradition for the Brazilian credit market. Using state-level data on outstanding volumes of credit and an index of judicial efficiency (based on the results of a survey conducted with businessmen on each state where they rate the quality of the local judiciary), they relate variation in judicial inefficiency to differences in outstanding volumes of credit across the states. The authors conclude, corroborating the institution-development hypothesis, that improving the efficiency of judicial enforcement is important for credit markets development.

is mostly a rhetorical issue as one can only test for *over*identification. With micro level evidence, these issues can be bypassed, and one can directly assess how market participants respond to varying institutional environments. Creditor protection and financial deepening is an example. If there is evidence that creditors price judicial risk or restrain quantities in the face of weak protection, then it becomes much more compelling that legal protection induces financial deepening. In this case, one could be much more confident that the causality from creditor protection to income is of first-order, as opposed to demand driven explanations, such as protection being a superior good.

A third reason is omitted factors. Several other countries' characteristics might determine both institutional setting (such as legal origin and level of credit protection, the usual explanatory variables) and economic performance (the usual regressand). Consider again the Acemoglu-Johnson-Robinson strategy (2001) for finding exogenous variation in institutional soundness to estimate the institution-economic performance link.[2] For former colonies, one conceivable alternative story is the type of colonization. Suppose that, for sheer coincidence, while countries with a French civil law tradition (usually interpreted as "unsound" institutions) occupied lands that had valuable goods for the European market (silver in Peru and sugar in Brazil, for instance), countries with common law tradition ("sound" institutions) arrived at places that had few "tradable" goods with Europe (early English colonization of the United States). Suppose as well that this trade feature determined how exploitative colonization was and that exploitation had long-lasting effects. In this case, the (omitted) driving force is whether there were comparative advantages to be explored. However, sound institutions and (later) economic performance would still relate empirically although causal interpretation would not be warranted. We do not claim the institutional settings do not matter and that the legal tradition only enters the picture through trade "causing" both institutional settings and economic performance. The crucial point is that, with micro-level evidence, it is unnecessary to be concerned about such alternative explanations.

Finally, measurement is intrinsically more problematic with aggregate data. In La Porta et al. (1998), (country-level) creditor protection is measured by characteristics of the countries' corporate laws and by several indexes.[3] Besides the inherent arbitrariness in constructing such indexes, theory not always provides clear guidance in interpreting the results. For

2. Acemoglu, Johnson, and Robinson (2001) document that "better institutions" arose in countries where mortality rates due to native diseases were low when colonizers originally arrived. This, according to the authors, shifts the equation that determines institutions but not the equation that determines current economic performance.

3. They have indexes for, among others, efficiency of the judicial system, risk of expropriation, and risk of repudiation of contracts by government.

example, is it theoretically clear that restricting the behavior of managers always increases the amount of finance *in equilibrium?* It is conceivable that, if you sufficiently restrict managers' behavior, the size of debt and equity market will be small, for reasons pertaining to the supply of securities? Without a clear theoretical support, an empirical finding that restricting managers' behavior is associated with "larger" equity, and debt markets are subject to criticisms that micro evidence is not. One such criticism is the presence of nonlinearities in the creditor protection-market performance relation.[4]

It might seem puzzling the *relative* lack of micro evidence on the institution-development *nexus.* We conjecture that this is due to the scarcity of a fortunate coincidence: data on both the relevant economic decision locus (firms, consumers) linked to variation on institutional settings. La Porta et al.'s (2004) study on the formality of legal procedures and the quality of the legal system is something of an exception.[5] They do not, however, directly associate market-level performance with different institutional settings.

In this work, we take advantage of a particular set of events that provide variation on a relevant institutional setting, and we are able to associate this variation with data on the relevant economic decision locus. The empirical setting is the market for Payroll Debit Loans in Brazil, which are personal loans with principal and interest payments directly deducted from the borrowers' payroll check. Automatic deduction from payroll, in practice, makes a collateral out of future income. In June 2004, a high-level federal court upheld a regional court ruling that had declared payroll deduction illegal.[6] The decision by the federal court has a case-specific nature, that is, only applies to this particular dispute. There is, however, evidence from market practitioners that there was an increase in the perceived probability that the decision could establish precedent and turn useless the future income collateral. Using personal loans without payroll deduction as a control group, a difference-in-differences procedure assesses whether the judicial decision had an impact on market performance. Our results suggest that the decision had an adverse impact on banks' risk perception, on interest rates, and on the amount lent. In this sense, this is direct evidence of market participants' reaction to institutional risk.

Our theory is simple to the point of trivial: an increase in the chance of expropriating the collateral should shift the supply of loans inward, worsening market performance. Whether the empirical consequences are first

4. Dubey, Geanakoplos, and Shubik (2005) show that with incomplete markets that intermediate levels of debtor punishment can induce a larger quantity of credit that extreme levels of debtor punishment.

5. In this paper, the authors study the link between formality of the legal system and the time elapsed to evict nonpaying tenants and to recover a bounced check. Furthermore, they associate formality with other measures of judicial system performance, such as corruption and access to justice.

6. The court ruled at the very end of June (28th). However, the press release was on July 1.

order is far from self-evident. This is, indeed, the goal of the paper: investigate whether a clear-cut shift in the institutional setting has microeconomic consequences. Evidence from market practitioners is ambiguous. While some important players had the perception that the decision could have strong adverse effects, equally important ones thought the effect would be second order.

The market-level evidence is a complement, not a substitute, to the aggregate-level evidence. Indeed, our results in no way contradict the literature. On the contrary, they corroborate it. While aggregate evidence indicates that institutional differences are of first-order importance in explaining variation in countries' performances, micro- and market-level evidence evaluates directly the implicit assumption necessary to interpret the aggregate evidence as indeed causal.

The result has an additional interest given the empirical application. Payroll lending is one of the workhorses of the recent Brazilian credit market expansion. Brazil, in the La Porta-Lopez-Silanes-Shleifer-Vishny tradition, is a French civil law country, with low creditor protection. Credit markets are relatively underdeveloped. Recently, however, it has made several efforts toward a more creditor friendly institutional environment. Courts may be particularly important in an environment with weak creditor protection, where other protective institutions, such as laws, are weak or inexistent.

The paper is organized as follows. Section 5.2 outlines the recent evolution of credit market in Brazil and the chronology of payroll lending, emphasizing the relevant events, such as the approval in congress of the law regulating payroll lending for retirees and the judicial decision on the legality of payroll deductions. Section 5.3 presents the data, and section 5.4 presents the empirical strategy. We argue that the presence of an identical product, except for deduction in payroll, provides a good control for associating changes in the institutional environment to market changes in payroll lending. Results are presented and discussed in section 5.5. Section 5.6 concludes.

## 5.2 Credit Market in Brazil: Recent Evolution and Payroll Lending

In recent years, bank lending experienced a pronounced increase in Brazil, especially in lending out of banks' "free lending funds" (those not earmarked by mandatory programs). Between July 1999 and September 2005, the free loans/gross domestic product (GDP) ratio went from 8.3 percent to 17.1 percent (figure 5.1). This free loan segment now represents 67 percent of total banking credit, changing positions with directed credit operations, that now stands at 33 percent.[7]

7. Numbers for December, 2005. Banking credit portfolios in Brazil have two types of loans: free market operations, where banks can set quantity and prices according to their profit maximizing behavior; and compulsory directed credit operations, mostly channeled to housing and rural sectors at subsidized interest rates.

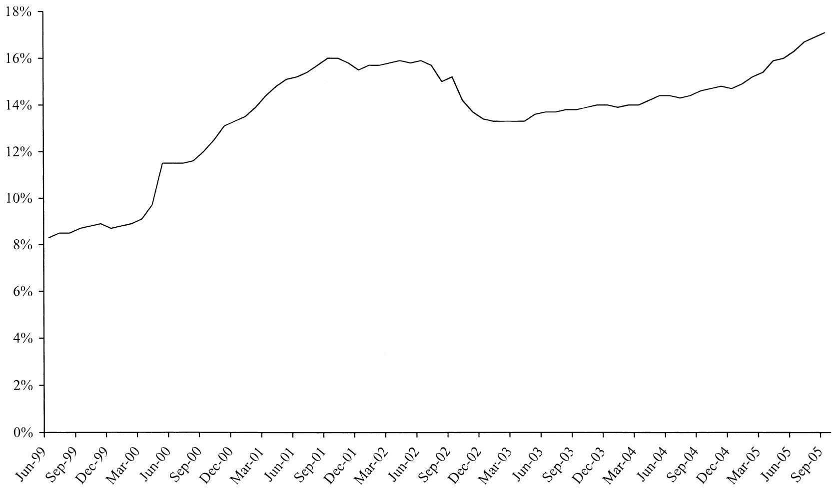


**Fig. 5.1 Private bank credit/GDP ratio, 1999 to 2005**

Interestingly, this tendency of financial deepening took place during a period of tight monetary policy.[8] Despite this fact, free market lending expanded remarkably. Several factors help explain this trend.

These specific factors are all linked to institutional reforms that took place in Brazil since the end of 1999. Measures included efforts to reduce information asymmetries in credit markets (such as the new credit ranking and provisioning regulation, through Resolution 2.682/99, and the Central Bank Credit Information System (SCR), implemented in 1999 and improved in 2000 and 2004); more efficient instruments of collateral recognition and contract enforcement (as the so-called *Cédula de Crédito Bancário,* a claim with faster execution procedures, in 2001 and 2004;[9] a better insolvency resolution system (through a new bankruptcy law, approved by Congress in the end of 2004); and regulation of creative credit instruments, such as payroll lending. They provided an improved institutional environment and possibly led to the observed higher volumes of credit concessions by the Brazilian banking sector. As suggested in the previously cited literature, the evolution toward a more creditor friendly environment might have engendered this initial movement of financial deepening in Brazil.

8. Brazil adopted inflation target and floating exchange rate regimes in 1999 during a liquidity crisis, exchange rate devaluation, and inflation pressure. Interest rates were the main instrument used to stabilize the economy. Inflation targets are set by Nacional Monetary Council, and basic interest rates are defined monthly by Central Bank in Monetary Policy Committee (COPOM) meetings.

9. The SCR brings detailed information on borrowers' credit contracts of over R$5,000.00 (roughly US$2,200.00).

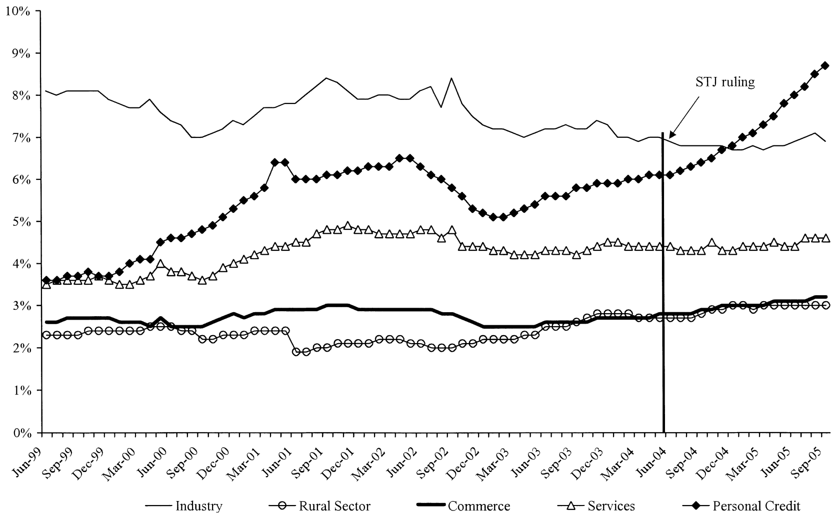


**Fig. 5.2 Evolution of private bank credit as a percentage of GDP, by economic sector**

Nevertheless, this rapid expansion path—more pronounced during the last two years—is not observed in all credit market segments. On the contrary, this acceleration is mainly explained by growing volumes of consumer lending. Credit to this segment, which in 1999 represented 3.6 percent of GDP (or 9 percent of total private bank credit portfolio), reached outstanding volumes that amount to 8.7 percent of GDP in 2005 (or 31 percent of total private bank credit portfolio). Consequently, since December 2004, personal loans became the biggest part of total bank loans, with an even higher participation than industrial credit, that has been stable around 6.9 percent of GDP (figure 5.2).

Consumer credit loans in Brazil can be divided into three main types of loans: the personal loan, for consumption purposes; loans for vehicle acquisition; and *Cheque Especial,* a consumer overdraft facility. It is, however, in the personal loan category—the largest category, that a major growth is observed (52 percent during the last twelve months), as shown in figure 5.3.

This paper is concerned with personal loans, which are further divided into two subcategories: the standard loan contract (hereafter standard loan), and a special type of personal loan contract that has an automatic monthly payment deducted from the borrower's salary. This is the payroll lending operation (*Crédito Consignado em Folha de Pagamento,* hereafter payroll loan), which represents over 35 percent of all consumer credit in Brazil and whose growth path has shown a particularly noticeable increase. Figure 5.4 shows the evolution of payroll lending operations and its in-

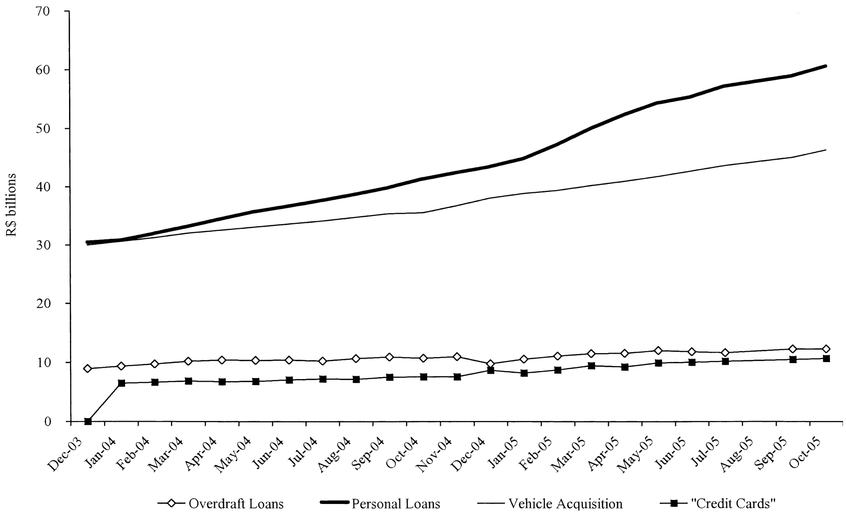


**Fig. 5.3 Evolution of consumer lending in Brazil, by category**

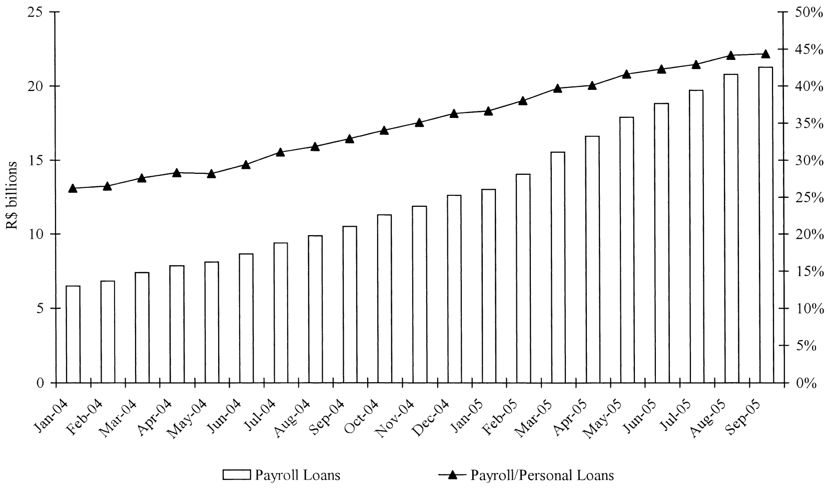


**Fig. 5.4 Evolution of payroll loans, absolute and as proposition of total personal loans**

creasing participation on total personal loans for the thirteen largest active banks in this segment.[10]

Payroll lending has existed in Brazil since the beginning of the 1990s. It

10. Brazilian Central Bank collects this data for this small—but representative—sample of banks since January 2004. It now aims to expand it to all banks operating with this specific type of credit.

was restricted to government personnel and was originally operated by peculium institutions, which had the possibility to act as trusts before public administration agencies.[11] But since the second half of the 1990s, some financial institutions identified in this type of loan a good business opportunity, with low credit risk and high return. Those banks entered this credit market through the acquisition of peculium institutions already registered as trustees.

### 5.2.1 Payroll Loans: Description of the Product, Chronology of Events, and Practitioners' Opinions

The decisive expansion of payroll lending operations occurred in September 2003, when the government sent to Congress a provisory law (*Medida Provisória* [MP] 130), subsequently turned into Law 10.820/03.[12] The law regulated the possibility of salary consignation for private-sector formal workers and for retired workers from private sector and pensionaries covered by the National Institution of Social Security (INSS).[13]

In practice, payroll deduction turns future income into a collateral. Evidently, future income is valuable as a collateral insofar as it is not too volatile. This is precisely why payroll lending is mainly used by the following three types of borrowers. Before the 2003 law extended regulation to private-sector retirees, banks lent to public servants, which have employment stability. Banks then started operating with private-sector workers, but in association with the labor unions and employers. Contracts are collective, which mitigates idiosyncratic income risk. Finally, after the December 2003 law, banks started operations with retirees from the private sector, which also have a constant income flow. The main risk lenders face is death, which is diversifiable and insurable.

Lenders, however, face another peril: judicial risk. Collateral has value only if courts recognize it as such. Payroll lending in Brazil provides an excellent empirical setting to assess judicial risk. In 2002, a public servant of the city of Porto Alegre (the capital of the state of Rio Grande do Sul) sued Banco Sudameris claiming the payroll deduction on his salary was illegal.[14] A state-level court (Tribunal de Justiça do Rio Grande do Sul) ruled for the plaintiff. The decision did not draw much attention for two reasons. First,

11. Law 8.112/90 admits the possibility of payroll consignation for government personnel.

12. *Medida provisória* is a legislative device in which the executive sends a bill to congress that is effective immediately, pending approval. It has an urgency status that forces the legislator to appreciate its merit. For practical purposes, it is almost equivalent to a full-blown law.

13. The Brazilian pension system, a pay-as-you-go scheme, is publicly managed by the INSS.

14. The deduction was R$58.66 (roughly US$22 by then) to cover amortization and interest expenses on a R$1.015 loan. The precise claim was that wages are essential for subsistence and therefore cannot be pawned. Furthermore, the monthly nominal interest rate of 3.8 percent was ruled "abusive." See *Valor Econômico* 07/02//2004. For the actual decision, see the STJ Web site at http://www.stj.gov.br.

by that time, payroll lending was not such an important credit instrument. Second, the decision did not set a precedent, once it was related to a claim that started before the 2003 law, and had been ruled by a state-level court. Sudameris appealed to the second highest ranking federal court in the country, the Superior Tribunal de Justiça (STJ).[15] In late June 2004, the STJ upheld the regional court ruling. Although technically this decision also did not set precedent on the issue, it could signal the direction of future rulings.[16] In this case, the future income collateral could become useless. At the time, Minister Edson Vidigal, from the STJ, declared that "[when] analyzed through the salary perspective, the consignation can be suspended," and "[banks] might have to search for alternative forms of guarantees."[17]

Statements by some key practitioners suggest that banks perceived this as a hazard to their payroll loans operations. Right after the decision, the Chief of Judicial Operations of Federação Brasileira de Bancos (FEBRABAN, the main bankers' association), Johan Albino Ribeiro, declared to the press that "undoubtedly there will be a repercussion in terms of higher interest rates" since "[one] of the elements that sustain the low interest rates is the low risk on these loans. If the legality of the contract is contested, the risk increases."[18] Luis Marinho, then the head of Central Única dos Trabalhadores (UT), the main workers' union, reported that he had received phone calls from several bankers informing "[that] banks would hit the break on new loans, at least temporarily, until they have a better understanding of the extension of the STJ decision."[19]

However, whether banks indeed reacted to the decision in an economically meaningful way is not obvious. Indeed, it was not even clear whether, legally, the court ruling would have a lasting effect. As it was noted, the decision only applied to one specific claim, related to a public servant and which took place before the December 2003 formal regulation. Therefore the STJ decision could not, technically set precedent for future lawsuits. Several banking lawyers thought the law regulating payroll loans (Law 10.820/03) was crystal clear.[20] In this sense, all the decision could signal was the courts' mood toward payroll loans. Furthermore, banks could have simply ignored it. Indeed, Gabriel Jorge Ferreira (a former head of FEBRABAN), from UNIBANCO (the third largest private bank in Brazil), declared that "[the program] is still intact, and I do not think there

15. Hierarchically, the STJ stands between the STF (Supremo Tribuinal Federal), the equivalent of the American Supreme Court, and the TFJs (Tribunais Federais de Justiça), the equivalent to the American Federal Circuit Courts.

16. The STJ rulings are case specific and do not set precedent.

17. See *Gazeta Mercantil,* July 16, 2004.

18. See *Valor Econômico,* July 2, 2004.

19. Mr. Marinho would later be appointed Minister of Labor. See *Universo Online,* July 4, 2004, http://an.uol.com.br/2004/jul/04/0eco.htm.

20. See *Valor Econômico,* July 2, 2004, August 13, 2004.

will be an upward pressure in interest rates."[21] Indeed, this is precisely our object of study: whether there is evidence that this judicial hazard had a first-order impact on market performance. In our application, an affirmative answer would be even more meaningful given the ambiguity of both the (practical) legal consequences of the ruling and the bankers' reactions. As figure 5.4 shows, it is clear that the court ruling has not prevented the recent growth of payroll loans. There is, nonetheless, a couple of interesting contrafactual questions left to ask. Absent the decision, would this growth have been more pronounced? Would terms be better (i.e., lower interest rates)?

## 5.3 Data

Using original data from the SCR, we constructed a data set on payroll and standard loans. For both types of credit contracts, we have bank-level monthly data over a period starting in January 2003 and ending in May 2005. There is, initially, data for 109 active banks on outstanding volumes of payroll and standard personal lending operations. We have bank-level information on the total amount of loans, average risk rating, average interest rate, number of credit contracts, and average size of the credit contract.[22]

The data has information on loan contracts above R$5,000 (US$2,270). An average sized contract is R$84,719 (US$38,508). This strongly indicates that contracts in the data are mainly indirect, that is, with entities such as labor unions and governmental agencies, which intermediate the negotiation, and afterwards refer the bank to their employees or members. Contracting directly with individuals began mostly after the December 2003 law, which regulated payroll lending to private-sector retirees. Because it took at least another five months for a significant group of banks to be chartered by the INSS, the fact that these loans do not show in our data is relatively immaterial.[23]

In order to keep consistency among observations, banks had to satisfy several criteria to be part of the final sample used. First, only banks that consistently operated in both credit products were included. This avoids picking up unrelated (to the court ruling) entry and exit decisions, which are but noise for our purposes. Only banks that supplied both standard and payroll loans for the whole January 2004 to December 2004 period were included. Second, banks that had inconsistent pricing behavior were excluded. For example, several banks had annual nominal interest rates at 12

21. See *Universo Online,* July 4, 2004, http://an.uol.com.br/2004/jul/04/0eco.htm.

22. Interest rate is weighted by the volume of new concessions at each risk category. Credit risk rating goes from 1 (or AA operations: less risk) to 10 (or HH operations: maximum risk), following provisioning and classification criteria set by Nacional Monetary Council regulation.

23. The December 2003 law required the bank to be chartered by the INSS in order to supply payroll lending to private-sector employees. The first bank to be charted was the Caixa Econômica Federal (a federal government bank), in May 2004.

percent, which are clearly out of line with the rest of the market. Twelve percent operations are either reporting errors or special loans such as those to own employees, which we conjecture to have a different risk assessment nature. Other banks had inconsistent structural breaks on the interest rate series.[24] Finally, it is not clear whether government-owned banks (both state and federal) have the same objective function as their private counterparts. The literature is ambiguous with this respect. Although some works suggest that there is no evidence that public owned banks are less efficient than the private counterparts (Altubas, Evans, and Molyneux 2001), there is little controversy over their different lending behavior (Sapienza 2002). And, for Brazil, even if government-owned banks had the same objective function as private banks, payroll loans is an important piece of policy for the current federal government, and federally owned banks might be responding to public policy rather than maximizing profits regarding payroll loans.[25] For these reasons, government-owned banks were excluded.

After these adjustments, the sample consists of forty banks, representing 67.8 percent of total payroll lending volumes as of May 2005. The sample includes four out of the five major private Brazilian banks.

## 5.4 Empirical Strategy

The opinions voiced by market participants in the press suggest the three economic variables that might have been affected by the June 2004 STJ ruling: risk assessment, the pricing of loans, and the amount lent. The empirical strategy consists in comparing the evolution, over a period of time that contains the ruling, of two products: payroll and standard loans. The difference in their evolution over the period is interpreted as the causal effect of the STJ decision, as in any difference-in-differences model.

### 5.4.1 The Control Group

As mentioned in section 5.2, although payroll lending has existed since 1990, only in December 2003 was legislation regulating its application to private-sector formal workers and retirees and pensionaries of social security passed. Moreover, only since January 2003 have we had available—and good quality—split data on payroll and standard personal loans.

24. It is important to emphasize that we identified some problems with the interest rate variable in SCR data set. For this reason, we are less confident about the interest rate results than the other results presented in section 5.5. The SCR regulation states that interest rates must be reported on a yearly basis. Nevertheless, not only do inconsistent numbers such as zero or very low rates abound but also rates that seem to be monthly or contract period based systematically appear. Those observations were discarded.

25. Nonprofit maximizing behavior should not come as a surprise in Brazil when analyzing public banks' portfolios. Banco do Brasil (BB) and Caixa Econômica Federal (CEF), the two largest government-owned banks are, respectively, the major players in rural and housing subsidized credit. Banco do Brasil's outstanding rural credit portfolio represents 52 percent of all directed and subsidized rural credit in Brazil. The CEF, as of January 2005, accounted for 42 percent of total subsidized housing finance operations in Brazil.

The object of interest is a supply effect: has the court decision shifted the supply of payroll loans? We do not, however, pursue the strategy of searching for exogenous variation to estimate the supply directly. As it will become clear in the following, a reduced-form object is estimated for price, risk, and quantity. The strategy consists of using standard personal loans as a control group. This way, one can gauge the effect of the court decision above and beyond unobserved concurrent factors that might have affected both the demand and supply of payroll loans.[26]

Standard personal loans are a reasonable control group for payroll personal loans. The two products are the same, with the exception of the payroll deduction.[27] That is, both products are personal lending operations, consumption oriented, and have no formal collateral or real guarantee attached to them. Finally, because standard loans do not have payroll deduction, they were not directly affected by the June 2004 court ruling.

A fair question is why standard loans exist at all given the presence of an apparently superior very similar credit instrument. As a matter of regulation, payroll loans were confined to special classes of borrowers up until the December 2003 law and the subsequent chartering of banks to provide these loans on a more general basis.[28] In particular, it could be the case that public-sector employees were significantly more present in payroll vis-à-vis standard loans. This, however, does not seem to be the case, especially for our specific sample: payroll lending with the observed average size consists of both private-sector employees (through agreements with private companies or professional associations) and public servants.

While differences in the composition of the pool of borrowers is not a threat to our identification strategy, whether these two pools of borrowers *changed differently over the sample period* is. There are two reasons why this does not seem to be the case. First, the main change in composition of the pool of borrowers occurred during 2005, when banks started getting chartered by the Social Security Agency to lend to private-sector retirees. Therefore, there were no significant changes in the compositions of the pool of borrowers in the two groups. Second, economic conditions could have changed differently for the two groups, holding constant the composition of both pools. This would happen if, for instance, the public sector was downsizing at the time or if the private formal sector was experiencing a particularly turbulent period. Neither was the case.

Table 5.1 presents summary statistics on the variables that are used as regressands in the following analysis. As expected, the average interest rate is

26. We do not have overall demand shifters, that is, exogenous variation to estimate the supply, let alone product specific (to payroll loans, for instance) demand shifters. For example, there is no compelling economic reason why seasonality (a candidate) would affect payroll loans differently than standard loans.

27. As a matter of regulatory taxonomy, standard and payroll loans are two subcategories of personal loans.

28. See section 5.2.

**Table 5.1** **Summary statistics**

| | Mean | | Standard deviation | |
|---|---|---|---|---|
| | Whole period | Subsample (month >12 and <18) | Whole period | Subsample (month >12 and <18) |
| Average interest rate (% points) | | | | |
| Treatment: Payroll | 45.07 | 46.08 | 12.21 | 8.80 |
| Control: Standard | 56.67 | 53.93 | 24.62 | 26.05 |
| Total amount of loans (R$) | | | | |
| Treatment: Payroll | 6.83E+07 | 5.93E+07 | 1.38E+08 | 1.13E+08 |
| Control: Standard | 6.54E+07 | 5.90E+07 | 1.43E+08 | 1.19E+08 |
| Average risk (from categories 1 to 10) | | | | |
| Treatment: Payroll | 2.51 | 2.63 | 0.55 | 0.66 |
| Control: Standard | 3.17 | 3.31 | 0.99 | 1.13 |

*Source:* Banco Central do Brasil.

*Notes:* Subsample of forty banks included in the regression analysis. Market averages, weighted by bank size of operations, except for total amount of loans.

lower in payroll than in standard loans: the instruments are very similar, and the former has wages as collateral. Similarly, standard loans are riskier, which is consistent with a higher voluntary—and involuntary—default probability. The amount lent in payroll loans is higher than in standard loans and has increased more pronouncedly over the sample period.[29]

When one compares the summary statistics for the control and treatment groups, a few points emerge. First, for payroll loans, both interest rate and risk were slightly higher than average on the subperiod before the court ruling. For standard loans, the interest rate was below average, and risk was slightly above average. This is important for our purposes as the different types of loans could be, on the months before the ruling, on different parts of a mean-reversing process. This does not appear to be the case, and, if anything, interest rates should tend to increase more (decrease less) for standard loans, vis-à-vis payroll loans, if a mean-reversing force is operative. It is similar for risk.

As for amount lent, one can see, from both table 5.1 and figure 5.4, an increase in both categories over the period, with a more pronounced increase for payroll loans. The two categories are following, over time, different paths, which could lower the value of standard loans as a control group. However, if anything, the pronounced upward trend in payroll loans would make it particularly difficult to document a *decrease* in payroll loans, relative to standard loans.

29. For the thirteen banks of the sample mentioned in section 5.2, granting of payroll loans increased by 66.7 percent during the last twelve months. Outstanding volumes more than doubled during the same period, while total personal loans increased by 50.1 percent (Nota Economica para Imprensa [NEI] and Banco Central do Brasil [BCB] 2005).

### 5.4.2 The Specifications

The interest rate and the quantity models are quite similar. An observation is a product $i$, offered by a bank $b$, at a month $t$. There are two products, personal credit with and without payroll automatic debit deduction. Let DECISION be a categorical variable that assumes the value 1 for July 2004 and all later months. It denotes the treatment period.[30] PAYROLL is a categorical variable that assumes the value 1 if the product is personal loan *with* payroll deduction. It identifies the treatment group. The estimated model for the interest rate is

$$\begin{aligned}\Delta \log(\text{INTEREST})_{itb} = \beta_0 &+ \beta_1\text{PAYROLL}_{itb} + \beta_2\text{DECISION}_t \\ &+ \beta_3\text{DECISION}_t \times \text{PAYROLL}_{it} \\ &+ \Omega\text{MONTH}_t + \text{Controls} + \varepsilon_{itb}.\end{aligned}$$

$\text{INTEREST}_{ibtr}$ is the average interest rate on all loans given by bank $b$ on product $i$, at month $t$. The panel unit is a pair bank product. We are interested in the level of log effect, but the data is first-differenced to eliminate fixed effects of the pair bank product. Controls include the log of the average risk on the banks' portfolios, the (lagged) total number of loan operations, and the (lagged) average size of the loan operations. Risk is included for obvious reasons as it should determine interest and is affected by the decision. Total number of loans is included because, as we have seen, payroll and nonpayroll loans have different rates of expansion over the sample period. Because expansion might affect the quality of the loan portfolio, the total number of operations should be controlled for. The average size of operations is included as it is conceivable that banks reacted to the judicial decision by decreasing exposure on operations by decreasing their size.

The main parameter of interest is $\beta_3$, the difference-in-differences coefficient. If the judicial decision had an impact on banks' pricing of payroll loans, then $\beta_3$ should be positive. We run a ordinary least squares (OLS) procedure on this equation, with the two modifications. First, we weight observations by the size of banks' operations on payroll and standard loans to arrive at an average market response. Second, we correct for between-panel correlation and within-panel autocorrelation.

The model can be viewed as a reduced form in which prices (in this case interest rates) are regressed on exogenous variables. As in any reduced form, there could be supply (which is of interest) and demand effects (not of interest) on the parameters. After controlling for period specific effects, estimates should be clean of most demand effects, and $\beta_3$, the main coefficient of interest, should capture a supply response to the ruling. Note that,

30. Rigorously, the decision took place in June 2004. It was, however, at the very end of the month (the 28th), so banks only had time to react to it in July. Therefore, all estimated models consider the treatment period to start in July 2004.

precisely to mitigate capturing demand effects, we lag variables such as total operations and average operations.

The quantity model is similar except that we do not control for the total number of operations and the average size of operations. These variables are excluded because the dependent variable, TotalLoans, is the product of average size and number of loans and therefore would unduly capture most of the variation in TotalLoans. With the model in logs, in fact, OLS will make both coefficients equal to one and report an $R^2$ of 1.

$$\begin{aligned}\Delta \log(\text{TotalLoans})_{itb} = \beta_0 &+ \beta_1\text{PAYROLL}_{itb} + \beta_1\text{DECISION}_t \\ &+ \beta_3\text{DECISION}_t \times \text{PAYROLL}_{it} \\ &+ \Omega\text{MONTH}_t + \text{Controls} + \varepsilon_{itb}\end{aligned}$$

The main control now is the first difference in the log of average risk on the banks' portfolio. Again, the main parameter of interest is $\beta_3$, the difference-in-differences coefficient. If banks reacted to the judicial decision by restricting quantity, then $\beta_3$ should be negative. We estimate the parameters by an OLS and an instrumental variables (IV) procedure. Different from the interest rate equation, there is empirical reason to believe the lag of the dependent variable belongs to the right-hand side, and there is also reason to believe that there is serial correlation on the error term. In this case, OLS could produce inconsistent estimates (see Arellano and Bond 1991).[31] Similar to the interest model, we weight observations by the size of banks' operations in personal lending, and standard errors are corrected for between correlation and within-panel autocorrelation.

For the risk perception model, an observation is a product $i$, offered by a bank $b$, at a month $t$. In the first specification, the dependent variable, $\text{RISK}_{ibt}$, is a dummy variable, that assumes the value 1 if the average risk on product $i$ loans given by bank $b$ at month $t$ is above the median risk for that bank over the period considered. In the second specification, $y_{ibt}$ is the average risk on product $i$'s loans given the bank $b$'s at month $t$. The estimated model is

$$\begin{aligned}\text{RISK}_{itb} = \beta_0 &+ \beta_1\text{PAYROLL}_{ibt} + \beta_2\text{DECISION}_t + \beta_3\text{DECISION}_t \\ &+ \text{PAYROLL}_{ibt} + \text{Controls} + \Omega\text{MONTH}_t + \varepsilon_{ibt}.\end{aligned}$$

$\text{Controls}_{bit}$ are variables that affect risk (such as average size of loans and total number of loans). Because in the case when RISK is a dummy, it is unnatural to first difference the data, so to account for bank-product fixed effects, we include bank dummies in both specifications to maintain homogeneity. With the model in levels, when RISK is the average risk in the

31. Several economic stories could be told to justify the lag of Δ log (Total Loans) to belong, or not, to the right-hand side of both the interest and the quantity equations. Because this is not our variable of interest, we take an agnostic empirical approach and evaluate whether empirically it belongs to the equation and take proper econometric steps to correct (i.e., look for exogenous variation) if it does.

bank portfolio, one has to worry that the lagged dependent variable might belong to the equation, and the lagged average risk is included.[32] When RISK is a dummy, however, including lagged dummies will unduly absorbs most variation: unless the dummy oscillates wildly, it replicates itself most periods, and most variation in the dependent variable is explained by its lag. It is important to notice that this happens not because of the economics of the dynamics of riskiness on these loans but by the way the dependent variable is constructed. Incidentally, this effect is especially pronounced if the hypothesis to be tested is true: the dummy should assume lots of 1 values after the decision and a lot of 0 values before the decision.

Again, the main coefficient of interest is $\beta_3$, the difference-in-differences coefficient. If the judicial decision had an impact on banks' risk perception on payroll loans, then $\beta_3$ should be positive: risk assessment on payroll loans increased compared to standard loans. We run a logit procedure on this equation, again weighting observations by the size of banks' operations in personal lending.

Notice that in all models, variation among banks is used. This is crucial as the main economic decision unit is a bank. Although the judicial decision hit banks at the same time ($\text{DECISION}_t$ does not vary over $b$), banks potentially differ in their response to the decision, and this provides variation to estimate the coefficient of interest. In the end, the response of an average bank is estimated, with larger banks counting more than smaller ones.

## 5.5 Results and Discussion

### 5.5.1 The Risk Equation

We start with the risk equation. In table 5.2, the dependent variable is a dummy for whether bank $b$'s average risk on the product operation (standard and payroll loans) is above the median risk for the whole sample (January 2003 to June 2005). The main hypothesis is tested in column (1). The sample is restricted to five months before the decision and five months after the decision. The coefficient associated with the difference-in-differences regressor ($\beta_3$) is 0.357, and it is quite precisely estimated (it is significant at the 1 percent level). This means that, relative to standard loans, the probability that the operation on payroll loans was above the median risk increased. The model is nonlinear, and there is no immediate way to interpret "above the median risk" is an economically meaningful way, so it is difficult to evaluate this coefficient quantitatively. One can, however, state that, qualitatively, risk perception on payroll loans increases in period following

32. In reason for this is that banks, by pricing risk, might scare off better borrowers. Hence, higher risk today could cause higher risk tomorrow. One has then to worry about dynamic panel biases (Arellano and Bond 1991). See section on results for more on this.

**Table 5.2 Risk equation: Results 1**

| | Subsample | | | |
|---|---|---|---|---|
| | Month >Feb/04 and <Dec/04 (1) | Month >Feb/04[a] (2) | Month >Sep/03 and <July/04[a] (3) | Month >July/04[b] (4) |
| Payroll loan | –0.184 | –0.220* | –0.386** | 0.153 |
| | (0.137) | (0.120) | (0.176) | (0.116) |
| Judicial decision | –0.391*** | –0.590*** | | |
| | (0.172) | (0.154) | | |
| Payroll loan · Judicial decision | 0.357*** | 0.166 | | |
| | (0.078) | (0.128) | | |
| Log(number of operations) | –0.154** | –0.017 | –0.309* | 0.068 |
| | (0.071) | (0.050) | (0.181) | (0.060) |
| Log(average size operation) | 0.071 | –0.056** | 0.102 | –0.020 |
| | (0.061) | (0.026) | (0.157) | (0.026) |
| Dummy robust | | | –0.607*** | –0.296* |
| | | | (0.175) | (–0.184) |
| Payroll loan · Dummy robust | | | (0.122) | (0.181) |
| No. of observations | 543 | 993 | 626 | 667 |

*Source:* Banco Central do Brasil.

*Notes:* Dependent variable: dummy for average risk above median. Logit marginal effects estimates. Robust standard errors in parentheses. Control group: Loans without payroll deduction. Weighted by size of banks operation. Bank and month dummies included. Judicial decision taking effect on July/2004 (month 12).

[a]Dummy if month > 13.

[b]Dummy if month > 24.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

the court decision. The probability of the average risk on the banks' portfolio being above the median decreases over the subsample period for both loans (coefficient on Judicial Decision, –0.391). However, it decreases much less for payroll deduction loans, only –0.184. Expansion in the number of operations is associated with less risk (a 1 percent increase in the number of operations decreases the probability of being above the median in roughly 15.4 percent), which is likely to indicate that a larger number of operations (and probably a lower average size) provide better diversification, although this result is not robust to different subsamples.

Although month-specific dummies are included, it can always be the case that, for some unaccounted reason, risk perception was decreasing less for payroll deduction loans, relative to plain personal loans, and this had nothing to do with the court ruling. For this reason, we first expand the period under consideration to all months after the law regulating payroll loans passed through congress. If the estimated difference-in-differences had nothing to do with the judicial decision, one would expect that the estimated coefficient on the interaction term to remain somewhat constant. As one can see in column (2), this is not case. Expanding the sample makes the "effect" of the judicial decision decrease by half, and it is no longer statistically significant, although the sample is almost twice the size. Additionally, faux treatment dummies are specified to check whether the same pattern occurs if we consider artificial treatment dates. In column (4), the fake treatment is month twenty-five, and the sample is restricted on purpose to exclude the months before the judicial decision. The estimated fake difference-in-differences coefficient has a reverse sign, and it is well estimated. If anything, the discrepancy between standard and payroll loans was the opposite for this subsample. Finally, the fake treatment period is put in month fourteen, and the sample is restricted to months before the judicial decision (column [3]). Again, the coefficient has the opposite sign, that is, risk increases in *standard* loans relative to payroll loans in this subsample with a fake treatment period at fourteen. Most likely, this captures the effect of the bill regulating payroll loans passing through congress.

Results are similar when risk is measured by the average risk rating on the banks' portfolio (see table 5.3). There are two differences though. First, we difference the log of the data to eliminate for fixed effects.[33] Second, with average risk rating as the dependent variable, one has to account for the possibility that the dependent variable has persistence over time. For this reason, several different specifications are applied. First, an OLS model is used in which the first and second lags of the dependent variable are included as explanatory variables. The standard errors of the estimated coefficients are corrected for between-panel correlation and within-panel autocorrelation. Again, banks' risk perception on payroll loans increased relative to standard loans: the estimated coefficient on the difference-in-differences parameters is 0.014, and it is significant at the 1 percent level (column [1]). Economically, risk perception increased in payroll loans by roughly 1.4 percentage points. In column (2), a model for the dynamics of the errors term is imposed, and the parameters are estimated by a feasible generalized least squares (FGLS) procedure. The results for the parameter of interest ($\beta_3$) are exactly the same.

There is, however, the possibility that there is persistence both in the process of the dependent variable and the unobserved factors that affect risk (the error term). Columns (1) and (2) suggest the second lag of the $\Delta$ log (Average Risk) does not belong to the equation. Therefore, it arises as a

33. This is tantamount to controlling for fixed effects and should be the preferred procedure. When the dummy for risk above median is used as a dependent variable, it is not natural to first-difference the data, and therefore bank dummies are included. See Wooldridge (2002).

Table 5.3 **Risk equation: Results 2**

| | Subsample | | | | | |
|---|---|---|---|---|---|---|
| | Month >Feb/04 and <Dec/04[a] (1) | Month >Feb/04 and <Dec/04[d] (2) | Month >Feb/04 and <Dec/04[e] (3) | Month >Feb/04[d] (4) | Month >Sep/03 and <July/04[b,d] (5) | Month >July/04[c,d] (6) |
| Payroll loan | –0.010** | –0.006** | –0.012** | –0.013* | 0.002 | 0.002 |
| | (0.004) | (0.003) | (0.005) | (0.007) | (0.004) | (0.006) |
| Judicial decision | 0.009* | 0.007 | –0.001 | 0.001 | | |
| | (0.005) | (0.005) | (0.007) | (0.009) | | |
| Payroll loan · Judicial decision | 0.014*** | 0.014*** | 0.009 | 0.006 | | |
| | (0.006) | (0.005) | (0.006) | (0.008) | | |
| Δlog(average risk)$t-1$ | 0.544*** | 0.499*** | 0.497*** | 0.493*** | 0.460*** | 0.505*** |
| | (0.101) | (0.030) | (0.030) | (0.009) | (0.016) | (0.012) |
| Δlog(average risk)$t-2$ | 0.008 | 0.002 | | | | |
| | (0.101) | (0.024) | | | | |
| ΔLog(number of operations) | 0.073 | 0.065*** | 0.073*** | 0.025*** | 0.037*** | 0.022*** |
| | (0.090) | (0.010) | (0.010) | (0.005) | (0.006) | (0.007) |
| ΔLog(average size operation) | 0.103** | 0.008*** | 0.018*** | 0.002** | –0.000 | 0.002** |
| | (0.004) | (0.002) | (0.004) | (0.001) | (0.001) | (0.001) |
| Dummy robust | | | | | 0.002 | –0.015* |
| | | | | | (0.005) | (0.009) |
| Payroll loan · Dummy robust | | | | | –0.006 | –0.017** |
| | | | | | (0.004) | (0.008) |
| No. of observations | 543 | 543 | 543 | 993 | 626 | 667 |

*Source:* Banco Central do Brasil.

*Notes:* Dependent variable: Δlog( average risk). Robust standard errors in parentheses. Control group: Loans without payroll deduction. Weighted by size of banks operation. Month dummies included. Judicial decision taking effect on July/2004 (month 12).

[a]OLS estimates, with standard error of estimated coefficients corrected for between panel correlation and within panel autocorrelation using the Praiss-Winsten procedure.

[b]Dummy if month > 13.

[c]Dummy if month > 24.

[d]Feasible generalized least squares with AR(1) model for within panel autocorrelation.

[e]IV estimates with Δlog(average risk)$t-2$ as instrument for ΔLog(average risk)$t-1$.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

natural instrument for $\Delta$ log (Average Risk)$_{t-1}$ under the identifying assumption the error term has only one period persistence.[34] Now there is not enough independent variation to estimate the parameter of interest: the *p*-value of estimation is roughly 13 percent. The difference-in-differences coefficient is, nonetheless, still positive, although with a lower magnitude (0.009). Columns (4) to (6) present the same robustness checks as in table 5.3. Results and corresponding interpretations are qualitatively similar.

Results could be driven by two factors unrelated to the STJ ruling but implied by heterogeneity in the dynamics of the treatment and control groups. First, as table 5.1 shows, standard loans are, as expected, riskier than payroll loans. If there are general institutional advances in credit markets during the period and if there are decreasing returns in risk improvement, then one should observe a decrease in riskiness of standard vis-à-vis payroll loans because the former started at a higher level of risk. However, if this was the case, one would expect that the same pattern would emerge for all subsamples of whole period. As columns (3) and (4) in table 5.2 indicate, risk perception on payroll loans *decreases* vis-à-vis standard loans in the periods before and after the STJ ruling. The same is true in table 5.3 (columns [5] and [6]).

Second, as figure 5.4 shows, payroll loans boomed during the period, possibly due to the approval of the December 2003 law. Expansions might be risk-increasing, that is, the marginal borrower may be worse than the inframarginal ones. If this is the case, the pool of borrowers on payroll lending would be changing, compared to standard lending, in such a way that would produce the result regardless of the court ruling. There are, however, at least two reasons why this story cannot rationalize the results. First, the number of operations is controlled for. In table 5.3, for example, changes in the log of average risk are explained by the court ruling with variation above and beyond changes in log of number and average size of operations. Indeed, because the model is in first differences, results are not only controlled for the fact that larger banks might have lower risk borrowers, but also for within-bank expansions of payroll vis-à-vis standard operations. Second, the same argument as in the last paragraph applies. Figure 5.4 shows that payroll operations rose, relative to standard ones, *throughout the period.* Hence, if the changing pool of borrowers argument would apply, one should verify the same increase in riskiness of payroll vis-à-vis standard operations *throughout the period.* As columns (3) and (4) in table

34. Exactly because the second lag does not appear to be a explanatory variable, using further lags as instrument would not be warranted because they do not arise naturally as shifts to the endogenous variable that are not related to the unobserved determinants of risk perception (the error term). As with any identifying assumption, it is impossible to verify it empirically. Because the data is in the first difference of logs, there is no compelling reason why adjustments to unobserved shocks to risk would take more than a month to be incorporated to the banks' credit rating decision.

5.2 and columns (5) and (6) in table 5.3 show, this does not seem to be the case.

### 5.5.2 The Quantity Equation

The results for the quantity equation are presented in tables 5.4 and 5.5. Column (1) of table 5.4 presents the simplest possible model: OLS omitting $\Delta$ log (Amount of Loans)$_{t-1}$ as an explanatory variable and no period dummies. As expected, operations of payroll loans are larger (6.5 percent more), and quantities of both standard and payroll loans appear to be increasing over time (coefficient on Judicial Decision, 3.8 percent on average in the subperiod between February 2004 and October 2004), as figure 5.4 suggested. Despite the markedly different slopes of standard and payroll loans, the judicial decision did have a negative effect on payroll loans: relative to standard loans, payroll loans decrease when one compares before and after the court ruling. Indeed, after controlling for average risk, payroll loan quantities decreased 5.8 percent, between the five-month subperiod before the court ruling and the five-month subperiod after the ruling. Inclusion of period dummies hardly changes the results (column [2]). Results are, however, slightly different when the lag of the dependent variable is included: one can see (column [3]) that part of the difference-in-differences coefficient was capturing some variation of the $\Delta$ log (Amount of Loans)$_{t-1}$. Results, however, remain considerably similar.

The presence of the lag of the dependent variable poses again the challenge of searching for exogenous variation to estimate the coefficient associated with $\Delta$ log (Amount of Loans)$_{t-1}$ as there could also be persistence on the error term. Again, we follow the strategy of using the second lag ($\Delta$ log [Amount of Loans]$_{t-1}$) as an instrument. Columns (4) and (5) of table 5.4 present the results. It does appear that part of the estimated coefficient in columns (1) to (3) are unduly capturing variation due to omission of explanatory variables (which are in the dynamics of the error term). The effect, however, still survives: in the most unfavorable specification, there is 3.7 percent difference in the trends of standard and payroll loans when periods before and after the court ruling is considered. This result is not terribly well estimated, but one could reject the null that it is zero at the 5.8 percent level (column [5]).

Table 5.5 presents different specifications. In columns (1) and (3), standard error estimates are corrected for between-panel correlation and within-panel autocorrelation. Notice that the estimates of the difference-in-differences parameters are even more precisely estimated. When a FGLS procedure is used, results are similar (column [2]). These results do not account for the possible omitted variable bias due to the presence of $\Delta$ log (Amount of Loans)$_{t-2}$ but do suggest that the statistical significance in table 5.4 is not due to underestimation of standard errors. Column (4) of table 5.5 checks the robustness of the results in the same spirit as in tables

Table 5.4 **Quantity equation: Results 1**

| | Subsample | | | | |
|---|---|---|---|---|---|
| | Month >Feb/04 and <Dec/04 (1) | Month >Feb/04 and <Dec/04 (2) | Month >Feb/04 and <Dec/04 (3) | Month >Feb/04 and <Dec/04: IV estimates[a] (4) | Month >Feb/04 and <Dec/04: IV estimates[a] (5) |
| ΔLog(amount of loans)$t-1$ | | | 0.346*** | 0.580*** | 0.586** |
| | | | (0.105) | (0.225) | (0.237) |
| Payroll loan | 0.065*** | 0.065*** | 0.044*** | 0.033* | 0.032* |
| | (0.017) | (0.017) | (0.014) | (0.019) | (0.019) |
| Judicial decision | 0.038** | 0.026 | 0.035** | 0.034** | 0.077* |
| | (0.016) | (0.018) | (0.015) | (0.016) | (0.041) |
| Payroll loan ·Judicial decision | –0.058** | –0.058** | –0.045** | –0.038** | –0.037* |
| | (0.024) | (0.025) | (0.021) | (0.022) | (0.022) |
| ΔLog(average risk) | 0.077 | 0.026 | 0.072 | 0.079 | 0.029 |
| | (0.468) | (0.446) | (0.437) | (0.420) | (0.404) |
| ΔLog(average risk)$t-1$ | –0.308* | –0.287 | –0.289 | –0.225 | –0.183 |
| | (0.180) | (0.202) | (0.187) | (0.218) | (0.240) |
| Date dummy? | No | Yes | No | No | Yes |
| No. of observations | 507 | 507 | 507 | 507 | 507 |

*Source:* Banco Central do Brasil.

*Notes:* Dependent variable: Δlog(amount of loans). OLS estimates. Robust standard errors in parentheses. Control group: Loans without payroll deduction. Weighted by size of banks operation. Bank dummies included. Judicial decision taking effect on July/2004 (month 18).

[a]Instrument: second lag of ΔLog(amount of loans).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

**Table 5.5 Quantity equation: Results 2**

| | Subsample | | | |
|---|---|---|---|---|
| | Month >Feb/04 and <Dec/04[a] (1) | Month >Feb/04 and <Dec/04[b] (2) | Month >Feb/04 and <Dec/04[a] (3) | Month July/04[c] (4) |
| ΔLog(amount of loans)$t$ – 1 | 0.222 | 0.150*** | 0.189 | |
| | (0.118) | (0.054) | (0.182) | |
| Payroll loan | 0.052*** | 0.057*** | 0.053*** | 0.036* |
| | (0.017) | (0.019) | (0.017) | (0.021) |
| Judicial decision | 0.035** | 0.057*** | 0.057*** | |
| | (0.017) | (0.022) | (0.019) | |
| Payroll loan · Judicial decision | –0.051*** | –0.053** | –0.052*** | |
| | (0.018) | (0.024) | (0.018) | |
| ΔLog(average risk) | 0.033 | 0.067 | 0.003 | –0.082 |
| | (0.144) | (0.093) | (0.151) | (0.103) |
| ΔLog(average risk)$t$ – 1 | –0.280** | –0.273*** | –0.278* | –0.161* |
| | (0.141) | (0.093) | (0.154) | (0.098) |
| Dummy robust | | | | 0.020 |
| | | | | (0.021) |
| Payroll loan · Dummy robust | | | | 0.015 |
| | | | | (0.034) |
| Date dummy? | No | Yes | Yes | Yes |
| No. of observations | 507 | 507 | 507 | 665 |

*Source:* Banco Central do Brasil.

*Notes:* Dependent variable: ΔLog(amount of loans). OLS estimates. Robust standard errors in parentheses. Control group: Loans without payroll deduction. Probability-weighted by size and bank operation. Bank dummies included. Judicial decision taking effect on July/2004 (month 18).

[a]Standard error of estimated coefficients corrected for between panel correlation and within panel autocorrelation using the Praiss-Winsten procedure.

[b]Feasible generalized least squares (FGLS) assuming errors within panels follow an AR(1) process.

[c]Dummy robust = 1, if month > 18, most favorable model: FGLS assuming errors within panels follow an AR(1) process.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

5.2 and 5.3: it appears that the estimated difference-in-differences coefficient is not due to a long-term pattern over the whole sample period. When the fake treatment period twenty-five is used and the sample is restricted to after the court ruling, the results disappear. Similar robustness results hold for the whole period and for only the period before the court ruling.

### 5.5.3 The Pricing Equation

The effect of the court ruling on the interest rates of payroll loans can be found in table 5.6. A couple of comments are necessary. Different from the quantity regression, the number of operations and the average size of the

**Table 5.6 Pricing equation: Results**

| | Subsample | | | | |
|---|---|---|---|---|---|
| | Month >Feb/04 and <Dec/04[a] (1) | Month >Feb/04 and <Dec/04[a] (2) | Month >Feb/04 and <Dec/04 (3) | Month >Feb/04 and <Dec/04[a] (4) | Month >July/04[a,b] (5) |
| ΔLog(interest rate)$t-1$ | 0.019 | 0.008 | 0.008 | | |
| | (0.193) | (0.193) | (0.039) | | |
| Payroll loan | –0.063*** | –0.061*** | –0.061 | –0.062*** | –0.019 |
| | (0.020) | (0.020) | (0.047) | (0.021) | (0.025) |
| Judicial decision | –0.075*** | –0.143*** | –0.143*** | –0.095*** | |
| | (0.028) | (0.035) | (0.063) | (0.031) | |
| Payroll loan · Judicial decision | 0.073*** | 0.071*** | 0.071 | 0.071*** | |
| | (0.024) | (0.024) | (0.060) | (0.026) | |
| ΔLog(average risk) | –0.464 | –0.475 | –0.475 | –0.480 | 0.408 |
| | (0.315) | (0.334) | (0.273) | (–0.325) | (0.570) |
| ΔLog(average risk)$t-1$ | 0.430** | 0.410* | 0.410* | 0.423** | 0.363 |
| | (0.221) | (0.239) | (0.379) | (0.179) | (0.582) |
| Log(average size of operation)$t-1$ | –0.004 | –0.016 | –0.016 | –0.011 | –0.134 |
| | (0.098) | (0.103) | (0.182) | (0.101) | (0.087) |
| Log(number of operations)$t-1$ | 0.049 | 0.045 | 0.045 | 0.045 | –0.036*** |
| | (0.024) | (0.052) | (0.044) | (0.053) | (0.013) |
| Dummy robust | | | | | 0.080* |
| | | | | | (0.47) |
| Payroll loan · Dummy robust | | | | | 0.057 |
| | | | | | (0.058) |
| Date dummy? | No | Yes | Yes | Yes | No |
| No. of observations | 507 | 507 | 507 | 507 | 665 |

*Source:* Banco Central do Brasil.

*Note:* Dependent variable: ΔLog(interest rate). See table 5.4 notes.

[a]Standard error of estimated coefficients corrected for between panel correlation and within panel autocorrelation using the Praiss-Winsten procedure.

[b]Dummy = 1 if month > 24.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

operation are included. We do so because there might be (dis)economies of scale involved in granting loans. Both variables are lagged one period to mitigate the possibility of capturing demand side effects. Second, it is important once again to emphasize that the data on prices is problematic, especially for interpretation on levels. Taking the log and first-differencing the data ameliorate somehow the problems with levels but do not solve it. Interpretation on changes, however, is less troublesome, and we proceed by doing so, especially as the results with interest rates are consistent with the results on quantities and risk perception.

Column (1) of table 5.6 shows the OLS results when the lag of the dependent variable is included, but not the period dummies. Consistent with the quantity and risk perception results and with the perception of important market participants, the court ruling appears to have induced an increase in the interest rate charged on payroll loans. After controlling for number of operations, average size of operations and risk, there is a marked difference (7.3 percent) between the trends of interest rates on payroll and standard loans before and after the court ruling. Consistent with the general perception in the market, interest rates on payroll loans are lower than those on standard loans (6.3 percent). Estimates suggest risk perception does indeed affect interest rate as expected: while one cannot reject the null hypothesis that contemporaneous changes in risk perception affect interest rates, one-period lagged increases in risk perception does induce an increase in prices of loans. After standard errors of estimation are corrected for between-panel correlation and within-panel serial correlation, the lag of the dependent variable does not appear to belong to the equation. This renders results less vulnerable to dynamic panel bias.

Columns (2), (3) and (4) of table 5.6 present slightly different specifications. Most noteworthy is column (3), in which the OLS standard errors of estimation are not corrected. Here, one cannot reject the null hypothesis that there are not differences between standard and payroll loans with respect to the court ruling. The estimates suggest that correction on the standard deviation provides better (more precisely) estimates for the difference-in-differences parameter. Column (5) of table 5.6 presents the same robustness check as in all other tables, and it is again consistent with the previous results.

## 5.6 Conclusion

The results in this paper suggest the conjecture of some market participants that the June 2004 court ruling had an adverse effect on the market performance of payroll loans. Results arise and are consistent among each other for risk perception, quantity of loans, and interest rates, with the data caveat for the latter. Data suggests that the ruling increased risk perception on payroll loans, which in turn led banks to restrict quantity and increase interest rates.

These results are far from obvious. Several key market players anticipated them, but not all. It could have been that lenders had ignored the ruling. As figure 5.4 eloquently suggests, the court ruling did not prevent the boom of payroll loans. It did, however, abate it, and made it such that terms to borrowers were worse.

This paper provides some evidence on the missing link of the institutions-economic performance nexus literature: the micro evidence. Far from contradicting the literature, our results corroborate it with evidence drawn from the unit of decision making: lenders in this case. It reinforces the policy recipes already implied by the literature. Better protection from expropriation most likely increases general welfare, as it improves market performance in informationally and incentive problematic markets, such as the credit market.

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## Comment Renato G. Flôres Jr.

This paper deals with an interesting problem, relevant both to the theoretical debate and to policy setting, the latter not only in present day Brazil. Though the authors' views on the theoretical impact of their results are somewhat enthusiastic, their effort to resort to microdata is most welcome, as well as the use of recent, program evaluation econometric techniques in a (micro) finance context. My comments will concentrate on this last point as, unfortunately, in spite of its virtues, the paper leaves several unanswered questions as regards methodological and econometric aspects related to their use of the differences-in-differences (DD) estimation technique.

First, however, I'm obliged to come to a minor point. The paper is not very friendly to someone who wants to understand what has actually been done. Different time lengths *seem* to have been used for the computations, labeling the months by ordinals is confusing (to my surprise, July 2004 is 18, not 19 . . . ), equations aren't numbered ("controls" are mentioned after each equation), figures have poor labeling, and the technical explanations suffer from a few black holes.

Ideally, as known, the methodology of the DD estimator requires perfect matching between the two populations, but for the treatment, and clearly defined *before* and *after* treatment periods. The matching population to the payroll loans (PLs) one was that of standard loans, and, notwithstanding the reasoning in section 5.4.1, it is not evident that, but for the July 2004 ruling, both populations suffered the same influences. Without entering into the more conceptual issue that the two types of *borrowers*—the ultimate *observational unit*—are probably very different in socioeconomic terms and so don't match, even accepting *loans* as the observational unit, questions arise: Were there not other, specific shocks to standard loans? What is the percentage of movers between the two popu-

Renato G. Flores Jr. is Professor in the Graduate School of Economics, Fundação Geteulio Vargas, Rio de Janeiro, Brazil.

lations, especially given the different stages in PL implementation? Perhaps the answers are on the safe side, but the authors should elaborate more on them. This point is enhanced by the fact that the final sample of banks may easily be biased, and statistics on the structure of the sample are rather incomplete.

Turning to the before and after periods, the impact of the ruling may have had a lag and even an anticipation effect. Combining this with the December 2003 legislation—also a treatment: the powerful Caixa Econômica Federal started to accept individual PLs in May 2004—better support and evaluation is needed for the two periods chosen.

As for the econometrics itself, monthly differences—justified on the grounds of eliminating idiosyncratic effects in the panel at stake—are used for the dependent variable in the interest rate and total loans cases. Then the linear DD model is directly specified for such differences in order to make possible the inclusion of the three classical, required dummies (otherwise, they would vanish when taking the differences). This raises interpretation problems not only in the very specification of the models—which proceeds as if the dependent was in levels—but on the meaning of the ultimate DD expectation itself. The same ambiguous treatment applies to the residuals, though some care is sometimes shown with their correlations. In this particular, the authors seem oblivious of the issues raised in the key contribution by Bertrand, Duflo, and Mullanaitan (2004), and I have some difficulty in explaining a series of values like those in table 5. 6, for instance. In overall terms, the nearly striking results often obtained for the DD coefficients—not supported by the informal analysis of stylized facts and trends—couldn't be an outcome of underestimated standard deviations as, beyond other problems, they might have incurred?

I think questioning along these lines qualify the paper as suggestive, but in order to be trusted certainly demand a more careful and methodologically attentive text.

## Reference

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