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Abstract

In December 2003, the Brazilian congress passed a law that led to a natural personal lending experiment. The law allows banks to offer loans with repayment through automatic payroll deduction, which, in effect, turns future income into collateral. We estimate the impact of the new law using auto loans as a control group. The law has caused a reduction in interest rates and an increase in the volume of personal credit.

KEY WORDS: Credit markets, collateral, difference-in-differences.

JEL CODES: G21; D01; C33; K00; E44.

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1. Introduction

In recent years, the development economics literature has established a causal link between financial development and economic performance at the macro level (King and Levine [1993], Levine and Zervos [1998], Levine, Loayza and Beck [2000]). Evidence also suggests that sound institutions explain financial deepening (Barth et al. [1983], La Porta et al. [1997, 1998], Djankov et al. [2007], Costa and De Mello [2008]). From a policy perspective, it is crucial to understand, both theoretically and empirically, which institutions matter for financial development. This paper contributes to this understanding by documenting the importance of strong collateral in explaining financial deepening. We document that the use of future income as collateral caused a large surge in personal loans in Brazil. Our results suggest that policies that strengthen collateral have a major impact on lenders' ability to underwrite, and they thus improve borrowers' access to finance.

In December 2003, the Brazilian Congress passed new legislation regulating the legal status of payroll lending, which consists of personal loans for which the principal and interest payments are directly deducted from the borrower's payroll check. In practice, payroll loans turn future income into collateral. The law regulates the procedures through which commercial banks underwrite payroll loans to private-sector employees[1] and to those receiving social security benefits from the *Instituto Nacional do Seguro Social* (INSS), the federally run pay-as-go pension system. Among other regulations, the law mandates that the principal and interest amount to no more than 30% of the borrower's income. INSS beneficiaries constitute the largest market for payroll lending (roughly 50% in 2008), and the law mandates that banks need to be chartered by the INSS to lend to recipients of social security benefits. The chartering process started in April 2004, and banks were chartered at different points in time.

Theory predicts that payroll lending will shift the supply of credit through different channels. The income stream of retirees and public servants is stable, so future income is a valuable guarantee in case of involuntary default. Lenders are left mostly with individual idiosyncratic risk such as death, which is largely diversifiable. In addition, the fact that the borrower loses part of her income in case of delinquency eliminates a significant part of the

[1] Public sector workers had already been eligible for payroll loans since 1991.

incentive for strategic default (over-borrowing is limited by law).[2] In summary, the presence of strong collateral mitigates moral hazard problems and reduces the consequences of involuntary default, thus reducing the costs of underwriting loans. Consequently, the supply of personal credit from chartered banks shifts outward. We call this the "direct effect" of the payroll loan law. The equilibrium response of non-chartered banks is theoretically ambiguous. On the cost side, the marginal costs of non-chartered banks either remain constant or increase, depending on the elasticity of the supply of funds for personal loans and on a composition effect (better borrowers now take payroll loans). On the strategy side, the response depends on whether the choice variable is a strategic substitute or a complement (i.e., prices or quantities, see Bulow et. al. [1983]). Despite the ambiguity, we argue in Section 3 that most theoretical models predict that the "indirect effect" is negative (i.e., unchartered institutions reduce quantities in equilibrium). Because personal loans are a homogeneous good, theory predicts that interest rates will drop across the board. In addition to measuring both the direct and indirect effects, we show that the aggregate impact of the law was to reduce interest rates and increase loan concessions, which is important for two reasons.[3] First, from a policy perspective, it is important to have a quantitative sense of the aggregate impact of the law because unchartered institutions are likely to reduce quantities. Second, computing the industry-level effects serves as a falsification test because most oligopoly models predict an increase in market-level quantities in equilibrium.

We explore two sources of variation to identify the impact of the introduction of the law. Payroll lending affects personal loans but not other credit categories. We use a difference-in-differences design in which car loans are the control group and personal loans are the treatment group. Both loans are collateralized, which is a desirable feature. However, personal loans are not earmarked for the purchase of a certain type of product. As we show in Section 5, other characteristics of personal and car loans are constant over time, thus making the *unconfoundedness* assumption credible (Rosenbaum and Rubin [1983]). We use personal loans in aggregate rather than payroll lending for two reasons. First, we do not have information on personal lending stratified between payroll and non-payroll loans; we only

[2] Immediately after the law passed, legal uncertainty arose about whether courts would actually enforce the deductions (Costa and De Mello [2008]). By the time the first agreements for automatic deduction were signed between banks and the INSS, most of this risk had dissipated because of a Supreme Court decision.
[3] The industry-level impact is similar to general equilibrium effects in the treatment effect literature. See Heckman, Lochner and Taber (1998) and Meyer (1995).

have information on personal loans in general. Second, from a policy perspective, the object of interest is the impact on total personal loans. The second source of variation is the fact that treatment was staggered in nature: banks were chartered to underwrite personal loans at different points in time.

Comparing payroll loans and automobile loans, we find that, after receiving the INSS charter, a typical chartered bank reduced annual real interest rates by 7.7 percentage points (from a pre-treatment level of 72 percentage points) and increased monthly concessions by R$46 million, starting from a base of R$30 million (US$27 and US$18 million, respectively). Because they were forced by competition, unchartered banks reduced both quantities and interest rates. At the industry level, the law caused an increase in lending and a reduction in interest rates. As expected, quantities increased less at the industry level.

Our findings contribute to the banking literature. Empirically establishing the importance of collateral is an elusive task because the presence of a guarantee is not exogenous. Safer borrowers may have more access to collateral, and more problematic borrowers may introduce collateral to compensate for their riskier profile. As we show in Section 5, by comparing personal and auto loans before and after chartering, our strategy recovers a more credible causal effect of collateral on the terms of lending.

The paper is organized as follows. Section 2 describes the new Payroll Lending Law. Section 3 briefly summarizes the theoretical arguments behind the direct and indirect effects. Section 4 describes the data and presents some descriptive statistics. Section 5 outlines the empirical strategy. The results are presented in Section 6. Finally, Section 7 concludes.

2. The Payroll Lending Law

Payroll lending has existed in Brazil since the establishment of Law 8,112, which was enacted in December 1990 to regulate the provision of such loans to public-sector retirees and public servants. Private-sector retirees and employees were not included in the scope of the law.[4] In September 2003, the executive branch sent congress new legislation on payroll loans (*Medida Provisória* 130), which was subsequently passed into law (Law 10,820,

[4] The stability of the future income stream is crucial for payroll deduction to be reliable as a guarantee. However, the law had little impact during the early 1990s because of macroeconomic instability that hindered the advance of financial intermediation in general.

December 2003).[5] The new law regulated the use of payroll loans, or salary consignation (called "consigned credit"), for private-sector employees and private-sector social security beneficiaries of the INSS.

The borrower's income constrains the size of payroll loans. Monthly deductions cannot be larger than thirty percent of the disposable wage or benefit, and loans must have a fixed payment during the amortization period.[6] Severance earnings can be used for the amortization of the remainder of the debt.[7] Employers have several obligations with respect to the amounts of the loans and the information that is passed on to the financial institutions and employees. For active private-sector employees, trade unions must act as an intermediary. Unions normally suggest a lender, but the employee is free to choose any financial institution.

In practice, private-sector retirees are the most important pool of borrowers. The reason for this is simple: the INSS is backed by the National Treasury, and the pension system is pay-as-you go. Thus, the lenders face sovereign risk plus an increased but diversifiable risk of death. The law mandates that the INSS authorize institutions before they can underwrite loans to retirees. Because this process took some time, the law became fully effective in April 2004, when the INSS authorized *Caixa Econômica Federal* (a federally owned S&L) to underwrite payroll loans.[8] Subsequently, *Banco de Minas Gerai*s became the first authorized private bank. As of December 2005, the INSS had chartered 44 financial institutions. Figure 1 shows the evolution of delinquency rates (loans delinquent for more than 30 days over the total stock of loans) for auto and personal loans from Jan-03 through

[5] A *Medida Provisória* (provisional measure) is a presidential decree, with the status of ordinary law, that takes effect immediately but is then subject to congressional approval or amendment. Congressional deliberation of provisional measures takes priority over consideration of other legislation. If Congress does not decide within the legal time frame, the president can reissue the measure.

[6] Disposable wage is the net of compulsory deductions, such as taxes, compulsory social security contributions, and alimony support.

[7] Severance pay comprises all accrued rights of employees on dismissal without cause. The most important item is the money on deposit in the Severance Indemnity Guarantee Fund (FGTS in Portuguese acronym). Employers must pay 8% of base pay monthly into a blocked account to establish an unemployment fund (separate from the regular unemployment insurance entitlement, which is very small and only lasts five months). Upon dismissal without cause or retirement (or for certain other reasons such as buying a home), the employee receives the money, and the employer must pay a 40% fine on top of the deposits made during the entire employment period.

[8] To determine the month in which an institution became able to underwrite payroll loans, we used the following criterion: when the date of signing of the agreement was in the first half of the month, we considered underwriting to have begun during that same month; otherwise, we considered it to have begun the following month.

Dec-08. Several important facts emerge from this picture. First, delinquency rates of auto and personal loans were following very similar trends before the chartering process began. When the chartering process began in Apr-04, 6.9% of auto loans were delinquent (more than 30 days overdue). For personal loans, this figure was 9.7%. The lower delinquency rate on auto loans explains the lower interest charged relative to personal loans before the law was enacted. The average maturity on personal loans is 10 months. Thus, the impact of the law on delinquency should take at least one year to manifest. Indeed, the delinquency rate on personal loans oscillated around 9 percentage points until mid-2006, when it started to decline steadily. In contrast, auto loan delinquency stabilized at the 7% level after mid-2006. By the end of 2007, 7.5% of personal loans were delinquent, only a little more than the percentage of delinquent auto loans. After 2007, delinquency on auto loans surpassed delinquency on personal loans.

In summary, the law reduced delinquency rates on personal loans, both in absolute terms and relative to auto loans. This change represents a reduction in the marginal cost of loan underwriting for chartered institutions. Furthermore, as of mid-2008, there was no evidence that problems had arisen due to the introduction of the law. If borrowers were excessively leveraged on personal loans, delinquency rates would have shot up by late 2008, when banks started to reduce new concessions. In fact, delinquency on auto loans, not personal loans, shot up in late 2008 as the global financial crisis unfolded.

Coincidentally or not, concessions for new payroll loans increased dramatically after the chartering process began, as seen in Figure 2, which suggests that payroll lending has been highly successful.

[FIGURE 1 HERE]

[FIGURE 2 HERE]

3. Some Theory

The law reduced the marginal cost of underwriting loans by introducing a strong guarantee. Because institutions were not chartered simultaneously (in fact, some institutions were not chartered at all during the sample period), there were two groups of institutions: chartered and unchartered. Theory predicts that the first impact of this law would be to reduce the marginal costs of underwriting for chartered institutions. For non-chartered

institutions, the marginal cost would stay constant at best. Costs could even increase because of a composition effect: borrowers with a constant stream of income, such as retires, public servants, and (to a lesser extent) unionized private-sector workers, could migrate to payroll lending. Precisely because of the constancy of income, these borrowers are safer than average. Thus non-chartered institutions could end up with a worse pool of borrowers. In summary, in most models of oligopolistic competition, the payroll lending law would result in an outward (producing more at the same prices) shift in the best response for chartered institutions. In contrast, the best response curves for unchartered institutions would either remain constant or shift inward.

The final impact of the law would depend on the strategic interactions stemming from the shift in the best response curves. Because payroll loans tend to be homogenous, and because margins are somewhat high in Brazil, it is more natural to think in terms of a Cournot model (i.e., quantities are the choice variables).[9] Because quantities are normally strategic substitutes, the best response curves are negatively sloped. An exception occurs when the firm is sufficiently large. Bulow et al (1989) show that, as long as the demand schedule is negatively sloped, having more than 50% of market share is a necessary condition for a firm to behave as a strategic complement competitor. In 2005, the number of banks underwriting personal loans was 112, and the Herfindahl-Hirschman Index (HHI) of market concentration was 770, which implies that the symmetric-equivalent number of firms was 13.[10] Thus, it is unlikely that any bank could act as a strategic complement competitor.[11]

In summary, theory predicts that chartered institutions will increase their quantities, unchartered institutions will reduce quantities, and interest rates will drop for both groups (because personal loans are a homogeneous good). The net effect depends on how strong the quantity reduction by unchartered institutions is. However, because the average marginal cost

[9] In 2005, Bradesco, the largest private bank in Brazil, had some $5.5 billion in profits, which amounted to a return on equity of 32%. This is twice the average return for European and American commercial banks. Other large private banks had similar returns. See "High Living," *The Economist*, May 18th, 2006.

[10] The symmetric-equivalent number of firms, or the effective number of firms, is defined as 10,000 times the inverse of the HHI.

[11] More precisely, quantities act as strategic substitutes if and only if the marginal revenue schedule is less negatively sloped than the demand schedule (this never happens with a linear demand schedule). Assuming that the marginal revenue is decreasing in quantity (a rather weak condition if diseconomies of scale are not too strong because, otherwise, the industry is producing too little, even for a monopolist), the firm's marginal revenue curve can only be more negatively sloped than the demand curve if the firm has more than 50% of market share. See Bulow et al (1989, pg. 500).

of underwriting drops for the industry, a Cournot model with different marginal costs predicts an increase in quantities at the industry level.[12]

4. Data and Descriptive Statistics

We used an original dataset of "call reports" from the Central Bank of Brazil. The call reports contain monthly information about the volume of new loans, interest rates, type of indexation (e.g., prefixed, interest rate indexation, exchange rate indexation and price indexation), and the type of borrower (household or firm) for all Brazilian financial institutions. Banks have to report data on maturity and default rates on a monthly basis. The data only include non-earmarked credit.[13]

We used information about two categories of consumer finance: personal and car loans. Personal loans typically involve screening by financial institutions and a prior relationship with the bank. Borrowers must have a checking account, and the bank normally deducts loan repayments whenever the outstanding balance allows this on the due date. Personal loans are divided into two categories: direct consumer credit (*crédito direto ao consumidor*, or CDC) and payroll lending. The difference is the presence of a guarantee: whereas direct credit is unsecured, the borrower's salary or social security benefit is used as collateral for payroll lending. Neither category of personal loan is tied to the purchase of any specific good, so the borrower can spend the proceeds as he or she pleases. Car loans are secured by the car. In both cases, we restrict our attention to pre-fixed-rate loans, the most common category of pricing (roughly 90%).

The data collected run from Jan-00 through Dec-06. We used monthly data, and the monthly volume of new loans was constructed by aggregating daily values. An observation consists of an institution/loan-category/month triplet. The monthly interest rate is obtained by weighting the daily rate by the daily volume of new loans underwritten by the institution. We obtained data on 112 financial institutions that offered personal loans and 57 that offered car

[12] Everything else remaining constant, the law reduces the marginal cost of underwriting loans to some borrowers (retirees) at some institutions. At most, composition effects redistribute borrowers among institutions, not changing the average cost of underwriting at the industry level.

[13] Two important credit categories are earmarked. Part of the funding of Banco do Brasil (the largest public bank) is earmarked for agricultural working capital. Funding from savings accounts, which receive special treatment in the form of implicit government guarantee, is earmarked for mortgages.

loans. We divided the sample into two subsamples. Our main subsample ran from Jan-03 through Oct-05. Out of the 112 institutions in the personal loan market, 40 were chartered by the INSS to supply payroll loans to pensioners at some point during the sample period. The main sample had 34 time-series units and 169 (57+112) cross-section units, totaling 5,746 observations. There were three reasons for the choice of the 2003-2005 sample period. The first reason pertains to potential biases in the estimate: a longer sample period increases the odds of capturing unobserved effects. This effect is particularly problematic when estimating the indirect effect because this effect is measured by estimating breaks in the times-series pattern of the data (see Section 5). We also needed data from some period of time before the chartering process began. Jan-03 is about one-and-a-half years before the chartering process began, so this choice strikes a good balance between reducing bias and having sufficient variation before the beginning of the chartering process.

Precision is another reason for the choice of the sample period. The indirect effect is an estimated coefficient on a year dummy. Thus, expanding the sample may *increase* the variance of the estimated indirect effect because it reduces the number of unchartered institutions. Again, restricting the analysis to Oct-05 struck a good balance between chartered and unchartered institutions in the sample. Finally, in Jul-06 the government imposed a cap on interest charged on personal loans, thus interfering with the market-based pricing process. Because we prefer data generated under normal pricing conditions, restricting the sample to before 2006 is a good idea.

Having said all that, we did extend the sample period from Jan-00 to Dec-06 to check whether the results were robust when the sample was modified (see Section 6.C). Beyond 2006, we are left with too few unchartered institutions to estimate the indirect effect.

Lending increased in both categories, reflecting the deepening of the Brazilian credit market during the sample period. In the pre-treatment period (Jan-03 through Mar-04), the average (across institutions) monthly volume of new personal loans was R$30 million (US$ 18 million). In the post-treatment period, the new concessions were on average R$49 million (US$29 million), a 63% increase. New auto loans increased from R$33 million to R$48 million (US$19 million to US$28 million), a 45% increase. Thus, a comparison of averages suggests that personal lending increased more than auto lending. *Mutatis mutandis*, interest rates were declining in general, reflecting an improvement in the performance of the

Brazilian credit market during the period. Again, the drop was more pronounced for personal loans, with annual real interest rates falling from 80 percentage points before the new law to 55 percentage points afterwards. The real rate on auto loans fell from 27 percentage points to 25 percentage points.[14] Thus, rates on personal loans fell significantly more than auto loan rates. The difference in levels reflects the fact that, despite the law, delinquency rates on auto loans were still lower than those on personal loans as of late 2005 (see Figure 1).

5. Empirical Strategy

We wanted to identify the average effect that the introduction of the law had on personal credit (i.e., the average impact of treatment on the treatment group). In an ideal experimental setting, we would have randomly selected some financial institutions to take payroll collateral while others remained unsecured. Unfortunately, the chartering process was not random. Nevertheless, we can evaluate how random the timing of chartering was using observables. We followed the literature on endogenous policy evaluation and estimated a log-normal duration model to study whether the timing of chartering was systematically associated with observables.[15] We ran a Tobit regression of the log number of months until chartering on the bank's characteristics, such as size (log(assets)), liquidity, deposits, leverage, and ownership (foreign/national, private sector/public sector). The data are upper-censored at the number of months until the end of the sample.[16] Table 1 shows that the time of chartering was not systematically related to institutional characteristics. This result is not definite evidence that our experiment was as good as a randomized one because chartering could still be related to unobservables.

[TABLE 1 HERE]

In the absence of a controlled randomized trial, we turned to non-experimental methods to emulate the ideal experiment. We used a difference-in-differences with fixed-effects design, which compares the outcomes in the treatment group before and after the

[14] Figures for both new loan concessions and interest rates were un-weighted averages across institutions. The purpose of these averages was to inform the regression analysis in the next Section, and they must not be viewed as measures of the aggregate impact of the law. See Section 6.D for the aggregate impact of the law.
[15] See, for example, Galiani et al (2005).
[16] For technical details on the implementation of the duration model, see Kiefer (1988).

intervention with outcomes in the control group while controlling for time-invariant heterogeneity. The inclusion of time period (month) dummies accounted for all common shocks (pure time-series). The treatment group consisted of personal loans and the control group consisted of auto loans. The cross-section unit was a pair consisting of financial institution and type of loan, and the time-series unit was a month. In summary, the identifying variation was the difference (at the bank level) between personal and auto loans, before and after adoption of the law. More specifically, we measured the **direct effect of the law** by estimating the following model:

$$y_{ijt} = c_i \times \delta_j + \varphi_t + \beta dI_{ijt} + \varepsilon_{ijt} \quad (1),$$

where y_{ijt} is the amount underwritten or the interest rate charged in category *j* (personal or auto loans) by institution *i* in month *t*. The right-hand side of the equation included the following: 1) a full set of institution/type-of-loan dummies ($c_i \times \delta_j$), which controlled for all pure cross-section-invariant unobserved heterogeneity; and 2) a full set of month dummies (φ_t) that controlled for all common shocks to the credit market, such as changes to monetary policy. We clustered observations at the institution/type-of-loan level, a standard procedure when using difference-in-differences methods (Bertrand et al [2004]). Only chartered institutions were included in the sample when estimating the direct effect.

The coefficient of interest is β, the difference-in-differences parameter associated with dI_{ijt}, a dummy variable that assumes a value of 1 for personal loans in all periods when institution *i* was chartered to underwrite payroll loans. The sample only includes banks that were chartered at some point during the sample period.

We measure the **indirect effects** of the law by estimating the following model:

$$y_{ijt} = c_i \times \delta_j + \varphi_t + \beta TREATMENT_t \times \delta_j + \varepsilon_{ijt} \quad (2).$$

Again, y_{ijt} is the amount underwritten or the interest rate charged in category *j* (personal or auto loans) by institution *i* in month *t*, $c_i \times \delta_j$ is a set of institution/type-of-loan dummies, and φ_t is a full set of month dummies. $TREATMENT_t$ is a dummy that assumes a value of 1

after time *T*. Because no "treatment" period existed for unchartered institutions, we considered different "artificial" treatment dates (T = May-04, Sept-04, Jan-05 and May-05). In this case, δ_j was again a dummy that assumed a value of 1 for personal loan concessions. We expected the impact on unchartered institutions to be stronger over time as more institutions were chartered.

Inspection of model (2) makes it clear why we preferred the sample period from Jan-03 through Oct-05. The precision of the estimate for β (the coefficient associated with $TREATMENT_t$) depends to a large extent on the number of cross-section observations (i.e., on the number of unchartered institutions). With the preferred sample, the sample is more balanced between chartered and unchartered institutions.

The fact that chartering was not random posed several additional challenges. Unobserved shocks to the car and personal lending markets could drive results. Perhaps creditor protection was improving throughout the sample period, and this improvement benefited uncollateralized personal loans more than car loans (which are collateralized by definition). Several facts suggest that time-varying unobserved loan characteristics did not drive the results. First, auto and personal loans are subject to similar aggregate demand and supply shocks, which are captured by the month dummies. Second, although it is conceivable that auto and personal loans are subject to different specific unobserved shocks, it is unlikely that different unobserved shocks would hit different banks at different times. In other words, the staggered nature of the chartering process is a strong source of identification. Third, from a demand perspective, personal and auto loans are taken out for different purposes and are thus very imperfect substitutes. Fourth, auto and personal loans are a small part of banks' balance sheets; thus, it is unlikely that they directly compete for funds at the margin. Finally, Figures 3 and 4 depict the series of interest rates and aggregate new personal and auto loans from Jan-03 through Apr-04, when the chartering process began.[17] Secular trends that were too dissimilar would have raised the suspicion that some unobservable characteristics drove the results. Visual inspection shows that the secular trends were similar in the treatment (personal loans) and control (auto loans) groups.

[17] In contrast with the figures in the descriptive statistics Section, in Figures 3 and 4, interest rates are computed by weighting institutions by their relative size in the market (auto or personal). New loan concessions are aggregated at the market level. Auto loans are lower than personal loans because fewer institutions underwrite auto loans.

[FIGURES 3 AND 4 HERE]

We also tested the equality of pre-treatment secular trends following a procedure proposed by Banerjee et al (2002). We ran separate regressions for auto and new personal loans and interest rates over the period of Jan-03 to Mar-04 (pre-treatment). We included institution and month dummies. We had three samples: chartered banks (for the direct effect), non-chartered banks (for the indirect effect). The equality of pre-treatment secular trends would be rejected if the coefficient was significantly different from zero. Table 2 shows that we cannot reject the zero-null hypothesis in any case.

[TABLE 2 HERE]

6. Results

6.A Direct Effects

Table 3 presents the estimation results of equation (1) for both dependent variables: new loan originations (R$ million per month) and interest rate (percentage points per year). The law caused chartered institutions to increase personal loans by R$46 million more than auto loans (column 1). Interest rates on personal loans fell by 7.70 percentage points more than rates for auto loans (column 2). Both effects are statistically significant at the 5% level. In summary, the law led to a shift in the supply of personal loans for chartered institutions.

[TABLE 3 HERE]

6.B Indirect Effects

Table 4 presents the estimates for equation (2) with interest rates (percentage points annually) as the dependent variable. Columns 1 to 4 report results for different pre- and post-treatment periods. The law also caused a reduction of roughly 5 percentage points in annual interest rates charged by non-chartered institutions. All of the results are statistically significant at the 5% level.

[TABLE 4 HERE]

Table 5 shows the same results for new loans. The precision of the estimates is low, with *p*-values ranging from 18% to 11%; thus, one should keep in mind the *caveats* in interpreting imprecise estimates. Nevertheless, the indirect effect suggests that the introduction of payroll loans caused a reduction in new lending by non-chartered institutions. An additional indication that the estimates capture something meaningful is the negative impact on unchartered institution quantities, which *increased* over time (from -R$15 million (US$9 million) to -R$20 million (US$12 million) per institution monthly) as more competitors were chartered, precisely as one would expect.

[TABLE 5 HERE]

The estimated indirect effect is precisely the expected strategic reaction of an oligopolist facing a competitor whose marginal costs have decreased. Because personal loans are homogeneous goods, unchartered institutions have no alternative but to reduce their prices. The impact on interest rates is immediate and roughly constant over time. The impact on quantities becomes stronger over time as more competitors are chartered. Assuming that market-level marginal revenue decreases with quantity, the optimal reaction of a firm with constant (or increasing) marginal costs is to reduce quantities.

6.C Robustness Analysis: 2000 – 2006 sample

We expanded the sample to probe the robustness of the results. We emphasize that the results are much more informative about the direct effect, due to both for bias and variance. Table 6 shows the results for the direct effect, which are very similar to those in Table 3.

[TABLE 6 HERE]

Tables 7 and 8 show the estimates for the indirect effect. Point estimates for the impact on interest rates and new concessions confirmed our expectations: unchartered institutions reduced both their interest rates and quantities. The magnitude of the impact on interest rates was somewhat reduced but was still in line with the result calculated using the 2003-2005 sample, roughly three percentage points versus five percentage points. In accordance with our expectations and the results in Tables 4 and 5, the indirect effect became stronger over time as more institutions were chartered. Precision was lower, as expected,

with *p*-values ranging from 0.25 to 0.08. Point estimates for quantities are larger than those obtained with the 2003-2005 sample because institutions chartered in 2006 were relatively small and the regression treats all observations equally. Again, the indirect effect on quantities increased over time.

In summary, the results obtained with the preferred 2003-2005 sample are not particular to that sample: when we expanded the analysis to 2000-2006, we obtained similar figures. The precision of the direct effect estimates was similar, as expected. Precision was reduced when estimating the indirect effect, which was also expected.

[TABLES 7 AND 8 HERE]

6.D Industry-level Impact of the Law

Chartered institutions expanded quantities (direct effect) and unchartered institutions restricted quantities (indirect effect). In principle, the net effect is ambiguous, but the fact that in both cases institutions reduced interest rates strongly suggests that net quantities increased.

When measuring the direct and indirect effects, we were interested in individual behavior. Thus, the unit of analysis was an institution, and all institutions were treated equally. The industry-level effect is an aggregate object. We still could have conducted the analysis at the institution level and applied weights to each institution according to its relative importance in the personal and auto loan markets. It is, however, more straightforward to aggregate across institutions, which naturally yields an industry-level object for quantities. To compute market-level average interest rates, we used the rates charged by institutions, weighted according to the total amount of auto or personal loans underwritten.[18]

Figure 5 depicts the aggregate difference in new concessions of auto and personal loans before and after the beginning of the chartering process. Figure 6 shows the same results for the industry-level interest rate.

[FIGURES 5 AND 6 HERE]

[18] The numbers on new loan concession presented in Section 5 are un-weighted averages across institutions and serve to inform the regression analysis. They are not measures of the aggregate impact of the law.

Both Figures 5 and 6 show a marked shift near the time of the law's implementation. Comparing the period before and after the chartering process began, monthly aggregate new concessions of personal loans increased by R$1.1 billion (US$0.6 billion) more than auto loans. The annual interest rates on personal loans dropped by 12 percentage points more than the rates on auto loans. Interest rates dropped markedly after the chartering process began, and then dropped again in mid-2005. New concessions increased at two distinct time points. This result is expected because, over time, more and more institutions were chartered, thus accentuating the aggregate impact of the law. The statistical significance of the aggregate impact is measured by regressing the differences in concessions and interest rates on a dummy representing the implementation of the law. Table 9 shows the results.

[TABLE 9 HERE]

The differences in the means in Figures 4 and 5 are significantly different from zero at the 1% significance level (columns 1 and 4). The figures suggest otherwise, but trends may be driving the results. Introducing linear and quadratic trends does reduce the mean differences before and after implementation of the law, but the differences are still significant both statistically and in practice (columns 2, 3, 5 and 6). The figures in Table 9 are not directly comparable to the estimates in Tables 3 and 4 because regression models (1) and (2) treated all institutions equally. Nevertheless, it is interesting to note that the magnitudes are similar. In column 6, interest rates dropped some 5.59 percentage points, which is between the 7.7 and 5 percentage points in Table 3 (direct effect) and Table 4 (indirect effect). New loan concessions in column 3 increased by R$460 million. The magnitude of the direct effect in Table 3 is R$46 million, and the magnitude of the indirect effect is - R$18 million on average in Table 4. There were 40 chartered and 72 unchartered institutions as of late 2005, respectively. Thus, the magnitude of the aggregate impact would be $40 \times 46 + 72 \times (-18) = \text{R\$}544$ million.

8. Conclusion

The introduction of payroll lending produced a large shift in the supply of personal loans in Brazil. Chartered institutions enjoyed a reduction in the marginal cost of underwriting personal loans. Much like Cournot competitors, non-chartered institutions

contracted their supply. The industry-level result is a net increase in loans and a drop in interest rates.

It is difficult to over-emphasize the importance of our results. Payroll lending is a novel collateralization instrument that has relatively low recovery costs because the guarantee is money, which is obviously the most liquid asset. Brazil is a country that scores low on creditor protection measures. In this context, a simple, credible collateral instrument is particularly valuable. The aggregate impact is strongly positive. Thus, in terms of policy, the instrument is relevant. Concerning external validity, many other countries share credit market characteristics similar to those of Brazil. Thus, it is not far-fetched to speculate that payroll lending will be a valuable instrument for financial deepening in weak creditor protection environments.

Our results do not provide insight into the longer-term effects of financial deepening based on consumer lending. These funds may be used to smooth out consumption expenses or to finance small businesses, which is an interesting avenue for future research. Evidence suggests that payroll lending is one of the major drivers in creating a strong internal consumer market in Brazil.

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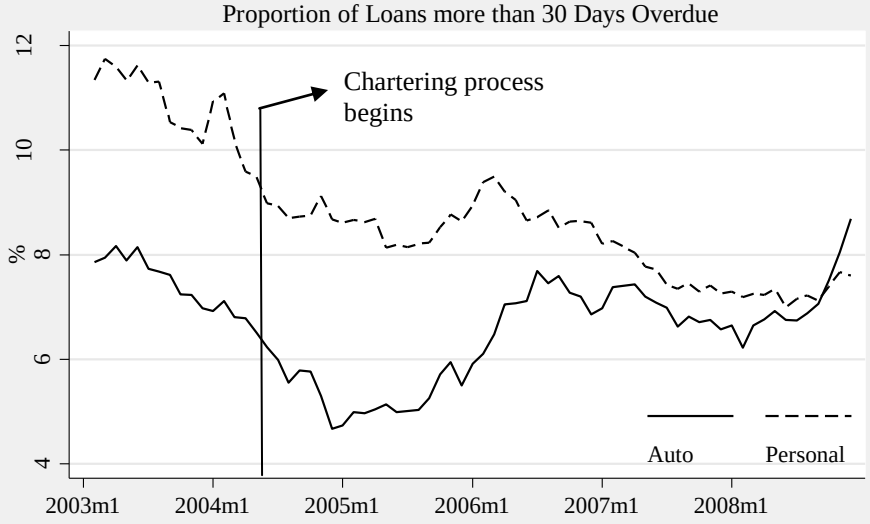
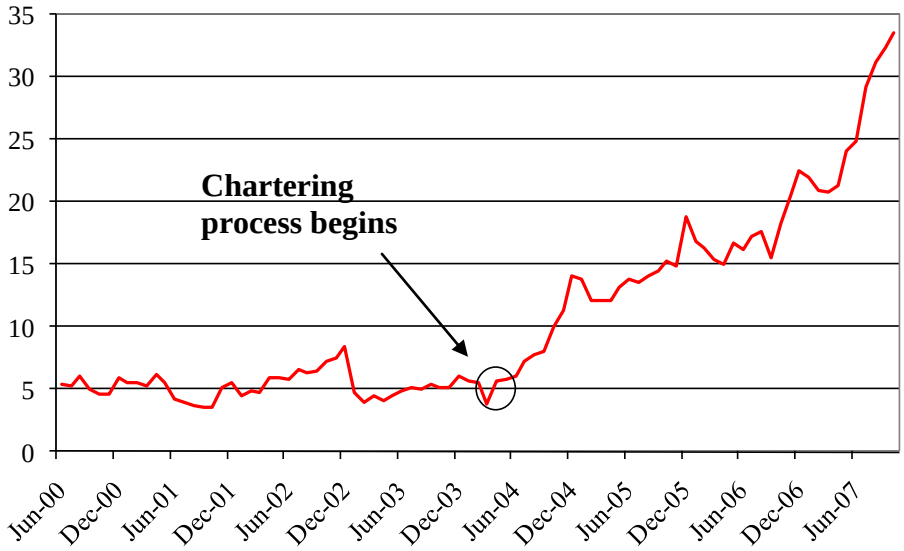
TABLES AND FIGURES

Fig. 1 Delinquency Rates : Auto versus Personal Loans



Source: Banco Central do Brasil

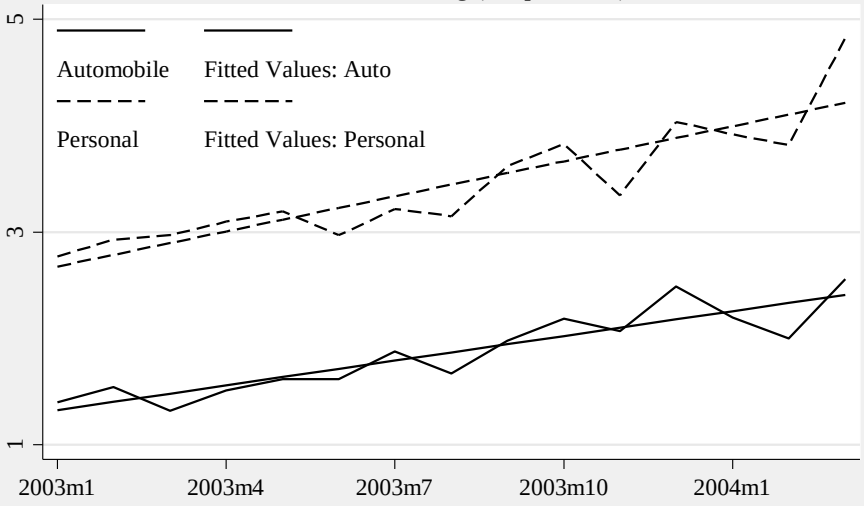
Fig. 2: Stock of Personal Loans (R$ billions)†



Source: Banco Central do Brasil. †: Total personal lending includes both payroll and non-payroll personal loans.

Fig. 3 Monthly New Loan Concessions

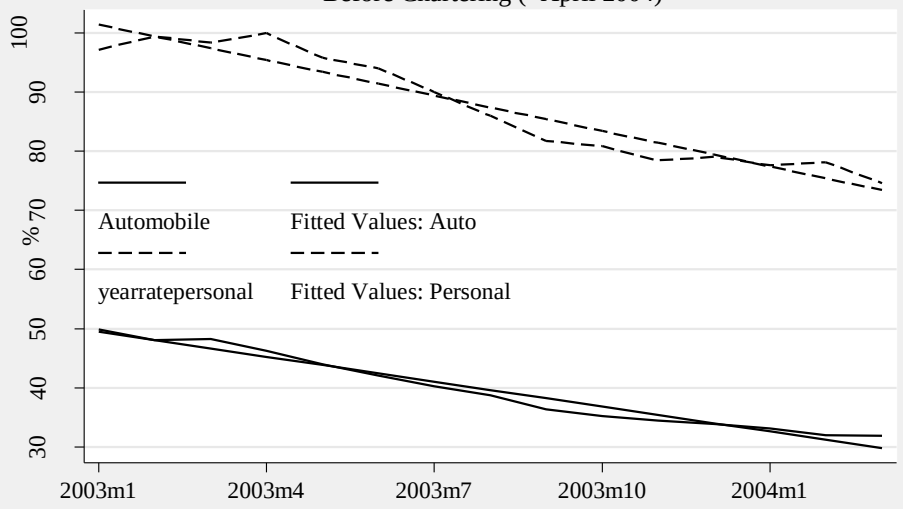
Before Chartering (< April 2004)



Source: Banco Central do Brasil

Fig. 4 Yearly Interest Rates

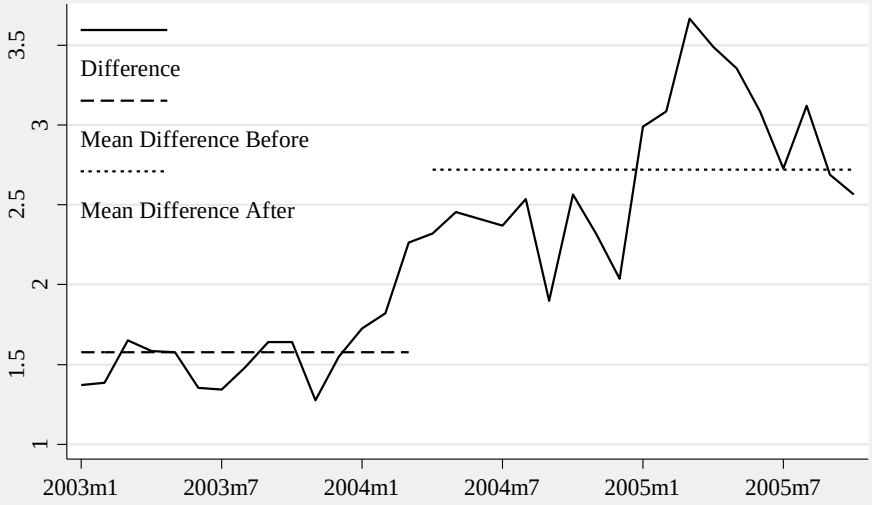
Before Chartering (<April 2004)



Source: Banco Central do Brasil

Fig. 5 Difference in Monthly New Concessions

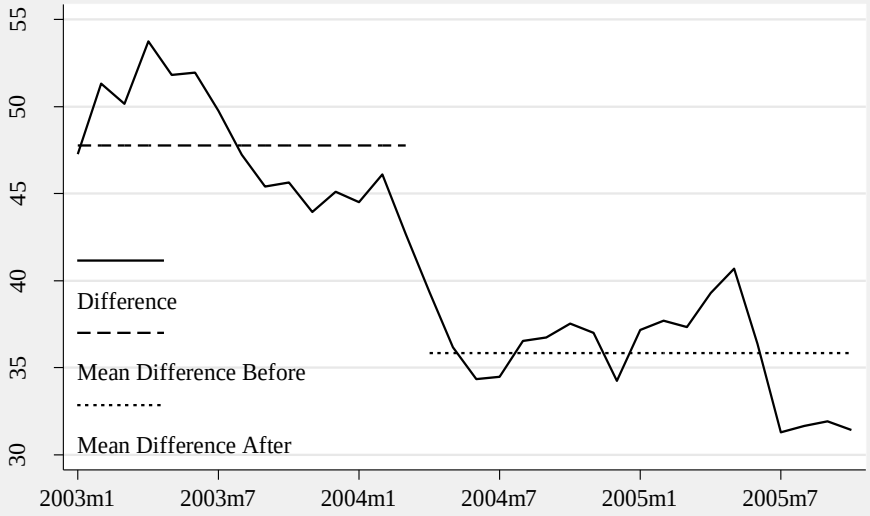
Personal Loans - Auto Loans



Source: Banco Central do Brasil

Fig. 6 Difference in Yearly Interest Rates

Personal Loans - Auto Loans



Source: Banco Central do Brasil

Table 1 - Log-Normal Duration Model for the Time until INSS Chartering

	Base date	
	Sept 2003	Dec 2003
Log(Assets)	-0.05	-0.02
	(0.03)	(0.01)
Liquidity	0.62	0.23
	(0.38)	(0.16)
Deposits/Credit	-0.42	0.10
	(1.01)	(0.53)
Leverage	-0.42	-0.39
	(0.82)	(0.43)
Dummy=1 if the bank is foreign	0.27	0.17
	(.21)	(.09)
Dummy=1 if the bank is private and domestic	-0.18	-0.03
	(.19)	(.08)
Dummy = 1 if the bank is universal	-0.22	-0.11
	(.14)	(.06)
Dummy=1 if the bank is part of a financial conglomerate	-0.06	-0.06
	(.12)	(.05)
Constant	4.6***	3.4***
	(0.6)	(0.28)
Number of observations	112	112

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates from a Tobit regression of time until INSS chartering. Upper censoring in December 2005. Liquidity = cash-in-hand+Treasury Bonds+Interbank)/assets. Leverage = Debt/Assets

Table 2 Difference Control Test (OLS Estimates)

	Chartered		Non-chartered	
	Interest rates (%)	New loans (R$ million)	Interest rates (%)	New loans (R$ million)
p -value	0.95	0.14	0.73	0.55)
Number of observations	1184	1363	1515	1680
R^2	3%	9%	3%	4%

Table 3: Difference-in-Differences Models for the direct effect

	New Loans (R$ millions, monthly)	Interest Rate (percentage points, annual)
	(1)	(2)
Personal Loans (=1)	46	-7.70%
	(20)**	(3.54)**
R^2	17%	24%
Number of observations	1793	1627

Source: Instituto Nacional do Seguro Social (INSS) and Banco Central do Brasil

* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors robust to clustering at the institution/type of loan level in parentheses

All specifications include a fixed-effects (institution/type of loan dummies), and year dummies.

Table 4: Difference-in-Differences Models for the indirect effect

Interest Rate (percentage points, annual)

	(1)	(2)	(3)	(4)
Personal Loans (=1)	-5.03	-4.91	-5.28	-5.03
	(2.18)**	(2.42)**	(2.55)**	(2.18)**
Threshold Point	May-04	Sep-04	Jan-05	May-05
R^2	16%	16%	16%	16%
Number of obs	3313	3313	3313	3313

Source: Instituto Nacional do Seguro Social (INSS) and Banco Central do Brasil

* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors robust to clustering at the institution/type of loan level in parentheses

All specifications include a fixed-effects (institution/type of loan dummies), and year dummies.

Table 5: Difference-in-Differences Models for the indirect effect

New Loans (R$ millions, monthly)

	(1)	(2)	(3)	(4)
Personal Loans (=1)	-15	-17	-17	-20
	(11)	(12)	(13)	(13)
Threshold Point	May-04	Sep-04	Jan-05	May-05
R^2	0.104	0.106	0.104	0.103
Number of obs	3625	3625	3625	3625

Source: Instituto Nacional do Seguro Social (INSS) and Banco Central do Brasil

* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors robust to clustering at the institution/type of loan level in parentheses

All specifications include a fixed-effects (institution/type of loan dummies), and year-type of loan dummies.

Table 6: Difference-in-Differences Models for the direct effect, 2000-2006

	New Loans (R$ millions, monthly)	Interest Rate (percentage points, annual)
	(1)	(2)
Personal Loans (=1)	42	-6.80
	(23)**	(3.40)**
R^2	18%	17%
Number of observations	4740	4452

Source: Instituto Nacional do Seguro Social (INSS) and Banco Central do Brasil

* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors robust to clustering at the institution/type of loan level in parentheses

All specifications include a fixed-effects (institution/type of loan dummies), and year dummies.

Table 7: Difference-in-Differences Models for the indirect effect, 2000-2006

Interest Rate (annual, percentage points)

	(1)	(2)	(3)	(4)	(5)
Personal Loans (=1)	-2.10	-2.67	-3.41	-3.54	-3.91
	(2.10)	(2.18)	(2.18)	(2.18)	(2.18)*
Threshold Point	May-04	Sep-04	Jan-05	May-05	May-06
R^2	14%	14%	14%	14%	14%
Number of obs	6672	6672	6672	6672	6672

Source: Instituto Nacional do Seguro Social (INSS) and Banco Central do Brasil

* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors robust to clustering at the institution/type of loan level in parentheses

All specifications include a fixed-effects (institution/type of loan dummies), and year-type of loan

Table 8: Difference-in-Differences Models for the indirect effect, 2000-2006

New Loans (R$ millions, monthly)

	(1)	(2)	(3)	(4)	(5)
Personal Loans (=1)	-34 (23)	-36 (24)	-37 (24)	-39 (24)	-39 (23)*
Threshold Point	May-04	Sep-04	Jan-05	May-05	May-06
R^2	0.127	0.128	0.128	0.127	0.12
Number of obs	7146	7146	7146	7146	7146

Source: Instituto Nacional do Seguro Social (INSS) and Banco Central do Brasil

* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors robust to clustering at the institution/type of loan level in parentheses

All specifications include a fixed-effects (institution/type of loan dummies), and year dummies.

Table 9: Industry-Level Impact (Newey-West Regressions)

	ΔConcessions (R$ billion, monthly)[a]			**ΔInterest Rate (percentage points, annual)[a]**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Dummy_Law[b]	1.14 (0.20)***	0.44 (0.20)**	0.46 (0.19)**	-11.92 (1.65)***	-6.21 (2.03)***	-5.59 (1.68)***
Time[c]		0.04 (0.01)***	-0.19 (0.80)		-0.33 (0.11)***	-7.96 (6.71)
Time Squared			0.00 (0.00)			0.01 (0.01)
R^2	68%	77%	77%	80%	86%	87%
Number of obs	34	34	34	34	34	34

Source: Instituto Nacional do Seguro Social (INSS) and Banco Central do Brasil

* significant at 10%; ** significant at 5%; *** significant at 1%

Standard errors robust to second-order auto-correlation in the error term.

a = Personal minus Auto Loans. Interest rates are averages weighted according to the amount of concessions underwrittern by institutions.

b = 1 if the chartering process has begun (after April-04)

c = time is a trend, i.e, time = 1, 2,…

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