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Consumer Credit Expansion in Open Economies

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Dissertation presented to the Programa de Pós-Graduação em Economia of the Departamento de Economia, PUC-Rio as partial fulfillment of the requirements for the degree of Mestre em Economia.

Advisor: Prof. Juliano Junqueira Assunção

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Abstract

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This work uses a quasi-experimental credit reform in Brazil to understand the effects of financial liberalization and the channels through which it promotes economic growth. We use panel data for all 5,500 Brazilian municipalities and exploit legal issues underlying the policy in order to quantify the macroeconomic impacts of eliminating credit barriers in an open economy. The reform appears to rise total borrowing, besides stimulating other banking services. Our findings also indicate substantial increases in wages and business profits, and the reduction of the informal sector. Furthermore, the analysis suggests the presence of important dynamic effects and that impacts are greater in more opened, wealthier, less rural and more financially developed economies.

Keywords

growth; consumer credit; financial deepening; multiplier effects;

Resumo

Mendes, K. D.; Assunção, J. J. (Orientador). **Expansão do Crédito ao Consumo em Economias Abertas**. Rio de Janeiro, 2014. 60p. Dissertação de Mestrado — Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Este trabalho utiliza uma reforma de crédito quase-experimental no Brasil para entender os efeitos da liberalização financeira e os canais através dos quais ela promove crescimento econômico. Utiliza-se dados em painel de todos os 5.500 municípios brasileiros e explora-se questões legais que envolvem essa política para quantificar os impactos macroeconômicos da eliminação de barreiras ao crédito em uma economia aberta. A reforma parece aumentar o montante total emprestado, além de estimular outros serviços bancários. Os achados indicam ainda um aumento substancial nos salários e nos lucros das firmas, e a redução do setor informal. Ademais, a análise sugere a presença de efeitos dinâmicos importantes e que os impactos sejam mais intensos em economias mais abertas, mais ricas, menos agrícolas e mais financeiramente desenvolvidas.

Palavras–chave

crescimento; crédito ao consumo; aprofundamento financeiro; efeitos multiplicadores;

Contents

List of Figures

List of Tables

1
Introduction

Although much confidence is placed in the power of financial deepening to promoting economic development, identifying macro-level causality in this relationship remains a nontrivial task, as financial interventions are usually endogenous or too small. In this sense, we investigate the aggregate consequences of eliminating major barriers to consumer credit in an open economy, by looking at an economy-wide reform taking place in multiple and diverse economic contexts. Thereby, this work poses three straightforward questions: (1) To what extent is a large-scale consumer credit expansion capable of generating growth? (2) What are the mechanisms behind this effect? (3) How do impacts vary according to the intervention's length and to different socioeconomic scenarios?

In order to answer them, we analyze the *consignado* credit service: a governmental initiative in Brazil aiming to rise quality and size of credit supply. The *consignado* is a credit mechanism intended to retirees and formal workers that attempts to reduce interest rates by eliminating the risk faced by lenders. Basically, the *consignado* allows banks to attach personal loan repayments to borrowers' income influx (salary or pension), in a way that they no longer have the chance to default. Because of its exceptional enforcement, the *consignado*-type contracts bypass the absence of collateral and, therefore, reach a vast population that would have little or no access to credit otherwise. This service was country-wide introduced in 2003 and rapidly expanded, currently representing about two-thirds of total personal loans in Brazil.

The *consignado* is a particularly interesting credit reform to investigate. First of all, it is a consumer credit service that it totally unbounded from any specific purpose, so that it is likely to stimulate various economic sectors. Also, the *consignado*'s aggregate-level relevance clears many issues as whether findings on small programs may be extrapolated to other and bigger economic environments, or if the impacts would remain the same if the program was scaled up.[1] Moreover, the *consignado* was not a specific program that afforded a credit shock only for a short period of time. Instead, it is an once-for-good credit reform that can grow wider as it becomes more familiar to both financial institutions and population. By analyzing seven years of on-treatment data, we are capable of following the service's spread over time and, indeed, explicitly investigating the relationship between the intensity of impacts and

[1] See Duflo (2004) and World Bank (2004) for an extensive discussion on this issue.

the policy's scale. Similarly, the *consignado*'s large size and long duration allow the assessment of its consequences in the long-run, which the micro-literature is often unable to observe. For instance, we may check the extent to which the effects of credit on investment and employment have also impacted wages, as a result of general equilibrium effects. Such indirect mechanisms may account for an important portion of the reform's overall impacts, and identifying them at the micro-level is of first-order importance to understanding macroeconomic engines. Finally, we assess these effects in a quite representative set up, due to the extremely diverse economic reality throughout Brazil. In terms of *per capita* income, for example, municipalities ranged from R$752 ($677 in PPP) to R$21,117 ($19,024 in PPP) in 2000, so that we could compare the poorest locations to sub-Saharan Africa, while the richest ones are similar to Portugal, Greece or South Korea.[2]

Although not a pure randomized treatment, the *consignado* was conveniently implemented in terms of identification strategy. In particular, the rules underlying the policy provide a crucial singularity with respect to pensioners and private sector retirees, who account for about thirty percent of *consignado*-type credit concessions. By law, financial institutions are required to apply for a formal permission in order to operate the *consignado* to this specific pool of borrowers. Such authorizations were assigned in a gradual and scattered way along the years, and we argue it is unlikely to relate to the relevant variables in this study. Hence, in order to explore the *consignado* as a quasi-experimental intervention, we restrict our analysis to the portion of credit that is contracted by this particular sub-group of users. These exogenous permissions are also noted in Sousa (2011), but explored in a totally different way. Sousa (2011) uses banks' demand deposit as an instrument for credit, but the existence of a direct relationship between the size of a bank and its volume of *consignado* is rather questionable, since many small institutions have precisely focused on this service. Differently, our instrumental variable design is based on a combination of both the intensive and the extensive dimension of the credit contracting. Most importantly, this framework enables us to construct robust and strong sets of instruments, capable of explaining around seventy percent of data variation.

We begin our analysis with the two-step estimation of the *consignado*'s average effects on output, formalization, income and banking. We find that implementing this reform in a given municipality can result in a sizable multiplier effect, as it yields, on average, a one to 1.96 impact of credit on the municipal GDP. When dismembering this final impact from an income

[2]Sources: Brazil's Human Development Atlas 2013 and World Development Indicators.

perspective, it shows to arise through the three different components we observe. More specifically, one extra monetary unit of consumer credit is likely to produce, on average, 1.51 units of business profit, 1.90 units of wage and 0.62 units of revenue from other sources, such as financial assets. Then, the sum of these impacts on the formal sector indicates a substantial reduction of informality, which is also observed when looking at the formal employment rate directly. Furthermore, there are evidence of further stimulus to general financial intermediation, through the opening of both public and private bank branches, and of actual increases in total borrowing, suggesting the reform does not merely relocate preexisting credit. These effects refer to the reform's aftermath, that is, the overall impacts of the municipal-level credit expansion on the economic activity of such municipality, when encompassing spillovers to non-borrowers, flow of funds from surrounding economies and other indirect effects.

As a second step, we explore the learning from our first stage regressions, namely, the fact that the credit taking rises gradually along the years following the new service's introduction. This finding is combined with a reduced form approach in order to examine the presence of varying marginal impacts of credit according to the reform's pervasion level. Indeed, impacts appear to change significantly, becoming generally stronger when increasing the size of the credit intervention. Though suggesting findings with respect to a small-scale program may be generally interpreted as a lower bound, this parsing urges caution when extrapolating results, since these impacts do not seem to increase linearly, nor proportionally and nor even monotonically.

Lastly, a heterogeneous effects analysis points out a positive correlation between the degree of local economic openness and the size of the reform's overall impacts. Moreover, all effects are greater in municipalities with higher GDP *per capita*, more *ex-ante* credit and less agriculture production in comparison to other economic sectors. Besides suggesting that the interaction with external markets results in a positive cash inflow, it implies that consumer credit multipliers are prone to be larger in richer and more sophisticated domestic economies, which we interpret in the light of a resources misallocation theory.

Despite of the *consignado*'s macroeconomic relevance, there is a small literature concerning this credit reform in Brazil and it is, to our knowledge, restricted to Coelho, De Mello and Funchal (2012), Costa and De Mello (2006), Madeira, Rangel and Rodrigues (2010) and Sousa (2011). The last two articles have looked at the impact of the *consignado* on a sample of households, focusing on its effects on seniors' health and occupational choice. In contrast,

we consider credit impacts in a much wider perspective and use population panel data on the totality of the Brazilian municipalities. On the other hand, Coelho, De Mello and Funchal (2012) and Costa and De Mello (2006) use a detailed database on credit contracts to evaluate changes in the volume and condition of loans. Though somewhat further from our scope, these two papers are, in many senses, complementary to the present work, in the effort of identifying the multiple mechanisms through which the *consignado* has reduced credit barriers.

Regarding the microfinance literature, Kaboski and Townsend (2012) is the solely other study we know of that assesses the long-term consequences of a large credit expansion. Although they look at a short-lived program in isolated villages, while we study a permanent reform in integrated economies, many of their findings on overall credit, wages and profits are consistent to ours. Nevertheless, some of their results are only marginally significant or not fully robust, and the impacts on income growth has disappeared in the long-run, possibly due to the sharp credit injection they examine. Moreover, Kaboski and Townsend (2012) and others such as Karlan and Zinman (2008) analyze consumer credit expansions that are not followed by lower interest rates, differently from the *consignado*. Burguess and Pande (2003, 2005) evaluates a long-term policy on the expansion of the financial system across rural regions in India and are closely related to our study as well. They find substantial rural poverty reduction and evidence of effects on GDP growth, but results are with respect to the opening of branches and not credit itself. Also, high default rates are a major concern in the policy they analyze, which is not the case here. In addition, our finding of great impact on profits are corroborated by Banerjee, Duflo, Glennerster, and Kinnan (2009), who investigate a randomized experiment in India and report sizable income effects on entrepreneurs.

The remainder of the work is organized as follows. Chapter 2 explains the relevant aspects of the *consignado* to our analysis and describes our database. After that, we present and discuss our empirical strategy, in chapter 3. Then, chapters 4, 5, 6 and 7 contain our results, and chapter 8 concludes.

2
Background and Data

2.1
The Credit Reform

The *consignado* is a type of personal loan that exists in Brazil since 1990. Under its initial regulation, however, the service remained restricted to public servants and public-sector retirees, and it could only be operated by state-owned banks. Besides that, the introduction of this new credit instrument has coincided with a highly adverse scenario for the Brazilian credit market in general[3], preventing the *consignado* from a significant expansion.

Short after credit services started to recover, the *consignado* was legally extended to private-sector employees and beneficiaries of the national institute of social security (INSS), which encompasses all private-sector retirees and recipients of survivor's pensions.[4] This new regulation, in 2003[5], emerged as part of a series of economic stimulus proposed in president Lula's first mandate, and the official stated aim for the intervention was to lower loans' interest rate through the elimination of default risks.

Indeed, the reform was largely successful in achieving its objective. According to Brazil's central bank data, the *consignado*'s average interest rate in December of 2012 was 24.5 percent a year while other personal loans presented an average of 68.3 percent. Moreover, the stock of *consignado* experienced 251.7 percent of growth in the period 2007-2012[6], comparing to 160.5 percent of other personal loans.

In order to clarify the reader's perception on the true relevance of the *consignado* with respect to total credit and some standard credit categories,

[3]Such background is summarized by three major facts. First, Brazil has faced a long hiperinflation period, which lasted until 1994. Second, the country's economic stabilization was followed by a period of exorbitant short-term bond interest rates, due to the Mexican (1994), Asian (1997) and Russian (1998) crisis, which culminated in a Brazilian currency crisis, in 1999. Third, the generalized distrust and uncertainty that surrounded Lula's presidential election in 2002 did not allow a significant interest rate reduction before mid-2003.

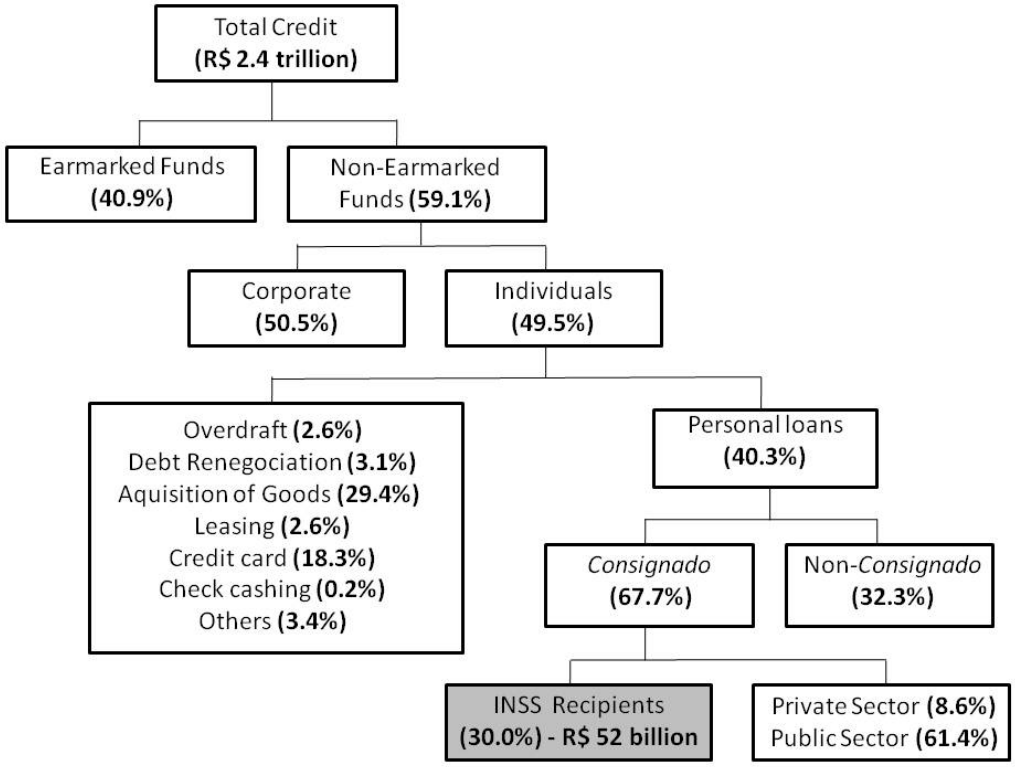
[4]The *Instituto Nacional do Seguro Social* – INSS is in charge of paying a wide variety of pensions, aids and benefits, but the possibility of contracting *consignado*-type loans is limited to retirees and recipients of survivor's pensions. As a practical matter, we refer to these two groups of beneficiaries as "INSS recipients".

[5]In more accurate terms, the extension of the *consignado* occurred first through an interim decree (Medida Provisória 130) in September of 2003 and then by a federal law (Lei Federal no. 10.820) in December of 2003.

[6]Data for an earlier period in not disclosed by Brazil's central bank due to methodological changes.

Figure I presents a summary of Brazil's credit balance in 2012. As shown in the figure, the *consignado* accounted for two-thirds of personal loans stock. Regarding the *consignado* to INSS recipients, in which we are particularly interested, its total stock was 52 billion reais (approximately 26 billion dollars) and represented thirty percent of the total *consignado*.

FIGURE I
Brazil's Total Credit Balance and Its Subcategories (2012)



Notes. Figure uses Brazil's central bank data to show the relative importance of the *consignado* to INSS recipients in terms of its participation in the Brazilian total credit balance in the year of 2012. Percentages in parenthesis refer to the portion represented by each category in terms of the category that is immediately superior.

2.1.1
Peculiarities and Practical Aspects

Banks ability to offer much lower interest rates for the *consignado* than for alternative loan contracts is due to the fact that the *consignado*'s monthly repayments are made through direct deductions from the borrower's paycheck. Differently from other credit instruments, when a *consignado*-type contract is signed, the responsibility of repaying the debt no longer rests with the borrower but, instead, lies on the employer or, similarly, on the INSS (regarding retirees and pensioners). In practice, the borrower's employer (or the INSS) deducts

the amount corresponding to the loan payment from the borrower's salary (or benefit) and pays the bank himself. The borrower, thence, only obtains the remaining portion of his income.[7] Thus, regarding employees, for example, as long as the borrower remains working for a particular company, the firm is obligated to repay the debt every month without defaulting, and it could be directly sued by the bank if refusing to do so. Anyway, this is rarely the case. Firms are very unlike to have a plausible reason to default, once the payments are entirely deducted from their payroll and they do not incur is any extra charge.[8]

From an economic perspective, we may interpret the *consignado* as an efficient way to convert the borrower's future income into solid collateral to the banks. In effect, the lender is fully insured, except in cases of bankruptcy or if the borrower leaves his job.[9] Regarding INSS recipients, the outstanding commitment offered by the *consignado* is even more evident. Once INSS payments are a lifelong right of every beneficiary, the death of the borrower is the only risk in these cases. Actually, according to Brazil's central bank data, the *consignado*'s default rate is less than 2% for INSS recipients and about 5-6% for private-sector employees, while the average default rate of other personal loans is around 8-9%.[10]

If on the one hand operating the *consignado* is very propitious to banks because borrowers basically give in their right to default, on the other hand, there are some key aspects that make it attractive from the borrowers' side as well. First, as already mentioned, the *consignado*-type contracts offer lower interest rates than any outside option. Second, the *consignado* overpasses the absence of a formal collateral and other barriers to contracting alternative types of credit. Third, the automatic deductions eliminate the necessity of proceeding with the payments manually, which is convenient to individuals that are little familiar with the banking system. Last, the *consignado*'s contracting process requires very simple documentation[11] on the borrower's side, being unlikely to select users because of complicated procedures.

By law, all borrowers are allowed to sign as many contracts of *consignado*

[7]For a better understanding of this mechanism it is important to note that the vast majority of formal wages in Brazil are payed on a monthly basis.

[8]In despite of being largely involved, the firms have absolutely no power over the contracts, in the sense that they cannot prevent the credit taking, choose financial institutions, or impose any other limitation on their own right.

[9]Even in these cases, up to 30% of the amount the employee receives when fired might be taken over by the bank, if specified in the contract.

[10]These values refer to 2012 monthly data and concern all payments delayed for 90 days or longer.

[11]Basically, the borrower just provides identity card, individual registration number (Cadastro de Pessoa Física – CPF) and payslip.

as they want, as long as the sum of the monthly repayments binds no more than thirty percent of the borrower's net salary or benefit. The *consignado*'s maximum contract length is 72 months (60 months to INSS recipients) and the interest rate, although upper bounded[12], varies according to the repayment period.

2.1.2 Quasi-Experimental Perspective

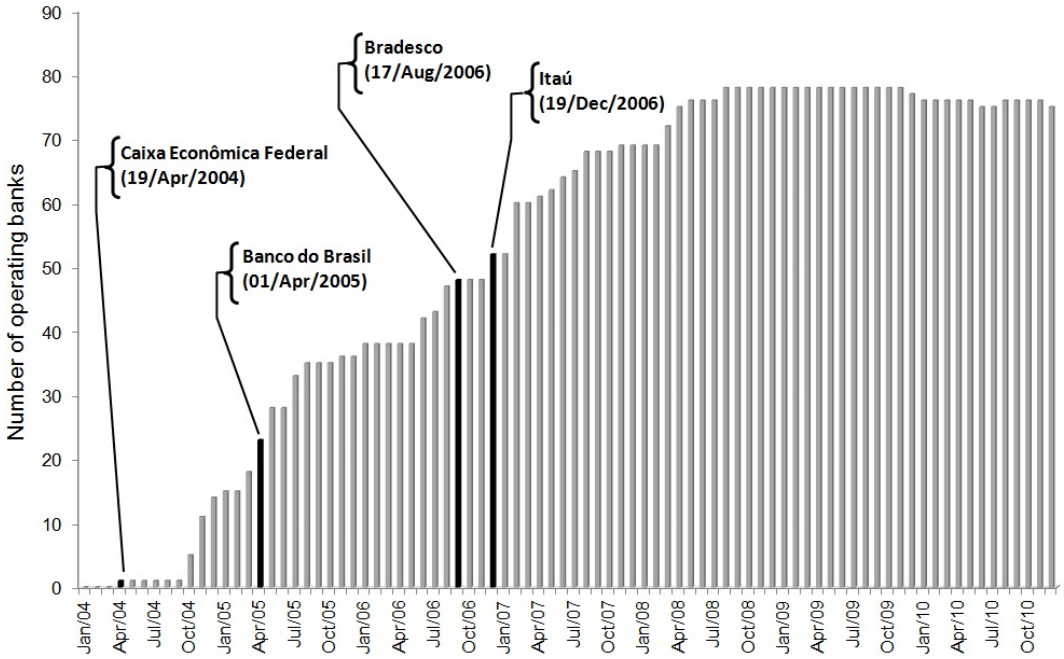
With regard to INSS recipients, it was established by the *consignado*'s new law in 2003 that the INSS itself would be in charge of further regulating the provision of this type of credit to its beneficiaries. Hence, the INSS has published several rules of procedure from which an essential feature has emerged concerning our empirical strategy. Namely, all credit institutions willing to offer *consignado*-type contracts to INSS recipients are required to sign up an official agreement first. Thus, banks did not start operating *consignado* to INSS recipients all at once when the federal regulation was approved. Besides, none of the financial institutions in Brazil owned branches in all the municipalities, meaning that, at the moment a new agreement was signed, that particular bank was potentially taking the *consignado* to municipalities were INSS recipients did not have access to the service until then.

These agreements have occurred gradually along the following years. The first permission was granted by INSS in April of 2004, when the second largest state-owned bank in Brazil (*Caixa Econômica Federal* – CEF) started to operate the service. Brazil's biggest state-owned bank (*Banco do Brasil*) signed the agreement only in April of 2005, when over 20 other financial institutions were already allowed to operate. Then, the two largest private banks in Brazil (*Itaú* and *Bradesco*) only joined the INSS's list in the second half of 2006. By December of 2010, when we end our analysis, 75 financial institutions were currently holding a permission to offer *consignado* to INSS recipients. Such process of agreements signature between 2004 (its beginning) and the end of 2010 is illustrated by Figure II.

For identification purposes, we assume that neither banks nor municipalities had somehow prepared for these agreements. In this way, we suppose that, at the time a bank started to operate the *consignado*, the distribution of branches throughout the country was an *ex-ante* decision of that institution and, therefore, dissociated from the signature. There are some *a priori* grounds

[12]This limit is periodically reviewed and, regarding INSS recipients, it is currently 34.5 percent a year.

FIGURE II
NUMBER OF FINANCIAL INSTITUTIONS AUTHORIZED TO PROVIDE *CONSIGNADO* TO INSS RECIPIENTS OVER TIME



Notes. Figure plots the evolution of the total number of financial institutions holding an agreement with INSS to provide *consignado* to its recipients, highlighting the date of signature of Brazil's four major banks. Data granted by INSS was used to compute the number of running agreements from its beginning until the end of 2010 on a monthly basis.

for believing so. First, besides the fact that opening and closing branches involve several unclear and conflicting aspects, banks are rather unlikely to significantly change their planning based on the perspective of operating the *consignado* in the future. It is more plausible that they would wait until after the permission to make any major decision. Second, INSS's criteria for choosing banks is not transparent, so that the signatures seem to be quite arbitrary and unexpected. Third, the presence of reverse causality is rather implausible, having that a particular location, in a universe of 5,500 municipalities, is probably too small for stimulating an agreement by itself.

This empirical set up leads us to a source of exogenous variation both over time and across municipalities, once there is an asymmetric and unanticipated increase in credit supply at the municipal level every time a new financial institution starts to operate the service.

There are two sources of bias we worry about. The first one relates to the fact that municipalities with a larger number of banks have more chance to be quickly provided with the *consignado*. These locations might be systematically distinct in term of size, wealth, labor market, and so on. Another concern comes

from the fact that we also need the distribution of banks to be random among municipalities with the same number of banks. In other words, if municipalities with only one branch of, say, bank A systematically differ from municipalities with only one branch of, say, bank B, then our estimation could be biased. For example, this situation arises if we think that some branches of state-owned banks are part of the government's development strategy and are placed in small towns for reasons beyond profit and efficiency. This is quite relevant to us because most of the variation in our database come from small towns, since larger cities surely had at least one branch of CEF and, therefore, had access to the *consignado* from its beginning. To make sure these two issues do not drive our results, besides allowing for year and municipal fixed effects, we do include the number of private and public banks in each municipality as control variables in our model. Nevertheless, the changes in estimates by disregarding these controls, if any, are absolutely irrelevant.

2.2 Data

We construct a panel database containing all the 5,500 Brazilian municipalities[13] from 2001 to 2010 on an annual basis. We analyze three years of pre-intervention data (2001-2003) and seven years (2004-2010) when the agreements signature and, therefore, the supply of *consignado* to INSS recipients, was already in course.

First of all, we utilize the flow of *consignado* (*per capita*) contracted by INSS recipients as the variable related to the credit reform. We have this information per municipality and per year, which was granted by the INSS. We refer to this variable as *consignado* to INSS, and it is the endogenous variable we intend to instrument. From the INSS, we have three additional information that are used to generate our instrumental variables: (1) the date of beginning and ending of all agreements between the INSS and financial institutions; (2) the number of INSS recipients per municipality and per year; (3) the annual total amount of benefit issued by the INSS to each municipality.[14] We also use a detailed dataset on the opening and closing of branches throughout the country. It was provided by Brazil's central bank and enables us to obtain the

[13]The Brazilian 2010 Census contained 5,565 municipalities, while the 2000 Census contained 5,507. Thus, in order to match our data we have chosen to aggregate it back to the 2000 Census political division. Nevertheless, some municipalities have emancipated from more than one original municipality and, therefore, we were unable to reconstruct the original division for these particular locations. In these (few) cases, we have aggregated the entire group into one. By doing so, we have obtained a total of 5,500 municipalities rather than 5,507, although considering the totality of the Brazilian territory.

[14]This variable encompasses all types of INSS benefits. Nevertheless, retirees and recipients of survivor's pensions represent about 80% of total benefits, according to INSS data.

number of branches of each bank per municipality and its variation along the years.

Our assessment of the *consignado*'s economic impacts is basically four-folded. First, we look at the effects of the *consignado* to INSS in terms of municipal gross domestic product (GDP) and formal employment rate, which is obtained from Brazil's statistical bureau (IBGE) and from Brazil's annual social information report (RAIS), respectively. Second, we desegregate income revenues in order to understand the net impact we see on GDP and formalization. For that, we use the annual amount of corporate income tax and other income tax collected by each municipality (from Brazil's secretariat of the federal revenue), and the average formal wage (from RAIS). By other income tax we mean the amount of income tax payed by individuals when excluding the portion of tax that is charged over wages. Therefore, this variable includes all other sources of income, such as rents and financial investments. Third, we look for changes in the number of public and private branches per municipality in order to identify general effects on banking. Forth, we use municipal data from Brazil's central bank (Estban) regarding total credit stock and total loans stock. These two variables enable the examination of whether agents simply substituted previous credit contracts to a cheaper type of loan or if the reform has truly expanded credit taking. In addition, we also use some other standard municipal information such as population, agricultural production ans distance to the state capital, which are publicly disclosed by IBGE.

All variables in our data set are available for all municipalities in all years, so that there are no dropouts. Besides, all monetary data we use are expressed in Brazilian currency (real – R$) and deflated to 2000. In 2000, the average real/dollar exchange rate was 1.83 and the purchasing power parity (PPP) conversion rate was 1.11. Lastly, variables are used in level. Considering them in a logarithmic scale has been discarded due to some informative null values we do not wish to disregard.

Basic descriptive statistics of the intervention variable, outcomes, instrument components and some other relevant municipal characteristics are presented in Table I. These information are reported for both the first and the last year under consideration. We see that most indicators grew enormously during the decade. For instance, the average total credit *per capita* jumped from R$305 to R$787, even though in 2010 most municipalities still had no private branches and at most one public branch. There are also evidence that both total credit and total loans have become more equality distributed, since their median increased five and six times, respectively. Besides, there is an enormous discrepancy between the mean and median of corporate income tax due to

TABLE I
SUMMARY STATISTICS

	Mean		Median		St. Dev.	
	2001	2010	2001	2010	2001	2010
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Intervention Variable						
Consignado to INSS per capita	—	42.76	—	40.93	—	40.22
Panel B: Outcome Variables						
Average wage *per capita*	437.17	903.15	259.92	613.36	604.19	994.37
Corporate income tax *per capita*	14.71	37.04	1.66	4.43	63.14	150.44
Formal employment rate	8.5%	13.3%	6.4%	10.6%	8.2%	10.3%
GDP *per capita*	4,168	5,674	3,117	4,348	4,592	6,308
Number of private bank's branches	1.9	2.3	0	0	30.6	34.7
Number of public bank's branches	1.4	1.5	0	1	8.0	10.1
Other income tax *per capita*	4.10	11.24	0.97	4.31	8.13	30.97
Total credit *per capita*	305.06	787.43	40.42	242.98	726.17	1,899.15
Total loan *per capita*	107.35	306.40	30.70	158.86	239.27	842.18
Panel C: Components of the Instrumental Variables						
INSS benefit *per capita*	278.19	520.75	273.71	512.11	182.31	321.57
Thousands of INSS recipients	3.6	5.1	1.1	1.5	24.5	31.3
Panel D: Other Municipal Characteristics						
Distance to the State Capital (Km)	251.8		226.0		163.3	
Population	31.3	34.7	10.5	11.1	188.6	204.4
Share of Agriculture in GDP	24.0%	21.1%	22.3%	17.9%	15.9%	15.2%

Notes. This table shows the mean, median and standard deviation of the main variables we use. Values refer to all 5,500 Brazilian municipalities and are reported for both the first (2001) and last (2010) year of analysis. The table was constructed using data from Brazil's statistical bureau (Instituto Brasileiro de Geografia e Estatistica: http://www.ibge.gov.br), Brazil's central bank (Banco Central do Brasil: http://www.bcb.gov.br), the annual social information report (RAIS) conducted by Brazil's ministry of labor and employment (Ministério do Trabalho e do Emprego: http://www.mte.gov.br), Brazil's secretariat of the federal revenue (Receita Federal do Brasil: http://www.receita.fazenda.gov.br) and Brazil's national institute of social security (Instituto Nacional do Seguro Social: http://www.previdencia.gov.br). Description of the variables: *Consignado to INSS per capita* is the annual flow of *consignado*-type credit that was contracted by INSS recipients divided by the population size; *Average wage per capita* is the average annual wage of formal employees divided by the population size; *Corporate Income Tax per capita* is the annual amount of corporate income tax divided by the population size; *Formal employment rate* is the total number of formal employees divided by the population size and multiplied by a hundred; *Other income tax per capita* is the annual amount of income tax payed by individuals disregarding the portion of the tax that is charged over wages (includes rents, income from financial investments, charges over heritages, among others) divided by the population size; *Total credit per capita* is the total stock of credit in December of each year divided by the population size; *Total loans per capita* is the total stock of loans in December of each year divided by the population size; *INSS benefit per capita* is the annual amount of benefit (considering all aid categories) issued to inhabitants of a municipality within a year divided by the population size; *Population* is the number of inhabitants in thousands. All monetary data are in Brazilian currency (real – R$) and at 2000 prices.

the concentration of formal firms in few municipalities. We also note that the average INSS benefit *per capita* is very expressive, being over half the size of formal wages. Finally, the average *consignado* to INSS *per capita* was R$43 in 2010 and it presents similar mean and median, suggesting it is more scattered across the country than most variables we consider.

3
Empirical Strategy

3.1
Instruments

We identify two major determinants of the amount of *consignado* to INSS in each municipality, they are: (1) the existence of at least one branch allowed to operate the service (extensive margin), as we discuss in section 3.1.1; (2) the total disposable INSS benefit (intensive margin), since only a fixed portion of the benefit can be used for loan repayments. From this, we propose the interaction between this two elements as an instrument for the *consignado* to INSS, our intervention variable.

Regarding the extensive margin, we use a dummy that indicates the presence of at least one operating branch in the municipality. For the transition years, that is, those when the first branch emerges (or disappears), we replace this dummy for the fraction of the year (with a month accuracy) in which the branch operated the *consignado*. Additionally, in order to avoid contamination due to the fact that the *ex-post* opening of branches is possibly endogenous, we modify this baseline variable by fixing the number of branches of each bank at its level the year before the bank signed the agreement (for the years after the permission).

For the intensive margin, our baseline variable is the total INSS benefit *per capita*. However, as the amount of benefit received by each person is heterogeneous, one could argue it relates to relevant aspects of the local economy, such as education, and that it is not well invariant during our period of analysis. Thus, in order to make sure it does not drive the results, we use the fraction of INSS recipients with respect to the total population in our preferred version. We choose the fraction of recipients because, besides being strongly linked to the amount of INSS benefit, it depends on demographic and social factors that change very slowly over time and, therefore, we take it as fixed in a ten-year horizon.

From this, we arrive to four instrumental variables for the *consignado* to

INSS in year t and municipality n:

$$\begin{aligned} IV1_{n,t} &\equiv benefitpc_{n,t} \times branch_{n,t} \qquad (baseline) \qquad (3\text{-}1) \\ IV2_{n,t} &\equiv benefitpc_{n,t} \times branchfix_{n,t} \\ IV3_{n,t} &\equiv fracbenef_{n,t} \times branch_{n,t} \\ IV4_{n,t} &\equiv fracbenef_{n,t} \times branchfix_{n,t} \qquad (preferred) \end{aligned}$$

where $benefitpc_{n,t}$ is the amount of INSS benefit *per capita* (in hundreds of reais); $fracbenef_{n,t}$ is the number of INSS recipients divided by the population size; $branch_{n,t}$ is the number of months of each year in which the municipality had at least one operating branch divided by 12; and $branchfix_{n,t}$ is the number of months of each year in which the municipality had at least one operating branch divided by 12 when for a bank that signed the agreement in year t we fix its distribution of branches from t onward at the $t-1$ level.

Lastly, we incorporate the fact that the impact of having an operating branch for the first time might differ from having it for a second year and so on. There are many possible reasons for that, for example, it could take time until agents realize the instrument is now available or, similarly, there could be a delay until the bank publicizes the service and until its employees are aware of how to operate it. So, we add a third interaction term to our instrumental variables, which are dummies for the number of years for which the municipality has already had *consignado* ("years of *consignado*"). As we analyze seven years of intervention (2004-2010), its possible to observe municipalities up to its seventh year of *consignado*. Thus, we use seven dummies and, therefore, obtain four groups of seven instruments. Generically, our vector of instruments is given by:

$$IVvec^{A}_{n,t} \equiv IVA_{n,t} \times YB_{n,t}, \qquad A = \{1, ..., 4\};\ B = \{1, ..., 7\} \qquad (3\text{-}2)$$

where $IVA_{n,t}$ are as in (1); and $YB_{n,t}$ are dummies for the number of years for which the municipality has had access to the *consignado* until the moment t.

3.1.1 Stylized Facts

We now briefly base our choice of using a dummy for the presence or not of operating branches in a municipality, rather than suggesting an intensity-varying measurement. Knowing that INSS recipients can only contract *consig-*

nado-type credit from a list of registered banks, we investigate the relationship between the distribution of branches and the actual utilization of the service through two stylized facts.

First of all, we have reasons for believing that the local markets of *consignado* are very little integrated with one another, in the sense that municipalities are basically restricted to their own credit supply. Although in legal terms the service could be contracted in a neighbor municipality within the same state, contracts by telephone are explicitly banned by the INSS rules of procedure.[15] This is quite relevant to us, given that the vast majority of INSS recipients are seniors and, therefore, unlikely to travel long distances for signing a credit contract. Hence, we expect the existence of *consignado* to INSS in a particular municipality to be highly correlated with the presence of a physical branch allowed to operate the service.

This idea is corroborated by Table II, which shows the relationship between having any positive amount of *consignado* to INSS and having at least one branch of a bank that had permission for using the instrument. By looking at these matrices, we see that the presence of operating branches in the municipality strongly indicates the presence of *consignado*-type contracts, in all years since its regulation.[16] However, it appears from the table that the reverse is not true, that is, that the absence of operating branches does not prevent the municipality from having access to the credit. This is a misleading conclusion. Though the numbers in the lower left cells are not so close to zero, in practice, the intensity of *consignado* to INSS in these municipalities is negligible, accounting for no more than 3% of the total in any year.

Thus, there are evidence that the signature of a new agreement indeed increases the supply of credit in municipalities that thereat start having an operating branch, but the impact of a new agreement on municipalities that already had one or more operating branches is unclear.

In order to address this remaining question, we look into two possible measures of the abundance of operating branches in these locations: (1) the number of operating branches divided by the number of INSS recipients; (2) the number of operating branches *per capita*. We do not find any strong reason to prefer either one. Figure III investigates the correlation between the density of operating branches and the intensity of *consignado*, by plotting these two

[15]The contracting of *consiginado* from a different state from where the benefit is issued is also forbidden according to the INSS rules of procedure.

[16]As an exception, in 2006, the number of municipalities with operating branches but without *consignado* to INSS rises to 46. This deviation from all other years is clearly due to the fact that *Itaú* (the largest bank in Brazil) obtained the INSS permission in December 19^{th} of that year. Our econometric approach uses a monthly measure for that, in order to avoid issues like this.

TABLE II
MATRIX OF *CONSIGNADO* AND OPERATING BRANCHES

			At least one operating branch	
			NO	YES
Some *consignado*	2004	NO	2,282	0
		YES	2,214	1,004
	2005	NO	1,753	5
		YES	1,081	2,661
	2006	NO	1,716	42
		YES	432	3,310
	2007	NO	1,744	6
		YES	412	3,338
	2008	NO	1,760	4
		YES	320	3,416
	2009	NO	1,751	3
		YES	311	3,435
	2010	NO	1,746	2
		YES	306	3,446

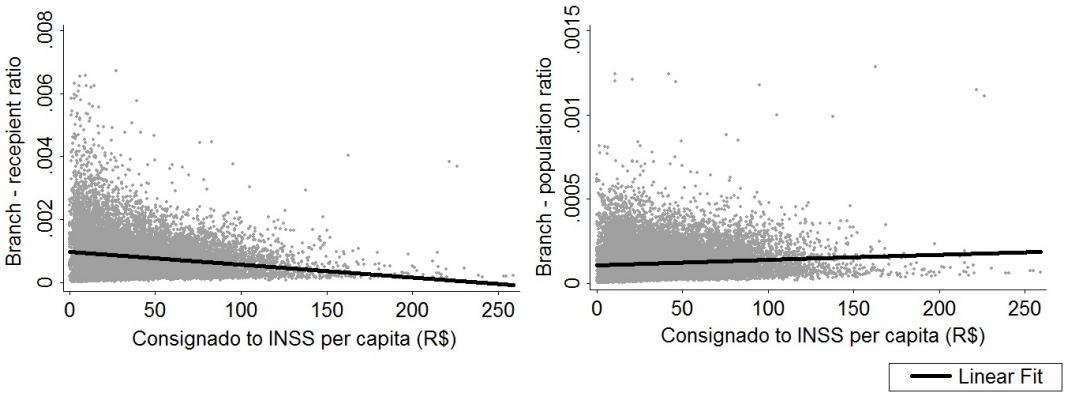
Notes. Table shows, for each post-reform year under consideration, the matrix that relates municipalities having at least one branch allowed to operate the *consignado* and municipalities having at least one resident that has contracted the service. Cell contents are the number of municipalities belonging to each category in each year.

measures against the amount of *consignado* to INSS in municipalities with one or more operating branches. Graphs show both the scatterplot and the best linear fit that results from a pooled OLS regression.

As shown in the figure, these two regressions yield results that point out in opposite directions. Moreover, the correlation coefficient between each measure and the amount of *consignado* is only -0.07 for the branch-recipient ratio and 0.09 for the branch-population ratio. From this, it appears that once the *consignado* is already established in a given municipality, having plenty of operating branches does not interfere significantly in the amount of credit that is contracted.

We conclude that the shock of *consignado* on the municipal credit market is mostly concentrated at the emergence of the municipality's first operating branch. Above all, the prudence of this approach can be confirmed empirically: (1) if the emergence of a first operating branch does not well represent a shock of credit, we would simply encounter no significant effects when using it as an instrument; (2) if the abundance of branches plays an important role in determining the amount of *consignado*, our instruments would fail to explaining a relevant portion of credit variation. As estimates show, both are certainly

FIGURE III
DENSITY OF OPERATING BRANCHES *VERSUS* AMOUNT OF *CONSIGNADO*



Notes. Figure uses 2004-2010 annual INSS data to investigate two different measures of the abundance of *consignado* services to INSS recipients in municipalities with one or more operating branches: 1) Branch-recipient ratio: number of operating branches divided by the number of INSS recipients in each location; and 2) Branch-population ratio: number of operating branches *per capita* in each location. The graphs plot the dispersion of the observations and the simple pooled ordinary least squares regression of the amount of *consignado* contracted by INSS recipients on each of these measures.

not the case.

3.2 Estimation

We would like to estimate the following model for the impact of the *consignado* to INSS in municipality n and year t on an outcome $y_{n,t}$:

$$y_{n,t} = \alpha_n + \alpha_t + \theta consigpc_{n,t} + X_{n,t}\phi + \varepsilon_{n,t} \tag{3-3}$$

where α_n are municipal-specific fixed effects; α_t are time-specific fixed effects; $consigpc_{n,t}$ is the amount of *consignado*-type credit *per capita* contracted by INSS recipients; and $X_{n,t}$ is a vector of socioeconomic variables (controls).

However, θ in equation (3) can be a biased estimator of the average (causal) impact of the *consignado* on $y_{n,t}$, once $consigpc_{n,t}$ is potentially endogenous with respect to non-observable variables in the error term $\varepsilon_{n,t}$. For this reason, we estimate the population parameter of interest by a two-step regression where $IVvec_{n,t}^{A=a}$ are used as instruments for $consigpc_{n,t}$. The first-stage is given by:

$$consigpc_{n,t} = \gamma_n + \gamma_t + IVvec_{n,t}^{A=a}\delta + X_{n,t}\lambda + \epsilon_{n,t} \tag{3-4}$$

where $IVvec_{n,t}^{A=a}$ is one of the four sets of instruments we proposed in section 3.1.

Then, in the second-stage regression, we have:

$$y_{n,t} = \eta_n + \eta_t + \beta \widehat{consigpc}_{n,t} + X_{n,t}\rho + u_{n,t} \tag{3-5}$$

where $\widehat{consigpc}_{n,t}$ are the fitted values obtained from equation (4).

For ensuring causality is indeed identified through β, we need:

$$\begin{aligned} \epsilon_{n,t}, u_{n,t} &\perp IVvec_{n,t}^{A=a} \\ \epsilon_{n,t}, u_{n,t} &\perp X_{n,t} \end{aligned} \tag{3-6}$$

Or, instead:

$$\epsilon_{n,t}, u_{n,t}, X_{n,t} \perp IVvec_{n,t}^{A=a} \tag{3-7}$$

which we are more comfortable in taking, since the estimates of δ and β are practically unchangeable to the inclusion of controls.

By relying on expression (7), we assume the lack of covariance between the instruments we use and all other municipal- and time-varying determinants of $consigpc_{n,t}$ or $y_{n,t}$. A sufficient condition for that would be the exogeneity of each element (extensive and intensive margins) composing $IVA_{n,t}$. In other words, it suffices if, after including fixed effects, the INSS agreements and the INSS benefit (or portion of INSS recipients) are unable to determine economic outcomes other than through the *consignado*. Still, even if this hypothesis does not apply, the consistency of β is guaranteed if the resulting interaction between them (that is, $IVA_{n,t}$ itself) is uncorrelated to the variables in $\epsilon_{n,t}$, $u_{n,t}$ and $X_{n,t}$.

4
Borrowing Trajectory

We begin by presenting the first-stage estimates, which enable us to follow the *consignado*'s expansion over the years. Table III and IV show these results for the four sets of instruments $IVvec^{A}_{n,t}$ we constructed in section 3.1. Columns (1) and (3) of table III and columns (1) and (3) of table IV estimate a POLS of $consigpc_{n,t}$ (the amount of *consignado* to INSS *per capita* in hundreds of reais) on each $IVvec^{A=a}_{n,t}$. In columns (2) and (4) of both tables we add municipal dummies, year dummies and controls, so that it turns into equation (4). Actually, we find no evidence of significant correlation between $IVvec^{A=a}_{n,t}$ and *any* element of $X_{n,t}$, as not even the inclusion of fixed effects substantially alter the estimation of δ (the coefficients associated with the instruments). Nevertheless, we include the number of private and public banks as controls because it is justifiable through the policy's design, as discussed in section 2.1.2.

We find all four sets of instruments to be strongly correlated with the intervention variable. We obtain an extremely good fit of the POLS regressions, so that around 70% of the variation in the intervention variable is accounted for by the variation in the instruments. When including fixed effects and controls, the R-squared increases just a little, to about 0.78. These findings suggest that, indeed, a very large proportion of the total amount of *consignado* to INSS is exogenously defined. As expected, the instrument sets largely pass a weak instrument test (F-test), which is reported in the last line of tables III and IV. All coefficients related to the instruments are statistically significant at a 99% confidence level and the point estimates from the POLS regressions are quite close to those with fixed effects and controls. We have also estimated columns (2) and (4) by the first difference method and the changes in the coefficients are in the order of 10^{-3}. Furthermore, we have added several other controls to these regressions, such as industrial production, stock of vehicles and population but, again, variations are economically negligible.

As one can also see from tables III and IV, the point estimates are even closer when switching from vector IV1 to IV2 or, similarly, from IV3 to IV4. These findings suggest that our caution in eliminating the post-intervention variation in the number of branches does not yield significant changes in results, which is confirmed by the equivalence of the second stage estimations.

Figure IV shows the coefficients for all four sets of instruments against years of *consignado*. Because we replace the number of INSS recipients in

TABLE III
FIRST STAGE (IV1 AND IV2)

Dependent Variable: *consignado* to INSS *per capita* (*consigpc*) in hundreds				
	Instrument: IV1 (baseline)		Instrument: IV2	
	INSS benefit (hundreds) *per capita* times the fraction of the year with an operating branch		Disregards the *ex-post* variation in the number of branches	
	(1)	(2)	(3)	(4)
Instrument × Y1 (dummy 1^{st} year)	0.0296***	0.0272***	0.0288***	0.0282***
	(0.000516)	(0.000559)	(0.000554)	(0.000586)
Instrument × Y2 (dummy 2^{nd} year)	0.0410***	0.0343***	0.0403***	0.0340***
	(0.000445)	(0.000514)	(0.000450)	(0.000509)
Instrument × Y3 (dummy 3^{rd} year)	0.0511***	0.0441***	0.0510***	0.0433***
	(0.000606)	(0.000664)	(0.000617)	(0.000668)
Instrument × Y4 (dummy 4^{th} year)	0.0559***	0.0528***	0.0553***	0.0519***
	(0.000729)	(0.000767)	(0.000743)	(0.000773)
Instrument × Y5 (dummy 5^{th} year)	0.0650***	0.0595***	0.0645***	0.0582***
	(0.000822)	(0.000895)	(0.000838)	(0.000901)
Instrument × Y6 (dummy 6^{th} year)	0.0899***	0.0787***	0.0895***	0.0768***
	(0.000883)	(0.00105)	(0.000885)	(0.00105)
Instrument × Y7 (dummy 7^{th} year)	0.0954***	0.0879***	0.0949***	0.0857***
	(0.00123)	(0.00134)	(0.00123)	(0.00133)
Private banks		-0.0118***		-0.0194***
		(0.00351)		(0.00368)
Public banks		0.00286***		0.00534***
		(0.000848)		(0.00178)
Constant	0.0246***		0.0292***	
	(0.000489)		(0.000525)	
Municipality dummies	no	yes	no	yes
Year dummies	no	yes	no	yes
R-squared	0.711	0.783	0.697	0.777
Observations	55,000	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500	5,500
F-test on interaction	5584	944.6	5350	909.6

Notes. Robust standard errors are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence. The F-test tests the joint significance of the instrument interaction terms.

hundreds (Table III) by the fraction of recipients (Table IV), the coefficients for IV3 and IV4 are plotted in the secondary axis. This is equivalent to correcting the estimates in Table IV by the average ratio between INSS recipients and the fraction of recipients, in order to directly compare the estimates across the two tables.

As the elements of each instrument set hold a temporal meaning to them, Figure IV enables us to observe the reform's evolution over time. The graph in Figure IV shows the average impact of an extra R$1 of disposable INSS benefit in terms of reais of *consignado* to INSS in municipalities that have had the

TABLE IV
FIRST STAGE (IV3 AND IV4)

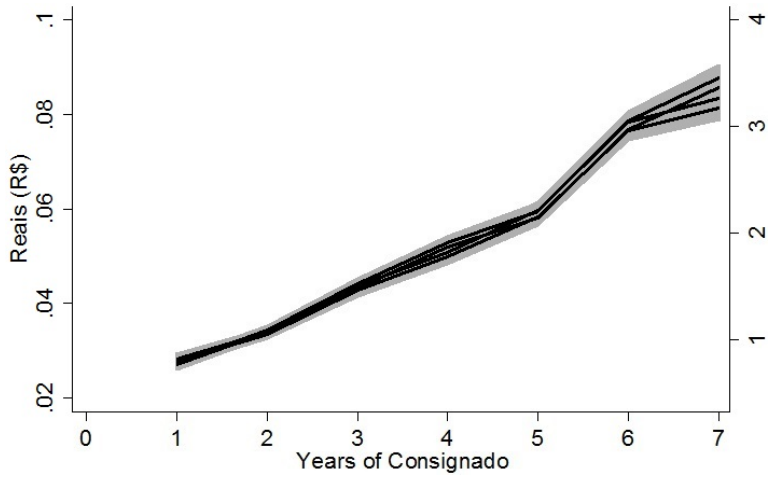
Dependent Variable: *consignado* to INSS *per capita* (*consigpc*) in hundreds				
	Instrument: IV3		Instrument: IV4	
	Uses the fraction of INSS recipients instead of the INSS benefit *per capita*		Uses the fraction of INSS recipients and disregards the *ex-post* variation in branches	
	(1)	(2)	(3)	(4)
Instrument × Y1 (dummy 1^{st} year)	0.944***	0.834***	0.917***	0.854***
	(0.0157)	(0.0176)	(0.0165)	(0.0182)
Instrument × Y2 (dummy 2^{nd} year)	1.342***	1.106***	1.317***	1.093***
	(0.0143)	(0.0169)	(0.0144)	(0.0166)
Instrument × Y3 (dummy 3^{rd} year)	1.750***	1.480***	1.744***	1.451***
	(0.0209)	(0.0231)	(0.0211)	(0.0231)
Instrument × Y4 (dummy 4^{th} year)	1.868***	1.775***	1.847***	1.744***
	(0.0270)	(0.0269)	(0.0274)	(0.0271)
Instrument × Y5 (dummy 5^{th} year)	2.416***	2.180***	2.399***	2.132***
	(0.0296)	(0.0331)	(0.0301)	(0.0334)
Instrument × Y6 (dummy 6^{th} year)	3.418***	2.989***	3.405***	2.919***
	(0.0340)	(0.0401)	(0.0341)	(0.0403)
Instrument × Y7 (dummy 7^{th} year)	4.010***	3.608***	3.989***	3.519***
	(0.0570)	(0.0574)	(0.0569)	(0.0569)
Private banks		-0.0345***		-0.0400***
		(0.00379)		(0.00395)
Public banks		0.000723		0.00304
		(0.00159)		(0.00251)
Constant	0.0240***		0.0285***	
	(0.000488)		(0.000525)	
Municipality dummies	no	yes	no	yes
Year dummies	no	yes	no	yes
R-squared	0.704	0.779	0.691	0.773
Observations	55,000	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500	5,500
F-test on interaction	5372	933.6	5157	896.7

Notes. Robust standard errors are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence. The F-test tests the joint significance of the instrument interaction terms.

service for different periods of time. The four sets of instruments yield results alike. The fraction of INSS benefit that is converted into *consignado* increases monotonically and in a fairly linear path. In the first year of *consignado*, for every R$100 of INSS benefit in the municipality, about R$2.80 of *consignado* was contracted, while in the seventh year this amount rises to about R$8.70 (out of a R$30 maximum[17]).

[17]Once that, by law, only up to 30% of the borrower's net INSS benefit can be used for the loans repayments, we expect, in equilibrium, no more than R$30 of *consignado* to be contracted when increasing total INSS benefit in R$100.

FIGURE IV
AMOUNT CONVERTED INTO *CONSIGNADO* PER
ONE REAL OF INSS BENEFIT



Notes. Figure plots the coefficients (associated with the instrument interaction terms) obtained in columns (2) and (4) of tables III and IV, which is interpreted as the average amount of *consignado* (in real - R$) that is contracted for every extra real of INSS benefit in a municipality. Each line plots (four one of the four sets of instruments) this trajectory by the number of years the municipalities have had a branch operating the service. Coefficients concerning IV3 and IV4 are plotted in the secondary axis, as they are not directly compared to those of IV1 and IV2. Region in gray is the union of the four 95 percent confidence intervals.

Two main conclusions emerge from Figure IV. First, there are evidence that the population adheres to the new service gradually and, therefore, it takes time until a credit reform such as the *consignado* fully spreads out. Second, it might take quite a long time. In the case of the *consignado*, the service seems to be still in a process of expansion even in municipalities that have already had it for seven years.

5
Average Economic Impacts

We now approach the average effects of the *consignado* to INSS on several economic variables. The second stage results we present in this chapter refer to the estimation of equation (5) using IV4 (portion of INSS recipients instead of total INSS benefit and no *ex-post* branch variation), which we consider to be the most robust instrument set with respect to possible confounding factors. Nevertheless, all instrument sets produce estimates that are statistically significant and that point out in the same direction. An easy comparison of all four specifications is enabled by Figure A (in the Appendix), in which we observe that the values presented here (IV4) are the least optimist of them. Moreover, it is important to have in mind that we are not calculating the direct or short-term effect of the *consignado*, so that the impacts refer to the overall result of the credit reform within a municipality, when considering all of its surrounding effects. A further discussion regarding the interpretation of these average estimates is carried out in section 5.5.

5.1
Employment and GDP

We start by looking at the *consignado*'s impact on the municipal aggregate income itself, which is reported in columns (1) and (2) of Table V. The second column of Table V suggests that R$1 of *consignado* is capable of generating, on average, R$1.96 in terms of municipal GDP, although it is not statistically greater than one at the five percent level.

Additionally, the last two columns of Table V consider the impact of R$100 of *consignado per capita* on the size of the formal labor market. According to column (4), this rise in credit is likely to increase formal employment rate in about 1.5 percentage points, which represents over 11% of the average rate in 2010. This is a first evidence that formalization is an important output of the mechanism through which credit generates GDP.

Furthermore, columns (1) and (3) show these estimations in only one stage. We find out that the average effect of the *consingnado* on GDP are equivalent with and without instrumentation, while the impact on formal employment rate is considerably underestimated in the OLS regression.

TABLE V
AVERAGE EFFECTS ON EMPLOYMENT AND GDP

	Dependent Variable:			
	GDP *per capita* (hundreds)		Employment rate (%)	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS
$Consig_{pc}$ (hundreds)	1.852**	1.964***	0.789***	1.482***
	(0.810)	(0.651)	(0.153)	(0.135)
Municipality dummies	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Instrument	IV4	IV4	IV4	IV4
R-squared (within)	0.061	0.061	0.200	0.199
Observations	55,000	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500	5,500

Notes. Robust standard errors are displayed in parentheses. Definition of dependent variables: *Employment rate* – formal employment rate (as described in Table I). Controls: number of private banks and number of public banks. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

5.2
Sources of Income

Table VI further investigates the engine that links credit and growth, by assessing impacts on the components of the municipal aggregate income. Such estimates suggest that the *consignado* has increased all three sources of income: profit, wages and individuals' non-wage revenues. First, according to column (2), an increase of R$100 in the *consignado* results in R$27.12 of corporate income tax revenues. Hence, if we consider the average tax rate as 18% of firms' profit[18], we may say that R$1 of *consignado* produces R$1.51 as profit.[19] Second, the estimate in column (4) directly indicates that R$1 of *consignado* implies in R$1.90 of wage bill, being greater than 1.65 at the five percent level. Third, column (6) says that, on average, R$100 of *consignado* results in extra R$12.41 of tax payment with respect to other revenues. Thus,

[18]This rate refers to firms taxed through their real profit, who account for 83-84% of firms' gross revenues, and for whom data are available. For 2005 and 2006 (the only two years related data from Brazil's secretariat of the federal are disclosed) the actual income tax rate represented 17.38% and 17.80% of their operating profit, respectively.

[19]Unfortunately, the very small firms (up to R$360,000 gross annual revenue) are exempt from income tax charges and, therefore, we are unable to track their expansion. Such firms, however, account for less than 0.5% of firm's gross revenues.

TABLE VI
AVERAGE EFFECTS ON INCOME

	Dependent Variable:					
	Corporate tax		Average wage		Other tax	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
$Consig_{pc}$ (hundreds)	20.27*** (3.934)	27.12*** (3.133)	121.9*** (14.58)	190.4*** (12.58)	7.852*** (0.566)	12.41*** (0.808)
Municipality dummies	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Instrument	IV4	IV4	IV4	IV4	IV4	IV4
R-squared (within)	0.004	0.004	0.257	0.256	0.029	0.028
Observations	55,000	55,000	55,000	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500	5,500	5,500	5,500

Notes. Definition of dependent variables: *Corporate tax* – corporate income tax *per capita* (annual); *Average wage* – average wage *per capita* (as described in Table I); *Other tax* – other income tax *per capita* (as described in Table I). Controls: number of private banks and number of public banks. Robust standard errors are displayed in parentheses. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

if we consider the average income tax rate as 20% of these non-wage incomes[20], we estimate that R$1 of credit generates R$0.62 of income.

From Table VI, the credit expansion appears to redistribute income towards employees within municipalities, since point estimates suggest larger increases in wages than in profit. However, by using the 2010 summary statistics in Table I, we have that by making the *consignado per capita* two and a half times bigger, one should expect firm's profit *per capita*, formal wage *per capita* and non-wage incomes *per capita* to increase by 73%, 21% and 110%, respectively. In this way, although income increase in the form of wages is larger than in the form of profit, the rise in profit is a lot more substantial is terms of its level in 2010.

Two additional comments are in order. First, the impacts we find on income might not be fully converted into real earnings, once the rise in equilibrium wages might also result in higher living costs, due to more expensive services.[21] Second, the impact we see on wages might not be fully due

[20]An average rate is hard to estimate in this case since non-wage incomes encompass several types of revenues. For example, financial applications are usually charged between 15 and 22.5% while charges on rents vary between zero and 27.5%. We take 20% as an approximation.

[21]All data have been deflated using national deflators, since annual deflators at the

to changes in the price of labor, as the increase in wage bill might also result from the entrance of new workers in the formal labor force. The presence of this second engine is actually suggested by the increase in formal employment rate we find. In fact, if we re-estimate the impact of the *consignado* on the average wage controlling for the formal employment rate, the coefficient we encounter is only 78.7 (significant at the one percent level). Therefore, although there is evidence of an important rise in the wage per worker, out of the R$1.90 the *consignado* generates in the wage bill, the larger portion of it (R$1.11) is attributed to the increase in the number of formal employees.

Finally, if we add up the impact of R$1 of credit on profit, wages and other incomes, we obtain R$4.03, which is yet higher than the final estimated impact on GDP. This is somewhat expected, since we presume the *consignado* to lower informality. Thus, by extrapolating these estimates and calculating GDP in terms of income, we find that R$1 of *consignado* shall reduce in R$2.07 the portion of GDP that is generated by the informal sector, as to obtain a net multiplier effect of 1.96.[22] Although informative, however, these estimates are rough approximations and one shall take them in qualitative terms, rather than quantitatively.

5.3 Banking

In Table VII, we examine the power of the *consignado* in stimulating overall banking, by estimating its effects on the number of private and public banks' branches among the municipalities. From the estimates in columns (2) and (4) we have that, on average, it takes R$119 of *consignado per capita* for a new private branch to open and R$271 for a new public branch. This is a sizable impact considering that in 2010 most municipalities still had no more than one branch. Also, the larger impact on private branches than on public ones is expected, since we presume public banks are not always driven by market incentives.

Now, recall that by solving limited commitment issues the *consignado* enables the supply of credit at reasonable interest rates for those devoid of collateral. Hence, a possible explanation behind these findings comes from the fact that the large volume of contracts, though in small amounts, dilutes banks' fixed costs significantly. This is very much in line with the microeconomic theory on banking, which states the presence of high fixed costs as a major

municipal level are not available. Thus, the changes in the relative prices that might occur across municipalities are not being accounted for.

[22] We assume no impact of the *consignado* on subsidies.

TABLE VII
AVERAGE EFFECTS ON BANKING

	Dependent Variable:			
	Private branches (hundreds)		Public branches (hundreds)	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS
$Consig_{pc}$ (hundreds)	0.940***	0.843***	0.367***	0.369***
	(0.266)	(0.110)	(0.0497)	(0.0259)
Municipality dummies	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Instrument	IV4	IV4	IV4	IV4
R-squared (within)	0.051	0.051	0.313	0.313
Observations	55,000	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500	5,500

Notes. Robust standard errors are displayed in parentheses. Definition of dependent variables: Private branches – number of private banks' branches; Public branches – number of public banks' branches. Controls: number of private banks and number of public banks. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

reason for the inability of commercial banks to expand profitably throughout the developing world.

Furthermore, we know from Table I that the average municipal population in 2010 was only about 35,000 inhabitants. Accordingly, the estimate in column (2) of Table VII, for example, suggests that, on average, a credit portfolio of R$4.2 million would be enough for justifying a new private branch. Though a quite small amount *per se*, such expansion in the *consignado* possibly evidences the perspective of further economic progress in the subsequent years. Thus, even if R$4.2 million of credit per year might not be sufficient for making a new branch economically attractive for the banks, it is so in the sense of pointing out a promising economic development in the near future.

Finally, by encouraging the installation of new branches, the *consignado* is likely to have boosted various banking services, that go far beyond consumer credit.

5.4 Credit Market

Lastly, we delve more deeply into the consequences of the *consignado* to the municipal credit markets. The coefficients in columns (2) and (4) of Table

VIII indicate that the *consignado* has positively impacted both total credit and total loans balance. It shows that the reform was successful in expanding the total borrowing in the municipalities, instead of simply crowding out other sources of credit. However, the magnitude of this impact is less clear since we consider the *flow* of *consignado* and the *stock* of total credit and total loans. Nevertheless, in relative terms, we see that the impact of the *consignado* on total credit is a lot grater than its effect on the subgroup of loans, indicating that other varieties of credit have also expanded.

TABLE VIII
AVERAGE EFFECTS ON THE CREDIT MARKET

	Dependent Variable:			
	Total credit (hundreds)		Total loans (hundreds)	
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS
$Consig_{pc}$ (hundreds)	6.565***	13.71***	3.381***	5.155***
	(0.435)	(0.241)	(0.208)	(0.900)
Municipality dummies	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Instrument	IV4	IV4	IV4	IV4
R-squared (within)	0.112	0.077	0.138	0.125
Observations	55,000	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500	5,500

Notes. Robust standard errors are displayed in parentheses. Definition of dependent variables: *Total credit* – stock of total credit *per capita* (as described in Table I); *Total loans* – stock of total loans *per capita* (as described in Table I). Controls: number of private banks and number of public banks. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

From the findings in the previous section, we presume the opening of new branches to be an important channel through which the *consignado* has stimulated other sources of credit. Besides, this stimulus might also arise from the demand side, since the *consignado* implies in a prompt rise of disposable income and, thereby, generates consumption. As a result of greater demand for goods and services, we expect an increment in production and, consequently, more investments (and higher demand for credit) from firms.

5.5 Discussion

Our estimates so far shall *not* be taken as an impact evaluation of the *Consignado* Credit Reform in Brazil. As we presume municipalities are (at least partially) open economies and, therefore, inter-municipal spillovers can possibly take place, to quantify the actual impact of this credit intervention (as a whole) on an specific municipality, say A, one should compute the sum of two effects: (1) the effect of the credit injection in municipality A on the economy of A (which we estimate here, on average); (2) the net effect of the credit injections in all other municipalities on the economy of A. The direction of this second effect is uncertain since more credit in neighbor locations might boost the economy in all the surrounding area or, in contrast, absorb labor and capital that would otherwise go to A. In order words, external credit reforms could imply in a complex inflow and outflow of resources in the domestic economy, so that the net flow of funds is unknown, and not embraced in the impacts we calculate. Thereby, our mean regressions aim to estimate the average aggregate effects of a solely independent credit reform in a small open economy (municipality), rather then the average impact of the "*consignado* program" in a Brazilian municipality.

However, once other municipalities surrounding one another are also experiencing these credit shocks in the mean time, a possible concern arises if we think that our instruments are correlated with the level of *consignado* in neighbor locations. In this case, we could be overestimating (or underestimating) the impact of the credit increase in a given municipality because coefficients could be capturing spillovers from somewhere else.

In order to verify it is not the case, we proceed with a robustness check (tables A - C, in the Appendix). We include the total *consignado* to INSS in the state of each municipality as a control variable in the model. The first stage coefficients we obtain are practically unchanged. The point estimates in the second stage equations are all slightly smaller than in the original specification and significant at the one percent level, even after accounting for state-level clusters. The only exception was the estimation concerning the effect on GDP, in which the coefficient was no longer significant. Moreover, for none of the outcome variables we can reject the hypothesis that both estimates are equal at the five percent level. We also re-estimate these average effects using the amount of *consignado* in the capital of each state as a control, and the results are exactly the same. All point estimates when controlling for the amount of *consignado* in the capitals are very similar to those when controlling for the amount of *consignado* in the states.

In sum, the estimates here refer to the multiplier effect of an independent, exogenous and large-scale credit injection in the domestic economy, when all possible spillovers both within the municipality (to non-borrowers) and across them are accounted for. Moreover, precisely because we examine longer run data, we do not believe we observe impacts that remained restricted within a municipality, so that new municipal-level, state-level and country-level market clearings may take place. Thus, as we are not willing to take municipalities as isolated economies from one another, our estimation shall not be seen as an attempt to calculating the size of the municipal-level general equilibrium effects explicitly.

6
Dynamic Effects

We now use our model's reduced form regressions, together with the coefficients we obtained in the first stage, to shed light to a major question in the microfinance literature: whether the marginal impacts of a credit program depend on time and scale.

First, recall that the coefficients in the fourth column of Table IV are estimated from the following equation:

$$consigpc_{n,t} = \gamma_n + \gamma_t + IVvec_{n,t}^{A=4}\delta + X_{n,t}\lambda + \epsilon_{n,t} \tag{6-8}$$

that is, our first stage regression while using IV4 as the instrument.

Also, we know from Figure IV that the coefficients δ grow approximately linearly when increasing years of *consignado*, which indicates that the service's adhesion augments gradually over time. In practice, therefore, we may say that, on average, municipalities that have had the *consignado* for one year experience a smaller-scale credit injection than those that have had the *consignado* for two years, and so on.

Second, we estimate the model's reduced form equation for IV4, which is given by:

$$y_{n,t} = \mu_n + \mu_t + IVvec_{n,t}^{A=4}\psi + X_{n,t}\xi + v_{n,t} \tag{6-9}$$

that is, we regress our outcome variables $y_{n,t}$ on the set of instruments directly.

Here, the vector of coefficients ψ also hold a temporal dimension to it and, in fact, each element of ψ can be interpreted as the impact of an increase in the portion of INSS recipients in municipalities with different years of *consignado*.

Now, note that under our exogeneity assumption, such impact is only generated through the "conversion" of the portion of INSS recipients into *consignado*, and this "conversion rate" was estimated by δ in equation (8). Thus, for each outcome $y_{n,t}$ we can calculate:

$$coefscale_i = \psi_i/\delta_i \tag{6-10}$$

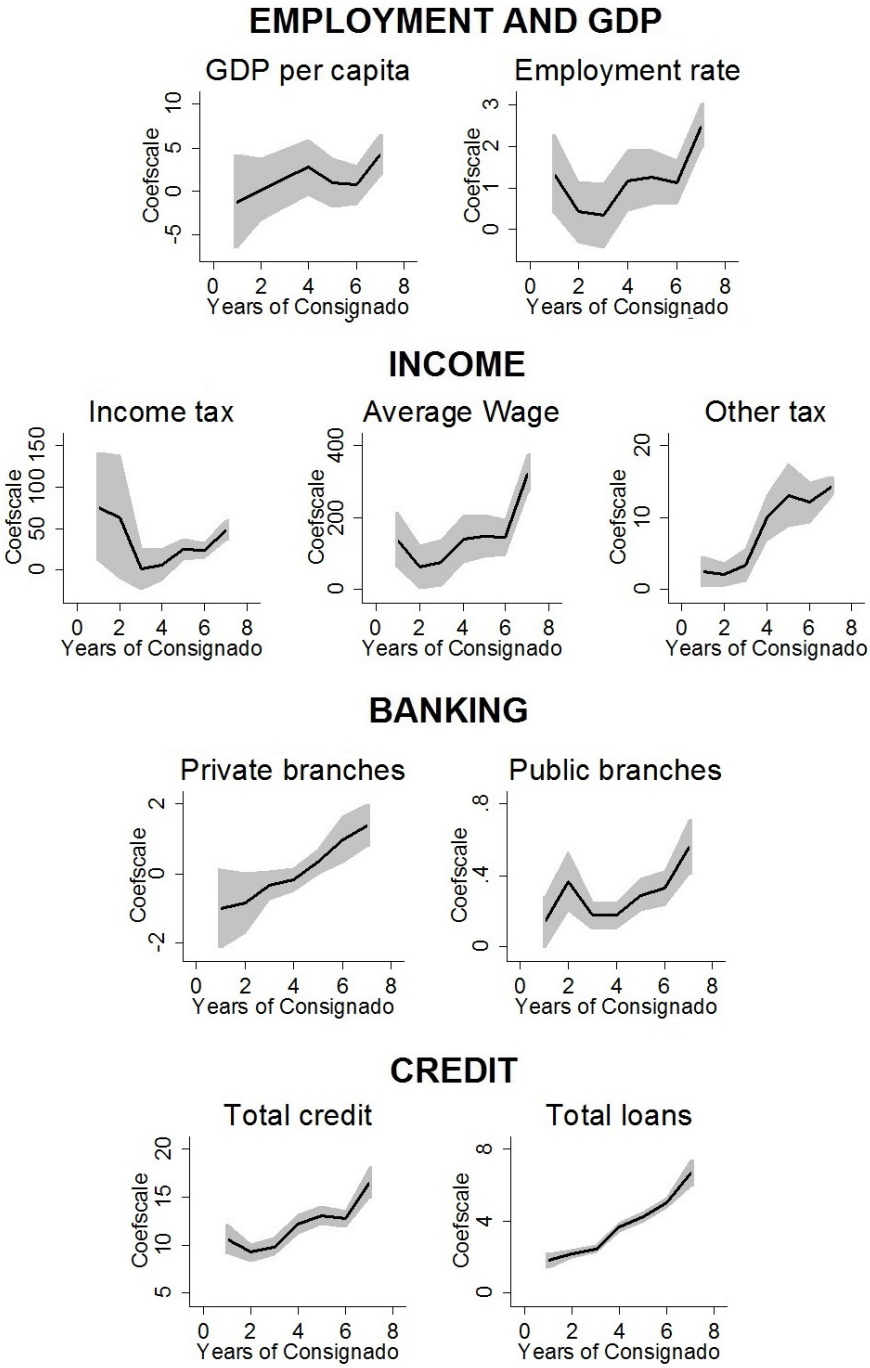
where i is the number of years of *consignado*.

$Coefscale_i$ is the impact of R$100 of *consignado* on $y_{n,t}$ for municipalities that have had at least one branch allowed to operate the *consignado* for i years. Thus, we have the impact of R$100 of *consignado* for different scales of this policy, and we know from Figure IV that it grows approximately linearly as i increases. These impacts are plotted in Figure V and, if the marginal impacts are indeed invariant with respected to the program's scale, one should expect to see flat lines.

As we find out, the hypothesis of a constant marginal impact of the *consignado* regardless of the policy's size is rejected at the five percent level for almost all variables. Actually, we generally observe increasing impacts when increasing scale, though some trajectories are not linear nor monotonic.

There are several possible explanations for the phenomenon we observe. For instance, the first group of individuals to use the service is probably not randomly assigned, so that it is likely to present a different consumption profile from users that adhere to the *consignado* in a second moment. Besides, the presence of dynamic effects may simply arise from economic engines that produce different multiplier effects as the program's magnitude varies. Though putting in doubt the validity of extrapolating the results of small or short-lived programs, Figure V provides evidence that such results are possibly a lower bound to the actual economic potential of the scaled-up version of a particular policy.

FIGURE V
VARIATION OF IMPACTS ACCORDING TO TIME AND SCALE



Notes. Figure plots the average impact of a hundred reais of *consignado* on each outcome variable according to years of *consignado*. This impact is estimated as the ratio between the reduced form coefficient associated with each instrument and its correspondent first stage coefficient. The region in gray defines the 95 percent confidence interval calculated by bootstrap.

7
Heterogeneity

From the economic theory, there are several reasons for believing the effects of a credit reform, such as the *consignado*, might differ across heterogeneous locations. In particular, various fixed or pre-intervention features might determine an economic environment that is more or less prone to change due to an economic stimulus.

In order to understand and quantify this possible variance in the reform's efficiency, we investigate observable heterogeneity in impacts. We introduce a second variable to be instrumented, namely, the interaction between our intervention variable and a given municipal characteristic of interest, say w. This approach accepts the examination of time-invariant variables that are otherwise captured by the fixed effects coefficients. Hence, our second stage becomes of the form:

$$y_{n,t} = \eta_n + \eta_t + \beta_1 \widehat{consigpc}_{n,t} + \beta_2 \widehat{consigpc_{n,t} \times w_{n,t=2003}} + X_{n,t}\rho + \nu_{n,t} \quad (7\text{-}11)$$

where β_2 estimates the differential impact of the *consignado* according to the 2003 level of $w_{n,t}$; $\widehat{consigpc}_{n,t}$ and $\widehat{consigpc_{n,t} \times w_{n,t=2003}}$ are the fitted values from the respective first-stage regressions.[23]

Tables IX - XII report the coefficients β_2 from equation (11), for all the outcomes we have considered in the standard average effects estimation. We estimate equation (11) using four different time-invariant z variables: (1) the distance from the municipality to the state capital (Table XI); (2) the GDP *per capita* in 2003 (Table X); (3) the share of agriculture in the municipal GDP in 2003 (Table XI); (4) the municipal total credit stock in 2003 (Table XII). All estimates in tables IX - XII are statistically different from zero at the five percent level and are consistent in terms of sign.

First, all the coefficients in Table IX are negative, implying that impacts are weaker as we go far from the capitals. We take the distance from the municipality to the state capital as a measure of isolation and, ultimately, as a proxy of its degree of economic openness. Thus, presuming that remote economies are less integrated with external markets, Table IX suggests that the spillovers across locations generate positive impacts and, therefore, more

[23]The first stage regressions concerning these new interaction terms are presented in Table D (in the Appendix) and also yield growing coefficients when increasing years of *consignado*.

opened economies are stronger benefited by the reform.

Second, all the coefficients in Table X are positive, indicating that wealthier municipalities in 2003 (just before the reform) experienced more *ex-post* development. At first sight, these findings insinuate that the implementation of this credit reform in multiple locations does not result in economic convergence among them. However, recall that we are estimating the consequence of a solely independent reform, as discussed in section 5.5. Thus, if we consider simultaneous reforms in economies that are at least partially integrated, its net impact is uncertain, and nothing may be concluded with respect to the inter-economy inequality level.

Third, tables XI and XII reinforce the idea that more developed municipalities before the reform are also more propitious to profit from the new credit environment. The coefficients in Table XI are all negative, stating that more agricultural economies are not so positively affected, while those in Table XII are positive, suggesting that municipalities with a more extent *ex-ante* credit market are able to more intensively profit from a further credit expansion.

A possible explanation for this is a tale of factors' misallocation persistence. Credit rationing (and financial frictions in general) might distort the stock of capital across firms, the number of firms itself and the selection into entrepreneurship, since talented-but-poor individuals are prevented from opening businesses. Hence, if less developed locations face a more severe credit constrain scenario, they are also likely to suffer from a worse pre-reform allocation of capital and labor. In this context, a reform that partially eliminates financial frictions can possibly lead to a reallocation of resources towards efficiency. As a consumer credit expansion to retirees, we do not expect such process as a direct effect of the *consignado* to INSS, but as a consequence of its stimulus to the financial market as a whole and of other aspects of the new equilibrium it produces. However, the initial degree of financial imperfections might interfere in the speed of such reallocation process. This is shown in Buera and Shin (2013), which examines the development dynamics following large-scale financial reforms that removes some sources of distortion. Thus, if it applies to our case, a more present misallocation of resources *ex-ante* could be responsible for a reduced impact in less developed municipalities.

TABLE IX
IMPACTS ACCORDING TO ISOLATION

	Dependent Variable:		
	(1)	(2)	(3)
Panel A: Employment and GDP	GDP *per capita* (hundreds)	Employment rate (%)	
$Consig_{pc}$ (hundreds) × distance to state capital (Km)	-0.0561*** (0.0172)	-0.0233*** (0.00341))	
R-squared	0.054	0.143	
Panel B: Income	Corporate tax	Average wages	Other tax
$Consig_{pc}$ (hundreds) × distance to state capital (Km)	-0.272** (0.132)	-3.088*** (0.376)	-0.0726*** (0.0259)
R-squared	0.003	0.161	0.016
Panel C: Banking	Private branches (hundreds)	Public Branches (hundreds)	
$Consig_{pc}$ (hundreds) × distance to state capital (Km)	-0.0192*** (0.00435)	-0.00412*** (0.00108)	
R-squared	0.028	0.307	
Panel D: Credit	Total credit (hundreds)	Total loans (hundreds)	
$Consig_{pc}$ (hundreds) × distance to state capital (Km)	-0.0720*** (0.0118)	-0.0462*** (0.00595)	
R-squared	-0.080	-0.229	
Municipality dummies	yes	yes	yes
Year dummies	yes	yes	yes
Controls	yes	yes	yes
Instrument	IV4	IV4	IV4
Observations	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500

Notes. All dependent variables are as defined in tables V – VIII. Robust standard errors are displayed in parentheses. Controls: number of private banks and number of public banks. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

TABLE X
IMPACTS ACCORDING TO *EX-ANTE* GDP *PER CAPITA* (2003)

	Dependent Variable:		
	(1)	(2)	(3)
Panel A: Employment and GDP	GDP *per capita* (hundreds)	Employment rate (%)	
$Consig_{pc}$ (hundreds) × GDP *per capita* in 2003 (hundreds)	0.189*** (0.0573)	0.0761*** (0.0107)	
R-squared	0.056	0.178	
Panel B: Income	Corporate tax	Average wages	Other tax
$Consig_{pc}$ (hundreds) × GDP *per capita* in 2003 (hundreds)	1.079*** (0.395)	10.21*** (1.095)	0.196** (0.0905)
R-squared	0.004	0.252	0.032
Panel C: Banking	Private branches (hundreds)	Public Branches (hundreds)	
$Consig_{pc}$ (hundreds) × GDP *per capita* in 2003 (hundreds)	0.0581*** (0.0149)	0.0141*** (0.00375)	
R-squared	0.021	0.297	
Panel D: Credit	Total credit (hundreds)	Total loans (hundreds)	
$Consig_{pc}$ (hundreds) × GDP *per capita* in 2003 (hundreds)	0.230*** (0.0391)	0.145*** (0.0198)	
R-squared	-0.047	-0.198	
Municipality dummies	yes	yes	yes
Year dummies	yes	yes	yes
Controls	yes	yes	yes
Instrument	IV4	IV4	IV4
Observations	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500

Notes. All dependent variables are as defined in tables V – VIII. Robust standard errors are displayed in parentheses. Controls: number of private banks and number of public banks. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

TABLE XI
IMPACTS ACCORDING TO *EX-ANTE* AGRICULTURE (2003)

	Dependent Variable:		
	(1)	(2)	(3)
Panel A: Employment and GDP	GDP *per capita* (hundreds)	Employment rate (%)	
$Consig_{pc}$ (hundreds) × agriculture in 2003 (%)	-0.264***	-0.108***	
	(0.0807)	(0.0142)	
R-squared	0.062	0.196	
Panel B: Income	Corporate tax	Average wages	Other tax
$Consig_{pc}$ (hundreds) × agriculture in 2003 (%)	-1.362**	-14.48***	-0.304**
	(0.598)	(1.517)	(0.126)
R-squared	0.005	0.256	0.030
Panel C: Banking	Private branches (hundreds)	Public Branches (hundreds)	
$Consig_{pc}$ (hundreds) × agriculture in 2003 (%)	-0.0898***	-0.0199***	
	(0.0201)	(0.00510)	
R-squared	0.057	-0.320	
Panel D: Credit	Total credit (hundreds)	Total loans (hundreds)	
$Consig_{pc}$ (hundreds) × agriculture in 2003 (%)	-0.331***	-0.214***	
	(0.0531)	(0.0251)	
R-squared	0.039	0.091	
Municipality dummies	yes	yes	yes
Year dummies	yes	yes	yes
Controls	yes	yes	yes
Instrument	IV4	IV4	IV4
Observations	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500

Notes. All dependent variables are as defined in tables V – VIII. Robust standard errors are displayed in parentheses. Controls: number of private banks and number of public banks. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

TABLE XII
IMPACTS ACCORDING TO *EX-ANTE* TOTAL CREDIT (2003)

	Dependent Variable:		
	(1)	(2)	(3)
Panel A: Employment and GDP	GDP *per capita* (hundreds)	Employment rate (%)	
$Consig_{pc}$ (hundreds) × total credit *per capita* in 2003 (hundreds)	0.629***	0.271***	
	(0.060)	(0.192)	
R-squared	0.027		
Panel B: Income	Corporate tax	Average wages	Other tax
$Consig_{pc}$ (hundreds) × total credit *per capita* in 2003 (hundreds)	3.779**	37.06***	0.793**
	(1.528)	(4.343)	(0.330)
R-squared	0.009	0.245	0.036
Panel C: Banking	Private branches (hundreds)	Public Branches (hundreds)	
$Consig_{pc}$ (hundreds) × total credit *per capita* in 2003 (hundreds)	0.261***	0.0545***	
	(0.0515)	(0.0127)	
R-squared	0.104	0.342	
Panel D: Credit	Total credit (hundreds)	Total loans (hundreds)	
$Consig_{pc}$ (hundreds) × total credit *per capita* in 2003 (hundreds)	0.851***	0.579***	
	(0.112)	(0.0499)	
R-squared	0.290	0.515	
Municipality dummies	yes	yes	yes
Year dummies	yes	yes	yes
Controls	yes	yes	yes
Instrument	IV4	IV4	IV4
Observations	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500

Notes. All dependent variables are as defined in tables V – VIII. Robust standard errors are displayed in parentheses. Controls: number of private banks and number of public banks. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

8
Conclusion

This study uses microeconometric identification to address macro-level questions regarding the consequences a large-scale consumer credit expansion due to the elimination of financial frictions. For that, we explore a quasi-experimental reform in Brazil that has extended the supply of credit (at low interest rates) to individuals devoid of traditional forms of collateral. As a result, we encounter substantial multiplier effects that culminate in a large and positive impact on aggregate income. We interpret these findings to beyond the evaluation of a specific initiative. Although looking at a particular intervention (and a restricted group of borrowers), we attempt to identify (and quantify) the potential outputs of a reform that crosses off important credit barriers in a relatively small, underdeveloped and open economy.

Ultimately, this work contributes to the debate concerning the role of banking in the process of economic development. Other than providing empirical evidence for credit's ability to promoting growth, we show that an exogenous credit injection is likely to stimulate other banking services and, thereby, yield a virtuous cycle towards financial inclusion. In addition, our estimates indicate that the efficiency of microfinance policies can vary significantly when implemented in distinct locations, and discusses the validity of generalizing the findings of a given program.

On the other hand, the reform we analyze enables access to credit through an unavoidable repayment commitment. Though increasing short-term disposable income, the unbridled expansion of mechanisms alike may result in severe indebtedness in the long-run, once individuals might not always make optimal intertemporal decisions. To understand the limits between the benefits and the traps that can arise in this case and the extent to which public policies shall desire banking expansion at any circumstance remain important topics for future research.

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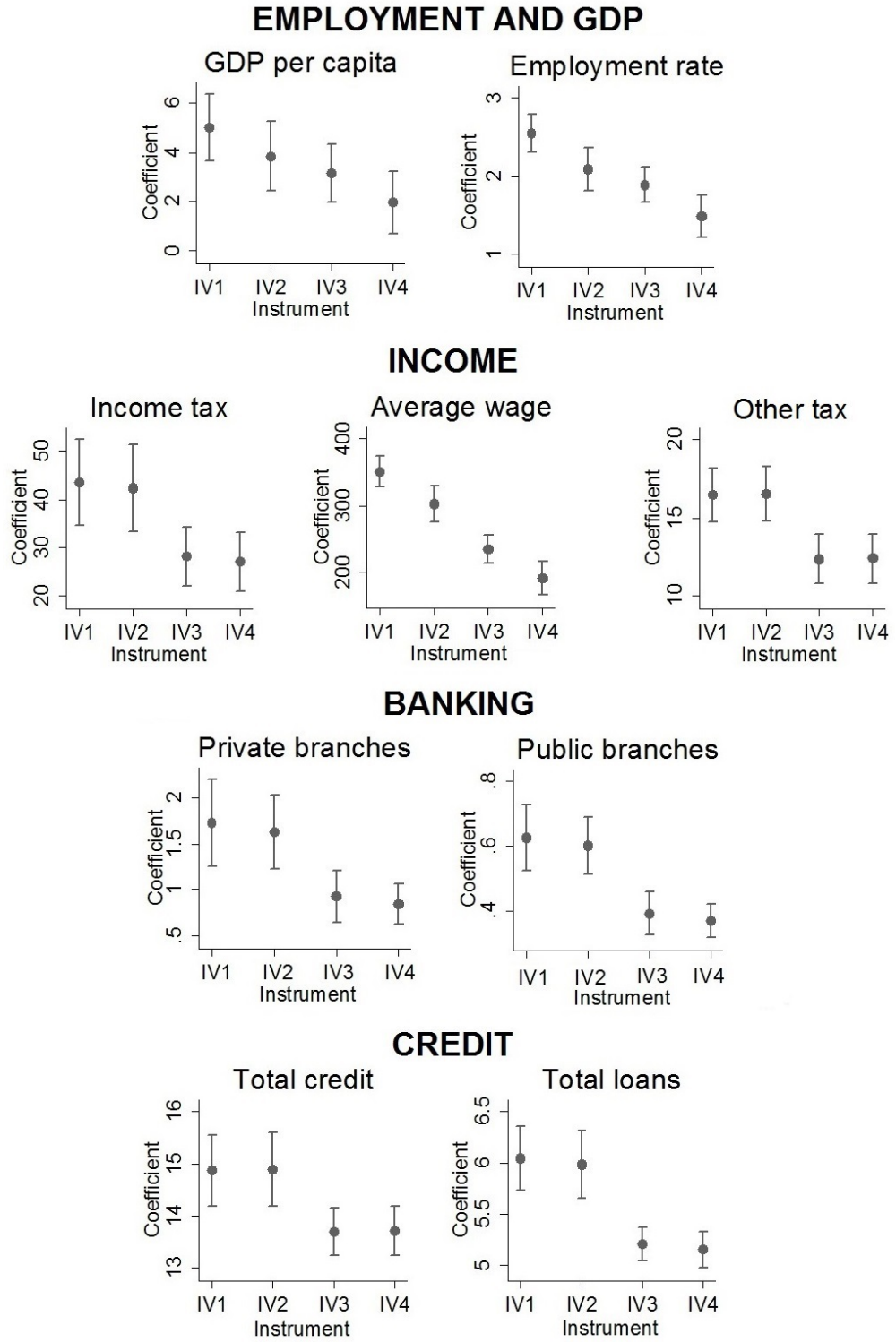
Appendix

FIGURE A
IMPACT OF THE *CONSIGNADO* TO INSS *PER CAPITA* BY INSTRUMENT SET



Notes. Figure plots second stage coefficients associated with the *consignado* to INSS *per capita* (hundreds) and their 95 percent confidence interval. Each graph regards a different outcome variables and shows these estimates for all four instrument sets proposed.

TABLE A
FIRST STAGE: ROBUSTNESS CHECK

Dependent Variable: *consignado* to INSS *per capita* (*consigpc*) in hundreds		
	(1)	(2)
IV4 × Y1 (dummy 1^{st} year)	0.834***	0.837***
	(0.0602)	(0.0590)
IV4 × Y2 (dummy 2^{nd} year)	1.054***	1.056***
	(0.0700)	(0.0689)
IV4 × Y3 (dummy 3^{rd} year)	1.420***	1.423***
	(0.106)	(0.107)
IV4 × Y4 (dummy 4^{th} year)	1.700***	1.700***
	(0.105)	(0.105)
IV4 × Y5 (dummy 5^{th} year)	2.102***	2.108***
	(0.169)	(0.168)
IV4 × Y6 (dummy 6^{th} year)	2.883***	2.883***
	(0.214)	(0.212)
IV4 × Y7 (dummy 7^{th} year)	3.433***	3.428***
	(0.203)	(0.200)
total *consignado* to INSS within the state (millions)	0.0000558***	
	(0.0000166)	
total *consignado* to INSS in the state capital (millions)		0.000247***
		(0.0000385)
Municipality dummies	yes	yes
Year dummies	yes	yes
Controls	yes	yes
Instrument	IV4	IV4
R-squared	0.781	0.784
Observations	55,000	55,000
Municipalities	5,500	5,500
F-test on interactions	437.3	487.6

Notes. Clustered standard errors are displayed in parentheses. Controls: number of private banks and number of public banks. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence. The F-test tests the joint significance of the IV4 interaction terms.

TABLE B
AVERAGE EFFECTS WHEN CONTROLLING FOR THE TOTAL CONSIGNADO WITHIN THE STATE

	Dependent Variable:		
	(1)	(2)	(3)
Panel A: Employment and GDP	GDP *per capita* (hundreds)	Employment rate (%)	
$Consig_{pc}$ (hundreds)	1.273	1.353***	
	(1.734)	(0.414)	
R-squared	0.063	0.201	
Panel B: Income	Corporate tax	Average wages	Other tax
$Consig_{pc}$ (hundreds)	23.35***	164.4***	11.76***
	(5.729)	(45.79)	(2.504)
R-squared	0.004	0.265	0.030
Panel C: Banking	Private branches (hundreds)	Public Branches (hundreds)	
$Consig_{pc}$ (hundreds)	0.702***	0.347***	
	(0.250)	(0.0507)	
R-squared	0.055	0.314	
Panel D: Credit	Total credit (hundreds)	Total loans (hundreds)	
$Consig_{pc}$ (hundreds)	13.74***	5.067***	
	(2.716)	(0.648)	
R-squared	0.076	0.128	
Municipality dummies	yes	yes	yes
Year dummies	yes	yes	yes
Controls	yes	yes	yes
Instrument	IV4	IV4	IV4
Observations	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500

Notes. All dependent variables are as defined in tables V – VIII. Clustered standard errors are displayed in parentheses. Controls: number of private banks, number of public banks and total amount of *consignado* to INSS within the state. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

TABLE C
AVERAGE EFFECTS WHEN CONTROLLING FOR THE TOTAL CONSIGNADO IN THE STATE CAPITAL

	Dependent Variable:		
	(1)	(2)	(3)
Panel A: Employment and GDP	GDP *per capita* (hundreds)	Employment rate (%)	
$Consig_{pc}$ (hundreds)	1.349	1.370***	
	(1.732)	(0.426)	
R-squared	0.063	0.201	
Panel B: Income	Corporate tax	Average wages	Other tax
$Consig_{pc}$ (hundreds)	23.88***	167.2***	11.83***
	(5.876)	(47.45)	(2.521)
R-squared	0.004	0.265	0.030
Panel C: Banking	Private branches (hundreds)	Public Branches (hundreds)	
$Consig_{pc}$ (hundreds)	0.704***	0.346***	
	(0.264)	(0.0531)	
R-squared	0.056	0.314	
Panel D: Credit	Total credit (hundreds)	Total loans (hundreds)	
$Consig_{pc}$ (hundreds)	13.77***	5.075***	
	(2.702)	(0.650)	
R-squared	0.076	0.128	
Municipality dummies	yes	yes	yes
Year dummies	yes	yes	yes
Controls	yes	yes	yes
Instrument	IV4	IV4	IV4
Observations	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500

Notes. All dependent variables are as defined in tables V – VIII. Clustered standard errors are displayed in parentheses. Controls: number of private banks, number of public banks and total amount of *consignado* to INSS in the state capital. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence.

TABLE D
FIRST STAGE: INTERACTING THE INTERVENTION VARIABLE WITH *EX-ANTE* CHARACTERISTICS

	Dependent Variable:			
	Consigpc × distance to capital (1)	*Consigpc* × GDP_{pc} in 2003 (2)	*Consigpc* × agriculture in 2003 (3)	*Consigpc* × total $credit_{pc}$ in 2003 (4)
IV4 × Y1 (dummy 1^{st} year)	256.2***	32.30***	26.64***	2.890***
	(7.699)	(1.417)	(0.641)	(0.254)
IV4 × Y2 (dummy 2^{nd} year)	305.9***	48.48***	29.24***	5.475***
	(7.230)	(1.422)	(0.606)	(0.259)
IV4 × Y3 (dummy 3^{rd} year)	392.4***	65.30***	35.88***	8.581***
	(9.209)	(1.973)	(0.740)	(0.376)
IV4 × Y4 (dummy 4^{th} year)	431.3***	88.67***	36.86***	11.56***
	(9.996)	(2.545)	(0.837)	(0.496)
IV4 × Y5 (dummy 5^{th} year)	572.6***	93.68***	54.38***	11.70***
	(13.90)	(2.771)	(1.172)	(0.471)
IV4 × Y6 (dummy 6^{th} year)	714.5***	149.1***	58.86***	22.87***
	(16.65)	(4.147)	(1.352)	(0.814)
IV4 × Y7 (dummy 7^{th} year)	732.3***	221.9***	43.17***	37.08***
	(23.01)	(7.917)	(1.522)	(1.679)
Municipality dummies	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Instrument	IV4	IV4	IV4	IV4
R-squared	0.560	0.388	0.575	0.265
Observations	55,000	55,000	55,000	55,000
Municipalities	5,500	5,500	5,500	5,500
F-test on interactions	336.4	215.0	393.3	119.2

Notes. Robust standard errors are displayed in parentheses. Controls: number of private banks and number of public banks. Significantly different than zero at 99 (***), 95 (**) and 90 (*)% confidence. The F-test tests the joint significance of the IV4 interaction terms.