Loan-To-Value Policy and Housing Loans: Effects on constrained borrowers.

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Abstract

This paper explores the effects of a loan-to-value (LTV) limit implemented on September 2013 on two subsidized segments of housing loans in Brazil, one of them through direct regulation by the macroprudential supervisor. We use comprehensive credit register information of housing loans augmented with a granular employment register. We focus on the average treatment effect on the treated borrowers, defined as the ones that would violate the LTV limit if allowed to do so. We use an adjusted difference in difference method to accommodate partially observed treatment status. Households constrained by regulation borrow housing loans with higher interest rates, shortened maturities, and, as expected, reduced loan amounts and LTV in the affected segment. These borrowers also purchase more affordable homes and are less likely to be in arrears 12 months in the future. Borrowers constrained for other reasons also meet the LTV threshold, but the resulting contract terms are more favorable, i.e. smaller interest rates and longer maturities, and they are also less likely to be in arrears 12 months in the future. We argue that the different housing loan contract outcomes for treated borrowers in each segment may be the result of a signaling effect from the macroprudential supervisor.

Keywords: LTV, loan-to-value ratio, mortgage, credit register, housing loans, macroprudential policy

JEL Classification: G21, G28

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1. Introduction

Macroprudential policies related to the housing sector represent a relevant share of the macroprudential tools used in several jurisdictions (Jacome and Mitra, 2015). One of the most common policies targeting the housing sector is imposing loan-to-value (LTV) limits for housing loans. The higher equity stake and lower leverage required by these policies are designed to increase borrower resilience and to lower bank losses during downturns. These expected effects of the policy are consistent with theoretical arguments (e.g. Campbell and Cocco (2015)) and empirical evidence (e.g. Demyanyk and Hemert (2011)). However, there are important transmission channels of LTV limits at the borrower level not well explored in the literature, including the impact on delinquencies and on contract terms at loan origination[1].

We argue that imposing LTV limits may endogenously shift several characteristics of the loan contract and therefore shift borrower behavior. Indeed, financial intermediaries may change loan terms in response to the policy, i.e. loan amounts, maturity and interest rates. As a result, otherwise highly leveraged households may settle with different loan terms, housing alternatives and repayment incentives. In fact, depending on the signal provided by the regulator and the priors and practices of financial intermediaries, related shifts may take place in other segments not originally targeted by the policy.

This paper contributes to the literature by focusing exactly on these changes in contract terms and in borrower behavior after the imposition of LTV limits, including effects in segments not originally targeted by the policy. In all cases, we focus on the effect of the policy on the subset of borrowers constrained by the policy, that is, the

[1] Regarding delinquencies, Campbell et al. (2015) is an exception to the statement, although they consider risk weights conditional on LTV, while we consider hard LTV limits - which requires a considerable departure in the methodology.

average treatment effect on the treated (ATT). We show this requires a novel identification strategy and apply it to a unique dataset with loan trajectories around the policy change.

It is natural to define constrained borrowers as the ones that would violate the LTV limit if allowed to do so. However, this creates a difficulty, since treatment status is observed only in the period before the policy limit is imposed. In principle, one could try to use borrower data from the period before the policy to estimate the propensity of borrowers being constrained and somehow use this information to recover the ATT parameter. Indeed, Botosaru and Gutierrez (2015) show this intuition is correct, proposing consistent and efficient estimators for the case of partially observed treatment status. This paper uses their estimator to recover the ATT effect of LTV limits on contractual terms.

The empirical contribution of the paper, building on this identification strategy, is estimating how contract terms and borrower behavior respond to a new regulation establishing a LTV cap of 90% for housing loans in Brazil in September, 2013. We consider two segments of housing loans and conduct independent estimation for each segment. The "target segment" is the one addressed in the regulation. The "non-target segment" is not formally subject to the LTV limit, but as we show in the paper, effectively adopted it contemporaneously (and this, therefore, constrained a subset of its borrowers). The main difference between both segments is the eligibility criteria, based on different house price ceilings and on borrower characteristics, so each segment concentrates very different kinds of borrowers. The repetition of the experiment in two independent segments offers a rare opportunity to compare the estimated effects.

We use a unique borrower-level dataset from the Brazilian supervisory credit register with loan contract information and loan repayment history for all housing loans in the period. We merge this data with the official employment register to augment the

set of individual borrower control variables, such as wage and employment. It is important to include the wage variable because it is a highly significant predictor of treatment status, and so it is crucial for identifying the parameters of interest. The dataset has over 1.3 million loans spanning a three-year period around this policy change, although we restrict the empirical analysis to subsets of this data to ensure the validity of some assumption necessary for identification.

We find evidence suggesting that treated borrowers in the target segment purchase more affordable houses, default less, and settle on housing loan contracts with less favorable terms, that is, higher interest rates and lower maturity. Reproducing the estimation procedure in the non-target segment, treated borrowers obtain more favorable loan terms after the new regulation, partially offsetting the lower leverage, while also showing less default risk. A possible explanation of these contrasting results is that the macroprudential supervisor signals excessive risks only in the target segment.

The policy measures in Brazil and our empirical approach are relevant to several similar policies adopted elsewhere. Indeed, most countries have some form of explicit or implicit LTV limit (Cerutti et al. (2015)). Yet, the international experience is heterogeneous (Darbar and Wu (2015)). Some jurisdictions implement simple, hard LTV limit as in Brazil in September 13; others combine LTV limits to complementary policies such as taxation and capital requirements; others still apply differentiated LTV limits by price buckets or geographical region. The methodology developed here for hard LTV limits can be adapted to other regulatory events by defining proper segments or isolating segments not affected by complementary policies[2].

[2] For example, in our empirical exercise, the LTV limit came with changes in eligibility criteria, and so we selected a subsample of our data not affected by that contemporaneous policy change. See below.

This wealth of policy experiments motivates a growing empirical literature that accommodates different approaches. A large part of the literature investigates the aggregate impact of LTV policies - which, to be clear, is not the object of this paper. For example, Igan and Kang (2011) find that the tightening of the LTV cap in South Korea results in lower transaction activity and slower price increases. Funke and Paetz (2012) find a small effect of LTV policy on housing prices, and a more lasting one in indebtedness. Similar results hold for other macroprudential measures (e.g. Akinci and Olmstead-Rumsey (2015)).

The empirical literature most closely related to this paper considers the impact of regulation on mortgage risk. Demyanyk and Hemert (2011) show high-LTV loans originated in the run-up to the US subprime crisis were more likely to be delinquent during the bust. Hallisey et al. (2014) document the same effect in Ireland, where mortgages with higher LTV and loan-to-income (LTI) ratios at origination are more likely to be in arrears in the future. Campbell et al. (2015) show that risk weights conditional on LTV in India affect loan delinquencies. Although this results suggest that a hard LTV limit would reduce mortgage risk, there is no evidence, as explored in this paper, of actual effects on delinquencies of policy induced hard LTV limits, much less on house choice and loan contract terms.

In summary, our main contribution to the literature is estimating borrower-level shifts in contract terms and borrower behavior resulting from LTV limit, along with the proposed empirical methodology to overcome the lack of observable treatment status when limits are binding. The estimated effects on delinquencies are in line with the priors suggested by the theoretical and empirical literature linking LTV with mortgage risk. Another important contribution to the policy debate is the suggestive evidence that LTV

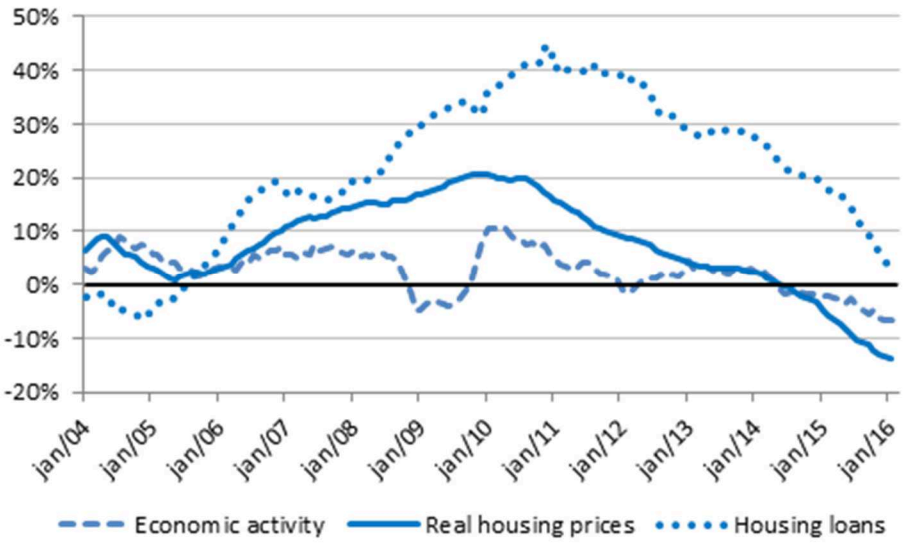
regulation seems more effective when accompanied by a signal by the macroprudential supervisor of perceived risks in segments of the mortgage market.

2. A Primer on Housing Finance in Brazil

According to Cerutti et al. (2015), Brazil is one of the few jurisdictions that experienced a credit boom in the aftermath of the financial crisis. This cycle arguably started around 2004, with lower macroeconomic uncertainty and the "Great Moderation" in other relevant trade partners. In the housing sector, legal changes that improved time to repossession in case of foreclosure provided additional momentum to housing loans and prices from the lows of the previous years.

As a result, housing finance in Brazil grew significantly since 2001, from less than 1% of GDP to 7% in 2013, while delinquency rates decreased from 7% to 1.6 % between 2004 and 2013. Pereira da Silva and Harris (2012) largely attribute this development to the legal improvements that promoted faster repossession processes, reducing the previously high loss-given-default for lenders and helped unlock the supply of housing loans. Figure 1 shows GDP growth, housing credit growth and real housing prices in Brazil to illustrate these developments, and highlights some relevant events.

Figure 1 - Economic activity, housing loans, and housing prices in Brazil, 2004-2015. All series are real annual growth rates.



The banking regulator responded to these developments by requiring lenders to follow stricter borrower monitoring processes for all housing loans as well as by implementing a LTV limit to a particular segment of the mortgage market. In order to characterize the measure and to locate it relative to similar measures in other jurisdictions, it is important to highlight the most important features of housing finance in Brazil.

The main lender in the housing loan market is the government-owned Caixa Economica Federal (henceforth CEF), with a large but declining market share of 68% as of 2015. CEF is widely considered to be specialized in housing loans, and has wide geographical coverage in Brazil. Other large banks in Brazil (Itaú, BB, Santander and Bradesco) are also important lenders, representing together 28% of the mortgage market. These other banks have a more universal bank profile, and have only recently began to allocate shares of their credit portfolios into housing loans. In Brazil, virtually all borrowers are domestic residents, and the loans are all denominated in local currency.

Housing loans in Brazil enjoy significant subsidy, which varies according to the funding source. Interest rates are subsidized, but borrowers must meet eligibility criteria.

The most relevant credit line is "SFH"[3]. In this case, funding is redirected from savings accounts. We call the second group "FGTS"[4], because it is a collection of smaller segments which share funding and eligibility characteristics. This last group has less stringent rules in terms of debt service to income (DSTI) and LTV, and also lower interest rates than SFH, but the borrower must meet an a maximum income limit. These credit segments were historically designed to foster homeownership to certain social strata, such as workers or low-to-middle classes.

The regulated (subsidized) interest rates are often lower than the yield of sovereign bonds, providing a significant incentive for households to borrow in either segment, if eligible. SFH loans are available to prospective borrowers of their first house and that are not homeowners in the same city. They are expected to borrow for residential purposes, and the house price must respect a maximum eligibility ceiling[5].

The vast majority of new SFH housing loans are non-recourse. Traditionally most housing loans follow a constant amortization schedule[6]. Unlike other jurisdictions, housing loans are not backed by governmental agencies, and interest rates are not deductible for tax purposes. Notice that the nature of the subsidy is on interest rates. Funds are (forcedly) redirected from savings accounts or provided by FGTS funds, but credit risk is carried by the banks operating these lines. All banks (private or public) have a spread over the subsidized interest rate. In practice, the interest rates of SFH loans lay between their funding cost (i.e., the yield on savings accounts, which is approximately 6%) and the maximum rate allowed in the credit segment (approximately 12%). The SFH

[3] Portuguese acronym for "National Housing System".
[4] Portuguese acronym for Workers' Severance Fund.
[5] This eligibility limit ceiling changes over time to accommodate changes in house prices. In fact, the same regulation that enacted the LTV limit for all SFH housing loans also increased the eligibility price limit up to R$ 750,000 from R$ 500,000. As a reference, these values represent 32.8 and 21.9 times the median national income in the twelve months ending in September, 2013.
[6] The LTV limit that we study, 90%, is valid for loans with this amortization schedule. Other amortization schedules, which are less prudent, were limited to a maximum of 80%. These loans are not material

credit segment also allows workers with formal private-sector employment contracts to frontload social contributions made by their employers as down payment[7].

The only housing loan segment that could offer competitive terms to the SFH is the FGTS segment. Although specific rules vary, they can be summarized by even lower interest rates than the SFH but stricter eligibility criteria: the maximum house price is considerably lower, and borrowers are limited by their wage. Borrowers that are not eligible for either segment – due to the price of the desired home, or willingness to finance a second unit, for example – have the outside option of a regular housing loan, with market interest rates. Overall, borrowers are strictly better by opting for the SFH loan if they are eligible, unless they are also eligible to FGTS loans.

LTV limit

In the context of the growth in housing price and housing credit in the country, the National Monetary Council (CMN)[8], introduced Resolution n. 4,271/2013 (CMN, 2013; henceforth "Resolution") in September, 2013. The Resolution required that SFH loans with the widely-used constant amortization schedule have a maximum LTV of 90% (the limit is more conservative, 80%, for other amortization schedules). Home equity lines of credit also were limited to a 60% LTV.

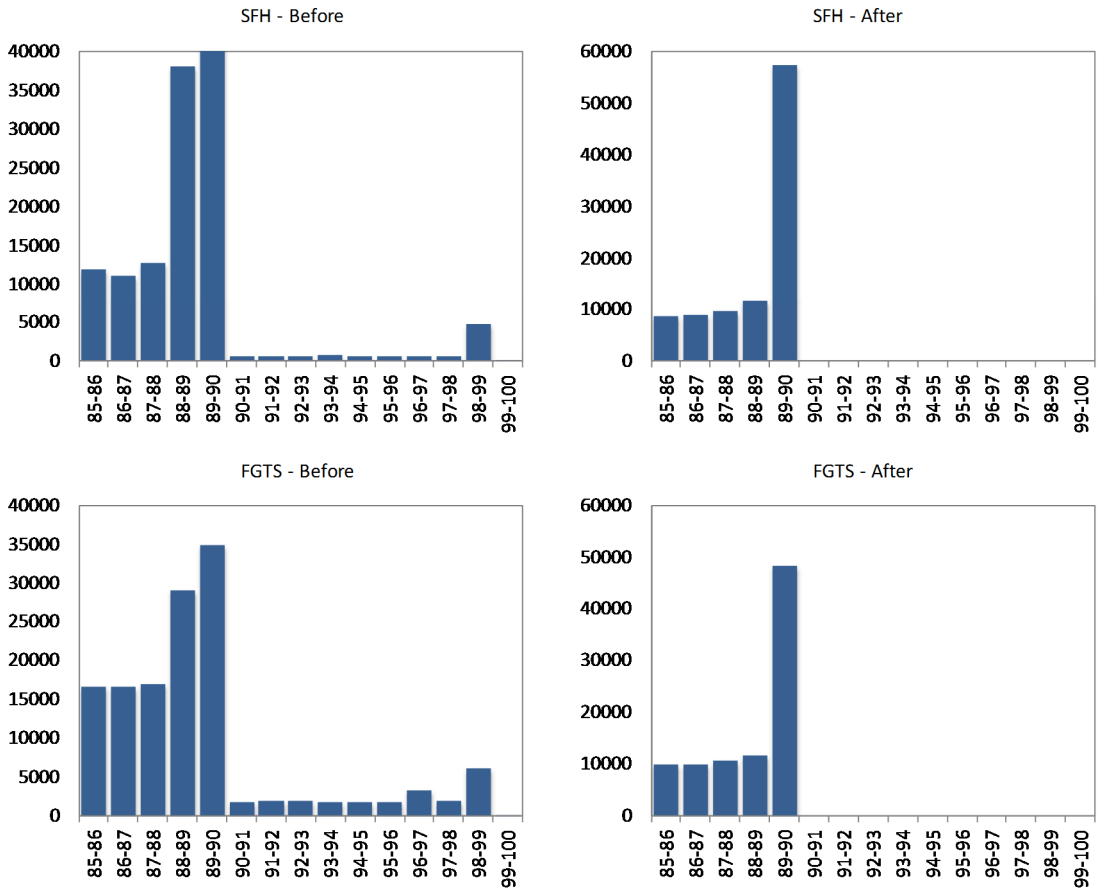
Segments other than the SFH are not addressed by the regulation and not mandated to comply with the LTV limit of 90%. However, data shows that this limit also affected market practices in the FGTS segment. Figure 2 illustrates the distribution of

[7] The social benefit is deposited in a fund called FGTS. The same fund also operates housing credit lines which represent some of the Fund´s assets. The granular data we use enables us to control for factors that may influence the marginal propensity to compensate for more down payment requirements: the employment register of each borrower has the employment type (government or private sector), wage, and tenure at current employment. Borrowers without formal jobs are identified by exclusion.

[8] The CMN is the main regulator of the financial system. The three members of the CMN are the Minister of Finance (Chairman), the Minister of Planning, Budget, and Management, and the Governor of the Central Bank of Brazil.

LTVs of new housing loans originated before the LTV regulation (January 2012 to September 2013) and after the regulation (until December 2014). Remarkably, the behavior appears to be similar. Considering this fact, and the significance of the FGTS segment, we also incorporate this segment in our analyses. Finally, it is important to highlight that the new regulation was unexpected to market participants and regulators have never used hard LTV limits in Brazil. Moreover, prior regulation strongly favored regulatory capital measures using risk weights (e.g. as a function of LTV or maturity)[9].

Figure 2. Frequency of new housing loans by LTV ranges.



[9] For example, see Martins and Schechtman (2013) for background and estimates for the impact of shifts in risk weights in auto loans made conditionally on loan maturity.

3. Methodology

This section presents the identification strategy. We follow Botosaru and Gutierrez (2015) very closely and refer to their paper for proofs and further conceptual elaboration on the particular differences-in-difference estimator adopted in this paper.

We define treated borrowers as the ones that would violate the LTV limit if allowed to do so. We consider two periods $t \in \{0,1\}$ representing a set of months before the policy and after the policy, respectively. Each borrower has two potential outcomes $Y_t(1)$ if exposed to treatment and $Y_t(0)$ if not exposed. The outcomes in our empirical application will refer to borrower repayment behavior 12 months in the future or loan contract terms, such as the LTV itself, loan amount, interest rate, maturity, and house price.

Notice that before the macroprudential regulation we can observe the treatment status of the borrowers. Indeed, treated borrowers have LTV greater than the limit (90% in our empirical application). However, after the policy shock, we can no longer distinguish constrained borrowers based on the contract characteristics. The methodology by Botosaru and Gutierrez (2015) is particularly designed for these cases where only partial treatment status is available.

Let then $D \in \{0,1\}$ represent treatment status, which is, therefore, observed only for $t = 0$. The parameter of interest is the average treatment effect on the treated (ATT), defined by $ATT \equiv E(Y_1(1) - Y_1(0)|D = 1)$. If treatment status were observed in both periods, under usual identifying assumptions, the parameter would be identified by $\theta \equiv [E(Y_1|D = 1) - E(Y_0|D = 1)] - [E(Y_1|D = 0) - E(Y_0|D = 0)]$ and one could use the sample analog of the expression for estimation and inference.

To be clear, the usual assumptions we refer to are (A1) parallel paths for treated and control group and (A2) no anticipation of the policy change. In this paper, to ensure

the parallel paths assumption, which we investigate graphically, we consider only borrowers with similar LTV levels, and ensure our results are robust to the range of LTV considered in the analysis.

The problem with the LTV limit is that we have partially-observed treatment status. Therefore, a proxy variable for treatment status is needed. Let Z be a time invariant variable observed in both periods and consider the propensity score $e_t(Z) \equiv Prob_t(D = 1|Z)$. Consider the following assumptions. (A3) stationarity: $e_0(Z) = e_1(Z) \equiv e(Z)$, meaning the policy does not affect the propensity score. (A4) relevance: $e(z_1) \neq e(z_1)$ for some z_1 and z_2, meaning the proxy variable is actually relevant to forecast treatment status. (A5) conditional independency: $E(Y_1(D)|D,Z) - E(Y_0(D)|D,Z) = E(Y_1(D)|D) - E(Y_0(D)|D)$, meaning that, conditionally on treatment status, the proxy variable may affect outcomes only homogeneously in both periods

For our application, we consider wage as the proxy variable. Although we cannot test the identifying assumptions, we argue that there are plausible. Income should have an impact on the propensity to leverage and this relation should not be time varying in the relevant time frame, at least as long as other joint determinants, such as debt levels, are not substantially different between the two periods for a specific candidate borrower. From another, less structural perspective, we can also postulate the assumptions hold by definition, since we are considering a counterfactual definition of treated borrowers as the ones that would have behaved in a certain direction in the past.

Botosaru and Gutierrez (2015) show that, for partially observed treatment status, assumptions A1-5 are sufficient to identify the ATT parameter. The result is simple. Let $\Delta E(Y|.) \equiv E(Y_1|.) - E(Y_0|.)$. It is clear that $\Delta E(Y|Z) \equiv \Delta E(\mathrm{Y}|\mathrm{Z}, \mathrm{D} = 1)e(Z) + \Delta E(\mathrm{Y}|\mathrm{Z}, \mathrm{D} = 0)\big(1 - e(Z)\big)$. Using the conditional independence assumption $\Delta E(Y|Z) \equiv \Delta E(Y|\mathrm{D} = 1)e(Z) + \Delta E(\mathrm{Y}|\mathrm{D} = 0)\big(1 - e(Z)\big)$. Stack this expression K times, one for

each value $\{z_k\}_{k=1..K}$ in the support of the proxy variable. This results in a linear system that can be solved for $\Delta E(Y|\mathrm{D}=1)$ and $\Delta E(Y|\mathrm{D}=0)$, and therefore for the θ which identifies the ATT parameter.

The estimator they propose is just the sample analog of these stacked system considering realized values of the proxy variable. Notice that this estimator, as in traditional differences-in-difference estimation, applies to a repeated cross-section sample, which is the case of our dataset. Botosaru and Gutierrez (2015) also show this is numerically equivalent to a just identified GMM estimator. The proposed GMM moment conditions allows one to deduce the asymptotic variance of the ATT parameter taking into account the uncertainty in the first step propensity score estimation. Our results are all based on this GMM estimator and associated asymptotic inference.

The authors also show in Monte Carlo experiments and applications that results are not sensitive to the model specification in the first step, which can be performed by an ordinary least squares, probit, or logit models. They also argue that the F-statistic of the first step regression should corroborate strongly the relevance assumption for the proxy variable. When presenting our results, we emphasize the F-statistic of the first step, focusing on the OLS estimation of the propensity score.

The methodology is designed to estimate the effect of a single policy intervention. In our application, the regulator also increased the price eligibility limit of the SFH housing loan segment, along with the establishment of the LTV limit in the same segment. To avoid confusion with possible effects of the increase in the home price eligibility limit, we only estimate the models with the subset of loans for which the home price was below R$ 450 thousand, thus well below the previous limit of R$ 500 thousand. As mentioned in the introduction, similar procedures might be feasible in other applications where LTV limits are used in conjunction with other measures.

4. Data

The Credit Information System (SCR), the credit register managed by Central Bank of Brazil (BCB), centralizes information about loans, endorsements, and lines of credit granted by all Brazilian financial institutions to individuals and corporate entities[10]. The information collected in the SCR comprises characteristics of the borrower, the debt contract, and the collateral; this information undergoes rigorous verification processes to ensure quality and consistency. In practice, the SCR is extensively used both for supervisory purposes by the BCB, and by lenders, when considering the riskiness of prospective borrowers. Table 1 summarizes the information we use from the SCR regarding all housing loans originated in the years 2012 to 2014.

Table 1. Housing Loans in Brazil 2012-2014

Descriptive statistics for the sample restricted to LTV higher than 85% and house price lower than BRL 450,000, which is the largest subsample used in our estimation.

SFH				N =	216,413
	Mean	St.Dev.	25%	50%	75%
Loan (Reais)	173,808	75,537	120,695	158,600	216,000
House Price (Reais)	196,049	85,188	136,260	179,866	245,401
Interest rate (p.p.)	9.08	0.48	8.85	8.85	9.14
Maturity (years)	29.88	6.60	26.92	32.08	35.00
	Yes	No			
Arrears next 12 months	2%	98%			
FGTS:				N =	228,313
	Mean	St.Dev.	25%	50%	75%
Loan (Reais)	88,084	21,685	74,638	83,363	99,800
House Price (Reais)	99,265	24,666	82,863	93,978	113,843
Interest rate (p.p.)	5.56	1.04	4.59	5.11	6.16
Maturity (years)	25.44	3.71	24.50	25.00	29.58
	Yes	No			
Arrears first 12 months	2%	98%			

10 The minimum threshold for granular information in the SCR is BRL 1,000 outstanding per borrower in each reporting month. Since this amount is very low for housing transactions, for practical reasons all housing loan contracts in Brazil are granularly detailed.

We merge loan-level information from the SCR to the official employment register of the Brazilian Ministry of Labor and Employment. This database contains information about each natural person that has at least one documented employment relationship in Brazil in a given year, and data about the employment contract with the employer. Self-employed persons, business owners and undocumented workers are not listed in the employment register. The individual data includes gender, age, years of education, and residential ZIP code. The employment information is described by employer identification, wage, tenure at current employment (as of end-year), and economic sector of employment. These two sources are merged to enable the use of several controls at the borrower level, which are summarized by Table 2.

Table 2. Borrowers Characteristics in Brazil 2012-2014

Descriptive statistics for the borrowers characteristics in sample restricted to LTV higher than 85% and house price lower than BRL450,000 and borrowers with formal jobs, which is the largest subsample used in our estimation when using the controls.

SFH:				N =	85,525
	Mean	St.Dev.	25%	50%	75%
Income (Reais)	7,203	7,165	3,594	5,657	8,755
Education (years)	8.15	1.33	7.00	9.00	9.00
Job Duration (years)	9.29	8.80	2.55	5.74	13.81
	Yes	No			
Male	63%	37%			
Govn. Employee	55%	45%			
FGTS:				N =	78,577
	Mean	St.Dev.	25%	50%	75%
Income (Reais)	2,437	1,557	1,465	2,160	2,989
Education (years)	6.92	1.63	7.00	7.00	8.00
Job Duration (years)	5.28	5.76	1.82	3.31	6.11
	Yes	No			
Male	67%	33%			
Govn. Employee	77%	23%			

Note: Estimates also control for economic sector up to three digit and zip code up to three digits. There are at most 1138 sectors iand 29,204 zip codes in the subsamples of the data considered in the paper.

5. Results

Tables 3 and 4 present the results for the SFH segment. The difference between the two tables is that the second controls for borrower characteristics. Recall the controls come from the employment registry, and therefore the second table refers to a subsample of borrowers that have formal employment relationships. Table Tables 5 and 6 present the analogous results for the FGTS segment, with the exact same structure.

In each table, the different LTV cutoff levels restrain the set of control borrowers. Alternative cutoff levels provide robustness checks against the possibility that the treated and control borrowers are not comparable. For this reason, our preferred specifications have higher LTV cutoffs, such as the last columns of each table. Results for all borrowers, including those without formal employment contracts, are broadly similar in spite of using less borrower controls, and we focus the discussion on the results with controls. Figure 3 illustrate the effects and visually corroborates the parallel trends assumption substituting treatment status for an indicator of its likelihood.

The imposition of a maximum LTV limit for new contracts improves the repayment behavior of treated borrowers, consequently reducing their risk: the probability of arrears of 15 days or more in the first twelve months of the loan[11] decreases by approximately 9 percentage points ("pp") and 2 pp in the SFH and the FGTS segment respectively. Importantly, this result is statistically significant when controlling for tenure at current employment, economic sector of employment, and other borrower characteristics that may correlate with job security, and consequently, repayment ability. Therefore, LTV limits meaningfully reduce the credit risk of treated borrowers, as expected.

[11] Note that we measure credit risk using repayment behavior in the first twelve months of the loan, but it is reasonable to assume that the incentive to default falls sharply due to the constant amortization schedule. Therefore, this information would be enough to make inferences on borrower credit risk.

Table 3. Average treatment effect on constrained borrowers, SFH loans only, no controls

Treated borrowers would have LTV>90% if allowed to do so. The table reports the average treatment effect on the treated for each variable, as calculated by the two stage estimator by Botosaru and Gutierrez (2015). The first stage uses pre-regulation data to estimate the propensity score to borrow with LTV>90%, conditional on house price and borrower income. F statistics refer to the first stage and, following Botosaru and Gutierrez (2015), show the first stage regression sucessfully discriminate treated borrowers from the control group. The second stage identifies the effect of the LTV cap using the first-stage propensities. Standard errors take into account the first stage estimation uncertainty. Columns show results estimated for borrowers with a minimum LTV increasingly closer to the capped LTV level of 90% for robustness. Minimum LTVs closer to 90% increase the adherence to the paralell trends assumption implicit in the estimator. Only borrowers that financed homes valued BRL 450,000 or lower are considered, to avoid confusion between the effects of the LTV cap and a simultaneous increase in housing price eligibility.

	LTV>80%	LTV>85%	LTV>87%	LTV>88%
LTV /1	-8.93	-9.65 **	-9.00 ***	-8.70 ***
	(5.89)	(4.42)	(3.22)	(2.60)
loan (log)	-0.25 *	-0.50 **	-0.64 ***	-0.59 ***
	(0.13)	(0.20)	(0.20)	(0.16)
interest rate (p.p.)	0.13	0.61 **	0.74 ***	0.70 ***
	(0.09)	(0.25)	(0.24)	(0.19)
maturity (years)	-2.25 **	-3.13 ***	-3.34 ***	-2.95 ***
	(1.06)	(0.96)	(0.86)	(0.76)
prob. arrears first 12 months (p.p.)	-10.52	-5.14	-3.31	-1.70
	(0.09)	(0.04)	(0.03)	(0.02)
house price (log) /2	-0.61	-0.33 **	-0.32 ***	-0.23 ***
	(0.37)	(0.13)	(0.10)	(0.07)
F (first stage)	1,093	1,104	1,110	1,129
N	168,588	121,812	99,305	86,868

/1 Before the LTV cap, the average LTV for SFH borrowers with LTV>90% is 96.52%. Hence, the effect of the cap on the average LTV should be around 6.52 p.p. Larger effects than this distance could be interpreted as evidence that treated borrowers would further increase their LTV if left unconstrained.
/2 The first stage regression for estimating the effect on house prices is conditional only on borrower income, not on house prices.

Table 4. Average treatment effect on constrained borrowers, SFH loans only, with controls

Treated borrowers would have LTV>90% if allowed to do so. The table reports the average treatment effect on the treated for each variable, as calculated by the two stage estimator by Botosaru and Gutierrez (2015). The first stage uses pre-regulation data to estimate the propensity score to borrow with LTV>90%, conditional on house price and borrower income. F statistics refer to the first stage and, following Botosaru and Gutierrez (2015), show the first stage regression sucessfully discriminate treated borrowers from the control group. The second stage identifies the effect of the LTV cap using the first-stage propensities. Standard errors take into account the first stage estimation uncertainty. Columns show results estimated for borrowers with a minimum LTV increasingly closer to the capped LTV level of 90% for robustness. Minimum LTVs closer to 90% increase the adherence to the paralell trends assumption implicit in the estimator. Only borrowers that financed homes valued BRL 450,000 or lower are considered, to avoid confusion between the effects of the LTV cap and a simultaneous increase in housing price eligibility. The sample is restricted to borrowers with formal employment. Controls include gender, years of education, tenure at current employment, a dummy for public-sector employment, economic sector, and ZIP code.

	LTV>80%	LTV>85%	LTV>87%	LTV>88%
LTV /1	-11.28 ***	-9.35 ***	-8.18 ***	-7.74 ***
	(2.67)	(1.49)	(0.99)	(0.77)
loan (log)	-0.43 ***	-0.46 ***	-0.49 ***	-0.42 ***
	(0.09)	(0.06)	(0.05)	(0.04)
interest (p.p.)	-0.04	0.39 ***	0.45 ***	0.42 ***
	(0.04)	(0.06)	(0.06)	(0.04)
maturity (years)	-0.30	-1.68 ***	-2.20 ***	-2.30 ***
	(0.58)	(0.40)	(0.37)	(0.34)
prob. arrears first 12 months (p.p.)	-16.69 ***	-11.09 ***	-9.34 ***	-8.60 ***
	(0.05)	(0.02)	(0.02)	(0.01)
house price (log) /2	-0.49 ***	-0.34 ***	-0.34 ***	-0.28 ***
	(0.10)	(0.05)	(0.04)	(0.03)
F (first stage)	1,093	1,104	1,110	1,129
N	68,296	48,614	39,517	34,557

/1 Before the LTV cap, the average LTV for SFH borrowers with LTV>90% is 96.52%. Hence, the effect of the cap on the average LTV should be around 6.52 p.p. Larger effects than this distance could be interpreted as evidence that treated borrowers would further increase their LTV if left unconstrained.
/2 The first stage regression for estimating the effect on house prices is conditional only on borrower income, not on house prices.

The housing loan contract terms and the house price of the treated borrowers in the SFH and FGTS segments diverge materially. In both segments, prospective borrowers that are constrained by the LTV cap must manage a viable alternative to the increased down payment requirement. Treated borrowers in each segment respond differently and obtain opposite outcomes in their housing loan contracts, even when controlling for individual borrower characteristics.

We estimate that the price of homes that are financed by treated SFH borrowers falls, on average, by 23% to 34% after the LTV regulation. The magnitude of these changes is economically significant, and suggests that treated SFH borrowers choose more affordable housing when faced with maximum LTV requirements.

Indeed, we find that the loan amount falls more than the home price itself, which is coherent with the lower LTVs we find for these treated SFH borrowers. This result holds for all combinations of individual controls and LTV cutoff levels. The reduction in LTV that we find for treated SFH borrowers is comparable to the difference between their average LTV before the regulation and the regulatory maximum of 90%. Therefore, these borrowers will purchase more affordable housing, but even still, they will only provide a down payment that accommodates to the minimum required amount.

In spite of these seemingly lower risk characteristics, treated SFH borrowers end up with less favorable housing loan contracts. The interest rates increase by 0.4 to 0.7 pp per year, and the contractual maturities are 1.5 to 3 years shorter. A simple simulation illustrates the economic significance of this result: for a constant loan size, monthly repayment values increase approximately 2% to 5% due to the higher interest rates and an additional 1% to 2% because of shorter maturities.

Table 5. Average treatment effect on constrained households, FGTS segment, no controls

Treated borrowers would have LTV>90% if allowed to do so. The table reports the average treatment effect on the treated for each variable, as calculated by the two stage estimator by Botosaru and Gutierrez (2015). The first stage uses pre-regulation data to estimate the propensity score to borrow with LTV>90%, conditional on house price and borrower income. F statistics refer to the first stage and, following Botosaru and Gutierrez (2015), show the first stage regression sucessfully discriminate treated borrowers from the control group. The second stage identifies the effect of the LTV cap using the first-stage propensities. Standard errors take into account the first stage estimation uncertainty. Columns show results estimated for borrowers with a minimum LTV increasingly closer to the capped LTV level of 90% for robustness. Minimum LTVs closer to 90% increase the adherence to the paralell trends assumption implicit in the estimator. Only borrowers that financed homes valued BRL 450,000 or lower are considered, to avoid confusion between the effects of the LTV cap and a simultaneous increase in housing price eligibility. Although FGTS segment was not required to impose the LTV cap at 90, it was de facto imposed by banks.

	LTV>80%	LTV>85%	LTV>87%	LTV>88%
LTV /1	-5.75 ***	-5.57 ***	-5.42 ***	-5.40 ***
	(0.58)	(0.43)	(0.38)	(0.37)
log_loan	-0.45 ***	-0.24 ***	-0.14 ***	-0.09 ***
	(0.04)	(0.02)	(0.02)	(0.02)
interest	-1.86 ***	-1.20 ***	-0.89 ***	-0.71 ***
	(0.20)	(0.10)	(0.08)	(0.07)
maturity	-1.49 ***	-0.60	-0.41	-0.43
	(0.48)	(0.50)	(0.53)	(0.56)
arrears15	-0.02 *	-0.01	-0.02 *	-0.02 *
	(0.01)	(0.01)	(0.01)	(0.01)
log_price /2	0.28	0.16	0.07	0.03
	(0.19)	(0.11)	(0.08)	(0.07)
F (first stage)	5,175	4,166	3,421	2,945
N	219,931	136,527	103,401	86,411

/1 The average LTV among LTV>90% minus 90 in SFH segment is -5.48. The effect on LTV should be around this. Effects higher than this show the statistic in the post regulation would be higher had regulation not been issued.
/2 The first stage regression for estimating the effect on house prices does not condition on house prices, only on household income.

Table 6. Average treatment effect on constrained households, FGTS segment, with controls

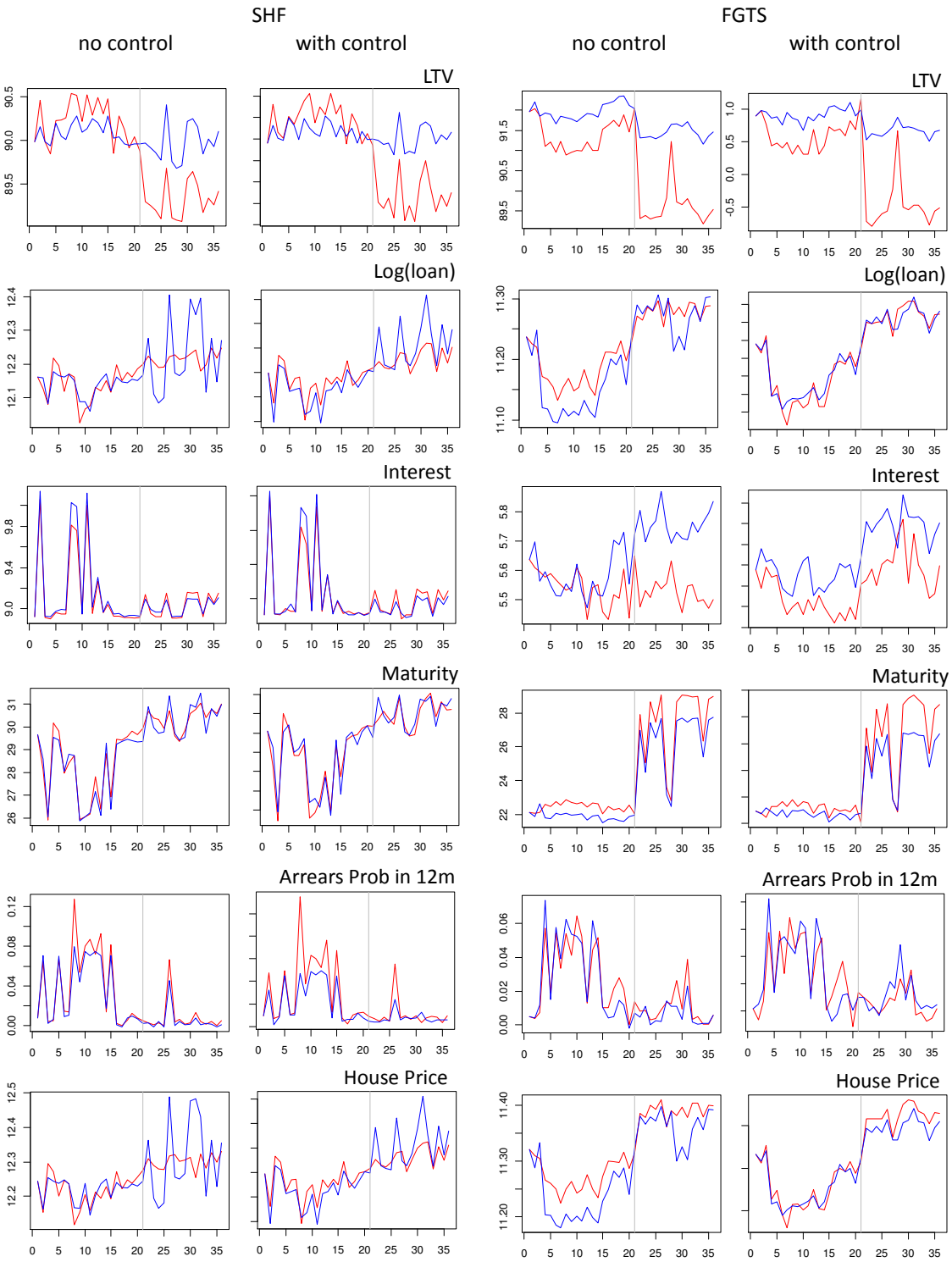
Treated borrowers would have LTV>90% if allowed to do so. The table reports the average treatment effect on the treated for each variable, as calculated by the two stage estimator by Botosaru and Gutierrez (2015). The first stage uses pre-regulation data to estimate the propensity score to borrow with LTV>90%, conditional on house price and borrower income. F statistics refer to the first stage and, following Botosaru and Gutierrez (2015), show the first stage regression sucessfully discriminate treated borrowers from the control group. The second stage identifies the effect of the LTV cap using the first-stage propensities. Standard errors take into account the first stage estimation uncertainty. Columns show results estimated for borrowers with a minimum LTV increasingly closer to the capped LTV level of 90% for robustness. Minimum LTVs closer to 90% increase the adherence to the paralell trends assumption implicit in the estimator. Only borrowers that financed homes valued BRL 450,000 or lower are considered, to avoid confusion between the effects of the LTV cap and a simultaneous increase in housing price eligibility. Although FGTS segment was not required to impose the LTV cap at 90, it was de facto imposed by banks. The sample is restricted to borrowers with formal employment. Controls include gender, years of education, tenure at current employment, a dummy for public-sector employment, economic sector, and ZIP code.

	LTV>80%	LTV>85%	LTV>87%	LTV>88%
LTV /1	-5.01 ***	-4.24 ***	-4.06 ***	-4.01 ***
	(0.17)	(0.11)	(0.10)	(0.10)
loan	-0.06 ***	0.00	0.02 *	0.02 **
	(0.01)	(0.01)	(0.01)	(0.01)
interest	-0.75 ***	-0.31 ***	-0.21 ***	-0.13 ***
	(0.04)	(0.03)	(0.03)	(0.03)
maturity	1.46 ***	1.93 ***	2.11 ***	2.13 ***
	(0.20)	(0.18)	(0.17)	(0.16)
arrears15	-0.04 ***	-0.03 ***	-0.02 ***	-0.02 ***
	(0.01)	(0.00)	(0.00)	(0.00)
log_price /3	0.19 ***	0.07 ***	0.01	-0.02
	(0.04)	(0.02)	(0.02)	(0.01)
F (first stage)	5,175	4,166	3,421	2,945
N	76,271	47,583	36,202	30,378

/1 The average LTV among LTV>90% minus 90 in SFH segment is -5.48. The effect on LTV should be around this. Effects higher than this show the statistic in the post regulation would be higher had regulation not been issued.
/2 The first stage regression for estimating the effect on house prices does not condition on house prices, only on household income.

Figure 3. Monthly averages of pseudo treated and non-treated

Pseudo-treated (red) and pseudo-non-treated (blue) averages. The policy date is the gray vertical line. Above average propensity of treated is defined as pseudo-treated and below average as pseudo-non-treated. The propensity is from the first stage regressions. The figures suggest the direction and economic significance of the effects. They also informally support the parallel trends assumption.



The fact that these borrowers are taking loans with shorter maturity, even as interest rates are higher, suggests that lenders push for less favorable contractual terms for treated SFH borrowers. Note that, for estimation purposes, the untreated borrowers are those with LTV close enough (from below) to the maximum limit.

Interestingly, housing loan contracts in the FGTS segment, left out of the LTV regulation but was also constrained in practice, had different outcomes than SFH loans. The price of financed homes of treated FGTS borrowers is unaffected by the LTV limit. Compensating for the higher down payment for the same home, both interest rates and loan maturity become more favorable and produce lower monthly installments, moving in an opposite direction when compared to loan contracts to treated SFH borrowers.

Several explanations can be found for the difference between outcomes of treated SFH and FGTS borrowers, such as a different average stock of disposable wealth owned by FGTS borrowers or other incentives that are observable to banks but not to us. *However, we cannot discard the possibility that the opposite responses are a result of a strong signal by the macroprudential supervisor about a buildup of systemic vulnerability in the specific segment targeted by the regulation, the SFH segment.*

The middle class, which largely relies on SFH loans to finance their housing, represented an important source of systemic risk in the U.S. during the 2007 housing bubble (Adelino, Schoar and Severino (2016)). In Brazil, the BCB is the banking supervisor and is widely considered to be *de facto* the main responsible for financial stability. Hence, it is reasonable to assume that the market interprets a separated regulation as a sign from the BCB that this specific segment may pose systemic vulnerabilities in September, 2013.

Two caveats to our results are worth mentioning. The LTV limit that we study was relatively high (90%) when compared to other caps established in several

jurisdictions, ranging from 70% to 80%. The effect could be subject to nonlinear dynamics, and thus our results would not be directly translatable to other settings. We are not able to control for prospective borrowers that were driven out of the housing loan market or postponed home ownership. The pre-regulation average LTV for treated borrowers were 96.5% and 95.5% for the SFH and FGTS segments, respectively – not far away from the regulatory limit. Therefore, we could safely argue that a relevant portion of the treated borrowers indeed adjusted their behavior and their housing loan contract.

6. Conclusion

We show evidence that unexpected LTV limit regulation affects housing loan contract terms and the subsequent behavior in the subset of borrowers constrained by the new regulation. Loan repayment behavior improves in both house loan segments considered in the paper, while loan contract terms other than LTV become less favorable to the borrower depending on the segment.

In the SFH segments, directly affected by the LTV regulation, the average housing loan contracts for treated borrowers have higher down payment requirements, higher interest rates, and shorter maturities. Borrowers apparently compensate these factors by purchasing more affordable homes. They also make higher down-payments as their LTV decrease approximately to the new maximum limit. The resulting outcome of all those shifts is an improved repayment behavior. Treated FGTS borrowers, who are also constrained to the same LTV limit, settle with housing loan contracts with lower interest rates and longer maturities, and finance homes at the same price level as before, with an overall positive effect on repayment behavior.

The comparison between both segments studied in this paper suggests that LTV regulation is more effective when accompanied by a signal by the macroprudential

supervisor of perceived risks in a particular segment. That is, in the segment targeted by the policy, and only in this segment, loan contract terms become broadly less favorable and house prices relatively lower. This is consistent with lower credit and house price growth, albeit in a small segment of the market. It clearly suggests borrowers in the targeted segment were relatively more constrained by the policy.

The methodology applied in this paper enables the study of policy measures that are both relevant and widespread: macroprudential policies that constrain the menu of possible debt contracts. These "asset-side macroprudential policies" (CGFS, 2012) constitute an important part of the supervisory toolbox to contain the buildup of systemic risk. The empirical approach suggested in the paper is therefore of broad significance.

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