

IMPACT ON MORTGAGE LENDING OF A REDUCTION IN SPANISH HOUSEHOLDS' BORROWING CAPACITY

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Abstract

This article examines, by means of a natural experiment, the effects on mortgage lending of exogenous changes in Spanish households' borrowing capacity. The nature of these changes allows their implications to be interpreted as analogous to those that could arise from the introduction of macroprudential borrower-based measures (BBMs) aimed at limiting leverage at loan origination. The empirical analysis conducted utilises data generated by a temporary change in 2011 in the tax treatment of principal residence purchases that was applied in regions under the ordinary tax regime, but not in those subject to special tax regimes, and that was subsequently retroactively reversed. This episode created an exogenous constraint on the borrowing capacity of potentially affected households without permanently altering their disposable income. Using micro data from the Banco de España's Central Credit Register and applying a difference-in-differences strategy with exhaustive controls for supply and demand, we find significant reductions in new mortgage loan amounts and loan-to-value (LTV) and loan-to-income (LTI) ratios, as well as in the future probability of default on those loans. The evidence aligns with the effects attributed to BBMs in the literature and provides relevant insights applicable to ex ante evaluation of BBMs in the Spanish context.

Keywords: macroprudential measures, borrower-based measures, BBMs, LTV, LTI, borrowing.

1 Introduction

Ex ante evaluation of macroprudential borrower-based measures (BBMs), designed to constrain borrower indebtedness and curb accumulation of systemic risk, is key to assessing their potential efficacy and appropriate calibration, and evaluating any potential unintended impacts on credit markets.

In mortgage lending, these tools include limits on the loan-to-value (LTV) ratio, the loan-to-income (LTI) ratio and the debt service-to-income (DSTI) ratio. Such measures have been widely adopted in multiple jurisdictions and there is credible evidence of their success in both bolstering banking system resilience and moderating credit cycles.

In Spain, the regulatory framework enabling the activation of BBMs was developed between 2018 and 2021 to strengthen financial stability by containing credit risk linked to excessive household borrowing.¹ Activating these tools imposes quantitative restrictions on mortgage

¹ These measures were introduced in Spain through [Royal Decree-Law 22/2018](#) and [Royal Decree 102/2019](#). Implementation was completed through [Banco de España Circular 5/2021](#), which amended [Banco de España Circular 2/2016](#) to include Articles 75 and 76.

lending standards (for instance, through caps on LTV, LTI or DSTI ratios) that exogenously constrain borrower leverage at origination. However, given the absence of sufficient signals of systemic risk, these tools remain as yet unused. This lack of practical application means there is little direct empirical evidence available on which to model their potential effects or properly calibrate their parameters.

In this context, using alternative approaches to assess, preliminarily, the potential impact of implementing a BBM policy in Spain is particularly relevant. Such approaches may include simulations based on theoretical models and reviews of evidence drawn from international experience (see Carro, Galán, Martorell and Vegas, 2025). A complementary strategy is to analyse past economic policy episodes that, while not constituting macroprudential measures, generated comparable effects on household borrowing decisions. Adopting this strategy, this article examines the temporary removal in 2011 of the tax deduction on principal residence purchases across most of Spain. We interpret this intervention as a shock that exogenously reduced the borrowing capacity of certain households and produced effects analogous to those of a BBM designed to limit borrower leverage relative to income. Annex 1 formalises this analogy between the fiscal shock and a BBM, treating it as a local and conditional equivalence valid for the set of households subject to the LTI/DSTI ratio constraints at origination.

The fiscal measure under analysis entered into force on 1 January 2011 and entailed the complete removal of the tax deduction for principal residence purchases for taxpayers residing in regions operating under the ordinary tax regime and receiving an income exceeding €24,000.² For affected households, the measure reduced the sum of future disposable income that could be committed to mortgage repayments, thereby constraining the maximum loan amount. Thus, like the various forms of macroprudential lending limits,³ this exogenous fiscal shock restricted household borrowing capacity.⁴ Unexpectedly, the deduction was reinstated in its original form on 30 December of the same year, allowing affected mortgage holders to benefit from the deductions for the tax year in which the loan was granted and thereby restore their disposable income as if the measure had never been introduced.

This combination of factors, coupled with the absence of concurrent changes in other policies,⁵ allows us to establish quasi-experimental conditions by comparing unaffected regions against

2 This fiscal reform was introduced by Law 39/2010 of 22 December 2010 on the State Budget for 2011. The threshold applies to the taxpayer's taxable income. For further details, see Section 3.

3 BBMs can take very different forms. While we have cited limits on LTV, LTI and DSTI ratios, BBMs can generally be expressed either as a constraint on the maximum loan amount (L) a household can access or as a cap on the fraction of the discounted future income (F) it can commit to the loan, defined as the sum of the present value of that income (ΣIncome). For instance, the constraint $LTV < 1$ implies $L < V$, where V denotes the property value, which can also be interpreted as a cap on the maximum fraction of future income committed, $F < V / \Sigma \text{Income}$. Similarly, the constraint $LTI < 1$ implies $L < I$, where I represents the borrower's current income, likewise restricting the fraction of future income that they can commit to $F < I / \Sigma \text{Income}$.

4 The analogy is only partial since a fiscal measure may also affect households for which a macroprudential constraint would not be binding and, more broadly, it may also affect the sum of available future income (ΣIncome). In the episode studied, however, this difference is limited, since the income shock was temporary and subsequently retroactively reversed, reinforcing its comparability with a constraint on borrowing at origination.

5 On 20 August 2011, a temporary reduction in value added tax (VAT), lowering the rate applicable to new-build home purchases from 8% to 4%, entered into force. While this measure could have triggered anticipation effects, the risk in this case is limited

regions⁶ in which the removal of the deduction generated an exogenous demand shock that influenced home purchase decisions and the terms and conditions of the mortgages taken out during this period. Moreover, the retroactive reinstatement of the deduction meant that, ex post, the disposable income of the households affected by its earlier removal was not actually reduced. Thus, the temporary elimination of the deduction operated solely through a reduction in new mortgage loan amounts and, consequently, through lower leverage, allowing the measure's effect on future default rates to be isolated.

In short, this shock's distinctiveness lies in three specific features: i) its non-application in the regions under special tax regimes (Basque Country and Navarre), where no further changes were made to the rate or other aspects of the tax, enabling us to construct a control group unaffected by the measure; ii) its focus on high-income households, which facilitates design of the identification strategy in the absence of individual household income data; and iii) its unexpected retroactive reinstatement at the close of 2011, which neutralises the permanent effect on disposable income and thus isolates the channel constituting financial leverage on loan risk.

The results show that, in the short term, the temporary removal of the home purchase deduction led to a 6% decrease in new mortgage loan amounts (compared with those originated in regions under special tax regimes, which were unaffected by the change), resulting in a 3.3 percentage point (pp) reduction in the LTV ratio and a 24 basis point (bp) reduction in the LTI ratio. Neither the property's appraisal value nor interest rates were impacted. As a consequence, mortgages granted in the areas affected by the measure recorded a decrease in future probability of default, firstly because borrower leverage at origination was lower and, secondly, because household disposable income was not compromised by virtue of both the temporary nature of the deduction's removal and its retroactive application in the same tax year. Moreover, while aggregate new mortgage lending fell by 23% there were no observed effects on consumer lending.

To conduct this analysis we drew on the Banco de España's Central Credit Register (CCR). The month-by-month information in this database allows us to identify all new mortgages taken out on principal residences between July 2010 and June 2011 across all of Spain. The database contains detailed information on each transaction (including the amount granted, term, interest rate applied and origination date), as well as relevant data on borrowers (such as their sector of activity and age). It also records the residence's postcode, enabling each household to be

since the estimation window closed in June 2011 and the reduction was announced unexpectedly as part of an emergency package. Regardless, for greater robustness we excluded from the sample all transactions identified as new-build purchases (based on construction date) without this exclusion significantly altering the results.

6 Ideally, having access to individual household income at loan origination would allow absolute precision when analysing the impact of the measure. No such data, however, are available. For this reason, as a proxy we use the average income of the postcode in which the borrower is located. In general, all the regions operating under the ordinary tax regime are affected to some degree by the removal of the deduction (as they include households with incomes above €24,000), while those operating under special tax regimes remain unchanged. Thus, in the baseline regression we work only with mortgages granted in regions under special tax regimes and with those granted to high-income households in regions under the ordinary tax regime. Section 4.1 provides a detailed explanation of the sample considered.

assigned an average income based on external data. The CCR likewise identifies the financial institution granting each loan, allowing exhaustive bank-level controls to be incorporated.

The article is structured as follows: First, we review the literature on the effects of macroprudential policies designed to limit borrower indebtedness on various mortgage indicators in a variety of countries. Next, in Section 3, we describe in detail the fiscal shock under analysis, discuss its analogy with a demand shock that constrains bank borrowing and explain the databases used. In Section 4, we set out the identification methodology adopted and analyse the effects of the temporary removal of the tax deduction on the characteristics of the new mortgages granted, supplementing the results with a set of additional robustness checks. Finally, in Section 5, we present the conclusions drawn from the analysis.

2 Review of the academic literature on the effects of borrower-based macroprudential policies

In the wake of the global financial crisis, research into the effects of macroprudential BBMs expanded substantially, yielding a vast body of both theoretical and empirical literature on the subject.⁷

The theoretical literature, which predominantly employs general or partial equilibrium models, indicates that BBMs impact credit, house prices and financial stability. Specifically, easing caps on LTV, LTI or DSTI ratios, among others, tends to drive up house prices and household indebtedness, thus heightening vulnerability and curbing consumption in the event of adverse shocks. These studies also find that such measures complement bank capital requirements and monetary policy while bolstering household resilience. More recent literature analysing agent-based models shows that BBMs reduce indebtedness, credit risk and probability of default.⁸

The empirical literature's first strand examines the relationship between loan origination standards and default risk, particularly in the US and UK mortgage markets. Broadly, it finds that high LTV and loan service-to-income (LSTI) ratios significantly increase default risk. Moreover, the effect is non-linear and intensifies sharply beyond certain thresholds.⁹ It also shows that simultaneously applying BBMs that cap loan amounts relative to both property values and borrowers' repayment capacity enhances their effectiveness.¹⁰ A notable study in this strand, focused on Spain (Galán and Lamas, 2025), reveals that mortgages with high loan-to-price (LTP) ratios had significantly higher probabilities of default, suggesting that LTP should complement LTV when assessing mortgage risk.

A second strand of the empirical literature examines whether BBMs, through their impact on macroeconomic and financial aggregates, have effectively contained vulnerabilities affecting

7 For a comprehensive review of this literature, see Carro, Galán, Martorell and Vegas (2025).

8 Herrera (2025); Ingholt (2022); Chen, Finocchiaro, Lindé and Walentin (2023); Alpanda and Zubairy (2017).

9 May and Tudela (2005); Ambrose, Capone and Deng (2001); Deng, Quigley and Order (2000).

10 Hejlová, Holub and Plašil (2021), and Haughwout, Peach and Tracy (2008).

financial stability. These studies show that while LTV and DSTI caps effectively curb excessive growth in household borrowing during expansionary phases of the financial cycle, in crises their activation has no significant impact since the banks themselves tend to implement stricter credit underwriting standards than required by the regulations.¹¹

Finally, a third strand of the empirical literature on the impact of BBMs uses granular loan- or borrower-level data, which enables the identification of transmission mechanisms and heterogeneous effects according to the stage of the business cycle or the institutional framework. Peydró, Rodríguez-Tous, Tripathy and Uluc (2024) show that in the United Kingdom, introducing an LTI ratio cap reduced both lending and default rates among lower-income borrowers while moderating house price growth in areas where banks were granting the highest-risk loans. Meanwhile, in the Netherlands, Bekkum, Gabarro, Irani and Peydró (2024) find that LTV caps reduced mortgage borrowing, house prices and default rates, particularly among low-income borrowers.

In conclusion, the literature reviewed provides evidence that BBMs are effective in reducing the probability of default, dampening credit growth in expansionary phases and, broadly, strengthening the resilience of the financial system. However, their effects vary according to the stage of the cycle and the financial conditions and are heterogeneous across borrowers depending on borrower income and other socio-economic characteristics. The literature also suggests that the flexible and simultaneous application of different BBMs enhances their effectiveness and reduces unintended consequences.

3 Details on the removal of the deduction for principal residence purchases in 2011, identification strategy and databases used

3.1 2011 change in deduction for principal residence purchases

In Spain, the tax deduction on investment in the principal residence had for decades been one of the most significant fiscal benefits available to taxpayers. Until 2010, the framework broadly allowed a 15% deduction in the tax payable on amounts invested (principal, interest, mandatory insurance, etc.)¹² in the purchase or refurbishment of the taxpayer's principal residence. The maximum amount eligible for relief was capped at just over €9,000 per annum and there were no limits by income. This deduction could therefore yield mortgage holders a maximum tax saving of €1,356 per year. Law 39/2010 on the State Budget for 2011 introduced a sweeping change by linking the deduction to the taxpayer's income and removing it entirely for annual incomes above €24,107.¹³

11 Claessens, Ghosh and Mihet (2013); Lim et al. (2011).

12 The deduction also applied to cash purchases, as it was based on the amount invested in acquiring the main home and there was no requirement to finance the purchase through third parties.

13 The Law stipulated that for new borrowers with a taxable income of €17,700 or less, the existing deduction would remain in place, while for those with taxable incomes between €17,700 and €24,107, the maximum amount eligible for deduction would be reduced linearly, disappearing entirely above the €24,107 threshold. The 2011 reform also included a transitional regime to protect those who had purchased their principal residence before 31 December 2010.

The 2011 framework also had a clear territorial dimension: the income-based restriction applied only to Spain's autonomous communities operating under the ordinary tax regime, leaving the Basque Country and Navarre – where regulatory power over personal income tax has been devolved – unaffected. In practice, this created a fiscal divide in 2011 between regions under the ordinary regime, where the new income threshold applied, and those under special tax regimes, which maintained their existing rules.

Thus, on 1 January 2011, the universal nature of the tax deduction for principal residence purchases was abandoned in favour of an income criterion that effectively eliminated the deduction for incomes above €24,000 in regions under the ordinary tax regime. This amendment, however, proved temporary. Following the general election on 20 November 2011, a change of government led to the reinstatement of the deduction in its original form, without income thresholds. [Royal Decree-Law 20/2011](#) of 30 December 2011 retroactively restored the deductions that had been in place prior to Law 39/2010, allowing taxpayers to benefit from the measure for the entire 2011 tax year.

The measure therefore reduced the expected current disposable income of households purchasing or refurbishing their principal residence. It is thus reasonable to expect that the measure would have generated a negative income effect, which in the short term would have had a direct negative impact on new mortgage loan amounts. This, in turn, would have directly affected the LTI and the LTV ratios.¹⁴

Moreover, given the unexpected retroactive reinstatement of the deduction, the financial risk associated with affected mortgages would have fallen without ultimately compromising the household's expected income prior to the introduction of the measure. This allows us to assume that, all other things being equal, the lower future risk of the affected loans would be attributable solely to the lower LTV or LTI ratios, making it possible to treat this event as a natural experiment with which to assess the causal effect of exogenous changes in financial ratios on the future probability of default.

In this analysis, we do not assume a priori that households are financially constrained. Instead, we use the estimated magnitude of the response of the loan amount to an exogenous change in income as indicative evidence of the presence of such constraints. In an environment without financial constraints, a transitory income shock would therefore be expected to have a limited effect on borrowing at origination, as it would not substantially alter the household's permanent income. This would not be the case if financial constraints existed. For financially constrained households, the level of indebtedness is capped at a certain percentage of their income, meaning variations in the debt held would be expected to be roughly proportional to variations in their income. If the estimated variation in debt following the removal of the deduction were equal to or greater than the observed variation in income, the effects found would be consistent with the existence of financial constraints. Conversely, a more muted

14 Assuming that property values were not affected in the short term by the removal of the deduction.

response in debt level would indicate that a significant portion of households are not subject to such constraints.

Section 4.1 shows that the results are consistent with the representative household being financially constrained. This interpretation is further reinforced by analysing the heterogeneity between households with financial constraints. In this regard, we observe that for a segment of the population less likely to be subject to financial constraints, the fiscal shock does not generate significant effects on the mortgage amount. This finding supports the hypothesis that the observed aggregate response is driven primarily by financially constrained households.

3.2 Databases

This study uses the Banco de España's CCR, a confidential credit database kept since 1984 that lists the stock of loans extended in Spain each month by all the banks operating in the country. The initial reporting threshold was very low (€6,000) and it has been further reduced over time. For the purposes of this analysis, we focus on loans to households and, specifically, on all mortgages on principal residences originated between 1 July 2010 and 30 June 2011, thus creating a time window around the shock under study.

The database provides detailed information on borrowers, loan terms and conditions and the lending institution. Regarding borrowers, it distinguishes between individuals with no business activity and sole proprietors (self-employed). For the former, it records employment status (employee, retiree, rentier, student, unemployed, among other categories), while for the latter it records the economic sector in which they operate, categorised according to the three-digit National Classification of Economic Activities. It also includes the borrower's postcode, foreign national status, gender and age.

The borrower's age allows us to assign a household reference person, defined as its oldest member, to each mortgaged household. This classification facilitates association of each mortgage with the corresponding household's sociodemographic characteristics. For instance, based on the postcode of residence we assign an income level to the mortgage, using for this purpose the average household income in that location as provided by the National Statistics Institute (INE). This figure is subsequently used as the denominator in the LTI ratio.

As for loan-specific information, the CCR records the purpose of the loan (enabling us to identify mortgages for the purchase of principal residences) and, at origination, the amount granted in euro, the term in months, the interest rate type (fixed or variable) and corresponding percentage, as well as the property's appraisal value in euro, from which the LTV ratio can be calculated. Additionally, the database documents the loan's status in terms of non-performance, allowing us to classify a loan as non-performing if, at any point in its lifetime up to 2025, it is in arrears for more than 90 days.

4 Identification strategy and results

4.1 Effects on mortgage transactions

The core analysis examines the impact of the 2011 fiscal shock on the characteristics of new mortgages originated for principal residence purchases. Specifically, it assesses whether the temporary removal of the tax deduction for principal residence purchases for high-income taxpayers in regions under the ordinary tax regime (which reduced their effective borrowing capacity in a manner analogous to a borrower-based macroprudential policy) affected various dimensions of mortgage transactions, such as the amount granted, the LTV ratio, the LTI ratio, the interest rate applied, the appraisal value of the property and the future probability of default. The validity of the results is tested using a broad set of robustness checks, including matching techniques designed to improve comparability between the treatment and control groups. Additionally, the analysis examines the aggregate effect on the total volume of mortgage lending and assesses whether there were any spillover effects on consumer lending.

Spain's institutional particularities provide an ideal context for causal identification, as the regions under special tax regimes enjoy full fiscal autonomy and were unaffected by the temporary removal of the deduction. Moreover, since the removal of the deduction affected only taxpayers in higher income brackets and given that Spain's higher-income households are well represented in the regions under the special tax regimes, the treatment group is defined as those households located in regions under the ordinary tax regime whose average income exceeds the median for that territory (around €30,000).¹⁵

This delimitation serves two key purposes. First, it precisely isolates the effect of the deduction's removal on high-income segments – the very taxpayers directly affected by the reform. Second, as noted earlier, it enhances comparability between treated and untreated mortgages by ensuring that households in regions under the ordinary tax regime have income levels closely aligned with those in regions under the special tax regimes. Consequently, the control group comprises households residing in regions under special tax regimes, where the deduction remained in force and unchanged. This selection strategy allows us to identify the fiscal shock's impact on the segments directly exposed to the reform more accurately.

The main estimate adopts a difference-in-differences (DiD) approach, comparing affected and control mortgage transactions before and after the reform's entry into force.

Formally, the treatment variable is defined as the interaction between the binary variable *Treated*, which takes the value 1 for new mortgages originated in high-income postcodes of regions operating under the ordinary tax regime (and 0 otherwise), and the binary variable *Post*, which takes the value 1 for mortgages originated in 2011 (and 0 otherwise). The coefficient of

¹⁵ The remaining mortgages in regions under the ordinary tax regime were excluded from this analysis. Although some of these mortgages were reincorporated into the dataset in the robustness checks, the estimates that included them showed no material differences.

the interaction Treated $\times$ Post captures, within a narrow window around the time of the regulatory change, the average effect of the fiscal shock on various mortgage characteristics in affected regions versus those under special tax regimes. The estimated equations take the following form:

$$\text{Mortgage Characteristics}_{hbt} = \beta \text{Treated}_{hbt} \times \text{Post}_t + \theta_{ht} + \eta_{bt} + \varepsilon_{hbt} \quad [1]$$

where the subscript h denotes the new mortgage granted, b identifies the lending bank, and t spans the months from July 2010 to June 2011. The dependent variable, Mortgage Characteristics, encompasses various dimensions of the mortgage transaction, including the amount granted, the LTV ratio, the LTI ratio, the interest rate applied, the property's appraisal value and the probability of default over the lifetime of the loan.¹⁶

To ensure conditional comparability between the treatment and control groups, we incorporate a comprehensive set of controls that capture both demand- and supply-side factors. Specifically, η_{bt} accounts for time-varying supply factors associated with the lending bank (bank-time fixed effects).¹⁷ Meanwhile, θ_{ht} includes a set of controls that proxy mortgage applicants' household wealth and characteristics through fixed effects linked to postcode, the household reference person's sector of activity and foreign national status, as well as through other compositional factors captured by interactions between foreign national status, female gender and sole proprietorship before and after the shock. We also add a time-varying fixed effect for fixed-rate mortgages to ensure comparability in the interest rate dimension across loans with different contractual structures. The idiosyncratic component unexplained by the remaining factors is denoted by ε_{hbt} . The standard errors in the estimates are clustered at postcode, bank and mortgage level.

In this type of analysis, it is essential to ensure that mortgages in the control group are as comparable as possible to those in the treatment group to obtain valid causal inferences. A preliminary test of averages provides an early indication of the degree of similarity between the two groups. Additionally, to further strengthen comparability between the treatment and control groups, we apply a prior matching procedure. The weights derived from this matching are subsequently incorporated into the estimates to reweight the sample and ensure greater similarity in the distribution of observed covariates between the two groups.

A DiD model also requires the parallel trends assumption to hold, whereby if the shock had not occurred the treatment group would have followed a similar trajectory to the control group. We assess this condition in the supplementary estimates presented in Annex 2, where the Treated variable is allowed to vary over time.

16 Only mortgages still outstanding in 2018 are considered since the Banco de España's CCR did not report all origination characteristics prior to that date. Nevertheless, the sample retains over 80% of new mortgages granted (both in number and amount). It should be noted, however, that this sample restriction may introduce potential survivor bias if mortgages with lower initial leverage are more likely to remain active over time.

17 Since the shock under analysis primarily affects household credit demand, we control for supply using bank-time fixed effects.

Table 1

Comparison of treated and untreated mortgages (a) (b)

	Baseline sample				
	Treated = 0		Treated = 1		Standardised difference
	(1) Average	(2) Standard deviation	(3) Average	(4) Standard deviation	(5) Test
Amount (€000s)	163.18	(87.84)	163.96	(97.97)	0.01
LTV (%)	62.70	(38.37)	79.70	(32.85)	0.34
LTI	4.79	(2.44)	4.45	(2.45)	-0.10
Interest rate (pp)	0.89	(0.63)	0.89	(0.68)	0.00
Future default (0/1)	0.02	(0.13)	0.02	(0.13)	0.01
Log (income)	10.43	(0.17)	10.50	(0.19)	0.29
Term (years)	29.75	(7.87)	28.92	(7.72)	-0.08
Appraisal value (€000s)	203.08	(93.03)	195.33	(95.20)	-0.06
Foreign national (0/1)	0.01	(0.08)	0.02	(0.12)	0.07
Self-employed (0/1)	0.01	(0.11)	0.02	(0.13)	0.04
Female (0/1)	0.34	(0.47)	0.39	(0.49)	0.08
Number of observations	6,510		44,795		

SOURCES: Banco de España, INE and authors' calculations.

- a** The table shows the average (columns 1 and 3) and standard deviation (columns 2 and 4) of a set of variables grouped according to whether they are considered treated (mortgages originated in high-income postcodes in regions under the ordinary tax regime) or untreated (regions under special tax regimes) for the matching analysis.
- b** The standardised difference column presents a statistic to assess the test of averages proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) suggest a heuristic threshold of 0.25 (in absolute value). The standardised difference statistic tests the null hypothesis of no differences in averages between the treatment and control groups using a scale- and sample-size-independent estimator. Values above this threshold would indicate systematic differences.

Table 1 presents a test of averages comparing characteristics of treated and untreated mortgages.¹⁸ The results show a high degree of comparability between the two groups across most observed characteristics (standardised difference statistic < 0.25 in absolute value).¹⁹ The average loan amount is virtually identical in both groups (around €163,000), as are the interest rate applied (0.89%), the contractual term (approximately 29 years) and the future probability of default (around 2%). The remaining covariates also exhibit minimal differences.

Two material differences nevertheless emerge. First, assigned income is higher in treated areas (approximately €37,000 versus €35,000 in untreated areas),²⁰ a pattern consistent with the criterion of constructing the treatment group from high-income areas in regions under the ordinary tax regime. This selection, however, improves comparability between the groups, as the average income of all households under the ordinary tax regime with incomes above

¹⁸ While the tests of averages are calculated on the full sample, the results remain unchanged when the comparison is restricted to the pre-treatment sample.

¹⁹ The standardised difference statistic, which tests the null hypothesis of no differences in averages between the treatment and control groups using a scale- and sample-size-independent estimator, was proposed by Imbens and Wooldridge (2009). Imbens and Rubin (2015) later suggested a heuristic threshold of 0.25 in absolute value, with values above this threshold indicating systematic differences.

²⁰ The average values are calculated on the income level and do not coincide with the exponential of the average income values expressed in logarithms shown in the descriptive tables.

Table 2

Impact of the removal of the house purchase deduction on mortgage characteristics (a)

Dependent variable	(1) Amount	(2) Value	(3) LTV	(4) LTI	(5) Interest rate	(6) Default
Treated × Post	-0.060*** (0.016)	0.006 (0.015)	-3.252** (1.141)	-0.244*** (0.077)	-0.001 (0.024)	-0.008* (0.004)
Fixed Effects: Bank × Time	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects: Postcode × Household	Yes	Yes	Yes	Yes	Yes	Yes
Other household and mortgage controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	51,305	51,305	51,305	51,305	51,305	51,305
Pseudo R2/R2	0.262	0.310	0.497	0.229	0.440	0.197

SOURCES: Banco de España, INE and authors' calculations.

a The table presents the results of estimation at mortgage-bank-time level using a Poisson regression model (columns 1 and 2) or a linear regression model (columns 3, 4, 5 and 6) to assess the impact of the removal of the principal residence purchase deduction in regions under the ordinary tax regime for the period between July 2010 and June 2011. "Treated" is a binary variable that takes the value 1 for new mortgages originated in high-income postcodes (above the median) in regions under the ordinary tax regime and 0 otherwise. "Post" is a binary variable that takes the value 1 for mortgages originated in 2011 (the year the fiscal change was introduced) and 0 otherwise. The "Amount" of the mortgage is measured in thousands of euro; "Value" refers to the property's appraisal value in thousands of euro; "LTV" is the loan-to-value ratio; "LTI" is the loan-to-income ratio (assigned by postcode); "Interest rate" is the percentage mortgage rate; and "Default" is a binary variable that takes the value 1 if the mortgage becomes non-performing at any point in its lifetime (up to 2025) and 0 otherwise. The controls used are explained in Section 4. Standard errors are clustered at postcode, bank and mortgage level. * p-value < 0.10; ** p-value < 0.05; *** p-value < 0.01.

€24,000 stands at around €30,000, a figure far more removed from that observed in the regions under special tax regimes than the one in our refined sample.

Second, the LTV ratio is substantially higher in the treatment group (80% versus 63%), indicating that high-income households in regions under the ordinary tax regime accessed significantly higher leverage, likely driven in large part by income differentials.

These differences underscore the need to carefully control for demand and compositional factors, as we do in the empirical analysis. Additionally, in the robustness checks we apply a matching procedure based on income level. The results show that, once this matching is performed, the observed differences are virtually eliminated and the main findings of the analysis remain unchanged.

Table 2 presents the results of estimating equation [1]. It is worth noting that, when the dependent variable is expressed in monetary terms, we use a Poisson pseudo-maximum likelihood model, whereas for the other dependent variables we use ordinary least squares (OLS) estimates.²¹

The estimated coefficients reveal that, in affected areas and compared with the regions under special tax regimes, the removal of the deduction led to a 6% decrease in average new mortgage

²¹ As Santos Silva and Tenreiro (2006) and Cohn, Liu and Wardlaw (2022) demonstrate, OLS estimates are potentially biased when logarithms of monetary amounts are used as dependent variables, a problem that does not arise with a Poisson model.

loan amount but did not significantly change the property's appraisal value. As a result, the LTV ratio fell by 3.3 pp and the LTI ratio by 24 bp. Interest rates showed no statistically significant variation. Finally, the future probability of default decreased by approximately 0.8 pp.

As discussed in earlier sections, the observed reduction in the future probability of default likely stems from lower initial leverage rather than from permanent changes in households' expected income. The fiscal shock acted as a temporary constraint on borrowing capacity at origination that reduced the LTV and LTI ratios. The deduction was retroactively reinstated later that same year, leaving expected income largely unaffected. Consequently, the improvement in solvency indicators solely reflects a lower debt burden at origination, which in turn reduces ex post default risk.

To minimise the differences observed between the treatment and control groups (documented in Table 1), we apply a matching technique based on income level, the primary variable responsible for the discrepancies identified. This matching procedure generates weights that are subsequently incorporated into the DiD model.

In our empirical context, the control group is substantially smaller than the treatment group, which means that conventional matching techniques would require discarding a significant portion of the sample and result in a loss of statistical efficiency and representativeness. To avoid these undesired outcomes, we adopt two approaches. First, we use entropy balancing (EB), a matching procedure that assigns weights to observations to equalise the averages of selected observable characteristics (in this case, income) without reducing the sample size. Second, we apply inverse probability weighting (IPW), which reweights control group observations so that the average of the covariates used (particularly income) is similar to that of the treatment group, again without eliminating observations. Both EB and IPW thus preserve the available information and enhance sample balance between two groups of highly unequal size (see Hainmueller (2012) and Hirano, Imbens and Ridder (2003)).

Table 3 replicates the test of averages presented in Table 1, incorporating the weights derived from EB. The results show that, after reweighting, the differences between the treatment and control groups are markedly smaller and cease to be statistically significant for all the characteristics considered. This confirms that the procedure achieves a high degree of convergence in the composition of the two groups, thus bolstering the validity of the subsequent causal analysis.

Table 4 presents the results of estimating equation [1] using the EB methodology. The findings confirm the robustness of the main estimates, as the same mortgage characteristics that proved significant in the baseline estimate (Table 2) retain their statistical significance and are of very similar magnitudes. Specifically, in affected areas new mortgage loan amounts fell by 6.1% versus those granted under special tax regimes, the LTV ratio decreased by 2.4 pp, the LTI ratio by 25 bp, and the future probability of default improved by 0.9 pp.

Annex 2 (Tables A2.1 and A2.2) shows the results of IPW. The estimates preserve both the sign and magnitude of those obtained through EB and for the unweighted sample. Ultimately, the

Table 3

Comparison of treated and untreated mortgages following EB matching (a) (b)

	EB-matched sample				
	Treated = 0		Treated = 1		Standardised difference
	(1) Average	(2) Standard deviation	(3) Average	(4) Standard deviation	(5) Test
Amount (€000s)	172.07	(96.90)	163.96	(97.97)	-0.06
LTV (%)	82.26	(29.88)	85.90	(29.66)	0.09
LTI	4.70	(2.44)	4.45	(2.45)	-0.07
Interest rate (pp)	0.87	(0.62)	0.89	(0.68)	0.02
Future default (0/1)	0.01	(0.12)	0.02	(0.13)	0.02
Log (income)	10.50	(0.22)	10.50	(0.19)	0.02
Term (years)	29.64	(7.90)	28.92	(7.72)	-0.07
Appraisal value (€000s)	213.09	(97.57)	195.33	(95.20)	-0.13
Foreign national (0/1)	0.01	(0.07)	0.02	(0.12)	0.07
Self-employed (0/1)	0.01	(0.11)	0.02	(0.13)	0.04
Female (0/1)	0.34	(0.47)	0.39	(0.49)	0.08
Number of observations	6,510		44,795		

SOURCES: Banco de España, INE and authors' calculations.

- a** The table shows the average (columns 1 and 3) and standard deviation (columns 2 and 4) of a set of variables grouped according to whether they are considered treated (mortgages originated in high-income postcodes in regions under the ordinary tax regime) or untreated (regions under special tax regimes) for the matching analysis. Matching is performed on income and the resulting weights stem from an EB approach.
- b** The standardised difference column presents a statistic to assess the test of averages proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) suggest a heuristic threshold of 0.25 (in absolute value). The standardised difference statistic tests the null hypothesis of no differences in averages between the treatment and control groups using a scale- and sample-size-independent estimator. Values above this threshold would indicate systematic differences.

Table 4

Impact of the removal of the house purchase deduction on mortgage characteristics. EB-matched sample (a)

Dependent variable	(1) Amount	(2) Value	(3) LTV	(4) LTI	(5) Interest rate	(6) Default
Treated × Post	-0.061*** (0.018)	-0.004 (0.016)	-2.377** (1.169)	-0.252*** (0.077)	0.001 (0.031)	-0.009** (0.004)
Fixed Effects: Bank × Time	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects: Postcode × Household	Yes	Yes	Yes	Yes	Yes	Yes
Other household and mortgage controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	51,305	51,305	51,305	51,305	51,305	51,305
Pseudo R ² /R ²	0.297	0.347	0.613	0.256	0.460	0.231

SOURCES: Banco de España, INE and authors' calculations.

- a** The table presents the results of a weighted estimation (following EB matching) at mortgage-bank-time level using a Poisson regression model (columns 1 and 2) or a linear regression model (columns 3, 4, 5 and 6) to assess the impact of the removal of the principal residence purchase deduction in regions under the ordinary tax regime for the period between July 2010 and June 2011. "Treated" is a binary variable that takes the value 1 for new mortgages originated in high-income postcodes (above the median) in regions under the ordinary tax regime and 0 otherwise. "Post" is a binary variable that takes the value 1 for mortgages originated in 2011 (the year the fiscal change was introduced) and 0 otherwise. The "Amount" of the mortgage is measured in thousands of euro; "Value" refers to the property's appraisal value in thousands of euro; "LTV" is the loan-to-value ratio; "LTI" is the loan-to-income ratio (assigned by postcode); "Interest rate" is the percentage mortgage rate; and "Default" is a binary variable that takes the value 1 if the mortgage becomes non-performing at any point in its lifetime (up to 2025) and 0 otherwise. The controls used are explained in Section 4. Standard errors are clustered at postcode, bank and mortgage level.
- * p-value < 0.10; ** p-value < 0.05; *** p-value < 0.01.

similarity of the results with and without matching supports the validity of the empirical strategy and provides further evidence that the estimated effects are not driven by observable imbalances between the groups but instead reflect the impact of the fiscal shock under analysis.

Finally, Table A2.3 in Annex 2 presents the estimate of equation [1]. It employs EB matching and allows the Treated variable to vary at bimonthly intervals over time to assess the parallel trends assumption. Overall, the results show no significant differences in the variables of interest between the treatment and control groups in the pre-shock period, validating the assumption that the control group provides an appropriate counterfactual for the treatment group.²²

4.1.1 Robustness checks

So far, in order to identify the fiscal shock's impact with greater clarity, we have classified as treated only those mortgages granted in high-income postcodes (above the median) in regions under the ordinary tax regime. This strategy seeks to minimise the risk of misclassification since we lack individual data on each household's exact income (and, in particular, on the taxable income to which the fiscal reform applies), which prevents us accurately identifying which mortgages were actually affected. By restricting the treatment group to high-income postcodes, we reduce the likelihood of including mortgages classified as treated that, in reality, were not.

To assess whether this restriction affects the results obtained so far, we include as a robustness check an alternative treatment definition that encompasses all mortgages granted in regions under the ordinary tax regime to households with an assigned income exceeding €24,000. The results, shown in Table 5, are consistent with the main findings, despite this assignment method potentially being subject to greater classification error and despite the larger income differences with the control group.

Specifically, the estimates show that, following the fiscal shock, the loan amounts of new mortgages granted in regions under the ordinary tax regime fell by 5.4% compared with those formalised in the Basque Country or Navarre. Likewise, the LTV ratio decreased by 3.2 pp and the LTI ratio by 22 bp. The future probability of default also fell by 0.5 pp, although like the estimates for the appraisal value and the interest rate this effect is not statistically significant.

As noted in the introduction, the removal of the deduction for principal residence purchases need not affect the borrowing decisions of all households potentially exposed to the fiscal change in the same way. Even among those within the measure's scope of application, the response may differ depending on the additional assets available to households (such as financial savings, land or other properties and assets) allowing them to reduce their reliance on borrowing.

²² Although the results are consistent whether using IPW matching or not applying any matching procedure, in the latter case some evidence emerges of pre-existing trends in the appraisal value.

Table 5

Impact of the removal of the house purchase deduction on mortgage characteristics. Treatment group with income greater than €24,000 (a)

Dependent variable	(1) Amount	(2) Value	(3) LTV	(4) LTI	(5) Interest rate	(6) Default
Treated × Post	-0.054*** (0.015)	0.005 (0.020)	-3.235*** (1.205)	-0.217*** (0.073)	0.009 (0.021)	-0.005 (0.004)
Fixed Effects: Bank × Time	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects: Postcode × Household	Yes	Yes	Yes	Yes	Yes	Yes
Other household and mortgage controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	96,046	96,046	96,046	96,046	96,046	96,046
Pseudo R2/R2	0.271	0.330	0.492	0.227	0.431	0.191

SOURCES: Banco de España, INE and authors' calculations.

a The table presents the results of a weighted estimation (following EB matching) at mortgage-bank-time level using a Poisson regression model (columns 1 and 2) or a linear regression model (columns 3, 4, 5 and 6) to assess the impact of the removal of the principal residence purchase deduction in regions under the ordinary tax regime for the period between July 2010 and June 2011. "Treated" is a binary variable that takes the value 1 for new mortgages originated in high-income postcodes (above the median) in regions under the ordinary tax regime and 0 otherwise. "Post" is a binary variable that takes the value 1 for mortgages originated in 2011 (the year the fiscal change was introduced) and 0 otherwise. The "Amount" of the mortgage is measured in thousands of euro; "Value" refers to the property's appraisal value in thousands of euro; "LTV" is the loan-to-value ratio; "LTI" is the loan-to-income ratio (assigned by postcode); "Interest rate" is the percentage mortgage rate; and "Default" is a binary variable that takes the value 1 if the mortgage becomes non-performing at any point in its lifetime (up to 2025) and 0 otherwise. The controls used are explained in Section 4. Standard errors are clustered at postcode, bank and mortgage level. * p-value < 0.10; ** p-value < 0.05; *** p-value < 0.01.

To illustrate the magnitude of the shock, we consider a representative household with an average mortgage. The temporary removal of the deduction for principal residence purchases implies a maximum annual reduction in net income of €1,356 (around 3.5% of the annual net income of the group considered, estimated at around €38,000).²³ Meanwhile, the empirical estimates show a reduction of close to 6% in new mortgage loan amounts.²⁴

While an annual flow (income) and a stock (loan principal) are not directly comparable, the two can be reconciled by acknowledging that a 6% reduction in principal translates into a proportional decrease in the annual amount repaid. For a representative mortgage, and given typical DSTI ratios of 35-45%, this reduction in the repayment amount is of a similar order of magnitude to the annual income loss associated with removal of the deduction for principal residence purchases.²⁵ Taken together, these results are consistent with the interpretation of the episode as an effective constraint on borrowing at origination (see Annex 1).

²³ Average value for high-income households in regions under the ordinary tax regime, the group considered affected by the reform in this study, as described in greater detail in Section 4.

²⁴ It should be noted that, at the time of loan origination, households did not anticipate the retroactive reinstatement of the tax deduction (a measure adopted following the change of government after the November 2011 general election) and therefore made their borrowing decisions as if the shock were permanent. From this perspective, the relevant comparison is between the expected loss of permanent income and the observed adjustment in borrowing at origination. While the text illustrates the magnitude of the shock using the maximum annual loss associated with removal of the deduction, an assessment in present value terms would not qualitatively alter the interpretation, since both the deduction and household income scale similarly when considered as permanent flows.

²⁵ Under a French amortisation schedule, for a representative mortgage of €163,000 at an interest rate of 0.9% and with a term of 29 years, a 6% decrease in the principal implies a reduction of approximately €385 in the annual repayment. With a DSTI limit of 35-45%, this reduction is consistent with a decrease in annual net income of between €855 and €1,100, comparable to the annual deduction of around €900 to which this representative mortgage would have been entitled.

It is also of interest to identify a subset of potentially less constrained households in order to assess whether the reform had a differential, or more limited, impact on them.

This task is made challenging by the lack of direct data on borrowers' wealth. To create a proxy for this information, we employ an indirect strategy that examines post-origination behaviour. Specifically, given that we know the initial loan amount, term and contractual rate of interest, we compare, for example at December 2016, the outstanding balance of the mortgages analysed with the simulated balance that would have existed at that date if a standard French amortisation schedule had applied from the start of the loan. Based on this comparison, we classify households as unconstrained if they have made significant early repayments. Operationally, we define as unconstrained those loans for which the simulated amortised percentage is greater than the median of its distribution.²⁶ Applying this variable isolates around 16% of the mortgages in the sample.

It should be emphasised that this metric is not a direct indicator of wealth and may reflect, in addition to greater financial headroom, factors such as the temporary availability of liquidity or preferences for debt reduction. In this regard, the aim of the exercise is not to identify every single household not subject to financial constraints at origination, but rather to isolate a subset with a lower likelihood of being constrained and to assess whether the effects of the fiscal shock are smaller in this group (see Annex 1).

Table 6 replicates the analysis in Table 2, distinguishing between households classified as unconstrained and the rest. The results yield two main conclusions. First, households classified as unconstrained show no significant changes in any of their credit standards following the fiscal reform, suggesting that their borrowing capacity was not compromised by the removal of the deduction. Second, the estimated effects for the remaining households are similar to those observed in the full sample, which is consistent with this group's predominance in the population analysed. Taken together, this heterogeneity indicates that the impact of the fiscal shock is concentrated among financially constrained households, reinforcing the interpretation that the tax change acts as an effective constraint on borrowing at origination, analogous to the introduction of macroprudential limits on the LTI ratio (see Annex 1).

4.2 The effects of the LTV or LTI ratios on future default

To identify the effect of the LTV or LTI ratio at mortgage origination on subsequent credit performance, it is important to note that, in the absence of an exogenous shock, both ratios are endogenous: higher-risk borrowers tend to opt for greater leverage because they have less equity, thus creating a positive correlation with default.

²⁶ The amortised percentage is defined as $\text{Max}(\text{Simulated Debt 2016} - \text{Actual Debt 2016}, 0) / \text{Initial Debt} \times 100$. The results remain qualitatively unchanged for the thresholds of the amortised percentage distribution between the 35th and 65th percentiles.

Table 6

Impact of the removal of the house purchase deduction on mortgage characteristics (a)

	(1) Amount	(2) Amount	(3) Value	(4) Value	(5) LTV	(6) LTV	(7) LTI	(8) LTI	(9) Interest rate	(10) Interest rate	(11) Default	(12) Default
Dependent variable	Unconstrained	Rest	Unconstrained	Rest	Unconstrained	Rest	Unconstrained	Rest	Unconstrained	Rest	Unconstrained	Rest
Treated × Post	-0.024 (0.049)	-0.059*** (0.015)	-0.016 (0.033)	0.014 (0.016)	1.123 (2.093)	-3.697*** (1.239)	-0.068 (0.260)	-0.257*** (0.079)	0.033 (0.070)	-0.016 (0.028)	-0.007 (0.007)	-0.010* (0.005)
Fixed Effects: Bank × Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects: Postcode × Household	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other household and mortgage controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,208	42,276	7,208	42,276	7,208	42,276	7,208	42,276	7,208	42,276	7,208	42,276
Pseudo R2/R2	0.437	0.268	0.436	0.318	0.622	0.502	0.407	0.244	0.615	0.441	0.292	0.213

SOURCES: Banco de España, INE and authors' calculations.

a The table presents the results of estimation at mortgage-bank-time level using a Poisson regression model (columns 1 and 2) or a linear regression model (columns 3, 4, 5 and 6) to assess the impact of the removal of the principal residence purchase deduction in regions under the ordinary tax regime for the period between July 2010 and June 2011. A household is classified as unconstrained if it made significant early mortgage repayments by 2016 (see Section 4.1.1). "Treated" is a binary variable that takes the value 1 for new mortgages originated in high-income postcodes (above the median) in regions under the ordinary tax regime and 0 otherwise. "Post" is a binary variable that takes the value 1 for mortgages originated in 2011 (the year the fiscal change was introduced) and 0 otherwise. The "Amount" of the mortgage is measured in thousands of euro; "Value" refers to the property's appraisal value in thousands of euro; "LTV" is the loan-to-value ratio; "LTI" is the loan-to-income ratio (assigned by postcode); "Interest rate" is the percentage mortgage rate; and "Default" is a binary variable that takes the value 1 if the mortgage becomes non-performing at any point in its lifetime (up to 2025) and 0 otherwise. The controls used are explained in Section 4. Standard errors are clustered at postcode, bank and mortgage level. * p-value < 0.10; ** p-value < 0.05; *** p-value < 0.01.

In this context, the fiscal shock under analysis provides a source of exogenous variation since its implementation was independent of household decisions, occurred within clearly defined temporal and geographical bounds and directly affected initial borrowing capacity. This makes it a valid means by which to identify the causal effect that LTV and LTI ratios exert on future probability of default. Formally, we use instrumental variables to estimate the following equation:

$$\text{Future Default}_{hb} = \gamma \text{LS}_{hbt} + \theta_{ht} + \eta_{bt} + \varepsilon_{hbt} \quad [2]$$

where LS (lending standard) denotes either the LTV or LTI ratio at origination, instrumented in the first stage through equation [1]. The remaining components of the equation were defined in Section 4.1. The fiscal shock's significance when used as an instrument is measured by the first-stage F-statistic, which yields values close to the heuristic threshold of 10 (8.1 for LTV and 10.0 for LTI).²⁷

27 An F-statistic of 10 or greater signals a strong relationship between the instrumental variable and the instrument, indicating the statistic's significance as an instrument. This threshold should be interpreted as a rule of thumb rather than a definitive criterion, particularly in cases – such as this study – where the instrument's exogeneity is robustly supported (Angrist and Pischke, 2009). Nevertheless, it is worth noting that a low F-statistic may lead the instrumental variable model to produce imprecise and high (in absolute value) estimates with wide confidence intervals. In such cases, the qualitative impact would be more significant than the quantitative one.

Table 7

Impact of changes in the LTV or LTI ratio on the future probability of mortgage default (a)

Dependent variable	Default	
	(1)	(2)
LTV	0.002** (0.001)	
LTI		0.036** (0.017)
Fixed Effects: Bank × Time	Yes	Yes
Fixed Effects: Postcode × Household	Yes	Yes
Other household and mortgage controls	Yes	Yes
F-statistic	8.1	10.0
Observations	51,305	51,305

SOURCES: Banco de España, INE and authors' calculations.

a The table presents the results of instrumental variable estimation at mortgage-bank-time level using a linear probability regression model to analyse the impact on the future probability of default of the LTV or LTI ratio at origination, instrumented by the removal of the principal residence purchase deduction in regions under the ordinary tax regime for the period between July 2010 and June 2011. "LTV" is the loan-to-value ratio; "LTI" is the loan-to-income ratio (assigned by postcode); and "Default" is a binary variable that takes the value 1 if the mortgage becomes non-performing at any point in its lifetime (up to 2025) and 0 otherwise. The controls used are explained in Section 4. Standard errors are clustered at postcode, bank and mortgage level. * p-value < 0.10; ** p-value < 0.05; *** p-value < 0.01.

The second-stage instrumental variable estimates are presented in Table 7. The estimates for both the LTV and LTI ratios indicate that greater initial leverage significantly increases mortgage credit risk. Conversely, exogenous reductions in these ratios improve loan credit quality. Specifically, a 1 pp decrease in the LTV ratio (or 1 bp in the LTI ratio) leads to an approximate 13% (2% for the LTI ratio) reduction in the probability of default relative to its average value. The estimated economic magnitude of the leverage ratios' impact on probability of default should be interpreted cautiously, given the instrument's limited significance.

4.3 Aggregate effects by postcode and bank

To i) verify the robustness at aggregate level of the results obtained at individual transaction level; ii) analyse the change in total lending; and iii) examine potential spillover effects on consumer credit, we conduct a complementary analysis of new transaction volumes by postcode and financial institution. This approach is relevant as it captures possible adjustments in credit supply and demand that may not manifest at the micro level but do emerge in aggregate financial institution decisions or in local credit composition.

As in the disaggregated analysis, we estimate a DiD model in which the treated postcodes match those used in the loan-level exercise. Here, the estimated equation is equivalent to equation [1], though in this case aggregated by postcode-bank-time. We also include bank-time and postcode-bank fixed effects to allow exhaustive control for supply factors and persistent local heterogeneity. Standard errors are clustered at postcode and bank level.

Table 8

Impact of the removal of the house purchase deduction on new loans. Aggregate analysis (a)

Dependent variable	House purchase			Consumer credit		
	(1) Average amount	(2) Total amount	(3) Number of loans	(4) Average amount	(5) Total amount	(6) Number of loans
Treated × Post	-0.067** (0.027)	-0.228*** (0.046)	-0.178*** (0.041)	-0.012 (0.045)	-0.009 (0.111)	0.048 (0.109)
Fixed Effects: Bank × Time	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects: Postcode × Household	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,090	20,738	20,738	72,172	65,207	65,207
Pseudo R2	0.390	0.491	0.160	0.480	0.575	0.313

SOURCES: Banco de España, INE and authors' calculations.

a The table presents the results of estimation at postcode-bank-time level using a Poisson model to assess the impact of the removal of the principal residence purchase deduction in regions under the ordinary tax regime for the period between July 2010 and June 2011. "Treated" is a binary variable that takes the value 1 for high-income postcodes (above the median) in regions under the ordinary tax regime and 0 otherwise. "Post" is a binary variable that takes the value 1 for mortgages originated in 2011 (the year the fiscal change was introduced) and 0 otherwise. The "Average amount" represents the sum of all new mortgages (in thousands of euro) divided by the number of new mortgages granted. Standard errors are clustered at postcode and bank level. * p-value < 0.10; ** p-value < 0.05; *** p-value < 0.01.

This approach enables us to assess the fiscal shock's impact on both average new loan amounts and aggregate credit volume, including potential reallocation to consumer loans following the regulatory change.

Table 8 presents the aggregate impact of the temporary removal of the deduction for principal residence purchases on the volume of new credit transactions, distinguishing between home mortgages and consumer loans. Columns (1) and (2) show the effects on the average amount and total amount of the new mortgages, respectively, while columns (4) and (5) contain the results for consumer credit. Finally, columns (3) and (6) analyse the changes in the number of mortgages (intensive margin) to detect whether anticipation effects extend beyond December 2010.²⁸

The results reveal a negative and statistically significant impact on mortgage lending in treated areas. Specifically, the estimated coefficient in column (1) indicates that average new mortgage loan amount fell by around 6.7%, consistent with the effects observed in the disaggregated analysis. Column (2) shows a 22.8% decrease in the total loan amount of the new mortgages granted. Column (3) helps clarify the aggregate effect by demonstrating that it largely stems from anticipatory behaviour by borrowers and/or heightened uncertainty, which led to postponement of borrowing decisions. In particular, following the fiscal shock, and even when

²⁸ The data show an unusually high volume of transactions in December 2010, attributable to anticipatory behaviour ahead of the fiscal measure's entry into force. To prevent this effect from distorting the results and to strengthen the robustness of the analysis, transactions from that month are excluded in this exercise. Nevertheless, the estimated effect remains influenced by the pre-December anticipatory behaviour, as shown by the figures for the number of transactions following the measure's entry into force.

December is excluded from the data, the number of new mortgages in treated areas fell by around 18%.

By contrast, no statistically significant effects on consumer credit (columns (4) and (5)) or on the number of consumer loans (column (6)) are found. Both the average amount and the total amount of the new loans yield statistically insignificant coefficients close to zero, suggesting no spillover or substitution effects between mortgage lending and consumer credit following the fiscal shock.

5 Conclusion

This article analyses the effects on mortgage lending in Spain of the temporary removal of the tax deduction for principal residence purchases. The institutional design of the reform – which did not apply to the Basque Country or Navarre and was later unexpectedly reversed with retroactive effect – provides quasi-experimental conditions particularly suitable for identifying exogenous changes in household borrowing capacity without permanently altering expected income. This feature allows the effects of the shock under analysis on financially constrained households' debt burden to be interpreted as analogous to those of a macroprudential BBM aimed at limiting borrower leverage through caps on the LTI or DSTI ratios.

The results show that the removal of the deduction led to a significant decrease in both new mortgage loan amounts and the main leverage indicators at origination without having an effect in the short term on property prices or interest rates. This evidence suggests that the measure operated primarily through the loan amount and its financial structure. Furthermore, the lower initial debt burden assumed by borrowers translated into a significant decrease in future probability of default, highlighting a clear channel through which constraints on borrowing at origination can improve mortgage credit quality.

Taken together, the findings point to the efficacy of imposing limits on mortgage lending standards as a means of curbing household indebtedness and reducing risk of future default. These results should nevertheless be interpreted with caution since the analysis relies on a proxy for household income and a specific time window. The evidence presented should therefore be assessed alongside that from other studies and contexts.

It is worth emphasising, moreover, that this analysis focuses primarily on the efficacy of constraints on borrowing at origination, as measured by their effects on mortgage lending and mortgage credit quality, and does not comprehensively address the potential spillover or general equilibrium effects associated with this type of measure. In particular, while some short-term effects on consumption are examined, the study does not systematically explore how a persistent restriction on mortgage lending might affect, in the longer term, equilibria in the owner-occupied and rental housing markets, the reallocation of credit between the real estate sector and other productive sectors, or potential distributional effects across different

household groups. Analysis of these channels would contribute to a more comprehensive evaluation of macroprudential policies and represent a natural avenue for future research.²⁹

In short, these results' qualitative consistency both with the international evidence on macroprudential policies designed to constrain borrower indebtedness and with previous studies by the Banco de España (which document the ability of these measures to reduce credit risk) reinforces confidence in the existence and economic relevance of the identified channel and in the efficacy of such measures. Overall, this article contributes to the debate in Spain on the suitability and design of macroprudential tools, providing empirical evidence of their potential to bolster financial stability.

²⁹ See Section 2 for a review of the theoretical and empirical evidence on the general equilibrium effects and potential spillovers associated with macroprudential BBMs (including effects on consumption, housing markets and credit reallocation between sectors).

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This annex presents a highly simplified theoretical framework designed to clarify the conditions under which a fiscal shock – one that affects borrowing capacity at loan origination – may produce empirical effects comparable to those of a macroprudential BBM, such as a cap on the LTV ratio, the LTI ratio or the DSTI ratio. The analogy is not proposed as a general equivalence between the two instruments, but rather as a local and conditional relationship valid for the subset of households whose borrowing decisions are effectively constrained at origination.

In this exercise, we consider a household purchasing a property of value V , financed by a mortgage of amount L . The household receives an exogenous income and decides its level of indebtedness at loan origination. To simplify the exposition, we ignore dynamic consumption decisions and focus exclusively on determination of initial leverage.

In the absence of credit constraints, the household would choose an internal level of borrowing, L^* , determined by its preferences and the effective cost of the debt. In the mortgage market, however, observed borrowing is limited by underwriting standards, which can be expressed as a set of quantitative constraints (BBMs) in terms of caps on the LTV, LTI and/or DSTI ratios. In brief:

$$L \leq \lambda V, L \leq \kappa I, p(r,T) L \leq \varphi I,$$

where $p(r,T)$ is the annual repayment per euro of principal (an annuity factor that depends on interest rate r and term T), I denotes the net disposable income relevant to debt servicing at origination, and λ , κ and φ are positive parameters that define the regulatory/credit thresholds for the LTV, LTI and DSTI ratios, respectively. The principal granted therefore satisfies:

$$L \leq \min \{L^*, \lambda V, \kappa I, \varphi I / p(r,T)\}$$

Within this framework, a BBM can be represented as an exogenous change in one of the regulatory parameters, such as a reduction in κ (the LTI cap), λ (the LTV cap) or φ (the DSTI cap). Such measures truncate the set of feasible borrowing options and affect only those households for which the constraint is binding at origination. Thus, a reduction in κ , λ or φ mechanically reduces the loan amount for constrained households but does not directly affect the borrowing of unconstrained households, except through general equilibrium effects.

This property is central to the interpretation of BBMs: their effects are concentrated on those borrowers with greatest initial leverage, who are precisely those of greatest interest from the standpoint of financial stability.

We now consider a fiscal shock associated with a mortgage-financed home purchase (for example, the removal of a tax deduction). This shock operates, in principle, as a change in the

net cost of financing: it alters either net repayment of the loan or the net income available to service the debt. It would thus lead to a higher effective repayment amount (due to the lower deduction) or a fall in the net income available to service the debt.

Unlike a BBM, this shock does not directly impose a regulatory constraint on borrowing, but instead operates through the effective cost of the debt and the household's net income, affecting interior optimum L^* (via prices). It could therefore affect all households, including those that are unconstrained. Nevertheless, its effects on initial borrowing depend crucially on whether the household is subject to credit constraints.

For unconstrained households, the fiscal shock may marginally alter optimal borrowing level L^* , but it does not change the set of feasible options. Consequently, in a static, short-term setting, the adjustment in borrowing L may be minimal or undetectable and is not comparable to a BBM except through general equilibrium effects.

For constrained households, however, the fiscal shock may be observationally equivalent to a BBM since it operates similarly to lowering the relevant cap. If the binding constraint is LTI ($L = \kappa I$), the change in loan amount would be proportional to the change in income. If it operates through DSTI [$L = \phi I / p(r, T)$], removing a deduction and consequently increasing effective repayment p or reducing net income I would mechanically lower the maximum loan amount, making the adjustment at the margin proportional to the change in income and inversely proportional to the change in effective repayment.¹

This mechanism precisely mirrors the effective truncation of borrowing at origination produced by an LTI/DSTI-type BBM, though in this case the truncation is induced by a fiscal change in the net cost of borrowing.

Under the assumptions that i) house prices and interest rates remain largely constant over a short time window, and ii) the credit origination process is subject to LTV, LTI and/or DSTI ratio caps, a fiscal shock that reduces the household's relevant net income or increases the effective loan repayment is equivalent, for households on which the constraint is binding, to a tightening of a BBM (i.e. a reduction in parameters κ or ϕ). In both cases, the effect materialises as an adjustment in the maximum loan amount and, therefore, in changes to the LTV and LTI ratios at origination.

This theoretical framework has several key implications for empirical interpretation: i) the effects of the shock are concentrated among financially constrained households, and ii) these households experience a reduction in initial leverage at origination. As a corollary, unconstrained households may not exhibit any statistically significant effects in the observed response.

The analogy between a fiscal shock and a BBM is not therefore generally applicable but is instead partial and conditional; it holds only insofar as the analysis focuses on the group of

1 The effect could be more pronounced in practice if combined with other concurrent constraints.

households whose borrowing capacity is limited by credit constraints at origination. From this perspective, a transitory fiscal shock that reduces the relevant net income at origination or increases the effective loan repayment amount can be interpreted as a natural and informative experiment that provides insights into the effects of a BBM policy, albeit subject to the limitations noted and bearing in mind that it is not directly extrapolatable to all households.

Table A2.1

Comparison of treated and untreated mortgages following IPW matching (a) (b)

	IPW-matched sample				
	Treated = 0		Treated = 1		Standardised difference
	Average	Standard deviation	Average (%)	Standard deviation	Test
Amount (€000s)	166.47	(90.77)	16396.08%	(97.97)	-0.02
LTV (%)	82.47	(29.88)	85.90	(29.66)	0.08
LTI	4.71	(2.42)	4.45	(2.45)	-0.07
Interest rate (pp)	0.88	(0.63)	0.89	(0.68)	0.01
Future default (0/1)	0.02	(0.12)	0.02	(0.13)	0.01
Log (income)	10.47	(0.17)	10.50	(0.19)	0.15
Term (years)	29.70	(7.88)	28.92	(7.72)	-0.07
Appraisal value (€000s)	207.77	(94.77)	195.33	(95.20)	-0.09
Foreign national (0/1)	0.01	(0.07)	0.02	(0.12)	0.07
Self-employed (0/1)	0.01	(0.11)	0.02	(0.13)	0.04
Female (0/1)	0.34	(0.47)	0.39	(0.49)	0.08
Number of observations	6,510		44,795		

SOURCES: Banco de España, INE and authors' calculations.

- a** The table shows the average (columns 1 and 3) and standard deviation (columns 2 and 4) of a set of variables grouped according to whether they are considered treated (mortgages originated in high-income postcodes in regions under the ordinary tax regime) or untreated (regions under special tax regimes) for the matching analysis. Matching is performed on income and the resulting weights stem from an IPW approach using inverse probability of treatment weighting.
- b** The standardised difference column presents a statistic to assess the test of averages proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) suggest a heuristic threshold of 0.25 (in absolute value). The standardised difference statistic tests the null hypothesis of no differences in averages between the treatment and control groups using a scale- and sample-size-independent estimator. Values above this threshold would indicate systematic differences.

Table A2.2

Impact of the removal of the house purchase deduction on mortgage characteristics. IPW-matched sample (a)

Dependent variable	(1) Amount	(2) Value	(3) LTV	(4) LTI	(5) Interest rate	(6) Default
Treated × Post	-0.063*** (0.017)	-0.004 (0.015)	-2.298** (1.136)	-0.259*** (0.082)	-0.013 (0.027)	-0.007* (0.004)
Fixed Effects: Bank × Time	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects: Postcode × Household	Yes	Yes	Yes	Yes	Yes	Yes
Other household and mortgage controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	51,305	51,305	51,305	51,305	51,305	51,305
Pseudo R2/R2	0.264	0.539	0.315	0.232	0.442	0.197

SOURCES: Banco de España, INE and authors' calculations.

a The table presents the results of inverse probability of treatment weighting estimation (after IPW matching) at mortgage-bank-time level using a Poisson regression model (columns 1 and 2) or a linear regression model (columns 3, 4, 5 and 6) to assess the impact of the removal of the principal residence purchase deduction in regions under the ordinary tax regime for the period between July 2010 and June 2011. "Treated" is a binary variable that takes the value 1 for new mortgages originated in high-income postcodes (above the median) in regions under the ordinary tax regime and 0 otherwise. "Post" is a binary variable that takes the value 1 for mortgages originated in 2011 (the year the fiscal change was introduced) and 0 otherwise. The "Amount" of the mortgage is measured in thousands of euro; "Value" refers to the property's appraisal value in thousands of euro; "LTV" is the loan-to-value ratio; "LTI" is the loan-to-income ratio (assigned by postcode); "Interest rate" is the percentage mortgage rate; and "Default" is a binary variable that takes the value 1 if the mortgage becomes non-performing at any point in its lifetime (up to 2025) and 0 otherwise. The controls used are explained in Section 4. Standard errors are clustered at postcode, bank and mortgage level. * p-value < 0.10; ** p-value < 0.05; *** p-value < 0.01.

Table A2.3

**Temporary impact of the removal of the house purchase deduction on mortgage characteristics.
EB-matched sample (a)**

Dependent variable	(1) Amount	(2) Value	(3) LTV	(4) LTI	(5) Interest rate	(6) Default
Sep-Oct 2010	0.005 (0.034)	-0.023 (0.020)	0.474 (2.054)	-0.101 (0.220)	-0.048 (0.039)	-0.005 (0.007)
Nov-Dec 2010	0.056 (0.039)	-0.001 (0.023)	0.900 (2.205)	0.157 (0.184)	-0.064 (0.048)	-0.003 (0.006)
Jan-Feb 2011	0.055 (0.050)	-0.006 (0.028)	-5.570** (2.679)	-0.311* (0.180)	-0.045 (0.055)	-0.012 (0.009)
Mar-Apr 2011	-0.064** (0.032)	-0.022 (0.019)	-0.368 (3.182)	-0.332** (0.134)	-0.059 (0.059)	-0.011 (0.010)
May-Jun 2011	0.023 (0.037)	-0.004 (0.023)	-0.322 (2.748)	0.018 (0.139)	-0.026 (0.038)	-0.012* (0.006)
Fixed Effects: Bank × time	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects: Postcode × household	Yes	Yes	Yes	Yes	Yes	Yes
Other household and mortgage controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	51,305	51,305	51,305	51,305	51,305	51,305
Pseudo R2/R2	0.298	0.347	0.613	0.257	0.460	0.237

SOURCES: Banco de España, INE and authors' calculations.

- a** The table presents the results of a weighted estimation (following EB matching) at mortgage-bank-time level using a Poisson regression model (columns 1 and 2) or a linear regression model (columns 3, 4, 5 and 6) to assess the impact of the removal of the principal residence purchase deduction in regions under the ordinary tax regime for the period between July 2010 and June 2011. "Treated" is a binary variable that takes the value 1 for new mortgages originated in high-income postcodes (above the median) in regions under the ordinary tax regime and 0 otherwise. "Post" is a binary variable that takes the value 1 for mortgages originated in 2011 (the year the fiscal change was introduced) and 0 otherwise. The "Amount" of the mortgage is measured in thousands of euro; "Value" refers to the property's appraisal value in thousands of euro; "LTV" is the loan-to-value ratio; "LTI" is the loan-to-income ratio (assigned by postcode); "Interest rate" is the percentage mortgage rate; and "Default" is a binary variable that takes the value 1 if the mortgage becomes non-performing at any point in its lifetime (up to 2025) and 0 otherwise. The controls used are explained in Section 4. Standard errors are clustered at postcode, bank and mortgage level. * p-value < 0.10; ** p-value < 0.05; *** p-value < 0.01.

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