Who Truly Bears (Bank) Taxes?
Evidence from Only Shifting Statutory Incidence

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Abstract

We show strong economic incidence effects, including distortionary effects, of *only* shifting statutory incidence (i.e., the agent on which taxes are levied), *without* any tax rate change. We exploit: (i) a policy change in Spain shifting an existing mortgage tax from being levied on borrowers to being levied on banks; (ii) some areas, for historical reasons, were tax exempted (or have different tax rates); and (iii) administrative matched credit register data. After the policy change we find: First, a strong – but not complete – tax pass-through for the average mortgage rate. Second, a *large* heterogeneity in the pass-through: larger for borrowers with lower income, fewer lending relationships, not working for the lender, or facing less banks, consistent with a bargaining power mechanism. Third, an increase in banks' risk-taking: more affected banks reduce costly mortgage insurance and expand into non-affected but ex-ante riskier consumer lending, experiencing higher ex-post defaults within consumer loans.

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1. Introduction

Taxation – given its impact on the economic decisions of agents – is one of the most studied issues in economics. The study of (economic) tax incidence, i.e. which agent bears the economic burden of the tax, helps to identify relevant characteristics of markets, such as price elasticities or existing frictions, and in doing so can serve as a sufficient statistic for welfare analysis of various policy measures (e.g. Chetty, 2009).

One key tax principle is that tax incidence is independent of which agent taxes are levied on, i.e. the irrelevance of statutory or physical incidence (e.g. Kotlikoff and Summers, 1987). Shifting the agent on which the tax is levied does not change the economic incidence of the tax, as price adjustments compensate such shift.[1] However, there are circumstances under which such principle can be violated (see e.g. Chetty et al., 2009; Weyl and Fabinger, 2013), and in such cases the decision of on which agent taxes are levied on may be of first order.

In this paper we analyze the overall and heterogeneous effects of *only shifting* the agent on which taxes are levied (i.e., shifting statutory incidence), without any change in tax rates or in any other policy. We revisit this key classical question by exploiting a tax shift in the banking industry (the credit market) in conjunction with supervisory (administrative) mortgage data.

While showing effects of a shift in statutory incidence is relevant (as in principle it should not matter for tax incidence), focusing on the banking industry, and in mortgages in particular, is also interesting. Not only are banks crucial due to their centrality for the economy and their strong moral hazard problems (Freixas and Rochet, 2008), but also very similar loans to different borrowers have different prices (rates), thereby allowing to identify possible heterogeneous tax incidence through different pass-through. Moreover, public debate about introducing taxes to banks due to their role in financial crises, e.g. expensive tax-payers' bailouts or central bank liquidity injections, is also salient (e.g. G20 proposal, IMF (2010)). Furthermore, taxes on real estate are also a key source of government revenues around the world (Besley, Meads and Surico, 2014; Best and Kleven, 2018) and soft lending standards in mortgages were at the core of the 2008 financial crisis (Jaffee et al., 2009; Freixas, Laeven and Peydró, 2015).

To study potential tax (economic) incidence and distortionary effects of shifting statutory incidence, we exploit: (i) a policy change in Spain in November 2018 that shifts a mortgage tax

[1] This principle is sometimes referred to as tax liability side equivalence, "Dalton's Law" (Hugh Dalton, 1922), invariance of incidence proposition, or physical neutrality and can be traced to Jenkin (1871-72). The study of tax incidence and its relevance for economics can be traced back to the studies of Quesnay.

from being levied on borrowers to being levied on lenders, without any change on the tax rates; (ii) the fact that some areas, for historical reasons, are exempt from paying this tax (or have different tax rates); and (iii) an exhaustive credit register matched with borrower and lender information.

We find that, after the policy change, the average mortgage rate increases consistently with a strong – but not complete – tax pass-through, of approximately 80% of the tax base. Importantly, we show a very large heterogeneity in the pass-through, which is larger for borrowers with lower income, less lending relationships, not working for the lender, or facing a smaller number of banks in their zip-code. Our results are consistent with a stylized model of differential bargaining power between banks and households under smaller tax saliency for households than banks. Our results also suggest that unobservable risk is not as a plausible explanation of the heterogeneity in the pass-through, given the large quantitative effects that we find and the different observed loan interest and default rates across key different borrower variables. Moreover, despite there is no change in the tax rate (which could have led to e.g. inefficiencies associated with tax increases), and consistent with the non-full pass-through, the tax shift induces higher bank risk-taking decisions: Banks more affected by the tax shift exhibit a reduction in their profits and, consistently, reduce costly mortgage insurance in case of loan default (especially so for weaker banks in terms of higher non-performing loans (NPLs)) and increase the likelihood of granting applications of non-directly affected but (much) ex-ante riskier consumer lending, without changing loan rates differentially but experiencing higher ex-post defaults within consumer loans (i.e. spillovers to the riskiest credit).

To the best of our knowledge, this is the first paper that shows economic effects of statutory incidence, without a tax rate change (or any other change in policy or tax evasion) but *only* due to a shift of levying a tax from one (to another) set of agents. Specifically, from borrowers to lenders in the credit market, or more generally from buyers/consumers to sellers/producers. The tax shift, and the banking setting and administrative datasets, allow us identification. Furthermore, as compared to Saez et al. (2012) and Kopczuk et al. (2016), our results suggest different mechanisms at work and novel results (see in the literature review subsection, the key differences of our study with these and other papers). In particular, our findings not only suggest strong overall but also heterogeneous economic incidence effects of shifting statutory incidence – affecting more people with lower income and more generally borrowers with less bargaining power with banks – and provide results consistent with distortionary effects of shifting statutory incidence in terms of reducing costly bank guarantees and increasing non-affected but much riskier lending, especially by more affected banks, even more for those with higher ex-ante NPLs, that have a higher likelihood of future help from taxpayers and/or central banks, proxying for higher bank moral hazard.

Overview of the paper. In the rest of this Introduction we provide an overview of the different sections of the paper and our contribution to the existing literature.

In Section 2 we explain the institutional details. On November 10th 2018, the Spanish government passed a law determining that a tax for legally documenting new mortgages (Actos Jurídicos Documentados) must be levied on banks from that date onwards. As Prime Minister Mr. Pedro Sanchez said: "*Never again Spaniards (households) will pay this tax, banks will pay it*".[2] The tax base is the so-called mortgage liability, which serves as an insurance for the bank in case of mortgage default (given that it is the maximum amount collateralized) and it is one of the features in the mortgage contract. Importantly, this tax is administered at the regional level (*comunidades autónomas)*, and ranges from 0.5% to 1.5% of the aforementioned mortgage liability, with a region in Spain, the Basque Country, with several provinces, where primary residence mortgages are exempt (for historical reasons) from such tax. As already explained, tax rates were not altered by the policy change. One relevant feature of mortgages in Spain (similarly in Europe) is that they are full-recourse: in case of mortgage default, the bank has the right to full repayment of the mortgage obligations, over and above the house (collateral), with present and future household wealth and income. Therefore, even in the 2008-14 financial crisis in Spain, mortgage defaults and loss given defaults were relatively low, see e.g. Bank of Spain's Financial Stability Report (2017).[3]

In Section 3 we explain the datasets. We exploit the exhaustive Spanish credit register (CIR), a proprietary database owned by Banco de España (the central bank in Spain) in its role as supervisor of the Spanish banking system. This administrative database contains the universe of household bank loans granted in Spain by all operating banks on a monthly basis. We observe multiple loan characteristics such as the loan rate, loan amount, the mortgage liability, the maturity, the zip-code of the borrower and of the real state property, the future credit performance of the loan (defaults) as well as the loan-to-value ratio.

We analyze new household loans granted between January 2018 and May 2019.[4] In our main analysis we focus on primary residence mortgages as they were (are) exempt from this tax in the

[2] See https://www.lavanguardia.com/economia/20181107/452788854769/pedro-sanchez-cambio-ley-corregir-tribunal-supremo-banca-pague-impuesto-hipotecas.html. For the law, see https://www.boe.es/diario_boe/txt.php?id=BOE-A-2018-15344, and for taxes in general in Spain, see https://www.agenciatributaria.es/AEAT.internet/en_gb/Inicio/. The legal procedures surrounding the change in the law started during October 2018 and are explained in detail in Section 2. It is important to note that this tax is not a tax-deductible business expense for the bank.

[3] The NPL ratio of mortgages in Spain evolved between a minimum of 1% in 2007 and 6% in 2014, the peak of the impact of the Great Recession and the Euro Area Crisis on mortgages.

[4] The policy change took place in November 2018. As we also explain in the main text, in June 2019 there was another change in mortgage regulation, so our sample stops in May 2019.

Basque Country and are the majority of mortgages in Spain. We also analyze secondary residence mortgages in a robustness exercise. Moreover, we also analyze other household loans (consumer credit), which were not subject to the policy change – i.e. spillovers of the policy change on the riskiest type of loans (in terms of defaults). For consumer credit, we have data on new loan applications that are not currently borrowing from the requesting bank. We know whether a loan application is granted, and for those granted applications, we know loan volumes, rates, maturity and defaults. For the mortgage and consumer credit datasets we have borrower-level information such as employment status, age, gender, job, leverage and credit history, and a proxy for income via the average income in their zip code.[5] Finally, we also match the data to supervisory bank balance sheets and income statements, e.g., the bank capital ratio, size, NPLs, liquidity ratio, ROA.

In Section 4 we explain the empirical strategy. As the tax rate depends on which region the house is located on, our main empirical strategy consists of a difference-in-difference analysis of those mortgages that because of their location are subject to the policy change (treatment group), and those mortgages not subject to it (control group). For the control group, we first exploit the Basque Country where primary residence mortgages were (are) exempt from the tax (i.e., tax rate of 0%), and then we also exploit loans granted in the areas on the administrative border between the control and treatment regions. Having a region in which the tax did not exist in practice, the Basque Country, allows us to have unaffected zip codes.[6] Moreover, we also analyze the change in outcomes over a very narrow time window (two weeks), and control in some regressions for many lender and borrower observables and unobservables (e.g. different type of borrowers or lenders that could be driving the results), as well as loan characteristics, and also check whether those controls change the estimated coefficients (following e.g. Oster 2019, and Altonji et al., 2005). We also exploit differences in the intensity of treatment as different regions have different tax rates, and hence they were differentially affected by the (central/ "federal") government policy.

To further understand the channels, we exploit borrower heterogeneities across, e.g., income and other proxies for borrower bargaining power (borrower-lender number of relationships, number of banks at the zip code level and working for the lender). Moreover, we also exploit that banks could be differentially affected by the policy, as banks differ in their regional exposure, and mortgages in treated areas represent a high fraction of their portfolio for some banks. Finally, we analyze potential distortions via bank risk-taking in costly mortgage insurance and in the riskiest

[5] We assign the zip code of the household to the zip code of the property associated to the mortgage.
[6] Not only is the tax in one region 0% but the policy shock is relatively small, and hence a difference in difference analysis identifies the effects, as a small shock will in principle not generate significant general equilibrium effects. The average tax for the median mortgage (which is around 116,000 Euros) accounts for 1,800 Euros.

segment of loans (consumer lending), also exploiting lender heterogeneities, not only being differentially affected by the policy but also measures of the strength of balance sheets that proxy for bank moral hazard problems, in particular bank NPLs (see e.g. Freixas and Rochet, 2008).

In Section 5 we summarize the results (the tables are at the end of the paper). We first proceed by analyzing the difference on the average mortgage rate before and after the policy change. We find on average a 10 basis-point increase in the yearly total mortgage rates when comparing mortgages granted in treated areas with those granted in the control region (the Basque Country). The estimated 10 basis points represents 5% of the average mortgage rate. This result is robust to introducing various borrower, lender, and location controls as well as a variety of fixed effects. Importantly, the estimated coefficient does not change when we saturate the regressions with controls and fixed effects. Moreover, despite of losing 99% of the sample, results are *identical* when we analyze the border local areas between the control region and other regions that were affected by the reform. Further, we find very similar estimates when we compare mortgages in the Basque Country either with mortgages in Catalonia and Madrid (regions with higher GDP in Spain) or with a matched sample of households. We perform various quantitative analyses that suggest that the average 10 basis point increase accounts for approximately 80% of the tax, not a complete pass-through (thus not consistent with the irrelevance of the statutory incidence).[7] Further, results suggest that the banking industry adjusts the mortgage rates rapidly, as the majority of the pass-through happens during the first two weeks of the tax shift and over the following months there are no further increases in the pass-through. We also find very similar estimated effects when the loan interest rate that we analyze is the last one in our sample (July 2020), instead of the loan rate at origination. Importantly, there are also parallel trends (between the treated and control groups) before the policy shift. Moreover, when we perform an intensity of treatment setting exploiting the different tax rates from all regions, results give exactly the same 10 basis points already mentioned.

We also document no change in the *observable* characteristics of those individuals that were granted a mortgage after (versus before) the policy change, which ameliorates the concern of endogenous selection by borrowers driving the results.[8] Moreover, controlling for borrower and

[7] Given data limitations we cannot analyze mortgage fees at the borrower-bank level. Nevertheless, in order to analyze potential effects in fees we analyze loan related fees at the bank level and do not find any differential effects of the policy on loan related fees across banks more vs. less affected by the policy shift, while we do find differential effects of (i) loan related interest income (higher for more affected banks, consistent with the main loan level results on pass-through) and (ii) total bank profits (lower for more affected banks), which suggest that fees are not driving the results.

[8] We also find no relevant aggregate quantity effects regarding the amount or volume of mortgages surrounding the policy, which is consistent with borrowers not changing their decisions regarding mortgages around the policy change. Nevertheless, results are very similar if we omit the period just before and just after the tax change (e.g., without the months of October and November 2018).

loan characteristics increases the R-square by 35 percentage points but keeps the estimated coefficient identical, thereby suggesting that *unobservables* are not driving the results (following Altonji et al., 2005; Oster, 2019). To further rule out possible changes in credit conditions as drivers of loan rate changes, we find that there is no change in other key characteristics such as the amount of the mortgage, the loan to value ratio or the maturity of the mortgage.

We then document the heterogeneity in the pass-through to mortgage rates and how it depends on both borrower (and bank) characteristics. We find a substantial lower pass-through for borrowers with higher income, higher amount of banking relationships, higher number of banks operating in their zip code and those borrowers working for the lender. For instance, households in the 75% compared to those in the 25% of the income distribution have 8 basis points less increase in the loan interest rates after the policy change. Similarly, households in zip codes with more banks (75% versus 25% of the distribution of banks' presence) have, on average, 2 basis points less increase in the loan interest rates. There are 7 basis points less increase for households with more banking relationships (again 75% versus 25% of the distribution of banking relationships). Further, if the borrower works for the lender, there is no pass-through. All these margins (heterogeneous results) are consistent with heterogeneity in borrowers' bargaining power with respect to the lender, and we provide a stylized model consistent with differential bargaining power mechanism under the assumption that for households (as compared to banks) tax saliency is smaller when they negotiate for a mortgage. Moreover, given the large quantitative effects that we find and the observed loan interest and default rates across different borrower variables, the results suggest that unobservable risk is not an explanation of the documented heterogeneity in the pass-through.[9] For example, borrowers with higher ex-ante number of bank relationships obtain lower pass-through on loan rates but default more ex-post.[10] Hence, we argue that these large heterogeneous effects are an unintended consequence of the tax shift, consistent with some borrowers having higher bargaining power.

[9] We find that the observed heterogeneity in pass-through between low and high income individuals and those with low and high bank relationships would only be consistent with probability of default changing in absolute terms by more than 3%. Given this very large increase in defaults rates given the small tax amount, and also given the different sign between defaults and loan interest rates for number of relationships (see also next footnote), results suggest that that the heterogenous effects we find are not consistent with borrower risk (ex-ante differences in observed loan interest rates and default probabilities of mortgages, even historical ones).

[10] We also find that a borrower with a higher number of bank relationships is associated to higher default rates on average (for previous research analysing how the number of bank relationships is positively related to borrower risk, see Detragiache, Garella and Guiso, 2000). Crucially, we find that these defaults *increase* after the policy shift for the treated (compared to the control) areas, despite that these borrowers obtain *lower* pass-through on mortgage rates. Hence borrower risk cannot explain the results on heterogeneity. For income, we find that higher income households have lower pass-through, but there are no differential effects for mortgage defaults after the policy shift for treated as compared to control areas (though on average higher income households have lower defaults). For mortgage defaults, we use loan delinquencies but also borrowers that ask for a loan moratorium during the Covid-19 crisis.

We argue that the fact that certain borrowers (high income, higher number of banking relationships, more banks in the local area, bank employees) experience a much smaller increase in their mortgage rates than other borrowers (i.e. weaker pass-through), when the tax is imposed to banks instead of to them, is evidence not consistent with statutory incidence being irrelevant for tax incidence, and suggest strong distributional effects of statutory incidence. Borrowers with weaker (versus stronger) pass-through have lower costs (net of taxes) of obtaining a mortgage after the shift in policy and, given that we do not find any other changes in mortgage characteristics (LTV, maturity, volume), the results suggest that borrowers with weaker pass-through increase their relative welfare as they obtain the same mortgage at a lower total cost relative to the borrowers with higher pass-through (e.g. lower income borrowers or with less number of lenders). For evidence not consistent with statutory incidence being irrelevant for tax incidence, note that these large heterogeneous effects are in addition to the strong but not complete pass-through to loan rates (that we described before) and to the distortionary effects that we will discuss below.[11]

Once the results suggest strong economic incidence of only shifting statutory incidence (without any tax rate change), we analyze whether the tax shift causes distortions in banks' risk-taking decisions. If banks do not fully pass-through the cost of the tax to borrowers (see the above results), the policy change may reduce bank revenues, thereby potentially increasing banks' risk-taking incentives (e.g. Keeley, 1990, Holmström and Tirole, 1997; Hellman, Murdock and Stiglitz, 2000; Freixas and Rochet, 2008; Freixas, Laeven and Peydró, 2015).

We first document that those banks more affected by the tax shift (i.e. those banks with a higher proportion of their assets as mortgages in regions affected by the tax shift) exhibit a higher increase in their loan interest rate income (consistent with our loan-level results documenting a positive pass-through to mortgage rates), no differential effects on loan fees, and suffer a higher relative decrease in their profitability (ROA).

Consistent with the non-full pass-through, and hence with a reduction in bank profitability, we find that banks more affected by the policy increase risk-taking by changing their mortgage and non-mortgage lending strategies. We find that, after the tax shift, more affected banks reduce more the costly mortgage liability (i.e. an insurance in case of mortgage default) and increase the

[11] We also document that some bank characteristics are key for the pass-through. The increase is higher the higher the capital to asset ratio of the bank, which is consistent with equity being the more expensive source of financing for the banks (Freixas and Rochet, 2008) and hence the shift being more costly for those banks. The increase is also higher for banks with higher ratio of household loans to total assets (consistent with these banks being more specialized in mortgages), and for banks with higher NPLs, consistent with higher costs of funding due to higher provisioning.

probability of granting applications to non-directly affected (but much riskier) consumer loans.[12] The amount set as mortgage liability serves as the collateral of the mortgage, i.e. it is the maximum amount that the bank can directly appropriate from selling the house in case of the mortgage defaulting. Hence, the tax shift induces banks to increase their risk as they become more exposed to loses (lower recoveries) in case of mortgage defaults, and also as they grant more consumer loans, which are substantially riskier than mortgages (e.g. higher default probability and higher loss given default as consumer loans are generally non-collateralized). This increase in bank risk-taking is probably an unintended consequence of the tax shift (note also that banks are generally bailed-out in case of strong distress and receive generous central bank liquidity injections), thereby highlighting the relevance of statutory incidence in affecting banks' risk-taking.

In particular, regarding the mortgage liability, we find that the reduction in mortgage liability after the tax shift is not only higher for more affected banks, but it is also higher for those mortgages in treated areas. Interestingly, we find that the reduction in mortgage liability is stronger for more affected banks with weaker ex-ante balance sheets, in terms of higher NPLs, which can proxy for those banks having larger moral hazard problems (see Freixas and Rochet, 2008). We also find that the reduction in mortgage liability is unrelated to observable characteristics of individuals (which, see the above results, are key drivers of pass-through to mortgage rates).[13]

Regarding non-affected household loans (i.e., consumer loans), we find that the probability of granting a consumer loan application after the tax shift is higher for more affected banks (with higher ex-ante mortgage volume in treated areas) and is also higher in treated areas.[14] We also find that there is no change in the conditions of granted consumer loans (loan rate, maturity or loan amount) but, after the policy change, there is an increase in the ex-post default rate of granted consumer loans in treated areas. This finding is consistent with a relaxation of the lending standards on consumer loans associated to the shifting of the mortgage statutory incidence, i.e. higher risk-taking via higher ex-ante risk, without changing loan rates, and with higher ex-post defaults.

Related literature. Our main contribution is to the literature analyzing the implications of tax interventions in markets in general. As previously argued, the literature analyzing tax incidence and how taxes affect economic decisions is ample, e.g. Kotlikoff and Summers (1987) and Fullerton and

[12] For example, loan rates for consumer loans are 9% in our sample period as compared to only 2% for mortgages

[13] The only borrower characteristic that we find is relevant both for pass-through in mortgage rates and mortgage liability is whether the borrower works for the lender. We also find that our results regarding the heterogeneity in pass-through to loan rates are robust to introducing (the endogenous) mortgage liability as a control.

[14] While we also find that the effect is stronger for more affected banks with weaker ex-ante balance sheets in terms of higher NPLs, this result is not significant at conventional levels.

Metcalf (2002) provide comprehensive reviews of the literature. The novelty of our study relies on its focus in one important aspect of taxation – statutory incidence – rarely analyzed empirically, as tax changes are normally associated to changes in tax *rates*, not just purely shifts in the agents on which taxes are levied. Moreover, we empirically analyze statutory incidence in one crucial market – the credit market – using a real policy change (in conjunction with administrative datasets).

Our paper is related to those studies analyzing how different tax characteristics determine its incidence. Chetty et al. (2009) shows the relevance of saliency for tax incidence. Saez et al. (2012) finds that, after an overall reform in payroll taxes in Greece, employers compensate for the extra employer payroll taxes but not for the extra employee payroll taxes. They argue that their results suggest that the mechanism at work is the inability of employers to pay similar workers differently when they are subject to different taxes, related to pay fairness norms and wage rigidities inside the firm. Kopczuk et al. (2016) shows the relevance of tax evasion capacity (technology) – the mechanism at work – of the producer on which taxes are levied for tax incidence in the diesel industry. By having different evasion capacities, the "effective" tax rate that different producers face (once they undergo tax evasion) is different, which in turn affects the incidence of the tax. In comparison to these studies, we analyze a policy change exploiting only a shift in statutory incidence – without a change in the tax rate (or even the "effective" tax rate due to tax evasion), or any other related policy – which significantly improves the empirical identification of (i.e., isolates) statutory incidence. Moreover, our results suggest different mechanisms at work and provide novel results. Our results show not only strong overall effects but also large heterogeneous effects on borrowers – on different household income and different borrower-bank connections, which are proxies for borrowers' bargaining power – not consistent with the irrelevance of the statutory incidence (shift); the empirical results are consistent with a stylized model with differential bargaining power under the assumption that tax saliency is smaller for households than for banks. Moreover, by analyzing banks and credit, and given that there is not a full pass-through, our results further suggest distortionary effects due to the change in statutory incidence: More affected banks by the policy, and even more those with higher moral hazard problems (proxied by ex-ante riskier assets, higher ex-ante NPLs), take substantial higher risk after the policy change. They do so by reducing costly mortgage insurance, and by having a higher likelihood of granting applications in the ex-ante riskiest type of loans (consumer credit), experiencing even higher ex-post defaults within consumer loans (despite of no change in ex-ante loan rates).

Our analysis on mortgage taxes relates to those papers analyzing the incidence of transaction taxes in housing markets. Best and Kleven (2018) and Besley et al. (2014) study the effects of

introducing stamp duty holidays in the UK. Crucially, our analysis differs from these previous studies as we analyze a change in statutory incidence without changing the tax rate (as a stamp duty holiday implies) and find important effects associated with only shifting statutory incidence. Moreover, our findings on banks and credit also relate to a large literature analyzing the economic consequences of distortions in credit markets (see e.g. Khwaja and Mian, 2008; Chodorow-Reich, 2014). Our results on how changes in market conditions affect banks' risk-taking relates to various studies analyzing the determinants of bank risk-taking decisions. Previous research has shown how banks' risk-taking can be shaped by various policy measures: e.g. capital requirements (Hellman et al., 2000), competition (Keeley, 1990) or monetary policy interventions (Jiménez et al., 2014), and how, in line with our findings, targeted interventions in a given market can have (unintended) spillover effects on other markets (Chakraborty et al., 2019). Our main contribution to this literature is to show how statutory incidence generates relevant effects for banks, resulting in higher risk-taking, and for borrowers, given the heterogeneous pass-through to different type of households.

In Section 2 we discuss the institutional details. Section 3 describes the data and Section 4 explains the empirical strategy. In Section 5 we summarize the results of the paper. Section 6 briefly presents some concluding remarks. Finally, we provide the tables at the end of the paper as well as a stylized model to rationalize the overall and heterogeneous results.

2. Institutional details

Mortgages in Spain are subject to an administrative tax that has to be paid upon the formalization of the mortgage (*Actos Juridicos Documentados*). This tax accounts for 1.5% of the mortgage value on average and is based on the so-called mortgage liability (*responsibilidad hipotecaria*). Mortgage liability is based on the value of the mortgage at inception and its main role is to determine the maximum amount that the lender can directly appropriate by selling the house in the case of mortgage default, i.e. the collateral amount of the mortgage. Hence, the mortgage liability can be seen as a costly insurance for banks: costly because of the tax, and insurance because of the collateral.

This tax is administered at the regional level (*comunidades autónomas*), and ranges from 0.5% to 1.5% of the aforementioned mortgage liability.[15] Interestingly, a region in Spain, the Basque Country, has primary residence mortgages exempt from such tax. The underlying reason

[15] Table 3 in the Appendix provides details on the exact base tax rate in each region. The exact tax rate depending on whether the mortgage is for primary residence, or the age of the borrower. To reduce this dispersion, only primary residential mortgages are considered in the main analysis and the age of the borrower is included as a control. In Spain, the mortgage liability of the average mortgage is around 1.5 times the amount of the loan.

from such exemption is that, for historical reasons, the Basque Country has a special tax system different from the other regions in Spain.[16]

Originally the tax was levied on the households who borrow via a mortgage, i.e. statutory incidence fell on borrowers. On the 18th October 2018, the Supreme Court in Spain stated a new mandate by which the agent that should pay the tax was the bank. However the mandate was not effective as one day later, on the 19th of October 2018, it was put on hold given the "important economic and social impact" of the issue. On the 6th of November 2018 the decision of the Court was to maintain the original mandate in which the tax was levied on households. The day after, 7th of November 2018, the prime minister of Spain, Mr. Pedro Sanchez, stated: "*Never again will Spaniards pay such tax, it will be paid by banks*".[17] On the 8th of November 2018 a new law by the central government – a Royal Decree – was approved declaring that the tax has to be paid by the banks granting the mortgage (Real Decreto-ley 17/2018) from that moment onwards. Such law started to be effective on the 10th of November 2018, shifting statutory incidence to lenders.

In short, on the 10th of November 2018 the tax shifted from being levied on households to being levied on banks – i.e., from borrowers (credit demand)/consumers to lenders (credit suppliers)/ producers. Given the timing of the rulings, there may be some anticipation effects being already present during October 2018.[18] Importantly, as tax rates were not altered by the policy change, there was only a change in statutory or physical incidence. In addition, these taxes are not a tax deductible expense for banks.

Another relevant development regarding the mortgage market in Spain is that on the 16th of March 2019 the Spanish government passed a new law (Ley 5/2019) regulating various aspects of mortgages in Spain, which would take effect three months later (this new law in great part was to transpose into the Spanish legislation the European directive 2014/17/UE). This suggests that after (or on) June 2019 there are other relevant developments in the mortgage market that could confound our results. For this reason, and also in order to have a balanced number of periods before

[16] In Spain (similarly in Europe), mortgages are full-recourse. That is, in case of loan default, the bank has the right to full repayment of the mortgage obligations over and above the house with present and future household wealth and income (over and above some minimum income that goes directly to the household). Therefore, even in the 2008-14 strong financial crisis, mortgage defaults and loss given defaults were relatively low, see e.g. Bank of Spain's Financial Stability Report (2017). In particular, in 2014 mortgage NPLs reached their peak at 6% and LGD was (approximately) 16%. Mortgage volume over GDP was around 50%. The defaults during the long crisis were to firms, mainly related to real estate (based on NPLs and LGDs), but not to households (Freixas, Laeven and Peydró, 2015).

[17] See https://www.lavanguardia.com/economia/20181107/452788854769/pedro-sanchez-cambio-ley-corregir-tribunal-supremo-banca-pague-impuesto-hipotecas.html. For this law, see https://www.boe.es/diario_boe/txt.php?id=BOE-A-2018-15344, and for taxes in general in Spain, see https://www.agenciatributaria.es/AEAT.internet/en_gb/Inicio/.

[18] Figures 2, 3 and Figure 4 in Appendix show how anticipatory effects were either inexistent or small. We also perform robustness analysis excluding October and November 2018, in order to eliminate possible anticipation effects, and find that results are very similar. See below the section on Results.

and after the main policy change, we start our analysis on January 2018 and end it on May 2019. Moreover, we show the estimated effects for the main regression for every month in our sample data to check for pre-trends; potential anticipation effects; how fast changes (pass-through) in loan prices occur; and finally, whether there are effects after March 2019 given the announcement of a new law that becomes effective on June 2019. Moreover, for robustness, we also analyze the last available loan rate (July 2020), instead of loan rates at origination, to check whether results are significant even two years after the policy shift, given the potential change in loan rates over the life of the loan.

The amount of new residential mortgages in Spain in 2018 was 43,284 million euros. The main lenders of residential mortgages in Spain are banks which represent above 98% of the total volume of mortgages.[19] Fixed (variable) rate mortgages in Spain represent 37% (63%) of new residential mortgages in 2018. The vast majority of variable rate mortgages (more than 99%) that were granted in 2018 were referenced to the Euribor (which was raising over our sample period). Further, different from the US, there is no securitization to public agencies (such as Fannie Mae and Freddie Ma) and the private market of securitization is very small (Jiménez et al. 2020). Moreover, a key feature of mortgages in Spain (similar to other European countries and some US states) is their full recourse nature. In case of mortgage default, the mortgage liability serves as the collateralized amount of the mortgage, and if proceeds of selling the house are not enough to fulfill the debt obligations, the debtor is still liable with present and future income and wealth. In such cases, while the debtor is still liable for the non-repaid part of the mortgage, there are various rules about a minimum amount of the borrower's income that cannot be seized in order to fulfill the debt obligation.

3. Datasets

In our study we combine three (matched) administrative datasets: (i) the Spanish Credit Register with information on loan level data on the universe of mortgages, including borrower (household) and lender (banks) characteristics; (ii) supervisory bank balance sheet information; and (iii) loan application data for consumer loans.

We exploit the Spanish Credit Register (CIR), a confidential loan-level database that contains all loans granted in Spain by any bank operating in the country since 1984 at a monthly frequency.[20]

[19] Different from US, where a key segment of lenders is fintech, in Spain fintech activity is very small, and they are concentrated on payments and firms.

[20] By banks we refer to commercial banks, savings banks and credit cooperatives. They provide 98% of mortgages, and they all take deposits and are regulated. Commercial banks provide 88.6% of all mortgages. We do not analyze other

For the purpose of the paper we analyze loans to households and, in particular, on all primary residence mortgages granted (in this case we have a region with 0% tax rate, unaffected) between January 1, 2018 and May 31, 2019. However, we also analyze: (i) secondary residence mortgages: statutory incidence also changed for these mortgages, and also there was no tax change rate in these mortgages, but all tax rates are positive for these mortgages though with different tax rates; and (ii) consumer lending: (directly) not affected by the change in the tax law, but with potential spillovers as loans to households are divided between mortgages and consumer lending.

Importantly, in 2016 the CIR was modified to, among other changes, reduce its reporting threshold from 6,000 euros to 0 euros, i.e. we have the universe of loans. The CIR improvement also affected the information reported on borrowers and loan conditions. In addition to the usual information about loan characteristics provided by the previous CIR (such as the type of instrument, currency, degree of collateralization, default status, the amount granted and the borrower nationality), many other characteristics of the loan were included or improved such as the loan interest rate (amount and type), the exact maturity, the mortgage liability, the loan to value ratio (LTV), defaults and the zip code of the property among others. Moreover, since 2016 the CIR also began to store information of the borrower such as her employment status (unemployed, public servant, student, banking group employee…), age and gender. We exploit all this information along with her credit history. Moreover, as we do not have borrower-level income, we proxy the gross income of the household with information at the zip code level. We use the average gross income of the households at the zip code level in 2016 (which is the last available year) provided by the Spanish Statistical Office (INE), where the the number of zip codes in our analysis is 11,752 (the population is just below 47 million people). We also proxy borrowers' bargaining power by including the number of banks granting loans in the zip code where the house is located and also the number of banking relationships that the borrower has as of December 2017.

For our main regressions, we focus on newly originated primary residence mortgages, excluding from our sample those households who have a self-employed worker among their members. We exclude self-employed workers as these workers sometimes use their residences as their workplace and we do not have their firm related information, which is very relevant. Moreover, we do not include renovations or refinancing of mortgages, given the special characteristics of these type of operations.[21] As a robustness, we also consider secondary residence

financial credit intermediaries (*entidades financieras de crédito*), which are substantially less regulated, do not take deposits and only represent 2% of mortgages.

[21] Primary residence mortgages are the main share of mortgages in Spain. Renovations and refinancing represent less than 20% of the mortgage market in Spain during 2018 (www.ine.es). In particular, renovations and refinancing

mortgages despite that for this type of mortgages tax rates were positive in all regions (the Basque Country only exempts primary mortgages).

As well as newly granted mortgages, in Section 5.2.2 we also analyze newly granted consumer loans. In particular, we also exploit a new dataset of consumer loan applications during the same time period. This database contains information of loan applications made by borrowers without current relationships with the lender. Once this information is merged with the CIR, it is possible to know whether the loan application was finally accepted by the bank and granted. For a more detailed description of the CIR see, for instance, Jiménez et al. (2012, 2014, 2017). We also have loan-level information on loan rates, volumes, maturity, and defaults. Importantly, consumer loans were not (directly) affected by the policy change and are substantially riskier than mortgages. Consumer loans exhibit much higher default probabilities and are generally uncollateralized, and consistently they have substantially higher loan rates than mortgages (see also Table 1).

Finally, we use the balance sheets and income statements of banks as of 31st December of 2017 (when our sample starts). The Bank of Spain, in its role of supervisor, periodically receives detailed information of bank's balance sheet and profit and loss accounts. In this paper, we consider the log of total assets as a proxy of the size of the bank, the capital ratio as the ratio of owns funds over total assets, the ratio of liquid assets over total assets, the return on assets (ROA) for total bank profitability, the NPL (non-performing loan) ratio capturing the risk profile of the bank and the household credit volume over total assets as a measure of the bank portfolio specialization on mortgages. We also include some other borrower-bank variables to capture the strength of the relationship such as whether the bank was the main lender of the household as of December 2017, whether the bank was the leader bank (highest mortgage market share) in the zip code, or how much mortgage exposure the bank has in the most affected areas. We also include the number of banks in every zip code to proxy for bank competition and the number of bank relationships a borrower has.

4. Empirical Identification

We start by analyzing the impact on loan interest rates of newly originated mortgages of shifting the statutory incidence of the mortgage tax. For economic tax incidence, we analyze overall pass-through as well as heterogeneous effects. We then analyze bank risk-taking by more affected banks and in more affected areas, in particular costly insurance (mortgage liability) and consumer lending (i.e., lending to households not affected by the change in the tax law).

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operations with just a change in loan rate and/or maturity do not have to pay the mortgage tax again (law 41/2007) and, hence, are not affected by the tax shift.

As explained in detail in the previous sections, on November 10, 2018, a Royal Decree (policy change) entered into force changing the taxpayer of the tax from the borrower to the bank, a shift in the statutory incidence, with heterogeneity across regions (in Spain this tax is transferred to the regions), and without a change in the tax rate (just a change on whom the tax is levied on). As there is no other policy event (such as a change in the tax rate or an overall reform) and the context is not one on (changing) tax evasion, but only shifting the statutory incidence, the setting (policy change and administrative data) significantly improves the empirical identification of (i.e., isolates) statutory incidence.

The territorial idiosyncrasies of Spain help with the identification strategy, as the Basque Country has its own tax regime, which means that the Royal Decree has no effect on this jurisdiction and different regions have different tax rates. Importantly, given that the Basque Country has a tax rate of 0% before and after the introduction of the law for the primary residence mortgages, we use a difference-in-differences specification to fit the quasi-experiment that arises after the modification of the law, where the control (unaffected) areas were the locations in the Basque Country and the treated areas the ones outside this region. As explained below, we also exploit zip codes around the border of the Basque Country and other regions (despite a reduction in 99% of the sample), as well as we also analyze all the regions via differential intensity in treatment, as tax rates vary across regions from 0% to 1.5% (there are 19 regions in Spain, including two autonomous cities; 50 provinces and 2 autonomous cities; and the number of zip codes in our analysis is 11,752).

We construct a treatment variable as the product of the dummies *Post* and *Treated*, where $Post_t$ refers to periods after November 10, 2018 (the day when the Royal Decree entered into force), and $Treated_i$ refers to all new mortgages of households whose property is located in the territory on which the Royal Decree applies. Hence, the control group are all mortgages on properties located in the Basque Country as they are not affected by the law.[22]

Thus, if we denote by $Interest\ rate_{ijt}$ the loan interest rate of mortgage *i* granted by bank *j* at day *t*, we estimate by OLS the following diff-in-diff regression:

$$Interest\ rate_{ijt} = \beta Treated_i * Post_t + X_{ij} + \eta_{ijt} + \varepsilon_{ijt} \quad (1)$$

[22] The case of Navarra is singular because although it has its own regional regime such as the Basque Country, it decided to change its own law to align with the rest of Spain as it had positive tax rates, different from the Basque Country (see Table A4). Results are the same if we exclude mortgages from Navarra.

where X_{ij} *is* a set of controls related to some head-of-the-household characteristics is a vector of household (oldest of the debtors of the mortgage) or household-bank (or bank) characteristics associated to a particular mortgage such as income, number of bank relationships, whether the borrower works for the bank, number of banks in the zip code, or bank NPL, η_{ijt} is a vector of fixed effects at mortgage type-bank-time (year:quarter, year:month or year:month:day) and ε_{ijt} is the error term.

We proceed by first showing the estimated effects without any control, then progressively saturating the regression and finally showing the results with all the controls. The set of household characteristics controls for observable and unobservable time-invariant household specific factors that affect equally the interest rates of all mortgages and allow us to reduce possible differences between the households assigned to the treatment and control groups. Note that we cannot include household or loan fixed effects as these are mortgages around the change in the law for buying a primary residence for the household (i.e. there is no repeating borrower over the household before and after, and there is generally one house purchase and hence one loan per household). Our set of household controls include a set of dummies depending on the specification we use: zip code*employment status and zip code*employment status*foreign, where zip code captures average household income and wealth, and employment status distinguishes between public servant, bank group employee (of the lender), student and unemployment, homeworker or rest of employees, and foreign is a dummy capturing whether the head of the family is a resident but with foreign nationality. The set of household controls also includes, in some specifications, other observable household characteristics such as her credit history and number of banking relationships. Moreover, we control in some regressions for other loan characteristics such as the maturity or the amount granted or mortgage liability to check the stability of the estimated coefficients.

The bank-time (η_{jt}) fixed effects control for observable and unobservable time-variant bank factors. In the most stringent specification we interact this set of bank-time effects with the type of mortgage loan (fix or variable rate). The inclusion of the type of mortgage loan by the time when the loan was granted by each bank has the advantage of homogenizing all mortgages and allow us to better compare loan rates. Standard errors are triple-clustered at the bank, time, and zip code level to allow for serial correlation across mortgages of the same bank and those granted in the same period or in the same zip code over time.[23]

[23] As robustness, we have also clustered the standard errors at regional level instead of at zip code level using the wild cluster bootstrap, and the estimated coefficients are still significant. Effects are also significant with region*pre/post clustering.

The coefficient β on the product of *Treated*Post* captures the impact on loan interest rates of the Royal Decree after its introduction in the regions where it applies with respect to the control group. We will analyze both the overall effects of the tax change, and also heterogeneous effects across difference household variables such as household income and borrower-bank connections. To test for heterogeneous effects in our variable of interest, we estimate the analogous of Eq. (1) including an additional interaction term. The equation takes the form:

$$Interest\ rate_{ijt} = \beta Treated_i * Post_t + \gamma Treated_i * Post_t * X_{ij} + X_{ij} + \eta_{ijt} + \varepsilon_{ijt}, \quad (2)$$

where, X_{ij} is a vector of household or household-bank (or bank) characteristics.

Additionally, to estimate the effects on other relevant mortgage related variables (such as maturity, loan to value ratio, loan amount or loan amount/mortgage liability, defaults) we replace the left-hand side variable of the above equations by each of the aforementioned loan variables. For consumer loans, we also study the likelihood of granting a loan application and loan outcomes such as loan volume, rates, maturity and defaults.

There are regional differences as the Basque Country is one of the richest regions in Spain. Regarding possible differences in observables between the treatment and control groups, columns 1 to 3 of Table A1 in the Online Appendix (at the end of the paper) illustrates the differences of mortgage loans between those granted in the Basque Country and outside. It shows, for some household, bank and loan observed characteristics, the average differences by treatment and control group. For comparison among groups, we use the Imbens and Wooldridge (2009) statistic, which avoids the sample-size dependence on the mean test by computing the difference of the means of each variable for the two groups normalized by the square root of the sum of the variances of the variables. Its absolute value is compared with 0.25, a heuristic value proposed by Imbens and Rubin (2015) to test whether the differences should be considered significant or not. As expected, the gross income of the households in the Basque Country seems to be larger (10.42 vs. 10.28), which is in line with the fact that the loan amount is higher and the interest rate lower (11.68 vs. 11.45 and 1.57 vs. 2.10, respectively). Moreover, the average bank that grants mortgages to households in the treatment group is bigger and riskier. This evidence highlights the importance of controlling for income proxies of the household and for bank factors, either through fixed effects or with observed characteristics.

The last three columns of Table 1 provide the same comparison but restricting the sample to the mortgage loans granted in the zip codes adjacent to the border of (both outside and within) the Basque Country. The observed differences diminish, making the two groups more similar. For

example, for household characteristics only the number of banks per household is significantly different. Nevertheless, in addition to controlling for different characteristics (observables and then unobservables through fixed effects), we will also test selection on further unobservables following Altonji et al. (2005) and Oster (2019), see the results section: e.g. the R-squared increases by 35 percentage points and the estimated coefficient slightly increases in absolute value but not different statistically, thereby suggesting significant results even if we could control for further unobservables. Moreover, the estimated effect is identical in the sample with all loans (with all regions) and in the restricted sample with loans only to areas in the border of the Basque Country, despite we lose 99% of observations.

In addition, we also test whether before versus after the policy shift there are differences in the observed variables. No household, loan or bank variable has a normalized difference higher than 0.25. The variable with the higher difference is loan rates (with an absolute normalized value equaled to 0.16), which is consistent with higher loan rates after the tax law in the treated areas (i.e., the pass-through). Therefore, results do not suggest a shift in the composition of household (demand characteristics) or bank (supply characteristics) for mortgages following the change in the Royal Decree. At this point it is important to recall that the tax represents less than 1% of the total cost of buying a house.

Crucially, to interpret the estimated coefficients as the causal impact of the new regulation on loan interest rates, we need to analyze the parallel trend assumption. The results suggest that this assumption is met in our study. Figure 2 plots the monthly estimated coefficients of Eq. (1), the time-varying difference in difference estimated coefficients, using January 2018 as the reference date. It shows that, before the tax law, the average loan rates are not different from zero, which shows the parallel trends assumption of the diff-in-diff specification (see the results section). Moreover, with the entrance into force of the policy shift, the interest rate of treated mortgage loans begins to diverge.

Complementary to our identification strategy based on regional differences in the introduction of the Royal Decree, we also analyze how banks that were differentially affected by the policy change reacted. To do so, we classify banks regarding the weight of their mortgage portfolio outside the Basque Country at the end of 2017 (when our sample starts) over their total assets, as banks most exposed to this type of mortgages should be the ones most affected by the change in the tax law. The equation that we estimate in this case is the following:

$$Interest\ rate_{ijt} = \beta High\ Exposure_j * Post_t + X_{it} + \eta_j + \varepsilon_{ijt}. \quad (3)$$

In this specification *High Exposure* is a dummy that takes the value of 1 if the ratio of mortgages outside the Basque Country over total assets of the bank that grants the loan is above the median value of the distribution, and 0 otherwise; and X_{it} is a set of household observable characteristics and loan controls, as in Eq. (1), and we also include the fixed effects zip code*employment status*foreign and type of mortgage loan (fixed or variable rate)*granted time (year:month:day); and η_j are bank fixed effects. We also estimate this equation by OLS and the standard errors are multi-clustered at three levels: bank, time, and zip code. In addition to loan interest rates, we also analyze volume, maturity, LTV, mortgage amount over liability and defaults.

Additionally, to test for possible heterogeneous effects of our variable of interest, we estimate the analogous of Eq. (3) including an additional interaction term. The equation takes the form:

$$Int.rate_{ijt} = \beta High\ Exposure_j * Post_t + \gamma High\ Exposure_j * Post_t * X_j + X_{it} + \eta_j + \varepsilon_{ijt} \quad (4)$$

where X_j is a vector of bank characteristics, in particular bank NPL which proxies for the strength of the bank balance sheet and hence it is a proxy on bank moral hazard issues (see Freixas and Rochet, 2008). The higher ex-ante bank NPL, the higher probability of bank failure, and hence higher need of rescue by the government or need of central bank public liquidity (implicit guarantees), or to activate the deposit insurance (explicit guarantees).

The previous analyses are done at the loan level but we also work at the bank level when the information does not allow a more granular approach (fees) or for some key variables such as bank total profits (also split by loan interest income versus loan fees). In particular, we explain the interest income of loans over total assets, loan fees over total assets and ROA of the banks using quarterly data from 2018Q1 to 2019Q2. The equation estimated in each case is the following:

$$Bank\ variable_{jt} = \beta High\ Exposure_j * Post_t + \eta_t + \eta_j + \varepsilon_{ijt}, \quad (5)$$

where η_t are year:quarter time dummies and η_j are bank fixed effects. We estimate the equations by OLS and we cluster standard errors at the bank level. Finally, we also analyze mortgage amount over liability and consumer lending in a regression similar to equation (1).

4.1 Summary statistics

The first part of the paper uses a loan database that consist on all new primary residence mortgage loans granted to households in Spain between January 1, 2018, and May 31, 2019.[24] Table 1 reports the mean, standard deviation, first, second and third quartile of the distribution of the main

[24] As a robustness exercise, in Section 5.1.2, we also show that results are the same if secondary residence mortgages are also taken into account.

variables that we use in the analysis. The sample is classified depending on the value of the dummies *Post* and *Treated*.

As Table 1 shows, 43.9% of mortgage loans were granted after the tax change, and the percentage of loans potentially affected by this measure represents 94.2% of the overall sample (for all the regions, see also Table 2) and 57.4% for the of the sample restricted to the border zip codes around the Basque Country (see also Table 3). The main dependent variable, the interest rate charged to the mortgage loans, has an average value of 2.07 with a large dispersion evidenced by a coefficient of variation of 48%. The average value of the maturity (in months) of the mortgage is 299 while the average loan amount (in euros) is 117,607. Mortgage defaults are 5.8%, which include loan delinquencies and borrower-driven moratoria during the Covid-19 crisis. We also study other key dependent variables for mortgage data: loan amount over mortgage liability (costly insurance for mortgages) with an average value of 73%; and LTV, with an average value of 65.9.[25]

Regarding household (borrower) characteristics, the log of the gross income (in euros) has a mean of 10.29 and a standard deviation of 0.19 (the average gross income is 29,995 Euros). For 8% of the households, the head of the family (older member of household) is a public servant, for 2.8% a pensioner, for 1.4% an employee of the same bank group that the bank that granted the loan, for 2.8% a student, and for 2% either home employed or unemployed. The omitted category includes the rest of employed workers. The average log of age (in months) of the head of the family is 6.15 (40 years old). The average value of the log of one plus the number of banks with which the household has a loan at the end of 2017 is 0.33 (that corresponds to 0.52 banks), and the average of the log of one plus the number of banks that have a branch operating in the zip code of the mortgage real estate is 1.9 (which corresponds to 5.7 banks).

Regarding the average characteristics of the lender just prior to our sample (at the end of 2017), the log of total assets has a mean of 18.62 (230,510 million euros), 8.49% is the average value for the capital ratio, and the non-performing loan (NPL) ratio has an average value of 6%. The average weight of loans to households over the total portfolio of the bank accounts for 26%. The bank that granted the mortgage was the main lender of the households as of December 2017 for the 16% of the loans analyzed and 24% of the times the main lender in a zip code is the provider of the mortgage. A key treatment variable for part of our analysis (Section 5.2.1) is whether the lender has a high exposure to mortgages outside the Basque Country, and, hence, it is potentially more affected by the change in the tax law. As we define this variable as whether the exposure is higher

[25] We analyze these variables as dependent variables in some regressions (see Table 7), but also as (endogenous) controls in some columns (e.g. last two columns in Table 2) to test the stability of the main estimated coefficient.

than the median, we have the average of high exposure being 0.54. We also use some bank level variables as dependent variables to check the overall impact of the law on some bank variables such as bank profits (ROA with an average of 0.51%), fees related to loans over total assets (with an average of 0.08%) and interest income from loans over total assets (with an average of 0.94%).

The last part of the paper uses the consumer loans database that includes the consumer loan applications, and, as in the previous part of the paper, all the new granted consumer loans. 51% of the loan applications are accepted and granted during our sample period. The average interest rate of newly granted consumer loans is 9.5%, much higher than that of mortgages (as these loans are much riskier), where the average (log) size and maturity of the consumer loan is 8.7 and 3.9, respectively (which corresponds to 9,693 euros and 58 months). The future default rate is also very high (12.2%) for this type of loans.

5. Results

In this section we first analyze in section 5.1 the effects that the shift in statutory incidence has on mortgage rates, documenting how there is a strong, positive (but not full) pass-through to mortgage rates, which is highly dependent on borrower characteristics, most of them related to borrowers' bargaining power. We then, in section 5.2, analyze the effects of the shift in the tax on two main risk-taking decisions of banks associated to loans to households: the mortgage liability ratio (costly insurance) and the probability of granting (much riskier) consumer credit (which was not affected by the law), suggesting that the policy change increases risk-taking by banks by more affected banks (consistent with the policy reducing bank profits given that banks do not fully pass through the tax).

5.1 Impact on mortgage interest rates

Table 2 reports the results of Equation (1), the difference-in-differences specification to capture the casual impact of the change in the tax law on the interest rate of mortgages. We show a step-by-step analysis where each new specification adds more controls to the previous ones (starting with no controls whatsoever in column (1) to fully saturating the regression in column (8)). To avoid different estimated coefficients due to changes in the sample, we use an identical sample of 168,250 mortgages, the one associated with the most saturated specification.

In column (1) of Table 2 there are no controls. The estimated coefficient (β) is 0.153**.[26] Column (2) includes bank, time (year:month) and type of the mortgage (fixed or variable rates) fixed effects.[27] The estimated coefficient decreases to 0.095** (though the two coefficients are not statistically different, as one standard deviation is around 0.5). Column (3) saturates model (2) with the triple interaction *bank*time*fixed/variable interest rate* fixed effects (the estimated coefficient equals 0.099**) and column (4) changes the time to control for *year:month:day* fixed effects. The estimated coefficient does not change significantly from the previous specification, 0.106**.

In the next estimations (columns) we control for potential unobservable confounding factors by proxying the income and wealth of households to reduce the differences between the mortgages in the control and in the treatment group (see also Table A1 of the Appendix and previous section). Column (5) adds the *zip code*employment status* fixed effects and column (6) splits these fixed effects into foreign and national households. Column (7) adds loan characteristics (size and maturity) and column (8) the rest of household controls explained in Table 1, in which the estimated coefficient on the treatment variable is 0.110***.

Thus, results suggest that, after the introduction of the Royal Decree, banks increase mortgage interest rates by around 10 basis points on average, which accounts for a 5% increase on mortgage rates (see Table 1). In terms of the quantification of the results with respect the potential interest rate that banks should have charged to fully compensate the cost of the new tax, we show, through simulations (see Section 5.1.4), that, on average, the 10 estimated basis points represents around 80% of the increase in cost due to the tax change. It is important to note that in this estimation we are assuming that banks react only through changes in the mortgage rates and not through changes in other fees as we do not have loan-level information about mortgage related fees. However, as we show in section 5.2, we do not find that more affected banks differentially change loan related fees;[28] moreover, total bank profits for more affected banks (the ones with higher volume of mortgages in treated areas) are reduced after the law as compared to less affected banks, again suggesting that banks do not fully pass-through the tax.

Finally, given that the R-squared increases from 34.1% in column (2) to 70.1% in column (8), doubling the R-squared and with an absolute increase of more than 35 percentage points, while the estimated value of the coefficient of interest does not decrease and it is very similar (0.095 versus

[26] *** implies statistically significant at 1%, ** significant at 5%, and * significant at 10%, in which the standard errors are corrected for multi-clustering at the bank, time and zip code level.
[27] The key control that changes the estimated coefficient is type of the mortgage (fixed or variable rates), which is necessary to compare mortgages within either fixed or variable rate.
[28] Given the assumptions we make in our simulation, which are explained in more detail in Section 5.1.4, we see this 80% figure as representing a conservative upper bound of the pass through.

0.106), results suggest that, following Oster (2019) and Altonji et al. (2005), the estimated effects do not suffer from biases due to (further unobservable) omitted variables.[29]

5.1.1 Adjoining zip codes to the border of the Basque Country

Despite the stability of the estimated coefficient across very different set of controls, to further increase the similarities between the treated and control groups, in this subsection we consider only those mortgage loans granted in the municipalities around the border of the Basque Country. In Table A1 of the Appendix we show that this strategy is useful to reduce differences in observable characteristics of households, but has the disadvantage of greatly reducing the number of observations: from 168,250 to 1,121, a 99% reduction in the sample. Nevertheless, Table 3 shows identical results.

Table 3 follows the same structure as Table 2 where we progressively saturate the specifications. Analogously, we find very similar results along all the columns within Table 3, and also between Table 3 and Table 2. In column (7), the most saturated regression, the estimated impact of the tax reform on mortgage loans is around 10 basis points, which is identical to Table 2 (although only statistically significant at 10% due to higher standard errors, as the number of observations is much lower and we still triple cluster standard errors). As in Table 2, column 2 and the last column of Table 3 have *identical* estimated coefficients despite the substantial change in controls (0.108 versus 0.100).

5.1.2 Further robustness tests

In Table 4 we show further robustness tests. First, we analyze whether there are regional differences in the impact of the tax reform depending on the tax rate charged by the regions before the introduction of the Royal Decree. As previously discussed, outside the Basque Country, tax rates were between 0.5% and 1.5%, with the more common base tax rate being 1.5% (see Table A4 in the Appendix). The first four columns of Table 4 exploit these regional differences. Column (1) replicates the same model that the one showed in the last column of Table 2 but for the mortgages charged with a tax lower than 1% (where the control group is still the Basque Country). The number of observations drops to 43,981 and the estimated coefficient is 0.073**. Column (2) considers only mortgages with a tax rate of at least 1%, plus all those granted in the Basque Country (the control group). The estimate is now higher, 0.118***, as expected. Column (3) follows a different approach in the treatment and uses as the control group all mortgages with a tax rate lower than 1%

[29] Altonji et al. (2005) and Oster (2019) analyse the sensitivity of the estimation results to the inclusion of observable and unobservable controls checking the stability of the explanatory variable of interest to significant increases in the R-squared.

(excluding those from the Basque Country) and as a treatment group all mortgages with a tax rate higher than 1%. In such case the estimated coefficient is 0.069**.

Column (4) presents the results when we include the level of the tax rate instead of the treated dummy. In such case, we obtain that the coefficient is 0.078**. Therefore, results suggest that, for banks, the intensity of the pass-through is proportional to the magnitude of the impact of the measure, captured by the ex-ante level of the tax rate. Moreover, the result derived from the intensity treatment is in line with the 10 basis points estimated for the baseline model given that if the tax rate increases (from 0%) to 1.28% (the average tax rate for treated regions), then the interest rate would increase 10.0 (=0.078*1.28*100) basis points.

With the aim of mitigating the effect of other possible contemporaneous shocks, or the impact of other subsequent spillover effects, column (5) only considers the mortgages granted in a window of 2 weeks before and after the Royal Decree. Again, there is an important reduction in the number of mortgages analyzed, of around 97%, but nevertheless the estimated coefficient on the treatment is 0.088**, which is very similar to our baseline estimation. Figure 2 (see below also) shows time-varying estimated coefficients for each month: there is a fast adjustment and then the coefficients keep constant over the following months. Moreover, while we do not observe any aggregate effects in the total volume of mortgages granted around the announcement (not reported), in column (6) we estimate our coefficients excluding October and November 2018 in order to exclude any strategic behaviors around the announcement date. The estimated coefficient is now 0.117***, again very similar to the main one.

Column (7) analyzes a much smaller set of mortgages related to secondary residence, which were subject in the Basque Country to a 0.5% tax rate, and, given the special status of the Basque Country, were not subject to a change in statutory incidence. We show how in such case the pass-through is still positive but lower, with an estimated coefficient of 0.054*. Column (8) of Table 4 substitute the interest rate at origination with the interest rate at July 2020 (the latest available date) to take into account whether there are some adjustments over the life of the loan for all mortgages or from some mortgages that have a mixed interest rate (during the first few months a fixed interest rate is paid and then it changes to a variable one). Results do not change (0.112***).

Finally, the last two columns of Table 4 restrict the treated group to two alternative groups that are more similar to the control group. In column (9) we consider all new mortgages granted in Madrid and Catalonia (which are the other richest regions in Spain along with the Basque Country), while in column (10) we use a synthetic Basque Country control group which we construct using

propensity score matching techniques (where we take into account all observed variables). Table A3 of the Appendix shows that this approach ensures a better fit of both groups (treated and control) and the estimated coefficients from columns (9) and (10) do not differ from our benchmark result.

Lastly, we analyze the key diff-in-diff assumption in which the validity of our results relies in the absence of pre-trends in the treatment versus control groups. Moreover, all of our results are based on the assumption that the banks reacted after the date the Royal Decree went into effect, not earlier, and for later dates an average effect is computed. All these assumptions can be checked allowing the coefficient on the *Treated* variable to vary over time. This is what we do in Figure 2, which can also be seen as a placebo test for the dates before the measure was taken (allowing us to further exclude possible anticipation effects). Figure 2 shows the year:month estimated coefficients for our baseline specification. The estimated coefficient is insignificant before November 2018 (2018M11) and then becomes statistically significant.[30] Note that effects are not increasing over time (e.g. as menu costs/sticky prices type of argument would imply), not even when we use the last available mortgage loan rate in July 2020.

5.1.3 Heterogeneity

We explore the existence of heterogeneous effects on the impact of the policy shift on mortgage rates at the household level (as well as at the bank and loan level) in Table 5, where we show the results of estimating Equation (2). We start with the household dimension by first introducing the interaction of the treatment variable with the log of gross income and then later adding, progressively, the rest of household variables (columns (1) to (3)). We then introduce bank characteristics, including bank-borrower variables, in particular the number of banks in the zip code of the household and the number of previous banking relationships (columns (4)). In column (5) we tests the robustness of the estimation controlling for (endogenous) loan characteristics (size and maturity), and column (6) also controls for loan amount/mortgage liability (the insurance for the bank). It is important to note that when we introduce interactions into the estimation, we demean all variables, so that the variables in levels reflect the average impact.

Our results suggest that borrower income as well as borrower variables further proxying for borrower bargaining power play a prominent role in the heterogeneous transmission of the tax to mortgage rates.

[30] The reason why the effect somewhat fades in May 2019 may be due to the entry of the new mortgage law (Ley 5/2019) in June 2019, which was approved in March 2019, as discussed in Section 2.

Looking at column (6), the negative and statistically significant coefficient of the interaction of the treatment variable with the log of the gross income of the household, -0.274**, indicates that the richer households are less affected by the pass-through (less increase in loan rates due to the policy change). A 30% increase in borrower's income, that corresponds to the difference between households in the 25% and 75% of the income distribution (see Table 1), decreases loan interest rates in 8 basis points after the tax change, similar to the average level effect that we find in Table 2.

Moreover, when the number of banks in the zip code of the mortgage (a proxy of bank competition) increases, the pass-through of the tax law is (relatively) lower (-0.034*, column (6)). This result is consistent with borrower's bargaining power, as with a higher number of banks, the borrower has more opportunities to switch to a lender that offers a lower loan interest rate. Further, the pass-through is lower for households with more bank relationships ex-ante (-0.102*, column (6)), which again is consistent with such households having a higher bargaining power as it is easier for them to find a cheaper mortgage offer. Effects are also quantitatively strong: an increase of the number of bank relations distribution, that corresponds to the difference between households in the 25% and 75%, (relatively) decreases loan interest rates after the tax change in 8 basis points.

Our results also show that there is a group of borrowers that is not affected by the policy change: borrowers who work in the lender's banking group (-0.170**, column (6)).[31] Mortgages to employees of banking groups are special given the existence of collective agreements between the bank and its workers which (generally) involve mortgage loans with an advantageous pre-established interest rate and they are negotiated every year.

The heterogeneity in the pass-through of the tax reflects that statutory incidence has highly asymmetric effects on borrowers. This highlights the relevance of borrower characteristics (most of them related to borrowers' bargaining power) for the overall effects of changing the statutory incidence of the tax. One relevant issue regarding our heterogeneity in pass-through results is that, even though we control for a variety of household characteristics, some of these variables could be proxying for different risk profiles of the borrower, which would also affect the pass-through. We analyze in more detail this issue in section 5.1.4 and our results suggest that observed differences in the pass-through cannot be explained by differences in unobservable borrower's risk.

[31] It is worth noting that the value of the coefficient depends on the omitted (reference) group and also that this variable is correlated with other characteristics of the borrower or of the loan. When it is included alone in the regression, the estimated coefficient is -0.115**, which is similar to the average effect of the pass-through but with the opposite sign, and hence there is no significant pass-through for these borrowers.

Finally, we also find heterogeneous results in the pass-through regarding bank variables. More capitalized banks and those more specialized on households are those for which the transmission of the policy change to loan interest rates is higher. For instance, an additional percentage point of the leverage ratio (or of the ratio between loans to households over total assets) increases the interest rate by 4.5% (0.5%). There is also some weaker evidence, column (6), that banks with riskier assets and hence weaker balance sheets (proxied by higher NPLs) increase the pass-through to loan rates after the tax reform.[32]

5.1.4 Pass-through: further quantitative analyses

In this section we analyze whether the observed pass-through is enough (a full pass-through) to compensate banks for the increase in their lending costs stemming from the statutory shift as well as whether the large borrower heterogeneity in the pass-through is explained by a change in borrower risk. The principle of statutory irrelevance predicts that the observed pass-through in mortgage rates should compensate banks for their increase in lending costs and, therefore, banks' profits should not be affected by the shift, i.e. we should observe a full pass-through (see also next subsection, for the analysis of bank-level profits, loan fees and loan interest income).

We first compute the present value of our estimated 10 basis points for the average mortgage and compare it to the observed tax, concluding that our analysis suggests that banks did not apply a full pass-through of the tax. Second, we perform a quantitative analysis, including a loan by loan analysis, to study the relevance of (unobserved) borrower risk factors as a main driver of the large documented heterogeneous pass-through, and conclude that observed heterogeneity cannot plausibly be explained by unobserved differences in risk. Third, we perform another loan by loan analysis but in this case we simulate the interest rates that would fully compensate the cost of the tax for banks for each mortgage granted in treated regions. Our analysis suggests, in line with the previous exercises, that there was not a full pass-through of the tax on average nor on the heterogeneous borrower effects (that we find significant in Table 5). Interestingly, by using loan specific discount rates, our loan by loan results further suggest that the observed heterogeneity in pass-through is not explained by unobserved risk factors.

[32] The increase is higher the higher the capital to asset ratio of the bank, which we argue is consistent with equity being the more expensive source of financing for the banks (Freixas and Rochet, 2008) and hence the shift being more costly for those banks. The increase is also higher for banks with higher ratio of household to total assets (consistent with these banks being more specialized in loans mortgages). Finally, the increase is higher both for smaller banks (as these banks tend to be more financially constrained) and for banks with higher NPLs (consistent with higher costs of funding due to higher provisioning).

As a first exercise, we compute the present value of our estimated 10 (yearly) basis points pass-through for the average mortgage. In doing so, given that we observe loan amount, maturity and interest rate of each mortgage, the main challenge is the choice of the appropriate discount rate, which is not observed. We proceed by assuming two discount rates: (i) a conservative discount rate that equals the average yield of the (safer) 10-year Spanish government bond in our sample period, which is 1.33% and (ii) a more realistic discount rate that equals the observed rate of the average mortgage, which is 2.1%, in line with a yield to maturity argument. Applying standard actuarial formulas and the characteristics of the average mortgage, we obtain that the estimated 10 basis points account for a present value of 1,472 or 1,332 Euros, respectively. Comparing these values with the average tax for treated mortgages after the shift, which accounts for 1,774 Euros, suggests that banks did not fully pass-through the cost of the tax when the statutory incidence shifted. Our results suggest an average pass-through ranging from 75% (with the borrower yield to maturity) to 83% (with the safer government bond), which suggests that the average pass-through was around 80%.[33] For example, considering the more realistic yield to maturity, this result suggests that the observed pass-through for the average mortgage is 3 basis points lower than the one that would result from a full pass-through; as a further check, we perform a loan by loan analysis using each loans' characteristics (maturity, loan amount, tax) and obtain that the observed pass-through for the average mortgage is 5 basis points lower than the full pass through.[34]

As a second exercise, we turn to analyze the relevance of mortgage defaults for our estimations. We do so by considering the possibility of mortgages defaulting, and as a result banks not obtaining all the promised payments in case of default. We assume that in case of mortgage default the loss given default is equal to 15%.[35] For the average mortgage, we obtain the increase in probability of default that would be consistent with a given (observed) increase in mortgage rates, assuming that bank profits remain equal. This analysis allows us to understand whether the large documented *heterogeneous* pass-through effects from Table 5 could be driven by unobserved risk characteristics of the borrowers

Using the characteristics of the average mortgage in our sample, we simulate the increase in mortgage rates that would be consistent with a 1% increase in the probability of default of the

[33] Using an extremely conservative rate equal to the minimum of the government bond yield in our sample equal to 0.75% (May 2019), the net present value accounts for 1556 Euros (an 87% pass-through).

[34] The difference between 3 and 5 basis points suggest possible relevant heterogeneous effects in the pass-through.

[35] This LGD is larger than any of the historical estimations (even in the worst months of the financial crisis) of banks' advance internal rate based (IRB) model parameters in Spain (recall that mortgages in Spain are full recourse). This choice results in a conservative estimation of the effects of default (in the sense of allowing default to have a larger explanatory power).

mortgage.[36] We obtain that the simulated difference in the pass-through between a risk-free mortgage and a mortgage that after the policy shift increases in 1% its probability of default is 2.1 (1.5) basis points, where we are assuming a conservative discount rate equal to the average of the Spanish Government bond yield (a discount rate equal to the average mortgage rate). This simulation highlights that our observed differences in heterogeneous pass-through for certain borrower characteristics such as income or number of bank relationships, which are 8 and 7 basis points (see Table 5) for individuals in the 75% vs. the 25% of the distribution, are not (plausibly) driven by differences in underlying risk profiles of those individuals. Note that for the period pre-policy shift, see Table 6 Panel A, we find very small differences in pricing and loan defaults based on differences income and number of relationships; moreover, the ex-ante number of bank relationships that a borrower has would even have the opposite sign to explain the change in pass-through after the policy shift.

As our two main (economically significant) measures of borrower heterogeneity are income and number of bank relationships, in Table 6 Panel A we perform a regression (exploiting all loan by loan heterogeneity) in order to obtain, before and after the policy shift, the difference in loan rates (column (1)), as well as the difference of the implied probabilities of default that are consistent with the observed differences in rates both ex ante and ex post.[37] Before the policy shift, for individuals in the 25% versus those in the 75% percentile of the distribution for income (number of bank relationships), the observed difference in mortgage rates is 7 (-7) basis points. This difference of mortgage rates before the policy shift would be consistent with a 3.6% (-3.6%) difference in the probability of default for those individuals with difference income (number of bank relationships).[38]

Moreover, after the policy shift, the observed difference in mortgage rates is 15 (0) basis points, consistent with the pass-through due to the shift in statutory incidence of 8 and 7 basis points shown in Table 5. This difference of mortgage rates during the period after the policy shift would be consistent with a probability of default of 6.8% (0%) for those individuals on the 75% vs. 25% of the distribution for income (number of bank relationships). Therefore, the difference in the probabilities of default in the pre- versus post-policy shift consistent with the observed differences in mortgage rates are 3.2% (3.6% versus 6.8%) for income and 3.6% (-3.6% versus 0%) for number of bank relationships. The result suggest that borrower risk profiles cannot explain the observed

[36] Our simulation is based on obtaining the probabilities of default which make the net present value for the bank of two mortgages with different observed mortgages rates equal.

[37] The regression is the same as the one in Table 5 but using province fixed effects instead of zip code level fixed effects to be able to estimate level effects.

[38] We find that, in line with these numbers, income (number of relationships) is negatively (positively) and statistically significant related with ex-post delinquencies. See column 2 of Table 6 Panel A.

heterogeneity in the pass-through, as the risk profiles of individuals would have had to change dramatically after the shift, both in value and direction (see also next paragraph). Regarding value, recall from previous sections that the tax on average is very small (around 1,800 Euros) compared to mortgage volume (around 116,000 Euros) and that e.g. the difference in income from the 75% (around 34,000 euros) to the 25% (25,000 euros) are 9,000 euros.

Furthermore, to show that borrower risk is not driving the results, in column (2) of Table 6 Panel A, we show that the defaults associated to the number of bank relationships after the policy shift in the treated versus control group increases, while the increase in mortgage rates (pass-through) is lower (column (1)). Hence, lower pass-through on mortgage rates cannot be due to lower borrower risk. Moreover, as we can also see in column (2), borrowers with more ex-ante number of bank relationships tend to have on average higher defaults (i.e., the result on defaults associated to households with higher number of bank relationships is a general result, not just after the policy shift).[39] Differently, for income, households with higher income tend to default less on average. However, notice that there is no further differential default effects after the policy shift for treated versus control areas depending on borrower income. All in all, results suggest that the heterogeneity we find in the pass-through is not driven by borrower risk.

Finally, to further analyze the observed pass-through we perform a loan by loan simulation analysis. Given that for each loan we observe the maturity, the loan amount, the interest rate charged and the cost of the tax (which, as previously explained, is based on the mortgage liability and the prevailing tax rate in the region), it is possible to simulate for each mortgage loan the interest rate that would compensate the cost of the tax for banks (allowing banks to obtain the same profits as when the tax was paid by the borrowers). This rate is an estimate of what the full pass-through interest rate would have been. We proceed by first computing this simulated interest rate for each treated mortgage before the tax shift. We then replace, for treated mortgages before the tax shift, the observed interest rate with the simulated interest rates, which give us a benchmark for the rates we should have observed before the tax shift if there had been a full pass-through in those mortgages. Finally, we compare our simulated rates (pre) with the observed rates (post) by estimating Eq. (2). If after the shift, there would have been a full pass through, we would expect a coefficient of 0 in the treated*post interaction (as well as in the treated*post*borrower key variables), as our simulated pre-rates would be equal to the observed post rates. If the observed pass-through is below (above) 100% we would expect a negative (positive) coefficient.

[39] For previous research analysing how the number of bank relationships is positively related to borrower risk, see Detragiache, Garella and Guiso (2000) based on Italian data.

Interestingly, by comparing the value of the coefficients for the interaction terms with the average pass-through observed we are able to obtain an estimation the magnitude of the observed pass-through.

In Table 6 Panel B we report the results of estimating Eq. (2) with our simulated mortgage rates for treated mortgages before the tax shift, following the analysis in Table 5. As before we use two different discount factors, in columns (1) to (4) we use the average of the Spanish Government bond yield, and in column (5) we use the observed interest rate as a discount rate (following a yield to maturity argument). We find that there is not a full pass through on average (*treated*post* interaction is negative in all columns but column (2)).[40] Columns (2) and (3) show that when we split the sample into two groups based on gross income, there is a group of borrowers for whom the pass through is complete and another one for whom there is not full pass through (below and above the third quartile of the gross income distribution, respectively). Moreover, there is also a not full pass-through for the key borrower variables driving the borrower heterogeneity in Table 5, i.e., negative coefficients for *treated*post*borrower* for income, number of bank relations, number of banks in the zip code, and whether the borrower works for the bank. Interestingly, we find that when we use a discount rate that incorporates unobservable loan risk characteristics, our coefficients of interest on heterogeneous borrower' characteristics maintain statistical and economic significance (column (5) of Table 6 Panel B). This suggests, in line with our previous exercises, that the difference in pass-through cannot be (plausibly) explained by risk factors which would already be incorporated in the mortgage rates.

5.1.5 The effects on other mortgage terms

Table 7 presents the estimation results of our baseline model for different dependent mortgage variables: the loan amount, the maturity, loan-to-value ratio and the mortgage liability ratio, defaults and additional (consumer) credit. For the first three regressions we resort to estimate a Poisson model in order to reduce possible biases arising from a classical log linear estimation (see Santos Silva and Tenreyro, 2006),[41] while we use an OLS estimation for the other variables. Robust standard errors are again corrected for clustering at the bank, year:month:day and zip code level. The mortgage liability ratio is the logit transformation of the ratio of the loan amount over the mortgage liability (given that in our data it is bounded by 0 and 1), thus higher values of this ratio,

[40] Interestingly we observe that the average pass-through is 7 basis points lower than the one that would be needed to obtain a full pass-through (based on column 2). Moreover, the difference in intensity of the pass-through we obtain when we analyze the average mortgage (3 basis points lower pass-through) and the one we obtain in the loan by loan analysis (7 basis points lower pass-through) suggests the existence of possible relevant non linearities in the intensity of the pass-through.

[41] Results are the same if we use an OLS estimation instead.

keeping the numerator fixed, are due to reductions in the mortgage liability. With additional (consumer) credit we test whether the policy change affected the other key household credit decision (consumer credit) when asking for a mortgage. This variable has an average value of 3.8% and since after the policy change households stopped being levied the tax, it could happen that they ask for different (perhaps fewer) consumer loans.

Columns (1) to (4) of Table 7 show that the tax reform has no effect on the loan amount (neither at loan level or intensive margin, column (1), nor at zip-code level or extensive margin, column (2)), on the maturity (column (3)), on the loan to value ratio (column (4)), on future defaults (column (6)), which include loan delinquencies as well as mortgages under moratorium driven by borrowers during the COVID-19 crisis, or on consumer loans by households that ask for a mortgage (column (7)).[42] However, it affects the mortgage liability ratio (column (5)). The coefficient on the mortgage liability ratio is positive and statistically significant (0.092*), which means that, given that the loan amount is unaffected (column (1)), banks on average decrease this (costly) insurance, increasing their risk (as they would hold lower collateral in case of loan default).

To be confident that this observed effect in the mortgage liability ratio is due to the introduction of the tax reform, Figure 3 shows the time-varying estimated coefficients for every month. The estimated coefficients are close to zero and insignificant until October 2018, and then they jump. In November it increases but it is not significant at conventional levels. In December 2018 the estimated coefficient becomes statistically significant, and stable after that month, which suggests that on average banks react by lowering the mortgage liability from the beginning, but more strongly after one month. Importantly, this risk-taking result is consistent with the reduction in bank profits (see next subsection) and the incomplete pass-through, as shocks to bank net worth (profits) imply more risk-taking in many banking theories (see Freixas and Rochet, 2008).

5.2 Impact on bank's risk-taking decisions

In this section we proceed to further investigate the possible risk-taking effects of the policy change. We proceed by first, using the identification strategy explained in Eq. (3) and Eq. (4), analyzing whether banks more affected by the policy—in terms of having a high proportion of their assets as mortgages outside of the Basque Country—react differently in the (loan-level) mortgage

[42] Until now we have seen that, after the policy change, banks react by modifying the interest rates of their mortgages. Table A2 in the Appendix shows evidence that the type of loan, borrower and bank characteristics are similar before and after the tax change (which also happens for the zip-codes around the border of the Basque Country). Based on the results of columns (1) to (4) in Table 7, we argue that the underlying reason for household demand not reacting to changes in the tax is given the small amount of the tax for borrowers with respect to the price of the house (the average mortgage tax accounts for 1,774 Euros on an average 116,731 Euros mortgage) and that there was an 80% pass-through on average (see previous subsection).

rates, mortgage liability, consumer loans, and if this is especially so for banks with higher moral hazard problems, proxied by ex-ante NPLs. We also analyze bank-level loan rates, fees related to rates and profits, to test whether more affected banks reduce their profits due to the tax reform (confirming our previous results on not a full pass-through) and to test if fee related income (and not only loan rate related income) is differentially affected. Once we analyze these results, we end in subsection 5.2.2 by analyzing in more detail the risk-taking in mortgage liability and consumer loans using the same treatment variable as in section 5.1, i.e. comparing loans in regions that were affected versus those in the control region.

5.2.1 Bank exposure results

In this subsection we first show the results of the estimation of Eq. (3), where banks are classified as more affected by the Royal Decree based on the ex-ante weight of their mortgage portfolio outside the Basque Country before the tax reform (more affected by the tax reform). As columns (1) and (3) in Panel A of Table 8 show, we find that, in line with the results in section 5.1, more affected banks increase more loan rates and reduce more mortgage liabilities of their loans. Interestingly, our results suggest that more affected banks risk related decisions are more distorted, as not only do they reduce more their mortgage liability, but also increase more the probability of granting consumer loans (which are substantially riskier than mortgages), as column (5) of Panel A in Table 8 shows. Moreover, results also show that the increase in loan rates, reduction in mortgage liability and increase in the probability of granting consumer loans is higher for those more affected banks by the policy change with ex-ante weaker balance sheets (proxied by higher ex-ante NPLs), see columns (2), (4) and (6).[43]

We next use bank-level information, Panel B of Table 8, where we find that banks more affected by the policy change increase the loan interest income (consistent with the loan-level results that show that they increase more the loan rates), but there is no differential change in fees related to loans and, given these results and the previous ones from Table 2 and 6, bank profitability consistently decreases more for more affected banks. That is, results suggest that more affected banks lose more with the reform.

These estimates show how, after versus before the policy change, banks that were more affected by the tax reform (banks more exposed to treated regions) as compared to banks less affected: (i) increase the interest rate of the mortgages by 11 basis points more; (ii) have a larger

[43] Note that the positive estimated coefficient for the probability of granting consumer loan applications for more affected banks that have higher NPLs is not significant at standard significance levels.

decrease in their ROA of 18% (close to 10 basis points) ; (iii) decrease more the mortgage liability (higher 3.4% ratio of loan amount over mortgage liability); (iv) and increase the probability of granting applications for consumer loans by around 3.7%. Note that all these results are consistent with previous results and also with standard moral hazard theories in banking, where the tax reform by reducing bank profits (due to the incomplete pass-through) implies higher risk-taking in the mortgage market (via reducing costly mortgage insurance) and spillovers in riskier markets (via higher granting of loan applications in consumer lending). Moreover, these differential effects are even stronger for banks with higher ex-ante NPLs, again consistent with bank moral hazard theories (Freixas and Rochet, 2008).

5.2.2 Further distortionary effects

We now proceed to, following the identification strategy of section 5.1 in which loans are classified as treated if they are located in the region on which the Royal Decree applies, exploit loan level information to further analyze the determinants of the aforementioned risk-taking effects: mortgage liability and probability of granting consumer loans.

Table 9 investigates the heterogeneity of the results for the mortgage liability ratio, similarly to Table 5. The most saturated model, presented in column (5), shows that the only borrower characteristic that affects the reduction in mortgage liability is whether the borrowers are employees of their banking group. It is important to note that we do not find that the mortgage liability is lowered for those individuals with differential bargaining power, e.g. higher income, more bank relationships or more banks competing in their neighborhood, which we find is the case for mortgage rates. Hence, our results suggest that the lower mortgage rates were not due to lower mortgage liabilities (and therefore lower tax related costs for the banks) for those individuals. Moreover, there are relevant bank characteristics that further increase the reduction in mortgage liability after the tax reform, as especially riskier banks (higher ex-ante NPL and ROA, which also proxies for riskier portfolio with higher profits and higher risk) further reduce the subsequent lower mortgage liability for treated loans. After the tax reform and for loans in treated areas, the impact on mortgage liability doubles for banks with more NPL ratio and increases by 50% for more profitable banks (where “more” in both cases implies comparing 75% versus 25% of the distribution).

Finally, in Table 10 we analyze the consequences that the shift on the tax has on consumer loans. Our objective is to further analyze whether banks modify their credit standards for consumer loans comparing treatment and control regions as in Equation (1). Importantly these loans were not affected by tax shift (i.e. they represent spillovers from the tax reform), and they are substantially

riskier than mortgages (defaults around 12% and with very high LGD, consistently with loan rates for consumer loans of 9.5% as compared to only 2% for mortgages).

We show the results on the probability of granting of loan applications in the first two columns of Table 10. In column (2) the sample is restricted to consumer loan applications made in the zip codes adjacent to the border of the Basque Country. In columns (3) to (5) we analyze the terms of granted consumer loans: interest rate, loan amount and maturity. Last column of Table 10 investigates the future performance of the loan to test whether the mortgage tax change increase banks' appetite for risk in consumer loans as a response of the policy change (i.e. they choose the riskier consumer loans within the set of the already risky consumer loans). The structure of the estimation is identical to the one of Table 7: a Poisson estimation for amount and maturity and an OLS for the rest. Standard errors are triple-clustered at the bank, year:month:day and zip code level. The time period analyzed is again 20018M1 to 2019M5. We have more than 1.7 million of consumer loans.

The estimated coefficient on the treatment variable is positive and significant in the analysis of loan applications, both for the whole sample and for the border areas surrounding the control group. For instance, the 0.023** coefficient implies that, in the treated areas after the tax reform, the granting application rate (of the riskiest segment of household loans) increases by 4.5%. Differently, there are no statistical effects neither on the loan interest rate nor in the amount or in the maturity of granted consumer loans. However, for future default the coefficient is positive and statistically significant (0.007**), which implies that after the tax shift in treated regions the probability of default of the new consumer loans granted after increases by 5.7% with respect to control regions, implying riskier strategies in non-affected loans to households (consumer lending), which are mostly given by banks more affected by the mortgage tax policy change. Note moreover that the increase in softer lending standards associated to higher ex-post defaults is not compensated with higher ex-ante loan interest rates.

All in all, the results suggest that banks pursue a higher risk-taking strategy both in consumer loans and in mortgages due to the mortgage tax shift on statutory incidence. Such evidence as well as results in Table 8 and Table 6 are consistent with banks not passing through all the cost imposed by the tax change and reacting to the lower bank profits by increasing their risk, especially by the banks more subject to moral hazard issues.

6. Conclusions

This paper analyzes the overall and heterogeneous effects of only shifting the agent on which taxes are levied (i.e., shifting statutory incidence), without any change in tax rates or tax evasion, which improves the identification of (i.e. isolates) statutory tax incidence. We revisit this key classical question by exploiting a tax shift in the banking industry (the credit market) in conjunction with supervisory mortgage data. In particular, to study (economic) incidence and potential distortionary effects of only shifting statutory incidence, we exploit: (i) a policy change in Spain in November 2018 that shifts a mortgage tax from being levied on borrowers to being levied on lenders, and crucially without any change on the tax rates; (ii) the fact that some areas, for historical reasons, are exempt from paying this tax (or have different tax rates); and (iii) matched administrative datasets (an exhaustive credit register with borrower and lender information).

We find that, after the policy change, the average mortgage rate increases (by 10 basis points), consistently with a strong (but not complete) tax pass-through, of approximately 80% of the tax. Importantly, we show a large heterogeneity in the pass-through, which is larger for borrowers with lower income, less number of lending relationships, not working for the lender, or facing a smaller number of banks in their zip-code. All these margins are consistent with heterogeneity in borrowers' bargaining power, and we provide a stylized model consistent with this differential bargaining power mechanism under the assumption that for households, as compared to banks, tax saliency is smaller. The heterogeneous estimates are quantitatively large and our analysis suggests that they cannot be explained by differences in borrower risk. In addition to the non-full pass-through and evidence not consistent with statutory incidence being irrelevant for tax incidence, the results further suggest that borrowers with weaker (compared to stronger) pass-through increase their relative welfare as they obtain the same mortgage at a lower total cost relative to the borrowers with higher pass-through.

Moreover, despite that there is no change in the tax rate (which could have led to e.g. inefficiencies associated with tax increases), and consistent with a non-full pass-through, we find that the shift in the statutory incidence of the tax changes key banks' decisions, in particular those related to banks' risk-taking. We find that banks more affected by the tax shift (those banks with a larger share of their assets affected by the tax shift) exhibit a decrease in their profits, and increase their risk-taking by reducing costly mortgage insurance in case of loan default and by increasing the likelihood of granting applications of non-directly affected but much ex-ante riskier consumer lending, experiencing higher ex-post defaults within consumer loans (not compensated with differentially higher ex-ante loan rates). Some of these differential effects are stronger for more affected banks with characteristics that proxy for higher moral hazard problems (those with weaker

balance sheets, in terms of higher ex-ante NPLs) that have a higher likelihood of future help from taxpayers' (government) bailouts and/or central banks' liquidity injections.

All in all, results suggest strong overall and heterogeneous economic tax incidence effects, as well as distortionary effects, of only shifting the agent on which the tax is levied, without changing the tax rates. That is, all important economic effects of only shifting statutory (or physical) incidence.

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TABLE 1

SUMMARY STATISTICS

		Mean	S.D.	P25	Median	P75
LOAN LEVEL						
MORTGAGES						
Interest rate of the mortgage	%	2.068	0.987	1.510	2.118	2.569
Log(Size of the mortgage)	Log(Euros)	11.465	0.656	11.060	11.486	11.878
Log(Maturity of the mortgage)	Log(Months)	5.656	0.330	5.497	5.720	5.900
Loan Amount/Mortgage Liability	%	73.123	13.476	66.667	77.700	83.262
Log(Loan to value (LTV) of the mortgage)	Log(%)	4.120	0.440	4.006	4.257	4.377
Future default	0/1	0.058	0.234	0.000	0.000	0.000
Additional credit	0/1	0.038	0.192	0.000	0.000	0.000
Treated	0/1	0.942	0.233	1.000	1.000	1.000
Treated Border	0/1	0.574	0.495	0.000	1.000	1.000
Post	0/1	0.439	0.496	0.000	0.000	1.000
Household Characteristics						
Log(Gross income)	Log(Euros)	10.291	0.189	10.138	10.281	10.435
Public servant	0/1	0.081	0.273	0.000	0.000	0.000
Banking group employee	0/1	0.014	0.118	0.000	0.000	0.000
Student	0/1	0.028	0.165	0.000	0.000	0.000
Unemployed or homemaker	0/1	0.020	0.139	0.000	0.000	0.000
Log(Age)	Log(Months)	6.153	0.238	5.974	6.155	6.321
Log(1+No.of banking relationships)	Log	0.331	0.396	0.000	0.000	0.693
Log(1+No. of banks in the zip code)	Log	1.902	0.596	1.609	2.079	2.303
Bank Characteristics						
Log(Total assets)	Log(1000Euros	18.626	1.502	17.613	19.546	19.546
Own funds/Total assets	%	8.491	2.883	6.156	7.125	9.560
Liquidity ratio	%	15.190	11.277	11.415	11.415	17.310
ROA	%	0.382	0.432	0.371	0.508	0.587
Non-performing loan (NPL) ratio	%	6.592	1.740	5.988	6.150	7.528
Loans to households/Total assets	%	26.049	8.366	23.019	27.290	31.139
Main bank	0/1	0.159	0.365	0.000	0.000	0.000
Leader bank in the zip code	0/1	0.241	0.428	0.000	0.000	0.000
High Exposure to Mortgages outside Basque Country	0/1	0.535	0.499	0.000	1.000	1.000
CONSUMER LOANS						
Loan application	0/1	0.507	0.500	0.000	1.000	1.000
Interest rate of the loan	0/1	9.493	4.828	6.688	8.785	10.416
Log(Size of the loan)	Log(Euros)	8.748	0.935	8.112	8.765	9.393
Log(Maturity of the loan)	Log(Months)	3.936	0.531	3.611	3.912	4.290
Future default	0/1	0.122	0.328	0.000	0.000	0.000
BANK LEVEL						
Interest Income of Loans/Total Assets	%	0.938	0.367	0.729	0.942	1.172
Loan Fees/Total Assets	%	0.079	0.101	0.022	0.044	0.081
ROA	%	0.509	0.428	0.355	0.532	0.729

Notes: This table reports means, standard deviations and first/second/third quartiles of the main variables used in the paper. For a definition of the variables see the Appendix.

TABLE 2

EFFECT OF THE SHIFT OF STATUTORY INCIDENCE ON MORTGAGE INTEREST RATES

Dependent Variable: Mortgage interest rate								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated*Post	0.153**	0.095**	0.099**	0.106**	0.102***	0.110***	0.107***	0.110***
	(0.066)	(0.047)	(0.038)	(0.041)	(0.038)	(0.033)	(0.034)	(0.034)
Bank Fixed Effects	No	Yes	-	-	-	-	-	-
Year:month Fixed Effects	No	Yes	-	-	-	-	-	-
Fixed/Variable/Mixed Interest Rate Fixed Effects	No	Yes	-	-	-	-	-	-
Bank*Year:month*Fixed/Variable/Mixed Interest Rate Fixed Effe	No	No	Yes	-	-	-	-	-
Bank*Year:month:day*Fixed/Variable/Mixed Interest Rate Fixed	No	No	No	Yes	Yes	Yes	Yes	Yes
Zip Code*Employment Status Fixed Effects	No	No	No	No	Yes	-	-	-
Zip Code*Employment Status*Foreigner Fixed Effects	No	No	No	No	No	Yes	Yes	Yes
Loan Characteristics	No	No	No	No	No	No	Yes	Yes
Household Characteristics	No	No	No	No	No	No	No	Yes
Observations	168,250	168,250	168,250	168,250	168,250	168,250	168,250	168,250
R-squared	0.024	0.341	0.429	0.596	0.665	0.676	0.697	0.701

Notes: The table above reports OLS regression results of the effect of the shift of statutory incidence on interest rates of new granted mortgages between 2018M1 and 2019M5 on the treatment variable *Treated*Post*. *Treated* is a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those granted in Basque Country, and zero otherwise. *Post* is a dummy variable that takes one for all the periods after 8 November 2018, and zero otherwise. Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at the bank, time and zip code level, and the corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

TABLE 3

SHIFT OF STATUTORY INCIDENCE ON INTEREST RATES: MUNICIPALITIES AROUND THE BORDER OF THE NON-TREATED ZIP CODES

Dependent Variable: Mortgage interest rate							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated*Post	0.177*	0.108**	0.094*	0.107*	0.131**	0.116**	0.100*
	(0.090)	(0.040)	(0.053)	(0.054)	(0.053)	(0.055)	(0.057)
Bank Fixed Effects	No	Yes	-	-	-	-	-
Year:month Fixed Effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Fixed/Variable/mixed Interest Rate Fixed Effects	No	Yes	-	-	-	-	-
Bank*Year:quarter*Fixed/Variable/Mixed Interest Rate Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes
Zip Code*Employment Status Fixed Effects	No	No	No	Yes	-	-	-
Zip Code*Employment Status*Foreigner Fixed Effects	No	No	No	No	Yes	Yes	Yes
Loan Characteristics	No	No	No	No	No	Yes	Yes
Household Characteristics	No	No	No	No	No	No	Yes
Observations	1,121	1,121	1,121	1,121	1,121	1,121	1,121
R-squared	0.033	0.516	0.583	0.632	0.657	0.682	0.690

Notes: The table above reports OLS regression results of the effect of the shift of statutory incidence on interest rates of new granted mortgages between 2018M1 and 2019M5 on the treatment variable *Treated*Post* for the zip codes adjoining to the border of the non-treated provinces (which are the three provinces of the Basque Country). *Treated* is a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those granted in Basque Country, and zero otherwise. *Post* is a dummy variable that takes one for all the periods after 8 November 2018, and zero otherwise. Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at the time and zip code level, and the corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

TABLE 4

EFFECT OF THE SHIFT OF STATUTORY INCIDENCE ON MORTGAGE INTEREST RATES: FURTHER ROBUSTNESS

Dependent Variable: Mortgage interest rate										
	Intensity								Matching	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Tax rate<1% & Basque Country	Tax rate?1% & Basque Country	Treated= (Tax rate?1%) Without Basque Country	Continous Treatment	Within two weeks arond treatment date	Without 2018M10 & 2018M11	Secondary residence mortgages	Interest rate July 2020	Basque Country vs Madrid & Catalonia	Basque Country vs synthetic B. Country
Treated*Post	0.073**	0.118***	0.069**	0.078**	0.088**	0.117***	0.054*	0.112***	0.078***	0.127***
	(0.027)	(0.041)	(0.031)	(0.030)	(0.041)	(0.039)	(0.033)	(0.028)	(0.023)	(0.025)
Bank*Year:month:day*Fixed/Variable/Mixed Interest Rate Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip Code*Employment Status*Foreigner Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	43,981	131,677	158,352	168,250	6,773	147,637	33,029	157,604	36,304	12,916
R-squared	0.674	0.718	0.703	0.701	0.743	0.706	0.701	0.740	0.774	0.685

Notes: The table above reports OLS regression results of the effect of the shift of statutory incidence on interest rates of new granted mortgages between 2018M1 and 2019M5 on the treatment variable *Treated*Post*. *Treated* is (except for column (3), (4) and (7)) a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those granted in Basque Country, and zero otherwise. *Post* is a dummy variable that takes one for all the periods after 8 November 2018, and zero otherwise. Column (1) restricts the sample to loans with a tax rate lower than 1% plus Basque Country. Column (2) restrict the sample to loans with a tax rate higher than 1% plus Basque Country. Column (3) re-defines the treated group to those loans with a tax rate higher than 1 and the sample does not include the Basque Country. Column (4) uses the continuous treatment instead the dummy. Column (5) restricts the sample to two weeks before and after the entry of the law. Column (6) drops 2018M10 and 2018M11. Column (7) uses only secondary residence mortgages and *Treated* takes the value of one for the secondary residence mortgage loans granted in any Spanish provinces but those granted in Basque Country, and zero otherwise. Column (8) replace the interest rates at origination for those at July 2020. Column (9) uses as the treated group new mortgages granted in Madrid or Catalonia. Column (10) uses as the treated group the synthetic Basque Country group obtained from propensity score matching techniques. Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at the bank, time and zip code level (except for columns (9) and (10) where the low number of banks do not allow to cluster in that dimension), and the corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

TABLE 5

EFFECT OF THE SHIFT OF STATUTORY INCIDENCE ON MORTGAGE INTEREST RATES: HETEROGENEITY

Dependent Variable: Mortgage interest rate	(1)	(2)	(3)	(4)	(5)	(6)
Treated*Post	0.133***	0.134***	0.145***	0.116***	0.104***	0.071***
	(0.035)	(0.033)	(0.033)	(0.026)	(0.027)	(0.031)
Household Characteristics						
Treated*Post*Log(Gross income)	-0.313**	-0.313**	-0.300**	-0.314**	-0.264**	-0.274**
	(0.122)	(0.122)	(0.121)	(0.123)	(0.112)	(0.110)
Treated*Post*Public servant		0.011	0.002	0.022	0.021	0.021
		(0.063)	(0.061)	(0.064)	(0.065)	(0.069)
Treated*Post*Banking group employee		-0.151*	-0.170**	-0.201**	-0.178*	-0.170**
		(0.084)	(0.084)	(0.096)	(0.092)	(0.083)
Treated*Post*Student		0.036	0.023	0.006	0.006	0.012
		(0.079)	(0.073)	(0.082)	(0.075)	(0.074)
Treated*Post*Unemployed or homemaker		0.019	0.044	0.076	0.056	0.062
		(0.128)	(0.126)	(0.139)	(0.138)	(0.137)
Treated*Post*Log(Age)			0.044	0.053	0.094*	0.086
			(0.055)	(0.058)	(0.055)	(0.052)
Treated*Post*Log(LTV)			0.023	-0.022	-0.029	-0.043
			(0.062)	(0.060)	(0.050)	(0.049)
Treated*Post*Log(1+No. of banks in the zip code)				-0.038*	-0.033*	-0.034*
				(0.020)	(0.019)	(0.019)
Treated*Post*Log(1+No.of banking relationships)				-0.102**	-0.101**	-0.102**
				(0.049)	(0.043)	(0.042)
Bank Characteristics						
Treated*Post*Log(Total assets)				0.005	0.014	-0.087**
				(0.021)	(0.022)	(0.036)
Treated*Post*Own funds/Total assets				0.053***	0.053***	0.093***
				(0.012)	(0.011)	(0.011)
Treated*Post*Liquidity ratio				-0.002	-0.002	-0.000
				(0.005)	(0.005)	(0.005)
Treated*Post*ROA				0.042	0.015	0.188
				(0.134)	(0.129)	(0.135)
Treated*Post*NPL ratio				-0.002	-0.008	0.076**
				(0.021)	(0.021)	(0.037)
Treated*Post*Loans to households/Total assets				0.010***	0.011***	0.011***
				(0.003)	(0.003)	(0.003)
Treated*Post*Main bank				0.085	0.081	0.080
				(0.055)	(0.056)	(0.056)
Treated*Post*Leader bank in the zip code				-0.028	-0.036	-0.047
				(0.033)	(0.030)	(0.033)
Bank*Year:month:day*Fixed/Variable/Mixed Interest Rate Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Zip Code*Employment Status*Foreigner Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Treated*Post*Loan Characteristics	No	No	No	No	Yes	Yes
Treated*Post*Loan Amount/Mortgage Liabitity	No	No	No	No	No	Yes
Observations	168,250	168,250	168,250	168,250	168,250	168,250
R-squared	0.701	0.704	0.705	0.705	0.706	0.707

Notes: The table above reports OLS regression results of the effect of the shift of statutory incidence on interest rates of new granted mortgages between 2018M1 and 2019M5 on the treatment variable *Treated*Post* and its interactions. *Treated* is a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those granted in Basque Country, and zero otherwise. *Post* is a dummy variable that takes one for all the periods after 8 November 2018, and zero otherwise. Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at the bank, time and zip code level, and the corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

TABLE 6, PANEL A

STATUTORY INCIDENCE ON INTEREST RATES: HETEROGERNEITY OF MAIN BORROWER VARIABLES

Dependent Variable:	Mortgage interest rate (1)	Future Default (2)
Treated*Post	0.157***	0.004
	(0.032)	(0.007)
Household Characteristics		
Log(Gross income)	-0.255***	-0.050***
	(0.038)	(0.038)
Treated*Log(Gross income)	-0.093	-0.018
	(0.090)	(0.020)
Post*Log(Gross income)	-0.006	-0.012
	(0.035)	(0.011)
Treated*Post*Log(Gross income)	-0.263**	0.015
	(0.100)	(0.030)
Log(1+No.of banking relationships)	0.094***	0.036***
	(0.019)	(0.009)
Treated*Log(1+No.of banking relationships)	-0.010	0.007
	(0.029)	(0.009)
Post*Log(1+No.of banking relationships)	-0.047**	-0.002
	(0.018)	(0.003)
Treated*Post*Log(1+No.of banking relationships)	-0.089**	0.034*
	(0.043)	(0.018)
Bank*Year:month:day*Fixed/Variable/Mixed Interest Rate Fixed Effects	Yes	Yes
Province Fixed Effects*Employment Status*Foreigner Fixed Effects	Yes	Yes
Loan Characteristics	Yes	Yes
Household Characteristics	Yes	Yes
Treated*Post*Loan Characteristics	Yes	Yes
Treated*Post*Loan Amount/Mortgage Liabitity	Yes	Yes
Observations	168,250	168,250
R-squared	0.677	0.072

Notes: The table above reports OLS regression results of the effect of the shift of statutory incidence on interest rates of new granted mortgages between 2018M1 and 2019M5 (column (1)) and future defaults (column (2)), on the treatment variable *Treated*Post*. Mortgage defaults include loan delinquencies up to 2020Q2 but also borrowers that ask for a loan moratorium during the Covid-19 crisis. *Treated* is a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those granted in Basque Country, and zero otherwise. *Post* is a dummy variable that takes one for all the periods after 8 November 2018, and zero otherwise. Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at the bank, time and zip code level, and the corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

TABLE 6, PANEL B

EFFECT OF THE SHIFT OF STATUTORY INCIDENCE ON INTEREST RATES: SIMULATION

Dependent Variable: Mortgage interest rate	(1)	(2)	(3)	(4)	(5)
		Government Bond			
		Below P75 Gross Income	Above P75 Gross Income		Yield to Maturity
Treated*Post	-0.046***	-0.002	-0.132***	-0.064**	-0.073**
	(0.015)	(0.019)	(0.026)	(0.032)	(0.032)
Household Characteristics					
Treated*Post*Log(Gross income)				-0.222*	-0.216*
				(0.127)	(0.127)
Treated*Post*Public servant				0.015	0.017
				(0.059)	(0.059)
Treated*Post*Banking group employee				-0.177**	-0.164**
				(0.076)	(0.076)
Treated*Post*Student				0.013	0.012
				(0.077)	(0.077)
Treated*Post*Unemployed or homemaker				0.061	0.059
				(0.124)	(0.124)
Treated*Post*Log(Age)				0.004	-0.003
				(0.053)	(0.053)
Treated*Post*Log(LTV)				-0.009	-0.009
				(0.039)	(0.039)
Treated*Post*Log(1+No. of banks in the zip code)				-0.033	-0.033
				(0.025)	(0.025)
Treated*Post*Log(1+No.of banking relationships)				-0.105**	-0.104**
				(0.046)	(0.046)
Bank*Year:month:day*Fixed/Variable/Mixed Interest Rate Fixed Effec	Yes	Yes	Yes	Yes	Yes
Zip Code*Employment Status*Foreigner Fixed Effects	Yes	Yes	Yes	Yes	Yes
Loan Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Treated*Post*Loan Characteristics	No	No	No	Yes	Yes
Treated*Post*Loan Amount/Mortgage Liabitity	No	No	No	Yes	Yes
Treated*Post*Bank Characteristics	No	No	No	Yes	Yes
Observations	168,250	125,335	39,477	168,250	168,250
R-squared	0.697	0.672	0.743	0.705	0.704

Notes: The table above reports OLS regression results of the effect of the shift of statutory incidence on simulated and observed interest rates of new granted mortgages between 2018M1 and 2019M5 on the treatment variable *Treated*Post* and its interactions. Simulated interest rates are computed for all mortgages before 8 November 2018. Simulated rates are obtained by assuming a discount rate equal to the average yield of the Spanish interest bond (1.33) for columns (1) to (4), and equal to the observed interest rate for column (5). For mortgages after 8 November 2018 we use the observed interest rates. *Treated* is a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those granted in Basque Country, and zero otherwise. *Post* is a dummy variable that takes one for all the periods after 8 November 2018, and zero otherwise. Columns (2) and (3) split the sample based on the third quartile of the distribution of gross income (below and above, respectively). Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at time and zip code level, and the corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

TABLE 7

EFFECT OF THE SHIFT OF STATUTORY INCIDENCE ON OTHER LOAN TERMS:

AMOUNT, MATURITY, LOAN TO VALUE, LOAN AMOUNT/MORTGAGE LIABILITY AND DEFAULTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	Loan Amount		Maturity	Loan to Value	Loan Amount/Mortgage Liability	Future Default	Additional Credit
		Zip-code Level					
Treated*Post	0.010	-0.012	-0.003	-0.014	0.092*	-0.005	0.003
	(0.013)	(0.028)	(0.004)	(0.011)	(0.048)	(0.007)	(0.004)
Bank*Year:month:day*Fixed/Variable/Mixed Interest Rate Fixed	Yes	No	Yes	Yes	Yes	Yes	Yes
Zip Code*Employment Status*Foreigner Fixed Effects	Yes	No	Yes	Yes	Yes	Yes	Yes
Loan Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip Code Fixed Effects	-	Yes	-	-	-	-	-
Year:month Fixed Effects	-	Yes	-	-	-	-	-
Observations	168,250	39,839	168,250	168,250	168,250	168,250	168,250
R-squared	0.728	0.891	0.367	0.631	0.862	0.197	0.202

Notes: The table above reports OLS (for columns (4) to (7)) and Poisson regression (for columns (1) to (3)) and results of the effect of the shift of statutory incidence on other loan terms of new granted mortgages between 2018M1 and 2019M5 on the treatment variable *Treated*Post*. *Treated* is a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those granted in Basque Country, and zero otherwise. *Post* is a dummy variable that takes one for all the periods after 8 November 2018, and zero otherwise. Columns (1) uses as dependent variable the loan amount in euros. Columns (3) uses as dependent variable the maturity is months. Columns (4) uses as dependent variable the log of the loan to value ratio. Columns (5) uses as dependent variable the logit transformation of the ratio of loan amount over mortgage liability. Columns (6) uses as dependent variable future defaults, which include loan delinquencies up to 2020Q2 but also borrowers that ask for a loan moratorium during the Covid-19 crisis. Column (7) uses as dependent variable a dummy take takes one whether the household gets a consumer loan in the same month of the mortgage or in the month before or after, and zero otherwise. Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at the bank, time and zip code level, and the corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

TABLE 8

BANK-LEVEL EX-ANTE DIFFERENTIAL EXPOSURE TO THE SHIFT OF STATUTORY INCIDENCE: EX-ANTE CLASSIFICATION OF BANKS

PANEL A: LOAN LEVEL

	(1)	(2)	(3)	(4)	(5)	(6)
	Mortgages				Consumer Loans	
Dependent Variable:	Interest Rate		Loan Amount/Mortgage Liability		Loan Application Granted	
High Exposure to Mortgages outside Basque Country*Post	0.113*	0.174***	0.125**	0.174***	0.019*	0.016**
	(0.064)	(0.052)	(0.063)	(0.060)	(0.011)	(0.008)
High Exposure to Mortgages outside Basque Country*Post*Bank NPL ratio		0.042*		0.105**		0.005
		(0.025)		(0.040)		(0.004)
Borrower Fixed Effects	No	No	No	No	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year:month:day*Fixed/Variable/Mixed Interest Rate Fixed Effects	Yes	Yes	Yes	Yes	No	No
Zip Code*Employment Status*Foreigner Fixed Effects	Yes	Yes	Yes	Yes	-	-
Province Fixed Effects*Application Year:month:day	No	No	No	No	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	-	-
Observations	168,250	168,250	168,250	168,250	889,366	889,366
R-squared	0.496	0.497	0.824	0.831	0.731	0.732

PANEL B: BANK LEVEL

	(1)	(2)	(3)
Dependent Variable:	Interest Income Loans/Total Assets	Loan Fees/Total Assets	ROA
High Exposure to Mortgages outside Basque Country*Post	0.054*	0.001	-0.093**
	(0.028)	(0.006)	(0.046)
Year:quarter Fixed Effects	Yes	Yes	Yes
Bank Fixed Effects	Yes	Yes	Yes
Observations	390	390	390
R-squared	0.973	0.35	0.693

Notes: Panel A above reports OLS regression results of the shift of statutory incidence on new mortgages and consumer loans applications between 2018M1 and 2019M5 on the treatment variable *High Exposure to Mortgages outside Basque Country*Post*. *High Exposure to Mortgages outside Basque Country* is a dummy variable that takes the value of one if the ratio of mortgages loans over total assets of the bank before the change in the regulation (December 2017) is above its median value (i.e., an ex-ante variable), and zero otherwise. *Post* is a dummy variable that takes one for all the periods after 8 November 2018, and zero otherwise. *Bank NPL* is the non-performing loan ratio of the bank at 2017M12. For Panel A, columns (1) and (2) use as dependent variable the interest rate, and columns (3) and (4) the logit transformation of the ratio of loan amount over mortgage liability. Panel B reports OLS regressions results at the bank level between 2018Q1 and 2019Q2. For Panel B, column (1) uses as dependent variable interest income of loans over total assets of the bank, column (2) the loan fees over total assets, and column (3) the ROA of the bank. Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at the bank, time and zip code level (for Panel A) and at the bank level (for Panel B), and the corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

TABLE 9

SHIFT OF STATUTORY INCIDENCE ON LOAN AMOUNT OVER MORTGAGE LIABILITY: HETEROGENEITY

Dependent Variable: Loan amount/Mortgage liability					
	(1)	(2)	(3)	(4)	(5)
Treated*Post	0.106**	0.104**	0.095***	0.099***	0.119**
	(0.051)	(0.049)	(0.034)	(0.035)	(0.047)
Household Characteristics					
Treated*Post*Log(Gross income)	-0.195*	-0.191*	-0.212*	-0.164	-0.111
	(0.109)	(0.108)	(0.122)	(0.127)	(0.101)
Treated*Post*Public servant		-0.038	-0.033	-0.001	0.004
		(0.059)	(0.055)	(0.051)	(0.049)
Treated*Post*Banking group employee		0.043	0.046	0.046	0.072*
		(0.044)	(0.037)	(0.043)	(0.039)
Treated*Post*Student		0.039	0.044	0.043	0.044
		(0.044)	(0.043)	(0.045)	(0.046)
Treated*Post*Unemployed or homemaker		-0.040	-0.062	-0.078	-0.084
		(0.055)	(0.046)	(0.060)	(0.062)
Treated*Post*Log(Age)			-0.064	-0.003	-0.009
			(0.064)	(0.045)	(0.040)
Treated*Post*Log(LTV)			-0.095	-0.014	0.008
			(0.089)	(0.059)	(0.067)
Treated*Post*Log(1+No. of banks in the zip code)				-0.001	0.002
				(0.022)	(0.021)
Treated*Post*Log(No.of banking relationships)				-0.009	0.001
				(0.027)	(0.030)
Bank Characteristics					
Treated*Post*Log(Total assets)				-0.026	-0.040
				(0.028)	(0.027)
Treated*Post*Own funds/Total assets				0.008	0.013
				(0.015)	(0.015)
Treated*Post*Liquidity ratio				0.006	0.006
				(0.004)	(0.004)
Treated*Post*ROA				0.225**	0.257**
				(0.107)	(0.104)
Treated*Post*NPL ratio				0.070***	0.072***
				(0.024)	(0.025)
Treated*Post*Loans to households/Total assets				0.003	0.002
				(0.005)	(0.005)
Treated*Post*Main bank				0.012	0.015
				(0.018)	(0.020)
Treated*Post*Leader bank in the zip code				0.027	0.024
				(0.030)	(0.028)
Bank*Year:month:day*Fixed/Variable/Mixed Interest Rate Fixed Effects	Yes	Yes	Yes	Yes	Yes
Zip Code*Employment Status*Foreigner Fixed Effects	Yes	Yes	Yes	Yes	Yes
Loan Characteristics	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Treated*Post*Loan Characteristics & Interest Rate	No	No	No	No	Yes
Observations	168,250	168,250	168,250	168,250	168,250
R-squared	0.861	0.861	0.861	0.862	0.863

Notes: The table above reports OLS regression results of the logit transformation of the effect of the shift of statutory incidence on the rate of the loan amount over mortgage liability of new granted mortgages between 2018M1 and 2019M5 on the treatment variable *Treated*Post* and its interactions. *Treated* is a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those granted in Basque Country, and zero otherwise. *Post* is a dummy variable that takes one for all the periods after 8 November 2018, and zero otherwise. Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at the bank, time and zip code level, and the corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

TABLE 10

EFFECT OF A SHIFT OF STATUTORY INCEDENCE ON CONSUMER LOANS

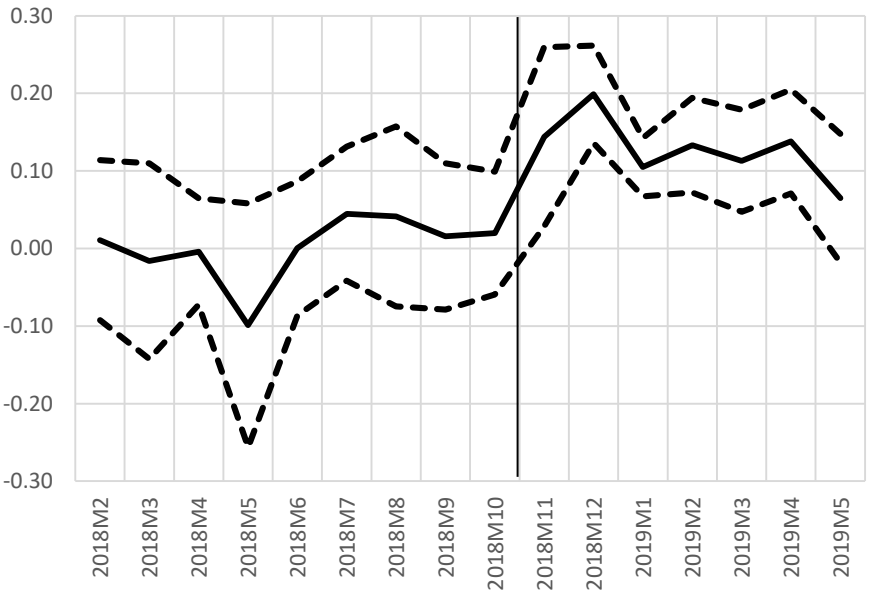
	(1)	(2)	(3)	(4)	(5)	(6)
	Loan Applications		Newly Granted Loans			
		Adjoining zip codes				
Dependent Variable:	Loan Application Granted		Interest Rate	Loan Amount	Maturity	Future Default
Treated*Post	0.023**	0.095**	0.042	0.011	-0.002	0.007**
	(0.011)	(0.044)	(0.050)	(0.022)	(0.003)	(0.003)
Borrower Fixed Effects	Yes	Yes	No	No	No	No
Bank Fixed Effects	-	Yes	-	-		-
Year:month:day Fixed Effects	-	Yes	-	-		-
Bank*Year:month:day Fixed Effects	Yes	No	-	-	-	-
Bank*Year:month:day*Fixed/Variable/Mixed Interest Rate Fixed Effects	No	No	Yes	Yes	Yes	Yes
Zip Code*Employment Status*Foreigner Fixed Effects	-	-	Yes	Yes	Yes	Yes
Zip Code*Bank Fixed Effects	No	No	Yes	Yes	Yes	Yes
Loan Characteristics	No	No	Yes	Yes	Yes	Yes
Household Characteristics	-	-	Yes	Yes	Yes	Yes
Observations	889,366	4,587	1,760,791	1,760,791	1,760,791	1,760,791
R-squared	0.731	0.748	0.495	0.598	0.348	0.187

Notes: The table above reports regression results of the shift of statutory incidence on consumer loans. In column (1) and (2) the dependent variable is a dummy that takes the value of 1 if at least a loan application is granted for the borrower in the following three months given the loan application, and 0 otherwise. Column (2) is similar to column (1) but for the zip codes adjoining to the border of the non-treated provinces. Column (3) analyzes the interest rates of new granted consumer loans, column (4) the loan amount in euros, column (5) the maturity in months and column (6) the future default of the consumer loans granted. Columns (4) and (5) estimates a Poisson model while an OLS model is used in the other cases. The time period is 2018M1-2019M5. Coefficients are listed in the first row, robust standard errors are reported in the row below which are corrected for clustering at the bank, time and zip code level for all columns but (1), where borrower level is added. The corresponding significance levels are in the adjacent column. "Yes" indicates that the set of characteristics or fixed effects is included, "No" that is not included and "-" that is comprised by the included set of fixed effects. *** Significant at 1%, ** significant at 5%, * significant at 10%.

APPENDIX

FIGURE 1

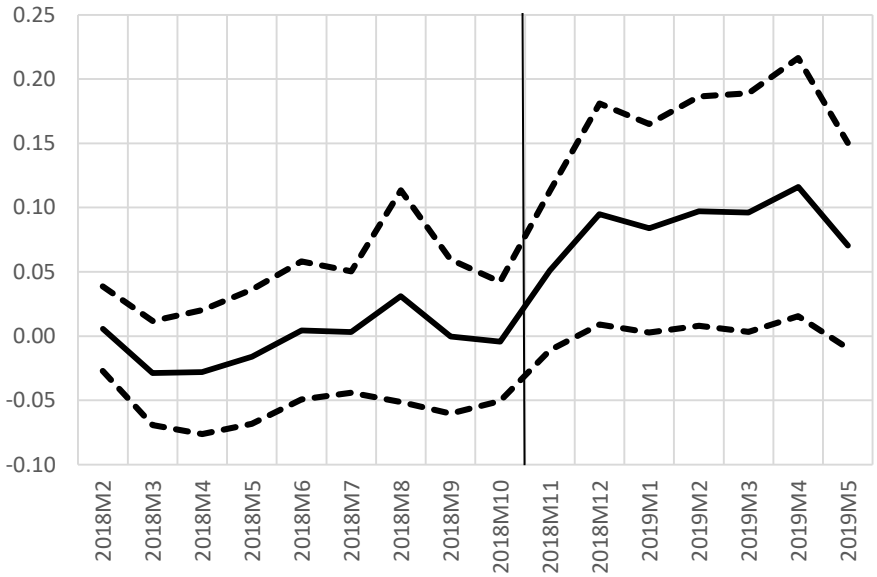
EFFECT OF THE SHIFT OF STATUTORY INCIDENCE ON INTEREST RATES BY MONTH



Notes: The table above reports the time-varying coefficients of OLS regression results using the specification of Table 2, column (8), of the interest rates of new granted primary residence mortgage loans between 2018M1 and 2019M5 on the variable *Treated*Time* dummies. *Treated* is a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those loans granted in the provinces of the Basque Country, and zero otherwise. Confidence bands are at 90%.

FIGURE 2

EFFECT OF THE SHIFT OF STATUTORY INCIDENCE ON THE RATIO OF LOAN AMOUNT OVER MORTGAGE LIABILITY BY MONTH



Notes: The table above reports the time-varying coefficients of OLS regression results using the specification of Table 7, column (4), where the dependent variable is the logit transformation of the rate of the loan amount over mortgage liability of new granted mortgages between 2018M1 and 2019M5 on the variable *Treated*Time* dummies. *Treated* is a dummy variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those loans granted in the provinces of the Basque Country, and zero otherwise. Confidence bands are at 90%.

TABLE A1

COMPARING TREATED WITH NON-TREATED MORTGAGES. MEAN TESTS

	All sample			Adjoining Zip Codes		
	Treated=0	Treated=1	Normalized Differences	Treated=0	Treated=1	Normalized Differences
	Mean	Mean	test	Mean	Mean	test
Household Characteristics						
Log(Gross income)	10.42	10.28	-0.63	10.37	10.34	-0.20
Public servant	0.05	0.08	0.10	0.03	0.06	0.11
Banking group employee	0.04	0.01	-0.12	0.02	0.00	-0.11
Student	0.02	0.03	0.05	0.01	0.02	0.02
Unemployed or homemaker	0.01	0.02	0.04	0.01	0.02	0.08
Log(Age)	6.15	6.15	0.00	6.15	6.12	-0.09
Log(LTV)	4.05	4.12	0.12	4.10	4.16	0.12
Log(1+No. of banks in the zip code)	1.92	1.90	-0.02	1.65	2.00	0.32
Indebted	0.45	0.46	0.01	0.52	0.43	-0.12
Log(1+No.of banking relationships)	0.32	0.33	0.02	0.36	0.31	-0.10
Bank Characteristics						
Log(Total assets)	17.87	18.67	7.17	17.93	18.47	0.25
Own funds/Total assets	7.98	8.52	-1.67	8.07	8.04	-0.01
Liquidity ratio	13.39	15.30	0.14	12.87	11.41	-0.19
ROA	0.54	0.37	-0.33	0.55	0.52	-0.10
NPL ratio	5.67	6.65	0.34	5.59	5.93	0.11
Loans to households/Total assets	31.93	25.69	-0.50	32.67	29.25	-0.27
Main bank	0.18	0.16	-0.04	0.22	0.16	-0.11
Leader bank in the zip code	0.15	0.25	0.17	0.22	0.29	0.11
Loan Characteristics						
Log(Loan amount)	11.68	11.45	-0.27	11.59	11.32	-0.36
Log(Loan maturity)	5.72	5.65	-0.15	5.71	5.64	-0.19
Interest rate	1.57	2.10	0.41	1.73	1.93	0.20
No. of Observations	9,703	158,547		477	644	

Notes: This table reports means of a set of variables of the new mortgages granted between 2018M1 and 2019M5. Mortgages are classified depending on the *Treated* dummy, which is variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those loans granted in the provinces of the Basque Country, and zero otherwise. Columns (3) and (6) report the normalized difference test proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) suggested a heuristic threshold of 0.25 in absolute value. The normalized difference statistic tests the null of no differences in means between treated and control group through a scale-and-sample-size-free estimator.

TABLE A2

COMPARING TREATED MORTGAGES BEFORE AND AFTER THE SHOCK. MEAN TESTS

	Before the shock Post=0		After the shock Post=1		Normalized Differences
	Mean	S.D.	Mean	S.D.	test
Household Characteristics					
Log(Gross income)	10.29	(0.19)	10.29	(0.19)	-0.02
Public servant	0.08	(0.27)	0.08	(0.27)	0.01
Banking group employee	0.01	(0.12)	0.01	(0.12)	0.00
Student	0.03	(0.16)	0.03	(0.17)	0.01
Unemployed or homemaker	0.02	(0.14)	0.02	(0.14)	0.01
Log(Age)	6.15	(0.24)	6.16	(0.24)	0.02
Log(LTV)	4.13	(0.43)	4.11	(0.46)	-0.04
Log(1+No. of banks in the zip code)	1.93	(0.60)	1.87	(0.58)	-0.07
Indebted	0.45	(0.50)	0.47	(0.50)	0.03
Log(No.of banking relationships)	0.32	(0.39)	0.34	(0.40)	0.03
Bank Characteristics					
Log(Total assets)	18.74	(1.44)	18.48	(1.56)	-0.12
Own funds/Total assets	8.64	(2.93)	8.30	(2.81)	-0.08
Liquidity ratio	15.18	(10.35)	15.21	(12.37)	0.00
ROA	0.40	(0.40)	0.36	(0.47)	-0.06
NPL ratio	6.66	(1.66)	6.51	(1.83)	-0.06
Loans to households/Total assets	25.74	(8.40)	26.45	(8.30)	0.06
Main bank	0.16	(0.37)	0.15	(0.36)	-0.02
Leader bank in the zip code	0.26	(0.44)	0.22	(0.42)	-0.05
Loan Characteristics					
Log(Loan amount)	11.47	(0.65)	11.46	(0.67)	0.00
Log(Loan maturity)	5.66	(0.32)	5.66	(0.34)	0.00
Interest rate	1.97	(0.89)	2.20	(1.09)	0.16
No. of Observations	94,466		73,784		

Notes: This table reports means of a set of variables of the new mortgages granted between 2018M1 and 2019M5. Mortgages are classified depending on the *Post* dummy, which is a variable that takes one for all the periods after 8 November 2018, and zero otherwise. Column (5) reports the normalized difference test proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) suggested a heuristic threshold of 0.25 in absolute value. The normalized difference statistic tests the null of no differences in means between treated and control group through a scale-and-sample-size-free estimator.

TABLE A3

COMPARING NON-TREATED MORTGAGES.WITH DIFFERENT TREATED GROUPS MEAN TESTS

	Basque Country vs Madrid & Catalonia			Basque Country vs synthetic Basque Country		
	Treated=0	Treated=1	Normalized Differences	Treated=0	Treated=1	Normalized Differences
	Mean	Mean	test	Mean	Mean	test
Household Characteristics						
Log(Gross income)	10.42	10.38	-0.22	10.42	10.41	-0.08
Public servant	0.05	0.08	0.09	0.05	0.06	0.03
Banking group employee	0.04	0.02	-0.07	0.04	0.03	-0.02
Student	0.02	0.02	0.02	0.02	0.02	0.00
Unemployed or homemaker	0.01	0.01	-0.01	0.01	0.01	0.01
Log(Age)	6.15	6.18	0.08	6.15	6.15	0.00
Log(LTV)	4.05	4.08	0.05	4.05	4.06	0.02
Log(1+No. of banks in the zip code)	1.92	1.80	-0.16	1.92	1.91	-0.02
Indebted	0.45	0.49	0.05	0.45	0.45	-0.01
Log(1+No.of banking relationships)	0.32	0.35	0.06	0.32	0.31	-0.01
Bank Characteristics						
Log(Total assets)	17.87	19.19	1.21	17.87	17.90	0.02
Own funds/Total assets	7.98	8.21	0.09	7.98	7.98	0.00
Liquidity ratio	13.39	15.59	0.16	13.39	13.27	-0.01
ROA	0.54	0.43	-0.26	0.54	0.54	0.02
NPL ratio	5.67	6.53	0.33	5.67	5.59	-0.03
Loans to households/Total assets	31.93	25.73	-0.51	31.93	32.01	0.01
Main bank	0.18	0.19	0.02	0.18	0.17	-0.01
Leader bank in the zip code	0.15	0.33	0.30	0.15	0.15	0.00
Loan Characteristics						
Log(Loan amount)	11.68	11.64	-0.04	11.68	11.70	0.02
Log(Loan maturity)	5.72	5.64	-0.17	5.72	5.72	0.02
Interest rate	1.57	2.15	0.44	1.57	1.69	0.11
No. of Observations	9,703	28,284		9,703	6,840	

Notes: This table reports means of a set of variables of the new mortgages granted between 2018M1 and 2019M5. Mortgages are classified depending on the *Treated* dummy, which is variable that takes the value of one for the primary residence mortgage loans granted in any Spanish provinces but those loans granted in the provinces of the Basque Country, and zero otherwise. The first analysis (first three columns) uses as the treated group new mortgages granted in Madrid or Catalonia. The second analysis (last three columns) uses as the treated group a synthetic Basque Country group obtained from propensity score matching techniques. Columns (3) and (6) report the normalized difference test proposed by Imbens and Wooldridge (2009), for which Imbens and Rubin (2015) suggested a heuristic threshold of 0.25 in absolute value. The normalized difference statistic tests the null of no differences in means between treated and control group through a scale-and-sample-size-free estimator.

TABLE A4

BASE TAX RATE BY REGION

Region (Comunidades Autonómas and Ciudades Autonómas)	(Base) Tax rate
Andalucía	1.5%
Aragón	1.5%
Asturias	1.2%
Baleares	1.2%
Comunidad Valenciana	1.5%
Canarias	1%
Cantabria	1%
Castilla La Mancha	1.25%
Castilla y León	1.5%
Cataluña	1.5%
Ceuta	0.5%
Extremadura	1.2%
Galicia	1.5%
La Rioja	1%
Comunidad de Madrid	0.75%
Melilla	0.5%
Murcia	1.5%
Navarra	0.5%
Basque Country	0%

Notes: This table reports the base tax rate for primary residence mortgages in each of the Spanish regions (Autonomous Communities and Autonomous Cities of Ceuta and Melilla).

A.1. A stylized model of statutory incidence

This section presents a stylized model, in the spirit of Chetty et al. (2009), which highlights how, in the presence of tax saliency issues, the statutory incidence irrelevance principle does not hold. In this setup we show how, consistent with our empirical findings, when a tax shifts from being levied on the borrower to being levied on banks, shift in statutory incidence, the change in the mortgage rate is smaller (and not full) for borrowers with bargaining power.

We assume a two period model in which in period 1, a borrowers needs to ask for a mortgage of size B in order to purchase a house of price P. In period 2 the borrower repays the mortgage paying (1+r)B to the bank, being r the equilibrium mortgage rate. We assume that agents discount utility with a discount factor, $0<\beta_i<1$, which can be agent specific, and that buying a house gives the borrower a discounted value equal to vP, where v>1.

The main friction of the model is that when borrowers ask for a mortgage they are not aware (salient) of the existence of a mortgage related tax in period 1, which for notional simplicity we model as being proportional to the mortgage amount, tB. We assume that in period 1 before purchasing the house the borrower has to obtain a mortgage from her bank. When obtaining the mortgage, which we call the price setting stage, the borrower does not internalize the existence of the tax and, therefore, accepts any contract that provides her with positive perceived utility $U_p>0$. We can write the perceived utility of the borrower in period 1 as

$$U_p = vP-P+B-\beta_i B(1+r).$$

At the moment of purchasing the house, after the mortgage rate has been set, the borrower realizes about the tax as she has to pay it (becomes salient). We assume that at that moment, the borrower can decide not to buy the house, and therefore not undertake the mortgage, obtaining an outside option that we normalize to 0.

Hence, a contract can only exist in equilibrium if at the moment of purchase (once the borrower becomes aware of the tax) it gives the borrower non negative utility, $U_h>0$. Where

$$U_h=vP-P+B-\beta_i B(1+r)-tB.$$

In line with Chetty et al (2009), we assume that the bank (producer) is always aware of the existence of the tax and, therefore, we assume that tax unawareness/ saliency issues only happens at the borrower (consumer) level.

The utility of the bank is therefore equal to

$$U_l=-B+\beta_b B(1+r)$$

We now resort to analyzing how statutory incidence affects the equilibrium mortgage rates under two different assumptions: (i) no borrower's bargaining power and (ii) positive borrower's bargaining power.

A 1.1. Mortgage rate setting in the absence of borrower's bargaining power

We first analyze how mortgage rates are set and react to changes in statutory incidence in the absence of borrower's bargaining power. In this section the bank is aware of the existence of the tax and, hence, takes into account that if it sets mortgage rates that are too high, the borrower does not undergo the purchase of the house and, hence, the mortgage will not be undertaken.

In such case the monopolistic bank sets a mortgage rate that makes the utility of the borrower, U_h, (which is the participation constraint of the borrower) equal to zero. We can determine that when the borrower has to pay the tax the equilibrium rate is determined by

$$V-\beta_i B(1+r)-tB = 0$$

where $V=vP-P+B$. This results in an equilibrium mortgage rate equal to

$$1+r^* = (V-tB)/\beta_i B$$

When the tax is paid by the bank, shift in statutory incidence, the participation constraint of the borrower relaxes, and the bank can increase the mortgage rate. In such case the banks sets a mortgage rate determined by

$$V-\beta_i B(1+r) = 0.$$

Resulting in an equilibrium mortgage rate equal to

$$1+r^{**} = (V-tB)/\beta_i B.$$

By comparing these two rates we can obtain how, in the absence of borrower's bargaining power, the mortgage rate depends on the statutory incidence and also that, when there is a change in statutory incidence, there is a pass through equal to t/β_i.

A 1.2. Mortgage rate setting in the presence of borrower's bargaining power

We now analyze the same setup assuming that the borrower has bargaining power. We assume that the equilibrium mortgage rate is set by the outcome of a Nash bargaining process between the borrower and the bank at the price setting stage. This mortgage rate is the one that splits the perceived surplus of the borrower and the bank. It is relevant to recall that banks perceived and actual utility at the moment of bargaining is the same, U_b, as following Chetty et al. (2009) we assume that banks are aware of the taxes. However, borrower's perceived utility is independent of the tax (as we assume it is not aware of its existence) and, therefore, there is a difference between borrower's perceived and actual utility, $U_p<U_h$.

We assume that the mortgage rate is set in order to equally split the perceived gains from trade, equal bargaining power assumption between the bank and the borrower, which means that the equilibrium loan rate satisfies $U_p=U_b$.

When the tax is paid by the borrower the rate is determined by

$$V-\beta_i B(1+r) = -B+\beta_b B(1+r).$$

Resulting in an equilibrium mortgage rate equal to

$$1+r^* = (V+B)/(\beta_i+\beta_b)B$$

When the tax is paid by the bank, statutory incidence shift, the rate is equal to

$$V-\beta_i B(1+r) = -B(1+t)+\beta_b B(1+r).$$

Resulting in an equilibrium mortgage rate equal to

$$1+r^{**} = (V+B(1+t))/(\beta_i+\beta_b)B$$

By comparing the two equilibrium rates we can determine how, when the statutory incidence shifts from the borrower to the bank, the pass-through is equal to $t/(\beta_i+\beta_b)$. This means that under borrower's bargaining power and tax unawareness pass-through to mortgages rates in not complete $t/(\beta_i+\beta_b)< t/\beta_b$.

Comparing this pass-through with the one obtained under no borrower's bargaining power, we can conclude that, in the presence of borrower's tax unawareness, the shift in statutory incidence results in a smaller change in the equilibrium mortgage rates when borrowers have bargaining power, note how $t/(\beta_i+\beta_b) < t/\beta_i$..

A 2 Alternative bargaining setup

In this section we show how our main theoretical results regarding incomplete pass-through in the presence of borrower's bargaining power and borrower's tax unawareness hold in a related model. The main change is that price setting is determined by one time take it or leave it offers instead of a Nash bargaining process.

In this setup we assume that the utility of each agent is not perfectly observed by other agents. That is, we assume that the bank receives a signal about the value of the house for the borrower v=vi+ei, where vi is the real value and ei follows a uniform (-si,si). The borrower receives a signal about bank's operational cost equal to c=cb+eb, where cb are the real operating costs and eb follows a uniform (-sb,sb). Borrowers can receive signals about the operating cost by for example observing different posted prices of banks or observing different interbank market rates among other signals.

We assume that when the bank has the bargaining power it offers a take it or leave it offer to the borrower in period 1 and when the borrower has the bargaining power she offers a take it or leave it offer to the bank in period 1. As in the previous setup we assume that the borrower is unaware of the existence of the tax in period 1 in the price setting stage and becomes aware when she has to pay the house. For expositional simplicity we assume no discounting between periods.

A 2.1 Mortgage rate setting with bank's take it or leave it offers

We first analyze a setup in which the bank holds all the bargaining power and offers a take it or leave it offer to the borrower. The bank internalizes that the borrower can decide not to buy the house (and hence not undertake the mortgage) when it becomes aware of the tax.

The mortgage will be undertake as long as the utility of the borrower is positive

$$V_i - B(1+r) - tB >= 0$$

where $V_i = v_i P - P + B$. For simplicity we assume that P=B=1 and therefore we can rewrite the previous expression as

$$v_i - (1+r) - t >= 0.$$

Given that the bank observes the signal v the probability of a mortgage being undertaken is equal to

$$Pr(v - e_i - (1+r) - t >= 0).$$

$$Pr(v - \beta_i(1+r) - t > e_i >= 0) = G(v - (1+r) - t)$$

Bank's objective function is to maximize its expected profits

$$G(v - (1+r) - t) * (r - c_b)$$

Given the properties of the uniform distribution it is direct to obtain that the equilibrium loan rate is equal to

$$r^* = (v - 1 - t + c_b) / 2$$

Following the same steps we can obtain that when statutory incidence shifts and the bank has to pay the tax the costs of the bank increase to $c'_b = c_b + t$ and the rate is equal to

$$r^{**} = (v - 1 + c'_b) / 2 = (v - 1 + c_b + t) / 2$$

By comparing the two mortgage rates we can observe how there is a full pass-through of the tax as the mortgage rate increases by t.

A 2.2 Mortgage rate setting with borrower's take it or leave it offers

In this subsection we assume that the borrower has the full bargaining power and makes a take it or leave it offer to the bank. As previously explained the borrower receives a signal about the true operating costs of the bank.

The mortgage will be accepted as long as the bank obtains non negative profits

$$r - c_b >= 0$$

Given that the borrower receives a signal $c = c_b + e_b$ about the operating cost of the bank the probability of an offer being accepted is equal to

$$Pr(r\text{-}c\text{-}e_b >= 0)=G(r\text{-}c)$$

The borrower's objective function is to maximize its perceived expected profits. Given that when she pays the tax she is not aware of the tax the borrower maximizes the expected perceived utility

$$G(r\text{-}c)*(v_i\text{-}(1+r))$$

Given the properties of the uniform distribution it is direct to obtain that the equilibrium loan rate is equal to

$$r^*=(v_i\text{-}1\text{-}c)/2$$

One the tax is levied on banks two assumptions can be made. The first one is that the borrower's signal about the operating costs of the bank does not change, in which case the equilibrium rate does not change and therefore there would be no pass-through. The alternative assumption is that the borrower receives a signal that incorporates the effect of the tax as for example banks' posted prices increase. Once the statutory incidence shifts to the bank, the borrower receives higher signals for bank's operational cost. Hence, the new signal is $c'=(c+t)+e$.

Following the same steps as before we can show that the equilibrium rate in this case would be equal to

$$r^{**}=(v_i\text{-}1\text{-}c')/2=(v_i\text{-}1\text{-}c+t)/2$$

By comparing the two rates we can observe how when borrowers make take it or leave it offers, there is not a full-pass through and the pass through is equal to t/2. Comparing this result with that obtained when banks make take it or leave it offers, we predict that, in line with our empirical findings, pass-through is lower when borrowers have bargaining power than when they do not.