

Mortgage Constraints: Household Responses, Market Adjustment, and Financial Stability*

Claes Bäckman[†]

Ragnar Enger Juelsrud[‡]

Ella Getz Wold[§]

October 15, 2026

Abstract

Mortgage constraints are widely used to reduce household leverage, but lower credit volumes alone say little about who bears the cost or whether financial vulnerability falls. We decompose the effect of a mortgage constraint tightening into three components: the direct effect on households, the equilibrium incidence through house prices, rents and credit supply and, finally, the corrective benefit. We use this framework to synthesize household-level, housing-market, lender, cross-country and credit-crisis evidence. The direct effect falls on young and low-wealth households, largely through fewer mortgage originations. Aggregate credit responses are several times larger than mechanically aggregated micro responses, consistent with general equilibrium amplification, although settings and outcomes differ across studies. Quantitative evidence on the corrective benefit is scarce, and few of the 154 tightening episodes we examine occur when credit and house-price growth is high.

*We thank the editor, David Romer, and five referees for helpful comments. We gratefully acknowledge research support from the Leibniz Institute for Financial Research SAFE. Claes Bäckman gratefully acknowledges funding from the European Union through the Marie Skłodowska-Curie Individual Fellowship YoHo, project 101151984. Views and opinions expressed are those of the authors only and do not necessarily reflect those of the European Union or the European Research Executive Agency (REA). Neither the European Union nor the granting authority can be held responsible for them.

[†]Leibniz Institute for Financial Research SAFE. Email: claes.backman@gmail.com

[‡]University of Oslo and CEPR. Email: ragnar@juelsrud.no

[§]BI Norwegian Business School. Email: ella.g.wold@bi.no

1 Introduction

Few macroprudential policies intervene as directly in household choices as those regulating mortgage borrowing. Limits on loan-to-value (LTV) and loan-to-income ratios, debt-service burdens, and amortization requirements (“mortgage constraints”) are used in most advanced economies. Authorities justify them primarily by the need to restrain credit growth and household leverage, with financial stability as the ultimate objective. A natural evaluation of such policies therefore asks whether household credit fell after a rule was introduced. Yet, a lower credit volume is an incomplete measure of policy success for three reasons. First, it says nothing about which households gave up borrowing and what the lost borrowing capacity was worth to them. Second, it does not capture the market responses that redistribute gains and losses across buyers, owners, renters, and lenders. Third, it does not reveal whether financial vulnerability was reduced or merely relocated to other borrowers, lenders, or assets.

The evidence needed to address these shortcomings is spread across several strands of the literature. Household-level studies identify who is constrained and how those households adjust. Studies of housing markets and financial intermediaries examine changes in prices, lending conditions, and the allocation of credit. Cross-country studies estimate aggregate policy effects on credit. And a separate literature relates credit growth, house prices, and debt servicing to subsequent financial distress. These estimates are rarely interpreted together.

In this article, we provide a common framework that organizes the literature on mortgage constraints. We decompose the effect of a mortgage tightening into three components: the direct effect on households whose effective borrowing capacity falls, the incidence of the equilibrium changes that follow, and the corrective benefit from lower financial vulnerability. We use this framework to review and synthesize the literature. Along the way, we classify the objectives authorities state when introducing or adjusting mortgage constraints, compare household-level and aggregate credit estimates, and compare the timing of policy changes relative to crisis risk. We end with a discussion of where the gaps in the literature are most pressing.

On the direct effect, the household-level evidence gives a consistent picture of who is affected and how they respond. LTV limits fall mainly on households with little wealth or housing equity, while income-based limits fall on households whose desired borrowing is high relative to income (DeFusco, Johnson and Mondragon, 2020; Peydró et al., 2026). Young and first-time buyers are especially exposed under both (Kinghan and McCann, 2019; Aastveit, Juelsrud and Wold, 2022). Some affected households still buy housing, but typically with a smaller mortgage, more equity from savings or family, or a cheaper property (Van Bakkum et al., 2024; Aastveit, Juelsrud and Wold, 2022; Tzur-Ilan, 2023). On the other hand, an often sizable share of potential buyers delays the purchase or remains a renter (Aastveit, Juelsrud and Wold, 2022; Eerola, Lyytikäinen and Ramboer, 2023). The same decline in borrowing can therefore reflect costly exclusion from homeownership or relatively easy substitution toward savings, family support, cheaper housing, or renting. The quantitative credit response alone is thus a poor guide to the private burden of the policy.

The direct credit effect is also small relative to aggregate estimates. Applying household-level treatment effects to the exposed share of new originations and scaling by the outstanding debt stock implies a reduction in annual household credit growth of about 0.16–0.27 percentage points for a typical tightening, mostly through fewer mortgage originations. Estimates from macro-level studies, however, are between 5–12 times larger (Cerutti, Claessens and Laeven, 2017; Akinci and Olmstead-Rumsey, 2018). Equilibrium amplification is one possible explanation for the difference. Household-level designs compare more and less exposed households within a market and therefore difference out changes in prices and credit conditions common to all households. Macro estimates retain these market-wide responses, at the cost of potentially weaker identification. The gap is consistent with amplification, although differences in samples and policy instruments could also contribute. It is also worth highlighting that there is substantial variation in the reforms between studies. For instance, some papers study reforms that make small adjustments to mortgage constraints, whereas other studies investigate the effect of the initial activation of policies or blunter measures of policy change.

The incidence of the potentially large general equilibrium effects depends on which channel is the driving force. The evidence here is thinner than for the direct credit effects, but points to three margins. First, house-price effects of mortgage regulation are small and imprecise in cross-country studies (Araujo et al., 2020) but larger in within-country studies (Acharya et al., 2022; Peydró et al., 2024; Chi, LaPoint and Lin, 2024). Lower house prices redistribute gains from net sellers to net buyers (Fagereng et al., 2025), suggesting that mortgage constraints have distributional effects through house prices. Lower house prices also tighten LTV constraints further through lower collateral values, which affects credit-constrained households. Second, rents appear to rise where mortgage constraints keep households out of ownership, so renters may bear part of the cost (Gete and Reher, 2018; Castellanos, Hannon and Paz Pardo, 2024). Third, lenders reallocate credit toward less exposed borrowers, regions, and asset classes (Acharya et al., 2022; Peydró et al., 2024; Cizel et al., 2019), which lowers financial vulnerability only if the new lending is safer than the lending it replaces. Few studies, however, quantify overall winners and losers, and the evidence on the equilibrium incidence of mortgage constraints remains largely qualitative.

The final component is the corrective benefit with respect to financial stability, which is the main rationale for mortgage constraints. Direct evidence from aggregate shocks are scarce. One exception is Peydró et al. (2024), who document that UK areas more exposed to the loan-to-income limit, and therefore less leveraged, had more resilient house-price growth and fewer mortgage defaults after the Brexit referendum. Evidence on household consumption is more mixed, since lower leverage comes with smaller liquid buffers that can leave households less able to smooth consumption after job loss (Aastveit, Juelsrud and Wold, 2026). A few papers associated tightening of mortgage constraints with subsequently fewer defaults (Van Bakkum et al., 2024; DeFusco, Johnson and Mondragon, 2020). At the aggregate level, the “GDP-at-risk” literature finds that macroprudential tightening improves the lower tail of the future GDP distribution (Franta and Gambacorta, 2020; Brandão-Marques et al., 2020), although the specific contribution of mortgage constraints is less firmly established (Fernández-Gallardo, Lloyd and

Manuel, 2023).

A much larger literature links credit to crises and can be informative for thinking about whether the corrective effects of mortgage constraints are high or low. Long historical panels show that credit growth, and in the postwar period mortgage credit in particular, predicts financial crises (Schularick and Taylor, 2012; Jordà, Schularick and Taylor, 2016), and that the risk is especially elevated when rapid household credit growth coincides with rapid house-price growth (Greenwood et al., 2022). Financial crises, in turn, are often followed by persistent, and sometimes deep, recessions (Jordà, Schularick and Taylor, 2013; Cerra and Saxena, 2008; Romer and Romer, 2017). An implication of this literature is that the corrective benefit of mortgage constraints is likely to be largest when credit and house prices grow rapidly together. We place 154 mortgage-constraint tightening episodes on a “crisis map” that sorts country-years by prior household credit growth and house-price growth. Only ten episodes fall in the region where both are growing exceptionally fast. This could mean that authorities acted preemptively, before credit and house prices reached the high-risk region, or that the policies were used when historical risk was low and as such had small corrective benefits. Distinguishing between these would require a counterfactual path for credit, house prices, and lending standards.

We end by summarizing our views on directions for future research in the context of our framework. In terms of direct effects, the main gap concerns the households that are kept out of the market. Following them over time would show whether mortgage constraints delay homeownership or exclude some households permanently, as well as shed light on other adjustments that might be informative about the overall burden of the policies. In terms of equilibrium incidence, direct and market-level responses should be estimated for the same reform, so that amplification and its channels can be estimated and understood in the same setting, and studies should trace whether credit removed from the regulated segment reappears elsewhere. In terms of the corrective benefits of mortgage constraints, it would be fruitful to measure it more directly. Following exposed and non-exposed households through downturns, for instance, would show whether lower leverage dampens the fall in spending and whether this protects the incomes of other households. Tightening episodes should also be evaluated by their effect on the subsequent path of credit and house prices, not only by the economy’s position when the policy was introduced. Finally, many countries implement multiple borrower-based measures, in addition to bank-based constraints aimed at targeting the same external costs. A holistic approach to evaluating and calibrating macroprudential policies would be a useful and potentially impactful step for policy.

The article proceeds as follows. Section 2 documents the use, objectives, and design of mortgage constraints. Section 3 introduces our organizing framework. Section 4 reviews policy exposure, implementation, and household adjustments. Section 5 connects these responses to aggregate effects and equilibrium incidence. Section 6 evaluates the evidence on corrective benefits. Section 7 sets out directions for future research, and Section 8 concludes.

2 Mortgage constraints: adoption and objectives

Mortgage constraints are now a standard part of the policy toolkit, but the term covers instruments that regulate different dimensions of mortgage borrowing and have been introduced for different reasons. This section distinguishes the main policy tools, documents their spread across countries, and examines the objectives policymakers attach to them and the economic rationales behind those objectives. We describe the statutory design of these rules only briefly. Who is exposed to them, how lenders implement them, and which households end up constrained are examined in Section 4.

2.1 Policy tools

Borrower-based mortgage constraints regulate either how much a household may borrow at origination or how quickly the resulting debt must be repaid. The most common origination constraints link borrowing to the value of the property or to household income, either directly or through required debt service.

Loan-to-value (LTV) limits cap the mortgage relative to the value of the property, requiring the borrower to finance the remaining share with equity. For a given property value, a lower maximum LTV implies a larger down payment. Because permitted borrowing is tied to the property value, changes in house prices feed directly into borrowing capacity. A collateral constraint of this form, in the tradition of [Kiyotaki and Moore \(1997\)](#), is also a common representation of borrowing frictions in quantitative housing models (e.g., [Favilukis, Ludvigson and Van Nieuwerburgh, 2017](#); [Berger et al., 2018](#); [Kaplan, Mitman and Violante, 2020](#)).

Income-based limits instead tie borrowing to household income. Loan-to-income (LTI) limits cap mortgage debt relative to income, and debt-to-income (DTI) limits cap total household debt. Unlike an LTV limit, these constraints are not mechanically relaxed for homeowners when house prices rise ([Greenwald, 2026](#); [Grodecka, 2020](#)). Debt-service-to-income (DSTI) limits cap required debt *payments* relative to income, so their tightness also depends on interest rates, maturity, and the repayment schedule. Most mortgage defaults are triggered by cash-flow shocks rather than by negative equity alone ([Ganong and Noel, 2023](#)), so DSTI limits act on a margin that is directly relevant for repayment risk.¹

Amortization requirements and *maturity limits* operate differently: they do not cap the mortgage at origination but determine how quickly it must be repaid. They raise required payments and reduce short-run cash-flow flexibility while accelerating the accumulation of housing equity. In practice, they may also reduce borrowing at origination, because households adjust loan size to affect the repayment path relative to, for instance, their income.

Countries frequently combine several of these instruments, so the constraint that binds for a

¹Terminology differs across jurisdictions and databases. In the United States, “DTI” refers to the ratio of monthly debt payments to income, which is a DSTI limit in our terminology. The rule studied by [DeFusco, Johnson and Mondragon \(2020\)](#) is therefore a DSTI limit.

given household can change with house prices, income, and interest rates. Regulations also differ in how strictly the limits apply. Some are hard caps on individual loans, while others allow lenders to exceed the limit for a share of new lending, either through a flexibility quota (Kinghan and McCann, 2019) or by capping the share of high-ratio loans in a lender’s new lending (Peydró et al., 2024), as in the UK limit on mortgages at or above 4.5 times income. Some regimes also treat first-time buyers, refinancing, or new construction differently. These features determine how far the limit written in regulation translates into the limit households face, a wedge we return to in Section 4.2.

2.2 Adoption and scope

Mortgage constraints have spread widely since the early 1990s. Panel (a) of Figure 1 shows the cumulative number of countries in the Integrated Macprudential Policy (iMaPP) database (Alam et al., 2019) that have used an LTV or income-based limit. Amortization requirements and maturity limits are not included. Of the 39 advanced economies in the sample, 34 (87 percent) have used an LTV or income-based limit at some point. Adoption is less universal but still substantial among emerging market and developing economies: 52 of 122 countries (43 percent) have used at least one such measure. Collateral-based limits such as LTV caps are somewhat more common than income-based limits in both groups.

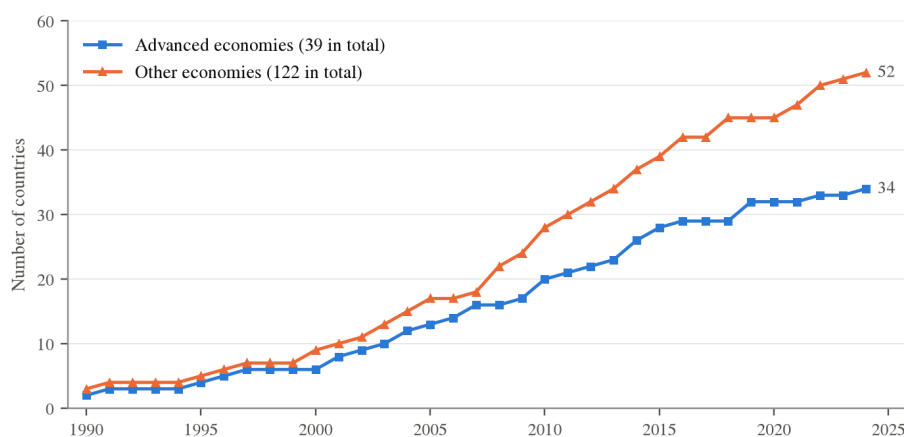
Some countries, such as Hong Kong and Korea, introduced LTV or income-based limits before the global financial crisis. Adoption accelerated after 2008, however. By then, the expansion of mortgage credit supply was widely understood to have fueled the boom in house prices and household leverage and to have deepened the ensuing bust (Mian and Sufi, 2009, 2011; Favara and Imbs, 2015; Favilukis, Ludvigson and Van Nieuwerburgh, 2017), although the relative importance of credit supply and house-price beliefs remains debated (Kaplan, Mitman and Violante, 2020). The post-crisis wave established mortgage constraints as a central *macroprudential* instrument (Moretti and Riva, 2025).

2.3 Objectives and rationales

Mortgage constraints are much used, but what exactly do they set out to achieve? To systematically characterize this, we use the objectives that national authorities report in the ECB’s Macprudential Policies Evaluation Database (MaPPED), which covers 113 macroprudential actions on residential lending standards in 18 European countries. We group the reported objectives into four categories (*credit and household leverage*, *financial stability*, *bank sector*, and *house prices*) and a residual for other objectives. Authorities can report several objectives for the same measure, so the shares do not sum to 100 percent. Appendix A describes the data.

Panel (b) of Figure 1 shows that credit and household leverage are the most frequently cited concerns: 81 percent of measures report at least one credit-related objective, and sectoral credit

(a) Cumulative number of countries that have used an LTV or income-based limit



(b) Stated objectives of borrower-based mortgage constraints

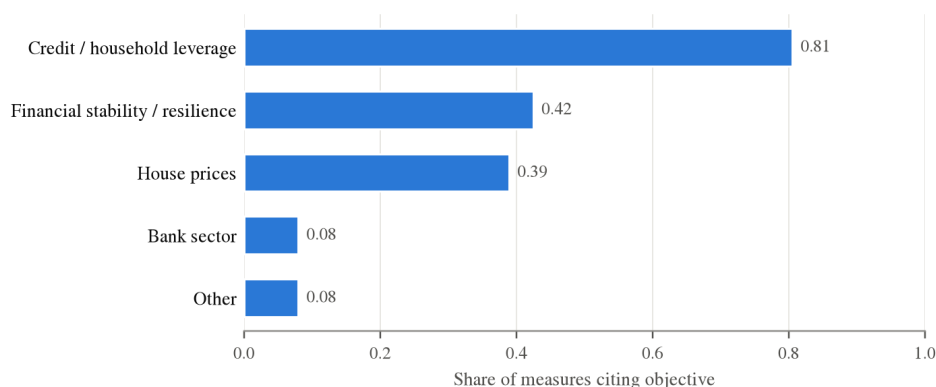


Figure 1: Adoption and stated objectives of mortgage constraints

Notes. Panel (a): data from the Integrated Macprudential Policy (iMaPP) database (Alam et al., 2019). A country is counted from the first year in which it has an LTV or income-based limit in force; the sample covers 39 advanced economies and 122 emerging market and developing economies. Panel (b): share of the 113 policy actions in the MaPPED sample (Budnik and Kleibl, 2018) citing an objective in each of the four broad categories or the residual. Objectives are multi-select and unranked, so shares do not sum to 100 percent. See Appendix A for the sample definition and mapping.

growth alone is cited by 70 percent.² Financial-stability objectives, including resilience, appear in 42 percent of measures and house-price developments in 39 percent. Objectives referring specifically to the banking sector appear in only 8 percent. Authorities thus cite credit growth, an intermediate target, nearly twice as often as financial stability, the ultimate objective that motivates macroprudential policy.

The academic literature offers at least two broad externality-based rationales for macroprudential policy, and both appear in the motivations policymakers provide. The first is a *pecuniary externality*. Individual borrowers do not internalize that higher leverage can amplify forced asset sales following an adverse shock. The resulting decline in asset prices reduces other borrowers'

²Because MaPPED covers only European countries, panel (b) speaks to a narrower set of countries than panel (a).

collateral values and tightens their borrowing constraints (Lorenzoni, 2008; Bianchi, 2011; Dávila and Korinek, 2018; Jeanne and Korinek, 2019; He and Kondor, 2016). The second is an *aggregate demand externality*. When monetary policy is constrained, borrowers do not internalize the effect of their borrowing decisions on aggregate demand (Farhi and Werning, 2016), and deleveraging by constrained borrowers can push the economy into a liquidity trap (Korinek and Simsek, 2016). Swedish and UK authorities, for instance, appeal to this rationale when they link high household debt to sharp consumption cuts after macroeconomic shocks. Both rationales correspond to what the framework in Section 3 calls the external cost of financial vulnerability. Section 6 reviews the evidence on the size of that cost.

A third rationale, consumer protection, does not rest on an externality. The US Ability-to-Repay/Qualified Mortgage rule of 2014, the rule studied by DeFusco, Johnson and Mondragon (2020), primarily intended to strengthen mortgage underwriting practices rather than restraining credit growth or reducing systemic risk. Such a rule targets failures in private borrowing and lending decisions themselves. In our framework, this changes the interpretation of the direct effect: if households or lenders misjudge repayment risk, part of the reduction in borrowing could represent a private gain rather than a cost.

Given the prominence of credit and leverage among the stated objectives, much of the empirical literature naturally asks whether mortgage constraints reduce household credit growth, and our review starts there as well. But lower credit is valued because it is expected to reduce financial vulnerability, and restricting borrowing is unlikely to be costless for the households it constrains or for the markets they trade in. The next section therefore develops a framework for identifying the relevant economic margins for a more holistic overview of the effects of mortgage constraints.

3 Framework

A mortgage constraint works through a sequence of steps, summarized in Figure 2. The statutory rule changes the effective borrowing capacity households face. Households in turn respond by adjusting their borrowing, saving, housing, tenure, or other margins.³ These responses, in turn, may move house prices, rents, and lender behavior, spreading the effects beyond the households directly constrained and determining how the policy changes aggregate outcomes.

In Appendix B, we outline a framework to capture these adjustments. In the context of that framework, the sequence discussed above provides three types of effects that are relevant for economic welfare: the direct effect on households whose effective borrowing capacity changes, the incidence of the equilibrium changes that follow, and potentially the corrective effects on the externalities associated with less restricted or unrestricted borrowing. In this section we summarize and go through the key organizing equation that emerges from that framework, while Appendix B provides more details.

³Lenders might also adjust their behavior in response to these policies, something we return to in Section 4.

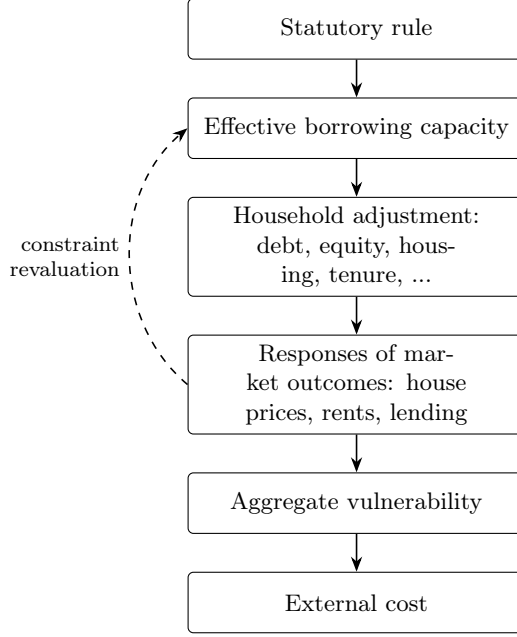


Figure 2: From the statutory rule to the three welfare terms

Notes. The figure shows the sequence of policy effects behind equation (2): the first three boxes correspond to term (I) (Section 4), the fourth to term (II) (Section 5), and the last two to term (III) (Section 6). The dashed arrow indicates that market outcomes can feed back into effective borrowing capacity, as when lower house prices reduce collateral values under an LTV limit.

3.1 The organizing equation

Before stating the key organizing equation, we introduce some notation. Let τ denote the statutory policy parameter, normalized so that a higher value permits more borrowing. For an LTV constraint, τ is the maximum loan-to-value ratio. For an LTI constraint, it is the maximum loan-to-income ratio. Let $\kappa_i(\tau)$ denote household i 's statutory borrowing capacity, measured in currency. Under an LTV rule, $\kappa_i(\tau) = \tau P h_i$, where P is the price per unit of housing and h_i is the amount purchased; under an LTI rule, $\kappa_i(\tau) = \tau y_i$, where y_i is the household income used by the lender when applying the rule. A change in the LTV limit thus affects statutory capacity in proportion to the property value, and a change in the LTI limit in proportion to income, so the instrument determines both the size and the distribution of statutory exposure. DSTI limits and amortization requirements fit the same notation, as Appendix B.1.1 shows. When presenting the framework below, we will consider one generic borrowing constraint κ .

The capacity households face in practice may differ from the statutory limit. Let $\tilde{\kappa}_i$ denote effective borrowing capacity and define implementation pass-through ϵ_i by

$$\frac{\partial \tilde{\kappa}_i}{\partial \tau} = \epsilon_i \frac{\partial \kappa_i}{\partial \tau}. \quad (1)$$

Pass-through is one when the statutory change reaches the household one-for-one and smaller when exemptions, lender discretion, or other implementation margins insulate the household.

Let λ_i denote the shadow value of relaxing household i 's borrowing constraint. It is zero when the constraint does not affect the household's attainable choices and positive when additional capacity would improve them. Because it is a marginal concept, it does not capture the loss of a household displaced from ownership altogether, a case we return to below.

Mortgage constraints can also change market outcomes. Let x_j denote an equilibrium outcome such as house prices, rents, mortgage spreads, or market-wide underwriting conditions. Let m_{ij} measure the direct money-metric exposure of household i to that outcome, holding borrowing capacity fixed. For market prices, m_{ij} corresponds to the household's net trading position. Net buyers and net sellers of housing, for example, hold opposite positions with respect to house prices, and borrowers and the owners of lending institutions hold opposite positions with respect to mortgage spreads.

Some market outcomes also feed back into borrowing capacity. This channel is captured by $\partial\tilde{\kappa}_i/\partial x_j$. Under an LTV rule, a decline in the house price reduces the collateral against which a household can borrow. Under an income-based rule, there is no corresponding mechanical effect of house prices on the statutory limit, although interest rates and spreads affect capacity under a DSTI limit.

Finally, let Z collect aggregate outcomes that potentially impose external costs. These may include aggregate household credit, the distribution of leverage and liquidity, the composition of new lending, and house-price growth. Let $\Psi(Z)$ denote the external social cost of this aggregate vulnerability. It is external because individual borrowers and lenders take aggregate vulnerability as given, whereas losses they expect to bear themselves are already reflected in their choices.

Using this notation, we show in Appendix B that the social welfare effect of a marginal policy change can be written as

$$dW = \underbrace{\left[\int \lambda_i \epsilon_i \frac{\partial \kappa_i}{\partial \tau} di \right] d\tau}_{\text{(I) Direct effect}} + \underbrace{\sum_j \left[\int \left(m_{ij} + \lambda_i \frac{\partial \tilde{\kappa}_i}{\partial x_j} \right) di \right] dx_j}_{\text{(II) Equilibrium incidence}} - \underbrace{\nabla \Psi(Z) \cdot dZ}_{\text{(III) Corrective effect}}. \quad (2)$$

Equation (2) is a first-order welfare decomposition for a marginal policy change, derived under the assumptions that households optimize given prices and take aggregate vulnerability as given. It does not provide a welfare estimate without additional empirical and normative inputs, and the loss of households displaced from ownership altogether, which is not marginal, is discussed separately below. However, because the equation identifies the information needed to evaluate the policies from a welfare perspective, we use equation (2) as a starting point for selecting and synthesizing the literature. We now discuss each of the three terms in more detail.

Direct household effects. Term (I) values the policy-induced change in effective borrowing capacity. Since a tightening has $d\tau < 0$ for the policy parameters considered here, the term is negative for households with $\lambda_i > 0$ whenever the statutory change passes through to effective capacity. The magnitude of this effect depends on statutory exposure $\partial\kappa_i/\partial\tau$, pass-through ϵ_i ,

and the shadow value λ_i .

The responses we observe in the data – depleting savings, buying a cheaper home, or turning to family members for help – are ways in which households cope with a loss of borrowing capacity. They should therefore not be counted as separate costs in addition to that loss. They are still informative, however, as they reveal the alternatives available to constrained households, indicate how costly the constraint is, and show how the policy reshapes household balance sheets.

Households that stop buying altogether require separate treatment. Some may have been close to indifferent between owning and renting. For them, the private cost of postponing a purchase is small, even though an entire mortgage disappears from the credit statistics. Others may strongly prefer to own but are kept out by a down-payment requirement, a rejected application, or a bank that has exhausted its exemption quota. Their welfare loss can be substantial and is not captured by the marginal shadow value λ_i . We therefore treat this loss as part of the direct effect, but outside the first-order expression in equation (2). Credit data alone cannot distinguish between marginal households that choose not to buy and households that are effectively excluded from homeownership. Evidence on those who do not obtain a mortgage is therefore central to Section 4.

Our framework assumes that households make borrowing decisions in accordance with their own welfare. If borrowers instead underestimate repayment risk, a mortgage constraint could protect them from taking on debt they later regret. This creates an additional benefit that partly offsets the direct cost of the constraint and provides a welfare rationale for the consumer-protection objective discussed in Section 2. We develop this extension in Appendix B.3.1.

Equilibrium incidence. Term (II) captures welfare effects that operate through endogenous market responses, and each response dx_j is valued through two channels. The first is the household’s market exposure m_{ij} . For instance, a decline in house prices benefits net buyers and imposes losses on net sellers, a change in rents redistributes between renters and landlords, and a change in mortgage spreads between borrowers and the owners of lending institutions. The second is the revaluation of borrowing capacity: if an equilibrium outcome enters the effective constraint, a change in it has an additional value for households with $\lambda_i > 0$. For instance, under LTV limits, a policy-induced decline in house prices reduces collateral values and may tighten capacity for existing homeowners beyond the statutory change.

The two channels aggregate differently. Every purchase is another household’s sale, so for market prices the exposures m_{ij} sum to zero and the first channel is a transfer between households. Yet, policymakers might attach different weights to different population groups, making the potential redistribution important. The second channel does not net out, as a price decline that reduces collateral values lowers borrowing capacity for every constrained household, with no offsetting gain elsewhere.

Corrective effects. Finally, Term (III) values the policy-induced change in the external costs. We often refer to this as “financial vulnerability” or “financial stability.” Since Z is a vector, $\nabla\Psi(Z)$ is the vector of partial derivatives of the external cost with respect to each of its components: the marginal external cost of one more unit of aggregate credit, of a higher debt-service burden, of faster house-price growth, and so on. The dot product $\nabla\Psi(Z) \cdot dZ$ multiplies each of these marginal costs by the policy-induced change in the corresponding component and sums them, which is the total change in the external cost. If a tightening moves Z in a direction that lowers the external cost, then the corrective effect is positive. The term thus combines two empirical relationships: the total effect of the policy on each outcome in Z and the marginal external cost of each outcome, $\nabla\Psi(Z)$.

Because $\nabla\Psi$ is evaluated at Z , the marginal external cost of each component can depend on the level of the others. In other words, the cross-partials of Ψ need not be zero. The evidence in Section 6.1 that credit growth predicts crises mainly when it coincides with rapid house-price growth, compressed spreads, or loosening lending standards (Greenwood et al., 2022; Krishnamurthy and Muir, 2025; Kirti, 2018) is consistent with positive cross-partials, and suggests that the corrective benefit of a given reduction in credit depends on the state in which the tightening occurs.

4 Direct effects on households

We start by considering the first term. Statutory design does not directly determine who is constrained. Incidence also depends on how lenders implement the rule and on the resources households can draw on to meet it. We therefore first examine which households are exposed to different types of constraints and discuss how lender discretion and family support shape their effective incidence. We then review how affected households adjust their borrowing, savings, housing choices, and timing of homeownership. Together, this evidence shows who bears the direct effects in term (I) and how mortgage constraints change household credit and balance sheets.

4.1 Policy exposure

Policy exposure, $\partial\kappa_i/\partial\tau$, depends on the instrument. An LTV limit binds mainly for households with insufficient wealth or housing equity to meet the down payment, and the statutory change is largest for expensive properties. LTI and DTI limits bind for households whose desired debt is high relative to current earnings. DSTI limits additionally depend on interest rates and the amortization schedule. These differences create systematic demographic exposure. Young and first-time buyers have accumulated little wealth or housing equity and are often early in their earnings profiles. Consistent with this, they emerge across the literature as among the groups most affected by mortgage constraints (Bolliger et al., 2025; Eerola, Lyytikäinen and Ramboer, 2023; Allen et al., 2020; Caloia, 2024; Kinghan, McCarthy and O’Toole, 2022; Kelly, O’Malley

and O’Toole, 2015; Peydró et al., 2026).

Exposure varies across space. Under an LTV limit, statutory exposure $\partial\kappa_i/\partial\tau = Ph_i$ scales with the property value, and under any limit the share of buyers close to the threshold is larger where house prices are high relative to incomes and wealth. Both the direct effect and the market responses reviewed in Section 5 are therefore concentrated in urban and high-price markets. This is where the Irish limits bound most tightly (Acharya et al., 2022), where Israeli capital charges on high-LTV loans had their largest price effects (Laufer and Tzur-Ilan, 2021), and from where affected Israeli buyers were pushed outward (Tzur-Ilan, 2023). The same logic applies across countries: where household leverage is high, ownership is strongly preferred, or credit-financed housing booms are common, more borrowers are close to the limits before any tightening, which raises both the direct response and the scope for amplification (Section 5.2.3).

Exposure also changes with market conditions. Rising house prices increase the down payment needed to purchase a given home, making an LTV limit more restrictive for prospective first-time buyers. The same price growth, however, creates equity for existing homeowners and may relax their LTV constraint even when the statutory cap is unchanged. LTV incidence can therefore become increasingly concentrated outside existing homeownership during a prolonged housing boom (Ortalo-Magné and Rady, 2006). Interest-rate changes similarly alter the restrictiveness of DSTI limits, while income growth changes exposure to LTI and DTI limits (Bhutta and Ringo, 2021). Consequently, incidence cannot be inferred from the headline threshold alone, as it reflects the interaction between the policy tool and the distribution of income, wealth, house prices, and existing equity.

4.2 Implementation and effective incidence

Statutory exposure does not necessarily translate into an equivalent change in effective borrowing capacity. This is captured by the pass-through parameter ϵ_i in our framework. Two observationally similar households covered by the same headline limit may face very different effective constraints depending on their lender, collateral, and access to outside resources.

Lender discretion is potentially an important source of heterogeneity. Many mortgage regulations allow banks to exempt a fraction of originations from the statutory limits. Evidence from Ireland shows that banks manage these quotas actively rather than allocating exemptions randomly, with lower-income households in high-price areas particularly likely to receive one (Kingham and McCann, 2019). The UK cap on the share of high loan-to-income originations provides further evidence of this allocation. Lenders constrained by the cap issue fewer high-LTI mortgages and charge more for them, with the largest effects on low-income borrowers. Lenders with spare capacity partly offset this contraction by expanding high-LTI lending, but primarily to other groups rather than to low-income borrowers (Peydró et al., 2024). Discretion also extends beyond formal exemption quotas. Banks also differ in the collateral they recognize, the property valuations they accept, and in whether their own underwriting standards were already tighter than the statutory limit. For a household whose lender’s internal standard binds, a statu-

tory tightening changes effective capacity by less than the statutory change, so pass-through is below one in this case too. The same statutory limit can therefore translate into very different borrowing opportunities across households and lenders.

Family resources provide another important source of heterogeneity. A parental guarantee, co-borrower arrangement, or pledge of additional collateral can change the mortgage capacity recognized by the lender and thereby affect ϵ_i . A cash transfer used for the down payment operates differently, as it does not ordinarily change the statutory mortgage limit but enables the household to satisfy it with less mortgage borrowing. Both forms of support weaken the effective force of regulation from the prospective buyer's perspective.

The empirical evidence indicates that these family resources matter. [Bolliger et al. \(2025\)](#) find that pre-death bequests became a stronger determinant of housing-market entry after Switzerland introduced LTV limits, while [Wold et al. \(2024\)](#) find that wealthy parents became more predictive of entering homeownership as Norwegian LTV limits tightened. The same mechanism operates in reverse for a loosening: [Tracey and Van Horen \(2025\)](#) find that the gains from the UK Help-to-Buy program were concentrated among households unlikely to have had access to family transfers. In addition, [Benetton, Kudlyak and Mondragon \(2022\)](#) show that parents borrow against their own housing to help their children enter the housing market, but that the scope varies with mortgage constraints. Tighter mortgage constraints can therefore increase the importance of family background.

4.3 How households adjust

Once effective borrowing capacity falls, households can reduce mortgage debt, contribute more equity, change the housing they consume, or delay or abandon a purchase. These are alternative responses to the same change in borrowing capacity, and they matter both for the private consequences of regulation and for how the policy changes total credit and household balance sheets.

4.3.1 Credit adjustments

The micro literature has focused on household mortgage responses, which combine an intensive margin, that is, a smaller mortgage among households that still purchase, and an extensive margin, that is, a postponed or abandoned purchase that removes an entire origination. The largest credit responses come from the extensive margin, i.e. delayed or abandoned house purchase. Yet, the intensive margin has received most attention in the literature. In this section we first discuss papers that estimate both margins, as these are especially useful for scaling up micro estimates to aggregate credit effects through back-of-the-envelope calculations. The purpose of this aggregation is to compare micro-implied aggregate credit effects with the direct macro effects in Section 5.

DeFusco, Johnson and Mondragon (2020) study the 43 percent DTI threshold (a DSTI limit in our terminology) in the US, using a bunching design. Among affected high-DTI borrowers, 20 percent shifted to DTI ratios below the threshold and 15 percent left the market altogether. This translates into 2.7% of all affected mortgages shifting down to a lower DTI-level, and 2% leaving the market. As the authors discuss, the actual impact on total credit growth was trivial, as the policy only applied to the jumbo market. A naive up-scaling to the full market would still imply a fairly modest reduction in household mortgage growth of 0.16 percentage points.⁴ Because loans affected by the intensive margin reduce leverage only slightly, while loans affected by the extensive margin eliminate the whole mortgage, more than 90% of this effect comes from the extensive margin.

Eerola, Lyytikäinen and Ramboer (2023) follow the methodology in DeFusco, Johnson and Mondragon (2020) and apply it to a Finnish LTV-cap, studying the impact on first-time buyers. They find somewhat larger effects, with 4% of first time buyer loans being shifted to lower LTV-levels, and 8% of first-time buyer loans leaving the market, without reporting estimates for aggregate credit growth. Another paper providing both an intensive and an extensive margin estimate is Aastveit, Juelsrud and Wold (2022), who study a Norwegian LTV-cap. They predict desired LTV-ratios based on household characteristics, and use this treatment measure in a difference in difference design. They find that affected households reduce their purchase probability by 6%, and that buyers reduce debt by 11%. According to their back-of-the-envelope calculations, the implied reduction in aggregate household credit growth is 0.27 percentage points.

On the intensive margin side, Van Bakkum et al. (2024) find that a Dutch LTV-cap reduced debt by 9.5% for affected borrowers. Also amortization requirements can reduce borrowing at origination even though they regulate the repayment path. Exploiting a Swedish reform that eliminated interest-only mortgages above a loan-to-value ratio of 50 percent, Bäckman, Moran and Van Santen (2026) document substantial bunching below the threshold and approximately 5 percent lower borrowing, concentrated among relatively wealthy households with substantial unused borrowing capacity, which points to sensitivity to required monthly payments rather than to a binding constraint.

4.3.2 Other adjustments

Changes in mortgage debt provide only a partial account of how households respond to borrowing constraints. Households may also rebalance their financial portfolios, alter consumption, change the housing bundle they purchase, delay or abandon homeownership, and adjust their behavior following adverse income shocks. These responses are important in their own right, but they are also informative about the overall burden of the policies, as well as about how the policies

⁴Extrapolating the estimated jumbo-market responses to the full US purchase-mortgage market of \$600 billion, we assume that 2 percent of loans are eliminated and 2.7 percent shrink by 4.4 percent. Dividing forgone lending by the \$8.17 trillion mortgage stock reported by the New York Fed for end-2014 gives $(0.02 + 0.027 \times 0.044) \times 600/8,170 \times 100 \approx 0.16$ percentage points. The extensive margin accounts for approximately 94 percent of this hypothetical full-market effect.

affect financial fragility.

Portfolio rebalancing. LTV constraints can lead households to replace mortgage debt with housing equity financed from their own assets. Both [Van Bakkum et al. \(2024\)](#) and [Aastveit, Juelsrud and Wold \(2022\)](#) find that LTV-constrained buyers draw down liquid assets when purchasing a home. [Boar, Gorea and Midrigan \(2022\)](#) show evidence suggesting that such a drain of liquidity can be costly to households. The decline is temporary among Dutch buyers, whereas Norwegian buyers remain less liquid for at least three years. In principle, households could also finance the additional equity contribution by selling less liquid financial assets. However, [Aastveit, Juelsrud and Wold \(2022\)](#) find no corresponding response in other financial assets, suggesting that the adjustment in their setting is concentrated in readily available liquidity rather than reflecting a broad reallocation of the financial portfolio. Whether the loss of liquidity leaves these households less able to absorb later shocks is part of the corrective effect, and we return to it in Section 6.2.

Consumption in normal times. Households could respond to tighter borrowing constraints by lowering consumption. However, we do not know of any evidence in support of this adjustment. [Aastveit, Juelsrud and Wold \(2026\)](#) find no discernible effect on consumption among affected Norwegian buyers in normal times. Importantly, their results apply to households that purchase a home despite the constraint. They do not capture consumption effects among households that delay or abandon a purchase. Such households may save more to reach the new down payment requirement, or they may save less if purchasing in the near future becomes too costly or unattainable. [Balke, Karlman and Kinnerud \(2024\)](#) find evidence of the latter channel in a quantitative model studying LTV caps.

Housing quality and location. Households can also respond by changing the housing bundle they purchase. This margin includes not only dwelling size and quality, but also location, commuting time, neighborhood characteristics, and access to employment and amenities. It is the primary response of LTV-constrained households in [Higgins \(2026\)](#), and it is present under LTV limits as well. Other studies, such as [Chi, LaPoint and Lin \(2024\)](#), find evidence pointing toward households buying cheaper houses. Using Israeli data, [Tzur-Ilan \(2023\)](#) shows that households affected by an LTV limit purchase cheaper and smaller homes and move farther from central Tel Aviv. These responses demonstrate that mortgage constraints can alter real housing consumption even when a household remains a homeowner. A reduction in purchase price should therefore not automatically be interpreted as a proportionate welfare loss, as it may reflect lower physical quality, a less preferred location, or simply a relatively inexpensive substitute. Nevertheless, changes in commuting distance, amenities, and neighborhood socioeconomic composition indicate that the consequences may extend beyond the financial characteristics of the mortgage.

Transition to homeownership. For some households, adjustment occurs not through the characteristics of the mortgage or property, but through whether and when they purchase a home. This is the extensive margin that dominates the credit response in Section 4.3.1, viewed from the household’s side. DeFusco, Johnson and Mondragon (2020) and Aastveit, Juelsrud and Wold (2022) document declines in purchases among affected households. Eerola, Lyytikäinen and Ramboer (2023) find that the Finnish LTV limit reduced first-time entry by 16 percent among affected households, or about 8 percent of all first-time apartment buyers with a mortgage, with the response concentrated among households below median income. Bolliger et al. (2025) similarly document reduced entry following Swiss LTV regulation. The extensive housing response is therefore quantitatively important and unevenly distributed: young, low-income, and first-time buyers are particularly likely to experience delayed access to ownership.

The welfare interpretation of these effects depends on the available alternatives. If the marginal household is close to indifferent between buying now, buying later, and renting, a substantial entry response can coexist with a relatively small private cost per household. Conversely, down-payment indivisibilities, credit denial, long delays, imperfect rental substitutes, or a strong preference for ownership may make the cost substantial. Sodini et al. (2023) discuss the benefits of homeownership along several channels, pointing to potentially large costs for those who delay or forgo becoming homeowners.

Labor-market search. Lower leverage may also improve labor-market resilience. Brown and Matsa (2020) show that job seekers with recourse mortgages in depressed housing markets apply for fewer jobs that require relocation. Consistent with this, Kabas and Roszbach (2023) find that displaced homeowners affected by the Norwegian LTV limit search longer and more broadly, switch occupations and industries more often, and obtain better-paid jobs. These results suggest that lower leverage reduces the pressure to accept a new job quickly, although this evidence again does not capture effects operating through delayed or forgone homeownership.

The policy tool can influence the margin of adjustment. The literature also highlights that the policy tool matters for the margin along which households adjust. Higgins (2026) shows that Irish households respond differently to LTV and LTI limits. LTV-constrained households contribute more equity and largely preserve the value of the property purchased, whereas LTI-constrained households primarily purchase cheaper properties. The form of the constraint therefore determines whether adjustment occurs mainly through household balance sheets or through housing demand.

4.4 What the evidence establishes about term (I)

Taken together, these findings can be summarized in three main sets of results. First, the direct incidence is concentrated rather than widespread. Young, first-time, and low-wealth households are most exposed, and those without family resources have fewer ways to mitigate

a tightening. Second, adjustment occurs along real and financial margins that reveal how much the constraint binds. Households deplete liquidity, alter housing quality and location, and delay or forgo ownership. Third, policy design matters. LTV and income-based limits expose different households and induce different substitutions, while lender implementation determines who ultimately faces the constraint.

The observed adjustments indicate that additional borrowing capacity is valuable to a non-negligible group and suggest that its value varies with liquid wealth, family support, and available housing alternatives. Quantifying the overall welfare effect is challenging, but a few studies make progress. [Fuster and Zafar \(2021\)](#) elicit households' willingness to pay for a comparable home under different down-payment requirements and other financing conditions. [Karapetyan, Kvaerner and Rohrer \(2024\)](#) use the introduction of an LTV cap in Sweden to quantify the willingness to pay for unregulated debt that can be used to bypass the LTV cap, finding that homebuyers are willing to pay 7.3 SEK to avoid 1 SEK of equity down payment. These studies show how financing constraints affect housing demand, but they do not identify willingness to pay for an additional unit of effective borrowing capacity for all constrained borrowers. In principle, that would require offering constrained households small increases in capacity at randomly varying fees. Mortgage-pricing notches provide a related revealed-preference design ([Best et al., 2020](#); [DeFusco and Paciorek, 2017](#)), but translating mortgage-demand elasticities into the shadow value of a regulatory constraint requires a model, and such designs generally cover only households that obtain a mortgage. A natural next step is therefore a common money-metric valuation that also includes households who delay or forgo a purchase.

5 Aggregate effects and equilibrium incidence

The household-level evidence in Section 4 compares households with different exposure to mortgage constraints while holding common market conditions fixed. A policy tightening can, however, change those market conditions. Lower housing demand may affect house prices and rents, while lenders may adjust mortgage pricing, underwriting, and the allocation of credit. These responses spread the effects of the policy beyond the households whose borrowing capacity changes directly.

In term (II) in the framework, market adjustment matters in two ways. First, changes in prices and lending conditions create gains and losses according to the positions m_{ij} that households hold. A fall in house prices, for example, benefits prospective buyers but reduces the wealth of existing owners, while higher rents redistribute from tenants to landlords. Second, a market response can feed back into effective borrowing capacity through $\partial\tilde{\kappa}_i/\partial x_j$. Section 5.1 reviews these market responses and what we know of their incidence.

The same adjustments determine how the direct credit response documented in Section 4 translates into the total change in household credit. Section 5.2 therefore turns to aggregate credit growth, the outcome most closely aligned with policymakers' stated objectives and the key

stability-relevant variable entering term (III). Comparing aggregate estimates with the direct effects implied by household-level studies reveals a sizable aggregation gap, which is consistent with quantitatively important general equilibrium amplification but does not identify it.

5.1 Market adjustment and equilibrium incidence

We organize the evidence around three market outcomes: house prices, rents, and mortgage terms and credit allocation.⁵ For each, the relevant questions are whether the policy changes the market outcome, who gains and loses from that change, and whether it feeds back into borrowing capacity or aggregate credit.

5.1.1 House prices

Mortgage constraints can lower house prices by reducing how much prospective buyers are able to bid, and whether they bid at all. The impact on prices, however, depends critically on market segmentation and housing supply elasticities. This is evident in theoretical models, where the impact of credit access on house prices varies strongly with modeling assumptions. In [Kaplan, Mitman and Violante \(2020\)](#), for instance, a change in credit conditions moves homeownership and leverage but has little effect on prices, because the option to rent leaves few households constrained in their consumption of housing services. In [Favilukis, Ludvigson and Van Nieuwerburgh \(2017\)](#), by contrast, credit conditions affect the housing risk premium and have much larger price effects. [Greenwald and Guren \(2025\)](#) relate these differences in results to the degree of segmentation between owner-occupied and rental housing. They argue that with a frictionless rental sector, landlords absorb the demand of constrained buyers and prices are pinned down by rents, whereas with segmented markets, credit-driven demand moves prices. [Guren \(2026\)](#) offers a longer discussion of how credit affects house prices in structural models.

The empirical evidence on house price effects is mixed. [Araujo et al. \(2020\)](#) survey a large number of studies and find weaker and more imprecise effects of macroprudential policy on house prices than on credit. Cross-country LTV tightenings reduce household credit by about 6 percent and real house prices by about 8 percent in [Richter, Schularick and Shim \(2019\)](#). Within-country studies based on micro data generally find larger, negative effects. They use cross-sectional variation for sharper identification, but do not necessarily capture aggregate effects. See [Gatt \(2024\)](#) and [Peydró et al. \(2026\)](#) for surveys. [Acharya et al. \(2022\)](#) show that the Irish LTV and LTI limits slowed annual house-price growth in the urban markets most exposed to the rules from well above 20 percent to around 4 percent, while growth in less exposed markets remained stable. Their results further point toward a role for expectations, as Irish households revised their future house-price expectations downward in response to the policy. [Kelly, McCann and](#)

⁵Other amplification channels could also be relevant, such as the interaction of macroprudential policy and monetary policy. For instance, a mortgage tightening that weakens housing demand and inflationary pressure may induce a more accommodative policy rate. [Richter, Schularick and Shim \(2019\)](#) do not find sizable impacts of LTV-changes on output or inflation in advanced economies, however.

O’Toole (2018) also find lower Irish prices through the credit-availability channel. Bäckman and Lutz (2025) study a mortgage constraint *loosening* and estimate that the introduction of interest-only mortgages in Denmark raised house prices by about 36 percent over the subsequent boom.

Several studies exploit regional variation in mortgage constraints or variation in the treatment of different sub-markets. In New Zealand, Armstrong, Skilling and Yao (2019) estimate relative price effects of around 3 percent by comparing regulated properties with exempt new construction, whereas Andrews (2024) finds no significant response in the national house-price index. Han et al. (2021) show that a targeted increase in down-payment requirements reshaped asking prices and bidding around the regulatory threshold in Canada, but find no differential decline in house-price growth across more- and less-exposed Toronto neighborhoods.

Chi, LaPoint and Lin (2024) use an LTV tightening that only applied to selected districts of Taiwan to compare sales on either side of district borders. After the 2014 tightening, prices on the treated side fell by about 6 percent relative to the untreated side and did not recover when the limits were lifted. Their estimates imply a leverage–house price elasticity between 0.75 and 1. In Hong Kong, more expensive properties faced lower LTV caps, and Nielsen (2019) uses this variation to estimate an elasticity of house prices with respect to the LTV limit of about 0.8. In Israel, higher capital charges on high-LTV mortgages lowered the prices of homes likely to be bought with such loans by about 2.5 percent, with larger effects in expensive regions (Laufer and Tzur-Ilan, 2021). House price effects can also build over time. Johnson (2020) uses a 1999 divergence in the debt-to-income rules of Fannie Mae and Freddie Mac to show that counties more exposed to the tighter rule saw an immediate relative decline in house prices that grew over the following years, and a smaller boom and bust during the 2000s.

In sum, theory tells us that the house-price responses should be largest where housing supply is inelastic, owner-occupied and rental markets are segmented, and constrained buyers represent a large share of demand. While the empirical evidence on house-price effects is somewhat mixed, it seems to support a qualified conclusion. Mortgage constraints do not always lower national house-price growth, but studies tend to find that they can produce sizable price responses in exposed markets, roughly in proportion to the tightening in leverage where limits target specific locations or price segments. Differences in the degree of segmentation or in housing supply elasticities offer an alternative explanation for why changes in mortgage constraints do not always lower house prices, and there is little research on how important these factors are for differences across studies.

The benefits of lower house prices are not evenly distributed. Prospective buyers benefit, while existing owners and net sellers experience capital losses (Fagereng et al., 2025). Because a homeowner who stays in place is hedged against changes in the cost of housing (Sinai and Souleles, 2005), a price change is mainly a transfer between those who will buy and those who will sell rather than a change in aggregate wealth (Buiter, 2010). Younger and lower-wealth households are more likely to be prospective buyers, whereas older and wealthier households own a larger share of the housing stock. A decline in prices can therefore offset part of the

direct burden on prospective buyers. In terms of feedback effects, lower house prices directly reduce mortgage capacity under an LTV limit by lowering collateral values for existing owners who buy again (Stein, 1995; Lamont and Stein, 1999). This collateral feedback is absent from a pure LTI or DTI limit, although house-price changes may still affect desired borrowing and the distribution of households across constraints.

5.1.2 Rents

Mortgage constraints may also affect rental markets. Households that delay or abandon a purchase add to rental demand. At the same time, investors may purchase some of the properties no longer bought by prospective owner-occupiers and make them available for rent. The effect on rents depends on which response dominates and on the elasticity of rental supply. Rules that also effectively restrict mortgages for secondary homes or investment properties may place additional upward pressure on rents by limiting the supply response.

The empirical evidence is limited but points consistently in one direction. For mortgage constraints themselves, direct evidence comes from the Irish setting. Castellanos, Hannon and Paz Pardo (2024) apply the county-level exposure measure of Acharya et al. (2022) to rents and find that, in the counties where the 2015 limits were most binding, house prices grew less and rents grew more over the following two years. Specifically, a one-standard-deviation increase in exposure is associated with 4.2 percent lower house-price growth and 2.5 percent higher rent growth. The authors calibrate a life-cycle model to the Irish market in which the limits raise rents by about 4 percent at the peak and 3 percent in the long run, with the losses concentrated among young and middle-income households. Evidence on broader credit tightenings points the same way. In the United States, metropolitan areas whose lenders tightened mortgage standards more after 2008 saw homeownership fall and rents rise, with annual rent growth about 2 percentage points higher than it would otherwise have been (Gete and Reher, 2018).

The estimated increase in rents also speaks to the segmentation question raised in Section 5.1.1. Rents rising while prices fall in the exposed Irish counties indicates that rental supply did not expand in step with rental demand. The Irish result thus illustrates how a policy that lowers the price of owner-occupied housing can nevertheless leave prospective buyers worse off, given that they remain in the rental market for some time.

Higher rents transfer resources from tenants to landlords and may therefore reinforce the distributional pattern of the direct effects. The households most likely to remain renters because of a mortgage constraint are young and have relatively little wealth, while rental property is disproportionately owned by wealthier households. Rent changes do not mechanically alter mortgage capacity under an LTV or LTI limit, but they can do so indirectly by affecting disposable income and the rate at which prospective buyers accumulate a down payment. Despite the importance of this channel, the reduced-form evidence on rents remains limited and few studies consider the supply side of rental markets.

5.1.3 Mortgage terms and credit allocation

Lenders can respond to mortgage constraints by changing interest rates, fees, underwriting standards, and the allocation of lending across borrowers, regions, and asset classes. The UK cap on the share of high-LTI originations, discussed in Section 4.2, shows that the policy changes both the price of mortgage credit and who receives it. Constrained lenders charged more for high-LTI loans, and lenders with spare capacity expanded such lending mainly to borrowers other than low-income ones (Peydró et al., 2024).

The Irish evidence shows that reallocation can extend beyond the regulated segment. Banks most exposed to the LTV and LTI limits shifted mortgage lending from lower-income to higher-income households and from urban to rural markets. They also increased lending to riskier corporate borrowers and expanded their holdings of high-yield securities (Acharya et al., 2022). Reallocation also crossed borders: Irish banking groups took on more leveraged mortgage lending in the United Kingdom after the Irish limits were introduced, with the largest increases in LTV and LTI ratios for first-time buyers (McCann and O’Toole, 2019). The initial reduction in constrained mortgage lending was therefore partly accompanied by additional credit elsewhere.

These responses spread the incidence of the policy. Borrowers who face higher mortgage rates or lose access to credit are worse off, while households and firms that receive reallocated lending may benefit. Bank owners may gain or lose depending on how the policy affects margins, loan volumes, and portfolio risk. Reallocation also matters for the corrective benefit. A reduction in high-LTV mortgages lowers financial vulnerability only to the extent that the displaced credit does not reappear in an equally or more fragile form.

5.2 From direct to aggregate credit effects

Given that house prices, rents and mortgage terms are affected, a natural question to explore is the overall implications for credit. For instance, policy-induced movements in house prices can amplify the effects of mortgage constraints on aggregate credit. By reducing the purchasing capacity of constrained households, a tightening can put downward pressure on house prices. Under an LTV limit, lower prices also create a feedback effect on mortgage capacity. Lender responses may reinforce or offset this mechanism. We now turn to macro estimates of the effect of mortgage constraints on *credit*. Comparing these estimates with the household-level effects in Section 4 reveals a sizable aggregation gap and illustrates the importance of market-level adjustments for understanding the overall effect on credit, which is a key input into the overall effectiveness of mortgage constraints for reducing external costs.

5.2.1 Macro estimates of credit effects

The macro literature estimates the relationship between mortgage constraints and aggregate credit growth using variation across countries and over time. This avoids extrapolating from

a particular group of exposed households, but it creates other comparability challenges. Some studies compare periods in which a constraint is in place with periods in which it is absent, while others study discrete tightening and easing actions. Policy measures, credit aggregates, and estimation horizons also differ.

[Cerutti, Claessens and Laeven \(2017\)](#) use indicators for whether borrower-based constraints are in place. In advanced economies, annual real household credit growth is 1.4 percentage points lower when a strictly enforced LTV cap is in place and 1.8 percentage points lower when a DTI limit is in place, although the LTV estimate is imprecise. The point estimates for emerging economies are several times larger. [Akinci and Olmstead-Rumsey \(2018\)](#) instead record tightening and easing actions. They estimate that an additional LTV tightening lowers annualized housing-credit growth in the following quarter by about 1.4 percentage points, and a DSTI tightening by about 2 percentage points.

In general, meta-analyses supports these numbers, albeit with slightly smaller estimates. Combining results across 58 studies, [Araujo et al. \(2020\)](#) find that a typical macroprudential tightening, which includes a number of tools outside of mortgage constraints, lowers year-on-year total credit growth by roughly 0.6 percentage points. [Malovaná et al. \(2025\)](#) focus on borrower-based measures and convert the estimates to a common outcome. Their unadjusted means imply reductions in annual credit growth of about 0.9 percentage points for LTV measures, 1.2 percentage points for income-based measures, and 2 percentage points when the two are used jointly. The authors further apply an adjustment for potential publication bias, i.e. selective reporting of estimates with the expected sign and statistical significance. With this adjustment, the pooled range across instruments is approximately 0.6–1.1 percentage points. However, the sample of studies in [Malovaná et al. \(2025\)](#) also contains a sizable share of micro-level studies, and are hence not directly comparable. Our preferred macro-range is therefore 1.4 - 2 percentage points based on the estimates in [Cerutti, Claessens and Laeven \(2017\)](#) and [Akinci and Olmstead-Rumsey \(2018\)](#).

5.2.2 The aggregation gap

The aggregate estimates are substantially larger than the direct effects documented in Section 4, which implied reductions in annual aggregate credit growth of approximately 0.16–0.27 percentage points. These calculations include both smaller mortgages among continuing buyers and the loss of entire originations when households do not purchase. The preferred macro estimates are about five to twelve times larger. We refer to this as the “aggregation gap.”

The aggregation gap is relevant for both remaining terms of the framework. If market adjustment accounts for a meaningful share of the larger aggregate response, the household-level evidence captures only part of the policy’s incidence, making term (II) quantitatively important. The total credit response also enters term (III), where it is valued according to its effect on financial vulnerability. Understanding the source of the gap therefore matters for both who bears the costs of the policy and how large its potential corrective benefit may be.

The gap should not, however, be automatically interpreted as an estimate of general equilibrium amplification. The micro and macro studies differ in their samples, countries, policy changes, credit measures, estimation methods, and time horizons. Moreover, macro estimates may also capture anticipation effects, concurrent policy changes, or other developments around the time of a tightening, while naive extrapolation of micro results to the aggregate economy is a rough approximation at best. The cross-sectional comparisons in micro studies might also suffer from contaminated control groups (see, for instance, [Berg, Reisinger and Streitz, 2021](#)). The direction of the resulting bias depends on which households the spillover affects most. If lower house prices reduce borrowing more among comparison households, for instance because exposed households are already at the limit and cannot adjust further, the estimate understates the direct effect and the aggregation gap overstates amplification. If the spillover instead falls mainly on exposed households, as with the collateral feedback under an LTV limit, or if lenders reallocate credit toward the comparison group, as in Ireland, the estimate overstates the direct effect and the gap understates amplification. Appendix [C.2](#) discusses diagnostics that can distinguish these cases.

With these caveats in mind, we discuss what share of the aggregation gap could plausibly result from general equilibrium amplification mechanisms such as house-price declines in the next subsection.

5.2.3 How much amplification would close the gap?

Direct estimates of equilibrium amplification arising from changes in mortgage constraints are rare. It is therefore instructive to explore general equilibrium amplification to other credit shocks and see whether these shocks generate amplification on the order of what is implied by the estimates discussed above. We express each estimate below as the ratio of the total response to the direct response, where a ratio of one indicates no amplification. Such ratios are not model-free: the mapping from regional reduced-form estimates to an aggregate causal effect depends on the class of models assumed to generate the data ([Mian, Sarto and Sufi, 2026](#); [Sarto, 2025b](#)), and we therefore use them only to judge orders of magnitude.

[Mian, Sarto and Sufi \(2026\)](#) estimate that the total regional response to the US credit expansion of the 2000s was approximately 4.5 times the direct response identified within local markets. [Huber \(2018\)](#) studies a lending cut at a large German bank and finds that the total county employment response is approximately 2.6 to 3.9 times the direct effect on borrowing firms, so local general equilibrium effects account for roughly 60 to 75 percent of the total. Using a different approach, [Sarto \(2025a\)](#) separates propagation through house prices from propagation through lender balance sheets for US counties during the same credit expansion.⁶ His estimates imply a ratio of total to direct effects of about 5.6, ranging from 4.2 where housing supply is elastic to 7.7 where it is inelastic.

⁶[Sarto \(2025a\)](#) reports the multiplier as the general equilibrium response net of the direct effect: 2.90 on average, and 1.99 and 4.22 in counties with elastic and inelastic housing supply, respectively. This uses Sarto's more conservative decomposition. To make the numbers comparable to those of [Mian, Sarto and Sufi \(2026\)](#) and [Huber \(2018\)](#), we add Sarto's estimated direct effect of 0.631 from Table 2 to the extra effect $\hat{M}$ in Table 7 and divide by the direct effect. The ratio of the total to direct effect is then $(2.9 + 0.631)/0.631 = 5.6$.

The estimates above are regional in nature, and regional effects need not coincide with national effects. They may overstate aggregate amplification when housing demand or lending shifts from exposed to less exposed areas, but understate it when time fixed effects absorb common national changes in house prices or bank funding. Geographic openness matters as well. Credit shocks spill across regions when households purchase housing elsewhere, making national effects larger than local estimates, while international leakage may weaken this amplification in small open economies. Consistent with this, [Mian, Sarto and Sufi \(2026\)](#) find larger credit multipliers in less open metropolitan areas, and [Gertler and Gilchrist \(2019\)](#) find that aggregate house-price shocks have larger employment effects than local shocks. Monetary policy can provide an offset ([Chodorow-Reich, 2019](#)). While country-specific euro-area mortgage regulations are unlikely to move the common policy rate, central banks with independent monetary policy could in principle ease in response to weaker housing demand.

Lender networks can also move the regional-to-national correction in either direction. A shock that reduces lender net worth may contract credit across all markets served by the affected banks, causing regional estimates to understate aggregate amplification. [Sarto \(2025a\)](#), for example, finds substantial amplification from shocks originating in other regions. By contrast, a borrower-based limit that removes lending opportunities without reducing bank capital may induce lenders to re-deploy credit toward less exposed regions, borrowers, or asset classes. Such reallocation makes regional effects larger than the aggregate contraction, as illustrated by Ireland, where mortgage issuance changed little after the 2015 limits ([Acharya et al., 2022](#)). The correction from regional to national effects therefore depends on whether the dominant mechanism is propagation or reallocation, and mortgage constraints may activate both simultaneously.

With these caveats noted, Figure 3 uses the estimates from related credit settings to illustrate how amplification could affect the comparison. Applying multipliers of 3 to 7.7, the range spanned by the regional estimates above, to the household-level effects of 0.16–0.27 percentage points produces total effects of 0.5–2.1 percentage points, which spans the cross-country macro estimates of 1.4–2 percentage points. The overlap, however, is confined to the upper part of the multiplier range: the macro estimates correspond to five to twelve times the household-level effects, so only amplification of the size Sarto (2025) finds in counties with inelastic housing supply, or the upper end of the regional credit multipliers, closes the gap, and the German and US employment benchmarks of 2.5–4.5 do not. Because the benchmarks come from other types of credit shocks, the exercise serves as a plausibility check rather than an estimate of amplification from mortgage constraints. Its conclusion is that equilibrium effects at the upper end of those found elsewhere in the credit literature are sufficient to reconcile the two sets of estimates, but, importantly, they are yet to be estimated for mortgage constraints, and the gap may also reflect upward bias in the macro estimates.

5.3 What the evidence establishes about term (II)

The evidence suggests that the market responses collected in x in term (II) may be quantitatively important, but also clearly illustrates that more evidence is necessary on the size of spillovers

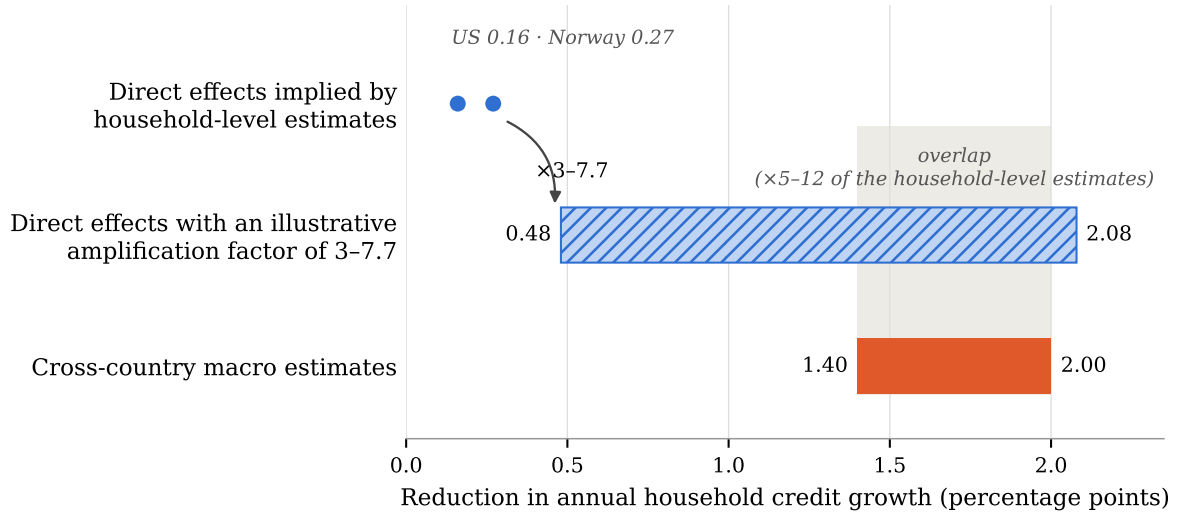


Figure 3: Illustrative magnitudes required to reconcile micro and macro estimates
Notes. Notes. The household-level estimates are back-of-the-envelope aggregate implications of DeFusco et al. (2020) for the United States (0.16) and Aastveit et al. (2022) for Norway (0.27). The hatched bar applies factors of three to 7.7 to these estimates, the range of the regional multipliers reviewed in the section above, with 7.7 the estimate of Sarto (2025) for counties with inelastic housing supply. The shaded band marks the overlap with the macro range, which corresponds to factors of five to twelve. The macro range summarizes the cross-country studies reviewed above. The comparison is illustrative because the underlying studies differ in countries, policy tools, periods, outcomes, and research designs.

within a single market. Aggregate credit estimates are several times larger than the effects obtained by extrapolating from household-level studies. This gap does not directly identify general equilibrium amplification, as the underlying studies differ in their settings, methods, and outcomes. It is nevertheless consistent with sizable responses in house prices, lending conditions, and credit allocation. Evidence from related credit shocks also shows that general equilibrium effects of this magnitude are plausible.

The incidence of these responses depends on the channel. Lower house-price growth benefits prospective buyers, who tend to be younger and less wealthy than existing owners. This may offset some of the adverse distributional effects identified in term (I). House-price declines also reduce housing wealth for existing owners and, under an LTV limit, can lower the collateral available to homeowners who wish to move or refinance. This latter effect enters term (II) through the term $\partial \tilde{\kappa}_i / \partial x_j$, and is absent from a pure income-based limit. A fall in house prices can therefore benefit first-time buyers while tightening borrowing capacity for existing owners.

Other market responses may, however, reinforce the direct burden. Higher rents fall on households that remain in the rental market because they cannot buy, potentially offsetting part of the gain from lower house prices for prospective buyers. The distributional incidence of lender reallocation is less clear. Credit that is withdrawn from highly leveraged borrowers or expensive regions may be redirected toward households with lower leverage, borrowers in cheaper areas, or firms and assets outside the regulated market. These groups are generally less exposed to the direct effect in term (I). The available evidence therefore shows that term (II) can redistribute

the effects of mortgage constraints across households and markets in ways that differ from direct statutory exposure. What it does not yet provide is a joint estimate of these gains and losses, or a money-metric estimate of term (II) as a whole.

6 Corrective effects

We now turn to the final term in our organizing equation, term (III), which captures the macro-financial rationale for mortgage constraints. A tightening generates a corrective benefit when it moves the stability-relevant outcomes in Z , such as credit growth, the composition and servicing burden of debt, and house-price growth, in a direction that lowers the probability or severity of financial distress. In principle, the effect could also be negative, for instance if mortgage constraints deplete households' liquid buffers. We separate the discussion into three parts. First, we discuss which outcomes matter for financial stability, focusing on the role of credit in particular, in line with the literature. This captures the term $\Psi(Z)$ in term (III). Second, we consider direct changes in stability-relevant outcomes Z , including—but not limited to—household credit. Finally, we discuss reduced-form evidence of the full term (III), coming from the GDP-at-risk literature.

6.1 What matters for the corrective benefits?

A large literature focuses on the ex ante prediction of financial crises. A central insight from that literature is that credit expansions are one of the most robust predictors of financial crises in long historical panels (Schularick and Taylor, 2012). In the postwar period, mortgage lending accounts for much of this relationship (Jordà, Schularick and Taylor, 2016). Increases in household debt relative to GDP also predict weaker subsequent output growth across countries (Mian, Sufi and Verner, 2017).

Panel (a) of Figure 4 illustrates, however, that this relationship is state dependent, using estimates from Greenwood et al. (2022). The cells report the probability that a financial crisis begins within three years for country-years sorted by prior household credit growth and real house-price growth. Crisis probability is 36.8 percent when the three-year change in household debt relative to GDP is in its top quintile and real house-price growth is in its top tercile, the region Greenwood et al. (2022) call the R-zone (“red zone”), against 3.3 percent in the middle cell and roughly 7 percent in country-years where neither credit nor house prices are elevated. In their regression decomposition, unusually high credit growth alone adds about 9 percentage points to the three-year crisis probability and unusually high house-price growth alone adds essentially nothing, but when both are high the probability rises by about 30 percentage points. Risk is therefore particularly high during joint credit and house-price booms.

Other evidence reinforces the interpretation that the impact of credit depends on other economic conditions. The housing booms that end most badly are leveraged booms rather than

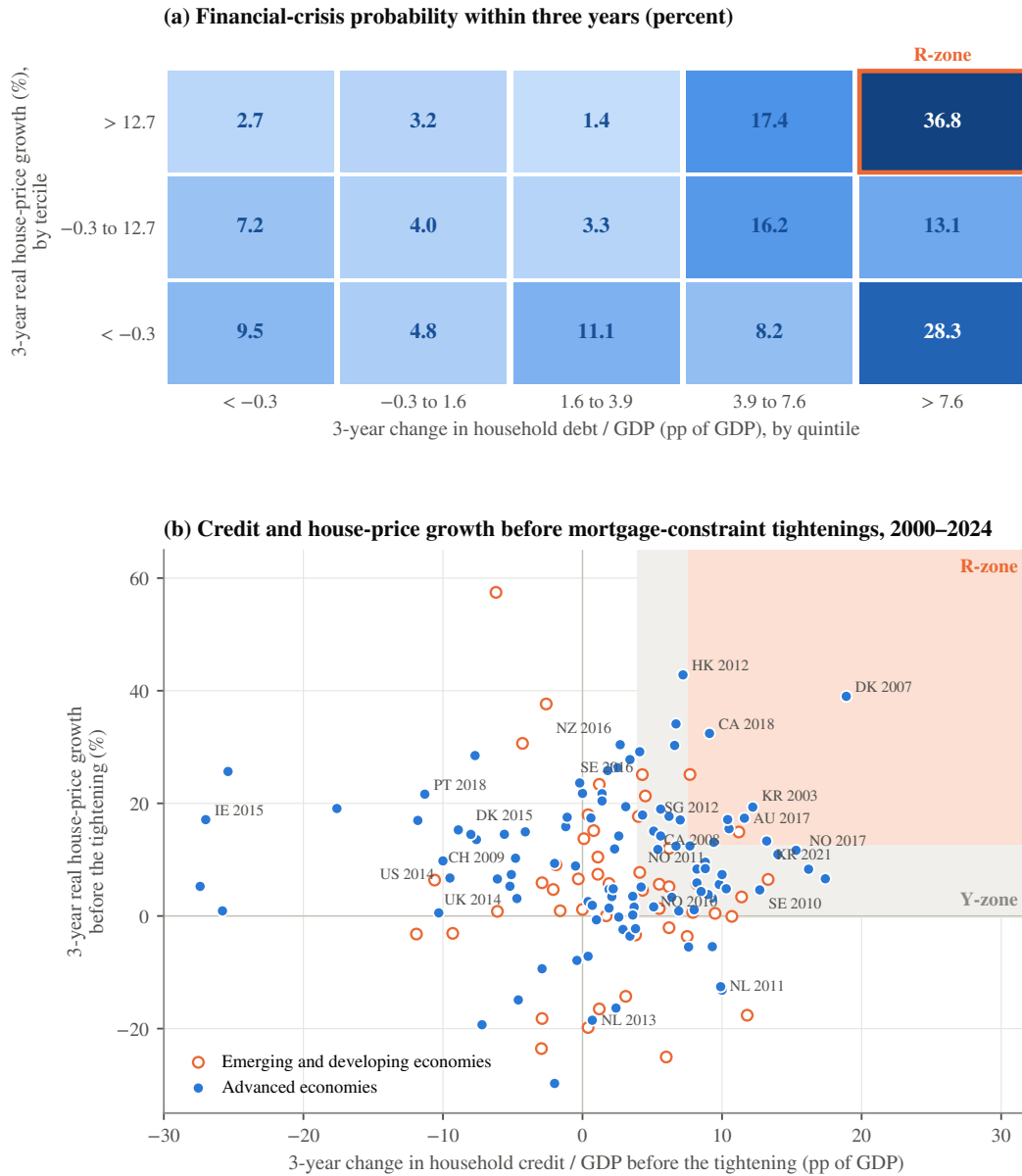


Figure 4: Crisis risk and mortgage-constraint tightenings on the credit and house-price map

Notes. Panel (a) reproduces Table 3, Panel D of [Greenwood et al. \(2022\)](#). Each cell reports the probability, in percent, that a financial crisis begins within three years for country-years sorted by the three-year change in household debt relative to GDP and three-year real house-price growth. The sample covers 42 countries from 1950 to 2016; crisis dates are from [Baron, Verner and Xiong \(2021\)](#). The outlined cell is the household R-zone. Cell values are conditional crisis probabilities and should not be interpreted as causal policy effects. Panel (b): each marker represents a tightening episode involving an LTV limit, an income-based limit, or a household loan restriction recorded in the IMF iMaPP database ([Alam et al., 2019](#)). Consecutive tightening actions within twelve months are treated as one episode. Positions are measured at the end of the quarter before the first action using the three-year change in credit to households relative to GDP and the three-year change in the BIS real residential property-price index. The shaded regions are the Y-zone and, nested within it, the R-zone, defined using the thresholds in [Greenwood et al. \(2022\)](#). Labels identify selected advanced-economy episodes, including the reforms studied in the household-level literature reviewed in Section 4. Observations from 2020 to 2024 are affected by unusually large movements in nominal GDP.

equity-price booms or housing booms without credit growth (Jordà, Schularick and Taylor, 2015), and booms financed at compressed spreads are followed by particularly severe outcomes (Krishnamurthy and Muir, 2025). Credit growth under stable lending standards contains much less information (Kirti, 2018). Not only does the overall credit volume matter, but so does its composition. Credit expansions concentrated in household and non-tradable-sector lending are followed by growth reversals in a way that lending to tradable firms is not (Müller and Verner, 2024).

Credit also matters for the macroeconomy once a crisis has occurred. Financial-crisis recessions produce much deeper and more persistent output losses than normal recessions, particularly after credit-intensive expansions (Jordà, Schularick and Taylor, 2013), and in a broad panel of countries output remains around 7.5 percent below its prior path a decade after a banking crisis (Cerra and Saxena, 2008). Credit growth deepens recessions even when no crisis follows: a 10-percentage-point larger three-year rise in credit to GDP before a recession is associated with about 2 percent lower output three years after the peak, with household credit mattering at least as much as corporate credit (Bridges, Jackson and McGregor, 2017). Romer and Romer (2017) temper this picture somewhat for advanced economies. Using a narrative measure of financial distress for 24 OECD countries from 1967 to 2012, they find that output falls by about 6 percent at the trough and remains roughly 5 percent below its prior path after five years, a persistent but more moderate loss.

The predictive content of credit and the depth of financial crises suggest that curbing credit growth is associated with potentially large corrective benefits. However, given the non-linearity in crisis risk discussed above and especially the importance of housing market conditions, a relevant question for assessing the potential corrective benefits is whether the observed tightenings typically occur while countries are experiencing the combinations of high credit and house-price growth.

Panel (b) of Figure 4 places the 154 tightening episodes recorded in the IMF iMaPP database between 2000 and 2024 and across 38 countries on a “crisis map” using the country’s position in the quarter before the recorded action date. Tightening inside the R-zone is in fact uncommon: only 10 of the 154 episodes occur there, including 8 of the 105 episodes in advanced economies. House-price growth is in the top tercile in 56 episodes, but only 10 of these also have household credit growth in the top quintile, and household debt relative to GDP is falling in 46 episodes.

That most tightenings occur outside the R-zone does not necessarily imply that their corrective benefits are small. Mortgage constraints are preventive and forward-looking, and optimal implementation timing might very well be at intermediate credit and house-price growth levels. That few episodes fall inside the R-zone is thus consistent both with policies rarely being used when historical risk is actually at its highest *and* with successful preemption. Distinguishing the two requires a credible estimate of the path that credit, debt service, lending standards, and house prices would have followed without intervention, not only the economy’s position at implementation.

As a final point, we note that while the crisis-prediction literature discussed above has focused mainly on credit, the micro literature has increasingly emphasized that financial vulnerability depends not only on the amount of debt, but also on the composition of household balance sheets. Households with substantial housing wealth may remain vulnerable if they hold little liquid wealth or face large required debt payments (Mian, Rao and Sufi, 2013; Kaplan, Violante and Weidner, 2014). Consistent with this, Ganong and Noel (2020) find that improving borrowers' short-run liquidity has much larger effects on default and consumption than reducing mortgage principal without lowering current payments. Credit growth and leverage are therefore important indicators of crisis risk, but they do not fully capture households' financial vulnerability.

6.2 Changes in stability-relevant outcomes

The previous subsection highlighted the key role of credit as a crisis predictor, echoing the stated policy objectives. The household-level studies in Section 4 imply a modest decline in aggregate credit growth, while the macro studies reviewed in Section 5 find larger effects. Mortgage constraints also appear to lower house-price growth in markets where the rules bind strongly, although the evidence is less consistent at the national level. These changes move two central predictors of financial crises in the direction of lower risk.

Household-level evidence provides more detail on the composition of the credit response. Much of the reduction in mortgage debt occurs through the extensive margin, as some households delay or abandon a purchase and therefore do not originate a mortgage. Among households that continue to buy, mortgage constraints generally reduce leverage and required debt payments. Both responses should make household balance sheets more resilient to declines in income or house prices. Some continuing buyers, however, meet larger down-payment requirements by drawing down liquid assets. This creates a leverage–liquidity trade-off. The household has less debt but may also have a smaller buffer available when income falls.

Evidence on consumption smoothing illustrates this trade-off. Van Bakkum et al. (2024) find that Dutch homeowners affected by the LTV limit are better able to smooth consumption, as proxied by car ownership, once their liquidity buffers have been rebuilt. Affected Norwegian homeowners instead reduce debit-card spending more following job loss (Aastveit, Juelsrud and Wold, 2026). The difference is consistent with the persistence of the liquidity response. Liquid assets recovered relatively quickly among Dutch buyers but remained lower for at least three years among Norwegian buyers.

The evidence on repayment outcomes points more consistently toward lower vulnerability, although the magnitude varies. DeFusco, Johnson and Mondragon (2020) calculate that applying the US debt-to-income rule to the whole mortgage market would have lowered the five-year default rate of the 2007 and 2008 cohorts by 0.35 to 0.40 percentage points, small relative to default rates of 24 to 34 percent among loans not repaid early. Van Bakkum et al. (2024) find that Dutch households affected by the LTV limit were 0.8 percentage points less likely to enter arrears, against a baseline rate of 3.3 percent. The effect was concentrated among below-median-

income households. [Peydró et al. \(2024\)](#) examine resilience following an aggregate shock. After the Brexit referendum, low-income borrowers in areas more exposed to the UK loan-to-income limit defaulted roughly one-fifth to one-third less often than borrowers in less exposed areas.

Taken together, the evidence suggests that mortgage constraints move most observed components of Z toward lower financial vulnerability. Credit growth, mortgage originations, leverage, and default risk decline, while house-price growth may also slow in the most exposed markets. Some forces can work in the opposite direction, however. Liquid buffers may fall among households that continue to buy, and lenders may redirect credit toward risks outside the regulated segment.

6.3 Reduced-form effects

A small literature estimates the total effect of macroprudential tightening on the distribution of future GDP growth. This is commonly referred to as the “GDP-at-risk” approach. The evidence from the GDP-at-risk literature broadly indicates that macroprudential tightening reduces risks to future growth, but is less conclusive about which instruments provide this protection.

[Franta and Gambacorta \(2020\)](#) find that tighter LTV limits narrow the distribution of future GDP growth by raising its lower tail and lowering its upper tail, without reducing mean growth.⁷ [Brandão-Marques et al. \(2020\)](#) likewise estimate net benefits from macroprudential tightening and find borrower-based measures particularly beneficial when credit vulnerabilities are high. Using narratively identified shocks in twelve European economies, however, [Fernández-Gallardo, Lloyd and Manuel \(2023\)](#) find that the statistically significant improvement in the lower tail is driven mainly by lender-based measures. Taken together, the studies support a stabilizing role for macroprudential policy overall, but do not establish that borrower-based measures are consistently the main source of improved growth-at-risk.

6.4 What the evidence establishes about term (III)

The crisis literature provides the clearest evidence on the first component of term (III), that is, the impact of stability-relevant variables on financial stability $\nabla\Psi(Z)$. Rapid household credit growth is a robust predictor of financial distress, particularly when it coincides with rapid house-price growth, and credit-intensive booms are followed by deeper downturns. Other features of household balance sheets have received less attention in the crisis-prediction literature. The available evidence nevertheless suggests that the allocation of debt, required debt payments, lending standards, and household liquidity also matter for how strongly households respond when income or house prices fall.

⁷The mean effect is not zero everywhere, however. Using LTV changes in 56 economies, [Richter, Schularick and Shim \(2019\)](#) estimate that a 10-percentage-point reduction in the maximum LTV lowers output by about 1.1 percent over four years, comparable to a 25-basis-point increase in the policy rate. The estimate is imprecise and confined to emerging economies. For advanced economies, the output cost of a tightening is indistinguishable from zero.

Mortgage constraints appear to move most of these stability-relevant outcomes Z in a favorable direction. Aggregate credit growth falls, fewer mortgages are originated, and households that continue to buy generally take on less leverage. House-price growth also declines in some of the markets most exposed to the constraints, and the evidence on arrears and defaults points toward moderately improved resilience. Not every response reduces vulnerability, however. Some continuing buyers draw down liquid assets, and lenders may redirect credit toward other borrowers, regions, or asset classes.

Reduced-form evidence on the full term (III) from the GDP-at-risk literature points in the same direction. Macroprudential tightening tends to improve the lower tail of future GDP growth, although the specific contribution of mortgage constraints is less firmly established. Overall, existing research supports a positive corrective effect, but does not yet provide a quantitative estimate of term (III) for mortgage constraints. Such an estimate would require a causal mapping from each change in Z to external losses, as well as evidence on how that mapping varies with initial credit growth, house-price growth, and other financial conditions.

7 Directions for future research

Throughout the discussion, we have highlighted a few gaps in the literature in the context of our framework. In this section, we summarize them. The best-measured object in the literature, the direct credit response of exposed households, is only a small part of the total response, and it does not reveal the full private burden of the policy, the equilibrium adjustments that follow, or the corrective benefit. In our view, one more estimate of this response is therefore less valuable than research on what happens to households that are kept out of the market, how much of the aggregate response comes from market adjustment, where displaced credit goes, and how large the external costs of household leverage actually are. We summarize this in the form of six areas for future research.

Measure the full, direct burden on households. Much of the direct credit response comes from the extensive margin, yet most of what we know about adjustment comes from households that still buy. A key question is therefore whether mortgage constraints delay homeownership by a year or two or exclude some households permanently. This distinction determines how the extensive margin should be interpreted in term (I). Linking mortgage applications and rejections to credit registers, housing transactions, rental contracts, and data on household and parental wealth would show how long entry is delayed, how much delayed households pay in rent, what and where they eventually buy, and whether family resources determine who enters (Wold et al., 2024; Benetton, Kudlyak and Mondragon, 2022). Such data exist, for instance, in the Nordic countries. Followed over a longer horizon, the same data could also show whether tighter constraints change the age profile of homeownership and the transmission of housing wealth across generations. Since households that never buy do not reveal their willingness to pay for borrowing capacity through their choices, survey designs that elicit it directly, as in

Fuster and Zafar (2021), are a natural complement.

Estimate direct and equilibrium effects in the same setting, and identify the channel. The aggregation gap in Section 5.2.2 is based on comparing studies of different countries, instruments, outcomes, and horizons, and on amplification estimates borrowed from other credit shocks. The tools needed to do better exist, but have rarely been applied to mortgage constraints. A study that estimates the household response within markets and the market-level response across markets with different average exposure, for the same outcome and horizon, delivers a regional multiplier directly (Mian, Sarto and Sufi, 2026; Huber, 2023). Such studies should also report the diagnostics for heterogeneous spillovers suggested by Berg, Reisinger and Streitz (2021) and Huber (2023). To the best of our knowledge, no study of a borrower-based measure has properly estimated and analyzed the general equilibrium amplification of mortgage constraints. Mortgage constraints also offer a natural test of the mechanism. The collateral channel operates under LTV limits but not under income-based limits, so house-price spillovers to credit should be weaker for LTI-constrained households. Settings where both types of limits were introduced simultaneously can be used to test this. Loan-level data with lender identifiers would make it possible to separate propagation through housing markets from propagation through lenders (Sarto, 2025a), and estimates of the segmentation between owner-occupied and rental markets outside the United States (Greenwald and Guren, 2025) would help discipline the step from regional to national effects.

Trace where the displaced credit goes. A tightening only lowers financial vulnerability if the credit it removes does not reappear elsewhere in an equally fragile form. Section 5.1.3 discusses several examples of such leakage, including Irish banks expanding riskier corporate lending and mortgage lending in the United Kingdom (Acharya et al., 2022; McCann and O’Toole, 2019). Households also find ways around the constraints, for instance by paying a large premium for unregulated debt (Karapetyan, Kvaerner and Rohrer, 2024) or through parents borrowing against their own homes (Benetton, Kudlyak and Mondragon, 2022). Most studies, however, measure lending in the regulated segment only. The relevant outcome for term (III) is the distribution of leverage and debt service across the whole system, including debt held by parents, by consumer-credit and non-bank lenders, and by foreign affiliates. Credit registers covering all lenders and household balance-sheet data that link generations make this measurable in several countries. Without such evidence, a fall in regulated mortgage credit is only weak evidence of a fall in aggregate vulnerability.

Study households when downturns occur and measure external costs of high leverage in a downturn. The main rationale for mortgage constraints is that high household leverage imposes costs on others. Several authorities explicitly refer to an aggregate demand externality, where highly indebted households cut consumption sharply after a shock and thereby reduce demand and incomes for everyone else (Section 2). The normative models behind this rationale are most useful for telling empirical researchers what to measure. In Farhi and Werning (2016)

and [Korinek and Simsek \(2016\)](#), the externality is the product of how sharply indebted households cut spending after a shock and how far monetary policy fails to offset the resulting fall in demand. Neither object has been measured in a setting with mortgage constraints, and doing so requires two steps. The first is to establish whether households whose mortgages were originated under tighter constraints actually cut consumption less when house prices fall or incomes decline. Although evidence from outside the macroprudential literature supports this channel ([Mian, Rao and Sufi, 2013](#)), the effect could differ for households constrained by regulation, since lower leverage comes together with lower liquid buffers ([Van Bakkum et al., 2024](#); [Aastveit, Juelsrud and Wold, 2022](#)). The second is to establish whether lower spending among indebted households reduces income and employment among other households. Regional evidence from the US housing bust and the Hungarian foreign-currency debt crisis suggests that such spillovers through local demand can be large ([Mian and Sufi, 2014](#); [Verner and Gyöngyösi, 2020](#)), but we are not aware of studies that estimate them in a setting with mortgage constraints. The designs discussed above could be used for this purpose, by linking regulatory exposure at origination to outcomes in a later downturn for both exposed households and non-exposed households in the same local market. The second factor in the externality, the monetary offset, could be studied by comparing countries inside and outside a currency union, or periods with and without a binding lower bound on interest rates. The pecuniary externality in [Dávila and Korinek \(2018\)](#) calls for an analogous exercise: measuring how much forced sales by levered households move house prices, and how many other borrowers' constraints tighten as a result, which is the collateral feedback in term (II).

Understand whether mortgage constraints keep countries out of the “R-zone.” Few tightenings occur in the joint credit and house-price boom where historical crisis risk is highest. As discussed in Section 6.1, this is consistent both with policy being used too late and with successful preemption. Separating the two requires an estimate of the path that credit, debt service, lending standards, and house prices would have followed without the tightening. The main obstacle is endogeneity, as authorities tighten in response to the very credit growth they cite as their objective. Policy series therefore need to be purged of their predictable component or identified narratively ([Richter, Schularick and Shim, 2019](#)). With more than 150 tightening episodes across nearly 40 countries, event-study and synthetic-control designs on the crisis map are now feasible. The question such designs should answer is whether a tightening keeps an economy from entering the region where the link between vulnerability and crisis risk in term (III) is steepest.

Evaluate mortgage constraints jointly. Most studies evaluate one instrument at a time, but many countries use several at once. Norway combines LTV and debt-to-income limits with amortization requirements and an interest-rate stress test, Ireland introduced LTV and LTI limits together in 2015, and Sweden combines amortization requirements with an LTV limit. As discussed in Section 2, the instruments bind for different households, and the constraint that binds for a given household can change with house prices, income, and interest rates. The effect of

tightening one instrument therefore depends on the others in place. A tighter LTV limit may, for instance, have little effect on a household that is already constrained by an income-based limit, but bind more often after house prices fall. The instruments also induce different adjustments, as LTV-constrained households draw on their savings while LTI-constrained households buy cheaper homes (Higgins, 2026). Meta-analytic estimates suggest that the credit effect is larger when LTV and income-based limits are used together (Malovaná et al., 2025), but few studies show how the instruments interact for the same population of borrowers. Loan-level data that record all the relevant ratios at origination would show how many households are constrained by each limit, and by several limits at once, and how the response to a change in one limit depends on the others. Such evidence is needed to evaluate the overall stance of borrower-based policy rather than each instrument in isolation. A further extension would include lender based measures, such as bank capital requirements, as well, acknowledging that different types of macroprudential regulation target similar systemic vulnerabilities.

8 Conclusion

Mortgage constraints are usually introduced to restrain credit and household leverage, but their effects extend beyond the amount borrowed. Exposure depends on the policy instrument, household resources, and lender implementation. Households respond through debt, liquid savings, housing choices, and the timing of homeownership. The direct credit response is modest and comes in large part from fewer mortgage originations.

Aggregate estimates are larger than the household-level results can explain mechanically. Housing markets, lenders, and expectations provide plausible amplification channels, and amplification within the range found in related credit settings can account for much of the difference. These market responses also spread the incidence to existing owners, renters, lenders, and households that receive reallocated credit. The corrective benefit depends on whether the resulting changes prevent the build-up of vulnerable balance sheets. Historical crisis risk is highest during joint credit and house-price booms, while most tightenings occur outside that state.

A decline in credit therefore shows that the policy reduced leverage, but not whether it improved welfare. That assessment also requires the value of the borrowing capacity households lose, the incidence of equilibrium changes, and the path financial vulnerability would have followed without the policy. The evidence reviewed here identifies many of these components and some areas for future research where the gain from more knowledge is especially high.

References

- Aastveit, Knut Are, Ragnar Enger Juelsrud, and Ella Getz Wold. 2022. “The leverage-liquidity trade-off of mortgage regulation.” *Norges Bank Working Paper 6/2022*.
- Aastveit, Knut Are, Ragnar Enger Juelsrud, and Ella Getz Wold. 2026. “Does mortgage regulation stabilize consumption?” *Working Paper*.
- Acharya, Viral V, Katharina Bergant, Matteo Crosignani, Tim Eisert, and Fergal McCann. 2022. “The Anatomy of the Transmission of Macroprudential Policies.” *Journal of Finance*, 77(5): 2533–2575.
- Akinci, Ozge, and Jane Olmstead-Rumsey. 2018. “How Effective Are Macroprudential Policies? An Empirical Investigation.” *Journal of Financial Intermediation*, 33: 33–57.
- Alam, Zohair, Adrian Alter, Jesse Eiseman, Gaston Gelos, Heedon Kang, Machiko Narita, Erlend Nier, and Naixi Wang. 2019. “Digging Deeper—Evidence on the Effects of Macroprudential Policies from a New Database.” International Monetary Fund IMF Working Paper 19/66.
- Allen, Jason, Timothy Grieder, Brian Peterson, and Tom Roberts. 2020. “The impact of macroprudential housing finance tools in Canada.” *Journal of Financial Intermediation*, 42.
- Andrews, Antony. 2024. “Contradictory Results: Reassessing the Impact of Loan-to-Value Restrictions on House Price Inflation.” *Australian Economic Review*, 57(3): 245–254.
- Araujo, Juliana, Manasa Patnam, Adina Popescu, Fabian Valencia, and Weijia Yao. 2020. “Effects of Macroprudential Policy: Evidence from Over 6,000 Estimates.” International Monetary Fund IMF Working Paper 20/67.
- Armstrong, Jed, Hayden Skilling, and Fang Yao. 2019. “Loan-to-value ratio restrictions and house prices: Micro evidence from New Zealand.” *Journal of Housing Economics*, 44: 88–98.
- Bäckman, Claes, and Chandler Lutz. 2025. “Mortgage Innovation and House Price Booms.” *Journal of Urban Economics*, 145: 103725.
- Bäckman, Claes, and Natalia Khorunzhina. 2024. “Interest-Only Mortgages And Consumption Growth: Evidence From A Mortgage Market Reform.” *International Economic Review*, 65(2): 1049–1079.
- Bäckman, Claes, Patrick Moran, and Peter Van Santen. 2026. “Mortgage design, repayment schedules, and household borrowing.” *The Review of Financial Studies*, 39(8): 2549–2594.
- Bailey, Michael, Eduardo Dávila, Theresa Kuchler, and Johannes Stroebel. 2019. “House Price Beliefs and Mortgage Leverage Choice.” *Review of Economic Studies*, 86(6): 2403–2452.

- Balke, Kasper Kragh, Markus Karlman, and Karin Kinnerud.** 2024. “Down-payment requirements: Implications for portfolio choice and consumption.” Working Paper.
- Baron, Matthew, Emil Verner, and Wei Xiong.** 2021. “Banking Crises Without Panics.” *Quarterly Journal of Economics*, 136(1): 51–113.
- Baumeister, Christiane, and James D. Hamilton.** 2023. “A Full-Information Approach to Granular Instrumental Variables.” Working Paper, University of California, San Diego.
- Benetton, Matteo, Marianna Kudlyak, and John Mondragon.** 2022. “Dynastic Home Equity.” Centre for Economic Policy Research CEPR Discussion Paper 17464.
- Berger, David, Veronica Guerrieri, Guido Lorenzoni, and Joseph Vavra.** 2018. “House prices and consumer spending.” *The Review of Economic Studies*, 85(3): 1502–1542.
- Berg, Tobias, Markus Reisinger, and Daniel Streitz.** 2021. “Spillover effects in empirical corporate finance.” *Journal of Financial Economics*, 142(3): 1109–1127.
- Best, Michael Carlos, James S. Cloyne, Ethan Ilzetzki, and Henrik J. Kleven.** 2020. “Estimating the Elasticity of Intertemporal Substitution Using Mortgage Notches.” *Review of Economic Studies*, 87(2): 656–690.
- Bhutta, Neil, and Daniel Ringo.** 2021. “The Effect of Interest Rates on Home Buying: Evidence from a Shock to Mortgage Insurance Premiums.” *Journal of Monetary Economics*, 118: 195–211.
- Bianchi, Javier.** 2011. “Overborrowing and Systemic Externalities in the Business Cycle.” *American Economic Review*, 101(7): 3400–3426.
- Boar, Corina, Denis Gorea, and Virgiliu Midrigan.** 2022. “Liquidity Constraints in the U.S. Housing Market.” *Review of Economic Studies*, 89(3): 1120–1154.
- Bolliger, Elio, Adrian Bruhin, Andreas Fuster, and Maja Ganarin.** 2025. “The Effect of Macroprudential Policies on Homeownership: Evidence from Switzerland.” *Journal of Urban Economics*, 146: 103749.
- Brandão-Marques, Luis, Gaston Gelos, Machiko Narita, and Erlend Nier.** 2020. “Leaning Against the Wind: A Cost-Benefit Analysis for an Integrated Policy Framework.” International Monetary Fund IMF Working Paper 2020/123.
- Bridges, Jonathan, Christopher Jackson, and Daisy McGregor.** 2017. “Down in the Slumps: The Role of Credit in Five Decades of Recessions.” Bank of England Staff Working Paper 659.
- Brown, Jennifer, and David A Matsa.** 2020. “Locked in by leverage: Job search during the housing crisis.” *Journal of Financial Economics*, 136(3): 623–648.
- Budnik, Katarzyna Barbara, and Johannes Kleibl.** 2018. “Macroprudential regulation in the European Union in 1995-2014: introducing a new data set on policy actions of a macroprudential nature.” ECB working paper.

- Buiter, Willem H.** 2010. “Housing Wealth Isn’t Wealth.” *Economics: The Open-Access, Open-Assessment E-Journal*, 4(2010-22): 1–29.
- Caloia, Francesco.** 2024. “Borrower-Based Measures, House Prices and Household Debt.” *Journal of International Money and Finance*, 143.
- Castellanos, Juan, Andrew Hannon, and Gonzalo Paz Pardo.** 2024. “The aggregate and distributional implications of credit shocks on housing and rental markets.” ECB working paper.
- Cerra, Valerie, and Sweta Chaman Saxena.** 2008. “Growth Dynamics: The Myth of Economic Recovery.” *American Economic Review*, 98(1): 439–457.
- Cerutti, Eugenio, Stijn Claessens, and Luc Laeven.** 2017. “The Use and Effectiveness of Macroprudential Policies: New Evidence.” *Journal of Financial Stability*, 28: 203–224.
- Chi, Chun-Che, Cameron LaPoint, and Ming-Jen Lin.** 2024. “Spatially Targeted LTV Policies and Collateral Values.” Working paper, SSRN 4426887.
- Chodorow-Reich, Gabriel.** 2019. “Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?” *American Economic Journal: Economic Policy*, 11(2): 1–34.
- Cizel, Janko, Jon Frost, Aerd Houben, and Peter Wierds.** 2019. “Effective Macroprudential Policy: Cross-Sector Substitution from Price and Quantity Measures.” *Journal of Money, Credit and Banking*, 51: 1209–1235.
- Dávila, Eduardo, and Anton Korinek.** 2018. “Pecuniary Externalities in Economies with Financial Frictions.” *Review of Economic Studies*, 85(1): 352–395.
- DeFusco, Anthony A., and Andrew Paciorek.** 2017. “The Interest Rate Elasticity of Mortgage Demand: Evidence from Bunching at the Conforming Loan Limit.” *American Economic Journal: Economic Policy*, 9(1): 210–240.
- DeFusco, Anthony A, Stephanie Johnson, and John Mondragon.** 2020. “Regulating Household Leverage.” *Review of Economic Studies*, 87(2): 914–958.
- Eerola, Essi, Teemu Lyytikäinen, and Sander Ramboer.** 2023. “The Impact of Mortgage Regulation on Homeownership and Household Leverage.” *VATT Working Paper*.
- Fagereng, Andreas, Matthieu Gomez, Émilien Guin-Bonenfant, Martin Holm, Benjamin Moll, and Gisle Natvik.** 2025. “Asset-Price Redistribution.” *Journal of Political Economy*, 133(11): 3494–3549.
- Farhi, Emmanuel, and Iván Werning.** 2016. “A Theory of Macroprudential Policies in the Presence of Nominal Rigidities.” *Econometrica*, 84(5): 1645–1704.
- Favara, Giovanni, and Jean Imbs.** 2015. “Credit supply and the price of housing.” *The American Economic Review*, 105(3): 958–992.

- Favilukis, Jack, Sydney C Ludvigson, and Stijn Van Nieuwerburgh.** 2017. “The Macroeconomic Effects of Housing Wealth, Housing Finance, and Limited Risk Sharing in General Equilibrium.” *Journal of Political Economy*, 125(1): 140–223.
- Fernández-Gallardo, Álvaro, Simon Lloyd, and Ed Manuel.** 2023. “The Transmission of Macroprudential Policy in the Tails: Evidence from a Narrative Approach.” Bank of England Staff Working Paper 1027.
- Franta, Michal, and Leonardo Gambacorta.** 2020. “On the Effects of Macroprudential Policies on Growth-at-Risk.” *Economics Letters*, 196: 109501.
- Fuster, Andreas, and Basit Zafar.** 2021. “The Sensitivity of Housing Demand to Financing Conditions: Evidence from a Survey.” *American Economic Journal: Economic Policy*, 13(1): 231–265.
- Gabaix, Xavier, and Ralph SJ Koijen.** 2024. “Granular instrumental variables.” *Journal of Political Economy*, 132(7): 2274–2303.
- Ganong, Peter, and Pascal Noel.** 2020. “Liquidity versus Wealth in Household Debt Obligations: Evidence from Housing Policy in the Great Recession.” *American Economic Review*, 110(10): 3100–3138.
- Ganong, Peter, and Pascal Noel.** 2023. “Why Do Borrowers Default on Mortgages?” *Quarterly Journal of Economics*, 138(2): 1001–1065.
- Gatt, William.** 2024. “Loan-to-Value Limits as a Macroprudential Policy Tool: Developments in Theory and Practice.” *Journal of Economic Surveys*, 38(1): 232–267.
- Gertler, Mark, and Simon Gilchrist.** 2019. “The Channels of Financial Distress During the Great Recession: Some Evidence on the Aggregate Effects.” New York University and NBER Working Paper.
- Gete, Pedro, and Michael Reher.** 2018. “Mortgage Supply and Housing Rents.” *Review of Financial Studies*, 31(12): 4884–4911.
- Greenwald, Daniel.** 2026. “The mortgage credit channel of macroeconomic transmission.” *Journal of Financial Economics*, *accepted*.
- Greenwald, Daniel L, and Adam Guren.** 2025. “Do Credit Conditions Move House Prices?” *American Economic Review*, 115(10): 3559–3596.
- Greenwood, Robin, Samuel G. Hanson, Andrei Shleifer, and Jakob Ahm Sørensen.** 2022. “Predictable Financial Crises.” *Journal of Finance*, 77(2): 863–921.
- Grodecka, Anna.** 2020. “On the effectiveness of loan-to-value regulation in a multiconstraint framework.” *Journal of Money, Credit and Banking*, 52(5): 1231–1270.
- Guren, Adam.** 2026. “Reassessing the 2000s Housing Cycle After Two Decades of Research.” National Bureau of Economic Research Working Paper 35863.

- Han, Lu, Chandler Lutz, Benjamin Sand, and Derek Stacey.** 2021. “The Effects of a Targeted Financial Constraint on the Housing Market.” *Review of Financial Studies*, 34(8): 3742–3788.
- He, Zhiguo, and Péter Kondor.** 2016. “Inefficient investment waves.” *Econometrica*, 84(2): 735–780.
- Higgins, Brian.** 2026. “Differential Effects of LTV and LTI Limits on Mortgage Choices.” Available at SSRN 6534382.
- Huber, Kilian.** 2018. “Disentangling the Effects of a Banking Crisis: Evidence from German Firms and Counties.” *American Economic Review*, 108(3): 868–898.
- Huber, Kilian.** 2023. “Estimating General Equilibrium Spillovers of Large-Scale Shocks.” *Review of Financial Studies*, 36(4): 1548–1584.
- Jeanne, Olivier, and Anton Korinek.** 2019. “Managing credit booms and busts: A Pigouvian taxation approach.” *Journal of Monetary Economics*, 107: 2–17.
- Johnson, Stephanie.** 2020. “Mortgage Leverage and House Prices.” Working Paper, Rice University.
- Jordà, Òscar, Moritz Schularick, and Alan M. Taylor.** 2013. “When Credit Bites Back.” *Journal of Money, Credit and Banking*, 45(s2): 3–28.
- Jordà, Òscar, Moritz Schularick, and Alan M. Taylor.** 2015. “Leveraged Bubbles.” *Journal of Monetary Economics*, 76: S1–S20.
- Jordà, Òscar, Moritz Schularick, and Alan M Taylor.** 2016. “The Great Mortgaging: Housing Finance, Crises and Business Cycles.” *Economic Policy*, 31(85): 107–152.
- Kabas, Gazi, and Kasper Roszbach.** 2023. “The price of leverage: learning from the effect of LTV constraints on job search and wages.” Available at SSRN 3835232.
- Kaplan, Greg, Giovanni L. Violante, and Justin Weidner.** 2014. “The Wealthy Hand-to-Mouth.” *Brookings Papers on Economic Activity*, 45(1): 77–153.
- Kaplan, Greg, Kurt Mitman, and Giovanni L Violante.** 2020. “The Housing Boom and Bust: Model Meets Evidence.” *Journal of Political Economy*, 128(9): 3285–3345.
- Karapetyan, Artashes, Jens Soerlie Kvaerner, and Maximilian Rohrer.** 2024. “Inefficient regulation: Mortgages versus total credit.” *Review of Finance*, 28(1): 311–351.
- Kelly, Robert, Fergal McCann, and Conor O’Toole.** 2018. “Credit conditions, macroprudential policy and house prices.” *Journal of Housing Economics*, 41: 153–167.
- Kelly, Robert, Terence O’Malley, and Conor O’Toole.** 2015. “Designing Macro-Prudential Policy in Mortgage Lending: Do First Time Buyers Default Less?” Central Bank of Ireland Research Technical Paper 02/RT/15.

- Kinghan, Christina, and Fergal McCann.** 2019. “Lending above macroprudential mortgage limits: The Irish experience since 2015.” Central Bank of Ireland.
- Kinghan, Christina, Yvonne McCarthy, and Conor O’Toole.** 2022. “How Do Macroprudential Loan-to-Value Restrictions Impact First Time Home Buyers? A Quasi-Experimental Approach.” *Journal of Banking & Finance*, 138: 105678.
- Kirti, Divya.** 2018. “Lending Standards and Output Growth.” International Monetary Fund IMF Working Paper 18/23.
- Kiyotaki, Nobuhiro, and John Moore.** 1997. “Credit Cycles.” *Journal of Political Economy*, 105(2): 211–248.
- Korinek, Anton, and Alp Simsek.** 2016. “Liquidity Trap and Excessive Leverage.” *American Economic Review*, 106(3): 699–738.
- Krishnamurthy, Arvind, and Tyler Muir.** 2025. “How Credit Cycles across a Financial Crisis.” *Journal of Finance*, 80(3): 1339–1378.
- Lamont, Owen, and Jeremy C. Stein.** 1999. “Leverage and House-Price Dynamics in U.S. Cities.” *RAND Journal of Economics*, 30(3): 498–514.
- Laufer, Steven, and Nitzan Tzur-Ilan.** 2021. “The Effect of LTV-Based Risk Weights on House Prices: Evidence from an Israeli Macroprudential Policy.” *Journal of Urban Economics*, 124: 103349.
- Lorenzoni, Guido.** 2008. “Inefficient credit booms.” *The Review of Economic Studies*, 75(3): 809–833.
- Loutskina, Elena, and Philip E Strahan.** 2015. “Financial integration, housing, and economic volatility.” *Journal of Financial Economics*, 115(1): 25–41.
- Malovaná, Simona, Martin Hodula, Zuzana Gric, and Josef Bajžík.** 2025. “Borrower-based macroprudential measures and credit growth: How biased is the existing literature?” *Journal of Economic Surveys*, 39(1): 66–102.
- Matthes, Christian, Naoya Nagasaka, and Felipe Schwartzman.** 2025. “Estimating the Missing Intercept.” *Working Paper*.
- McCann, Fergal, and Conor O’Toole.** 2019. “Cross-Border Macroprudential Policy Spillovers and Bank Risk-Taking.” *International Journal of Central Banking*, 15(4).
- Mian, Atif, Amir Sufi, and Emil Verner.** 2017. “Household Debt and Business Cycles Worldwide.” *Quarterly Journal of Economics*, 132(4): 1755–1817.
- Mian, Atif, and Amir Sufi.** 2009. “The consequences of mortgage credit expansion: Evidence from the US mortgage default crisis.” *The Quarterly journal of economics*, 124(4): 1449–1496.
- Mian, Atif, and Amir Sufi.** 2011. “House prices, home equity-based borrowing, and the us household leverage crisis.” *American economic review*, 101(5): 2132–2156.

- Mian, Atif, and Amir Sufi.** 2014. “What Explains the 2007–2009 Drop in Employment?” *Econometrica*, 82(6): 2197–2223.
- Mian, Atif, Andrés Sarto, and Amir Sufi.** 2026. “Estimating Credit Multipliers.”
- Mian, Atif, Kamalesh Rao, and Amir Sufi.** 2013. “Household Balance Sheets, Consumption, and the Economic Slump.” *Quarterly Journal of Economics*, 128(4): 1687–1726.
- Milgrom, Paul, and Ilya Segal.** 2002. “Envelope Theorems for Arbitrary Choice Sets.” *Econometrica*, 70(2): 583–601.
- Moretti, Laura, and Luca Riva.** 2025. “The Impact of Borrower-Based Measures: An International Comparison.” *Central Bank of Ireland Research Paper Series*.
- Müller, Karsten, and Emil Verner.** 2024. “Credit Allocation and Macroeconomic Fluctuations.” *Review of Economic Studies*, 91(6): 3645–3676.
- Nielsen, Kasper Meisner.** 2019. “Loan-to-Value Limits and House Prices.” Working paper, Hong Kong University of Science and Technology.
- Ortalo-Magné, François, and Sven Rady.** 2006. “Housing Market Dynamics: On the Contribution of Income Shocks and Credit Constraints.” *Review of Economic Studies*, 73(2): 459–485.
- Peydró, José-Luis, Francesc Rodriguez-Tous, Jagdish Tripathy, and Arzu Uluc.** 2024. “Macroprudential Policy, Mortgage Cycles, and Distributional Effects: Evidence from the United Kingdom.” *Review of Financial Studies*, 37(3): 727–760.
- Peydró, Jose-Luis, Francesc Rodriguez-Tous, Jagdish Tripathy, and Arzu Uluc.** 2026. “Distributional consequences of borrower-based macroprudential tools.” In *Research Handbook of Macroprudential Policy*. 220–240. Edward Elgar Publishing.
- Richter, Björn, Moritz Schularick, and Ilhyock Shim.** 2019. “The Costs of Macroprudential Policy.” *Journal of International Economics*, 118: 263–282.
- Romer, Christina D., and David H. Romer.** 2017. “New Evidence on the Aftermath of Financial Crises in Advanced Countries.” *American Economic Review*, 107(10): 3072–3118.
- Sarto, Andrés.** 2025a. “The Channels of Amplification: Dissecting the Credit Boom that Led to the Global Financial Crisis.” *Working Paper*. Revise and resubmit, *Review of Economic Studies*.
- Sarto, Andrés.** 2025b. “Recovering Macro Elasticities from Regional Data.” *Journal of Political Economy Macroeconomics*. Accepted.
- Schularick, Moritz, and Alan M Taylor.** 2012. “Credit Booms Gone Bust: Monetary Policy, Leverage Cycles, and Financial Crises, 1870–2008.” *American Economic Review*, 102(2): 1029–1061.
- Sinai, Todd, and Nicholas S. Souleles.** 2005. “Owner-Occupied Housing as a Hedge against Rent Risk.” *Quarterly Journal of Economics*, 120(2): 763–789.

- Sodini, Paolo, Stijn Van Nieuwerburgh, Roine Vestman, and Ulf von Lilienfeld-Toal.** 2023. “Identifying the Benefits from Homeownership: A Swedish Experiment.” *American Economic Review*, 113(12): 3173–3212.
- Stein, Jeremy C.** 1995. “Prices and trading volume in the housing market: A model with down-payment effects.” *The Quarterly Journal of Economics*, 110(2): 379–406.
- Tracey, Belinda, and Neeltje Van Horen.** 2025. *Credit constraints and the redistribution of housing wealth*. Centre for Economic Policy Research.
- Tzur-Ilan, Nitzan.** 2023. “Adjusting to Macroprudential Policies: Loan-to-Value Limits and Housing Choice.” *Review of Financial Studies*, 36(10): 3999–4044.
- Van Bakkum, Sjoerd, Marc Gabarro, Rustom M Irani, and José-Luis Peydró.** 2024. “The Real Effects of Borrower-Based Macroprudential Policy: Evidence from Administrative Household-Level Data.” *Journal of Monetary Economics*, 147: 103574.
- Verner, Emil, and Gyözö Gyöngyösi.** 2020. “Household Debt Revaluation and the Real Economy: Evidence from a Foreign Currency Debt Crisis.” *American Economic Review*, 110(9): 2667–2702.
- Wold, Ella Getz, Knut Are Aastveit, Eirik Eylands Brandsaas, Ragnar Enger Juelsrud, and Gisle James Natvik.** 2024. *The housing channel of intergenerational wealth persistence*. Working Paper.
- Wolf, Christian K.** 2023. “The missing intercept: A demand equivalence approach.” *American Economic Review*, 113(8): 2232–2269.

A Data and coding behind Figure 1(b)

Panel (b) of Figure 1 is constructed from the 2017 Macroprudential Policies Evaluation Database (MaPPED) from Budnik and Kleibl (2018). MaPPED documents the life cycle of macroprudential and closely related policy instruments in all EU member states and the United Kingdom, recording for each policy action its instrument category and subcategory, the market segment targeted, the type of action (activation of a new tool, change in level, change in scope, deactivation), whether it tightened or loosened the stance, its main character (macroprudential, microprudential, or other), and a multi-select field, “Objective of the measure,” in which the authority’s stated motivation is recorded. The objectives field is populated from national regulatory documents by the country experts who compiled the database.

We keep policy actions whose main character includes “Macroprudential” and whose category is “Lending standards restrictions,” which is MaPPED’s label for borrower-based instruments, and then restrict to actions whose market segment includes residential real estate. This yields 113 actions in 18 countries. The sample comprises 49 loan-to-value limits, 28 debt-service-to-income limits, 12 maturity and amortization restrictions, 5 other income requirements for loan eligibility, 2 debt-to-income limits, 1 loan-to-income limit, and 16 other restrictions on lending standards. Of these, 89 are tightenings and 58 are activations of a new tool. Ninety-seven are classified as purely macroprudential, with the remainder carrying a joint macroprudential and microprudential (or other) character. Announcement years, where recorded, range from 1982 to 2017, with 71 of the 91 dated actions falling in 2010 or later. The unit of observation is the policy action rather than the country-instrument, so countries that adjusted a given limit repeatedly contribute several observations.

MaPPED’s objectives field is multi-select and unranked. A measure may list several objectives, but the database does not identify the primary one. We therefore report the share of measures citing each objective. The mapping in Figure 1(b) into four broad categories plus a residual is as follows. *Credit and household leverage* collects “General credit growth,” “Sectoral credit growth,” and “Household leverage.” *House prices* corresponds to “Developments in asset prices.” *Financial stability / resilience* collects “Other addressing the resilience of the financial system,” “Currency risks,” “Interconnectedness / contagion risks,” “Market liquidity,” and “Exposure concentration.” *Bank sector* collects “Bank leverage,” “Maturity mismatches on banks’ balance sheets,” and “Bank-level oversight (microprudential).” *Other* collects “Level playing field / competition,” “Harmonisation,” and “Other.” We count a measure in a category if it cites any objective belonging to it, so categories overlap and shares do not sum to one.

We place “Currency risks” under financial stability rather than credit, on the grounds that the foreign-currency mortgage measures in Central and Eastern Europe that account for most of these cases were motivated by household exposure to exchange-rate shocks rather than by the volume of borrowing as such. Moreover, we place “Bank-level oversight (microprudential)” under the bank sector. Neither affects the ranking in Figure 1(b).

B Derivation of the organizing equation

This appendix derives equation (2) and states the assumptions behind it. The framework starts with optimizing households and adds a reduced-form external cost of financial vulnerability of the kind studied in the normative macroprudential literature (Lorenzoni, 2008; Bianchi, 2011; Farhi and Werning, 2016; Korinek and Simsek, 2016; Dávila and Korinek, 2018). The purpose is not to calculate welfare, but to show how the objects discussed in the main text enter a first-order welfare expression.

The notation and sign convention are the same as in the main text. A higher τ permits more borrowing, so a tightening has $d\tau < 0$. Statutory capacity is κ_i , effective capacity is $\tilde{\kappa}_i$, and implementation pass-through ϵ_i is defined in equation (1). We use η_i for the Lagrange multiplier on the borrowing constraint in utility units and λ_i for the same object in money units.

B.1 Environment

Households and markets. There is a unit continuum of households indexed by i . Household i chooses tenure (own or rent), a consumption plan, housing h_i (quantity, quality, and location), mortgage debt b_i ($b_i = 0$ for renters), liquid saving, and other financing such as unsecured credit or family transfers. We collect these choices in the vector a_i , and preferences are $U_i(a_i; Z)$, where Z is defined below.

Households take as given a vector of market outcomes $x = (x_1, \dots, x_J)$, which includes the house price P , the rent R , mortgage spreads and fees s , and lender underwriting standards beyond the statutory rule. The budget constraint is written as $B_i(a_i; x) \leq 0$, where B_i is the present value of expenditure less the present value of resources. For a buyer, B_i includes the purchase price Ph_i , less the mortgage b_i , plus the present value of debt service. For a renter, it includes the present value of rent. Payments at later dates are valued at the household's own discount factor, so all objects below are measured in units of the numéraire at the date of the policy change.

B.1.1 Borrowing constraints

Household i faces the constraint

$$b_i \leq \tilde{\kappa}_i(\tau, x), \tag{B.1}$$

where effective capacity $\tilde{\kappa}_i$ is statutory capacity κ_i adjusted for how the rule is implemented. The instruments in the main text differ in which variables enter κ_i :

- *LTV limit:* $\kappa_i = \tau Ph_i$, where τ is the maximum loan-to-value ratio. Capacity rises with the house price, $\partial \kappa_i / \partial P = \tau h_i > 0$, which is the collateral constraint of Kiyotaki and Moore (1997).
- *LTI limit:* $\kappa_i = \tau y_i$, where y_i is the income the lender uses. Capacity does not depend

on the house price. A DTI limit has the same form applied to total debt, so mortgage capacity is τy_i less the household's other debt.

- *DSTI limit*: required payments may not exceed a share τ of income. With payments equal to $\alpha(r + s, T) b_i$, where α is the annuity factor for the mortgage rate $r + s$ and maturity T , capacity is $\kappa_i = \tau y_i / \alpha(r + s, T)$. Because α increases with the rate, $\partial \kappa_i / \partial r < 0$ and $\partial \kappa_i / \partial s < 0$.
- *Amortization requirement*: the requirement restricts the repayment path rather than the loan at origination. It gives one constraint per period, and λ_i below is the present value of the money-metric multipliers on these constraints. A household can therefore be constrained by an amortization requirement while having unused capacity at origination, as in the Swedish evidence (Bäckman, Moran and Van Santen, 2026).

Implementation enters through effective capacity. With lender exemptions e_i , for instance allocated under a quota (Kinghan and McCann, 2019), effective capacity is $\tilde{\kappa}_i = \kappa_i + e_i$. If lenders grant more exemptions when the rule is tightened, $\partial e_i / \partial \tau < 0$, and equation (1) gives

$$\epsilon_i = 1 + \frac{\partial e_i / \partial \tau}{\partial \kappa_i / \partial \tau} < 1.$$

Lender underwriting standards enter $\tilde{\kappa}_i$ in the same way, which is why we include them in x .

B.1.2 The household problem

Conditional on owning, household i solves

$$V_i^o(\tau, x, Z) = \max_a U_i(a; Z) \quad \text{s.t.} \quad B_i(a; x) \leq 0 \quad [\mu_i], \quad b(a) \leq \tilde{\kappa}_i(\tau, x) \quad [\eta_i], \quad (\text{B.2})$$

where $\mu_i > 0$ is the marginal utility of the numéraire and $\eta_i \geq 0$ is the utility value of one more unit of borrowing capacity. The value of renting, $V_i^\rho(x, Z)$, is defined in the same way without the mortgage. The household's value is $V_i = \max\{V_i^o, V_i^\rho\}$, and $S_i \equiv V_i^o - V_i^\rho$ is its ownership surplus.

B.1.3 Aggregates and the external cost

The vector Z collects aggregate outcomes associated with financial vulnerability, such as total household credit $\int b_i di$, the distribution of leverage and debt service, the composition of new lending, and house-price growth. These outcomes affect households, for instance through the probability and depth of a financial crisis, but each household takes them as given. The external cost $\Psi(Z)$ is the money-metric value of these effects summed over households. Only its gradient enters the derivation, so we define it directly:

$$\nabla \Psi(Z) \equiv - \int \frac{1}{\mu_i} \frac{\partial V_i}{\partial Z} di. \quad (\text{B.3})$$

As an example, suppose a crisis occurs with probability $\pi(Z)$ and that

$$U_i(a; Z) = (1 - \pi(Z)) U_i^{NC}(a) + \pi(Z) U_i^C(a),$$

where U_i^{NC} and U_i^C are utility in the no-crisis and crisis states. Then $\partial V_i / \partial Z = -\nabla \pi (U_i^{NC} - U_i^C)$ at the household's optimum, and

$$\nabla \Psi(Z) = \nabla \pi(Z) \mathcal{C}, \quad \mathcal{C} \equiv \int \frac{U_i^{NC} - U_i^C}{\mu_i} di \geq 0.$$

The external cost thus equals the change in crisis probability times the money-metric loss in a crisis, summed over households. Households take their own losses in a crisis into account when they choose how much to borrow, given π . What they do not take into account is the effect of their borrowing on π . The evidence that credit-financed booms are followed by deeper recessions (Jordà, Schularick and Taylor, 2013; Cerra and Saxena, 2008) bears on $\mathcal{C}$, and the evidence that credit growth predicts crises (Schularick and Taylor, 2012; Greenwood et al., 2022) bears on $\nabla \pi$. Since $\nabla \pi$ is evaluated at Z , the marginal effect of credit on crisis probability can depend on the other elements of Z . The state dependence documented by Greenwood et al. (2022) and Krishnamurthy and Muir (2025) is consistent with positive cross-partials $\partial^2 \pi / \partial Z_k \partial Z_l$ between credit growth and house-price growth or credit spreads, which is why the crisis map in Section 6.1 sorts tightening episodes on both dimensions.

The market outcomes x and the aggregates Z are determined in equilibrium by housing and rental market clearing, lender behavior, and expectations. We do not solve for this equilibrium. We only require that the equilibrium path $x(\tau)$, $Z(\tau)$ is differentiable, and we treat $\dot{x} \equiv dx/d\tau$ and $\dot{Z} \equiv dZ/d\tau$ as the responses estimated in the empirical literature. These are present values of the response paths, and λ_i is the present value of the multipliers on the borrowing constraint at each date and in each state.

B.1.4 Assumptions

Assumption B.1 (Price taking). *Households and lenders take x and Z as given.*

Assumption B.2 (Envelope theorem). *Conditional on tenure, the envelope theorem applies to problem (B.2) (Milgrom and Segal, 2002). At regulatory thresholds the derivatives are one-sided, and we use the derivative in the direction of a tightening. Households bunching at a threshold are constrained households with $\eta_i > 0$.*

Assumption B.3 (Rational borrowing). *Households maximize their own welfare with correct beliefs. We relax this in Section B.3.1.*

Assumption B.4 (Marginal reform). *The tightening $d\tau < 0$ is small and permanent.*

Assumption B.1 is the source of the policy rationale. Because no household or lender takes into account the effect of its own borrowing on Z , private optimization does not lead to the social

optimum, and the external cost remains in the welfare expression. We abstract from compliance and screening costs and from lender profits other than those captured by spreads.

B.2 Derivation

Step 1: Envelope conditions. Fix tenure at the household's optimum. The Lagrangian of problem (B.2) is

$$\mathcal{L}_i = U_i(a; Z) - \mu_i B_i(a; x) + \eta_i [\tilde{\kappa}_i(\tau, x) - b(a)].$$

By Assumption B.2, the derivatives of the value function with respect to τ , x , and Z equal the partial derivatives of $\mathcal{L}_i$ at the optimum. Changes in the household's own choices have no first-order effect on its value:

$$\frac{\partial V_i}{\partial \tau} = \eta_i \frac{\partial \tilde{\kappa}_i}{\partial \tau} = \eta_i \epsilon_i \frac{\partial \kappa_i}{\partial \tau}, \quad (\text{B.4})$$

$$\frac{\partial V_i}{\partial x_j} = -\mu_i \frac{\partial B_i}{\partial x_j} + \eta_i \frac{\partial \tilde{\kappa}_i}{\partial x_j} = \mu_i m_{ij} + \eta_i \frac{\partial \tilde{\kappa}_i}{\partial x_j}, \quad (\text{B.5})$$

where the second equality in (B.4) uses equation (1), and

$$m_{ij} \equiv -\frac{\partial B_i}{\partial x_j} \quad (\text{B.6})$$

is household i 's net position in market j . For the house price, m_{ij} is net housing sold, negative for buyers and positive for sellers. For the rent, it is net rental supply, negative for renters and positive for landlords. For the mortgage spread, it is negative and proportional to b_i for borrowers and positive for the owners of lenders. The derivative with respect to Z is $\partial V_i / \partial Z = \partial U_i / \partial Z$, which enters equation (B.3).

Step 2: Money metric. We measure welfare in units of the numéraire by weighting each household by $\omega_i \equiv 1/\mu_i$, evaluated before the policy change, so that one unit of the numéraire has the same value whoever receives it. Welfare is

$$W(\tau) = \int \omega_i V_i(\tau, x(\tau), Z(\tau)) \, di, \quad (\text{B.7})$$

and the money-metric shadow value of borrowing capacity is

$$\lambda_i \equiv \frac{\eta_i}{\mu_i} \geq 0. \quad (\text{B.8})$$

Step 3: Aggregate. Differentiating equation (B.7) along the equilibrium path and substituting equations (B.3)–(B.8) gives the organizing equation.

Proposition B.1 (Organizing equation). *Under Assumptions B.1–B.4,*

$$\frac{dW}{d\tau} = \underbrace{\int \lambda_i \epsilon_i \frac{\partial \kappa_i}{\partial \tau} di}_{\text{(I) Direct effect}} + \underbrace{\sum_j \left[\int \left(m_{ij} + \lambda_i \frac{\partial \tilde{\kappa}_i}{\partial x_j} \right) di \right] \dot{x}_j}_{\text{(II) Equilibrium incidence}} - \underbrace{\nabla \Psi(Z) \cdot \dot{Z}}_{\text{(III) Corrective effect}}. \quad (\text{B.9})$$

Proof. By the chain rule, $dW/d\tau = \int \omega_i [\partial V_i / \partial \tau + \sum_j (\partial V_i / \partial x_j) \dot{x}_j + (\partial V_i / \partial Z) \cdot \dot{Z}] di$. Substituting equations (B.4) and (B.5), using $\omega_i \mu_i = 1$ and $\omega_i \eta_i = \lambda_i$, gives terms (I) and (II). The last term equals $-\nabla \Psi \cdot \dot{Z}$ by equation (B.3). $\square$

Multiplying equation (B.9) by $d\tau$ and using $dx_j = \dot{x}_j d\tau$ and $dZ = \dot{Z} d\tau$ gives equation (2) in the main text. Grouping the same terms by whether a household is constrained ($\lambda_i > 0$) or not, instead of by term, gives a decomposition into an effect on constrained households, an effect on unconstrained households, and crisis prevention. The two decompositions add up the same terms in a different order. Ours follows the objects that the different empirical literatures estimate.

B.3 Properties used in the main text

Household adjustments. No behavioral response appears in equation (B.9). When a household responds to a tightening by drawing down liquidity, buying a cheaper home, delaying a purchase, or relying on family support, the change in its welfare is fully captured by the shadow value λ_i times the change in capacity, because the household was already optimizing. The adjustments documented in Section 4 should therefore not be added to term (I) as separate costs. They remain informative, however, as they show that the constraint binds, they help discipline estimates of λ_i , they change the composition of debt that enters Z , and they generate the market responses $\dot{x}$.

Transfers and constraint revaluation in term (II). Each market response enters term (II) through two channels. The first, $\int m_{ij} di$, is a transfer between households. Let n_{ijt} be household i 's net sales in market j at date t and Q_{it} its discount factor for date t , with $Q_{i0} = 1$, so that $m_{ij} = \sum_t Q_{it} n_{ijt}$. Market clearing requires $\int n_{ijt} di = 0$ at each date, so that

$$\int m_{ij} di = \sum_{t \geq 1} \text{Cov}(Q_{it}, n_{ijt}). \quad (\text{B.10})$$

For a price paid once at the date of the policy change, such as the purchase price of a home, the transfer channel sums to zero and affects only who gains and who loses. For payments spread over time, such as rents and mortgage spreads, the sum is zero only if households on the two sides of the market value future payments equally. Constrained households value future payments less: in a two-period model in which household i can borrow at rate r , $Q_{i1} = (1 - \lambda_i)/(1 + r)$. The covariance in equation (B.10) is the distributive pecuniary externality of Dávila and Korinek

(2018), and it is small when the exposed households on the two sides of a market have similar shadow values. The main text treats the first channel as a transfer, which is exact for house prices and an approximation for rents and spreads.

The second channel, $\int \lambda_i \partial \tilde{\kappa}_i / \partial x_j di$, does not net out. Under an LTV limit, $\partial \tilde{\kappa}_i / \partial P = \tau h_i$, so a lower house price reduces capacity for every constrained household. The two channels can work in opposite directions for the same household. For a first-time buyer with $m_{iP} = -h_i$, the net exposure to the house price is $-h_i(1 - \lambda_i \tau) < 0$, so a lower price is still a gain, although a smaller one than the transfer channel alone suggests. For an owner who sells a home of size h_i^0 to buy a home of size h_i , the net exposure is $-(h_i - h_i^0) + \lambda_i \tau h_i$, which is positive when the household trades up only modestly and is constrained, so a lower price is a loss (Stein, 1995). Under LTI and DTI limits, $\partial \tilde{\kappa}_i / \partial P = 0$ and only the transfer channel remains.

Households displaced from ownership. Under free tenure choice and a smooth distribution of ownership surplus, a small tightening moves only households that are close to indifferent between owning and renting ($S_i \approx 0$). Their welfare loss is second order, because the envelope theorem applies to the chosen tenure, while their effect on credit is first order, because each of them removes an entire loan. If instead households with $S_i > 0$ are kept out of ownership by a down-payment requirement, a rejected application, or an exhausted exemption quota, their loss is first order and is not captured by λ_i . Let $n_R(\tau)$ be the mass of such households and $\bar{s}_R$ the average money-metric surplus S_i/μ_i of those who are newly kept out by the tightening. The tightening then adds

$$-\bar{s}_R \frac{dn_R}{d\tau} d\tau < 0 \quad (\text{B.11})$$

to the direct effect. Missing mass in bunching designs and entry responses, such as those in Eerola, Lyytikäinen and Ramboer (2023), measure dn_R only if displaced households are rationed rather than close to indifferent. They are also silent on $\bar{s}_R$.

B.3.1 Internalities

Suppose households overvalue borrowing, for instance because they expect house prices to grow faster than they will or neglect repayment risk. Let households choose as if they maximized $\hat{U}_i = U_i + \mu_i \delta_i b_i$, where δ_i is the money-metric overvaluation of a marginal unit of debt, while welfare is evaluated with U_i . The envelope theorem applies to the problem households actually solve, so terms (I)–(III) are unchanged, with λ_i and m_{ij} evaluated at the households' choices. Since $U_i = \hat{U}_i - \mu_i \delta_i b_i$, welfare picks up one additional term:

$$\frac{dW}{d\tau} = [\text{right-hand side of (B.9)}] - \underbrace{\int \delta_i \frac{db_i}{d\tau} di}_{(\text{IV}) \text{ Internality correction}}, \quad (\text{B.12})$$

where $db_i/d\tau$ is the total change in household i 's borrowing, including the response to market outcomes. A tightening reduces borrowing, so with $\delta_i > 0$ term (IV) times $d\tau$ equals

$-\int \delta_i db_i di > 0$, a benefit. The behavioral response that drops out of the welfare expression under Assumption B.3 now matters, as in the theory of corrective taxation. If overvaluation is concentrated among constrained borrowers, terms (I) and (IV) partly offset for the same households, and the net effect on these households can be positive if δ_i is large. Data linking house-price expectations to leverage choices (Bailey et al., 2019) offer one way to measure δ_i .

B.4 Limits

The organizing equation has four limits. First, it is a first-order expression. Observed changes in LTV and income limits are often several percentage points, and the second-order terms are then not negligible. Larger reforms require a structural model. Second, the envelope argument does not hold when households are rationed out of ownership or borrow more than is good for them. We state both cases as separate terms, equations (B.11) and (B.12), but for down-payment constraints rationing may be common. Third, the equation adds money-metric gains and losses without distributional weights, and the transfer channel nets out exactly only for one-time prices. The main text therefore reports incidence by group, so that other weights can be applied. Fourth, the present-value convention compresses timing. Announcement effects, transition paths, and changes in beliefs are part of $\dot{x}$ and $\dot{Z}$ but are not modeled separately.

C A practitioner’s guide to estimating general equilibrium amplification

This appendix first shows how household-level estimates can be translated into an implied effect on aggregate credit growth, facilitating comparisons between micro and macro estimates. We illustrate the calculation using [DeFusco, Johnson and Mondragon \(2020\)](#). The second part provides an overview of methods for measuring equilibrium amplification and moving closer to an estimate of the aggregate effect. [Berg, Reisinger and Streitz \(2021\)](#) provide a related guide.

C.1 Calculating the implied micro effect

The object of interest is the change in annual aggregate credit growth, in percentage points, implied by estimated household-level responses. The calculation applies the estimated responses to a specified flow of new originations and normalizes forgone lending by the outstanding debt stock:

$$\Delta(\text{credit growth}) \approx \frac{\Delta(\text{originations})}{\text{debt stock}} \times 100.$$

An important distinction is whether the calculation covers the market segment studied or extrapolates its responses to a broader market. The former measures the studied segment’s contribution to aggregate credit growth. The latter is a hypothetical exercise requiring an assumption about the applicability of the estimated responses outside that segment.

The calculation combines the extensive and intensive margins. Let e denote the share of counterfactual originations eliminated by the policy, s the share shifted to smaller loans, and Δ the average proportional loan reduction among shifted borrowers. Both e and s are measured relative to the same counterfactual market, including borrowers who no longer originate a loan. With annual origination volume O and outstanding debt stock B , the implied effect is

$$\Delta g \approx - \underbrace{\left[e + s\Delta \right]}_{\text{Extensive} + \text{intensive margin}} \frac{O}{B} \times 100. \quad (\text{C.1})$$

Because s is measured relative to all counterfactual originations rather than only surviving borrowers, no additional survival adjustment is needed. If the estimated responses instead refer only to borrowers above the regulatory threshold, they must first be weighted by that group’s share of the relevant market.

Equation (C.1) is exact only under the appropriate volume weighting. Bunching estimates typically identify shares of loans rather than shares of lending volume. Applying these shares to dollar originations therefore assumes that the counterfactual loan sizes of eliminated and shifted borrowers are adequately represented by the market average. Otherwise, the calculation requires separate average loan sizes for these groups.

Table C.1: Back-of-the-envelope aggregation of the credit responses in [DeFusco, Johnson and Mondragon \(2020\)](#)

Input or calculation	Value
Share of high-DTI jumbo loans shifted below the threshold	20%
Share of all jumbo purchase loans shifted, s	2.7%
Illustrative loan reduction among shifted borrowers, Δ	4.4%
Share of high-DTI jumbo loans eliminated	15%
Share of all jumbo purchase loans eliminated, e	2%
Full US purchase-mortgage market, O	\$600bn
Outstanding household mortgage stock, B	\$8.17tn
Forgone originations: intensive margin	\$0.71bn
Forgone originations: extensive margin	\$12.00bn
Implied change in annual mortgage credit growth	−0.16 percentage points
Extensive-margin share of forgone originations	94%

Notes. Loan shares are based on the bunching estimates in Table 3 of [DeFusco, Johnson and Mondragon \(2020\)](#). The \$600bn purchase-market volume and the extensive-margin extrapolation of \$12bn are reported in the paper. The intensive-margin calculation applies the paper’s illustrative 4.4 percent loan reduction to the 2.7 percent of loans shifted below the threshold. The mortgage stock is from the Federal Reserve Bank of New York, *Quarterly Report on Household Debt and Credit*, Q4 2014; home equity lines of credit are reported separately. The calculation extrapolates jumbo-market responses to the full purchase market and is therefore a hypothetical full-market effect, not an estimate of the actual aggregate effect of the 2014 rule.

An illustration using [DeFusco, Johnson and Mondragon \(2020\)](#). The authors study the US Ability-to-Repay/Qualified Mortgage rule introduced in 2014. The rule increased the regulatory cost of originating jumbo mortgages with debt-service-to-income ratios above 43 percent, while conforming mortgages were exempt. Their bunching estimates imply that 15 percent of counterfactual high-DTI jumbo loans were eliminated and another 20 percent shifted below the threshold. Expressed relative to the entire counterfactual jumbo purchase market, these shares are approximately 2 percent and 2.7 percent.

Following the authors’ extrapolation, we apply these jumbo-market responses to the full US purchase-mortgage market of \$600 billion. For shifted borrowers, we use the paper’s illustrative 4.4 percent loan reduction. This is a calculation for a representative borrower above the threshold, not an estimate of the average reduction among borrowers who adjusted. The implied reduction in originations is

$$[0.02 + 0.027 \times 0.044] \times \$600\text{bn} = \$12.71\text{bn}.$$

Dividing by the \$8.17 trillion mortgage stock reported by the Federal Reserve Bank of New York for end-2014 gives

$$\Delta g \approx -\frac{12.71}{8,170} \times 100 = -0.156 \text{ percentage points},$$

or approximately −0.16 percentage points. The extensive margin accounts for approximately 94 percent of the total.

Four considerations matter when interpreting this exercise. First, the population to which the

responses are applied must be explicit. Applying jumbo-market responses to all purchase originations assumes that the same proportional responses would occur outside the jumbo segment. Restricting the calculation to jumbo purchase lending would instead give the studied segment's contribution to aggregate credit growth, which is much smaller.

Second, count shares and volume shares need not coincide. High leverage or high debt service relative to income does not, by itself, establish that affected loans are larger than average. Researchers should therefore report the relevant average loan sizes or explain the assumptions used to translate loan counts into lending volumes. The size of the intensive adjustment also needs to be distinguished from the share of borrowers adjusting: bunching identifies the latter more directly than the former.

Third, the debt-stock denominator must match the outcome used in the comparison. Household mortgage debt, total household credit, and total bank credit are different aggregates. A mortgage-stock denominator delivers an implied effect on mortgage credit growth. Comparison with macro estimates for a broader credit aggregate requires recalculating the effect using the corresponding stock.

Fourth, the horizon must be specified. Equation (C.1) measures forgone lending over one year relative to a stated debt stock. Longer-run effects depend on subsequent originations, repayments, and whether excluded borrowers postpone their purchases or remain out of the market. For an exact contribution to annual credit growth, the denominator should be the beginning-of-year stock; the illustration above uses the end-2014 stock as an approximation.

The resulting number is a partial-equilibrium accounting exercise rather than an estimate of the full aggregate policy effect. It does not recover market-wide changes in house prices, lending conditions, or other equilibrium responses that the micro design does not identify. This limitation is related to the missing-intercept problem discussed by [Wolf \(2023\)](#). In the DeFusco illustration, extrapolation beyond the regulated segment introduces an additional assumption about external validity.

Nevertheless, reporting these calculations can make micro estimates easier to compare across studies and with macro benchmarks. Researchers should state the market covered, distinguish the studied segment's contribution from any broader extrapolation, decompose the result into extensive and intensive margins, and report sensitivity to assumptions about loan sizes and the amount by which shifted loans shrink.

C.2 Estimating general equilibrium amplification

To provide a full estimate of the total effect of a policy, researchers need to estimate the total effect, including any general equilibrium amplification usually removed by fixed effects. This section provides an overview of recent methods that try to estimate the total effect of a policy in different ways.

Before we discuss how recent studies have attempted to estimate general equilibrium amplification or spillovers, we need a precise definition of what we are after. The general equilibrium multiplier is the ratio of the total effect to the direct effect, for a stated outcome, at a stated level of aggregation, over a stated horizon. The evidence in Section 5.2.2 shows that this multiplier differs by outcome, that it differs by the state of the market, and that it is endogenous to the policy itself. For example, Acharya et al. (2022) estimate that equilibrium effects accounted for 94 percent of the cross-sectional credit response to borrowers’ distance from the Irish limits during the pre-introduction boom, but only 23 percent of the response to the limits themselves. This gives ratios of total to direct effects of roughly 17 and 1.3. The first number describes the credit and house-price feedback of the boom and the second the policy response, which is imprecisely estimated, and the authors caution that the decline cannot be attributed to the limits alone. In addition, the regional multiplier is different from the national multiplier, as discussed in Section 5.2.3, and will also differ depending on the monetary policy response (Chodorow-Reich, 2019). A claim about “the” multiplier should therefore always say the multiplier of what, where, and when.

Direct estimation of the multiplier. The most immediately feasible approach requires no data beyond what existing micro studies already use. Suppose the outcome of household i in market m depends on its own exposure to the constraint and on an equilibrium object that is a function of average exposure in the market. When spillovers are homogeneous and linear, and own and market-average exposure are unrelated to household-specific trends and to other local shocks, a single regression of the outcome on own exposure and on the leave-out mean of exposure among other units in the same market, with time fixed effects, recovers both objects at once (Huber, 2023):

$$y_{imt} = \alpha_i + \delta_t + \beta (E_i \times \text{Post}_t) + \gamma (\bar{E}_{-i,m} \times \text{Post}_t) + \varepsilon_{imt}, \quad (\text{C.2})$$

where E_i is the household’s pre-policy exposure and $\bar{E}_{-i,m}$ is the mean exposure of other households in market m . The direct effect is β , the spillover is γ , and the ratio of total to direct effects is $(\beta + \gamma)/\beta$. Because δ_t absorbs effects common to all markets, this ratio is a regional rather than an aggregate multiplier. Bäckman and Khorunzhina (2024), studying the introduction of interest-only mortgages in Denmark, estimate within-municipality spillovers from payment-constrained to leverage-constrained households through house prices using a design similar to this.

The equivalent two-regression version estimates the within-market effect under market-time fixed effects and then re-estimates on data collapsed to the market level with time fixed effects only, recovering the multiplier as the ratio (Mian, Sarto and Sufi, 2026). Acharya et al. (2022) implement this comparison for the Irish limits. The two procedures are closely related, although they coincide only under equal market weighting and homogeneous effects. The one-regression version of Huber (2023) delivers a standard error on the spillover directly, whereas inference on the multiplier itself requires the joint uncertainty of the total and direct estimates.

In practice the design requires three construction choices. The market definition should correspond to where the equilibrium object lives, which for housing spillovers means a local housing market such as a county, municipality, or commuting zone. The exposure measure should be defined before the policy, from the pre-reform distance to the limit or a prediction of violation based on pre-reform characteristics. Finally, the specification should be estimated on the same outcome, instrument, and horizon as the partial-equilibrium estimate to allow for the comparison.

Diagnostics. [Huber \(2023\)](#) shows that the leave-out-mean regression is biased when there are omitted spillover types, nonlinear direct effects, or measurement error. The decomposition is valid when spillovers are homogeneous and thus affect treated and untreated units alike. In that case, market-time fixed effects absorb the spillovers for the individual unit, and removing the market-time fixed effect estimates the effect with the spillovers included. However, this is a hard assumption to fulfill, especially for mortgage constraints. [Berg, Reisinger and Streitz \(2021\)](#) show that homogeneity fails when the treatment acts as a binding constraint. Imagine, for example, a policy that loosens the LTV constraint, which creates spillovers to the untreated units through increases in house prices. Treated households at the LTV constraint will not be able to respond to rising house prices in the same way as the untreated households, as they are bound by the collateral constraint. More generally, a household at its borrowing limit cannot respond fully to a market-level spillover, which the unconstrained neighbors can. In this case, [Berg, Reisinger and Streitz \(2021\)](#) show that fixed effects can amplify rather than remove the bias, although this is not always the case. [Berg, Reisinger and Streitz \(2021\)](#) recommend estimating the outcome as a function of the share of exposed households in the market separately for exposed and unexposed households, without market fixed effects, and plotting the two fitted lines together with the market average.

[Huber \(2023\)](#) adds a complementary test: the estimated spillover should be smaller for units less exposed to a hypothesized channel. A spillover that is equally large for less exposed units is likely to be mechanical. Under an LTI limit, for example, the collateral channel is absent, so the house-price spillover on credit should be weaker for LTI-constrained than for LTV-constrained households. Huber shows that omitted spillover types, nonlinear direct effects, and measurement error in exposure can all produce such mechanical bias, and that additional leave-out means for other channels, flexible functional forms, and an instrument for own exposure that is uncorrelated with the exposure of others can address it. The direction of the resulting bias in the multiplier depends on which group the spillover affects most. If lower prices reduce borrowing more among comparison households than among exposed households already at the limit, the within-market gap understates the direct effect and the multiplier is overstated. If the spillover falls mainly on exposed households, as with the collateral feedback under an LTV limit, or if lower prices draw new buyers into the comparison group, or if lenders reallocate credit toward the comparison group, as the Irish evidence documents, the gap overstates the direct effect and the multiplier is understated.

Interpreting the output. The interpretation of the results depends on several factors that researchers ought to be aware of. First, when several spillover channels operate at once, a likely case when it comes to mortgage constraints, a single leave-out mean does not isolate each type of spillover, and cross-market channels contaminate the comparison across markets as well as within them. For example, mortgage constraints can plausibly generate spillovers through local housing demand, lender balance sheets (Loutskina and Strahan, 2015), and the rental market (Castellanos, Hannon and Paz Pardo, 2024).

Second, idiosyncratic, within-market measurement error in exposure attenuates the within-market coefficient more than the market-level one, because aggregation averages it out, and therefore inflates the ratio of the total to direct effects. Error in the market-level component of exposure biases the ratio in the opposite direction (Mian, Sarto and Sufi, 2026). Predicted-treatment designs guarantee some mis-measurement, since households respond with family transfers, cheaper properties, or different contract terms (Tzur-Ilan, 2023).

Third, the output is a regional multiplier, not the missing intercept. National equilibrium responses, including the policy rate, system-wide credit reallocation, and the regulator’s reaction, sit in the time fixed effects. Mian, Sarto and Sufi (2026) show that regional amplification and aggregate dampening can coexist. A researcher interested in the national multiplier needs to turn to alternative approaches, with their own caveats and difficulties. We turn to these next.

Alternative approaches. Two alternatives do not rely on the within-market comparison and therefore avoid the contamination just described, although each carries assumptions of its own. The aggregate component can be recovered from regional data directly, either by combining a regional VAR with factor analysis (Sarto, 2025b) or by estimating local and aggregate effects jointly in a Bayesian framework that treats micro estimates as priors (Matthes, Nagasaka and Schwartzman, 2025). Where mortgage lending is concentrated among a few lenders, as it is in most countries that have adopted these policies, the idiosyncratic shocks of large lenders can be aggregated into an instrument for aggregate outcomes (Gabaix and Koijen, 2024; Baumeister and Hamilton, 2023). This is one of the few designs available when the policy variation is purely national and there is no cross-section to exploit, although it identifies propagation from a lender shock rather than from the policy itself.

Another approach is to try to estimate a single structural parameter that governs the spillover. For housing, Greenwald and Guren (2025) identify the degree of market segmentation as an important parameter that governs how credit affects house prices and thus generates spillovers. The degree of market segmentation is identified by the ratio of the price-rent response to the homeownership response to an identified credit shock (Greenwald and Guren, 2025). It has been estimated for the United States alone, and estimating it in the countries that supply the micro evidence would discipline the micro-to-macro extrapolation more than any additional household-level estimate.

The source of the amplification. For a researcher interested in identifying the source or mechanism behind the spillover, the mediation approach of [Sarto \(2025a\)](#) addresses that directly. The approach exploits the fact that the housing channel propagates within geographically segmented markets while the intermediary channel propagates over lender networks that span them. Estimating the source requires loan-level data with lender identifiers, which exist in several of the countries that supply the micro evidence, but it has not yet been applied to a policy shock.