Mortgage Borrowing Constraints and the Local Consumption Multiplier

Belinda Tracey and Neeltje van Horen *

November 2025

Abstract

Easing mortgage borrowing constraints generates a sizable local consumption multiplier. Exploiting geographic variation in exposure to the UK's Help to Buy program, which lowered the minimum down payment requirement from 10% to 5%, we find that home purchases in average-exposure districts rose by 18.1%. Instrumenting home sales with ex ante exposure, we estimate elasticities of 0.29 for household consumption and 0.52 for car sales. Three mechanisms drive the response: new home buyers each spend an additional £3,200, their demand stimulates local employment and income, and in tight markets, house prices rise.

JEL classification: E21, E44, G21, G51, R21

Keywords: Mortgages, down payment, household consumption, local multiplier, Help-to-Buy program

---

*Belinda Tracey is at the Bank of England (belinda.tracey@bankofengland.co.uk), CEPR and CfM and Neeltje van Horen (n.vanhoren@uva.nl) is at the Bank of England, University of Amsterdam and CEPR. We are grateful for comments and suggestions from an anonymous referee, Christian Badarinza, Diana Bonfim, Felipe Carozzi, Matthieu Chavaz, Joao Cocco, Maarten de Ridder, Martijn Droes, Angus Foulis, Benedict Guttman-Kenney, Joe Hazell, Atif Mian, Cameron LePoint, Daniel Paravisini, Ricardo Reis, Vincent Sterk, Paolo Surico, participants at Finance in the Tuscan Hills Workshop (Florence); Third Workshop on Household Finance and Housing (London); AREUEA-National,; AREUEA-ASSA, EEA-ESEM, AREUEA Singapore Virtual Conference, 7th Emerging Scholars in Banking and Finance Conference, the RES Annual Conference, and seminar participants at New York Fed, Villanova WiFI, University of Amsterdam, Bank of England, Humboldt University, King's College London and Amsterdam Business School. We thank the Department for Transport for providing data on regional new car registrations and the Ministry of Housing, Communities and Local Government for providing data on the Help To Buy Equity Loan scheme. This paper was also produced using statistical data from ONS. The use of the ONS statistical data in this paper does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This paper uses research datasets which may not exactly reproduce National Statistics aggregates. The views expressed in this paper are those of the authors, and not necessarily those of the Bank of England or its committees.

1 Introduction

Mortgage borrowing constraints shape both who can buy a home and when (Stein, 1995; Ortalo-Magne and Rady, 2006). When these constraints bind, households are forced to delay home purchases, and the complementary spending associated with moving does not occur (Best and Kleven, 2017; Benmelech et al., 2023; Favara et al., 2025). Relaxing these constraints can therefore stimulate demand by increasing housing transactions: new home purchases trigger spending on home-related goods and services. This spending can, in turn, raise demand for locally supplied goods and services, prompting firms to expand employment and income and thereby amplifying consumption. In tight housing markets, increased transactions may also put upward pressure on prices, reinforcing demand through wealth and collateral effects (Campbell and Cocco, 2007; Mian et al., 2013; Guren et al., 2020). This paper asks: when mortgage policy drives a sharp increase in home purchases, to what extent does that give rise to local consumption effects?

To address this question, we exploit the UK's Help to Buy (HTB) program, a large-scale mortgage market intervention that provides a natural setting to study how easing down payment constraints affects both housing transactions and local demand. Introduced in March 2013, HTB aimed to support households who could afford mortgage payments but struggled to meet high down payment requirements. It enabled eligible buyers to purchase a home with a 5 percent down payment instead of the 10 percent required at the time. By directly targeting the primary borrowing constraint faced by first-time and young buyers (Linneman and Wachter, 1989; Fuster and Zafar, 2021), HTB greatly expanded access to mortgage credit.

To identify the effects of HTB on home purchases and consumption, we exploit geographic variation in *ex ante* exposure to the policy (Mian and Sufi, 2012, Berger et al., 2020). Although HTB was implemented nationwide, its effects varied across regions because relaxing the down payment requirement primarily benefited liquidity-constrained buyers. These households are not randomly spread across the UK, but tend to be concentrated in areas with a more suitable housing supply. Since local housing market characteristics evolve slowly, it is reasonable to assume that districts with historically higher shares of low-down payment buyers were more affected by HTB.

Using administrative mortgage data, we define HTB exposure as the local district share of home purchases made with a low-down payment mortgage during 2005–2007, a period when such mortgages were widely available.[1] This measure strongly correlates with the take-up of low-down payment mortgages after 2013 and accurately picks up time variation. We implement a difference-in-differences design that compares changes in home sales and household consumption between high- and low-exposure districts over the 2009–2018 period. Districts with a historically low share of low-down payment buyers serve as a control group, as HTB unlikely affected home purchases there.

Our identification strategy assumes that, absent the policy, home purchases and consumption would have followed similar trends across high- and low-exposure districts. To address concerns about systematic differences between high- and low-exposure districts, we include district and region-time fixed effects to absorb time-invariant district heterogeneity and regional shocks, and use district-time fixed effects where feasible. We test for parallel

[1] By low–down payment mortgages, we mean loans with a down payment below 10 percent (also referred to as "5 percent down payment" or "95 LTV" mortgages). Most have deposits near 5 percent due to UK mortgage pricing, as discussed in Section 3.

trends in the pre-period and ensure our results hold when using propensity score matching.

We first estimate the impact of easing borrowing constraints on home sales. In districts with average exposure to HTB, annual home purchases were 18.1 percent higher than in districts with minimal exposure, relative to their 2012 levels. The response was concentrated among first-time and younger buyers, consistent with the view that down payment constraints play a central role in housing market fluctuations (Ortalo-Magne and Rady, 2006). Before HTB, home sales evolved similarly across high- and low-exposure districts; a persistent divergence appears only after the policy's introduction. Exploiting within-district variation, we show that the increase was driven primarily by purchases with 5 percent down payments, in line with HTB driving the housing response. Results are robust to excluding London, controlling for concurrent policy interventions, and applying propensity score matching, and are not explained by migration.

We next examine household consumption. Using administrative data on new car registrations, we find that car sales rose significantly more in high-exposure districts following HTB's introduction. As with home purchases, pre-trends in car purchases were parallel across exposure groups, with a persistent divergence emerging after 2013. The estimates are robust to excluding London and to matching on observables using propensity scores. In districts with average HTB exposure, car sales rose by 20.7 percent.

Evidence from the Living Cost and Food Survey (LCFS) further confirms a broad-based consumption response. To account for the repeated cross-section structure of the data, we follow Browning et al. (1985) and Deaton (1985) and construct a pseudo-panel by grouping households into synthetic cohorts, defined by the birth year of the household head and HTB exposure region. We control for changes in cohort-level demographics. Between 2013 and

2018, annual household consumption increased by 6.3 percent in districts with average HTB exposure, with gains concentrated in non-durables and services.

We then quantify the local consumption multiplier: the elasticity of consumption with respect to home sales induced by HTB. Specifically, we relate cumulative growth in local consumption from 2013 to 2018 to cumulative growth in home sales over the same period, instrumenting home sales with our HTB exposure measure in a local projection framework (Jorda, 2005). The estimates show that a 1 percent cumulative increase in home sales raises cumulative car sales by 0.52 percent and total household consumption by 0.29 percent. For a district with average HTB exposure (108 percent more home sales), this implies 57 percent cumulative increase in car sales and 31 percent cumulative increase in consumption over 2013–2018, indicating strong, persistent local responses to easing borrowing constraints.

In the last part of the paper we turn to the mechanisms behind the local consumption multiplier. We begin by testing whether part of the consumption response reflects higher spending by home buyers. Home purchases typically trigger substantial expenditures on furnishings, appliances, and services such as moving, decorating, or renovation (Best and Kleven, 2017; Benmelech et al., 2023; Favara et al., 2025). To isolate this direct consumption effect, we construct a novel dataset linking our administrative mortgage data to transaction-level expenditure data from Money Dashboard. We estimate spending dynamics around the time of home purchase using an event-study design that tracks monthly consumption of home buyers over the 24-month window surrounding the transaction. To address treatment timing heterogeneity, we apply the imputation estimator of Borusyak et al. (2024), using "not yet treated" renters and "never treated" homeowners as controls. Household and region-month fixed effects absorb compositional differences and region-specific shocks.

We estimate that home buyers increase their spending by £3,200 in the year following purchase, with over half of that increase (£1,800) occurring in the first three months. The largest and most persistent gains are in home-related goods and services, which remain elevated for at least two years. Non-durable consumption rises for up to 18 months, while durables show no significant change. These patterns are unlikely to reflect confounding factors: spending remains flat in the months prior to purchase, and estimates are robust to excluding households experiencing major life events such as childbirth or income shocks. The evidence supports a direct and sustained spending response to home purchases.

This rise in home-buyer spending may, in turn, generate broader local demand spillovers. As new buyers demand more goods and services, local firms may respond by hiring workers, raising wages, or expanding supplier purchases. These adjustments increase income for other households in the area, thereby further stimulating consumption. This amplification mechanism contributes to the indirect component of the local consumption multiplier: spending generated through local demand spillovers beyond the direct consumption response of home buyers.

We test this channel by examining how labor market outcomes evolve in regions more exposed to HTB-induced home sales. Following Mian et al. (2020), we interpret changes in the ratio of non-tradable to tradable employment as reflecting shifts in local household demand. Using district-level data, we compare changes in employment across high- and low-exposure areas in the same difference-in-differences framework. To complement this analysis, we use LCFS data to study household income dynamics. We find that non-tradable employment, the ratio of non-tradable to tradable employment, and household income increased significantly in high-exposure districts. These patterns suggest that household demand spurred gains in local income and non-tradable jobs, magnifying the initial buyer-driven increase in consumption and strengthening the local consumption multiplier.

We also examine whether construction activity or house price growth contributed to the consumption response. Construction activity increased during the HTB period, but the expansion was broad-based and not disproportionately concentrated in high-exposure areas. By contrast, house prices increased more in high-exposure locations: after controlling for local supply elasticity, annual house price growth in the average-exposure district exceeded that in minimum-exposure districts by 2.8 percentage points overall, 1.8 percentage points outside London, and 5.2 percentage points in London. These patterns imply that housing-wealth and collateral channels complemented the buyer-driven demand impulse, with stronger effects in tighter markets.

Taken together, the findings reveal three reinforcing demand-side mechanisms through which easing down payment constraints raises consumption. First, new buyers increase spending in the year after purchase. Second, house price gains, especially in supply-constrained markets, bolster spending through wealth and collateral effects. Third, local demand spillovers, operating via increases in non-tradable employment and household income, propagate the initial consumption response beyond the buyers themselves. Quantifying the local consumption multiplier, we show that not only refinancing and equity-withdrawal policies affect consumption; policies that directly raise housing transactions do as well.

Our paper contributes to the literature on mortgage credit policy and household spending. Prior research examines interventions that alter the terms of existing mortgages or unlock home equity, via refinancing, renegotiation, or equity withdrawal, and shows that these can affect consumption (e.g., Agarwal et al., 2017; Agarwal et al., 2023; DiMaggio et al., 2017; Defusco, 2018). Recent work studies the impact of LTV regulation on consumption smoothing and reaches mixed conclusions: Aastveit et al. (2022) find that LTV tightening reduces smoothing in Norway, while Van Bekkum et al. (2024) find the opposite in the Netherlands.

We instead study a relaxation of the down payment constraint that raises home purchases and quantify the resulting local consumption multiplier. This links our paper to the local multiplier literature, which typically examines fiscal transfers (e.g., Nakamura and Steinsson, 2014; Suárez Serrato and Wingender, 2016; Chodorow-Reich, 2019; Auerbach et al., 2020; Pennings, 2021; Beraja et al., 2019). We show that mortgage-market interventions can activate similar propagation mechanisms, revealing a new type of policy shock capable of generating local multipliers.

We further add to the literature on spending around home purchases (e.g., Engelhardt, 1996; Best and Kleven, 2017; Benmelech et al., 2023; Favara et al., 2025) by showing that the buyer response is accompanied by broader local demand effects: higher transactions raise demand for locally produced services and are followed by increases in non-tradable employment and income, consistent with labor-market propagation. Together with the presence of a housing-wealth channel in supply-constrained markets (e.g., Campbell and Cocco, 2007; Mian et al., 2013; Aladangady, 2017; Guren et al., 2020; Aruoba et al., 2022), these patterns indicate multiple reinforcing demand-side mechanisms underlying the local consumption multiplier.

Finally, we extend the literature on the UK's Help to Buy program, which has focused on the Equity Loan scheme and housing market outcomes (e.g., Finlay et al., 2016; Carozzi et al., 2024; Benetton et al., 2022; Tracey and Van Horen, 2025). Our analysis covers the full program and moves beyond housing market responses to document broader macroeconomic spillovers.

The remainder of the paper is structured as follows. The next section provides the policy background. Section 3 describes the data and Section 4 introduces the empirical strategy. Section 5 reports the results on the effects of HTB on the housing market and consumption and Section 6 explores the mechanisms driving the consumption response. Section 7 concludes.

2 Help-to-Buy Program

The UK's Help-to-Buy (HTB) program, announced in March 2013, was designed to ease borrowing constraints in the wake of the global financial crisis. Its stated aim was to help households that could afford monthly mortgage payments but struggled to meet large down payment requirements, a key barrier to homeownership in the UK. As then–Chancellor George Osborne put it in his 2013 Budget speech, "for anyone who can afford a mortgage but can't afford a big down payment, our [HTB] Mortgage Guarantee will help you buy your own home" (Chancellor of the Exchequer, 2013). At the time, most lenders required a minimum down payment of 10 percent, making market entry especially difficult for young and first-time buyers. HTB addressed this directly by enabling purchases with as little as a 5 percent down payment.

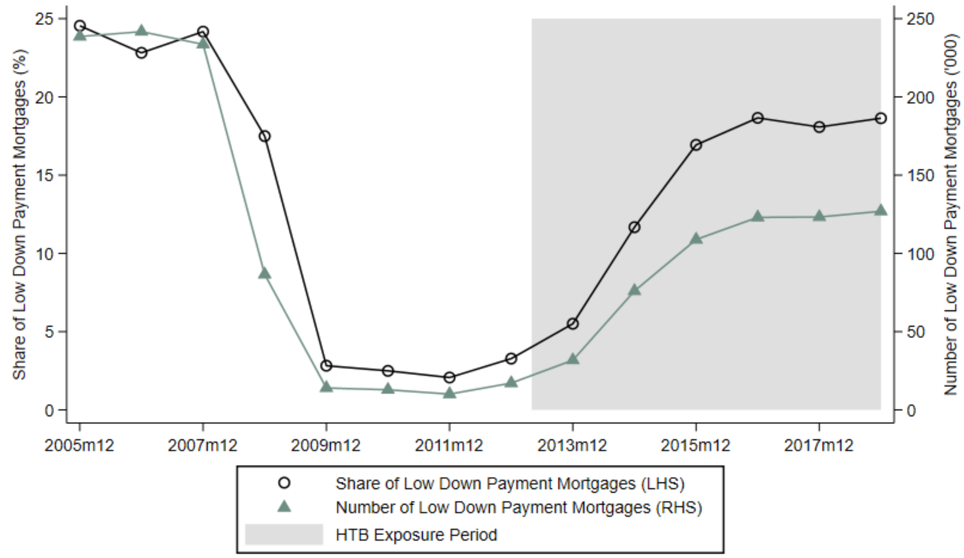
HTB included two schemes that shared a 5 percent down payment requirement but differed in design and scope. The Equity Loan (EL) scheme, in place from April 2013 to December 2020, targeted first-time buyers and home movers purchasing new-build properties priced under £600,000 (£300,000 in Wales). Borrowers received a government equity loan of up to 20 percent of the property value (40 percent in London from 2016), with the remainder financed by a standard mortgage. The equity loan was interest-free for the first five years. Mortgages under the scheme were subject to strict criteria, including a maximum loan-to-income (LTI) ratio of 4.5 and a capital repayment requirement.[2] Unlike the MG scheme, the EL scheme allowed borrowers to contribute more than 5 percent to the down payment, and a minority of participants chose to do so (Benetton et al., 2022).

The Mortgage Guarantee (MG) scheme, in effect from October 2013 to December 2016, applied to all properties priced below £600,000 and was not restricted to new-builds. Under MG, the government guaranteed 20 percent of the property's value to lenders, reducing their effective exposure to 75 percent LTV. This lowered lender risk and encouraged them to reintroduce low-down payment mortgages. Unlike the EL scheme, the MG scheme did not involve direct financial assistance to borrowers and was discontinued in 2016 once the market for low-down payment mortgages had recovered (Carney, 2016). Buy-to-let and second home mortgages were excluded from both the EL and MG schemes. Table A.2 in the Online Appendix further summarizes the two schemes and their requirements.

HTB's impact on the housing market appears immediate and sizable. As Figure 1 shows, the share and volume of low-down payment mortgages, virtually absent from 2009 to 2012, began rising in 2013 with the program's launch and stabilized after 2016, indicating a normalization of access. This is unsurprising: by reducing the minimum down payment from 10

[2]The EL scheme also loosened the income constraint, since the equity loan did not count toward the LTI ratio, allowing buyers to purchase more expensive homes (Benetton et al., 2022; Finlay et al., 2016). However, Finlay et al. (2016) document that the down payment—not income—was the binding constraint for most prospective buyers. The Online Appendix Table A.1 shows little difference in the average income between EL and MG borrowers.

Figure 1: Number and Share of Low-Down Payment Mortgages



Notes : The figure shows the share and number of low-down payment mortgages before and during the HTB exposure period. The shaded area indicates the period that HTB is in effect.

to 5 percent, HTB doubled borrowers' purchasing power through the LTV's built-in leverage effect and directly alleviated the main constraint facing young and first-time buyers: accumulating a deposit (Linneman and Wachter, 1989; Fuster and Zafar, 2021). In line with evidence that post-crisis credit tightening depressed transactions and disproportionately excluded first-time buyers (e.g. Van Bekkum et al., 2024; Defusco et al., 2020; Carozzi, 2020), HTB appears to have reversed these trends.

While these aggregate patterns point to a significant impact, evaluating how the easing of borrowing constraints affected home purchases and household consumption requires comparison with a counterfactual. To do this, we exploit geographic variation in exposure to the program across UK districts. The next section describes the data underlying our analysis, before turning to the empirical methodology in Section 4.

3 Data

Our analysis draws on several data sources. For home sales, we focus on transactions financed with a mortgage, which covers the vast majority of UK home purchases. Our source is the Financial Conduct Authority's Product Sales Database (PSD), which records all regulated mortgages issued in the UK since April 2005. The PSD provides detailed information on each mortgage, including the lender, issuance date, loan size, property value, down payment, and postcode, alongside borrower characteristics such as buyer type (first-time buyer or home mover), age, income, and employment status. We exclude refinancing and buy-to-let mortgages.[3]

We define low–down payment mortgages as those with a down payment below 10 percent. These are often referred to as 95 LTV or 5 percent down payment mortgages, since most cluster near that threshold due to product pricing. In the UK, mortgage interest rates change sharply at discrete down payment thresholds (5, 10, 15, . . . , 40 percent). Borrowers just above a threshold face higher rates (see Online Appendix Figure A.1), creating strong incentives to "bunch" down payments at these points (Best et al., 2020; Robles-Garcia, 2019). Low-down payment mortgages include nearly all MG mortgages and a subset of EL mortgages, as some EL borrowers opted for higher down payments. We identify low–down payment EL mortgages by matching PSD records with data from the UK Department for Levelling Up, Housing and Communities, following Benetton et al. (2022).[4]

To assess the impact of easing borrowing constraints on household consumption, we

[3]While available for refinancing, very few such mortgages were issued, likely due to the high interest rates charged.

[4]We thank the authors for sharing their programs and data with us, with the permission of the UK Ministry of Housing, Communities and Local Government.

draw on two sources: administrative data on car purchases and household survey data. Car purchases represent a key durable good and are commonly used to study consumption responses to credit shocks (see, e.g., DiMaggio et al., 2017; Mian and Sufi, 2012). We use district-level data on all new car registrations from 2009 onward, provided by the UK Department for Transport.[5] To our knowledge, this is the first study to use such granular UK car sales data.

To capture broader consumption responses, we use the Living Costs and Food Survey (LCFS), the UK's most comprehensive survey on household spending and widely used in the literature (e.g., Campbell and Cocco, 2007; Cloyne et al., 2020). The LCFS surveys approximately 5,000 households annually, providing detailed expenditure data along with household income and demographic variables. We classify weekly household expenditures into three mutually exclusive categories: (i) home-related expenditures, including certain durable goods, non-durable goods and services tied to the home; (ii) non-durable consumption, covering a broad range of everyday goods and services, including semi-durables like clothing and leisure items; and (iii) durable expenditures, including motor vehicles and durable personal or leisure goods not related to the home. Total household consumption equals the sum of these three categories. Following Cloyne et al. (2020), mortgage and rental payments are excluded from our expenditure categories.

As controls, we include additional household-level variables from the LCFS: age of household head, household size, the number of children, and the share of outright owners. We deflate spending categories and income to 2016 prices using the Consumer Price Index including owner occupiers' housing costs (CPIH). We provide more detail about the construction

[5] We re-scale the 2009 data to account for the fact that data collection began in April of that year.

of the spending categories in Section A of the Online Appendix.

To study the mechanisms behind the consumption response, we use various additional data sources. To assess whether households change their spending after a home purchase, we construct a novel monthly panel that links PSD mortgage data to transaction-level spending data from Money Dashboard (MDB), a financial application that provided users a consolidated view of their bank accounts, credit cards, and spending activity. MDB data span January 2013 to December 2018 and provide transaction records across all linked accounts, offering granular expenditure data at daily frequency, free from recall bias. Expenditures are aggregated to the user-month level and classified into the same three categories as used in the LCFS: home-related, non-durable, and durable spending.

We identify new home buyers by merging MDB with the PSD using the postcode, birth year, monthly payment amount, and payment date. This linkage allows us to observe spending dynamics before and after purchase. The final matched dataset consists of 5,172 users, of whom 3,370 purchased a home between January 2013 and December 2018. These users generated over 15 million transactions across the sample window. Additional details on the data construction and variable definitions are provided in Section B of the Online Appendix.

To examine whether HTB-induced home sales triggered local demand spillovers via the labor market, we use two data sources. Employment data come from the Business Structure Database (BSD), which records all UK firms registered for tax purposes and provides information on geographic location and number of employees. We aggregate establishment-level data to the district-year level and classify employment as tradable or non-tradable using the industry mappings in Burstein et al. (2020). Tradable sectors include agriculture, manufacturing, and mining; non-tradable sectors consist primarily of local services. For income, we use household-level data from the LCFS, which reports gross income from both labor and non-labor sources. A detailed description of data construction is provided in Section C of the Online Appendix.

To examine local demand spillovers arising from construction activity or house price growth, we use district-level data on the number of new dwellings for which building work has commenced, sourced from the UK Department for Levelling Up, Housing and Communities. We obtain house price data from the Land Registry UK House Price Index. We utilize the district-level index, which measures the changes in the value of all residential property transactions within a district.

Table 1 presents summary statistics for the main variables used in the analysis. Summary statistics are reported separately for the pre-HTB (2009–2011) and post-HTB periods (2013–2018). Table A.3 in the Online Appendix contains variable definitions and data sources.

4 Empirical Methodology

Our empirical strategy proceeds in two stages. In the first stage, we estimate the reduced-form response of home sales and consumption to HTB exposure using a difference-in-differences framework. In the second stage, we quantify the local consumption multiplier by estimating the elasticity of local consumption with respect to HTB-induced home sales, using an instrumental variable (IV) local projection approach. This setup allows us to trace the effect of credit-induced changes in housing transactions on local consumption.

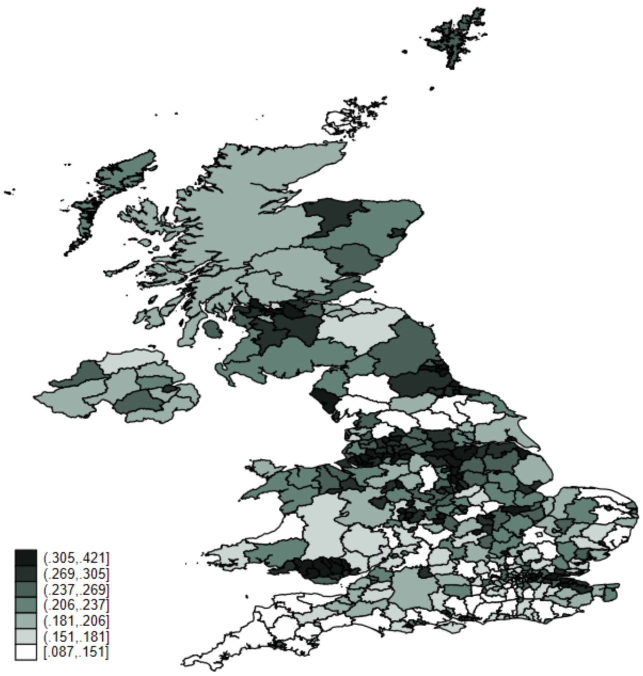
4.1 HTB Exposure and Identification Strategy

We estimate the effect of easing mortgage borrowing constraints on home sales and local consumption by exploiting cross-district variation in *ex ante* exposure to HTB. This approach follows prior quasi-experimental designs that leverage regional heterogeneity in exposure to nationwide policies (e.g. Mian and Sufi, 2012; Berger et al., 2020; Agarwal et al., 2017). Although HTB was implemented uniformly across the UK, its impact varied across locations due to pre-existing differences in local housing market characteristics. We use this variation to compare outcomes between more and less exposed districts, thereby constructing a counterfactual for what would have happened in the absence of the policy.

HTB primarily benefited liquidity-constrained households, typically younger and first-time buyers. These households are not randomly spread across the UK, but tend to cluster in areas with more affordable housing, suitable property types, and attractive local amenities. Because these features change slowly, the historical prevalence of low–down payment borrowing provides a reasonable proxy for the local presence of constrained buyers at the time HTB was introduced. Districts with few such buyers serve as natural control areas, as housing demand in these areas would be largely unaffected by the policy. This interpretation assumes no spillovers from treated to control districts through migration; Section D of the Online Appendix provides evidence that such moves unlikely explain our results.

We measure a district's HTB exposure as the share of low-down payment mortgages originated between 2005 and 2007. During this period, low-down payment mortgages were widely available, and households who wanted such mortgages could typically obtain them. Although these products largely disappeared after the global financial crisis, the underlying

Figure 2: Help-to-Buy Exposure across the United Kingdom



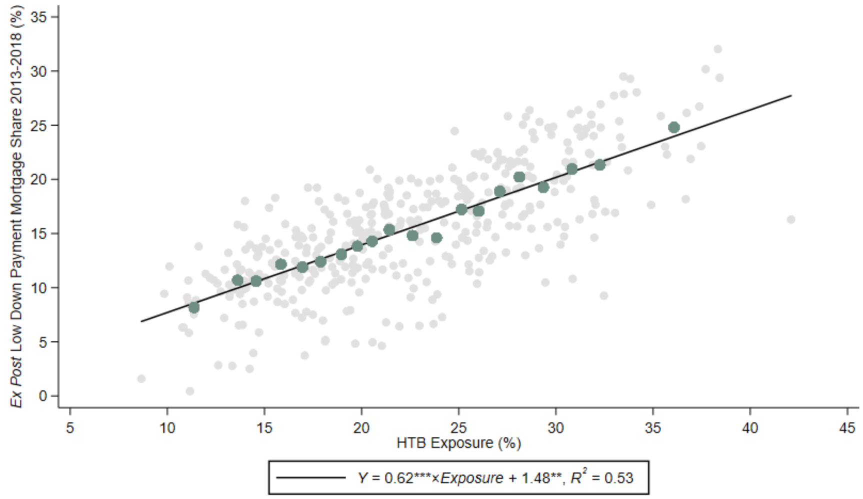
Notes : The figure shows the geographic distribution of HTB exposure across UK districts; darker shading indicates higher exposure.

demand for them likely persisted. As the measure is based on pre-policy borrowing patterns, it is plausibly exogenous to HTB and reflects latent credit constraints at the time the policy restored access to low-down payment mortgages.

Our sample comprises 379 local authority districts (referred to as "districts") across England, Wales and Scotland, covering 97 percent of the UK population and 98 percent of mortgage originations. Northern Ireland is excluded due to data limitations. Apart from Greater London, districts reflect distinct housing and labor markets, comparable to US core-based statistical areas (CBSAs). We also group our districts into the 12 major UK regions (referred to as "regions") using the highest level of the Nomenclature of Territorial Units for Statistics classification in the UK.

Figure 2 plots the geographic distribution of HTB exposure. Exposure varies consider-

Figure 3: Help-to-Buy Exposure and Ex Post Low-Down Payment Mortgages



Notes: The figure plots the relationship between HTB exposure and the share of low–down payment mortgages in 2013–2018 at the district level. Blue dots show a binscatter, where districts are grouped into 20 equal-sized bins based on HTB exposure and the mean of each variable is plotted for each bin; grey dots depict individual districts. The dashed line shows the fitted regression line.

ably across districts, ranging from 8.7 percent to 42.1 percent, with a mean of 22.6 percent. Crucially, exposure does not exhibit strong spatial clustering: all major UK regions include both high- and low-exposure districts. This lack of spatial clustering strengthens identification by allowing us to control for region-specific shocks, such as different macroeconomic and housing market cycles, by using region-time fixed effects, and estimate the impact of the policy change using variation in HTB exposure across districts within the same region.

Figure 3 provides further empirical validation of our exposure measure. Districts with higher pre-crisis shares of low–down payment borrowing experienced greater take-up of such mortgages during 2013–2018. The fitted regression line implies that pre-crisis low–down payment shares explain 53 percent of the cross-sectional variation in post-2013 shares, confirming a strong link between ex ante exposure and ex post take-up. Evidence on the timing of this response and the absence of pre-trends is presented in Section 5.

4.2 Baseline Estimation Framework

To estimate the effect of HTB on housing and consumption outcomes, we implement a difference-in-differences specification of the form:

$$Y_{d,t} = \sum_{s \neq 2012} \mathbb{I}_{t=s} \times \text{Exposure}_d \times \beta_s + \delta_d + \theta_{r,t} + u_{d,t} \tag{1}$$

where $Y_{d,t}$ denotes the outcome of interest in district d and year t, which we winsorize at the 1st and 99th percentiles. Exposure_d is the district's pre-crisis exposure to HTB; δ_d are district fixed effects and $\theta_{r,t}$ region-time fixed effects. Where feasible, we estimate specifications including district-time fixed effects to control for time-varying local shocks. We estimate the model over the period 2009 to 2018, using 2012 as the base year, and bootstrap standard errors, clustering at the district and time level.

We conduct a range of robustness checks but, for ease of exposition, report results from a simplified pre-post version of Equation 1:

$$Y_{d,t} = \beta_1 \text{Pre}_t \times \text{Exposure}_d + \beta_2 \text{Post}_t \times \text{Exposure}_d + \delta_d + \theta_{r,t} + u_{d,t} \tag{2}$$

where Pre_t is a dummy variable equal to 1 for the period 2009 to 2011 and Post_t is a dummy variable equal to 1 for the period 2013 to 2018.

Identification relies on the assumption that, absent the policy, outcomes in high- and low-exposure districts would have followed parallel trends. A potential concern is that HTB exposure correlates with pre-existing differences that could also affect outcomes. Table A.5 in the Online Appendix shows that HTB exposure is positively correlated with the unemployment rate, median income, and the average loan-to-income ratio in 2012, and negatively correlated with average house prices in 2012 and income growth over 2008—2011. While these patterns do not necessarily imply bias, they underscore the need to account for potential confounders. Accordingly, all specifications include a rich set of fixed effects and we verify that the results are robust to alternative controls and specifications. In addition, we test for and confirm parallel pre-trends in key outcomes, and document that the timing of observed changes aligns with the introduction of HTB.

As an additional robustness check, we apply propensity score matching to improve comparability between high- and low-exposure districts. The matching covariates are the significant pre-policy covariates reported in Table A.5 along with regions. This procedure yields 91 high-exposure districts (above-median HTB exposure) matched to 91 low-exposure counterparts (below-median).

4.3 Household Consumption Pseudo-Panel Specification

To complement our administrative evidence, we analyze household consumption using the LCFS. While the survey provides detailed expenditure data, it also presents empirical challenges: households are observed only once, and the annual sample comprises only 5,000 households. To address these limitations, we construct a pseudo-panel following Deaton (1985) and Browning et al. (1985), grouping households into synthetic cohorts defined by birth year and region of HTB exposure.

Drawing on the age of the household head, we assign households into one of eight birth year quintiles that roughly correspond to 10-year birth year cohorts. As the small LCFS sample does not support reliable district-level cohort analysis, we aggregate districts into ten exposure-regions based on the decile distribution of HTB exposure. Each exposure-region comprises districts in the same decile, and its level of exposure is the average of the constituent districts. This structure yields a balanced panel of 80 region–birth year cohorts observed annually over 2009–2018, allowing us to track average consumption across groups with similar fixed characteristics but different exposure to HTB.

For each exposure region-birth year cohort we compute average total household consumption and its three components: home-related expenditures, non-durable consumption, and durable expenditures for each year. Cohort-level controls follow Campbell and Cocco (2007) and include the average of (the log of) age of household head, household size, the number of children and the share of outright owners. These controls help account for compositional differences across cohorts, such as variation in family structure, that could otherwise bias the estimated effects of HTB.

Given the limited number of observations per cohort-year, estimating separate coefficients for each year would yield imprecise and noisy estimates. We therefore adopt a pre/post specification that pools post-HTB years to concentrate statistical power and obtain more precise estimates. Our regression specification is as follows:

$$\begin{aligned} Y_{g,c,t} = \quad & \beta_1 \text{Pre}_t \times \text{Exposure}_g + \beta_2 \text{Post}_t \times \text{Exposure}_g + \boldsymbol{\gamma} \text{Cohort}_{g,c,t} \\ & \gamma_c + \delta_g + \theta_t + u_{g,c,t} \end{aligned} \tag{3}$$

where $Y_{g,c,t}$ denotes total household consumption, or one of its sub-components, for exposure-region g, birth-year cohort c, and year t; Exposure_g is the region's pre-crisis exposure to HTB; $\mathbf{Cohort}_{g,c,t}$ is the vector of time-varying cohort-level controls; Pre_t and Post_t are defined as above. γ_c are birth-year cohort fixed effects, δ_g are exposure-region fixed effects, and θ_t are time fixed effects. Given the small number of region and cohort clusters, standard errors are bootstrapped using a heteroskedasticity-robust estimator.

4.4 Local Projections and the Local Consumption Multiplier

While the difference-in-differences framework provides reduced-form estimates of the effect of HTB on home sales and household consumption, it does not quantify how strongly consumption responds to policy-induced changes in housing activity. To capture this response, we estimate instrumental variable local projections, following Jorda (2005) and Jorda et al. (2015).

Our IV-local projections are similar to recent work that estimates fiscal multipliers from government spending shocks (e.g., Miyamoto et al., 2018; Ramey and Zubairy, 2018). In the first stage, pre-crisis HTB exposure instruments for cumulative growth in home sales, isolating variation that is plausibly exogenous to local consumption dynamics. In the second stage, we regress cumulative growth in consumption on the instrumented cumulative growth in home sales. The resulting coefficient, which we refer to as the *local consumption multiplier*, measures the percentage increase in consumption associated with a 1 percent policy-induced rise in home sales, providing a direct estimate of how credit-easing induced housing activity transmits to local consumption.

We estimate:

$$\sum_{j=0}^{5} (\ln Y_{d,t+j} - \ln Y_{d,t-1}) = \beta \sum_{j=0}^{5} (\ln X_{d,t+j} - \ln X_{d,t-1}) + \psi(L) W_{d,t-1} + \theta_r + u_d \tag{4}$$

where $Y_{d,t}$ is the outcome variable and $X_{d,t}$ denotes the number of home sales in district d

and year t. The cumulative log differences capture total growth in each variable from 2013 to 2018 relative to 2012, summarizing the overall response over the HTB period. Thus, $t = 2013$ in Equation 4. Although the specification can be estimated for multiple horizons, we focus on the cumulative horizon to capture the total effect of the policy. The term $\psi(L)\, W_{d,t-1}$ includes three lags of the first difference of $\Delta \ln Y_{d,t-1}$ to account for serial correlation, and θ_r are region fixed effects. Standard errors are heteroskedasticity-robust.

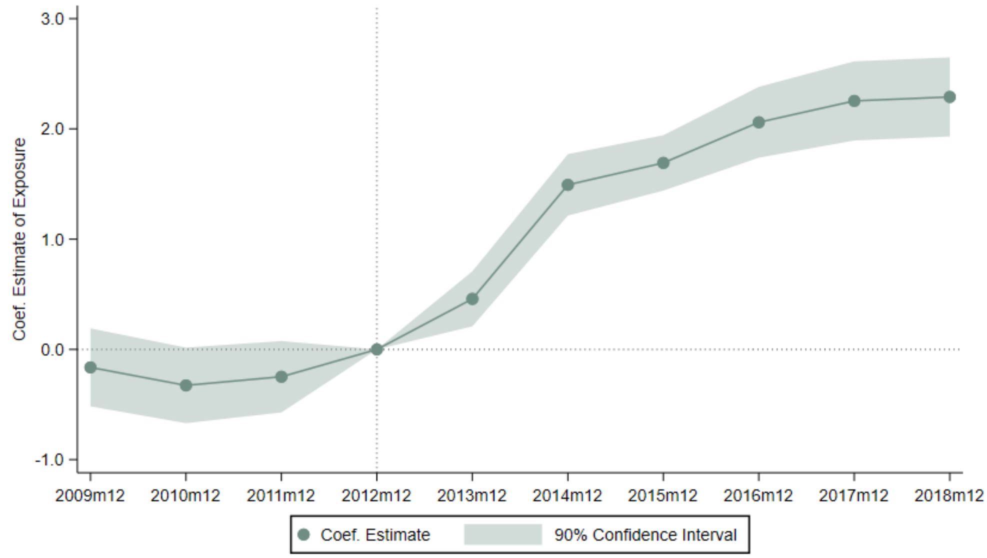
The specification above is applied at the district level when the dependent variable is car sales. When we use total household consumption from the LCFS, the model is estimated at the HTB-exposure-region level rather than by district, to account for the survey's sampling design. In that case, region fixed effects are replaced by birth-cohort fixed effects, and we include the synthetic cohort controls from Equation 3 evaluated at $t - 1$.

We now turn to the results from the empirical specifications described above. The methodological framework for analyzing the underlying mechanisms is introduced separately in Section 6.

5 The Housing Market and Consumption Response

This section documents the impact of easing mortgage borrowing constraints through HTB on household consumption. We first estimate the policy's effect on home sales, showing that HTB significantly increased transactions in more exposed areas. We then assess how this expansion translated into higher consumption and quantify the response relative to the change in home sales by estimating the local consumption multiplier.

Figure 4: The Effect of HTB on Home Sales



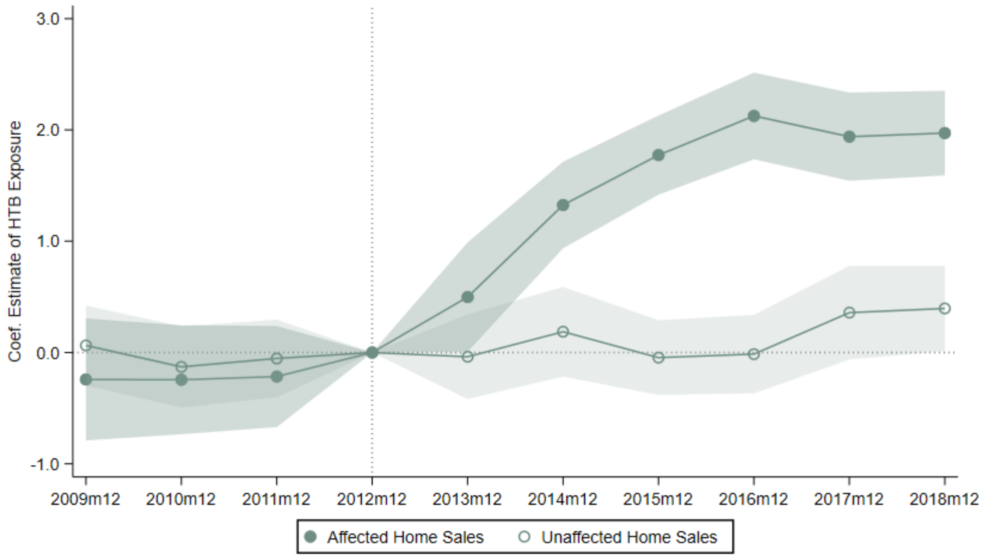
Notes : The figure presents estimates of β_s from Equation 1 for each year, where the outcome variable $Y_{d,t}$ equals the number of home sales in a given district and year. The base year is 2012.

5.1 Home Sales

Figure 4 plots the coefficient estimates β_s, tracing the evolution of home sales in response to HTB across districts with different levels of HTB exposure. Pre-policy estimates are insignificant, indicating parallel trends across high- and low-exposure districts. From 2013 onwards, sales rise more sharply in high-exposure districts, with the divergence accelerating in 2014 as both HTB schemes became fully operational. The gap persists through 2018, stabilizing from 2016 reflecting the recovery in the market for 5 percent down payment mortgages (Carney, 2016). The modest increase in 2013 reflects the lag between initiating and completing a purchase; because the MG scheme was introduced only in late 2013, most of its impact appears from 2014.

The effect is economically meaningful: in 2013, home sales in a district with average HTB exposure were 4.9 percent higher than in a district with minimum exposure, relative

Figure 5: The Effect of HTB on Home Sales By Down Payment



Notes: The figure presents estimates of β_s from Equation 1 for each year, where the outcome variable $Y_{d,i,t}$ equals the number of home sales in a given district, year, and down payment category i. *Affected* home sales have a down payment of 5 or 10 percent, while *Unaffected* home sales have a down payment of 15 percent or more. The base year is 2012. The shaded regions represent 90 percent confidence intervals.

to their home sales in 2012. By 2018, this gap had widened to 24.3 percent, consistent with a sustained credit-induced expansion in housing market activity.

We next assess whether the observed increase in home sales was concentrated in the segment of the mortgage market directly targeted by the policy. If the policy exposure is the driving force behind the observed rise in transactions, the response should be limited to mortgage types affected by HTB. To test this, we exploit the discrete pricing structure in the UK mortgage market around down payment thresholds (Section 3) and divide home sales into two groups: *Affected* homes sales with down payments of 5 or 10 percent, and *Unaffected* home sales with down payments of 15 percent or more.[6] While HTB explicitly enabled purchases with 5 percent down payments, the EL scheme also permitted higher down payments, and minority of participants contributed more than the minimum (Benetton et

[6]As explained in Section 3, 5 percent down payment mortgages have deposits between 5 and 9.9 percent, and 10 percent mortgages between 10 and 14.9 percent, with most bunched at the thresholds.

al., 2022). As a result, both 5 and 10 percent down payment mortgages were plausibly affected by the policy.

To examine this distinction, we re-estimate Equation 1 as a triple-difference specification that includes an interaction between HTB exposure and an indicator for affected mortgages, along with the corresponding two-way interaction terms. Figure 5 plots the resulting coefficient estimates separately for the affected and unaffected groups. In the pre-policy period, estimates for both segments are close to zero, indicating parallel trends. Following the introduction of HTB, affected home sales rose more sharply in high-exposure districts. In contrast, there is no measurable effect on unaffected home sales throughout the post-policy period. This pattern reinforces the interpretation that the increase in housing market activity was driven by the easing of borrowing constraints induced by HTB.

To probe the heterogeneity in the response and assess the robustness of the results, we turn to a triple-difference version of the pre/post model in Equation 2, interacting HTB exposure with both the post-policy period and down payment categories. Table 2 reports the results. As expected, column 1 shows that the response is largest for 5 percent down payment mortgages, the segment most directly affected by HTB. The effect for 10 percent down payments is smaller but still positive, consistent with the broader reach of the EL scheme. In line with the results in Figure 5, we find no significant effect among mortgages with higher down payments.

Columns 2 through 5 show that the results are robust across a range of alternative specifications. Column 2 includes district-time fixed effects to absorb local shocks; the estimated effects remain stable, suggesting the increase in home sales was not driven by unobserved district-level trends. Column 3 excludes Greater London, where housing market dynamics differ markedly due to the high presence of international and buy-to-let investors. The results remain robust, with even larger effect sizes. In column 4, we re-estimate the model on a propensity score matched sample of high- and low-exposure districts. The estimated effect for 5 percent down payments remains statistically significant, though somewhat attenuated, highlighting some sensitivity to sample composition.

We also consider potential confounding from concurrent policies. In particular, the 2014 cap on high LTI lending may have reduced credit supply in districts more exposed to constrained lenders. We adapt the constrained-lender exposure measure from Peydro et al. (2024) to the district level and interact it with the pre- and post-policy indicators.[7] Column 5 shows that accounting for this channel does not affect the main results, consistent with most low-down payment mortgages falling below the LTI threshold.

Finally, we assess potential spillovers from migration. While housing mobility could, in principle, dilute the treatment effect, residential mobility in the UK is limited. About 68 percent of UK moves occur within the same postcode. As districts usually contain multiple postcodes, an even higher percentage occurs within districts (Lomax, 2020). Furthermore, long-distance moves are typically driven by education and employment and thus unrelated to housing affordability (Thomas et al., 2019). A formal analysis of migration patterns in Section D and Table A.6 of the Online Appendix shows that there was no net inflow into high-exposure districts following HTB.

Finally, we examine whether the policy impact was stronger for buyers more likely to face binding liquidity constraints. Prior work highlights that such constraints are particularly

[7] Specifically, in line with Peydro et al. (2024), we first define constrained lenders as those that issued 15 percent or more of their mortgages with an LTI of 4.5 or higher in 2012. We then compute the district-level *LTI Exposure* measure as the proportion of mortgages issued by these lenders in 2012.

relevant for first-time and younger buyers (e.g. Linneman and Wachter, 1989; Engelhardt, 1996; Haurin et al., 1997). Using a triple-difference version of Equation 2, we interact HTB exposure with indicators for first-time buyers and younger buyers (ages 20–39). Table A.7 in the Online Appendix shows that while HTB increased home purchases across all groups, the effect was significantly larger for these two segments. The results are robust to replacing district and time fixed effects with district-time fixed effect (columns 2 and 6), excluding London districts (columns 3 and 7), and become even larger when restricting the sample to propensity score matched districts (columns 4 and 8). These findings support the interpretation that HTB was particularly effective in stimulating home buying among households most affected by down payment constraints.

5.2 The Consumption Response

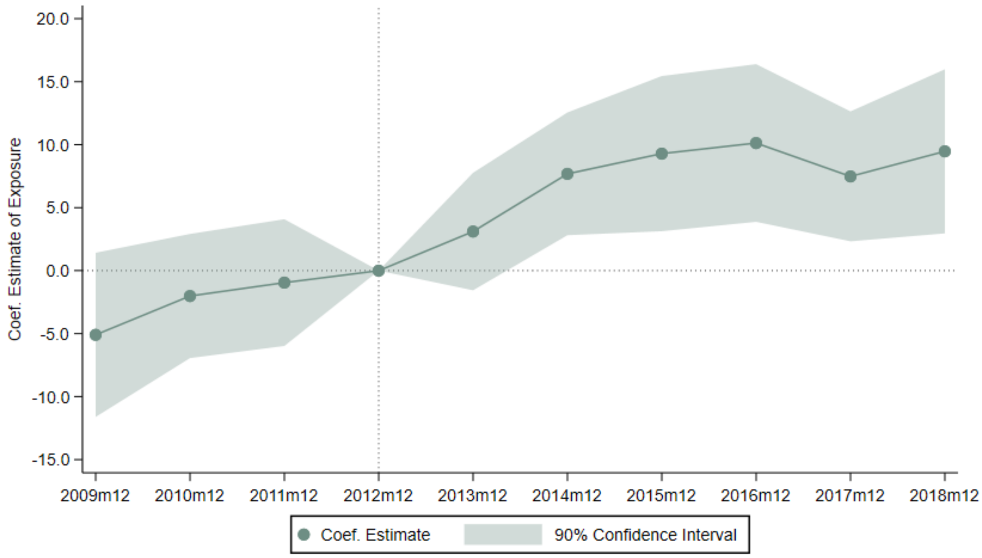
We next examine whether the expansion in home sales was accompanied by changes in local household consumption. If easing borrowing constraints spurred home purchases, and those purchases led to higher spending by buyers or generated local spillovers, we should observe a corresponding increase in consumption in high-exposure areas following the policy.

5.2.1 Car Sales

We begin by examining whether HTB affected new car purchases, a key durable consumption good unrelated to housing. We estimate Equation 1 replacing the outcome variable with the number of new car registrations at the district-year level.

Figure 6 plots the estimated β_s coefficients, capturing the evolution of car sales across districts with different levels of HTB exposure. The estimates show no evidence of pre-trends

Figure 6: The Effect of HTB on Car Sales



Notes : The figure presents estimates of β_s from Equation 1 for each year, where the outcome variable $\mathrm{Y}_{d,t}$ equals the number of new car registrations in a given district and year. The base year is 2012.

and reveal a significant, sustained divergence after 2013, with car sales rising more strongly in high-exposure districts. The timing mirrors the pattern observed for home sales, with the response emerging as both HTB schemes became fully operational.

The effect is economically meaningful. In 2013, car sales in a district with mean HTB exposure were 8.2 percent higher than in a district with minimum exposure, relative to their difference in 2012, although this difference is not statistically significant. By 2014, the first year with a significant effect, car sales were 20.3 percent higher, and by 2018 the gap had widened to 25.0 percent. This pattern is consistent with a persistent increase in local consumption following the expansion in housing market activity.

Table A.8 in the Online Appendix confirms the robustness of these results using estimates from Equation 2. Consistent with Figure 6, we find no significant difference in pre-HTB trends between high- and low-exposure districts and a positive, statistically significant effect thereafter. The estimated coefficient remains similar in magnitude and significance when excluding London or when restricting the sample to propensity score matched districts, indicating that the increase in car sales is not driven by compositional differences or by London-specific dynamics.

5.2.2 Household Survey

Evidence from the LCFS confirms a broad-based consumption response. Table 3 reports estimates from Equation 3. Column 1 shows that total household consumption rose significantly more in high-exposure regions after the introduction of HTB, with no evidence of differential pre-trends. The result is robust to excluding London and becomes slightly larger in magnitude (column 2). In economic terms, households in regions with average HTB exposure spent about 6.3 percent more per year during the post-period than those in minimum-exposure regions, relative to their difference in 2012.

Columns 3–5 break down this response by major spending category. The most significant effect arises for non-durable consumption unrelated to housing, which accounts for roughly 75 percent of total spending. In regions with average HTB exposure, non-durable spending was 5.9 percent higher than in minimum-exposure regions, significant at the 1 percent level. Home-related and durable expenditures increased by 8.8 percent and 9.8 percent, respectively, though the former is only marginally significant and the latter statistically insignificant. The weaker precision for these components likely reflects that home-related spending is concentrated among recent buyers, who represent only about 6 percent of LCFS households, while durable purchases are infrequent, high-value items that account for only a small share of total expenditure.

5.3 Local Consumption Multiplier

The preceding results show that HTB increased both home sales and household spending in high-exposure areas. To quantify the strength of this relationship, we now estimate the local consumption multiplier, defined as the elasticity of cumulative consumption growth with respect to cumulative home sales growth.

Table 4 reports the results. Column 1 indicates that a 1 percent increase in home sales over 2013–2018 is associated with a 0.52 percent rise in car sales over the same period. Given that HTB raised cumulative home sales by roughly 108 percent in an average-exposure district, this implies a 56 percent increase in local car sales during the program period. Column 3 shows a comparable estimate for household consumption: a 0.29 percent increase in consumption for each 1 percent rise in home sales, equivalent to a 31 percent cumulative increase in local consumption in an average-exposure district.

Columns 2 and 4 exclude London districts, where housing-market dynamics differ markedly. The estimated elasticities remain statistically significant and are somewhat larger in magnitude, indicating that the relationship between housing activity and consumption is not driven by London-specific effects. The Kleibergen–Paap F-statistics in all specifications exceed the conventional threshold of 10, confirming instrument strength.

These estimates indicate a persistent local consumption response to HTB-induced housing activity. Importantly, the results should be interpreted as relative effects across local areas rather than aggregate national impacts. In other words, they measure how much more consumption increased in districts where home sales rose due to HTB relative to less-exposed areas. This distinction is central to the interpretation of local multipliers (Nakamura and

Steinsson, 2014): while national multipliers may be attenuated by general equilibrium effects or crowding out, cross-district variation in policy exposure identifies within-country differences in consumption responses.

To assess instrument validity, we conduct two diagnostic tests in the spirit of Goldsmith-Pinkham et al. (2020). First, we compare the covariates that predict HTB exposure with those that predict car sales and household consumption. Table A.9 in the Online Appendix shows no overlap in predictors, suggesting that omitted variable bias is unlikely to drive the results. Second, we examine the geographic balance of the instrument by regressing district-level HTB exposure on regional fixed effects. The resulting R^2 of 0.2, compared with 0.8 in the full specification with controls, suggests that most of the variation in HTB exposure occurs within rather than between regions. Together, these checks support the interpretation that HTB exposure provides plausibly exogenous variation in local housing activity.

Taken together, the evidence reveals a sizeable local consumption response to HTB-induced home purchases. The next step is to understand the channels through which this response arose.

6 Mechanisms

Easing mortgage borrowing constraints through HTB increased both home purchases and household consumption. In this section, we investigate the mechanisms underlying this consumption response. We first test whether the rise in consumption reflects higher spending by new home buyers. Next, we investigate whether this buyer-driven demand raised local income and employment, thereby amplifying consumption through the labor market. Finally, we assess whether house price growth or construction activity contributed to the overall increase in consumption.

6.1 Direct Home Buyer Response

A natural starting point in understanding the local consumption multiplier is the spending behavior of new home buyers. Purchasing a home typically entails substantial expenditures on furnishings, appliances, and services such as moving or renovation (Best and Kleven, 2017; Benmelech et al., 2023; Favara et al., 2025). It may also relax financial constraints, by halting the need to accumulate savings or replacing rent with lower mortgage payments, allowing buyers to increase discretionary spending. These mechanisms suggest that part of the local consumption response could stem directly from the households purchasing homes.

We examine this channel using the matched MDB–PSD panel, which links mortgage records to transaction-level expenditure data and allows us to track monthly spending over the 24 months before and after a home purchase. The high-frequency structure of these data allows us to estimate within-household consumption dynamics around a home purchase during the HTB period.

We implement an event-study design centered on the month of home purchase using the imputation estimator of Borusyak et al. (2024), which constructs counterfactual household spending from "not yet treated" renters who purchased outside the event window and "never treated" homeowners, thereby avoiding bias from already-treated units serving as controls.

We estimate the following specification using only untreated households:

$$Y_{i,t+h} = \alpha_i + \theta_{rt} + \varepsilon_{i,t+h}, \qquad D_{it} = 0, \tag{5}$$

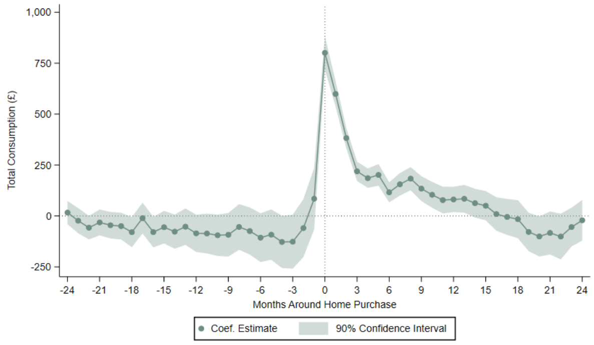
where $Y_{i,t+h}$ denotes monthly consumption of household i in month $t + h$, measured as total consumption or one of its categories: home-related, non-durables, or durables. α_i are household fixed effects and $\theta_{r,t}$ are region-month fixed effects that absorb local and aggregate shocks.

Fitted values from this regression provide predicted spending in the absence of a purchase. For treated households, the difference between observed and predicted spending captures the change in monthly consumption associated with the purchase. We align households by the purchase month and average these effects for each month before and after purchase to obtain $\widehat{\beta}_h$. Treatment effects are estimated over a 24-month window before and after the home purchase. The model is estimated on data for 2013–2018, and standard errors are clustered at the household level.

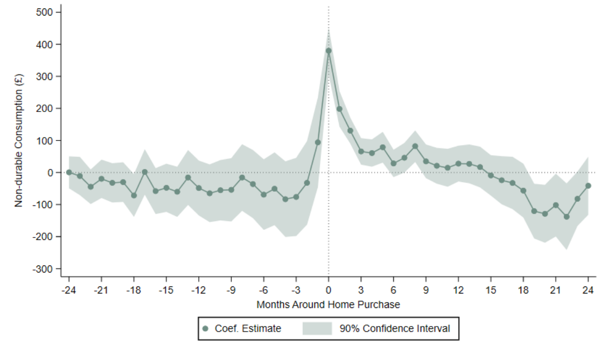
Figure 7 (panel a) shows the path of total household consumption around home purchase, relative to the control group. Spending rises sharply at purchase and remains elevated for over a year. In levels, monthly spending increases by £800 in the purchase month, £600 in month $t + 1$, and £400 in month $t + 2$, before stabilizing at roughly £150 above baseline. Aggregated over the first twelve months post-purchase, this corresponds to an additional £3,200 per household (about 17 percent of average annual consumption). Pre-trends are flat, indicating no anticipatory behavior or underlying momentum.

Panels b–d decompose the response by expenditure category. Home-related expenditures account for more than half of the total increase, with the largest gains concentrated in the first three months after purchase. This category remains elevated for at least two years. Non-durable spending also rises by about £350 in the purchase month and stays above pre-purchase levels for about 9 months. In contrast, we detect no statistically significant change

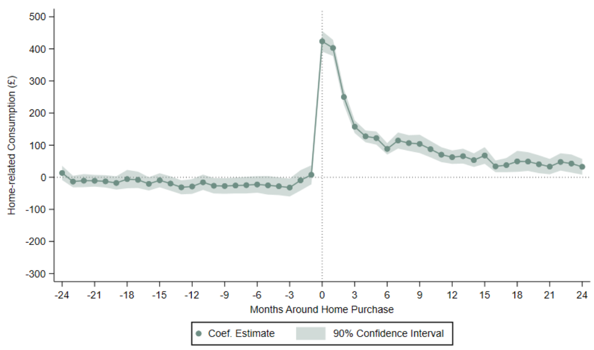
Figure 7: The Effect of a Home Purchase on Household Consumption



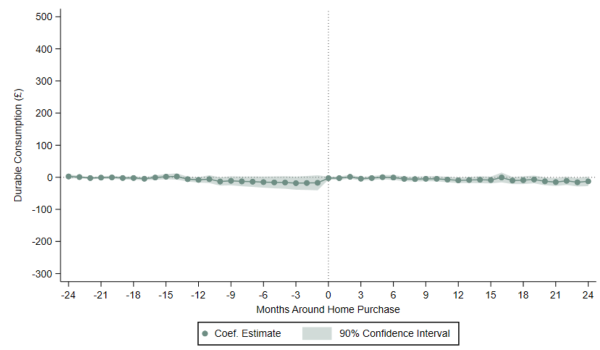
(a) Consumption



(b) Non-durable Consumption



(c) Home-related Expenditures



(d) Durable Expenditures

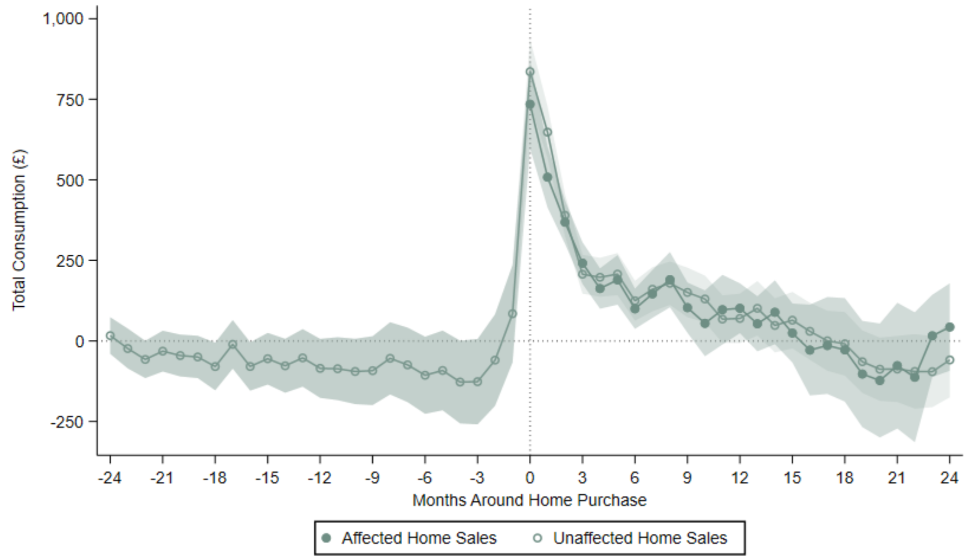
Notes: The figure presents $\widehat{\beta}_h$ for the 24 months before and after a home purchase, expressed relative to the month before purchase. The shaded regions represent 90 percent confidence intervals.

in durable expenditures unrelated to housing.

To verify that the same households driving the increase in home purchases also raise their spending after buying, we split buyers again into two groups: *Affected*, those with down payments of 5 or 10 percent (i.e. targeted by HTB), and *Unaffected*, those purchasing with down payments of 15 percent or more. Figure 8 shows that both groups exhibit similar absolute increases in post-purchase spending, confirming that the direct spending response is present among those households driving the policy-induced rise in home sales.

The results are robust to several additional checks that rule out alternative explanations. First, controlling for contemporaneous household income leaves the estimated treatment ef-

Figure 8: The Effect of a Home Purchase on Household Consumption by Down Payment Size



Notes: The figure presents $\widehat{\beta}_h$ for the 24 months before and after a home purchase, expressed relative to the month before purchase, where $\widehat{\beta}_h$ is estimated separately for *Affected* and *Unaffected* home sales. The shaded regions represent 90 percent confidence intervals.

fect essentially unchanged, indicating that the observed spending response is not driven by temporary income shocks or changes in labor-market conditions following the purchase. Second, excluding households that likely experienced the birth of a child, captured by significant shifts in child-related spending, yields similar estimates, suggesting that family composition changes are also not driving the results (Online Appendix Table A.10).[8]

These results indicate that home purchases trigger a substantial and sustained rise in household spending. Buyers increase their expenditure by roughly 17 percent in the year following purchase, with the largest gains concentrated in home-related goods and services. The elevated spending levels for more than a year suggest that home purchases generate a meaningful boost to demand, rather than merely shifting expenditure to the month of

[8]To identify significant shifts in child-related spending, we conduct a two-sample t-test comparing the mean child-related spending in the 24 months before a home purchase to the 24 months after. Households are excluded if the p-value for the null hypothesis of equal means is less than 5%.

purchase. We next examine whether this buyer-driven increase in demand propagated more broadly within local economies.

6.2 Local Effects through the Labor Market

The increase in consumption triggered by home buyers can generate broader local spillover effects if firms respond to the increase in demand by expanding employment or raising wages. As buyers spend more on goods and services produced within the local economy, nearby businesses absorb this demand and adjust their operations accordingly. They may hire additional workers, extend hours, or raise pay, thereby boosting income for other households in the area. These income gains, in turn, support further consumption, setting off a feedback loop that amplifies the initial effect.

To test whether local spillover effects through the labor market contributed to the consumption response, we examine how employment and household income evolved across districts with different levels of HTB exposure. If firms were responding to buyer-driven demand, we would expect stronger growth in non-tradable employment, which depends on local spending, while tradable employment should not rise, as firms in those sectors sell to national or global markets. Following Mian et al. (2020), we interpret changes in the ratio of non-tradable to tradable employment as a key indicator for a household demand channel of credit supply expansion.

We re-estimate Equation 2, using as outcome variable $Y_{d,t}$ either the total number of non-tradable employees or the ratio of non-tradable to tradable employment in each district-year. The specification again includes district and region-time fixed effects, and we report bootstrapped standard errors clustered by district and year.

Results are reported in Table 5. Non-tradable employment rose significantly more in high-exposure districts following the introduction of HTB, consistent with a local labor market response. The effect remains and becomes even stronger when excluding London (column 2) and for the matched sample (column 3). The non-tradable to tradable employment ratio also increases significantly more in high-exposure districts after HTB, indicating a reallocation of labor toward locally oriented activities (columns 7–9).

Using income data from the LCFS, we assess whether this rise in non-tradable employment is mirrored in broader gains in household income. Since the LCFS does not support analysis at the district level, we estimate the same difference-in-differences specification as for employment, but at the level of HTB exposure regions (see Section 4.3). The results, shown in columns 7–8, indicate that household income rose more strongly in high-exposure districts following HTB. The effect is robust to excluding London and confirms that the employment response translated into broader gains in household resources.

Taken together, these results support the presence of a local amplification mechanism: increased spending by new buyers raises household demand, which in turn stimulates local employment and income, reinforcing the initial consumption response.

6.3 Local Effects through House Prices and Construction

In addition to labor market spillovers, increased housing market activity may affect local consumption through two additional channels: residential construction and house prices. New construction can generate income and employment in building-related sectors, while rising house prices may increase the wealth of existing homeowners and thereby support further spending. In this section, we assess whether either mechanism contributed meaningfully to the observed amplification in local consumption.

6.3.1 Construction Activity

A stated aim of HTB was to stimulate housing supply. To assess whether residential construction rose more in areas where the policy had greater effect, we re-estimate Equation 2 using the number of "housing starts", defined as new dwellings for which building work commenced at the district-year level, as the outcome variable.

Table 6 shows that housing starts increased during the HTB period, but not disproportionately in high-exposure districts (columns 1–2). Columns 3–5 allow the response to vary with local housing supply elasticity, proxied by the "Share of Developable Land" measure of Hilber and Vermeulen (2016).[9] The results indicate that construction rose mainly in areas with more elastic supply (column 3), a pattern that holds when excluding London (column 4) and when restricting the sample to matched districts (column 5).

Overall, construction activity expanded during the HTB period, but mainly in areas with more elastic housing supply rather than in those with constrained buyers most affected by the policy. Construction therefore does not appear to have been a major source of amplification in local consumption.

[9] The variable measures the share of potentially developable land already built on by 1990 in English districts. We subtract it from one, so higher (lower) values indicate more (less) elastic housing supply.

6.3.2 House Price Growth

HTB increased housing transactions, yet construction activity did not expand disproportionately in districts more exposed to HTB. In markets where supply cannot adjust quickly, stronger demand potentially translates into higher prices. Consistent with this, Carozzi et al. (2024) show that HTB's EL scheme raised the prices of new builds, particularly in supply-constrained areas. Higher prices, in turn, can stimulate spending through standard housing-wealth and collateral channels (e.g., Campbell and Cocco, 2007; Mian and Sufi, 2011; Mian et al., 2013; Guren et al., 2020) and this may also affect local non-tradable employment (Mian and Sufi, 2014).

To examine whether house prices increased more in high-exposure districts following HTB, we again re-estimate Equation 2, now using the log change in the district-level house price index as the dependent variable. To account for district-level heterogeneity in supply responsiveness, all specifications include interactions of the pre- and post-HTB indicators with our measure of local housing supply elasticity.

Table 7 shows that HTB affected house price growth, though the magnitude varied considerably across regions. In the full sample, annual house price growth in an average-exposure district was 2.8 percentage points higher than in a minimum-exposure district after the introduction of HTB (column 1). Outside London, the effect was 1.8 percentage points; in London, it rose to 5.2 percentage points (columns 2–3). Because the specification controls for local supply elasticity, this heterogeneity most likely reflects tighter market conditions and stronger demand pressure in London. In the matched sample, the estimated effects are smaller and less precise (column 4), potentially reflecting reduced cross-sectional variation once observables are balanced.

Taken together with our earlier results, these estimates suggest that HTB stimulated consumption through three mutually reinforcing channels. New buyers increased spending in the year following their purchase. House prices rose, particularly in tighter markets, generating additional housing-wealth and collateral effects that supported local consumption. Non-tradable employment and income also rose, while construction activity remained flat, showing that the rise in consumption was further amplified by higher local earnings and job creation rather than by higher building activity. Overall, the evidence points to a policy-induced expansion in housing transactions that set off a sequence of demand responses, initiated by buyer spending, complemented by house-price gains, and strengthened by labor-market adjustment, that together generated a sustained increase in local consumption.

7 Concluding Remarks

This paper documents that relaxing mortgage-market borrowing constraints can generate a sizable and persistent increase in local consumption. Exploiting geographic variation in exposure to the UK HTB program, a large-scale intervention that reduced down payment requirements, we show that easier credit access led to an 18.1 percent increase in home purchases in districts with average exposure to the policy. Each of these new home buyers, in turn, spent an additional £3,200 in the year following their purchase, primarily on home-related goods and services and non-durables.

The rise in buyer spending sets off a broader propagation mechanims. As demand for locally provided goods and services increases, firms respond by hiring workers and raising incomes, especially in the non-tradable sector. These local spillovers extend consumption beyond the initial group of home buyers. Rising house prices further strengthen the consumption response through wealth and collateral effects. In our baseline estimates, a 1 percent increase in home sales translates into a 0.52 percent increase in car sales and a 0.29 percent increase in total household consumption over six years, an elasticity that is both economically and statistically significant.

These findings advance our understanding of how credit policy transmits to the real economy. Whereas prior work has emphasized refinancing-based channels and house price-driven wealth effects for existing homeowners (e.g., **?**), we highlight a different mechanism: a local consumption multiplier triggered by a surge in housing transactions. By showing that a relaxation of the down payment constraint can activate a sequence of demand responses that propagate through the local economy, our results complement recent evidence that housing transactions themselves are an important driver of local demand (Best and Kleven, 2017; Benmelech et al., 2023; Favara et al., 2025).

While the short-run effects are clearly expansionary, the longer-run implications are less certain. A larger share of households now carry high-LTV mortgages, which may increase the financial system's vulnerability to adverse shocks if these new high-leverage borrowers are more likely to default (Gupta and Hansman, 2022). Moreover, recent evidence suggests that credit expansions can raise consumption in the short term but may lead to greater volatility or lower average spending over the cycle, particularly when debt burdens are high (Mian et al., 2021; Garber et al., 2024).

Overall, our results underscore that mortgage credit policy can influence real activity well beyond its immediate effects on homeownership. By targeting borrowing constraints, policies such as HTB can stimulate housing transactions and trigger localized demand spillovers that raise employment, income, and consumption. Whether such stimulus ultimately improves welfare depends on how these gains interact with financial stability risks, the sustainability of household debt, and the distribution of spending gains. These are questions we leave to future research.

References

Aastveit, Knut Are, R Juelsrud, and Ella Getz Wold, "Does mortgage regulation stabilize household consumption," Technical Report, Working paper 6/22. Norges Bank 2022.

Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, Tomasz Piskorski, and Amit Seru, "Policy intervention in debt renegotiation: Evidence from the Home Affordable Modification Program," *Journal of Political Economy*, 2017, *125* (3), 654–712.

_ , _ , Souphala Chomsisengphet, Tim Landvoigt, Tomasz Piskorski, Amit Seru, and Vincent Yao, "Mortgage refinancing, consumer spending, and competition: Evidence from the Home Affordable Refinance Program," *The Review of Economic Studies*, 2023, *90* (2), 499–537.

Aladangady, Aditya, "Housing wealth and consumption: Evidence from geographically linked microdata," *American Economic Review*, 2017, *107* (11), 3415–3446.

Aruoba, S Borağan, Ronel Elul, and Sebnem Kalemli-Özcan, "Housing wealth and consumption: The role of heterogeneous credit constraints," NBER Working Paper 30591, National Bureau of Economic Research 2022.

Auerbach, Alan, Yuriy Gorodnichenko, and Daniel Murphy, "Local fiscal multipliers and fiscal spillovers in the USA," *IMF Economic Review*, 2020, *68* (1), 195–229.

Bekkum, Sjoerd Van, Marc Gabarro, Rustom M Irani, and José-Luis Peydró, "The real effects of borrower-based macroprudential policy: Evidence from administrative household-level data," *Journal of Monetary Economics*, 2024, *147*, 103574.

Benetton, Matteo, Philippe Bracke, João F Cocco, and Nicola Garbarino, "Housing consumption and investment: evidence from shared equity mortgages," *The Review of Financial Studies*, 2022, *35* (8), 3525–3573.

Benmelech, Efraim, Adam Guren, and Brian T Melzer, "Making the house a home: The stimulative effect of home purchases on consumption and investment," *The Review of Financial Studies*, 2023, *36* (1), 122–154.

Beraja, Martin, Andreas Fuster, Erik Hurst, and Joseph Vavra, "Regional heterogeneity and the refinancing channel of monetary policy," *The Quarterly Journal of Economics*, 2019, *134* (1), 109–183.

Berger, David, Nicholas Turner, and Eric Zwick, "Stimulating housing markets," *Journal of Finance*, 2020, *75* (1), 277–321.

Best, Michael Carlos and Henrik Jacobsen Kleven, " Housing market responses to transaction taxes: Evidence from notches and stimulus in the UK," *Review of Economic Studies*, 2017, *85*, 157–193.

_ **, James S Cloyne, Ethan Ilzetzki, and Henrik J Kleven**, "Estimating the elasticity of intertemporal substitution using mortgage notches," *Review of Economic Studies*, 2020, *87* (2), 656–690.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting event-study designs: robust and efficient estimation," *The Review of Economic Studies*, 2024.

Browning, Martin, Angus Deaton, and Margaret Irish, "A profitable approach to labor supply and commodity demands over the life-cycle," *Econometrica*, 1985, *53* (3), 503–543.

Burstein, Ariel, Gordon Hanson, Lin Tian, and Jonathan Vogel, "Tradability and the labor-market impact of immigration: Theory and evidence from the United States," *Econometrica*, 2020, *88* (3), 1071–1112.

Campbell, John Y. and Joao F. Cocco, "How do house prices affect consumption? Evidence from micro data," *Journal of Monetary Economics*, 2007, *54* (3), 591–621.

Carney, Mark, "Letter from the Governor to the Chancellor regarding the Help to Buy: Mortgage Guarantee scheme," 2016. Letter from the Governor to the Chancellor regarding the Help to Buy: Mortgage Guarantee scheme.

Carozzi, Felipe, "Credit constraints and the composition of housing sales. Farewell to first-time buyers," *Journal of the European Economic Association*, 2020, *18* (3), 1196–1237.

_ **, Christian A.L. Hilber, and Xiaolun Yu**, "On the economic impacts of mortgage credit expansion policies: Evidence from help to buy," *Journal of Urban Economics*, 2024, *139*, 103611.

Chancellor of the Exchequer, "Oral statement to Parliament of the Budget 2013," Houses of Parliament. London, United Kindom 2013.

Chodorow-Reich, Gabriel, "Geographic cross-sectional fiscal spending multipliers: What have we learned?," *American Economic Journal: Economic Policy*, 2019, *11* (2), 1–34.

Cloyne, James, Clodomiro Ferreira, and Paolo Surico, "Monetary policy when households have debt: New evidence on the transmission mechanism," *Review of Economic Studies*, 2020, *87* (1), 102–129.

Deaton, Angus, "Panel data from time series of cross-sections," *Journal of Econometrics*, 1985, *30* (1-2), 109–126.

Defusco, Anthony, "Homeowner borrowing and housing collateral: New evidence from expiring price controls," *Journal of Finance*, 2018, *73* (2), 523–573.

_ , Stephanie Johnson, and John Mondragon, "Regulating Household Leverage," *Review of Economic Studies*, 2020, *87*, 914–958.

DiMaggio, Marco, Amir Kermani, Benjamin J Keys, Tomasz Piskorski, Rodney Ramcharan, Amit Seru, and Vincent Yao, "Interest rate pass-through: Mortgage rates, household consumption, and voluntary deleveraging," *American Economic Review*, 2017, *107* (11), 3550–3588.

Dynan, Karen E., Wendy Edelberg, and Michael G. Palumbo, "The effects of population aging on the relationship among aggregate consumption, saving, and income," *American Economic Review*, 2009, *99* (2), 380–386.

Engelhardt, Gary V, "Consumption, down payments, and liquidity constraints," *Journal of Money, Credit and Banking*, 1996, *28* (2), 255–71.

Favara, Giovanni, James Graham, and Geng Li, "Of house and home-related goods: The home purchase channel of expenditure," Finance and Economics Discussion Series 2025-045, Board of Governors of the Federal Reserve System (U.S.) 2025.

Finlay, Stephen, Peter Williams, and Christine Whitehead, "Evaluation of the Help to Buy Equity Loan Scheme," Technical Report, Department for Communities and Local Government 2016.

Fuster, Andreas and Basit Zafar, "The sensitivity of housing demand to financing conditions: evidence from a survey," *American Economic Journal: Economic Policy*, 2021, *13* (1), 231–265.

Garber, Gabriel, Atif Mian, Jacopo Ponticelli, and Amir Sufi, "Consumption smoothing or consumption binging? The effects of government-led consumer credit expansion in Brazil," *Journal of Financial Economics*, 2024, *156* (C).

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift, "Bartik instruments: What, when, why, and how," *American Economic Review*, 2020, *110* (8), 2586–2624.

Gupta, Arpit and Christopher Hansman, "Selection, leverage, and default in the mortgage market," *The Review of Financial Studies*, 2022, *35* (2), 720–770.

Guren, Adam M, Alisdair McKay, Emi Nakamura, and Jon Steinsson, "Housing Wealth Effects: The Long View," *The Review of Economic Studies*, 04 2020, *88* (2), 669–707.

Haurin, Donald R., Patric H. Hendershott, and Susan M. Wachter, "Borrowing Constraints and the Tenure Choice of Young Households," *Journal of Housing Research*, 1997, *8* (2), 137–154.

Hilber, Christian A. L. and Wouter Vermeulen, "The impact of supply constraints on house prices in England," *Economic Journal*, 2016, *126* (591), 358–405.

Jorda, Oscar, "Estimation and inference of impulse responses by local projections," *American Economic Review*, 2005, *95* (1), 161–182.

_ , Moritz Schularick, and Alan M. Taylor, "Leveraged bubbles," *Journal of Monetary Economics*, 2015, *76* (S), 1–20.

Linneman, Peter and Susan Wachter, "The Impacts of Borrowing Constraints on Homeownership," *Real Estate Economics*, 1989, *17* (4), 389–402.

Lomax, Nik, "Household mobility: Where and how far do we move?," mimeo 2020.

Mian, Atif, Amir Sufi, and Emil Verner, "How does credit supply expansion affect the real economy? The productive capacity and household demand channels," *Journal of Finance*, 2020, *75* (2), 949–994.

_ and _ , "House prices, home equity-based borrowing, and the US household leverage crisis," *American Economic Review*, 2011, *101* (5), 2132–56.

_ and _ , "The effects of fiscal stimulus: evidence from the 2009 Cash for Clunkers program," *The Quarterly Journal of Economics*, 2012, *127* (3), 1107–1142.

_ and _ , "What explains the 2007-2009 drop in employment?," *Econometrica*, 2014, *82*, 2197–2223.

_ , Kamalesh Rao, and Amir Sufi, "Household balance sheets, consumption, and the economic slump," *The Quarterly Journal of Economics*, 2013, *128* (4), 1687–1726.

—, **Ludwig Straub, and Amir Sufi**, "Indebted demand," *The Quarterly Journal of Economics*, 2021, *136* (4), 2243–2307.

Miyamoto, Wataru, Thuy Lan Nguyen, and Dmitriy Sergeyev, "Government spending multipliers under the Zero Lower Bound: Evidence from Japan," *American Economic Journal: Macroeconomics*, 2018, *10* (3), 247–277.

Nakamura, Emi and Jón Steinsson, "Fiscal stimulus in a monetary union: Evidence from US regions," *American Economic Review*, 2014, *104* (3), 753–792.

Ortalo-Magne, Francois and Sven Rady, "Housing market dynamics: On the contribution of income shocks and credit constraints," *Review of Economic Studies*, 2006, *73* (2), 459–485.

Pennings, Steven, "Cross-region transfer multipliers in a monetary union: Evidence from social security and stimulus payments," *American Economic Review*, 2021, *111* (5), 1689–1719.

Peydro, Jose-Luis, Francesc Rodriguez-Tous, Jagdish Tripathy, and Arzu Uluc, "Macroprudential policy, mortgage cycles, and distributional effects: Evidence from the United Kingdom," *The Review of Financial Studies*, 2024, *37* (3), 727–760.

Ramey, Valerie A. and Sarah Zubairy, "Government spending multipliers in good times and in bad: Evidence from US historical data," *Journal of Political Economy*, 2018, *126* (2), 850–901.

Robles-Garcia, Claudia, "Competition and incentives in mortgage markets: The role of brokers," Stanford University Working Paper 3796 2019.

Serrato, Juan Carlos Suárez and Philippe Wingender, "Estimating local fiscal multipliers," Technical Report, National Bureau of Economic Research 2016.

Stein, Jeremy C, "Prices and trading volume in the housing market: A model with downpayment effects," *The Quarterly Journal of Economics*, 1995, *110* (2), 379–406.

Thomas, Michael, Brian Gillespie, and Nik Lomax, "Variations in migration motives Over distace," *Demographic Research*, 2019, *40* (38), 1097–1110.

Tracey, Belinda and Neeltje Van Horen, "Credit constraints and the redistribution of housing wealth," Bank of England Working Papers 1147, Bank of England 2025.

Table 1: Summary Statistics

Variable Name (Unit)	Pre Help-to-Buy Mean	Pre Help-to-Buy Median	Pre Help-to-Buy Std. Dev.	Post Help-to-Buy Mean	Post Help-to-Buy Median	Post Help-to-Buy Std. Dev.
Panel A: District-level Variables						
Low-Down Payment (%)	2.66	2.37	1.38	13.63	13.28	6.42
HTB Exposure (%)	22.66	22.01	6.62	22.66	22.01	6.62
Home Sales ('000)	1.27	1.02	0.81	1.64	1.37	1.02
First-time Buyer Sales ('000)	0.49	0.36	0.39	0.77	0.58	0.6
Home Mover Sales ('000)	0.78	0.67	0.46	0.88	0.76	0.52
Younger Buyer Sales ('000)	0.82	0.64	0.59	1.13	0.9	0.81
Older Buyer Sales ('000)	0.45	0.39	0.25	0.52	0.46	0.3
Car Sales ('000)	5.14	3.19	6.65	6.25	3.73	8.05
Non-tradable Employment ('000)	59.01	43.76	44.51	63.33	48.17	47.09
Non-tradable/Tradable Employment Ratio	10.24	6.6	10.63	11.52	7.13	12.59
Home Starts	312.53	240	272.34	455.32	350	377.51
House Price Growth (%)	0.89	0.84	3.85	4.96	4.64	4.02
Unemployment Rate (%)	7.2	6.87	2.35	4.94	4.5	1.99
Median Weekly Income (£)	449.17	431.46	76.13	435.5	420.54	65.07
Average House Prices (£'000)	199.82	182.89	87.13	225.77	194.75	127
Average Loan-to-Income Ratio	2.99	2.96	0.25	3.26	3.24	0.35
Panel B: Cohort-level Variables from Survey Data						
Total Household Consumption (£)	466.45	468.58	120.10	473.59	477.57	117.76
Non-durable (excl. Home-related) (£)	353.79	352.43	89.26	353.71	356.14	85.89
Durable (excl. Home-related) (£)	35.94	34.98	15.47	37.90	37.41	16.78
Home-related Expenditures (£)	78.99	78.04	24.32	76.06	75.60	23.03
Gross Household Income (£)	861.33	880.41	274.30	860.89	876.00	265.09
Panel C: Household Variables from Transaction-level Data						
Total Household Consumption (£)				1,806.87	1,401.80	1,635.18
Non-durable (excl. Home-related) (£)				1,619.63	1,276.49	1,439.82
Durable (excl. Home-related) (£)				16.49	0.00	115.62
Home-related Expenditures (£)				170.76	30.96	572.45

Notes: The table presents summary statistics for the key variables used in our analyses. Summary statistics are reported for the pre-HTB period (2009–2012) and the post-HTB period (2013–2018). There are 379 UK districts included in our sample. In the pre-HTB period, there are 1,511 district-level observations and 319 cohort-year observations. Transaction-level data are not available in the pre-HTB period. In the post-HTB period, there are 2,266 district-level observations, 480 cohort-year observations, 142,656 household-level observations from the transaction-level data. Cohort-year variables are in weekly amounts, while household-level Variables in monthly amounts. Variables are deflated to 2016 values.

Table 2: The Effect of Help-to-Buy on Home Sales by Down Payment Size

	All Districts (1)	All Districts (2)	Excl. London (3)	PSM (4)	All Districts (5)
$\text{Pre}_t \times \text{Exposure}_d$	-0.086				
	(0.055)				
$\text{Post}_t \times \text{Exposure}_d$	0.095				
	(0.160)				
$\text{Post}_t \times \text{Exposure}_d \times \text{Down Payment}_{10\%}$	0.524**	0.534***	0.778***	-0.096	0.507***
	(0.211)	(0.050)	(0.052)	(0.129)	(0.061)
$\text{Post}_t \times \text{Exposure}_d \times \text{Down Payment}_{5\%}$	0.907***	0.940***	1.172***	0.538***	1.028***
	(0.244)	(0.059)	(0.062)	(0.145)	(0.075)
$\text{Pre}_t \times \text{LTI Exposure}_d \times \text{Down Payment}_{5\%}$					-0.095
					(0.140)
$\text{Post}_t \times \text{LTI Exposure}_d \times \text{Down Payment}_{5\%}$					0.308*
					(0.162)
Model Statistics					
N	11352	11350	10373	5757	11350
R^2	0.795	0.979	0.980	0.982	0.979
Control Variables					
$\text{Post}_t \times \text{Down Payment}_i$	Yes	No	No	No	No
$\text{Exposure}_d \times \text{Down Payment}_i$	Yes	No	No	No	No
Fixed Effects					
District	Yes	No	No	No	No
Region × Time	Yes	No	No	No	No
Down Payment	Yes	No	No	No	No
District × Time	No	Yes	Yes	Yes	Yes
District × Down Payment	No	Yes	Yes	Yes	Yes
Time × Down Payment	No	Yes	Yes	Yes	Yes

Notes: This table reports the effect of HTB on home sales. The dependent variable is the number of mortgaged home sales within each down payment bucket at the district-year level. Pre is a dummy equal to 1 for 2009–2011 and Post a dummy equal to 1 for 2013–2018, with 2012 as the omitted base year. Exposure is the district share of low–down payment mortgages during 2005–2007. Columns (1), (2), and (5) include all districts; column (3) excludes London; and column (4) restricts the sample to propensity score matched districts. Column (5) additionally controls for LTI exposure, defined as the share of mortgages issued by LTI constrained lenders in 2012. Bootstrapped standard errors, clustered by district and year, are reported in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Table 3: The Effect of Help-to-Buy on Household Consumption

	Consumption		Non-durable	Durable	Home-related
	All Districts	Excl. London	All Districts	All Districts	All Districts
	(1)	(2)	(3)	(4)	(5)
$\text{Pre}_t \times \text{Exposure}_r$	107.919	138.369	51.917	29.646	21.641
	(86.001)	(93.969)	(64.346)	(18.548)	(30.682)
$\text{Post}_t \times \text{Exposure}_r$	206.847***	233.290***	149.493***	22.442	46.531*
	(71.8124)	(86.232)	(57.650)	(16.445)	(24.976)
Model Statistics					
N	799	795	799	799	799
R^2	0.850	0.847	0.949	0.515	0.577
Control Variables					
Cohort Characteristics	Yes	Yes	Yes	Yes	Yes
Fixed Effects					
HTB-Region	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes
Birth Year Group	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates for Equation 3 during 2009–2018, and shows the effect of HTB on household consumption. The dependent variable is total household consumption, non-durable and services consumption, durable expenditures, or home-related expenditures; non-durable and services consumption and durable expenditures exclude home-related expenditures. Pre is a dummy variable equal to 1 for the 2009–2011 period. Post is a dummy variable equal to 1 for the 2013–2018 period. The base year is 2012. Exposure equals the average exposure across the districts assigned to the region, where district-level exposure equals the share of low-down payment mortgages in a district during 2005-2007. HTB-Regions represent districts grouped into Exposure deciles. Column 2 which excludes London districts. Standard errors are heteroskedasticity-robust and are shown in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Table 4: The Effect of Local Home Sales on Local Consumption

	Car Sales		Consumption	
	All Districts	Excl. London	All Districts	Excl. London
	(1)	(2)	(3)	(4)
Cumulative Elasticity	0.523***	0.528**	0.287**	0.424**
	(0.187)	(0.252)	(0.129)	(0.166)
Model Statistics				
N	379	346	79	77
K-Papp F-Stat	59.930	36.998	274.868	163.105
Fixed Effects				
Region	Yes	Yes	n/a	n/a
Birth Cohort	n/a	n/a	Yes	Yes

*Notes*The table presents coefficient estimates for Equation 4, and shows effect of HTB induced changes in home sales on local consumption. The dependent variable is the cumulative log change in car sales (columns 1–2) or household consumption (columns 3–4) over the 2013–2018 period relative to 2012. Exposure, measured as the district share of low–down payment mortgages originated during 2005–2007, is used as an instrument for the cumulative change in home sales. Columns (1) and (3) include all districts; columns (2) and (4) exclude London. Heteroskedasticity-robust standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively

Table 5: The Effect of Help-to-Buy on Local Demand

	Non-tradable Employment			Non-tradable/Tradable Employment Ratio			Income	
	All Districts	Excl. London	PSM	All Districts	Excl. London	PSM	All Districts	Excl. London
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Pre}_t \times$ Exposure	2.179	-1.605	0.410	5.357	-2.518	-3.853	106.458	85.552
	(4.396)	(3.061)	(5.913)	(4.894)	(1.581)	(3.662)	(218.574)	(169.185)
$\text{Post}_t \times$ Exposure	7.525**	10.704***	17.855***	10.516*	2.831**	8.980***	443.501**	405.542**
	(3.817)	(2.726)	(5.759)	(5.529)	(1.298)	(2.849)	(207.324)	(169.385)
Model Statistics								
N	3790	3460	1820	3790	3460	1820	799	796
R^2	0.993	0.995	0.991	0.925	0.956	0.959	0.867	0.870
Control Variables								
Cohort Characteristics	No	No	No	No	No	No	Yes	Yes
Fixed Effects								
District	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Region $\times$ Time	Yes	Yes	Yes	Yes	Yes	Yes	No	No
HTB-Region	No	No	No	No	No	No	Yes	Yes
Time	No	No	No	No	No	No	Yes	Yes
Birth Year Group	No	No	No	No	No	No	Yes	Yes

Notes: This table reports the effect of HTB on local demand indicators. The dependent variables are non-tradable employment (columns 1–3), the non-tradable-to-tradable employment ratio (columns 4–6), and household income (columns 7–8). *Pre* is a dummy equal to 1 for 2009–2011 and *Post* a dummy equal to 1 for 2013–2018, with 2012 as the omitted base year. *Exposure* is the district share of low-down payment mortgages during 2005–2007 in columns (1)–(6), and the average exposure across districts assigned to the corresponding HTB region in columns (7)–(8). Columns (1), (4), and (7) include all districts; columns (2), (5), and (8) exclude London; columns (3) and (6) restrict the sample to propensity score matched districts. Bootstrapped standard errors, clustered by district and year, are reported in parentheses for columns (1)–(6); heteroskedasticity-robust standard errors are used for columns (7)–(8). ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Table 6: The Effect of Help-to-Buy on Construction

	All Districts	All Districts	All Districts	Excl. London	PSM
	(1)	(2)	(3)	(4)	(5)
Pre_t	0.005				
	(0.037)				
Post_t	0.154***				
	(0.033)				
$\text{Pre}_t \times$ Exposure	-0.002	0.119	0.174	0.221	-0.094
	(0.159)	(0.183)	(0.211)	(0.247)	(0.468)
$\text{Post}_t \times$ Exposure	-0.038	-0.320*	0.032	-0.016	-0.172
	(0.143)	(0.167)	(0.180)	(0.226)	(0.444)
$\text{Pre}_t \times$ Elasticity			0.037	0.040	0.059
			(0.067)	(0.068)	(0.100)
$\text{Post}_t \times$ Elasticity			0.237***	0.246***	0.279***
			(0.061)	(0.067)	(0.090)
Model Statistics					
N	3220	3220	3220	2890	1520
R^2	0.680	0.704	0.706	0.730	0.717
Fixed Effects					
District	Yes	Yes	Yes	Yes	Yes
Region $\times$ Time	No	Yes	Yes	Yes	Yes

Notes: This table reports the effect of HTB on home starts. The dependent variable is the number of housing starts at the district–year level. Pre is a dummy equal to 1 for 2009–2011 and Post a dummy equal to 1 for 2013–2018, with 2012 as the omitted base year. Exposure is the district share of low–down payment mortgages during 2005–2007. Elasticity is defined as 1 minus the "Share of Developable Land" measure of Hilber and Vermeulen (2016), where higher values indicate more elastic housing supply. Columns (1)–(3), and (5) include all districts; column (4) excludes London; and column (5) restricts the sample to propensity score matched districts. Bootstrapped standard errors, clustered by district and year, are reported in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Table 7: The Effect of Help-to-Buy on House Prices

	All Districts	Excl. London	London only	PSM
	(1)	(2)	(3)	(4)
$\text{Pre}_t \times$ Exposure	-0.041	-0.058	-0.039	-0.126*
	(0.045)	(0.039)	(0.126)	(0.066)
$\text{Post}_t \times$ Exposure	0.200***	0.126***	0.371***	-0.004
	(0.041)	(0.036)	(0.125)	(0.064)
Model Statistics				
N	3220	2890	330	1520
R^2	0.609	0.594	0.637	0.657
Fixed Effects				
District	Yes	Yes	Yes	Yes
Region $\times$ Time	Yes	Yes	Yes	Yes

Notes: This table reports the effect of HTB on house price growth. The dependent variable is the log change in the district-level house price index. Pre is a dummy equal to 1 for 2009–2011 and Post a dummy equal to 1 for 2013–2018, with 2012 as the omitted base year. Exposure is the district share of low–down payment mortgages during 2005–2007. All regressions control for local housing-supply elasticity. Column (1) includes all districts; column (2) excludes London; column (3) includes only London; and column (4) restricts the sample to propensity score matched districts. Bootstrapped standard errors, clustered by district and year, are reported in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Online Appendix

A Living Costs and Food Survey Data

We use the Living Costs and Food Survey (LCFS) to obtain our household-level consumption data. Formerly known as the Expenditure and Food Survey (EFS) and the Family Expenditure Survey (FES), the LCFS represents the most comprehensive survey on household spending in the UK. It is conducted by the UK Office of National Statistics, and collects expenditure information from around 5,000 households across the UK throughout each year. Respondents provide a detailed expenditure diary for their household over a two week period. It also gathers information about each respondent's household income and demographic profile. Our study includes survey data from Q1:2009 to Q4:2018.

We define *Non-durable Consumption*, *Durable Expenditures, Home-related Expenditures* and *Total Household Consumption* as follows:

- *Non-durable Consumption*: includes food, alcohol, tobacco, fuel, light and power, clothing and footwear, personal services, non-durable personal goods, fares, leisure services, non-durable leisure goods, and motoring expenditures.

- *Durable Expenditure*s: includes motor vehicles, durable personal goods, durable leisure goods. This covers spending on jewelry, television set purchases, personal computers, audio-visual equipment, among others.

- *Home-related Expenditure*s: includes household services, non-durable household goods, and durable household goods. This covers spending on furniture and furnishings, bedroom textiles, kitchenware, electric and home appliances, among others.

- *Total Household Consumption*: is the sum of *Non-durable Consumption*, *Durable Ex-*

penditures and *Home-related Expenditures,*

Following Cloyne et al. (2020), mortgage and rental-related costs are excluded from our expenditure measures.

All expenditure variables are deflated to 2016 values using the UK Consumer Prices Index including owner-occupiers' housing costs (CPIH), and each expenditure measure is winsorized at the 1st and 99th percentiles.

Cohort-level controls include the proportion of outright homeowners, the average age of the household head, the number of household members, and the number of children. We exclude year–region–birth cohort cells with ten or fewer observations, households reporting negative or missing income, and those living rent-free or in social housing. Our dataset includes private renters, outright owners, and mortgaged owners.

The LCFS provides both annual and quarterly probability weights. Following Dynan et al. (2009), and consistent with the use of demographic selection criteria, we do not apply these weights in our main analysis.

B Money Dashboard Data

There are around 300 distinct "tags" to categorize transactions in the Money Dashboard (MDB) app. We use this tag information for debit transactions and define *Non-durable Consumption*, *Durable Expenditures, Home-related Expenditures* and *Total Household Consumption* as follows:

- *Non-durable Consumption*: includes tagged transactions such as food, groceries, alcohol, tobacco, take-away, fuel, light and power, clothing and footwear, personal services, non-durable personal goods, fares, leisure services, non-durable leisure goods, and motoring expenditures.

- *Durable Expenditures*: includes tagged transactions such as motor vehicles, durable personal goods, durable leisure goods. This covers spending on cars, bikes, caravans, jewelry, personal computers, audio-visual equipment, sports equipment, music equipment, among others.

- *Home-related Expenditures*: includes household services, non-durable household goods, and durable household goods. This covers spending on furniture and furnishings, lighting, garden, soft furnishings, bedroom textiles, kitchenware, electric and home appliances, television set purchases, tradesmen fees, among others.

- *Total Household Consumption*: is the sum of *Non-durable Consumption*, *Durable Expenditures* and *Home-related Expenditures.*

We exclude duplicate transactions, business accounts, users younger than 16 or older than 100, and transactions without tag information (around 10 percent of all records). We also remove users or observations with incomplete or implausible spending histories, which can arise when online banking details change, leaving them unable to collect new data. Specifically, we exclude users with mean total consumption in the top or bottom percentile, those with the standard deviation of mean total consumption in the top percentile, and observations in which spending falls below 25 percent of a user's average consumption.

C Business Structure Database (BSD)

We obtain our employment data from the Business Structure Database (BSD). The BSD dataset is an annual snapshot taken from the Interdepartmental Business Register (IDBR), which is a live register of data collected by Her Majesty's Revenue and Customs based on tax records. It therefore provides details about the geographic location and number of employees for the universe of active firms that are registered for income tax purposes in the UK. The BSD data reports information for "enterprises" and "local units". An enterprise is defined as the overall business organization. An enterprise can be made up of one or more local units, such as a shop, branch, or factory. Given our interest in regional employment, and locally non-tradable employment specifically, our regional employment measures are based on the employment at the local unit level.

We classify non-tradable and tradable employment following the approach of Burstein et al. (2020).

- *Non-tradable Employment*: includes firms in service-producing industries.
- *Tradable Employment*: includes firms in goods-producing industries, such as agriculture, mining and manufacturing.

Table A.4 presents the 4-digit SIC-07 codes and description, along with their industry classification used to generate our employment measures. The construction industry (SIC-07 classes 41.10-43.99) is excluded from our tradable and non-tradable employment measures, following Burstein et al. (2020) and Mian and Sufi (2014).

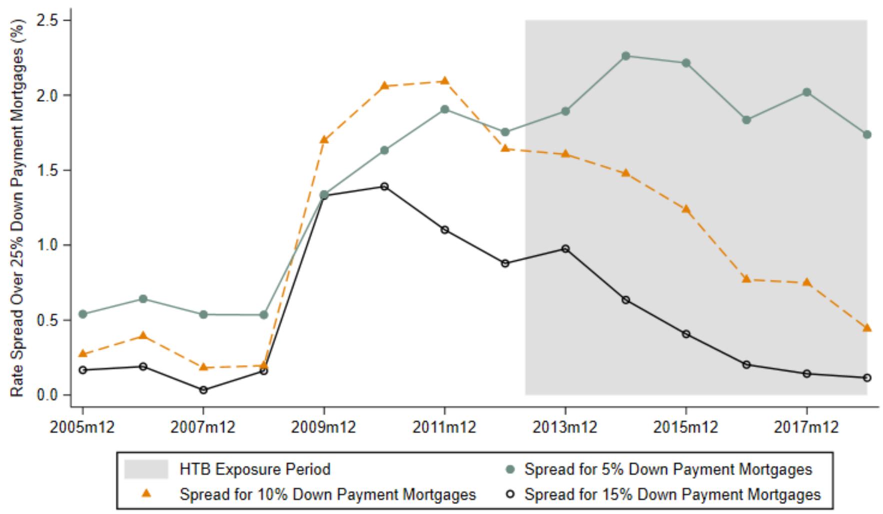
D Internal Migration

To test whether relaxing the down payment constraint led to housing-related migration between districts in the UK, we re-estimate Equation 2 from the main text. The outcome variable is net migration flows, defined as the number of individuals moving into district d from another UK district minus the number moving out of district d to elsewhere in the UK—scaled by the district's population. The model includes district and region-time fixed effects and bootstrapped standard errors are clustered by district and year.

Table A.6 reports the results. Column 1 shows that relaxing the down payment constraint is associated with a decline in net migration flows into high-exposure districts after the introduction of HTB. The result remains when excluding districts in the London area (column 2), but becomes smaller and statistically insignificant when restricting the sample to propensity score matched districts (column 3). This pattern suggests that increased in-migration to high-exposure districts is unlikely to drive our findings, consistent with evidence that longer-distance moves in the UK typically reflect employment or education motives rather than housing-market policies.

E Additional Figures and Tables

Figure A.1: Interest Rate Spread for Low-Down Payment Mortgages



Notes: The figure plots the average interest rate spread of 5 percent (low down payment), 10 percent and 15 percent down payment mortgages, relative to 25 percent down payment mortgages over the period 2005–2018. The shaded area indicated the period that HTB is in effect.

Table A.1: Comparison Low- and High-Down Payment Buyers

	Low-Down Payment (EL)	Low- Down Payment (MG)	High-Down Payment
Age (years)	31.35	31.78	37.00
	(6.85)	(6.99)	(9.81)
Gross Income (£)	51,405.30	53,174.28	61,991.40
	(29628.26)	(57801.26)	(825110.7)
Down Payment (£)	12,418.83	19,356.92	103,141.50
	(5986.42)	(22547.29)	(186088.3)
Property Value (£)	229,684.74	195,662.55	284,306.27
	(95596.94)	(128301.9)	(323225.6)

Notes: The table presents the mean and standard deviation (placed below the mean, in brackets) for several key loan and borrower characteristics and for three different home buyer-types: low-down payment buyers that purchased with an HTB Equity Loan (EL); low-down payment buyers that purchased with an HTB Mortgage Guarantee (MG) or other low-down payment mortgage; and high-down payment buyers, who did not use HTB. Buyers include both first-time buyers and home movers. A low-down payment buyer purchases a property with a down payment around 5 percent, and high-down payment buyers includes all other buyers. The statistics are calculated for the post HTB period, from 2013 to 2018. All variables are deflated to 2016 values.

Table A.2: The Help-to-Buy Program Requirements

Requirements	Equity Loan (EL)	Mortgage Guarantee (MG)
Period	Q2 2013 - Q4 2020	Q4 2013 - Q4 2016
Minimum down payment	5%	5%
Government Participation	Government equity loan of 20% (40% in London from 2016)	Government guarantees 20% of mortgage made by lender
Qualifying Property	New builds	Any property
	Value $<$ £600k (£300k in Wales)	Value $<$ £600k
Qualifying Borrowers	First-time buyers and home movers	First-time buyers, home movers and remortgagor
Qualifying Loan	LTI ratio $<$ 4.5	LTI ratio $<$ 4.5
	Ratio excludes EL component	Ratio includes MG component

Notes: The table describes the requirements for the two main Help-to-Buy program schemes: the Equity Loan (EL) scheme and the Mortgage Guarantee (MG) scheme.

Table A.3: Variable Descriptions and Sources

Variable Name	Variable Description	Data Source
Loan-level Variables		
Low Down Payment	Takes the value 1 if down payment less than 10 percent and 0 otherwise	Product Sales Database
District-level Variables		
Exposure	Share of low-down payment mortgages issued between 2005– 2007	Product Sales Database
Home Sales	Total number of mortgaged home sales	Product Sales Database
Car Sales	Total number of new car registrations	UK Department for Transport
Non-tradable Employment	Total number of employees for all firms in service-producing industries	Business Structure Database
Tradable Employment	Total number of employees for all firms in goods-producing industries, including agriculture, mining and manufacturing	Business Structure Database
Home Starts	Total number of individual dwellings for which building work has commenced	UK Department for Levelling UP, Housing and Communities
House Price Growth	Log difference in house price index	Land Registry House Price Index Data
Cohort-level Variables		
Total Household Consumption	Average real weekly household consumption	Living Food and Cost Survey
Non-durable (excl. Home-related)	Average real weekly non-durable consumption	Living Food and Cost Survey
Durable (excl. Home-related)	Average real weekly durable expenditures	Living Food and Cost Survey
Home-related Expenditures	Average real weekly home-related expenditures	Living Food and Cost Survey
Gross Household Income	Average real weekly household income	Living Food and Cost Survey
Household-level Variables		
Total Household Consumption	Average real monthly household consumption	Money Dashboard
Non-durable (excl. Home-related)	Average real monthly non-durable consumption	Money Dashboard
Durable (excl. Home-related)	Average real monthly durable expenditures	Money Dashboard
Home-related Expenditures	Average real monthly home-related expenditures	Money Dashboard

Table A.4: Industry Definitions for Employment Measures

SIC-07 Class	SIC-07 Description	Industry Classification
01.11-03.22	Agriculture, forestry and fishing	Tradable
05.10-09.90	Mining and quarrying	Tradable
10.11-33.20	Manufacturing	Tradable
33.13	Repair of electronic and optical equipment	Non-tradable
33.15	Repair and maintenance of ships and boats	Non-tradable
45.11-45.19	Sale of motor vehicles	Non-tradable
45.32	Retail trade of motor vehicle parts and accessories	Non-tradable
45.40	Sale, maintenance and repair of motorcycles and related parts and accessories	Non-tradable
47.11-47.99	Retail trade, except of motor vehicles and motorcycles	Non-tradable
56.10-56.30	Food and beverage service activities	Non-tradable
35.11-35.30	Electricity, gas, steam and air conditioning supply	Non-tradable
36.00-39.00	Water supply, sewerage, waste management and remediation activities	Non-tradable
45.11-47.99	Wholesale and retail trade; repair of motor vehicles and motorcycles	Non-tradable
49.10-53.20	Transportation and storage	Non-tradable
55.10-56.30	Accommodation and food service activities	Non-tradable
58.10-63.99	Information and communication	Non-tradable
64.11-66.30	Financial and insurance activities	Non-tradable
68.10-68.32	Real estate activities	Non-tradable
69.10-75.00	Professional, scientific and technical activities	Non-tradable
77.11-82.99	Administrative and support service activities	Non-tradable
84.11-84.30	Public administration and defence; compulsory social security	Non-tradable
85.10-85.60	Education	Non-tradable
86.10-88.99	Human health and social work activities	Non-tradable
90.01-93.29	Arts, entertainment and recreation	Non-tradable
94.11-96.09	Other service activities	Non-tradable
97.00-98.20	Activities of households as employers; undifferentiated goods and services producing	Non-tradable
99.00	Activities of extraterritorial organizations and bodies	Non-tradable

Table A.5: Correlation Between Help-to-Buy Exposure and District Variables

	All Districts	Excl. London
District-level Variables	(1)	(2)
Log Unemployment Rate, 2012	0.068***	0.062***
	(0.008)	(0.009)
Log Median Income, 2012	0.141***	0.149***
	(0.019)	(0.019)
Log Average House Price, 2012	-0.180***	-0.191***
	(0.012)	(0.014)
Log Average Loan-to-Income, 2012	0.124***	0.133***
	(0.038)	(0.042)
Change in Unemployment Rate, 2008 to 2011	0.008	0.012
	(0.013)	(0.014)
Change in Median Income, 2008 to 2011	-0.059**	-0.062**
	(0.025)	(0.026)
Change in Average House Price, 2008 to 2011	0.076	0.095*
	(0.048)	(0.049)
Change in Average Loan-to-Income, 2008 to 2011	-0.024	-0.027
	(0.017)	(0.017)
Model Statistics		
N	375	343
R^2	0.815	0.818

Notes: The table presents coefficient estimates for the regression of HTB exposure on different district-level variables and a constant. The specification includes region fixed effects. Standard errors are heteroskedasticity-robust and are shown in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Table A.6: The Effect of Help-to-Buy on Internal Migration

	All Districts	Excl. London	PSM
	(1)	(2)	(3)
$\text{Pre}_t \times \text{Exposure}_d$	0.197	0.216	0.790
	(0.372)	(0.319)	(0.747)
$\text{Post}_t \times \text{Exposure}_d$	-0.966**	-0.783***	-0.927
	(0.377)	(0.293)	(0.750)
Model Statistics			
N	3790	3460	1820
R^2	0.636	0.624	0.586
Fixed Effects			
District	Yes	Yes	Yes
Region × Time	Yes	Yes	Yes

Notes: This table reports the effect of HTB on net internal migration flows. The dependent variable is the district-level net migration rate, defined as the number of individuals moving into district d from another UK district minus the number moving out of district d, scaled by the district's population. Pre is a dummy equal to 1 for 2009–2011 and Post a dummy equal to 1 for 2013–2018, with 2012 as the omitted base year. Exposure is the district share of low–down payment mortgages during 2005–2007. Column (1) includes all districts, column (2) excludes London, and column (3) restricts the sample to propensity score matched districts. Bootstrapped standard errors, clustered by district and year, are reported in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Table A.7: The Effect of Help-to-Buy on Home Sales by Buyer-type

	Buyer-type							
	First-time				Younger			
	All Districts	All Districts	Excl. London	PSM	All Districts	All Districts	Excl. London	PSM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Pre}_t \times \text{Exposure}_d$	-0.119				-0.119			
	(0.075)				(0.073)			
$\text{Post}_t \times \text{Exposure}_d$	0.467***				0.286***			
	(0.085)				(0.090)			
$\text{Post}_t \times \text{Exposure}_d \times \text{Buyer-type}_b$	0.733***	0.733***	0.699***	1.181***	1.039***	1.039***	0.867***	1.481***
	(0.118)	(0.051)	(0.046)	(0.118)	(0.169)	(0.059)	(0.057)	(0.118)
Model Statistics								
N	7580	7580	6920	4080	7580	7580	6920	4080
R^2	0.904	0.979	0.980	0.975	0.854	0.976	0.979	0.973
Control Variables								
$\text{Post}_t \times \text{Buyer-type}_b$	Yes	No	No	No	Yes	No	No	No
$\text{Exposure}_d \times \text{Buyer-type}_b$	Yes	No	No	No	Yes	No	No	No
Fixed Effects								
District	Yes	No	No	No	Yes	No	No	No
Region × Time	Yes	No	No	No	Yes	No	No	No
Buyer-type_b	Yes	No	No	No	Yes	No	No	No
District×Time	No	Yes	Yes	Yes	No	Yes	Yes	Yes
$\text{District}\times\text{Buyer-type}_b$	No	Yes	Yes	Yes	No	Yes	Yes	Yes
$\text{Time}\times\text{Buyer-type}_b$	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Notes: This table reports the effect of HTB on home sales across buyer types. The dependent variable is the number of mortgaged home sales by buyer type. Pre is a dummy equal to 1 for 2009–2011 and Post a dummy equal to 1 for 2013–2018, with 2012 as the omitted base year. Exposure is the district share of low–down payment mortgages during 2005–2007. Columns (1)–(4) report results for first-time buyers and home movers, respectively; columns (5)–(8) distinguish younger (20–39 years old) and older (40–59 years old) buyers. Columns (1), (2), (5), and (6) include all districts; columns (3) and (7) exclude London; and columns (4) and (8) restrict the sample to propensity score matched districts. Bootstrapped standard errors, clustered by district and year, are reported in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Table A.8: The Effect of Help-to-Buy on Car Sales—Robustness

	All Districts (1)	Excl. London (2)	PSM (3)
$\text{Pre}_t \times$ Exposure	-2.686	-3.024	-1.504
	(2.669)	(2.883)	(4.838)
$\text{Post}_t \times$ Exposure	7.860***	8.408***	8.237**
	(2.429)	(3.124)	(3.727)
Model Statistics			
N	3790	3460	1820
R^2	0.913	0.912	0.911
Fixed Effects			
District	Yes	Yes	Yes
Region $\times$ Time	Yes	Yes	Yes

Notes: This table reports the effect of HTB on car sales. The dependent variable is the number of new car registrations in each district and year. Pre is a dummy equal to 1 for 2009–2011 and Post a dummy equal to 1 for 2013–2018, with 2012 as the omitted base year. Exposure is the district share of low–down payment mortgages during 2005–2007. Column (1) includes all districts, column (2) excludes London, and column (3) restricts the sample to propensity score matched districts. Bootstrapped standard errors, clustered by district and year, are reported in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Table A.9: Balance Test for Help-to-Buy Exposure

	Exposure	Car Sales	Pass	Exposure	Consumption	Pass
District-level Variables	(1)	(2)	(3)	(4)	(5)	(6)
Log Unemployment Rate, 2012	0.068***	-0.085	Yes	0.132***	3.110	Yes
	(0.008)	(0.206)		(0.002)	(1.935)	
Log Median Income, 2012	0.141***	0.463	Yes	0.021***	-2.129	Yes
	(0.019)	(0.496)		(0.003)	(2.338)	
Log Average House Price, 2012	-0.180***	-0.247	Yes	-0.117***	2.353	Yes
	(0.012)	(0.308)		(0.002)	(1.740)	
Log Average Loan-to-Income, 2012	0.124***	-1.165	Yes	0.126***	-5.232	Yes
	(0.038)	(1.375)		(0.014)	(11.617)	
Change in Unemployment Rate, 2008 to 2011	0.008	-0.118	Yes	0.098***	6.426	Yes
	(0.013)	(0.615)		(0.007)	(4.848)	
Change in Median Income, 2008 to 2011	-0.059**	0.448	Yes	0.068**	-12.692	Yes
	(0.025)	(0.725)		(0.029)	(24.890)	
Change in Average House Price, 2008 to 2011	0.076	-1.328	Yes	-0.490***	7.676	Yes
	(0.048)	(1.237)		(0.032)	(29.034)	
Change in Average Loan-to-Income, 2008 to 2011	-0.024	0.404	Yes	0.379***	3.853	Yes
	(0.017)	(0.499)		(0.007)	(3.628)	
Owner Share, 2012				0.001	0.307	Yes
				(0.001)	(1.224)	
Log Household Head Age, 2012				-0.011	-13.592**	Yes
				(0.010)	(5.615)	
Log Household Size, 2012				0.002	-7.868***	Yes
				(0.002)	(1.425)	
Log Number of Children, 2012				-0.001	1.498***	Yes
				(0.001)	(0.530)	
Model Statistics						
N	375	375		80	80	
R^2	0.815	0.029		1.000	0.466	
Fixed Effects						
Region	Yes	Yes		n/a	n/a	
Birth Cohort	n/a	n/a		Yes	Yes	

Notes: This table reports balance tests for HTB exposure. The dependent variable in columns (1) and (4) is Exposure, defined as the district share of low–down payment mortgages during 2005–2007. In column (2), the dependent variable is the cumulative log change in new car sales from 2013 to 2018 relative to 2012 In column (4), the dependent variable is the cumulative log change in total household consumption from 2013 to 2018 relative to 2012. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.

Table A.10: The Effect of a Home Purchase on Household Consumption—Robustness

	Baseline (1)	Income Control (2)	Child-related Check (3)
Avg. Treatment-on-the-Treated	114.7561***	115.8635**	107.4488**
	(33.1390)	(40.0098)	(35.2438)
Model Statistics			
N	142656	91583	130218
Fixed Effects			
Household	Yes	Yes	Yes
Region × Time	Yes	Yes	Yes

Notes: The table reports the average treatment effect of the treated (ATT) obtained by averaging imputed treatment effects across all post-purchase months. Column (1) presents our baseline estimates for total consumption. Column (2) includes monthly income as a control variable. Column (3) excludes households that experienced a statistically significant change in their mean child-related expenditures, comparing the 24 months prior to the purchase with the 24 months after the purchase. All regressions include household and region-time fixed effects. Standard errors are clustered at the district level. ***, **, and * indicate statistical significance at the 1 percent, 5 percent and 10 percent confidence level, respectively.