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On the economic impacts of mortgage credit expansion policies: Evidence from help to buy☆

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ABSTRACT

We take advantage of two spatial discontinuities in Britain's Help to Buy (HtB) scheme to explore the effectiveness and distributional implications of mortgage credit expansion policies. Employing a Difference-in-Discontinuities design, we find that HtB significantly increased house prices and had no detectable effect on construction volumes in severely supply constrained and unaffordable Greater London. Conversely, HtB did increase construction numbers without a noticeable effect on prices near the English/Welsh border, an affordable area with comparably lax supply conditions. While HtB did not help would-be-buyers in already unaffordable areas, it boosted the financial performance of developers participating in the scheme.

1. Introduction

Government policies directed at stimulating demand or supply in mortgage markets are widespread throughout the world. Examples of credit market interventions include mortgage interest deductions in the United States, India or Sweden, mortgage guarantees in the United States or the Netherlands, and government loans for home purchases in France or the United Kingdom. Most of these interventions have the explicit goal of making homeownership more affordable and thus accessible. In a context in which housing affordability problems are increasingly pervasive, especially in large desirable cities, new policies are discussed – and implemented – frequently.

Many recent housing and credit expansion policies also tie homeownership subsidies to the purchase of newly built homes. Examples include policies implemented in countries as diverse as Australia, Ireland, Mexico, and Lithuania, in addition to the policy investigated in our paper. Governments thereby seek to stimulate new housing construction, which in turn should further aid making owner-occupied

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housing more affordable.

In this paper, we employ a unique setting – spatial discontinuities in the generosity of an equity loan scheme that is tied to the purchase of newly built homes[1] – to shed light on the effectiveness and distributional effects of such policies.

In April 2013, the British government launched a new flagship housing credit policy: Help to Buy (HtB). The program was initially implemented in England, but Welsh and Scottish versions were put in place shortly thereafter. We set out to explore the causal impact of HtB on housing construction, house prices, and the financial performance of residential developers who participated in the scheme. To do so, we focus on the HtB 'Equity Loan Scheme', which provides an equity loan for up to 20 % of the housing unit's value – or 40 % within the Greater London Authority (GLA) – to buyers of new build properties. The Equity Loan Scheme is by far the most salient and popular of the four HtB schemes and the one requiring the biggest budget. It is often referred to simply as "Help to Buy" and henceforth, unless we note otherwise, when we refer to HtB we mean the Equity Loan Scheme.[2]

HtB expands housing credit and thus increases demand for housing. To explore how such a positive demand shock in the housing market affects construction and prices, we develop a simple theoretical framework with heterogeneous households and credit constraints. Our model predicts that the impact of the policy depends crucially on the responsiveness of supply to prices. In a setting with responsive supply, HtB can be expected to mainly stimulate construction numbers as intended by the policy.[3] However, when supply is unresponsive (i.e., regulatory constraints or physical barriers to residential development impede a supply-response), the effect of the policy may be mainly to increase house prices, with the unintended consequence of making housing less rather than more affordable.

In our empirical analysis, we implement a Difference-in-Discontinuity design to compare changes in house prices and construction activities across jurisdictional boundaries. We separately analyze properties sold on either side of the GLA boundary and on either side of the English/Welsh border. In both cases we only consider housing purchases close to the respective boundaries. In Wales the scheme only applied to a subset of the properties that were eligible in England. Likewise, the London scheme that was implemented in 2016 offered larger government equity loans (as a share of house values) for dwellings inside the GLA compared to those available for purchase outside this region. Our main estimates exploit these spatial discontinuities to study the effect of HtB on house prices and construction activity.

We focus on the spatial discontinuities in the generosity of the HtB-scheme near the GLA boundary and the English/Welsh border for three reasons. First, our research design requires spatial discontinuities in the scheme's conditions, which can be found at these boundaries. Second, no other relevant policy that could differentially affect housing market outcomes in the GLA, in England, or in Wales was introduced in the years around the introduction of HtB.[4] Third, the two areas differ starkly in their regulatory land use restrictiveness and in barriers to physical development: While the GLA is the most supply constrained and the least affordable area in the UK – and arguably one of the most supply constrained areas in the world – housing supply is comparably responsive to demand shocks near the English/Welsh border.

Consistent with our theoretical predictions, we find that differences in the intensity of the HtB-treatment have heterogeneous effects depending on local supply conditions. In the GLA, the introduction of the more generous London version of the Equity Loan Scheme led to a significant increase in prices for new build units of nearly 8 %. However, it had no appreciable effect on construction activity. Conversely, in the areas around the English/Welsh border, we find a significant effect on construction activity but none on prices. The introduction of the more generous HtB-price threshold on the English side of the border increased the number of new build sales per ward and month by about 0.4 compared to the Welsh side. We confirm these findings in an event study analysis of dynamic effects, which allows us to trace the impact of differences in the scheme on both prices and construction over time.

Our results indicate that HtB fails to trigger more construction activity, but instead causes house prices to increase inside the GLA, precisely the region that is most affected by the 'affordability crisis'. While access to homeownership is improved in principle (credit constraints are relaxed), the present value of the financial burden associated with the purchase of a home further increases. In fact, our back-of-the-envelope calculations reveal that house prices of newly built units inside the GLA increased by about twice as much as the interest rate subsidy induced by the favorable rates on HtB lending. This finding is consistent with full or nearly full capitalization of the implicit interest rate subsidy into higher house prices plus an additional positive price effect due to a credit relaxation-induced demand stimulus.

Our findings have distributional implications. The main beneficiaries of HtB may be developers and landowners, who are in a good position to take advantage of the policy-induced increase in demand for new build housing, rather than struggling first-time buyers. To evaluate the effect of the policy on developers, we provide evidence indicating that the scheme has caused an improvement of the financial performance of developers participating in the scheme: larger revenues as well as higher gross and net profits.

We subject our main empirical findings to a long list of robustness checks. First, and perhaps most crucially, we demonstrate – employing two alternative strategies – that our findings are not driven by displacement of demand across the boundaries. Second, we conduct two placebo tests. The first utilizes the boundary of the Greater Manchester area, which is not subject to a spatial discontinuity in the HtB-scheme. The second displaces the GLA boundary and the English/Welsh border spatially. These tests provide support for our identification strategy. Third, we show that our results are not sensitive to using the full sample of new build properties rather than just using data for properties that are HtB-eligible. Fourth, we show that our findings are not sensitive to (i) choosing alternative bandwidths, (ii) making alternative assumptions about the length of the construction lag, or (iii) varying the period used in the analysis around the English/Welsh border.

Finally, we investigate how HtB affected other housing market outcomes. We show that developers altered the (price range of) properties

[1] Policies that tie homeownership subsidies to the purchase of new build properties are increasingly widespread (OECD, 2019). However, the research on this type of policies is scant. One exception is Agarwal et al. (2020) who study the impact of a stamp duty relief on purchases of newly built homes in Sydney, Australia, finding significant bunching just at and below the threshold price up to which the relief was granted.

[2] At the time of implementation, HtB consisted of four schemes: The Equity Loan Scheme, Mortgage Guarantees, Shared Ownership, and Individual Savings Accounts (ISA). All four schemes aim to help credit constrained households to buy a property. The Mortgage Guarantees scheme ceased at the end of 2016. The HtB-ISA closed for new entrants in November 2019 and any bonus must be claimed by 2030. In April 2017, the British government introduced a new Lifetime ISA scheme. In contrast to HtB ISA, it is only open to individuals aged 18-39 and the money saved can also be used to fund a pension. None of these schemes will confound our results because they do not vary at the spatial boundaries we investigate.

[3] The then Chancellor George Osborne was explicit when announcing the 'Equity Loan Scheme' – available only to buyers of *newly built* homes – that a main aim of the scheme is to stimulate housing construction. He noted: *"This [the HtB equity loan scheme] means more homes … because Britain has to get building"*.

[4] One might be concerned that the reform of the Stamp Duty Land Tax (SDLT) during 2014 – i.e., the removal of the slab element of the tax – could differentially affect housing market outcomes on either side of the two boundaries. However, this reform was implemented equally across England and Wales and would be expected to have similar impacts on similar markets.

they built to meet HtB eligibility conditions by documenting significant price bunching effects, after the implementation of the policy, right below the respective eligibility thresholds in England and Wales. We also find that HtB led to a positive price spillover on the market for existing homes, indicating that the increase in demand induced by the policy was not restricted to the new build market.

Our paper relates to previous studies looking at the effects of credit conditions and credit market policies on housing markets. Previous research has mainly focused on the effect of credit supply on house prices (see Ortalo-Magne and Rady, 1999; Mian and Sufi, 2009; Duca et al., 2011; Favara and Imbs, 2015; Justiniano et al., 2019; Anenberg et al., 2019). These and other studies provide theoretical and empirical credence to the notion that expansions in credit supply may lead to higher prices.

On the policy evaluation front, a few studies have explored the impact of demand subsidies on housing market outcomes. Hilber and Turner (2014) examine the impact of the U.S. mortgage interest deduction (MID). They find that the MID boosts homeownership attainment only of higher income households in markets with lax land use regulation. In tightly regulated markets with inelastic long-run supply of housing, the MID lowers homeownership attainment, presumably because higher house prices also raise down payment constraints of would-be-buyers. Sommer and Sullivan (2018) estimate a dynamic structural model of the housing market to study the effect of removing the MID and predict this would result in a substantial reduction in house prices. Finally, a related literature has explored the effect of credit expansion policies in the US – such as FHFA guarantees and GSE lending – on homeownership attainment, finding mixed results.[5]

This paper also links to previous research on housing and land supply, including work on the effects of supply constraints on the responsiveness of housing markets to economic shocks (Hilber and Vermeulen, 2016), the origin of supply restrictions (Saiz, 2010; Hilber and Robert-Nicoud, 2013) and their consequences (see Gyourko and Molloy, 2015 and the references therein). We contribute to this literature by studying, in depth, the effect on housing supply of arguably the most important new British housing policy since the implementation of Right to Buy in 1980.

Finally, we are not the first to study the HtB policy. Finlay et al. (2016), combining qualitative and non-causal quantitative methods, estimate that since its introduction HtB has generated 43 % additional new homes. Szumilo and Vanino (2018) provide evidence that HtB increased the lending volume in Greater London. In a similar vein, exploiting geographic variation in exposure to HtB, Tracey and van Horen (2021) find that HtB increased home sales and consumption of non-housing related items. Benetton et al. (2018) utilize the HtB equity loan scheme to investigate the pricing of mortgage credit, demonstrating that a lower down-payment is associated with a higher interest rate at origination, and a higher ex-post default rate. Lastly, Benetton et al. (2022) explore the effect of HtB on households' house purchase and financing decisions. They find that households take advantage of an increase in the HtB maximum equity limit to buy more expensive properties.

Our paper makes several important contributions to these strands of the literature. It is the first paper to study the causal impact of credit expansion policies on (i) the price of newly built homes, (ii) construction volumes, and (iii) profits of participating developers. These dimensions (and inferring causal effects rather than just establishing correlations) are especially important in the case of HtB. The first two dimensions are important because two of the central aims of this scheme are to improve housing affordability for would-be-buyers and to encourage developers to build more homes. The third dimension is important insofar as it helps us to evaluate the distributional effects of the policy. Our paper is also the first to demonstrate that the effectiveness of HtB varies spatially and crucially depends on local supply conditions. We show that the policy is ineffective – arguably even counterproductive – in the most supply constrained region in England (the GLA) where the housing affordability crisis is most severe and newly built housing is most desperately needed. The ineffectiveness of the policy in the GLA arises from two of the causal effects we study here: the unintended price effects (driven by demand stimuli in a setting with inelastic supply) and the lack of an effect on construction (driven by the inelastic supply).

The rest of this paper is structured as follows. Section 2 describes the details of the HtB Equity Loan Scheme and provides a simple theoretical framework to guide the empirical analysis. Section 3 presents the data sources and discusses the housing supply conditions in the two regions we use in our empirical analysis. Section 4 discusses our empirical strategy in detail and presents our main results for the effect of the policy on housing prices, construction volumes and developer performance. Section 5 provides robustness checks for these findings, while Section 6 presents additional results on price bunching and the effect of HtB on the transaction price of existing homes. In Section 7 we gauge the magnitude of the HtB-interest subsidy and compare it to the HtB-induced increase in the price of newly built homes. Section 8 concludes.

2. Background and theoretical framework

2.1. Background: the help to buy equity loan scheme

Since the launch of the English version of the HtB Equity Loan Scheme in April 2013 until December 2019, over 263,000 properties were bought in England with a government equity loan provided by the scheme. The total value of these loans is £15.3 billion, with the value of properties purchased under the scheme totaling £70 billion (Ministry of Housing, Communities and Local Government, 2020).[6]

The English HtB scheme offers government loans of up to 20 % of a unit value to households seeking to buy a new residence. It is available to both first-time buyers and home-movers, but it is restricted to the purchase of new build units with prices under £600,000.

Given that the prevalent maximum Loan-to-Value (LTV) ratios offered by British banks to first-time buyers were around 75 % during this period, the scheme offers a substantial reduction in the down payment needed to buy a property. With the government loan covering part of the down payment, buyers are only required to raise 5 % of the property value as a deposit. The explicit goal of the Equity Loan Scheme is that this reduction in the deposit required to the borrower helps households overcome credit constraints.

The Equity Loan Scheme can also help reduce household borrowing costs by reducing interest payments on the combined loan. This occurs via two channels. The first is that no interest or loan fees on the equity loan must be paid by the borrower for the first five years after the purchase of the house. Subsequently, there is a charge, which depends on the rate of inflation. We calculate the implied subsidy provided through this channel in Section 7. Secondly, by raising the combined

[5] See for example Bostic and Gabriel (2006), Gabriel and Rosenthal (2010) and Fetter (2013). Berger *et al.* (2020) find positive effects of the US First-Time Homebuyer Credit on housing sales, prices and transitions into homeownership. See Olsen and Zabel (2015) for a review of the previous US literature. Hilber and Schöni (2016) provide a comparison of US policies with policies in the UK and Switzerland. An evaluation of the French *Pret a Taux Zero* policy – which provides a down payment subsidy to low and middle-income first-time buyers – can be found in Gobillon and le Blanc (2008).

[6] The Ministry of Housing, Communities and Local Government (2020) provides a comprehensive overview and numerous summary statistics relating to the HtB Equity Loan Scheme in England. In the case of Wales, since the launch of the Welsh HtB in January 2014 until December 2019, 10,215 properties were bought with a government equity loan (StatsWales 2020).

deposit to 25 %, the equity loan keeps borrowers away from high-LTV-high-interest products that are available in the commercial mortgage market.[7]

The government equity loan can be repaid at any time without penalty. The equity loan is interest-only so, unless borrowers want to sell the property, they do not need to repay the loan at all. When the property is sold, the government will reclaim its 20 % equity stake of the sale price. The government thus participates in capital gains and losses.[8]

In our analysis we exploit differences between the English, Welsh, and London versions of the Equity Loan Scheme. Regional differences in the scheme are summarized in Table 1. The Welsh scheme was introduced in January 2014 and provided support for the purchase of properties with prices under £300,000.[9] The London scheme was introduced in February 2016 and offered an equity loan of up to 40 % of the unit's price for properties under £600,000 located within the GLA. As we will show in Section 6, the price constraints in both countries, England and Wales, were binding, with substantial bunching of new property sales at these price points emerging after the introduction of the policy.

2.2. Theoretical framework

In this sub-section we develop a theoretical framework to guide our empirical analysis.[10] Specifically, we develop a simple model of the housing market with heterogeneous households and credit constraints. It is a partial equilibrium model in that we abstract from potential effects of changing credit conditions for new builds on the price of the existing stock. We will abstract from the presence of a market for existing homes in this exposition.

The framework illustrates how a relaxation of credit conditions affects housing quantities and prices, and how these effects depend on the costs of developing new stock. A relaxation of credit constraints leads to both an increase in prices and an expansion in quantities. Under suitable assumptions – made explicit below – the relative magnitude of the two effects depends on the responsiveness of supply to prices. For low (high) supply responsiveness, the price effect is stronger (weaker) and the quantity effect is weaker (stronger). The theoretical insights from this framework can be summarized by the cross-elasticities of quantity and prices taken over the credit conditions parameter and a building cost shifter. We also show that a relaxation of credit constraints, conditional on developers having some market power, can be expected to increase developer profits.

Table 1
Equity loan scheme in different regions in UK (applies to new build only).

Region	Introduction date	House value up to	Loan from government
England	April 2013	£600,000	Up to 20 %
London	February 2016	£600,000	Up to 40 %
Wales	January 2014	£300,000	Up to 20 %

Suppose a two-period economy with a unit mass of households with preferences over a numeraire consumption good c and housing h, as given by a period utility $u(c,h)$ which is continuous, strictly increasing, and differentiable in both arguments. Assume in addition that $\lim_{h\to\infty} u(c,h) = \infty$ if $c > 0$ and $u(c, h) > 0 \; \forall \; c, \; h > 0$. Households enjoy utility at the end of periods 1 and 2, and the discount factor is $\beta > 0$.

Households can only obtain a fixed amount of housing consumption $h > 0$ if they buy a new unit and obtain housing consumption normalized to 0 otherwise. We can think of these alternatives as a choice between renting and buying. In this interpretation, this formulation is similar to those used in models featuring warm glow from ownership (Iacoviello and Pavan, 2013; Kiyotaki et al., 2011; Carozzi, 2020).

Households receive an endowment e in period 1 and a location specific income w in period 2, which can be used for consumption or to buy property. Households are heterogeneous in the initial endowment e, which is continuously distributed over the unit interval $[0,1]$ with cumulative density function F_e.

New build units are homogeneous and can be bought in period 1 for an endogenous price P. Credit is available for the purchase of property, yet a minimum down payment is required corresponding to a fraction $(1-\gamma)$ of the property value. Credit and savings pay interest r. We assume that $w > \frac{\gamma}{1-\gamma}(1+r)$ which ensures that, for sufficiently large h, demand for new build units is determined solely by the credit constraint.[11] Hence, demand is given by the mass of agents that can afford a down payment $Q_D = 1 - F_e((1-\gamma)P)$. Note that demand is downward sloping as the function F_e is strictly increasing.

There is a unit of developable land which can be used to build – at most – a unit of housing units. Development costs for new build units depend on local supply conditions and are heterogeneous across land plots. We assume that the development costs are uniformly distributed in the $[0,\nu]$ interval, with $\nu(1-\gamma) > 1$. We assume land is owned by price-taking firms which will develop their plot if the price is smaller than or equal to development costs. As a result, the new build inverse supply curve for competitive developers is given by $P = \nu Q$. High values of ν correspond to higher average development costs and, therefore, to a weaker response of quantities to a change in prices. Conversely, low values of ν are associated with a more responsive supply schedule. We can substitute this expression in demand to obtain an implicit definition for new build equilibrium quantities:

$$Q^* = (1 - F_e((1-\gamma)\nu Q^*)) \tag{1}$$

By differentiating this expression, we can obtain the following four statements regarding the responses of equilibrium prices and quantities to changes in credit conditions (γ), and development costs (ν):

$$\frac{dQ^*}{d\nu} < 0 \quad \frac{dP^*}{d\nu} > 0 \quad \frac{dQ^*}{d\gamma} > 0 \quad \frac{dP^*}{d\gamma} > 0 \tag{2}$$

The first two inequalities indicate that an increase in development costs results in a reduction in equilibrium quantities and an increase in equilibrium prices.[12] The latter two inequalities imply that both quantities and prices respond positively to an expansion of credit. This follows from the increase in demand associated with a credit expansion. The extent to which a change in credit conditions will translate into a change in quantities or prices depends on both the distribution of the initial endowment F_e and the price responsiveness of supply (through ν).

Proposition 1 – The effect of a credit expansion on prices and

[7] This enables households to gain access to more attractive mortgage rates from lenders who participate in the scheme. Eligibility conditions require borrowers to have a suitable credit score and to be able to cover the monthly repayments. Benetton *et al.* (2018) report that lenders adjust mortgage interest rates of HtB borrowers in response to additional default risk associated with lower down payments, but this adjustment is small: it only accounts for 10% of the difference in market interest rates between 75%- and 95%-LTV mortgages.

[8] This also implies that buyers expecting high capital gains may opt not to participate in the HtB-scheme despite the interest rate subsidy.

[9] Scotland also introduced a HtB Equity Loan Scheme in 2013; however, we are not able to exploit the discontinuities at the English/Scottish border. This is because the Scottish Land Registry did not identify new build units until 2018.

[10] The model builds on Hilber and Vermeulen (2016) who consider a similar setting but abstract from the role of credit conditions.

[11] Note that $P \leq \frac{1}{1-\gamma}$. Assumption $w > \frac{\gamma}{1-\gamma}(1+r)$ will therefore ensure that in period 2 all agents are able to pay the remaining part of any loans taken for the purchase of a property, including interest. Large enough h ensures buying property in period 1 is incentive compatible for all households. See the theoretical Appendix.

[12] See proofs in theoretical Appendix.

quantities depends on the distribution of development costs, as measured by ν. Specifically, if e is uniformly distributed and $\nu(1-\gamma) > 1$, then $\frac{d^2Q}{d\gamma d\nu} < 0 < \frac{d^2P}{d\gamma d\nu}$.

Proof: See theoretical Appendix

Proposition 1 states that, under the specified parameter conditions, the effect of credit expansion on quantities will be smaller, and the effect on prices larger, in higher ν markets (i.e., in markets with tighter supply constraints and thus more inelastic long-run supply of housing). This intuition will help us account for regional differences in our estimates of the impact of HtB reported in the next sections.

The assumption of uniform endowments is a sufficient condition, but it is not necessary. Intuitively, this assumption results in linear demand curves. Without linear demand curves we can have that either the first or the second inequality is not satisfied. Hence, the conclusions derived from the uniform case may or may not follow with more general assumptions on the distribution of endowments. This *ex-ante* ambiguity partly motivates the empirical analysis below.

The statements in the derivatives in (2), as well as Proposition 1, are derived for the case of competitive land and housing markets.

Proposition 2 – A credit expansion will result in an increase in total developer profits. That is, the sum of equilibrium profits across developers $\Pi(P)$ is increasing in γ.

Proof: See theoretical Appendix

This result hinges on the assumption that developers own all land, preventing entry from other firms from eroding profits. The notion that developers have some market power is reasonable in our case, as the residential construction market is characterized by substantial concentration and high returns. We test empirically whether Proposition 2 is satisfied in Section 4.5.

3. Data and descriptive statistics

3.1. Main data sources

Our empirical analysis employs geo-located data on housing sales in England and Wales, including information on unit characteristics and transaction prices. Our main data source is the Land Registry Price Paid Data (or short 'Land Registry'), which covers most residential and all new build residential transactions in England and Wales. The dataset includes property sales from 2010 to 2019, recording the transaction price, postcode, address, the date the sale was registered (which proxies for the transaction date), and categorical data on dwelling type (detached, semi-detached, flat or terrace), freehold or leasehold and whether the home is a new build property. We use the National Statistics Postcode Lookup Directory to match properties in the dataset to coordinates and wards.

Between 2010 and 2019, the Land Registry recorded 948,553 sales of new housing units. We use these as a proxy for construction activity. All sales are geo-coded using address postcodes. In our spatial discontinuity analysis, we use new build transactions taking place near the GLA boundary and near the English/Welsh border. We select all new build transactions within 5km from the GLA boundary and within 10km from the English/Welsh border after removing a small set of observations that we identify as being sold in bulk between developers.[13] We use a 10km bandwidth for the latter exercise because transactions and populations near the English/Welsh border are sparser. According to the 2011 Census, the average number of persons per hectare for the wards used in our GLA boundary sample is 34.3, while the average number of persons per hectare for the wards used in our English/Welsh border sample is 10.2. We also use areas near the Greater Manchester boundary in a placebo test. Greater Manchester is the second largest travel to work area in the United Kingdom and arguably the one most comparable to London.

In addition, we use Energy Performance Certificate (EPC) data that contains information on the floor area and other physical characteristics of newly built units. We match this data to the Land Registry to augment the latter dataset with additional information on the transacted newly built units.[14] Demographic neighborhood characteristics at ward level are collected from the 2011 Census. We include the fraction of married residents, and the fraction of residents with level-4 and above educational qualifications as controls in some specifications.

3.2. Descriptive statistics

In Panel A of Table 2, we present summary statistics for the sample of new build sales located within 5 kilometers of the GLA boundary and taking place between January 2010 to December 2019. The sample consists of 41,357 transactions of newly built properties. The average price is £389,440, and the average size of these properties is 86.8 square meters. Panel B of Table 2 shows the descriptive statistics for the sample of new build transactions within 10 kilometers of the English/Welsh border taking place between 2010 to 2019. The average price in this region is £232,536, and the average size of these properties is 101.4 square meters.

When estimating the effect of the policy on housing construction, we assemble a ward by month panel using data from January 2010 to December 2019. We obtain ward-level observations by aggregating from individual new build sales. Panels C and D of Table 2 document the descriptive statistics of our estimation sample for the construction effect. The datasets for the GLA boundary-area and the English/Welsh border-area consist of 411 wards and 204 wards, respectively. The propensity to have at least one new build transaction in any month and ward is 0.2 for both the GLA sample and the English/Welsh sample. On average, 0.8 new units are built each month in a ward near the GLA boundary and 0.5 near the English/Welsh border.

To conduct our analysis of developer performance, we construct a developer/construction company panel that covers 78 companies during the period 2010 to 2019. The panel includes financial information of these companies from Orbis. It also includes information on whether the companies are registered with an HtB agency. A builder must be registered with one of the regional government offices managing the scheme for its properties to be eligible for a HtB equity loan. The full sample of developers is our *Difference-in-Differences* sample. It is obtained after combining a list of the main builders in the United Kingdom from Zoopla – one of the main property websites in the country – with financial data

[13] The number of transactions for the resulting samples are reported in Appendix Table A1. We exclude a total of 1041 sales. These are transfers to non-private individuals, in bulk of over 20 sales within the same building and month. We exclude these from our sample as they likely correspond to transfers between companies within a conglomerate and thus do not represent genuine market transactions. This exclusion does not affect any of the main results in the paper.

[14] EPCs provide information on buildings that consumers plan to purchase or rent. Since 2007 an EPC has been required whenever a home is constructed or marketed for social rent, private rent, or sale. We use a dataset that contains all EPCs issued between 2008 and 2020. The dataset includes the type of transaction that triggered the EPC, the energy performance of properties and their physical characteristics. Following Koster and Pinchbeck (2022), we merge the EPC data into the Land Registry using a sequential match strategy. First, we match a Land Registry sale to certificates using the primary address object name (PAON; typically, the house number or name), secondary address object name (SAON; typically, the identification of separate unit/flat), street name, and full postcode. We then retain the certificate that is closest in days to the sale or take the median value of characteristics where there is more than one EPC in the same year as the sale. We repeat this exercise for unmatched properties but allow one of the PAONs or SAONs to be different. Our final round of matching is on the full postcode. The matched dataset provides us total floor area; whether the property has a fireplace or not; total energy consumption and total CO_2 emission of the property.

Table 2
Descriptive statistics: regression sample.

	Observations	Mean	SD	Max	Min
Panel A: London, price effect					
House price (£)	41357	389440	279818	7850000	27720
Inside GLA (HtB treatment)	41357	0.59	0.49	1	0
Post London HtB	41357	0.44	0.50	1	0
Total floor area (m^2)	41357	86.8	48.5	797.5	1.5
Terrace	41357	0.18	0.38	1	0
Flat	41357	0.65	0.48	1	0
Detached	41357	0.08	0.27	1	0
Semi-detached	41357	0.09	0.29	1	0
Leasehold	41357	0.67	0.47	1	0
Energy consumption (kWh/m^2) [1)]	41357	102.2	69.2	1038	-128
Fireplace	41357	0.19	0.39	1	0
CO2 emissions (tons/year) [1)]	41357	1.4	1.1	36.9	-2
Distance to boundary (m)	41357	2515.4	1395.7	4999.3	4.8
Panel B: English/Welsh border, price effect					
House price (£)	11574	232536	111155	1554500	16260
In England (HtB treatment)	11574	0.54	0.50	1	0
Post English HtB	11574	0.78	0.42	1	0
Total floor area (m^2)	11574	101.4	40.7	575	3.5
Terrace	11574	0.19	0.39	1	0
Flat	11574	0.14	0.34	1	0
Detached	11574	0.48	0.50	1	0
Semi-detached	11574	0.20	0.40	1	0
Leasehold	11574	0.26	0.44	1	0
Energy consumption (kWh/m^2) [1)]	11574	106.3	47.8	1076	-31
Fireplace	11574	0.18	0.38	1	0
CO2 emissions (tons/year) [1)]	11574	1.9	1.2	61	-0.5
Distance to boundary (m)	11574	5028.3	2828.4	9981.3	11.2
Panel C: London, construction effect (ward-level sample)					
Number of units constructed	49320	0.84	3.33	93	0
Any new build in ward, by month	49320	0.20	0.40	1	0
Inside GLA (HtB treatment)	49320	0.54	0.50	1	0
Post London HtB	49320	0.29	0.45	1	0
Distance to boundary (m)	49320	2773.4	1631.8	9214	186.7
Panel D: English/Welsh border, construction effect (ward-level sample)					
Number of units constructed	24480	0.47	1.68	73	0
Any new build in ward, by month	24480	0.17	0.38	1	0
In England (HtB treatment)	24480	0.39	0.49	1	0
Post English HtB	24480	0.58	0.49	1	0
Distance to boundary (m)	24480	5439.9	3119.6	14592.7	324.2

[1])Energy consumption and CO2 emissions can have a negative sign if the property makes a net contribution to the energy grid (e.g., because of solar panels).

from Orbis. This list includes residential developers, commercial developers, and construction companies. We present the descriptive statistics for this sample in Panel A of Table 3. The average turnover of these companies is £540 million, and the average net profit before tax is £64 million. In addition, we include hand-coded data on the fraction of properties sold through the scheme from annual reports in a selected sample of 30 residential developers. This is our *intensity* sample, and Panel B of Table 3 documents its descriptive statistics. Finally, to mitigate the concern of more profit-driven developers (with different characteristics) self-selecting into the HtB-scheme, we compute the ratio of HtB completions relative to the number of total new build transactions at the NUTS-1 level as an instrument for the observed HtB-status of developers and link this local HtB-intensity to developers using their headquarters' address information. This is our *HtB completion* sample and covers 69 companies. We report the summary statistics for this sample in Panel C of Table 3.

Table 3
Descriptive statistics: developer sample.

	Observations	Mean	SD	Max	Min
Panel A: HtB dummy sample					
Revenue (million £)	535	539.5	924.9	4874.8	1.8
Gross profit (million £)	535	110.8	212.4	1204.7	0.6
Net profit before tax (million £)	535	64.4	162.5	1090.8	0.03
HtB dummy	535	0.89	0.31	1	0
Post English HtB	535	0.78	0.41	1	0
Panel B: HtB intensity sample					
Revenue (million £)	223	1150	1172.8	4874.8	6.1
Gross profit (million £)	223	223.1	275.8	1204.7	0.7
Net profit before tax (million £)	223	143.1	229.7	1090.8	0.1
HtB intensity	223	0.24	0.20	0.68	0
Post English HtB	223	0.78	0.41	1	0
Panel C: HtB completion sample					
Revenue (million £)	493	573	954.2	4874.8	1.8
Gross profit (million £)	493	117.5	219.4	1204.7	0.6
Net profit before tax (million £)	493	68.5	168.3	1090.8	0.03
HtB completion ratio	493	0.38	0.07	0.45	0.22
Post English HtB	493	0.77	0.42	1	0

3.3. Local supply conditions

Below, we report separate estimates of the impact of the generosity of the HtB schemes obtained from a sample of properties near the GLA boundary, and a sample of properties near the English/Welsh border. We choose these two areas because they both provide an ideal quasi-experimental setting to identify the economic effects of HtB.

One crucial difference between our two focal areas – the area near the GLA boundary and the area near the English/Welsh border – is that the former has overall vastly less responsive supply, driven by both, tighter local planning regulations and a relative scarcity of undeveloped developable land (Hilber and Vermeulen, 2016). As shown above, theory suggests that the positive impact of HtB on house prices should be much larger – and the positive impact on new construction much smaller – in the area near the GLA boundary.

To illustrate the differences in supply conditions between the areas, we employ several measures that capture long-term housing supply constraints. These measures are the share of land designated as green belt (provided by the Ministry of Housing, Communities and Local Government), the average planning application refusal rate taken over the period from 1979 to 2008, the average share of developed developable land, and the average elevation range (all derived from Hilber and Vermeulen, 2016). We calculate these measures for the areas employed in our analysis using Local Planning Authority (LPA)-level data and LPA surface areas as weights.[15] We also report similar descriptive statistics for LPAs around the Manchester boundary, as we use this region in a placebo test (reported in Section 5).

[15] We do not currently have data for LPAs on the Welsh side of the English/Welsh border. We expect that the differences between the GLA and the English/Welsh border area would be even more striking when taking account of the data from the Welsh LPAs.

Table 4 (rows 1 to 4) illustrates the differences in supply conditions between the three areas. The most striking difference between the two focal areas lies in the share of 'green belt' land. Land in green belts is typically off limits for any development (residential or commercial) and thus represents a 'horizontal' supply constraint. This share is 66.5 % for local authorities along the boundary of the GLA but only 3.8 % for English local authorities along the English/Welsh border. Another measure to capture physical supply constraints is the share of developable land already developed. This share is 27.6 % for local authorities along the GLA boundary (with developable land mostly being green belt) but only 6.3 % for English local authorities along the English/Welsh border.

The arguably quantitatively most important long-term supply constraint are restrictions imposed by the British planning system (Hilber and Vermeulen, 2016). The weighted average of the planning application refusal rate is 35.6 % for local authorities along the GLA boundary and 27.2 % for English local authorities along the English/-Welsh border.

While the area near the English/Welsh border is subject to greater topographical (slope related) supply constraints, Hilber and Vermeulen (2016) demonstrate that these constraints, while statistically significant, are quantitatively unimportant in explaining local price-earnings elasticities.

Lastly, it is important to point out that the area near the GLA boundary is not only characterized by vastly more restrictive supply conditions, but these constraints are also significantly more binding in practice, simply because aggregate housing demand there is much stronger. To illustrate this point, consider a ten-story height restriction in the heart of a superstar city such as London and compare it to the same constraint in the desert. The restriction is extremely binding in the former location, while completely irrelevant in the latter.

To explore the differences in supply responsiveness across the three areas further, we employ the estimated coefficients from Hilber and Vermeulen (2016) to compute an implied house price-earnings elasticity. Table 4 (rows 5 and 6) reports our estimated elasticities based on these coefficients. Using the OLS estimates, we find that the price-earnings elasticity along the GLA boundary (0.40) is higher than that of the area along the Greater Manchester boundary (0.28), which in turn is higher than the elasticity near the English/Welsh border (0.25). As two of the three supply constraints measures employed in their estimation, refusal rate and share developed land, are likely endogenous, we employ the instrumental variable strategy proposed in Hilber and Vermeulen (2016).[16] This provides exogenous variation in our supply constraint measures, which we use to re-compute the unbiased price-earnings elasticities. The rank order remains unchanged. The GLA has again the highest elasticity (0.21) followed by Greater Manchester (0.16) and the English/Welsh border area (0.13).

In the next section, we outline our identification strategy and discuss how we measure the impact of HtB on house prices, construction activity and the financial performance of developers.

Table 4
Supply constraints measures and implied price-earnings elasticities.

Region	English/Welsh border	GLA boundary	Greater Manchester boundary
Share of land in green belts	3.77 %	66.5 %	52.6 %
Average refusal rate 1979-2008	27.2 %	35.6 %	25.1 %
Average share of developed land	6.3 %	27.6 %	18.2 %
Average elevation range	476.0	143.9	382.3
Implied price-earnings elasticity (OLS)	0.252	0.403	0.284
Implied price-earnings elasticity (IV)	0.127	0.205	0.164

Notes: The refusal rate, share developed land and elevation range are weighted by the surface area of the Local Planning Authority. Data on refusal rates, share developed land and elevation range come from Hilber and Vermeulen (2016). The green belt shape file comes from the Ministry of Housing, Communities and Local Government.

4. Main empirical analysis

Our empirical strategy is designed to test the impact of HtB on house prices and housing construction by exploiting spatial differences in the intensity of the HtB policy. HtB Wales, which rolled out nine months after the English scheme, offered a government-backed loan for the purchase of new build properties under £300,000, compared to £600,000 in England. Thus, the policy was more generous on the English side of the boundary. There were also differences in the intensity of the HtB policy between the GLA and its surroundings, starting in 2016. In this case, the difference lies in the size of the government-backed loan available to households. London-HtB offers loans of up to 40 % of a new build's value, while this figure is 20 % outside the GLA boundary. We exploit these regional policy differences in a Difference-in-Discontinuities design combining time variation in prices and new build construction with local variation in policy intensity around the regional boundaries.

An alternative identification strategy could be to exploit the price threshold that determines the eligibility for the HtB scheme. This would require conditioning on the outcome of interest, which could lead to selection bias in our estimates. For this reason, we only report the findings of this strategy in our robustness check section.

The samples of new build properties used in the analyses of prices and construction effects near the GLA boundary and the English/Welsh border are illustrated in Panels A and B of Fig. 1, respectively.[17] Our boundary approach is meant to ensure that we are comparing properties affected by similar economic and amenity shocks, as compared to a standard Difference-in-Differences strategy that simply takes whole regions as treatment and control groups. The identifying assumption in both cases can be likened to the typical assumption of parallel trends: in the absence of the policy, prices and construction on either side of the boundary would have followed a parallel evolution over time.

4.1. Graphical illustration

Fig. 2 depicts the evolution of the price of new build homes on both sides of the GLA boundary and the English/Welsh border, respectively,

[16] To address the endogeneity of the refusal rate measure, Hilber and Vermeulen (2016) use the average refusal rate over the entire sample period (1979 to 2008) and employ two instrumental variables: an instrument derived from a policy reform in 2002 – the change in the delay rate pre- vs. post-reform – and the share of votes to the Labour party at the General Election of 1983. To deal with the endogeneity of the share developed measure, they use historic population density in 1911. For a detailed discussion of the identification strategy and the respective identifying assumptions, we refer the interested reader to Hilber and Vermeulen (2016, Sub-section 3.2).

[17] Appendix Figure B1 depicts the corresponding map for our placebo sample of new build sales near the Greater Manchester boundary.

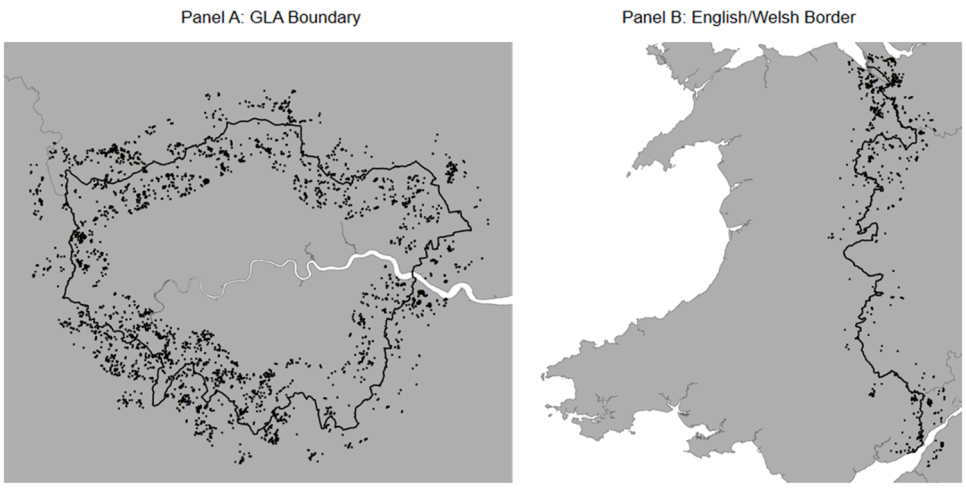

Fig. 1. New Builds near the GLA Boundary and English/Welsh Border
Note: In Panel A, solid black line represents the boundary of the Greater London Authority (GLA). Each of the black dots represents a new build sale taking place during our sample period within 5km of the boundary. In Panel B, solid black line represents the English-Welsh border. Each of the black dots represents a new build sale taking place during our sample period within 10km of the boundary.

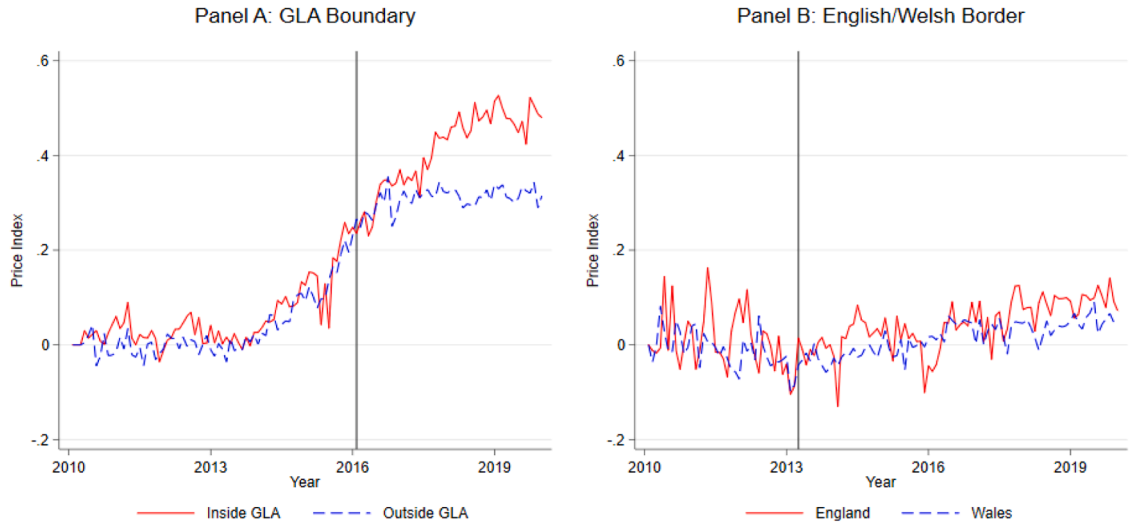

Fig. 2. Price Index for New Build Homes
Note: Price indices for new build units on each side of the corresponding boundary, normalized so that January 2010 is equal to 0. In Panel A, the vertical line represents January 2016. In February 2016, London's Help to Buy scheme was implemented. In Panel B, the vertical line represents March 2013. In April 2013, Help to Buy was implemented in England.

and indicates that prices moved in parallel prior to the implementation of the policy.[18] Panel A shows that the gap between new build house prices inside and outside of the GLA starts to widen only after the introduction of London's Help to Buy scheme. The gap appears in 2017, in line with the fact that the proportion of HtB sponsored purchases in London did not grow substantially until that year.[19] Panel B, in contrast, shows no substantial divergence between price indices on both sides of the English/Welsh border.

Fig. 3 depicts the average number of units built by ward at the GLA boundary and the English/Welsh border, respectively. Again, we see that the evolution of building activity followed reasonably parallel trends prior to the implementation of the policy. Panel A shows no substantial divergence between housing construction inside and outside of the GLA after the implementation of HtB, while Panel B reveals that a gap emerged in the building activity on both sides of the English/Welsh border after the policy was introduced, indicating that the more generous English scheme stimulates construction at the English/Welsh border where supply is less constrained.

Overall, these graphs indicate that more generous versions of the HtB policy triggered a price but no noticeable quantity response in the supply inelastic areas around London. Conversely, the policy generated a quantity but no significant price response in the relatively supply elastic areas around the English/Welsh border. This is in line with intuition and with our theoretical framework, which suggests that price and quantity responses to shifts in demand depend on the shape of the

[18] The price index is constructed by estimating a linear regression of log prices on property characteristics (property type dummies for detached, semi-detached and terraced properties, log property size, a leasehold dummy, measures of energy efficiency) and postcode fixed effects. The lines in Panels A and B of Fig. 2 correspond to time dummies included in that specification.

[19] The proportion of HtB completions relative to the number of new build transactions in London increases from 10.7% in 2015 and 12.7% in 2016, to 24.5% and 31.9% in 2017 and 2018, respectively. This is likely because it takes time for developers and home buyers to learn about and adjust to the new HtB scheme in London.

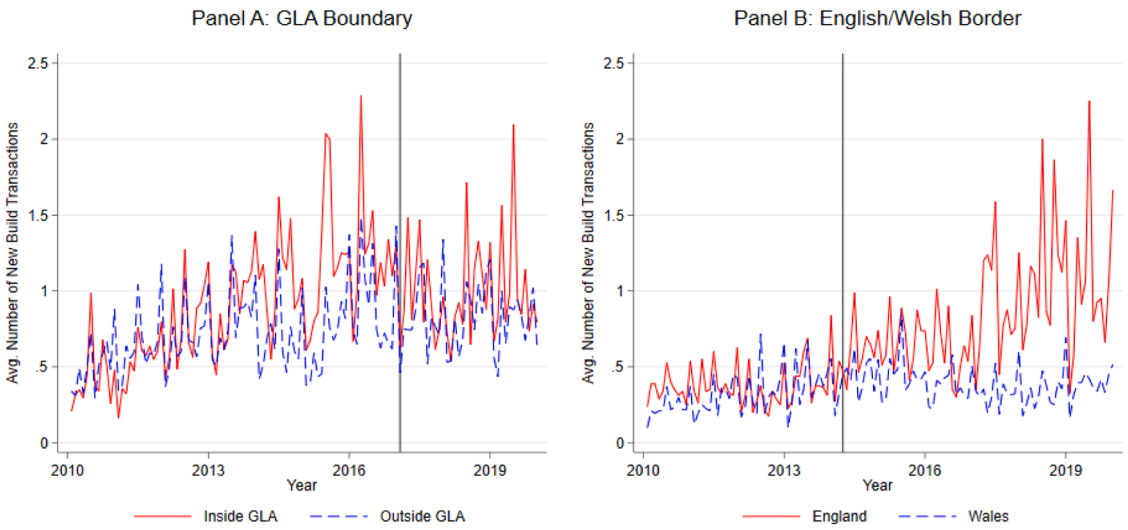

Fig. 3. Housing Construction
Note: Graphs portray the average number of new build transactions per month on each side of the corresponding boundary, where the average is taken across wards. In Panel A, the vertical line represents January 2017 (12 months after the implementation of London's HtB-scheme in February 2016). In Panel B, the vertical line represents March 2014 (12 months after the implementation of England's HtB-scheme in April 2013).

supply curve. Below, we document reduced form estimates for the magnitudes of these effects.

4.2. The impact of Help to Buy on house prices

We explore the impact of differences in the generosity of the HtB-schemes at the London boundary and at the English/Welsh border on prices P by employing a Difference-in-Discontinuities framework. We estimate the following equation:

$$\ln(P_{iljt}) = \phi_l + \beta HtB_l \times Post_t + \delta_t + \gamma' X_i + \tau' Z_j \times d_y + \gamma_y Distance_l \times d_y + \varepsilon_{iljt} \quad (3)$$

where i indexes individual properties, l indexes the postcode, j indexes the (ward-level) neighborhood, t indexes the month, and y indexes the year. The variable HtB_l is a dummy that takes value 1 in the region with a more generous HtB policy (i.e., inside the GLA or on the English side of the English/Welsh border). The variable $Post_t$ represents a dummy taking value 1 if individual transaction i occurs after the change in policy (e.g., London's HtB was introduced in February 2016, so $Post_t$ takes value 1 from March of that year). A postcode fixed effect, represented by ϕ_l, is included to account for fixed differences in amenities and other local characteristics across locations. Likewise, we include a set of individual housing characteristics X_i to account for differences in the attributes of sold units.[20] We include a set of (year-month) time dummies δ_t to account for aggregate changes in prices in each sample. A vector of neighborhood characteristics Z_j interacted with year dummies d_y are included to account for time-varying changes in neighborhood characteristics unrelated to HtB. Finally, we include the distance to the boundary interacted with year dummies d_y to account for potential time varying shocks that differ spatially.[21] Our parameter of interest is β. It measures the effect of differences in the intensity of the HtB policy on the price of new build properties.

We estimate this equation by OLS on new build properties, clustering standard errors at the ward-level to account for potential spatial correlation in local price shocks. In the case of the London HtB scheme, we use a 5km bandwidth around the GLA boundary. We use a 10km bandwidth around the English/Welsh border because of the lower density at that border. In the robustness checks section, we show that our results are robust to alternative bandwidth choices.

Panel A of Table 5 presents results obtained from estimating Eq. (3) using the sample of transactions around the GLA boundary. We include different sets of covariates sequentially from columns 1 to 5. Column 1 controls for time effects and independent linear terms in distance of each property to the GLA boundary. Column 2 adds a vector of housing characteristics such as type of the property, energy consumption, and freehold vs. leasehold. Column 3 adds postcode fixed effects. In column 4 we include neighborhood characteristics from the Census interacted with year effects. Finally, in column 5, we allow for heterogeneous spatial price trends by controlling for interactions between distance from the GLA boundary and year dummies. Our preferred specification is column 5.

The resulting estimates show that London's HtB policy increased the price of newly built houses inside the GLA by between 5 and 8 % depending on the specification, with 4 out of 5 estimates being at least statistically significant at the 10 % level and with the most rigorous specification being significant at the 1 % level. The average price of a new build property in London in 2015 was £425,000, suggesting that – according to our preferred estimated effects of 7.94 % – homebuyers paid £33.75k more for a newly built property inside the GLA because of London's HtB.[22] In Section 7, we compare this amount to the implicit interest subsidy provided by the equity loan granted by the scheme.

Panel B of Table 5 summarizes the results from estimating Eq. (3) for the sample of properties around the English/Welsh border. Again, we successively include additional controls from columns 1 to 5. We find statistically insignificant effects of the policy on the price of newly built

[20] These controls are included to account for the fact that the policy may induce a change in the characteristics, or the location of the units built by the developers. Crucially, our set of controls includes the logarithm of the total floor area. In addition, we control for the dwelling type, freehold vs. leasehold, whether the property has a fireplace, energy consumption and CO_2 emissions.

[21] In an alternative specification, we omit the postcode fixed effects and control flexibly for distance to the boundary by estimating different linear terms in the distance, specified separately on either side.

[22] The preferred estimate of the effect of the introduction of the London HtB scheme on new build prices is 0.0764 (see column 5 in Panel A of Table 5). Applying the Halvorsen and Palmquist (1980) correction, we obtain an effect equal to $e^{0.0764} - 1 = 0.0794$. We use this correction when making quantitative interpretations of all our dummy variable coefficients in our log-linear specifications throughout the remainder of the paper.

Table 5
Price effect.

Specifications	(1)	(2)	(3)	(4)	(5)
Panel A: GLA boundary					
HtB × Post[1)]	0.0534*	0.0589*	0.0506	0.0796***	0.0764***
	(0.0317)	(0.0307)	(0.0355)	(0.0270)	(0.0283)
N	41357	41357	41357	41357	41357
R^2	0.6388	0.6555	0.9277	0.9283	0.9283
Panel B: English/Welsh border					
HtB × Post[2)]	0.0801	0.0682	0.0012	0.0053	0.0090
	(0.0498)	(0.0493)	(0.0294)	(0.0295)	(0.0281)
N	11574	11574	11574	11574	11574
R^2	0.6683	0.7028	0.9167	0.9171	0.9173
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes
Distance to boundary on each side	Yes	Yes	No	No	No
Housing controls[3)]	No	Yes	Yes	Yes	Yes
Postcode FEs	No	No	Yes	Yes	Yes
Census variables by year[4)]	No	No	No	Yes	Yes
Distance by year	No	No	No	No	Yes

Notes: The dependent variable in all columns is the natural log of the new build transaction price. All columns control for the logarithm of total floor area. Standard errors are clustered at the ward level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

[1])HtB × Post is a dummy taking value 1 for transactions inside the GLA after February 2016, when London's HtB was first introduced.

[2])HtB × Post is a dummy taking value 1 for transactions in England after April 2013, when the English version of HtB was introduced.

[3])Housing controls include dwelling type, freehold vs. leasehold, whether the property has a fireplace, energy consumption and CO2 emissions.

[4])Neighborhood controls (from the 2011 Census) are the percentage of (1) married residents and (2) residents with level-4 and above educational qualifications at the ward level.

properties in all five columns.[23] After we control for postcode fixed effects in columns 3 to 5, the point estimates remain positive but small, ranging between 0.1 to 0.9 %. The estimates in these columns are reasonably precise, allowing us to rule out positive effects of over 4.6 % with 90% confidence. The upper bound of the 95 % confidence intervals for these estimates are below the point estimates obtained using data for the GLA boundary reported in Panel A. Using a stacked regression combining data for both regions, we can reject the null of equal effects at the GLA boundary and the English-Welsh border at all conventional statistical levels. This allows us to state with confidence that differences in the generosity of the policy at the GLA boundary led to positive price effects that were larger than those observed at the English/Welsh border.

4.3. The impact of Help to Buy on housing construction

The government's equity loan is available only for the purchase of newly built units. In this way, the government attempts to ensure the policy triggers additional residential construction. To formally test whether this is the case, we estimate the effect of differences in the intensity of the policy on construction activity. We again use a Difference-in-Discontinuities specification, diverging from the one employed to study price effects in that we obtain our estimates using a ward level panel built by aggregating new build counts at the ward level for every month. We estimate:

$$New\ builds_{jt} = \omega_j + \beta HtB_j \times Post_{t-12} + \delta_t + \tau' Z_j \times d_y + \gamma_y Distance_j \times d_y + \varepsilon_{jt} \quad (4)$$

where j indexes wards, t indexes months, and y indexes years. The dependent variable is now $New\ builds_{jt}$, which represents the number of new build transactions in ward j and period t. As above, the variable HtB_j is a dummy taking value 1 in the area with a more generous HtB policy. The variable $Post_{t-12}$ represents a dummy that takes value 1 if transactions in ward j occur after the difference in policy arises. The variable is lagged by twelve months to account for the likely delayed response of construction to the policy shock.[24] We include a set of ward fixed effects, represented by ω_j and time fixed effects δ_t.[25] Z_j are neighborhood characteristics from the 2011 Census interacted with year dummies d_y. In addition to controlling for ward fixed effects, we include the distance to the boundary interacted with year dummies to account for potential time varying shocks that differ spatially. In all specifications we cluster standard errors at the ward level.

Our parameter of interest is β, which measures the effect of differences in the intensity of HtB on new construction. The differences in intensity are not the same across the English/Welsh border and across the GLA boundary. Therefore, we obtain separate estimates for these two exercises.

Panel A of Table 6 summarizes the results from estimating Eq. (4) for

Table 6
Construction effect.

Specifications	(1)	(2)	(3)	(4)
Panel A: GLA boundary				
HtB × $Post_{t-12}$[1)]	0.0015	0.0015	-0.1312	-0.0964
	(0.1688)	(0.1695)	(0.1930)	(0.1909)
N	49320	49320	49320	49320
R^2	0.0115	0.1545	0.1568	0.1579
Panel B: English/Welsh border				
HtB × $Post_{t-12}$[2)]	0.3988***	0.3988***	0.3771***	0.3951***
	(0.1438)	(0.1444)	(0.1311)	(0.1286)
N	24480	24480	24480	24480
R^2	0.0269	0.2302	0.2371	0.2387
Year-month fixed effects	Yes	Yes	Yes	Yes
Distance to boundary on each side	Yes	No	No	No
Ward fixed effects	No	Yes	Yes	Yes
Census variables by year [3)]	No	No	Yes	Yes
Distance by year	No	No	No	Yes

Notes: The dependent variable in columns 1 to 4 corresponds to the number of new builds in that ward-month pair. Standard errors are clustered at the ward level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively.

[1])HtB× $Post_{t-12}$ corresponds to a dummy taking value 1 for wards inside the GLA after January 2017 – which is one year after the implementation of London's HtB.

[2])HtB× $Post_{t-12}$ corresponds to a dummy taking value 1 for wards in England after March 2014 – which is one year after the implementation of the English version of HtB.

[3])Neighborhood controls (from the 2011 Census) are the percentage of (1) married residents and (2) residents with level-4 and above educational qualifications at the ward level.

[23] This finding does not depend on the ordering of introducing controls. If we introduce Census variables-by-year controls in our column 2 specification instead of postcode fixed effects, we also obtain a small and insignificant coefficient.

[24] Construction lags in the UK tend to be long by international standards, often exceeding 12 months. As a robustness check, we estimate a contemporaneous specification with qualitatively similar findings.

[25] We also provide estimates that are obtained by controlling flexibly for the distance to the boundary, omitting ward fixed effects.

the sample of wards around the GLA boundary. We define the post-HtB period as extending from February 2017 to December 2019 – starting one year after the implementation of London's HtB – to allow for a one-year construction lag. Panel A reveals that London's HtB had no significant effect on ward-level construction volumes. Coefficients are insignificant and small in all specifications, suggesting that the increase in the size of available equity loans inside the GLA-boundary did not lead to an increase in housing construction. The estimates allow us to rule out large positive effects of the difference in HtB generosity on new construction. In particular, we can rule out positive effects larger than 0.15 units per ward and month with 90 % confidence.

In a similar vein, Panel B of Table 6 provides estimates of Eq. (4) for wards around the English/Welsh border. As above, the post-treatment period is defined as starting one year after the introduction of the English HtB-scheme. We find a significant and positive effect of HtB on housing construction in all specifications. Our estimates suggest that the higher eligibility threshold on the English side of the border increased the number of new build transactions at each ward per month by 0.4 on average.[26]

As in the case of prices, when using a stacked regression combining data for both regions, we can reject the null of equal effects of differences in the policy on housing construction at the GLA boundary and the English-Welsh border at all conventional statistical levels. These results are consistent with the predictions from our theoretical framework that indicate that HtB has differential effects in London and the areas around Wales as a consequence of differences in supply conditions in the two areas.

4.4. Event study

Next, we employ an event study design to test for the parallel trend assumption and to estimate the time-varying effects of HtB on housing prices and construction.

We first estimate a specification similar to Eq. (3) but replace the first interaction term with a sum of interactions of the HtB_l variable with year dummies and a conformable set of coefficients. Panel A of Fig. 4 presents our estimates for the dynamic effects of differences in the HtB scheme inside versus outside the GLA boundary on house prices. The coefficient of the year before the treatment is normalized to zero and the vertical lines represent the 95 % confidence intervals. Differences in house prices inside and outside the GLA boundary are stable before 2016, consistent with the parallel trend assumption. The estimated coefficients become positive and statistically significant after 2016, suggesting a significant impact of London HtB on housing prices inside the GLA. The positive coefficients for 2018 and 2019 are larger than the coefficients shown in Table 5. This is because the overall price effect of HtB represents the average of the time-varying effects after the difference in HtB arises.

Panel B of Fig. 4 presents the dynamic effects of differences in the HtB scheme at the English/Welsh border on house prices. We don't observe significant changes in the difference in house prices between the English and the Welsh side either before or after the introduction of the more generous HtB policy in England. Overall, these time-varying estimates further confirm the parallel trend assumption for our identification strategy and corroborate our findings in Table 5.

We can extend our analysis of dynamic effects to the case of new construction. To do so, we estimate a specification akin to Eq. (4) but again replace the interaction term with a sum of interactions of HtB_j with a set of year dummies. In line with specification (4), we normalize the coefficient of the treatment year to zero to account for the likely delayed response of construction to the policy shock. Panel C of Fig. 4 presents the time-varying effects of the more generous London HtB-scheme on housing construction inside the GLA boundary. Consistent with Table 6, there is no significant change in construction inside the GLA either before or after the introduction of the London HtB policy. Panel D of Fig. 4 presents the time-varying effects of the more generous HtB-scheme in England on housing construction on the English versus the Welsh side of the border. We observe an insignificant difference in housing construction between the English and the Welsh side before 2013, which indicates that the pre-trend is reasonably parallel. After the introduction of the HtB policy in England in 2013, the point estimates become positive and are statistically significant at the 5 % level after 2016. The larger coefficients in more recent years reflect the construction lag in response to the policy shock. Overall, the dynamic effects of HtB on housing construction further confirm the parallel trend assumption and corroborate our findings in Table 6.

4.5. The impact of Help to Buy on the financial performance of developers

Uncovering how HtB affected the financial performance of developers can help us identify some of the beneficiaries of the policy. Theoretically, the HtB policy can be expected to induce an increase in revenue of existing developers participating in the scheme.[27] Moreover, as stipulated in Proposition 2, barriers to entry and imperfect competition in housing production and land markets imply that the policy should translate into increased profits. This, however, hinges on the increase in revenue not being fully offset by an increase in the cost of land after the implementation of the policy.[28]

Lack of detailed information on the location of developers' assets prevents us from deploying the spatial techniques used in our analysis of price and construction effects. To nevertheless study the effects of HtB on the financial performance of developers empirically, we instead employ a Difference-in-Differences strategy and use our developer dataset, covering 78 large British developers and construction companies.[29] The dataset includes information on the developers' financial performance and, crucially, on the participation of these firms in HtB. We use our dataset to compare how the change in the performance of firms before and after 2013 varied with their participation in the scheme. For this purpose, we estimate a fixed effects model specified as:

$$\ln\left(Financial\ performance_{ky}\right) = \beta HtB_k \times Post_y + \alpha_k + \delta_y + \varepsilon_{ky} \tag{5}$$

$Financial\ performance_{ky}$ is an indicator of various measures of financial performance for developer k in year y. We look at turnover (i.e., total revenues), gross profits, and net profits before taxes. The measure HtB_k captures a developer's engagement with the policy. The variable $Post_y$ takes value 1 in 2013 and in subsequent years. Finally, α_k is a developer fixed effect and δ_y represents a set of year dummies.

Estimates of β will capture the impact of the policy on the developers' financial performance measures under the assumption that unobservables ε_{ky} are uncorrelated with $HtB_k \times Post_y$ conditional on individual and year fixed effects.

To ensure the internal validity of our Difference-in-Differences model we first visually inspect the crucial parallel trend assumption. We estimate a specification similar to Eq. (5) but interact the HtB_k variable with a full set of year dummies. In this case, variable HtB_k is a

[26] Unreported estimates focusing on the sub-sample of properties that are eligible for HtB in England and not Wales (i.e., properties priced at between £300K and £600K) yield even larger estimates. We have focused our main analysis on the total sum of new builds to avoid constructing our outcome variable using information on prices, which are themselves an endogenous outcome of the policy.

[27] The increased supply could in principle be taken up exclusively by new entrants. Yet the presence of economies of scale in housing production and the learning curve required to navigate the British planning system mean that the volume of new entrants will probably be very small.

[28] In our model, this is ruled out because land is owned by developers, so land rents are included in profits.

[29] Our regression samples only cover a small number of relatively large developers and are thus only partially representative of the entire industry.

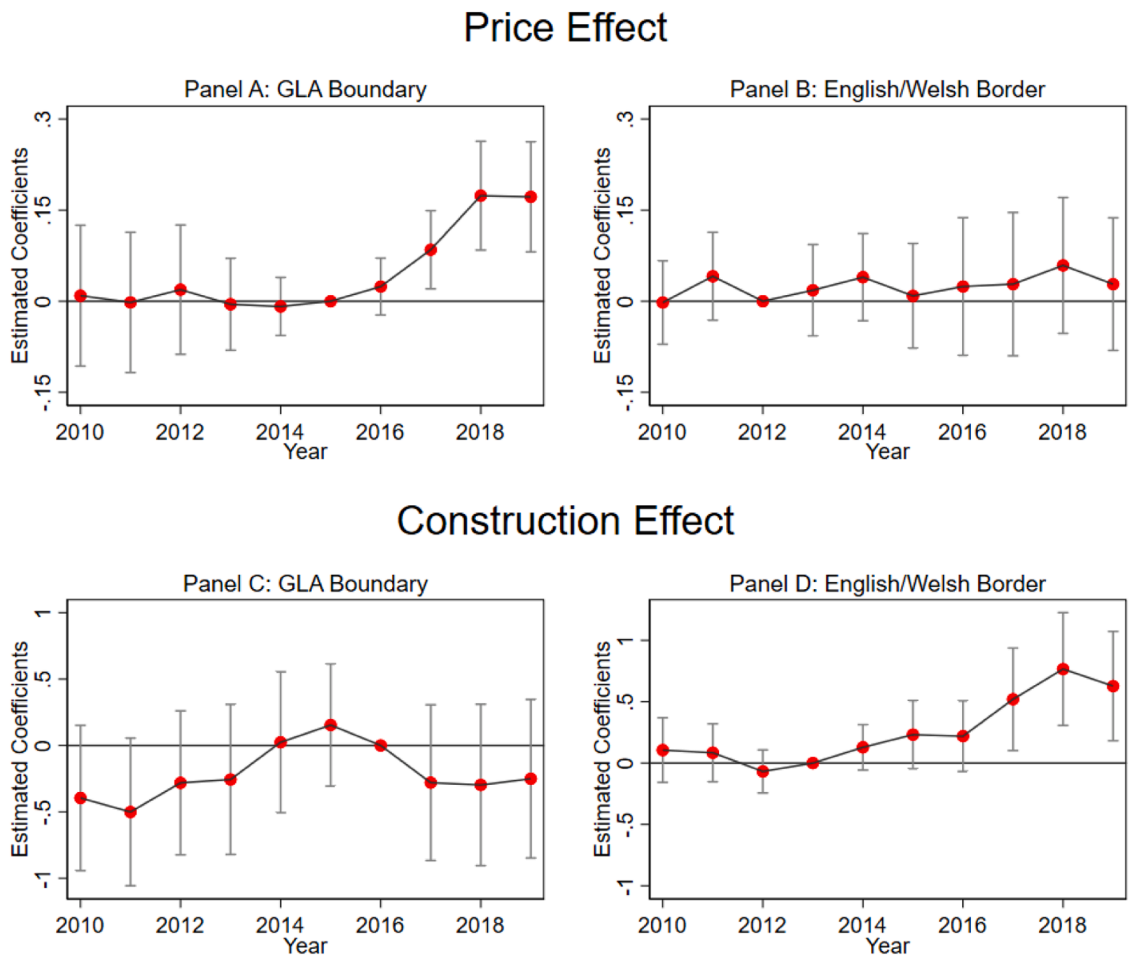

Fig. 4. Event Study Graphs for Price and Construction Effects
Note: Panels A and B correspond to event study graphs for price effects. We estimate a specification similar to Eq. (3) but replace the first interaction term with a sum of interactions of the HtB_l variable with year dummies and a conformable set of coefficients. Panels C and D correspond to event study graphs for effects on new construction. We estimate a specification similar to Eq. (4) but replace the first interaction term with a sum of interactions of the HtB_j variable with year dummies and a conformable set of coefficients. Red points correspond to the annual estimated coefficients. Vertical lines correspond to 95 % confidence intervals around those estimates. The coefficient of the year before the treatment is normalized to zero.

dummy taking value 1 if the developer is included in one of the regional HtB-registries of developers in England or Wales. Fig. 5 plots the estimated annual impacts of the HtB policy on the difference of the three financial performance indicators between HtB- and non-HtB-developers. The pre-trends are reasonably parallel, that is, we observe no change in the differences of the three performance indicators between the HtB- and non-HtB-developers before 2013. However, we observe positive and statistically significant effects of HtB on HtB-developers' revenues, gross profits, and net profits after the introduction of the policy. The plots in Fig. 5 both provide support for the parallel trend assumption and are consistent with the notion that participating developers improved their financial performance because of HtB. An additional implication is that, on the supply side of the residential market, the benefits of the scheme may not have gone exclusively to landowners.

Now turning to our estimates of Eq. (5), we use two alternative measures to capture a developer's engagement with HtB. The first is the dummy described in the previous paragraph, obtained from HtB developer registries. We can estimate this specification for our full Difference-in-Differences sample of 78 developers. Our second measure of HtB_k is based on detailed information on the fraction of the units produced by developer k that were sold under the HtB scheme, averaged over our sample period. Because this information is only available for a subset of companies, we can only estimate this specification with our reduced 'intensity sample' covering 30 developers.

Table 7 presents our estimates for the effect of the scheme on revenues, gross profits, and net profits before taxes. Estimates in Panel A indicate that participation in HtB – as measured with our registration dummy – increases revenues substantially, with HtB-participants obtaining over 57 % higher revenues compared to non-participants. Coefficients for gross and net profits are even larger, indicating a large effect on developer performance.

Panel B presents estimates of the effects for our continuous measure of HtB participation using our intensity sample. The first column shows that a 1 percentage-point increase in the fraction of HtB-properties supplied by a developer, leads to a roughly 1 % increase in revenue. The effect is large and significant. The estimates for gross profits and net profits, displayed in columns 2 and 3 are even larger, suggesting that changes in costs – e.g., costs of acquiring land – did not offset the changes in revenue. Again, these estimates suggest that the policy improved the financial performance of residential developers.

Some caution is warranted when interpreting the findings in Panels A and B of Table 7. There are substantial observable differences in characteristics between the developers self-selecting into the HtB-scheme and other developers in the sample. For example, luxury developers typically are in the control group, as they will not normally be registered with HtB. We can only give our estimates a causal interpretation if these differences have a time-invariant influence on the financial performance of developers. While we would argue that this assumption is plausible,

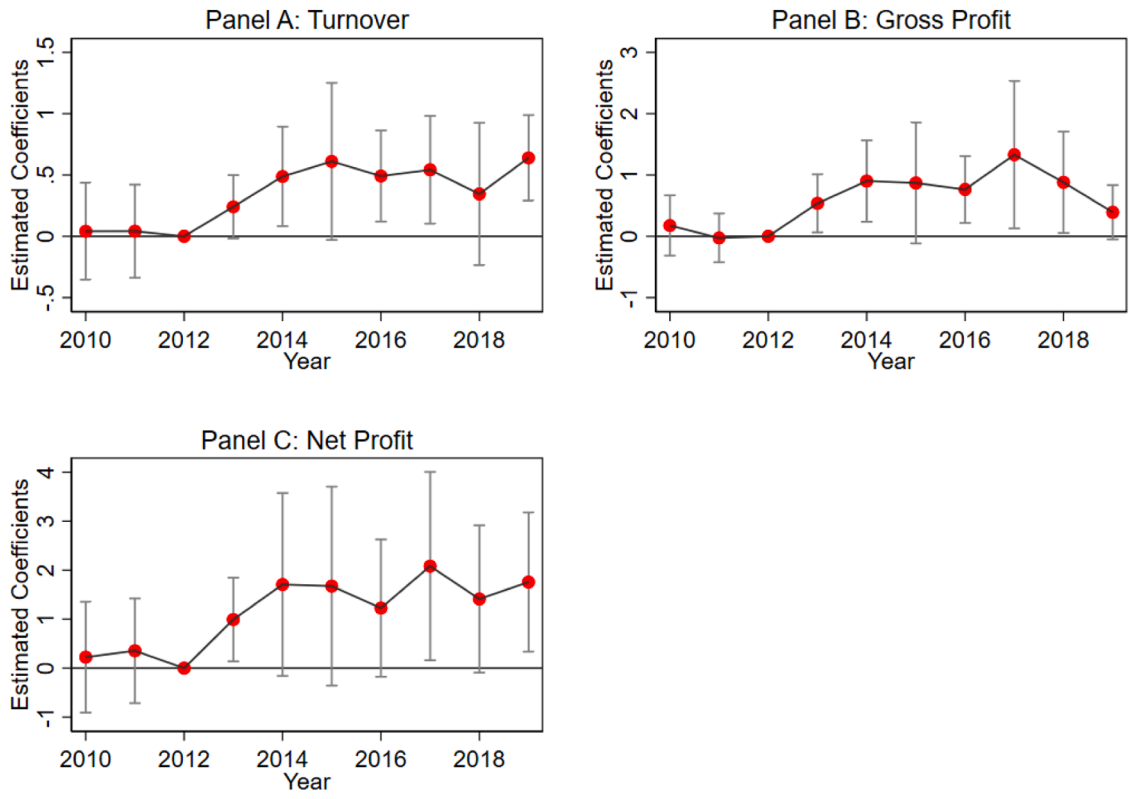

Fig. 5. Event Study Graphs for Developers' Financial Performance
Note: We estimate a specification similar to Eq. (5) but interact the HtB_k variable with a full set of year dummies. Red points correspond to the annual estimated coefficients. Vertical lines correspond to 95 % confidence intervals around those estimates. The coefficient of the year before the treatment is normalized to zero. Help to Buy was implemented in England in April 2013, hence the 2013 financial data contains both pre- and post-information.

we address the concern of possible self-selection by employing an Instrumental Variable approach.

Our instrument for a developer k's engagement in HtB is the ratio of HtB completions in the NUTS-1 region, in which developer k has its headquarters, relative to all completions in that region. This measure can be expected to be strongly correlated with developer k's engagement in HtB. However, it is not directly affected by developer k's characteristics. As such the instrument helps us address the concern that inherently more profit-driven developers may self-select into the HtB-scheme and that this may explain the better financial performance of these developers, rather than HtB itself increasing developer returns and profits.[30]

Panel C of Table 7 reports second stage estimates of the effect of HtB-registration on developer performance, using our local HtB exposure-instrument in the first stage. The resulting second-stage estimates are consistent with the Difference-in-Differences estimates reported in Panel A and are statistically significant at least at the 5 % level. Panel D of Table 7 reports the corresponding first-stage result, indicating that developers headquartered in regions that have become more HtB-intensive over time have become more likely to participate in the scheme. The instrument is, however, rather weak – with an Kleibergen- Paap F-statistic of 5.2 – so we must interpret our IV results with caution.[31] That said, somewhat reassuringly, results are qualitatively similar when we use the local HtB exposure measure directly rather than as an instrument – see Panel E of Table 7.

While the various estimates reported in Table 7, individually, must be interpreted with caution, taken together, they clearly point to a causal positive effect of HtB on the financial performance of participating developers.

5. Robustness checks

We now turn to a series of robustness checks and placebo tests to confirm our main findings and provide additional validation to our research strategy. First, we look at whether our findings can be explained by displacement of demand across the English/Welsh and GLA boundaries. Second, we provide estimates employing displaced versions of the boundaries used in the main analysis and the area around the Greater Manchester boundary to construct suitable placebos. A final set of tests considers robustness to an alternative measure for construction activity, varying chosen bandwidths, or the period used in the analysis.

5.1. Displacement of homebuyer demand across boundaries

The potential displacement of demand across either the GLA boundary or the English/Welsh border is an important threat to our identification strategy. Displacement could occur if the policy induces short-distance sorting of prospective buyers so that, for example,

[30] The identifying assumption is that developers are more likely to become engaged in HtB in local markets in which demand for HtB-equity loans has become stronger over time and that, conditional on developer and year fixed effects, the uptake in HtB loans in a local area only affects developer k's financial performance via affecting developer k's propensity to become engaged in HtB.

[31] Following Andrews *et al.* (2019) and Sun (2018), we also report the Anderson-Rubin 90% confidence interval for our estimates in Panel C of Table 7.

Table 7
Effects on financial performance of developers.

Specifications Dependent variable	(1) Ln (Turnover)	(2) Ln(Gross profit)	(3) Ln(Net profit before tax)
Panel A: DID sample ($N = 535$)			
HtB dummy × Post[1)]	0.4509*** (0.1309)	0.7957*** (0.2711)	1.3686*** (0.4148)
Panel B: HtB intensity sample ($N = 223$)			
HtB intensity × Post[2)]	1.0086** (0.4481)	1.6070** (0.6659)	1.9312* (1.1237)
Panel C: Local HtB completion ratio as instrument for HtB dummy ($N = 493$)			
HtB dummy × Post[1)]	0.6017** (0.2798)	1.1914*** (0.4003)	1.7708*** (0.5509)
K.-P. F-statistics	5.183	5.183	5.183
Anderson-Robin 90 % CI	(0.25, 1.60)	(0.63, 2.49)	(0.99, 3.46)
Panel D: First stage ($N = 493$)			
HtB completion ratio × Post[3)]	2.4650** (1.0827)		
Panel E: Local HtB completion ratio ($N = 493$)			
HtB completion ratio × Post[3)]	1.4832** (0.6043)	2.9368** (1.2112)	4.3649** (1.8420)
Year FEs	Yes	Yes	Yes
Developer FEs	Yes	Yes	Yes

Notes: Standard errors are clustered at the developer level. *, **, and *** represent 10%, 5%, and 1% significance levels, respectively. K.-P. refers to Kleinbergen-Paap first-stage F-statistics.

[1])HtB dummy equals 1 if a developer is registered at one of the HtB regional offices. Post is a dummy that takes value 1 if the financial data is after 2012. Help to Buy was implemented in England in April 2013, hence the 2013 financial data contains both pre- and post-information.

[2])HtB intensity is defined as the 5-year average ratio of HtB-completions relative to all property completions by the developer.

[3])HtB completion ratio represents the number of HtB completions relative to the number of new build transactions at the NUTS-1 level.

demand for housing falls outside the GLA boundary as a result of the policy. This would violate the stable unit treatment value assumption required to interpret our quantitative estimates of the price or construction effects as the outcome of the policy. Fortunately, if we assume demand displacement is relatively local – i.e., occurring mostly over short distances – we can use our samples to evaluate whether this is indeed a problem and what is its impact on each set of estimates. We do so employing two different strategies.

The first is to reproduce our Difference-in-Discontinuities baseline estimates, sequentially dropping the transactions closest to the boundary. If the displacement of demand across the boundary of interest is important and happens over relatively short distances, then excluding observations next to the boundary should partially correct our estimates for demand sorting. Estimates for the price effect of London's HtB obtained after excluding different bands around the GLA boundary are reported in Panel A of Appendix Figure B2. We observe that we can exclude transactions taking place within up to at least 2 km of the GLA boundary – 40 % of the bandwidth – without a significant effect on our estimates. In Panel B, we report an analogous figure for the area around the English/Welsh border. Again, excluding observations within 40 % of the bandwidth around the border does not affect the conclusion that differences in the HtB scheme's generosity did not lead to a significant price effect.

It is also possible that our housing construction estimates are biased by demand sorting. To explore this, we obtain estimates after iteratively excluding newly built properties close to the boundary. In Panel A of Appendix Figure B3, we report the coefficients that capture the impact of HtB on construction near the GLA boundary for the different sample restrictions. All coefficients are statistically indistinguishable from zero. We provide results for the construction effect of HtB near the English/Welsh border, again for different sample restrictions, in Panel B. The estimated coefficients are fairly stable between 0.3 and 0.4 and the point estimates are substantially larger in absolute value compared to the coefficients estimated for the area near the GLA boundary.

The second strategy seeks to directly test whether there is any evidence of displacement across our boundaries of interest. To do so, we follow the intuition in Turner et al. (2014) and compare transaction prices close to and far away from the boundary *within* each side.[32]

We focus on the statistically significant effects of HtB, that is, the price effect at the GLA boundary and the construction effect at the English/Welsh border.

When looking at the price effect in London and the role of demand displacement around the GLA boundary, we split the sample into two sub-samples corresponding to property sales on each side of the boundary. The displacement hypothesis has specific predictions regarding how demand changes *within* each spatial band around the border. In the case of the sub-sample of properties *inside* of the GLA, a local displacement of demand would result in an increase in new build prices close to the boundary relative to prices further inside the region. Conversely, for sales *outside* of the London region, displacement of demand would reduce prices close to the boundary relative to prices further out into the periphery. These predictions are easily testable using a modified version of Eq. (3) in which we replace $HTB_l \times Post_t$ with $close_l \times Post_t$, where $close_l$ is a dummy taking value 1 for properties within 2.5km of the boundary. Estimates for London, split by sub-sample, are reported in the first and third column of Appendix Table A2. The results are not consistent with evidence of displacement. The estimates for $close_l \times Post_t$ are insignificant and small in both sub-samples. Statistical power is quite low in these sub-samples, partly because we are using a binary variable to capture distance. To avoid this, we can use another modified version of Eq. (3) but now replacing $HtB_l \times Post_t$ with $dist_l \times Post_t$ where $dist_l$ is a linear term in distance to the GLA boundary. Estimates for the coefficient on $dist_l \times Post_t$ for each sub-sample are provided in columns 2 and 4 of Appendix Table A2. The coefficient is insignificant and small outside of London. Importantly, the estimate is not negative inside of London, as displacement would predict in this case. We conclude from these analyses that local displacement of demand across the London boundary is negligible.

When considering the statistically significant construction effect at the English/Welsh border, we can test directly for evidence of displacement by estimating a version of Eq. (4) in which $HtB_j \times Post_{t-12}$ is replaced with $close_j \times Post_{t-12}$ where $close_j$ is a dummy taking value 1 for wards with centroids within 5km of the boundary. We estimate this separately for the English and Welsh sub-samples around the border with the dependent variable being the number of new build sales in that ward-month pair. The results, which we report in columns 1 and 3 of Appendix Table A3, are not consistent with the pattern that would emerge if displacement of demand from the Welsh to the English side of the boundary was important.[33] We report a similar analysis using a linear term for distance in the interaction for completeness in columns 2 and 4 of Appendix Table A3. These results confirm the earlier finding.

Collectively, these estimates indicate local demand displacement is either not taking place or is negligible. As a result, we believe this is unlikely to induce a substantial bias in our baseline estimates of the effects of HtB.

[32] Specifically, we follow the approach in the external effect regressions in Section 2.5 of Turner *et al.* (2014).

[33] Displacement would result in a negative coefficient for $close_j \times Post_{t-12}$ for the Welsh subsample and a positive coefficient for the English subsample. Instead, Appendix Table A3 shows an insignificant coefficient in the English subsample and a marginally significant positive coefficient in the Welsh subsample.

5.2. Placebo tests

We consider two types of placebo tests for our analysis of the effect of differences in policy intensity on the price of new build properties and construction. First, we replicate our estimates for both outcomes (price and construction) using sales of units within 5km of the boundary of the *Greater Manchester* area. No specific HtB-scheme was put in place in this area, so the eligibility conditions and the maximum size of the loan do not vary at this boundary. Estimates for the Manchester placebo for prices and construction are reported in Appendix Tables A4 and A5, respectively. Both tables indicate no statistically significant effects at the boundary, as expected.

Second, we displace the boundaries – i.e., the GLA boundary and the English/Welsh border – relative to their initial positions. In the case of the GLA boundary, we report three estimates per outcome: one obtained by keeping the initial boundary in place (this simply reproduces the headline estimates at the GLA boundary), one obtained by displacing the boundary 5km closer to the center of London, and, finally, one obtained by displacing the boundary 5 km further out. The distances of these displacements ensure that we only use observations on one side of the boundary in each of the placebos. We conduct a similar analysis for the English/Welsh border, displacing the boundary by 10 km either into Wales or into England. Results for these placebos, alongside our main estimates, for both price and construction effects, are illustrated in Appendix Figure B4 (Panels A to D). Seven out of eight estimates obtained for displaced boundaries are statistically insignificant, as expected. The only exception is for one point estimate in Panel A, which is marginally significant and has the opposite sign compared with our findings from Table 5. These results together confirm our headline estimates.

5.3. Other robustness checks

Our main empirical analysis uses the full sample of new build properties near the corresponding boundary. However, some of these units are not eligible for HtB support as they have prices above the threshold defined by the policy. An alternative is to estimate the effect of the policy on prices only for properties below that threshold. This strategy more precisely identifies the impact of the policy on house prices on eligible properties.

We do not pursue this strategy as our baseline, because it requires conditioning on one of our outcomes of interest. We report results for this alternative sample in Appendix Table A6. We confine this robustness check to the GLA sample as in the case of the English/Welsh border, the eligibility criteria changes across the boundary, rendering the check problematic. We conduct two exercises: The first is to estimate our baseline model (Eq. 3) only using the sample of eligible units. The second is to add to the baseline specification a dummy, $Eligible_i$, for eligible properties (equaling 1 for properties with prices below the £600,000 threshold), and a triple interaction term $HtB_i \times Post_t \times Eligible_i$. Reassuringly, we obtain coefficient estimates in both exercises that are in line with the effects of our overall analysis.

Next, we use an alternative variable to construction activity to test for the robustness of the results reported in Table 6. We estimate a specification similar to Eq. (4) but change the dependent variable to a dummy taking value 1 if there are any new build sales in ward j and period t. The results are reported in Panels A and B of Appendix Table A7. In line with our findings above, Panel A reveals that London's HtB had no detectable effect on the probability that any new build property was sold in a ward, as the estimates are insignificant and small in all specifications. Conversely, all the estimated coefficients in Panel B are positive and statistically significant, indicating that the more generous version of HtB led to an increase in housing construction on the English side of the English/Welsh border.

We conducted robustness tests involving the choice of bandwidth in our empirical analyses. Our main estimates are obtained using observations within 5 km of the GLA boundary and 10 km around the English/Welsh border. Results with alternative bandwidths for each exercise are reported in columns 1 and 2 of Appendix Tables A8 and A9, respectively, and indicate no substantial difference in the magnitude of the estimated effects of interest, showing our results are robust to reasonable bandwidth choices.

Our construction estimates in Table 6 allow for a one-year construction lag to incorporate the fact that demand cannot instantaneously translate into more new build sales. Our results are robust to this choice of timing. Column 3 of Appendix Table A8 and column 4 of Appendix Table A9 report estimates of contemporaneous construction effects (i.e., the post-treatment-period is defined as starting on the implementation date of the policy) for the London boundary and the English/Welsh border samples, respectively. Again, we find that HtB does not have a significant impact on housing construction at the GLA boundary but increases construction significantly at the English/Welsh border.

Finally, we test whether our findings are robust to varying the period used in the analysis around the English/Welsh border. The English version of HtB was implemented 9 months earlier than the Welsh one. Thus, the relevant estimated effects obtained in Tables 5 and 6 must be interpreted as weighted averages of the impact of the different eligibility conditions of HtB at the border (i.e., the fact that the price threshold on the English side of the border is twice that in Wales) and differences arising from the timing of implementation in both locations. To cleanly identify the effect of the different eligibility conditions, we drop observations between April and December 2013 (i.e., the time period with only English HtB) and replicate our main estimates with the resulting sample. Results are reported in columns 3 and 5 of Appendix Table A9. The estimated price effect at the English/Welsh border continues to be statistically insignificant, while the estimated construction effect continues to be positive and significant.

6. Additional results: price bunching and the price of existing homes

In this section, we explore whether the HtB scheme induced changes in the types of units produced by developers and in the transaction prices of existing homes. We first hypothesize that eligibility conditions induce developers to produce smaller units and/or units which are not priced above the scheme's price thresholds. We test this hypothesis by documenting that the policy induced substantial price bunching of new build units at the corresponding price eligibility thresholds in both England and Wales. Second, we use data on existing home transactions and our baseline empirical strategy to test for differences in the price of existing homes resulting from differences in the generosity of the HtB scheme across the boundaries.

6.1. Price bunching

The English HtB policy is only available for properties purchased under 600,000 GBP. As a result, the English HtB program led to significant bunching of sales right below the price threshold. Panels A and B of Fig. 6 show two histograms of new build frequencies for prices between £550,000 and £650,000 in England. The left Panel A represents properties sold in the period from January 2012 to March 2013, before the implementation of HtB in England. The right Panel B corresponds to a histogram for properties sold between April 2013 and December 2019, after HtB was introduced. We observe a substantial increase in bunching in the price distribution of new builds just below £600,000. Panels C and D of Fig. 6 represents similar histograms for Wales before and after the introduction of its own version of HtB for prices between £250,000 and £350,000. We can see that the introduction of HtB also led to bunching, albeit somewhat less pronounced, of new build sales just below the corresponding threshold – in this case corresponding to £300,000. The fact that bunching is also observed in Wales is important because it shows that the £300,000 threshold induces a change in market outcomes. It therefore motivates the strategy used to measure price and

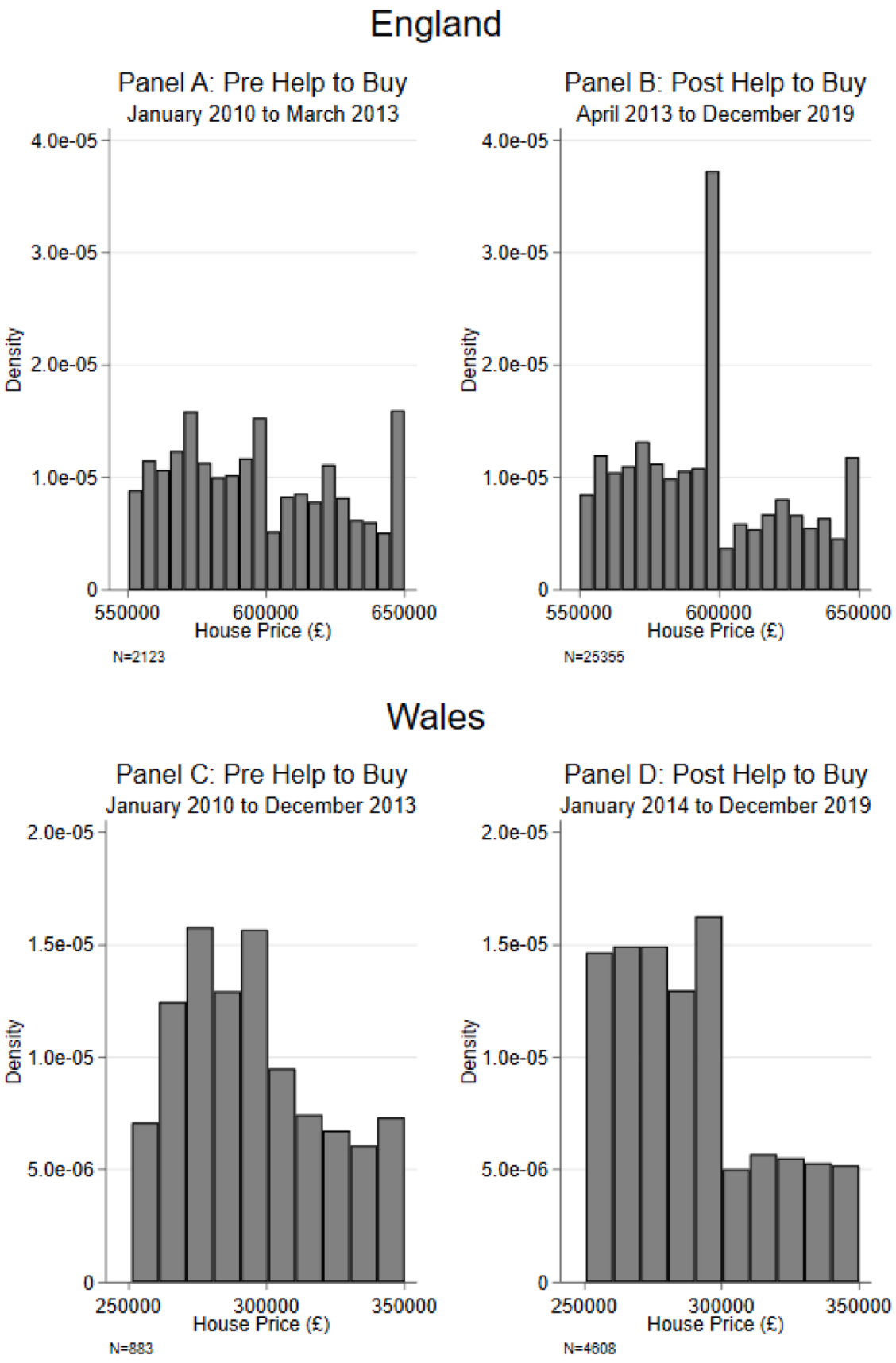

Fig. 6. Histogram of New Build House Prices
Note: Panels A and B correspond to the price distribution histograms of new build sales in England before and after the implementation of Help to Buy in England. Panels C and D correspond to the price distribution histograms of new build sales in Wales before and after the implementation of Help to Buy in Wales.

quantity effects at the English/Welsh border.

One issue to consider when identifying the degree of price bunching is round-number bunching. To deal with this issue, we employ three strategies. We first show the evolution of new build sale volumes by price bins over time. In Appendix Figure B5 we group sales for England into £10,000 price bins and then plot the evolution of the fraction of new builds over total sales for each bin from 2010 to 2019. The black line represents the price bin of interest, £590,000 to £600,000. Grey lines correspond to the other bins between £510,000 and £700,000. We can see that a gap between the black and the grey line appears in 2014 and widens substantially from 2015, implying a significant amount of bunching of new builds at £600,000 after this year, conditional on round-number bunching in the price distribution of all sales.

Second, we use the total number of sales – of new builds *and* existing units – to normalize for a baseline level of round-number bunching. Appendix Figure B6 shows the fraction of new builds over total sales for England and for £5000 price bins averaged over the period between April 2013 and December 2019. We observe significant bunching at £600,000. Appendix Figure B7 repeats this exercise for Wales, where we again observe significant bunching at £300,000.

Finally, we apply the method recently developed in Chetty et al. (2011), Kleven (2016) and Best and Kleven (2017) to estimate the size of bunching in England formally. To do so, we construct a counterfactual price distribution for newly built units using information on sales excluding the region around the bunching thresholds. Following Kleven (2016), we estimate this counterfactual distribution by calculating the number of new build transactions in 100 GBP bins and use these to estimate:

$$S_b = \sum_{q=0}^{3} \phi_q (p_b - 600000)^q + \sum_{r \in R} \rho_r 1\left\{\frac{p_b}{r} \in \mathbb{N}\right\} + \varepsilon_b \tag{6}$$

where b indexes price bins. The dependent variable S_b measures the number of new build transactions in bin b. The first two sums provide an estimate of the counterfactual price distribution. The first sum is a third-degree polynomial on the difference between the price at bin b and the cut-off of £600,000. The second sum estimates fixed effects for round numbers, with $\mathbb{N}$ representing the set of natural numbers and $R = \{5000, 10000, 25000, 50000\}$ representing a set of round numbers. We estimate this equation with data for new build transactions in England taking place between April of 2013 (the introduction of HtB in England) and December of 2019, excluding transactions with prices between £590,000 and £630,000. We then obtain differences between this estimated counterfactual distribution and the observed distribution of prices to estimate bunching effects induced by HtB.

The difference between the size of the spike just under the threshold and the gap just above the threshold can be used to estimate the size of the local effect of HtB on new building activity. This can be driven by changes in the types of properties sold after accounting for local shifting in prices induced by the policy.

Appendix Figure B8 illustrates the difference between the observed density of property transactions and our estimated counterfactual density around the £600k notch. The counterfactual distribution is obtained by estimating Eq. (6). We observe substantial bunching below the cut-off of £600,000 and a large hole in the distribution above the cut-off. Using our counterfactual price distribution, we estimate there are 3123 *more* transactions for properties valued from £590,000 to £600,000 and 1272 *less* transactions for properties valued from £600,000 to £630,000.[34] These estimates suggest that HtB leads to a significant shift in housing construction away from properties above the price threshold, towards properties below the threshold.

[34] These numbers amount, respectively, to 12.3% and 5% of all sales in the £550000-£650000 range.

6.2. The price of existing homes

We can also use our Difference-in-Discontinuities design to estimate the effect of HtB on the transaction price of *existing* homes. These properties cannot be purchased using a HtB equity loan but may be affected indirectly by displacement of local demand to or from the new build market and directly by spillovers from that market. To test whether this is the case, we estimate Eq. (3) using data on the existing home transactions close to the GLA boundary and the English/Welsh border, respectively.

Appendix Table A10 summarizes the estimation results. Panel A of Table A10 displays the impact of the HtB scheme on the price of existing homes at the GLA boundary. All five columns report positive and statistically significant coefficients, ranging from 1.3 to 2.5 %. Compared with the 7.94 % price effect for new builds at the GLA boundary, the effect for existing homes is much smaller, which is in line with our expectations, as the HtB scheme only targets the new build sector. We can rationalize the positive price effect for existing homes by considering the possibility that demand shifts across markets, in a setting where some homebuyers are either unwilling or unable to buy through HtB. In practice, some homebuyers were unwilling to participate in HtB because they did not want to forgo high expected future capital gains. Others, such as buy-to-let investors, were ineligible to participate. In such a setting, the presence of a more generous version of HtB inside the GLA will attract buyers who are willing and eligible to use the program and increase prices there. This will crowd out homebuyers unwilling or unable to use the HtB program off to the market for existing homes. Depending on the relative size of demand shifts between markets, this can induce higher prices of existing homes in the affected area.

Panel B of Table A10 presents our estimates for the price of existing homes close to the English/Welsh border. All five columns report positive price effects between 0.4 and 1.6 %, though three out of five coefficients are statistically insignificant. We could rationalize this effect in the same way as we rationalize the effect for the GLA. However, the fact that we do not find statistically significant price discontinuities for new builds at the English/Welsh border casts some doubt on this interpretation. An alternative and more plausible interpretation is that the positive effect emerges as a local amenity from new construction on the English side of the border (see e.g., González-Pampillón, 2022). In any case, this shows that in both regions under consideration, we find positive and small effects of the expansion of credit induced by HtB on the price of existing units, highlighting another unintended consequence of the policy.

7. Back-of-the-envelope calculation: price effect vs. interest subsidy

Our preferred empirical estimate from Table 5 indicates that the introduction of London's HtB led to a 7.94 % increase in house prices inside relative to outside the GLA. The policy effect can operate via two main channels. First, as discussed in the theoretical section, the reduction in required down payments can increase demand, leading to higher prices. Second, the government equity loan has a lower interest rate than that typically paid for mortgage loans and during the first five years the equity loan carries no interest. This interest rate subsidy could also result in higher demand, and an increase in prices. Because both effects result from the policy, it is not easy to disentangle them empirically. However, we do know the size of the interest rate subsidy in each period. We can combine this with prevailing interest rates on mortgages, discount rates and reasonable assumptions for the expected appreciation of house prices to obtain the present discounted value of that subsidy. Using these numbers, we can decompose the 7.94 % total price effect into an interest rate subsidy and a credit relaxation effect.

We compare discounted cash flows for two hypothetical households buying a property using different forms of financing. Household A buys a property using a London HtB equity loan for 40 % of the property value,

a 55 % LTV mortgage and a 5 % down payment. Household B buys a property of the same price using an English HtB equity loan for 20 % of the property value, a 75 % LTV mortgage and a 5 % down payment. For the sake of simplicity, we consider a 10-year time window.[35] We also assume that both mortgages are 10-year fixed-rate with equal rates. Under these assumptions, the value of the interest rate subsidy accruing to household A can be obtained by comparing two figures: (i) the discounted value of payments for a 20 % reduced-interest HtB-equity loan after subtracting the proceeds from saving the 20 % cash excess in a standard household portfolio, and (ii) the stream of payments arising from a 20 % non-HtB 10-year fixed rate mortgage.

HtB equity mortgages require no interest to be paid in the first 5 years since the purchase. In year 6 the interest rate is 1.75 %. After that, the interest rate is the interest rate from the previous year times (1 + (1 %+Retail Price Inflation in %)). Assuming the RPI stays constant at 3.07 % (the average rate between 2010 and 2019), we can trace out future payments on all HtB loans. We assume a 10-year fixed-rate mortgage costs a nominal interest of 3.27 % based on the Bank of England data of rates for January 2016. This pins down the path of interest payments for mortgages. The interest on savings is assumed to be equal to 1.4 %, which is also taken as the discount rate in our present value calculations.[36] Finally, the expected yearly growth rate for house prices – necessary to value foregone capital gains on the 20 % equity stake of the government – is assumed to be 1.53 %, which was the average growth rate of real house prices in the UK between 2010 and 2019.

Based on these assumptions, we find that the net present value of the interest rate subsidy is 4.11 % of the value of the purchased property. This figure is 52 % of our total estimated price effect. If we assume that the subsidy is fully capitalized into higher prices, this implies that nearly half (48 %) of the effect of London's HtB scheme on prices operates via a relaxation of household credit conditions. Given that the average price of a new build house in London in 2015 (that is, before the introduction of the London version of the HtB scheme) was £425,000, the increase in the price resulting from the relaxation of credit constraints alone amounts to roughly £16,300 (£425,000 × (0.0794-0.0411)).

Our calculation of the present discounted value of the interest rate subsidy depends on assumptions regarding mortgage rates, inflation expectations, etc. A sensitivity analysis reveals that this figure is particularly responsive to the expected appreciation rate of house prices and the return on savings. A higher appreciation rate of prices reduces the value of the subsidy. A higher return on savings has the opposite effect. Yet, given the other parameters of the exercise, either the expected capital gains would have to be unreasonably small (i.e., lower than 0.5 %), or the returns on investment unreasonably large (i.e., as large as mortgage rates), for the interest subsidy to explain away our price effect. Hence, it is unlikely that our estimated price effect for the GLA can be explained by interest rate subsidies alone. At least in part, it must reflect the relaxation of credit constraints as well.

8. Conclusions

In 2013 the UK government announced the HtB scheme, which provides different forms of assistance to households aiming to buy a property as owner-occupiers. We exploit large differences in the intensity of implementation of the policy's Equity Loan Scheme across two regional boundaries to estimate the effect of the policy on the price of newly built homes and on construction volumes.

We estimate different effects depending on the boundary (and corresponding supply conditions) under consideration. In the case of the GLA, we find that the more generous London HtB program led to higher new build prices but had no discernible effect on construction volumes. Both of these effects are arguably contrary to the policy's objectives which are to improve affordability and promote new construction. The estimated effects are more encouraging from the policymaker's point of view in the relatively supply-elastic market around the English/Welsh border, with no significant effect on the price of new builds and a substantial and statistically significant effect on construction activity. Yet, the housing affordability crisis in the UK tends to be most severe in the supply inelastic markets of the South East and especially in the GLA.

Our empirical strategy estimates local effects in two areas only. However, we can use the insights from our theoretical framework and related research to anticipate where the policy will result in larger effects on prices or quantities. In Appendix Figure B9, we report the estimated house price-earnings elasticities for 353 LPAs in England from Hilber and Vermeulen (2016), based on their IV-specification. In line with our expectations, the figure suggests relatively high price-earnings elasticities in the GLA and the South East, indicating more constrained supply conditions and, hence, more price inelastic supply of housing in these regions. It is in these locations where we therefore expect the HtB policy to result in stronger price effects. Conversely, we expect stronger effects on quantities in the more supply elastic areas of the West Midlands and the North West. Appendix Figure B9 also indicates the estimated house price-earnings elasticity is higher in central London compared to the periphery of the GLA and, therefore, we would expect an even higher house price increase in these central locations as a result of the HtB scheme than the one reported in our estimates for the GLA boundary.

Our findings thus imply that HtB has stimulated housing construction in the 'wrong areas'. It has stimulated construction in areas where it is comparably straightforward to build (because easily developable land is less scarce and/or because planning constraints are less rigid) rather than in areas where productivity and employment concentration are highest and new housing is most needed. This is consistent with observed patterns in the intensity of HtB-construction across England and Wales, illustrated in Appendix Figure B10: The policy has led to the construction of housing outside of the green belt areas of the most productive agglomerations in the UK (London, Oxford, and Cambridge). This is in line with other stylized facts that suggest that workers increasingly commute excessively long distances through green belts to get from their place of residence to their workplace.

Contrary to the policy's title, HtB may have done little to 'help' the mostly young credit constrained households in the most unaffordable areas of the country with the strongest house price growth. There are two reasons for this. First, the policy pushed up house prices, increasing housing costs rather than housing consumption in square meters. That is, the policy-induced house price increase more than offset the alleviating effect of the subsidy on housing affordability. Second, the design of the HtB Equity Loan Scheme is such that those borrowers who took advantage of the scheme to gain access to the owner-occupied housing ladder, unlike existing homeowners, do not participate in the same way in future capital gains. This is because, at the time of sale, they must pay back the equity loan percentage times the market value of the house. If

[35] We assume household A pays off half of the equity loan in a single installment in year 10 and household B pays off an equivalent amount of the mortgage in the same way. After these payments, both agents are left with a HtB loan amounting to 20% of the property's initial price and a mortgage amounting to 55% of the initial price (minus any amortization paid in the intervening years). Note that if household A repays the HtB equity loan earlier – which is plausible, given the evidence in Benetton *et al.* (2022) – this would reduce the present discounted value of the interest rate subsidy even further. For this reason, we consider our estimate of the interest rate subsidy to be an upper bound of the subsidy actually enjoyed by households.

[36] The interest on savings is computed by observing assets held by recent buyers – buyers purchasing property in the previous two years - as recorded in the 2016 Wealth and Assets Survey. Most households in this group have their wealth in a combination of traditional savings accounts and ISAs (Individual Savings Accounts). One-year limited access ISAs are particularly popular, and they pay an interest of roughly 1.4%. Only about 1 in 10 recent buyers holds stocks or other risky assets. The average portfolio of a recent buyer (by size) – which is not quite representative of the most common portfolio – includes 10% invested in stocks. This increases total return on savings to roughly 2.6%.

the house price increases, so does the amount that the borrower owes the government.

So, who benefited from HtB, if not the credit constrained households in the most unaffordable areas? Landowners in supply-constrained areas are amongst the main beneficiaries. Moreover, our analyses of the financial performance of developers and of the effects on the price of existing homes indicate that developers and existing homeowners benefited too. This suggests that HtB not only had limited effects on affordability but may have also led to unwanted regressive distributional effects.

CRediT authorship contribution statement

Felipe Carozzi: Conceptualization, Methodology, Data curation, Formal analysis, Writing – original draft, Writing – review & editing. **Christian A.L. Hilber:** Conceptualization, Methodology, Data curation, Formal analysis, Writing – original draft, Writing – review & editing. **Xiaolun Yu:** Conceptualization, Methodology, Data curation, Formal analysis, Writing – original draft, Writing – review & editing.

Supplementary materials

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