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Piero Montebruno, Olmo Silva and Nikodem Szumilo

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Houghton Street
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WC2A 2AE

geog.comms@lse.ac.uk
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Judge Dread:

Court severity, repossession risk and demand in mortgage and housing markets.*

Piero Montebruno[†], Olmo Silva[‡] and Niko Szumilo[§]

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Abstract: *We study the impact of borrower protection on mortgage and housing demand. We focus on variation in the likelihood that a house is repossessed – conditional on the mortgage being in arrears and taken to court – coming from heterogeneity in the severity of judges that adjudicate on repossession cases in England and Wales. We develop a simple theoretical framework that shows that too much borrower protection restricts supply, while not enough restricts demand. Market outcomes depend on which side dominates. To test the predictions of our model, we exploit exogenous spatial variation in repossession risk created by the boundaries of courts' catchment areas. In our setting, housing market characteristics, borrower attributes and mortgage rates do not change discontinuously across these boundaries – allowing us to isolate the causal effects of severity. We find that the impact of severity is negative on both mortgage sizes and house prices. This pattern suggests that judges in our sample are too strict and that demand determines market outcomes. Furthermore, we find that our measure of borrower protection does not react to market conditions – causing frictions in credit and housing markets.*

Keywords: house repossessions, mortgages, house prices, housing demand, mortgage default

JEL codes: G21, G51, R21

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[†] Centre for Economic Performance, London School of Economics.

[‡] Department of Geography and Environment, London School of Economics.

[§] The Bartlett School of Construction and Project Management, University College London. Corresponding author: n.szumilo@ucl.ac.uk

Introduction

Economic downturns often motivate policies aimed at protecting borrowers from bankruptcies and reignite debates about the optimal level of borrower protection (Dávila, 2020). Since the seminal work of Gropp et al. (1997), research has highlighted both favourable and adverse effects of borrower protection. Broadly speaking, the theoretical literature suggests that protecting borrowers can increase credit demand and be beneficial for those who run into financial difficulties. However, borrower protection also reduces credit supply, resulting in high credit prices and low credit availability for borrowers with fewer assets (Gropp et al., 1997; Dávila, 2020; Gordon, 2017). The empirical literature supports some of these arguments: protecting borrowers reduces supply (Cerqueiro & Penas 2017; Dagher & Sun 2016; Pence 2006), but protection benefits those who struggle to meet their obligations (Dobbie et al., 2017; Dobbie & Song, 2015, Cespedes et al. 2020).

Surprisingly, little evidence has been gathered on how demand from borrowers reacts to borrower protection – and the impact this has on credit and asset markets[5]. This seems an important omission not only because it is a key aspect of the debate around borrower protection, but also because borrowers' sensitivity to financial risk is a key parameter in the household economics (Campbell, 2006; Badarinza et al., 2016; Mitman, 2016; Kuchler & Stroebel, 2009) and financial intermediation literatures (Mian & Sufi 2009, Mian et al., 2015; Justiniano et al., 2019; Besley et al., 2013). Indeed, Dobbie et al. (2017) advocate that existing quantitative models should be complemented with robust empirical estimates of how borrowers respond to varying degrees of protection.

In this paper, we study how households react to changes in the loss they can expect when they are forced to default. Specifically, we focus on the impact this has on their demand in credit and asset markets. We concentrate on housing and mortgage markets as mortgage commitments are the largest proportion of household debt and they can be clearly connected to a single asset. Moreover, we expect households' demand to strongly react to borrower protection in this context as a (non-strategic) default, leading to a housing repossession, can be a ruinous life event. Indeed, surveys show that the risk of a housing repossession is often quoted as the most feared economic risk for households (Ford et al., 2001). Furthermore, Ganong & Noel (2020) show that when strategic defaults are rare, demand may respond strongly to borrower protection policies. Our key argument is that while too much borrower protection limits lending (constrained credit supply), not enough protection limits borrowing (constrained demand). Critically, both cases reduce mortgage stock and house prices (compared to an optimal level of protection). We rationalize this claim with a simple model of mortgage and credit markets in England and Wales and provide supporting empirical evidence.

To illustrate our arguments, we move away from the focus of this literature on the complex US context and concentrate on a setting that is more 'convenient' for our research as well as relatively common across the world – thus offering generalizable conclusions. More precisely, the institutional

[5] A notable exception is Severino & Brown (2020) which we discuss in more detail later.

setting of England and Wales allows us to avoid several issues that are notoriously problematic for this literature. First, we study a national credit market with different levels of borrower protection in different regions – but the same mortgage interest rates. This simplifies the supply side of our market and allows us to focus on the effect of changes in borrower protection on mortgage stock and housing values while ignoring (i.e., holding constant) the effects on interest rates coming from changes in risk (see some related US evidence in Goodman & Levitin, 2014). Second, in our settings, all mortgages are full recourse loans. This means that defaults are triggered by unexpected life events rather than strategic motives (Ford et al., 2001), which simplifies how we model the problem faced by households when taking mortgage loans. Unlike in most of the literature, our borrowers do not decide to default (or plan to have an option to default) but are forced to do so by an exogenous shock. The latest evidence suggests that even in the US, strategic mortgage defaults are rare (Ganong & Noel 2020). Importantly, this means that repossession 'insurance' is potentially very valuable and the main channel of any demand-side reaction.[6] Third, we study a specific form of borrower protection – i.e., variation in the probability that a house is repossessed conditional on the mortgage being delinquent. This is a form of insurance against repossession for households who *cannot* (instead of *not wanting to*) repay their loans. This provides a simple and intuitive measure of the level of protection offered to borrowers and translates into clear outcomes for lenders and borrowers.[7] Finally, we use quasi-random assignment of judges to areas as an exogenous variation in the local measure of repossession risk based on the severity of the local courts which adjudicate on cases assigned on the basis of predetermined catchment areas. This provides an opportunity to base our identification on a novel combination of popular strategies in this literature – boundary discontinuity (Dagher & Sun 2016; Pence 2006) and judge assignment (Cespedes et al., 2020; Dobbie et al., 2017).

We begin our analysis with a simple model of how repossession insurance created by court severity affects mortgage and housing demand in our setting. We start by micro-founding demand and supply equations to show how the probability of a successful repossession affects each side of the mortgage credit market (borrowers and lenders). We show that credit demand is a decreasing function of severity, while credit supply is an increasing function of severity. Furthermore, we model mortgage demand as derived from housing demand which means that demand in housing and mortgage markets are affected in the same way by repossession risk. This allows us to make predictions about the impact of severity on house prices and explain interactions between credit and housing markets.

Conceptually, the risk of a house being repossessed can be separated from the risk of not being able to maintain repayments of a mortgage loan (i.e., being delinquent). Indeed, in many countries, delinquent debtors are allowed to stay in their properties while they make arrangements with creditors

[6] According to Severino & Brown (2020), borrower protection can affect demand via insurance, moral hazard and adverse selection. In our case, the latter two are unlikely to be relevant because UK mortgages are full recourse and repossessions can be economically devastating for households – so strategic defaults and opportunistic behaviour are exceedingly rare (Ford et al., 2001).
[7] It can also be easily conceptually isolated from other effects of financial distress such as a lower credit score.

to repay their loan. In practice, however, isolating the impact of a repossession risk is difficult for two reasons. First, a repossession usually cannot occur without a loan being delinquent, which means that the two risks are correlated. Second, the risk of a repossession affects both demand (households) and the supply (lenders) of the credit – so outcomes depend on the interaction between the two.

To address the first problem, we exploit exogenous variation in the likelihood that a delinquent loan is turned into a repossession created by the legal framework for mortgage repossessions in England and Wales. Specifically, we exploit the following features: *i*- pre-determined catchment areas allocate repossession cases to specific local courts; and *ii*- quasi-randomly assigned judges in those courts have the ability to exercise their judgment in deciding whether a house on which a delinquent loan is secured should be repossessed or not (Cowan et al., 2006). This results in repossession cases being heard by judges that are 'stricter' or 'softer' – possibly as a result of their personal inclinations and previous legal/work experiences. In practice, we compare outcomes across boundaries of areas where judges have different propensities to rule in favour of lenders or borrowers – but housing market characteristics (and most likely the characteristics of the cases seen by the judges) do not change discontinuously. This allows us to focus on the impact of the difference in what we call 'judge severity'. The approach is similar to the method used by Dobbie and Song (2015) who focus on a judge-specific measure of severity and a random assignment of judges to bankruptcy cases. Following this framework, we measure severity as the ratio of repossession orders issued by a local court to the number of claims submitted to that local court.[8]

To address the second issue, we use our theoretical framework to devise an empirical strategy that yields estimates specific to the demand side of the market. An important corollary of our framework is that (for a given interest rate) there is a theoretical equilibrium level of severity that equates supply to demand. However, we argue that severity is exogenous to market conditions and determined by persistent judges' preferences (we test this assumption empirically) – implying that it is unlikely to be in an equilibrium set by supply and demand.[9] Given exogenous severity and fixed mortgage rates, observed lending quantity is determined by (the lower of) demand or supply at that level of severity. This means that changes in the severity index translate into changes in observed lending stock by sliding either along the supply curve or along the demand curve – rather than through simultaneous movements of both to achieve an equilibrium.[10] Leveraging this insight, our empirical analysis focuses on measuring the impact of court severity in locations where responses to an exogenous shock in severity are most likely determined by 'sliding along' the demand curve – which we label *demand-driven* areas.

[8] We define a judge to have a high severity index if they are more likely to rule in favour of the lender. Recognising the potential limitations of our measure, we perform a number of tests to ensure that our proxy is not affected by the decision of the lender to submit a case or by the condition of the local credit and housing markets.
[9] This is particularly true in our boundary-discontinuity design (BDD) where housing market conditions and the socio-economic characteristics of borrowers and homeowners move smoothly across the boundaries of courts' catchment areas. Nonetheless, we observe differences in judges' behaviour on the 'strict' and 'soft' sides, meaning that either severity is at the equilibrium on one side but not the other; or it is out of equilibrium on both sides.
[10] We present the full analysis in Section 2.

To identify these areas in our data, we note that in *demand-driven* locations supply is 'irrelevant' so changes to credit supply should not affect lending quantities or house prices. In other words, an exogenous (negative) credit supply shock would *not* have a (negative) effect on prices. To operationalise this intuition, we exploit a shock stemming from a reform to the way in which the biggest mortgage lender in the UK (Lloyds Bank; with around 20% of the market) handled delinquent mortgages. Following years of pressure from the financial regulator, the bank was forced to considerably reduce the number of repossession cases it submitted to courts and reduced its credit supply to account for increasing expected losses on delinquent loans as a result of taking fewer cases to court (this is in line with predictions of the literature; Gropp et al. 1997; Dagher & Sun, 2016). This event provides us with a negative credit supply shock and allows us to characterise *demand-dominated* areas as locations where house prices do not decrease when credit supply falls.[11]

Our reduced-form results show that increasing the judge severity index by one standard deviation decreases house prices by around 3% and the size of mortgage loans by around 2.5% (using our favourite specifications). When translated into a pure demand effect, one standard deviation increase in severity decreases housing demand by approximately 4.5%. We also find that demand 'dominates' in the vast majority of the areas we examine. Based on these results, we perform a counterfactual exercise in which we estimate the effect of a simple policy: reducing the severity index in courts that are 'too strict' to the severity level of the adjacent (and less strict) court across the closest catchment-area boundary. We find that this policy would increase the average house price in England and Wales by 2.65%; create around £171bn of housing wealth; increase mortgage stock by £6.2bn; and generate around £354m per annum in additional transaction taxes. However, we are unable to say whether this policy would increase welfare – as the potential benefits for *current* owners would have to be weighed up against any losses for *would-be* owners (amongst the many complicating factors).

Besides our headline findings, our work provides additional insights into the effect of court severity on housing and credit markets. First, we show that the ratio of repossession orders to claims remains constant even when the type of cases submitted to courts changes due to an exogenous shock. This suggests that judges do not adjust their preferences based on cases they observe and reinforces our argument that in many locations, judges are either too strict or lenient for the market to be in a mortgage lending equilibrium. Importantly, if the legal system's tendency to rule in favour of borrowers or lenders is not determined by market conditions, it can be a source of frictions in housing and credit markets. Next, our analysis highlights the fact that, when borrowers are sensitive to the risk of a repossession, market outcomes are more likely to be determined by demand. Conversely, when households do not perceive a repossession as a big risk, supply will dominate. In our institutional setting where mortgages are full-recourse and repossessions can have dramatic consequences for evicted households, we are

[11] Note that, at the same time, this policy change meant that demand for Lloyds' loans likely increased due to insurance motives – borrowers knew they were less likely to be taken to court in the case of a delinquency. We return to this issue and how we deal with it empirically in Sections 3 and 4.

more likely to identify the demand side of the credit and housing markets. This is at variance with the evidence and institutional framework characterising the US.

Borrower protection research

Before moving on, we provide a brief overview of the research on borrower protection. This literature is extensive and we do not aim to review it in full. Instead, we highlight some key papers in the debate that are relevant to our work. The topic started attracting interest from economists in the late 1980s when the US integrated its local mortgage markets with the national capital market. As some states offered more protection to mortgage borrowers than others, a national debate surrounding the costs and benefits of protecting borrowers begun.[12]

Meador (1982) was amongst the first to note that the price of credit should vary according to borrower protection laws and Gropp et al. (1997) formalized this notion outlining the impact of protection on the supply and demand of credit. Several bankruptcy-law reforms fuelled continued interest from economists – at times with contrasting conclusions. Athreya (2002) modelled consumption smoothing and concluded that eliminating bankruptcy would increase welfare, while Li & Sarte (2006) argued against this idea showing that capital formation and labour input would decrease. White (2007) looked at the impact of borrower protection on credit card debt and concluded less protection should be accompanied by bank regulation to limit credit oversupply. More recently, Davila (2020) developed an approach that balances costs and benefits of borrower protection to set an optimal level of borrower protection. The author argues that the impact on demand can be neglected if borrowers make strategic default decisions. However, Ganong & Noel (2020) point out that this is likely not the case even in the US – and suggest that the impact of borrower protection on demand can be strong.

While most of those studies supported their theoretical arguments with data, there is also a purely empirically-focused strand of the borrower protection literature. Pence (2006) was the first to exploit a boundary discontinuity design looking at boundaries between states with different foreclosure laws. The study finds that when foreclosure laws favour the lender, loan sizes increase. Dagher & Sun (2016) use a similar design to show that this effect comes from an increase in credit supply. The authors also find no impact on loans eligible to be securitized, which offer the same terms to borrowers in all states. Relatedly, there is evidence that since foreclosure laws that favour the lender increase credit supply to risky borrowers, they result in more foreclosures during a recession (Mian et al., 2015).

Importantly for our study, this literature has focused primarily on the role of supply and the impact of borrower protection on lender behaviour. While the demand side is usually recognised, it receives little attention. This is surprising in the light of the many studies that find that protecting borrowers has important implications for the decision to default (Indarte, 2020; Cespedes et al., 2020; Dobbie & Song,

[12] While the majority of the literature focuses on the US, examples of international studies of borrower protection include India (Visaria, 2009), Brazil (Ponticelli & Alencar, 2016), Italy (Jappelli et al., 2005) as well as international comparative research (Haselmann et al., 2010).

2020; Pattison, 2020) and can significantly improve outcomes of households in financial difficulties (Dobbie & Song, 2015; Dobbie et al., 2017). A notable exception is Severino & Brown (2020) who consider the impact of borrower protection on market outcomes (lending stock levels). Like us, they note that protecting borrowers does not have a clear effect on lending stock as it has opposing effects on supply and demand – the outcome depending on which effect dominates.

The remainder of the paper is structured as follows. Section 1 outlines the legal process of a repossession in England and Wales and its interaction with housing and mortgage markets. Section 2 presents the theoretical framework. Section 3 presents the data and the empirical strategy. Section 4 presents and interprets the results. Section 5 reports our counterfactual policy exercise. Section 6 concludes by offering final remarks.

1. County court judges, mortgage repossessions and credit markets

1.1. The legal process of a repossession

When a borrower stops making repayments on a mortgage loan in England and Wales, the lender has to follow a regulated process that involves contacting the borrower and asking for a plan to repay the arrears. The lender can refuse the proposed plan and start court action called a mortgage possession case. The case is heard by a local court and the judge can issue one of three possible decisions. They can dismiss the case, which usually happens if the lender did not follow the proper procedure before submitting the case. They make a suspended order, which means that the house is not repossessed as long as the borrower complies with terms set by the judge but can be repossessed without a hearing if the rules are not followed. These terms can include making specified payments or improving the borrower's economic position (for example, by seeking employment). Finally, the judge can give an outright repossession order, which results in the title to the property being transferred to the lender and the borrower being forced to leave (evicted). After taking possession of the property, the lender has an obligation to sell it at a fair price, either through an auction or an estate agent. The proceeds from the sale are used to cover the borrower's obligations to the lender, the cost of court and administrative action and repay any other secured creditors. If anything is left, it is returned to the borrower. Anecdotally, the sale of a repossessed property can take a very long time while the lender goes through internal administrative processes or waits for the market to improve.

1.2. Courts' and judges' assignment to repossession cases

Possession cases are heard in the County Court (CC). The physical court in which the case is heard, sometimes referred to as a hearing centre, is determined by the postcode of the property. Possession cases are considered local community issues and, although they can be submitted online or to any physical court location, they are automatically transferred to the hearing centre that deals with the area where the property is located. The catchment area for each hearing centre is defined by a list of postcodes based on historical court counties. These areas are not overlapping with other geographical divisions (e.g., other administrative boundaries) and are not used for allocating other types of cases (criminal cases are heard by a different court and family law cases are not bound by catchment areas).

Each hearing centre has a fixed set of judges that rule over its cases. To begin with, the Ministry of Justice (MoJ) assigns judges to one of five regions. A judge can only rule on cases in the region they have been assigned to. Furthermore, courts within regions are organized into groups and judges assigned to a group only travel between courts within the group. This means that a case assigned to a hearing centre will be heard by one of the judges assigned to the group the hearing centre belongs to. In practice, however, judges are more ‘residential’ – meaning they tend to mostly hear cases in one of the CCs within the group and occasionally travel to address specific needs.[13]

In our data, we identify 30 groups based on documentation published by the MoJ in 2014 and 138 CCs as currently detailed by the MoJ. While groups can change over time (courts may switch groups), in our data group geography has remained largely unchanged. The average and median numbers of CCs per group are 7.6 and 5.5, respectively – with the top 10% and bottom 10% of the distribution of the number of CCs per group being 16 and 2.

1.3. Court hearing, decision-making and judges’ strictness

A possession hearing usually takes between five and eight minutes and the judge has some discretion in issuing an order. Although in principle, the judge should solely focus on determining if the borrower has a chance of repaying the arrears (not the whole loan), they are entitled to consider factors such as how much can the borrower afford to pay now and in the future, any temporary difficulties that the borrower is experiencing and the reason for accumulating the arrears (Whitehouse, 2009). This means judges can be ‘stricter’ or ‘softer’ – and that their personal inclinations as well as previous legal and work experiences can affect their decision making (Cowan et al., 2006). Indeed, some judges will have been promoted to such roles from lower administrative levels of the courts’ system; others might instead have come from the private sector having worked as lawyers and decided not to represent cases at the ‘bar’ (i.e., not to become barristers). Such different routes are likely to have a significant bearing on the tendency of judges to decide in favour or against the borrower/lender.

Figure 1 presents a map of CC groups with the implied group-level severity – measured by the number of repossessions over the number of cases presented to the CCs in a group over the years 2001 to 2018.[14] The severity index varies between 18.5% and approximately 41% – meaning that between a quarter and two-fifths of the cases submitted to courts result in repossession orders. While some of the differences in severity are surely explained by characteristics of the cases seen by judges, it is also likely that they reflect judges’ personal inclinations to side with either the borrower or the lender. For our

[13] An initial assignment of judges to groups and courts was drawn up in the early 1980s. Following that, new judges are assigned to posts on the basis of vacancies created by retirement, death or job changes of sitting judges. This is done through an open recruitment for the relevant duties.

[14] Group boundaries have been created by ‘aggregating’ the boundaries of postcodes that form the catchment areas of CCs that belong to the same group.

empirical analysis, we exploit discontinuities in judges' severity across the seventy-one boundaries that delineate these groups – as judges should not move between CCs across group boundaries. However, we measure severity at the CC level because: *1*- cases are assigned to CCs through catchment areas (and not to *any* of the CCs within the group); and *2*- judges mostly operate in one hearing centre even if occasionally they cover needs in other courts in the same group.[15]

In order to validate the institutional underpinning of our boundary discontinuity design, we tracked judges' assignments to CCs by scraping data over eight consecutive working weeks from CourtServe.[16] Over the 40 days during which we collected data, we find 184 judges holding 2,443 hearings. Of these, 158 judges (86%) hold hearings in only one CC (for a total of 2,006 hearing, or 82% of the total). Among the judges that do not hold hearings in only one CC, we find that 21 judges (11.4%) hold 59 hearings (2%) in another CC in the same group. On the other hand, 5 judges (2.6%) hold 15 hearings (0.6% of the total) in a different CC from their 'modal' one and travel to a different group. Of these, 3 (60%) travel to an adjacent group – so this very small violation of our assumption is likely driven by the slight re-drawings of groups occasionally implemented by the MoJ (discussed above). All in all, the evidence confirms our understanding of the legal framework and the validity of our research design.

1.4. Mortgage delinquencies and repossessions: households' perspective

Mortgage delinquencies in England and Wales are mostly caused by economic hardship triggered by life events such as redundancies, physical or mental health issues or family disputes (Croucher et al., 2003). This means that households facing repossessions are usually in a vulnerable position and have very few options but to resist the repossession process. Indeed, a household with enough income to secure another residence would usually choose to sell their house if they cannot afford to continue to repay the mortgage and avoid the additional costs of a repossession. When evicted, households usually struggle to find another accommodation. Although some can be housed in public housing (especially those with children who are classified as priority), it is very common for evicted households to experience a series of forced moves while they try to get back on their feet (Nettleton & Burrows, 2001).

Mortgage borrowers in England and Wales are not routinely informed about the CC their case would be heard in if they are delinquent or about the judges they are likely to face. For example, during the process of conveyancing (i.e., the legal procedures required to purchase a house), solicitors are not required to disclose to would-be homebuyers the CC with legal jurisdiction in the area of their purchase

[15] Nonetheless, we test the robustness of our analysis to alternative levels of aggregation – for example, by using severity at the group level (see Sections 3 and 4).
[16] This web service (provided by Courtel Communications) lists courts where judges hold hearings on a given day as well as other information – such as hearing room numbers and the details of the individuals involved – in approximately 3,000 different daily webpages. An important shortcoming of CourtServe is that it lists judges by family name (and title) only – so there are cases of homonymous (same name) judges sitting at the same time on the same day in CCs across the country. To disambiguate such cases, we supplement these data with information from lists produced by the MoJ that provide full details of all judges eligible to sit in CCs. Using these additional data, we identify judges with non-ambiguous family names in CourtServe and focus our analysis on those.

or the risk of a repossession order (conditional on being delinquent). While we cannot rule out that buyers receive this information from estate agents, sellers in financial difficulties or that this information is salient to buyers and sellers who are more likely to default, it is likely that the information about the judge's preferences is transmitted indirectly. For example, it has been well documented that repossessed properties are easy to identify, sell for lower prices and reduce the value of local amenities (Campbell et al., 2011). This effect has also been reported for properties that are delinquent but not repossessed (Whitaker & Fitzpatrick, 2013). When households (buyers and owners on the verge of financial distress) observe more evidence of delinquencies, it can affect their perceived risk of a repossession. Indirectly, observing more repossessions (caused by a stricter judge) can inform households about the risk of their house being repossessed (Mian et al., 2010).

2. Theoretical framework

Court strictness affects economic decisions through the mortgage credit market, so we start by modelling household-level demand and supply for mortgage loans. We model the demand side treating mortgage demand as endogenous to housing demand. We derive mortgage demand from a standard household demand for housing services model under risk in the spirit of Campbell and Cocco (2007). This approach allows us to show the impact of an exogenous change in the risk of a repossession on both housing and mortgage demand. Next, we outline the decision of a bank to provide a loan and show how credit supply is affected by court strictness. Importantly, we keep supply and demand characterisations as simple as possible and emphasize tractability and clarity because the only aims of our framework are to show: *i*- if demand and supply change with severity; and *ii*- if they are increasing or decreasing functions of severity. We also describe the implications of our model for house prices. Finally, we discuss some key empirical implications and provide an intuition of how to reconcile our framework with evidence from US studies.

2.1. Housing and mortgage demand

2.1.1 Housing consumption with certainty

We consider a one-period model in which a household's utility is delivered by consuming housing and non-housing goods denoted by h and x, respectively. Our model has only one period because there is only one allocation decision. However, events occur in sequence.[17] Households start with savings A and some *ex-ante* information about income I_n they will receive as soon as they buy a house. They buy a house for ph (where p denotes the price of housing consumption and h sets housing consumption) using all available assets A and taking a mortgage loan L to finance the rest of the purchase so that $L = ph - A$. After they purchase the house, households receive income and pay interest r on the loan. Next, they

[17] A multi period model yields the same qualitative results but in more complex analytical forms.

consume housing and non-housing goods throughout the period. At the end of the period, they sell the house, pay the principal back, receive their savings and start again.

With the price of non-housing consumption normalized to 1, the budget constraint is: $I_n + L + A = x + ph + Lr$. Combining with the liquidity constraint gives:

$$I_n - r(ph - A) = x \quad (1)$$

The key decision is to allocate the budget between housing and non-housing consumption in a way that maximises utility. This allocation decision is made at the time of purchasing the house, so this purchase determines utility for the period.

2.1.2 Income risk and delinquency

We add to the above problem by allowing income to be uncertain – the amount of income a household actually receives is revealed after the house is purchased. There is a probability q that income is lower than I_n and takes the value of I_u.[18] When income is I_u, it is only enough for essential non-housing consumption and nothing else (we think of such income as coming from social benefits). This means that the borrower is delinquent and does not pay the interest on the loan. We assume that a delinquency is triggered by an unexpected life event (e.g., divorce, death of a spouse, or unexpected unemployment spell) and therefore that q is not related to the size of the loan. The budget constraint becomes $I_u = x$ ($L + A = ph$ so these terms cancel out). Stated differently, lending and savings pay for the house; income pays for consumption; and the interest on the loan is not paid. When the loan is delinquent, the lender will attempt to repossess the property. They succeed with probability d and a repossession sets housing consumption of the borrower to zero. If the lender is unable to repossess the property, the household can consume all of its income as non-housing and it can also maintain its housing consumption. Note that this introduces an insurance element into the credit market as the utility of borrowing increases as d decreases.

In order to keep the analysis simple, we made two important adjustments. First, we assume that house prices are constant. This allows us to keep equations as simple as possible. The analysis can be adjusted to incorporate varying house prices, but the qualitative implications of the model would be the same. Second, we treat q as exogenous rather than interacting with r. This is necessary to obtain a closed-form solution to the model and is rationalized by our institutional setting. Indeed, an exogenous r (unrelated to q and d at the local level) is an important feature of our empirical set-up. We keep these assumptions in mind when designing our empirical strategy and interpreting our results.

2.1.3 Choice under risk

Consumption preferences are given by an additive isoelastic function with parameter σ (Campbell and Cocco, 2007; 2003; 2015):

[18] For simplicity we model risk based on possible states, but the analysis could be extended to continuous risk distributions and offer the same qualitative conclusions.

$$U(x,h) = \frac{x^{1-\sigma}}{1-\sigma} + \frac{h^{1-\sigma}}{1-\sigma} \quad (2)$$

This function has additive separable utility with CRRA risk preferences and risk aversion given by σ.[19] Table 1 characterises the three possible states the household can expect and their corresponding probabilities. The expected utility with income and repossession risks is given by:

$$\mathrm{E[U(x,h)]} = [1-q]\left[\frac{(I_n - r(ph-A))^{1-\sigma})}{1-\sigma} + \frac{h^{1-\sigma}}{1-\sigma}\right] + q\left[(1-d)\left(\frac{{I_u}^{1-\sigma}}{1-\sigma} + \frac{h^{1-\sigma}}{1-\sigma}\right) + d\,\frac{{I_u}^{1-\sigma}}{1-\sigma}\right] \quad (3)$$

Which gives housing and mortgage demands as:

$$h = \frac{I_n + Ar}{\left(\frac{rp(1-q)}{1-qd}\right)^{\frac{1}{\sigma}} + rp} \quad (4)$$

$$L = \frac{p(I_n + Ar)}{\left(\frac{rp(1-q)}{1-qd}\right)^{\frac{1}{\sigma}} + rp} - A \quad (5)$$

These give intuitive results of housing demand increasing with income and assets but decreasing with prices. Furthermore, they make it clear that the impact of risk on demand depends on risk aversion. For $\sigma < 1$ (risk-seeking) more risk will increase demand, while for a risk-averse case ($\sigma > 1$) increasing risk will reduce demand. All else equal, in areas where courts are strict, housing and mortgage demand will be lower for an intuitive value of $\sigma > 1$.

2.2. Credit supply

The bank has a cost of providing a mortgage given by r_a, which includes the cost of capital and originating the loan and is lower than r. It accepts the application if the return it expects to receive on the loan equals or exceeds this cost ($r_a \leq E(r)$) and increases profit by increasing the total value of accepted loans.

We assume that the risk on mortgage loans is limited to the risk of not receiving the interest while there is no risk to the principal being returned: prices are constant, so the bank sells the property for the same value it was purchased and recovers the loan. We also assume that the interest on the loan is lost only when the bank is unable to repossess the property when the borrower is delinquent. When the loan is delinquent, but the house is repossessed, the bank is able to invest the funds recovered from selling

[19] Note that closed-form analytical solutions for other utility functions cannot be obtained. Nonetheless, numerical solutions for other common consumption preferences (including Cobb-Douglas presented in the Appendix) give the same qualitative result; $\partial h/\partial d < 0$.

the property at the same rate so incurs no loss.[20] The states and probabilities for the bank are given below in Table 2. Assuming the lender is risk-neutral, the lending problem is:

$$r_a \leq (1 - q + qd)r \qquad (6)$$

This suggests that keeping interest rates and the cost of lending constant, more loans should be accepted in places where the risk of delinquency is lower but the probability of a successful repossession is higher. All in all, this analysis shows that mortgage supply increases in the index of court severity.

2.3. House prices

Aggregate housing demand is set by the sum of demand from individual households subject to their corresponding mortgage demand being satisfied by banks. For simplicity, we ignore housing supply and assume that prices are set by demand.[21] Through the constraint credit availability places on housing demand, credit supply can affect prices (Mian & Sufi, 2009). Aggregate credit supply is given by the total value of all accepted loans. To make our analysis of the impact of credit constraints on house prices clearer we model r_a as a function of loan size[22] so that there is a maximum loan the lender is willing to give to a household (denoted by L^s) set by the condition $r_a(L^s) = (1 - q + qd)r$. Aggregate mortgage lending is given by the sum over n households living in the area who can borrow either their demanded amount L or the maximum a bank is willing to lend them L^s: $\sum_{i=1}^{n} \min\,(L_i, L_i^s)$. The aggregate housing demand is therefore $\sum_{i=1}^{n} \min\,(h_i, L_i^s + A_i)$. Note that since court severity has a negative impact on h but a positive impact on L_i^s, the impact of court severity on housing demand does not have a clear sign. This makes the impact of increasing severity an empirical problem.

2.4. Empirical implications

Our empirical goal is: *i-* to estimate the impact of court strictness on house prices; and *ii-* to explain such link by providing evidence for the credit market mechanism. The first objective is a simple reduced-form estimation exercise that shows whether prices are mostly controlled by demand factor – i.e., prices are negatively affected by severity – or supply factors – i.e., prices are positively affected by severity. The second objective is more challenging because both supply and demand for credit are affected simultaneously by severity, and the mechanism in question relies on both.

To illustrate our problem, we define the probability that a delinquency results in a repossession as a function of court severity denoted by C. With an exogenous shock in C – for which we will use boundary discontinuities – we are able to study the impact of changes in court strictness on lending and house prices. As already discussed, it is important to note that judges' preferences are not a flexible

[20] Note that we ignore timing issues: as soon as a borrower is delinquent, there is a decision on repossession; and as soon as there is a repossession decision, the house is sold and the funds are invested at the same rate originally agreed with the borrower.

[21] This is a common assumption in urban economics as housing supply is slow to adjust (see Mayer & Somerville, 2000). Our model can incorporate elastic housing supply and yield the same qualitative conclusions.

[22] This assumption is not required for our results, but it makes them considerably easier to present. It is also consistent with weights applied in the UK under internal-risk based (IRB) risk modelling.

parameter set by the market. This is especially evident in our boundary-discontinuity design where housing market conditions and socio-economic characteristics are essentially identical across CC group's boundaries – and yet judges rule differently on the 'strict' and 'soft' sides. We further test this 'disequilibrium severity' claim later in the paper by showing that judges do not adjust their severity when conditions in the market change.

In our institutional context, interest rates do not react to changes in severity – at least not at the local level and across court catchment-area boundaries. This means that C will likely not be at the market equilibrium set by supply and demand. This is presented in Figure 2, which plots supply and demand as functions of court severity (for a given interest rate)[23]. Here C^* denotes the theoretical equilibrium level of court severity that would maximize lending denoted as L^*. When court severity is not at the equilibrium, credit will be constrained either by demand or by supply. For a level of severity higher than C^* and denoted by C^+, households will demand less credit than in the equilibrium. Moreover, the level of lending will decrease as severity increases – and this change will be determined by 'sliding up' along the demand curve. Conversely, for a level of severity lower than C^* denoted by C^-, lenders will supply less credit than in the equilibrium. In addition, the level of lending will increase as severity increases as determined by 'sliding up' along the supply curve. Therefore, for $C < C^*$ it follows that $\frac{\partial L}{\partial C} > 0$, while for $C > C^*$ we would have $\frac{\partial L}{\partial C} < 0$.

As discussed above, credit demand is derived from housing demand. However, the credit market also affects the housing market via the supply side – as credit supply can impose a limit on housing demand. Indeed, the literature finds strong support for the impact of credit supply on house prices (Mian & Sufi, 2009; Szumilo, 2020). This means that the housing demand we observe in the data is either set by 'unconstrained' housing demand derived from the utility maximization spelled out in Equation 4; or the maximum house price a household can finance when credit supply is constrained.

We illustrate this in Figure 3, which shows how house prices are set in those two cases.[24] In the first case (left-side plot), demand in the housing market is unaffected by credit supply. This diagram represents housing demand in areas where severity is at $C^+ > C^*$ in Figure 2. In the second case (right-side plot), credit supply imposes a cap on demand – corresponding to areas where severity is at $C^- < C^*$ in Figure 2. This distinction is relevant in our context because it makes it clear that changes in the housing demand derived from the utility function will not affect house prices in the presence of credit supply constraints. This can be understood by considering an outward shift in demand in the right-side plot: as the majority of the demand schedule is flat at the level constrained by credit supply, such shift

[23] If interest rates were allowed to change with severity across boundaries, the market could clear through such adjustments (subject to different interest-rate elasticity of supply and demand, as interest rates have opposing effects on the two sides of the market). However, even with varying rates, severity would still have an optimal level at which house prices and lending are maximised. Although it would be more difficult to model, exogenous severity would remain a friction even with varying interest rates (for example in the US).

[24] Once again, we assume a vertical and completely inelastic housing supply. An elastic housing supply would not change our conclusions.

will have no impact on prices.[25] This is clearly not the case in the left-side panel where demand is unconstrained. Importantly, the same diagram (right-side) also shows that when credit supply restricts house prices, changes in credit supply will determine changes in house prices – through a shift up or down of the horizontal part of the constrained demand schedule – even holding constant the housing demand side. On the other hand, credit supply does not directly move 'unconstrained' housing demand in the left-side plot – and so has no effect on house prices.

Such interactions of credit and housing markets are useful in empirically determining if court strictness is above or below the equilibrium level C^*. This is because, by studying whether house prices react to a shock to credit supply, we can determine if house prices in the market are limited by credit supply (denoted as C^- in Figure 2) or not. In our empirical work, we exploit an exogenous shock that reduces credit supply. Specifically, we leverage changes in repossession procedures introduced by the biggest mortgage lender in the UK – Lloyds Bank – following pressure from the regulator in 2015. These led the bank to reduce its credit supply to account for increasing expected losses on delinquent loans. We therefore classify markets where this shock has a negative impact on house prices as limited by credit supply. Conversely, all remaining areas are treated as dominated by demand.[26]

2.5. Recourse versus non-recourse mortgages

Figure 2 also helps illustrate the difference between recourse and non-recourse loans. In institutional settings with mortgage recourse, the loss (for the bank) when the loan is delinquent is lower as other household assets can be used to cover any losses incurred by the bank. In section 2.1, we ignore recourse as prices do not vary, so lenders recover their investment from selling the house. However, it is clear that (all else equal) demand for full-recourse mortgage loans would be lower than the level pinned down by our expressions. Notably, this would shift the credit demand curve inwards and reduce the equilibrium level of court severity. At the same time, a lower loss in default would shift the supply curve outwards. This helps to explain why in the US, where loans are non-full-recourse, the impact of increasing severity on loans size is positive, while in the UK, it can be negative: court severity is likely to be above C^* in most areas of the UK, but below C^* in the US.[27] A similar increase in severity would therefore result in opposite effects on prices and loan sizes. The model in Kosem (2019) relaxes our full-recourse assumption and yields predictions consistent with findings from the US.

[25] Note that an inward shift of the demand could have an impact on prices if it is sizeable enough for the downward sloping part of the demand to intersect supply. Such cases do not seem to empirically occur in our data.

[26] In reality, this shock also likely increased demand due to insurance effects – borrowers knew they were less likely to be taken to court in the case of a delinquency. This means that, in areas dominated by demand, prices likely increased. Empirically, we therefore classify supply-dominated places are areas where the shock had a negative impact on prices; and demand-dominated places as those where the impact was either non-negative or positive. We return to these issues in Section 4.

[27] Note that UK and US markets will have different levels of equilibrium severity C^*.

3. Data and estimation strategy

3.1. Data sources and descriptive evidence

Data on house prices come from the Land Registry and consist of records of every arms-length transaction in England and Wales, geocoded at postcode level using the National Postcode Directory. We use the years 2001-2018 to line up housing market information with court severity data. Data on repossession claims and orders at the CC (as well as group and Local Authority District, LAD) level comes from public records of the MoJ and start in 2001. The same data at ward level come from a Freedom of Information (FoI) request submitted to this Ministry.[28] Similarly, the list of postcodes that belong to each hearing centre was obtained through an FoI request. Our data on mortgage lending by banks come directly from the lending banks and give the total stock of mortgage lending per quarter in a postcode sector since 2014.[29] We also have access to transaction-level data from the largest building society in the UK (Nationwide PLC). These data include the price of a transaction, the mortgage advances and some characteristics of the property. Finally, we use data from the 2001 and 2011 Census at Output Area (OA) level on socio-economic characteristics of neighbourhoods.[30]

In order to carry out our boundary-discontinuity research design, house transactions were assigned to CCs and groups using the ‘mapping’ provided by the MoJ, and straight-line distances to the closest boundaries were calculated. Geographical manipulations and distance calculations were carried out using ArcGIS. To obtain comparable samples of transactions across boundaries, we exclude all newly built houses.[31] Data were winsorized and transactions in the top and bottom 1% of the yearly price distribution were dropped. The number of observations in our full sample is 15,292,907 in 30 CC groups with 138 CCs and 71 boundaries. For our main empirical analysis, we use a ‘boundary sample’ that only includes transactions within the 25th percentile of the boundary-specific distance distribution. This spatially varying ‘distance window’ is used to factor in differences in density of transactions in more urban/rural areas. However, fixed distance corridors (e.g., 2km) are used in our robustness checks and provide similar results. The number of observations in the boundary sample is 3,824,307 in 30 groups with 71 boundaries and 117 CCs.

Our main proxies for severity ratios are measured as the number of mortgage repossession orders divided by mortgage repossession claims either at the CC level or overall in a group, and either on a yearly basis or on average across all years in our sample (2001-2018). This approach has potential limitations as highlighted by Dobbie & Song (2015) who employed a similar strategy. In particular, for this index to be a meaningful measure of the ‘risk’ households react to, the characteristics of cases

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[28] A ward is an electoral region in the UK with around 5,500 residents. In 2014, there were around 9,500 wards in the UK. They are considerably smaller than a local authority and usually smaller than a postcode sector.
[29] A postcode sector is a geography based on contingent postcodes. It includes around 3,000 commercial and residential addresses and its geographical size varies based on building density.
[30] An OA is a census geography with around 100 residents.
[31] While this did not affect the results, using only existing structures allows for a better comparison across boundaries.

assigned to different judges should be the same and judges' severity ratios should be persistent over time. While the latter is easy to demonstrate with our data (see later discussions), we do not observe characteristics of individual cases. However, we show that the severity ratios do not change even when characteristics of submitted cases change due to an exogenous event (we develop this point in Section 3.2). Furthermore, we do not find evidence that the number of cases submitted to courts or socio-economic characteristics of areas changes across the boundaries (see later discussions). This suggests that the differences in severity indices are driven by preferences of judges.

Nonetheless, we go one step further and define severity using an alternative measure that deals with the possibility that our key proxy is biased by the type of cases submitted (i.e., the possibility that a judge's severity can affect the type of cases they see). This alternative uses social housing repossession ratios. The idea is based on the fact that the same judges rule on social tenant and private mortgage cases so their severity ratios can be 'extracted' from decisions on either type of cases. However, while mortgage repossession cases are submitted to courts on the basis of a decision taken by a private lender, social tenant repossession cases are submitted following standardised procedures of public institutions. These procedures (like all social housing decisions) are set at LAD level, and each LAD has its own procedure for submitting social claims to courts. However, houses from the same LAD belong to more than one CC – so submission procedures are unlikely to be biased by any one's court expected severity. We measure LAD severity as the ratio of social tenant repossession orders issued for properties within its boundaries to the number of claims submitted. To obtain a court-specific measure of severity based on social claims, we average the LAD severity ratio for all LADs within the catchment area of a court and weigh each LAD's index by the share of the catchment area of the court it covers.

Table 3 shows descriptive statistics for the full sample and for locations close to the boundaries. Prices are slightly higher in the boundary sample – with fewer leasehold properties and more detached houses. On the other hand, the different severity indexes we consider are similar in both samples. We also find that the correlations between different measures of severity are relatively high. Using the boundary sample, we find that CC-level severity measured at a yearly frequency has a 0.39 correlation with CC-level severity averaged over the years; and a 0.31 correlation with time-averaged group-level severity. This suggests that judges' severity is fairly persistent over time. To further validate this point, we considered the raw correlations between the various severity measures calculated for the years up to/after 2009. We found these to be relatively high and always in excess of 0.55.

Since we are interested in the spatial distribution of severity, we investigate geographical patterns in more detail. First, we use CC- and group-level data to investigate the correlation between judges' severity across group boundaries. We find that the correlation between a CC's severity and the average severity of all CCs in the adjacent group is low – at 0.17. Conversely, the correlation of severity across CCs that belong to the same group is more than four times larger – at 0.69.

Next, we use ward level data to investigate fine-grained spatial patterns of correlation in severity across wards that belong to the same CC – and across CCs that belong to the same group. Our results are presented in Appendix Table A1.[32] In Panel A, we regress ward-level severity on: *i*- the average severity measured across all other wards that belong to the same CC; or *ii*- all other wards in the same group – excluding those belonging to the CC in consideration. Given how the analysis is structured, this approach is in the spirit of 'spatial spillovers' regressions.[33] We find that severity is highly positively correlated over space within a CC catchment area. There is also a positive correlation within the same group, although the magnitude of this association is lower than within the same CC catchment area – both conditional and unconditional on the correlation detected within CCs, across wards (see Column 3 and 6 of Panel A). Next, in Panel B, we study the correlation between distance to the boundary and severity ratios. We find no systematic evidence that severity varies across wards that belong to the same CC as we move closer or further away from the group boundaries.

All in all, this descriptive analysis suggests that courts apply the same level of severity within their jurisdictions. There is some correlation across CCs within the same group – which is expected as judges can move across CCs belonging to the same group (although they are mostly residential as discussed above). However, such correlation is clearly not evident as we cross group boundaries. This supports our boundary discontinuity strategy, which we describe in the next sub-section.

Finally, we also exploit the ward level data to study whether changes in the severity ratio across boundaries are driven by the number of cases submitted to a court (rather than the number of repossession orders issued by judges). We find this is not the case – as shown in Appendix Figure A1 and further corroborated in additional regression analysis (not tabulated for space reasons).

3.2. Methods I: Identifying the causal effect of court severity

Our first goal is to estimate the causal impact of severity on house prices and mortgage values. Standard regression techniques would however yield biased estimates because of the unobservable factors that simultaneously drive housing and credit markets as well as judges' decisions making. To by-pass this issue, we use a spatial boundary discontinuity design (BDD) similar to Mian et al. (2016) and Gibbons et al. (2013). To formalise ideas, we would like to estimate the following relationship:

$$P_{icgbt} = \alpha + \beta_C C_{icgbt} + \Lambda X_{icgbt} + T_t + g(c) + \epsilon_{igcbt} \quad (7)$$

Where P_{icgt} denotes the (log of) transaction price of house i in the catchment area of court *c* belonging to group g matched to boundary b at time *t* (recorded in the Land Registry data); C_{icgbt} denotes the (standardized) severity ratio for court *c* belonging to group g closest to boundary *b* at time *t*; X_{icgbt} is a vector of housing characteristics with associated coefficients Λ; T_t are time shocks (we use year and month dummies); $g(c)$ is an unknown function that captures the impact of unobservables

[32] The note to the table provides more details of the sub-sample of boundaries, CCs and groups we can employ when using ward level data.
[33] We also experimented with the reverse the approach and regressed CC- or group-averaged ward-level severity on severity in a given ward. This specification is in the spirit of *Moran I*'s statistics used in quantitative geography to detect spatial patterns in data. We find similar patterns. Results are available upon request.

related the location of court c on house prices; and ϵ_{igcbt} is an error term. The same regression can be used to measure the impact of court severity on loan size and loan-to-value (LTV) ratios by replacing the dependent variable and using data from Nationwide PLC.

In the above regression the key parameter of interest is β_C, which captures the reaction of prices (or mortgage values) to a (one standard deviation) change in the severity index. However, estimating Equation (7) by standard by standard OLS techniques would be problematic because of the term $g(c)$. This term captures the possible impact of any of the following unobservables: i- differences in housing and credit market conditions in areas falling into the catchment areas of different CCs – which affect house prices, mortgage quantities and court severity; ii- group-wide shocks (possibly time-varying) that impact housing and credit markets as well courts' behaviour; and iii- location-specific features and geographical attributes – proxied by a property's coordinates – which could drive house prices and mortgage borrowing/lending and be correlated with court severity.

To deal with these issues, we follow a spatial BDD that exploits discontinuities in severity across boundaries of CC groups. To begin with, we restrict our analysis to properties that are close to the boundaries that divide groups – namely, those that fall within the 25th percentile of the boundary-specific distance distribution. We then include in our model boundary fixed effects so that identification is obtained by comparing properties in close distance to the same boundary by either on the 'strict' or the 'soft' side when it comes to court severity. The underlying assumption of our research design is that housing and credit market characteristics – as well as the characteristics of the cases seen by the judges – do not change discontinuously when crossing group boundaries. Below, we provide strong balancing evidence to support this assumption. This BDD approach allows us to focus on the impact of the difference in court severity while holding other unobservable factors 'locally' fixed.

To strengthen identification, we also control for third-order polynomials in distance from the boundary. These account for possible spatial trends in house prices and mortgage conditions as we move away from the group boundaries – which in turn could be correlated with court severity because of the specific location of hearing centres. Finally, we control for group or group-by-year effects to account for the impact of group-wide (possibly time-varying) shocks that affect housing, credit markets and courts' decision making. In practice, we estimate the following version of Equation (7):

$$P_{icgbt} = \alpha + \beta_c C_{icgbt} + \Lambda X_{icgbt} + \sum_{d=1}^{3} \delta_d dist^d + T_t + B_b + \Gamma_g + \epsilon_{igcbt} \tag{8}$$

Where most variables were discussed above; B_b are boundary dummies; the $dist^d$ terms capture the non-linear (third-order polynomial) impact of distance to the boundary; and Γ(g) are either group or group-by-year effects. To account for the possible correlation in the error term of transactions located along the same boundaries, we cluster standard errors at the boundary level.

As already discussed, we exploit discontinuities in severity across boundaries that delineate groups because judges do not move between CCs across group boundaries. However, we mainly measure severity at the CC-level because cases are assigned to CCs through catchment areas, and judges mostly operate in one hearing centre (even if occasionally they cover needs in other courts in the same group). This means some additional variation comes from changes in severity along boundaries – *within* the same group.[34] In order to allay any related concerns, we also estimate models that measure judges' severity at the group level. Furthermore, we experiment with court severity measures averaged across all years in our sample to reduce the possible impact of 'noise' in our key variable of interest. Finally, following the BDD literature, we perform a number of robustness checks by varying the cut-off distance to the boundary to determine our estimation sample; and by leaving the observations very close to the boundary out of our sample (i.e., a 'doughnut' approach).

3.3. Methods II: Addressing possible biases in courts' severity measures

One key challenge to identifying the impact of court severity comes from the fact that the ratio of repossessions to claims could be a biased measure of severity. This could be a problem if: *i*- the type of cases submitted depends on the severity of the judge; or *ii*- if judges change their severity based on the type of cases they see.

To address the first issue, we replace the severity index with the LAD-based index described above and for which the bias from the type of submitted cases is likely to be minimal (see Section 3.1). To address the second issue, we note again that the severity ratio is likely exogenous to local market conditions close to group boundaries. This is because such severity is likely to be determined by cases from the whole CC – and not just those close to the boundary. Appendix Table A1 (discussed above) confirms this intuition: judges apply the same level of severity irrespective of how proximate to the boundaries the cases they assess are.

To further test if judges change their severity in response to changes in market conditions, we exploit an exogenous shock to the type of cases submitted to courts. The shock is based on the fact that one of the biggest lenders in the UK – Lloyds Bank – treated their struggling (in financial difficulties) customers unfairly (compared to other lenders) between 2011 and 2015. As a result, it was ordered by the Financial Conduct Authority (FCA) to stop such practices and review its procedures.[35] The FCA also clarified that Lloyds failed to take struggling customers' financial circumstances into account when arranging repayments and when initiating court proceedings (FCA, 2020).

Our intuition is that such unfair treatment of struggling customers led to more repossession claims being submitted to courts in locations where Lloyds had a larger market share. However, these unfair claims should not have resulted in repossession orders if judges take into account the characteristics of the cases they see (and adjust their relative severity): the cases were easily resolvable – and should not

[34] Note that this helps with the estimation of models that condition on group and group-by-year fixed effects.
[35] The FCA also issued a fine of almost £100m (in 2020) against Lloyds Bank for not providing enough support to customers in arrears.

have been brought to court. Once the bank introduced a policy that brought its procedures in line with the rest of the market in 2015, the 'additional' claims were no longer submitted to courts.

To test if judges react to cases they see, we therefore design a quasi-difference-in-difference (DiD) design that compares the number of repossession orders issued by judges in years when they would see the additional 'unfair' cases relative to the number of orders they issue in years when they only see cases that any fair lender would submit in areas with high or low penetration of Lloyds in the local mortgage market. Due to data limitations, we are unable to analyse changes occurring at the beginning of the period highlighted by FCA – i.e., the increase in cases unfairly submitted to court between before/after 2010.[36] We therefore focus on the change that occurred in 2015 when (according to FCA) Lloyds introduced a decisive policy to improve its advice to struggling customers.

In order to carry out our analysis, we use data on repossession orders and claims at ward level by year (discussed above). For each ward, we then construct an index of exposure to Lloyds using data from the bank itself on the total stock of mortgage lending by postcode sector and dividing it by the combined stock of mortgage lending of the seven biggest lenders in the UK from UK Finance (covering around 75% of the market) in the first quarter of 2015. To translate postcode sectors to wards, we first assign the Lloyds exposure index to each full postcode and then average the index at Ward level using postcodes it covers. This allows us to run the following event-study regression:

$$C_{wt} = \alpha + \sum \beta_t T_t \times Lloyds_w^{2015} + \Psi_w + T_t + \epsilon_{wt} \qquad (9)$$

Where C_{wt} denote severity in ward w at time t; Ψ_w and T_t are ward and year fixed effects, respectively; and inside the summation term we have a set of interactions between time dummies for each year in the sample (2010-2018) and the value of the Lloyds exposure index – with associated coefficients β_t. We use 2018 as the benchmark period (omitted year) as we believe this represents the latest year in which Lloyds' practices 'converged' to other lenders' standards. We present the findings from estimating Equation (9) graphically in an event-study fashion. This is helpful to highlight possible dynamic effects and to assess the parallel-trend assumption required for DiD. However, we also tabulate simple DiD results that replace the year interactions with a 'post-2015' dummy.

The key insight provided by these regressions is a test of whether the severity of judges reacts to the type of cases they see. To provide evidence that Lloyds did submit an 'anomalous' – and presumably 'unfair' – number of cases to courts, we also test if judges actually see any additional number of claims by using this variable on the left-hand side of Equation (9). Finally, we perform a falsification test using social tenant number of cases and ratios (instead of 'private' repossession cases). The intuition behind such falsification test is that the number of social tenant cases submitted to the court (and the behaviour of the courts) should not be affected by changes in the policy of Lloyds – a private mortgage lender.

[36] This is because our Lloyds exposure index can only be constructed after 2015. Prior to that date, Lloyds reported its lending stock together with TSB Bank PLC which was part of the Lloyds Banking Group.

3.4. Methods III: Identifying the impact of court severity on demand

Our final objective is to estimate the effect of court severity on housing and mortgage demand. As noted in Section 2.4, the overall reduced-from effect estimated in Equation (8) captures the average reaction to severity across all boundaries. In some places such effect could be positive while it could be negative in others – depending on whether demand or supply considerations dominate. As highlighted by our theoretical framework, in order to capture a 'pure demand' effect we would need to ensure that strictness is above C^* to begin with. We operationalise this idea by exploiting the spatial density of our data and focussing on boundaries where severity is likely to be set 'too high' for the (local) credit market to clear (i.e., at $C^+ > C^*$ in Figure 2).

The first step in this direction is to document how significant the heterogeneity in the price effect of severity is across the boundaries in our sample. Areas where the effect is clearly negative are locations where demand considerations likely dominate; the opposite would be true for areas where the price effect of severity is positive. To do so, we estimate specifications where we allow the impact of severity to differ by boundary:

$$P_{icgbt} = \alpha + \sum \beta_{C_b} C_{icgbt} \times B_b + \Lambda X_{icgbt} + \sum_{d=1}^{3} \delta_d dist^d + T_t + B_b + \Gamma_g + \epsilon_{igcbt} \tag{10}$$

Where most terms were already defined and the expression inside the first summation term denote interactions between severity and boundary dummies, allowing for the estimation of a boundary-specific price impact of severity.

While this descriptive analysis is revealing about the extent of the heterogeneity in the price effect of severity – and possibly suggestive of locations where demand factors dominate – this evidence still does not reveal whether changes in severity as we cross boundaries in our BDD set-up entail only movements along the demand curve – or a mix of supply and demand considerations (or using the notation of our theoretical framework: movements from low level of C^+ to higher level of C^+, thus remaining above C^*; as opposed to moving from C^- to C^+. See Figure 2 for further clarity).

To find areas that are clearly demand dominated, we exploit once again the shock provided by the change in how Lloyds approached its vulnerable customers. Indeed, taking fewer cases to court meant that the bank was likely to face bigger losses when a case was delinquent. In order to remain profitable, the bank therefore reduced its supply of credit – and only approved safer loans. This created a negative credit supply shock. This intuition is consistent with the existing literature on supply-side reactions to borrower protection (Pence 2006; Dagher & Sun 2016).[37] However, when the probability of being repossessed even when being delinquent decreases (because fewer cases are taken to court), demand should increase. This is the insurance channel highlighted in our model and discussed above. In short,

[37] Although the supply side (credit provision) response to this shock is not the focus of our paper, in Appendix Figure 2 we show empirical evidence consistent with a reduction in supply by Lloyds after the change in policy.

we interpret the 'Lloyds event' as a simultaneous shock to demand and supply shifting the former outwards and the latter inwards.

Because the shock occurs simultaneously to demand and supply, it can affect prices in areas where severity is both above and below C^*. However, the impact of this shock would have opposite signs in those areas. The analysis of Figure 3 in Section 2.4 crystallised these intuitions. Leveraging these insights, we identify boundaries where the Lloyds event had a negative impact on prices and classify them as limited by credit supply. Conversely, we classify boundaries where the shock had a positive (or non-negative) impact as markets where demand is pent up by $C^+ > C^*$. We label these areas as 'demand constrained' markets for which the impact of severity on prices should represent the 'true' elasticity of credit demand to court strictness. Empirically, we run the following regression:

$$P_{icgbt} = \alpha + \sum \lambda_b B_b \times Lloyds_{icgb}^{2015} \times I(\mathrm{T} \geq 2015) +$$
$$+\Lambda X_{icgbt} + \sum_{d=1}^{3} \delta_d dist^d + T_t + B_b + \Gamma_g + \epsilon_{igcbt} \quad (11)$$

Where $Lloyds_{icgb}^{2015}$ captures the penetration of Lloyds bank in the postcode of transaction *i* belonging to court *c* in group *g* matched to boundary *b* and time-fixed in 2015; and $I(\mathrm{T} \geq 2015)$ is an indicator function for years after 2014 (all other terms have already been defined). Once again, note that lending data is available at the postcode sector level, so we assign the same Lloyds exposure index to all postcodes belonging to the same postcode sector. Equation (11) is a quasi-DiD regression where each boundary can be affected by the Lloyds treatment differently and this heterogeneous effect is captured by λ_b.[38] We focus on boundaries where $\lambda_b \geq 0$ as these are areas where the shock affects prices through an outward expansion of demand – while supply plays no role. This allows us to estimate the 'pure demand' effect from the following regression:

$$P_{icgbt} = \alpha + \beta_C^D C_{icgbt}^D + \beta_C^S C_{icgbt}^S + \Lambda X_{icgbt} + \sum_{d=1}^{3} \delta_d dist^d + T_t + B_b + \Gamma_g + \epsilon_{igcbt} \quad (12)$$

Where C_{icgbt}^D denote severity in 'demand dominated' locations where the impact of the Lloyds event is positive, while C_{icgbt}^S indicates the severity index in other areas that are 'supply constrained'.

4. Results

4.1. Reduced form BDD

We start by presenting graphical evidence on discontinuities in our key variable of interests in Figure 4. This presents BDD plots for house prices (left-hand side), loan values (central panel) and court

[38] Our empirical specifications also include two-way interactions between post-2015 indicator and boundary dummies; and between Lloyds' lending initial exposure ($Lloyds_{icgb}^{2015}$) and boundary dummies. These are not added to Equation (11) for notational simplicity.

severity (right-hand side) in 2km-windows from a group boundary. Positive distances correspond to areas with stricter judges, while negative values represent areas with softer courts. By construction, court severity is higher to the right of a group boundary. More interestingly, we find lower prices and smaller mortgages in areas where judges are stricter. This supports our conjectures. Note that strictness 'ordering' considers group-level severity to mirror the descriptive evidence provided in Figure 1. While this exploits the seventy-one discontinuities across group boundaries, this approach neglects variation at the CC level, which instead we exploit in our regressions. As a result, the evidence is 'noisy' – especially when considering loan values which are only tracked in the smaller Nationwide sample. We next turn to our regression analysis which uses all the variation in the data.

Table 4 shows the results of our reduced-form specifications of the association between court severity and house prices. All columns consider CC-level yearly severity standardized in the full sample and the log of house prices. The coefficients can therefore be interpreted as percentage changes in house values for a one standard deviation change in court severity. Throughout, standard errors are clustered at the boundary level, and all specifications add year and month effects.

In the first and second columns, we present OLS regressions results in the full and in the boundary sample – mostly for comparison. These show that severity is strongly and negatively correlated with house prices. However, OLS are likely biased by unobserved housing market and socio-economic conditions driving housing costs and judges' decision making. So, in Columns (3) to (5), we exploit the BDD detailed in Section 3.2 to identify the causal effect of severity on house prices. Although the impact is notably reduced, we still find significant and negative associations between house prices and court severity even with additional controls and various levels of fixed effects. Our most stringent specification – which controls for group-by-year fixed effects (Column 6) – still identifies a clearly negative and very significant effect: a one standard deviation (in the national distribution) increase in judges' severity decreases house prices by 3.3%. As a further robustness check, we tried a specification including boundary-by-year effects (alongside group fixed-effects) which returned again a negative 3.3% estimate, significant at better than the 1% level (not tabulated).

In Appendix Table A2, we present a number of robustness checks on these results. To begin with, in Columns (1) to (4), we keep the same 'treatment' (CC-level yearly severity) but use: *i*-a 'doughnut approach' to exclude properties within a given distance of a boundary; and *ii*-different distance thresholds to define the boundary sample. Compared to the equivalent estimates in Column (5) of Table 4, our results are virtually unaffected. Next, in Columns (5) to (7) we use different measures of severity – including the LAD-based proxy discussed above (to by-pass endogeneity concerns in relation to observed court severity). This does not affect our findings. In particular, when using the LAD-based proxy, we find a -0.027% effect of severity on house prices – comparable to the finding we obtained using our most stringent specification in Column (6) of Table 4.[39] Last, we test the impact of severity after 2009 to account for changes that occurred during the financial crisis (and to align our estimates to the period where we can measure the Lloyds Bank 'shocks'). This exercise confirms our evidence so far: the impact of court severity on house prices is negative and approximately 3%.

Next, we turn to the credit market. The data used to produce such evidence comes from Nationwide and covers purchases that were funded with loans originated by the company. Our results are presented in Table 5. Columns (1) and (2) focus on prices; Columns (3) and (4) look at loan values; and finally, Columns (5) and (6) focus on loan-to-value ratios. Once again, we report OLS estimates in Columns (1), (3) and (5) for comparison, while the other columns present results from regressions that exploit our BDD design to identify the causal effect of judges' severity.

Columns (1) and (2) show that even within the set of properties tracked by the Nationwide data we find a negative and significant association between severity and house prices. The negative 2.5% effect found in Column (2) is statistically the same as the negative 3.3% impact estimated using the full sample (see Column 6 of Table 4). As predicted by the theoretical model, Columns (3) and (4) reveal that the impact of court strictness on loan sizes is virtually the same as the impact of severity on prices. A one-standard deviation change in the CC-level yearly severity index reduces mortgage advances by 2.2% – very close to the comparable estimate of 2.5% in Column (2). This pattern confirms our intuition that prices move down as severity increases as a reflection of reduced demand for credit. To conclude, Columns (5) and (6) study the impact of judges' severity on LTV ratios. Unsurprisingly – given the evidence – we find that there is no effect on LTV ratios as mortgage and house prices decrease by similar amounts as court severity increases.

Before moving on, we validate our BDD approach by providing balancing evidence. This is presented in Appendix Table A3. The top panel focuses on housing characteristics obtained from the Land Registry (which are used as controls in most of our BDD specifications). The results show that house attributes do not vary significantly across the boundary as a function of judges' severity. Panels B and C further the analysis by considering socio-economic characteristics extracted from the 2001 and 2011 Census data. Census data is aggregated at the OA level and is time fixed at decadal intervals (in 2001 and 2011). In order to perform our balancing test, we first merge Census data at the OA level to our transaction data using a mapping between postcodes and OAs. We then collapse the data to the OA level – averaging variation across postcodes within OAs (and across years) in a way that mirrors variation in the original 'geography' of the data on which we apply our BDD design. We find that only household age and education levels are slightly unbalanced – while all other variables move continuously across boundaries. Given the different level of aggregation used for the Census balancing analysis, in the Panel D we document the impact of judges' severity on house prices using this collapsed

[39] The specification that considers our LAD-based severity proxy adds LAD fixed effects. Note further that all specifications that consider averaged severity indexes include group (not group-by-year) fixed effects.

dataset. We still find a negative and significant impact of severity on house prices – similar in magnitude to the estimates presented in Table 4.

4.2. Judges' behaviour and characteristics of loans

Our theoretical framework argues that judges' severity is unlikely to be set at a (local) market clearing level that equates demand and supply of credit. The evidence discussed in Section 3 suggests that this is the case: judges tend to apply the same level of severity to all cases across the wards within their jurisdiction. Furthermore, judges' severity does not change in relation to the cases' proximity to the boundaries that divide groups.

In Table 6, we provide further evidence on this issue. To do so, we test whether judges react to a shock to the characteristics of the loans they assess when deciding on repossession cases – or instead are intrinsically 'stricter' or 'softer' irrespective of variation in the specific features of mortgage default cases they judge upon. As discussed in Section 3.3, we implement this test by exploiting the fact that Lloyds – the biggest UK mortgage lender – was forced by the UK regulator (Financial Conduct Authority) to significantly revise its procedures for dealing with delinquent customers in 2015. This generated a 'shock' to the type of cases submitted to courts before and after 2015, which is a function of Lloyds' local lending penetration (which we can observe in 2015 at the postcode sector level).

In Column (1) and (2), we present estimates of Equation (9) where year dummies (used for the event-study analysis) are swapped for an indicator identifying years after 2014 (as in a standard DiD setting). As clear from Columns (1), when the bank changed its policy, the number of repossession claims submitted to courts in areas it dominated decreased significantly. However, this change had no effect on the severity index of local judges. This suggests that judges do not significantly react to the type of cases they are presented with – and remain either 'strict' or 'soft'. In Columns (3) and (4), we provide additional evidence by using a discrete version of the Lloyds penetration 'treatment' – instead of the continuous indicator used in the previous two columns. Specifically, we identify areas as 'high' or 'low' Lloyds' penetration if the bank's incidence in the share of local lending is above or below the median of sample distribution of Lloyds' lending share. The two columns confirm our finding. When the bank brought its policies in line with those of other lenders, 14.6% fewer cases were submitted to courts in areas where Lloyds penetration was high. However, such adjustment did not correspond to any change in judges' severity.

In order to check that other local shocks and unobservables do not affect our findings, in the last two columns of the table we provide a falsification test using the impact of the policy change on social tenant claims and repossession orders. This check shows that the changes in the bank policy had no effect on those outcomes. This suggests that other unobserved factors are not driving our findings.

Finally, to ensure that there was no delayed response in the severity index to the type of cases submitted or differential pre-trends in areas with high/low penetration of Lloyds Bank's lending, we present a graphical event-study analysis in Figure 5. The top two panels focus on private cases submitted to courts and repossession decisions. The plots clearly show a step change in the number of cases between 2014 and 2015. However, there is no corresponding change in severity or a trend that could be induced by it. The bottom two graphs instead focus on social tenant cases as a 'falsification test'. Once again, we do not find any reaction in these outcomes to the Lloyd's fine.

Overall, it is clear that between 2014 and 2015 the number of mortgage repossession cases submitted to courts decreased in places where Lloyds had a bigger share of the market. However, the severity ratio of judges who preside over those cases was not affected. All in all, this evidence strongly suggests that judges' severity is not significantly affected by the cases they assess – and is instead a more persistent reflection of their innate tendency to be 'strict' or 'soft'.[40]

4.3. A 'pure demand' effect of courts' strictness

In this section, we focus on providing evidence on the impact of court severity on housing and credit demand. The approach we follow is informed by the model we developed in Section 2 and focuses on areas where the credit shock triggered by the Lloyds fine did not have a negative impact on house prices. This identifies boundaries where housing (and credit) demand is not constrained by credit supply – which we labelled as 'demand dominated'.

We start our analysis of a 'pure demand' response to courts' strictness by presenting descriptive evidence showing boundary-specific estimates of the price effect of judges' severity. These are presented in Appendix Figure A3. The left-hand side plot depicts estimates that come from a specification that controls for group fixed effects, while the central plot presents estimates from a model that controls for group-by-year fixed effects (see Equation 10 in Section 3.4 for more details). The two figures show that in the majority of boundaries the price effect is negative. This suggests that demand side considerations are likely to dominate. Indeed, for both sets of estimates, the 75th percentiles of the distribution is still clearly negative (more information is reported in the notes to the figure). Importantly, the two sets of estimates are strongly and positively correlated as presented in the right-hand side diagram – with a raw correlation of nearly 0.75. This implies that a consistent set of 'demand constrained' boundaries is likely to emerge from our analysis – irrespective of the exact specification we adopt.

This evidence is reinforced in Appendix Figure A4 where we present the association between boundary-specific estimates of the price effect of severity and boundary-specific estimates of the impact of the Lloyds shock on prices (stemming from Equation 11). This diagram shows that there are a number of areas where the Lloyds' shock had a negative effect on prices. As discussed, such negative effect can only occur in locations where housing demand is limited by credit supply (or becomes limited due to

[40] We further investigated whether similar patterns can be found for judges that we define as 'strict' or 'soft' prior to 2010, when the Lloyds events that lead to the fine of 2015 started. Our evidence suggests no closing or widening of the gap between strict and soft judges before/after 2015 and in places with high Lloyds penetration. Stated differently, irrespective of the cases they saw between 2010 and 2014 and subsequently between 2015 and 2018, strict judges and soft judges did not see a convergence in their repossession decisions.

the shock). Conversely, areas where the impact of the shock is (or becomes) positive are mostly dominated by demand. In detail, we find that the mean and median of the Lloyds shock effect distribution are 0.164 and 0.114, respectively (with a standard deviation of 0.336). Furthermore, approximately 35% percent of the areas display negative effects, with the remaining 65% showing positive effects. Importantly, the scatter plot also reveals that areas with a positive price effect of the Lloyds shock are also the areas where the price impact of severity was more negative. This supports our intuition that these locations are likely to be 'demand driven'. We therefore study the impact of severity on prices separately in areas with positive and negative Lloyds-shock effects to pin down the 'pure' demand effect of judges' strictness.

We present these results in Table 7, where we tabulate results from estimating Equation (12). Column (1) presents evidence coming from a specification of the 'first step' used to estimate the boundary-specific Lloyds' credit shocks (Equation 11) that does not control for judges' severity. We then define 'demand driven' areas as boundaries where the credit shock had a positive and significant impact on prices. We find that in those places our estimate of the price effect of court strictness is sizeable, significant and negative at approximately -5%. In areas where Lloyds had non-positive effects instead, there is no relation between prices and judges' severity. The difference between these two coefficients is statistically significant. Next, Column (2) presents similar findings using estimates of the Lloyds' shock that condition on the impact of severity on prices (i.e., adding judges' strictness to Equation 11).[41] Once again, we find that our estimate of the 'true demand' effect to court strictness is negative and sizeable – at approximately -4.7% (and significantly different from the negative 0.6% estimated for boundaries where supply dominates).

We tested the robustness of our results along a number of dimensions. First, we defined 'demand driven' areas as boundaries where the price effect of the Lloyds' shock was positive, significant and above the median of distribution of the boundary-specific price estimates of the credit contraction. Results are reported in Column (3) and yield virtually identical 'pure demand' estimates.[42] Second, we used group-by-year fixed effects in the estimation of Equation (11) as well as in the specification of Equation (12). Results are presented in Column (4). While this makes the differences between 'demand driven' areas and other boundaries less stark, the negative price effect of severity is still much more sizeable and significant in areas where the Lloyds' price effect was positive and above the median. This lends further support to our analysis. Finally, we considered 'discrete' Lloyds shock in Equation (11), where we replaced the incidence of Lloyds at the local level ($Lloyds_i^{2015}$) with dummies that identify locations where Lloyds' penetration is above 33% of the local market mortgage share (the median in the mortgage penetration distribution). This did not significantly affect our findings.

[41] The price effect of judges' strictness conditional on the boundary-specific Lloyds' shocks and controls is -0.015 significant at better than the 5% level.
[42] Once again, the difference in the estimates between 'demand constrained' areas and other boundaries is statistically significant.

The last two columns of the table investigate the impact of price severity in demand-dominated areas on loan values (Column 5) and LTV ratios (Column 6). The specification we use is identical to the one used in Column (4). Consistently with our model, we find that the impact of severity on mortgage quantities mirrors the effect we detect on house prices – and as a result we find no association between court strictness and LTV ratios in neither demand nor supply dominated areas.

5. Policy application: counterfactual exercise

Our analysis suggests a simple policy recommendation: since we find that, on average, courts are too strict, judges should become 'softer'. To provide a quantitative assessment of the impact of such policy on housing and credit markets, we perform a simple counterfactual exercise based on our results.

In this application, we make conservative assumptions and try to obtain a lower bound effect.[43] First, we need to decide on the magnitude of the recommended changes in strictness. It is clear that strictness needs to be lower but reducing it too much would lead to an undersupply of credit. While we do not know where the equilibrium point is, we want to reduce severity by a margin that will plausibly move severity towards it (or at it) – but not cross it. To this aim, we concentrate on local changes across boundaries and assign the severity of the 'softer' side to the 'stricter' side so that both sides have the same level of severity. Using this approach allows us to apply the boundary-specific estimate of the treatment effect obtained above and so get a credible local impact of a change in severity on housing and credit markets. To further ensure that that we do not set the severity level below the equilibrium, we only use boundaries in which we find that the impact of a credit supply reduction has no negative impact on prices (i.e., the demand-constrained areas identified above).

We define and apply our 'treatment' as follows. To begin with, the treatment intensity is measured as the change in severity implied by moving severity of 'strict' areas down to the severity of the 'softer' side multiplied by the corresponding boundary-specific estimates of the impact of severity on prices obtained above. Then, we only apply this treatment to locations on the 'stricter' side of such boundary – as locations on the 'soft' side would experience a zero reduction in severity.

Around half of the housing stock is treated. In 2018, there were 404,741 transactions in treated areas (51% of all transactions) with an average price of £191,030. Applying the treatment would increase the average transaction price in the treated part of the sample by 5.66% to £201,842. In England and Wales, the average transaction price would increase by 2.65%. Naturally, there would be a corresponding impact on the size of the average mortgage used for each of those transactions as LTVs do not change (see our evidence above), so the average mortgage size would also increase. Importantly,

[43] One exception is the assumption that interest rates do not react to changes in severity. While this is true at the local level for our boundary-specific estimates, national interest rates may react to large-scale changes in severity.

higher transaction values would increase the revenue from transaction taxes. Under the 2015 tax regime the increase would be around £354m per annum[44].

In addition, there would also be a large impact of the policy on the housing stock that is not transacted but still receives the treatment that affects house values. The increase in house values would apply to each property in the treated housing stock and would add a total of £171bn of housing wealth (total capitalisation of the housing market) to the economy[45]. Indeed, the main impact of this policy on the mortgage market would not be through mortgages of houses that change owners – but through homeowners who extract housing wealth by increasing their borrowing (via refinancing). As shown by Cloyne et al. (2019), the elasticity of mortgage demand to house prices is 0.2-0.3, so we assume that mortgage stock would increase by 0.25% for every 1% increase in house prices. Note that in our exercise the supply of credit is irrelevant as we only apply our treatment to demand-constrained locations where credit supply plays no role. Applying our treatment to the stock of mortgage loans in each treated area[46] suggests that mortgage stock would increase by a total of £6.2bn (0.7% increase in total UK mortgage stock). This estimate is especially relevant for two reasons. First, it is a considerable expansion of mortgage lending which has implications for both lenders and households. Second, this wealth can be used for non-housing consumption.

While the policy would have some clear advantages for homeowners, it would likely have some negative welfare effects on renters. First and foremost, it would increase current price-to-income ratios, making it harder to get a mortgage – thereby making housing less affordable overall. It would also encourage taking more debt – at a time when households have more debt than ever. On balance, although this policy would reduce an important market friction, it is unclear whether it would have positive total welfare effects. This is especially so in the UK context where housing affordability is an important issue likely caused by frictions in housing supply (Hilber & Vermeulen, 2016).

An alternative policy would be to ensure that prices can accurately reflect severity which could allow the market to clear. Importantly, allowing mortgage rates to adjust to severity would not nullify the impact that exogenous court severity has on housing and mortgage markets – instead, this effect would be transmitted through interest rates (Severino & Brown 2020). However, even with severity

[44] Note that stamp duty tax thresholds that created bunching were abandoned in 2014 and new rules introduced in 2015. However, there were also later reforms that changed the rules for first-time buyers and second homes. Those are neglected here and the estimate we report is obtained simply by increasing the price of each treated transaction in 2018 by the treatment effect taken from its closest boundary and applying the 2015 tax rules to the new price.
[45] We estimate this number by increasing the estimated value of each house in the housing stock in the treated LSOAs by the treatment effect defined by its closest boundary. LSOA is Lower Layer Super Output Area – a census geography of around 1500 people. It is the smallest geographical area for which we have housing stock estimates. Data on housing stock by LSOA come from the Consumer Data Research Centre and has been provided by the Valuation Office Agency. House values in LSOAs are estimated based on Land Registry transactions in 2018 or projections of values of earlier transactions into that year.
[46] Data on the stock of mortgage lending per postcode sector has been provided UK Finance. It is reported by the seven biggest lenders and covers around 75% of the total lending market. Where different parts of a postcode sectors are assigned to different boundaries, the mortgage stock data is assigned to each boundary based on the share of transactions assigned to the boundary.

reflected in the cost of borrowing, there would still be a level of severity that maximizes lending and house prices. If severity is exogenous and creates a friction – as in our settings – we would still not be able rely on the market to arrive at this point without a policy intervention.

6. Conclusions

This paper examines the impact of repossession 'insurance' – i.e., the likelihood that a house cannot be repossessed despite the mortgage being in arrears due to a court decision – on demand in housing and mortgage markets. It begins with a theoretical analysis of the impact that such insurance has on supply and demand. This framework offers two important insights. First, there is an optimal level of borrower protection that maximises lending stock and house prices. Second, moving away from the optimal level (in either direction) lowers lending stock and house prices. This results in a simple prediction: changes in borrower protection should affect lending levels and house prices – positively if there is a change towards the optimal level and negatively otherwise.

We then provide estimates of the impact this insurance channel has on the demand in credit and housing markets in England and Wales. We find that a one standard-deviation change in the probability that a house is repossessed – conditional on being delinquent and taken to court – decreases demand in credit and housing markets by 3.5%-4.5%.[47] These parameters are important inputs for quantitative models that compare the welfare costs and benefits of consumer protection or try to estimate general-equilibrium outcomes (as also proposed by Dobbie et al., 2015). Indeed, they suggest that demand effects are an important outcome of borrower protection policies. Our paper offers new empirical evidence in support of the demand response advocated by Ganong and Noel (2020) and suggests that Davila's (2020) theoretical claims that the demand side can be ignored may not be realistic.

We also raise an important novel point about the need of borrower protection to react to changes in the market. Indeed, we argue that if the level of protection is exogenous to market conditions, it will create a friction. Empirically, we provide evidence that in England and Wales preferences of judges are exogenous to market conditions – and show the effect this has on very important parts of the economy: housing and credit markets. This finding has important implications for the economy as highlighted by our own results and similar literature from the US.

Overall, we find that in England and Wales average house prices would be higher and mortgage loans bigger if judges were marginally more likely to rule in favour of the borrower. Interestingly, while 'softer' judges would make mortgage credit more accessible, such change would translate into a decrease in housing affordability as measured by price to income ratios. Such effect would be similar to the impact on credit and housing markets of one of the current UK flagship housing finance policies – namely, the Help-To-Buy scheme. While this policy makes mortgages more affordable and encourages borrowing (Szumilo and Vanino, 2018), it also increases house prices (Carozzi et al., 2020).

[47] The impact on the housing demand is a lower bound as we assume fixed housing supply.

Importantly, while prices would rise in response to reducing severity at the margin, loans could become smaller and prices would decrease if judges become too reluctant to issue repossession orders (we do not estimate the point at which this reversion would occur). This effect would occur because banks reduce credit supply when it becomes more difficult to repossess a delinquent loan. This conclusion links our paper to research on the US mortgage market where loan sizes are larger when courts favour the lender.

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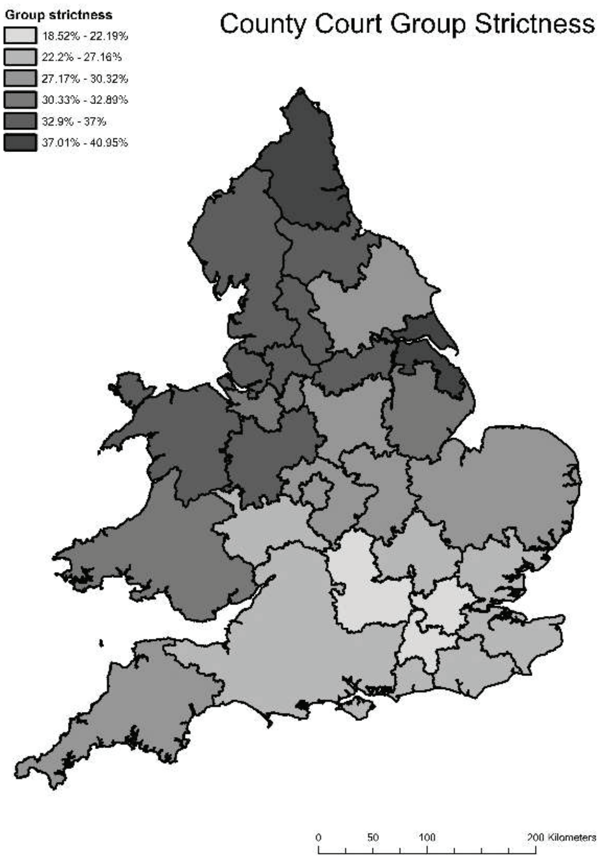
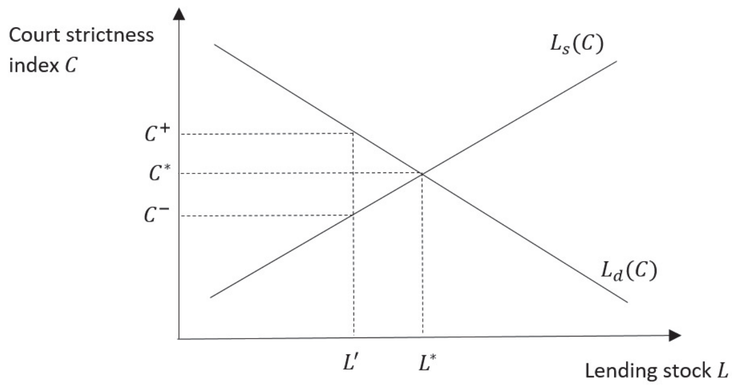
Tables and figures

Figure 1. Boundaries of county court groups and their strictness



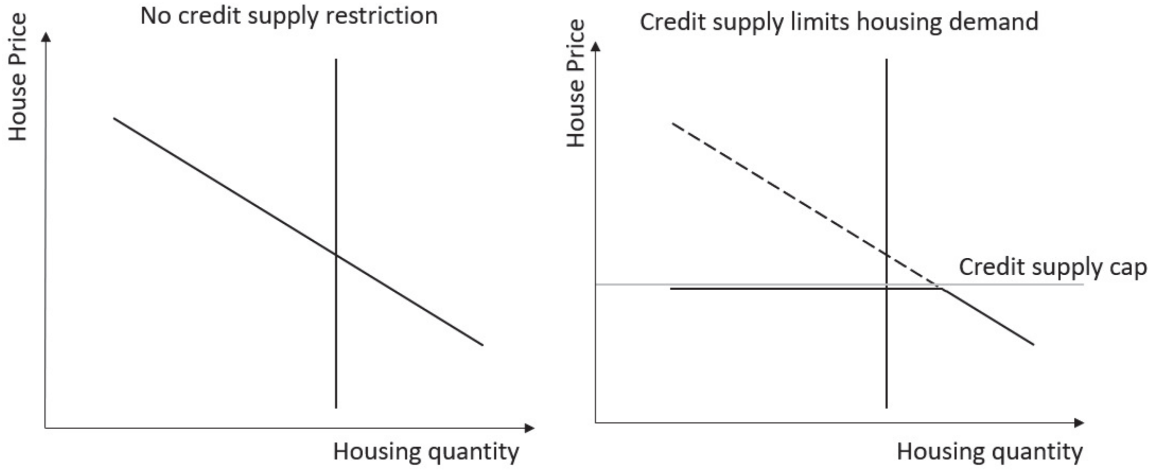
Notes: Data comes from the Ministry of Justice. Strictness ratios defined as the number of mortgage repossession orders divided by the number of claims at the group level. Groups are based on group-level reports by the Ministry of Justice from 2014.

Figure 2. Supply and demand as functions of court severity



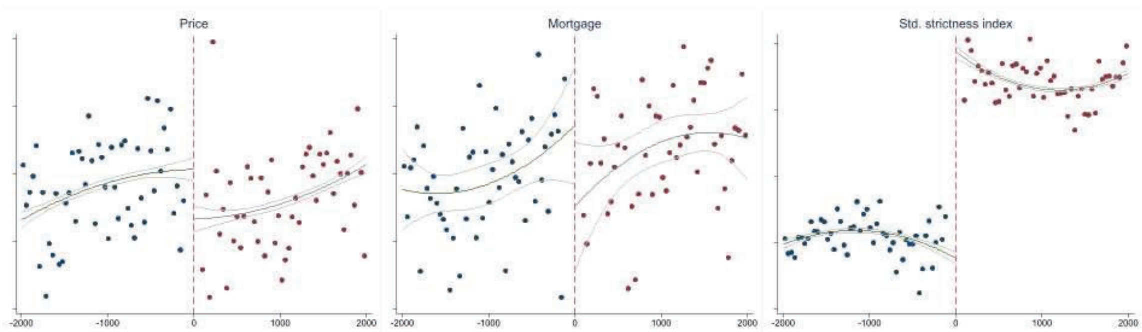
Notes: Court strictness C is the probability that a judge will rule in favour of the lender (conditional on cases being the same), $L_s(C)$ denotes the supply of credit as a function of court strictness (all else equal), $L_d(C)$ denotes the demand for credit as a function of court strictness (all else equal), L denotes lending stock.

Figure 3. The interplay between credit demand, house prices and credit supply restrictions



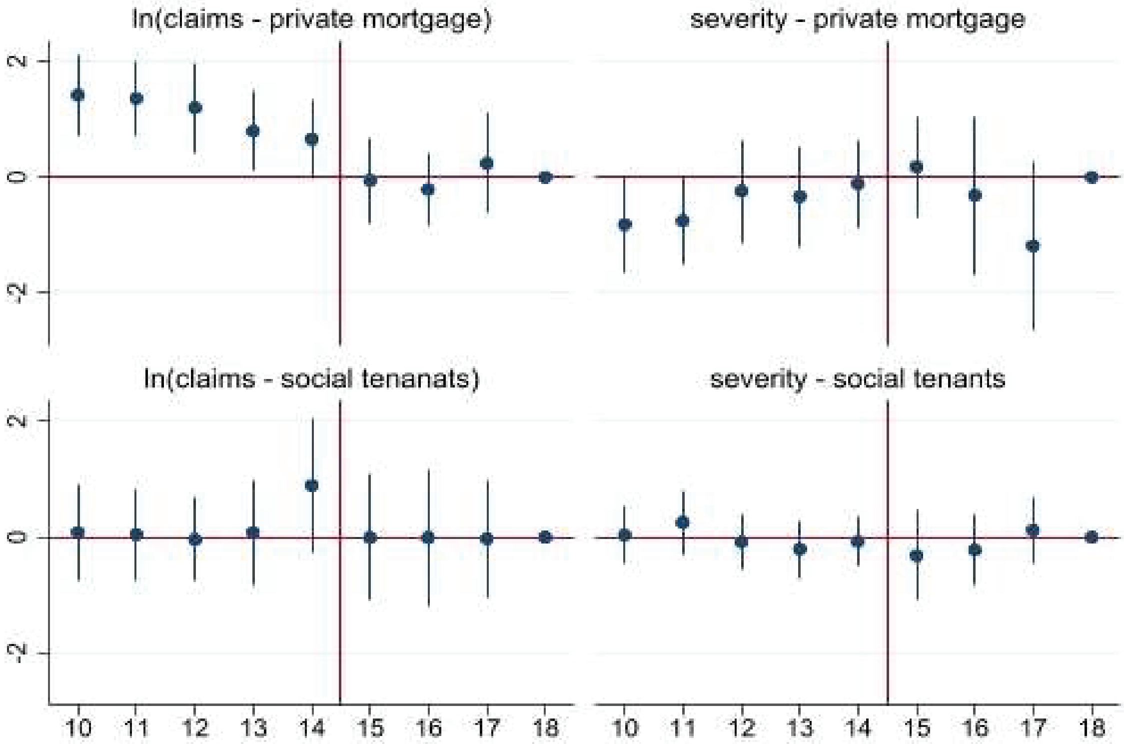
Notes: the figure illustrates how demand interacts with supply in the housing market to set house prices in two scenarios: with housing demand unrestricted by credit supply (left) and with housing demand restricted by credit supply (right). Downward sloping lines represent housing demand derived from a utility function. Vertical lines represent housing supply (assumed constant). The horizontal grey line in the right-hand side plot represents the limit on housing demand imposed by credit supply.

Figure 4. Boundary discontinuity graphs.



Note: The figures plot variables given in the title of each plot against the distance to the closest group boundary. Negative distance (left hand side) represents the 'softer' side of the boundary while positive distance values represent the 'stricter' side (right hand side). Each dot represents one of 50 bins of 40m on each side of the boundary (based on distance to the boundary). All results are adjusted for distance to the boundary and its polynomials to the third order, year and month effects, so the dots represent the mean residual from the regression of the variable on those controls. Continuous lines represent quadratic fit and 95% confidence intervals. Price, mortgage, and strictness figures use the same sample as the main results. The sample used to produce price and strictness figures contains 1,144,886 observations while the sample for mortgages contains 110,615 observations.

Figure 5. Event study of Lloyds fine 'shock'



Note: The figures present regression coefficient (and 95% confidence intervals) of the variables in the headings on dummies obtained by interacting year identifiers with a continuous indicator for Lloyds banking penetration (and controlling for ward fixed effects). The omitted group is year 2018. See Equation (9) for details of the specification used. More information on the sample and variable definitions can be found in Table 3 and in the main text.

Table 1. Expected states and corresponding probabilities for a borrower

State	Income	Budget constraint	Probability	$U(x,h)$
Baseline	I_n	$I_n - r(ph - A) = x$	$1-q$	$\frac{x^{1-\sigma}}{1-\sigma} + \frac{h^{1-\sigma}}{1-\sigma}$
Delinquency 1 No repossession	I_u	$I_u = x_1$	$q(1-d)$	$\frac{x_1^{1-\sigma}}{1-\sigma} + \frac{h^{1-\sigma}}{1-\sigma}$
Delinquency 2 With repossession	I_u	$I_u = x_1$	qd	$\frac{x_1^{1-\sigma}}{1-\sigma}$

Notes: the table illustrates the different possible outcomes (states) faced by a borrower entering the mortgage market, their corresponding probabilities, budget constraints and utilities defined in terms of the transformed utility function. I_n is income in the baseline case, I_u is income when the income is lower, r is the interest on the loan, p is house price, h is housing consumption, x is non housing consumption, q is the likelihood of a delinquency, d is the likelihood of a repossession of a delinquent property.

Table 2. Expected states and corresponding probabilities for a lender

State	Probability	Profit	E(Profit)
Baseline	$1-q$	$r - r_a$	$(1-q)(r-r_a)$
Delinquency 1 No repossession	$q(1-d)$	$-r_a$	$-q(1-d)r_a$
Delinquency 2 With repossession	qd	$r - r_a$	$qd(r-r_a)$

Notes: the table illustrates the different possible profit outcomes (states) faced by a lender which correspond to the states faced by the borrower (detailed in Table 1). r is the interest on the loan, r_a is the cost of providing the loan, q is the likelihood of a delinquency, d is the likelihood of a repossession of a delinquent property.

Table 3. Key descriptive statistics – Full sample and boundary sample

	Full Sample		Boundary sample	
	Mean	Std. Dev.	Mean	Std. Dev.
House price (Land Registry)	199883	144861	208308	144934
LTV (Nationwide)	70.3%	21.8%	69.9%	220.4%
Loan size (Nationwide)	152141	81558	158168	83403
Judges' severity index (CC level, yearly)	0.198	0.107	0.196	0.105
Judges' severity index (CC level, averaged)	0.198	0.033	0.196	0.032
Judges' severity index (Group level, averaged)	0.197	0.044	0.195	0.045
Judges' severity index (LAD based, averaged)	0.222	0.029	0.220	0.030
Distance to group boundary (metres)	15555	19061	4675	5531
Boundary specific 25th percentile of distance	7683	6612	--	--
Property is: detached (%)	21.91	--	25.80	--
Property is: flat (%)	17.06	--	12.97	--
Property is: semi-detached (%)	29.02	--	31.25	--
Property is: terraced (%)	32.01	--	29.98	--
Property is: leasehold (%)	22.29	--	18.38	--

Note: CC=County court. LAD=Local authority district. Data on house prices and house characteristics come from the Land Registry. Data on judges' severity was obtained from publicly available information posted on the website of the Ministry of Justice (MoJ) and from data obtained through Freedom of Information (FoI) requests. Geographical manipulations and distance calculations were carried out using ArcGIS. Samples include observations between 2001 and 2018 and second-hand transactions only (newly built are excluded). Data have been winsorized and transactions in the top and bottom 1% of the yearly price distribution have been dropped. Boundary refers to the boundaries separating CC groups (not the boundaries of CC catchment areas). Number of observations in the full sample: 15,292,907 in 30 groups with 138 county and 71 boundaries. Boundary sample only includes transactions within the 25th percentile of the boundary specific distance distribution. Number of observations in the boundary sample: 3,824,307 in 30 groups with 117 county and 71 boundaries. Data on loan size and LTV comes from Nationwide and is available between 2004 and 2017. Number of observations in the lending sample 885,118 and 237,832 in the full and boundary samples, respectively. Judges' severity is defined as number of repossessions divided by number of cases seen by judges. Averaged figures refer to the average across all years between 2001 and 2018. Group level severity is defined by counting all repossessions and all cases across CCs belonging to the same group. LAD level severity index defined as follows. We measure the LAD severity ratio by dividing all social tenant repossession orders issued for properties within its boundaries by the number of claims submitted. To obtain a court-specific measure of severity based on social claims, we average the LAD severity ratio for all LADs within the catchment area of this court and weigh each LAD's index by the share of the catchment area of the court it covers. This yields an index of severity based on types of cases that are submitted to the court on criteria unaffected by the severity of its judges.

Table 4. The impact of judges' severity on house prices

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample	Boundary Sample	Boundary Sample	Boundary Sample	Boundary Sample	Boundary Sample
	OLS	OLS	BDD	BDD	BDD	BDD
Judges' severity index (standardized)	-0.266 (0.031)***	-0.257 (0.031)***	-0.067 (0.009)***	-0.068 (0.009)***	-0.062 (0.008)***	-0.033 (0.008)***
Year and month	Yes	Yes	Yes	Yes	Yes	Yes
Distance controls	No	No	No	Yes	Yes	Yes
Housing characteristics	No	No	No	No	Yes	Yes
Group FEs	No	No	No	No	Yes	No
Group-by-year FEs	No	No	No	No	No	Yes

Note: The table reports coefficients and standard errors in parenthesis (clustered at the boundary level) of a regression of log of house prices on an index of judges' severity (CC-level, yearly) standardized in the full sample and controls as detailed in the notes. Samples and number of observations detailed in Table 3. BDD=boudnary discontinuity design. All BDD regressions control for boundary fixed effects. Distance controls include third-order polynomials in distance from the boundary (measured in metres). ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table 5. The impact of judges' severity on mortgage markets.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log of house price		*Log of mortgage advances*		*Loan to value ratios*	
	Full Sample	Boundary Sample	Boundary Sample	Full Sample	Boundary Sample	Boundary Sample
	OLS	BDD	BDD	OLS	BDD	BDD
Judges' severity index (standardized) yearly	-0.147*** (0.024)	-0.025*** (0.007)	-0.130*** (0.025)	-0.023*** (0.007)	0.009*** (0.002)	0.002 (0.002)
Year and month	Yes	Yes	Yes	Yes	Yes	Yes
Distance controls	No	Yes	Yes	No	Yes	Yes
Housing characteristics	No	Yes	Yes	No	Yes	Yes
Group-by-year FEs	No	Yes	Yes	No	Yes	Yes

Note: The table reports coefficients and standard errors in parenthesis (clustered at the boundary level) of a regression of log of house prices (Columns 1 and 2); log of mortgage advances (Columns 3 and 4); or loan to value ratios (Columns 5 and 6) on an index of judges' severity (CC-level, yearly) standardized in the full sample and controls as detailed in the notes. Samples sizes as follows. 553,754 (full sample) and 149,369 (boundary sample) along 69 county court group boundaries. BDD=boudnary discontinuity design. All BDD regressions control for boundary fixed effects. Distance controls include third-order polynomials in distance from the boundary (measured in metres). ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table 6. The Lloyds fine 'shock' and judges' behaviour

	(1)	(2)	(3)	(4)	(5)	(6)
	Private mortgage delinquencies				Social tenants (falsification)	
	Continous treatment		*Discrete Treatment*		*Continous treatment*	
	Log Cases Submitted	Severity index	Log Cases Submitted	Severity index	Log Cases Submitted	Severity index
Treatment Effect	-1.087 (0.276)***	0.118 (0.202)	-0.146 (0.059)**	-0.032 (0.038)	-0.239 (0.150)	-0.087 (0.166)
Year and ward effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports coefficients and standard errors in parenthesis (clustered at the boundary level) of a regression of the variable in the heading on a Lloyds fine 'treatment' indicator controlling for ward and time effects. Regressions run at the ward level and only considering wards that contain postcodes within the 25th percentile of the boundary-specific distance distribution. Wards spanning more than one group, more than one boundary or containing more than one county courts have been dropped. Furthermore, only wards with positive number of cases submitted are considered so that severity can be meaningfully computed. Finally, only the years 2010 to 2018 are considered as Lloyds intensity cannot be consistently defined prior to this period due to a number of significant restructuring events that affected the banking group. Number of observations: 3,838 in Columns (1) to (4); 2,767 in Columns (5) and (6). Treatment in Columns (1), (2), (5) and (6) is defined as the interaction of a dummy identifying years from 2015 (included) with the penetration of Lloyds banking measured as Lloyds' share of all mortgage lending at the postcode sector level (mapped onto wards via postcodes). Lloyds penetration descriptive statistics as follows. Mean: 0.3389; median: 0.3347; standard deviation: 0.0811. Treatment in Columns (3) and (4) is defined as the interaction of a dummy identifying years from 2015 (included) with a dummy identifying areas where the penetration of Lloyds is above the median of the distribution. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table 7: House price effects of severity in demand constrained boundaries

	Positive, significant Lloyds shock effects		Lloyds shock effects Above median			
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable is:	Log of prices	Log of prices	Log of prices	Log of prices	Log of advances	LTV ratios
Severity –	-0.050	-0.047	-0.050	-0.039	-0.032	0.001
Constrained boundaries	(0.012)***	(0.011)***	(0.012)***	(0.015)***	(0.009)***	(0.002)
Severity –	-0.008	-0.006	-0.016	-0.018	-0.017	0.003
Non-constrained boundaries	(0.005)	(0.005)	(0.009)	(0.008)**	(0.007)**	(0.002)
Year and month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Housing characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Group FEs	Yes	Yes	Yes	No	No	No
Group-by-year FEs	No	No	No	Yes	Yes	Yes

Note: The table reports coefficients and standard errors in parenthesis (clustered at the boundary level) of a regression of log of house price on judges' severity index (CC-level, yearly) and controls as detailed in the table. Lloyds 'shock' refers to the lending supply contraction caused by the fine imposed on Lloyds in 2015 (see body text for more detail). The boundary-specific shock is measured by the triple interaction between: *i*- a dummy identifying years after 2014; *ii*- an indicator for Lloyd's penetration measures as the bank's share of all mortgage lending at the postcode sector level (mapped into our data via postcodes); and boundary indicators. The regression in Column (1) uses estimates of the Lloyds Bank shock that come from a 'first-step' specification (Equation 10 in the main body text) that does not control for judges' severity. Lloyds' shocks used in all other regressions come from specifications that further control for judges' severity in the first step. Demand constrained areas in Columns (1) and (2) are defined as boundaries where the Lloyds' supply shock had a positive and significant effect on prices. Demand constrained areas in Columns (3) and (4) are defined as boundaries where the Lloyds' supply shock was positive, significant and above the median of the distribution of the boundary-specific Lloyds' shock estimated effects. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Appendix

A. Numerical solution for a Cobb-Douglas utility function.

In order to demonstrate that our qualitative results – housing demand decreasing with severity – hold even with Cobb-Douglas consumption preferences, we solve this case numerically. First, we specify the expected utility with the following preferences:

$$U_1 = x^{\sigma} h^{1-\sigma}$$

Where σ is a parameter. This yields the expected utility faced by a household given by the following equation:

$$\mathrm{E}[\mathrm{U}_1(\mathrm{x}, \mathrm{h})] = [1-q][I_n - r(ph - A))^{\sigma} h^{1-\sigma}] + q[(1-d)({I_u}^{\sigma} h^{1-\sigma})]$$

The main objective of our numerical solution is to show that $\partial h/\partial d < 0$. To this end, we begin with creating a grid. The dimensions of the grid as well as their extreme values and intervals are defined in Appendix Table A4. The grid has 445.5m points and we calculate utility for each of them using the equation above. From these points we keep the combination of all parameters that offers the highest utility and drop all combinations that violate our basic assumptions (such as that households cannot be lenders). This yields 24.33m of utility-maximizing choices of housing consumption for each set of parameters. We drop all instances where more than one housing choice offers the maximum utility for the same set of parameters (293k cases). We also exclude from our analysis choices made at the minimum and maximum level of housing consumption as these include many choices that are restricted by the size of the grid and not by the budget constraint. This leaves us with 9.5m unique optimal housing choices for different sets of parameters.

The simplest way to demonstrate the qualitative solution is to show that the change in chosen housing consumption will decrease as severity increases. Indeed, an increase of one grid interval in the severity ratio (increase by 0.02) reduces housing consumption by an average of 0.00012, by ten grid intervals by 0.0012 and by fifteen intervals by 0.0018. In fact, increasing d results either in no change in housing consumption (due to grid granularity) or in its decrease. There are no instances in which increasing severity would result in higher housing consumption. This is consistent with our claim that housing demand decreases with severity.

B. Credit supply and the change in Lloyds policy.

Although we are limited by data availability on credit supply (total as well as by bank), we can provide some evidence consistent with the claim that Lloyds reduced their credit supply after 2015 and once they adopted a policy that provided repossession insurance to their vulnerable customers (by treating them more fairly when they are delinquent). Note that this credit supply reduction is consistent with evidence from the literature on how banks react to more borrower protection cited in the introduction.

First, we note that the share of Lloyds new mortgage lending at a national level decreased from 19.5% in 2014 to 15.6% in 2015 (CML Economics, 2014). This could be driven by the national competition becoming more intense as well as Lloyds supplying less credit. To show that this is likely driven by the latter, we focus on areas where Lloyds had a large share of the market in 2013. The rationale is that credit in these areas was driven mainly by Lloyds. Therefore if the overall credit supply in those areas falls relative to other areas in the UK, this is most likely due to a reduction in credit supply by Lloyds.

We are able to track the stock of mortgage lending at postcode sector level (by bank) for the seven biggest lenders accounting for over 73% of total lending in the UK using data collected from UK Finance and directly from the lenders. Postcode sectors are areas with around 3,000 residential and commercial addresses. To show that the total lending stock decreased in 2015 in areas dominated by Lloyds compared to other areas in the UK we use an event study regression of the following form:

$$LS_{it} = \alpha + \beta_1 Lloyds\ exposure_i \times Quarter_t + \beta_t + \beta_i + \epsilon_{it}$$

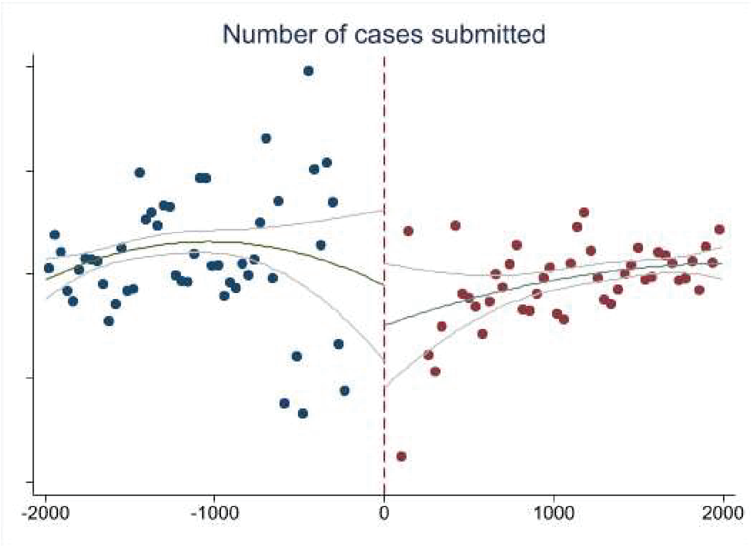
Where LS_{it} denotes combined lending stock in sector i in quarter t, $Lloyds\ exposure$ is a dummy variable that equals one if Lloyds has more than two thirds of combined lending in this sector in 2013[48] and β_1 denotes the impact of Lloyds exposure on combined lending over time.

Results are presented in Figure A2 and show a sharp decrease in the combined stock of credit in places where Lloyds dominated the mortgage market in 2013. Since Lloyds lost market share in the market for new lending (CML Economics, 2014) and mortgage credit stock in areas where it used to dominate the market decreased compared to other areas of the UK, it seems that the scant data we are able to access supports the premise that Lloyds reduced their credit supply when their customers became better protected.

[48] While this cut-off is an arbitrary measure of exposure to Lloyds, we have tested other specifications and they give similar results.

Appendix Figures and Tables

Figure A1. Discontinuity in cases submitted to courts

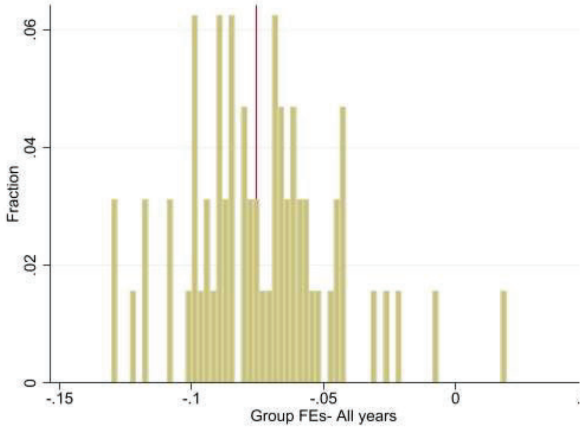


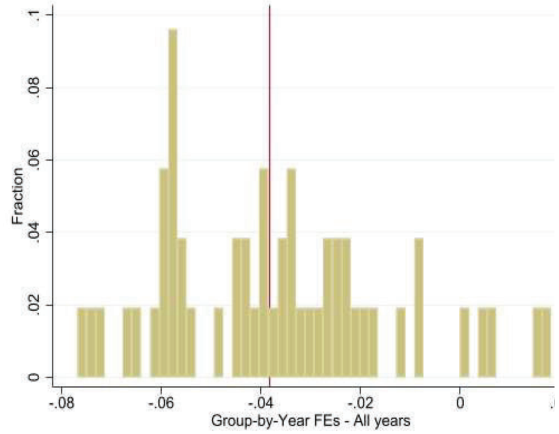
Note: The figures plot the number of cases submitted to courts against the distance to the closest group boundary. Negative distance (left-hand side) represents the 'softer' side of the boundary while positive distance values represent the 'stricter' side (right hand side). Each dot represents one of 50 bins of 40m on each side of the boundary (based on distance to the boundary). All results are adjusted for distance to the boundary and its polynomials to the third order, year and month effects, so the dots represent the mean residual from the regression of the dependent variable on those controls. Continuous lines represent quadratic fit and 95% confidence intervals. Data at the ward level. See Appendix Table A1 for more details.

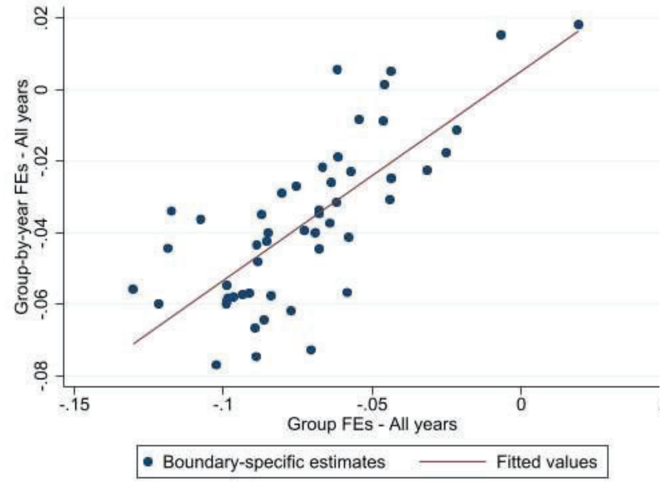
Figure A2. Credit stock of 7 biggest lenders in areas dominated by Lloyds.

Note: The figure plots coefficients of a regression of the natural logarithm of combined lending by seven biggest lenders on time dummies interacted with an indicator of being dominated by Lloyds in 2013 at postcode sector level. Q2 2016 is used as baseline as the regression includes both quarter and sector fixed effects. Sample size is 88,680. Being dominated by Lloyds is defined as at least two thirds of lending in a sector in 2013 coming from this lender.

Figure A3. Boundary-specific price-effect estimates – Distributions and correlation

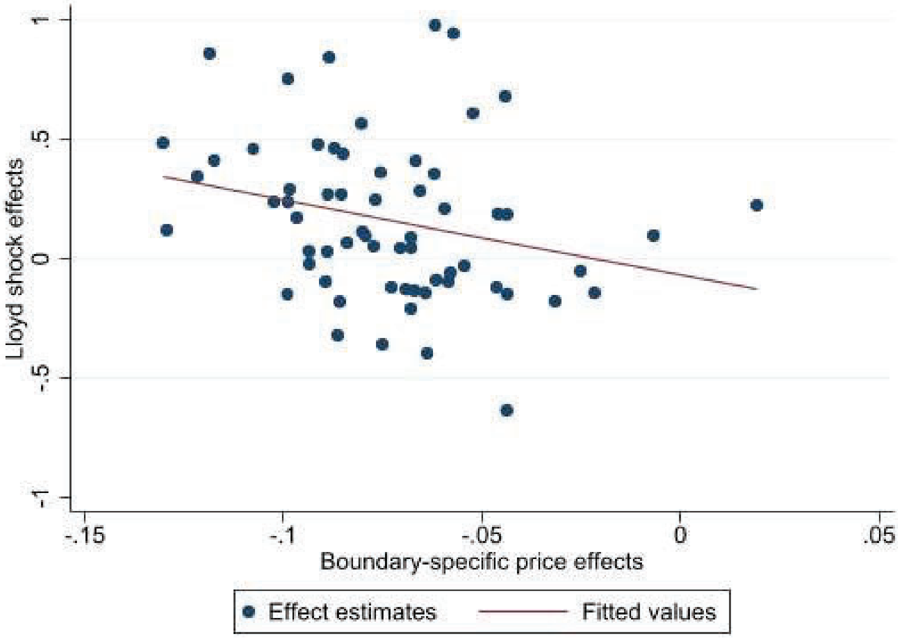






Note: The figures present distribution and scatter plot of boundary-specific estimates of the impact of judges' severity (standardized; CC-level, yearly) on log of house prices. Estimates obtained from regressions that interact severity with boundary indicators and control for: year and month effects; distance to the boundary (cubic polynomial); property characteristics; and group effects (left-hand side panel) or group-by-year effects (central panel). See Equation (10) for details of the specification used. The left and central panel display distributions of such estimates with mean value represented by the red vertical line. The right-hand side panel presents a scatter plot of the estimates in the left and central panel with linear fit line. The estimation sample only includes boundary with more than 500 observations. Other boundaries are dropped because of limited variation in the CC severity index conditional on all controls and fixed effects resulting in outlier estimates. Number of used boundary in the left-hand side plot: 64 (out of 71); number of used boundaries in the central plot: 52 (out of 71). Our main regression findings are virtually unchanged when focussing on this sub-sample of boundaries. A specification equivalent to the one of Column (6), Table 4 yields an effect of -0.034 (0.009) on the sample including 64 boundaries. More results are available from the authors. Boundary-specific estimates' distributions as follows. Estimates controlling for group effects (left panel): mean -0.074; median -0.075; standard deviation 0.028; skewness 0.558; 25th percentile -0.090; 75th percentile -0.059. Estimates controlling for group-by-year effects (central panel): mean -0.037; median -0.038; standard deviation 0.023; skewness 0.477; 25th percentile -0.057; 75th percentile -0.024. Raw correlation between the two sets of estimates: 0.7478. Equivalent diagrams were produced considering only years after 2009. These provided very similar findings and are not tabulated for space reasons (they are available from the authors upon request).

Figure A4. Scatter plot of boundary specific estimates of Lloyds' lending 'shock' and price effect of judges' severity



Note: The figure presents scatter plots of boundary-specific Lloyds' lending supply shock effects (coming from the specification detailed in Equation 11 in the body text and further used in Table 7, Column 2) against boundary-specific price effects of judges' severity (also displayed in Appendix Figure A3, left-hand side plot) More information on the definition of the Lloyds shock can be found in Table 7 and in the body text.

Table A1. Further descriptive evidence on judges' severity – ward level data

	Full sample			Boundary sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Regressions of ward severity on spatial lags						
Within CC, leave one out mean	0.626 (0.027)***		0.577 (0.022)***	0.554 (0.045)***		0.484 (0.039)***
Rest of group, excluding CC		0.414 (0.088)***	0.203 (0.043)***		0.423 (0.100)***	0.235 (0.061)***
Panel B: Regressions of ward severity on distance to the boundary						
Distance	-0.0020 (0.0003)***	-0.0012 (0.0011)	-0.0004 (0.0022)	-0.0007 (0.0019)	0.0068 (0.0047)	-0.0106 (0.0069)
Distance^2			0.0000 (0.0000)			0.0013 (0.0008)*
Distance^3			0.0000 (0.0000)			0.0000 (0.0000)
Specification	*Cross-sectional*	*Within CC*	*Within CC*	*Cross-sectional*	*Within CC*	*Within CC*

Note: The table considers ward-level data on repossessions and cases submitted to judges at CCs in our main data. Ward-level data contain a finer level of variation than our core data (measured either at the CC level or at the group level). All wards were attributed to one CC only, one group only and one boundary only. Wards that span more than one CC, group or boundary were dropped from the analysis. We also only consider wards with non-zero number of cases in order to calculate ward-level judges' severity measures (defined as repossessions over cases submitted). This leaves us with 60 of the original 71 boundaries and all 30 groups. Our core estimates of the price effect of judges' severity in this sample are virtually identical to those presented below (available upon request). The ward data was first matched to our transaction level data using a mapping between postcodes and wards. The data was subsequently collapsed at the ward-by-year level. This ensures that the ward level data weighs the postcode-level variation in transactions within postcodes belonging to the same ward (across years and in terms of distance to the boundaries) to maintain the same 'geography' as in the original data. Panel A reports results from regressions of ward level severity on 'leave-one out' mean severity in the CC (i.e., considering all other wards in the same CC except for the one under consideration) and/or 'rest of the group' severity (i.e., considering all other wards in the group excluding those in the same CC as the ward under consideration). Panel B reports results from regressions of ward level severity on distance to the boundary. Distance collapsed from transaction level data to ward-by-year observations to maintain the underlying 'geography' of transaction data at the postcode level. All regressions control for averaged (collapsed) year effects. Regressions in Panel B further control for averaged (collapsed) month effects and for CC dummies in Columns (2), (3), (5) and (6). Columns (1) to (3) consider all wards and postcodes in the data; number of observations: 35,255. Columns (4) to (6) only consider postcodes within the 25th percentile of the boundary-specific distance distribution before collapsing at the ward level; number of observations: 11,678. Distance coefficient multiplied by 1000; quadratic distance coefficient multiplied by 1000^2; cubic distance coefficient multiplied by 1000^3. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table A2. Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	200m Doughnut	1st Distance Percentile Doughnut	2km Boundary Corridors	10th Distance Percentile Corridors	CC Averaged Severity Index	Group Averaged Severity Index	LAD Averaged Severity Index	Boundary Sample – Post 2009
Judges’ severity index	-0.062	-0.062	-0.061	-0.055	-0.049	-0.061	-0.027	-0.031
(standardized)	(0.008)***	(0.008)***	(0.010)***	(0.008)***	(0.018)***	(0.017)***	(0.013)**	(0.006)***
Year and month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Housing char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group FEs	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes
LAD FEs	No	No	No	No	No	No	Yes	No

Note: The table reports coefficients and standard errors in parenthesis (clustered at the boundary level) of a regression of log of house prices on an index of judges’ severity standardized in the full sample and controls as detailed in the notes. All regressions apply the BDD design in the boundary sample. Distance controls include third-order polynomials in distance from the boundary (measured in metres). Columns (1) to (4) use the CC-level, yearly judges’ severity index. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table A3: Balancing evidence

	(1)	(2)
	Mean of Dependent Variable	Coefficient on Judges severity CC-level, yearly
Panel A: Housing characteristics		
Property is: detached (%)	0.258	-0.001 (0.004)
Property is: flat (%)	0.129	-0.004 (0.003)
Property is: semi-detached (%)	0.312	0.004 (0.003)
Property is: terraced (%)	0.299	0.002 (0.003)
Property is: leasehold (%)	0.184	-0.005 (0.003)
Panel B: Census 2001 characteristics		
Average household size	2.401	0.003 (0.006)
Average number of bedrooms	5.741	-0.014 (0.020)
Housing overcrowding index	0.046	-0.001 (0.001)
Population density	0.319	-0.006 (0.006)
Average age	40.266	0.196 (0.104)*
Employment rate	0.958	0.000 (0.001)
Unemployment rate	0.042	-0.000 (0.001)
Qualification at Level 4 or 5 (degree)	0.203	-0.007 (0.004)*
White ethnic background	0.951	0.003 (0.005)
Ownership with mortgage	0.414	0.004 (0.003)
Male population	0.487	0.000 (0.000)
Panel C: Census 2011 characteristics		
White ethnic background	0.914	0.005 (0.008)
Qualification at Level 4 or 5	0.233	-0.007 (0.004)*
Ownership with mortgage	0.363	0.003 (0.003)
Panel D: House prices		
Log of house prices	--	-0.046 (0.014)***

Note: The table reports mean of the relevant variable in Column (1) and regression coefficients of the variable on the standardized index of judges' severity (measured at the CC level and yearly) in Column (2). Standard errors are clustered at the boundary level. Each cell in Column (2) comes a different regression. All regressions consider data in the boundary sample and apply a BDD design (controlling for boundary fixed effects). Regressions considering housing characteristics are run at the property level and include year and month dummies, distance to the boundary controls (cubic polynomial) and group-by-year FEs. Regressions considering Census characteristics (Panels B and C) are run at the Output Area (OA) level. More specifically, the original property-level data was merged with time fixed and OA-level census variables using a mapping between property postcodes and Census OAs. The data was then collapsed at the OA level to ensure that the variation of the collapsed Census OA characteristics represents the variation in years and postcodes contained in the main property-level data. Regressions control for OA-averaged third-order polynomials in distance to the boundary, year and month effects; and for group dummies. Panel D presents the estimate of the impact of judges' severity on house prices in the collapsed sample. This is not balancing evidence, but a check that main results remain consistent in the OA-collapsed sample. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table A4. Parameters of the gird for a numerical solution with Cobb-Douglas preference.

Parameter	Definition	Low	High	Interval	Points
q	Prob. of delinquency	1%	5%	1%	5
d	Prob. of a successful repo. claim	20%	58%	2%	20
I_u	Low case income	£6,000	£12,000	£3,000	3
h	Housing consumption	20m	220m	310m	30
I_n	Expected income	£15,000	£150,000	15,000	10
r	Mortgage interest rate	5%	9%	1%	5
p	Unit price of housing	£1,000	£19,000	£1,000	10
A	Household savings	0	£200,000	£20,000	11
σ	Prob. of delinquency	0.1	0.9	0.1	9

Notes: Delinquency rate is based on data reported by UK Finance in 2020 here. Probability of a successful repossession claim is based on observed ratios reported in the main body of the paper. Low case income is based on the income support allowance from the English government through Universal Credit in 2020. Housing consumption is based on the size of housing units reported by EPC data. Expected income is based on the minimum wage in 2020. Unit price of housing is based on data from Rightmove reported here.