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JUDGE DREAD: COURT SEVERITY, REPOSSESSION RISK AND DEMAND IN MORTGAGE AND HOUSING MARKETS*

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We study the impact of borrower protection on mortgage and housing demand using variation in the likelihood that houses are repossessed coming from heterogeneity in preferences of local judges. We develop a framework that provides conditions to identify the impact of repossession risk on housing and credit demand, holding supply fixed. Empirically, we exploit exogenous variation in risk created by boundaries of court catchment areas. We find that a one-standard-deviation decrease in borrower protection decreases borrowing and house prices by 4.5%.

Economic downturns often motivate policies aimed at protecting borrowers from bankruptcies and reignite debates about the optimal level of borrower protection. While an extensive literature studies the impact of stronger creditor protections on credit markets and asset prices, it focuses mainly on the impact on the supply side (see Section 1 for a literature review). Conversely, little evidence has been gathered on how borrowers, i.e., the demand side, react to being protected. This is an important omission because such reactions are a key aspect of the debates around borrower protection, and because borrowers' sensitivity to financial risk is a key parameter in the household economics and financial intermediation literatures. Early evidence shows that borrower protection affects choices related to running a business as it provides a level of wealth insurance (Fan and White, 2003). A more recent study from Severino et al. (2024) supports this idea and documents that borrower protection can affect both supply and demand in credit markets, with the demand effect dominating in their sample. In this light, it is not surprising that the lack of empirical estimates on how borrowers respond to protection has been noted as a key limitation of the literature (Dobbie *et al.*, 2017).

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The data and codes for this paper are available on the Journal website. They were checked for their ability to reproduce the results presented in the paper. The authors were granted an exemption to publish parts of their data because access to these data is restricted. However, the authors provided the Journal with temporary access to some of the data, and a simulated or synthetic dataset for the others, which allowed the Journal to run their codes. The synthetic/simulated data and the codes for the parts subject to exemption are also available on the Journal website. They were checked for their ability to generate the tables and figures presented in the paper; however, the synthetic/simulated data are not designed to reproduce the same results. The replication package for this paper is available at the following address: https://doi.org/10.5281/zenodo.14671405.

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In this paper, we fill this gap by studying how households' demand for credit reacts to changes in the loss they can expect when they are forced to default. Our key contribution is to isolate the demand response to borrower protection and quantify its impact on mortgage demand and house prices. We concentrate on mortgage markets because mortgage commitments are the largest proportion of household debt and because they can be clearly connected to a single asset (allowing us to estimate the direct impact on asset prices).

Our main argument is that, while too much protection limits lending (constrained supply), not enough protection limits borrowing (constrained demand) and there is an equilibrium level of protection that maximises lending/borrowing. To investigate the demand side, we move away from the focus of the literature on the complex US context. Instead, we concentrate on an institutional framework that is more suitable for our research and common across the world (Japan, South Korea, France, Germany and many others follow similar systems), offering generalisable conclusions. More precisely, the institutional setup of England and Wales allows us to avoid several issues that are problematic for this literature in the US. First, we study a national credit market with different levels of borrower protection in different areas, but the same mortgage interest rates.[1] This simplifies the supply side of our market and allows us to focus on the effect of changes in borrower protection on the mortgage stock and housing values while holding constant the effect on interest rates coming from changes in risk (see some related US evidence in Goodman and Levitin, 2014). Second, in our settings, all mortgages are full-recourse loans. This means that defaults are mostly triggered by unexpected life events rather than strategic motives (Ford *et al.*, 2001; Lambrecht *et al.*, 2003; Benetton, 2021). This simplifies how we model the problem faced by households when taking mortgages. Unlike in most of the literature, our borrowers do not decide 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 and Noel, 2020), making our insights applicable to such no-recourse contexts. 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 *do not want to*) repay their loans. We show that this measure is correlated to individuals' ex ante survey-based perception of risk of being evicted, i.e., it is anchored in their expectations of the likelihood that they will be repossessed if taken to court.

Even in our context, studying the impact of borrower protection on households' demand in credit and asset markets faces two important challenges. First, while 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), in practice a repossession cannot occur without a loan being delinquent, which means that the two risks are correlated and it is difficult to separately identify their effects. Second, the risk of a repossession affects both the demand (households) and supply (lenders) of credit, so outcomes depend on the interaction between the two and isolating the demand response is challenging.

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. We devise a boundary discontinuity design (BDD) that compares outcomes across boundaries of areas where judges have different propensities to rule in favour of lenders

[1] In the UK, mortgage rates are determined by loan-to-value ratios. Borrowers apply for products via menus that are not differentiated across space. Approval probability depends on borrower characteristics, which can of course vary over space. Our analysis empirically takes care of this issue.

or borrowers, but housing market characteristics and the number of repossession cases submitted do not change discontinuously.[2] This allows us to focus on the impact of the difference in judge severity, which we measure as the ratio of repossession orders issued by a local court to the number of claims submitted to that court.[3]

To address the second issue and isolate demand from supply, we exploit the fact that court severity is not determined by interactions of supply and demand, but is set exogenously to local conditions (which we show empirically).[4] Using a simple theoretical framework, we show that, when courts are stricter than the equilibrium level of strictness dictated by market conditions, supply of credit will be higher than demand for it. This means that in areas where courts are too strict, mortgage quantity will be set solely by demand (at the given level of strictness and with interest rates that do not adjust locally as in our setting). In these areas, any change in market outcomes that we observe in response to changes in severity is driven by moving along the demand curve, while holding supply fixed. Leveraging this insight, our empirical analysis focuses on measuring the impact of court severity in such locations, which we label *demand-dominated* areas.

To identify these areas, we note that in demand-dominated locations credit supply is irrelevant (these areas are demand constrained) so changes to credit supply should not affect lending quantities or house prices. In other words, an exogenous credit supply shock should *not* have an effect on prices. To operationalise this idea, we study a change in the mortgage supply by the largest lender in the UK forced onto the bank by the financial regulator. This event provides us with a negative credit supply shock, allowing us to characterise demand-dominated areas as locations where house prices do not decrease when credit supply falls.[5]

Our evidence shows that a one-SD increase in severity—corresponding to an increase in the repossession probability over the lifetime of an average mortgage of around 48%—decreases both housing and credit demand by around 4.5%.[6] How sizeable is this effect? Since demand estimates are not available in the literature (to the best of our knowledge), we compare our findings with those of US studies that focus on the response of credit supply to borrower protection. Pence (2006) found that loan sizes are 3%–7% smaller in default friendly states. Similarly, Dagher and Sun (2016) showed that loan rejection rates increase by 3%–4% in states that favour borrowers. This suggests that our evidence provides an economically meaningful and 'credible' demand-side reaction to court severity for settings in which demand-side considerations prevail. In our context, we find that demand dominates in the majority of the areas we examine, so our findings are highly relevant for most of the housing market.

[2] A similar approach is used by Pence (2006) and Dagher and Sun (2016).

[3] We perform a number of tests to ensure that our proxies are not affected by the decision of the lender to submit cases to courts or by the conditions of the local housing and credit markets.

[4] This is particularly apparent locally in our BDD: although housing market conditions and homeowners' socio-economic characteristics move smoothly across the boundaries of court catchment areas, 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 the equilibrium on both sides.

[5] This change also meant that demand for Lloyds' loans possibly increased due to insurance motives—borrowers knew that they were less likely to be taken to court in the case of a delinquency. We return to this issue below.

[6] To work out this change, we have reasoned as follows. The probability of a house being repossessed before the mortgage is repaid is given by the cumulative probability that at some point a life event forces a default resulting in a repossession. A typical mortgage lasts 25–30 years and each year banks apply to repossess around 1% of mortgaged properties. Assuming that annual default risks are uncorrelated across years, at the average severity ratio of 0.2 this results in a cumulative repossession risk of around 4.9%–5.8% and increasing that ratio by one SD to 0.3 increases the risk to 7.2%–8.6%. These 2.0 and 2.8 percentage points both represent a 48% increase.

Based on these results, we perform a counterfactual exercise in which we estimate the impact of reducing severity 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 change would increase the average house price in England and Wales by 2.65%, create around £171 billion of housing wealth, increase mortgage stock by £6.2 billion and generate around £354 million per annum in additional transaction taxes. We do not comment on how this policy would affect welfare as its welfare effects would be complex and their analysis would require a more structural approach. However, we note that such a policy would have two important benefits. First, it would be easy to implement by simply revising rules on how much discretion judges have in repossession cases. Second, more uniform and less subjective court decision-making would reduce spatial differences in access to justice.

Besides our main findings, our work provides novel insights into the effect of court severity on housing and credit markets. First, we show that the ratio of repossession orders to claims remains fairly constant even when the type of cases submitted to courts changes due to an exogenous shock. This suggests that judges' preferences are 'sticky' and that they do not quickly adjust their preferences based on the characteristics of the cases they observe. This supports our argument that they are either too strict or too lenient for the market to be in a mortgage lending equilibrium, thus acting as a source of friction. Second, 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 likely dominate. In our institutional setting where mortgages are full recourse, we are 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.

1. Links with Previous Borrower Protection Research

In this section, 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 that are relevant to our work.

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) formalised this notion outlining the impact of protection on the supply and demand of credit. Several US 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 and 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 that less protection should be accompanied by bank regulation to limit credit oversupply. More recently, Dávila (2020) developed an approach that balances costs and benefits of borrower protection to set an optimal level of borrower protection. The author argued that the impact on demand can be neglected if borrowers make strategic default decisions. However, Ganong and Noel (2020) pointed out that this is likely not the case even in the United States and suggested that the impact of borrower protection on demand can be strong. While the majority of the literature focuses on the United States, Ponticelli and Alencar (2016) showed that improving protection for creditors in Brazil increased the use of secured loans, which suggests that demand effects dominate. Other examples of international studies of borrower protection include India (Visaria, 2009), Italy (Jappelli *et al.*, 2005), as well as international comparative research (Haselmann *et al.*, 2010).

While most of those studies supported their theoretical arguments with data, there is also a purely empirical strand of the borrower protection literature. Pence (2006) was the first to exploit a BDD looking at boundaries between states with different foreclosure laws. The study found that, when foreclosure laws favour the lender, loan sizes increase. Dagher and Sun (2016) used a similar design to show that this effect comes from an increase in credit supply. 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).

Since we focus on the demand effect, it is useful to note that multiple studies noted strong effects of borrower protection on choices made by borrowers at different stages in the life cycle of a mortgage loan. First, there is evidence that borrower protection can significantly improve outcomes of households in financial difficulties (Dobbie and Song, 2015; Dobbie *et al.*, 2017; Argyle *et al.*, 2021). Not surprisingly, this means that protecting borrowers has important implications for the decision to default. Ghent and Kudlyak (2011) showed that borrowers react to the loss they are likely to face when they default and that in full-recourse states they are more likely to default on their mortgages when the house is likely in negative equity. Multiple studies support this finding, suggesting that borrowers react to borrower protection (Cespedes *et al.*, 2020; Dobbie and Song, 2020; Pattison, 2020; Indarte, 2023).

There are two additional studies that are similar to our work. First, Severino and Brown (2020) considered the impact of borrower protection on market outcomes (lending stock levels). Like us, they noted 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.[7] Second, Fan and White (2003) noted that borrower protection provides insurance in default and should affect the demand side. They developed a model similar to our framework in which not enough borrower protection limits demand and supported it with data on the decision to start, own and close a business. However, we are not aware of any study that quantifies the impact of borrower protection on mortgage demand (isolated from the supply effect) and that links this directly to asset prices.

2. County Court Judges, Mortgage Repossessions and Credit Markets

2.1. *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 if the lender did not follow the proper procedures. They can 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

[7] Gross *et al.* (2021) also studied this trade-off (greater insurance versus a higher cost of credit) using the shock provided by the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act.

economic position.[8] 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 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.

2.2. *Courts' and Judges' Assignment to Repossession Cases*

Repossession cases are heard in the County Court (CC). The physical court in which the case is heard is determined by the postcode of the property (UK postcodes correspond to individual streets). Repossession cases are considered local community issues and, although cases can be submitted online or to any physical court location, they are automatically transferred to the CC that deals with the postcode where the property is located. The catchment area for each CC 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 CC 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 organised into groups and judges assigned to a group only travel between courts within the group. This means that a case assigned to a CC will be heard by one of the judges assigned to the group the CC belongs to. In practice, judges are residential, meaning that they tend to mostly hear cases in one of the CCs within the group and occasionally travel to address specific needs.[9]

In our data, we identify thirty groups based on documentation published by the MoJ in 2014 and 138 CCs. While groups can be amended 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% being 16 and 2.

2.3. *Court Hearing, Decision-Making and Judges' Strictness*

A repossession hearing usually takes between five and eight minutes. 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 the borrower can afford to pay now and in the future, temporary difficulties that the borrower is experiencing and the reason for accumulating the arrears (Whitehouse, 2009). This means that 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).

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

[8] The judge could order an 'attachment of earnings order' or wage garnishment. Such a decision—and its terms—would be mandated by the same judge and as part of the same hearing as opposed to being a process led by a debt collector (sanctioned by a separate court) as in the setting analysed by Cheng *et al.* (2021).

[9] The 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.

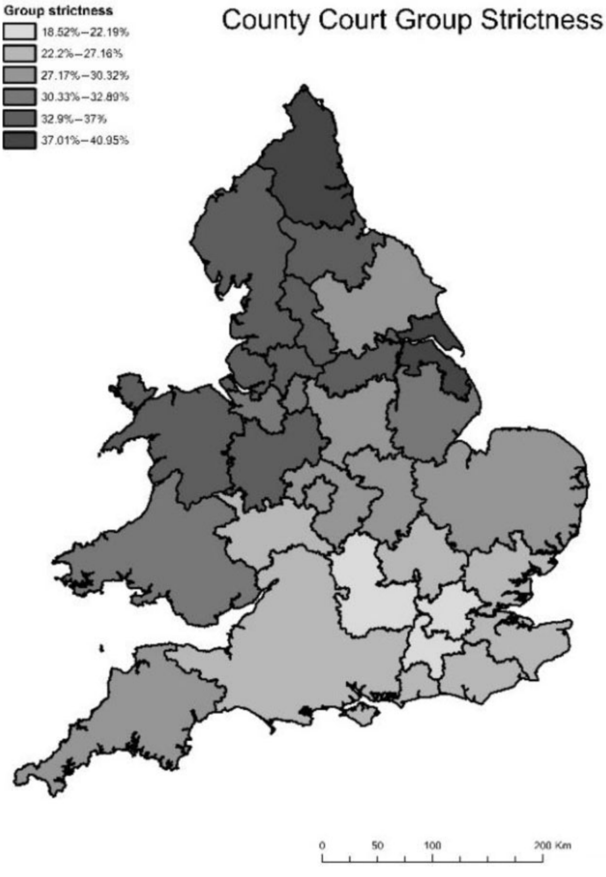

Fig. 1. *Boundaries of County Court Groups and Their Strictness.*
Notes: Data come from the Ministry of Justice. Strictness ratios are defined as the number of mortgage repossession orders divided by the number of claims at the group level. Groups are based on group-level reports by the Ministry of Justice from 2018.

2001 to 2018.[10] The severity index varies between 18.5% and approximately 41%, i.e., between a quarter and two-fifths of the cases submitted to courts result in repossession orders. Another way to interpret our severity ratio is to consider the impact this has on the probability of the repossession of a house during a typical thirty-year mortgage. We estimate the probability that the borrower stops making payments (due to an exogenous life event) and the lender submits a possession claim to be around 1% by dividing the number of cases taken to court every year by the universe of mortgages. Multiplying this figure by the average severity ratio gives the probability of a repossession in any given year of 0.198%. Assuming that life events are uncorrelated Poisson-type shocks, the cumulative likelihood of an average house being repossessed at some point over

[10] Group boundaries have been created by aggregating the boundaries of postcodes that form the catchment areas of CCs that belong to the same group. Court postcodes are available online at the MoJ's Github repository. Groups were instead drawn from information found in County Court Annual Reports 2006–7, and cross-validated with maps created by Her Majesty's Courts Service from 2003 and 2012.

twenty-five or thirty years is 4.9% or 5.8%. Increasing the severity ratio by one SD (0.107%) increases this figure to 7.2% or 8.6%.

While some of the differences in severity are explained by characteristics of the cases seen by judges, it is also likely that they reflect judges' personal inclinations to side with the borrower or the lender. To avoid confounding factors that drive spatial differences in repossession rates that are not related to preferences of judges—for example underlying levels of borrowers' financial distress—our empirical analysis exploits local discontinuities in the severity households face across group boundaries. This approach is based on the fact that judges do not move between groups and so group boundaries effectively separate areas where households can expect to face different severity, although underlying socio-economic characteristics and housing market conditions do not jump discontinuously (we provide ample balancing evidence in Section 3 below).[11]

However, we measure severity at the CC level because judges mostly operate in one hearing centre, so local severity (i.e., at the CC level rather than on average in the group) is the most accurate reflection of the local levels of severity households can expect if they face court hearings.[12] As we discuss later in the paper, severity is court specific as it varies across CCs in the same group, but shows little variation within the boundaries of courts. Importantly, distance to the court has no discernible impact on severity within its catchment area.

To validate the institutional underpinning of our BDD, we tracked judges' assignments to CCs by scraping data over eight consecutive working weeks from CourtServe.[13] Over the forty days during which we collected data, we found 184 judges holding 2,443 hearings. Of these, 86% of the judges hold hearings in only one CC (83% of the hearings). Among the judges that do not hold hearings in only one CC, 11.5% of them hold 2% of the hearings in another CC in the same group. On the other hand, 2.5% of the judges hold 0.6% of the hearings in a different CC from their modal one and travel to a different group. Of these, 60% travel to an adjacent group—this very small violation of our assumption is likely driven by the 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.

2.4. *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). When evicted, households usually struggle to find another accommodation. Although some can be accommodated in public housing, it is common for evicted households to experience a series of forced moves while they try to get back on their feet (Nettleton and Burrows, 2001; Abramson, 2021).

While it is intuitive that the prospect of a repossession would affect household choices, it is less clear that households are informed about the level of risk they face in different areas due to

[11] Unfortunately, available data do not contain judges' characteristics or those of the cases submitted to court.

[12] We test the robustness of our analysis to alternative levels of aggregation.

[13] This web service lists courts where judges hold hearings on a given day. A 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 data, we identify judges with non-ambiguous family names in CourtServe and focus our analysis on those.

differences in judge severity. Before we present evidence showing that people indeed seem to be aware of local severity, it is worth noting that not all households need to be informed of this risk for housing and credit markets to be affected. In fact, some households are more likely to worry about repossession than others. Specifically, it is plausible that, when a household begins to struggle with their mortgage repayments, they are likely to seek information about the chances that their house would be repossessed if their case went to court. If they learn that judges in the area tend to favour the lender, they might prefer to sell their house and move to a smaller unit—thus reducing their demand—than let their case go to court. Similarly, households who faced financial difficulties before or know someone who did are more likely to be aware of local judge severity.

In fact, many UK residents have some experience of a mortgage delinquency or repossession in their family or social network as the 'great repossession crisis' of the early 1990s is still an important reference point in social and housing market history. Ever since that crisis—when over 1 million people experienced repossessions—local and national numbers of repossessed homes, repossession claims and unfair repossession cases have regularly been covered by national and local media. In addition to the economic hardship they cause, these reports often highlight non-economic effects of repossessions and cover their adverse impact on mental health[14] and social outcomes.[15] Furthermore, those reports often include localised (CC or Local Authority District, LAD) numbers of repossession orders, and maps showing repossession hot spots.

Repossessions are also regularly reported to the parliament as an important political issue, and advice is given to members of the parliament on how to help their constituents understand and avoid repossessions (Barton *et al.*, 2021). Additionally, charities concerned with housing, and social issues (e.g., Shelter or the Joseph Rowntree Foundation) continually raise awareness of repossession risks at the local level by speaking to the media, publishing reports and offering free advice (Reynolds, 2011; Carlyon, 2012). Indeed, those charities regularly publish reports stating that hundreds of thousands of UK households live in fear of a repossession. Finally, when it comes to 'raw data', information about severity is public and freely available from the MoJ at the CC level. In sum, information about repossession risk is similar to information about air pollution, crime or school quality—it is a topic of public debate and households can easily find information relevant to a specific property and location.

To check if households living in different areas are aware of the local repossession risk they face, we investigated the correlation between our measure of severity aggregated at the Government Office Region level and housing-related risk perceptions using data collected by the charity Shelter (n.d.).[16] A survey of 13,268 households conducted by the polling agency YouGov asked if respondents 'feared being evicted'. In Online Appendix Table A1, we show that the survey is representative of the UK adult population as captured by the 2011 census.

Online Appendix Figure A1 shows that the share of respondents who said that they were afraid of being evicted is positively correlated with judges' severity. The correlation persists after weighing each region by its population (as measured by the 2011 census). Naturally,

[14] See the 'I'm worried I'll lose my home' report by the BBC (http://news.bbc.co.uk/newsbeat/hi/the_p_word/newsid_7687000/7687016.stm) or the 'Chainsaw death was "carefully thought through suicide"' article by the *Independent* (https://www.independent.co.uk/news/uk/home-news/chainsaw-death-was-carefully-thought-through-suicide-1025503.html).

[15] See http://news.bbc.co.uk/1/hi/business/6357331.stm and https://www.bbc.co.uk/news/health-27796628.

[16] Unfortunately, the data provided by the charity do not allow for a finer level of geographical aggregation.

socio-economic characteristics differ between regions, so there could be omitted factors affecting both severity and the share of respondents who fear a repossession. To partly address this issue, we regress the regional eviction fear rate on the judges' severity index, while controlling for regional unemployment rates and the share of people who are married or live in civil partnerships as controls.[17] Results are presented in Online Appendix Table A2. The correlation between eviction fears and severity remains strong even with controls. Finally, we use historic severity rates measured in the years 1990–5 as an instrument for severity in 2018. Although socio-economic profiles of different regions are likely to have changed since 1995, gaps in severity have remained stable.[18] Indeed, the first stage shows that historic severity is a strong predictor of current severity. Even with the instrument and control variables, the correlation between severity and eviction fears remains strong and positive. Albeit very aggregated, this evidence suggests that households' beliefs of eviction risk are shaped by local severity ratios.

Risk information can also be transmitted indirectly as observing more repossessions in an area (caused by a stricter judge) can inform households about the risk of their house being repossessed (Mian *et al.*, 2010). Indeed, repossessed properties are easy to identify, sell for lower prices and reduce the value of local amenities (Campbell *et al.*, 2011; Carlyon, 2012; Europe Economics, 2018). This means that observing more repossessions can lower demand by making repossession risk more salient—the channel we focus on—but also by reducing amenity value through a physical externality—a possible confounder in our analysis. Our BDD should take care of such issues because it is hard to imagine that any such physical externality would discontinuously stop at the boundaries of CCs. Nonetheless, to ensure that this channel is not affecting our results, we replicate our results using properties that are not affected by nearby repossessions (i.e., are located beyond 0.5, 1, 2.5 km distance buffers).[19]

Finally, it is worth reiterating that credit access and the credit supply that households face do not discontinuously vary across boundaries of CC groups. In simple terms, the UK mortgage market is dominated by large lenders who offer the same products everywhere. Their pricing models (i.e., the interest they charge) are based mainly on loan-to-value ratios and the type of product (e.g., fixed or adjustable rates). Applications are approved or denied primarily on the basis of loan-to-income ratios. To account for local information, lenders rely on valuers who follow nationwide instructions on what to include in their analysis and do not consider court severity. Local branches do not contribute any information to the process. Differences in credit supply across the country do exist, but are based on loan-to-income ratios. This variation is mechanical since bank thresholds do not vary over space, but price-to-income ratios do. As we already discussed, these differences are driven by socio-economic conditions that do not discontinuously jump across court boundaries. Similarly, at the national level interest rates may react to the average severity ratio. However, at the local level, interest rates are the same across the boundary. In short, households on opposite sides of a county court and group borders face the same credit supply conditions.

[17] We choose these two controls because they mirror the tendency of mortgage defaults in the UK to be driven by unexpected unemployment or family circumstanced, and the tendency of judges to reflect on such issues when adjudicating on repossession cases. In column (6), we also include household disposable income.

[18] This approach deals with shocks affecting repossession rates in the medium/short term. Long-run geographical differences (i.e., persisting from 1995 to 2018) that correlate with severity would still bias our estimates.

[19] Biswas *et al.* (2021) investigated the impact of Vacant Property Registration Ordinances using an approach that combines triple differences with boundary discontinuities. Their approach assumes that physical externalities do not spill over across administrative boundaries. Our robustness checks deal with such a possibility.

3. Data and Key Stylised Facts

3.1. *Data Sources and Data Construction*

Data on house prices come from the HM Land Registry (2020) and consist of records of every arms-length transaction in England and Wales, geocoded at the postcode level (Office for National Statistics, 2016). We use the years 2001–18 to line up housing market information with court severity data. Data on repossession claims and orders at the CC (as well as group and LAD) level come 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.[20] Similarly, the list of postcodes that belong to each hearing centre was obtained through an FoI request. Our data on mortgage lending by banks (UK Finance, 2020) come directly from the lending banks and give each lender's total stock of mortgage lending per quarter in a postcode sector since 2014.[21] We also have access to transaction-level data from the largest building society in the UK (Nationwide Building Society, n.d.). These include the price of a transaction, mortgage advances and some characteristics of the property. Finally, we use a range of supplementary datasets to show balancing across court boundaries (listed in the notes to Table 2 below). The most notable are the 2001 and 2011 censuses (Office for National Statistics, 2001, 2011) at output area—(OA) level data on socio-economic characteristics of neighbourhoods.[22]

To carry out our BDD, 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. To obtain comparable samples of transactions across boundaries, we exclude all newly built houses (representing less than 10% of transactions).[23] Data were trimmed 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, the optimal-bandwidth corridors of Calonico *et al.* (2014) are used in an extensive set of robustness checks—discussed below and fully confirming our main results. The number of observations in the boundary sample is 3,824,307.

3.2. *Judges' Severity: Definitions and Key Facts*

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 (2001–18). Dobbie and Song (2015) employed a similar proxy and noted that, for this index to be a meaningful measure of the risk households react to, the characteristics of cases assigned to different judges should be the same and judges' severity ratios should be fairly persistent over time. While the latter is easy to demonstrate (see the later discussions), we do not observe the characteristics of individual cases.

[20] 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.

[21] 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.

[22] An OA is a census geography with around a hundred residents.

[23] This restriction does not affect the results, but allows for a better comparison across boundaries. We show in our balancing checks that supply of new built homes does not change across CC boundaries.

Table 1. *Key Descriptive Statistics—Full Sample and Boundary Sample.*

	Full sample		Boundary sample	
	Mean	SD	Mean	SD
House price (Land Registry)	199,883	144,861	208,308	144,934
LTV (Nationwide; %)	70.3	21.8	69.9	220.4
Loan size (Nationwide)	152,141	81,558	158,168	83,403
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)	15,555	19,061	4675	5531
Boundary-specific 25th percentile of distance	7,683	6,612	–	–
Property is detached (%)	21.91	–	25.80	–
Property is a 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	–

Notes: Data on house prices and house characteristics come from the Land Registry. Data on judges' severity were obtained from the Ministry of Justice (2020). Samples include observations between 2001 and 2018 and second-hand transactions only (newly built are excluded). Data have been trimmed and transactions in the top and bottom 1% of the yearly price distribution have been dropped. Boundary refers to the boundaries separating CC groups. Number of observations in the full sample is 15,292,907 in 30 groups with 138 counties and 71 boundaries. Boundary sample includes transactions within the 25th percentile of the boundary-specific distance distribution. Number of observations in the boundary sample is 3,824,307 in 30 groups with 117 counties and 71 boundaries. Data on loan size and LTV come from Nationwide and are available between 2004 and 2017. Number of observations in the lending sample is 885,118 in the full sample and 237,832 in the boundary sample. Judges' severity is the number of repossessions divided by the number of cases seen by judges. Averaged figures refer to the average across all years. Group-level severity is defined by counting all repossessions and all cases across CCs belonging to the same group. LAD-level severity index is constructed by dividing social tenant repossession orders issued for properties within LAD boundaries by the number of claims submitted. More details are provided in Online Appendix A.2.

However, in our analysis, we show that judges' severity does not change significantly even when the characteristics of submitted cases changed due to an exogenous event. We develop this point later (in Section 4.3 below). Furthermore, we find no evidence that the number of cases submitted to courts or socio-economic characteristics of areas changes across the boundaries. This suggests that the differences in severity indices are driven by preferences of judges. To further check if case characteristics affecting judge decisions are a problem, we describe an alternative measure of severity that is unlikely to be affected by this issue and use it as a robustness check (see Section 4.3). This alternative confirms our key findings.

Table 1 shows descriptive statistics for the full and boundary samples. Prices are slightly higher in the boundary sample, with fewer leasehold properties and more detached houses. 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. 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 high, always above 0.55 and stronger if we consider shorter and adjacent periods. For example, the correlation for the CC-level severity index averaged between 2010 and 2014 and the same index after 2015 is 0.67, while the correlation between the index averaged pre-2009 and the average severity after 2015 is only 0.39. Similarly, the correlation between the average severity index pre-2009 and the average index over the 2010–14 period is 0.62. This suggests that the high level of persistence is indeed due to judges being consistently more/less lenient—with any time variation mostly driven by judges' turnover (due to death, retirement and career changes). We return to this point below, where we show that our results do not change if we use yearly varying or time-averaged proxies for severity, suggesting that individuals face persistent risk over time on opposite sides of court boundaries.

Finally, we study patterns of geographical variation in severity measures. 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. We also find that severity is highly positively correlated over space within a CC catchment area and that there is a positive correlation within the same group (the magnitude of this association is much lower than within the same CC catchment area). Lastly, we study the correlation between distance to the boundary and severity ratios and find no systematic evidence that they are related. Overall, courts tend to apply the same level of severity within their jurisdictions. More details are presented in Online Appendix A.1.

3.3. *Balancing Evidence*

Since our methodology is based on a BDD design, we provide evidence that supports this approach. To start, we show that some key variables that are likely to affect housing and credit markets are balanced across court boundaries. Our results are presented in Table 2, which focuses on regression results. Balancing graphs are only presented in Online Appendix Figure OA1 because a graphical analysis—straightforward in traditional regression discontinuity designs—is not easy to implement and interpret in our spatial setting (Calonico *et al.*, 2015; Keele and Titinuk, 2015; Cattaneo *et al.*, 2021). First, our BDD differs from a standard Regression Discontinuity Design as it includes multiple cut-off points (one for each boundary) and the intensity of the treatment at each of these points differs. Second, in our regression analysis we use CC-level severity, but group boundaries, so we have severity variation on the same side of the cut-off along the boundary. While our main approach cannot be easily presented graphically, the key insights from Table 2 are replicated in our graphical analysis. The first column of Table 2 presents results using the 25th percentile boundary-specific corridors described above, while the second column uses the Calonico *et al.* (2014) optimal bandwidths (with linear distance).

Panel A focuses on housing characteristics from the Land Registry (used as controls in most specifications). These do not significantly vary across the boundary as a function of judges' severity. Panel B uses several datasets related to housing and credit markets and shows that the variables we consider are balanced. The numbers of transactions or loans originated do not change across the boundary. Neither does the interest rate paid by the borrowers, the length (in years) of the mortgage granted or the share of first-time buyers.[24] There is also no difference in judgements issued by courts on consumer credit (non-mortgage-related) debt.[25] Critically, the number of cases submitted does not change at the boundary. Although we do not have data on local defaults, we argue that in our setting they are mostly driven by exogenous, rather than strategic factors (see Section 2). Therefore, the fact that the number of defaults that result in a

[24] In our analysis, we consider the initial rate attached to the mortgage contracts tracked in the Survey of Mortgage Lenders. After an initial period of three to five years, UK mortgages normally 'reset' to either a new fixed-rate mortgage contract or a floating rate one. While we only observe the initial rate, we have no reason to believe that subsequent interest charges or other features of the loans are unbalanced across court boundaries.

[25] Unlike mortgage cases, consumer credit cases can be heard at any physical court location agreed by the lender and borrower, so consumer-credit severity variation is unlikely correlated with housing repossession severity.

Table 2. *Balancing Evidence.*

	(1)	(2)	(3)
		Coefficient on judges' severity	
	Mean	25th percentile	Optimal linear
Panel A: housing characteristics			
Property is detached (%)	0.258	−0.001 (0.004)	−0.003 (0.005)
Property is a flat (%)	0.129	−0.004 (0.003)	0.001 (0.004)
Property is semi-detached (%)	0.313	0.004 (0.003)	−0.002 (0.004)
Property is terraced (%)	0.300	0.002 (0.003)	0.004 (0.003)
Property is leasehold (%)	0.183	−0.005 (0.003)	0.000 (0.004)
Panel B: housing and mortgage market characteristics			
Number of housing transactions (log)	0.409	0.001 (0.002)	0.001 (0.002)
Share of new properties transacted	0.009	0.000 (0.000)	0.000 (0.000)
Number of Nationwide loans (log)	0.072	−0.001 (0.001)	−0.000 (0.001)
Share of first-time buyers (Nationwide)	0.402	−0.003 (0.004)	0.008 (0.005)
Interest rate on mortgage (SML, 2001 only)	5.562	0.012 (0.022)	0.027 (0.022)
Length of mortgage, years (SML, 2001 only)	21.897	−0.012 (0.140)	−0.093 (0.134)
Repossession claims submitted to CCs	9.625	0.276 (0.267)	0.290 (0.209)
Average order in British pounds: consumer debt (log)	7.211	−0.006(0.007)	−0.006 (0.011)
Count of orders > £1,000: consumer debt (log)	1.667	0.001(0.001)	0.000 (0.002)
Panel C: amenities			
Council tax (log)	6.004	−0.039 (0.048)	0.011 (0.054)
Value added, secondary schools (2007–11)	−0.046	−0.003 (0.005)	0.012 (0.008)
Distance to secondary schools (log)	8.209	0.029 (0.026)	−0.025 (0.034)
Value added, primary schools (2007–11)	−0.035	−0.001 (0.006)	0.001 (0.005)
Distance to primary schools (log)	7.51	0.019 (0.028)	−0.034 (0.026)
Burglaries (log)—ward, London only	1.663	0.046 (0.190)	0.187 (0.200)
Panel D: 2001 census characteristics			
Average household size	2.401	0.003 (0.006)	0.006 (0.010)
Housing overcrowding index	0.046	−0.001 (0.001)	0.002 (0.002)
Unemployment rate	0.042	−0.000 (0.001)	0.002 (0.002)
Qualification at level 4 or 5 (degree)	0.203	−0.007 (0.004)*	−0.010 (0.005)*
White ethnic background	0.951	0.003 (0.005)	−0.007 (0.010)
Male population	0.487	0.000 (0.000)	−0.000 (0.000)
Share in social housing	0.135	0.001 (0.003)	0.007 (0.005)
Mean income (log)—LSOA, London only	10.413	0.050 (0.059)	0.107 (0.056)
Panel E: 2011 census characteristics			
Average household size	2.386	0.003 (0.008)	0.006 (0.013)
Housing overcrowding index	0.031	−0.001 (0.001)	0.002 (0.003)
Unemployment rate	0.037	−0.000 (0.001)	0.001 (0.001)
Qualification at level 4 or 5 (degree)	0.233	−0.007 (0.004)*	−0.008 (0.007)
White ethnic background	0.914	0.005 (0.008)	−0.007 (0.013)
Male population	0.493	−0.000 (0.000)	0.000 (0.001)
Share in social housing	0.126	−0.001 (0.003)	0.003 (0.004)
Mean income (log)—LSOA, London only	10.762	0.082 (0.077)	0.139 (0.073)

case being submitted to court does not change across the boundary suggests that the decision to submit a case is unlikely to depend on the severity of the judge. This lends support to our research design in which judges on opposing sides of boundaries preside over a similar number of hearings and scrutinise similar cases, and yet adjudicate with different levels of severity.[26]

[26] In fact, the regression results we present later are mostly unaffected if we focus on judges' repossession orders—as opposed to the ratio of repossessions to cases. We return to this point below.

Table 2. *Continued*

	(1)	(2)	(3)
		Coefficient on judges' severity	
	Mean	25th percentile	Optimal linear
Panel F: 2001–11 changes in census characteristics			
Average household size	0.018	0.005 (0.005)	0.004 (0.005)
Housing overcrowding index	0.015	−0.001 (0.000)	0.000 (0.001)
Unemployment rate	0.005	0.000 (0.001)	0.000 (0.001)
Qualification at level 4 or 5	−0.031	−0.001 (0.001)	−0.000 (0.001)
White ethnic background	−0.036	0.002 (0.002)	−0.001 (0.002)
Male population	−0.006	0.000 (0.000)	0.001 (0.000)
Share in social housing	0.01	0.000 (0.001)	0.003 (0.002)
Mean income (log)—LSOA, London only	0.349	0.032 (0.020)	0.032 (0.020)

Notes: The table reports the mean of the relevant variable in column (1) and regression coefficients of the variable on the standardised index of judges' severity (measured at the CC level and yearly) in columns (2) and (3). Column (2) uses observations within the 25th percentile of the boundary-specific distance distribution. Column (3) uses optimal linear bandwidth. SEs are clustered at the boundary level. Each cell in columns (2) and (3) comes from a different regression. All regressions include 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 in column (2) and linear distance in column (3)) and group-by-year FEs. All other regressions are run at the postcode-by-year level (panels B and C) or postcode level (panels D, E and F). Census data are available at the OA level. Income data are available at the lower layer super output area (LSOA) level and for London. Survey of Mortgage Lenders (SML) data are only available in year 2001 at the LAD level (Department for Transport, Local Government and the Regions, 2002). Data on judgements from County Court on consumer debt are only available '01-'15 data are from CDRC (2020). Data on school quality (value added, five closest schools for each transaction postcode) and distance to schools (closest five) come from the Department for Education (only in years 2007–11). Data on burglaries are only available at the ward level for London (from the Metropolitan Police; Crime data, 2006–10). Council tax data (2001–18) are from the government online collection. OA, LSOA and ward-level data were first matched to our transaction-level data using a mapping between postcodes and these geographies. Data on income are from Office for National Statistics (2020). The data were subsequently collapsed at the postcode-by-year/postcode level to ensure that the observations weigh the postcode-level variation in transactions within postcodes belonging to the higher levels of aggregation (across years and in terms of distance to the boundaries). This maintains as best as possible the same geography as in the original data. * Significant at the 10% level.

Panel C shows that amenities correlated to house prices (e.g., school quality, crime and public services approximated by local taxes) are also balanced across our boundaries. Panels D and E consider households' socio-economic characteristics from the 2001 and 2011 censuses. They move smoothly across boundaries except for the education level in 2001. Finally, panel F shows changes of these variables (2001 to 2011) and finds that these are also balanced.

To conclude this section, we present some graphical analysis of how our key variables—severity, house prices and mortgage quantities—move across boundaries. Once again, we consider this evidence as suggestive for the reasons discussed above, and mostly helpful to illustrate the data structure. Our findings are reported in Figure 2, which presents graphs for court severity (left-hand panel), house prices (central panel) and loan values (right-hand panel) and in 2 km windows from a group boundary. Positive distances correspond to areas with stricter judges, while negative values represent areas with softer courts. In the figure, strictness ordering considers *group-level* (not CC-level) severity to mirror the descriptive evidence provided in Figure 1 and so that the two sides of the boundary can be lined up graphically. Once again, we emphasise that, while this approach exploits the seventy-one discontinuities across group boundaries, it neglects variation at the CC level, which we exploit in our regressions. As a result, the evidence is noisy when considering loan values, which are only tracked in the smaller Nationwide sample. By construction, we find that court severity is higher to the right of a group boundary. We also

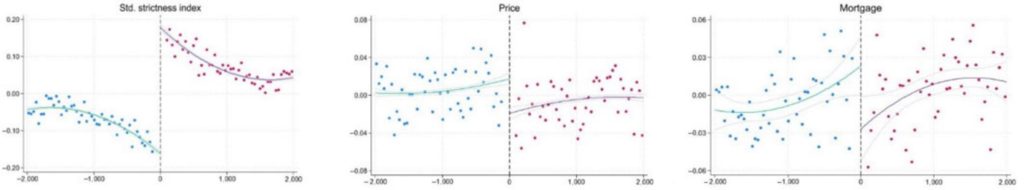

Fig. 2. *Boundary Discontinuity Graphs.*
Notes: The figure plots variables given in the title of each plot against the distance to the closest group boundary. Negative distances (left-hand side) represent the softer side of the boundary, while positive distances (right-hand side) represent the stricter side. Each dot represents one of the fifty bins of 40 m on each side of the boundary. All results are adjusted for distance to the boundary and its polynomials to the third order, and year and month effects, so the dots represent the mean residual from the regression of the variable on those controls. Continuous lines represent quadratic fits and 95% confidence intervals. The plots use the same variables and samples as the main results (values in natural logarithms). See Table 1 for details. Number of observations for prices around 885,000 and for mortgages is around 234,000.

find lower prices and smaller mortgages in areas where judges are stricter. This supports our conjectures, which we next test more rigorously.

4. Reduced-Form Estimates

4.1. *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 yield biased estimates because of the unobservables that simultaneously drive housing and credit markets, as well as judges' decisions. To by-pass this issue, we use a BDD similar to Gibbons *et al*. (2013) and Mian *et al*. (2015).

To formalise ideas, we estimate the relationship

$$P_{i(cgbt)} = \alpha + \beta_C C_{cgbt} + \Lambda X_{i(cgbt)} + \theta_t + g(c) + \epsilon_{i(gcbt)}, \tag{1}$$

where $P_{i(cgbt)}$ denotes the (log of the) 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);[27] C_{cgbt} denotes the (standardised) severity ratio for court c belonging to group g closest to boundary b at time t; $X_{i(cgbt)}$ is a vector of housing characteristics with associated coefficients Λ; the θ_t are time shocks (we use year and month dummies); $g(c)$ is an unknown function that captures the impact of unobservables related to the location of court c on house prices and $\epsilon_{i(gcbt)}$ 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.

In the above regression the key parameter of interest is β_C. Estimating (1) 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 in the catchment areas of different CCs; (*ii*) group-wide shocks, possibly time varying; and (*iii*) location-specific features and geographical attributes, proxied by a property's coordinates.

[27] The notation *i(cgbt)* highlights the repeated cross-section (non-panel) nature of our transaction data.

To deal with these issues, we use a spatial BDD that exploits discontinuities in severity across boundaries of CC groups. We make some important identifying assumptions. First, the severity borrowers face when taken to court changes discontinuously at the group boundary. This is imposed by our institutional setting and—as shown above—it is clear that defaulting borrowers will face different sets of judges who have different levels of severity across group boundaries. Second, the changes in severity at the boundary are unrelated to other correlates of house prices and mortgage sizes. The balancing evidence in Table 2 supports this assumption. Furthermore, since the boundaries we exploit were set a long time ago (see Polden, 1999) and are not used for other administrative or justice-related functions, it is unlikely that they will be correlated with other features of the housing market in a way that could drive our results. Finally, individuals do not sort across boundaries based on judges' severity. We argue that this is because, while people are aware of the repossession risk for specific properties, they do not know exactly where the boundaries of court catchment areas lie, so they cannot sort on them. As discussed, the way to find out which court a house belongs to is to use a postcode lookup. From there, households can find the specific CC's severity. However, the boundaries of court catchment areas are not in the public domain and are not known to the public. In this setting, it is likely that people know the severity of the court their house (or a specific set of houses) falls under, but they could not move to a house 'across the boundary' that belongs to a different court because they would struggle to figure out where such a boundary lies.[28]

To implement our approach, we begin by restricting 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. This creates boundary-specific samples that reflect differences in population density across boundaries (an important consideration for spatial discontinuity analysis). Below, we discuss alternative sample selections, including the optimal bandwidth following Calonico *et al.* (2014). We then include in our model boundary fixed effects, so that identification is obtained by comparing properties close to the same boundary. We also control for third-order polynomials in distance from the boundary. These account for possible spatial trends in prices and mortgage conditions as we move away from the boundaries that could correlate to court severity. We also experiment with lower-order distance polynomials to deal with over-fitting issues flagged by the literature on (non-geographical) discontinuity design. 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 (1):

$$P_{i(cgbt)} = \alpha + \beta_c C_{cgbt} + \Lambda X_{i(cgbt)} + \sum_{d=1}^{3} \delta_d dist^d + \theta_t + \mathrm{B}_b + \Gamma_g + \epsilon_{i(gcbt)}. \quad (2)$$

Most variables are discussed above, with the B_b the boundary dummies, the $dist^d$ terms capturing the non-linear (third-order polynomial) impact of distance to the boundary and the Γ_g either group or group-by-year effects. We cluster SEs at the boundary level.[29]

As 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

[28] Following the literature that exploits a judge fixed-effect design, we also implicitly assume 'monotonicity': the mapping between our measure of severity and the 'true' judges' strictness is monotonic across all boundaries (i.e., the underlying 'first stage' of our reduced-form analysis is positively signed everywhere).

[29] We experimented with the Calonico *et al.* (2014) robust confidence intervals, but always found these to be less conservative than the clustered alternative. As a result, we have opted for the more conservative approach.

severity at the CC level because cases are assigned to CCs through catchment areas, and judges mostly operate in one hearing centre. This means that some additional variation comes from changes in severity along boundaries *within* the same group.[30] To allay any related concerns, we also estimate models that use 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 and provide evidence that judges' severity is correlated over time, and thus predictable by individuals.

4.2. *Reduced-Form BDD*

Table 3 shows the results of our reduced-form specifications. All columns consider CC-level yearly severity standardised in the full sample and the log of house prices. The coefficients can be interpreted as percentage changes in house values for a one-SD change in court severity (approximately a 10-percentage-point change in order-to-claim ratios). All specifications include year and month effects. In the first and second columns, we present OLS regression results in the full and boundary samples for comparison. These show that severity is strongly and negatively correlated with house prices. In columns (3) to (8), we exploit the BDD detailed above. 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 specifications—which control for group-by-year or boundary-by-year effects and distance-by-boundary polynomials (columns (6) to (8))—show that a one-SD (in the national distribution) increase in judges' severity decreases house prices by 3.3%. Importantly, we find that such an association is driven by the negative impact of repossession orders on house prices rather than the number of cases submitted to courts. As shown in Figure 3 (left-hand panel), conditional on group-by-year effects, the effect of repossession orders on house prices is −2.7% (significant at the 5% level), while there is no effect of the number of cases on housing values (−0.0012, not significant).

In the other plots of Figure 3, we present an extensive series of robustness checks on these results. In the left-hand panel, we show several additional specifications, including one where we check that our results are unaffected if we exclude properties within 1 km of a repossessed home to dispel the possibility that our findings are explained by physical dis-amenity externalities (using a 0.5 or 2.5 km threshold does not affect our conclusions). In the middle panel, we test different distance polynomials and bandwidth selection methods (including the optimal approach of Calonico *et al.*, 2014). Finally, in the right-hand panel we show how our results differ across price deciles and find that cheaper properties are affected more. We discuss these tests in more detail in Online Appendix A.2, but our reduced-form evidence is clearly robust.

Next, we turn to the credit market using data from Nationwide. Our results are presented in Table 4. Columns (1) to (3) focus on prices, columns (3) to (6) look at loan values and columns (7) and (9) 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) to (3) 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

[30] This variation helps with the estimation of models that control for group and group-by-year fixed effects.

Table 3. *The Impact of Judges' Severity on House Prices.*

	(1) Full sample	(2)	(3)	(4)	(5) Boundary sample	(6)	(7)	(8)
	OLS	OLS	BDD	BDD	BDD	BDD	BDD	BDD
Judges' severity index (std)	−0.266	−0.257	−0.067	−0.068	−0.062	−0.033	−0.033	−0.032
	(0.031)***	(0.031)***	(0.010)***	(0.010)***	(0.008)***	(0.008)***	(0.008)***	(0.007)***
Distance controls	No	No	No	Yes	Yes	Yes	Yes	No
Distance by boundary	No	No	No	No	No	No	No	Yes
Housing Xs	No	No	No	No	Yes	Yes	Yes	Yes
Group FEs	No	No	No	No	Yes	No	Yes	Yes
Group-by-year FEs	No	No	No	No	No	Yes	No	No
Bound.-by-year FEs	No	No	No	No	No	No	Yes	Yes

Notes: The table reports coefficients and SEs in parentheses (clustered at the boundary level) of a regression of the log of house prices on an index of judges' severity (CC level, yearly) standardised in the full sample and controls as detailed in the sample description. Samples and the number of observations are as detailed in Table 1. All regressions control for year and month fixed effects. 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. Judges' severity is standardised (mean = 0, SD = 1 approximately in both the full and boundary samples). Mean log price is 11.99 in the full sample and 12.04 in the boundary sample.

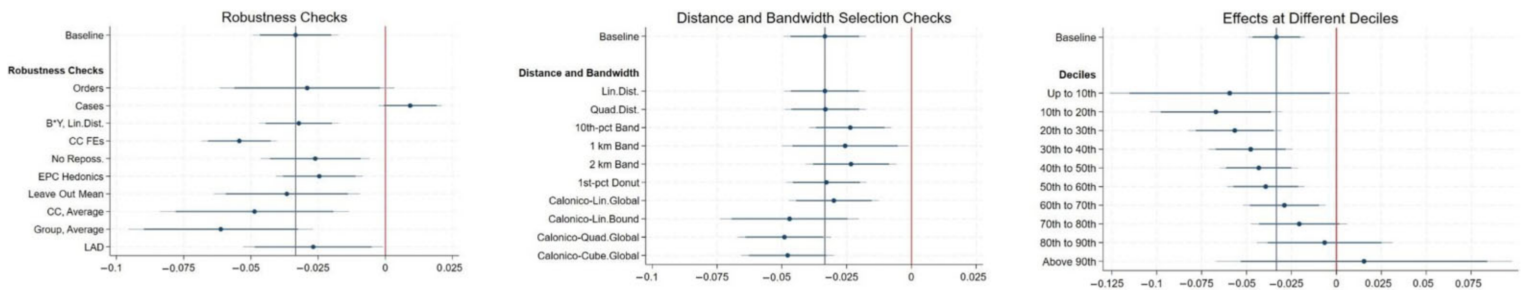

Fig. 3. *Robustness Checks and Variations on Our Main Specification.*

Notes: The different plots report point estimates (solid dot) with 95% and 90% confidence intervals (bold and light lines, respectively) obtained from different regressions of the log of house prices on judges' severity. These results test the robustness of our main findings (Table 3, column (6); this baseline estimate is reported at the top of each panel). More details are provided in Online Appendix A.2. The specification with 'EPC hedonics' includes data from the EPC (2018) on energy performance certificates.

Table 4. *The Impact of Judges' Severity on Mortgage Markets.*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log of house price			Log of mortgage advances			LTV ratios		
	Full sample, OLS	Boundary sample, BDD		Full sample, OLS	Boundary sample, BDD		Full sample, OLS	Boundary sample, BDD	
Judges' severity index (std)	−0.147 (0.024)***	−0.025 (0.007)***	−0.024 (0.007)***	−0.130 (0.025)***	−0.023 (0.007)***	−0.022 (0.008)***	0.009 (0.002)***	0.002 (0.002)	0.002 (0.002)
Distance controls	No	Yes	No	No	Yes	No	No	Yes	No
Distance by boundary	No	No	Yes	No	No	Yes	No	No	Yes
Group FEs	No	No	Yes	No	No	Yes	No	No	Yes
Group-by-year FEs	No	Yes	No	No	Yes	No	No	Yes	No
Bound.-by-year FEs	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The table reports coefficients and SEs in parentheses (clustered at the boundary level) of a regression of the log of house prices (columns (1) to (3)), the log of mortgage advances (columns (4) to (6)) or loan-to-value ratios (columns (7) to (9)) on an index of judges' severity (CC level, yearly) standardised in the full sample and controls as detailed in the sample description. Sample sizes are 553,754 (full sample) and 149,369 (boundary sample) along sixty-nine county court group boundaries. All regressions control for year and month fixed effects, and for housing characteristics. 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. Judges' severity is standardised (mean = 0, SD = 1 approximately in both the full and boundary samples). Mean log price is 12.21 in the full sample and 12.26 in the boundary sample. Mean log mortgage advances is 12.69 in the full sample and 12.72 in the boundary sample. Mean LTV is 0.703 in the full sample and 0.699 in the boundary sample.

2.5% effect found in column (2) is very close to the negative 3.3% impact estimated using the full sample (column (6), Table 3). Columns (4) to (6) reveal that the impact of court strictness on loan sizes is virtually the same as the impact of severity on prices. A one-SD change in severity reduces mortgage advances by 2.3%—very close to the estimate of 2.5% in column (2). To conclude, columns (7) to (9) study the impact of judges' severity on LTV ratios. We find no effect on LTV ratios as mortgage and house prices decrease by similar amounts as court severity increases.

Before moving on, we emphasise again that our reduced-form estimates still potentially contain both supply and demand reactions—an issue we address in Sections 5 and 6 below. Even then, our negative estimates clearly show that demand-side considerations prevail in settings (like ours) where mortgages are full recourse and lending markets are uniform over space (and especially across court boundaries).

4.3. *Addressing Possible Biases in Courts' Severity Measures*

To be able to claim that the reduced-form effects presented above are causal, we need to ensure that the ratio of repossessions to claims is not a biased measure of severity. This could be a problem if (*i*) the type of cases submitted depends on the severity of the judge, (*ii)* judges change their severity based on the type of cases they see (and types differ across boundaries).

To address the first issue, in Figure 3 (left-hand panel) we replace the court severity index with a LAD-based index for which the bias from the type of submitted cases is likely to be minimal (see Online Appendix A.2). The results are similar to our main specification and support our causal interpretation. 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 for the whole CC, and not just those close to the boundary. We discussed this point above (results in Online Appendix A.1): severity does not vary with distance to the boundaries within the same CC.

To further investigate whether judges' severity changes 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 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. Such unfair treatment led to more repossession claims being submitted to courts in locations where Lloyds had a larger market share. However, these claims should not have been submitted by the bank in the first place (as they were resolvable) and should not have resulted in repossession orders if judges took into account the characteristics of the cases they saw and adjusted severity. Once the bank was fined and bought its procedures in line with the rest of the market in 2015, the additional claims were no longer taken to court.

Using this intuition, we design a quasi-difference-in-difference (DiD) design comparing repossession orders issued by judges in years when they saw the additional unfair cases relative to the number of orders they issued in years when they only saw cases that any fair lender would submit in areas with a high/low penetration of Lloyds in the local mortgage market. More details are provided in Online Appendix A.3. Overall, the evidence coming from this analysis suggests that judges' severity is not significantly affected by the cases they assess and tends to be 'sticky'.

5. Towards a 'Pure' Demand Estimate: A Theoretical Framework and Its Implications

As highlighted in the introduction, housing and mortgage demand are likely negatively affected by severity, while mortgage supply is an increasing function of severity. The reduced-form estimates presented above contain both margins of adjustment and do not disentangle demand from supply reactions. In this section, we lay out a theoretical framework to guide our empirical work when the goal is to identify the impact of severity on credit and housing demand, holding the supply side of the market fixed. We provide full details in Online Appendix A.4. Here we restrict our discussion to (*i*) its main building blocks and (*ii*) the implications that are key to our research design aimed at identifying a 'pure' demand effect.

5.1. *Theoretical Underpinnings*

Court strictness affects economic decisions through the credit market, so we start by modelling household demand and bank supply for mortgage loans. Starting with the demand side, we treat mortgage demand as endogenous to housing demand. We derive mortgage demand from household demand for a housing service under risk in the spirit of Campbell and Cocco (2007). This approach allows us to show the impact of a change in the risk of a repossession on housing and mortgage demand. We consider a one-period model in which a household's utility is delivered by consuming housing and non-housing goods. To purchase a property, households use their assets and a mortgage—connecting mortgage demand to housing demand.

A key element of our framework is that, although ex ante households are expected to receive a given level of income I_n, the income they actually receive is revealed after the house is purchased and, with probability q, this takes value I_u with $I_u < I_n$. When income is lower than expected, it is only enough for essential non-housing consumption and the borrower is delinquent (we think of I_u as unemployment or other social benefits). The lender will attempt to repossess the property and succeed with probability C (given by court severity). If the lender is unable to repossess the property, the household can continue to occupy it. This introduces an insurance element into the credit and housing market as the utility of borrowing and owning a property (with a loan) increases as C decreases. Consistently with our full-recourse context and evidence on defaults in the UK, we assume that a delinquency is triggered by an exogenous life event rather than strategic motives. Therefore, we treat q as unrelated to the size of the loan. Clearly, this is a simplification: the characteristics that predict an individual's likelihood of such shocks (e.g., job losses) are likely to be tied to the size of the loan they obtain, generating some correlation between q and the extent of leverage. However, as shown in Table 2, all household characteristics we investigate are balanced across CC boundaries, suggesting that in our empirical design such issues are not a major consideration. Similarly, in our model, we treat r as exogenous rather than interacting with q. We keep this assumption in mind when interpreting our results.

Next, we outline the decision of a bank to provide a loan and show how credit supply is affected by court strictness. Specifically, risk-neutral banks accept a loan request if the expected return—after taking into account default risk q and repossession probability C—exceeds the origination cost. Banks increase profits by increasing the total value of accepted loans. This simple formulation 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. Consequently, mortgage supply should increase in the index of court severity.

An important assumption we make is that the interest rate charged by banks does not reflect local differences in the probability of successfully repossessing a property when borrowers default. Stated differently, interest rates do not price in local changes in repossession risk. This is consistent with our institutional setup in which mortgage rates are determined by loan-to-value ratios and do not vary over space, and certainly not across the boundaries of CC catchment areas. Instead, borrowers apply for mortgage products via menus that are not differentiated across space. Approval probability depends on borrowers' characteristics, which can vary over space. Our BDD design empirically takes care of this as we show that household characteristics are balanced across boundaries.

Finally, we close our framework by modelling how house prices are determined. For simplicity, we assume housing supply to be fully inelastic, although this assumption can be relaxed without altering our conclusions.[31] Aggregate housing demand is instead set by the sum of demand from households subject to their corresponding mortgage demand being satisfied by banks. This means that, on the one hand, housing (via credit) demand will increase in areas with less severe courts because of the insurance mechanisms discussed above, and, on the other hand, credit supply will be reduced—because banks offer fewer mortgages in areas with less severe courts—constraining housing demand. In turn, this means that the impact of court severity on housing prices does not have a clear sign.

5.2. *Empirical Implications*

Our empirical goal is to estimate how severity C (the probability that a delinquency results in a repossession) affects demand for housing and mortgage credit, holding supply fixed. As discussed, judges' preferences are not a flexible parameter set by the market. This is evident in our boundary-discontinuity design: housing markets and socio-economic characteristics are identical across CC boundaries, but judges rule differently on strict/soft sides, meaning that either severity is at the equilibrium on one side, but not the other, or it is out of the equilibrium on both sides. In Online Appendix A.3, we provide suggestive evidence of this disequilibrium claim by showing that judges tend not to adjust severity when conditions in the market exogenously change (see also the discussion in Section 4.3). Finally, we reiterate that interest rates do not react to changes in severity—certainly not locally and across court boundaries. Overall, this means that C is unlikely to be at the market equilibrium set by supply and demand.

This intuition is presented in Figure 4. The top panel plots mortgage supply and demand as functions of court severity for a given interest rate. Here C^* denotes the theoretical equilibrium level of court severity that would maximise lending, denoted by 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 and lending will increase as severity increases (sliding up along the supply curve). Therefore, $\partial L/\partial C$ will be positive and identify the elasticity of supply for $C < C^*$, while for $C > C^*$, $\partial L/\partial C$ will be

[31] This assumption is a common assumption in urban economics as housing supply adjusts very slowly (see Mayer and Somerville, 2000; Hilber and Vermeulen, 2016).

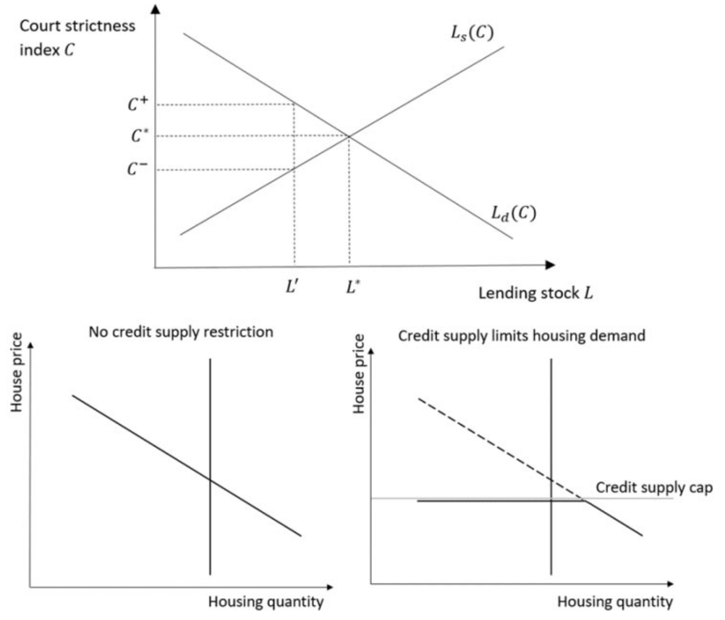

Fig. 4. *Supply and Demand as Functions of Court Severity.*
Notes: Top panel: court strictness C is the probability that a judge will rule in favour of the lender (conditional on cases being the same), $L_s(C)$ denotes the supply of credit as a function of court strictness (all else equal), $L_d(C)$ denotes the demand for credit as a function of court strictness (all else equal) and L denotes lending stock. Bottom panel: the plots illustrate how demand interacts with supply in the housing market to set house prices in two scenarios: with housing demand unrestricted by credit supply (left-hand plot) and with housing demand restricted by credit supply (right-hand plot). 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 plot represents the limit on housing demand constrained by credit supply.

negative and identify the elasticity of demand. This means that if we find areas where $C > C^*$, we can estimate demand elasticity from the impact of severity on lending stock.

5.3. *Identifying Areas Where Courts Are Too Strict*

We use the interaction between credit and housing markets to empirically determine if court strictness is above or below the equilibrium level C^*. As discussed above (and detailed in Online Appendix A.3), we think of mortgage demand as derived from housing demand. However, the credit market also affects the housing market via credit supply, as limited credit supply can impose a limit on housing demand. The literature finds strong support for the impact of credit supply on house prices (Mian and Sufi, 2009; Szumilo, 2021). This means that the housing demand we observe in the data is either set by unconstrained housing demand derived from the utility maximisation (see Online Appendix A.4) or the maximum house price a household can finance when credit supply is constrained. We illustrate this point in the bottom panels of Figure 4, which show how house prices are set in those two cases. In the first case (left-hand 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 the second case (right-hand plot), credit supply imposes a cap on demand, corresponding to areas where severity is at $C^- < C^*$.

This distinction is relevant because it makes it clear that changes in the housing demand derived from changes in utility 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-hand plot: as most of the demand schedule is flat at the level constrained by credit supply, such a shift will have no impact on prices.[32] This is not the case in the left-hand panel where demand is unconstrained. Importantly, the right-hand diagram 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 the horizontal part of the constrained demand schedule—even holding constant the unconstrained housing demand side. Conversely, credit supply does not directly move unconstrained housing demand in the left-hand plot, and so has no effect on house prices.

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. In our empirical work, we exploit the changes in repossession procedures introduced by Lloyds Bank (see Online Appendix A.3). These changes drove the bank to reduce its credit supply to account for increasing expected losses on delinquent loans (see Online Appendix A.5 for more details of the credit supply shock). We therefore classify markets where this shock has a negative impact on house prices as limited by credit supply.[33] All remaining areas are instead treated as dominated by demand, which we exploit to identify a 'pure' demand effect of judges' severity in credit and housing markets.[34,35]

6. A 'Pure' Demand Effect of Courts' Strictness

6.1. *Boundary-Specific Effects of Strictness*

As highlighted by our theoretical framework, to capture a 'pure' demand effect, we 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 4).

To begin with, we document the heterogeneity in the price effect of severity across the boundaries in our sample. Areas where the effect is clearly negative are locations where demand

[32] 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.

[33] The fact that Lloyds Bank adjusted its credit supply in response to the regulator's action does not contradict our previous argument that banks do not adjust their offer locally, depending on court severity. Stated differently, while it can be expected that banks—and specifically Lloyds—react to a nationwide decrease in the likelihood that a repossession case is successful, the institutional settings in which they operate make it very unlikely that they react to geographical differences in courts' severity across group boundaries.

[34] This shock also possibly increased demand due to insurance effects—borrowers knew that they were less likely to be taken to court in the case of a delinquency. This means that, in areas dominated by demand, prices possibly increased. Empirically, we classify supply-dominated areas as those where the shock had a negative impact on prices, and demand-dominated areas as those where the impact was non-negative. We return to these issues below.

[35] This dichotomous characterisation of the local lending and housing markets abstracts from within-area, across-borrower heterogeneities. It is possible that even within areas that we identify as demand constrained a subset of individuals respond to an increase in the supply of credit, i.e., for a subgroup of individuals, demand might not be constrained by judges' severity. In this case, our estimates for demand-constrained areas would be smaller than pure demand estimates because they would contain a supply reaction. To minimise this possibility, in our empirical work we progressively tighten our definition of demand-constrained areas. This does not affect our conclusions, suggesting that borrowers' heterogeneity might not be a meaningful issue.

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:

$$P_{i(cgbt)} = \alpha + \sum \beta_{C_b} C_{cgbt} \times B_b + \Lambda X_{i(cgbt)} + \sum_{d=1}^{3} \delta_d dist^d + \theta_t + \mathrm{B}_b + \Gamma_g + \epsilon_{i(gcbt)}. \quad (3)$$

Most terms have already been defined and the expression inside the first summation term denotes interactions between severity and boundary dummies, allowing for the estimation of a boundary-specific price impact of severity.

The results are presented in Online Appendix Figure A4. The top left-hand plot depicts estimates that come from a specification that controls for group fixed effects, while the right-hand plot controls for group-by-year fixed effects. The plots show that in most boundaries the price effect of severity is negative. This suggests that demand-side considerations dominate in our sample. Furthermore, the two sets of estimates are strongly correlated (bottom left panel)—with a raw correlation of nearly 0.75. This descriptive analysis suggests that most locations are demand dominated. However, it still does not reveal whether changes in severity as we cross boundaries entail only movements along the demand curve, or a mix of supply and demand (using the notation of our theoretical framework: movements from a low level of C^+ to a higher level of C^+, thus remaining above C^*, as opposed to moving from C^- to C^+; see Figure 4).

6.2. *Demand-Dominated Areas*

To find areas that are clearly demand dominated, we exploit the shock provided by the changes at Lloyds Bank (see Online Appendices A.3 and A.5) when the bank changed its policy of dealing with delinquencies to pursuing only obvious cases. Taking fewer cases to court meant that the bank was likely to face bigger losses when a loan was delinquent. To protect its profitability, the bank reduced its supply of credit and only approved safer loans. This resulted in a negative credit supply shock.[36] However, when the probability of being repossessed decreases because fewer cases are taken to court, demand should increase (according to the insurance channel highlighted in our theoretical framework). So, 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 4 (Sections 5.2 and 5.3) 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 non-negative (positive or null) impact on markets where demand is pent up by $C^+ > C^*$. We label these areas as demand constrained for which the impact of severity on prices should represent the true elasticity of housing and credit demand to court strictness. Empirically,

[36] Although our data on the supply side (credit provision) are very limited, Online Appendix Figure A5 shows empirical evidence consistent with a reduction in supply by Lloyds after the change in policy. This is consistent with the existing literature on supply-side reactions to borrower protection (Pence, 2006; Dagher and Sun, 2016).

we estimate this regression;

$$P_{i(cgbt)} = \alpha + \sum \lambda_b B_b \times Lloyds^{2015}_{i(cgb)} \times I(\mathrm{T} \geq 2015) + \Lambda X_{i(cgbt)} + \sum_{d=1}^{3} \delta_d dist^d + \theta_t + \mathrm{B}_b + \Gamma_g + \epsilon_{i(gcbt)}, \quad (4)$$

where $Lloyds^{2015}_{i(cgb)}$ 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).[37] Note again that lending data are available at the postcode sector level, so we assign the same Lloyds exposure index to all postcodes belonging to the same postcode sector.

The bottom right panel of Online Appendix Figure A3 presents the association between boundary-specific estimates of the price effect of severity and corresponding estimates of the impact of the Lloyds shock on prices (coming from (4)). This diagram shows that there are areas where the Lloyds shock had a negative effect on prices. As discussed, such a negative effect should only occur in locations where housing demand is limited by credit supply. Conversely, areas where the impact of the shock is non-negative should be dominated by demand.[38] The scatter plot also reveals that areas with a positive price effect of the Lloyds shock are areas where the price impact of severity was more negative, supporting our intuition that these locations are demand dominated. 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 strictness.

6.3. *Model-Informed (Structural) Demand Estimates*

Equation (4) 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. 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 regression

$$P_{i(cgbt)} = \alpha + \beta^D_C C^D_{i(cgbt)} + \beta^S_C C^S_{i(cgbt)} + \Lambda X_{i(cgbt)} + \sum_{d=1}^{3} \delta_d dist^d + \theta_t + \mathrm{B}_b + \Gamma_g + \epsilon_{i(gcbt)}, \quad (5)$$

where $C^D_{i(cgbt)}$ denotes severity in demand-dominated locations where the impact of the Lloyds event is non-negative and $C^S_{i(cgbt)}$ indicates the severity index in areas that are supply constrained. The parameter β^D_C pins down our structural (model-informed) estimates of the elasticity of demand for housing and credit with respect to judges' severity.

In Table 5, we tabulate results from estimating (5). Column (1) presents results where the first step used to estimate the boundary-specific Lloyds credit shocks (see (4)) 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 around -5%. In areas where Lloyds had non-positive effects, prices

[37] Our empirical specifications also include two-way interactions between post-2015 indicator and boundary dummies, and between Lloyds' lending initial exposure and boundary dummies. These are not added to (4) for notational simplicity.

[38] We find that the mean/median of the Lloyds shock impact distribution are 0.164/0.114. Additionally, around 35% of the areas have negative effects, with the remaining showing positive impacts.

Table 5. *Model-Informed (Structural) Pure Demand Effects.*

	Positive Lloyds shock effect		Lloyds shock effects above the median			
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Log of prices	Log of prices	Log of prices	Log of prices	Log of advances	LTV ratios
Severity—demand-constrained boundaries	−0.050	−0.047	−0.050	−0.039	−0.032	0.000
	(0.012)***	(0.011)***	(0.012)***	(0.015)***	(0.009)***	(0.002)
Severity—non-constrained boundaries	−0.008	−0.006	−0.016	−0.018	−0.017	0.003
	(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

Notes: The table reports coefficients and SEs in parentheses (clustered at the boundary level) of a regression of the log of house prices, the log of advances or the LTV on judges' severity index (CC level, yearly) with controls as detailed in the table. Lloyds shock refers to the lending supply contraction caused by the fine imposed on Lloyds in 2015 (see the main text and Online Appendix A.5 for more details). The regression in column (1) uses estimates of the Lloyds Bank shock that come from a first-step specification that does not control for judges' severity. Estimates of Lloyds' shocks used in all other columns 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) to (6) 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% and 5% levels.

and judges' severity are unrelated. The difference between these two coefficients is statistically significant. Column (2) presents similar findings using estimates of the Lloyds shock that control for judges' strictness (in (4)). Once again, we find that our estimate of the 'pure' demand effect is negative and sizeable—at approximately −4.5%—and significantly different from the −0.6% estimate for supply-dominated areas.

Using country-level averages, we can further illustrate the magnitude of the effect. Around 71,000 cases went to court every year (approximately 1% of all mortgage loans) in our sample period. Of these, nearly 20% ended with a repossession. On average, a one-SD increase in severity (10 percentage points) would have translated into 7,600 more houses being repossessed every year (representing approximately 1% of the yearly volume of transactions). As noted in Section 2.3, this translates into a change in the probability of a repossession over the lifetime of a typical mortgage from 5.77% to 8.76%. As discussed in the introduction, this magnitude is comparable to the results from US studies that focus on the response of credit supply to borrower protection (Pence, 2006; Dagher and Sun, 2016).

We tested the robustness of our results along many 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 the boundary-specific price effects of the credit contraction. Results are reported in column (3) and yield virtually identical estimates. Second, we used group-by-year fixed effects in the estimation of (4) and (5). 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. Finally, we considered a discrete Lloyds shock in (5), where we replaced the incidence of Lloyds at the local level with dummies for locations where Lloyds' penetration is above 33% of the local market mortgage share (median level of penetration). This did not affect our findings.

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 that used in column (4). Consistently with our theoretical framework, the impact of severity on mortgage quantities mirrors the effect we detect on house prices.

7. Quantifying Our effects: A Policy Counterfactual Exercise

In this section, we provide a quantitative assessment of our estimated effects by performing a counterfactual in which we make judges 'softer'. Locally, this is a simple policy to implement as it could be achieved by allowing judges to travel across groups. Changing severity for the whole country would also be relatively simple as it would only need a revision of the rules on repossession proceedings and restrictions on how much discretion judges have. Details of our calculations are provided in Online Appendix A.6.

We find that 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. Higher transaction values would increase the revenue from transaction taxes. Under the post-2015 tax regime, this increase would be around £354 million per annum.[39]

Importantly, 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 £171 billion of housing wealth (total capitalisation of the housing market) to the economy.[40] 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 via refinancing. Cloyne *et al.* (2019) showed that the elasticity of this type 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. Applying our treatment to the stock of mortgage loans in each treated area suggests that mortgage stock would increase by a total of £6.2 billion (0.7% increase in total UK mortgage stock).[41] This estimate is highly relevant as this wealth could be used to finance non-housing consumption.

While the policy would have clear advantages for homeowners, it would also 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

[39] Stamp duty tax thresholds that created bunching were abandoned in 2014 and new rules were 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.

[40] 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. An LSOA is a census geography of around 1,500 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 in that year.

[41] 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.

would also encourage taking more debt at a time when household debt levels are already high. These considerations are particularly relevant in the UK context where housing affordability is a topical issue—most likely caused by frictions in housing supply (Hilber and Vermeulen, 2016). We therefore could only envisage this policy if implemented alongside interventions targeted at removing long-standing supply-related housing market failures.

An alternative policy would be to ensure that mortgage prices can accurately reflect severity. This could be done by dealing with any information and institutional bottlenecks that prevent lenders from accessing data on severity, and acting on it by differentiating mortgage costs to reflect repossession risk. Allowing mortgage rates to adjust to severity would however not nullify the friction that exogenous or 'sticky' court severity causes in housing and mortgage markets—instead, this effect would be transmitted through interest rates (Severino *et al*., 2024).

8. Conclusions

Our paper offers new empirical evidence in support of the demand response to borrower protection advocated by Ganong and Noel (2020) and suggests that the theoretical claims of Dávila (2020) that the demand side can be ignored may not be realistic.

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. Notably, while softer judges would make mortgage credit more accessible, such a change would translate into a decrease in housing affordability as measured by price-to-income ratios. Such an effect would be similar to the impact on credit and housing markets of one of the current UK flagship housing finance policies—the help-to-buy scheme. While this policy encourages borrowing (Szumilo and Vanino, 2021), it also increases house prices (Carozzi *et al*., 2020).

Importantly, while prices would rise in response to reducing severity at the margin, loans could become smaller, and prices 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 overly difficult to repossess a delinquent loan. This insight links our paper to research on the US mortgage market where loan sizes are larger when courts favour the lender (Pence, 2006; Dagher and Sun, 2016).

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

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