

# THE IMPACT OF REPOSSESSION RISK ON MORTGAGE DEFAULT.

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## Abstract

I study the effect of removing repossession risk on a borrower's decision to default on their mortgage payments. Reducing default costs may result in strategic default, particularly during crises when homeowners can be substantially underwater. I analyze difference-in-differences variation in repossession risk generated by an unexpected legal ruling in Ireland that prohibited collateral enforcement on delinquent residential mortgages taken out after a particular date. I estimate that borrowers defaulted by 0.3 percentage points more each quarter after the ruling, a relative increase of approximately one half. In addition to a high loan-to-value ratio, low liquidity is associated with a larger treatment effect, suggesting both equity and consumption-based motivations for the strategic defaulters.

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# 1 Introduction

Debt relief policies that reduce repossession risk for mortgage borrowers, such as foreclosure moratoria, are often considered by governments during economic crises because of the negative externalities and social disruption of widespread evictions.<sup>1</sup> However, these policies come with a potentially large moral hazard cost. By lowering the costs of default, reducing repossession risk makes default more attractive. This association may be particularly strong during recessions, when underwater homeowners have strong incentives to default. Absent repossession risk, borrowers who would have otherwise carried on paying now default, substantially reducing the net benefits of the policy.

In this paper, I provide estimates of the magnitude of the impact of removing repossession risk on default, by examining a recent natural experiment that took place in Ireland. Clean estimates of the moral hazard costs of debt relief policies such as foreclosure moratoria are typically rare. Since restricting debt relief to a particular group of borrowers partially negates its benefits, natural experiments are hard to come by. Though foreclosure moratoria were widely used across the United States during the Panic of 1819 and the Great Depression, (Alston, 1984; Skilton, 1943) and also less extensively during the recent Great Recession internationally (Gabriel et al., 2017; Artavanis and Spyridopoulos, 2018), so far it has not been possible to find situations in which both the moratorium applies differentially to similar borrowers and the necessary data is available.

Ireland, however, offers an interesting setting to study this question. Owing to a poorly drafted change in Irish law governing property transfers and its later unexpected discovery during a court case, the repossession regime of existing mortgage contracts was retroactively altered. Specifically, in July 2011 a judge ruled that properties mortgaged before December 2009 could no longer be repossessed in the event of default.

To estimate the impact of repossession risk on mortgage default, I compare the performance of two loan vintages separated by this cut-off date, both before and after the legal ruling (known as the “Dunne judgment”). From regulatory panel data, I use a sample of loans originated in a 180-day window around the cut-off date and construct matched treatment and control groups to obtain the difference-in-differences variation in repossession risk. The groups are similar on observables before the Dunne judgment, which is consistent with the necessary exogeneity of the origination cut-off date.

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<sup>1</sup>A recent literature from the United States foreclosure crisis shows large negative externalities of foreclosures, including on house prices (Campbell et al., 2011; Mian et al., 2015); underinvestment in the housing stock (Melzer, 2017); and aggregate demand externalities (Mian et al., 2015).

Although both groups follow parallel trends before the ruling, their default rates diverge markedly afterwards, with the largest effect observed in the quarter immediately following.

I estimate that the Dunne judgment increased the quarterly default rate by 0.3 percentage points. In the control group, a 0.3 percentage point higher default rate is associated with variation between the median loan-to-value ratio (LTV) plus thirty eight percentage points, or the median interest rate plus two percentage points. When scaled by the estimated counterfactual, this translates to a relative effect size of between 40% and 60%.<sup>2</sup>

To gauge the robustness of the result, I estimate a variety of models under different assumptions about confounding factors. Alongside a straightforward difference-in-differences regression, I include several specifications with additional borrower and loan covariates, and high-dimensional fixed effects. Since treatment status is determined by a loan’s origination date, loan vintage is a nuisance parameter introduced by my research design. I therefore also estimate models that adjust for potentially confounding cohort effects, by including vintage-calendar time parameters. The significance of the treatment effect is robust to these different specifications. I show graphical evidence consistent with excess default only for loans issued before the cut-off date, though a regression discontinuity specification fails to rule out a null treatment effect. The treatment effect is also statistically significant in a permutation exercise, in which I simulate the null distribution of the treatment effect with one thousand placebo analyses.

Standard models of mortgage default, known as “double-trigger” theories, demonstrate that borrowers will default if the intrinsic financial value of the mortgage does not exceed the costs of defaulting.<sup>3</sup> In fact, recent empirical research finds that large costs are required to match the aggregate or available micro data from the US default crisis (Gerardi et al., 2015; Schelkle, 2018; Laufer, 2018). Substantial default costs therefore act as a barrier against mortgage defaults during a recession, when prices and/or incomes fall.<sup>4</sup> Default costs are both pecuniary and non-pecuniary in nature (Guiso et al., 2013). Pecuniary costs include future rental payments rent and losing a permanent hedge against

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<sup>2</sup>Table 2 contains a range of absolute effect sizes.

<sup>3</sup>The intrinsic mortgage value comprises the option value of repayment - reflecting the expectation of investment profit from potential positive equity in future and the ability to default in future - minus the mortgage payment amount. Campbell and Cocco (2015) provide a quantitative model of default grounded in optimisation behaviour by borrowers. Schelkle (2018); Laufer (2018) provide structural models. See Fuster and Willen (2017) for a straightforward discussion and Foote and Willen (2018) for a review of the state-of-the art and historical development of mortgage default models.

<sup>4</sup>Recounting US government deliberations on debt policies in response to the Great Recession, Goolsbee (2014) recognizes the importance of default costs in acting as a wedge between the intrinsic value of a mortgage and its value to the homeowner.

future rental payments (Sinai and Souleles, 2005), while the loss of utility derived from a customized home, a social network and local amenities are non-pecuniary costs of default. A borrower experiences all of these costs from loss-of-ownership. Removing or substantially reducing the risk of repossession may therefore increase delinquency rates during a crisis, when the financial value of mortgages may be substantially diminished.

The main contribution of this paper is to provide clean estimates of the default response of borrowers to a large reduction in one of the most important default costs: repossession risk. Empirical estimates of the sensitivity of strategic behaviour to default costs are important for understanding the costs of foreclosure moratoria, but also for debt relief policies that influence default costs more generally. Evidence about the effects of such programs is particularly important in light of the experience of the Great Recession, when efforts to alleviate household financial crises were hampered by financial-intermediation frictions.<sup>5</sup>

The closest paper to mine is that of Mayer et al. (2014a), who find a substantial strategic response to a modification program. Agarwal et al. (2017a) find no evidence of strategic default in response to HAMP, but that policy was designed specifically to minimize incentives for strategic behavior. Ghent and Kudlyak (2011) use cross-state variation in recourse status, which is largely due to historical precedent, and find positive impacts of non-recourse mortgages on default. Conversely, Collins and Urban (2015) find no effect of an eight month foreclosure moratorium on default.

My data also allow me to estimate the relevant margins of the excess default. Identifying the reasons for strategic default is also important because policymakers might be willing to accept the moral hazard cost in order to target debt relief to a specific group.

The double trigger model clarifies two reasons why borrowers default more often when default costs fall. First, borrowers default at a lower home-equity threshold under smaller default costs. In a purely financial sense, paying the mortgage on an underwater home is rational if the possibility of positive equity in the future justifies the interest payment today. Some borrowers who were previously paying an underwater mortgage did so, not because of this option value of repayment, but because of the high costs of default. Absent these default costs, the bet on future house prices does not warrant

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<sup>5</sup>A large recent literature has documented various frictions in financial intermediation that dampen the pass-through of monetary easing or hinder endogenous renegotiation. These frictions include a lack of market for negative equity refinancing; barriers to refinancing for the unemployed; frictions in loan modifications arising from information asymmetries, securitisation, and organisational constraints (Agarwal et al., 2017a; Beraja et al., 2019; DeFusco and Mondragon, 2018; Piskorski et al., 2010; Hurst et al., 2016; Adelino et al., 2013; Agarwal et al., 2011).

payment. Second, liquidity-constrained borrowers also default more. When default costs fall, these strategic defaulters are borrowers who choose to no longer sacrifice consumption to pay the mortgage and instead to default on their payments.<sup>6</sup>

To investigate the empirical relevance of these two margins, I analyze heterogeneous treatment effects. For a subset of loans, I link the borrower’s liquid account balance at the same bank and estimate triple difference regressions. In separate models using pre-treatment covariates, I find large treatment effects for both highly levered and low-liquidity borrowers. The treatment effect is approximately 1.5 percentage points in the lowest tercile of the liquid wealth distribution and 2 percentage points in the highest tercile of the LTV distribution. Borrowers with high liquid wealth or low loan-to-values show no pattern of excess default.

Therefore, a second contribution of my paper is to demonstrate that, in addition to equity-based motivations, liquidity constraints are an important margin of the decision to strategically default when costs fall. Though default costs act as an important wedge in the relationship between negative equity and default, they also inhibit a household’s ability to smooth consumption. In addition to correcting foreclosure externalities, a further goal of debt relief policies is to allow borrowers to correct their incomplete debt contracts and therefore smooth consumption in a crisis (Bolton and Rosenthal, 2002; Piskorski and Seru, 2018). For example, the Home Affordable Modification Program aimed both to reduce foreclosures and to stimulate consumption (Ganong and Noel, 2018). My estimates of the liquidity margin are consistent with a model in which borrowers with high marginal utility of consumption default in order to better smooth consumption. One implication is that these borrowers are likely to have high marginal propensities to consume (MPC) out of reduced repossession risk.

Finally, my results demonstrate using quasi-experimental variation that a household’s sensitivity to default is related to the costs of this decision. Estimates of the sensitivity of defaults to these costs are important in answering questions about how households make decisions about financial leverage (Bailey et al., 2019), which can ultimately play a role in the both the boom and bust of house price bubbles (Geanakoplos, 2010).

The rest of this paper is as follows. Section 2 discusses the institutional details of the natural experiment. Section 3 discusses the empirical research design and data used; section 4 introduces the

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<sup>6</sup>Fuster and Willen (2017) argue that a consumption-portfolio choice with constraints can replicate such behavior. Because of high marginal utility of current consumption, liquidity-constrained borrowers discount the investment payoff from the option a lot more than their unconstrained counterparts with an identical contract and similar default costs, and therefore act as if impatient.

regression specification and presents the main results and Section 5 discusses threats to identification and alternative modes of inference. Section 6 presents a discussion of the magnitude of the effect and external validity, and an analysis of heterogeneous treatment effects. Section 7 concludes.

## 2 The Dunne Judgment and the Irish Mortgage Market

It is not for the court to supply  
that which is not contained in the  
2009 Act.

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Justice Dunne, July 2011

### 2.1 The 2011 Dunne Judgment: a Natural Experiment

In early summer 2011, at the peak of the Irish crisis, several appeals against residential repossession proceedings were being heard before the High Court in Dublin. Irish house prices were down 50% from their credit-boom peak, unemployment tripled to 14%, and mortgage defaults and foreclosures were climbing. On the 25th July 2011, Ms Justice Dunne ruled in the case of *Start Mortgages Ltd & Ors v Gunn & Ors* that a 2009 Government Act intended to update Irish repossession law, was mistaken, and had unintentionally repealed repossession law. The landmark ruling effectively removed any risk of repossession for mortgage borrowers in the short run. Banks could no longer petition the Irish courts to enforce their rights to collateral repossession. Popular online media outlets were reporting on the judgment that night and major national newspapers in Ireland all carried the story the following morning (RTE.ie, 2011; McDonald, 2011; Gartland, 2011a,b).

Borrowers were aided in the appeals cases by a group of lawyers and business people called *New Beginning* and the group further helped to publicize the ruling. For example, RTÉ, the national broadcaster, released a story on its online news website the afternoon of the ruling.

Hundreds of other people who are in arrears with mortgages created before December 2009 could also be affected by the decision. New Beginning says it puts a question mark over the entire repossession system since 2009.

This is the third sentence of the piece (RTE.ie, 2011) and demonstrates clearly the ramifications of

the ruling for borrowers with mortgages originated before 1st December 2009, the cut-off date that I use for identification in this study.

The source of the legal lacuna begins with the *Land and Conveyancing Law Reform Act, 2009* (henceforth “Land Act”). The Land Act was passed by the Irish legislature in 2009 to replace older legislation used in repossession proceedings: the Registration of Title Act, 1964. Section 62(7) of the 1964 law was replaced by article 8 in the 2009 Land Act. Nearly two years later, a lacuna was discovered, when Justice Dunne noted in her ruling on the 2011 case that “those lenders who did not have an entitlement to apply for an order pursuant to s. 62(7) by December 1, 2009, *are not in a position to avail of the provisions of the 2009 Act to apply for an order of possession as their right to apply for such an order is not saved by the provisions of the 2009 Act*” (my emphasis). In other words, if a lender had not applied for possession of the collateral under the section 62(7) of the old law by 1st December 2009, they could not do so after. However, loans issued after this date were still in fact covered by the new 2009 law and lenders could still petition the court for repossession after Justice Dunne’s decision in 2011.

Newspaper and online evidence from the time suggests that the cut-off in the ruling was salient. As well as RTE.ie (2011), Gartland (2011b), writing in *The Irish Times* the following day, noted the cut-off in the third sentence: “The new Act was introduced on December 1st, 2009, and it only applied to mortgages created after that date, the judge found”. Following up the next month, Gartland (2011c) wrote about the immediate impact of the ruling, noting the *New Beginning* group’s claims of increased contact from borrowers after the judgment.

How was it that lenders continued to issue foreclosure notices against borrowers when the law was defunct? In response to the initial working paper of this study (O’Malley, 2018), a member of the *New Beginning* group, Andrew Robinson, writes in the *Irish Independent* that lenders were in such haste to issue foreclosures in 2010 and 2011 that they failed to take adequate care in issuing the pleadings (Robinson, 2018). He claims that Irish banks’ strategies for dealing with the rising defaults at the time was to issue tenders for high-volume, low-price work and the resulting lack of attention to detail meant that nobody noticed the legal lacuna. Robinson (2018) claims that lenders at the time “sought an illegal order when a cursory check of their pleadings would have uncovered the error”.

Although it is hard to quantify how many foreclosures were avoided by the ruling, the flow of repossessions was halted. Data from the Irish courts shows that as the stock of mortgages in default

continued to head towards 150,000 (roughly one-in-eight outstanding mortgages), the number of orders *granted* for possession fell in 2011 and continued to fall while the lacuna was in place (Arthur Cox, 2016).

Unlike the majority of their U.S counterparts, Irish mortgages are recourse in nature. Lenders can therefore pursue borrowers for any residual sums owed after they have taken possession of the collateral. This fact, taken together with the uncertainty surrounding the length of the repossession moratoria means that a borrower’s repossession risk was not entirely removed. It seems likely that borrowers understood that the ruling was in some way temporary and that legislation would be enacted to repair it. Commenting on the latter prospect, O’Neill (2011) writes:

There have already been some indications of possible appeals of Ms Justice Dunne’s decision. However, such an appeal will not occur quickly and, in any event, the decision may very well be upheld – leaving aside its dramatic consequences, *prima facie*, it appears well reasoned.

The loophole was closed two years later, when in July 2013, a new Land and Conveyancing Law Reform Act 2013 was signed into law by the legislature. After which, the number of repossession cases in the Circuit Court nearly quadrupled from 2013 to 2014 (Arthur Cox, 2016).

### 3 Data and Sample Selection

The data are a panel data set of loans from the Central Bank of Ireland’s loan-level database, collected for stress testing the major Irish lending institutions during the recent financial crisis. The data cover four large banks, collectively covering about two thirds of the Irish mortgage market at the time. The data are observed quarterly from one year pre-judgment to one year post: eight quarters in total from October 2010 to July 2012. At each quarter, the data contain current information on loan performance, estimated loan-to-value ratios, interest rates and product types. The data also contain detailed information recorded at loan origination, including the origination date and details about the borrower, their total income, their NUTS3 region of residence and their year-of-birth.<sup>7</sup>

As part of the research design, I limit the sample to loans originated 180 days either side of the

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<sup>7</sup>Nomenclature des Unités Territoriales Statistiques (NUTS) is the European Union geocoding standard. There are eight NUTS3 regions in Ireland, of which Dublin Region is the largest.



cut-off date specified in the Dunne judgment. I also limit the sample to mortgages taken out by homeowners rather than investors. All loans in the sample are ‘primary-loans’, meaning they have no secondary loans attached to them, nor are they equity release loans from a borrower’s larger portfolio which made up roughly one-third of loan originations during the period (Irish Banking Federation, 2010). If a control-group loan was originated for equity release or as part of a larger loan portfolio, then the effect of the Dunne judgment on the borrower’s incentives is less clear.

Since the assignment of loans to treatment and controls groups is not truly random, I use a matching algorithm to make the two groups observably similar at the first observation. Though the identification is relatively clean, there nonetheless remains the chance that the two groups are not comparable on average pre-treatment. I apply a two-step greedy matching procedure, first estimating a treatment-propensity score as a logistic function of observable factors associated with loan default, and then dropping observations that do not receive a nearest-neighbor match in the second stage (Imbens and Rubin, 2015). Coupled with regression adjustment in Section 4, this matching procedure ensures the estimates are doubly robust (Imbens, 2004).<sup>8</sup> The results without matching are qualitatively similar.

The data set used for the remainder of this paper is composed of loan-time 80,272 observations. Of 7,913 loans, 4,488 are no longer subject to repossession after the Dunne judgment.

### 3.1 Summary Statistics

Table 1 shows both the overall mean and group means of relevant characteristics of the loans. As the loan sample is defined narrowly and matched, there are few meaningful differences and most are mechanical and due to the treatment group loans being older by definition. For example, treatment loans have lower outstanding balances, the borrowers are older and they have a lower term to maturity. The treatment group do have slightly higher interest rates on average, but this translates into only small differences in the average monthly installment.

Table 1 also shows the proportions of discrete variables by treatment and control groups. Treatment and control observations are generally balanced across regions, but control observations are more likely

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<sup>8</sup>Bank, interest rate, interest rate type (fixed, variable), original loan-to-value ratio, income and NUTS region are the factors used in matching. I use the R package *MatchIt* to perform matching (Ho et al., 2011). Greedy matching entails matching observations without replacement. It matches the first treatment observation to a control and then continues to the next unmatched treatment observation, ignoring the effect of the previous step on potential subsequent matches. The algorithm therefore does not attempt to optimally match observations between groups, but is easier to implement and the loss over optimal matching is usually small (Imbens and Rubin, 2015).

Table 1: **Summary statistics.** This table shows mean values of continuous control variables (top pane) and proportions in percent of discrete control variables (bottom pane). Estimates are computed on data at the time point immediately preceding the Judgment (March 2011). Also shown is the key variable dictating treatment status: distance from cut-off. This variable measures the number of days between loan origination date and the 1st December cut-off date.

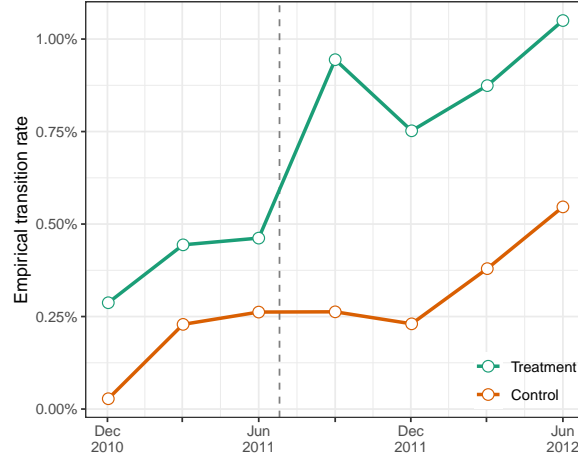
	Mean		
	All	Control	Treatment
Treated	0.50		
Interest Rate (%)	4.11	3.89	4.33
Loan-to-value Ratio (%)	77.42	78.59	76.25
log(Income)	10.95	10.96	10.95
Borrower 1 Year-of-birth	1973	1973	1972
Outstanding Balance (€)	176,080.10	180,299.20	171,861.10
Term Remaining (months)	276.26	283.38	270.12
Monthly installment (€)	952.54	939.62	965.49
Distance from cut-off (Origination days since 1st December 2009)	-4.12	88.9	-97.2
	Proportion (%)		
	All	Control	Treatment
Fixed Interest Rate	40.34	55.56	25.11
First-Time Purchaser	49.57	53.06	46.07
NUTS3 Region			
Border	10.64	9.56	11.71
Dublin	30.01	32.81	27.22
Mid East	13.64	13.49	13.79
Mid West	9.65	9.12	10.18
Midlands	5.03	4.71	5.36
South East	8.76	8.09	9.43
South West	13.73	13.76	13.71
West	8.54	8.48	8.61

to be first-time home purchasers. There is a considerable imbalance between fixed and variable interest rates between the two groups however, and if not statistically controlled for would be a valid threat to identification.

## 4 The Impact of the Dunne Judgment on Mortgage Default

To test whether the removal of repossession risk after the Dunne judgment had a causal impact on mortgage default, I model the default (flow) outcomes of the two groups over both the pre- and post-judgment time periods. The outcome of interest is the default-transition of loan  $i$ , in treatment group  $j$  at time  $t$ .

Figure 1: **Empirical default rates by group.** This figure plots the empirical default rates over time, by treatment group. Data are quarterly transition-to-delinquency rates. The dashed vertical line indicates time of Dunne judgment ruling. Treatment groups are loans originated up to six months before the the cut-off date specified in the judgment. Control group are loans originated within the six months afterwards.



$$\text{default}_{ijt} = (90\text{DPD}_{ijt} = 1 \mid 90\text{DPD}_{ijs} = 0 \text{ for all } s < t)$$

At each time point, the dependent variable takes the value 1 if the loan *transitioned* to non-performing status since the last observation. Non-performing status is defined as the standard Basel definition of ninety-days past due. I define the transition to default as an absorbing state. Therefore once a loan is coded in default, it drops out of the sample thereafter.

Figure 1 shows the empirical default rates by group over the sample period. Following parallel trends before the judgment, the default rates then diverge. The treatment group default rate rises by roughly half a percentage point in the first period after the ruling, consistent with a causal impact of removing repossession risk on mortgage default. The default rate for the treated loans remains elevated for the remainder of the sample period.

#### 4.1 Regression Model

To test whether the judgment had a statistically significant impact on the default rate and to adjust for potentially confounding variation, I estimate a series of regression models. The models are variants of

the panel difference-in-differences specification in Eq 1, in which  $\beta^{DD}$  is the treatment effect of interest, to be estimated from the data. Under the assumption that the error term is exogenous conditional on the model covariates,  $\hat{\beta}^{DD}$  is the causal impact of the Dunne judgment on default and an estimate of the sensitivity of mortgage default to the removal of repossession risk.

$$P(\text{default}_{ijbgrt}) = \alpha + c_i + \beta^{DD}(\text{Treatment}_j \times \text{Post}_t) + \mathbf{X}'_{it}\Psi + \phi_{rt} + \tau_{bgt} + \epsilon_{ijbgrt} \quad (1)$$

The specification assumes that the probability of mortgage default for loan  $i$ , in treatment group  $j$ , of product type  $g$ , issued by bank  $b$ , to borrower in region  $r$ , observed at time  $t$  is an additive function of a loan effect that controls for the time-invariant risk of each loan; a matrix of loan characteristics that are associated with loan default (e.g. negative equity); several group-time fixed effects; an idiosyncratic loan-level error term and an additive treatment effect for the impact of the Dunne judgment ( $\beta^{DD}$ ). Identification of  $\beta^{DD}$  relies on a comparison of the within-loan variation in default outcomes over time, between the treatment and control groups. This specification also assumes no effect of origination date on loan default beyond the effect absorbed by a treatment group or loan dummy. This is equivalent to assuming that loans issued at different times have a constant default probability differential, an assumption which I relax in further specifications by controlling for a linear time trend interacted with month of origination dummies. In that case, identifying variation comes from deviations from this trend in the post periods, between treatment and control loans.

The group compositions are generally still imbalanced in product types and this may present a threat to valid causal inference (Table 1). For example, the 2011 ECB interest rate hikes may be more likely to be passed-through to the treatment group and confound the estimated treatment effect. In the Irish market, because banks retain the option to change interest rates on variable rate loans rather than maintain a fixed margin, pass-through is not complete. I therefore also estimate specifications containing a product type-bank-time fixed effect. This term non-parametrically controls for any confounding variation coming from changes in bank-specific interest rate policies by isolating the within bank-product type variation. Intuitively, by comparing default rates for variable-rate mortgages in Bank  $A$  at time  $T$  across treatment and control groups, this variation should not bias the estimate. I also control for the level of the interest rate in several specifications through the main effect or the borrower's payment amount, though there is little remaining variation in specifications featuring this

high-dimensional fixed effect. To account for within-loan correlation in the error term, I cluster all specifications at the individual loan level.

## 4.2 Regression Results

Estimation results of various derivative specifications of Equation 1 by ordinary least squares are presented in Table 2. Column 1 shows results from a model with only the basic ingredients of the difference-in-differences model: an intercept, a treatment group dummy, a post period dummy and the interaction of the two. In each subsequent column, I add or subtract various combinations of covariates and fixed effects to gauge how robust the estimates are.

Starting in specification (6), I allow the baseline transition rate to vary with vintage by first adding origination-month fixed effects. In further permutations, I add vintage effects such as a linear trend for each monthly cohort (7-8). Specification (9) shows the results while including the high dimensional bank-product type-time fixed effects. Specifications (10) and (11) add a linear interaction of origination date and calendar time, (12) adds vintage month dummies, (13) adds a linear origination date term interacted with the post dummy and the final specification contains a linear interaction between vintage and calendar time.

Across all specifications, the difference-in-differences (DD) coefficient is shown in the first row and is statistically significant from zero at at least the 5% level for all specifications bar the final two, which are significant at 10%. The estimated treatment effect is roughly 0.3 percentage points for most specifications. The estimates of 0.5, 0.43 and 0.48 percentage points in specifications (8), (11) and (12) respectively are higher than the rest. According to this evidence, the Dunne judgment had a causal impact on the estimated counterfactual default rate of approximately 0.3 percentage points. Using the parameters of specification (1) to compute the counterfactual in the post period, this translates to a relative effect size of around 60%.

The initial gap in the default rates between treatment groups in Figure 1 is a concern, since we would not expect to see such a difference between loans issued so close together. The results in table 2 suggest that either these loans were observably worse on origination characteristics or were worse credit risk on unobservable characteristics. For example, the treatment parameter which measures the pre-period average difference in default rates is statistically greater than zero in the baseline specification but becomes zero once either covariates are adjusted for or the baseline hazard is allowed to vary

Table 2: **Difference-in-difference regression** results. Each column shows results from a specification related to Equation 1, labeled (1) to (14). The bottom rows of each pane show which fixed effects are included in each specification. If a cohort trend is included in the model, the bottom row also signifies the type. Treated  $\times$  Post is the coefficient of interest ( $\beta^{DD}$ ) and is isolated in the first row of both panes. The remaining estimates are coefficients on control variables. Loan-clustered standard errors are shown in parentheses.

	Default					
	(1)	(2)	(3)	(4)	(5)	(7)
Treated $\times$ Post	0.0034*** (0.0009)	0.0036*** (0.0009)	0.0038*** (0.0009)	0.0038*** (0.0009)	0.0033*** (0.0009)	0.0033*** (0.0009)
Treated	0.0021*** (0.0006)	0.0007 (0.0006)	0.0006 (0.0007)	0.0006 (0.0006)	0.0022*** (0.0006)	
Post	0.0016*** (0.0005)	-0.0001 (0.0005)	0.0004 (0.0005)	0.0005 (0.0005)		0.0016*** (0.0005)
Installment (€)		-0.000001 (0.000001)				
Loan-to-value (%)		0.0001*** (0.00001)				
Negative equity		0.0007 (0.0008)	0.0037*** (0.0007)	0.0037*** (0.0007)		
$\log$ (Outstanding balance)			0.0042*** (0.0005)	0.0041*** (0.0005)		
Borrower 1 year-of-birth		-0.0002*** (0.00004)	-0.0002*** (0.00004)	-0.0002*** (0.00004)		
$\log$ (Income at origination)		-0.00003 (0.00007)	-0.0024*** (0.0006)	-0.0024*** (0.0006)		
Interest rate			-0.0008 (0.0007)	-0.0011** (0.0005)		
Above median interest rate			0.0011 (0.0008)			
Variable rate		0.0038*** (0.0005)	0.0038*** (0.0005)	-0.0053* (0.0027)		
Variable rate $\times$ interest rate				0.0023*** (0.0007)		
<i>Fixed effects</i>						
Region		Y	Y	Y	Y	Trend
Time						
Vintage (month)						
Observations	80,259	80,084	80,251	80,251	80,255	80,259
	(8)	(9)	(10)	(11)	(12)	(13)
Treated $\times$ Post	0.0050** (0.0020)	0.0029*** (0.0009)	0.0035*** (0.0014)	0.0043*** (0.0015)	0.0048** (0.0020)	0.0035* (0.0019)
Treated	0.1149 (0.1298)	0.0013** (0.0006)	-0.0001 (0.0012)			
Installment (€)						
Loan-to-value (%)						
Negative equity						
$\log$ (Outstanding balance)			0.0042*** (0.0005)	0.0041*** (0.0005)	-0.00003*** (0.00001)	-0.00003*** (0.00001)
Interest rate			-0.0008 (0.0007)	-0.0011** (0.0005)	0.0007*** (0.0001)	0.0007*** (0.0001)
Above median interest rate			0.0011 (0.0008)		0.0021 (0.0014)	0.0022 (0.0014)
Variable rate						
Origination date			-0.0001 (0.0003)		0.0041 (0.0031)	0.0044 (0.0032)
Origination date $\times$ time			0.0000 (0.000000)			
Origination date $\times$ post				-0.000000** (0.000000)	-0.00002** (0.00001)	
<i>Fixed effects</i>						
Loan				Y	Y	Y
Time	Y	-		Y	Y	-
Bank-Product-Time		Y		Y	Y	
Vintage (month)						
Vintage (Linear)-Time				Y	Y	Y
Observations	80,259	80,259	80,259	80,259	80,259	80,084

Note: \* p<0.1; \*\* p<0.05; \*\*\* p<0.01

with vintage effects. In both cases, the estimated DD parameter is stable and statistically significant. The remaining specifications argue against this gap biasing the treatment effect. If the treatment group would have diverged in the counterfactual scenario regardless of treatment, due to unobserved differences, we would expect the vintage trends to absorb this. For example, in specifications with cohort effects, the treatment effect is identified from deviations in the pre-period trends. There is no reason why deviations from trend should occur for only the treatment group.

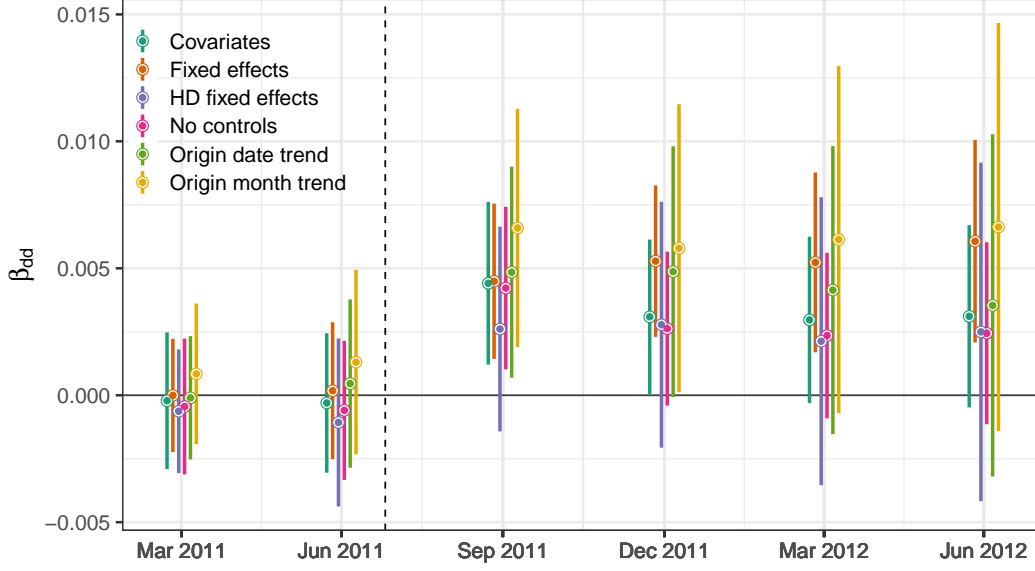
### 4.3 Event Study Specification

The previous section describes how the difference-in-differences specification computes the counterfactual outcome of the treatment group using the post-treatment outcome of the control group; and what assumptions are needed to identify the causal impact of the judgment on default. However, a natural analysis of the research design is to compare default rates between treatment-control groups at each time period, rather than just between the pooled pre- and post-judgment periods. The key implication of the hypothesis of this paper is that default rates should diverge only after the time of the judgment: July 2011. An event study model differs from Equation 1 in one key aspect: the treatment<sub>g</sub> × post<sub>t</sub> parameter is replaced by a full set of treatment and observation quarter interactions. This estimator allows free coefficients for each time period’s treatment effect: periods before the judgment should show no difference in outcomes from the first-period difference between treatment and control groups. Though this estimator will be less efficient than Equation 1, it provides a test of the parallel trends assumption in the standard model. Of course, parallel trends in the post period cannot be tested, but this model can provide evidence against it.

$$\begin{aligned} \text{default}_{ibfgrt} = & \alpha + c_i + \sum_{t \neq \text{Dec 2010}} \left\{ \beta_t \times \mathbb{1}(\text{quarter}_t) + \beta_t^{DD} \times \mathbb{1}(\text{quarter}_t) \times (\text{Treated}_g) \right\} \\ & + \mathbf{X}'_{it} \Psi + \phi_{r,t} + \tau_{b,f,t} + \epsilon_{ibfgrt} \end{aligned} \quad (2)$$

The hypothesis of interest here is that for all quarters before July 2011, the  $\beta_t^{DD}$  should be statistically zero and some subset of the  $\beta_t^{DD}$  coefficients in the post period should be statistically greater than zero. The estimated counterfactual for the treatment group is quite flexible in this model. The first period  $\beta^{DD}$  is constrained to be 0 by construction, and for all subsequent periods, the change in

Figure 2: **Event study coefficients.** This figure shows the coefficients on the interaction of calendar-quarter fixed effects and treatment dummy. In each time period, the point estimates come from six different specifications, labeled in the legend. 95% confidence intervals calculated using loan-clustered standard errors are also shown.



the control group's time trend is used to estimate the counterfactual change in the treatment group. Any deviation from this prediction is considered the causal impact of the judgment. To see why this is a strict test, consider that any significant deviation from the prediction in the two periods before the judgment will show up as a non-zero treatment effect.

The identifying assumptions from Section 4.1 carry over to here: there must be no unaccounted for group-time varying confounders. For this reason, this model inherits the same set of control variables and fixed effects as Equation 1. Inference is straightforward again and standard errors are clustered at the individual-loan level. The sources of variation in this model are similar to the previous pooled model: the treatment effect is identified from the within-loan default variation away from its sample mean, between the treatment and control groups at each time point.

The results of several event-study specifications are shown graphically in Figure 2. Each date on the x-axis shows the corresponding estimate of the non-parametric treatment effect with the vertical dashed line showing the time point of the Dunne judgment. Each  $\beta_{DD}$  point shows the difference in default rates from the first period difference. The treatment groups were evidently following parallel pre-trends that diverge in the first period after the ruling. Six specifications are shown along with a description in the



legend. The first post-period effect is generally the most precisely estimated and confidence intervals widen afterwards. However, the pattern is similar across specifications, demonstrating evidence that the treatment effect occurred only after the time of the ruling.

#### 4.4 Month-of-Origination Non-Parametric Treatment Effect

To further rule out competing explanations for the treatment effect, in a similar manner to the event study, I now allow the treatment effect to vary non-parametrically in the month-of-origination instead of in quarter-of-observation.<sup>9</sup> This test should reveal a significant treatment effect only for the months prior to December 2009, which was the cut-off month. Identification in this model is similar to the regression discontinuity design (RDD), in that the treatment effect is expected to jump discretely in the first month before the cut-off. Unlike the standard RDD, this hybrid model identifies the effect from the cross-sectional *difference* in the relationship compared to the December effect, after the event.

I estimate the following specification:

$$\text{default}_{iot} = \alpha_i + \sum_{o \neq \text{Dec 2009}} \left\{ \beta_o \times \mathbb{1}(\text{Origination}_o) + \beta_0^{DD} \times \mathbb{1}(\text{Origination}_o) \times \text{Post}_t \right\} + \epsilon_{iot} \quad (3)$$

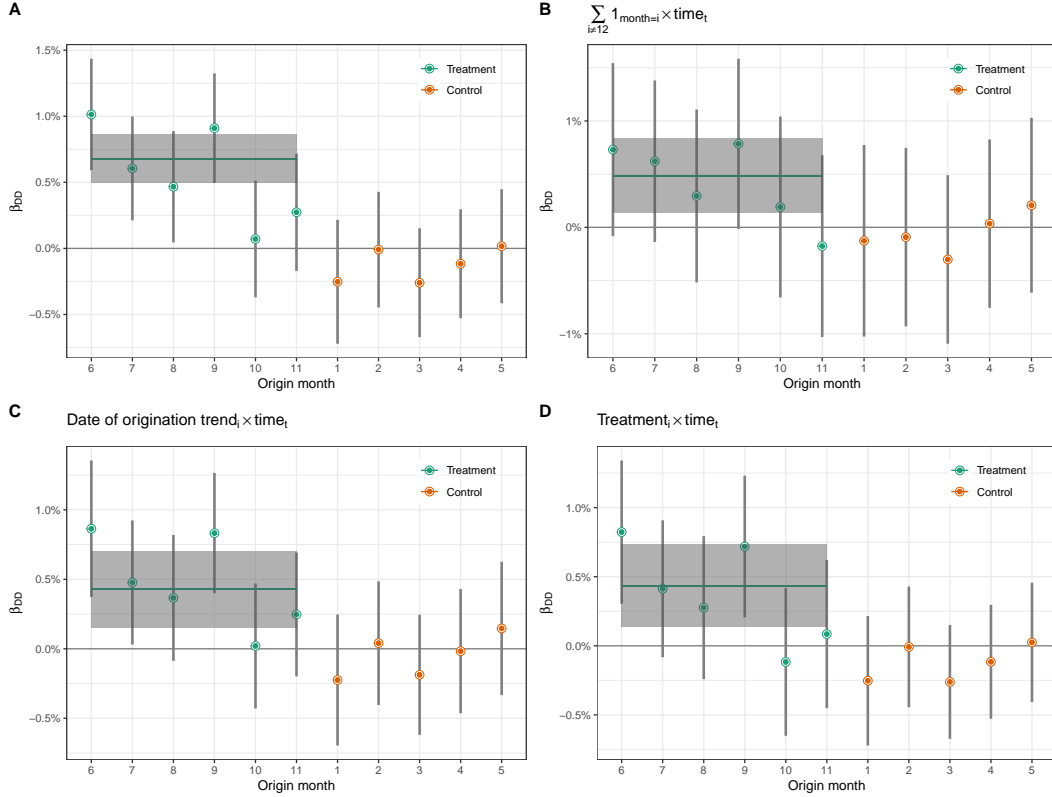
Estimates of the  $\beta_0^{DD}$  coefficients from four specifications are shown in Figure 3, along with the equivalent pooled effect from each specification. The patterns are generally consistent with the treatment effect being present only for treatment months, though the effect is stronger in earlier months. It is also possible that earlier months have higher treatment effects due to unobserved borrower differences. Therefore, in further specifications I add vintage-calendar time parameters to control for differential pre-trends for each cohort. In this case, identification of the month-dummy interactions relies on deviations from their pre-judgment linear trend. In panel A, in which there is no attempt to control for pre-trends, earlier months have statistically larger treatment effects than later months. However, this difference reverts to the mean in panel B, in which I adjust for cohort trends. The individual month dummies are rarely statistically significant at the conventional level, though the pooled effects are in all cases.

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<sup>9</sup>I thank the referees for suggesting variants of this test and the discussion in this section.

Figure 3: **Month-of-origination non-parametric treatment effects.** This figure plots the non-parametric month-of-origination treatment effects. Each point is a coefficient on the effect of each month, relative to December 2009 (12 is omitted). The estimates come from regressions based on Equation 3. The  $\beta_0^{DD}$  coefficients, corresponding to origination-month treatment effects, are shown for along different specifications, alongside pooled estimates (horizontal lines). Each model controls for cohort-time effects differently and the functional form is shown as the caption for each pane.

$$\beta_{DD} \times \sum_{i \neq 12} 1_{\text{month}=i} \times \text{Post}_t$$



An equivalent regression discontinuity estimate shows no statistical effect of the treatment. This could be due to a lack of power in the RD estimate. In a Monte Carlo simulation, I found that specifications similar to Equation 3 have similar mean-square error and bias, compared to appropriate regression discontinuity estimates. Therefore, the weak treatment effects observed in Figure 3 for the months of November and October could be the reason why the RD estimate finds no effect. Since RD assumes the effect varies linearly with loan origination month, it will show a null treatment effect for the patterns observed in Figure 3.

## 5 Robustness of Treatment Effect

To gauge the sensitivity of the treatment effect, I perform two additional analyses. In the first, I change the bandwidth of the sample selection procedure and allow greater or fewer numbers of loans to enter the analysis. I then examine how the estimated effect changes in each analysis. In the second, I conduct a permutation inference exercise to estimate the uncertainty of the treatment effect.

### 5.1 Varying the bandwidth of estimation

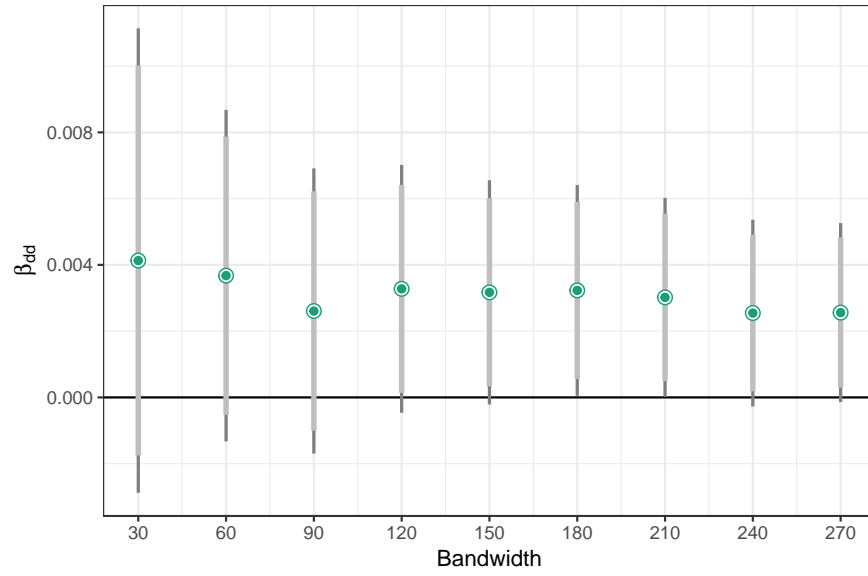
A reasonable specification should not permit large changes in the treatment effect when the parameters are altered. One obvious parameter to change is the bandwidth used in the sample selection procedure, as described in Section 3. Since the threshold of 180 days is essentially arbitrary, I repeat the analysis on bandwidths from 30 days to 270, in 30 day increments. I re-analyze the model in specification 1, and allow each origination-month cohort to have its own pre-treatment trend. This seems sensible as it is unlikely that vintage cohorts will be on parallel default-trends as the bandwidth is progressively widened.

Results are shown in Figure 4 and show that the treatment effect is stable across specifications. The treatment effect is not statistically different from zero in the smaller bandwidths, possibly due to small sample size.

### 5.2 Permutation Inference

Since treatment is applied at the group level, there are essentially two independent clusters under study, complicating conventional statistical inference. One alternative mode of inference is randomization or

Figure 4: **Sensitivity of the treatment effect to chosen bandwidth.** This figure shows how variation in the treatment effect, when the estimation bandwidth is changed. Each point is the estimated treatment effect from a model estimated on a new sample using a different bandwidth. The bandwidths are shown on the x-axis. The model is similar to that in Eq 1, with the addition of cohort-specific linear time trend. Vertical lines are the ranges of 90% (light gray) and 95% (dark gray) confidence intervals.



permutation-based inference. In a randomized experiment, permutation inference is undertaken by repeatedly randomizing the treatment assignment vector; re-estimating and storing the treatment effect under these different permutations; and calculating the  $p$ -value as the location of the true effect in this simulated null distribution.

I conduct a similar permutation exercise to estimate the significance of the effect size by simulating a null distribution using many ‘fake’ natural experiments. Specifically, I take a random 999 unique loan origination dates (sampled without replacement) from the data base on all outstanding loans in Ireland during the sample period and analyze the corresponding placebo treatment effect. The idea is to test whether research designs such as mine will often demonstrate positive treatment effects in the Irish mortgage data, even when there should be no effect at all.

Inference in this case is similar to Ganong and Jäger (2017), who provide theoretical and simulation evidence on the effectiveness of randomization inference in the analysis of regression kink designs.

To create the estimates for each fake experiment, I follow the same procedure as I did in the main analysis: I separate the data into treatment and control cohorts 180 days either side of the cut-off; I select the sample using matching; estimate the difference-in-differences regression model and store the estimates. Finally, I calculate the  $p$ -value by measuring the position of the absolute value of the true estimate in the null distribution. It is therefore the probability that I would observe an effect size that large, when there is in fact no true effect.

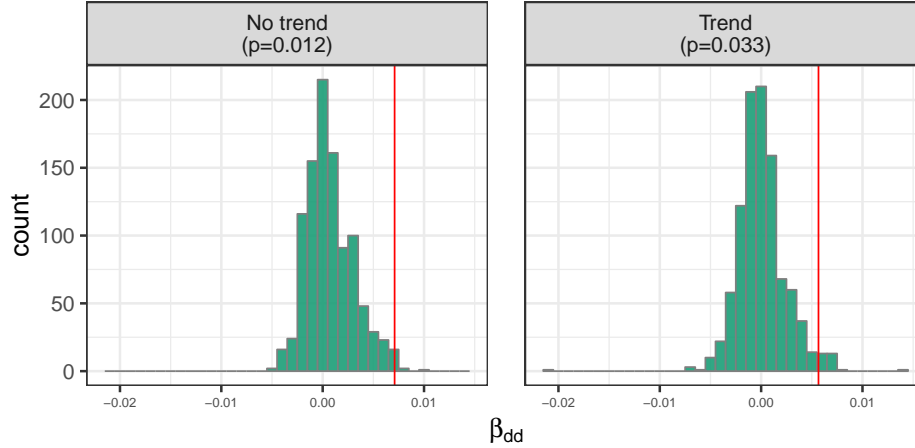
The results for two models, one with and the other without an origination month time trend, are presented in Figure 5. Both histograms of null distributions show that the observed effects, with  $p$ -values below 5%, are unlikely to have occurred by chance.

## 6 Magnitude and Mechanisms

### 6.1 Discussion of Mortgage Default Theory

In double-trigger models, borrowers generally default when the option value of repayment net of the interest payment falls below their own personal default costs (e.g. Foote and Willen, 2018; Ganong and Noel, 2018). In other words, for these borrowers the discounted expected financial gain from paying

Figure 5: **Permutation exercise.** Graphs shows two null distributions of 999 “placebo” analyses. Left panel shows the analysis for a model with no cohort trends; the right shows the same model with origination-month trends. True treatment effects are shown with horizontal red lines and associated p-values in the plot titles.



the mortgage no longer justifies repayment, when compared with their outside option of default.<sup>10</sup> For example, a borrower whose mortgage is deep underwater will default if rent is cheap compared with the interest payment. A less underwater borrower will not default if they believe they will benefit from future house price appreciation by continuing to pay over the odds for the house. Similarly, myopic borrowers heavily discount the option value of repayment, and may default if interest payments are high compared with default costs.

There are therefore two types of borrower who are on the margin of defaulting at current default costs: borrowers in deep negative equity who expect little financial gain from repayment and liquidity-constrained borrowers who discount the future to a high degree.

When the borrower is in deep negative equity, the mortgage is less likely to generate a positive investment profit. In this case, the repayment option is deep out-of-the-money and the borrower is better off defaulting in a purely financial sense.<sup>11</sup> Default costs lead some of these borrowers to continue to pay. Therefore, when default costs fall, borrowers who were on this margin now default.

<sup>10</sup>Negative equity is a necessary condition for default in these models, since a borrower in positive equity can avoid default costs by selling the home.

<sup>11</sup>Since repayment today allows a borrower to choose whether to repay or default next period, mortgage contracts contain some optionality. Though mortgage contracts in Ireland contain recourse, default costs are limited by the borrower’s downside in bankruptcy. Therefore, repayment is an asymmetric bet on future house prices. If prices remain static or fall, then the borrower is no worse off. However, if prices rise the borrower profits if the increase is enough to lead to positive equity. This asymmetry resembles the payoff from a call option. See Foote and Willen (2018) for an extensive discussion of the option-theoretic approach to mortgage valuation.

Liquidity-constrained borrowers generally do not have the ability to choose when to default. These borrowers default because they receive an income shock and simply cannot afford to make the payments. However, there will be some liquidity-constrained borrowers who can choose when to default: these borrowers continue to repay by cutting consumption in order to avoid the default costs. Because they discount the expected future payoff from repayment so highly, these borrowers would default if not for default costs.<sup>12</sup> When these costs are removed, repayment by sacrificing current consumption is less valuable than the default option.

The policy implications of the excess default I identify in Section 4 are impacted by which margin contributes more strongly. Depending on the benefit of smaller foreclosure externalities, the moral-hazard cost may be large enough to dissuade policymakers from implementing a foreclosure moratoria. There is some subtlety to this cost though. If the policy only induces liquidity-constrained defaulters, then these borrowers are likely to have high marginal utility of current consumption and therefore high marginal propensities to consume. By allowing these households to more cheaply share their income risk, the policy may induce additional consumption.

In this section, I outline evidence for the two margins and discuss the interpretation of the estimated magnitude from Section 4.

## 6.2 Discussion of Effect Size and External Validity

Double-trigger theory, as outlined above, states that borrowers will default when costs are reduced because of both equity and liquidity-based concerns. Therefore, the distributions of borrower equity and available liquidity will both matter to the external validity of this paper. Implementing a foreclosure moratorium in a jurisdiction with a strong labor market where borrowers have substantial equity in their homes would likely not increase the delinquency rate in any economically meaningful sense. In 2011, the Irish economy had recently suffered from a collapse in real estate prices (homes lost around half their value over the period 2008-2012) and a tripling of the unemployment rate. Both factors increased the likelihood that the removal of repossession risk led to excess delinquency. Though a different jurisdiction may not have witnessed an effect size as large as the one I observe here, such a macroeconomic crisis is exactly when policymakers would wish to implement a debt relief policy.

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<sup>12</sup>In a consumption-based portfolio choice model with constraints, liquidity shocks cause borrowers to have high current marginal utility of income and therefore discount the future highly (Fuster and Willen, 2017).

These specific conditions resemble an ideal state to learn about the effects of reducing default costs, such as repossession risk.

The external validity of the result also relies heavily on the legal and societal context in which it was observed. All of these factors influence how borrowers perceive the remaining costs of default once repossession risk is removed. Mortgage contracts in Ireland are recourse in nature and therefore the incentives to strategically default are muted because the borrower cannot “walk away” from any residual debt balances. Though Irish borrowers have strong legal protections in debt proceedings, lenders do have access to a borrower’s assets through an *execution against goods* order (Citizens Information, 2016). If borrowers believed that the Dunne judgment period was unlikely to last forever, they would have expected to experience these costs at a later date. Without rare garnishee orders however, lenders do not have access to a borrower’s future income. Borrowers in deep negative equity or with severe liquidity constraints might then expect their future default costs to be insignificant, when compared with their current situation.

Were repossession risk permanently removed with certainty, some underwater or constrained borrowers would still not default because of the impact on their future access to credit. This is particularly a concern for US borrowers since access to credit markets and loan terms are highly dependent on a borrower’s credit score (Agarwal et al., 2017b). Creditworthiness similarly factors into credit supply decisions in Ireland. The Irish Credit Bureau is an industry-run private firm that collects and supplies credit histories to its member institutions.<sup>13</sup> Credit reports typically carry 24-months worth of repayment history and loans remain on the database for 5 years after the loan has expired, regardless of whether the debt was fully repaid (Citizens Information, 2018). Such information sharing between lenders would also diminish to returns to a strategic defaulter.

Moral and social costs are often recognized as a large impediment to mortgage default. In their wide-ranging study on strategic default, Guiso et al. (2013) document the contribution of notions about morality in the decision to default. Interestingly, they also document that perceptions about fairness also temper the reluctance to default. During the Irish downturn, such feelings were widespread. In the height of the Irish crisis, Kelly (2010) argued that not only would falling house prices contribute to ever greater numbers of defaults, but so too would the spreading perception that Irish banks’ loose lending standards had caused the crisis in the first place. Coupled with growing numbers of borrowers

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<sup>13</sup>The Central Bank of Ireland operates the more comprehensive Central Credit Register since 2017.



defaulting before the Dunne judgment, such feelings likely made Irish borrowers more comfortable with defaulting in the years of the financial crisis.

My estimates of the average treatment effect of the judgment are smaller in absolute magnitude than the point estimate in Mayer et al. (2014b), who find around a 1.5 percentage point increase in quarterly delinquency rates following the announcement of a mortgage modification program. Their effect size translates into a 10% increase over the counterfactual, which is in fact smaller than the 40-60% relative effect size I estimate here. Ghent and Kudlyak (2011) find a 30% relative increase in the delinquency rate in non-recourse US states when compared with recourse states. To give context for the effect size in my sample, using control group data, I estimate a simple linear model measuring the association between LTV and interest rates with default.<sup>14</sup> Using this cross-sectional comparison, I find that 0.3 percentage points is roughly comparable with increasing the median LTV by 38 percentage points and the median interest rate by 2 percentage points.

Other factors that may contribute to the large effect size arise deliberately from the internal validity of the natural experiment. The loans that I analyze in Section 4.2 are on average just over 1.5 years old at the time of the judgment. It is possible that low attachment to the home is one factor working in favor of finding a large treatment effect in this environment, which would tally with the evidence in Guiso et al. (2013). Since the loans I analyze were issued at the tail-end of the Irish credit boom when price had already fallen, they are less likely to be in substantial negative equity. This could actually favor finding a large effect if borrowers with extremely high negative equity default, even with high default costs. Since they defaulted at high rates before costs were reduced, they will not demonstrate a large relative treatment effect from the Dunne judgment. Similarly, if the sample contained a larger number of severely liquidity-constrained borrowers, the treatment effect might also be smaller since these borrowers default regardless of default costs.

## 6.3 Evidence on the Margins of Excess Default

### 6.3.1 Household Deposit Data

In addition to collecting data for stress testing the assets of the Irish banking sector, the Central Bank of Ireland also collected data on the liabilities of the largest banks in December 2011. These data

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<sup>14</sup>I adjust for time-region fixed effects and also include a negative equity dummy to account for non-linearity in CLTV.

include account-level information on the deposits of Irish households at these banks. It is possible to link a sub-sample of borrowers in the loan data to these deposit data to obtain a measure of a household’s liquid wealth on the 31st December 2011, five months after the Dunne judgment. The data contain information on the average account balance over the 12 months from December 2010-2011, as well as the 6 months from July 2011-December 2011. I use these data to calculate the implied average account balance in the first 6 months of the year: the 6-months roughly prior to the Dunne judgment. To aggregate up to the borrower level, I calculate the average across accounts, weighted by the account balances in December 2011. This variable measures household liquidity in the six months prior to the Dunne judgment. I then match this variable to the loan data from Section 3 by the unique borrower identifier. Figure 6 shows that the liquidity measure is distributed similarly across treatment groups.

There are some important caveats to these data. The first is that due to differences in reporting across banks, this linking is only possible for one bank in the sample and so any inferences are limited in their external validity. The analysis of heterogeneous treatment effects is also limited to a relatively small sample size of 1347 loans, which will mean estimates are likely to be noisy. The third is that these estimates are static and do not take into account changes in liquidity. For example, a zero account balance may be entirely normal for one borrower because they do not use this account, and signify binding liquidity constraints for another. Finally, these account balances could be endogenous, if borrowers reduce their account balances in anticipation of default.

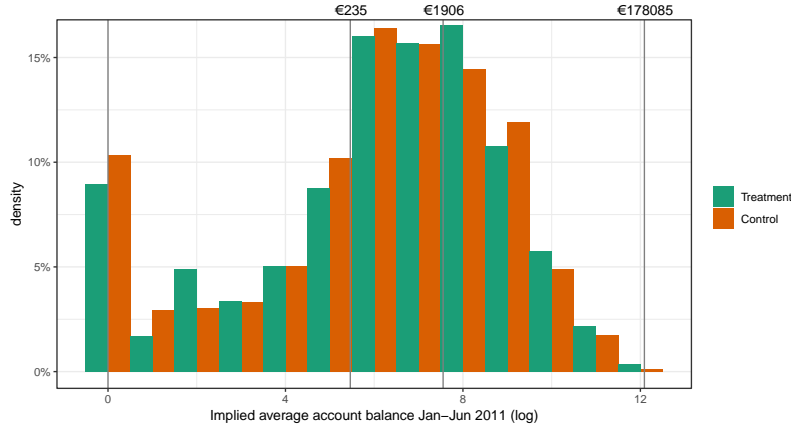
### 6.3.2 Heterogeneous Treatment Effects

I estimate a series of triple difference regression models to separately estimate the magnitudes of the equity-based and liquidity-based reasons for the excess default. I first interact the difference-in-difference variable with the terciles of the liquid wealth distribution.

$$\text{default}_{ijt} = \alpha_i + \beta(\text{treatment}_j \times \text{post}_t) + \sum_{k \neq 1} \theta_k \times (\text{treatment}_j \times \text{post}_t \times \mathbb{1}_{T_{ik}}) + \phi_t + \epsilon_{ijt} \quad (4)$$

$T_{ik}$  denotes an indicator variable for whether borrower  $i$  has liquid wealth in the  $k$ -th tercile of the distribution and  $\phi_t$  is a vector of time fixed effects. If the treatment effect is driven by a liquidity effect,

Figure 6: **Distribution of borrower account data.** Figure shows the estimated liquid account balances for borrowers, by treatment group. Data are the implied average account balance over the period January to June 2011, the six months before the Dunne judgment. Data are calculated using the 12-month and 6-month average balances calculated at December 2011.  $\text{Balance}_{Jan-Jun} = 2 \times \text{Balance}_{Jan-Dec} - \text{Balance}_{Jun-Dec}$ . 1 is added to account balances before the natural log is calculated. Boundary values (in Euro) of the terciles are shown at the top of the grid.



then the coefficient on the difference-in-difference term ( $\beta$ ) should be positive and large (the effect when liquidity is in the first tercile). The triple difference coefficients  $\theta_k$  identify how the treatment effect declines in each of the remaining terciles.

In a second model, I interact the difference-in-difference term with terciles of the distribution of the loan to value ratio at the time point prior to the Dunne judgment. High loan-to-value ratios have three effects on default. First, there is a pure causal effect of a high loan-to-value ratio in the double-trigger model: borrowers default because they believe that their property is now a bad investment. Second, as a friction to refinancing: with a leverage ratio above 100%, a new mortgage is in effect a large unsecured loan and supply is therefore rare. In that case, a high loan-to-value ratio acts as a constraint because the borrower is unable to refinance to avoid default. This is less important in Ireland, as monetary pass-through is less complete than in the U.S, for example. Finally, there is adverse selection associated with high loan-to-value ratios at *origination* which creates a strong correlation between the observed *current* loan-to-value ratio and adversely selected borrowers (Gupta and Hansman, 2019). For example, in the model of Bailey et al. (2019), a borrower wishing to maximize their exposure to housing because they have low default costs will put down a low down payment, taking a high loan-to-value loan. This last point is an important caveat, since it is not possible to separate the effects here. Gupta and Hansman

(2019) attribute approximately 40% of the association between leverage and default to selection.

The results from this analysis are presented in Table 3. Column (1) shows estimates of triple difference regressions of the liquidity effect and column (2) results of regressions on the equity effect.

In column (1), the treatment effect is driven by the lowest tercile of the wealth distribution, with an estimated treatment effect of 1.5 percentage points. This result is statistically significant at the 1% level using conventional standard errors and at 10% using loan-clustered errors. The predicted treatment effect for terciles 2 and 3 are both negative. The second tercile effect is not significant but the third is significant and completely reverses the main treatment effect.

Column (2) examines the loan-to-value interaction effect. The triple difference interactions show that going from the first tercile to the third tercile of LTV (equivalent to reducing equity from an average of 40% to 111%) is associated with an increase of 2 percentage points. This estimate is statistically significant at the 5% level when using either method of estimating standard errors.

In the results, borrower liquidity and equity are both large channels associated with the propensity to default, once repossession risk is removed.

## 7 Conclusion

Reducing repossession risk lowers the cost of mortgage default, encouraging strategic defaults for both equity-based and liquidity-based reasons. In this paper, I estimate the impact of repossession risk on mortgage default by examining delinquency rates in Ireland following a legal ruling that halted home foreclosures. Using difference-in-differences variation from the ruling, I find that borrowers defaulted at substantially higher rates than they were otherwise likely to. Using a sample of loans linked to borrowers' liquid account balances, I find large roles for both equity and liquidity margins.

Though the relative effect size I identify is large, the absolute effect size is in fact small. The loans studied in this paper were originated after the peak of the Irish credit bubble and are less risky on observable factors, and possibly also on unobservables. Whether 50% or 0.3 percentage points is the correct estimate for transporting the results to other scenarios depends on the functional form used to scale the treatment effects and I do not attempt to answer this question in this paper.<sup>15</sup>

To my knowledge, these results are the first empirical estimates of both moral-hazard costs and

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<sup>15</sup>For example, a proportional hazard model would deliver the large relative estimates, while an additive model delivers the smaller absolute value.

Table 3: **Heterogeneous treatment effects.** This table shows the estimated coefficients from two models, based on Equation 4. Column (1) show results from model examining the liquidity effect, in which the treatment effect is allowed to vary with terciles of the liquid wealth distribution. Column (2) examines the equity effect, showing results from a similar model, in which the treatment effect varies with terciles of the current loan-to-value (LTV) distribution. Conventional standard errors are shown in parentheses and loan-clustered standard errors are shown below them in brackets.

	Default	
	Liquidity (1)	LTV (2)
Treated $\times$ Post	0.0150 (0.0053)*** [0.0088]*	-0.0046 (0.0052) [0.0051]
Post $\times$ Account Balance T2	-0.0034 (0.0049) [0.0045]	
Post $\times$ Account Balance T3	-0.0069 (0.0049) [0.0039]*	
Post $\times$ LTV T2		-0.0035 (0.0050) [0.0046]
Post $\times$ LTV T3		-0.0063 (0.0050) [0.0041]
Treated $\times$ Post $\times$ Account Balance T2	-0.0102 (0.0074) [0.0103]	
Treated $\times$ Post $\times$ Account Balance T3	-0.0166 (0.0075)*** [0.0090]*	
Treated $\times$ Post $\times$ LTV T2		0.0115 (0.0074) [0.0077]
Treated $\times$ Post $\times$ LTV T3		0.0198 (0.0074)*** [0.0087]**
Observations	7998	7998
Loan FE	Y	Y
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

liquidity benefits of foreclosure moratoria, which were important debt relief policies in both the Great Depression and recent Great Recession. Though I provide direct evidence on some of the important channels of ex post debt relief policies, I did not focus on the general equilibrium implications of the results, nor did I analyze the financial stability consequences of excess losses for lenders. If lenders expect to regularly experience elevated losses from defaulting borrowers during macroeconomic crises, then the cost of borrowing may be higher ex ante. Future research might attempt to quantify the magnitude of such equilibrium effects on future generations of mortgage borrowers.

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