A Robustness Checks

In this appendix, we provide an in-depth robustness check analysis of the points made by LaCour-Little, Pavlov & Wachter (2022) in their paper. The paper makes two comments related to the rounding of loan amounts and conforming limits in high-cost counties. We find that our results are robust to addressing their concerns. First, the solution suggested by LaCour-Little et al. (2022) to round up limits leads to systematic overestimates of the share of conforming loans; leading to biased econometric estimators. Second, the small number of high-cost counties means that our analysis is robust to this recoding.

While the two points are helpful (and we analyze them detail in this appendix), our analysis finds that, in their independent replication, LaCour-Little et al. (2022) miscoded treatment years for almost all hurricanes, and such errors are bunched at the conforming loan limit. We also find that the event study design has missing treatment years, and that sometimes observations can be treated in multiple years *at the same time.* This analysis, and the robustness checks of this appendix, are available at the open source archive https://github.com/aouazad/Mortgage-Securitization-Natural-Disasters-Reply.git.

We also use this opportunity to revisit our core question using a regression discontinuity design (RDD) estimator that has three key advantages. First, this RDD estimator provides a detailed and visual description of the treatment effect of billion-dollar disasters on discontinuities. It explains where the effects of this paper and Ouazad & Kahn (2022) are coming from: they are driven by economically and statistically significant discontinuities exactly at the conforming loan limit. Second, using the latest regression discontinuity approach allows us to estimate the effects for a large range of bandwidths. We find that effects are robust to bandwidths from 2% to 20% around the conforming loan limits, with significance levels from 95% to 99%. Third, the effects are located in a narrow 2-3% bandwidth in year $t+1$ (approval and origination rates) and in year $t+1$ and year $t+2$ (securitization rates), but are broader and affect the entire window in years $t+2$ to $t+3$ (approval and origination rates) and in years $t+3$ to $t+4$ (securitization rates).

This Appendix proceeds as follows. Section B presents the current evidence for the Climate Securitization Hypothesis in the United States, with three different identification strategies in three different contexts. Section C analyzes the LaCour-Little et al. (2022) critique, an independent construction of the data and econometric specifications. It finds very serious mistakes in the construction of the data, rendering the estimates uninterpretable. Section C.2 discusses the point that loan amounts are rounded and the fix suggested of rounding conforming limits up. We find that LaCour-Little et al.'s (2022) approach yields a systematically upward biased and imprecise count of conforming loans. Section C.5 shows that the small number of high-cost counties, and their location in the mid-west and the West coast makes it unlikely that this affects results. Section D tests the Climate Securitization Hypothesis, replicates Ouazad & Kahn (2022), and provides a granular, non-parametric, point by point description of *where the effects come from.* The treatment effects display sharp discontinuities in approval, origination, securitization at the limit.

B Additional Evidence for the *Climate Securitization Hypothesis*

Sastry (2021) demonstrates how mortgage lenders transfer flood risk to the government and underinsured households by taking advantage of strict flood insurance coverage limits and staggered flood map updates. The paper shows that lenders' risk management proceeds by equalizing delinquency rates inside and outside of flood zones. This is achieved through a combination of insurance requirements and credit rationing, which results in a shift in the types of mortgages offered in flood zones towards borrowers who are wealthier and have higher credit quality.

Figure 3 of the December 2022 version of the paper presents empirical support for the *climate securitization hypothesis, as* the adjustment of leverage (LTV at origination) only occurs at a statistically significant level when a mortgage is either privately securitized (other than through the Government Sponsored Enterprises) or held on the balance sheet.

Nguyen, Ongena, Qi & Sila (2022) finds that, on average, lenders charge higher interest rates for mortgages in areas projected at risk of sea level rise. The effects are driven by long-term loans. The paper explores whether such a sea level rise (SLR) premium depends on a loan's eligibility to be securitized by Fannie Mae or Freddie Mac. As the pricing of securitization in guarantee fees (g-fees) by Fannie Mae and Freddie Mac depends on a Loan Level Performance Adjustment (LLPA) matrix that is independent of SLR or other forms of climate risk, the SLR premium is likely to be smaller for loans eligible for securitization to the agencies. The paper finds evidence consistent with the *Climate Securitization Hypothesis* as the SLR premium is significantly (economically and statistically) higher for jumbo mortgages that are not eligible for securitization by the agencies (Table VIII, page 1538).

Bakkensen, Phan & Wong (2023) presents both a theoretical model and an empirical analysis of the choice of debt when agents have beliefs over the future evolution of risk in a specific area. The model suggests that pessimistic agents take on more leveraged loans when the collateral is risky. This is consistent with the evidence in Hertzberg, Liberman & Paravisini (2016) suggesting a positive correlation between the leverage and the pessimistic beliefs of the agents. In Bakkensen et al. (2023), this leverage also manifests in longer maturity loans. The paper provides empirical evidence for the climate securitization hypothesis. First, the authors find robust evidence of higher leverage in places affected by Sea Level Rise risk. Second, this effect (SLR exposure interacted with climate belief) is stronger for the conforming loan segment – which banks can securitize loans and sell to the GSEs – than for the nonconforming loan segment. This latter point is consistent with the Climate Securitization Hypothesis.

C The LaCour-Little et al. (2022) Critique

LaCour-Little et al. (2022) suggests that lenders do not systematically increase the approval, origination and securitization rates of conforming loans relative to jumbo loans in the aftermath of natural disasters. As such they argue that there is insufficient evidence rejecting the null hypothesis that lenders have the same lending standards before and after a natural disaster. The authors' argument is based on a reexamination of Ouazad & Kahn's (2022), which found that approval, origination and securitization rates increase in the conforming segment relative to the jumbo segment. LaCour-Little et al. (2022) make two claims. First, that as loan amounts in thousands in the publicly-available mortgage data are rounded to the nearest integer, the conforming loan limits need to be rounded *up*. Second, that, in high cost counties, the geographic variation in the conforming loan limit affects the statistical test of the hypothesis.

This paper examines this alternative evidence thanks to the replication files provided by the authors of LaCour-Little et al. (2022). Close inspection of these data suggests significant errors in basic data construction of the LaCour-Little et al. (2022), such as incorrectly coded treatment years, missing hurricanes, and an incorrect event-study design. Such errors could have been averted with simple descriptive statistics, which are absent from the paper. This paper also replicates Ouazad & Kahn's (2022) findings, and conducts additional robustness checks to show the sensitivity of statistical tests of the climate securization hypothesis to choices such as the regression discontinuity bandwidth.

C.1 Significant Errors in Data Construction in LaCour-Little et al. (2022): The Miscoding of Hurricane Treatment Years

Data for LaCour-Little et al. (2022) was accessed in March 2023 and stored at the link in this footnote.[1] We find that the event study has incorrectly coded hurricane years for a significant share of the observations. This is presented on Table A, which suggests that the problem is broadly affecting all hurricanes of the sample. For hurricane Frances, which occured between Aug 24, 2004 – Sep 10, 2004, in time t+1, 32% of the observations are coded as treated in 2005 or in 2016. LaCour-Little et al. (2022) has no observation for hurricane Charley (2004) and hurricane Dennis (2005), while Ouazad & Kahn (2022) has 7,108 observations for these hurricanes. For hurricane Jeanne (2004), 33% of the observations are coded as treated in 2005 and 2016. For hurricane Katrina (2005), treatment years are coded as 2004, 2005, 2008, 2012. LaCour-Little et al. (2022) has a very small number of observations for Hurricanes Rita, Dolly, and Ike.

This miscoding of treatment years in the "as_of_year" variable has direct impacts on the estimation as this variable "as_of_year" is used in two positions of the core regression of *"run_regressions.R":* as a fixed effect, that controls for, for instance, the impact of the great financial crisis on origination, approval, securitization rates; and as an interaction term to capture how the level of discontinuity at the conforming loan limit varies across years.

This miscoding is also not driven by a desire to code years differently for hurricanes happening later in the year: the publicly-available Home Mortgage Disclosure Act data over this time does not include variables for the month of origination. Many miscoded years are many years before or after the actual year of the hurricane.

Perhaps even more concerning is the correlation between this miscoding of treatment years and the conforming / jumbo loan status of a mortgage application. Figure A suggests that the miscoding peaks at the conforming loan limit, especially pronounced for Hurricane Katrina (2005). There the

[1] http://www.ouazad.com/papers/lacour_little_data_archive.zip

share with the wrong treatment year peaks at 35% of the observation at the limit, and then drops to almost 0 right above the limit.

We provide a formal test that this miscoding of treatment errors peaks at the limit, using a regression discontinuity design. Table B presents the results of such RD design where the dependent variable is 1 when the treatment year is incorrect. For instance, hurricane Katrina's observations coded for any year other than 2005. Column 1 is the simple regression with the "Below the Conforming Loan Limit" dummy variable as the sole explanatory variable. Columns 2,3,4 use a polynomial of the log distance of the loan amount to the conforming loan limit. Standard errors are double clustered at the ZIP and year levels, as in Cameron, Gelbach & Miller (2008). In all 4 columns, there is a bunching of the coding errors at the limit, significant at 5%. It is intriguing that apparently random coding errors could be so related to the main focus of the analysis, the conforming loan limit.

We remark that LaCour-Little et al. (2022) updated their archive in April 2023 to remove the main CSV data file. We accessed their archive before such removal, and also remark that the code is identical in the previous archive and the current one. In contrast, the main data frame for Ouazad & Kahn (2022) has been available throughout on the RFS Dataverse since 2021 and will remain so. In addition, the code for this current paper is available at the GitHub archive https://github.com/aouazad/Mortgage-Securitization-Natural-Disasters-Reply.git.

There are other important errors in the paper. A major issue is the incorrect event study design. As the data set has a flat longitudinal structure, this subjects the analysis to the classic issue of the staggered difference-in-differences problem. We display in the table below the sum of the indicator variables for $Time = -4$ to $Time = +4$. In Ouazad & Kahn's (2022), this sum is always equal to 1 in the treatment group. Surprisingly, in the LaCour-Little et al. (2022), this sum can be 0, 2, or 3. This is not due to the reference dummy variable since $\mathbf{1}(Time = 0)$ and $\mathbf{1}(Time = -1)$ are included in the sum. Therefore we are observing two major anomalies in the analysis: first, that there are observations with multiple time dummy variables equal to 1 at the same time. 653 mortgages have $\mathbf{1}(Time = +1)$ and $\mathbf{1}(Time = +2)$ equal to 1 at the same time. 568 mortgages have $\mathbf{1}(Time = +2)$ and $\mathbf{1}(Time = +3)$ equal to 1 at the same time. Perhaps even more surprising is that a large chunk of treated observations ($Treatment = 1$) may have no dummy at all equal to 1 at any point from $Time = -4$ to $Time = +4$, i.e. $\sum_k \mathbf{1}(Time_{it} = k) = 0$. This suggests that the

Table A: Errors in LaCour-Little et al. (2022) – Miscoding of Hurricane Treatment Years

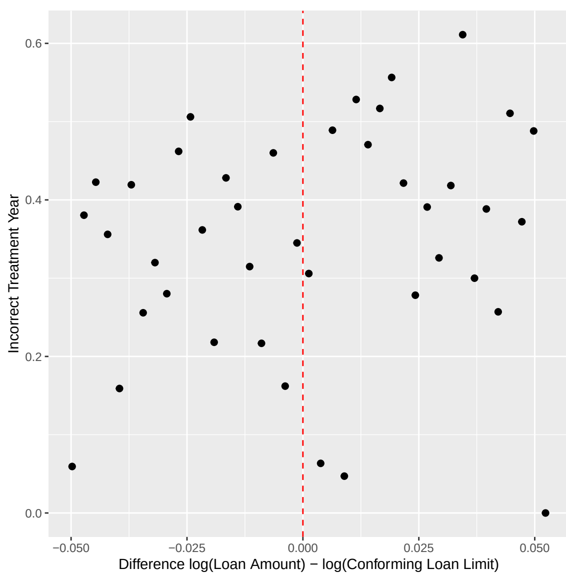
This table presents the analysis of the file "originated_05_CT_treatment.csv" provided by the LaCour-Little coauthorship team. This file is used in the main regression of their paper, titled "run_regressions.R." The replication package of these authors was accessed in April 2023 and is stored for your convenience at this GitHub archive.

		Time After Treatment			
Hurricane	Year of Treatment in LaCour-Little et al.	t+1	t+2	t+3	t+4
Frances (2004)	2004	1288	837	698	463
	2005	620	537	338	212
	2016	13	14	13	19
Charley (2004)	No Observation in LaCour-Little et al. (2022)				
Ivan (2004)	2004	239	145	165	148
	2005	1		1	2
Jeanne (2004)	2004	653	433	398	220
	2005	314	290	143	104
	2016	13	14	13	19
Dennis (2005)	No Observation in LaCour-Little et al. (2022)				
Wilma (2005)	2004	991	620	537	338
	2005	2468	2199	1322	643
Katrina (2005)	2004	2	1		1
	2005	862	841	554	286
	2008	77	99	92	100
	2012	281	238	303	341
Rita (2005)	2005	5	3	2	5
	2008	5	1	2	
Ophelia (2005)	2005	121	120	179	99
Gustav (2008)	2005	86	73	85	77
	2008	86	108	98	109
	2012	138	89	131	151
Ike (2008)	2005	5	3	2	5
	2008	38	32	28	46
Dolly (2008)	2008	2	2		
Irene (2011)	2005	12	5	15	15
	2011	20	66	59	83
	2012	32	31	36	42
Sandy (2012)	2011	14	32	31	36
	2012	1254	980	1180	1476
Isaac (2012)	2005	144	136	157	147
	2008	76	96	91	99
	2012	281	238	303	343
Matthew (2016)	2004	13	10	6	13
	2016	740	900	819	1428

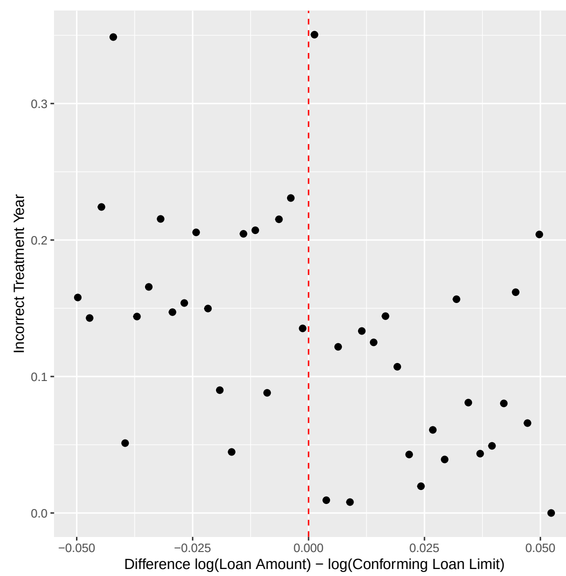
Figure A: Errors in LaCour-Little et al. (2022) – Miscoding of Treatment Year by Distance to the Conforming Loan Limit

These figures plot, for each hurricane, the share of mortgages with the wrong treatment year in LaCour-Little et al. (2022). For instance, Table A shows that 29.5% of the observations for hurricane Katrina have treatment years in 2004, 2008, and 2012. These graphs below plot such share at each distance of the conforming loan limit. They suggest that misclassification tends to peak right before the conforming loan limit, and then drop.

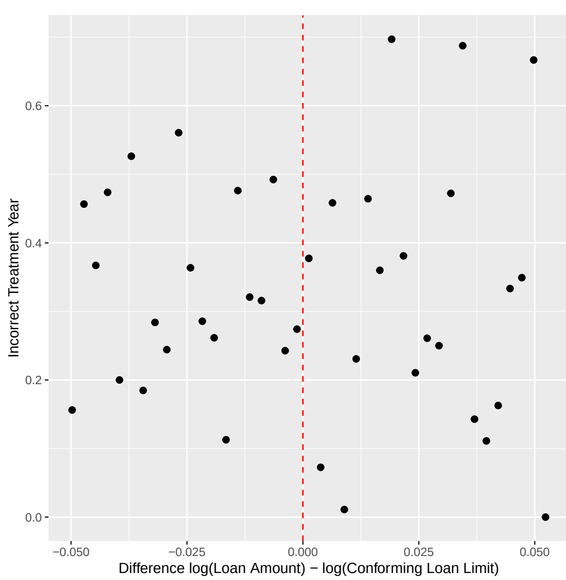
(a) Hurricane Frances (2004)



(b) Hurricane Katrina (2005)



(c) Hurricane Isaac (2012)



(d) Hurricane Matthew (2016)

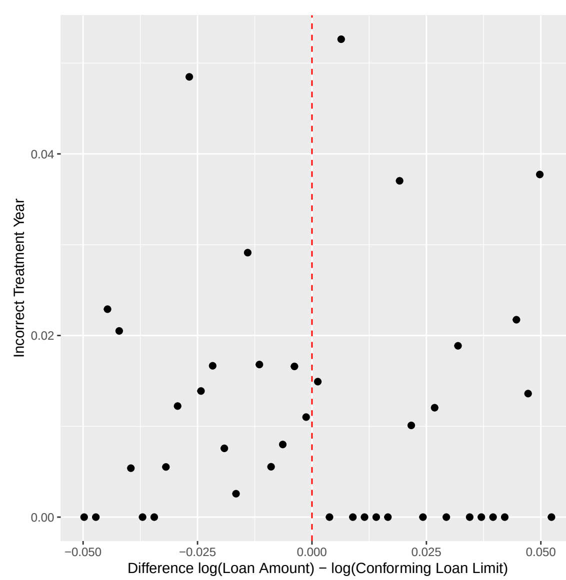


Table B: Errors in LaCour-Little et al. (2022) – A Formal Test of the Peak of Hurricane Treatment Year Errors at the Conforming Loan Limit

This table presents a formal test that the miscoding of treatment years peaks at the conforming loan limit in the data of LaCour-Little et al. (2022)*. We perform a regression discontinuity design estimation at the conforming loan limit. The dependent variable is 1 if the year of treatment was miscoded. For instance, as Table A shows, observations for many hurricanes in* LaCour-Little et al. (2022) *are mistakenly coded in a different year. Many Katrina observations are treated in 2004, 2008, and 2012. In this case, the dependent variable is 1. The right-hand side of each regression has the discontinuity and a polynomial of order 1 (column 2), 2 (column 3), 3 (column 4) in the log difference of the loan amount with the conforming loan limit. Standard errors are double-clustered at the ZIP and year level. The number of observations is the number of treated observations at any point between* $t - 4$ *and* $t + 4$ *in* LaCour-Little et al. (2022).

Dependent Variable:	1 if Incorrect Treatment Year			
Model:	(1)	(2)	(3)	(4)
Variables				
(Intercept)	0.0741***	0.0507*	0.0391	0.0298
	(0.0255)	(0.0277)	(0.0231)	(0.0272)
Below Conforming Limit	0.0541***	0.0847***	0.0999***	0.1121***
	(0.0184)	(0.0283)	(0.0268)	(0.0322)
Δ log(Loan Amount)		7.980**	9.818***	11.29***
		(3.373)	(3.229)	(3.866)
Δ log(Loan Amount)^2			4.686**	5.353**
			(1.796)	(1.956)
Δ log(Loan Amount)^3				-3.504
				(2.195)
Fit statistics				
Observations	173,870	173,870	173,870	173,870
R^2	0.00518	0.00710	0.00812	0.00870
Adjusted R^2	0.00517	0.00709	0.00810	0.00867

Clustered (ZCTA5 & as_ of_ year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The replication package of these authors was accessed in March 2023 and is stored for your convenience at GitHub archive..

regression's reference point is incorrectly specified, since the control group will include observations in the pre- and post-treatment periods.

Table C: Errors in LaCour-Little et al. (2022) – Missing Time Indicator Variables

This table presents, for the treatment group only, the sum of the indicator variables for the number of years relative to the hurricane. In a well-designed event study, each treated observation should have only one time dummy variable equal to 1. Except for the reference time period (e.g. -1*). In contrast, in LaCour-Little (2022), more than 93,000 observations are in the treatment but have no corresponding time dummy. And more than 6,200 observations have multiple time dummies equal to 1 at the same time. There is no dummy variable for times before* -4 *and no dummy variable for times after* $+4$.

	$\sum_{k=-4}^{k=+4} \mathbf{1}(Time_{it} = k)$			
	0	1	2	3
Number of Treated Observations	93,231	35,885	6,057	161

Finally, although the sample is extended to 2020, it does not include any billion-dollar hurricane for that time period such as hurricane Harvey (2017). This means that treated observations may be part of the control group.

The evidence presented in this section suggests that LaCour-Little et al. (2022) exhibits major flaws that render the results uninterpretable.

C.2 Rounding of Loan Amounts in Home Mortgage Disclosure Act Data

The previous section has displayed major errors in the empirical work of LaCour-Little et al. (2022). There are interesting points worthy of further investigation in LaCour-Little et al. (2022). The first one is that the rounding of loan amounts in HMDA affects the estimation and the fix suggested by LaCour-Little et al. (2022). The second point is that high cost counties' conforming loan limits play a significant role in the estimation. We investigate both of these points in turn to see if they affect evidence on the *Climate Securitization Hypothesis.*

Intuition and Descriptive Statistics

In Home Mortgage Disclosure Act data, loan amounts are reported by rounding loan amounts *to the nearest thousand.* For instance, on page 12 of the 2013 "Guide to HMDA Reporting, Getting It Right!":

> **Loan amount.** Report the dollar amount granted or requested in thousands. For example, if the dollar amount was \$95,000, enter 95; if it was \$1,500,000, enter 1500. Round to the nearest thousand; round \$500 up to the next thousand. For example, if the loan was for \$152,500, enter 153. But if the loan was for \$152,499, enter 152.

Figure B, case #1, shows what this implies for the difference between actual loan amounts and observed loan amounts when zooming in on, for instance the range of loan amounts between \$424,000 and \$425,000. This range is relevant for Clay County, Florida, which had a conforming loan limit of \$424,100. Loan amounts between 424 and 424.5 (not included) are reported as 424, and loan amounts between 424.5 (included) and 425 are reported as 425. The conforming loan limit is the vertical green dotted line. In this case, only loans between 424 and 424.1 can be conforming loans, but when rounding, all loans between 424 and 424.5 (not included) are reported as conforming. This *overestimates* the number of conforming loans.

Case #2 suggests that the rounding can also lead to an *underestimation* of the number of conforming loans. In the case of Collier Count, Florida, the limit is 450.8. Loans between 450 and 450.5 (not included) are counted as conforming, while the true count is larger, as it should include 450 to 450.8. On balance thus, the approach employed in Ouazad and Kahn (2022) leads to symmetric noise. In practice also, the ±10% or ±5% around loan amounts is substantially wider than \$1000, ranging from 381 to 467, leading to fewer rounding issues than Figure B would suggest.

LaCour-Little et al. (2022) argues that:

> "The correct comparison would **round the conventional (sic)** [2] **loan limit in the second term *above to the nearest \$1000***, so that the variable "diff_log_loan_amount" becomes zero and the loan is correctly classified as being at or below the FHA conventional limit." (page 34 of the October 31, 2022 version, accessed in January 2023, emphasis is ours).

We show below that this approach is incorrect as the share of conforming loans systematically upward biased. In both cases #1 and #2 of Figure B, the LaCour-Little et al. (2022) approach would count all loans between 424 and 425 (case #1) and between 450 and 451 (case #2). This

[2] This excerpt of LaCour-Little et al. (2022) should, of course, say the *conforming* loan limit.

uses the red dotted line in both graphs. While it generates bunching graphs with no overlapping point (and thus "seem cleaner") it leads to high and systematic levels of misclassification.

The econometric exercise performed below estimates the properties of the discontinuity estimator implied by Ouazad and Kahn's (2022) approach with those estimators implied by the LaCour-Little et al. (2022) approach. The exercises suggests that LLPW estimators exhibit larger variances, less precision, higher absolute biases, and slower speeds of convergence to their asymptotic value.

A third approach, not suggested in LaCour-Little et al. (2022), is to round the conforming loan limit in the same way as for HMDA loan amounts, to the nearest integer. It is easy to see that it also does not address this issue as it leads to greater over- or underestimation of the share of conforming loans. Overall, the approach of Ouazad & Kahn (2022) leads to the smallest bias and variance across the three approaches.

C.3 The Rounding of Loan Amounts: Simple Notations and the Economics of Rounding

We can deepen our understanding of the impact of the rounding of loan amounts by (a) understanding under what restrictive set of assumptions the LaCour-Little et al. (2022) approach is correct (always bunching) and (b) by performing an econometric analysis of the estimators using both the LaCour-Little et al. (2022) approach and the Ouazad & Kahn (2022) approach. Such econometric analysis will measure the bias and the precision of the estimators.

The true amount of loan i denoted L_i^* is a latent (unobserved) variable in HMDA, unless one relies on private data sets such as those sold by Corelogic. The observed loan amount L_i is the true loan amount L_i^* rounded to the nearest integer. This can be denoted as $L_i = \lfloor L_i^* \rceil$.

Let's then denote by $\overline{L}^*$ the conforming loan limit in the county of loan i. A necessary (but not sufficient) condition for a loan to be conforming is that the true loan amount $L_i^* \leq \overline{L}^*$. We denote this binary variable by $C_i^* = \mathbf{1}(L_i^* \leq \overline{L}^*)$ the classification of loan i. One can see that comparing the reported HMDA loan amount L_i to the exact conforming loan limit $\overline{L}^*$ may lead to an imperfect classification. We denote a HMDA-based classification as$C_i^H = \mathbf{1}(L_i \leq \overline{L}^*)$. Thus at this stage we have two binary indicator variables: $C_i^* \in \{0, 1\}$ for the true classification (conforming is 1), and $C_i^H \in \{0, 1\}$ for the HMDA-based classification.

Using data where numbers are not rounded is an obvious albeit non-free solution. Using expensive data hinders the public debate over the climate securitization hypothesis.

LaCour-Little et al. (2022) suggests rounding the limit up. This leads to a third classification, denoted $C_i^{LL} = \mathbf{1}(L_i \leq \lfloor \overline{L}^* \rceil)$. It is easy to see that across all classifications C_i^* (the true one), C_i^H (the one inferred from HMDA), C_i^{LL} (the LaCour-Little), the LaCour-Little approach generates the highest share of conforming loans:

$$C_i^{LL} \geq \max\{C_i^*, C_i^H\} \tag{1}$$

This is visible on Figure B. Meanwhile, the HMDA-based classification C_i^H can either be higher or lower than the true classification C_i^*:

$$\text{There are mortgages } i \text{ and } j \text{ such that } C_i^H \geq C_i^*, \quad \text{and} \quad C_j^* \geq C_j^H \tag{2}$$

This is a first hint that the measure C_i will be less biased than the LaCour-Little approach.

The gap between the HMDA-based classification C_i^H and the LaCour-Little specification C_i^{LL} depends on the share of mortgages in the jumbo segment. Perhaps surprisingly, the LaCour-Little approach C_i^{LL} is equal to the true classification C_i^* only when *all mortgages are always bunched in the conforming segment,* i.e. when there is no jumbo loan in the window. This is unlikely to be true.

The main reason that the is that there are sizeable jumbo applications and originations around the conforming loan limit even for narrow windows. The volume of jumbo mortgage applications and originations does not decline as the window narrows. This is likely due to a few factors. First, the agencies retreated from the mortgage market in 2004-2008, in the wake of the accounting irregularities; this matches the expansion of the private-label mortgage market.[3] Second, a significant share of borrowers choose to borrow using jumbo loans due to either the characteristics of their house or their FICO or LTV.

C.4 Mismeasurement of Bunching in the Cross-Section using LaCour-Little et al.'s (2022) Rounding

LaCour-Little et al. (2022) suggests that conforming loan limits should be rounded up, "The correct

[3]"Accounting Irregularities at Fannie Mae" by Chairman Christopher Cox U.S. Securities & Exchange Commission. Accessible at https://www.sec.gov/news/testimony/2006/ts061506cc.htm

Table D: The Share of Jumbo Loans Remains Substantial Even Close to the Conforming Loan Limit

This table presents the share (column 2) of loans with loan amounts above the conforming loan limit, for each size of the window around the conforming loan limit. This suggests that the method suggested by LaCour-Little et al. (2022), of rounding up the conforming loan limit, would misclassify approximately 27 to 30% of mortgages as conforming when they are jumbo. The window size is the $\max \log(\textit{Loan Amount}) - \log(\textit{Conforming Limit})$.

(1) Window Size	(2) Share of Jumbo Loans $\in [0, 1]$
±1%	0.297
±2%	0.289
±3%	0.283
±4%	0.290
±5%	0.277
±10%	0.283
±15%	0.278
±20%	0.278

comparison would round the conventional (sic) loan limit in the second term above to the nearest $1000." We examine the econometric properties of such an approach below and suggest that this alternative approached yields biased and imprecise estimators.

Econometric Framework and Monte Carlo Tests in the Cross-Sectional Bunching Case

We conduct Monte-Carlo tests to assess the econometric properties of discontinuity estimators when using either the true measure, the Ouazad and Kahn (2022) approach, and the LaCour-Little et al. (2022) approach. We do so by following standard econometric practice. We postulate a true model and estimate the parameters using either of the imperfect classification methods C_i^H (Ouazad and Kahn (2022)) or C_i^{LL} (LaCour-Little et al. (2022)).

The true model here is one where the probability of approval $P(\text{Approval}_i = 1)$ is discontinuous at the true conforming loan limit. This is modelled as a logit:

$$P(\text{Approval}_i = 1) = P(\text{Approval}_i^* \geq 0) = P(\alpha^* + \beta^* C_i^* - \varepsilon \geq 0) \tag{3}$$

The true model depends on the true classification C_i^*. Of course, in econometric practice, the probability of approval depends on a host of covariates such as the creditworthiness of the borrower, the location and characteristics of the house, the characteristics of the lender. The insights of our approach extend to this more general class of econometric models.

Bunching at the conforming loan limit can be estimated in multiple ways. A popular approach is to consider an OLS regression. Another approach, which we leave to the reader for further analysis, is a logit regression. As logit regressions entail other issues such as incidental parameter problems (Chamberlain 1980, Lancaster 2000) when including fixed effects in a more comprehensive regression. We thus focus on the least squares approach.

The data is generated according to 3. The regression is:

$$\text{Approval}_i = \hat{\alpha}^s + \hat{\beta}^s C_i^s + \epsilon_i^s,$$

where $s \in \{*, H, LL\}$ is the choice of the classification of the loan: using the true discontinuity, using the HMDA discontinuity, and using the conforming loan limit. $\hat{\beta}^*$ is the OLS estimate based on the true discontinuity. We compare $\hat{\beta}^H$ and $\hat{\beta}^{LL}$ to $\hat{\beta}^*$.

Results for the Static Bunching Case

We consider a case with $N = 1,000$ observations, true values $\alpha = 0.2$ and $\beta = 0.1$, and $S = 10,000$ simulations. Each approval decision is 1 with probability $F(\alpha^* + \beta^* C_i^*)$, where F is the cdf of the logit.

The first visible statistic is that across the two methods, the LaCour-Little approach is that which misclassifies loans more extensively. In the Monte Carlo samples, misclassifications are as follows:

County	Share Misclassified HMDA	Share Misclassified LLPW
Clay County, Florida	0.006	0.014
Collier County, Florida	0.010	0.010

This table suggests that in the case of Clay County the share of misclassified loans (1.4%) with the LaCour-Little et al.'s (2022) approach is more than double (0.6%) that of the HMDA approach of Ouazad & Kahn (2022). In the case of Collier county, the share misclassified is the same in both the HMDA approach and the LLPW overestimate the share of conforming loans. Overall, using HMDA loan amounts provides a lower share of misclassifications.

We then turn to the regression results. Figure B presents the distribution of the difference between the estimator $\hat{\beta}^H$ obtained using the HDMA loan amounts as in Ouazad & Kahn (2022) and the estimator $\hat{\beta}^*$ on true data with no misclassification. This is the blue line. The distribution of the difference $\hat{\beta}^{LL} - \hat{\beta}^*$ is the orange line. As the graph makes clear in both case 1 (subfigure (a), Clay County, FL) and in case 2 (subfigure (c), Collier County, FL), the LaCour-Little et al. (2022) is exhibits higher standard deviation, while Ouazad & Kahn's (2022) approach yields only a minor increase in standard deviation compared to the estimator using the true classification. Subfigures (b) and (d) present the distribution of the absolute difference $|\hat{\beta}^s - \hat{\beta}^*|$ for $s = H$ and for $s = LL$. In both cases, the absolute bias is higher in the LaCour-Little et al. (2022) than with Ouazad & Kahn's (2022) approach.

Figure C shows that the estimator based on LaCour-Little et al.'s (2022) performs worse regardless of sample size. Subfigure (a) displays the absolute deviation $|\hat{\beta}^s - \hat{\beta}^*|$ from the true value for sample sizes ranging from 50 to 2000. The absolute deviation is substantially higher at all Ns. Subfigure (b) shows that the estimator is also less precise. The standard deviation of LaCour-Little et al.'s (2022) estimator is higher at all Ns.

Conclusion

HMDA loan amounts are rounded to the nearest integer. Conforming loan limits are typically at an exact multiple of thousands of dollars. With HMDA, the approach of Ouazad & Kahn (2022) yields the smallest share of misclassifications, a lower absolute deviation, and is more precise. While using private data with exact loan amounts (e.g. Corelogic data) would address this issue, this is not what LaCour-Little et al. (2022) suggest, and their approach yields biased and imprecise estimators.

C.5 Point #2: The Role of High Cost Counties' Specific Conforming Loan Limits

The Geographic Distribution of High-Cost Counties

Conforming loan limits vary by county and by year, as noted by Ouazad & Kahn (2022). In any given year, roughly 100 to 200 counties (out of 3,143) are deemed "high-cost counties." Figure E presents a map of conforming loan limit $ by county, and delineates the boundaries of states of the

Atlantic coast and the Gulf of Mexico. These are the states most exposed to hurricanes and are thus the focus of any analysis of the impact of hurricanes on mortgage securitization. Figure E suggests that most coastal counties are not high cost counties. Many high cost counties are in the San Francisco Bay Area, the Los Angeles area, and the Seattle area. A notable exception is the New York City area, which both experienced Hurricane Sandy in 2012 and is a high-cost area. Yet, there is no difference between Ouazad & Kahn's (2022) and LaCour-Little et al.'s (2022) conforming loan limits in the New York metropolitan area. This is a first intuitive indication that the quantitative importance of LaCour-Little et al.'s (2022) point may be small. The next section investigates this point.

D Testing the Climate Securitization Hypothesis: Securitization Dynamics in the Aftermath of Natural Disasters

We reestimate Ouazad & Kahn's (2022) baseline specification using the limits provided by LaCour-Little et al. (2022). The results are presented on Figure F for all three outcome variables, approval, origination, and securitization conditional on approval. They are similar to the original findings in Ouazad & Kahn's (2022) Figure 8. Tables E to G provide further statistical tests for each bandwidth. The null hypothesis here is that the lending standards in the conforming segment are not differentially changing after a billion-dollar disaster relative to the overall market. A *rejection* of the null hypothesis with a positive and statistically significant estimate for approval or origination rates suggests that lending standards are becoming more lenient in the conforming segment as compared to the jumbo segment. This is what Figure F suggests: approval probabilities increase gradually all the way to +6 percentage points in year 3 (subfigure (a)), the origination probabilities increase all the way to +5 percentage points in year 3 (subfigure (c)), and the probability of securitization conditional on origination increases by approximately 12 percentage points in year 4 (subfigure (e)). The bars indicate the double-clustered standard errors at the ZIP and year levels.

The timing is also similar to Ouazad & Kahn (2022): in the first 3 years following a natural disaster, approval, origination rates increase significantly and then taper off. In the third and fourth year, securitization rates $P(\text{Securitized}|\text{Originated})$ increase in turn, suggesting that lenders are changing their securitization practices in the conforming segment conditional on origination.

For each figure, the right-hand column is for the impact on the mortgage market regardless of the conforming segment. There there is a systematic decline in approval, origination, and securitization rates: approval rates decline by up to 7 percentage points (subfigure (b)), origination rates by 7 percentage points as well (subfigure (d)), and securitization rates by up to 10 percentage points (subfigure (f)), indicating that the origination and securitization activity in the jumbo segment tapers off significantly after a natural disaster. This is likely due to both the difficulty of securitizing jumbo loans in the private label market and the reluctance of lenders to originate and hold.

Hence what likely explains the difference between LaCour-Little et al. (2022) and Ouazad & Kahn (2022) is the significant errors of LaCour-Little et al. (2022) documented in Section C.1 and the incorrect rounding of conforming loan limits.

Inspecting the Mechanism: Treatment Effects at Each Distance of the Conforming Loan Limit

What drives the results of both Ouazad & Kahn (2022) and this paper? Is the effect driven by treatment effects far away from the conforming loan limit, or by sharp discontinuities exactly at the limit? The approach described in this section provides appealing and intuitive graphical approaches to an understanding of the main treatment effects, presented Figures G–I. Open source code is available at the link on the cover page.

To understand the method, consider a distance $\delta \in [-0.10, +0.10]$ to the conforming loan limit. We would like to estimate the impact $\text{TE}(\delta)$ of a billion dollar event on approval probabilities exactly at δ. We also would like to control for year, ZIP, and disaster confounders, as in the main regression. This is performed by estimating the coefficients ξ_t for $t = -4, -3, -2, 0, 1, 2, 3, 4$ that minimize:

$$\min \sum_{it} \Bigg(\text{Approval}_{it} - \sum_{t=-4}^{+4} \xi_t(\delta) \cdot \text{Treated}_{j(i)} \times \text{Time}_{t=y-y_0(d)} - \text{Year}_{y(t,d)} - \text{Disaster}_d - \text{ZIP}_{j(i)} \Bigg)^2 K\left(\frac{\Delta \text{Loan Amount}_{it} - \delta}{h} \right) \quad (4)$$

while weighting observations by their distance $\Delta\text{Loan Amount}_{it} - \delta$. Hence when $\delta = -0.10$, the coefficients $\xi_t(\delta)$ measure the treatment effect for loan amounts 10% below the conforming loan limit.

When $\delta = +0.10$, the coefficients $\xi_t(\delta)$ measure the treatment effect for loan amounts 10% above the conforming loan limit. K is a Gaussian kernel and we use the bandwidth $h = 1\%$. Robustness to bandwidth choice is presented on Tables E–G. When considering $\delta < 0$, the treatment effects are estimated using observations in the conforming segment. When $\delta > 0$ the treatment effects are estimated using observations in the jumbo segment.

An advantage of this approach is it flexibility and its visual representation. A drawback may be the difficulty of obtaining standard errors; this is addressed in the next subsection where discontinuities at the conforming loan limit are formally tested using double clustering at the Year and ZIP levels.

Figure H presents the estimation results for the approval rate, using 40 distances, and a bandwidth of 1%. Subfigure (a) is for the treatment effects at time $t = +1$, (b) for time $t = +2$, (c) for time $t = +3$, (d) for time $t = +4$. The graph suggest that most of the treatment effects occur at the conforming loan limit. In time $t = +1$, the approval rate declines for jumbo loans in the two bins at the immediate right of the limit, while the treatment effects in the jumbo segment for loan amounts from 2.5 to 10% above the limit are similar to treatment effects in the conforming segment. In time $t = +2$, the situation changes drastically: the treatment effects drop sharply for almost all loan amounts in the jumbo segment, and the discontinuity (drop) in approval rates is visible at the conforming loan limit. A similar scenario appears in time $t = +3$ (subfigure (c)). In time $t = +4$, we should not expect a discontinuity in approval *rates*, as documented earlier and consistent with the baseline findings of both this paper (Figure F) and Ouazad & Kahn's (2022) Figure 8.

Figure G presents the estimation results when the outcome variable is the origination rate of mortgage applications. Mortgages are originated when they are approved by the lender and accepted by the borrower. The picture is qualitatively similar to those for the approval rate (Figure H), suggesting that it is not borrowers' behavior that is driving results, but rather lenders.

Figure G presents the estimation results when the outcome variable is the securitization rate of mortgage *originations*. We should expect these results to be different from those of the origination and approval rates, given that the securitization probabilities conditional on origination picks up in years +3 and +4. In practice, we observe that there are discontinuities in the impact of billion dollar disasters on securitization probabilities in each time period $t = +1$ to $t = +4$. The discontinuity in the treatment effect become large: over +10 percentage points in time $t = +3$, over +16 percentage points in time $t = +4$. The next subsection provides a formal significance test of these discontinuities controlling for the year, ZIP, disaster confounders, and the interaction of year, ZIP, disaster f.e.s with the discontinuity as in Ouazad & Kahn's (2022).

Regression Discontinuity Bandwidth and Estimation Results

The previous section D suggested that a significant part of the mechanism underlying the securitization hypothesis is at the limit or in a window around the limit. This suggests the use of the regression discontinuity framework, for which an extensive literature provides methodological guidance, including Imbens & Lemieux (2008) and Cattaneo & Titiunik (2022). The approach here performs such regression discontinuity while keeping the same of controls and fixed effects that are present in the baseline specification Ouazad & Kahn's (2022): year, ZIP, disaster, year×Below the conforming loan limit.

The regression discontinuity estimator of the impact of a billion dollar event is estimated as in Cattaneo & Titiunik (2022). We set $\delta = 0$ (at the limit) in specification 4 and start by estimating the treatment effects exactly on the left side of the conforming loan limit:

$$\min \sum_{it} \Bigg(\text{Approval}_{it} - \sum_{t=-4}^{+4} \xi_t(\delta) \cdot \text{Treated}_{j(i)} \times \text{Time}_{t=y-y_0(d)} - \text{Year}_{y(t,d)} - \text{Disaster}_d - \text{ZIP}_{j(i)} \Bigg)^2 K\left(\frac{\Delta\text{Loan Amount}_{it}}{h}\right), \tag{5}$$

for $\Delta\text{Loan Amount}_{it} < 0$ (conforming segment). This yields estimates of treatment effects $\xi_t(\delta = 0^-)$. We do the same estimation, but setting $\delta = 0$ and considering only mortgages of the jumbo segment. This yields estimates of treatment effects $\xi_t(\delta = 0^+)$. As in Cattaneo & Titiunik (2022), the impact of a billion-dollar disaster on the discontinuity in approval rates at the conforming loan limit is:

$$\tau_{RD} = \xi_t(\delta = 0^-) - \xi_t(\delta = 0^+) \tag{6}$$

The innovation here is that we estimate the impact of billion dollar disasters on the regression discontinuity controlling for the Year fixed effects, the Disaster specific fixed effects, the ZIP code fixed effects. Since the fixed effects are different on each side of the conforming loan limit, this also controls for year × below limit, ZIP ×below limit and disaster × below limit confounders.

This RD design of Cattaneo & Titiunik (2022) is straightforward to implement. It corresponds to the regression:

$$
\begin{aligned}
Outcome_{it} = {} & \alpha \cdot \text{Below Conforming Limit}_{ijy(t,d)} + \gamma \text{Below Conforming Limit}_{ijy(t,d)} \times \text{Treated}_{j(i)} \\
& + \sum_{t=-T}^{+T} \xi_t \cdot \text{Treated}_{j(i)} \times \text{Time}_{t=y-y_0(d)} \\
& + \sum_{t=-T}^{+T} \tau_t \cdot \text{Below Conforming Limit}_{ijy(t,d)} \times \text{Treated}_{j(i)} \times \text{Time}_t \\
& + \sum_{y=1995}^{2016} \zeta_y \cdot \text{Below Conforming Limit}_{ijy(t,d)} \times \text{Year}_{y(t)} \\
& + \sum_d \chi_d \cdot \text{Below Conforming Limit}_{ijy(t,d)} \times \text{Disaster}_d \\
& + \sum_j z_j \cdot \text{Below Conforming Limit}_{ijy(t,d)} \times \text{ZIP}_{j(i)} \\
& + \text{Year}_{y(t,d)} + \text{Disaster}_d + \text{ZIP}_{j(i)} + \varepsilon_{it},
\end{aligned} \tag{7}
$$

weighted by the distance of each loan to the conforming loan limit $K\left(\frac{\Delta\text{Loan Amount}_{it}}{h}\right)$. The parameters of interest are the treatment effects τ_t for $t = 1, 2, 3, 4$. They measure the discontinuity in treatment effects at the conforming loan limit.

We use a Gaussian kernel for $K(\cdot)$, popular in a number of seminal papers such as DiNardo, Fortin & Lemieux (1996), Connor, Hagmann & Linton (2012), Duclos, Esteban & Ray (2004), Barone-Adesi, Engle, Mancini et al. (2008). This fixed effect panel regression is similar to the main specification of Ouazad & Kahn (2022), where observations are weighted by the values $K_i = K\left(\frac{\log L_i - \log \overline{L}^*}{h}\right)$ and the bandwidth h varies between 1% and 20%. The standard errors are double-clustered by ZIP and year.

Tables E–G present the results. For each table, the upper panel is for bandwidths h of 1% to 4%, in increments of 1 ppt. The lower panel is for bandwidths h of 5, 10, 15, 20%. Table E is for the approval rate, Table F is for the origination rate, Table G for the securitization rate in the universe of originated mortgages.

Each table reports both the impacts for the mortgage market ($\text{Treated}_{j(i)} \times \text{Time}_{t=y-y_0(d)}$ indicator variables) and for the conforming market ($\text{Below Conforming Limit}_{ijy(t,d)} \times \text{Treated}_{j(i)} \times \text{Time}_t$

indicator variables).

Results suggest that the bandwidth of 1% to 3% maximizes the mean squared error for the approval and origination outcome variables respectively. Results suggest that billion dollar disasters increase the discontinuity in approval probabilities by up to 6.48% (**), increase the discontinuity in origination probabilities by 6.08% (**), and the discontinuity in securitization probabilities by up to 17.7% (***). The timing of the effects also matches the timings of the main regression: an increase in approval and origination rates, followed by an increase in securitization rates. The code is available at *this GitHub archive.*.

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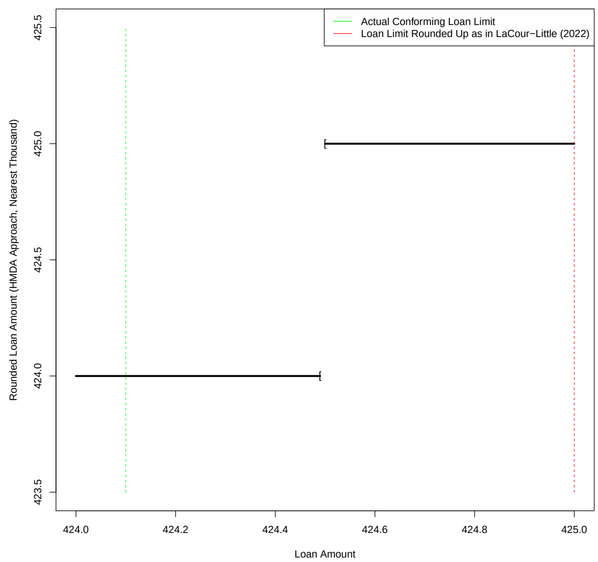
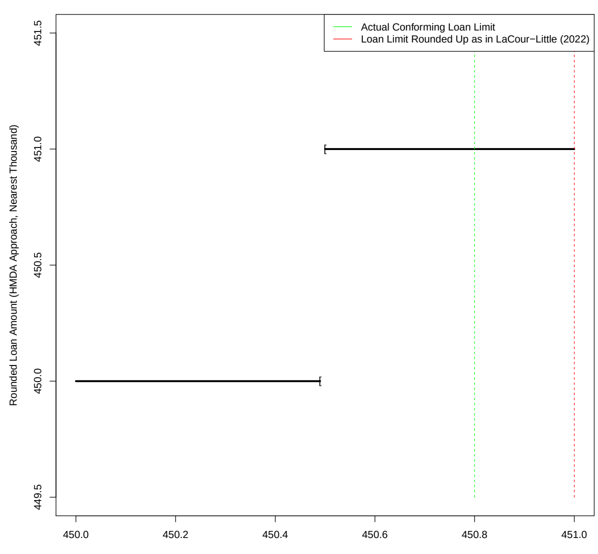
Figure B: Rounding and Loan Amounts: Why LaCour-Little et al.'s (2022) Approach Systematically Biases the Counts of Conforming Loans in Two Cases

These figures are an illustration of the rounding of loan amounts to the nearest integer. The true loan amount L^* is on the horizontal axis. L, the observed loan amount is on the vertical axis. $\overline{L}^*$ the true conforming loan limit, is the green line. The conforming loan limit rounded up as in LaCour-Little et al. (2022) is the red line. The LaCour-Little et al. (2022) approach yields a systematic overestimation of the number of conforming loans. In contrast, the approach of Ouazad & Kahn (2022) yields either an overestimation (case 1) or an underestimation (case 2) of the number of conforming loans.

(a) Case #1: Clay County, Florida, Limit $424,100



(b) Case #2: Collier County, Florida, Limit $450,800

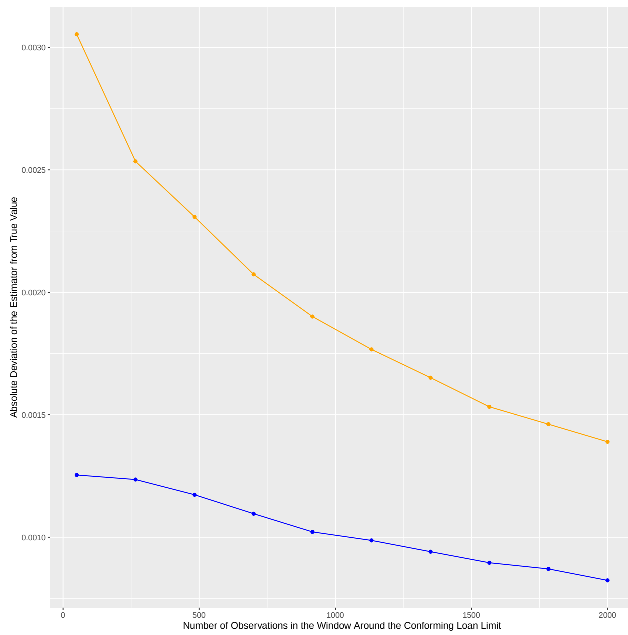


Code available at *https://github.com/aouazad/Mortgage-Securitization-Natural-Disasters-Reply.git*.

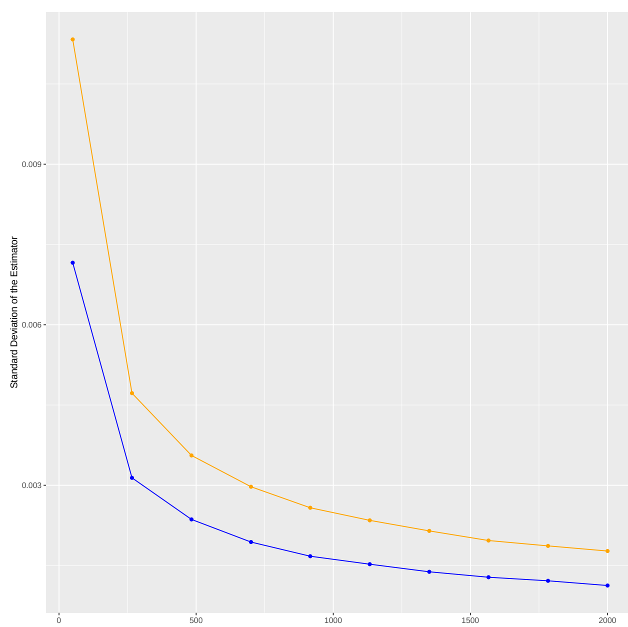
Figure C: Bias and Imprecision of the LaCour-Little et al. (2022) rounding approach with Cross-Sectional Bunching Estimators – Bias and Standard Deviation vs Number of Observations

The top panel present the evolution of the absolute deviation $|\hat{\beta}^s - \hat{\beta}^*|$ with respect to the number of observations in the window around the conforming loan limit, in the Monte Carlo Simulation. The orange line is using the LaCour-Little et al. (2022) approach. The blue line uses Ouazad & Kahn's (2022) approach. The bottom panel presents the standard deviation $SD(\hat{\beta}^s)$ of the estimators. Same color code. LaCour-Little et al.'s (2022) yields higher absolute deviations and higher variances for all sample sizes.

(a) Absolute Deviation of the Cross-Sectional Bunching Estimators from the True Value



(b) Standard Deviation of the Cross-Sectional Bunching Estimators

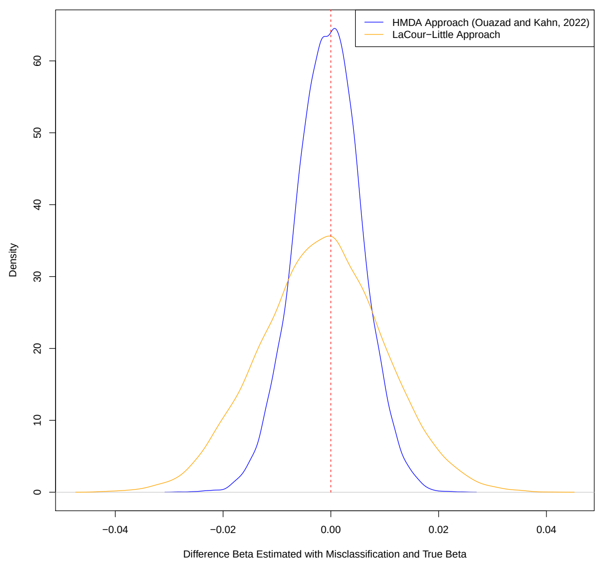


The parameters of the Monte Carlo simulations are $N \in [50, 2000]$, $\alpha = 0.2$, $\beta = 0.1$.

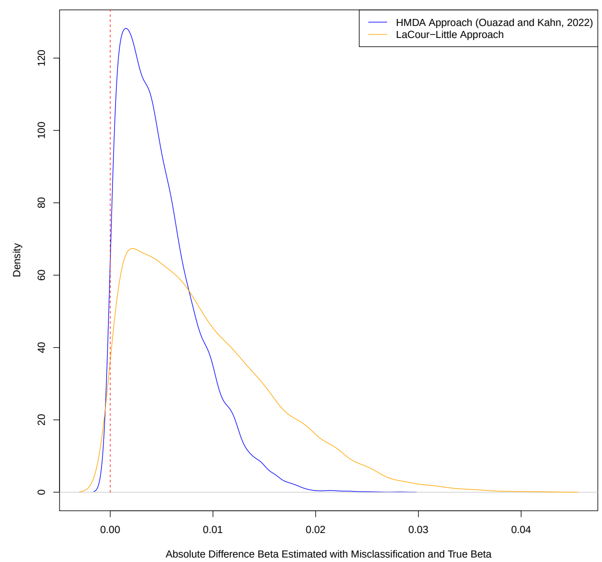
Figure D: Rounding and Loan Amounts: A Monte-Carlo Analysis of Variance and Bias

These figures present the distribution of the difference between the cross-sectional bunching estimator $\hat{\beta}^s$ with misclassification and the estimator $\hat{\beta}^*$ on the true data. We consider two possible misclassifications: $s = H$ when using the classification of conforming loans using the observed HMDA loan amount and the actual conforming loan limit, as in Ouazad & Kahn (2022)*;* $s = LL$ *when the using the LaCour-Little classification based on rounding the conforming loan limit to the highest integer. The LaCour-Little approach leads to higher variance and higher absolute bias.*

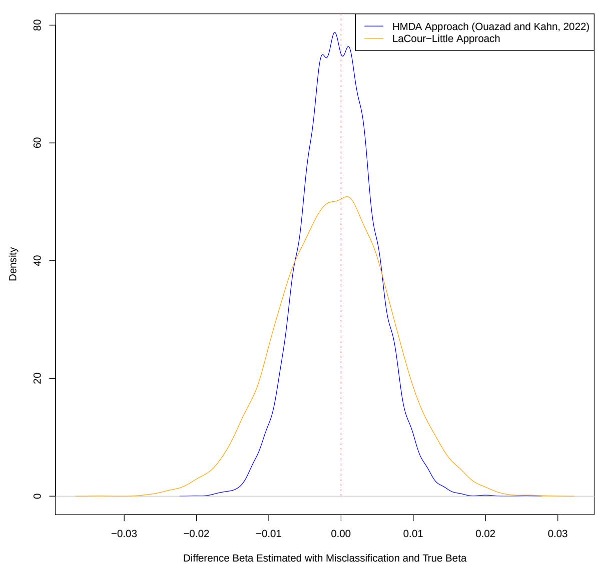
(a) Case #1, Clay County, FL, Distribution of $\hat{\beta}^s - \hat{\beta}^*$



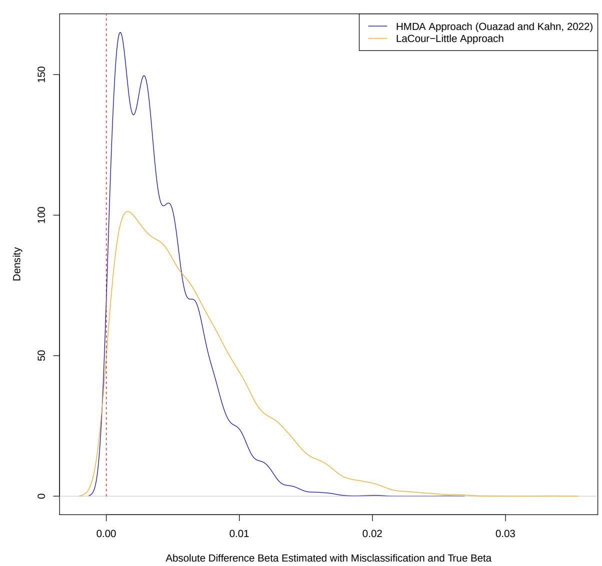
(b) Case #1, Clay County, FL, Distribution of $|\hat{\beta}^s - \hat{\beta}^*|$



(c) Case #2, Collier County, FL, Distribution of $\hat{\beta}^s - \hat{\beta}^*$



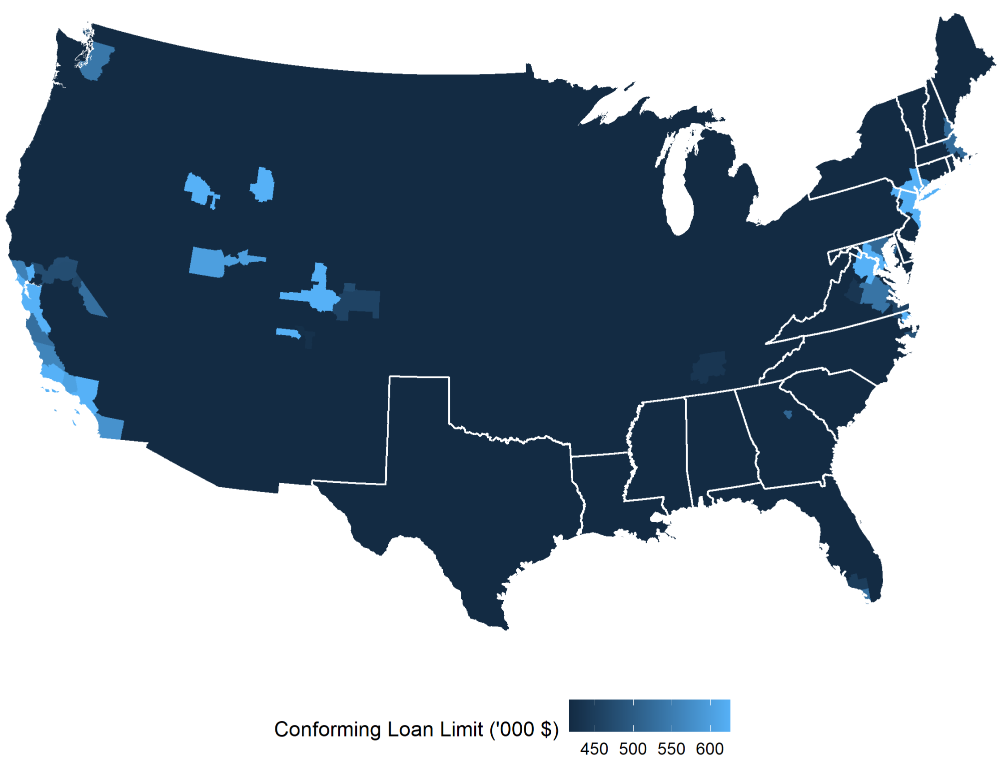
(d) Case #2, Collier County, FL, Distribution of $|\hat{\beta}^s - \hat{\beta}^*|$



The parameters of the Monte Carlo simulations are $N = 1,000$, $\alpha = 0.2$, $\beta = 0.1$.

Figure E: Geographic Distribution of Conforming Loan Limits and High Cost Counties

This map presents the county-level conforming loan limit in 2016 (insights are similar when mapping other years post HERA). The white dotted line are the boundaries of the Atlantic states and states of the Gulf of Mexico, where most hurricanes occur and is the focus of Ouazad & Kahn (2022). *Most high-cost counties are outside the area of the Atlantic states and the Gulf of Mexico, with the exception of the New York-Newark-Jersey City, NY-NJ-PA metro area. Yet there both LaCour-Little et al. (2022) and Ouazad & Kahn (2022) find significant impacts of hurricane exposure on origination probabilities in the conforming segment.*

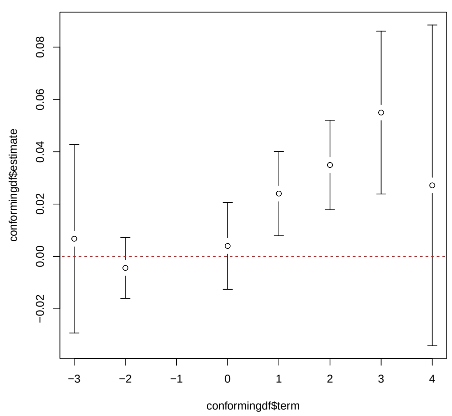


Sources: County shapefile (2014) from the US Census Bureau, conforming loan limits as in LaCour-Little et al. (2022).

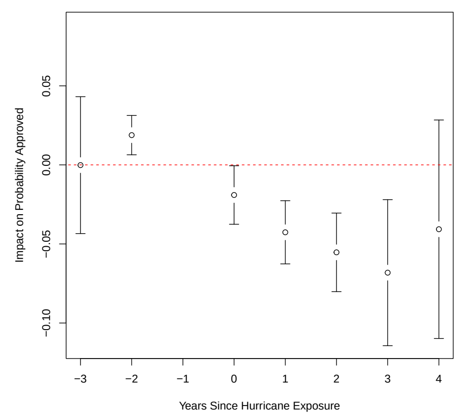
Figure F: Treatment Effects – Replication of Ouazad and Kahn (2022) with Limits Provided by LaCour-Little (2022)

These four graphics present the impact of a billion-dollar event on the approval, origination rates and securitization conditional on origination for mortgage applications (Section D). The left column present the impact on conforming loans only (the coefficients of the Below Limit × Treated × Time k). The right column presents the impact for the entire mortgage market in the window around the conforming loan limit. Confidence intervals at 95% obtained by double-clustering standard errors at ZIP and year levels. These results are similar to Ouazad & Kahn's (2022) *Figure 8 and the source code is available at this GitHub archive..*

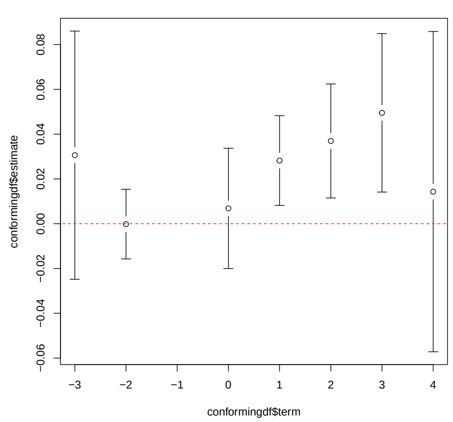
(a) Approval Probabilities, Conforming Market



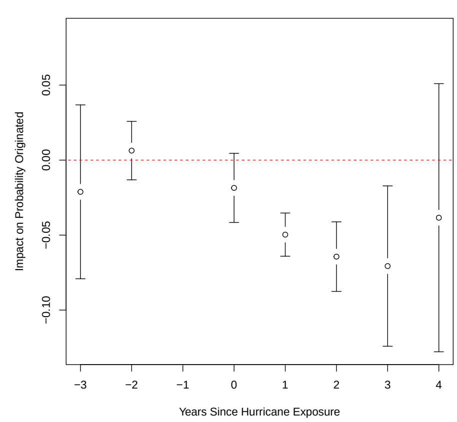
(b) Approval Probabilities, Overall Market



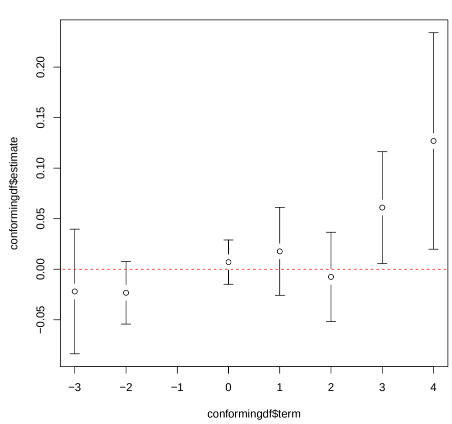
(c) Origination Probabilities, Conforming Market



(d) Origination Probabilities, Overall Market



(e) P(Securitization|Origination), Conforming Market



(f) P(Securitization|Origination), Overall Market

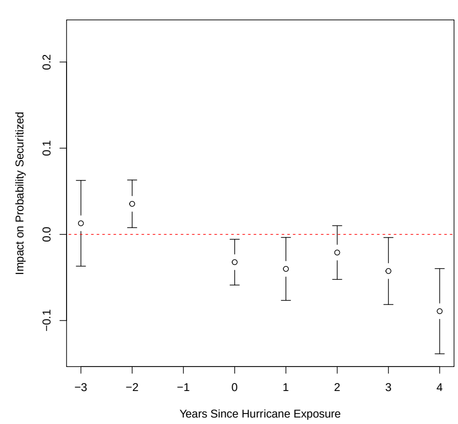


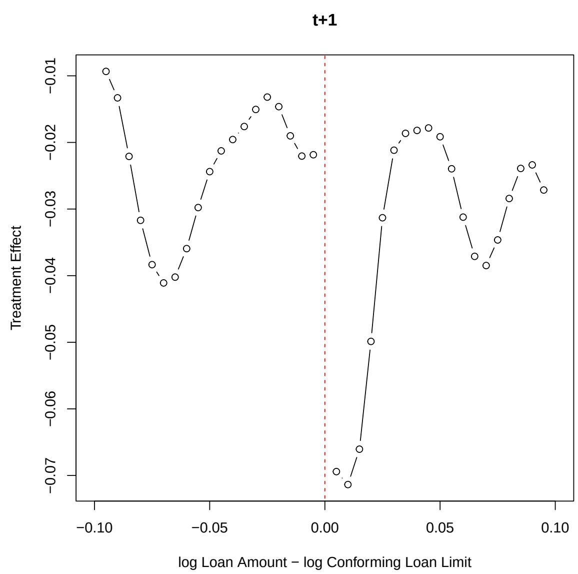
Figure G: Inspecting the Mechanism: Treatment Effect by Distance to the Conforming Loan Limit

The graphs below inspect the mechanism driving the results presented in Figure F using local polynomial regressions. Each point in Subfigure (a), (b), (c), (d) is a separate estimation of the impact of a billion dollar disaster on origination rates. For each point, we weigh observations according to the distance to the conforming loan limit.

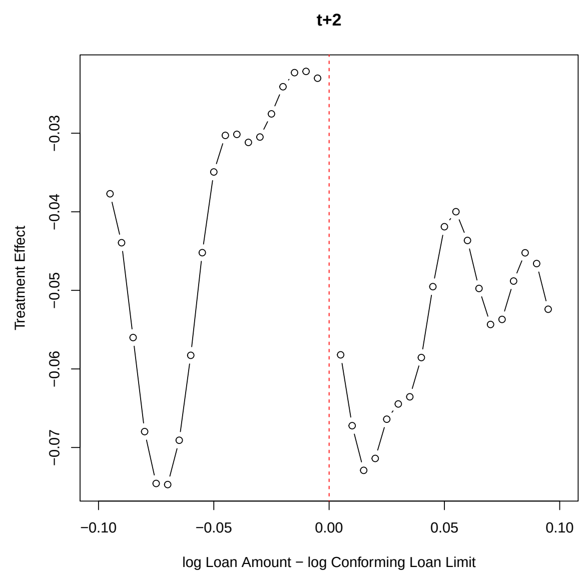
$$Originated_{it} = \sum_{t=-4}^{+4} \xi_t \cdot Treated_{j(i)} \times Time_{t=y-y_0(d)} + Year_{y(t,d)} + Disaster_d + ZIP_{j(i)} + \varepsilon_{it}$$

The weights are the kernel $K((\Delta \log Loan\ Amount - Distance\ to\ Conforming\ Loan\ Limit)/h)$*, where each of the 40 regressions uses a different* Distance to the Conforming Loan Limit $\in [-0.10, 0.10]$*. These regressions display flexible results free of assumptions on rounding or on the choice of the window. Figures suggest that (a) the discontinuity at the limit is key to the results, (b) the discontinuity is sharp in time* $t = 1, 2, 3$*, and smoother in* $t = 4$*. Tests of statistical significance are performed on Table F.*

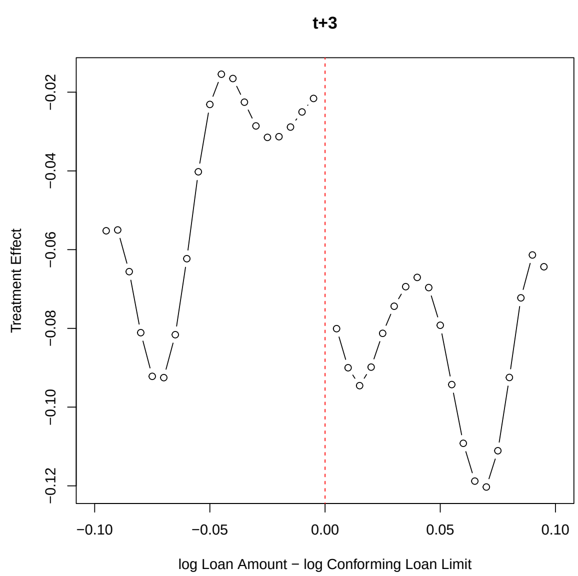
(a) Originated, time $t + 1$



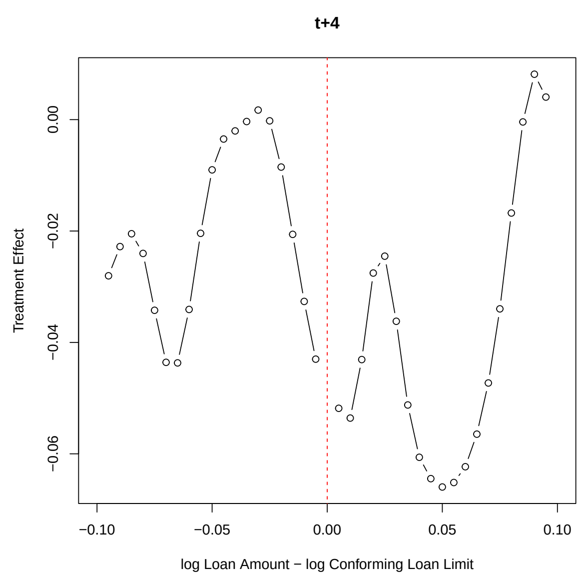
(b) Originated, time $t + 2$



(c) Originated, time $t + 3$



(d) Originated, time $t + 4$



Open source code available at this link.

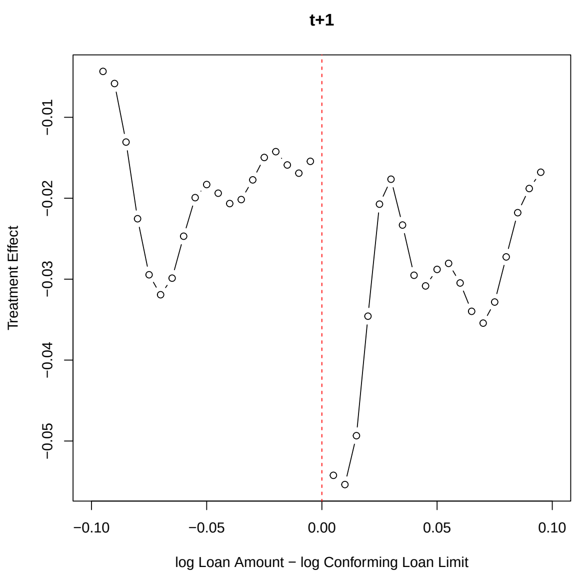
Figure H: Inspecting the Mechanism: Treatment Effect by Distance to the Conforming Loan Limit

The graphs below inspect the mechanism driving the results presented in Figure F using local polynomial regressions. Each point in Subfigure (a), (b), (c), (d) is a separate estimation of the impact of a billion dollar disaster on approval rates. For each point, we weigh observations according to the distance to the conforming loan limit.

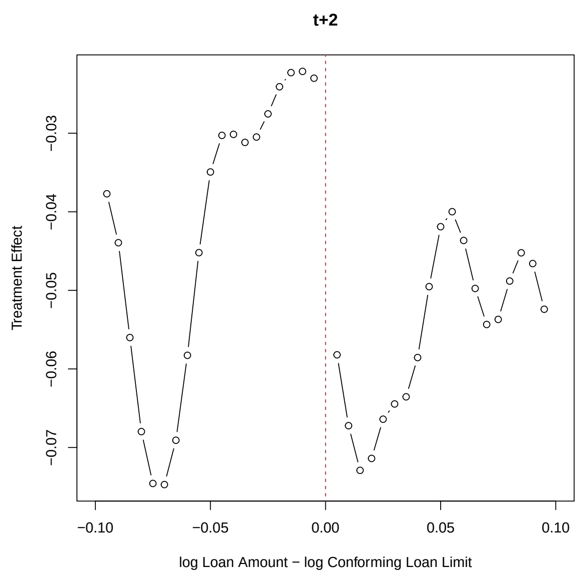
$$Approval_{it} = \sum_{t=-4}^{+4} \xi_t \cdot Treated_{j(i)} \times Time_{t=y-y_0(d)} + Year_{y(t,d)} + Disaster_d + ZIP_{j(i)} + \varepsilon_{it}$$

The weights are the kernel $K((\Delta \log \mathit{Loan\,Amount} - \mathit{Distance\,to\,Conforming\,Loan\,Limit})/h)$*, where each of the 40 regressions uses a different* Distance to the Conforming Loan Limit $\in [-0.10, 0.10]$ *and the bandwidth* $h = 1\%$*. These regressions display flexible results free of assumptions on rounding or on the choice of the window. Figures suggest that (a) the discontinuity at the limit is key to the results, (b) the discontinuity is sharp in time* $t = 1, 2, 3$*, and smoother in* $t = 4$*. Tests of statistical significance are performed on Table E.*

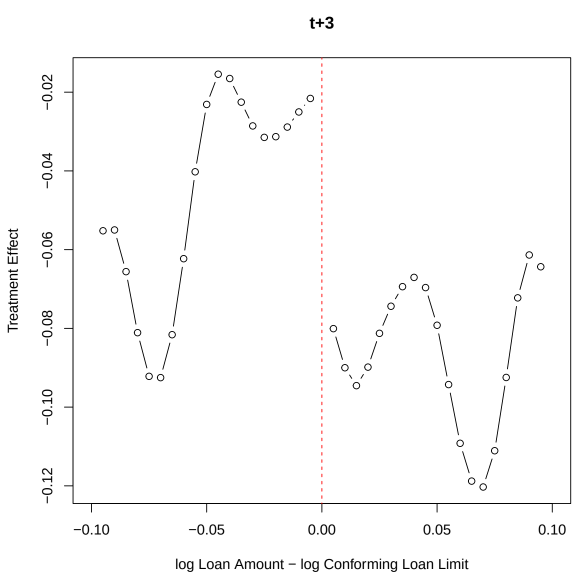
(a) Approved, time $t+1$



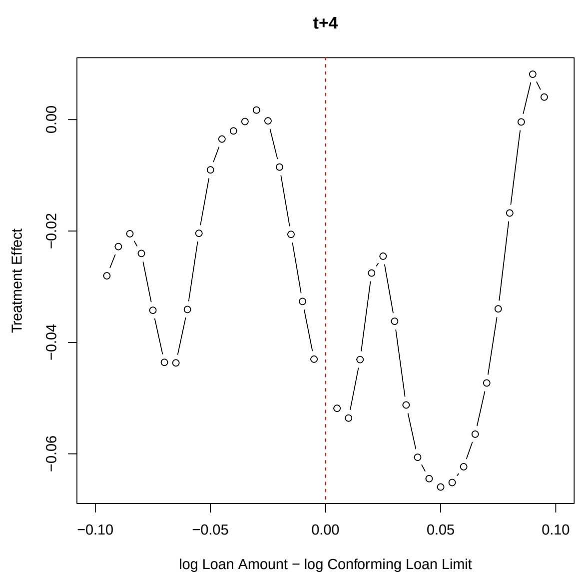
(b) Approved, time $t+2$



(c) Approved, time $t+3$



(d) Approved, time $t+4$



Open source code available at this link.

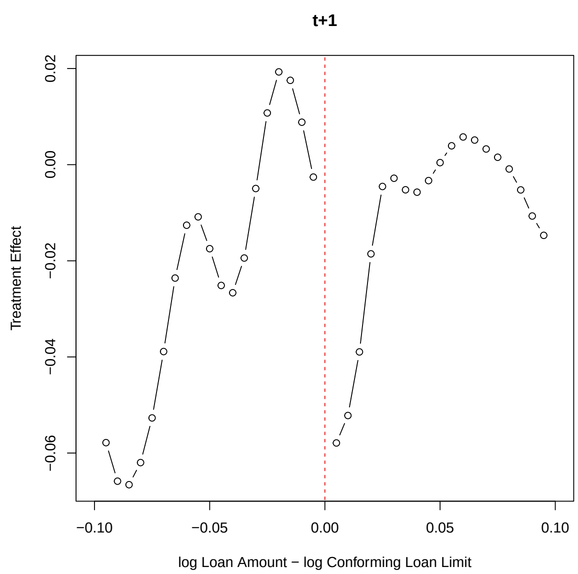
Figure I: What Drives Results? Inspecting the Mechanism: Treatment Effect by Distance to the Conforming Loan Limit

The graphs below inspect the mechanism driving the results presented in Figure F using local polynomial regressions. Each point in Subfigure (a), (b), (c), (d) is a separate estimation of the impact of a billion dollar disaster on GSE securitization rates for the sample of originated mortgages. For each point, we weigh observations according to the distance to the conforming loan limit.

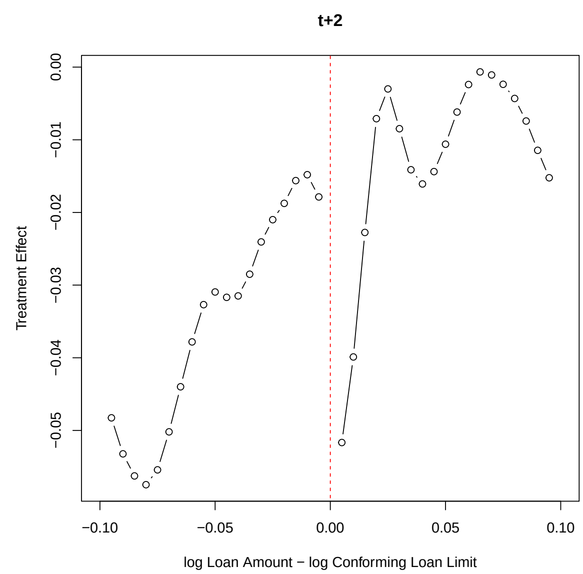
$$\textit{Securitized Conditional on Origination}_{it} = \sum_{t=-4}^{+4} \xi_t \cdot \textit{Treated}_{j(i)} \times \textit{Time}_{t=y-y_0(d)} + \textit{Year}_{y(t,d)} + \textit{Disaster}_d + \textit{ZIP}_{j(i)} + \varepsilon_{it}$$

The weights are the kernel $K((\log \textit{Loan Amount} - \textit{Distance to Conforming Loan Limit})/h)$*, where each of the 40 regressions uses a different* Distance to the Conforming Loan Limit $\in [-0.10, 0.10]$. *These regressions display flexible results free of assumptions on rounding or on the choice of the window. Figures suggest that (a) the discontinuity at the limit is key to the results, (b) the discontinuity is sharp and growing in every time period* $t = 1 \ldots 4$. *Tests of statistical significance are performed on Table G.*

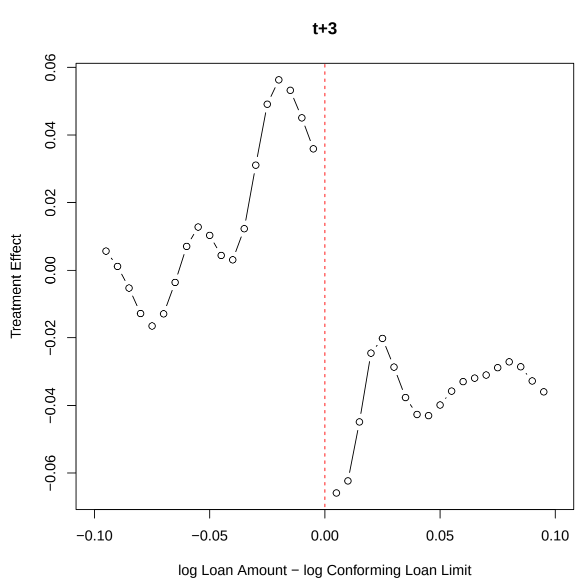
(a) Securitized Conditional on Originated, time $t+1$



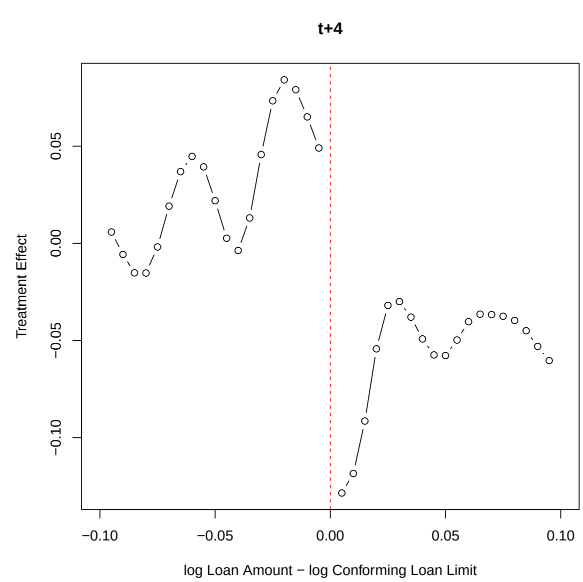
(b) Securitized Conditional on Originated, time $t+2$



(c) Securitized Conditional on Originated, time $t+3$



(d) Securitized Conditional on Originated, time $t+4$

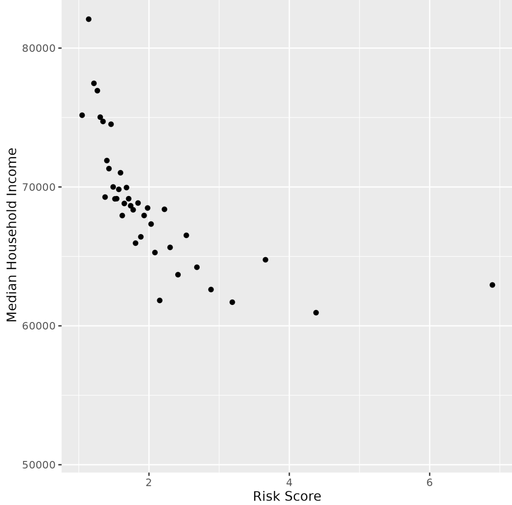


Open source code available at this link.

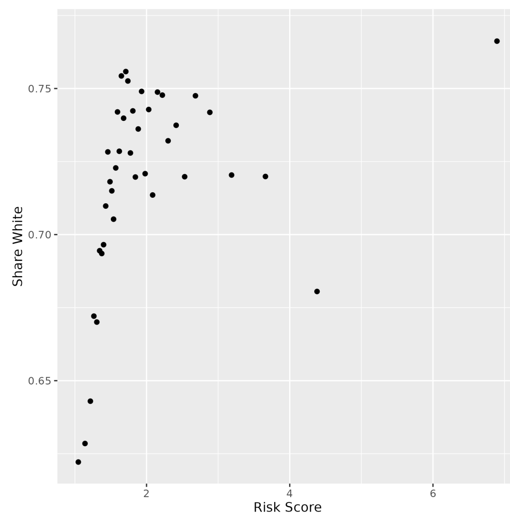
Figure J: Would a Geographic Pricing of G-Fees Imply Redlining? First Street Risk Factor and Zip-Level Demographics

These four figures display census demographics at the ZIP level against the average ZIP-level First Street Flood Risk Score. Demographics from the 2020 Census accessed through the National Historical Geographic Information System of the University of Minnesota. Each code indicates the corresponding Census table. Each plot is binned using 40 quantiles of the risk score.

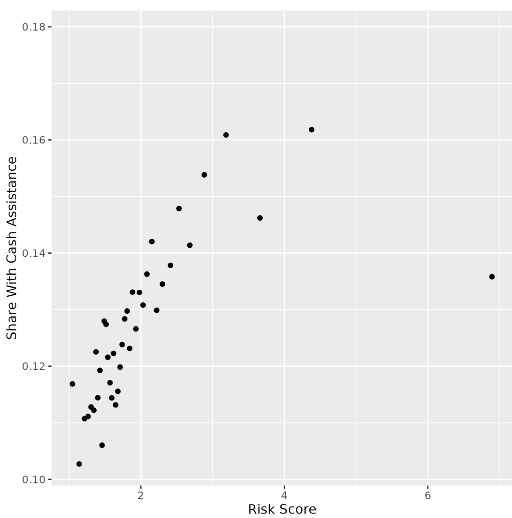
(a) Household Income (AMR8E001)



(b) Share White (B02001)



(c) Share on Cash Public Assistance or Food Stamps/SNAP (B19058)



(d) Rent as Share of Household Income (B25071)

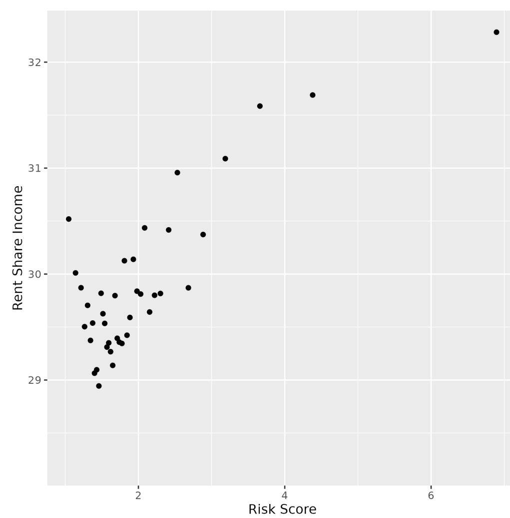


Table E: Regression Discontinuity Estimate of the Impact of Billion-Dollar Disasters on Approval Probabilities

Dependent Variable:	Approved = 0,1			
Bandwidth:	±1%	±2%	±3%	±4%
Treated × Time 0	-0.0195**	-0.0213**	-0.0209*	-0.0198*
	(0.0084)	(0.0098)	(0.0102)	(0.0101)
Treated × Time +1	-0.0507***	-0.0486***	-0.0455***	-0.0432***
	(0.0111)	(0.0105)	(0.0109)	(0.0111)
Treated × Time +2	-0.0598**	-0.0596***	-0.0575***	-0.0548***
	(0.0220)	(0.0156)	(0.0141)	(0.0136)
Treated × Time +3	-0.0755**	-0.0719**	-0.0717***	-0.0731***
	(0.0300)	(0.0245)	(0.0232)	(0.0225)
Treated × Time +4	-0.0565	-0.0478	-0.0457	-0.0451
	(0.0461)	(0.0383)	(0.0337)	(0.0303)
Treated × Time 0 × Below Limit	-0.0037	0.0011	0.0027	0.0028
	(0.0141)	(0.0115)	(0.0101)	(0.0096)
Treated × Time 1 × Below Limit	0.0383**	0.0348***	0.0300***	0.0268***
	(0.0149)	(0.0080)	(0.0061)	(0.0067)
Treated × Time 2 × Below Limit	0.0526*	0.0501***	0.0425***	0.0360***
	(0.0275)	(0.0162)	(0.0117)	(0.0100)
Treated × Time 3 × Below Limit	0.0648**	0.0609***	0.0583***	0.0570***
	(0.0270)	(0.0188)	(0.0160)	(0.0146)
Treated × Time 4 × Below Limit	0.0208	0.0264	0.0284	0.0291
	(0.0430)	(0.0351)	(0.0297)	(0.0259)
Year × Below Limit	Yes	Yes	Yes	Yes
Zip × Below Limit	Yes	Yes	Yes	Yes
Disaster × Below Limit	Yes	Yes	Yes	Yes
Observations	2,572,574	2,572,574	2,572,574	2,572,574
R^2	0.07088	0.07076	0.06983	0.06887
Dependent Variable:	Approved = 0,1			
Bandwidth:	±5%	±10%	±15%	±20%
Treated × Time 0	-0.0182*	-0.0127	-0.0115	-0.0111
	(0.0097)	(0.0075)	(0.0067)	(0.0064)
Treated × Time +1	-0.0408***	-0.0316***	-0.0287***	-0.0277***
	(0.0107)	(0.0086)	(0.0080)	(0.0078)
Treated × Time +2	-0.0524***	-0.0450***	-0.0428***	-0.0420***
	(0.0131)	(0.0127)	(0.0133)	(0.0135)
Treated × Time +3	-0.0737***	-0.0740***	-0.0743***	-0.0745***
	(0.0218)	(0.0215)	(0.0218)	(0.0220)
Treated × Time +4	-0.0442	-0.0396	-0.0384	-0.0381
	(0.0281)	(0.0252)	(0.0249)	(0.0249)
Treated × Time 0 × Below Limit	0.0022	-0.0019	-0.0036	-0.0042
	(0.0089)	(0.0062)	(0.0054)	(0.0051)
Treated × Time 1 × Below Limit	0.0242***	0.0140**	0.0103*	0.0088
	(0.0066)	(0.0056)	(0.0055)	(0.0056)
Treated × Time 2 × Below Limit	0.0310***	0.0176***	0.0134***	0.0118**
	(0.0087)	(0.0047)	(0.0045)	(0.0048)
Treated × Time 3 × Below Limit	0.0553***	0.0465***	0.0428***	0.0413***
	(0.0136)	(0.0105)	(0.0094)	(0.0090)
Treated × Time 4 × Below Limit	0.0289	0.0256	0.0241	0.0236
	(0.0234)	(0.0194)	(0.0186)	(0.0183)
Year × Below Limit	Yes	Yes	Yes	Yes
Zip × Below Limit	Yes	Yes	Yes	Yes
Disaster × Below Limit	Yes	Yes	Yes	Yes
Observations	2,572,574	2,572,574	2,572,574	2,572,574
R^2	0.06790	0.06482	0.06413	0.06398

Clustered (ZCTA5CE10 & year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table F: Regression Discontinuity Estimate of the Impact of Billion-Dollar Disasters on Origination Probabilities

Dependent Variable:	Originated = 0,1			
Bandwidth:	±1%	±2%	±3%	±4%
Treated × Time 0	-0.0153	-0.0204	-0.0211	-0.0202
	(0.0146)	(0.0136)	(0.0128)	(0.0124)
Treated × Time +1	-0.0655***	-0.0627***	-0.0568***	-0.0527***
	(0.0155)	(0.0114)	(0.0094)	(0.0091)
Treated × Time +2	-0.0491	-0.0649***	-0.0665***	-0.0653***
	(0.0336)	(0.0176)	(0.0135)	(0.0131)
Treated × Time +3	-0.0700**	-0.0848**	-0.0869**	-0.0891***
	(0.0328)	(0.0325)	(0.0311)	(0.0301)
Treated × Time +4	-0.0431	-0.0455	-0.0477	-0.0488
	(0.0532)	(0.0491)	(0.0436)	(0.0385)
Treated × Time 0 × Below Limit	-0.0135	-0.0021	0.0027	0.0039
	(0.0258)	(0.0215)	(0.0177)	(0.0155)
Treated × Time 1 × Below Limit	0.0457	0.0427**	0.0356**	0.0304**
	(0.0261)	(0.0177)	(0.0125)	(0.0106)
Treated × Time 2 × Below Limit	0.0246	0.0408	0.0393**	0.0349**
	(0.0434)	(0.0244)	(0.0171)	(0.0143)
Treated × Time 3 × Below Limit	0.0501	0.0607**	0.0608**	0.0604***
	(0.0320)	(0.0262)	(0.0218)	(0.0194)
Treated × Time 4 × Below Limit	-0.0083	0.0060	0.0140	0.0177
	(0.0508)	(0.0432)	(0.0355)	(0.0290)
Year × Below Limit	Yes	Yes	Yes	Yes
Zip × Below Limit	Yes	Yes	Yes	Yes
Disaster × Below Limit	Yes	Yes	Yes	Yes
Observations	2,572,574	2,572,574	2,572,574	2,572,574
R^2	0.06821	0.06919	0.06900	0.06849
Dependent Variable:	Originated = 0,1			
Bandwidth:	±5%	±10%	±15%	±20%
Variables				
Treated × Time 0	-0.0186	-0.0114	-0.0089	-0.0079
	(0.0120)	(0.0087)	(0.0072)	(0.0066)
Treated × Time +1	-0.0495***	-0.0382***	-0.0341***	-0.0325***
	(0.0088)	(0.0073)	(0.0070)	(0.0070)
Treated × Time +2	-0.0641***	-0.0611***	-0.0601***	-0.0598***
	(0.0131)	(0.0138)	(0.0146)	(0.0149)
Treated × Time +3	-0.0901***	-0.0903***	-0.0907***	-0.0909***
	(0.0292)	(0.0282)	(0.0283)	(0.0284)
Treated × Time +4	-0.0480	-0.0396	-0.0362	-0.0350
	(0.0348)	(0.0281)	(0.0268)	(0.0264)
Treated × Time 0 × Below Limit	0.0036	-0.0022	-0.0052	-0.0065
	(0.0140)	(0.0091)	(0.0077)	(0.0073)
Treated × Time 1 × Below Limit	0.0267**	0.0143	0.0095	0.0075
	(0.0100)	(0.0085)	(0.0086)	(0.0088)
Treated × Time 2 × Below Limit	0.0312**	0.0230**	0.0207**	0.0199**
	(0.0132)	(0.0097)	(0.0090)	(0.0091)
Treated × Time 3 × Below Limit	0.0586***	0.0483***	0.0446**	0.0432**
	(0.0177)	(0.0151)	(0.0152)	(0.0154)
Treated × Time 4 × Below Limit	0.0184	0.0115	0.0069	0.0048
	(0.0244)	(0.0156)	(0.0140)	(0.0136)
Year × Below Limit	Yes	Yes	Yes	Yes
Zip × Below Limit	Yes	Yes	Yes	Yes
Disaster × Below Limit	Yes	Yes	Yes	Yes
Observations	2,572,574	2,572,574	2,572,574	2,572,574
R^2	0.06785	0.06584	0.06548	0.06545

Clustered (ZCTA5CE10 & year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table G: Regression Discontinuity Estimate of the Impact of Billion-Dollar Disasters on Securitization Probabilities, Conditional on Origination

Dependent Variable:	Securitized = 0,1			
Bandwidth:	±1%	±2%	±3%	±4%
Treated × Time 0	-0.0183	-0.0253	-0.0290*	-0.0296*
	(0.0150)	(0.0149)	(0.0146)	(0.0141)
Treated × Time +1	-0.0604***	-0.0594***	-0.0560**	-0.0506**
	(0.0176)	(0.0191)	(0.0198)	(0.0197)
Treated × Time +2	-0.0572***	-0.0483*	-0.0436*	-0.0384
	(0.0180)	(0.0228)	(0.0232)	(0.0227)
Treated × Time +3	-0.0594*	-0.0671**	-0.0661**	-0.0627**
	(0.0307)	(0.0287)	(0.0281)	(0.0277)
Treated × Time +4	-0.1310***	-0.1272***	-0.1199***	-0.1116***
	(0.0438)	(0.0316)	(0.0285)	(0.0269)
Treated × Time 0 × Below Limit	0.0083	0.0193	0.0208	0.0193
	(0.0292)	(0.0232)	(0.0183)	(0.0155)
Treated × Time 1 × Below Limit	0.0466	0.0567*	0.0496	0.0398
	(0.0314)	(0.0309)	(0.0299)	(0.0288)
Treated × Time 2 × Below Limit	0.0342	0.0290	0.0202	0.0118
	(0.0234)	(0.0259)	(0.0268)	(0.0269)
Treated × Time 3 × Below Limit	0.0872**	0.1042***	0.1002***	0.0933***
	(0.0372)	(0.0262)	(0.0249)	(0.0246)
Treated × Time 4 × Below Limit	0.1663***	0.1773***	0.1688***	0.1583***
	(0.0440)	(0.0413)	(0.0418)	(0.0432)
Year × Below Limit	Yes	Yes	Yes	Yes
Zip × Below Limit	Yes	Yes	Yes	Yes
Disaster × Below Limit	Yes	Yes	Yes	Yes
Observations	2,049,035	2,049,035	2,049,035	2,049,035
R^2	0.06641	0.07239	0.07727	0.08178

Dependent Variable:	Securitized = 0,1			
Bandwidth:	±5%	±10%	±15%	±20%
Treated × Time 0	-0.0292**	-0.0271*	-0.0263	-0.0259
	(0.0136)	(0.0147)	(0.0158)	(0.0163)
Treated × Time +1	-0.0456**	-0.0340*	-0.0309	-0.0297
	(0.0192)	(0.0187)	(0.0192)	(0.0195)
Treated × Time +2	-0.0342	-0.0270	-0.0254	-0.0249
	(0.0222)	(0.0221)	(0.0225)	(0.0226)
Treated × Time +3	-0.0594**	-0.0520*	-0.0501*	-0.0494*
	(0.0273)	(0.0273)	(0.0277)	(0.0279)
Treated × Time +4	-0.1045***	-0.0882***	-0.0830***	-0.0809**
	(0.0262)	(0.0267)	(0.0274)	(0.0277)
Treated × Time 0 × Below Limit	0.0174	0.0125	0.0117	0.0115
	(0.0136)	(0.0117)	(0.0121)	(0.0124)
Treated × Time 1 × Below Limit	0.0313	0.0157	0.0139	0.0136
	(0.0273)	(0.0226)	(0.0216)	(0.0214)
Treated × Time 2 × Below Limit	0.0057	-0.0024	-0.0027	-0.0027
	(0.0267)	(0.0243)	(0.0229)	(0.0224)
Treated × Time 3 × Below Limit	0.0867***	0.0688**	0.0638**	0.0621**
	(0.0246)	(0.0263)	(0.0276)	(0.0282)
Treated × Time 4 × Below Limit	0.1487***	0.1226**	0.1146**	0.1115*
	(0.0448)	(0.0502)	(0.0523)	(0.0532)
Year × Below Limit	Yes	Yes	Yes	Yes
Zip × Below Limit	Yes	Yes	Yes	Yes
Disaster × Below Limit	Yes	Yes	Yes	Yes
Observations	2,049,035	2,049,035	2,049,035	2,049,035
R^2	0.08595	0.09977	0.10505	0.10724

Clustered (ZCTA5CE10 & year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*