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Abstract

This paper investigates the Deepwater Horizon oil spill and its mortgage lending implications for the affected Gulf coast areas. We construct a unique dataset by using information from the Shoreline Cleanup Assessment Technique program and mapping those observations to the Home Mortgage Disclosure Act lending dataset. Our difference-in-differences estimation results show that denial rates of mortgage applications rise between 2 and 6% as a result of the Deepwater Horizon oil spill, whilst accounting for fixed effects at the lender and census tract levels. We also find that the effect is larger on refinance mortgages compared to home purchase mortgages, and national banks respond more aggressively compared to other mortgage lenders.

JEL Classification: G21, Q53, R11

Keywords: Mortgage Lending, Environmental Damage, Oil Spill, Difference-in-Differences

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1 Introduction

An environmental disaster occurred in the Gulf of Mexico, as an explosion on the mobile offshore drilling unit Deepwater Horizon caused the oil platform to capsize and sink. The Deepwater Horizon oil spill, also referred to as the BP oil spill, began on April 20, 2010 and continued 87 days until it was capped on July 15, 2010. However, during this 87-day period, four million barrels of oil were released into the Gulf of Mexico. The disaster created significant environmental damage to the whole region, covering several states across the adjoined shorelines; see Figure 1.

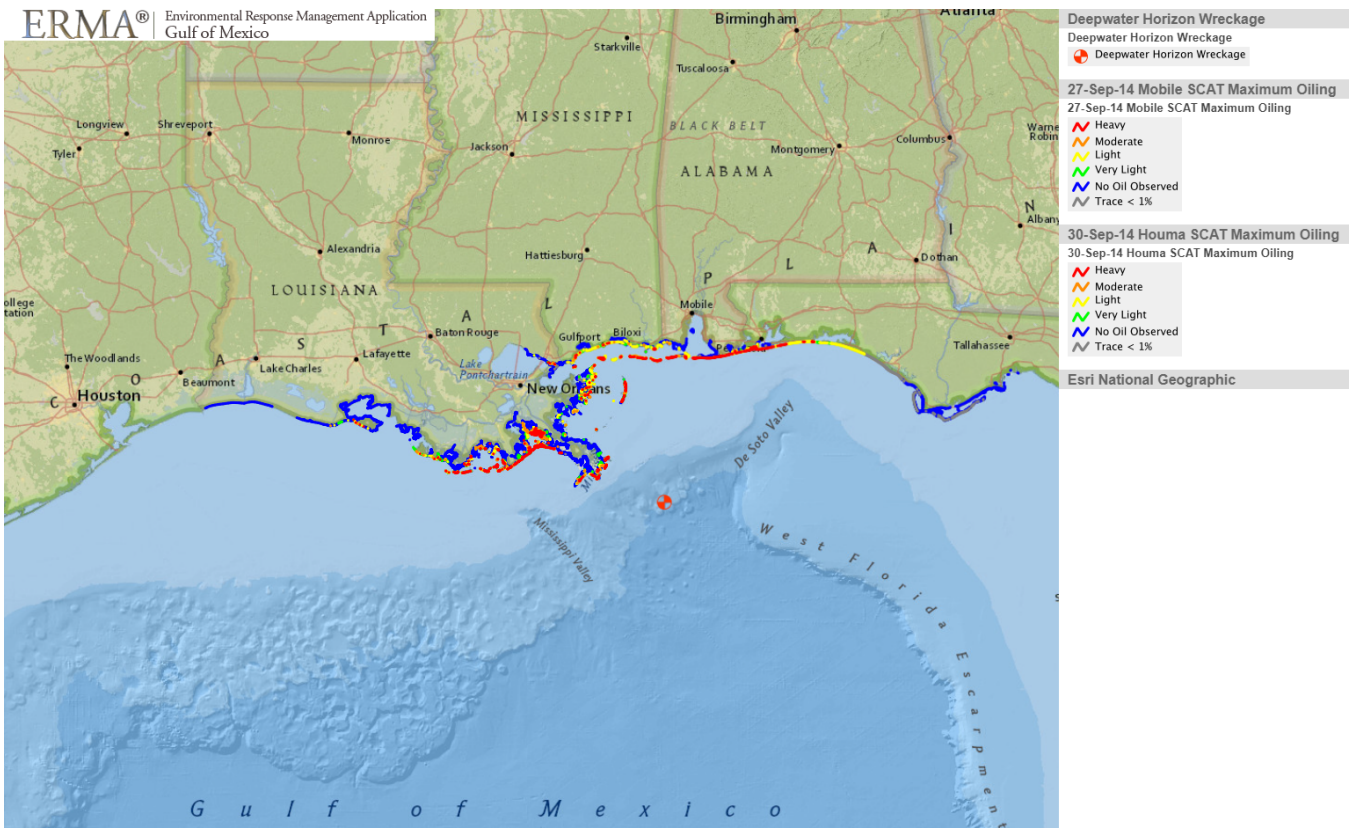


Figure 1: The plot depicts the affected areas along the Gulf coastline together with the Deepwater Horizon wellhead located in the Gulf of Mexico. The full set of coordinates can be retrieved from the Environmental Response Management Application website which also implements the Shoreline Cleanup Assessment Technique dataset that records the oil spill affected areas.

While the estimated environmental costs of this disaster are unprecedented, the Deepwater Horizon oil spill has left behind more than environmental damage. According to a report

released in August 2010 by Clear Capital, a real estate data provider, 23.8% of the surveyed real estate brokers reported a negative impact on their markets due to the oil spill and more than half of those stating a negative effect also reported a decrease in housing values by 5-15%.[1] In academia, Cano-Urbina et al. (2019) provide the first comprehensive evidence on the economic impact of the Deepwater Horizon oil spill on residential housing markets along the coastline of the Gulf of Mexico. The authors report the negative and significant effect of this disamenity on home values as 4-8%, which translates to $3.8 - $5.0 billion costs of damage borne by housing markets. Academics and policymakers are progressively alarmed that such disasters can lead to a sudden decline in collateral values in the housing market and create fluctuations in consumption and borrowing capacity of households (Campbell and Cocco, 2007). As a result, lending responses of banks with respect to the increased liquidity needs of communities in the affected areas set the stage for the speed of local economic recovery in the housing market (Allen et al., 2022). Following this line of thought, this paper approaches the Deepwater Horizon oil spill event, one of the largest environmental disasters in world history, as a quasi-natural experiment and examines the mortgage lending implications of this sudden and rare disaster.

To the best of our knowledge, we are the first to investigate the effects of the Deepwater Horizon oil spill on mortgage lending and therefore fill a gap in the existing literature on this topic. Our analysis not only provides evidence on how the BP oil spill altered bank lending, but also shows that oil catastrophes imply significant negative economic externalities. We make use of the Shoreline Cleanup Assessment Technique (SCAT) data, which surveyed over 4,300 miles of the Gulf Coast, and link the affected segments to census tract data via their proximity to the oil polluted areas. We do this by retrieving the Topologically Integrated Geographic Encoding and Referencing (TIGER) shapefiles from the U.S. Census Bureau for the states of Alabama, Mississippi, Texas, Georgia, Florida, and Louisiana. Each shapefile contains information about the boundary of a census tract in the form of latitude and longitude coordinates. We then

[1] See https://www.clearcapital.com/impact-of-bp-oil-spill-reaches-beyond-gulf-coast/ for the report.

compute the distances between all boundary points of a census tract and each polluted segment recorded in the SCAT survey. The affected census tracts are being grouped into various distance brackets according to their calculated minimum proximity to the polluted shore area. In a final step, we match these census tracts with those in the Home Mortgage Disclosure Act (HMDA) dataset. This allows us to comment on the magnitude of the bank lending effect in relation to an area's proximity to oil pollution. For each census tract we extract a rich set of specific mortgage loan application information, which forms the basis of our difference-in-differences (DID) estimation.

Allowing for a rich set of controls, our estimation results draw a clear and unambiguous picture. Compared to unaffected areas, regions located within 150 miles from the oil polluted coastline experience a statistically significant increase in mortgage denial rates between 2 and 6%. Furthermore, this impact is long-lasting, which reaches its peak two years after the oil spill and stays significant even seven years later. Our estimation results also indicate that the denial rate of refinance mortgage applications, relative to home purchases, rises by an additional 2-3% in areas that lie within 25 miles of the polluted coastline. Moreover, we observe that national banks exhibit a much stronger lending contraction compared to other mortgage lenders, including regional and community banks, credit unions, and other private lenders.

Our paper adds to the existing literature on the BP oil spill and its adverse economic effects. Cano-Urbina et al. (2019) investigate the implications of the Deepwater Horizon oil spill on house prices in the affected coastal regions. Using ZIP code level housing price data from the Federal Housing Finance Agency, the study's estimation results show that the Deepwater Horizon event caused a significant drop in house prices between 4 and 8%. The estimates imply a lower bound of the oil spill damage between $3.8 and $5.0 billion. Furthermore, the authors observe that the negative impact on house prices is more pronounced in housing markets that are located in close proximity to the coastal regions of the Gulf of Mexico. Siegel et al. (2013) and Winkler and Gordon (2013) also study the housing price implications of the BP oil spill, but focus on the price of coastal condominiums in the state of Alabama. While Siegel

et al. (2013) report a 12.1% decrease in condominium sale prices (per square foot), as a result of the BP oil spill, Winkler and Gordon (2013) find a 7-8.8% reduction in sale prices and a 50% drop in the sales volume from July through December 2010. Both studies analyze the implied BP oil spill effects on house prices over a shorter time horizon and focus on either specific regions or cities within the state of Alabama, instead of the entire Gulf coast region. There are also studies which have analyzed the BP oil spill outside the housing market context. Whitehead et al. (2016) measure the impact on tourism data on canceled recreational trips to Northwest Florida with estimated aggregate damages in the region of $207 million. Aldy (2014) quantifies the heterogeneous oil spill implications on wages and employment in the Gulf Coast, whereas Barrage et al. (2014) study the advertising impacts on consumer responses to news about unobserved product quality.[2]

Although the Deepwater Horizon oil spill is an industrial rather than natural disaster, the economic ramifications share some similarities. Hence, our paper is also related to the growing literature on bank lending implications of natural disasters. It is important to understand the lending responses of banks following a disaster, as financial intermediaries face increased liquidity needs of affected local communities and a higher exposure to liquidity risk. Furthermore, lenders' responses to increased demand for loans are crucial in determining the relationship between credit booms and financial instability and banks are more likely to default due to the increased bank stress, insolvency, non-performing loans, leverage, and even bank run risk after a natural disaster (Dell'Ariccia et al., 2012; Klomp, 2014; Noth and Schüwer, 2018). We add to the prior line of empirical papers, which report a negative impact of natural disasters on bank lending and tightened lending standards for residential mortgages (Berg and Schrader, 2012; Choudhary and Jain, 2017; Duanmu et al., 2022; Keys and Mulder, 2020; Nguyen et al., 2020; Xu

[2]Our paper is also indirectly related to the growing literature on climate change and its real estate implications. Baldauf et al. (2020) find that residential real estate prices reflect differences in beliefs about climate changes. In particular, homes located in neighborhoods where the majority of residents believe that climate change is happening sell for about 7% less than those in other neighborhoods. Similarly, Bernstein et al. (2019) also find that homes exposed to sea level rise sell for about 7% less than their equivalent unexposed counterparts. In contrast to these studies, Murfin and Spiegel (2020), by exploiting cross-sectional differences in relative sea level rise, find no price effects.

and Xu, 2020). Our results are supportive of the heterogeneous negative impact on bank lending across geographic regions depending on the proximity to the center of the disaster (Berg and Schrader, 2012; Nguyen et al., 2020). Our finding of the long-lasting, negative impact on mortgage lending is of great importance to the speed of local economic recovery in the housing market after the oil spill disaster.

Furthermore, our findings of heterogeneous responses of different types of mortgage lenders add to the line of literature examining the heterogeneity in lending responses of nationally operating banks versus local banks, following a natural disaster (Berg and Schrader, 2012; Chavaz, 2016; Keys and Mulder, 2020; Nguyen and Wilson, 2020). These results are significant in understanding the heterogeneous impacts of natural disasters on bank performance, credit risk, equity capital, and solvency of geographically diversified versus locally operating banks (Koetter et al., 2020; Walker et al., 2022). They also imply that local lenders play an important role in the recovery of affected housing markets (Allen et al., 2022), due to their better access to soft information which leads to higher mortgage originations and lower interest rate spreads (Ergungor, 2010).

Last but not least, our observation that lending is even further reduced for refinance mortgages, relative to home purchase mortgages, has important implications for financial stability due to the "ratchet effect" of refinancing, as noted by Khandani et al. (2013). The ratchet effect signifies the inability of homeowners to decrease leverage symmetrically during periods of declining home prices (for instance, due to a catastrophic event like oil spill) as their ability to increase leverage by refinancing during housing booms. This situation can therefore lead to an unintended simultaneity in homeowner leverage.

The outline of this paper is as follows. Section 2 surveys related literature and develops our main hypotheses. Section 3 describes the construction of the dataset and empirical identification strategy. Section 4 provides empirical evidence on the impact of Deepwater Horizon oil spill on mortgage lending together with a series of robustness checks. Section 5 conducts further analyses regarding refinance versus home purchase loans and national banks versus

other lenders. Section 6 concludes the paper.

2 Related literature and hypothesis development

Prior evidence on bank lending implications of natural disasters is mixed. On the one hand, some studies report that banks decrease access to credit in affected regions by tightening lending standards and therefore declining more loan applications (Berg and Schrader, 2012; Choudhary and Jain, 2017; Duanmu et al., 2022; Keys and Mulder, 2020; Nguyen et al., 2020; Thomas, 2001; Xu and Xu, 2020). One prominent reason is the potential loan loss for lenders after the decline in collateral values, bank capital, and repayment capabilities of borrowers (Bos et al., 2022). Also, banks may face the liquidity risk of synchronous deposit withdrawals and increased drawdowns on lines of credit that reduce the availability of funds for lending (Nguyen et al., 2020). On the other hand, some past evidence states that banks increase access to lending in affected areas to meet the increased demand for loans in those localities. One possible channel for banks is to cut their lending to unaffected areas in which bank branches are not located (Cortés and Strahan, 2017; Ivanov et al., 2022). Another approach followed by banks is to finance the increased demand for loans in the affected areas with the proceeds from government security sales (Bos et al., 2022), loan sales (Cortés and Strahan, 2017) or by managing their balance sheets (Allen et al., 2022; Cortés and Strahan, 2017). Increased capital inflow from government subsidies for natural disasters and insurance payments also add to the lending capacity of banks (Ivanov et al., 2022).

Besides charging higher interest rates (Nguyen et al., 2020), one particular course of action taken by lenders is to tighten lending standards in order to tackle the increased risk resulting from reduced collateral values after a disaster has occured. For instance, Berg and Schrader (2012) analyze the access to credit in Ecuador after volcanic activity and find that loan approval rates drop dramatically. Similarly, Duanmu et al. (2022) find that banks reduce loan acceptance rates in counties where a natural disaster hit due to the increased credit risk perceptions of loan

officers. Also, Thomas (2001) notes that environmental pollution magnifies the risk perception of commercial and industrial mortgage lenders and leads to a decline in loan origination. Likewise, Xu and Xu (2020) document the decline in mortgage origination in areas after pipeline hazards.

The observed decline in home prices after the Deepwater Horizon oil spill (Cano-Urbina et al., 2019) leads therefore to a reduction in collateral values, which in turn can create negative loan equity and increase the loan-to-value ratio. As option theory models state, borrowers might strategically default on their mortgages as a put option, when they have negative home equity (Foster and Van Order, 1984). Moreover, deteriorating local economic conditions after a disaster may also contribute to these defaults. Increased likelihood of borrower default and higher foreclosure rates can heighten the credit risk of mortgage lending. Consequently, in our first hypothesis, we theorize that:

H1: Following the Deepwater Horizon oil spill, denial rates of mortgage loan applications in affected regions increase relative to those in the unaffected regions.

Next, we conjecture that lenders deny refinance mortgage applications more aggressively than those of home purchases in affected regions after the oil spill. Borrowers usually refinance their mortgage loans to secure a better interest rate and payment deal or borrow funds against their properties; see the Residential Finance Survey conducted by the U.S. Census Bureau. However, given the decline in the collateral value, increase in the loan-to-value ratio, and possible negative loan equity after the oil spill, lenders would be less willing to grant lower interest rate deals and cash out opportunities to refinancers (Caplin et al., 1997).[3] For instance, if the collateral damage due to the disaster is greater than the accumulated (positive) home equity before the disaster, the equity buffer absorbing the losses from the house price decline will not be sufficient to cover the value damage. Hence, the loan-to-value ratio will increase and create additional risk for the lender when it comes to the refinance mortgage application.

[3]However, it is important to note that lenders may not adjust interest rates or lower the loan amounts in refinancing transactions due to their upward biased house price valuations and underestimations of risk in regions with a high risk of natural disasters (Bakkensen and Barrage, 2021; Garbarino and Guin, 2021).

Furthermore, households lack flexibility of deleveraging since refinancing works like a locked ratchet during times of declining house prices, due to indivisibility and occupant-ownership of residential real estate (Khandani et al., 2013). For these reasons, lenders consider refinance mortgages more risky than home purchase mortgages (Dell'Ariccia et al., 2012; Munnell et al., 1996). Therefore, we theorize that:

H2: Following the Deepwater Horizon oil spill, denial rates of refinance loan applications increase more than those of home purchase loan applications in the affected regions.

Finally, we investigate whether there is heterogeneity in lending responses of national banks versus various other financial intermediaries, such as regional and community banks, credit unions, and other private lenders. National banks may have more sophisticated models to evaluate and analyze natural disaster risk (Stiroh, 2020). However, local banks have an information advantage in local areas and communities (Allen et al., 2022; Garmaise and Moskowitz, 2009). Also, they tend to have closer proximity and stronger relationships with local customers via their local branches, enabling them to better assess borrower quality and collateral value using soft information (Agarwal and Hauswald, 2010; Cortés and Strahan, 2017; Duanmu et al., 2022; Nguyen and Wilson, 2020). Therefore, local lenders are more likely to be in superior position compared to national lenders in dealing with the disaster risk without further increasing the denial rate or tightening the lending standards (Agarwal and Hauswald, 2010; Duanmu et al., 2022; Nguyen and Wilson, 2020). In addition, banks that do not have well-built relationships with their borrowers are more likely to suffer from information asymmetry during highly uncertain times like natural disasters (Bolton et al., 2016). Due to these factors, geographically diversified national banks may have higher denial rates and tighter lending standards for mortgage applications compared to other financial intermediaries. For instance, Chavaz (2016) find that local banks issue a higher share of new mortgage loans in disaster-hit areas relative to diversified banks. Similarly, Berg and Schrader (2012) report that informational advantage of local borrowers and bank-borrower relationships can lower the lending restrictions after natural

disasters. Furthermore, Duanmu et al. (2022) show that natural disasters bring about tighter lending standards among diversified banks yet do not cause any significant effects on that of local banks. In light of these arguments, we hypothesize that:

H3: Following the Deepwater Horizon oil spill, national banks increase mortgage denial rates more aggressively than other mortgage lenders that are geographically less diversified.

3 Data and methodology

We make use of the SCAT oil survey data, which contains a detailed account of the polluted segments along the coastline, and map those segments to the HMDA lending data via the recorded census tracts. This section describes the data in more detail and discusses how we combine the information of both datasets.

3.1 SCAT oil survey and HMDA data

The SCAT oil survey data is publicly available as part of the ERMA Geographic Information System (GIS) tool from which we retrieve the shapefiles of the affected coastline segments.[4] The SCAT program was operational from May 2010 until April 2014 and it involved 18 teams simultaneously performing ground surveys of over 4,300 miles of shoreline and covering over 28,000 miles with repeat surveys. One of the main objectives of the SCAT teams was to record the degree of pollution and the exact location of each inspected zone. The severity of oil pollution was grouped into nine categories, which can be ranked from heavy oil layers to no oil: (1) heavy oil, (2) moderate oil, (3) light oil, (4) very light oil, (5) heavy tar balls, (6) moderate tar balls, (7) light tar ball, (8) negligible tar ball, and (9) no oil. We retrieve the SCAT survey shapefiles and extract coordinates for each reported polluted segment excluding the ones with no oil pollution.

[4]The collected geo-data is part of the Natural Resource Damage Assessment (NRDA) scheme and can be accessed via the web-based ERMA GIS tool https://erma.noaa.gov/gulfofmexico.

The HMDA datasets from 2007 to 2017 consist of detailed mortgage lending information, such as owner-occupancy, loan purpose, lien status or loan type, provided by U.S. financial institutions under the Home Mortgage Disclosure Act. Additionally, lenders must submit data on the race, gender, gross income, and ethnicity of a borrower or mortgage applicant. The HMDA also requires financial institutions to collect data on the location of the property which include census tract information to which the loan application is linked. Furthermore, lenders must also report whether a loan application was approved by the institution but not accepted by the applicant, withdrawn, denied, or closed for incompleteness. Collecting information on the above mortgage data, and using the census tracts of the affected states along the Gulf coast, allows us to link SCAT survey with the above HMDA data. The next subsection describes the exact steps how we combine both datasets.

3.2 Linking SCAT and HMDA data

In order to link both datasets, we first retrieve from the U.S. Census Bureau the TIGER/Line shapefiles for the year 2010 of the six affected U.S. states, namely Alabama, Mississippi, Texas, Georgia, Florida, and Louisiana.[5] Each shapefile contains information on the coordinates that define the boundaries of a census tract within a given state. We merge the shapefiles with the HMDA data along the state code, county code, and census tract number. Out of the 17,964 unique census tracts in the HMDA data of the six affected states, 14,368 (about 80%) are well matched. For those census tracts that cannot be matched based on the 2010 shapefiles, due to changes in census tracts over time, we use the year 2000 shapefiles instead. This adds another 2,889 (about 16%) census tracts to our final sample.[6]

Census tracts' coordinates in the shapefiles are of Cartesian form and are converted back into longitude and latitude measures. We then compute the minimum distance in miles, based on longitude and latitude measures, between each census tract in the six affected states and any

[5]The TIGER/Line state specific shapefiles are available from https://www.census.gov/geographies/mapping-files/time-series/geo/tiger-line-file.html.

[6]Without using these additional observations, our results remain unchanged.

polluted segment as reported in the SCAT survey. We follow a similar approach as outlined in Cano-Urbina et al. (2019) and group census tracts into different mile ranges based on their minimum distances to oil pollution; see Table 1. The table also reports the total number of loan applications, the number of applications that have been denied, and the denial rate in the census tracts within each distance bracket. Only loan applications associated with one- to four-family housing are included in the sample and both manufactured housing and multi-family housing are excluded. We also exclude those applications with a purpose of home improvement and focus on home purchase and refinancing.

Table 1: Summary statistics by distance brackets

Distance (miles)	$(0, 25]$	$(25, 50]$	$(50, 75]$	$(75, 100]$	$(100, 125]$	$(125, 150]$
Census tracts	774	363	330	244	295	467
Applications	1,335,790	531,646	646,239	235,411	320,369	608,255
Denials	653,636	261,996	311,477	112,593	149,291	300,249
Denials%	48.93	49.28	48.20	47.83	46.60	49.36
Distance (miles)	$(150, 175]$	$(175, 200]$	$(200, 300]$	$(300, 400]$	> 300	> 400
Census tracts	989	1,722	3,939	4,187	8,110	3,923
Applications	1,685,166	3,055,314	8,127,596	8,046,232	15,968,037	7,921,805
Denials	817,590	1,487,137	3,969,999	3,987,649	8,130,824	4,143,175
Denials %	48.52	48.67	48.85	49.56	50.92	52.30

3.3 Empirical strategy

The DID method serves as an appropriate identification strategy for the causal effects of the BP oil spill on mortgage lending for two reasons. First, the oil spill represents an unexpected and exogenous shock to the residential housing markets along the coastline on the Gulf of Mexico which extends to the mortgage market in the affected areas. There is no reason to believe that housing or mortgage market factors contributed to the happening of the oil spill. Second, there is no systematic pattern that determines which locations along the Gulf coast were affected by the oil spill, as this is dependent on the movement of the seawater rather than any housing or mortgage market factors. We use the following DID specification to identify the causal effects of the Deepwater Horizon oil spill event on the denial rate of mortgage loan

applications:

$$\text{denial}_{i,t} = \alpha + \beta\text{treated}_{i,t} + \gamma\text{after}_{i,t} + \delta\text{treated}_{i,t} \times \text{after}_{i,t} + \Theta \boldsymbol{X}_{i,t} + u_{i,t}, \tag{1}$$

where $\text{denial}_{i,t}$ equals one if the loan application i in year t is denied and zero otherwise; $\text{treated}_{i,t}$ equals one if the loan application i in year t is collateralized by a property located in a census tract within a certain distance from any detected oil pollution and zero otherwise; $\text{after}_{i,t}$ equals one if the loan application i is initiated in the year of the Deepwater Horizon oil spill, 2010, or later and zero otherwise; the vector $\boldsymbol{X}_{i,t}$ represents a rich set of control variables, including whether the loan purpose is for home purchase or refinancing, applicant income, loan amount, census tract fixed effects, lender fixed effects, loan type fixed effects, owner occupancy fixed effects, applicant race fixed effects, preapproval status fixed effects; see variable definitions in Table 2.

The parameter δ captures the causal effects of the Deepwater Horizon oil spill on banks' denial rate on mortgage loan applications in the affected areas. It is worth noting that, while our dependent variable is binary, we use a linear probability model rather than logit or probit frameworks for three reasons. First, to account for the cross-sectional variation across census tracts and lenders, our regression includes a rich set of census tract and lender fixed effects. Both the census tract and lender identifiers have a few thousand unique categories. Compared to logit and probit models, a linear probability model is much less computationally demanding in this case.

Second, when the dependent variable has a moderate probability of ones, a linear probability model turns out to fit equally well as logit and probit models. As pointed out by Hellevik (2009) and Von Hippel (2015, 2017), a linear probability model has several advantages with regards to the points discussed above. Given our large set of observations and controls, fitting a logit or probit model to the data is computationally cumbersome, as the maximum likelihood estimation requires an iterative process. In contrast, applying the linear probability model,

which uses the method of least squares, is not dependent on iterative steps and is therefore much faster with equivalent consistent estimates. In addition to this, Timoneda (2021) found that the linear probability and logistics frameworks produce similar results when the dependent variable consists of a proportion of ones between 25 and 75%, which is the case in our data; see Table 1.

Third, the parameters of a linear probability model are easy to interpret. In the linear probability model, a one-unit change in an independent variable yields a certain percentage change in the probability that the dependent variable equals one. However, the interpretation of the estimated coefficients in the logit or probit framework is rather difficult, since the marginal effect depends not only on the estimated coefficients but also where one evaluates it.

Table 2: Variable definitions

Variable	Value	Definition
loan type	1	Conventional (any loan other than FHA, VA, FSA, or RHS loans)
	2	FHA-insured (Federal Housing Administration)
	3	VA-guaranteed (Veterans Administration)
	4	FSA/RHS (Farm Service Agency or Rural Housing Service)
owner occupancy	1	Owner-occupied as a principal dwelling
	2	Not owner-occupied
	3	Not applicable
loan purpose	1	Home purchase
	2	Home improvement
	3	Refinancing
applicant race	1	American Indian or Alaska Native
	2	Asian
	3	Black or African American
	4	Native Hawaiian or Other Pacific Islander
	5	White
	6	Information not provided by applicant in mail, Internet, or telephone application
	7	Not applicable
preapproval	1	Preapproval was requested
	2	Preapproval was not requested
	3	Not applicable
action taken	1	Loan originated
	2	Application approved but not accepted
	3	Application denied by financial institution
	4	Application withdrawn by applicant
	5	File closed for incompleteness
	6	Loan purchased by the institution
	7	Preapproval request denied by financial institution
treated	0	Census tracts in the control group
	1	Census tracts in the treatment group
after	0	Loan applications between 2007 and 2009
	1	Loan applications between 2010 and 2012
denial	0	action taken<=2
	1	action taken>=3
refinancing	0	loan purpose=1
	1	loan purpose=3
applicant income		In millions of dollars
loan amount		In millions of dollars

4 Empirical evidence

This section presents the empirical results of our regression model and tests the parallel trends assumption that underlies the DID estimator. We also conduct a placebo study to validate the causal interpretation of our estimated results. To be consistent with the Deepwater Horizon timeline, we use the 2007-2012 sample for our main analysis. This leaves three years in both the pre- and post-treatment periods. To show the time-varying impact of the oil spill, we also extend the sample to the next five years, i.e. 2013-2017, for our post-hoc analyses.

4.1 Main analysis

In our main analysis, we consider various definitions of the treatment group: 0-25, 25-50, 50-75, 75-100, 100-125, 125-150, 150-175, and 175-200 miles from any detected oil pollution (i.e., negligible tar balls or more). The choice of going up to 200 miles is based on the finding that the estimated treatment effect diminishes to zero for the distance brackets $(150, 175]$ and $(175, 200]$. Census tracts are included in the control group if their minimum distance from oil pollution is between 200 and 300 miles, which leaves more than 8 million observations in the control group.[7] The sample is restricted to mortgage loan applications initiated between 2007 and 2012, with 2007-2009 in the pre-treatment period and 2010-2012 in the post-treatment period. Table 3 presents our DID estimation results.[8]

[7]This choice is generally in line with Cano-Urbina et al. (2019). We also use census tracts between 300 and 400 miles from oil pollution as an alternative control group and find similar empirical results.

[8]Instead of using the ordinary least squares estimator that is computationally demanding when including multiple sets of dummies variables, we use the estimator developed by Correia (2019) for linear models with many levels of fixed effects. For the distance bracket $(0, 25]$ for example, the model absorbs census tract fixed effects with 4687 categories, lender fixed effects with 4834 categories, loan type fixed effects with 4 categories, owner occupancy fixed effects with 3 categories, applicant race fixed effects with 7 categories, and preapproval fixed effects with 3 categories. The estimator drops the base category of each fixed effects variable, except for census tract. Instead, it omits the dummy variable "treated" from the regression. While the Correia (2019) estimator is computationally much more efficient, it yields the same estimation results with other alternatives, such as the ordinary least squares estimator with many sets of dummies variables. This estimator has been widely employed in economics, finance, and real estate research (Adams et al., 2018; Charnoz et al., 2018; Munch and Schaur, 2018; Guest, 2021; Lopez, 2021).

Table 3: Treatment effects of the BP oil spill on mortgage lending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(0,25]	(25,50]	(50,75]	(75,100]	(100,125]	(125,150]	(150,175]	(175,200]
after	-0.00438**	-0.00229	0.00324*	-0.000201	-0.000859	-0.00514**	-0.00486**	-0.00676***
	(-2.84)	(-1.44)	(2.05)	(-0.12)	(-0.52)	(-3.21)	(-3.17)	(-4.65)
treated × after	0.0341***	0.0490***	0.0576***	0.0494***	0.0250***	0.0211***	0.00233	0.00162
	(23.43)	(24.92)	(31.64)	(17.00)	(9.85)	(10.72)	(1.79)	(1.54)
refinancing	-0.0529***	-0.0540***	-0.0533***	-0.0539***	-0.0536***	-0.0533***	-0.0537***	-0.0521***
	(-87.97)	(-86.18)	(-85.87)	(-83.86)	(-83.85)	(-84.85)	(-89.69)	(-93.37)
applicant income	-0.0781***	-0.0816***	-0.0833***	-0.0815***	-0.0818***	-0.0798***	-0.0783***	-0.0808***
	(-16.93)	(-15.87)	(-16.42)	(-15.56)	(-15.71)	(-16.15)	(-16.81)	(-18.18)
loan amount	0.132***	0.133***	0.132***	0.131***	0.132***	0.130***	0.133***	0.123***
	(10.94)	(10.14)	(10.31)	(9.94)	(9.99)	(10.24)	(11.13)	(10.80)
_cons	0.505***	0.505***	0.501***	0.505***	0.504***	0.507***	0.506***	0.509***
	(270.94)	(253.52)	(257.62)	(252.50)	(251.79)	(261.84)	(275.59)	(287.14)
N	3913355	3596726	3647307	3446546	3479680	3594587	4006127	4571066
adj. R^2	0.266	0.269	0.269	0.269	0.270	0.271	0.268	0.266

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. t statistics calculated from robust standard errors are reported in parentheses. The control group is defined as census tracts whose minimum distance to oil pollution is between 200 and 300 miles. The treatment group is defined as census tracts whose minimum distance to oil pollution is within 0-25, 25-50, 50-75, 75-100, 100-125, 125-150, 150-175, and 175-200 miles, respectively in columns (1) to (8). The dummy variable "after" equals zero in 2007 to 2009 and one in 2010 to 2012. All regressions include fixed effects of census tract, lender, loan type, owner occupancy, applicant race, and preapproval status.

Table 3 shows that the mortgage loan denial rate is about 5.4% higher for home purchases than refinances. Other things being equal, the denial rate increases with loan amount and decreases with applicant income. As the coefficient of "treated×after" shows, the BP oil spill produces significant treatment effects on mortgage lending across distance brackets $(0, 25]$ through $(125, 150]$. For example, the chance of denial increases by 3.4% in areas within 25 miles from the polluted coastal segments. This effect intensifies and the probability of denial increases by 4.9 and 5.8% for the distance brackets $(25, 50]$ and $(50, 75]$, respectively. However, as we move further away from the polluted coastline, the magnitude of the effect on loan denials shrinks and eventually becomes insignificant for distance brackets $(150, 175]$ and $(175, 200]$. These results support our first hypothesis that the denial rate of mortgage loan applications in affected

regions increases relative to unaffected regions following the Deepwater Horizon oil spill event.

The question remains why the census tracts in very close proximity to the polluted coast segments experience a relatively smaller increase in loan rejections compared to those between 25 and 75 miles away from oil pollution. An explanation for this could be that homes in the distance bracket $(0, 25]$ are situated in prime locations along the coast and therefore have a more inelastic demand, which further implies that their collateral values remain less affected by the oil spill. A similar effect can be observed of properties in sought-after cities, such as New York, London, or Paris; see for example Cheshire and Sheppard (2017), Rossi-Hansberg et al. (2010) and Rosenthal (1999) on the importance of location and its impact on house prices.

Dell'Ariccia et al. (2012) study the lending standards and credit demand using data from the subprime mortgage market leading up to the financial crisis and find that denial rates are negatively and significantly associated with the number of mortgage applications. While the oil spill event tends to affect denial rates and credit demand simultaneously, we follow Dell'Ariccia et al. (2012) and further control the number of mortgage applications in the census tract where the property that secures the loan is located in Equation (1) for a robustness check. Results are presented in the Appendix Table A1. Focusing on all lenders rather than merely subprime lenders, our results indicate that the denial rate increases with the number of mortgage applications in the census tract. This finding is different from that of Dell'Ariccia et al. (2012) but intuitive in general, because more loan applications mean higher competition on the demand side and therefore a lower probability of being approved. Nevertheless, when controlling for the number of mortgage applications, our estimates of the treatment effects of the oil spill event stay robust.

Another concern one might have is that our sample period coincides with the aftermath of the 2007 housing market collapse, which was driven by the unprecedented growth of the subprime mortgage market. As home values plummeted after the burst of the housing bubble, the majority of lenders began to tighten their lending criteria for both prime and subprime mortgage loans. This can be seen from Figure A2 in the appendix, retrieved from the Federal

Reserve Economic Data (FRED). One might suspect that the general tightening of mortgage loan standards might have contributed to our estimates reported in Table 3. In our opinion, this is likely a false concern for three reasons. First, our specification includes a post-treatment indicator which accounts for the market-wide change in mortgage loan standards between the pre- and post-treatment periods. Second, our model includes census tract fixed effects which capture the spatial difference in subprime mortgage lending prior to the housing market crash, which is associated with demographic factors in a certain geographic location; see Calem et al. (2004). Third, the inclusion of lender fixed effects takes into account the heterogeneity across various lenders, which might be affected by the financial crisis to different extents.

One might also be concerned that our results could be distorted by major natural disasters such as hurricanes, which frequently affect some of the Gulf coast regions under investigation. Between 2007 and 2012 four destructive hurricanes occurred: Hurricanes Ike and Gustav in 2008, followed by Hurricane Irene in 2011 and Hurricane Sandy in 2012. Out of the four disasters, Hurricane Sandy was the most destructive, with costs amounting to around $70 billion. Despite the large negative effects of these hurricanes, we believe their impact on our results is only limited. The reasons for this are twofold. First, the four hurricanes under consideration did not affect all six Gulf coast states simultaneously. For example, Hurricane Ike affected Louisiana and Texas and Gustav further affected Mississippi and Alabama. Both hurricanes happened during our pre-treatment period of the Deepwater Horizon oil spill, and their potential impact on the denial rate of mortgage loans can be ruled out via a parallel trends test, which will be conducted in the next subsection. Hurricanes Irene and Sandy, both originated in the Atlantic Ocean, had some minor impact on Florida's eastern coastlines but none of the Gulf coast regions. Second, the trajectory of hurricanes not only covers coastal regions but can also move through interior areas far away from the coastline by more than 200 miles. Figure A1 in the appendix, which shows the movement of Hurricane Gustav, verifies this behavior. Hurricane Gustav hit the Gulf coast states Alabama, Louisiana, Mississippi and Florida before moving northwest through interior regions of the U.S. We find similar patterns

for Hurricane Ike, where the movements of the storm system did not simultaneously impact all of our six Gulf coast states.

In a nutshell, we find strong empirical support for tightened lending standards in the affected areas. Given that the impact of the Deepwater Horizon oil spill on the denial rate of mortgage loan applications diminishes to zero after exceeding the 150 mile threshold, the remainder of this paper only reports the estimation results for distance brackets up to 150 miles.

4.2 Parallel trends test

One of the central assumptions of the DID estimator is the requirement of parallel trends between the treatment and control groups prior to the treatment. In our main analysis, the pre-treatment period is defined as 2007, 2008, and 2009 combined, and the post-treatment period consists of 2010, 2011, and 2012. In order to test the parallel trends assumption, we use 2009 as the base year and define a "pre2009" dummy variable that equals one for years strictly before 2009 and a "post2009" dummy variable that equals one for years strictly after 2009. We therefore test the parallel trends assumption using the following specification:

$$\begin{aligned}\text{denial}_{i,t} &= \alpha + \beta\text{treated}_{i,t} + \gamma_1\text{pre2009}_{i,t} + \gamma_2\text{post2009}_{i,t} \\ &\quad + \delta_1\text{treated}_{i,t} \times \text{pre2009}_{i,t} + \delta_2\text{treated}_{i,t} \times \text{post2009}_{i,t} + \Theta \boldsymbol{X}_{i,t} + u_{i,t}. \end{aligned} \tag{2}$$

The parallel trends assumption holds when the coefficient of the interaction between the treatment indicator and "pre2009" dummy, δ_1, is not significantly different from zero.

Table 4: Parallel trend test

	(1) (0,25]	(2) (25,50]	(3) (50,75]	(4) (75,100]	(5) (100,125]	(6) (125,150]
pre2009	-0.0421***	-0.0443***	-0.0448***	-0.0442***	-0.0439***	-0.0427***
	(-48.22)	(-50.12)	(-50.78)	(-49.62)	(-49.34)	(-48.30)
post2009	-0.0281***	-0.0271***	-0.0217***	-0.0248***	-0.0254***	-0.0289***
	(-17.50)	(-16.36)	(-13.24)	(-14.57)	(-14.96)	(-17.41)
treated × pre2009	-0.00322	0.00176	-0.00279	0.00233	0.0122***	-0.00144
	(-1.70)	(0.69)	(-1.16)	(0.63)	(3.78)	(-0.57)
treated × post2009	0.0323***	0.0515***	0.0569***	0.0513***	0.0338***	0.0203***
	(16.45)	(19.64)	(23.34)	(13.06)	(9.91)	(7.67)
refinancing	-0.0544***	-0.0556***	-0.0550***	-0.0554***	-0.0551***	-0.0548***
	(-90.29)	(-88.56)	(-88.43)	(-86.11)	(-86.06)	(-87.11)
applicant income	-0.0780***	-0.0815***	-0.0832***	-0.0814***	-0.0817***	-0.0797***
	(-16.80)	(-15.73)	(-16.27)	(-15.42)	(-15.56)	(-16.00)
loan amount	0.133***	0.135***	0.133***	0.132***	0.133***	0.132***
	(10.94)	(10.14)	(10.31)	(9.94)	(9.99)	(10.24)
_cons	0.534***	0.535***	0.531***	0.534***	0.533***	0.536***
	(279.85)	(263.06)	(267.06)	(261.80)	(260.95)	(270.77)
N	3913355	3596726	3647307	3446546	3479680	3594587
adj. R^2	0.266	0.269	0.270	0.269	0.271	0.271

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. t statistics calculated from robust standard errors are reported in parentheses. The control group is defined as census tracts whose minimum distance to oil pollution is between 200 and 300 miles. The treatment group is defined as census tracts whose minimum distance to oil pollution is within 0-25, 25-50, 50-75, 75-100, 100-125, and 125-150 miles, respectively in columns (1) to (6). The dummy variable "pre2009" equals one in 2007 and 2008 and zero otherwise, and "post2009" equals one in 2010 to 2012 and zero otherwise. All regressions include fixed effects of census tract, lender, loan type, owner occupancy, applicant race, and preapproval status.

Focusing on the period before 2009, it is evident from Table 4 that the coefficients of this interaction are not statistically different from zero. We can therefore conclude that the assumption of parallel trends during the pre-treatment period holds. The only exception is the distance bracket $(100, 125]$, where the interaction of the "pre2009" dummy and the treatment indicator appears to be statistically significant. However, we do not see this as a matter of concern, since the magnitude of the causal effects diminishes with increasing distance from the coastline; see Table 3. Furthermore, the next closest distance bracket $(125, 150]$ fulfills the parallel trends

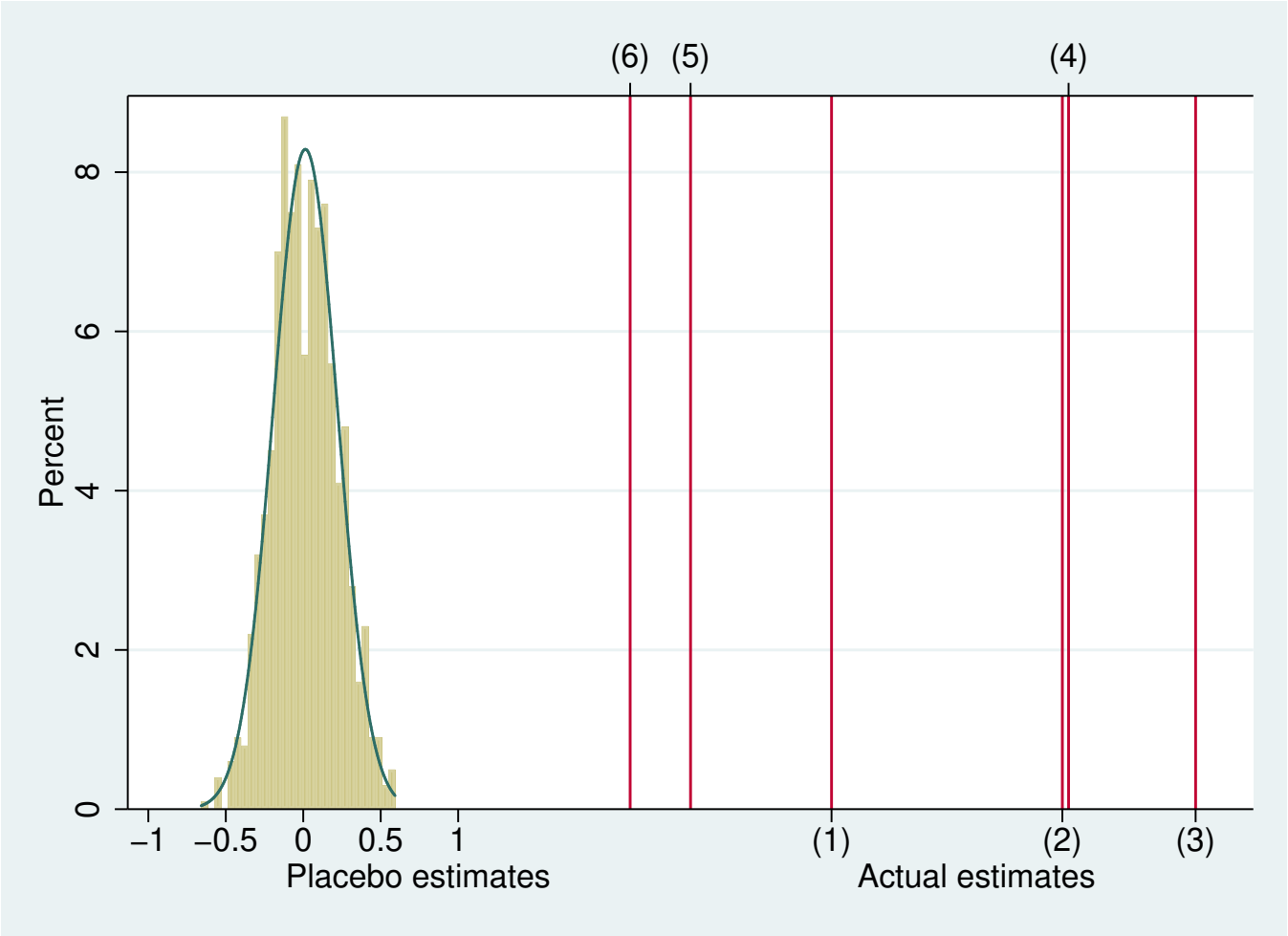
requirement again.

4.3 Placebo study

To provide additional support for the interpretation of our estimates as the causal effects of the oil spill on mortgage lending, we conduct a placebo study with randomized samples. In particular, we randomly assign census tracts of more than 200 miles from oil pollution into a treatment group and a control group, and use the same model specification (1) to estimate the hypothetical treatment effects. This ensures that our findings from the main analysis are not driven by the underlying research design. Table 3 shows clearly that the magnitude of the treatment effects diminishes as we move away from the polluted coastal segments to distance brackets $(150, 175]$ and $(175, 200]$. Therefore, census tracts exceeding a distance of 200 miles can be assumed to remain unaffected by the Deepwater Horizon catastrophe. Our placebo estimates, based on 1,000 random samples, are centered around zero and are significantly smaller than the actual estimates of the causal effects as reported in Table 3. Figure 2 presents the estimation results using the placebo sample and validates the causal interpretation of our estimates.

The result of this placebo study is important for the interpretation of our estimated causal effects. In the natural disaster context, Cortés and Strahan (2017) find that, in order to meet the heightened credit demand in affected markets, financially integrated banks choose to reallocate funds away from other unaffected but connected markets. One of the implications of their finding is that banks' lending standards can be affected by the BP oil spill event not only in affected regions of our treatment group but also in unaffected regions of the control group, which may invalidate the use of census tracts between 200 and 300 miles from oil pollution as the control group. We provide two explanations for this concern. First, our placebo study shows that the hypothetical treatment effects of the BP oil spill in census tracts more than 200 miles from oil pollution are zero on average. Second, supposed the denial rate also increased in unaffected regions due to banks' reallocation of funds, the true impact on the denial rate in

affected regions would be even larger than our estimates reported in Table 3. In other words, our estimates may understate but not overstate the casual effects of the BP oil spill.



(1) to (6) denote the estimated causal effects associated with distance brackets $(0, 25]$ to $(125, 150]$.

Figure 2: Placebo study with randomized samples

4.4 Time-varying effects

While our main analysis combines 2010, 2011, and 2012 into the post-treatment period, treatment effects can be varying over time. In this subsection, we therefore explore the persistence of the oil spill effect by defining new post-treatment periods. We re-estimate our DID

specification with an extended sample from 2007 to 2017 based on the following specification:

$$\text{denial}_{i,t} = \alpha + \beta \text{treated}_{i,t} + \sum_{T=2010}^{2017} \gamma_T \text{d}_{T,i,t} + \sum_{T=2010}^{2017} \delta_T \text{treated}_{i,t} \times \text{d}_{T,i,t} + \Theta \boldsymbol{X}_{i,t} + u_{i,t}, \qquad (3)$$

where $d_{T,i,t}$ ($T = 2010, ..., 2017$) is a dummy variable that equals one if $t = T$ and zero otherwise. In Equation (3), the pre-treatment period remains as 2007 to 2009. The post-treatment period includes the years from 2010 to 2017, and the parameter δ_T ($T = 2010, ..., 2017$) captures the year-specific treatment effects of the oil spill event.

As Table 5 shows, we find significant treatment effects across all six minimum distance brackets over the post-treatment periods starting in 2010 and ending in 2017. Consistent with our results above, we observe that the contraction in lending activity has been the strongest for census tracts located within a 25-50 mile and 50-75 mile distance from oil pollution. Furthermore, comparing the estimates with the findings reported in Table 3, we can see that the magnitude of the oil spill effect has changed over time, peaking in 2012. The increased probability of loan rejection remains the highest for all distance brackets during this period with the distance category $(50, 75]$ experiencing a 8.2% heightened chance of loan denial, which also represents the maximum value of all the estimates. After 2012, the impact of the Deepwater Horizon disaster on lending activity starts to decline, and reaches near zero values but remains statistically significant in 2017. The only exceptions are the minimum distance brackets $(25, 50]$ and $(50, 75]$, where the denial rate is still slightly elevated by around 2%.

We still observe the same outcome as before, i.e., the impact of the oil spill peaks mainly for census tracts belonging to distance bracket $(50, 75]$ and then declines again as we move further away from the coastline. Therefore, Table 5 provides evidence regarding the time-varying effects of the Deepwater Horizon oil spill and illustrates the persistent nature of the disaster in terms of its impact on mortgage lending to regions located in close proximity to the polluted coastline. Our time-varying findings are supportive of the evidence presented by Nguyen et al. (2020).

Table 5: Time-varying effects

	(1) (0,25]	(2) (25,50]	(3) (50,75]	(4) (75,100]	(5) (100,125]	(6) (125,150]
d_{2010}	0.0233***	0.0239***	0.0282***	0.0256***	0.0261***	0.0231***
	(17.67)	(17.67)	(20.92)	(18.55)	(19.01)	(17.05)
d_{2011}	0.00783***	0.00871***	0.0132***	0.0103***	0.0108***	0.00756***
	(5.92)	(6.43)	(9.77)	(7.47)	(7.91)	(5.59)
d_{2012}	-0.0162***	-0.0147***	-0.0101***	-0.0133***	-0.0128***	-0.0162***
	(-13.30)	(-11.80)	(-8.15)	(-10.46)	(-10.13)	(-12.99)
d_{2013}	-0.0195***	-0.0185***	-0.0136***	-0.0171***	-0.0166***	-0.0199***
	(-16.07)	(-14.84)	(-10.94)	(-13.43)	(-13.13)	(-16.00)
d_{2014}	0.000259	0.000383	0.00534***	0.00171	0.00239	-0.000526
	(0.21)	(0.30)	(4.20)	(1.31)	(1.85)	(-0.41)
d_{2015}	0.00674***	0.00669***	0.0116***	0.00801***	0.00871***	0.00574***
	(5.51)	(5.33)	(9.30)	(6.26)	(6.85)	(4.58)
d_{2016}	0.0102***	0.0101***	0.0151***	0.0115***	0.0121***	0.00900***
	(8.46)	(8.15)	(12.20)	(9.04)	(9.57)	(7.25)
d_{2017}	-0.0345***	-0.0347***	-0.0315***	-0.0339***	-0.0324***	-0.0359***
	(-26.32)	(-25.57)	(-23.44)	(-24.73)	(-23.73)	(-26.74)
treated $\times d_{2010}$	0.0273***	0.0489***	0.0442***	0.0376***	0.0103**	0.00767**
	(13.54)	(18.03)	(17.53)	(9.39)	(2.97)	(2.84)
treated $\times d_{2011}$	0.0345***	0.0546***	0.0663***	0.0490***	0.0255***	0.0211***
	(16.61)	(19.41)	(25.61)	(11.99)	(7.09)	(7.28)
treated $\times d_{2012}$	0.0466***	0.0691***	0.0824***	0.0641***	0.0479***	0.0366***
	(27.03)	(27.50)	(36.38)	(17.39)	(14.83)	(15.35)
treated $\times d_{2013}$	0.0320***	0.0489***	0.0680***	0.0563***	0.0385***	0.0290***
	(18.66)	(19.30)	(29.68)	(15.11)	(12.01)	(12.16)
treated $\times d_{2014}$	0.0223***	0.0330***	0.0514***	0.0472***	0.0310***	0.0284***
	(11.58)	(11.56)	(19.55)	(11.44)	(8.66)	(10.65)
treated $\times d_{2015}$	0.0123***	0.0165***	0.0273***	0.0382***	0.0226***	0.0332***
	(6.76)	(6.07)	(10.85)	(9.49)	(6.61)	(12.91)
treated $\times d_{2016}$	0.0104***	0.0260***	0.0332***	0.0247***	0.0256***	0.0266***
	(5.97)	(9.96)	(13.85)	(6.45)	(7.73)	(10.89)
treated $\times d_{2017}$	0.00370*	0.0231***	0.0205***	0.00951*	0.00874*	0.0114***
	(2.00)	(7.78)	(7.55)	(2.24)	(2.46)	(4.44)
refinancing	-0.0178***	-0.0201***	-0.0198***	-0.0198***	-0.0193***	-0.0185***
	(-40.73)	(-44.25)	(-43.86)	(-42.77)	(-41.68)	(-40.84)
applicant income	-0.0588***	-0.0527***	-0.0495***	-0.0583***	-0.0594***	-0.0586***
	(-17.06)	(-9.68)	(-8.66)	(-15.05)	(-15.12)	(-15.57)
loan amount	0.0473***	0.0464***	0.0466***	0.0461***	0.0485***	0.0470***
	(7.31)	(7.18)	(7.12)	(6.93)	(6.92)	(7.01)
_cons	0.458***	0.459***	0.453***	0.458***	0.457***	0.459***
	(352.52)	(350.50)	(336.78)	(338.56)	(330.21)	(343.82)
N	8076176	7400947	7497020	7142775	7208287	7447264
adj. R^2	0.230	0.232	0.232	0.231	0.232	0.233

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. t statistics calculated from robust standard errors are reported in parentheses. The control group is defined as census tracts whose minimum distance to oil pollution is between 200 and 300 miles. The treatment group is defined as census tracts whose minimum distance to oil pollution is within 0-25, 25-50, 50-75, 75-100, 100-125, and 125-150 miles, respectively in columns (1) to (6). All regressions include fixed effects of census tract, lender, loan type, owner occupancy, applicant race, and preapproval status.

5 Further results

In this section, we test the next two hypotheses by exploring how the oil spill affects the denial rate of refinance versus home purchase loans and whether national banks respond differently to the event compared to other mortgage lenders.

5.1 Heterogeneous effects on home purchase and refinance loans

We interact the dummy variable “refinancing" with both the time dummy “after" and the treatment dummy “treated", to detect how the causal effects on loan rejections can be heterogeneous between these two loan purposes. In terms of pre- and post-treatment periods, control variables, and fixed effects, we adopt the same specification for the DID estimation as outlined in Table 3.

For census tracts located in very close proximity to the polluted coast segments, the estimate shows that the probability of denial increases by an additional 2.6% for refinance loan applications relative to their home purchase counterparts. Other census tracts that are further away from oil pollution have a smaller and even close to zero disparity between the two loan purposes in terms of the causal effects of the oil spill on mortgage loan rejections. These results support our second hypothesis that the denial rate of refinance loan applications increases more than that of home purchase loan applications in affected regions following the Deepwater Horizon oil spill disaster.

The stronger lending contraction in refinancing compared to home purchases is in line with the “ratchet effect" pointed out by Khandani et al. (2013). Refinancing increases homeowner leverage when the housing market is on the way up. However, during periods of declining house prices, refinancing lacks the ability to symmetrically decrease homeowner leverage. Suppose a home was purchased at the peak of the housing market with a loan-to-value (LTV) of 80%, the LTV ratio at refinancing might be much higher than 80% following a sharp decline in its collateral value caused by the oil spill. Lenders therefore tend to deny more of the refinance

loan applications in affected areas following the Deepwater Horizon oil spill. Hence, our results are in line with the previous evidence presented by Munnell et al. (1996) and Dell'Ariccia et al. (2012), who have found that refinance loans are deemed to be more risky by lenders than home purchase loans.

Table 6: Differential effects on home purchase and refinance loans

	(1) (0,25]	(2) (25,50]	(3) (50,75]	(4) (75,100]	(5) (100,125]	(6) (125,150]
after	0.0315***	0.0331***	0.0385***	0.0346***	0.0339***	0.0302***
	(19.00)	(19.36)	(22.70)	(19.70)	(19.40)	(17.62)
treated × after	0.0195***	0.0413***	0.0591***	0.0472***	0.0188***	0.0116***
	(9.24)	(14.09)	(22.31)	(10.66)	(5.27)	(4.00)
refinancing	-0.0258***	-0.0256***	-0.0257***	-0.0259***	-0.0265***	-0.0259***
	(-32.67)	(-31.94)	(-32.17)	(-32.18)	(-32.97)	(-32.41)
refinancing × treated	-0.00104	-0.00921***	0.00856***	-0.00201	0.0170***	0.00535*
	(-0.59)	(-3.77)	(3.77)	(-0.56)	(5.65)	(2.27)
refinancing × after	-0.0619***	-0.0612***	-0.0613***	-0.0604***	-0.0604***	-0.0612***
	(-58.54)	(-57.59)	(-57.74)	(-56.60)	(-56.72)	(-57.61)
refinancing × treated × after	0.0261***	0.0160***	-0.000775	0.00683	0.00931*	0.0173***
	(10.19)	(4.49)	(-0.24)	(1.28)	(2.06)	(4.87)
applicant income	-0.0768***	-0.0801***	-0.0819***	-0.0801***	-0.0805***	-0.0785***
	(-16.68)	(-15.65)	(-16.19)	(-15.34)	(-15.50)	(-15.92)
loan amount	0.132***	0.133***	0.131***	0.130***	0.131***	0.130***
	(10.94)	(10.15)	(10.31)	(9.95)	(10.00)	(10.25)
_cons	0.490***	0.490***	0.485***	0.489***	0.489***	0.491***
	(260.62)	(244.38)	(248.11)	(243.31)	(242.64)	(252.09)
N	3913355	3596726	3647307	3446546	3479680	3594587
adj. R^2	0.267	0.269	0.270	0.270	0.271	0.271

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. t statistics calculated from robust standard errors are reported in parentheses. The control group is defined as census tracts whose minimum distance to oil pollution is between 200 and 300 miles. The treatment group is defined as census tracts whose minimum distance to oil pollution is within 0-25, 25-50, 50-75, 75-100, 100-125, and 125-150 miles, respectively in columns (1) to (6). The dummy variable "after" equals zero in 2007 to 2009 and one in 2010 to 2012. The dummy variable "refinancing" equals zero for loans associated with home purchases and one for those associated with refinancings. All regressions include fixed effects of census tract, lender, loan type, owner occupancy, applicant race, and preapproval status.

An increase in the denial rate in affected relative to unaffected areas does not mean that

lenders reject a larger number of mortgage applications though. Facing declining house prices and shrinking household wealth, both potential and existing homeowners in affected areas may have a higher demand for credit and lenders may approve more applications even with a lower approval rate. In Table 7, we report the results of census tract level regressions to test how the oil spill affects the number of loan applications, the number of approvals, and the number of denials. The dummy variable "after" controls for nation-wide changes in economic conditions that affect lending standards, and census tract fixed effects control for time-invariant heterogeneity across geographic locations. Compared to the first three years of the sample period, loan applications, approvals, and denials all decline during the second three years, a result in line with the general cooling-off period of the housing market. Compared to unaffected areas in the control group, census tracts within 25 miles from oil pollution experience 61 more mortgage applications on average for the purpose of home purchase, roughly 45% of which are approved and 55% are denied. These census tracts have even more mortgage applications for the purpose of refinancing, compared to the control group. Among the 69 more refinance applications, however, only 9.5% are approved and more than 90% are denied. This verifies our conclusion that refinance mortgages are more risky than purchase mortgages. It also supports the existing literature which states that lenders increase access to lending, with tightened lending standards, in affected areas to meet the increased demand for loans in those localities.

Table 7: Effects on loan applications, approvals, and denials

	Home purchase			Refinancing		
	(1)	(2)	(3)	(4)	(5)	(6)
	Applications	Approvals	Denials	Applications	Approvals	Denials
after	-120.7***	-63.84***	-57.18***	-130.6***	-20.24***	-110.4***
	(-32.76)	(-35.74)	(-28.21)	(-28.10)	(-11.80)	(-33.93)
treated × after	60.60***	27.93***	32.61***	68.72***	6.515*	61.96***
	(11.90)	(10.42)	(12.32)	(10.03)	(2.21)	(13.48)
_cons	319.8***	164.7***	156.4***	456.8***	191.9***	265.9***
	(148.42)	(156.11)	(132.52)	(166.81)	(185.92)	(139.20)
N	5976	5940	5962	5978	5958	5970
adj. R^2	0.873	0.879	0.854	0.834	0.891	0.748

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. t statistics calculated from robust standard errors are reported in parentheses. The control group is defined as census tracts whose minimum distance to oil pollution is between 200 and 300 miles. The treatment group is defined as census tracts whose minimum distance to oil pollution is within 25 miles. The dummy variable "after" equals zero in 2007 to 2009 and one in 2010 to 2012. All regressions include census tract fixed effects.

5.2 Heterogeneous responses of national banks vs other lenders

Having investigated the differential effects of the Deepwater Horizon spill on bank lending activity, we now turn to the banking sector itself and examine whether national banks have behaved differently in terms of their issuance of loans, compared to other financial intermediaries such as regional and community banks, credit unions, and other private lenders. National banks are geographically diversified while regional and community banks, credit unions, and private lenders usually focus on local markets and have better relationship with local customers.

Table 8 reports the statistically significant estimates when accounting for the loan originator type. While local lenders also tighten lending standards (as captured by the coefficient of "treated × after"), across all six distance brackets, loan applicants experience an additional 3 to 9% increase (as captured by the coefficient of "national × treated × after") in the likelihood of rejection, given the originator is classified as a national bank. In line with our previous findings, national banks decrease lending by an additional 8.7% for properties located in the distance bracket $(50, 75]$, the largest contraction among all distance brackets. These results sup-

port our third hypothesis that there is heterogeneity in lending response of mortgage lenders between national banks and other mortgage lenders, with a more aggressive increase in mortgage denials among national banks.

These regression results also speak for the regulatory responses to the 2007 financial crisis. To restore the safety and soundness of the banking industry and financial stability in general, the Federal Reserve is mandated to closely monitor large national banks and financial institutions in the U.S. after the 2007 financial crisis. As the coefficient of "national $\times$ after" shows, national banks have significantly higher denial rates between 2007 and 2009 (our pre-treatment period) compared to 2010 and 2012 (our post-treatment period).

Our findings are in conformity with the results reported by Berg and Schrader (2012) and Nguyen et al. (2020). National banks may contract lending more in the affected regions compared to the other financial institutions since their physical distance to borrowers and transaction-lending business model may complicate the collection and processing of soft information effectively to stabilize lending. This points to the significance of local lenders, which play an important role in the recovery of local economies affected by catastrophic events.

Table 8: Heterogeneous responses of national banks vs other lenders

	(1)	(2)	(3)	(4)	(5)	(6)
	(0,25]	(25,50]	(50,75]	(75,100]	(100,125]	(125,150]
after	-0.00102	0.000819	0.00761***	0.000909	0.000153	-0.00376*
	(-0.66)	(0.51)	(4.79)	(0.55)	(0.09)	(-2.34)
treated × after	0.0183***	0.0252***	0.0260***	0.0278***	0.00631*	0.00750***
	(10.80)	(11.13)	(12.39)	(8.37)	(2.16)	(3.35)
national × treated	0.0259***	-0.00212	0.00135	-0.00869	-0.00766	0.0189***
	(10.89)	(-0.61)	(0.43)	(-1.78)	(-1.85)	(6.08)
national × after	-0.113**	-0.129**	-0.0722	-0.122*	-0.0999*	-0.0915*
	(-3.21)	(-2.88)	(-1.68)	(-2.44)	(-2.10)	(-1.98)
national × treated × after	0.0301***	0.0676***	0.0868***	0.0703***	0.0570***	0.0347***
	(9.44)	(14.77)	(20.94)	(10.45)	(9.92)	(8.03)
refinancing	-0.0527***	-0.0539***	-0.0530***	-0.0538***	-0.0535***	-0.0532***
	(-87.65)	(-85.98)	(-85.35)	(-83.79)	(-83.79)	(-84.72)
applicant income	-0.0781***	-0.0816***	-0.0834***	-0.0815***	-0.0819***	-0.0797***
	(-16.95)	(-15.89)	(-16.45)	(-15.58)	(-15.72)	(-16.15)
loan amount	0.132***	0.133***	0.131***	0.130***	0.131***	0.130***
	(10.94)	(10.14)	(10.31)	(9.94)	(9.99)	(10.24)
_cons	0.522***	0.526***	0.511***	0.525***	0.521***	0.522***
	(85.05)	(66.99)	(68.04)	(60.44)	(62.69)	(64.96)
N	3913355	3596726	3647307	3446546	3479680	3594587
adj. R^2	0.266	0.269	0.269	0.269	0.270	0.271

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. t statistics calculated from robust standard errors are reported in parentheses. The control group is defined as the census tracts whose minimum distance to oil pollution is between 200 and 300 miles. The treatment group is defined as the census tracts whose minimum distance to oil pollution is within 0-25, 25-50, 50-75, 75-100, 100-125, and 125-150 miles, respectively in columns (1) to (6). The dummy variable "after" equals zero in 2007 to 2009 and one in 2010 to 2012. The dummy variable "national" equals one if the lender name contains "NA" and either "BANK" or "BK". All regressions include fixed effects of census tract, lender, loan type, owner occupancy, applicant race, and preapproval status.

6 Conclusion

The explosion of the Deepwater Horizon oil rig located in the Gulf of Mexico caused severe adverse effects on maritime life and regions along the coastline. This paper focuses on the economic implications of the oil spill by investigating changes in mortgage lending activity to areas in varying degrees of proximity to the oil spill. For this reason we construct a unique dataset, merging information from the SCAT survey with the HMDA lending data. Retrieving the TIGER census data for the Gulf coast states Alabama, Mississippi, Texas, Georgia, Florida, and Louisiana allows us to compute the distance between the polluted coastal segments and the individual census tracts within a state. Having ranked the census tracts in terms of their minimum distances to the affected segments, we then perform a difference-in-differences estimation to investigate the effects of the Deepwater Horizon oil spill on mortgage lending.

Our findings show that mortgage lenders reduce their lending by 2% and 6% to areas within a 150 mile distance to the polluted coastal segments. This result also holds for the refinancing of mortgages, where the denial rates increased by an additional 2-3% for a minimum distance of 25 miles. In addition to this, the impact of the oil spill is found to be long-lasting and results in a higher loan rejection rate by national banks compared to other financial intermediaries. As our findings show, the Deepwater Horizon oil spill not only causes severe adverse environmental effects but also implies negative economic externalities in the form of tightening lending standards.

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Appendix

Table A1: Controlling for the number of mortgage applications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(0,25]	(25,50]	(50,75]	(75,100]	(100,125]	(125,150]	(150,175]	(175,200]
after	0.00121	0.00366*	0.00908***	0.00559***	0.00494**	0.000646	0.00102	-0.000855
	(0.77)	(2.26)	(5.65)	(3.35)	(2.97)	(0.40)	(0.65)	(-0.58)
treated × after	0.0309***	0.0446***	0.0529***	0.0455***	0.0212***	0.0180***	0.000246	-0.000462
	(21.06)	(22.53)	(28.84)	(15.64)	(8.36)	(9.09)	(0.19)	(-0.44)
number of applications	0.0157***	0.0167***	0.0162***	0.0161***	0.0162***	0.0161***	0.0163***	0.0163***
	(17.33)	(18.22)	(17.79)	(17.54)	(17.55)	(17.64)	(18.46)	(18.99)
refinancing	-0.0530***	-0.0542***	-0.0535***	-0.0540***	-0.0538***	-0.0535***	-0.0539***	-0.0523***
	(-88.25)	(-86.50)	(-86.16)	(-84.16)	(-84.15)	(-85.15)	(-89.98)	(-93.66)
applicant income	-0.0781***	-0.0815***	-0.0833***	-0.0815***	-0.0818***	-0.0797***	-0.0783***	-0.0808***
	(-16.99)	(-15.93)	(-16.48)	(-15.63)	(-15.77)	(-16.21)	(-16.87)	(-18.25)
loan amount	0.131***	0.132***	0.131***	0.130***	0.131***	0.129***	0.133***	0.123***
	(10.93)	(10.14)	(10.30)	(9.94)	(9.99)	(10.24)	(11.13)	(10.80)
_cons	0.483***	0.482***	0.478***	0.482***	0.481***	0.484***	0.483***	0.486***
	(229.78)	(217.53)	(218.20)	(215.59)	(215.44)	(222.81)	(233.04)	(245.99)
N	3913355	3596726	3647307	3446546	3479680	3594587	4006127	4571066
adj. R^2	0.266	0.269	0.269	0.269	0.270	0.271	0.268	0.266

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. t statistics calculated from robust standard errors are reported in parentheses. The control group is defined as census tracts whose minimum distance to oil pollution is between 200 and 300 miles. The treatment group is defined as census tracts whose minimum distance to oil pollution is within 0-25, 25-50, 50-75, 75-100, 100-125, 125-150, 150-175, and 175-200 miles, respectively in columns (1) to (8). The dummy variable "after" equals zero in 2007 to 2009 and one in 2010 to 2012. The number of applications is expressed in thousands. All regressions include fixed effects of census tract, lender, loan type, owner occupancy, applicant race, and preapproval status.

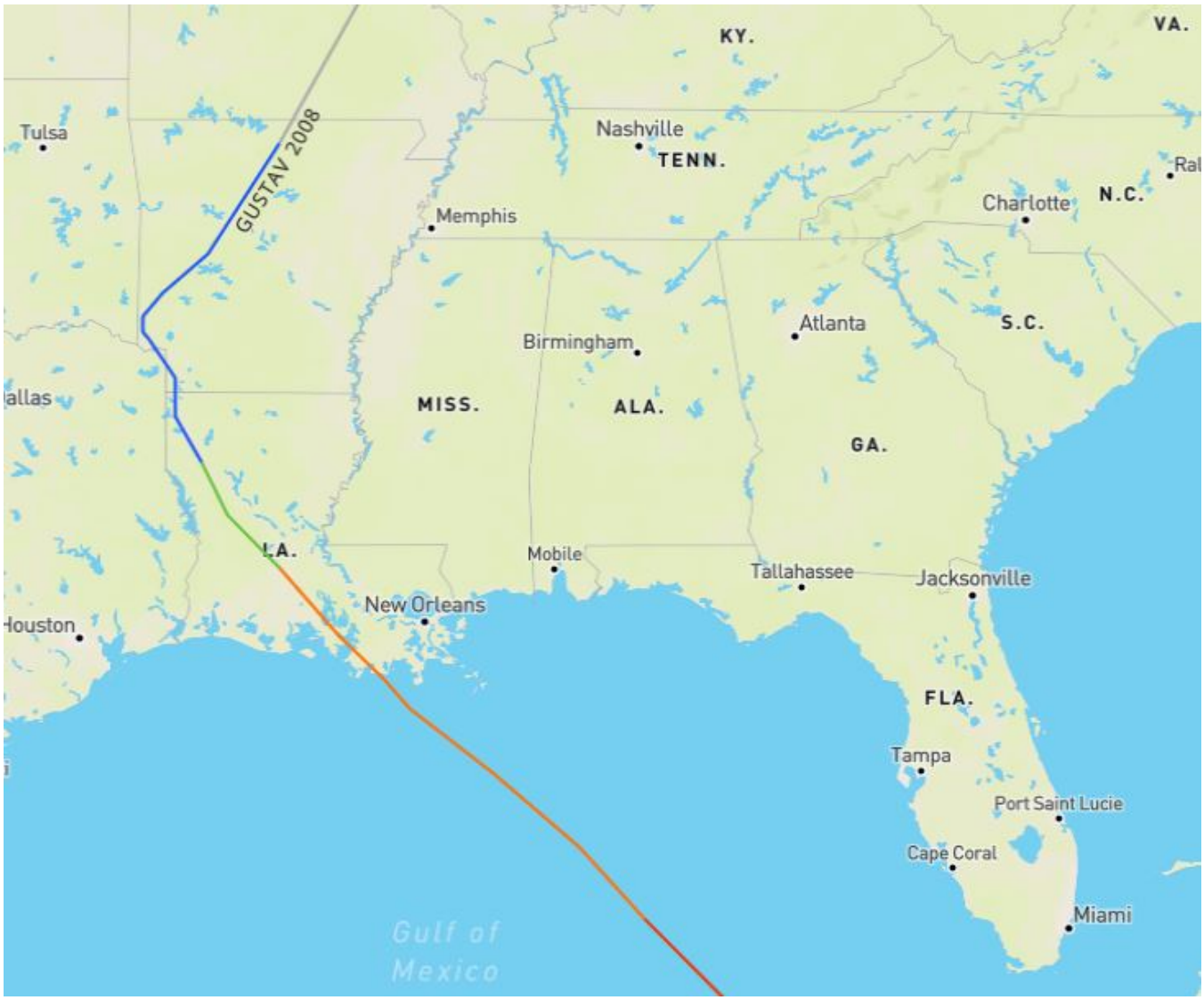

Figure A1: The chart depicts the trajectory of Gustav occurring in 2008, retrieved from https://coast.noaa.gov/hurricanes.

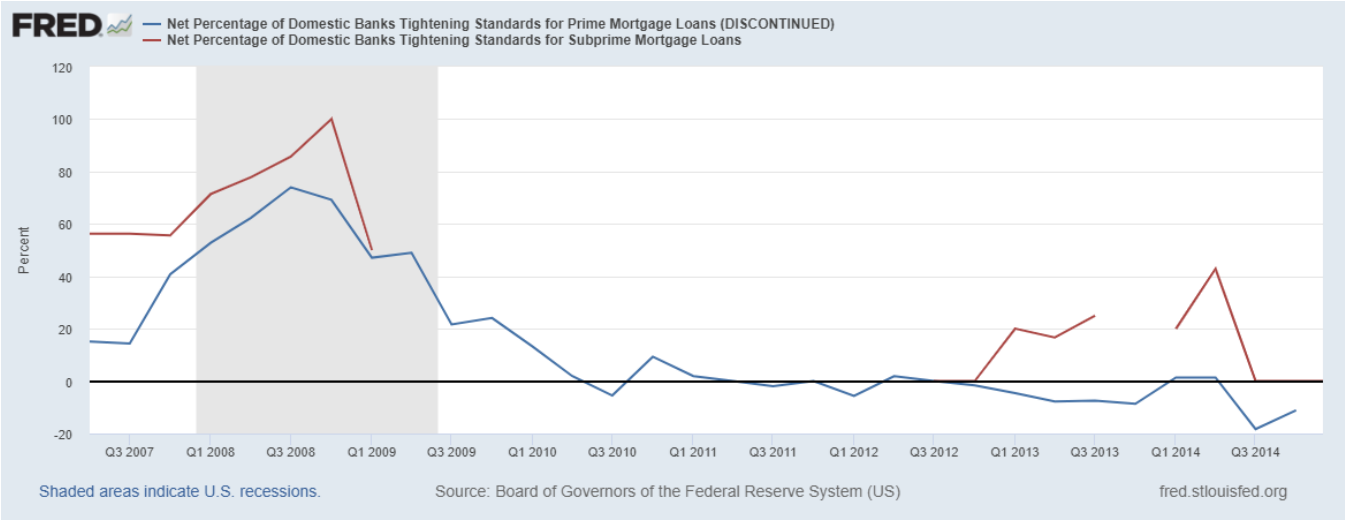

Figure A2: Tightening standards for mortgage loans