The Imitation Game: How Encouraging Renegotiation Makes Good Borrowers Bad*

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Abstract

We show that commercial mortgage borrowers behave opportunistically to obtain principal reductions. We develop a model in which lenders cannot perfectly observe borrowers' use values and renegotiation is costly. We then exploit a tax rule change that reduced the cost of renegotiation. Consistent with the model predictions, borrowers with high private use values of the property are more likely to transfer into special servicing when lenders have a higher capacity to negotiate principal reductions after the rule change. Our results suggest substantial asymmetric information between borrowers and lenders, as well as adverse consequences of principal forgiveness for lenders.

Keywords: principal forgiveness, CMBS, asymmetric information, loan renegotiation, IRS Revenue Procedure 2009-45.

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1 Introduction

The infrequency of principal modification in residential mortgages after sharp declines in real estate prices is puzzling.[1] While such modifications are preferable to a foreclosure for a particular loan, they may increase the risk of borrowers on other loans strategically defaulting in an attempt to extract similar modifications. For borrowers to be able to behave opportunistically to the disadvantage of lenders, there must be substantial asymmetric information between borrowers and lenders such that financially healthy borrowers can imitate unhealthy ones–lenders must not be able to observe borrowers' true willingness and ability to pay.

In this paper, we provide evidence of substantial asymmetric information between borrowers and lenders even in commercial real estate (CRE) loans. We document this asymmetric information by focusing on the impact of principal writedowns on borrower behavior in commercial mortgage-backed securities (CMBS). Principal writedowns (also known as discounted payoffs, or DPOs hereafter) are a type of loan modification in which the special servicer accepts repayment of an amount less than the current unpaid principal balance on the loan. We ask whether a DPO induces other borrowers to behave opportunistically in anticipation of receiving the same type of principal writedown.[2]

We begin by presenting a principal-agent model of negotiation between a borrower and

[1]Adelino, Gerardi, and Willen (2013) find that the vast majority of seriously delinquent residential mortgages received no concessionary modification whatsoever, with principal reduction being exceptionally rare. Ghent (2011) finds that principal reduction was similarly rare during the Great Depression.

[2]Anecdotal evidence suggests the CMBS industry is aware of strategic behavior on the part of CMBS borrowers. For example, consider the following excerpt from the prospectus for the deal BANK 2018-BNK15 in which the issuer discloses to investors the behavior of sponsors of certain loans in the pool:

> With respect to the Harvard Park Mortgage Loan (3.1%), three properties owned by the related sponsors have been subject to discounted payoffs since 2011. In November 2011, Basin Street Properties, which is owned by the related sponsors, placed its Petaluma Garage Retail property into a strategic default, and negotiated a discounted payoff of $4,500,000 on the $7,275,000 loan. In September 2014, Basin Street Properties negotiated a discounted payoff of $1,500,000 on a $6,160,000 mezzanine loan and repurchased at auction a $16,000,000 loan secured by its park Center Tower property. In June 2015, Basin Street Properties negotiated a discounted payoff of $15,000,000 on the $23,274,042 outstanding loan on its Cal Center property.

According to the prospectus, the borrower Basin Street Properties placed a loan into strategic default and was able to negotiate a substantial DPO.

a lender/special servicer that captures the key institutional features of the CMBS market, including regulation. Borrowers vary in their private use value of the property, and lenders cannot observe whether that value is high or low. High private use value types ("high types" hereafter) are willing to pay the full amount of their existing loan balance, whereas low private use value types ("low types" hereafter) would rather default than pay the full amount. Borrowers can request a transfer into special servicing and try to negotiate a DPO, a decision that depends on their expected payoff. The special servicer can choose to either grant the DPO request or initiate a foreclosure. Because the borrower does not know with certainty which strategy the servicer will choose, the expected payoff of bargaining is increasing in the likelihood that the servicer is willing to do a DPO. Additionally, the expected payoff is decreasing in the reputational and legal costs of requesting a transfer. The model has two key empirical implications. First, high type borrowers are more likely to transfer if they expect servicers to have a high capacity to negotiate DPOs. Second, high types are more likely to request a DPO when the expected cost of renegotiating the loan decreases.

In the second part of the paper we take these predictions to the data. Establishing the effect of past servicer behavior on opportunistic behavior can be empirically challenging, so our empirical strategy exploits a 2009 IRS tax rule change that provides exogenous variation in the cost of renegotiation. The main focus of the rule change was to allow borrowers that had not experienced any material decline in cash flows to get a term extension without triggering a tax event. Given the extraordinarily difficult financing environment during the Global Financial Crisis (GFC), borrowers could not refinance healthy commercial mortgages with balloon payments coming due. Absent the rule change, these loans would have defaulted despite not experiencing any adverse fundamental shock. However, the language of the rule was sufficiently broad as to allow a broader set of modifications, including DPOs, for loans that were not experiencing distress. Prior to the rule change, a loan had to either be in default or near default to be eligible for a modification.

We proxy for servicers' current capacity to negotiate DPOs using multiple measures of whether they negotiated a DPO for other borrowers in the past. We first show that a loan is more likely to be transferred into special servicing following this rule change and when the special servicer has recently negotiated a DPO on a different loan. This is consistent with the predictions of our model: renegotiation requests have higher expected payoff when it is likely that the servicer is willing to provide a DPO and when the cost of obtaining a transfer is low. We show that this result is robust to a variety of controls and fixed effects, including MSA-by-time fixed effects which account for time-varying local economic conditions.

The timing of the rule change coincides with the GFC, a time at which unobservably low-quality loans may have become distressed. As such, it is possible that servicers' DPO behavior in the recent past on other loans may be correlated with unobservable *ex ante* loan quality in a way that matters specifically because of the rule change timing, in which case the interaction between servicer DPO propensity and the post-rule change indicator does not in fact capture the effect of the rule change. To alleviate this concern, we divide our loans into those likely to be affected by the rule change and those likely to be unaffected. Prior to the rule change a loan could be transferred if the servicer expected a default in the very near future (two to three months). Therefore, that loans that had experienced delinquency prior to the rule change would have been less affected relative to loans that had never experienced delinquency. Similarly, loans that had been on the servicer watchlist (a list of loans or borrowers at heightened risk of distress) prior to the rule change would have plausibly been less affected relative to loans that had never had a heightened risk of distress. In a triple-difference framework, we show that loans that *ex ante* would have been more affected are almost twice as likely to be transferred into special servicing, which is consistent with high type borrowers behaving strategically. We further show that transferred loans are more likely to never become 60+ days delinquent. Taken together, these results are consistent with high types being more likely to transfer because *ex ante* high quality should be positively correlated with ex-post good loan performance.

Our paper relates to literature on the potential for strategic responses to loan modifications. Both Mayer, Morrison, Piskorski, and Gupta (2014) and Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2017) empirically examine whether non-liquidity-related default occurs in residential mortgages in response to modifications. The former finds that non-distressed borrowers are more likely to default when a legal settlement forces a lender to offer principal reductions to distressed borrowers. In contrast, the latter finds no evidence that principal reductions induce strategic defaults. A broader literature on residential mortgage defaults attempts to disentangle liquidity-motivated defaults from those motivated by "strategic" reasons.[3] An earlier theoretical literature (see Riddiough and Wyatt, 1994; Wang, Young, and Zhou, 2002) raised the possibility of asymmetric information as a barrier to mortgage renegotiation but did not provide empirical evidence. Our model differs from these models as we posit that the asymmetric information is about the borrower's use value rather than default costs.

Our empirical results are evidence of the impact of principal writedowns on commercial real estate borrower behavior, indicating that information asymmetries between borrowers and lenders that impede otherwise efficient debt renegotiation are likely also important for residential mortgages and other debt markets. Our finding that high-value borrowers are able to imitate bad borrowers is important because this has the potential to constrain lenders' ability to modify distressed loans efficiently. Although we study the CRE market specifically, our results speak to the potential impact of principal forgiveness in residential mortgages as well. Given that residential mortgage servicers have significantly less ability to assess borrowers' ability to pay, the fact that we see evidence of imitation in CRE suggests it is likely that such behavior would be present in residential real estate as well.

In contemporaneous work, Dinc and Yönder (2022) provide evidence of strategic behavior by commercial mortgage borrowers that complements ours. They show that many

[3] See, for example, Foote, Gerardi, and Willen (2008), Elul, Souleles, Chomsisengphet, Glennon, and Hunt (2010), Adelino, Gerardi, and Willen (2013), Guiso, Sapienza, and Zingales (2013), Maturana (2017), Ganong and Noel (2020), Cespedes, Parra, and Sialm (2021), and Low (2021).

defaulting commercial mortgage borrowers continue to make payments on other obligations, indicating that they are not financially distressed in the sense that they lack the cash flow needed to make payments. In the language of our model, Dinc and Yönder (2022) provide evidence that bad borrowers (low types), i.e., those with private use values below the face value of the mortgage, default strategically. In contrast, we provide evidence on how good borrowers (high types) behave strategically to the disadvantage of lenders without necessarily defaulting. Our evidence of strategic behavior is based on the fact that some borrowers with a high present value of retaining control of an asset imitate those with a low value in an attempt to obtain a modification. In another contemporaneous paper, Glancy, Kurtzman, and Loewenstein (2022) build on the insight of Black, Krainer, and Nichols (2020) to study the differences in modification propensity between bank and CMBS loans and how these differences affect *ex ante* loan terms and the sorting of borrowers between lender types. Related to our work, they show that bank borrowers strategically default more often than CMBS borrowers. Unlike their paper, we focus within the CMBS market on different borrower types, and our interest is in the impact that asymmetric information has on borrower behavior when renegotiation is costly.

Our paper also relates to the corporate finance literature that examines corporate and sovereign debt renegotiation, including Hackbarth, Hennessy, and Leland (2007), Bolton and Jeanne (2007), Davydenko and Strebulaev (2007), Valta (2016), Antill and Grenadier (2019), and Campello, Ladvika, and Matta (2019). Our model is stylized to capture the specific regulatory and institutional framework of the CMBS market such that we can use it to guide our empirical analysis. In contrast to these models, we introduce a cost to the borrower of requesting a loan modification and study the effect of its changes.

Finally, our results speak broadly to the unintended consequences of regulation designed to encourage loan renegotiation. This is particularly important in light of recent real estate market turmoil and the response of regulators. In April 2020, in response to anticipated distress due to the COVID-19 pandemic, the IRS issued a rule that further expands the scope for CRE loan forbearance and modifications prior to default.[4] This rule directly parallels the rule we exploit in our empirical analysis. Although these types of policies, which are designed to encourage proactive renegotiation, may allow efficient pre-default resolution of certain loans, they may also encourage borrowers who otherwise would perform to use the additional renegotiation flexibility to extract concessions from servicers. Importantly, we do not evaluate whether the rule change reduced defaults on financially healthy loans with balloon payments coming due during the GFC.

The remainder of this paper proceeds as follows. Section 2 discusses the institutional details, Section 3 presents a model of DPO negotiation, Section 4 describes the data and methodology, Section 5 presents the results, and Section 6 concludes.

2 Institutional Overview

Unlike residential mortgages, most commercial mortgages have balloon payments due at the end of their terms such that the borrower must either refinance the loan or risk default. Absent any major change in the property's ability to generate cash flows, refinancing a CMBS loan is usually routine. Like residential mortgages, commercial mortgages are usually securitized using a Real Estate Mortgage Investment Conduit (REMIC) structure. REMICs themselves are exempt from federal taxes, and only the income earned by investors in the MBS is subject to federal tax. The tax-exempt status of the REMIC rests in part on whether it adheres to rules governing the types of mortgages it can hold. So long as the REMIC holds "qualifying mortgages," it remains tax-exempt, but it may lose this status if a non-trivial portion of the mortgage pool loses qualifying status. The nature of a REMIC is that the trust cannot engage in active management of loans held in the trust without being taxed as a corporation.

One reason a loan may lose its qualifying status is if it is modified, because significant modifications may be treated as an exchange of the original loan for a new (modified) loan.

[4] See `https://www.irs.gov/irb/2020-26_IRB` for more information on IRS Revenue Procedure 2020-26.

Because REMICs are prohibited from purchasing new mortgages or exchanging mortgages currently in the pool for others, a modification that constitutes an exchange or new purchase would threaten the REMIC tax exemption. The modifications permitted under REMIC rules largely come from the residential mortgage market given the much longer history of securitization of residential mortgages in the modern era. While there was some securitization of commercial mortgages in the 1920s (Goetzmann and Newman, 2010), the modern CMBS market dates to the mid-1990s while the modern RMBS market dates to the early 1980s. Without any major financial crises to test the CMBS market, no changes had been made to the REMIC rules to allow for term extensions.

When securitized commercial mortgages become distressed, the borrower may request that the master servicer transfer the loan to a special servicer. The special servicer is responsible for working the loan out and/or initiating foreclosure. The servicer has many workout options available, including modifications such as term or interest rate changes, or DPOs. Although the borrower and special servicer can engage in discussions about modifications prior to a transfer or default (see Internal Revenue Service (2009) Section 3.11), the actual workout process can only begin after the master servicer transfers the loan into special servicing. Once transferred, the borrower can engage directly with the special servicer and begin modifications or other renegotiations. Thus, the transfer event is the most significant event with respect to renegotiating the terms of the loan.

We note two important institutional details about the relationship between the master servicer and the special servicer. First, the special servicer is selected *ex ante*. The master servicer does not have scope to select a different special servicer after the deal has been originated. This means the master servicer cannot select a special servicer conditional on realized loan distress. The only stakeholder that can change the special servicer is the B-piece buyer. Second, the master servicer has no incentive not to transfer the loan upon the borrower's request. This is because the master servicer must advance principal and interest payments to the CMBS bondholders when the loan is delinquent, unless it transfers the loan to special servicing. Transferring the loan removes the need for the master servicer to advance payments.

2.1 The IRS rule change

The barriers to loan modification that the REMIC tax rules created became a significant issue in 2007 as financial crisis-related mortgage distress increased. In response and in order to allow for more efficient distressed loan resolution, the IRS, beginning in December 2007, issued a series of Revenue Procedures that provided safe harbor provisions for *residential* MBS REMICS. These procedures stated that significant loan modifications would not trigger an IRS challenge of the tax-exempt status of REMICS, provided the loans met certain criteria.[5]

For securitized commercial real estate loans, these barriers were removed in September of 2009. Prior to September 2009, modifications did not nullify a loan's qualifying status and hence did not threaten the REMIC tax status, so long as the modification was made either (1) after the loan had actually defaulted or (2) when default was "reasonably foreseeable." The "reasonably foreseeable" criterion was usually interpreted narrowly such that only defaults expected within, e.g., two to three months qualified.[6] Thus, prior to the rule change, a loan could only be transferred to special servicing and subsequently modified if it had experienced a default event or if a default was imminent. Note that although transfer to special servicing itself would not threaten the tax status of the REMIC, transfer is a necessary condition for loan modification. Hence, transfers were in effect limited to cases in which the subsequent modifications would have been acceptable under the REMIC tax rules.

The definition of a default depends on the loan documents and the CMBS deal's Pooling and Servicing Agreement (PSA), but a standard definition is 60+ days delinquent, which means the loan has missed more than two monthly payments. Therefore, prior to the rule

[5] These Revenue Procedures include Rev. Proc. 2007-72, Rev. Proc. 2008-28, and Rev. Proc. 2008-47. See Beeman (2009) for a discussion.

[6] See, e.g., `https://www.seyfarth.com/news-insights/irs-announces-new-remic-rules.html` for legal industry commentary on the reasonably foreseeable standard.

change, a transfer and modification could take place after the loan became 60+ days delinquent, or if there was a reasonably foreseeable likelihood of it doing so within a few months.

In September 2009, the IRS issued Revenue Procedure 2009-45 (Internal Revenue Service, 2009). This rule significantly relaxed the criterion that required either an actual or reasonably foreseeable default in order for a loan to be modified without negative tax consequences. Section 5 of the Procedure states that:

> This revenue procedure applies to a modification...if... Based on all the facts and circumstances, the holder or servicer reasonably believes that there is a significant risk of default of the pre-modification loan upon maturity of the loan or at an earlier date. This reasonable belief must be based on a diligent contemporaneous determination of that risk, which may take into account credible written factual representations made by the issuer of the loan if the holder or servicer neither knows nor has reason to know that such representations are false. In a determination of the significance of the risk of a default, one relevant factor is how far in the future the possible default may be. There is no maximum period, however, after which default is per se not foreseeable. For example, in appropriate circumstances, a holder or servicer may reasonably believe that there is a significant risk of default even though the foreseen default is more than one year in the future. Similarly, although past performance is another relevant factor for assessing default risk, in appropriate circumstances, a holder or servicer may reasonably believe that there is a significant risk of default even if the loan is performing.

In particular, the procedure allows a transfer and modification so long as the servicer believes there is risk of default at some point in the future, but it does not specify a definite time frame. Additionally, the procedure provides for the determination of default based on borrower representations.

The text of the rule change, along with industry commentary (see, e.g., Globe Street, 2009; NYSBA, 2008), point to term extensions as a key motivation for modifying existing REMIC rules. Given the longer history of the modern residential MBS market, the REMIC rules in place prior to 2009 were designed with residential mortgages in mind and did not foresee the need for common modifications of commercial mortgages (see pp. 2-3 of NYSBA, 2008). However, the extraordinary lack of securitized financing available during the GFC made refinancing difficult such that the inheritance of the residential rules became a major problem.[7]

Section 2.02 of the rule change (Internal Revenue Service, 2009) is instructive as to the focus on term extensions for financially healthy loans. It reads

> The current situation in the credit markets is affecting the availability of financing and refinancing for commercial real estate. In particular, borrowers under many of the commercial mortgage loans that will mature in the next few years are concerned that they will encounter great difficulty in obtaining refinancing for these loans. Because they had always anticipated using the proceeds from refinancing to satisfy the principal balance due at maturity, these borrowers are often at risk of defaulting when their loans mature. This may be true even for loans in which the underlying commercial real estate is providing more than enough cash flow to satisfy debt service before maturity.

Section 7 of Internal Revenue Service (2009) also suggests term extensions for healthy loans as a primary motivation.

Importantly for our analysis, nothing in the rule change is intended to change the outcomes for low type borrowers. The existing REMIC rules already permitted modifications for borrowers experiencing financial distress (i.e., borrowers with a low use value of the property). Rather, the goal of the rule change is merely to keep borrowers that want to stay in

[7] In addition to term extensions, the rulemaking discussion also refers to the possibility of allowing modifications that change the recourse status of the loan (NYSBA, 2008). A change in recourse status had not been previously covered because, in contrast to commercial mortgages where recourse provisions are negotiated on a case by case basis, state law largely determines the recourse status of residential mortgages.

the property from defaulting due to an inability to obtain a new loan. Despite this, the language of the rule change was sufficiently broad to allow the borrower to request a DPO, or another type of modification, instead of a term extension.

2.2 Outcomes of borrower-lender renegotiation

Once loans are transferred into special servicing, the borrower and lender negotiate over a variety of potential resolutions. For our purposes, these resolutions can be grouped into three types. The first type is a loan modification, which can take the form of temporary interest rate reductions, maturity date extensions, amortization changes, forbearance, or a combination of these. Modifications allow the borrower to retain control of the property, but they do not result in any principal reduction. The second type is a DPO, which involves writing off some of the outstanding principal. The third type is a foreclosure, which we consider to include any resolution type in which the borrower surrenders control of the property. This can include actual foreclosure, short sale, or deed in lieu of foreclosure.

From among this set of post-transfer resolution possibilities, we focus on DPOs for two reasons. First, unlike a modification, a DPO provides an immediate and permanent principal reduction. Second, unlike a foreclosure, a DPO allows the borrower to retain the property. Thus, DPOs are a potentially high-payoff concession that both permanently modifies the loan terms and allows borrowers to retain control of the property.

The potential for favorable outcomes like a DPO gives rise to the potential for financially healthy borrowers to imitate distressed borrowers if they believe there is a chance they can obtain one. The incentive of borrowers to do so will depend on (1) their expectation about receiving a DPO and (2) their expected cost to obtaining a transfer. The expected cost will depend in part on the presence of regulations, such as Revenue Procedure 2009-45, that govern when and under what circumstances loans can be transferred without jeopardizing the REMIC tax status. In the following section, we formalize this intuition in a model of renegotiation between the borrower and lender/special servicer.

3 Model of DPO Negotiation

Motivated by the discussion in Section 2, we develop a principal-agent model of DPO negotiations between a lender (principal, she) and a borrower (agent, he). We assume that both parties are risk-neutral and maximize their expected payoffs. The borrower has a non-recourse mortgage of M dollars secured by a property with a market value of P.[8] If the lender forecloses on the property, she will recover F dollars, with $F < P$ due to various administrative costs and inefficiencies associated with a foreclosure.

The borrower privately values the property at u dollars. The investor-specific valuation u can differ from the market value P due to a number of factors. For example, the current owner's entrepreneurial skills and priorities may differ from those of potential buyers. (P is sometimes referred to as the market value of the property, while u is the investment value.) Unlike P, M, and F which are publicly known, u is the borrower's private information.

The timeline of the negotiation is as follows. At time $t = 0$, the borrower with private value u decides whether to request a DPO. Due to legal and reputational considerations, the borrower must pay a cost $c(u, R)$ to request a DPO. We assume that $c(u, R)$ is a continuously differentiable function of the borrower's private value u and the level of regulation R and, for $u \geq M$, $c(u, R)$ is strictly increasing and convex in u. In addition, $c(u, R)$ is increasing in R, i.e., a higher R corresponds to a level of regulation that makes DPO procedures more costly for borrowers.

At time $t = 1$, upon receiving a DPO request, the lender chooses between a foreclosure and a DPO D, with $D < M$. If the lender and the borrower agree on the DPO, the lender gets paid D instead of F, the mortgage is terminated, and the borrower will extract utility u from owning the property. If the lender proceeds with a foreclosure, the borrower is given the final opportunity to repay the entire loan M and retain the property at time $t = 2$. If the borrower does not pay M, he loses the property and the lender recovers F on the loan.

[8] While many commercial mortgages held on banks' balance sheets are recourse (Glancy, Kurtzman, Loewenstein, and Nichols, forthcoming), and especially loans used to finance development, the overwhelming majority of CMBS loans are non-recourse.

There are several reasons why requesting a DPO is costly for the borrower. First, for high types, it requires costly effort to hide or manipulate financial information in order to make it appear as though financial distress is imminent. For example, the higher the private use value, the greater the difficulty a borrower will have concealing cash flows, as compared to a borrower who is truly close to distress and lacking cash flows to pay the mortgage. Second, in the presence of imperfect information, there are reputational costs associated with default such as a higher cost of future credit. Furthermore, default by the borrower may encourage existing tenants to default on their lease obligations and make it more difficult to attract new tenants.

We assume that the lender can commit to a negotiation strategy, which allows her to make a credible take-it-or-leave-it offer of D to the borrower. In practice, this commitment is possible since lenders play a repeated game by negotiating loans with multiple borrowers. On the other hand, the DPO negotiation is a one-shot game for the borrower. As a result, the borrower lacks a commitment mechanism and chooses a subgame perfect strategy over the course of the negotiation.

3.1 DPO negotiation with complete information

We start our analysis of DPO negotiations with a benchmark case in which the lender knows the borrower's value u. While the equilibrium with complete information is straightforward, it highlights tensions between the lender and the borrower that will remain relevant in a setting with incomplete information. We set $c(u, R) = 0$ in the complete information case since the borrower's cost of requesting a DPO is primarily due to the effort to imitate a bad borrower which is not possible under complete information. Because the cost of requesting a DPO is 0 in this case, all borrowers request a DPO.

If $u \leq F$, it is optimal for the lender to proceed with a foreclosure since the borrower will not pay more than F to retain the property. If $F < u < M$, it is optimal for the lender to offer the borrower DPO $D = u$, where u is the maximum amount the borrower is willing to pay to retain the property. When $u \geq M$, proceeding with a foreclosure is optimal again. In this case, however, the borrower will agree to repay the entire loan amount M in order to avoid losing the property. We summarize our observations in the following proposition.

Proposition 1 *If a borrower whose value u is known to the lender requests a DPO, the subsequent negotiation between the borrower and the lender results in the following equilibrium outcomes*

(i) When $u \leq F$, the DPO negotiation ends in a foreclosure, with the lender's payoff of F, the borrower's payoff of 0.

(ii) When $F < u < M$, the DPO negotiation ends in a DPO $D = u$, with the lender's payoff of u, the borrower's payoff of 0.

(iii) When $u \geq M$, the DPO negotiation starts with a foreclosure procedure and ends in a full payout, with the lender's payoff of M and the borrower's payoff of $u - M$.

We will refer to borrowers with $u \geq M$ as high type borrowers (borrowers that place a high private value on retaining and operating the property), and borrowers with $u < M$ as low type borrowers (borrowers that place little value on retaining and operating the property). Part (iii) of Proposition 1 says that when the lender knows she is dealing with a high-value borrower, she will not agree to a DPO. Thus, in order for a high type to successfully negotiate a DPO, he has to imitate a low type. High types can be seen as "good" borrowers since they are willing to pay the entire loan amount, unlike "bad" low types who are going to default on their loans. In the full information case, high types cannot successfully imitate low types despite the possibility of obtaining a DPO giving them the incentive to do so.

3.2 DPO negotiation with asymmetric information

We now consider a setting in which the lender negotiates a DPO with a borrower without knowing his private value u. The lender believes that the private value u of a borrower of a distressed loan is distributed according to the cumulative distribution function $\Phi(u)$ and the probability density function $\phi(u)$.

We normalize $c(u, R) = 0$ for $u < M$ since a low type is about to lose the property to a foreclosure anyway, and requesting a DPO does not create additional legal or reputational problems. Therefore, $c(u, R)$ represents the incremental cost associated with requesting a DPO for high types.

If the lender decides to proceed with a foreclosure, a borrower with $u \geq M$ will agree to pay M to retain the property. However, a borrower with $u < M$ will refuse to pay M, resulting in the lender's payoff of F. Thus, the lender's expected payoff L^F from pursuing a foreclosure is given by

$$L^F = F\Phi(M) + M(1 - \Phi(M)).$$

If the lender decides to proceed with DPO D, a borrower with $u \geq D$ will agree to pay D to retain the property. On the other hand, a borrower with $u < D$ will refuse to pay D, resulting in a foreclosure outcome with a payoff F to the lender. Thus, the lender's expected payoff $L(D)$ from pursuing DPO D is given by

$$L(D) = F\Phi(D) + D(1 - \Phi(D)).$$

Let D^* denote the DPO that maximizes the lender's payoff

$$D^* = \arg\max_{D \leq M} \{F\Phi(D) + D(1 - \Phi(D))\}.$$

We note that when $D = M$, the DPO is equivalent to a foreclosure, i.e., $L(M) = L^F$. Thus, the lender chooses a DPO over a foreclosure if and only if $D^* < M$. Let $L'(D)$ denote the derivative of $L(D)$ with respect to D:

$$L'(D) = -(D - F)\phi(D) + 1 - \Phi(D). \tag{1}$$

Then, $L'(M) < 0$ is a sufficient condition for a DPO being preferred over a foreclosure.

Indeed, when $L'(M) < 0$, a small reduction in mortgage repayment would increase the lender's expected payoff. Plugging $D = M$ into equation (1) yields Proposition 2.

Proposition 2 *If*

$$(M - F)\phi(M) > 1 - \Phi(M), \tag{2}$$

then the lender strictly prefers a DPO over a foreclosure, when the borrower's private value is not observable.

To interpret equation (2), assume that the lender offers the borrower a small discount on loan repayment, i.e., $D = M - \varepsilon$, for some small $\varepsilon > 0$. This will reduce the lender's payoff by ε with probability $1 - \Phi(M)$, which is the probability that $u > M$, and the borrower is willing to pay M to avoid a foreclosure. On the other hand, the DPO increases the lender's payoff by $M - \varepsilon - F$ with probability $\phi(M)\varepsilon$. Indeed, $\phi(M)\varepsilon$ is the probability that $u \in [M - \varepsilon, M]$; i.e., $\phi(M)\varepsilon$ is the probability that the borrower would switch from accepting a foreclosure to paying $M - \varepsilon$. Thus, the lender is better off with the DPO if

$$(M - \varepsilon - F)\phi(M)\varepsilon > (1 - \Phi(M))\varepsilon. \tag{3}$$

In the limit $\varepsilon \to 0$, equation (3) becomes (2).

We note that equation (2) is a sufficient condition for a DPO to be the preferred solution. Even if equation (2) does not hold, the lender may prefer a DPO over a foreclosure depending on the model parameters. To focus on the interesting case, from now on, we assume that M, F, and $\Phi(u)$ are such that the lender strictly prefers a DPO over a foreclosure, i.e., $D^* < M$.

3.3 DPO request decisions

The lender has limited capacity to process DPO requests due to a limited number of employees with the skills required to do DPOs. Because mortgage delinquency tends to be low for long periods of time and then surges, lenders may not be able to rapidly train enough skilled employees to accommodate peak demand to process every DPO request.[9] We model the limit to lender capacity as introducing uncertainty in whether the lender will process the DPO request. In particular, the lender starts negotiating a DPO with probability $\delta(\kappa, R)$, and with probability $(1 - \delta(\kappa, R))$, the lender proceeds with a foreclosure. We assume that δ is increasing in the lender-specific DPO capacity κ and weakly decreasing in the level of regulations R. In other words, a lender with higher capacity κ is more likely to process a DPO request, while a higher level of regulation R makes DPOs less likely.[10] More stringent regulation makes DPOs less likely because the lender must be very certain that modifying the loan would not jeopardize the REMIC tax status. This requires the lender to exert time and effort to be certain that the borrower is in or close to default. In contrast, less stringent regulation allows the lender to modify loans that are currently performing, so long as the lender believes the loan will default eventually without the modification, which lowers the level of time and effort required for a given loan. This allows lenders to process more DPO requests holding the level of time and resources (employees) constant.

DPO capacity may be conceptually distinct from willingness to negotiate DPOs. A lender may have the capacity to do DPOs (i.e., they may have more than enough employees and not face financial constraints), yet they may be unwilling to do DPOs if they expect higher NPV from making other modifications. On the other hand, lenders may be willing to do DPOs and yet lack the resources to negotiate them. From the borrower's perspective, whether it is capacity, willingness, or both, does not matter, because borrowers request DPOs when they expect the lender to be more likely to offer one. In order to maintain consistent language in

[9]See Holden, Kelly, McManus, Scharlemann, Singer, and Worth (2012), Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2017), Calem, Jagtiani, and Maingi (2021), Aiello (2022), and Kim, Lee, Scharlemann, and Vickery (2021) for evidence on and discussion of limits in mortgage servicer capacity.

[10]Although we introduce uncertainty about whether the borrower will receive a DPO using constraints to lender capacity, it is also possible that there is uncertainty due to lender-specific policies that make certain lenders more lenient than others. If borrowers learn that one lender is more lenient and likely to negotiate DPOs, they may request them more often. Because the focus of our model is on how borrowers react to changes in their perception of the likelihood of a DPO, and not on why lenders choose particular DPO policies, we do not model other sources of uncertainty. As long as there is any uncertainty in whether the borrower will receive a DPO conditional on requesting one, the specific source of uncertainty does not matter for our analysis.

the model, we refer to κ as capacity.

The borrower's expected payoff $B^{DPO}(u)$ conditional on a DPO request is given by

$$B^{DPO}(u) = \begin{cases} 0, \quad if \quad u \leq D^* \\ \delta(\kappa, R)(u - D^*), \quad if \quad D^* < u < M \\ \delta(\kappa, R)(u - D^*) + (1 - \delta(\kappa, R))(u - M) - c(u, R), \quad if \quad u \geq M. \end{cases}$$

If the borrower does not request a DPO, he will either pay M or lose the property to foreclosure, resulting in the following payoff

$$B^N(u) = \begin{cases} 0, \quad if \quad u < M \\ u - M, \quad if \quad u \geq M. \end{cases}$$

The expression for $B^{DPO}(u)$ indicates that bad borrowers have no cost of requesting a DPO. As in the complete information case, bad borrowers therefore always request a DPO regardless of the value of $\delta(\kappa, R)$. A key difference with the complete information case is that some good borrowers make DPO requests depending on both the cost of a request and the likelihood of its success, as given by $\delta(\kappa, R)$.

The net gains associated with a DPO request are given by

$$\Delta B^{DPO}(u) \equiv B^{DPO}(u) - B^N(u) = \begin{cases} 0, \quad if \quad u \leq D^* \\ \delta(\kappa, R)(u - D^*), \quad if \quad D^* < u < M \\ \delta(\kappa, R)(M - D^*) - c(u, R), \quad if \quad u \geq M. \end{cases} \tag{4}$$

A borrower with $u \leq D^*$ has nothing to gain or lose by requesting a DPO since he loses his property to a foreclosure in any case. A borrower with $D^* < u < M$ gains from a DPO by paying less than his private use value. A high type borrower, i.e., $u > M$, benefits from a DPO by paying less than the full loan amount M that he would be paying otherwise. However, this borrower is paying the additional cost $c(u, R)$ while requesting a DPO. The next proposition characterizes conditions under which a high type decides to request a DPO.

Proposition 3 *There is a threshold $\bar{u}(\kappa, R) > M$ such that it is optimal for borrowers with $u \leq \bar{u}(\kappa, R)$ to request DPOs, and for borrowers with $u > \bar{u}(\kappa, R)$ to pay M without requesting a DPO. The threshold $\bar{u}(\kappa, R)$ is increasing in κ and decreasing in R.*

Proof *A borrower with private value $\bar{u}$ is indifferent between requesting a DPO and paying M. According to equation (4), $\bar{u}$ must solve the following equation*

$$\delta(\kappa, R)(M - D^*) - c(\bar{u}, R) = 0.$$

Because $c(u, R)$ is continuous, strictly increasing, and a convex function of u and $c(M, R) = 0$, there exists a unique solution $\bar{u}$ to the above equation and it must be greater than M.

According to the implicit function theorem, we have

$$\frac{\partial \bar{u}}{\partial \kappa} = \frac{\frac{\partial \delta}{\partial \kappa}(M - D^*)}{\frac{\partial c(\bar{u}, R)}{\partial \bar{u}}} > 0,$$
$$\frac{\partial \bar{u}}{\partial R} = \frac{\frac{\partial \delta}{\partial R}(M - D^*) - \frac{\partial c(\bar{u}, R)}{\partial R}}{\frac{\partial c(\bar{u}, R)}{\partial \bar{u}}} < 0.$$

The inequalities follow from the fact that $c(u, R)$ is increasing in both u and R, while δ is increasing in κ and decreasing in R. Thus, $\bar{u}(\delta, R)$ is increasing in δ and decreasing in R. Q.E.D.

Proposition 3 says that a lender with a higher DPO capacity κ will receive more DPO requests due to the inflow of high types. In particular, the mass of high types with $u \in (M, \bar{u}(\kappa, R))$ who request DPOs is increasing in κ. Intuitively, because of the costs associated with DPO requests, high types pursue DPOs only if they have a high enough chance to succeed. Because $u < M$ for bad borrowers, Proposition 3 also states that bad borrowers always request a DPO regardless of the value of κ or R. Although R does not affect the volume of DPO *requests* from bad borrowers (because bad borrowers always request a DPO),

R does affect the volume of DPOs that lenders actually grant to bad borrowers because R affects the probability that the lender grants a DPO through the function $\delta(\kappa, R)$.

Finally, Proposition 3 states that relaxing regulations, i.e., lowering R, will prompt more high types to request DPOs due to lower costs of DPO requests and higher probability of DPO approval. As a direct consequence, we have Corollary 1 that says that relaxing regulations also increases the probability that a DPO request ends up in a full payoff of the mortgage principal if the density of high-value borrowers is sufficiently high.[11]

Corollary 1 *Conditional on a DPO request, the probability of a full payoff decreases in R, provided the density of high-value borrowers is sufficiently high.*

Proof *Let X denote the number of DPO requests from low-value borrowers. Since there is no additional cost for those borrowers to request a DPO (see equation (4)), X does not depend on R. Let $\theta(u)$ denote the density of high-value borrowers. According to Proposition 3, the number Y of DPO requests coming from high-value borrowers is a function of $\bar{u}(\kappa, R)$ and is given by*

$$Y(\bar{u}) = \int_{M}^{\bar{u}(\kappa,R)} \theta(u)du.$$

Low types never repay the loan in full, while high types who requested DPOs fully repay their loan with probability $(1-\delta)$. Thus, the probability of a full payoff conditional on a DPO request equals

$$\pi(R) \equiv \frac{(1-\delta)Y(\bar{u})}{X+Y(\bar{u})}.$$

Differentiating $\pi(R)$ yields

$$\pi'(R) \equiv \frac{(1-\delta)X\theta(\bar{u})\frac{\partial\bar{u}}{\partial R} - \frac{\partial\delta}{\partial R}Y(\bar{u})\left(X+Y(\bar{u})\right)}{\left(X+Y(\bar{u})\right)^2}.$$

Proposition 3 says that $\frac{\partial\bar{u}}{\partial R} < 0$. If $\theta(\bar{u})$ is sufficiently high, then $\pi'(R) < 0$. Q.E.D.

[11] A high density of high-value borrowers in consistent with empirically observed default losses in pre-2008 vintage CMBS deals. Flynn, Ghent, and Tchistyi (2020), for example, show that 2000-2007 vintage CMBS deals had suffered less than 10% cumulative losses as of December 2018 on average.

3.4 DPO request decisions with anticipated regulatory change

In this subsection, we extend our model to allow borrowers to delay DPO requests in anticipation of a regulatory change that can reduce costs of DPO requests for borrowers and increase DPO likelihoods. In particular, we consider a two-period extension of our model in which the level of regulation R_2 in the second period is lower than that in the first, i.e., $R_2 < R_1$. Let $c_t(u) \equiv c(u, R_t)$ and $\delta_t(\kappa) \equiv \delta(\kappa, R_t)$ denote the costs of DPO requests and DPO probabilities in periods $t = 1$ and 2, with

$$c_2(u) = \alpha c_1(u),$$
$$\delta_2(\kappa) = \beta \delta_1(\kappa),$$

where $\alpha < 1$ and $\beta > 1$ due to weaker regulations in the second period. As before, we assume DPO with probabilities $\delta_1(\kappa)$ and $\delta_2(\kappa)$ are increasing in lender's specific capacity κ.

In period 1, borrowers must decide whether to request a DPO in the first period or postpone the request until the second period, which would require staying current on their loans. There is a cost $\rho(u)$ associated with delaying a DPO request for low-value borrowers, who prefer foreclosure over debt repayment. In other words, the cost of servicing the debt exceeds the value generated by the property for low-value borrowers. Thus, we assume $\rho(u)$ is a continuous function that is strictly decreasing for $u \leq M$. We normalize $\rho(u) = 0$ for the high-value borrowers with $u \geq M$.

Requesting a DPO in the second period increases the probability of a DPO by $\delta_1(\kappa)(\beta - 1)$. In addition, the cost of a DPO request is reduced by $(1 - \alpha)c_1(u)$ for high-value borrowers, while low-value borrowers pay the delay cost $\rho(u)$. Thus, delaying a DPO request results in the following net payoff for a borrower

$$W(u) = \begin{cases} -\rho(u), \quad if \quad u \leq D^* \\ \delta_1(\kappa)(\beta-1)(u-D^*) - \rho(u), \quad if \quad D^* < u < M \\ \delta_1(\kappa)(\beta-1)(M-D^*) + (1-\alpha)c_1(u), \quad if \quad u \geq M. \end{cases} \tag{5}$$

Proposition 4 *There is a threshold $\hat{u}(\kappa) \in (D^*, M)$ such that it is optimal for borrowers with $u \leq \hat{u}(\kappa)$ to request DPOs in period 1 and for borrowers with $u > \hat{u}(\kappa)$ to request DPOs in period 2. The threshold $\hat{u}(\kappa)$ is decreasing in κ.*

Proof *A borrower who is indifferent between requesting a DPO in the first period and delaying the request to the second period has private value $\hat{u}$ such that $W(\hat{u}) = 0$. We note $W(u)$ is a continuous and strictly increasing function of u. Moreover, $W(u) < 0$ for $u \leq D$ due to the delay cost $\rho(u)$, and $W(u) > 0$ for $u \geq M$ since $\beta > 1$ and $\alpha < 1$. As a result, $\hat{u} \in (D^*, M)$ and*

$$\delta_1(\kappa)(\beta-1)(\hat{u}-D^*) - \rho(\hat{u}) = 0.$$

By the implicit function theorem,

$$\frac{\partial \hat{u}}{\partial \kappa} = -\frac{\frac{\partial \delta_1}{\partial \kappa}(\beta-1)(\hat{u}-D^*)}{\delta_1(\kappa)(\beta-1) - \frac{\partial \rho}{\partial \hat{u}}} < 0,$$

where the inequality follows from the fact that $\frac{\partial \delta_1}{\partial \kappa} > 0$, $\frac{\partial \rho}{\partial \hat{u}} < 0$, and $\beta > 1$. Thus, $\hat{u}(\kappa)$ is decreasing in κ. Q.E.D.

The fact that $\hat{u}(\kappa)$ is decreasing in κ implies that if a lender has a higher intrinsic DPO capacities, then more of its borrowers will delay DPO requests to period 2. Intuitively, lenders with higher intrinsic DPO capacities are more capable of taking advantage of the relaxed regulations, which benefits their borrowers through higher DPO probabilities in the second period.

Thus, Propositions 3 and 4 predict different effects in the first period. In a setting with a constant regulatory environment or unanticipated regulatory changes, Proposition 3 predicts that a lender with a higher DPO capacity κ will receive more DPO requests in every period. On the other hand, in a setting with anticipated regulatory easing, Proposition 4 predicts the same effect in the second period only. In contrast, Proposition 4 predicts a lender with a higher DPO capacity κ will receive fewer DPO requests in the first period.

3.5 Empirical implications

Propositions 3 and 4 state that after a decrease in R, the number of DPO requests will increase because more high types will request DPOs, and a DPO request is more likely to lead to full repayment of the loan. Importantly, because all low types always request a DPO, the number of DPO requests from them does not depend on R in the model. These results imply a number of testable hypotheses, including several related to Revenue Procedure 2009-45, which significantly relaxed conditions necessary for any type of loan renegotiation, including DPOs. This rule change corresponds to a reduction in R in the model. Prior to the rule change, borrowers had to default on their loans (or be very close to default) in order for DPO negotiations to take place. After September 2009, Revenue Procedure 2009-45 allowed borrowers to request a transfer to the special servicer and negotiate DPOs while being current on their loans. Consistent with the model assumptions, the new rule primarily benefits high types. Indeed, it is optimal for low types to make DPO requests even if default is required to do so since $u < M$ for them. On the other hand, defaults for high types result in reputational and legal costs that are otherwise avoidable. In practice, there may be some reputational costs for borrowers that are on the margin between being a low and high type that prevent them from requesting a DPO. Our empirical estimation addresses this issue by looking at how the rule change differentially affects high- and low-type borrowers.

Because CMBS loan transfer from the master servicer to the special servicer is a necessary condition for a DPO negotiation, we proxy for DPO requests in the data using transfers. As a result, we have the following hypotheses:

Hypothesis 1: CMBS loans are more likely to be transferred into special servicing when servicer DPO capacity is perceived to be high at any time when regulatory costs are fixed and not expected to change.

Hypothesis 2: Transferred loans are more likely to fully pay off *ex post*.

Finally, Proposition 4 says that if the rule change was anticipated, then fewer borrowers will request DPOs prior to the rule change when a lender has a higher perceived DPO capacity. As discussed in Section 2.1, between December 2007 and July 2008, the IRS issued three Revenue Procedures that granted RMBS borrowers the same types of tax rule concessions that were eventually granted to CMBS borrowers. Therefore, it is very likely that CMBS borrowers anticipated the rule change in the period of time leading up to September 2009. This motivates our final hypothesis:

Hypothesis 3: Prior to the regulatory change in September 2009, CMBS loans are less likely to be transferred into special servicing when servicer DPO capacity is perceived to be high.

4 Data and Empirical Methodology

To test the main implications of the model, we use data on private-label CMBS loans originated between January 2002 and December 2007 from Trepp. We use 2002 as the starting point because the average (and median) loan in the sample has a maturity date of roughly 10 years, which means that the average loan has more than two years remaining to maturity at the time Revenue Procedure 2009-45 went into effect. This reduces the likelihood that any relation between DPOs and transfers in the time period immediately surrounding the rule change is confounded by borrower behavior that is driven by the need to refinance imminently-maturing loans.[12] Furthermore, we use December 2007 as the end point to focus only on loans originated before Revenue Procedure 2009-45 went into effect and before the onset of the GFC. This ensures that our results are not confounded by the possibility that new borrowers issuing loans in the post-rule change period are inherently more opportunistic. Thus, we are able to focus on the change in existing borrower behavior pre- and post-rule change.

For this set of loans, we measure transfers and other time-varying loan characteristics during the September 2007-September 2011 period. This gives us a balanced number of months on either side of the rule change (which occurred in September 2009).[13]

4.1 Main variable construction

In the model, high types request a DPO based on the levels of κ and R. In practice, such a DPO request is formally made once the borrower's loan has been transferred from the master servicer to the special servicer. Transfers are readily observable in the Trepp data, whereas DPO requests (or requests for any other type of workout) subsequent to transfer are not. However, because transfer is a necessary condition for a DPO request to occur, we proxy for DPO requests using transfers to the special servicer. We identify transfers using Trepp's field for transfer dates. For loans that are transferred into special servicing, the transfer date field indicates the month in which the transfer occurs, and this field is missing for loans that are never transferred. Using this field, we define our main dependent, $transfer_{i,s,t}$, as an indicator that is equal to 1 if loan i serviced by special servicer s is transferred in month t. For loans that experience a transfer at some point in the sample, this variable is set to 0 in months prior to transfer and missing in months following the transfer. For loans that never

[12] To further address concerns that any observed relation between DPOs and transfers is mechanically driven by loans approaching repayment and borrowers acting in response to the need for imminent refinancing, we include in all our regressions a control for the age of the loan, as well as origination time and current time fixed effects.

[13] For our analysis of outcomes following transfer, we extend the loan performance data to December 2020. This is because post-transfer outcomes such as default or full payoff may take time to occur, and we wish to ensure that loans originated in December 2007 have sufficient time to experience default or full payoff.

experience a transfer, this variable is always set to 0. Hence, the dependent variable only takes a value of 1 in months in which a loan is transferred.

The model implies that higher perceived special servicer DPO capacity κ will increase the propensity for high types to request a DPO after a reduction in R. Although a servicer's true DPO capacity is unobservable to the borrower (and the econometrician), we assume it is correlated with whether a servicer has recently negotiated a DPO. To construct our DPO variable, we first identify DPOs by combining Trepp's workout and prepayment code fields. For each loan that is in special servicing Trepp provides a workout code. The workout code can change during the duration of special servicing based on the strategy the special servicer is pursuing. For example, a servicer may initially pursue a modification strategy but then switch to a foreclosure strategy after six months. Additionally, Trepp lists prepayment codes for loans that either voluntarily prepay or are liquidated after a default.

We first identify all loans that have a prepayment code that indicates a DPO. We then add to that set any loan without a prepayment code of DPO but for which the last workout code available at the time of liquidation indicates a DPO.[14]

After identifying all the DPOs in our sample, we define our first main independent variable of interest as follows. For loan i serviced by special servicer s in month t, the variable $DPO[w]_{s,t}$ is equal to 1 if servicer s negotiated a DPO on a different loan (any loan besides i) in a window of time w prior to month t, and 0 otherwise. When this indicator is equal to 1, the borrower for loan i expects servicer s to have high DPO capacity given s negotiated a DPO for another loan in the near past.[15] In our analysis, we use various windows of time,

[14]We do not rely strictly on the workout code because we find it to be an inaccurate indicator of DPOs in particular. When we check the loans which Trepp codes as being in DPO against the actual delinquency commentary in Bloomberg, we find a significant number of discrepancies. For example, we find a significant number of loans that Trepp codes as DPO but for which the delinquency commentary indicates another workout strategy such as foreclosure or modification or note sale. Similarly, we find a number of loans that Trepp codes as not being in DPO but for which the delinquency commentary indicates there is a DPO being pursued. Although we have not checked every serviced loan in Trepp, we have found that a number of these discrepancies occur when the loan is being "dual-tracked" in two different workout procedures. In these instances, the delinquency commentary will indicate that a loan is being dual-tracked in, e.g., a foreclosure and a DPO. This indicates that the servicer is considering both options, but it is not clear whether either option is actually favored by the servicer.

[15]We use an indicator for DPO, rather than the proportion of transferred loans that ultimately receive

including $[t-7, t-10]$ months, $[t-6, t-9]$ months, $[t-5, t-8]$ months, and $[t-4, t-7]$ months.

The advantage of the DPO indicator variable is that it only uses information on the servicer's very recent DPO activity. It is reasonable to assume that borrowers are aware of DPOs their servicer has done in the recent few months, but borrowers may be less informed about DPOs their servicer has done several years ago. This means that using a proxy for the servicer's entire history of DPO activity may not accurately capture the information borrowers use in their decisions. However, we explore two alternative measures of κ in Section 5.3 that rely on the servicer's entire history of DPO activity. Our main results are unchanged when using these measures.

We define this variable at a lag since borrowers may not respond immediately to DPOs they observe. It takes time for a given borrower to learn about DPOs their special servicer negotiates with other borrowers, and it also takes time for a borrower to determine whether there is a significant likelihood of receiving a similar favorable workout if they are transferred. Additionally, once a borrower decides to seek a transfer into special servicing, it may take time for the master servicer to actually agree to this. We further define this variable using a window of time to account for the fact that borrowers may base their decision on special servicing outcomes they observe over a period of time, rather than in a single month.

Finally, the model implies that regulations R that make DPOs costly will reduce the number of high types requesting DPOs. In our setting, Revenue Procedure 2009-45 generates a reduction in R. This change went into effect on September 15, 2009.[16] Therefore, we define the pre-regulation time period of September 2003-September 2009 as the time period in which R is high, and the post-regulation period as October 2009-September 2015 as the period in

DPOs, because an individual DPO is easily observable from the borrowers' perspective, whereas it is unlikely that a borrower would be able to precisely determine the proportion of all recently transferred loans that have received DPOs by their special servicer.

[16]The IRS made the change in tax law retroactive to January 1, 2008, in order to avoid jeopardizing the tax treatment of REMICs in which loans were modified prior to default in 2008 and the beginning of 2009. This retroactive application will not impact our results because we focus on the transfer event itself and not the tax treatment of REMICs.

which R is low. Our second independent variable of interest, $Post$, is therefore equal to 0 between September 2003 and September 2009, and equal to 1 between October 2009 and September 2015.

4.2 Methodology

Our empirical methodology estimates the relation among transfers (DPO requests), past special servicer DPOs, and renegotiation costs. We use the regulation change to identify variation in the cost of renegotiation that changes the incentives for strategic behavior on the part of healthy borrowers. Specifically, we use the following specification to test Hypotheses 1-3:

$$transfer_{i,s,t} = \beta_0 + \beta_1 DPO[w]_{s,t} + \beta_2 Post_t \times DPO[w]_{s,t} + \beta_x Cont_{i,s,t} + \epsilon_{i,t} \qquad (6)$$

where $transfer_{i,s,t}$, $DPO[w]_{s,t}$, and $Post_t$ are defined above, and controls include loan origination characteristics (loan-to-value ratio (LTV), coupon, occupancy rate, and debt service coverage ratio (DSCR)) and characteristics at the time of transfer (age, ratio of current unpaid balance to origination balance, LTV, occupancy rate, and DSCR). We include originator, origination quarter, deal type, and property type fixed effects in all specifications. Additionally, we include either special servicer and MSA-by-quarter fixed effects, or special servicer-by-MSA and quarter fixed effects. The servicer and MSA-by-quarter fixed effects are particularly important as they allow us to account for characteristics of the servicer and local economic conditions that are correlated with the propensity to grant DPOs and the likelihood of a transfer. Alternatively, using servicer-by-MSA and quarter fixed effects accounts for MSA-specific strategies that servicers employ.[17]

[17] An important institution in the CMBS structuring process is the B-piece buyer, who purchases the first-loss piece of the deal and directs the special servicer on how to deal with distressed loans. Although we cannot observe the identity of the B-piece buyer in our data, in most cases, the special servicer is directly affiliated with the B-piece buyer. Therefore, the inclusion of special servicer fixed effects implicitly accounts for B-piece buyer-specific financial incentives that might confound our results. Nevertheless, we conduct additional robustness analysis in columns 4 through 8 of Table 12 in which we include deal fixed effects,

We place three restrictions on our estimation sample. In order to alleviate concerns that servicers who do DPOs are unobservably different from those that do not, and that those differences are correlated with our main independent or dependent variables, we restrict our sample to loans serviced by special servicers who negotiated at least one DPO prior to the IRS rule change and who do at least one DPO following the rule change. We also require each special servicer in our sample to service at least two deals.[18] Finally, we require each loan in our estimation sample to have at least 12 months of performance data available.

As an example of the timing in equation (6), take the DPO and transfer visually represented by Figure 2. This loan is transferred in January 2008 such that $transfer_{i,s,t}$ is equal to 1 in January 2008. If we use a $[t-3, t-6]$ DPO window, then the variable $DPO[-3,-6]_{s,t}$ is equal to 1 if special servicer s negotiated a DPO for a different loan between July and October 2007, and 0 otherwise.

Proposition 3 states that only high types will request a DPO when servicer capacity is high and/or the regulation-induced cost of a transfer is low, whereas low types always request a DPO because they are insensitive to changes in κ and R. Equation (6) therefore identifies the effect of increases in perceived servicer capacity and a reduction in renegotiation costs on high-type borrower behavior.

It is possible, however, that an observed increase in transfers is not because of the rule change specifically but rather because of differences in proximity to loan distress for high types that are correlated with servicing behavior in the recent past that we cannot observe. For example, assume two different loans, each with $u \geq M$, that have identical size and for

rather than special servicer fixed effects in our main specifications. Deal fixed effects account directly for the size of the B-piece at origination. If variation in the size of the B-piece, and therefore the B-piece buyers incentives, confounds our results, then this type of fixed effect should account for this. Our analysis indicates that the main results are robust to controlling for B-piece buyer incentives, as our coefficients of interest remain significant after including deal fixed effects.

[18]In defining our dependent variable, any loan that is transferred into special servicing and subsequently receives a DPO is excluded. In other words, our dependent variable is equal to 1 when a loan is transferred but does not ultimately receive a DPO as part of the workout. Loans that are never transferred always have a value of 0. Loans that are transferred and do not subsequently receive a DPO have 0 prior to the transfer, 1 in the month of the transfer, and are missing otherwise. Finally, loans that are transferred and subsequently receive a DPO are always missing. Our results are not sensitive to this restriction, and the primary results without this restriction are available from the authors upon request.

which other observables are the same. Assume the private use value u greatly exceeds M for one loan, whereas for the other loan $u = M$. If the second loan is close to distress in the few months immediately surrounding the rule change, then it might transfer for a mix of strategic and liquidity reasons. In this case, although the loan is technically a high type, it is not clear that we would expect it to respond to the change in R. On the other hand, for the first loan with u strictly greater than M that is far from distress in the few months surrounding the rule change, a response to the change in R would be more clearly strategic.

To address this concern and establish the strategic nature of transfer efforts more clearly, we must characterize high-type borrowers based on the *ex ante* likelihood of distress and therefore the likelihood of being strongly affected by the rule change, and then compare the response of borrowers that are likely to be strongly affected with those that are unlikely to be strongly affected.

Prior to the rule change, delinquent loans and loans that were expected to default in the near future could be transferred to the special servicer without any tax penalty. These loans are the low types in our model ($u < M$); hence, we would not expect these loans to have been affected by the rule change. To identify the high type loans would have been more vs less affected, we rely on the fact that some loans would have met, or been close to meeting, these criteria at some point prior to the rule change. These are loans that experienced a heightened risk of distress or actually became delinquent, and yet were never transferred into special servicing. We consider these loans to have been less affected by the rule change because they previously demonstrated observable risk of distress. On the other hand, there are other loans that exhibited no risk of distress and were never delinquent prior to the rule change. We consider those loans to have been more affected because the lack of previously observed distress risk makes it very unlikely that the borrower would have been eligible for a transfer prior to the rule change. However, following the rule change, these borrowers had wider scope for strategically requesting transfers.

We construct two empirical proxies for how strongly a loan should have been affected by the rule change. First, we classify a loan as more likely to be affected if it never experienced 30+ day delinquency prior to August 1, 2009. Any loan that was 30+ days past due in at least one month once prior to August 1, 2009, is classified as less likely to be affected.

Our second empirical proxy is based on whether the loan was on the servicer watchlist at any point prior to August 1, 2009. According to the CRE Finance Council Investor Reporting Package, the watchlist is "a monthly report prepared by the Master Servicer pursuant to specific guidelines."[19] Loans appear on the watchlist when financial conditions or borrower issues indicate a heightened risk of distress. For example, if debt service coverage ratios drop below a certain threshold or the borrower fails to submit required financial statements, the master servicer can place the loan on watchlist. Although watchlisted loans do not necessarily become delinquent, being on the watchlist is a type of early warning sign for distress. We therefore classify loans as likely to be strongly affected by the rule change if they have never been on the servicer watchlist, whereas loans that were on the watchlist at least once prior to the rule change are classified as unlikely to be strongly affected. In the language of our model, loans that were previously on the watchlist due to a heightened risk of distress are ones for which u is close, or even equal, to M. In contrast, loans that were never on the watchlist are ones for which u is much greater than M.

Once each loan is classified as either likely or unlikely to be strongly affected, we reestimate equation (6) by including a triple interaction term, i.e.,

$$\begin{aligned} transfer_{i,s,t} = \beta_0 + \beta_1 DPO[w]_{s,t} + \beta_2 Post_t \times DPO[w]_{s,t} + \\ \beta_3 Post_t \times DPO[w]_{s,t} \times LikelyAffected_{i,s} + \beta_x Cont_{i,s,t} + \epsilon_{i,t}. \end{aligned} \tag{7}$$

The variable $LikelyAffected_{i,s}$ is an indicator equal to 1 if loan i will be strongly affected by the rule change (because it was not delinquent or, alternatively, never on the watchlist prior to the rule change), and 0 if loan i will be less affected (because it was either delinquent

[19]See Commercial Real Estate Finance Council (2016) for more details.

or on the watchlist prior to rule change). The coefficient β_3 therefore captures the incremental change in behavior for high types most strongly affected by the rule change. As noted in Section 3.5, it is possible that in the data some low types near the margin of being a high type might transfer at higher rates following the rule change. If this is the case, it would bias β_3 toward 0. Thus, β_3 would constitute a lower bound on the true magnitude of the effect.

In addition to identifying *ex ante* exposure to the rule change, we use *ex post* loan performance to establish the strategic nature of transfers. *Ex ante* higher private use value u should be positively correlated with *ex post* better loan performance, so, as a result, Corollary 1 states that, conditional on receiving a transfer, the probability of a borrower fully paying off the loan increases as the regulatory cost decreases. Empirically, therefore, we expect that loans transferred in the post-rule change period should perform differently from other loans.

To study loan performance we construct two additional transfer indicator variables. The first is based directly on Corollary 1 and conditions $transfer$ on the amount the borrower ultimately pays off relative to the remaining principal balance at the time of transfer. We construct the amount paid off using several Trepp fields. First, we use the prepayment and workout strategy codes to identify loans for which there is either a "Full Payoff" or "Full Payoff at Maturity." Second, we add to this set of loans any loan for which the size of the payoff relative to the remaining unpaid principal balance is at least 95%. To do this, we use the Trepp field *curunschedprin* to define the amount of the payoff at the time the loan is resolved, and the field *disposedamount* to define the balance at the time of resolution payoff. We then define a variable *unschedsize* equal to the size of the payoff relative to the disposed amount. Finally, we consider a loan to have received a full payoff when *unschedsize* is greater than or equal to 95%.

After identifying loans that receive full payoffs, whether at or prior to maturity, we define a variable *Full payoff transfer* equal to 1 if the loan is transferred and subsequently fully pays off, and 0 otherwise. Thus, this variable is equal to 0 for loans that transfer and do not fully pay off by December 2020, or loans that are in servicing but not yet resolved by December 2020, or loans that are never transferred at all.

In addition to loan payoff, we create a second proxy for *ex ante* high valuation by focusing on whether loans default prior to, or after, transfer. We define an indicator variable *Transfer (No default)* equal to 1 if a loan is transferred and never defaults, and 0 otherwise. We define default as being more than 60+ days delinquent at least once between the time the loan is originated and December 2020. This variable is therefore equal to 1 when a loan is transferred but is never 60+ days past due at any point before or after the transfer occurs, and 0 when the loan is transferred but is 60+ days past due at some point (or when the loan is never transferred). This variable thus captures borrowers who are never seriously distressed, yet still obtain a transfer into servicing. Thus, we consider a lack of default to be consistent with higher *ex ante* quality. Finally, we reestimate equations 6 and 7 using these two variables as the dependent variables.

5 Results and Discussion

Table A1 defines our variables. Tables 1 and 2 summarize the data for our multivariate estimation at the loan and loan-month level.[20] As shown in Table 1, we have 28,269 loans in our estimation sample, of which 11% are transferred into special servicing (*Transfer*) and 2.9% receive DPOs (*DPO indicator*). Table 1 also describes the origination characteristics of the loans in our sample. The average loan is securitized at an LTV of 67% and a coupon rate of 6%. Additionally, the average loan has an origination DSCR of 1.6 and an origination occupancy rate of 94%.

Table 2 shows that, at the loan-month level, our DPO variables of interest ($DPO[-4,-7]$, $DPO[-5,-8]$, $DPO[-6,-9]$, and $DPO[-7,-10]$) have means between 61% and 65%. This

[20]Note that the composition of Table 2 is slightly different from that of Table 1. This is because we do not include the loans that receive DPOs in our sample of transferred loans for the multivariate analysis. (The DPO variable is used to define the independent variables, but loans that are DPOed following transfer do not appear on the left-hand side.) Because Table 1 summarizes all the loans, including those that receive DPOs, it contains a larger set of loans than is summarized in Table 2.

indicates that, for the average loan, the special servicer negotiates a DPO for a different loan in a recent window of time roughly 60% of the time. Put another way, the average loan sees the special servicer recently negotiate a DPO for a different borrower in seven months of the year, and does not see a recent DPO in five months of the year.

Figure 1 illustrates time trends in both DPOs and transfers during our estimation sample period. The solid series is total transfers during each month in the sample period, and the dashed series is total DPOs each month. The red vertical line is placed at September 2009 when the rule change occurred. The figure shows a significant increase in transfers to the special servicer beginning in late 2008 at the onset of the financial crisis. DPOs are low until 2009, and there is a significant increase in DPOs beginning in 2010 and going through 2011 as loans that went into distress during the peak of the financial crisis conclude their workouts.

5.1 Main results

Table 3 shows results for estimating equation (6) using a probit model.[21] We show the results for four windows: $[t-4, t-7]$, $[t-5, t-8]$, $[t-6, t-9]$, and $[t-7, t-10]$. Columns 1 through 4 include MSA-by-quarter and special servicer fixed effects, whereas columns 5 through 8 include special servicer-by-MSA and quarter fixed effects. All columns include origination loan characteristics (LTV, coupon, occupancy rate, DSCR), current loan characteristics (age, ratio of current unpaid balance to origination balance, LTV, occupancy rate, and DSCR), and originator, origination quarter, property type, and deal type fixed effects.

Across specifications, the coefficients on the interaction term $Post \times DPO$ indicate the impact of DPOs is positive and significant following the IRS rule change. This is consistent with Hypothesis 1 and implies that loans sensitive to servicing capacity are more likely to be transferred following the drop in regulatory cost. The MSA-by-quarter fixed effects in

[21] In all our multivariate analyses, we exclude data from 2009 quarter 3. This ensures that our time fixed effect estimates are not impacted by the fact that the rule changed occurred in the middle of September 2009, and that we can cleanly separate the pre- and post-rule change time periods. Our results are not sensitive to this restriction, however.

column 1 account for unobserved changes in local economic conditions that may be correlated with DPO and transfer activity. Similarly, the MSA-by-servicer fixed effects in column 2 account for unobserved correlation between MSA-specific strategies employed by the special servicer and transfers and DPOs.[22,23]

The negative coefficient on *DPO* indicates that borrowers are less likely to transfer when DPO capacity is perceived to be high prior to the rule change. This is consistent with Hypothesis 3 and the predictions of the two-period extension of the model in Section 3.4, specifically Proposition 4. Proposition 4 states that when regulations are not fixed and are expected to change, loans are less likely to be transferred when κ is perceived to be high. Indeed, it is very likely that CMBS borrowers expected regulations to change because the IRS was relaxing modification rules for *residential* MBS borrowers beginning nearly two years prior to the CMBS rule change. In December 2007 it issued Revenue Procedure 2007-72, and this was followed by Revenue Procedure 2008-28 in June 2008 and Revenue Procedure 2008-47 in July 2008. All three Revenue Procedures granted to RMBS borrowers the same types of tax rule concessions that were eventually granted to CMBS borrowers. This successive relaxation of tax rules for RMBS loans makes it likely that CMBS borrowers anticipated similar relief.

Finally, we estimate equation (6) with individual quarter fixed effects instead of a single *Post* indicator, because quarter fixed effects pick up unobservable characteristics of each quarter and therefore capture differences in transfer likelihood from the pre to the post period. Nevertheless, it is possible to also include the standalone *Post* indicator alongside the quarter fixed effects. To test the robustness of our results to this, in Appendix F we reestimate the specifications that use MSA-by-servicer fixed effects with the inclusion of the

[22]Because *DPO* varies at the servicer-month level, we do not include servicer-by-quarter fixed effects. To account for the possibility that servicers change their propensity to offer DPOs following the rule change, in Appendix B we estimate specifications in which we interact each servicer indicator variable with *Post*. The results, reported in Table B1, show that doing so does not qualitatively impact our main results.

[23]To address concerns about the incidental parameters problem that can arise in nonlinear models with many fixed effects, in Appendix C we show the results of estimating equation (6) using a linear probability model. The results, reported in Table D1, are qualitatively unchanged.

Post indicator. The results, reported in Table F1, are qualitatively unchanged from the main results. Consistent with the likelihood of transfer being higher in the post period, in which the effects of the financial crisis are realized, the *Post* indicator is positive and significant across specifications.

To understand the economic magnitudes of the estimates in Table 3, we report marginal effects in Appendix E. Consider the specification in column 1 of Table E1A in which DPOs are measured in the four-to-seven month window prior to transfer. The marginal effect of $DPO \times Post$ going from 0 to 1 is a roughly 0.2% increase in the probability a loan is transferred. Given that the unconditional likelihood of a transfer in the full sample is also 0.2% (Table 2), this represents an economically meaningful increase. The marginal effects of the interaction terms for columns 2 through 4 are all close to 0.2% as well, and the marginal effects for columns 5 through 8 are all roughly 0.1%, which is still roughly half the full sample transfer likelihood of 0.2%.

5.1.1 Triple differences specification results

Overall, the results in Table 3 are consistent with borrowers responding to increases in perceived servicer DPO capacity by requesting transfers. Because our model predicts that only high types are sensitive to changes in perceived servicing capacity, the results in Table 3 are consistent with high types requesting transfers strategically in hopes that the servicer will negotiate a DPO.

One potential concern with this interpretation is that, although the model predicts that only high types respond to changes in perceived capacity, the positive coefficient on $Post \times DPO$ may be the result of unobserved factors that are positively correlated with both DPO and transfers for low types. In other words, low type transfers that happen to occur when servicer capacity is high (when $DPO = 1$) due to factors that are omitted from equation (6) (despite the fact that this equation is saturated with time-varying controls and fixed effects) may be driving the positive coefficient on $Post \times DPO$.

To further establish that the results in Table 3 capture the strategic nature of the increase in transfers for high types, we focus on the triple difference design presented in equation (7). In this design, the sample of loans is split into those that will be strongly affected by the rule change ($LikelyAffected = 1$ in equation (7)), which we term the treatment group, and those that will not be strongly affected by the rule change ($LikelyAffected = 0$ in equation (7)), which we term the control group. Connecting this design to the model and the discussion in Section 4.2, we consider the high-type loans that should have been more affected to be the treatment group, whereas the high type loans that should have been less affected are the control group. Using this classification, equation (7) compares transfer likelihood for treatment and control groups before and after the rule change, conditional on a DPO by the special servicer.

We first show that loans in the treatment group can plausibly be considered loans that would only request a transfer for strategic reasons. To do this, we analyze four observable loan characteristics that should be correlated with quality: DSCR, LTV, occupancy rate, and interest rate. If our classification of treatment vs. control loans accurately maps to different levels of strategic incentives for high types, then treatment loans should be less risky along these observable dimensions than control loans. Table 4 presents the results of difference-in-means tests for these observables. In Panel A, the treatment group is loans that were never on the servicer watchlist prior to August 2009. In Panel B, the treatment group is loans that were never 30+ days delinquent prior to August 2009. Columns 2 and 3 present estimation sample means by group, and column 4 presents the difference with associated significance levels (specifically, the control mean minus the treatment mean.) The first four rows of each panel summarize loan observables at origination, whereas rows 6-8 summarize observables during August 2009, which is the last full month prior to the rule change. Because the vast majority of CMBS loans are fixed rate, we do not summarize the interest rate during August 2009 separately from the interest rate at origination. The table shows that control group loans have significantly lower DSCR, higher LTV, and lower occupancy rate in August

2009, and that they are similarly riskier than treatment loans at origination, consistent with these loans being observably lower quality. Differences in quality are evident both for the watchlist-based treatment/control grouping in Panel A and the delinquency-based grouping in Panel B.

Next, we inspect the trends in our dependent variable, *transfer*, for treatment and control groups. Specifically, for each month in our sample period, we plot the fraction of all outstanding loans that are transferred for both treatment and control groups. Figure 3 shows the results when treatment loans are defined as those that were never on the watchlist prior to August 1, 2009. Figure 4 shows the same plot, except where treatment loans are defined as those that were never delinquent prior to August 1, 2009. The figures show trends that are consistent with the differences shown in Table 4. Focusing on Figure 3, the plot shows that prior to the rule change as the financial crisis was beginning, both treatment and control groups displayed an upward trend in the frequency of transfers each month. However, the transfer frequency of control loans increases at a faster rate pre-rule change, which is consistent with control group loans being observably riskier and therefore more sensitive to adverse fundamental cash flow shocks.

Focusing on the post-rule change period, there is a sharp decline in transfer frequency for control group loans. This downward trend should not be interpreted as control loans being affected by the rule change. On the contrary, the downward trend is a mechanical result of the large number of control group loans that were transferred prior to the rule change. Once a loan is transferred, it drops from the sample. Because the number of loans in control and treatment groups is fixed, as control group loans are transferred prior to the rule change and immediately after it, the total number remaining shrinks. As time in the post period elapses, there are significantly fewer loans in the control group that have not been transferred. Therefore, transfers for control group loans display a mechanical downward trend.

In contrast, Figure 3 shows that the transfer frequency for treatment group loans jumps markedly after the rule change, and it remains significantly higher in the post period compared to the pre period, while displaying no significant downward trend. Figure 4 shows trends that are qualitatively similar, although they are more difficult to observe visually due to the fact that there are so few loans in the control group.

The implications of the difference in trend for control and treatment groups in the post period are twofold. First, the difference supports that control group loans are observably riskier than treatment group loans, as summarized in Table 4, and that our treatment and control group categorization accurately captures a distinction between high and low levels of strategic incentives. Second, the difference suggests that the rule change results in a sustained increase in transfer frequency for treatment group loans, consistent with response to strategic motives, rather than simply negative cash flow shocks.

Having shown that our two treatment and control categorizations are consistent with high-type (treatment) loans being observably different than low-type (control) loans, we estimate equation (7) with our main dependent variable, *transfer*. Tables 5A and 5B show the results. In both Tables, the triple interaction terms are positive and significant across specifications. This indicates that treatment group loans, which should have greater incentive to behave strategically given they are further from distress, respond incrementally more to the rule change than control group loans. To interpret economic effects, take column 1 of Table E1B in Appendix E in which the servicer watchlist categorization is used to define treatment and control groups. The marginal effect of $DPO \times Post \times NeverWatchlist$ going from 0 to 1 is 0.25%, whereas the marginal effect of $DPO \times Post$ is 0.13%. This indicates that, for this particular DPO window, treatment group loans are nearly twice as likely to transfer conditional on a DPO by their servicer in the Post period compared to control group loans. This marginal effect for treatment loans relative to control loans is slightly larger than the average effect of $DPO \times Post$ of 0.2% reported in column 1 of Table E1A, which corresponds to the same time window.

To illustrate the results in a higher frequency event study setup, we estimate a dynamic triple differences specification and plot the coefficients for the dynamic triple interaction terms. We estimate

$$transfer_{i,s,t} = \beta_0 + \beta_1 DPO[w]_{s,t} + \sum_{q=2007q3}^{2011q3} \gamma 1_q Quarter_q + \sum_{q=2007q3}^{2011q3} \gamma 2_q (Quarter_q \times DPO[w]_{s,t}) + \sum_{q=2007q3}^{2011q3} \gamma 3_q (Quarter_q \times DPO[w]_{s,t} \times LikelyAffected_{i,s}) + \beta_x Cont_{i,s,t} + \epsilon_{i,t}. \quad (8)$$

The $\gamma 3_q$ coefficients capture the difference in transfer probability for treatment loans compared to control loans, conditional on a DPO in time window $[w]$, in each quarter of the sample period. We interact $DPO[w]_{s,t} \times LikelyAffected_{i,s}$ with quarter indicators in order to remain consistent with the fact that we use quarter-level time fixed effects in Tables 5A and 5B. We exclude 2009Q3 (the quarter in which the rule change occurred) which allows us to interpret the $\gamma 3_q$ coefficients relative to the time period of the rule change. A $\gamma 3_q$ larger than 0 indicates that treatment loans are more likely to transfer following a recent DPO by their special servicer than control loans in that particular quarter, relative to the quarter in which the rule change occurs.

We estimate equation (8) using a setup analogous to columns 1 through 4 of Table 5A. That is, we define treatment and control using the watchlist categorization (this is our preferred categorization because it generates a relatively balanced number of treatment and control loans), and we use MSA-by-quarter fixed effects. In Figure 5 we plot the quarterly $\gamma 3_q$ coefficients for each respective DPO time window in an event study setup. Across all four time windows, the coefficients from 2007Q3 to 2009Q2 are nearly always at or below 0, consistent with treatment group loans not transferring at higher rates than control loans, conditional on a recent DPO, prior to the rule change. In contrast, the coefficients from 2009Q4 to 2011Q3 are consistently above 0, which is consistent with treatment group loans transferring at higher rates post-rule change. (Note that some of the triple interaction coefficients drop out in the [-7,-10] window). To check the robustness of our results to including the quarter of the rule change, in Figure 6 we produce the same plots for our preferred treatment and control categorization, except we include 2009Q3 (the quarter that contains the rule change). The excluded category is 2009Q2, such that the coefficients can be interpreted relative to the quarter immediately preceding the rule change. The results are qualitatively unchanged. Overall, the event study results in Figures 5 and 6 are consistent with the results in Table 5A.

5.1.2 Loan performance after transfer

As a second approach to establishing the strategic nature transfers we document in Table 3, we turn to the results on loan performance subsequent to transfer. Because Corollary 1 predicts that full payoffs increase following a reduction in the cost of being transferred, we expect loans that transfer following a DPO in the post-regulation period to perform differently from loans transferred following a DPO prior to the regulation. To analyze such differences in performance, we estimate whether DPOs are associated with the variables *Full payoff transfer* and *Transfer (No default)*. Tables 6 and 7 report the results. In Table 6, the $Post \times DPO$ interactions are positive and significant across specifications, suggesting that borrowers are more likely to fully pay off following a transfer in the post-regulation change period, which is consistent with Hypothesis 2. In terms of delinquency, in Table 7 the interaction terms are again positive and significant. This suggests that loans transferred following a DPO in the post-period are more likely to never experience 60+ day delinquency status (whether before or after transfer), compared to loans transferred following a DPO in the pre-period.

These results are consistent with high types imitating low types in order to obtain a transfer into special servicing. This is because high-type borrowers are those willing to pay the full loan amount *ex ante*. Therefore, they should be less likely to experience serious delinquency and more likely to fully pay off the loan balance *ex post*. In particular, if they fail to successfully negotiate DPOs, high-type borrowers optimally decide to fully pay off their loans, while low-type borrowers lose their properties to foreclosure. The results taken together show that borrowers are able to behave strategically to the detriment of lenders and do so more as renegotiation costs are reduced.

5.2 Impact of the rule change on DPO profitability

In this section, we conduct back-of-the-envelope calculations to evaluate how the IRS rule change affected the profitability of DPOs for lenders taking into account the strategic behavior of borrowers.

The lender's expected profit V associated with granting a DPO can be represented as

$$V(p^L, p^H) = (D^* - F)p^L - (M - D^*)p^H, \tag{9}$$

where p^L is the probability that the DPO is granted to a low-value borrower and $(D^* - F)$ is the corresponding gain to the lender, represented by the difference between the DPO and foreclosure payoffs. Similarly, p^H is the probability that the DPO is granted to a high-value borrower, and $(M - D^*)$ is the corresponding loss to the lender, represented by the difference between the mortgage balance and the DPO.

During the 24 months prior to and 24 months after the rule change, the average foreclosure payoff was 41.0% of the remaining loan balance, while the average DPO was 49.3%. Thus, we estimate that the average DPO gain $(D^* - F)$ associated with granting a DPO to a low-value borrower was 8.3% of the remaining loan balance, and the average loss $(M - D^*)$ associated with granting a DPO to a high-value borrower was 50.7% of the remaining loan balance. Thus, in the absence of strategic behavior, lenders recover 8.3% more of the loan balance with a DPO than with a foreclosure, which translates into 20% $(\frac{0.083}{0.41})$ higher overall dollar recovery.

Since we do not observe borrower types, and thus p^L and p^H in equation (9) directly, we must rely on indirect evidence of strategic behavior. We use an increase in the number of transfers that result in full payoffs (based on the variable *Full payoff transfer*) as a proxy for the effect of the rule change on strategic transfers. Indeed, according to our definition, a high-value borrower is willing to pay the full loan amount in order to retain their property. Thus, full payoff transfers are consistent with strategic behavior of high-value borrowers. We identify the relative difference between p^L and p^H before and after the rule change, rather than identifying the level of each. Without loss of generality, we thus normalize $p^H_{pre} = 0$ and $p^L_{pre} = 1$.

Our approach is based on the observation that after the rule change, there was a significant increase in full payoff transfers of loans that were not on the servicer watchlist, while full payoff transfers of loans that were on the watchlist were relatively stable. In particular, there were 317 and 332 full payoff transfers of watchlisted loans in the 12 months before and after the rule change, respectively. (We use this short period before and after the rule change to best capture change in strategic incentives due to the rule change.) In contrast, the number of full payoff transfers of loans that were not on the watch list almost doubled from 131 to 237 across the same time span. For our back of the envelope calculations, we assume that the disproportionate increase in the number of full payoff transfers for loans not on the watchlist is due to strategic behavior of high-value borrowers. These assumptions imply that the percentage of strategic transfers went up by 5.4% after the rule change.[24] Given our normalization of $p^H_{pre} = 0$, the change in expected profit associated with granting a DPO is -3.2% because $V(1, 0) = 8.3\%$ and $V(0.946, 0.054) = 5.1\%$.

Under the same set of assumptions, we repeat our calculations for the 24-month periods before and after the rule change. For this time period, we estimate that the percentage of strategic transfers went up by 7.1% after the rule change. As a result, the profitability of a DPO decreased from 8.3% before the rule change to 4.1% after the rule change when we consider this expanded period.

Overall, our back of the envelope calculations indicate that strategic transfers negatively

[24] We estimate the disproportionate increase in the number of full payoff transfers of loans not on the watchlist to be 100 (derived as $237 - 131 * (\frac{332}{317})$), which is equal to 5.4% of all loans that were transferred to the special servicer and not granted DPOs in the 12 months after the rule change.

affected lenders' expected payoffs from DPOs after the rule change. Because they avoid deadweight costs associated with foreclosures and changes of ownership, DPOs are more efficient overall than foreclosures. However, mortgage markets may not capture the full advantage of DPO efficiency due to strategic behavior of high-value borrowers who are able to exploit asymmetric information between borrowers and lenders.

5.3 Alternative measures of perceived DPO capacity

Our measure of perceived DPO capacity, DPO, captures recent information on servicer DPO activity (up to 10 months prior) that borrowers will be aware of. In this section, we construct two alternative empirical proxies for κ that are based on the servicer's entire history of DPO activities and show that our main results are robust to using these.

The first alternative measure is $CumulDPO_{s,t}$, the cumulative number of DPOs done by special servicer s from the beginning of the sample period (September 2007) up to and including month $t-1$. Like the main DPO measure, this measure assumes borrowers can observe when the special servicer does a DPO. In contrast to our main measure, though, this alternative assumes that borrowers are more likely to perceive high special servicer capacity when the servicer has done more DPOs in the past. On the other hand, servicers that have done fewer cumulative DPOs are less likely to be perceived as having high capacity. Cumulative DPOs increase at different rates for different special servicers such that, like our main measure of perceived DPO capacity, this variable captures time variation in perceived DPO capacity across different servicers.

The second measure is $DPOIntensity_{s,t}$, the historical cumulative average DPO intensity as of month $t-1$. DPO intensity in a given month is defined as the number of DPOs divided by the number of loans in special servicing (servicing volume). We then calculate a cumulative moving average of this variable starting at the beginning of the sample period (September 2007). Scaling DPOs by the number of loans in special servicing allows the impact of one additional DPO to vary by special servicer. A servicer with 100 loans currently in servicing that does a DPO has a current DPO intensity of 1%, whereas a servicer with 25 loans in servicing that does a DPO has an intensity of 4%. Therefore, despite both servicers doing a single DPO in a month, the latter has a DPO intensity four times higher than the former. This measure assumes that borrowers are more likely to perceive high special servicer capacity when the servicer has a higher historical average DPO intensity.[25]

We reestimate equations (6) and (7) using $CumulDPO$ or $DPOIntensity$ as the main independent variable and report the results in Tables 8A and 9A. Columns 1 and 4 of each table show the results of equation (6) with both fixed effects structures, and columns 2, 3, 5, and 6 show the results of equation (7) with both fixed effects structures. Overall, the results are qualitatively unchanged relative to Tables 3, 5A, and 5B. The interaction terms in columns 1 and 4 are positive and significant, indicating that transfer probability is higher in the post rule change period when cumulative DPOs or historical DPO intensity is higher, whereas the standalone coefficients on the DPO variables are negative, consistent with strategic delay (see Hypothesis 3 and the two-period extension of the model in Section 3.4). Moving to columns 2, 3, 5, and 6, the triple interaction terms are positive and significant, consistent with the results in Tables 5A and 5B that show that treatment group loans respond more strongly than control group loans to changes in perceived DPO capacity in the post rule change period.

In Tables 8B and 9B, we use *Full payoff transfer* and *Transfer (No default)* as the dependent variables and reproduce the results of Tables 6 and 7 using $CumulDPO$ and $DPOIntensity$ as the independent variables of interest. The results indicate the both alternative measures of perceived DPO capacity are positively associated with likelihood of full payoff and no delinquency transfers in the post period.

[25] The assumption that borrowers can observe both historical DPOs and historical servicing volume are plausible, given borrowers have access to the same type of data that is available to econometricians from the CRE Finance Council Investor Reporting Package. Moreover, borrowers are also likely to have access to industry news and soft information that provides even timelier information on servicer activity.

5.4 The relationship between the master and special servicer

As described in Section 2, the master servicer has little incentive not to transfer loans into special servicing conditional on the borrower requesting a transfer. In the case of seriously delinquent or distressed loans (low types), the master servicer immediately removes the responsibility for advancing principal and interest payments to bondholders by transferring the loan. In the case of high types who are imitating low types, the master servicer removes any uncertainty about having to advance principal and interest in the future by transferring the loan. Additionally, the master servicer would be unable to condition its decision of whether to transfer the loan on its ability to change the special servicer once the borrower requests the transfer. This is because, once the deal has been originated, the special servicer is fixed and can only be changed by the B-piece buyer.

Despite a strong incentive to transfer any borrower that requests it, and despite the inability to select the special servicer after a loan requests a transfer, there may be the potential for characteristics of the master servicer to confound the interpretation of our results. For example, master servicers may differ in how quickly they approve transfers, and different master servicers may have different existing relationships with a given special servicer. We take three distinct approaches to addressing these concerns. Most importantly, we reestimate all our main regressions with the inclusion of master servicer fixed effects, which accounts for unobserved characteristics of master servicers that may be correlated with the probability of transfer and the main independent variables. Tables 10A, 10B, and 10C replicate Tables 3, 5A, and 5B with the inclusion of master servicer fixed effects. Similarly, Tables 11A and 11B replicate Tables 6 and 7 with master servicer fixed effects. The results are qualitatively unchanged when master servicer fixed effects are included.

Our second and third approaches entail using two alternative specifications that capture different aspects of the relationship between the master and special servicer. First, we include master-special servicer pair fixed effects, which account for the possibility that master servicers may have different transfer approval likelihood or approval speed depending on how they expected different special servicers to act. For example, a master servicer may be quicker to approve a transfer to a special servicer that is more efficient at loan workouts. This could be the case if the loan workout impacts the master servicer's reputation in some way, for example if an inefficient and lengthy workout harms the master servicer's reputation. Alternatively, we include individual deal fixed effects, which account for the possibility that the relationship between the master and special servicer depends on the particular deal. Deal fixed effects also partially account for incentives of the B-piece buyer as described in Footnote 17. Table 12 reports the results of these alternative approaches. We only reestimate our baseline equation (6) using MSA-by-quarter fixed effects for the sake of brevity. Columns 1 through 4 (5 through 8) include master-special servicer pair (deal) fixed effects. Across specifications, the main coefficient of interest, $DPO \times Post$, remains positive and significant, indicating the results are robust to controlling for characteristics of the master-special servicer relationship that may be correlated with both transfers and special servicers' DPO decisions.

5.5 Loans close to maturity

In this section, we alleviate potential concerns that our results are driven by the near-to-maturity loans that the rule change was designed to address. In our sample, roughly 96% of loans contain balloon provisions. We first identify the number of months remaining until the loan matures.[26] We then reestimate our main regressions in two ways.

First, we include a control for the remaining time to maturity, *remterm*. Tables 13A through 14B show the results of our main regressions including this control. Across specifications, *remterm* is negative and significant, consistent with loans that are far from maturity being less likely to transfer due to concerns about imminent balloon defaults. Importantly,

[26] We use the Trepp variable *Remaining term*, which is equal to the current number of remaining months to either the maturity date or the anticipated repayment date (ARD). For loans with a maturity date, this variable measures the number of months remaining until the balloon payment is due. For loans with an ARD, the borrower does not technically default if the remaining balance is not paid on the ARD. However, there is an automatic increase in interest rate and the loan is hyperamortized once the ARD is reached, giving borrowers with this type of loan a strong incentive to pay the remaining balance at the ARD.

though, $DPO \times Post$ remains positive and significant across specifications in Tables 13A, 14A, and 14B, consistent with our main results. Similarly, the triple interaction terms in Tables 13B and 13C remain positive and significant.

As an alternative to directly controlling for the remaining time to maturity, we exclude from the estimation any loan with a remaining time to maturity less than or equal to 12 months at the time of transfer. We report these results for our main specification (equation 6) and the triple differences specification (equation 7) in Appendix D. Our results are qualitatively unchanged after removing close-to-maturity loans.

6 Conclusion

We provide evidence of significant asymmetric information between borrowers and lenders in commercial real estate. Consistent with our model in which lenders cannot perfectly observe borrowers' use values and renegotiation is costly, we show that, following a 2009 IRS rule change that exogenously reduced the cost of renegotiation, loans are more likely to be transferred into special servicing, particularly when perceived servicer capacity is high. Loans with *ex ante* stronger strategic incentives respond more to the rule change, and loans that are transferred after the rule change are more likely to fully pay off.

Our results are important as they are the first to detail the impact of principal writedowns on commercial borrower behavior. Our findings are particularly salient in light of the REMIC safe harbor provisions granted in April 2020 in response to COVID-19-induced commercial real estate distress. These provisions are designed to increase the ability of borrowers and servicers to renegotiate prior to default. Our results suggest that policies that allow for such preemptive renegotiation may also encourage borrowers who otherwise would perform to attempt to extract concessions from servicers.[27]

[27]Anecdotal examples of strategic behavior in CRE loans occurred during the COVID-19 pandemic. For example, retailer The Gap was involved in litigation with some landlords and lenders over failure to pay rent for its stores. The Gap contends that state government-mandated shutdowns void lease agreements, whereas landlords and property owners such as Si-

mon Property Group contend that Gap has the ability to pay and is using the pandemic to cease rent payments or terminate lease agreements. See https://www.bloomberg.com/news/articles/2020-08-05/simon-countersues-gap-over-107-million-in-lease-payments and https://www.nytimes.com/2020/06/05/business/economy/coronavirus-commercial-real-estate.html.

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Figures

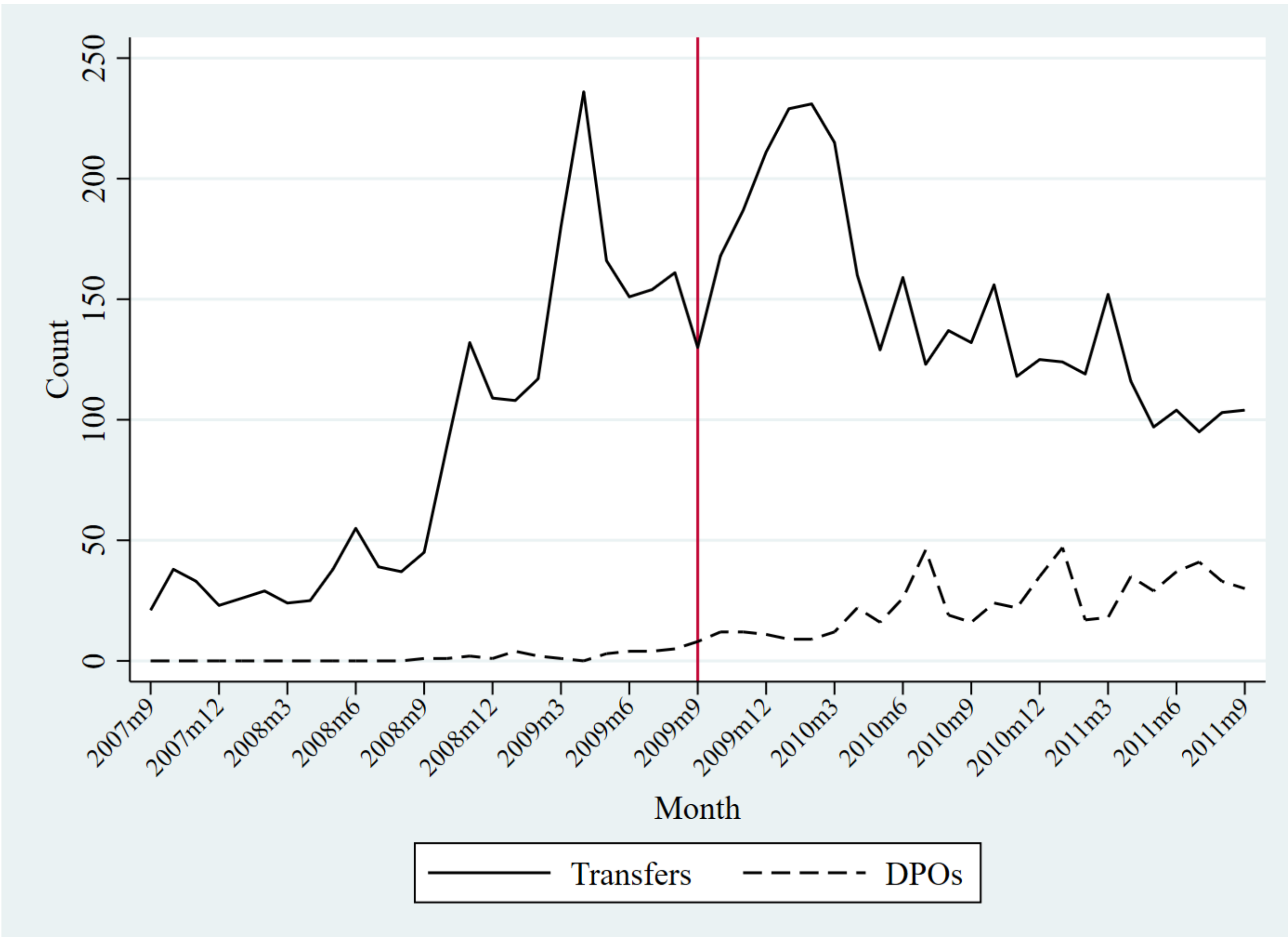


Figure 1: Transfers and DPOs over time

Notes: This figure plots the number of transfers and DPOs over the sample period. Data is from Trepp for nonagency CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. The vertical line is September 2009, the month of the IRS rule change.

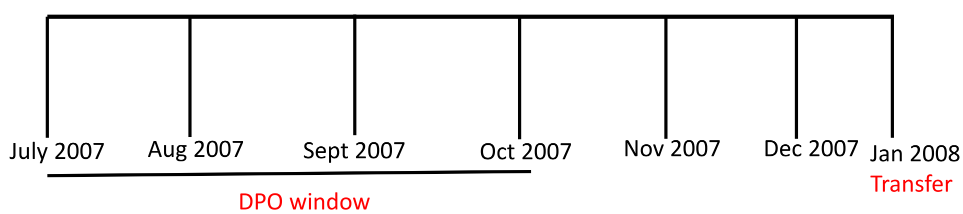


Figure 2: DPO-transfer example

Notes: This figure illustrates the timing in our empirical specification when we use a DPO window of three months to six months prior to transfer.

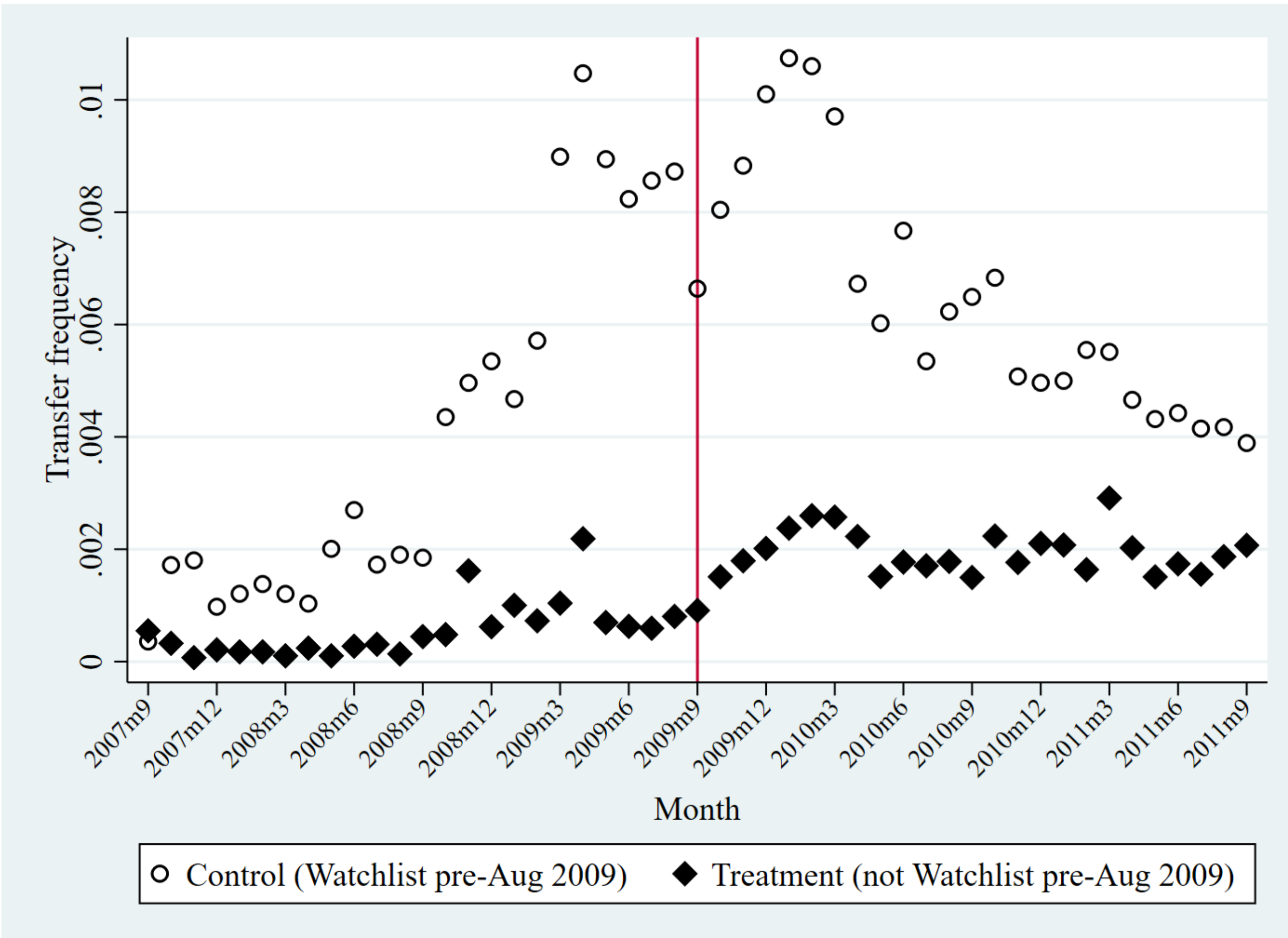


Figure 3: Trends in transfer frequency: Loans on watchlist vs. never on watchlist

Notes: 1) This figure plots the monthly frequency of transfers (number of transfers divided by number of loans outstanding) for treatment (never on watchlist prior to August 1, 2009) and control (on watchlist at least once prior to August 1, 2009) groups. 2) The vertical line is the date of the rule change. 3) Data is from Trepp for nonagency CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011.

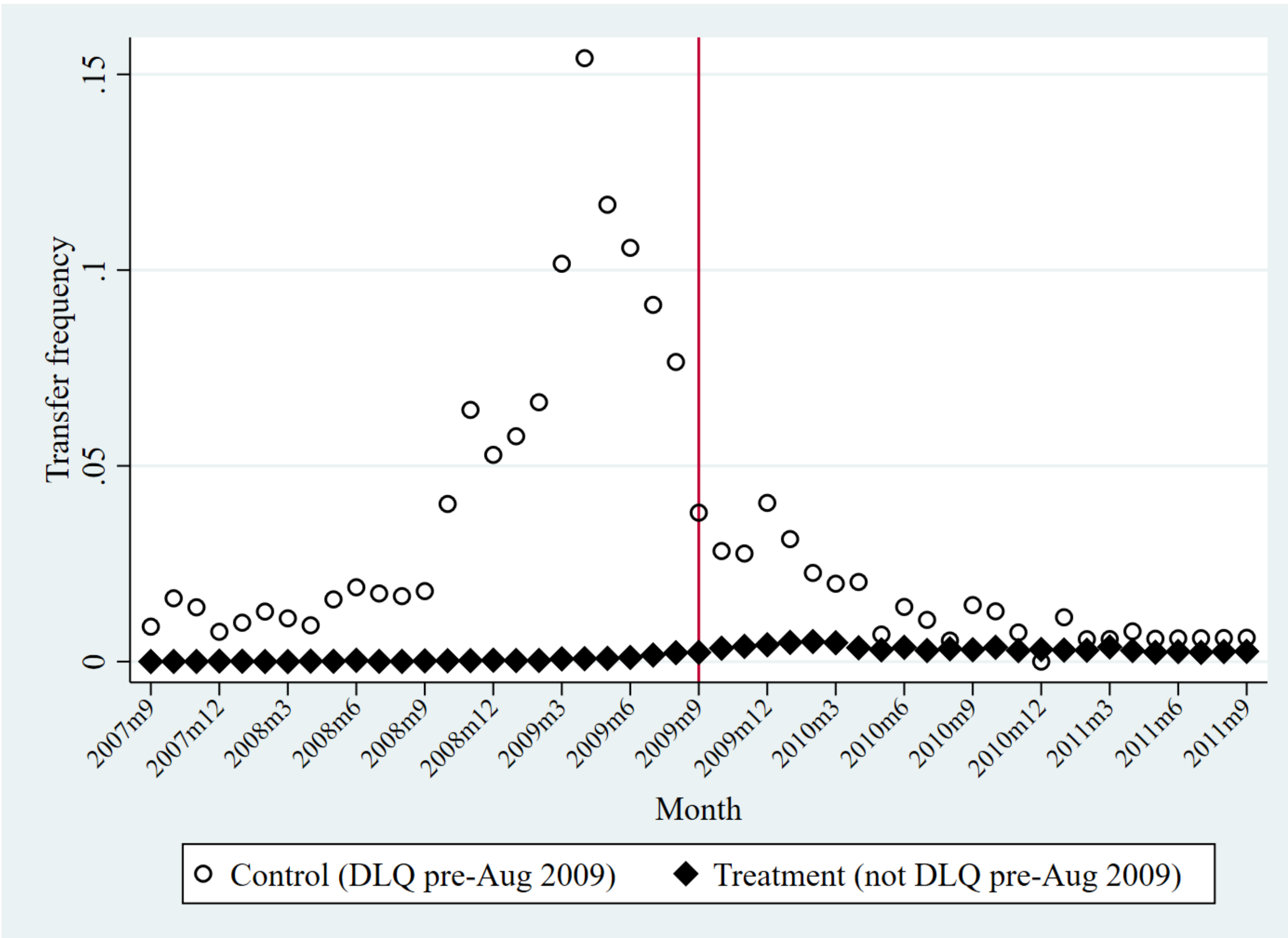
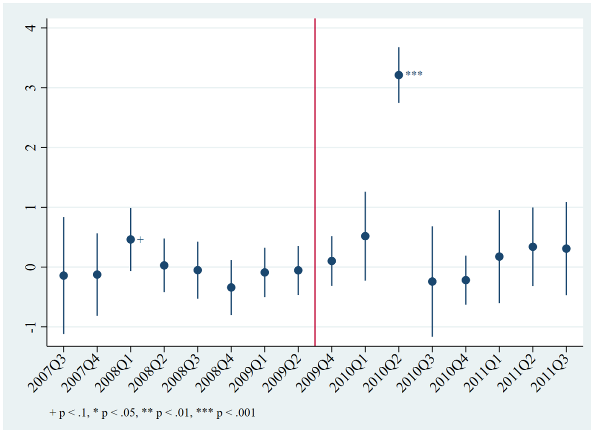
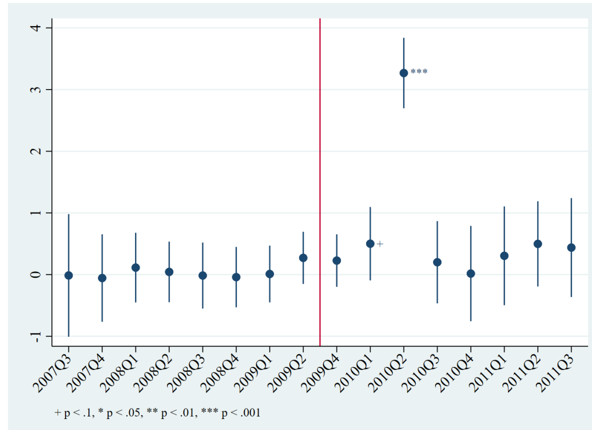


Figure 4: Trends in transfer frequency: Delinquent loans vs. never delinquent loans

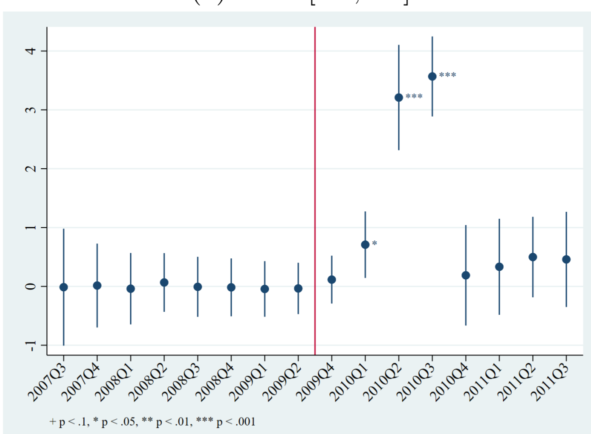
Notes: 1) This figure plots the monthly frequency of transfers (number of transfers divided by number of loans outstanding) for treatment (never 30+ days delinquent prior to August 1, 2009) and control (30+ days delinquent at least once prior to August 1, 2009) groups. 2) The vertical line is the date of the rule change. 3) Data is from Trepp for nonagency CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011.



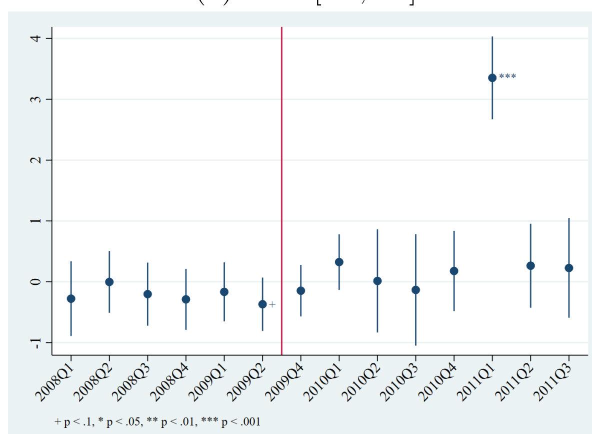
(a) $DPO[-4,-7]$



(b) $DPO[-5,-8]$



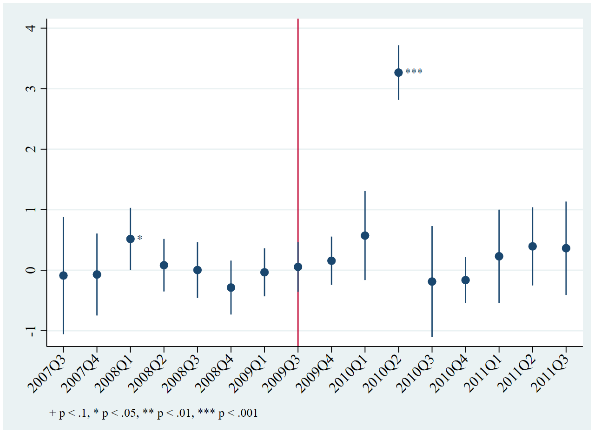
(c) $DPO[-6,-9]$



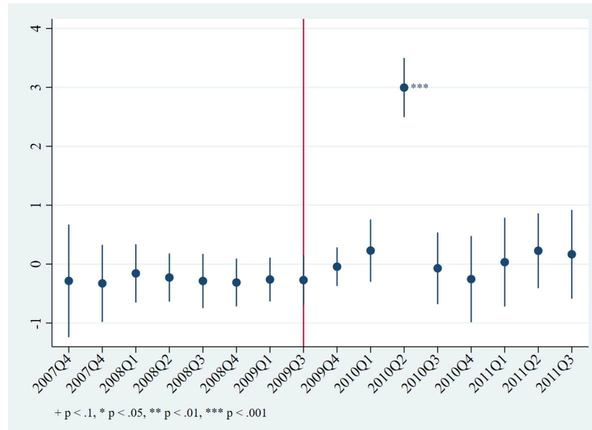
(d) $DPO[-7,-10]$

Figure 5: Dynamic triple differences coefficients: Watchlist (control) vs. no watchlist (treatment)

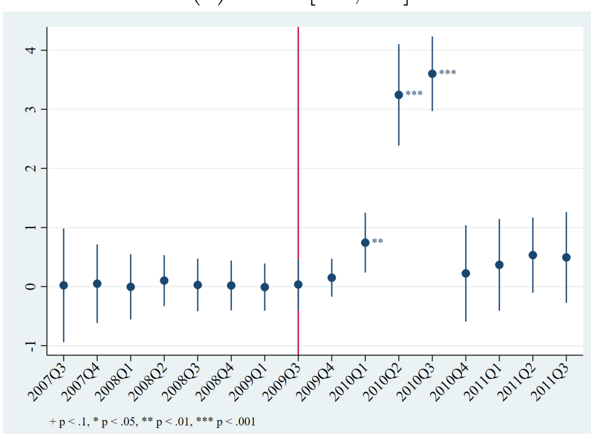
Notes: 1) This figure plots the quarterly triple interaction coefficients from estimating equation (8) using the set of controls and fixed effects included in Table 5A columns 1 through 4. The vertical line is the quarter of the rule change. The excluded interaction term is 2009 quarter 3, which contains the date of the IRS rule change, such that all coefficients should be interpreted relative to 2009 quarter 3. The control group is loans that appeared on the servicer watchlist at least once prior to August 1, 2009, and the treatment group is loans that never appeared on the servicer watchlist prior to August 1, 2009. 2) Data is from Trepp for nonagency CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. 3) $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$



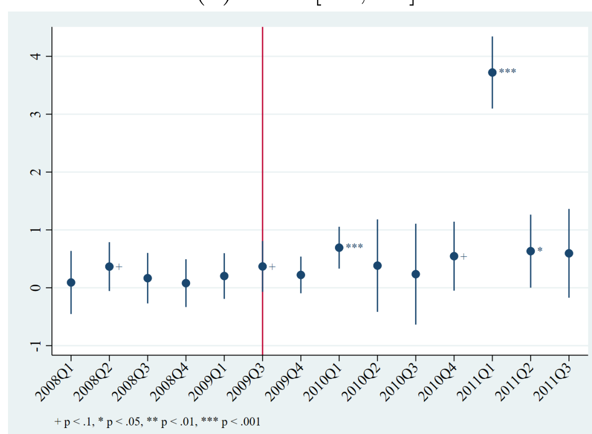
(a) $DPO[-4, -7]$



(b) $DPO[-5, -8]$



(c) $DPO[-6, -9]$



(d) $DPO[-7, -10]$

Figure 6: Dynamic triple differences coefficients: Watchlist (control) vs. no watchlist (treatment), including 2009Q3

Notes: 1) This figure plots the quarterly triple interaction coefficients from estimating equation (8) using the set of controls and fixed effects included in Table 5A columns 1 through 4. The vertical line is the quarter of the rule change. The excluded interaction term is 2009 quarter 2. The control group is loans that appeared on the servicer watchlist at least once prior to August 1, 2009, and the treatment group is loans that never appeared on the servicer watchlist prior to August 1, 2009. 2) Data is from Trepp for nonagency CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. 3) $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$

Tables

Table 1: Summary statistics: Loan level.

variable	**N**	**mean**	**p50**	**sd**	**min**	**max**
Origination year	28,269	2004.8	2005.0	1.6	2002.0	2007.0
Orig LTV	28,269	67.1	70.9	12.9	8.7	80.3
Orig coupon	28,269	5.98	5.91	0.56	4.84	7.54
Orig occ	28,269	94.4	98.0	8.2	62.9	100.0
Orig DSCR	28,269	1.63	1.48	0.73	1.07	9.31
Transfer	28,269	0.113	0.000	0.317	0.000	1.000
Full payoff transfer	28,269	0.029	0.000	0.167	0.000	1.000
Transfer (No delinquency)	28,269	0.016	0.000	0.124	0.000	1.000
Never on watchlist	28,269	0.608	1.000	0.488	0.000	1.000
Never delinquent	28,269	0.946	1.000	0.227	0.000	1.000
DPO indicator	28,269	0.029	0.000	0.167	0.000	1.000

Notes: 1) Summary statistics at the loan level for the multivariate estimation sample. 2009Q3 is excluded. Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. Full payoffs and delinquencies are measured as of December 2020. 2) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail.

Table 2: Summary statistics: Loan-month level (estimation sample)

variable	**N**	**mean**	**p50**	**sd**	**min**	**max**
Orig LTV	1,264,430	66.694	70.580	13.164	8.700	80.260
Orig coupon	1,264,430	5.977	5.907	0.555	4.837	7.540
Orig occ	1,264,430	94.512	98.200	8.164	62.900	100.000
Orig DSCR	1,264,430	1.638	1.480	0.756	1.070	9.310
Age	1,264,430	51.686	51.000	23.341	7.000	103.000
Balratio	1,264,430	0.935	0.945	0.055	0.690	1.000
Curr LTV	1,264,430	66.603	70.520	13.599	7.970	160.000
Curr occ	1,264,430	92.144	97.000	11.123	48.900	100.000
Curr DSCR	1,264,430	1.601	1.490	0.577	0.450	4.561
Transfer	1,264,430	0.002	0.000	0.045	0.000	1.000
Full payoff transfer	1,264,430	0.001	0.000	0.025	0.000	1.000
Transfer (No delinquency)	1,264,430	0.000	0.000	0.018	0.000	1.000
Never on watchlist	1,264,430	0.634	1.000	0.482	0.000	1.000
Never delinquent	1,264,430	0.974	1.000	0.161	0.000	1.000
Post	1,264,430	0.470	0.000	0.499	0.000	1.000
DPO[-4,-7]	1,264,430	0.636	1.000	0.481	0.000	1.000
DPO[-5,-8]	1,264,430	0.648	1.000	0.478	0.000	1.000
DPO[-6,-9]	1,264,430	0.625	1.000	0.484	0.000	1.000
DPO[-7,-10]	1,264,430	0.610	1.000	0.488	0.000	1.000
Remaining term	1,264,430	91.617	72.000	78.515	1.000	471.000
Cumulative DPO	1,264,430	32.323	13	48.582	0	233
DPO Intensity	1,264,430	0.016	0.017	0.01	0	0.035

Notes: 1) Summary statistics at the loan-month level for the multivariate estimation sample. 2009Q3 is excluded. Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. Full payoffs and delinquencies are measured as of December 2020. 2) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail.

Table 3: Impact of DPOs on transfer likelihood

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.23***	0.22***	0.22***	0.12**	0.17***	0.18***	0.19***	0.19***
	(0.056)	(0.050)	(0.048)	(0.049)	(0.055)	(0.050)	(0.047)	(0.045)
DPO	-0.085***	-0.095***	-0.11***	-0.12***	-0.078***	-0.091***	-0.10***	-0.15***
	(0.028)	(0.027)	(0.028)	(0.030)	(0.027)	(0.027)	(0.027)	(0.027)
Orig LTV	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***
	(0.0034)	(0.0034)	(0.0034)	(0.0033)	(0.0039)	(0.0039)	(0.0039)	(0.0039)
Orig coupon	0.041	0.042	0.042	0.039	0.034	0.034	0.034	0.034
	(0.026)	(0.026)	(0.026)	(0.026)	(0.027)	(0.027)	(0.027)	(0.027)
Orig occ	0.0048***	0.0048***	0.0048***	0.0048***	0.0046***	0.0046***	0.0046***	0.0046***
	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
Orig DSCR	0.13***	0.13***	0.13***	0.12***	0.14***	0.14***	0.14***	0.14***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Age	-0.012**	-0.012**	-0.013**	-0.0098*	-0.0098	-0.010*	-0.011*	-0.010*
	(0.0059)	(0.0059)	(0.0059)	(0.0060)	(0.0060)	(0.0060)	(0.0060)	(0.0061)
Balratio	0.94***	0.94***	0.95***	1.02***	0.87**	0.87**	0.88**	0.88**
	(0.35)	(0.35)	(0.35)	(0.35)	(0.36)	(0.36)	(0.36)	(0.36)
Curr LTV	0.00056	0.00057	0.00051	0.00027	0.00069	0.00071	0.00067	0.00068
	(0.0031)	(0.0031)	(0.0031)	(0.0031)	(0.0037)	(0.0037)	(0.0037)	(0.0037)
Curr occ	-0.018***	-0.018***	-0.018***	-0.018***	-0.017***	-0.017***	-0.017***	-0.017***
	(0.00065)	(0.00065)	(0.00065)	(0.00065)	(0.00065)	(0.00065)	(0.00065)	(0.00065)
Curr DSCR	-0.37***	-0.37***	-0.37***	-0.37***	-0.37***	-0.37***	-0.37***	-0.37***
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Observations	726,479	726,479	726,479	726,795	920,574	920,574	920,574	920,574
Pseudo-R^2	0.16	0.16	0.16	0.16	0.15	0.15	0.15	0.15
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. *DPO* is measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 4: Control and treatment group comparison

	Panel A: Servicer watchlist grouping		
	Control	Treatment	Control-Treatment
Origination DSCR	1.54	1.68	-0.138***
Origination LTV	68.9	65.9	3.035***
Origination occ rate	93.9	94.8	-0.888***
Origination interest rate	6.03	5.95	0.0859***
Observations	*11,083*	*17,186*	
DSCR Aug 2009	1.37	1.76	-0.387***
LTV Aug 2009	68.5	65.7	2.842***
Occ rate Aug 2009	88.2	93.9	-5.713***
Observations	*9,929*	*16,779*	

	Panel B: Delinquency grouping		
	Control	Treatment	Control-Treatment
Origination DSCR	1.50	1.64	-0.141***
Origination LTV	71.9	66.8	5.115***
Origination occ rate	93.9	94.4	-0.562*
Origination interest rate	6.16	5.97	0.192***
Observations	*1,534*	*26,735*	
DSCR Aug 2009	1.35	1.62	-0.269***
LTV Aug 2009	70.4	66.6	3.764***
Occ rate Aug 2009	89.3	91.8	-2.555***
Observations	*537*	*26,171*	

Notes: 1) Results of differences-in-means tests for control and treatment group loans. Columns 2 and 3 report estimation sample means, and column 4 reports the difference in means between control and treatment groups (significance levels are $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$). In Panel A, the control group loans are those that appeared on the servicer watchlist at least once prior to August 1, 2009, and the treatment group are loans that never appeared on the watchlist prior to August 1, 2009. In Panel B, the control group loans are those that were 30+ days delinquent at least once prior to August 1, 2009, and the treatment group are loans that were never 30+ days delinquent prior to August 1, 2009. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. All variables defined in Table A1. Variables are winsorized at the 1% level in each tail.

Table 5A: Triple differences design: Watchlist vs. no watchlist loans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×No Watchlist	0.29**	0.29***	0.36***	0.41***	0.28**	0.28***	0.33***	0.37***
	(0.12)	(0.10)	(0.093)	(0.087)	(0.12)	(0.11)	(0.093)	(0.086)
DPO×Post	0.15**	0.14**	0.13**	0.11**	0.096	0.10*	0.11**	0.097*
	(0.063)	(0.058)	(0.055)	(0.052)	(0.063)	(0.057)	(0.054)	(0.051)
DPO	-0.046	-0.056*	-0.053*	-0.096***	-0.050	-0.063**	-0.060*	-0.10***
	(0.032)	(0.032)	(0.032)	(0.032)	(0.031)	(0.031)	(0.031)	(0.031)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.17	0.17	0.17	0.17	0.16	0.16	0.16	0.16
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 5B: Triple differences design: Delinquent vs. never delinquent loans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×Never Delinquent	0.24	0.29**	0.43***	0.41***	0.33**	0.33**	0.44***	0.41***
	(0.16)	(0.15)	(0.13)	(0.12)	(0.17)	(0.15)	(0.12)	(0.12)
DPO×Post	0.020	-0.018	-0.19	-0.19*	-0.11	-0.089	-0.22**	-0.21**
	(0.14)	(0.13)	(0.11)	(0.11)	(0.15)	(0.13)	(0.11)	(0.10)
DPO	-0.054	-0.053	-0.081*	-0.16***	-0.069*	-0.082*	-0.12***	-0.19***
	(0.045)	(0.045)	(0.045)	(0.046)	(0.041)	(0.042)	(0.041)	(0.042)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.26	0.26	0.26	0.26	0.24	0.24	0.24	0.24
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 6: DPOs and full payoff transfers

	Full payoff transfer							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.33***	0.32***	0.30***	0.14*	0.30***	0.29***	0.28***	0.14*
	(0.094)	(0.088)	(0.083)	(0.074)	(0.097)	(0.088)	(0.082)	(0.073)
DPO	-0.21***	-0.19***	-0.18***	-0.17***	-0.22***	-0.20***	-0.18***	-0.17***
	(0.049)	(0.048)	(0.049)	(0.045)	(0.047)	(0.047)	(0.047)	(0.043)
Observations	516,249	516,249	516,249	516,249	742,218	742,218	742,218	742,218
Pseudo-R^2	0.13	0.12	0.12	0.12	0.12	0.12	0.12	0.12
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable is equal to 1 if the loan was transferred and realized a full payoff *ex post*, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Full payoffs are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 7: DPOs and no default transfers

	Transfer (No default)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.28**	0.38***	0.43***	0.25**	0.22*	0.33***	0.40***	0.22**
	(0.13)	(0.13)	(0.11)	(0.11)	(0.12)	(0.12)	(0.11)	(0.10)
DPO	-0.16***	-0.17***	-0.14**	-0.18***	-0.18***	-0.19***	-0.16***	-0.17***
	(0.060)	(0.062)	(0.062)	(0.062)	(0.058)	(0.060)	(0.059)	(0.057)
Observations	386,927	386,927	386,927	386,927	601,973	601,973	601,973	601,973
Pseudo-R^2	0.13	0.14	0.14	0.14	0.13	0.13	0.13	0.13
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable is equal to 1 if the loan was transferred yet never defaults, either prior to or after transfer, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Defaults are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 8A: Cumulative DPOs and transfers

	Transfer					
	(1)	(2)	(3)	(4)	(5)	(6)
Cumul DPO×Post	0.024***	0.018***	0.018***	0.022***	0.015***	0.018***
	(0.0037)	(0.0040)	(0.0053)	(0.0037)	(0.0040)	(0.0053)
Cumul DPO	-0.023***	-0.018***	-0.021***	-0.022***	-0.016***	-0.022***
	(0.0039)	(0.0042)	(0.0053)	(0.0038)	(0.0041)	(0.0053)
Cumul DP×Post×Never Watchlist		0.025***			0.026***	
		(0.0069)			(0.0072)	
Cumul DPO×Post×Never Delinquent			0.020**			0.020**
			(0.0083)			(0.0088)
Observations	726,479	726,479	726,479	920,574	920,574	920,574
Pseudo-R^2	0.16	0.17	0.26	0.15	0.16	0.25
MSA×Quarter FE	✓	✓	✓			
Servicer FE	✓	✓	✓			
MSA×Servicer FE				✓	✓	✓
Current Quarter FE				✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable is equal to 1 if the loan was transferred yet never defaults, either prior to or after transfer, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Defaults are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 8B: Cumulative DPOs and full payoff and no default transfers

	Full payoff transfer		Transfer (No default)	
	(1)	(2)	(3)	(4)
Cumul DPO×Post	0.023***	0.021***	0.020**	0.019**
	(0.0063)	(0.0058)	(0.0085)	(0.0076)
Cumul DPO	-0.022***	-0.020***	-0.019**	-0.018**
	(0.0065)	(0.0060)	(0.0089)	(0.0079)
Observations	516,249	742,218	386,927	601,973
Pseudo-R^2	0.13	0.12	0.14	0.13
MSA×Quarter FE	✓		✓	
Servicer FE	✓		✓	
MSA×Servicer FE		✓		✓
Current Quarter FE		✓		✓
Origination Quarter FE	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable in columns 1 and 2 is equal to 1 if the loan was transferred and realized a full payoff ex-post, and 0 otherwise. The dependent variable in columns 3 and 4 is equal to 1 if the loan was transferred yet never defaults, either prior to or after transfer, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Defaults are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 9A: Historical DPO intensity and transfers

	Transfer					
	(1)	(2)	(3)	(4)	(5)	(6)
DPO Intensity×Post	9.09***	6.35***	5.65	9.22***	6.38***	7.55
	(2.06)	(2.37)	(6.43)	(2.05)	(2.35)	(6.16)
DPO Intensity	-14.0***	-11.4***	-10.6***	-9.03***	-6.71**	-6.16
	(2.99)	(3.07)	(3.86)	(3.15)	(3.19)	(3.88)
DPO Intensity×Post×Never Watchlist		9.89***			10.6***	
		(3.60)			(3.68)	
DPO Intensity×Post×Never Delinquent			3.72			4.42
			(6.87)			(6.70)
Observations	726,479	726,479	726,479	920,574	920,574	920,574
Pseudo-R^2	0.16	0.17	0.26	0.15	0.16	0.24
MSA×Quarter FE	✓	✓	✓			
Servicer FE	✓	✓	✓			
MSA×Servicer FE				✓	✓	✓
Current Quarter FE				✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable is equal to 1 if the loan was transferred yet never defaults, either prior to or after transfer, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Defaults are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 9B: Historical DPO intensity and full payoff and no default transfers

	Full payoff transfer		Transfer (No default)	
	(1)	(2)	(3)	(4)
DPO Intensity×Post	10.3***	12.0***	15.5***	17.7***
	(3.08)	(3.04)	(4.16)	(4.04)
DPO Intensity	-15.5***	-10.7**	-7.45	0.081
	(4.60)	(4.72)	(7.46)	(7.16)
Observations	516,249	742,218	386,927	601,973
Pseudo-R^2	0.13	0.12	0.14	0.13
MSA×Quarter FE	✓		✓	
Servicer FE	✓		✓	
MSA×Servicer FE		✓		✓
Current Quarter FE		✓		✓
Origination Quarter FE	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable in columns 1 and 2 is equal to 1 if the loan was transferred and realized a full payoff ex-post, and 0 otherwise. The dependent variable in columns 3 and 4 is equal to 1 if the loan was transferred yet never defaults, either prior to or after transfer, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Defaults are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 10A: Impact of DPOs on transfer likelihood: Master servicer fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.23***	0.22***	0.22***	0.21***	0.17***	0.18***	0.19***	0.19***
	(0.056)	(0.050)	(0.048)	(0.046)	(0.055)	(0.050)	(0.047)	(0.045)
DPO	-0.085***	-0.095***	-0.11***	-0.15***	-0.078***	-0.091***	-0.10***	-0.15***
	(0.027)	(0.027)	(0.028)	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.16	0.16	0.16	0.16	0.15	0.15	0.15	0.15
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Master Servicer FE	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Spc Servicer FE	✓	✓	✓	✓				
MSA×Spc Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. *DPO* is measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 10B: Triple differences design: Watchlist vs. no watchlist, master servicer fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×Never Watchlist	0.30**	0.30***	0.37***	0.41***	0.29**	0.29***	0.33***	0.37***
	(0.12)	(0.10)	(0.093)	(0.087)	(0.12)	(0.11)	(0.093)	(0.086)
DPO×Post	0.15**	0.14**	0.13**	0.11**	0.094	0.10*	0.11**	0.097*
	(0.063)	(0.058)	(0.055)	(0.052)	(0.063)	(0.057)	(0.054)	(0.051)
DPO	-0.045	-0.055*	-0.053*	-0.096***	-0.050	-0.063**	-0.061**	-0.10***
	(0.032)	(0.032)	(0.032)	(0.032)	(0.031)	(0.031)	(0.031)	(0.031)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.17	0.17	0.17	0.17	0.16	0.16	0.16	0.16
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Master Servicer FE	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Spc Servicer FE	✓	✓	✓	✓				
MSA×Spc Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 10C: Triple differences design: Delinquent vs. never delinquent, master servicer fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×Never Delinquent	0.24	0.29**	0.43***	0.41***	0.33**	0.33**	0.44***	0.41***
	(0.16)	(0.15)	(0.13)	(0.12)	(0.17)	(0.15)	(0.12)	(0.12)
DPO×Post	0.024	-0.016	-0.18	-0.19*	-0.10	-0.088	-0.22**	-0.21**
	(0.14)	(0.13)	(0.11)	(0.11)	(0.15)	(0.13)	(0.11)	(0.10)
DPO	-0.053	-0.052	-0.080*	-0.16***	-0.069*	-0.082**	-0.12***	-0.19***
	(0.045)	(0.045)	(0.045)	(0.046)	(0.041)	(0.042)	(0.041)	(0.042)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.26	0.26	0.26	0.26	0.24	0.24	0.24	0.25
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Master Servicer FE	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Spc Servicer FE	✓	✓	✓	✓				
MSA×Spc Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 11A: DPOs and full payoff transfers, master servicer fixed effects

	Full payoff transfer							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.33***	0.32***	0.30***	0.14*	0.30***	0.29***	0.28***	0.14*
	(0.095)	(0.088)	(0.083)	(0.074)	(0.097)	(0.088)	(0.082)	(0.073)
DPO	-0.22***	-0.20***	-0.18***	-0.17***	-0.22***	-0.20***	-0.18***	-0.17***
	(0.049)	(0.048)	(0.049)	(0.045)	(0.047)	(0.047)	(0.047)	(0.043)
Observations	514,938	514,938	514,938	514,938	739,934	739,934	739,934	739,934
Pseudo-R^2	0.13	0.13	0.13	0.13	0.12	0.12	0.12	0.12
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Master Servicer FE	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Spc Servicer FE	✓	✓	✓	✓				
MSA×Spc Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable is equal to 1 if the loan was transferred and realized a full payoff ex-post, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Full payoffs are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 11B: DPOs and no default transfers, master servicer fixed effects

	Transfer (No default)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.28**	0.37***	0.43***	0.25**	0.22*	0.33***	0.40***	0.22**
	(0.13)	(0.13)	(0.11)	(0.11)	(0.12)	(0.12)	(0.11)	(0.10)
DPO	-0.16***	-0.17***	-0.14**	-0.18***	-0.18***	-0.19***	-0.16***	-0.18***
	(0.061)	(0.062)	(0.062)	(0.062)	(0.058)	(0.060)	(0.058)	(0.058)
Observations	385,910	385,910	385,910	385,910	600,299	600,299	600,299	600,299
Pseudo-R^2	0.14	0.14	0.14	0.14	0.13	0.13	0.13	0.13
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Master Servicer FE	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Spc Servicer FE	✓	✓	✓	✓				
MSA×Spc Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable is equal to 1 if the loan was transferred yet never defaults, either prior to or after transfer, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Defaults are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 12: Impact of DPOs on transfer likelihood: Master Service × Special Servicer (MS×SS) or Deal fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.22***	0.22***	0.23***	0.22***	0.23***	0.22***	0.22***	0.22***
	(0.056)	(0.050)	(0.047)	(0.046)	(0.056)	(0.050)	(0.047)	(0.046)
DPO	-0.086***	-0.095***	-0.11***	-0.16***	-0.086***	-0.094***	-0.11***	-0.16***
	(0.027)	(0.027)	(0.028)	(0.028)	(0.027)	(0.027)	(0.028)	(0.027)
Observations	725,138	725,138	725,138	725,138	711,068	711,068	711,068	711,068
Pseudo-R^2	0.16	0.16	0.16	0.16	0.17	0.17	0.17	0.17
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MS×SS Pair FE	✓	✓	✓	✓				
Deal FE					✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. *DPO* is measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 13A: Impact of DPOs on transfer likelihood: Controlling for time to maturity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.23***	0.22***	0.22***	0.22***	0.17***	0.18***	0.19***	0.19***
	(0.056)	(0.050)	(0.048)	(0.046)	(0.055)	(0.050)	(0.047)	(0.045)
DPO	-0.085***	-0.094***	-0.11***	-0.15***	-0.079***	-0.091***	-0.10***	-0.15***
	(0.027)	(0.027)	(0.028)	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)
Remterm	-0.0010***	-0.0010***	-0.0010***	-0.0010***	-0.0011***	-0.0011***	-0.0011***	-0.0011***
	(0.00018)	(0.00018)	(0.00018)	(0.00018)	(0.00020)	(0.00020)	(0.00020)	(0.00020)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.16	0.16	0.16	0.16	0.15	0.15	0.15	0.15
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. *DPO* is measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 13B: Triple differences design: Watchlist vs. never watchlist, controlling for time to maturity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×Never Watchlist	0.29**	0.29***	0.37***	0.41***	0.28**	0.28***	0.33***	0.37***
	(0.12)	(0.10)	(0.093)	(0.087)	(0.12)	(0.11)	(0.093)	(0.086)
DPO×Post	0.15**	0.14**	0.13**	0.11**	0.098	0.10*	0.11**	0.10**
	(0.063)	(0.058)	(0.055)	(0.052)	(0.063)	(0.057)	(0.054)	(0.051)
DPO	-0.044	-0.054*	-0.052	-0.096***	-0.050	-0.062**	-0.060*	-0.10***
	(0.032)	(0.032)	(0.032)	(0.032)	(0.031)	(0.031)	(0.031)	(0.031)
Remterm	-0.00095***	-0.00095***	-0.00095***	-0.00095***	-0.0010***	-0.0010***	-0.0010***	-0.0010***
	(0.00018)	(0.00018)	(0.00018)	(0.00018)	(0.00020)	(0.00020)	(0.00020)	(0.00020)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.17	0.17	0.17	0.17	0.16	0.16	0.16	0.16
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 13C: Triple differences design: Delinquent vs. never delinquent, controlling for time to maturity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×Never Delinquent	0.25	0.29**	0.43***	0.41***	0.34**	0.33**	0.44***	0.41***
	(0.16)	(0.15)	(0.13)	(0.12)	(0.17)	(0.15)	(0.12)	(0.12)
DPO×Post	0.015	-0.019	-0.18	-0.18*	-0.11	-0.086	-0.22**	-0.20**
	(0.14)	(0.13)	(0.11)	(0.11)	(0.15)	(0.13)	(0.11)	(0.10)
DPO	-0.052	-0.051	-0.079*	-0.16***	-0.069*	-0.082*	-0.12***	-0.19***
	(0.045)	(0.045)	(0.045)	(0.046)	(0.041)	(0.042)	(0.041)	(0.042)
Remterm	-0.00094***	-0.00094***	-0.00094***	-0.00094***	-0.0010***	-0.0010***	-0.0010***	-0.0010***
	(0.00020)	(0.00020)	(0.00020)	(0.00020)	(0.00022)	(0.00022)	(0.00022)	(0.00022)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.26	0.26	0.26	0.26	0.24	0.24	0.25	0.25
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 14A: DPOs and full payoff transfers, controlling for time to maturity

	Full payoff transfer							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.33***	0.32***	0.30***	0.14*	0.30***	0.30***	0.28***	0.15**
	(0.095)	(0.088)	(0.084)	(0.075)	(0.097)	(0.088)	(0.082)	(0.073)
DPO	-0.21***	-0.19***	-0.18***	-0.17***	-0.22***	-0.20***	-0.19***	-0.17***
	(0.049)	(0.049)	(0.049)	(0.046)	(0.047)	(0.047)	(0.047)	(0.044)
Remterm	-0.0018***	-0.0018***	-0.0018***	-0.0018***	-0.0019***	-0.0019***	-0.0019***	-0.0019***
	(0.00025)	(0.00025)	(0.00025)	(0.00025)	(0.00026)	(0.00026)	(0.00026)	(0.00026)
Observations	516,249	516,249	516,249	516,249	742,218	742,218	742,218	742,218
Pseudo-R^2	0.13	0.13	0.13	0.13	0.12	0.12	0.12	0.12
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable is equal to 1 if the loan was transferred and realized a full payoff ex-post, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Full payoffs are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table 14B: DPOs and no default transfers, controlling for time to maturity

	Transfer (No default)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.28**	0.38***	0.44***	0.25**	0.23*	0.34***	0.41***	0.22**
	(0.13)	(0.13)	(0.11)	(0.11)	(0.12)	(0.12)	(0.11)	(0.10)
DPO	-0.16***	-0.17***	-0.14**	-0.18***	-0.18***	-0.20***	-0.16***	-0.18***
	(0.060)	(0.062)	(0.062)	(0.063)	(0.058)	(0.060)	(0.059)	(0.058)
Remterm	-0.0020***	-0.0020***	-0.0020***	-0.0020***	-0.0020***	-0.0020***	-0.0020***	-0.0020***
	(0.00033)	(0.00033)	(0.00033)	(0.00033)	(0.00035)	(0.00035)	(0.00035)	(0.00035)
Observations	386,927	386,927	386,927	386,927	601,973	601,973	601,973	601,973
Pseudo-R^2	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The dependent variable is equal to 1 if the loan was transferred yet never defaults, either prior to or after transfer, and 0 otherwise. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-September 2009, with performance measured from September 2003-September 2015. Defaults are measured as of December 2020. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

A Variable Definitions

Table A1: Variable definitions

Variable	**Definition**
DPO indicator	Indicator equal to 1 if DPO was negotiated in loan-month, 0 otherwise
Transfer	Indicator equal to 1 if loan was transferred into special servicing, missing in all months following transfer, and 0 otherwise
Full payoff transfer	Indicator equal to 1 if loan was transferred into special servicing and received a full payoff, missing in all months following transfer, and 0 otherwise
Transfer (No default)	Indicator equal to 1 if loan was transferred but never 60+ days delinquent either before or after transfer, missing in all months following transfer, 0 otherwise
Post	Indicator equal to 1 for months between Oct 2009 and Sept 2011 and 0 for months between Sept 2007 and Sept 2009
Orig LTV	Origination loan-to-value ratio
Orig coupon	Origination loan interest rate
Orig occ	Origination occupancy rate
Orig DSCR	Origination debt service coverage ratio
Age	Age of loan in months
Balratio	Unpaid principal balance divided by origination balance
Curr LTV	Current loan-to-value ratio
Curr occ	Current occupancy rate
Curr DSCR	Current debt service coverage ratio
Never Watchlist	Indicator equal to 1 if loan was never on the servicer watchlist prior to August 1, 2009, and 0 otherwise
Never Delinquent	Indicator equal to 1 if loan was never 30+ days past due prior to August 1, 2009, and 0 otherwise
Remterm	Number of months remaining until maturity or the anticipated repayment date
Cumulative DPO	Cumulative sum of the number of DPOs at the special servicer level
DPO Intensity	Rolling average of the number of DPOs divided by the number of loans in special servicing in a given month

B Special Servicer-Times-*Post* Controls

Table B1: Robustness: Special servicer×*Post* controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.10*	0.11**	0.13**	0.12**	0.083	0.090*	0.11**	0.11**
	(0.061)	(0.054)	(0.051)	(0.049)	(0.061)	(0.054)	(0.050)	(0.048)
DPO	-0.064**	-0.069**	-0.077***	-0.12***	-0.040	-0.051*	-0.060**	-0.11***
	(0.029)	(0.029)	(0.029)	(0.030)	(0.029)	(0.029)	(0.029)	(0.029)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16
Servicer×Post FE	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
PropType FE	✓	✓	✓	✓	✓	✓	✓	✓
DealType FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. All specifications include special servicer×*Post* indicator variables. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

C Linear Probability Model

Table D1: Robustness: Linear probability model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.0021***	0.0020***	0.0021***	0.0017***	0.00082	0.00084	0.0011**	0.00086*
	(0.00052)	(0.00049)	(0.00047)	(0.00045)	(0.00061)	(0.00054)	(0.00049)	(0.00046)
DPO	-0.00051***	-0.00066***	-0.00086***	-0.0012***	-0.00033**	-0.00045***	-0.00061***	-0.00089***
	(0.00019)	(0.00019)	(0.00020)	(0.00020)	(0.00014)	(0.00015)	(0.00015)	(0.00015)
Observations	762,351	762,351	762,351	762,351	945,363	945,363	945,363	945,363
R^2	0.012	0.012	0.012	0.012	0.009	0.009	0.009	0.009
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating linear probability model regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

D Removing Near-to-Maturity Loans

In this section, we reproduce the results of Tables 3, 5A, and 5B after excluding loans with less than or equal to 12 months remaining to maturity at the time of transfer. That is, we exclude loans for which $remterm \leq 12$ at the time of transfer.

Table D1A: Impact of DPOs on transfer likelihood: Removing near-to-maturity loans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.19***	0.19***	0.22***	0.25***	0.14**	0.14***	0.19***	0.22***
	(0.058)	(0.052)	(0.050)	(0.048)	(0.057)	(0.052)	(0.049)	(0.047)
DPO	-0.073**	-0.081***	-0.12***	-0.17***	-0.066**	-0.077***	-0.11***	-0.16***
	(0.028)	(0.028)	(0.029)	(0.029)	(0.028)	(0.028)	(0.028)	(0.028)
Observations	704,259	704,259	704,259	704,259	905,889	905,889	905,889	905,889
Pseudo-R^2	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. *DPO* is measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table D1B: Triple differences design: Watchlist vs. never watchlist, removing near-to-maturity loans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×Never Watchlist	0.30**	0.30***	0.35***	0.41***	0.30**	0.30***	0.32***	0.38***
	(0.12)	(0.11)	(0.097)	(0.091)	(0.13)	(0.11)	(0.097)	(0.090)
DPO×Post	0.11*	0.11*	0.13**	0.14***	0.059	0.066	0.11*	0.12**
	(0.065)	(0.060)	(0.057)	(0.055)	(0.065)	(0.059)	(0.056)	(0.053)
DPO	-0.030	-0.035	-0.062*	-0.11***	-0.033	-0.042	-0.067**	-0.11***
	(0.033)	(0.033)	(0.033)	(0.034)	(0.032)	(0.032)	(0.032)	(0.032)
Observations	704,259	704,259	704,259	704,259	905,889	905,889	905,889	905,889
Pseudo-R^2	0.18	0.18	0.18	0.18	0.17	0.17	0.17	0.17
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table D1C: Triple differences design: Delinquent vs. never delinquent, removing near-to-maturity loans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×Never Delinquent	0.25	0.30**	0.53***	0.53***	0.34**	0.34**	0.52***	0.51***
	(0.17)	(0.15)	(0.14)	(0.13)	(0.17)	(0.15)	(0.13)	(0.12)
DPO×Post	0.00098	-0.044	-0.18	-0.17	-0.13	-0.12	-0.22**	-0.19*
	(0.15)	(0.13)	(0.12)	(0.11)	(0.15)	(0.13)	(0.11)	(0.10)
DPO	-0.044	-0.042	-0.080*	-0.15***	-0.054	-0.065	-0.11***	-0.18***
	(0.046)	(0.047)	(0.047)	(0.047)	(0.042)	(0.043)	(0.042)	(0.043)
Observations	704,259	704,259	704,259	704,259	905,889	905,889	905,889	905,889
Pseudo-R^2	0.28	0.28	0.28	0.28	0.26	0.26	0.26	0.26
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

E Marginal Effects

In this section, we tabulate marginal effects for the coefficients in Tables 3, 5A, and 5B. T-statistics are reported in parentheses below each marginal effect.

Table E1A: Impact of DPOs on transfer likelihood: Marginal effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.00196***	0.00194***	0.00195***	0.00187***	0.00120**	0.00125***	0.00134***	0.00133***
	(4.02)	(4.39)	(4.68)	(4.67)	(3.05)	(3.51)	(4.02)	(4.18)
DPO	-0.000743**	-0.000824***	-0.000933***	-0.00134***	-0.000555**	-0.000643***	-0.000732***	-0.00106***
	(-3.09)	(-3.48)	(-3.86)	(-5.56)	(-2.91)	(-3.37)	(-3.82)	(-5.55)
Orig LTV	0.0000990***	0.0000988***	0.0000993***	0.0000997***	0.0000791**	0.0000789**	0.0000792**	0.0000791**
	(3.36)	(3.36)	(3.39)	(3.41)	(2.85)	(2.84)	(2.86)	(2.87)
Orig coupon	0.000361	0.000361	0.000362	0.000366	0.000239	0.000239	0.000241	0.000243
	(1.60)	(1.60)	(1.60)	(1.62)	(1.24)	(1.24)	(1.25)	(1.27)
Orig occ	0.0000421***	0.0000422***	0.0000422***	0.0000423***	0.0000326***	0.0000327***	0.0000327***	0.0000327***
	(3.90)	(3.91)	(3.91)	(3.92)	(3.63)	(3.64)	(3.64)	(3.64)
Orig DSCR	0.00109***	0.00109***	0.00109***	0.00109***	0.000977***	0.000978***	0.000980***	0.000980***
	(7.02)	(7.01)	(7.01)	(7.00)	(7.42)	(7.42)	(7.44)	(7.44)
Age	-0.000102*	-0.000106*	-0.000110*	-0.000104*	-0.0000698	-0.0000727	-0.0000754	-0.0000709
	(-1.97)	(-2.05)	(-2.11)	(-2.00)	(-1.63)	(-1.70)	(-1.76)	(-1.65)
Balratio	0.00820**	0.00819**	0.00824**	0.00828**	0.00618*	0.00617*	0.00620*	0.00623*
	(2.71)	(2.70)	(2.72)	(2.73)	(2.45)	(2.45)	(2.47)	(2.48)
Curr LTV	0.00000485	0.00000496	0.00000442	0.00000400	0.00000486	0.00000503	0.00000476	0.00000483
	(0.18)	(0.18)	(0.16)	(0.15)	(0.19)	(0.19)	(0.18)	(0.19)
Curr occ	-0.000153***	-0.000153***	-0.000152***	-0.000152***	-0.000121***	-0.000121***	-0.000121***	-0.000121***
	(-24.02)	(-24.02)	(-24.01)	(-24.04)	(-23.44)	(-23.44)	(-23.44)	(-23.48)
Curr DSCR	-0.00319***	-0.00319***	-0.00319***	-0.00319***	-0.00260***	-0.00259***	-0.00259***	-0.00259***
	(-14.54)	(-14.53)	(-14.51)	(-14.51)	(-14.26)	(-14.25)	(-14.22)	(-14.21)
Observations	726,479	726,479	726,479	726,795	920,574	920,574	920,574	920,574
Pseudo-R^2	0.16	0.16	0.16	0.16	0.15	0.15	0.15	0.15
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. *DPO* is measured at the special servicer-month level and all other variables are at the loan-month level. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table E1B: Triple differences design: Watchlist vs. never watchlist, marginal effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×Never Watchlist	0.00249*	0.00252**	0.00315***	0.00350***	0.00197*	0.00199**	0.00234***	0.00260***
	(2.41)	(2.79)	(3.93)	(4.67)	(2.28)	(2.65)	(3.57)	(4.27)
DPO×Post	0.00128*	0.00125*	0.00112*	0.000971*	0.000676	0.000723	0.000741*	0.000684
	(2.34)	(2.50)	(2.36)	(2.15)	(1.52)	(1.80)	(1.96)	(1.92)
DPO	-0.000394	-0.000481	-0.000460	-0.000832**	-0.000353	-0.000442*	-0.000424	-0.000706**
	(-1.42)	(-1.76)	(-1.65)	(-2.99)	(-1.61)	(-2.02)	(-1.93)	(-3.24)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.17	0.17	0.17	0.17	0.16	0.16	0.16	0.16
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

Table E1C: Triple differences design: Delinquent vs. never delinquent, marginal effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post×Never Delinquent	0.00190	0.00226*	0.00338***	0.00324***	0.00216*	0.00213*	0.00285***	0.00263***
	(1.51)	(1.98)	(3.38)	(3.47)	(2.02)	(2.26)	(3.54)	(3.52)
DPO×Post	0.000158	-0.000138	-0.00145	-0.00146	-0.000684	-0.000577	-0.00144*	-0.00135*
	(0.14)	(-0.14)	(-1.63)	(-1.76)	(-0.72)	(-0.69)	(-2.02)	(-2.06)
DPO	-0.000423	-0.000415	-0.000632	-0.00124***	-0.000448	-0.000529	-0.000780**	-0.00121***
	(-1.20)	(-1.17)	(-1.79)	(-3.45)	(-1.69)	(-1.95)	(-2.92)	(-4.49)
Observations	726,479	726,479	726,479	726,479	920,574	920,574	920,574	920,574
Pseudo-R^2	0.26	0.26	0.26	0.26	0.24	0.24	0.24	0.24
Controls	✓	✓	✓	✓	✓	✓	✓	✓
MSA×Quarter FE	✓	✓	✓	✓				
Servicer FE	✓	✓	✓	✓				
MSA×Servicer FE					✓	✓	✓	✓
Current Quarter FE					✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.

F Inclusion of *Post* Indicator Variable

In this section, we reproduce the results of columns 5 through 8 of Table 3 with the inclusion of the *Post* indicator variable

Table F1: Impact of DPOs on transfer likelihood: Including *Post* indicator

	(1)	(2)	(3)	(4)
	DPO[-4,-7]	DPO[-5,-8]	DPO[-6,-9]	DPO[-7,-10]
DPO×Post	0.17***	0.18***	0.19***	0.19***
	(0.055)	(0.050)	(0.047)	(0.045)
DPO	-0.078***	-0.091***	-0.10***	-0.15***
	(0.027)	(0.027)	(0.027)	(0.027)
Post	0.96***	0.98***	1.00***	1.02***
	(0.31)	(0.31)	(0.31)	(0.31)
Observations	920,574	920,574	920,574	920,574
Pseudo-R^2	0.15	0.15	0.15	0.15
Controls	✓	✓	✓	✓
MSA×Quarter FE				
Servicer FE				
MSA×Servicer FE	✓	✓	✓	✓
Current Quarter FE	✓	✓	✓	✓
Origination Quarter FE	✓	✓	✓	✓
Prop Type FE	✓	✓	✓	✓
Deal Type FE	✓	✓	✓	✓
Originator FE	✓	✓	✓	✓
SE Clust by loan	✓	✓	✓	✓

Notes: 1) Results of estimating probit regressions of transfer likelihood on DPO measures and controls. The DPO variables are measured at the special servicer-month level and all other variables are at the loan-month level. Controls include origination LTV, origination coupon, origination occupancy rate, and origination DSCR, as well as age, the ratio of current unpaid balance to origination balance, current LTV, current occupancy rate, and current DSCR. 2) Data is from Trepp for CMBS deals originated from January 2002-December 2007, with performance measured from September 2007-September 2011. We exclude 2009Q3 from the estimation. 3) All variables defined in Table A1. Variables are winsorized at the 1% level in each tail. 4). $***p < 0.01$, $**p < 0.05$, and $*p < 0.1$. Standard errors clustered at the loan level.