Post-crisis Signals in Securitization: Evidence from Auto ABS

Elizabeth Klee and Chaehee Shin

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Elizabeth Klee[*]   Chaehee Shin[†]

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Abstract

We find significant evidence of asymmetric information and signaling in post-crisis offerings in the auto asset-backed securities (ABS) market. Using granular regulatory reporting data, we are able to directly measure private information and quantify its effect on signaling and pricing. We show that lenders "self-finance" unobservably higher-quality loans by holding these loans for longer periods to signal private information. This signal is priced in initial offerings of auto ABS and accurately predicts ex-post loan performance. We also demonstrate that our results are robust to exogenous shifts in the demand and supply of auto loans. Despite an environment of post-crisis enhanced transparency and securitization standards, signaling may be motivated by inattentive investors and regulations enforcing "no adverse selection" in constructing ABS.

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[*]Federal Reserve Board, E-mail: `elizabeth.c.klee@frb.gov`

[†]Federal Reserve Board, E-mail: `chaehee.shin@frb.gov`

1 Introduction

Shoddy securitization practices were at the heart of the Great Financial Crisis (GFC) (Gorton and Metrick, 2013). Pre-crisis, securitization was subject to widespread asymmetric information, with investors having little information on the quality of the underlying loans. This asymmetric information resulted in adverse selection and an associated "lemons problem" in securitization markets (Akerlof (1970), Downing, Jaffee, and Wallace (2008)). In some cases, to mitigate the adverse selection, issuers signaled the quality of the loans (Spence (1973), Adelino, Gerardi, and Hartman-Glaser (2019)).

In the wake of the GFC, the Congress and regulators demanded new practices aimed at solving the asymmetric information problem in securitization. In this paper, we explore the effect of two regulatory changes on asymmetric information and signaling in the auto ABS market. One change was that the SEC's Regulation AB was modified to expand the information available to investors, by requiring the disclosure of detailed information on each loan comprising an auto ABS. Before the regulatory change, auto ABS issuers were only required to provide aggregate summary statistics in the prospectus associated with the securities offering.[1] The other change was that the major rating agencies had to attest that the loans comprising the ABS satisfied selected representations and warranties ("reps and warranties") common to the particular type of ABS. For example, for auto ABS, a common rep and warranty was that "no adverse selection" was used in constructing the pool of auto loans eventually securitized into auto ABS.[2] Practically, this new requirement prohibited issuers from only including "lemons" in securitizations.

Our main result is that, even in a post-crisis regulatory environment, auto ABS issuers signal private information regarding borrower or collateral quality to investors. In theory, the regulatory changes should eliminate the need to signal in this market. In practice, we show that this is not the case. More generally, (1) we directly measure private information; (2) we demonstrate signaling in a market where adverse selection is expressly forbidden and in an environment of enhanced transparency; and (3) we observe signaling in post-crisis issuance of auto ABS, a security far

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[1]Refer to Securities and Exchange Commission, 17 CFR Parts 229, 230, 232, 239, 240, 243, and 249, Asset-Backed Securities Disclosure and Registration, available at https://www.govinfo.gov/content/pkg/FR-2014-09-24/pdf/2014-21375.pdf.

[2]Refer to Securities and Exchange Commission, 17 CFR Parts 232, 240, 249, and 249b, Nationally Recognized Statistical Rating Organizations, available at https://www.sec.gov/rules/final/2014/34-72936.pdf. See Appendix for more details.

simpler and and an environment less prone to private information than private-label MBS issued in the 2000s. Taken together, these results suggest that asymmetric information is inherent in securitization, and thus the benefits of risk pooling should be weighed against the costs of imperfect information.

Our direct measure of private information comes from the granular ABS loan-level filings. These filings include not only "hard" information regarding the borrower and the underlying collateral, but also, indicators of potential "soft" information that the issuer may have, but is not directly available to the investor.[3] Examples of hard information include variables commonly used in auto loan underwriting, such as borrower credit scores and payment-to-income ratios, and loan standards and terms. Examples of soft information include whether there was an underwriting exception — suggesting that the underwriters may have obtained additional information about the loan, and whether income was verified — suggesting additional information attained by the lender since income reporting is usually taken at the borrower's word.

Importantly, acquiring the indicators of soft information may be relatively costly to the investor. Specifically, in addition to the granular filings data, all ABS deals provide prospectuses to investors prior to sale, in which they include pages ("tear sheets") that disclose summary statistics on the collateral pool. There are three important distinctions between the tear sheets and the loan-level data. First, the tear sheet contains some, but not all, variables disclosed on the loan-level filings. Most often, the prospectuses include summary statistics of hard information, but omit statistics of soft information. Second, the tear sheets are easy to use, while the granular loan level data require sophisticated programming skills to extract and analyze, a costly exercise. And third, pre-crisis, only the tear sheets were available to investors as a source of information; the granular data were not reported (Neilson et al., 2019). As such, it may be costly for investors to change their process for evaluating ABS.

The existence of hard and soft information and costly information acquisition for investors present a conflict for issuers subject to a no-adverse-selection rule. In particular, prices in initial offerings of securities may not appropriately reflect the quality of the underlying pool. We demonstrate that issuers surmount this problem by signaling. Consistent with the predictions of Leland

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[3] Rajan, Seru, and Vig (2015) and Keys, Seru, and Vig (2012) use a similar soft-hard information distinction in the subprime MBS market.

and Pyle (1977), who suggest that entrepreneurs can signal unobservably higher quality projects to potential investors through self-financing, we show that issuers hold unobservably higher-quality loans for longer periods. Of note, the longer the issuer holds the loan, the higher the costs of funding it — a form of self-financing. We find that issuers hold loans with soft information longer by adjusting the length of time between originating the loan and eventual securitization, a practice known as "warehousing." Importantly, the prospectuses generally report the average warehousing time of the pool, further bolstering our theory that it serves as a signal of quality.

Our results suggest that signaling based on the private, soft information is economically significant, while that for the reported, hard information is not. Consistent with the hypotheses, our soft information indicators significantly predict the signaling variable: loans made under an underwriting exception experience warehousing times that are 7 to 15 days longer. Loans for which obligor income has been verified are also warehoused for longer periods. Loans made on used cars shorten warehousing times by roughly 7 to 8 days. All effects are statistically significant and economically meaningful. In contrast, warehousing times barely budge for hard information. For instance, a one standard deviation jump in obligor credit score (96 points) corresponds to only a 0.01 day increase in the number of warehousing days. Thus, while the effect is statistically significant, it is far less economically meaningful. We interpret this as evidence that signaling is used primarily to communicate soft information, not hard information. We conduct various robustness analyses to address other possibilities; our overall results are unchanged.

Beyond the massive filings information, the structure of the auto market and of auto ABS allows for other direct tests of signaling based on asymmetric information. We use three "experiments" that provide plausibly exogenous variation in collateral and borrower quality to gauge their effects on loan warehousing times. Our tests explore the effects of auto recalls, model updates, and "super sales" on signaling. Our results conform to our hypothesis regarding signaling: worse collateral and weaker borrowers suggest shorter warehousing times. We also find that the signal is sensitive to the cost of producing it. In particular, we illustrate that warehousing times significantly differ according to lender type: those that have lower warehousing costs are more likely to signal quality than lenders with higher warehousing costs.

The signal is reflected in ex-post performance and initial offerings pricing, further strengthening our interpretation of the results. A one standard deviation increase in signaling on soft information

lowers the odds ratio of charge-off in 3 months post-securitization by 41.3 percent, or in dollar amounts, $1,562 less charged-off dollars from the principal loan amount. Moreover, signaling is priced in initial offerings of the ABS securities. As our measure of signaling increases by one standard deviation, the spread at origination on ABS over comparable-maturity Treasury securities narrows by 0.15 percentage points. Of note, industry practice is to use the spread at origination to indicate the underlying quality of the loans. Given the statistical significance of our signaling measure, this suggests that the signal is used in part to overcome the lemons problem. That is, while we do see that, in general, securities conform to the no-adverse-selection reps and warranties requirement on information that is public, our private information results suggests a potential adverse selection problem that is solved through the warehousing time signal.

Our paper makes two main contributions to the literature. First, we contribute to the literature on asymmetric information in securitization markets. Pre-crisis work on securitization of bank loans highlighted the role of internal funding costs, premiums associated with the sale of loans, and the probability of bank default as key considerations regarding a bank's decision to sell loans rather than retain them on the balance sheet (Gorton and Pennacchi, 1995). Other papers highlighted the possibility of efficient risk sharing and enhanced liquidity as another reason for securitization (Merton, 1990). In the wake of the crisis, a host of papers illustrated the implications of asymmetric information in securitization, primarily exploring the MBS market. For example, Keys et al. (2010) demonstrate that the securitization process and asymmetric information led in part to lax screening. Moreover, Rajan, Seru, and Vig (2015) show that unreported information can be more important than reported information for predicting ex post performance of mortgages. In addition, Agarwal, Chang, and Yavas (2012) illustrate that there was adverse selection in the securitization of prime mortgages as a result of information asymmetries; Calem, Henderson, and Liles (2011) provide evidence that this behavior resulted in "cherry-picking" by some mortgage lenders. However, Albertazzi et al. (2015) discuss that, for reputation reasons, some mortgage lenders did not exploit all information asymmetries; still, Griffin, Lowery, and Saretto (2014) provide evidence to the contrary.

Against this backdrop, our contribution to the literature is to provide evidence of asymmetric information and signaling in a post-crisis regulatory environment. Adelino, Gerardi, and Hartman-Glaser (2019) illustrates that signaling was used to remedy informational asymmetries and the

signal they study is similar to ours – warehousing times. While their focus is on the pre-crisis mortgage market, our paper examines the post-crisis auto ABS market, and we directly measure asymmetric information instead of inferring it from ex-post performance. Neilson et al. (2019) show the general effect of improved information via the new ABS-EE filings requirements post crisis. Our contribution is that we use a further distinction of private vs. public information to identify the effect of the post-crisis regulation on these different parts in the information structure. Moreover, we exploit the requirement of "no adverse selection" in the construction of pools to gain further insight into the effect of private information on signaling and pricing. More generally, our paper demonstrates that, even with Dodd-Frank regulatory reforms, private information significantly affects pricing, similar to Furfine (2019) for the CMBS market.

Second, our paper contributes to the literature on consumer ABS markets. Relative to the literature on the MBS market, the literature on consumer ABS markets is thin. [4] Implications of any findings on auto loan securitization could be potentially large, as roughly 85 percent of U.S. households own some kind of vehicle, versus 64 percent who own a home (Bricker et al., 2017). In addition, auto ABS provides an ideal laboratory to study asymmetric information because most loans (in our data, all loans) are originated and securitized by the same entity and the loans are highly standardized, compared to other types of loans including mortgages.

The remainder of the paper is organized as follows. Section 2 discusses the background and the regulatory framework for auto ABS. Section 3 describes the data. Section 4 presents empirical results from the baseline estimation for signaling, and two sub-sample robustness tests. Section 5 discusses three experiments and reports results. Section 6 presents results on post-securitization auto loan performance. Section 7 discusses and tests for alternative hypotheses. Section 8 concludes.

2 Auto ABS: Background and Regulatory Framework

2.1 Background

Lending for automobiles has been one of the most commonly issued consumer credit loans, as automakers find it advantageous to issue loans to boost sales. For example, The General Motors

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[3] Faltin-Traeger, Johnson, and Mayer (2010) is an important exception regarding crisis-period pricing.

[4] 2016 Survey of Consumer Finances.

Acceptance Corporation (GMAC) was founded in 1919 to provide credit for new-car buyers.[5] Similar to other consumer credit markets and the mortgage market, there was a buildup of lending in the early-to-mid 2000s (Figure 1). At the same time, the rise was far less steep than that of mortgages, and in particular, the market did not completely shut down during the financial crisis. Today, auto loans are still one of the most commonly held forms of consumer credit: auto loans have climbed to about $1.2 trillion outstanding at the end of 2018.

Auto ABS was one the first consumer ABS to come to the market in the 1980s, when securitization of consumer loans grew in earnest. Auto ABS markets continued to function for most of the financial crisis, although they received some support through the Term Asset-Backed Securities Loan Facility (TALF) (Campbell et al., 2011). Moreover, some securitization markets shut down after the crisis — most notably, that for private-label mortgage-backed securities. However, the auto ABS market has continued, even in riskier segments. At the end of 2018, auto ABS outstanding stood near $225 billion, with issuance in that year around $107 billion.[6]

Auto ABS provides an ideal setting to study important economic questions for many reasons. First, compared to other types of securities, auto ABS is a "plain vanilla" security. Just a few loan characteristics — vehicle make and model, year, age, *etc.* — are enough to capture most variation in collateral quality. On the other hand, in most other securitizations, it is extremely difficult to summarize heterogeneous dimensions of collateral quality, such as those of commercial real estate loans, with just a few numbers. Second, there has been little direct government intervention in auto ABS outside of the financial crisis. It therefore allows a thorough look at the inherent market mechanisms. Lastly, intermediation chains are short, thereby significantly reducing the burden to consider other types of complexities caused by multiple intermediaries. For public deals, auto loan originators are nearly always the servicer and the securitizer of the loan, and the sponsor of the associated auto ABS.

Even though auto ABS are simple, most studies of securitization focus on the residential mortgage market; rightfully so, given its central role in the financial crisis, not to mention the $7 trillion in agency MBS.[7] Nevertheless, it is perhaps more challenging to explore issues related to adverse

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[5]Henry Ford disapproved of providing credit, and provided a form of savings account instead ("The Engine of Enterprise: Credit in America" by Rowena Olegario, Cambridge: Harvard University Press, 2016).

[6]Refer to SIFMA, U.S. ABS issuance and outstanding, available at https://www.sifma.org/resources/research/us-abs-issuance-and-outstanding/.

[7]Refer to SIFMA, U.S. Agency MBS issuance and outstanding, available at https://www.sifma.org/resources/

selection in securitization markets within the context of the mortgage market. It is not only complicated by the existence of implicit or explicit government support from the government-sponsored enterprises, but also, differences in collateral quality related to geographic region are substantial. Finally, in the residential mortgage market, the loan originator, securitizer, servicer and sponsor are often unrelated entities (Kim et al., 2018).

One complication of auto ABS relative to residential MBS to study asymmetric information in securitization markets is that there can be changes in loan terms and securitization related to auto demand, and these decisions are made in a very centralized way. That is, auto loan terms, credit availability, and securitization practices are often adjusted to accommodate auto demand or supply by the major automakers. Although there may be coordinated incentives at selected steps in the intermediation chain, because the mortgage intermediation chain is more segmented, such broad incentives are likely more limited in residential mortgage markets. That said, this auto ABS centralization would tend to bias our results towards not finding significant effects of private information, as changes in securitization practices from demand factors could swamp those from private information. In addition, our securitizations represent better quality borrowers and securities than those that comprise the privately-placed securitization market. Asymmetric information is likely greater in the privately-placed market. Our results should be interpreted against this backdrop accordingly.

2.2 Regulatory Framework

There are a few important regulatory characteristics about the auto ABS market that illustrate why the auto ABS market is a powerful laboratory for exploring the effects of asymmetric information. Specifically, as part of the Dodd-Frank Act, for all asset-backed securities transactions (auto ABS, MBS and others), rating agencies must verify that all "representations (reps) and warranties" have been satisfied.[8] These reps and warranties confirm a number of attributes of the loan. Some

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research/us-mortgage-related-issuance-and-outstanding.

[8] 17 CFR Parts 232, 240, 249, and 249b states that:

> As noted in Section 943 of the Dodd-Frank Act mandates that the Commission adopt rules requiring an nationally-recognized statistical rating organization (NRSRO) to include in any report accompanying a credit rating of an asset-backed security a description of the representations, warranties, and enforcement mechanisms available to investors and how they differ from the representations, warranties, and enforcement mechanisms in issuances of similar securities.

See Pub. L. No. 111-203, 943.

examples of these are that the loan is a "valid sale and binding obligation," the loan has scheduled payments, the loan can be prepaid, and the obligor has auto insurance.

The presence of one rep and warranty and the absence of another deserve particular mention. First, rating agencies must ensure that "no adverse selection" was used in the construction of the underlying loan pool. Because of this, originators are expected to securitize (or not) a broad spectrum of loans. Second, there is no rep and warranty for auto ABS dictating that issuers obtain the same level of documentation that is required of other securitizations. For example, income verification is required for mortgages underlying MBS, but not required for auto loans backing an ABS. One possible reason for this is that overall, auto loan amounts are relatively low.

Taken together, these reps and warranties suggest a possible need to signal and a possible actual signal in the auto ABS market. The adverse selection rule can present a problem to a lender that has private information about the borrower. Specifically, the lender would lose money on a loan that, according to observable characteristics, would command a lower price. In this situation, a lender would want to signal the underlying quality of the loan. Moreover, the lack of a sufficient documentation rule suggests that income verification or other information gathering is above and beyond the basic requirements for the loans. If the lender does obtain extra information, it can use this as a signal for the underlying quality of the loan. We use these characteristics in the analysis that follows.

3   Data

Our primary data comes from loan-level XML files and prospectuses associated with post-crisis reporting requirements under SEC Regulation AB. The reporting requirement went into effect on November 23, 2016. Under the reporting requirement, all filings of prospectuses for public securities offerings must also have accompanying loan-level information submitted in electronic format, using SEC form ABS-EE.[9] Prospectuses must be filed at least three days in advance of the public placement of securities.[10]

The loan-level XML files comprise the universe of public auto ABS issued from 2017 Q1 to

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[9]The requirement applies to all registered offerings backed by auto loans and leases, residential and commercial mortgages, and debt securities including re-securitizations.

[10] More information is available at https://www.govinfo.gov/content/pkg/FR-2014-09-24/pdf/2014-21375.pdf.

2019 Q1, and consists of 5,930,935 unique loans and 89 ABS securities issued by 15 issuers. The data contains a range of information on the originator, borrower, and collateral associated with each loan. Our main variable of interest, the loan warehousing time, is computed by measuring the number of months between loan's origination and securitization. Figure 2 shows the distribution of warehousing time in our data. The average warehousing time is 15 months. While the majority of loans in our sample is securitized within 10 months of origination, some securitized loans remain on lenders' books for up to five years.

Table 1 presents summary statistics of the remainder of auto loan characteristics from the loan-level XML files, as of loan origination time. Because all ABS securities in our data are public issuance, they mostly represent prime auto loans. An average borrower in our data has a credit score of 695 and takes out a loan on a \$25,427 (relatively new) car at 8% interest rate for 67 months. Nevertheless, as can be seen from the wide ranges of borrower and collateral characteristics in Table 1, our data also has a substantial number of subprime auto loans. As such, the data allow us to capture a broad view into securitization, across both prime borrowers and subprime borrowers, used and new cars, and a range of geographic locations, not confined to specific groups of borrower or collateral.

Figure 3 breaks down the data by origination year and originator type. Most originations in our data are associated with captive auto finance companies and other non-bank finance companies affiliated with auto retailers.[11] Over the period, these non-banks altogether comprised roughly 70 percent of auto loan origination. Banks also have a significant presence representing the remaining 30 percent of origination, with some banks (*e.g.*, Santander) lending primarily on the subprime market and other banks servicing a variety of borrowers.[12]

While several proxies for soft information on the loans are available in the XML exhibits, they are not included in any of the prospectuses for the deals. To exploit the difference in information disclosure between the loan-level electronic exhibits and prospectuses, we match each ABS from the XML files to the prospectuses filed to the SEC prior to the placement. All auto ABS prospectuses

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[10]Original terms for the loans range from 24 months to 85 months in our data.

[10]A large, remaining share of auto ABS is privately placed, and not offered on public markets. Privately-placed auto ABS is disproportionately backed by subprime auto lending, and has been the focus of some attention lately.

[11]Our data reflects nine captive auto finance companies: BMW, Daimler, Ford, GM, Honda, Hyundai, Nissan, Toyota, and Volkswagen. Carmax, a used car dealer, also originates a notable share of loans.

[12]Our data reflects five banks: Ally, California Republic, Fifth Third, Santander, and USAA.

have tear sheets that have summary statistics describing the composition of their collateral pools. Figure 4 shows an example tear sheet from an actual ABS deal prospectus. As in this example, all of the prospectuses matched to our data always report summary statistics on average warehousing time, obligor credit score, payment-to-income ratio, original interest rate, original loan amount, and original loan term. In contrast, none of them report summary statistics on any proxy for soft information, such as the share of the pool that has been verified on income/employment or made on under an exception. Because many ABS investors reportedly rely on deal prospectuses to make investing decisions, information in the XML files is relatively more costly to obtain. This distinction allows us refer to the first group of loan characteristics — that are disclosed both on electronic exhibits and prospectuses — as variables indicative of "hard" information and the second group of loan characteristics — that are only disclosed on the loan-level electronic exhibits and never on prospectuses — as variables carrying "soft" information.

For auto recall-related analyses, we supplement the above loan-level SEC dataset with data from the National Highway Traffic Safety Administration's Office of Defects Investigation, IHS Markit's New Vehicle Registration, and Polk's Vehicles in Operation. We also use Ravenpack News Analytics to obtain sentiment and news volumes data on recalls (see the Data Appendix for more details). For the analysis using model updates, we gather major model updates information on 44 makes during 2014-2018 from Kelley Blue Book. We use Bloomberg for ABS price analyses.

4 Empirical Results

4.1 Preliminaries

Figure 5 shows preliminary evidence of selective timing based on soft information. We divide the sample into two groups based on the soft characteristics described above. In Figure 5a, we divide the sample into loans made under an underwriting exception versus those made without any exception. In general, lenders make loans that conform to set underwriting guidelines. However, whenever necessary, lenders additionally obtain information about the collateral and the borrower, and make underwriting exceptions upon seeing good mitigating factors to approve a loan. Therefore, the loans approved under an exception have been verified of good soft information about the loan, controlling for the usual loan characteristics. Similarly, in Figure 5b, the sample is divided into

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loans made on new cars versus loans made on used cars.

Both sub-figures show a clear rightward shift in the empirical distribution of warehoused months for loans that were likely of better credit or collateral quality, as opposed to other loans. Of course, there could be other factors driving these results. In what follows, we perform a battery of tests to determine whether soft information is driving these results. The results withstand the tests, and confirm the intuition in the figures.

4.2 Baseline Estimation

Specification To examine the effects of various loan characteristics on a loan's warehousing time, we evaluate the following baseline specification on the cross-section of all observed loans:

$$w_{ijt^o t^s} = \beta_0 + \beta_1 \mathbf{S_{it^o}} + \beta_2 \mathbf{H_{it^o}} + FE_j + Y\text{-}Q \, FE_{t^o} + Y\text{-}Q \, FE_{t^s} + \epsilon_{ijt^o t^s} \tag{1}$$

Our dependent variable is $w_{ijt^o t^s}$, defined as the number of warehoused months for loan i that was originated at time t^o in state j and securitized at time t^s.

$\mathbf{S_{it^o}}$ is a matrix of variables that proxy for soft, or private, information the lender has regarding the quality of loan i originated at time t^o. We use three variables: "Income Verified," indicating that the underwriter verified the income of the borrower, "Underwriting Exception," indicating that the lender made an exception to its underwriting guidelines when extending the loan, and "Used Car" indicating whether the loan's underlying collateral is a used car.

We view our variables included in $\mathbf{S_{it^o}}$ as proxies for soft information for three reasons. First, as mentioned earlier, while there are indicators of verification and exception in the more detailed, loan-level electronic filings, summary statistics on these items are not reported on the associated prospectuses. Prospectuses provide useful information about the deal to potential investors in a convenient and accessible form; XML files require scouring and computing. For this reason, information costs can be lower from the prospectuses than from the XML files.

Second, even if investors used the loan-level filings to assess the pool quality, the lender still has private information on the actual verifications or exceptions made. Income verification is usually done selectively on those loans whose obligor has a low credit score, a high loan-to-value, *etc.* That

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[12]Indicators of used versus new cars are included in a subset of prospectuses.

the loan was extended tells us that there was confirmation of otherwise unobservable loan quality upon income verification. Similarly, that an underwriting exception was made and that the loan was ultimately extended implies presence of private information on the lender side — particularly, one that was indicative of better loan quality.

Third, while new cars represent uniform collateral, used cars can have soft information regarding quality. Although some lenders, such as banks, may not have private information on the quality of the car, others, such as captive auto finance companies or Carmax, are likely to have such private information.

In addition to our soft information measures, we include $\mathbf{H_{it^o}}$, which is a matrix of variables that proxy hard, or public, information on loan i originated at t^o. We include variables at the core of underwriting standards, including the obligor credit score and the payment-to-income ratio. We also incorporate information about the loan itself, including the original interest rate, amount, and term. Of note, and unlike the variables in $\mathbf{S_{it^o}}$, these hard information indicators are included in all of the prospectuses associated with the ABS deals in our data. Moreover, these are the variables on which, presumably, the reps and warranty of "no adverse selection" should be applied.

We include a number of other controls to eliminate potential confounding effects. For example, we control for geographic location of the loan by the state in which the loan was originated (FE_j), which proxies for macroeconomic conditions in the local area or differences in lending conditions or bankruptcy laws that could drive some of our results. In addition, we include fixed effects for the origination year-quarter (Y-$Q\,FE_{t^o}$), and fixed effects on the year and quarter the loan was securitized (Y-$Q\,FE_{t^s}$). Taken together, these factors control for possible secular trends in securitization markets.

Additionally, we check robustness using two more groups of fixed effects. The first group uses dummy variables indicating the vehicle manufacturer or the make to further control for unobservable factors associated with the underlying collateral. Manufacturer/make information represents most of the important differentiation across autos. The second group include dummy variables for the originator type. These indicators capture costs of warehousing, which can differ significantly across lender types. For example, banks generally have a low-cost deposit base that can be used to finance auto loans, while a nonbank captive auto financing company needs to rely on more expensive and risky wholesale funding. For all our specifications, we cluster standard errors at the ABS pool level.

Results Table 2 presents our baseline results. Each column presents results that include our $\mathbf{S_{it\circ}}$ and $\mathbf{H_{it\circ}}$ matrices; the specifications differ by the types of control variables used.

Overall, the results strongly suggest signaling in auto ABS markets. As shown in the top portion of the table, proxies of soft information carry significant effects on loan warehousing times in the majority of specifications. In particular, if the lender verified the borrower's income or made an underwriting exception, warehousing months are substantially longer. Importantly, the magnitudes are economically significant. Conditional on hard information on loan/borrower characteristics and various fixed effects, the standard deviation of warehoused months is close to one month, at 1.040. If income is verified, then the warehoused time increases by roughly another week, or 0.2 standard deviations. If there is an underwriting exception, warehousing times stretch even further, to about 11 to 15 days longer, or 0.35-0.48 standard deviations.

Soft information on the quality of the underlying collateral also plays an important role. Even after controlling for a range of characteristics, used cars experience shorter warehousing periods and new cars longer ones. The effects are statistically significant for most specifications and the economic magnitudes suggest that used cars shorten warehousing times by about 7 to 8 days, or 0.24-0.28 standard deviations. While statistics on the share of used car loans in the pool are available on some prospectuses, ABS issuers may still choose to signal in a simple way the quality of the underlying portfolio, as used cars, by their very nature, are subject to private information. Additionally, purchases of an older car may be a proxy for other soft information not observable to the investor.

The bottom portion of the table displays estimated coefficients for factors associated with hard information. In sharp contrast to our soft information results, proxies of hard information have smaller effects on warehousing times. The two primary underwriting variables, obligor credit score and payment-to-income ratio, do not show economically significant coefficients. All else equal, the 0.0004 coefficient on obligor credit score implies a 0.01 day fall in the number of warehousing days for every standard deviation decrease in credit score (96 points). So, while statistically significant, the effect is not economically meaningful. The coefficient on the payment-to-income ratio does not have any statistical significance under all specifications. Other factors, such as the original interest rate at which the loan was extended, the loan amount or the term, are not consistently

significant across specifications. Taken together, these suggest that the lender signals what it sees as the underlying — especially, unobservable — better quality of the loan than what the observable information indicates.

The results are robust across various specifications using different sets of fixed effects in columns (1)-(5). When we add the securitization year-quarter fixed effects, we see some coefficients drop in magnitude and others lose significance (column (1) versus column (2)). Most notably, we see the economic magnitude of the credit score coefficient drop by tenfold, suggesting there are secular trends in the credit score's effect on warehousing times. At the same time, there is little difference in the coefficients across specifications that control for vehicle manufacturer and make (columns (3) and (4)). This suggests robustness to our specification as well as some uniformity of factors affecting warehousing times by manufacturer. Looking broadly across columns (2) through (5), coefficients are relatively constant across specifications, save the magnitude and significance of income verification when controlling for lender type. We investigate the effects of different lender types in more detail in Section 7.1.

4.3 Robustness: Sub-sample Analyses

In this section, we conduct additional analyses to check robustness of our baseline results across different subgroups of auto loans and time periods.

New cars We first use the same baseline specification as in equation (1), but limit the sample to loans extended on new cars. We do this to eliminate any potential effect from unobserved factors related to collateral quality. Specifically, even after controlling for borrower characteristics and vehicle make/model, used cars are relatively more heterogeneous in quality than new ones, and therefore, in their collateral value. By focusing only on the sub-sample of new cars, we can eliminate altogether the potentially remaining soft information on the underlying quality of the collateral, and instead capture only soft information on the borrower.

Table 3 reports the results. Underwriting exceptions continue to lengthen warehousing times, with only a little less economic significance than in the baseline results. Income verification keeps its sign but loses statistical significance. Some of this result could reflect relatively more homogeneous new car buyer profiles with respect to income than for used car buyers, in that new car

buyers often have higher income and credit scores than used car buyers. However, there are still important effects of underwriting exceptions. If there is an underwriting exception, warehousing time lengthens by about 6 to 10 days, or 0.2-0.3 standard deviations. The results are statistically significant at the 1 percent level. As in the baseline, the economic magnitude of the effect of soft information on warehousing times swamps that of hard information. Coefficients on the obligor credit score and payment lose significance, perhaps bolstering the hypothesis regarding homogeneity of borrowers for new cars. At the same time, the original interest rate, loan amount, and term remain either statistically or economically insignificant.

New loans In another sub-sample analysis, we focus on loans originated after 2017, dropping loans originated during 2010-2016. Benefits from using this post-2017 sub-sample are two-fold. First, these loans were originated following the introduction of the new ABS-EE reporting requirement in 2016. Therefore, we can test whether our previous findings on signaling were mostly an artifact of the pre-reform period. Second, the sub-sample allows us to suppress the effects of remaining time fixed effects potentially correlated with our soft/hard information variables and the warehousing time. Because our main loan-level data comes from the reportings by ABS issuers since the 2016 reform, we only observe the loans securitized after 2017. By construction, warehousing time is mechanically higher for those loans that we observe to be originated in earlier years. If there is a remaining time trend in our soft/hard information variables (even after controlling for the year-quarter fixed effects as in the baseline), then it can confound identification of the effects of our soft/hard information variables on warehousing time.

Table 4 reports results based on the sample of post-2017 originated loans. In some cases, economic magnitudes of the effects of soft information variables are larger than those in the baseline results; in others, the results are roughly unchanged from the baseline. If income is verified, then the warehouse time increases by 8 to 9 days, or around 0.3 standard deviations. This effect is 1 to 2 days longer than the baseline effect, suggesting some strengthening of the signal with the advent of the reporting requirement. If a loan was extended under an underwriting exception, warehousing time increases to about 14 to 19 days longer, or 0.44-0.62 standard deviations. The effects, again, are larger in economic magnitude than those from the baseline effects (11-15 days). Coefficients on the indicator for a used car loan are similar to those in the baseline. Used car loans are warehoused

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for an average of 7 to 8 days, or around 0.2 standard deviations shorter than for new car loans.

Taken together, the results from the post-2017 originated loans sub-sample confirm our previous findings, and suggest that our results identify signaling of private information that remains even after the new disclosure requirement. More generally, these results highlight important information asymmetries inherent in auto ABS. Against this backdrop, we turn to further tests of private information below.

5   Evidence from Three Experiments

Other explanations may still be possible for our findings on signaling. For instance, there could be remaining unobserved soft information about the quality of each loan that may be correlated with the decision on the warehousing period. If the information is correlated with our variables of soft or hard information, an endogeneity issue would arise. Furthermore, the true hazard rate of securitization may not have been well captured because our data is only based on securitized loans. It could also be the case that the longer warehousing times are, more fundamentally, not a signal of private information. In this section, we bolster our previous findings on asymmetric information and signaling by using three variables that are plausibly exogenous, but can affect the warehousing period. These variables represent private information to lenders that they may want to signal to investors, so that it will be appropriately priced in the market.

5.1   Variations in Collateral Quality: Auto Recalls

Our first experiment uses instances of auto recalls to explore the effects of exogenous variation in the value of the collateral. As mentioned earlier, one strength of our analysis, relative to that on mortgages, is that the quality of cars is reasonably uniform for a given make and model of car. However, auto recalls represent an unexpected arrival of soft information regarding the underlying collateral quality. So that the loans associated with non-recalled autos (presumably of higher quality) are not priced and lumped together with loans for recalled autos (presumably lower quality), lenders signal the higher quality collateral by warehousing for longer times.

To explore the effect of recalls on warehousing times, we collect auto recalls data from the National Highway Traffic Safety Administration's Office of Defects Investigation. We extract data

on all instances of auto recalls that took place between 2009 and 2018 for the automakers in our data. Table 5 summarizes 1,601 recalls that took place during the period. A large majority are trivial recalls, involving, say, wiper blades or headlights, met only with slight annoyance by the car owner and handled with form letters and trips to the dealer. But some recalls involve more serious, safety-threatening issues that make the front pages for months.

To account for differential severity of recalls, we combine the actual recalls information with news reports on the recalls. Specifically, we use data on news volume and news sentiment on each recall, from "Aggregate Event Volume" and "Aggregate Event Sentiment" from Ravenpack News Analytics. Each of these data items measures the volume of events reported in financial news and the share of positive sentiment events, on a 91-day rolling basis (see Data Appendix for more details). The table shows wide ranges of average news volume, from 8.07 (Subaru) to 1741.33 (GM), and average sentiment, from the lowest (*i.e.*, the worst sentiment) 48.25 (FCA) to the highest (*i.e.*, the best sentiment) 61.42 (Mclaren).

We also take into account the number of affected cars relative to the total number of cars on the road. Because automakers have different market shares, some automakers will have many more cars than others. Therefore, even if the affected units amount to 0.1-0.2 million vehicles, it does not necessarily imply that the recall had a higher impact than a recall with a few hundreds or thousands of affected units; it depends on the automaker. Using data from IHS Markit's New Vehicle Registration and Polk's Vehicles in Operation, we scale the affected units of each recall by the number of operating cars of the make that is impacted by the recall ("scaled affected units").

In the end, we define $Recall\,Impact_{kt}$ as the (logarithm of) impact of recalls by an automaker k in month t, taking into consideration the severity of the recall using the adjustments described above.

$$Recall\,Impact_{kt} = ln\left(\frac{Scaled\,Affected\,Units_{kt} * News\,Volume_{kt}}{Sentiment_{kt}}\right) \qquad (2)$$

Scaled Affected Units$_{kt}$ is the total scaled affected units of k in month t. News Volume$_{kt}$ and Sentiment$_{kt}$ each takes an average of the news volume and the sentiment, for all recalls that took place to k in each month t. In other words, the average recall impact for an automaker k is higher, the higher the scaled affected units and the news volume, and the lower (or, the worse) the news sentiment. Overall, this recall impact measure seems to be consistent with heuristic evidence from

well-known recalls. Figure 6 shows the $Recall\ Impact_{kt}$ of three automakers that experienced well-publicized recalls: Toyota (gas pedal recalls of 2009-2011), GM (ignition switch recalls of 2014), and Volkswagen (emissions recalls of 2016). For example, the GM ignition switch issue was primarily focused on older cars, and so it should have less of an impact on sales according to our measure. By contrast, the Toyota gas pedal recall implicated newer cars, and so that characteristic, coupled with intense news coverage, causes the Toyota index to jump substantially more than does the GM recall in 2014.

Finally, we define our main variable, $1\text{-}Mo\ Post\text{-}Orig\ Recall\ Impact_{it}$, for loan i in month t as the recall impact of i's automaker k in month $t+1$, that is one month after loan i's origination.

$$1\text{-}Mo\ Post\text{-}Orig\ Recall\ Impact_{it} = Recall\ Impact_{ik,t+1} \tag{3}$$

Notice that, to strengthen the "surprise" element, we use the recall impact one month after the loan origination in our specification. By doing so, we eliminate any possible contamination of the coefficient from demand or other effects resulting from the recall. As in the baseline estimation, we keep our original proxies for soft and hard information.

We run our baseline equation, now adding this recall impact index. Table 6 Panel A presents the results. In Column (1), we run the regression on all loans save those extended by captive auto finance companies and, in Column (2), we run the same regression only on captive auto loans. Consistent with our hypothesis, Column (1) shows that warehousing times shorten for loans associated with recalls. In terms of economic magnitude, a 10% increase in the 1-month post origination recall impact results in around 7 days shorter warehousing time. The result is statistically significant at the 5 percent level.

Interestingly, in line with our signaling hypothesis, we see no differential effect on warehousing times when we run the same regression on loans whose lenders were captive auto finance companies, in Column (2). The absolute majority of auto loans extended by captive auto finance companies are made on their own cars. By contrast, other types of lenders extend loans on various makes of cars. Because investors are aware of this, they will immediately expect an ABS security issued by the recall-implicated automaker to include loans extended on the affected cars. But, in ABS securities issued by non-captive auto lenders, investors will be less certain regarding the extent of an auto

recall's impact on the collateral pool. As such, captive auto lenders do not need to signal, while other types of lenders do. In our regressions, this difference manifests as a significant coefficient on our recall impact variable only for the sample of non-captive auto loans.

5.2 Variations in Borrower Quality: "Super Sales"

Our second experiment uses "super sales," or periods of high incentives for car sales. In these super sales periods, lenders may be more willing to extend loans to unobservably worse borrowers in order to meet sales targets. Importantly, origination months are not summarized on auto ABS prospectuses. As a result, this information is both costly to obtain for inattentive investors and can proxy for private information. Our hypothesis is, then, that the month of origination could proxy for soft information the lender has about the borrower's credit quality.

To define periods of super sales, We identify months of the year that usually have substantial rebates and incentives for car purchases: Memorial Day (May), Black Friday (November), and end-of-year (December). We create two dummy variables, "Holiday" and "End-of-year". These indicators are equal to 1 if the loan was extended either in May or November, and in December, respectively, and 0 otherwise.

Table 6 Panel B summarizes the results. The results suggest that warehouse times are significantly shorter for loans extended during the super sale periods. For end-of-year origination, warehousing times are trimmed by nearly 50 days. Loans extended during holiday sales also experience nearly two weeks shorter warehousing periods than the average loan. Both effects are also statistically significant at the 1 percent level. Importantly, these findings are conditional on the key underwriting variables including the primary underwriting variables of credit score and payment-to-income ratio. It implies that during periods of heightened desire for sales, lenders may provide funds for borrowers who, while observationally equivalent to other borrowers in hard characteristics, would not necessarily get a loan during normal times. Consequently, for the market to price the securities accurately, lenders would need to signal the quality of their collateral.

5.3 Variations in Borrower Quality: Model Changes

Similar to the second experiment, our final experiment uses another instance when there could be extraordinarily heightened incentives for car sales — when there are major model changes.

To clear inventory of older models in advance of the introduction of new ones, automakers often take aggressive marketing tactics to sell older models. Our hypothesis is that, similar to the second experiment, lenders may be more willing to extend loans to unobservably worse borrowers in advance of the new model introduction.

To investigate this possibility, we gather 240 major model updates on 44 makes that took place during 2014-2018, from Kelley Blue Book.[13] Because data on exact start dates of the new model sale is not available (we only know the years of the major model updates), we approximate these dates by taking the earliest date of loan origination on the corresponding model that we observe in our data. We then define a dummy variable, "Model Update," that is equal to 1 if the loan was originated prior to a model update associated with the car, and 0 otherwise. Because our new model sale dates are estimates, we explore different windows of 1, 3, and 6 months prior to the first sale of each new model.

Table 7 displays the results. Conditional on our baseline specification plus the control for model update, if a loan was originated one month prior to a model update, the loan's warehousing time decreased by roughly 4 days, or 0.13 standard deviations. The effect is statistically significant at the 1 percent level. Loans originated significantly before a model update, for instance, 3 months and 6 months before, do show shorter warehousing times in Column (2) and (3), but the effects are not statistically significant. More generally, our findings imply that the heightened incentive for older model sales seems to be strongest closer to the immediate month of the new model sale.

6 Post-Securitization Loan Performance

Our baseline results found evidence on asymmetric information and signaling based on soft information observed by the lenders. The premise was that the proxies we used for soft information are indicative of some positive or negative borrower/collateral quality. In this section, we check the validity of our premise by examining if our *ex ante* proxies for soft information on borrower and collateral quality are consistent with *ex post* loan outcomes.

We start by measuring a pool-k securitized loan i's signaling of private information, $\hat{S}_{ikt^o}$, as

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[13]To the best of our knowledge, these updates represent "major" model updates that generally occur every three to five years, not minor updates that occur annually.

follows:

$$\hat{S}_{ikt^o} = \hat{\beta}_{11} \times Income\,Verified_{ikt^o} + \hat{\beta}_{12} \times Exception_{ikt^o} + \hat{\beta}_{13} \times Used\,Car_{ikt^o} \qquad (4)$$

We use the estimated coefficients, $\hat{\beta}_{11}$, $\hat{\beta}_{12}$, and $\hat{\beta}_{13}$, from the baseline estimation in Table 2 Column (3). In a nutshell, a higher $\hat{S}_{ikt^o}$ implies signaling of positive soft information on loan i (shown through the lender's longer warehousing). On the contrary, loans with low $\hat{S}_{ikt^o}$ indicate that those loans could be of unobservably worse quality by, for instance, not being verified of income or not having any exceptionally good credit factors, or by being extended on an used car.

Using the full panel data tracking each loan i securitized in pool k, we examine the relationship between the loan's summary statistic on signaling, $\hat{S}_{ikt^o}$, with its post-securitization principal charge-off. In general, if a borrower remains delinquent after extensive collection efforts, lenders net the sale proceeds from the repossessed vehicle, *etc.*, from the remaining loan amount and charge off the remaining balance owed by the borrower. Afterwards, the loan is usually replaced with another loan of a similar risk profile. Our measure of charge-off is, in particular, useful in learning about the intensive margins of loan performance because it continuously captures the dollar amount charged off, in addition to the extensive margins such as becoming a delinquent account. Furthermore, the measure also tracks the amount of the principal that is charged off due to loan modification.

Table 8 Panel A reports results from estimating a logit model of post-securitization probability of principal charge-off and Panel B reports those from running an OLS regression of the dollar amount of principal charge-off on $\hat{S}_{ikt^o}$. The outcomes show that our summary statistic for signaling is highly associated with the loan's probability of being charged off at 3 months of post-securitization and the associated amount is substantial. A one standard deviation increase in $\hat{S}_{ikt^o}$ lowers the odds ratio of charge-off in 3 months by 41.3%, or in dollar amounts, $1,562 less charged-off dollars from the principal loan amount. Since the average loan in our sample is $23,978, it implies that the economic magnitude is tantamount to nearly 7 percent of the original loan amount. The effects are similar and even tend to slightly increase in terms of charge-off probability when charge-off is defined based on longer windows of time since securitization. All effects are statistically significant at the 1 percent level.

Outcomes on hard loan characteristics are generally in line with our expectations, and are

economically meaningful. Loans with higher original interest rate, higher payment-to-income ratio, lower original loan amount, and/or higher term have higher probabilities of principal charge-off. A one standard deviation increase in credit score (96 points), payment-to-income ratio (4%), original interest rate (7%), and loan term (8 months) raise the odds ratio of loan's principal charge-off by 17.8, 8.9, 64.2, and 20.6 percent, respectively. A one standard deviation increase in original loan amount ($10,255) decreases the odds ratio by 18.1 percent.

7   Alternative Explanations

While we believe that our results are consistent with signaling in the auto ABS market, there may be alternative explanations. For example, our results could simply represent a "market for lemons": the bad loans, even though they are securitized faster, are priced the same as the better loans. This would be a factor if the information content implicit in the loans is not priced. In this section, we take two strategies to rule out these competing hypotheses. The first explores differences in warehousing times according to originator type. Originators face different funding costs—banks have deposit bases and so likely have lower funding costs, while non-banks and captive automakers rely on wholesale short-term funding. The second investigates pricing implications on the ABS pools. Both sets of results seem to point to signaling.

7.1   Funding Costs

As discussed in Section 3, there are different types of auto loan originators. One key distinguishing feature of these lenders is their funding costs. Because banks have a deposit base, their funding costs are usually considered to be lower than those of non-banks. On the other hand, non-bank lenders rely more heavily on short-term wholesale funding and debt financing, compared to banks. These alternative funding sources are often expensive, and in case of short-term funding, flighty. Therefore, this difference in funding costs across lender types may result in a difference in the average warehouse time of loans.

Table 9 indeed shows that warehousing times are longer for bank loans in general, compared to loans extended by captive auto finance companies and other non-bank auto finance companies. While the overall average warehousing time is 15 months for the whole sample, loans made by

captive autos have one month longer warehousing, at 16 months. Banks follow with 13 months of average warehousing. However, there is an important difference between warehousing times for Santander and other banks. In particular, Santander tends to focus on the subprime market, which has lower quality borrowers. Splitting the bank sample into two groups—one without Santander, and Santander itself—reveals that banks excluding Santander have the longest warehousing times on average. Non-bank finance companies have a shorter-than-average warehousing time of 12 months.

As discussed in (Leland and Pyle, 1977), the decision to self-invest by lengthening warehousing times depends on the relative risk pricing of good versus bad loans in the market, versus the costs of warehousing. If lender warehousing costs are important, then banks should be able to signal better loans through longer warehousing times than the non-banks. In particular, some nonbanks likely obtain funding from warehouse lenders. These lenders sometimes require that loans are securitized within a specific window. As such, it may be more expensive for nonbank lenders to fund loans for a longer time than banks do.

To evaluate this possibility, we explore the baseline specification in equation (1), but limit our samples to one lender type only. Table 10 presents the results. Columns (1)-(3) each report results from running the regression on the sub-samples of loans extended by banks, captive autos, and other non-banks. Looking across the lender types, all else equal, and for any one element of soft information, banks appear to shift warehousing times based on soft information most significantly. Banks warehouse loans whose obligor income has been verified for roughly 4 days longer, or 0.12 standard deviations. The effect is about half of that of the baseline result but is statistically significant at 1% level. Neither captive autos nor other non-bank finance companies has a significant coefficient on the indicator for income verified loans. Furthermore, for any single instance of underwriting exception, banks warehouse loans for around 17 more days, or 0.55 standard deviations. The economic magnitude is five times as long as that of captive autos. Loans made on used car have the least effect, with around 2 more days in banks' warehousing time. Again, non-bank lenders do not show any significant effects of used car loans on warehousing period. Our results suggest that warehousing times are consistent with funding cost differentials, further bolstering our signaling argument.

7.2 Pricing Implications

Another competing hypothesis is that our results capture only adverse selection, and not signaling. If this were the case, then we would assume investors to view the market as one for "lemons," where pooling occurs at an equilibrium in which only the lower-quality loans are sold, and the better ones are retained.

To investigate this possibility, we turn to an analysis of ABS pricing. We aggregate our loan-level data to the ABS pool-level. We define a pool k-level summary statistic of signaling based on private information by taking a weighted average of $\hat{S}_{ikt^o}$ of all loans securitized in pool k, where $\hat{S}_{ikt^o}$ is as defined in Equation (4):

$$
\begin{aligned}
\hat{S}_{kt} &= \sum_{i \in k} \omega_{ikt} \hat{S}_{ikt^o} \\
&= \sum_{i \in k} \omega_{ikt} \times (\hat{\beta}_{11} \times Income\,Verified_{ikt^o} + \hat{\beta}_{12} \times Exception_{ikt^o} + \hat{\beta}_{13} \times Used\,Car_{ikt^o})
\end{aligned}
$$

$$(5)$$

ω_{ikt} is the origination loan amount share of loan i in pool k. A higher $\hat{S}_{kt}$ implies that the pool k contains a higher share of unobservably good quality loans — in form of loans that are verified of income, that have exceptionally good credit factors, or that are made on new cars.

Table 11 describes the pool-level summary statistics of 89 auto ABS securities included in our sample. $\hat{S}_{kt}$ exhibits a high variation across the pools. While the average pool in our data has near zero change in warehousing time arising from soft information, the standard deviation of warehousing time change due to soft information is around 4 days. Among all pools, the signaling of the highest quality loans results in up to seven days longer warehousing times; the lowest quality loans experience up to a nine days shorter warehousing time. The average warehouse time of loans also greatly varies from 2 months to around 2 years across the pools. In addition to soft information, hard information can be associated with wider or narrower ABS spreads over comparable maturity riskless securities. Therefore, we also take into account other pool-level characteristics such as average maturity and credit score of the loans in the pool.

Based on the pool-level characteristics and our summary measure of signaling, $\delta_1 \hat{S}_{kt}$, we evaluate

the following specification:

$$r_{kt} = \delta_0 + \delta_1 \hat{S}_{kt} + \delta_2 X_{kt} + \gamma_t + \mu_{kt}. \tag{6}$$

Our dependent variable r_{kt} is the average yield spread of pool k issued at time t over comparable 5-year Treasury securities. X_{kt} is a vector of pool k characteristics at time t, including k's face amount and number of tranches, and the number and the average credit score of the loans included in k. γ_t is issuance time fixed effects. μ_{kt} is an error term. We control for the year and quarter of securitization to account for secular trends in the demand for auto ABS securities. We also control for lender type fixed effects to control for different demand and appetite for certain type's securitization (for instance, Santander's sub-prime auto ABS). We bootstrap the standard errors to control for understatement of the standard errors as a result of including our summary measure for signaling, $\hat{c}_{kt}$, as one of the right-hand side variables.

Table 12 presents our pricing analysis results. Consistent with our signaling hypothesis, we find that soft information is priced. In Column (1), if $\hat{S}_{kt}$ increases, all else equal, spreads on ABS fall. The effect is statistically significant at the 5% level. Economic magnitude is meaningful as well; a one standard deviation increase in $\hat{S}_{kt}$ is associated with a 0.15 percentage point, or a 0.39 standard deviations, decrease in the ABS spread.

In addition to $\hat{S}_{kt}$ — the weighted average of $\hat{S}_{ikt^o}$ of all loans in pool k — in Column (1), we explore other types of summary statistic for $\hat{S}_{ikt^o}$ in Columns (2)-(4), in terms of median, minimum, and standard deviation of $\hat{S}_{ikt^o}$. Results are similar for the median and the minimum levels of signaling for pool k collateral. As the median and the minimum $\hat{S}_{ikt^o}$ in pool k increase by one standard deviation, ABS spreads decrease by 0.08 and 0.03 percentage points respectively, or 0.23 and 0.08 standard deviations. The effect in terms of minimum $\hat{S}_{ikt^o}$ is statistically significant at the 5% level. The strongest economic effect is for the standard deviation of loan-level signaling in Column (4). As the pool's standard deviation of $\hat{S}_{ikt^o}$ increases by one (across-pool) standard deviation, the spread decreases by 0.24 percentage points, or 0.63 standard deviations. The result is consistent with self-financing signaling. Even if there is still some pooling in the pricing of the ABS, the existence and signaling of the higher-quality loans leads investors to view the overall market as favorable.

Outside of the private information, we find that various measures of public information are also

priced, in expected ways. Larger loans and higher credit scores tend to receive favorable pricing, though the size of the loan lacks in statistical significance. Results on number of tranches are neither economically nor statistically significant. Excessive tranching can indicate less self-financing and results in wider spreads.

8   Conclusion

In this paper, we leverage two different datasets with varying levels of information disclosure to define soft and hard information on auto loans. Such distinction allows for a direct measurement of asymmetric information and exploration of the effects. Our results suggest that asymmetric information is alive and well in the auto ABS market, despite the new post-crisis reporting requirements. While most previous studies find asymmetric information in residential mortgage markets, this paper underpins the inherent nature of asymmetric information in, more broadly, securitization by finding such evidence based on the data that only includes publicly-placed auto ABS, which are considered to comprise the most highly-rated securities.

Overall, this work brings to the forefront the question of whether securitization is beneficial risk-sharing, or if it is inherently risky money creation. Reporting requirements can eliminate some, but not all, risky aspects of securitization. As such, securitization is inherently risky, and should continue to be monitored. Our results can inform other regulatory initiatives surrounding securitization.

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Figures and Tables

Figure 1: Auto Loans Outstanding
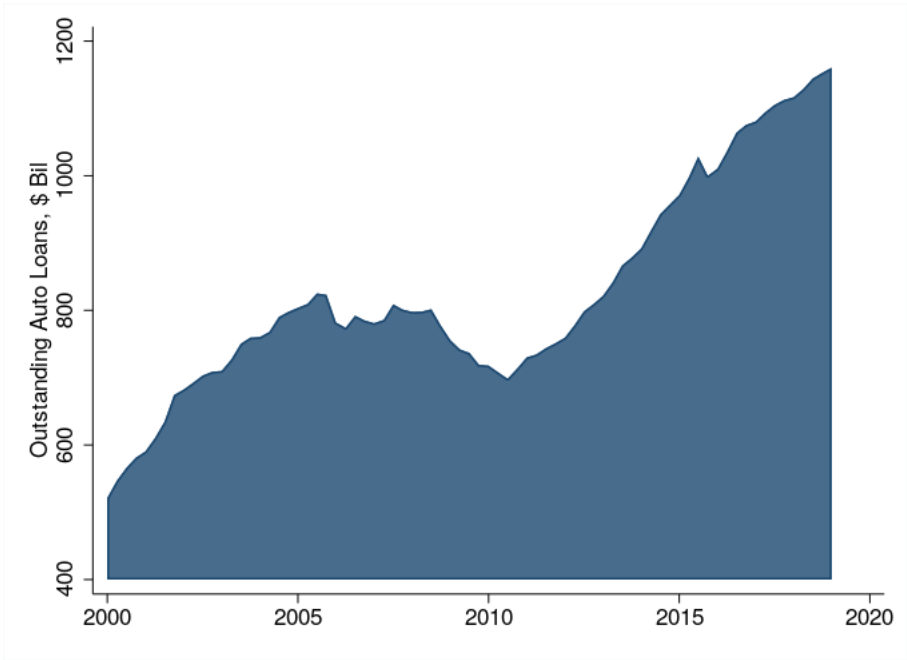
Source: Federal Reserve Board Statistical Release G.19, Consumer Credit, Automobile Loans.

Figure 2: Distribution of Warehoused Months
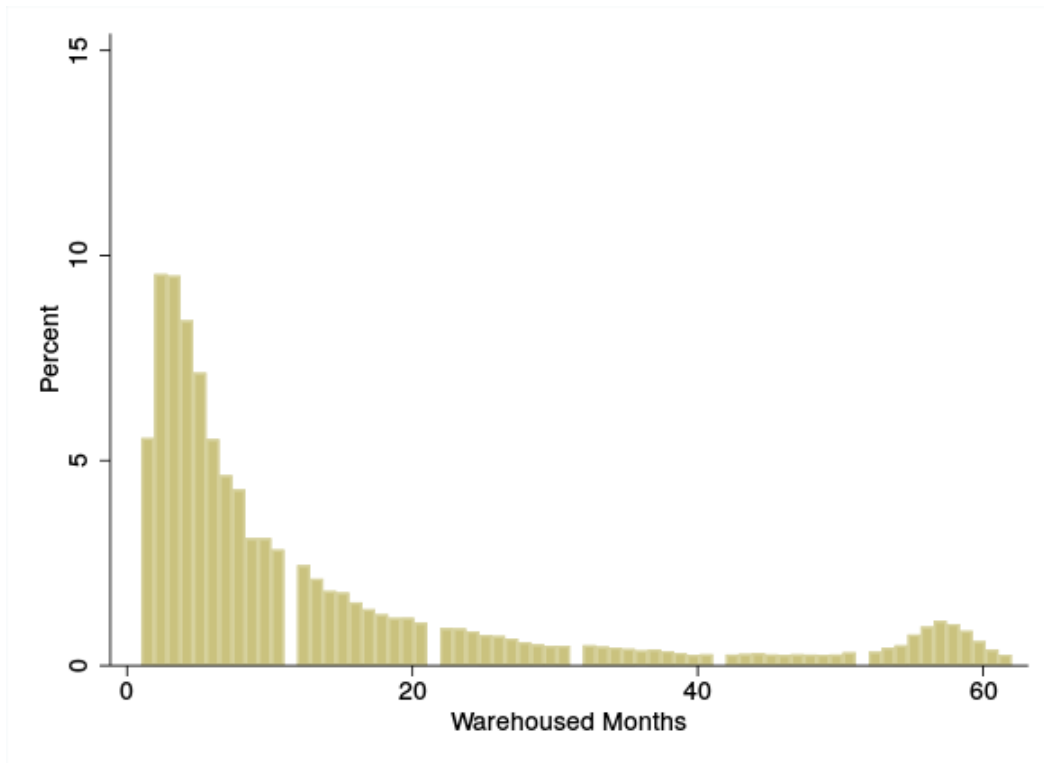
Note: This figure displays the distribution of warehoused months of loans in our dataset. The dataset comprises the universe of public auto ABS issued from 2017 Q1 to 2019 Q1, consisting of 5,930,935 loans and 89 ABS securities publicly issued by 15 issuers. Warehoused months are winsorized at bottom and top 1%. Source: SEC form ABS-EE.

Figure 3: Number of Loans by Origination Year and Lender Type, 2010-2018
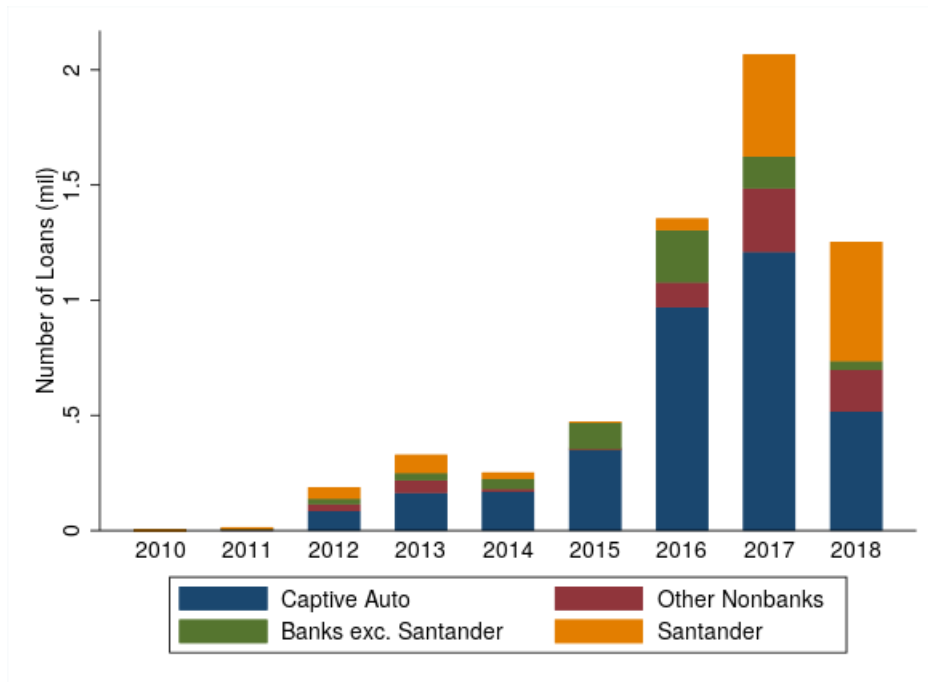
Note: This bar chart shows the total number of loans originated in each year from 2010 to 2018 by each lender type: captive auto finance companies (BMW, Daimler, Ford, GM, Honda, Hyundai, Nissan, Toyota, and Volkswagen), other non-bank finance companies (Carmax), banks exluding Santander (Ally, California Republic, Fifth Third, and USAA), and Santander.

Figure 4: Description of Pool Composition on Auto ABS Prospectuses: An Example

Composition of Receivables

The following tables show the characteristics or distributions of some characteristics of the pool of receivables on the cutoff date. The percentages in the following tables may not sum to 100.00% due to rounding.

Number of Receivables		64,956
Initial Pool Balance	$	1,727,999,800.87
Principal Balance:		
Average	$	26,602.62
Highest	$	99,451.05
Lowest	$	252.04
Original Amount Financed:		
Average	$	33,309.43
Highest	$	128,619.03
Lowest	$	1,018.31
Annual Percentage Rate (APR):		
Weighted average[1]		3.55%
Highest		21.50%
Lowest		0.00%
Original Term[2]:		
Weighted average[1]		65.2 months
Original term greater than 60 months (by principal balance)		57.59%
Longest		72 months
Shortest		12 months
Remaining Term[2]:		
Weighted average[1]		57.0 months
Remaining term greater than 60 months (by principal balance)		42.35%
Longest		72 months
Shortest		2 months
Scheduled Weighted Average Life[2][3]		2.49 years
Weighted Average Months After Origination (Seasoning)[1][2]		8.2 months
Credit Score:		
Weighted average[1] FICO® score[4] at origination		737
Weighted average[1] FICO® score[4] at origination for receivables with original terms greater than 60 months[2]		718
Percentage FICO® score[4] less than 650 (by principal balance)		14.36%
Percentage No FICO® score consumer[5] (by principal balance)		1.16%
Weighted Average[1] LTV[6] at Origination		96.78%
Weighted Average[1] PTI[7] at Origination		8.62%
Financed Vehicle — Subvened-APR Receivables[8]:		
Aggregate principal balance	$	947,688,833.90
Percentage of initial pool balance		54.84%
Financed Vehicle — Commercial Use[9]:		
Aggregate principal balance	$	384,298,039.31

Note: This example, from Ford Credit Auto Owner Trust 2018-A, shows a page from an auto ABS prospectus where the issuer discloses summary statistics of various characteristics on their collateral pool. Source: SEC EDGAR.

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Figure 5: Warehousing of Loans When Some Information is Unavailable on Deal Prospectuses
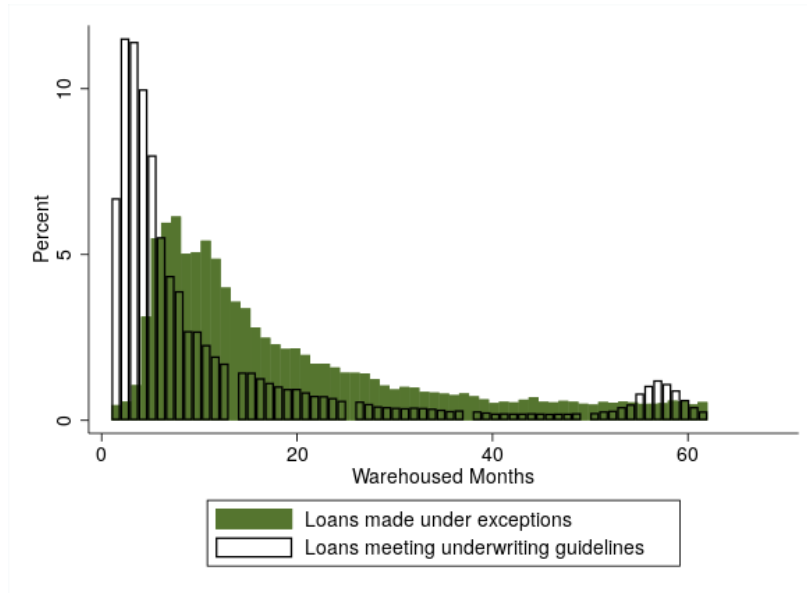
(a) "Exception" vs. "Normal" Loans
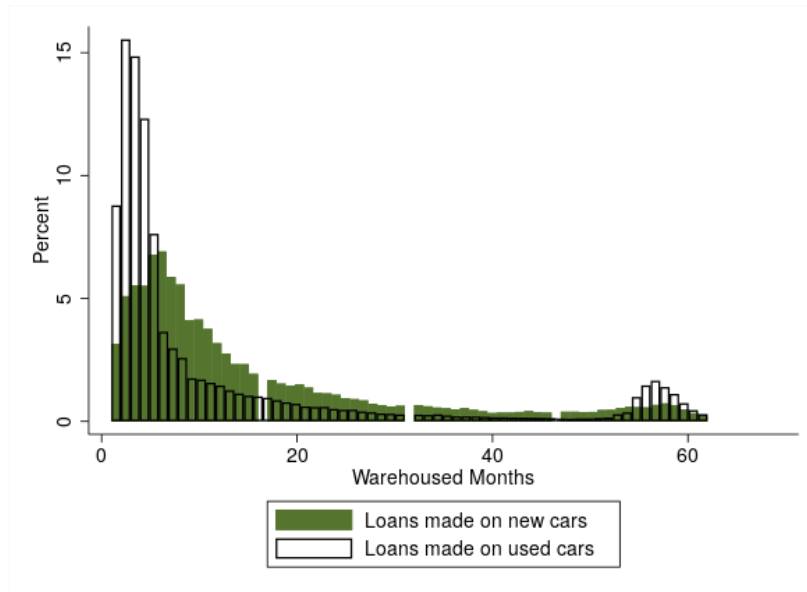
(b) New Car vs. Used Car Loans

Note: This figure shows a shift in the empirical distribution of warehoused months in an instance when only the lender knows that a loan is of good quality, *i.e.*, when the lender has private information that the loan is good. Subfigure (a) compares loans that were of exceptionally good quality and therefore extended under an underwriting exception ("Exception" loans), to loans extended without any exception ("Normal" loans). Subfigure (b) compares loans made on new cars against those made on used cars. The sample comprises of 5,930,935 loans collateralized in 89 public auto ABS issued by 15 issuers from 2017 Q1 to 2019 Q1. Warehoused months are winsorized at bottom and top 1%. Source: SEC form ABS-EE.

Figure 6: Recall Impacts of Select Auto Manufacturers, 2009-2018
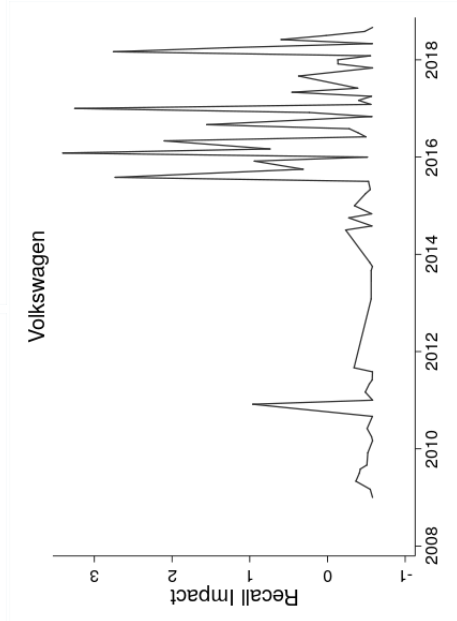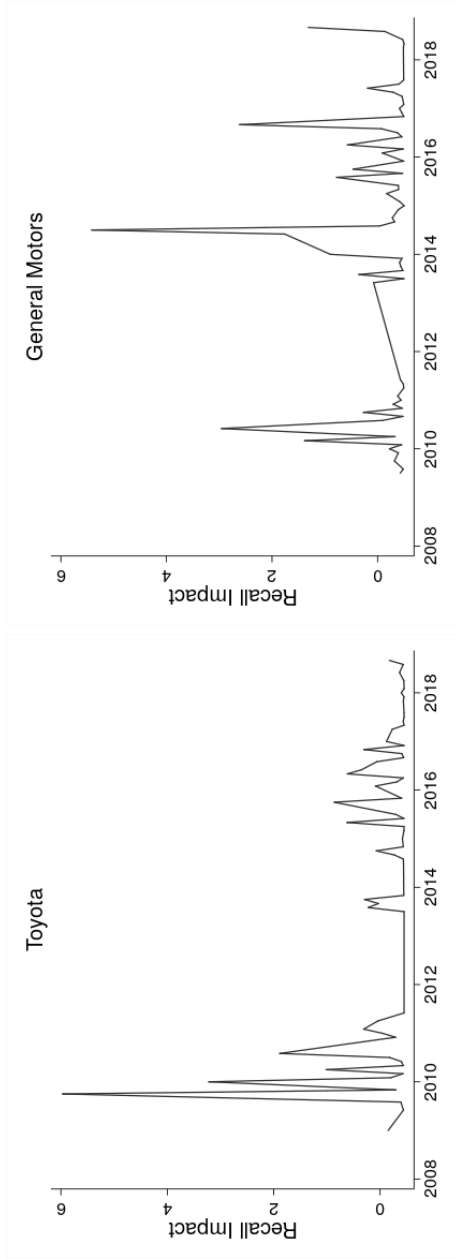
Figure 7: Note: These figures show the "Recall Impact" variable for Toyota, General Motors, and Volkswagen ("i") during 2009-2018 ("t", monthly), and captures the notorious recall incidents including Toyota gas pedal (2009-2011), GM ignition switch (2014), and Volkswagen diesel emissions (2016). Recall Impact is defined as $Recall\,Impact_{kt} = ln(Scaled\,Affected\,Units_{kt} * News\,Volume_{kt}/Sentiment_{kt})$ for an automaker k in month t. $Scaled\,Affected\,Units_{kt}$ adjusts the impact of each recall by scaling the affected units of recalled vehicles by the estimated number of the vehicles of recalled model-year that remain on the road at the time of the recall. $News\,Volume_{kt}$ and $Sentiment_{kt}$ each comes from Ravenpack News Analytics' "Aggregate Event Volume" and "Aggregate Event Sentiment," conditional on the relevance of the news event being larger than 75 (on a 1-100 scale); each measure the volume of events reported in financial news medium and the share of positive events, on a 91-day rolling basis. Source: National Highway Traffic Safety Administrations Office of Defects Investigation, IHS Markit's New Vehicle Registration, Polk's Vehicles in Operation, and Ravenpack News Analytics.

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Table 1: Summary Statistics of Borrower and Collateral Characteristics at Origination

	Mean	Stdev	Min	Max
Original Interest Rate	0.08	0.07	0.01	0.27
Original Loan Amount ($)	23,978	10,255	4,500	130,485
Original Loan Term (Months)	67	8	24	85
Obligor Credit Score	695	96	437	847
Vehicle Age (Years)	1.4	2.0	0.0	14.0
Used Car	0.43	0.49	0	1
Vehicle Value Amount ($)	25,427	11,524	4,539	135,310
Payment-to-Income Ratio	0.08	0.04	0.01	0.28
Income Verified	0.08	0.27	0.00	1.00
Employment Status Verified	0.07	0.25	0.00	1.00
Underwriting Exception	0.18	0.38	0.00	1.00
End-of-Year Origination	0.08	0.27	0.00	1.00
Holiday-Sale Origination	0.17	0.37	0.00	1.00

Note: This table presents summary statistics for our main variables. Our data comprises the universe of public auto ABS issued from 2017 Q1 to 2019 Q1, and consists of 5,930,935 loans and 89 ABS securities publicly placed by 15 issuers. "Underwriting Exception" indicates if the loan was an exception to underwriting criteria. "End-of-Year Origination" indicates if the loan was originated at the end of a year (defined here as in December). "Holiday-Sale Origination" indicates if the loan was originated in the months of May (Memorial Day sale) or November (Black Friday). "Used Car," "Income Verified," "Employment Status Verified," "Underwriting Exception," "End-of-Year Origination," and "Holiday Sales" each indicate a share of loans as a percentage satisfying each of the categorical conditions. Loans with "Original Interest Rate" over 100% or "Payment-to-Income Ratio" over 0.5 are dropped. "Original Interest Rate," "Original Loan Amount," "Original Loan Term," "Obligor Credit Score," "Vehicle Age," "Vehicle Value Amount," and "Payment-to-Income Ratio" are winsorized at each originator's bottom and top 1%. Source: SEC form ABS-EE.

Table 2: Asymmetric Information on Borrower and Collateral Characteristics

	Dependent Variable: Warehoused Months				
	(1)	(2)	(3)	(4)	(5)
"Soft" Information					
Income Verified	0.311	0.209**	0.223**	0.232***	0.0876
	(0.382)	(0.095)	(0.093)	(0.093)	(0.094)
Underwriting Exception	2.171***	0.496***	0.416***	0.397**	0.360***
	(0.681)	(0.099)	(0.100)	(0.096)	(0.093)
Used Car	0.012	-0.291***	-0.247***	-0.252***	-0.0106
	(0.416)	(0.084)	(0.081)	(0.080)	(0.044)
"Hard" Information					
Obligor Credit Score	0.00400***	0.000492**	0.000402*	0.000402**	0.000287
	(0.00113)	(0.000222)	(0.000211)	(0.000194)	(0.000197)
Payment-to-Income Ratio	0.139	-0.084	-0.108	-0.123	-0.024
	(1.097)	(0.172)	(0.131)	(0.117)	(0.148)
Original Interest Rate	-13.85***	0.122	0.113	-0.077	-1.490
	(3.109)	(1.174)	(1.162)	(1.122)	(1.073)
Original Loan Amount	3.22e-05*	-1.63e-06	6.89e-07	1.26e-08	-3.31e-06*
	(1.63e-05)	(1.80e-06)	(1.20e-06)	(1.03e-06)	(1.73e-06)
Original Loan Term	0.0356**	-0.00309	-0.00229	-0.00175	-0.00143
	(0.0148)	(0.00192)	(0.00161)	(0.00147)	(0.00185)
Geographic Location FE	X	X	X	X	X
Origination Y-Q FE	X	X	X	X	X
Securitization Y-Q FE		X	X	X	X
Vehicle Manufacturer FE			X		
Vehicle Make FE				X	
Lender Type FE					X
Observations	4,535,240	4,535,240	4,533,466	4,535,240	4,535,240
R^2	0.938	0.996	0.996	0.996	0.997

Note: This table reports results from regressions of loan's number of months for which it had been warehoused, on various independent variables of interest, controlling for public loan characteristics ("Original Interest Rate," "Original Loan Amount," "Original Loan Term," "Obligor Credit Score," and "Payment-to-Income Ratio"). "Income Verified" is a dummy variable that gives a value of 1 if the obligor has been verified of income at origination, and 0 otherwise. "Underwriting Exception" is a dummy variable that gives a value of 1 if the loan was an exception to underwriting criteria and 0 otherwise. "Used Car" is a dummy variable that gives a value of 1 if the vehicle at origination is a used car and 0 otherwise. For fixed effects, lender types are grouped by captive auto finance companies, other non-bank finance companies, and banks. All standard errors are clustered by ABS. Results using an alternative clustering by origination year-quarter are similar (unreported). ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Source: SEC form ABS-EE.

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Warehoused Months				

	(1)	(2)	(3)	(4)	(5)
"Soft" Information					
Income Verified	-0.405	0.0595	0.116	0.101	0.0363
	(0.441)	(0.104)	(0.0965)	(0.0935)	(0.113)
Underwriting Exception	1.873***	0.338***	0.197***	0.176***	0.330***
	(0.686)	(0.0879)	(0.0684)	(0.0617)	(0.0887)
"Hard" Information					
Obligor Credit Score	0.00593***	0.000410	0.000215	0.000240	0.000423
	(0.00144)	(0.000298)	(0.000239)	(0.000223)	(0.000287)
Payment-to-Income Ratio	0.0881	-0.0613	-0.0288	-0.0421	-0.0846
	(1.145)	(0.200)	(0.105)	(0.0927)	(0.171)
Original Interest Rate	-16.00***	-1.602	-1.854	-2.073*	-1.333
	(3.559)	(1.202)	(1.130)	(1.099)	(1.254)
Original Loan Amount	2.37e-05	-4.18e-06**	-2.99e-07	-8.31e-07	-4.24e-06**
	(1.47e-05)	(1.93e-06)	(8.00e-07)	(5.62e-07)	(1.94e-06)
Original Loan Term	0.0720***	-0.000827	0.00134	0.00216	-0.000853
	(0.0213)	(0.00232)	(0.00159)	(0.00137)	(0.00233)
Geographic Location FE	X	X	X	X	X
Origination Y-Q FE	X	X	X	X	X
Securitization Y-Q FE		X	X	X	X
Vehicle Manufacturer FE			X		
Vehicle Make FE				X	
Lender Type FE					X
Observations	2,286,735	2,286,735	2,286,479	2,286,735	2,286,735
R^2	0.925	0.996	0.996	0.996	0.996

Note: This table reports results from regressions of loan's number of months for which it had been warehoused, on various independent variables of interest, controlling for public loan characteristics ("Original Interest Rate," "Original Loan Amount," "Original Loan Term," "Obligor Credit Score," and "Payment-to-Income Ratio"), based on a sub-sample of loans originated on new cars. "Income Verified" is a dummy variable that gives a value of 1 if the obligor has been verified of income at origination, and 0 otherwise. "Underwriting Exception" is a dummy variable that gives a value of 1 if the loan was an exception to underwriting criteria and 0 otherwise. "Used Car" is a dummy variable that gives a value of 1 if the vehicle at origination is a used car and 0 otherwise. For fixed effects, lender types are grouped by captive auto finance companies, other non-bank finance companies, and banks. All standard errors are clustered by ABS. Results using an alternative clustering by origination year-quarter are similar (unreported). ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Source: SEC form ABS-EE.

Table 4: Robustness Analysis Using Post-2017 Originated Loans

	Dependent Variable: Warehoused Months				
	(1)	(2)	(3)	(4)	(5)
"Soft" Information					
Income Verified	0.571*	0.277***	0.292***	0.299***	0.0864
	(0.341)	(0.103)	(0.0990)	(0.0952)	(0.0876)
Underwriting Exception	2.478***	0.644***	0.587***	0.582***	0.462***
	(0.397)	(0.142)	(0.131)	(0.130)	(0.134)
Used Car	-0.521*	-0.261***	-0.248***	-0.254***	0.0423
	(0.277)	(0.0911)	(0.0867)	(0.0855)	(0.0522)
"Hard" Information					
Obligor Credit Score	0.00487***	0.000513**	0.000399*	0.000380*	0.000432*
	(0.000962)	(0.000235)	(0.000223)	(0.000225)	(0.000231)
Payment-to-Income Ratio	-0.607	-0.192	-0.237	-0.228	-0.0907
	(0.627)	(0.182)	(0.146)	(0.144)	(0.169)
Original Interest Rate	-16.24***	-1.332	-1.352	-1.434	-2.619***
	(2.186)	(1.166)	(1.126)	(1.089)	(0.952)
Original Loan Amount	1.52e-05*	1.15e-07	1.39e-06	9.39e-07	-2.40e-06
	(8.25e-06)	(2.01e-06)	(1.38e-06)	(1.40e-06)	(1.86e-06)
Original Loan Term	0.00725	-0.00197	-0.00166	-0.00134	-0.000104
	(0.00670)	(0.00176)	(0.00131)	(0.00125)	(0.00179)
Geographic Location FE	X	X	X	X	X
Origination Y-Q FE	X	X	X	X	X
Securitization Y-Q FE		X	X	X	X
Vehicle Manufacturer FE			X		
Vehicle Make FE				X	
Lender Type FE					X
Observations	2,658,123	2,658,123	2,657,791	2,658,123	2,658,123
R^2	0.439	0.924	0.925	0.925	0.930

Note: This table reports results from regressions of loan's number of months for which it had been warehoused, on various independent variables of interest, controlling for public loan characteristics ("Original Interest Rate," "Original Loan Amount," "Original Loan Term," "Obligor Credit Score," and "Payment-to-Income Ratio"), based on a sub-sample of loans originated after 2017. "Income Verified" is a dummy variable that gives a value of 1 if the obligor has been verified of income at origination, and 0 otherwise. "Underwriting Exception" is a dummy variable that gives a value of 1 if the loan was an exception to underwriting criteria and 0 otherwise. "Used Car" is a dummy variable that gives a value of 1 if the vehicle at origination is a used car and 0 otherwise. For fixed effects, lender types are grouped by captive auto finance companies, other non-bank finance companies, and banks. All standard errors are clustered by ABS. Results using an alternative clustering by origination year-quarter are similar (unreported). ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Source: SEC form ABS-EE.

Table 5: Auto Recalls and Average Impacts, 2009-2018

Automaker	Total Number of Recalls	Average Affected Units	Average Scaled Affected Units	Average News Volume	Average Sentiment	Average Recall Impact
Tesla	9	21,496	0.15	837.73	49.01	2.90
Toyota	100	265,081	0.01	1032.89	48.39	0.29
GM	144	164,953	0.00	1741.33	51.15	0.22
Honda	87	281,405	0.02	392.86	50.01	0.18
Volkswagen	121	39,944	0.01	980.46	49.35	0.17
Nissan	80	146,558	0.01	490.91	53.39	0.13
FCA	170	131,784	0.01	734.74	48.25	0.12
Ford	140	162,638	0.01	955.65	52.22	0.10
Hyundai	67	120,381	0.02	213.5	52.62	0.09
Kia	29	102,054	0.03	95.45	51.58	0.09
Suzuki	14	34,524	0.06	60.94	52.63	0.08
Mitsubishi	48	40,867	0.03	90.57	51.17	0.07
Aston Martin	7	1,411	0.05	53.95	48.62	0.06
BMW	100	47,684	0.01	260.25	53.99	0.06
Mclaren	2	241	0.11	31.98	61.42	0.06
Tata	54	7,470	0.01	343.24	55.4	0.06
Mazda	42	104,670	0.03	61.51	48.83	0.04
Daimler	289	7,816	0.00	416.39	54.98	0.01
Volvo	68	11,257	0.01	48.44	56.95	0.01
Subaru	30	102,760	0.04	8.07	45.82	0.00
Total	1,601					

Note: We collect data on all auto recalls from 2009 to 2018 for all automakers whose cars appear on our main dataset. "Affected Units," "Scaled Affected Units," "News Volume," "Sentiment," and "Recall Impact" are in averages. Average Affected Units are the average number of cars affected by the automaker's various recall instances. Average Scaled Affected Units adjust the impact of each recall by scaling the affected units of recalled vehicles by the estimated number of the vehicles of recalled model-year that remain on the road at the time of the recall. We winsorize observations at bottom and top 1% of scaled affected units to exclude outliers. Average News Volume and Average Sentiment Score each come from Ravenpack News Analytics' "Aggregate Event Volume" and "Aggregate Event Sentiment," conditional on the relevance of the news event being larger than 75 (on a 1-100 scale); each measure the volume of events reported in financial news medium and the share of positive events, on a 91-day rolling basis. Average Recall Impact is the average of the logarithm of Recall Impact defined as $Recall\ Impact_{it} = ln(Scaled\ Affected\ Units_{it} * News\ Volume_{it}/Sentiment_{it})$. Source: National Highway Traffic Safety Administrations Office of Defects Investigation, IHS Markit's New Vehicle Registration, Polk's Vehicles in Operation, and Ravenpack News Analytics.

Table 6: Exogenous Variations in Collateral/Borrower Quality: Auto Recalls and "Super Sales"

	Dependent Variable: Warehoused Months		
	Panel A: Originated Prior to Recall		Panel B: Super Sales
	(1) Non-Automakers	(2) Automakers	(3) All issuers
1-Mo Post-Orig Recall Impact	-0.0214**	-0.00698	
	(0.00998)	(0.0112)	
End-of-Year Origination			-1.705***
			(0.0389)
Holiday-Sale Origination			-0.390***
			(0.0628)
Income Verified	0.171***	0.0812	0.0227
	(0.0562)	(0.0635)	(0.0422)
Underwriting Exception	0.895***	0.118***	0.0294**
	(0.323)	(0.0433)	(0.0144)
Used Car	-0.176*	0.0249	0.0371***
	(0.0870)	(0.0230)	(0.0128)
Obligor Credit Score	0.000145	-4.84e-05	0.000119*
	(0.000236)	(0.000195)	(6.87e-05)
Payment-to-Income Ratio	-0.239*	0.0569	-0.0306
	(0.129)	(0.0823)	(0.0354)
Original Interest Rate	0.971	-1.041**	-0.972***
	(1.052)	(0.472)	(0.234)
Original Loan Amount	5.17e-08	-7.96e-07	4.68e-07**
	(1.60e-06)	(5.46e-07)	(2.22e-07)
Original Loan Term	-0.00122	0.000514	0.00123***
	(0.00255)	(0.00127)	(0.000464)
Geographic Location FE	X	X	X
Origination Y-Q FE	X	X	X
Securitization Y-Q FE	X	X	X
Vehicle Manufacturer FE	X	X	X
Observations	2,213,065	2,320,401	4,533,466
R^2	0.997	0.996	0.998

Note: This table reports results from robustness analyses using auto recalls and super sales. In Panel A, "1-Mo Post-Origination Recall Impact" is the logarithm of Recall Impact of the loan vehicle's automaker k during one month after the loan's origination, where Recall Impact is defined as $Recall\,Impact_{kt} = ln(Scaled\,Affected\,Units_{kt} * News\,Volume_{kt}/Sentiment_{kt})$ (see Data Appendix for more details). Column (1) shows results from a sub-sample of lenders that are not auto manufacturers and Column (2) shows results from the automakers. In Panel B, "End-of-Year Origination" and "Holiday-Sale Origination" each indicate whether the loan were originated at the end of a year (defined as in December) and or in months of May (Memorial Day sale) or November (Black Friday). "Income Verified" is a dummy variable that gives a value of 1 if the obligor has been verified of income at origination, and 0 otherwise. "Underwriting Exception" is a dummy variable that gives a value of 1 if the loan was an exception to underwriting criteria and 0 otherwise. "Used Car" is a dummy variable that gives a value of 1 if the vehicle at origination is a used car and 0 otherwise. All standard errors are clustered by ABS. Results using an alternative clustering by origination year-quarter are similar (unreported). ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Source: SEC form ABS-EE, National Highway Traffic Safety Administrations Office of Defects Investigation, IHS Markit's New Vehicle Registration, Polk's Vehicles in Operation, and Ravenpack News Analytics.

Table 7: Exogenous Variations in Borrower Quality: Model Updates

	Dependent Variable: Warehoused Months		
	(1) Originated -1 Mo	(2) Originated -3 Mo	(3) Originated -6 Mo
Model Update	-0.140***	-0.0306	-0.0268
	(0.0454)	(0.0393)	(0.0340)
Income Verified	0.213**	0.213**	0.213**
	(0.0901)	(0.0901)	(0.0901)
Underwriting Exception	0.376***	0.376***	0.376***
	(0.0901)	(0.0901)	(0.0901)
Used Car	-0.209***	-0.210***	-0.210***
	(0.0755)	(0.0755)	(0.0755)
Obligor Credit Score	0.000289	0.000289	0.000289
	(0.000206)	(0.000206)	(0.000206)
Payment-to-Income Ratio	-0.0995	-0.0996	-0.0995
	(0.127)	(0.127)	(0.127)
Original Interest Rate	-0.0750	-0.0709	-0.0718
	(1.160)	(1.160)	(1.160)
Original Loan Amount	5.17e-07	5.44e-07	5.42e-07
	(1.07e-06)	(1.06e-06)	(1.06e-06)
Original Loan Term	-0.00228	-0.00228	-0.00229
	(0.00161)	(0.00160)	(0.00161)
Geographic Location FE	X	X	X
Origination Y-Q FE	X	X	X
Securitization Y-Q FE	X	X	X
Vehicle Manufacturer FE	X	X	X
Observations	2,748,388	2,748,388	2,748,388
R^2	0.996	0.996	0.996

Note: This table reports results from robustness analyses using model updates. "Model Update" is a dummy variable that gives a value of 1 if the loan was originated 1, 3, and 6 months (Columns (1)-(3), respectively) prior to a model update on the model of the car associated with the loan, and 0 otherwise. "Income Verified" is a dummy variable that gives a value of 1 if the obligor has been verified of income at origination, and 0 otherwise. "Underwriting Exception" is a dummy variable that gives a value of 1 if the loan was an exception to underwriting criteria and 0 otherwise. "Used Car" is a dummy variable that gives a value of 1 if the vehicle at origination is a used car and 0 otherwise. All standard errors are clustered by ABS. Results using an alternative clustering by origination year-quarter are similar (unreported). ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Source: SEC form ABS-EE and Kelley Blue Book.

Table 8: Analysis of Post-Securitization Loan Performance

Post-Securitization	Panel A: Pr(Charge-off)			Panel B: Charged-off Principal Amount ($)		
	(1) +3-Month	(2) +6-Month	(3) +12-Month	(4) +3-Month	(5) +6-Month	(6) +12-Month
$\hat{S}_{ikt^o}$	-3.804***	-4.819***	-6.693***	-11154.640***	-11044.320***	-11073.890***
	(0.009)	(0.011)	(0.017)	(34.796)	(31.889)	(33.108)
Obligor Credit Score	0.001709***	0.002714***	0.003956***	3.183915***	3.834559***	4.291813***
	(0.000028)	(0.000033)	(0.000047)	(0.118503)	(0.117162)	(0.127389)
Payment-to-Income Ratio	2.129***	1.981***	2.141***	8521.455***	7425.434***	6680.906***
	(0.040)	(0.047)	(0.066)	(180.635)	(175.364)	(189.489)
Original Interest Rate	-14.655***	-12.659***	-11.483***	-40300.000***	-30376.070***	-24774.220***
	(0.059)	(0.068)	(0.094)	(224.950)	(223.957)	(242.752)
Original Loan Amount	-1.95E-05***	-1.81E-05***	-1.85E-05***	-6.58E-02***	-3.69E-02***	-3.01E-02***
	(1.84E-07)	(2.18E-07)	(3.14E-07)	(7.56E-04)	(8.01E-04)	(8.68E-04)
Original Loan Term	0.02337***	0.03308***	0.04037***	58.39854***	82.02102***	75.80086***
	(0.00021)	(0.00025)	(0.00036)	(0.94808)	(0.92599)	(0.98692)
Geographic Location FE	X	X	X	X	X	X
Origination Y-Q FE	X	X	X	X	X	X
Securitization Y-Q FE	X	X	X	X	X	X
Vehicle Manufacturer FE	X	X	X	X	X	X
Observations	2,588,328	2,047,686	1,211,694	2,475,795	2,012,456	1,209,821
(Pseudo) R^2	0.27	0.31	0.39	0.23	0.21	0.26

Note: This table reports results from logit regressions of post-securitization probability of default, repossession, and charge-off on $\hat{S}_{ikt^o}$, the loan-level summary statistic on signaling. We report coefficients using n 3-, 6-, and 12-month post-securitization loan outcomes. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Source: SEC form ABS-EE.

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Table 9: Distribution of Warehoused Months by Lender/Issuer Type

	Mean	Stdev	N
All issuers	15	16	5,930,935
Captive auto	16	15	3,470,732
Banks	13	18	1,798,168
Banks without Santander	19	16	621,601
Santander	10	19	1,176,567
Non-banks	12	19	662,035

Note: This table shows the average and the standard deviation of warehoused months by each lender/issuer type — captive auto finance companies (BMW, Daimler, Ford, GM, Honda, Hyundai, Nissan, Toyota, and Volkswagen), other non-bank finance companies (Carmax), banks exluding Santander (Ally, California Republic, Fifth Third, and USAA), and Santander. Source: SEC form ABS-EE.

Table 10: Asymmetric Information and Signaling by Lender Type

	Dependent Variable: Warehoused Months		
	(1) Banks	(2) Captive Autos	(3) Other Non-banks
"Soft" Information			
Income verified	0.125***	0.0816	-0.0147
	(0.0416)	(0.0634)	(0.0458)
Underwriting exception	0.571*	0.118***	
	(0.281)	(0.0433)	
Used	0.0665*	0.0244	0.0358
	(0.0367)	(0.0231)	(0.0202)
"Hard" Information			
Obligor Credit Score	-0.000212	-5.00e-05	-0.000230***
	(0.000306)	(0.000193)	(3.92e-05)
Payment-to-Income Ratio	0.0601	0.0591	-0.0735**
	(0.138)	(0.0828)	(0.0291)
Original Interest Rate	-1.359	-1.055**	0.133
	(1.118)	(0.477)	(0.117)
Original Loan Amount	1.15e-06	-8.22e-07	-4.48e-08
	(1.72e-06)	(5.56e-07)	(2.24e-07)
Original Loan Term	-0.00251	0.000598	0.00163***
	(0.00233)	(0.00124)	(0.000308)
Geographic Location FE	X	X	X
Origination Y-Q FE	X	X	X
Securitization Y-Q FE	X	X	X
Vehicle Manufacturer FE	X	X	X
Observations	1,613,667	2,320,401	599,398
R^2	0.997	0.996	0.999

Note: This tables shows the results of baseline estimation by each lender/issuer type — captive auto finance companies (BMW, Daimler, Ford, GM, Honda, Hyundai, Nissan, Toyota, and Volkswagen), other non-bank finance companies (Carmax), and banks (Ally, California Republic, Fifth Third, Santander, and USAA). "Income Verified" is a dummy variable that gives a value of 1 if the obligor has been verified of income at origination, and 0 otherwise. "Underwriting Exception" is a dummy variable that gives a value of 1 if the loan was an exception to underwriting criteria and 0 otherwise. "Used Car" is a dummy variable that gives a value of 1 if the vehicle at origination is a used car and 0 otherwise. All standard errors are clustered by ABS. Results using an alternative clustering by origination year-quarter are similar (unreported). ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Source: SEC form ABS-EE.

Table 11: Summary Statistics of ABS-level Characteristics

	Mean	Stdev	Min	Max
Average ABS Spread (%)	-0.06	0.38	-0.83	0.85
Face Amount ($ Mil)	1,200	310	421	2,230
Number of Loans	66,804	19,955	20,737	109,465
Number of Tranches	7	1	4	8
Average $\hat{S}_{kt}$ (Weighted)	-0.01	0.14	-0.25	0.29
Average Warehoused Months (Weighted)	14.16	5.92	2.16	23.91
Average Maturity (Weighted)	57	9	28	70
Average Credit Score (Weighted)	700	70	567	765
Total Number of ABS		87		

Note: We calculate the average spread by taking the unweighted average of coupon rates of different tranches within each pool and subtracting the 5-year Treasury yield. $\hat{S}_{kt}$ is a measure of weighted average signaling of loans in pool k, defined as follows:

$$\hat{S}_{kt} = \sum_{i \in k} \omega_{ikt} \hat{S}_{ikt^o}$$

$$= \sum_{i \in k} \omega_{ikt} \times (\hat{\beta}_{11} \times IncomeVerified_{ikt^o} + \hat{\beta}_{12} \times Exception_{ikt^o} + \hat{\beta}_{13} \times UsedCar_{ikt^o})$$

where ω_{ikt} is the origination loan amount share of loan i in pool k. We use the estimated coefficients, $\hat{\beta}_{11}$, $\hat{\beta}_{12}$, and $\hat{\beta}_{13}$, from the baseline estimation in Table 2 Column (3). Higher $\hat{S}_{kt}$ implies that the pool contains more unobservably good quality loans — in the form of loans that are verified of income, that have exceptionally good credit factors, or that are made on new cars. Source: Bloomberg ABS Backoffice, SEC form ABS-EE, and St. Louis FRED database.

Table 12: Pricing Analysis of Auto ABS

	Dependent Variable: Average Spread			
	(1)	(2)	(3)	(4)
$\text{Avg}(\hat{S}_{ikt^o})$ (Equally, $\hat{S}_{kt}$)	-1.065**			
	(0.494)			
$\text{Med}(\hat{S}_{ikt^o})$		-0.200		
		(0.274)		
$\text{Min}(\hat{S}_{ikt^o})$			-0.316**	
			(0.144)	
$\text{Stdev}(\hat{S}_{ikt^o})$				-1.722**
				(0.803)
Face Amount ($; Log)	-0.209	-0.132	-0.150	-0.197
	(0.249)	(0.274)	(0.246)	(0.244)
Number of Loans (Log)	-0.0612	-0.254	-0.136	-0.0744
	(0.234)	(0.224)	(0.219)	(0.236)
Average Credit Score (Weighted)	-0.00116*	-0.000840	-0.00123*	-0.00154**
	(0.000679)	(0.000696)	(0.000688)	(0.000780)
Number of Tranches	-0.0166	0.0195	0.0367	0.00112
	(0.0471)	(0.0438)	(0.0406)	(0.0452)
Securitization Y-Q FE	X	X	X	X
Lender Type FE	X	X	X	X
Observations	87	87	87	87
R^2	0.711	0.685	0.701	0.705

Note: We calculate the average spread by taking the (unweighted) average of coupon rates of different tranches within each pool and subtracting the 5-year Treasury yield. $\hat{S}_{ikt^o}$ is a measure of signaling on loan i in pool k, defined as follows:

$$\hat{S}_{ikt^o} = \hat{\beta}_{11} \times Income\,Verified_{ikt^o} + \hat{\beta}_{12} \times Exception_{ikt^o} + \hat{\beta}_{13} \times Used\,Car_{ikt^o}$$

We use the estimated coefficients, $\hat{\beta}_{11}$, $\hat{\beta}_{12}$, and $\hat{\beta}_{13}$, from the baseline estimation in Table 2 Column (3). Higher $\hat{S}_{ikt^o}$ implies that the loan i is of unobservably good quality than other loans by being verified of income or having exceptionally good credit factors, or by being made on a new car. Columns (1)-(4) each takes the weighted average, median, minimum, and standard deviation of $\hat{S}_{ikt^o}$ within the pool. For fixed effects, lender types are grouped by captive auto finance companies, other non-bank finance companies, banks excluding (subprime-focused) Santander, and Santander. All standard errors are bootstrapped. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Source: Bloomberg ABS Backoffice, SEC form ABS-EE, and St. Louis FRED database.

Appendix

A. Representations and Warranties: "No Adverse Selection" in US Auto ABS Market

In August 2014, the SEC amended Rule 17g-7 so as to require nationally recognized statistical rating organizations (NRSROs), when assigning a credit rating to an asset-backed security, to disclose: "(1) the representations, warranties, and enforcement mechanisms available to investors which were disclosed in the prospectus, private placement memorandum, or other offering documents for the asset-backed security and that relate to the asset pool underlying the asset-backed security; and (2) how they differ from the representations, warranties, and enforcement mechanisms in issuances of similar securities."

One of the representations and warranties typically addressed in US auto ABS deals is that there was no adverse selection in the creation of the pool of loans. Fitch Ratings describes it as: "No Adverse Selection: No selection procedures believed to be adverse to the noteholders have been utilized in selecting the receivables from other receivable of the sponsor that meet the criteria specified in the transaction documents."[14]

However, the SEC's rule only requires the rating agencies to disclose information on reps and warranties based on the offering and transaction documents provided by the ABS issuer to the rating agency. An example of this disclosure is:

> *..Moody's has not undertaken any other investigation into the accuracy of the issuer's statement.*
>
> In rating the Transaction, Moody's evaluates the representations, warranties and enforcement mechanisms contained in the offering and transaction documents solely as and to the extent described in its rating criteria. Further, Moody's rating may depend significantly on factors other than such representations, warranties and enforcement mechanisms. Moody's does not in this 17g-7 Report provide any opinion or recommendation as to the adequacy or effectiveness of the representations, warranties and enforcement mechanisms described herein (whether with respect to the Transaction or the Benchmark). *Investors must conduct their own analysis of the adequacy and effectiveness, and of the legal and other implications, of the representations, warranties and enforcement mechanisms in the Transaction.*[15]

Because rating agencies do not necessarily conduct their own analysis to confirm on reps and warranties, it ultimately falls upon the due diligence of deal issuers to ensure appropriateness of reps and warranties. A part of such reps and warranties-related information disclosed by the issuer to the rating agency can come from the deal prospectus. In addition to summary statistics of the pool as in Figure 4, the following excerpt from the prospectus of Honda Auto Receivables 2018-1 Owner Trust shows an example discussion of pool characteristics that can feed into the above

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[14]FitchRatings Special Report, 2016, "Representations, Warranties and Enforcement Mechanisms in Global Structured Finance Transactions".

[15]Moody's SEC Rule 17g-7 Report of R&Ws: Ford Credit Auto Owner Trust 2019-A Deal v1.1 Compared To Auto Loans v3.0 (highlights added by authors).

disclosure by rating agencies:

Characteristics of the Receivables The Receivables to be held by the trust will be selected from those motor vehicle retail installment sale contracts in AHFC's portfolio that meet several criteria as of the Cutoff Date. These criteria provide that each Receivable:

- was originated by a dealer located in the United States and the Obligor is not (according to the records of AHFC) a federal, state or local governmental entity;

- has a contractual annual percentage rate specified in the promissory note associated with each Receivable (which we refer to in this prospectus as the "APR") of at least 0.50%;

- has an original term to maturity of not more than 72 months;

- is not more than 30 days past due;

- has been entered into by an Obligor that was not in bankruptcy proceedings or is bankrupt or insolvent (according to the records of AHFC);

- is attributable to the purchase of a new or used Honda or Acura automobile or light-duty truck and is secured by that automobile

- provides for the related monthly payment on a Financed Vehicle owed by the related Obligor (each such payment, a "Scheduled Payment") according to the simple interest method (as described below); and

- except as otherwise permitted under the sale and servicing agreement, provides for scheduled monthly payments that fully amortize the amount financed by such Receivable over its original term (except that the first or last payment in the life of the Receivable may be minimally different from the level payment).

Payments on Receivables using the "simple interest method" will be applied first to interest accrued through the date immediately preceding the date of payment and then to unpaid principal. Accordingly, if an Obligor pays an installment before its due date, the portion of the payment allocable to interest for the payment period will be less than if the payment had been made on the due date, the portion of the payment applied to reduce the Principal Balance will be correspondingly greater, and the Principal Balance will be amortized more rapidly than scheduled.

The ability of the servicer to make modifications on the Receivables is not expected to have a material impact on the distributions on the notes described in this prospectus. For a description of the servicer's ability to make modifications to the Receivables, see "Description of the Transfer and Servicing Agreements—Servicing Procedures."

No selection procedures believed to be adverse to the noteholders will be utilized in selecting the Receivables from qualifying retail installment sale contracts or from the receivables in the pool. For a description of AHFC's loss and delinquency experience on its managed pool portfolio, see "The Sponsor, Originator, Servicer and Administrator—Servicing Experience."

B. Construction of "Recall Impact"

Data on vehicle recalls is from the National Highway Transportation Safety Administration's Office of Defects Investigation.[16]

To measure the impact of each recall, we scale them using two datasets. The first is Polk's Vehicles in Operation data which give the stock of vehicles in operation by model-year for years from 2000 onwards. The second dataset is IHS Markit's New Vehicle Registration which has information on flows at the make/model/model-year level.

We combine this information to create a variable named Scaled Affected Units. It is constructed as follows. The stock of vehicles-in-operation at the model-year level is available across all makes and models. Using this data, we calculate the share of all vehicles of a given model-year that are still on the road in subsequent years; this proxies for a model-year "survival rate". Next, we apply these survival rates to more granular new vehicle registration data at the manufacturer level to estimate the stock of vehicles-in-operation for a given manufacturer in each year of the sample. Finally, we scale the raw units recalled by the estimated stock of vehicles-in-operation.

Putting these steps together, we define and measure the "Recall Impact" by multiplying the Scaled Affected Units by the News Volume and dividing by Sentiment. We obtain data on News Volume and Sentiment from Ravenpack News Analytics' "Aggregate Event Volume" and "Aggregate Event Sentiment," conditional on the "Relevance" of the news event being larger than 75 (on a 1-100 scale). Aggregate Event Volume and Aggregate Event Sentiment each measure the volume of events reported in financial news medium and the share of positive sentiment events, on a 91-day rolling basis. Ravenpack determines scores on positive sentiment by "systematically matching stories typically categorized by financial experts as having short-term positive or negative financial or economic impact." (Source: Ravenpack News Analytics User Guide)

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[16]A detailed description and links to download the data are available at https://catalog.data.gov/dataset/nhtsas-office-of-defects-investigation-odi-recalls-recalls-flat-file.