How do consumers fare when dealing with debt collectors? Evidence from out-of-court settlements

Ing-Haw Cheng, Felipe Severino, and Richard R. Townsend[1]

May 2019

Do deals with debt collectors alleviate consumer financial distress? Using new data linking court and credit registry records, we examine civil collection lawsuits where consumers can settle out of court. Random assignment of judges with different styles generates exogenous variation in the likelihood of settlement negotiations. We find that settlements increase consumer financial distress. Settlements appear to increase distress by draining liquidity without improving access to credit or lowering total payments. Consumers might agree to distress-inducing deals for non-pecuniary reasons or because they are ill-informed of their options.

JEL codes: G00, D14, D18

Keywords: Debt collection, negotiation, financial settlements

---

[1] Cheng and Severino: Tuck School of Business at Dartmouth, Hanover, NH 03755. Townsend: Rady School of Management at the University of California-San Diego, La Jolla, CA 92093. We thank Emily Blanchard, Tony Cookson, Natalie Cox, Marco Di Maggio, Julia Fonseca, Umit Gurun, Juhani Linnainmaa, Andrey Malenko, Brian Melzer, Justin Murfin, Christopher Palmer, Jonathan Parker, Antoinette Schoar, Jonathan Zinman, and conference and seminar participants at Amsterdam Business School, Columbia University, the Cornell IBHF Symposium, the Consumer Financial Protection Bureau Research Conference, Dartmouth College (Tuck School of Business and Department of Economics), Erasmus University, Federal Reserve Bank of Chicago, Federal Reserve Bank of Philadelphia, ITAM, MIT, New York University (Stern-Real Estate), Northwestern University, Princeton University, RAPS/RCFS Winter Conference, SFS Cavalcade, Stanford University, Texas Finance Festival, University of Kentucky Finance Conference, UNC/Duke Corporate Finance Conference, University of Notre Dame, University of Southern California, and Yale Junior Finance Conference for comments. Adi Ilani and Sarah Hong provided valuable research assistance. This research received generous financial support from the Tuck School of Business and Alfred P. Sloan Foundation NBER Household Finance small grant program. TransUnion reviewed this research for compliance purposes with respect to non-disclosure of confidential or proprietary information related to data they provided. Corresponding author: Ing-Haw Cheng, Dartmouth College, inghaw.cheng@dartmouth.edu.

How do consumers fare when dealing with debt collectors? This question is one of growing importance. Approximately 14 percent of U.S. consumers have been under third-party debt collection in recent years (Federal Reserve Bank of New York, 2018), and the industry collects over $55 billion annually (Ernst and Young, 2013). Collection underpins the broader lending market by allowing creditors to recover on delinquent debt, and collectors employ an array of tools such as litigation and out-of-court negotiation to extract payments from consumers. However, debt collection is relatively understudied and not well-understood in the literature (Zinman, 2015). Specifically, we know little about debt collection from a household finance perspective, and whether the settlements that consumers negotiate with collectors alleviate consumers' financial distress. Policymakers have long been concerned about such negotiations, as consumers may be less sophisticated than collectors or unaware of their legal rights. To fill this gap, we provide the first estimates on the causal effect of settlement on consumer financial distress.

On the one hand, settlements may relieve consumer financial distress by resolving delinquent debt claims, cleaning up credit reports, and getting consumers back on track. Indeed, relieving distress is potentially a key motivation for consumers to settle, and evidence of settlements relieving distress might help mitigate several consumer-protection concerns. On the other hand, settlements may exacerbate financial distress by requiring significant up-front payments that drain consumers of liquidity, triggering a downward spiral. If settlements exacerbate financial distress, this might suggest that consumers agreeing to settlements are settling for other, potentially non-pecuniary, reasons, or that consumers may be ill-informed about their options during negotiations. Ultimately, the question is an empirical one.

There are two major challenges in estimating the causal effect of a collection settlement on consumer outcomes. The first is measurement: data tying consumer-collector negotiations to subsequent outcomes are scarce. The second challenge is identification: settlements do not occur randomly and depend on the negotiation context and unobservable consumer characteristics. For example, consumers who settle may tend to be unobservably wealthier than others. This would make consumers who settle appear to have better financial outcomes than observationally similar consumers who do not settle, even if settlement has a negative causal effect on financial outcomes.

To address these challenges, we study consumers who face litigation by a debt collector in state civil court. Litigation is one of the primary tools that collectors use to extract payments from consumers. In such lawsuits, a collector seeks a court judgment certifying the legal validity of their claim. Unlike federal bankruptcy court, the court is not a forum to discharge or consolidate debt. Rather, the role of the court and presiding judge is to verify that the collector has the proper legal status. If the collector wins (as they mostly do), they are then entitled to garnish the borrower's wages up to statutory limits; if the collector loses, the status quo resumes. Our main finding is that consumers who settle causally experience significantly higher financial distress than they would have compared to their alternative of going through the courts.

To address the measurement challenge, we assemble a unique dataset that links all court records from Missouri debt collection lawsuits from 2007-2014 with credit registry data from TransUnion. From the court records, we observe cases that concluded with an in-court ruling as well as cases that concluded with an out-of-court settlement between the two parties. The credit registry data allow us to track subsequent financial outcomes for each consumer. We focus on Missouri because, unlike many states, it has a centralized database of cases tried in different circuit courts, and several circuits assign judges to these cases randomly. Importantly, this database also tracks garnishments. Missouri is a representative state in terms of collection (Ratcliffe et al., 2014).

To address identification, we exploit the fact that judges are randomly assigned and have different empirical propensities to preside over cases that settle out of court. We attribute these different settlement propensities to differences in the style of how judges manage their case docket. According to Missouri debt collection attorneys we spoke to, prior to hearing a case, there is variation across judges in how much they encourage the parties to reach a settlement. For example, at the start of the day, some judges routinely ask all parties to talk to one another first to try to reach an agreement, while others do not. Consistent with the idea that settlement propensity is related to a judge's style, we find that is it persistent over time for a given judge. Because judges are assigned randomly, we can then instrument for settlement with judge settlement propensity. While settlement may be correlated with unobservable consumer characteristics that relate to financial distress (e.g., wealth), judge settlement propensity should not be.

Of course, judges may be able to take multiple actions that potentially influence subsequent consumer outcomes. For example, it may be that high settlement propensity judges tend to encourage out-of-court settlements by prompting negotiations but also tend to rule against consumers in cases that go to trial. Randomly drawing such a judge may cause an increase in the probability of financial distress, but only through the negative-ruling channel, not through the prompting-negotiations channel. To address this possibility, we control directly for a judge's tendency to rule against consumers throughout our analysis. Our identifying variation stems from judges' settlement propensities that are orthogonal to their ruling tendencies.

We begin by hand-verifying that judges in our sample are randomly assigned by obtaining the court procedure documents and speaking with the court clerk for every district in Missouri. We limit our analysis to districts we verify have random assignment. We find no significant differences in credit scores, balances, and other characteristics in the year prior to case disposition across borrowers who draw a high or low settlement propensity judge, consistent with random assignment. After case disposition, differences emerge.

More precisely, when we instrument for settlement using the settlement propensity of the judge assigned to the case, we find that settlement causes an increase in borrower financial distress by significantly increasing the probability of delinquency, bankruptcy, and foreclosure in the first year after case disposition. The effects are economically large. Settlement increases the probability of delinquency, bankruptcy, and foreclosure by 20%, 160%, and 130% over base rates. The results are robust across various specifications and sample restrictions. The immediate increase in financial distress from settlement translates into a long-run increase in distress: Settlement increases cumulative delinquency, bankruptcy, and foreclosure through long-run horizons of up to 4 years. Generally, only consumers who face severe financial distress seek bankruptcy relief or face foreclosure. For example, financial distress among bankruptcy filers has been shown to be sufficiently high that failure to obtain bankruptcy protection leads to increased mortality, lower earnings, and worse financial outcomes (Dobbie and Song, 2015; Dobbie et al., 2017).

Why don't deals alleviate financial distress for consumers? In principle, deals could reduce distress in at least two ways. First, a deal might reduce financial distress relative to garnishment by cleaning up a consumer's credit report and improving access to credit. Second, a deal might allow consumers to settle their debt at a discount. However, we find little evidence of either effect.

First, we do not find that settlement significantly improves access to credit either during the first year after disposition or cumulatively through four years after disposition. Settlement does not significantly affect the probability that a consumer would be classified as prime credit, nor does it lead to more new accounts, more credit inquiries, or a higher ratio of new accounts to inquiries for the consumer (akin to an acceptance rate). These null results are similar across consumers with high and low ex-ante credit scores, suggesting that settlement does not improve credit access for either of these groups.

Second, we show that settlement plausibly fails to reduce distress through discounts relative to garnishment for two reasons. First, garnishment already involves a large implicit discount for consumers. Even though one might imagine that garnishment recovery rates are close to 100% because garnishments are court-sanctioned, we find that garnishment recovers only 38% on average. Collectors may end up garnishing less than the full amount if the garnishment expires, the borrower declares bankruptcy, loses their job, moves to another state, or for other reasons. Second, settlement recovery rates are likely higher than garnishment recovery rates. While our court data do not include private settlement amounts, our analysis of a separate proprietary dataset from a large national collector suggests their average settlement recovery rate equals 84%, while their average garnishment recovery rate equals 43%.

Why do deals increase financial distress for consumers? The most likely reason is that settlements drain more liquidity than garnishment. According to Missouri debt collectors we spoke to, settlements typically require large up-front payments. The economic rationale is that delinquent borrowers lack the ability to credibly commit to a payment plan. In contrast, garnishment is a payment plan technology that conserves consumer liquidity by design by imposing legal caps on payments as a fraction of disposable income. Consistent with this anecdotal evidence and logic, we find in the large national collector's data that settlements recover 73% of their total payments within the first month, compared to 7% for garnishments. Thus, settlement payments are both larger in total and occur faster than garnishment payments.

Overall, settlements with collectors cause increased consumer financial distress, as settlements appear to drain liquidity without improving access to credit or lowering total payments. This finding raises a potential puzzle: Why do consumers agree to such settlements? We speculate on two possibilities. First, consumers may settle for reasons outside the scope of our credit data, including non-pecuniary reasons such as avoiding any stigma or stress associated with garnishment. As such, we caution that we do not reach conclusions about consumer welfare.

However, we also explore the possibility that unsophisticated or ill-informed consumers agree to deals that are worse than their outside option of having the court hear their case. Consumers may be unaware that garnishment is a payment plan that conserves liquidity, and they may deal with more-informed collectors. Two pieces of evidence are consistent with this possibility. First, consumers experience more financial distress when they strike deals with more-experienced debt collection attorneys compared to less-experienced ones. This is consistent with a necessary condition for consumers being ill-informed, as deals lead to worse outcomes when consumers potentially face a large disadvantage in negotiations. Second, consumers who settle not only experience more distress than consumers who go to court, they also experience more distress than consumers who lose in court. Higher distress is thus unlikely explained by a benefit of avoiding court-related uncertainty.

We conclude by discussing the generalizability of our results. We note that our findings only directly apply to the selected sample of consumers who collectors chose to litigate. It may be that the effect of settlement on financial distress is different for non-litigated consumers. However, it seems reasonable to think that the effect could be even larger among those who are not litigated. Non-litigated consumers may tend to have a lower ability to pay, such that the loss of liquidity from a settlement would be particularly distress-inducing for them. Consistent with this idea, we find in the large national collector data that the collector is less likely to litigate if the consumer lacks a verified job, home, or credit report. In addition, non-litigated consumers may tend to deal with less-scrupulous collectors than those who operate in our court setting, which could also lead to larger effects among the non-litigated.

Our main contribution is to provide the first micro-level evidence on how settlements between consumers and collectors affect subsequent consumer financial distress. Despite the scale of debt collection in the United States, the literature contains scant evidence on consumer-collector interactions (Hunt, 2007; Zinman, 2015). Our focus on out-of-court negotiations follows Egan, Matvos, and Seru (2018), Piskorski, Seru, and Vig (2011), and Liberman (2016), who examine securities arbitration, mortgage renegotiation, and the willingness of consumers to pay for a good credit reputation. The relatively few papers studying consumer debt collection have examined how regulations governing collection practices affect credit supply and consumer access to credit (Dawsey and Ausubel, 2004; Dawsey, Hynes, and Ausubel, 2013; Fedaseyeu, 2015; Fedaseyeu and Hunt, 2015; Fonseca et al., 2018).

Our findings point toward a need for further research on the collection process, which appears plagued with frictions and imperfections. First, understanding how consumers make decisions during the process is important given their liquidity constraints (Lusardi, Schneider, Tufano, 2011), the real effects of household financial distress (Dobbie and Song, 2015; Dobbie et al. 2017; Herkenhoff et al., 2016; Musto, 2005), and potential consumer unsophistication (Agarwal et al., 2015; Argyle et al. 2018; Beshears et al., 2018; Lusardi and Mitchell, 2014). Second, understanding how creditors deal with frictions in court-based recovery and how they may substitute toward out-of-court settlements is important given the central role of collection for well-functioning credit markets. We comment on these issues in the conclusion.

1. Background

1.1. Collection, settlements, and financial distress

Between 2007 and 2014, on average 6.0% of total consumer debt, or about 711 billion dollars, has been delinquent by 90 days or more. Consumers who fall significantly behind on their debt payments enter the collections process. Several of these consumers end up in foreclosure or bankruptcy, but the literature has paid comparatively little attention to the many consumers who do not. For example, while 1.4 million consumers experience foreclosure each year on average, a much larger number—14% of consumers with a credit file, or around 28 million—have at least one account in third-party collection, with an average balance of 1,400 dollars. The average flow rate into bankruptcy is 1.6 million consumers per year, but in aggregate, relatively few consumers in collection end up in bankruptcy (Dawsey and Ausubel, 2004; Dawsey, Hynes, and Ausubel, 2013; Fay, Hurst, and White, 2002; White, 1998).[1]

For short-term delinquencies, lenders often rely on in-house collections departments. For severely delinquent debt, many lenders rely on third-party collection agencies working on a fee-basis or sell off the debt outright to debt buyers. The key challenges facing collectors are first to determine which consumers likely have the financial resources to pay and then to locate them and extract as much cash as possible before other debt holders. For unsecured debt, their tools are negotiation and litigation. Litigation can lead to wage garnishment—the extraction of cash flows from a consumer's paycheck.

Policymakers have had long-standing concerns about potentially abusive collection practices and the possibility that consumers are unaware of their rights or of what will happen if they fail to pay. Several consumer-protection laws and regulations circumscribe how lenders or third-party collectors may interact with consumers. Among other protections, the Fair Debt Collection Practices Act (FDCPA) of 1977 limits when third-party collectors can contact a consumer, prohibits misrepresentation, lies, and deception, and prohibits the collection of amounts greater than the amount owed, which the collector must provide written notification of in a timely fashion. Several individual states have also enacted laws that provide protections that go beyond the FDCPA (Fedaseyeu, 2015; Fonseca et al., 2018). More recently, the Dodd-Frank Act of 2010 empowered the Consumer Financial Protection Bureau (CFPB) to oversee the industry. Motivated by their observation that "debt collection constitutes one of today's most important consumer financial concerns" (CFPB, 2014), the CFPB has explored potential new rules for the industry and published several educational resources for consumers that seek to educate consumers about their

[1] This discussion combines observations from Hunt (2007) as well as updated statistics and author calculations from the Federal Reserve Bank of New York (2018)'s quarterly report on household debt and credit. To calculate the number of consumers with accounts in third party collection, we assumed there were 200 million consumers with a credit file, a conservative number (Lee and van der Klaauw, 2010).

rights when dealing with collectors (CFPB, 2017). However, concerns persist, largely because a lack of data has limited our understanding of how consumers deal with collectors (Zinman, 2015).

Specifically, we know very little about settlements and out-of-court debt resolution between collectors and consumers. We know much more about how consumers fare when they resolve debt through formal legal mechanisms such as bankruptcy or through government-sponsored mortgage restructuring programs (Agarwal et al. 2017; Dobbie and Song, 2015; Dobbie et al., 2017; Ganong and Noel, 2018; Mian, Sufi, and Trebbi, 2015), or about negotiation in other contexts. Egan, Matvos, and Seru (2018) examine out-of-court settlements in securities arbitration and find that the endogenous arbitrator selection can lead arbitrators to compete by slanting towards firms who are informed about arbitrator reputations. Piskorski, Seru, and Vig (2010), Adelino, Gerardi, and Willen (2013), Agarwal et al. (2011), and Ghent (2011) study the mortgage market and find that frictions such as securitization impede renegotiation. Liberman (2016) estimates how much consumers are willing to pay to forgive credit card debt.

In the simplest formulation, a settlement can be a zero net-present-value transaction. However, a large literature shows that liquidity and debt overhang affect the financial health and consumption pattern of consumers, and settlements are likely to interact with these frictions in ways that differ from wage garnishment. With respect to debt overhang, recent research on mortgages and housing shows that the household debt overhang in the aftermath of the crash in U.S. house prices affected consumption patterns (Dynan, 2012; Mian, Rao, and Sufi, 2013). Melzer (2017) shows that households reduce housing investments when faced with debt overhang, while Bernstein (2018) shows that it affects labor supply. Dobbie and Song (2018) find that relaxing long-run debt constraints through interest rate write-downs significantly improves consumers' financial and labor market outcomes.

With respect to liquidity, Baker (2018) shows that credit and liquidity explain a significant portion of heterogeneity in consumption elasticities and summarizes a large prior literature on household liquidity. Ganong and Noel (2018) use a natural experiment to show that mortgage restructuring programs that conserve borrower liquidity have a large impact on default and consumption. In general, many consumers are liquidity-constrained: Lusardi, Schneider, and

Tufano (2011) document that many households self-report that they would have difficulty coming up with $2000 within 30 days, and the Federal Reserve reports that "four in 10 adults, if faced with an unexpected expense of $400, would either not be able to cover it or would cover it by selling something or borrowing money" (Board of Governors of the Federal Reserve System, 2018).

Considering these frictions, the effect of settlements on consumer financial distress is ambiguous. Settlements might alleviate debt overhang and financial distress much more quickly than wage garnishment, by potentially quickly cleaning up a consumer's credit report and getting them back on track towards a better credit profile. Evidence that settlement relieves financial distress may allay consumer-protection concerns by providing evidence that consumers obtain a key benefit from settling. On the other hand, settlements may exacerbate financial distress by draining liquidity, as debt collectors typically require significant up-front payments due to a lack of the borrower's ability to credibly commit. Wage garnishment enables consumers to commit to a payment plan backed by a court judgment. Garnishment conserves liquidity by design, as payments are limited by law. Ultimately, the question of how settlements affect financial distress is an empirical one, which this paper aims to address.

1.2. Setting: Collection in Missouri civil courts

We study how consumers and collectors fare in negotiations that occur after a collector has sued a consumer in state civil court to collect on non-mortgage debt, but before the court resolves the case. We limit our study to Missouri because, unlike most states, it has a centralized database of cases tried in different civil courts, and because several counties assign judges randomly in civil cases. Missouri is a representative state in terms of the percentage of consumers who are delinquent and the average amount of debt in collections (Ratcliffe et al., 2014). Missouri is also not particularly exceptional in terms of the law surrounding collections; a few other states such as Texas and Pennsylvania severely curtail the ability of collectors to garnish wages. Finally, debt collectors in Missouri are obligated to file the cases in the court associated with the borrower's address, which prevents collectors from shopping for judges. Focusing the scope of our study on this setting affords us several advantages in our empirical design, as we discuss in Section 2.

Before proceeding, we first review the key institutional details surrounding the litigation process. The plaintiff in a case is typically an attorney acting on behalf of the original lender or a debt buyer and seeks a court judgment certifying the validity of the debt. Debtholders can sue at any point before the state's statute of limitations expires on the debt, which is 10 years in Missouri. After the statute of limitations expires, the borrower still notionally owes the debt, but the debt holder legally cannot use the court system to aid in the collection of the debt.

After the debt holder files a lawsuit, the court attempts to serve the borrower with a summons. If the borrower cannot be located, the case is dismissed without prejudice, meaning that the debt holder does not win the case but retains the right to bring the case again in the future. If the borrower is successfully served, she must appear in court on the assigned date. If the borrower fails to appear on this "first appearance" date, the debt holder typically wins a "default judgment" against the borrower. If the borrower does appear, a subsequent hearing date is set, before which there may be additional court appearances required if additional legal issues come to the fore.

At the hearing, the judge determines whether there is sufficient evidence that the plaintiff has the proper legal status and documentation to collect on the debt. If the judge deems the evidence insufficient, she can dismiss the case. The dismissal can either be "without prejudice," meaning that the case can be brought again in the future, or "with prejudice," meaning that it cannot be.

If the plaintiff wins—the usual outcome—the court enters a judgment against the borrower. These often take the form of "Consent Judgments," essentially a judgment where a consumer has admitted they owe the debt. Typically, the judgment amount is for the principal plus interest and court fees. A judgment grants the debt holder the right to garnish the borrower's wages up to certain statutory limits. Missouri's cap on wage garnishment is 10% of disposable income, which is more stringent than the federal limit of 25%. The judgment itself does not initiate garnishment; the collector must separately contact the consumer's employer and notify them that they have a court-sanctioned judgment that allows them to garnish wages before receiving cash flows.

At any point in this process, a borrower can reach a negotiated settlement agreement with the collector. These settlements typically require significant up-front payment. The court then closes the case and records it as having been dismissed by the parties involved. If consumers offer to settle with a payment plan without significant up-front payment, collectors will often go to the court to obtain a judgment anyway so that they can begin garnishment if the consumer fails to follow through. As mentioned earlier, there are typically several court appearances before an actual hearing and several opportunities for negotiation. Once a case has either been settled or resolved in court, the court records the case as "disposed."

The judge determines the legal validity of the collector's claim. Importantly, the court is not a venue for debt discharge or consolidation, and the scope of items for the court to determine is relatively narrow. For example, according to our conversations with Missouri debt collection attorneys, consumers often provide non-legal arguments that the courts routinely disregard.

In contrast, our conversations with Missouri attorneys suggest that judges can and do influence whether parties negotiate settlements through the way that they manage their daily case docket. For example, on the first appearance date, a judge may state that if the two sides have not talked, they should see if something can be worked out. Typically, the case is set for trial at a later appearance. Before scheduling a trial date, some judges will again encourage the two sides to negotiate. Finally, on the trial date, some judges will one last time encourage talks, as by this point there is sometimes new information to discuss. Courts often have tables or side rooms designated for such negotiations. As noted above, many borrowers do not have a lawyer and rely on non-legal arguments, which are effectively a waste of the court's time. Thus, one reason a judge may encourage negotiation is to make efficient use of court resources.

2. Data and variables

2.1. Data for primary analysis

We assemble a unique dataset that contains all court records from Missouri debt collection lawsuits from 2007-2014 merged with credit registry data from TransUnion.

In the court records, there are 667,337 debt collection cases in Missouri's 45 court districts during our sample period. Our empirical design focuses on court districts where we are able to verify random judge assignment. We first obtain and hand-review the court procedure documents and look for evidence of random assignment. We then proceed to call the district court clerk on two separate occasions and speak with them about their practices to verify random assignment. This leaves us 203,298 cases in 10 court districts. Court districts correspond to one or more counties. The counties corresponding to the districts in our sample include high population counties (e.g., Jackson County) and typically exclude low population ones (e.g., Ozark County), where there may only be one relevant judge. Some high population counties (e.g., St. Louis) do not assign cases completely randomly, so we exclude them.

To examine consumer credit outcomes after case disposition, we link the court records with detailed credit registry data from TransUnion. This link was performed by TransUnion based on names and standardized addresses as well as birthdates and social security numbers when available. We purchase 9 years of credit files, 2007-2015, to match with the court data. Each credit file contains a snapshot of the consumer's credit profile at the beginning of that year. The matched data returned to us by TransUnion was anonymized and stripped of these personal identifiers. TransUnion was able to match approximately 87% of consumers from the court records to their database, leaving us with a sample of 176,769 cases.

To arrive at our final sample, we apply three further filters. First, we require that the consumer's case is assigned to a judge for which we can construct a judge settlement propensity measure described in Section 2. Second, we require that we observe the consumer's credit file both in the January before disposition (which we will call time 0) and in the January after. Finally, we require that the data indicates the borrower was appropriately served. After applying these filters, our final sample consists of 82,218 cases assigned to 43 judges. Online Appendix Table A1 describes the effect of each filter in detail.

From the court records, we can observe cases that concluded with a ruling in favor of one party or the other. Following the guidance of the court clerks we spoke with, we categorize a case a being settled out of court if the defendant was successfully served but the case was ultimately "Dismissed by Parties." Figure 1 shows the type of case outcomes included in the analysis: "Settlement" refers to an out-of-court bilateral arrangement and represents about 17% of the outcomes. "Consent judgment" represents 17% of outcomes, dismissal (with or without prejudice) represents 5%, and "default judgment" represents 62%.

Table 1 Panel A reports summary statistics. Conditional on no settlement, 94% of cases end in judgment, and 6% end in dismissal, with an average judgment amount of 2,967 dollars. The average total garnishment is only 736 dollars. The average length of time from filing to disposition is 88 days. Most cases in our sample correspond to a unique defendant.

Table 1 Panel B reports the characteristics of borrowers in our sample in the year before the disposition date of a case (column 1) and compares these with the overall population of credit users and the population of borrowers who declare bankruptcy (columns 2-4, from Dobbie et al., 2017). The average credit score of consumers in our sample of 536 falls below several industry risk metrics for prime credit and is also lower than both the average population score and the score of bankruptcy filers. (The 25th, 50th, and 75th percentiles in our sample are 492, 531, and 575, respectively; analogous statistics from other studies were not available for comparison.) Unsurprisingly, borrowers in our sample have a higher likelihood of having a collection flag than bankruptcy filers (76% vs. 47%), with higher collection balances.[2]

2.2. Proprietary data from a large national collector

Our main dataset consists of Missouri court records merged with TransUnion data. The main dataset tracks how much borrowers pay through garnishment, but not how much litigated borrowers pay in private settlements. One question is thus how payment amounts and structures might differ between garnishment and settlement. The main dataset also tracks borrowers who have been sued, but collectors in broader contexts often negotiate with consumers outside of litigation. Thus, a second important question is how these groups of borrowers might differ.

To address these questions, we supplement our analysis in Sections 5 and 7 with data from a large national debt collector on their collection efforts on several portfolios of debt they purchased from a large national bank between 2009-2011. The data from the collector are completely separate from the main dataset. The accounts in the portfolios span several states. The data describe the most recent action, payments, and outcomes for each account as of 2015, as well as whether the collector verified a borrower has a job, home, and credit report.

[2] This fraction is less than 1 because collectors do not always report collections on a consumer to the credit bureaus.

3. Empirical strategy

We are interested in estimating the effect of settlement on consumer financial distress after a case is disposed. A naïve empirical design would use OLS to estimate equations of the form:

$$y_i = \alpha_{cs} + \beta S_i + \Gamma_0 X_i + \Gamma_1 J_{ijcs} + u_i, \tag{1}$$

where y_i is consumer i's outcome in the period of interest (e.g., 1 year after disposition), S_i is an indicator of whether the case was settled out of court, α_{cs} is a court c-by-calendar disposition year s fixed effect (e.g., Jackson Circuit x 2008) to account for court-specific time-varying trends, and X_i is a set of controls that include age, credit score, days-to-disposition, homeownership status, and a flag for previous bankruptcy filings, measured in the January before case disposition (denoted as year 0). We bin age (5-year bins), credit score (50-point bins), and days-to-disposition (30-day bins). The variable J_{ijcs} reflects the tendency for a judge j in circuit c assigned to case i to rule against consumers in year s, a control variable we discuss in more detail below.

Our three primary credit variables of interest y_i are flags for whether a consumer is delinquent on debt, has recently filed for bankruptcy, or has recently experienced foreclosure. However, the error term u_i in Equation 1 likely contains unobserved borrower characteristics affecting financial distress that are correlated with settlement S_i: $E\left[S_i u_i | X_i, J_{ijcs}\right] \neq 0$, biasing the OLS estimates. Underlining this concern, Table 2 columns 1-3 suggest that consumers who settle differ from those who do not on several observable dimensions. Consumers who settle tend to have higher average credit scores (562 vs. 530; medians are 556 vs. 526), mortgage balances ($46,000 vs. $28,000), more trade lines (4 vs. 3), and lower collection balances ($5,800 vs. $7,300). If consumers who settle are unobservably wealthier than those who do not, then the coefficient β in Equation 1 would be biased towards finding that settlement improves financial outcomes.

To overcome this identification challenge, our empirical strategy exploits the random assignment of judges. There is significant variation in the fraction of cases a judge presides over that end with a settlement, or a judge's "settlement propensity," consistent with variation in how judges manage their case dockets as discussed in Section 1.

Considering these differences in judge style, we estimate judge-year specific settlement propensities following a leave-out estimate methodology. Specifically, we compute:

$$\text{Judge settlement propensity: } SP_{ijcs} = \frac{\sum_{k=1}^{n_{jcs}} S_k - S_i}{n_{jcs}-1} - \frac{\sum_{k=1}^{n_{cs}} S_k - S_i}{n_{cs}-1}, \qquad (2)$$

where n_{jcs} is the number of cases assigned to judge *j* in court *c* in year *s* and n_{cs} is the number of cases in front of the broader court. This ratio SP_{ijcs} represents the leave-out average settlement rate of judge *j* in court *c* in year *s* minus the rate in court *c* in year *s* (see, e.g., Kling 2006; Chang and Schoar 2008; Doyle 2007, 2008; Aizer and Doyle, 2015; Dobbie and Song, 2015; and Dobbie et al. 2017). We follow Dobbie and Song (2015) in subtracting the leave-out average settlement rate of the broader court to remove any court-level heterogeneity in settlement rates. We first estimate judge settlement propensities in the full unmatched sample of cases and include only cases where the judge heard a minimum of 10 cases and had a 5% case share per judge-year within the final sample of cases where we confirmed the defendant was served.

We use judge settlement propensity SP_{ijcs} to instrument for settlement S_i in Equation 1. Specifically, the first-stage equation is:

$$\text{First stage: } S_i = a_{cs} + b\, SP_{ijcs} + G_0 X_i + G_1 J_{ijcs} + v_i, \qquad (3)$$

where SP_{ijcs} is the leave-out settlement rate in Equation 2 and the remaining variables are defined as in Equation 1. The second stage equation is Equation 1, where we estimate the parameters using standard instrumental variable techniques:

$$\text{Second stage: } y_i = \alpha_{cs} + \beta S_i + \Gamma_0 X_i + \Gamma_1 J_{ijcs} + u_i. \qquad (4)$$

We cluster standard errors at the judge level to account for across-time correlations between cases and cross-sectional co-movements within a judge-court-year.

Our identifying assumption is that the random assignment of judges with different settlement propensities generates variation in the probability that the two parties settle that is orthogonal to consumer heterogeneity: $E\left[SP_{ijcs} u_i | X_i, J_{ijcs}\right] = 0$. Under this assumption, the second-stage coefficient β on settlement is the causal impact of settlement on subsequent outcomes relative to walking away from the negotiating table.

If the identification assumption holds, consumers assigned a higher settlement propensity judge have similar characteristics as consumers assigned a lower settlement propensity judge, yet they are more likely to conclude their case with a settlement. In reduced form, our empirical strategy compares the financial outcomes of these two groups of consumers.

Of course, judges may be able to take multiple actions with the potential to influence the subsequent financial distress of consumers. For example, it may be that high settlement propensity judges do tend to encourage out-of-court settlements by prompting negotiations but that they also tend to rule against consumers in cases that go to trial. This would contaminate our identification strategy, as consumers who draw high settlement propensity judges would have different outcomes from those who draw low settlement propensity judges simply because they receive different rulings in court. Furthermore, consumers who draw high settlement propensity judges may agree to worse deals than otherwise if they also anticipate worse rulings in court. These concerns motivate our inclusion of J_{ijcs} in Equations 3 and 4. The variable J_{ijcs} represents the propensity of a judge to enter a negative judgment against a consumer, which we compute by taking the average in-court collector win rate for the judge and subtracting the overall average for the court, analogous to how we compute settlement propensity. Our identifying variation thus stems from variation in judge settlement propensities orthogonal to the tendency of judges to rule against consumers.

While the identification assumption is untestable, we nonetheless can test whether SP_{ijcs} is correlated with observable characteristics. If judges are randomly assigned, it should be uncorrelated. We sort cases by whether SP_{ijcs} is above or below the sample median and report the average year-0 consumer characteristics across these two groups in Table 2 columns 4-6. In contrast to columns 1-3 that report statistics by endogenous settlement, columns 4-6 show no significant economic or statistical differences in the year prior to case disposition between consumers who draw a high versus low settlement propensity judge, consistent with random assignment.

4. Settlement and financial distress

4.1. Baseline results

We start by graphically depicting the key element of our first-stage relationship in Figure 2. The figure plots a settlement indicator against our leave-one-out measure of judge settlement propensity in a binned scatterplot. To construct the plot, we first regress an indicator for settlement on court-by-year fixed effects and calculate residuals. We calculate the mean residual in each judge-by-year bin and add the grand mean settlement rate to aid in the interpretation of the plot. The solid line shows the best linear fit estimated on the underlying microdata.

Table 3 reports the first-stage regression estimates. The first column corresponds to Figure 2, where we control for court-by-year fixed effects and otherwise only include the judge settlement propensity as a right-hand-side variable. Column 2 adds the in-court collector win rate J_{ijcs} as a control. Column 3 includes all controls from Equation 3 and is our main first-stage specification. Table 3 and Figure 2 show a strong positive relationship between judge settlement propensity and settlement. The F-statistics are quite high and easily surpasses the rule-of-thumb threshold for weak instruments (Stock and Yogo, 2005). In the Online Appendix, we show that judge settlement propensities are persistent over time, lending credence to the idea that it is a judge-specific effect.

Table 4 reports our main second-stage results from estimating Equation 4 for the dependent variables of delinquency, bankruptcy, and foreclosure in the year after case disposition. Columns 1, 3, and 5 report OLS estimates. The OLS coefficients suggest that borrowers who settle are 5% less likely to file for bankruptcy and 1% less likely to experience foreclosure a year after case disposition, with effectively zero effect on delinquency rates. Given the average bankruptcy and foreclosure rates of 5% and 3% among borrowers who go court (Table 2, Column 2), these are potentially large effects. However, these effects could be driven by unobserved differences between borrowers who settle and those who do not, as Table 2 suggests.

Columns 2, 4, and 6 of Table 4 report second-stage results from the instrumental variables estimation of Equation 4. Settlement leads to 13%, 11%, and 4% *increases* in the probability of delinquency, bankruptcy, and foreclosure, which are statistically reliably different from zero at the

10%, 1%, and 1% levels, respectively. The effects are economically significant as well. Relative to the rates of delinquency, bankruptcy, and foreclosure reported in Table 2 (columns 4 and 5) of 53%, 7%, and 3%, the point estimates in Table 4 suggest that settlement increases these rates by a multiple of 1.2, 2.6, and 2.3, respectively.

Given our identification strategy, we interpret these as causal local average treatment effects: individuals who were induced to settle through the random judge they drew experienced higher rates of delinquency, bankruptcy, and foreclosure than they would have had they not settled. Comparing the OLS estimates and IV estimates highlights the severity of the endogeneity problem when attempting to isolate this causal effect. Unobserved consumer heterogeneity such as wealth confounds the endogenous OLS estimates of the relationship between settlement and distress.

Table 5 explores whether settlement increases long-run cumulative distress. The table reports IV estimates of Equation 4 with long-run outcomes calculated over 2-year to 4-year horizons as dependent variables. In column 1, the dependent variable is the cumulative number of years for which a borrower is delinquent through year T; in column 2, the number of bankruptcy declarations through year T, and in column 3, the number of foreclosures through year T. Longer horizons include smaller samples because later cohorts of cases drop out from our analysis, and all else equal we should expect larger standard errors as horizons increase.

The estimates in Table 5 show that the immediate increase in financial distress from settlement translates into a long-run increase in cumulative distress. The economic and statistical magnitudes of the effect of settlement on distress are large. For example, through year T=4, the cumulative effect of settlement on the number of years for which a borrower is delinquent is +0.45, an effect that is statistically reliably different from zero at the 1% level. Notably, the cumulative effects of settlement on our distress variables tend to weakly increase through time, suggesting that there is no long-term reversal of the 1-year effects in Table 4. If settlements increased distress in the first year, but then reversed and lowered distress in subsequent years, we should have seen long-run coefficients fall below the estimates in Table 4. Instead, the estimated 4-year effect on delinquency of +0.45 is larger than the estimated 1-year effect in Table 4 of +0.13. Similarly, the 4-year effect of bankruptcy is +0.24 and is larger than the 1-year effect of +0.11; for foreclosure, both the 1-

and 4-year effects are +0.04. These results suggest that settlements increase distress for consumers, some of whom declare bankruptcy, while others continue in delinquency and "informal" bankruptcy (Dawsey, Hynes, Ausubel, 2013).

Overall, consumers who settle incur more financial distress, leading to higher rates of subsequent delinquency, bankruptcy, and foreclosure. Generally, only consumers who face severe distress seek bankruptcy relief or face foreclosure. For example, distress among bankruptcy filers is sufficiently high that failure to obtain bankruptcy protection leads to increased mortality, lower earnings, and worse financial outcomes (Dobbie and Song, 2015; Dobbie et al., 2017). Thus, our results suggest that deals with collectors increase distress to economically high levels.

4.2. Robustness

4.2.1. What about other judge actions? The random assignment of judges ensures that we are estimating a causal effect stemming from judge actions, rather than a correlation driven by unobservable borrower characteristics. However, as discussed in Section 3, judges may be able to take multiple actions with the potential to influence the subsequent financial distress of defendants, and their tendency to take these actions may be correlated. For example, it may be that high settlement propensity judges do tend to encourage out-of-court settlements by prompting negotiations but that they also tend to rule against consumers in cases that go to trial.

Fortunately, other than prompting negotiations, ruling against a consumer in court is likely the only other action a judge could take that would affect the consumer's subsequent financial distress. Moreover, this action is entirely observable, which means that we can simply control for the judge's propensity to rule against consumers. Equation 4 and the main results in Table 4 directly control for J_{ijcs}, the rate at which judge j in court c of case i rules against consumers in year s. Thus, our identification stems from the component of a judge's settlement rate that is orthogonal to the judge's collector win rate. Table 6 Panel A, Columns 1-3 show that our results remain similar whether or not we control for judges' in-court ruling tendencies. Columns 4-6 go further and show that our results are robust to controlling for judge gender and political party affiliation, which are potentially correlated with the way a judge rules in court.

4.2.2. What about default judgments? From Figure 1, a large proportion of cases include default judgments, or cases where the consumer fails to appear on the first appearance date despite having been successfully served. We include these in our sample to sharpen the estimates of control variables on outcomes. Nevertheless, these borrowers may also be systematically different. To address this, Table 6 Panel B reports results where we separately control for default judgments, and the results are broadly in line with those of Table 4. This is consistent with the random allocation of judges and our identifying assumption.

4.2.3. Placebo test. As a placebo test, we conduct the same analysis assuming a false disposition year equal to two years before the true disposition year. This choice ensures that the time periods in the placebo analysis do not overlap with the analysis in Table 4. Specifically, we estimate Equation 4 as follows. We measure outcomes y_i in the false disposition year plus one, borrower characteristics X_i just prior to false disposition, and instrument for the true settlement indicator S_i with the true judge settlement propensity SP_{ijcs} controlling for the true judge in-court collector win rate J_{ijcs}. As we would expect, Table 6 Panel C reports results that are not statistically reliably different from zero.

5. Mechanism

5.1. Why don't deals alleviate financial distress?

In principle, deals could reduce distress relative to garnishment in at least two ways. First, a deal might reduce financial distress by cleaning up a consumer's credit report and improving access to credit. Second, a deal might allow a consumer to settle their debt at a discount. However, we find little evidence of either effect.

5.1.1. Access to credit

Table 7 examines whether settlements lead to more access to credit at the 1-year horizon. In Panel A, we estimate Equation 4 but with measures of credit report health and access to credit as dependent variables. In Column 1, we test whether settlement causes consumers to have a credit profile that would be classified as prime credit using common industry benchmarks.[3] The estimated coefficient on settlement is 0.15, but is statistically indistinguishable from zero. Likewise, Columns 2 and 3 report that the effects of settlement on the net number of new accounts and the number of new credit inquiries for a consumer are statistically indistinguishable from zero. This suggests that consumers who settle are neither receiving new accounts nor using any potential improvement in credit profiles to seek more credit compared to those who go to court. Column 4 reports that the effect of settlement on the ratio of new accounts to credit inquiries is also statistically indistinguishable from zero. If credit profiles had improved significantly, we would have expected the coefficient in Column 4 to be positive and statistically distinguishable from zero, as a higher proportion of a given number of inquiries (which might result from loan applications) would translate into actual new accounts (Akey, Heimer, and Lewellen, 2018).

Next, we examine whether the effect of settlement on financial distress varies with a consumer's pre-case-resolution credit score. This addresses the possibility that settlement impacts low- or high-score individuals differentially. For example, higher-score consumers may benefit more from settlement by avoiding a judgment. Table 7 Panel B presents results where we interact settlement with an indicator ("Low credit score") equal to one if a consumer had a credit score below the median of the distribution in the January before case disposition. We employ a similar two-stage least squares strategy where we additionally instrument the interaction of "Settlement x Low credit score" with "Settlement propensity x Low credit score." Using the same measures as in Panel A, we do not find a differential effect of settlement across the two groups.

Table 7 suggests that settlements generate little credit profile improvement over a one-year horizon. However, improvements in credit profiles or access to credit might only realize over longer horizons. For example, a settlement may set a consumer on a slow but steady path to credit improvement that takes longer than a year to realize.

Table 8 examines this possibility by estimating Equation 4 but with long-run outcomes calculated over 2-year to 4-year horizons as dependent variables. In column 1, we examine

[3] Our contract with TransUnion prohibits analysis of credit scores as a dependent variable. The reason is to prevent users from reverse engineering or revealing information about the score.

whether a consumer is prime credit at the end of year *T*. Columns 2 and 3 report the effect of settlement on the cumulative number of net new accounts and credit inquiries over each horizon, while column 4 reports the effect of settlement on the ratio of the cumulative number of new accounts to the cumulative number of inquiries. For each variable and horizon, we estimate an effect that is statistically indistinguishable from zero. Estimated effects for longer horizons such as T=5 are also statistically indistinguishable from zero, although the sample size drops further as later cohorts drop out; we omit these results for brevity.

Overall, Tables 7 and 8 show that we observe little evidence that settlements generate short-run (1-year) or long-run (up to 4-years) improvements in access to credit or credit profiles, and contrast with Tables 4 and 5 which show that settlement increases financial distress over the same horizons. Demyanyk, Koijen, and Van Hemert (2011) and Demyanyk (2018) show in a mortgage context that the steepest drop in credit scores occurs after the first missed payment, and that further events have little impact on credit scores. One concern might then be that we would not expect to see much movement in credit scores even if the borrower's underlying access to credit was improving. However, Table 8 shows little evidence for improvement on a variety of measures. Notably, Demyanyk, Koijen, and Van Hemert (2011) and Demyanyk (2018) also find that credit scores improve 1-year after borrowers miss three payments, which they attribute to borrowers using money saved from missed mortgage payments to pay down other debt. In our context, settlement might use up money for other payments, and thus have the effect of increasing distress.

5.1.2. Discounts

Settlements might allow consumers to settle their debt at a discount or low recovery rate for the collector. Table 9 provides evidence that potential discounts in settlement plausibly fail to reduce distress relative to garnishment for two reasons. First, even though one might imagine that garnishment recovery rates are close to 100% because garnishments stem from court judgments, we show that average garnishment recovery rates already exhibit significant implicit discounts for consumers. Second, we find little evidence supporting the hypothesis that settlement recovery rates fall below garnishment recovery rates; if anything, settlement recovery rates are likely higher.

Our first finding in Table 9 is that garnishment involves a significant discount for consumers. Row 1 shows that the average recovery rate in garnishment for the Missouri court data equals 38%.[4] This low recovery rate is consistent with the idea that collectors often end up garnishing less than the full amount due to several frictions in the garnishment process. In particular, the garnishment might fail or be cut short if the garnishment is not renewed, the borrower declares bankruptcy, changes their job, moves out of state, or other reasons (Hynes, 2005, 2008).[5] Indeed, many cases end in judgment but yield zero garnishment cash flows. Even among cases that yield positive cash flows, however, the recovery rate in the Missouri court data is 71% (Row 2).

We can benchmark these recovery rates from the Missouri court data to the recovery rates of a large national collector ("the collector") on several portfolios of debt they purchased between 2009-2011. As Section 2.2 notes, the collector's data is completely independent of the Missouri court data. Garnishment recovery rates from the collector (43% nationally from Row 3, 47% in Missouri from Row 6) are close to the recovery rates in the Missouri court data (38%, Row 1). The modest difference arises because the collector has a higher fraction of cases that yield positive garnishment cash flows. Conditional on positive garnishment cash flows, recovery rates for our collector (68% nationally from Row 4, 66% in Missouri from Row 7) are comparable to those in the Missouri court data (71%, Row 2). Overall, the data suggest that significant implicit discounts for consumers in garnishment are a widespread phenomenon.

Our second finding is that settlement recovery rates are, if anything, higher than garnishment recovery rates. Because our court data do not include data on private settlement amounts, we rely exclusively on our data from the large national collector for this portion of the analysis. Table 9 shows that the collector's average settlement recovery rate (84% nationally, Row 5) is much higher than the average garnishment recovery rate (43% nationally, Row 3), an economically large

[4] The average recovery rate of 38% differs from what one would obtain by dividing the average garnishment payment of $736 by the average judgment amount of $2,967 from Table 1. This latter quantity equals 25% and differs from 38% due to Jensen's inequality.

[5] Consistent with barriers to garnishment during our sample period, Missouri enacted garnishment reforms effective 2015 that streamline the garnishment process for collectors, particularly with so-called continuous wage garnishments that are in force until the debt is repaid in full or the borrower changes jobs (see Missouri §525.040 and several practitioner-oriented discussions of these issues, e.g., https://www.lewisrice.com/publications/continuous-wage-garnishment-approved-in-missouri/).

difference that is statistically reliably different from zero at the 1% level. The settlement recovery rate is also higher than the garnishment recovery rate conditional on positive cash flows (68%, Row 4). Comparisons of the collector's cases within Missouri (Rows 6-8) yield similar insights.

We caveat these findings as follows. First, the finding that settlement recovery rates exceed garnishment recovery rates relies on data from the large national collector rather than the Missouri court case data, and these two datasets may differ in systematic ways. However, the collector's average garnishment recovery rate across all states, as well within Missouri, are similar to the garnishment recovery rates in the Missouri court data, providing no red flag of obvious differences. Second, differences between garnishment and settlement recovery rates may reflect endogenous selection of borrowers. However, we can check whether this might matter for our estimates of average garnishment recovery rates in the Missouri court data using a Heckman (1976, 1979) selection model. Such an analysis delivers the average garnishment recovery rate irrespective of endogenous selection. These estimates imply a recovery rate of 30%, lower than the endogenous average of 38%.[6]

5.2. Why do deals increase financial distress?

The most likely reason that settlements increase distress relative to garnishment is that they drain more liquidity. According to Missouri debt collectors we spoke with, settlements typically require large up-front payments. The economic rationale is that delinquent borrowers lack the ability to credibly commit to a payment plan. In contrast, garnishment is a payment plan technology that conserves consumer liquidity by imposing legal caps on payments as a fraction of disposable income. In Missouri, this cap is 10%, which is in-between the federal cap of 25% and the effectively-0% cap in states that prohibit garnishment (e.g., Pennsylvania).

[6] The Heckman analysis proceeds as follows. First, within the Missouri court data, consider the sample of judgment cases containing data on garnishment recovery rates combined with cases that settle. The second stage dependent variable is the garnishment recovery rate, which is present only for the judgment cases and is missing for the settlement cases. The right-hand side of the second stage contains only a constant term, whose interpretation is the true average garnishment recovery rate accounting for selection into judgment and away from settlement. Our instrument for being in the selected sample of the judgment cases is the judge settlement propensity, which negatively predicts selection. We control for judge in-court borrower win rates in the selection equation.

Consistent with this anecdotal evidence and logic, we find in the large national collector's data that settlements recover payments significantly more quickly than garnishment. Figure 3 illustrates this using the collector's data for cases across all states. Panel A plots the collector's recovery rate in the months following case disposition for settled versus judgment cases. The two curves asymptote through time to the average recovery rates reported in Table 9 of 84% for settlement and 43% for judgment. Moreover, in the month after case disposition, settlement recovery rates are much higher (61%) than garnishment recovery rates (3%), indicative of significant up-front payments. Panel B sharpens this point by plotting the recovery rate in the months following case disposition as a fraction of the total amount ultimately recovered. We consider only garnishees who make at least one payment so that these curves asymptote to 1 by construction. For settled cases, 73% of total payments are made up front, compared to 7% for garnishment. Results are similar within the collector's Missouri cases (see Online Appendix Figure A2). Thus, settlement payments are both larger in total and occur faster than garnishment payments. As a result, settlements likely drain consumers of liquidity to a greater extent than garnishment.

6. Why might consumers agree to settlements?

Overall, settlements with collectors cause increased consumer financial distress, as settlements appear to drain liquidity without improving access to credit or lowering total payments. This raises a puzzle: Why would consumers agree to such settlements, considering their outside option of going to court and facing potential garnishment? Settlements may benefit consumers in ways outside the scope of our credit data. For example, by avoiding garnishment, settlements may avoid stress or potential social stigma. Due to the possibility of these non-pecuniary benefits, we caution that we do not reach conclusions about overall consumer welfare.

However, another possibility is that unsophisticated or ill-informed consumers agree to deals that are worse than their outside option. The broader literature on consumer protection suggests that this may be plausible as consumers are often ill-informed or exhibit biases when making financial choices (Beshears et al. 2018; Lusardi and Mitchell, 2014; Zinman, 2015). In our context, consumers may be unaware that garnishment enables them to commit to a payment plan with federally mandated limits. More broadly, consumers may be unsophisticated negotiators on average. We explore this hypothesis in two ways.

6.1. The effect of collector experience

First, we examine whether consumers experience more financial distress when negotiating with highly experienced collectors. Sophisticated, experienced collectors who understand that consumers are ill-informed or biased may use this advantage in negotiations to settle on terms that are less favorable to consumers. These less favorable terms would lead to more financial distress. Because our measure of collector experience ultimately relies on the number of cases a collector handles in our sample, one alternative is that experienced collectors are constrained on time and instead settle on terms more favorable to consumers. These more favorable terms would lead to less financial distress. Ultimately, the question is an empirical one.[7]

In Table 10 Panel A, we repeat our baseline analysis including an interaction term between the "Settlement" indicator and a "High collector experience" indicator, equal to one if the collector's attorney was in the top 1% of attorneys in our data in terms of the number of cases they filed. The top 1% of attorneys represent collectors in approximately 50% of the cases in our sample. We then instrument for "Settlement" with "Judge settlement propensity" and we instrument for "Settlement x High collector experience" with "Judge Settlement Propensity x High collector experience." In columns 1 and 2, we estimate a statistically significant positive coefficient on the interaction term, indicating that consumers experience higher rates of delinquency and bankruptcy when negotiating with experienced attorneys. These results are consistent with a necessary condition for consumers being ill-informed, as deals lead to worse outcomes when consumers are potentially at a significant disadvantage in negotiations.

6.2. Reduction in court-related uncertainty

Second, we examine an alternative where consumers are well-informed about their outside options and strike deals that incur more distress to avoid court-related uncertainty. For example, risk-averse consumers may be willing to accept the risk of worse outcomes in exchange for a

[7] Because consumers are anonymized in our merged data, the data has limited ability to speak to how outcomes vary with consumer financial literacy or sophistication.

reduction in uncertainty in the "gamble" of going to court and losing. However, most consumers lose and do not actually face that much uncertainty in the court outcome.

In Table 10 Panel B, we go further and show that, on average, consumers who settle experience more financial distress than even those who lose in court. We re-estimate our main specification in a sample that excludes consumers who win in court and find estimates that are consistent with estimates in Table 4. These results suggest that avoidance of court-related uncertainty is not a benefit of settlement, as risk aversion cannot justify choosing a certainty-equivalent outcome worse than the outcome associated with losing the gamble.

7. External validity

7.1. Litigated vs Non-litigated debt collection

Collectors may often negotiate with consumers outside of litigation. Given that our sample only consists of litigated consumers, two important questions are how litigated and non-litigated consumers differ, and how the effect of settlement might differ across these groups.

To explore these questions, we again make use of the data we obtained from a large national collector. The data contain information on several portfolios of debt purchased by the collector between 2009-2011. Table 11 reports summary statistics for this data. For each account, the collector either litigated (11%), called or lettered the borrower without subsequent litigation (7%), warehoused the debt (33%), or sold the debt on to another collector (49%), as of 2015.

The overall picture from Table 11 is that the collector litigates cases for "better off" consumers where the collector could successfully verify that the consumer had a job, a home, and could pull the consumer's credit report. If the collector could only verify that the consumer owned a home, but could not verify a job or credit report, the collector tended to contact the consumer but not litigate. Failing those, the collector tended to warehouse or sell the loan. Warehoused loans were largely those for which the loans were extremely unlikely to yield any recovery. The average outstanding balance at purchase is about \$6,000 for litigated cases and \$6,500 for non-litigated "call or letter" cases. Litigated cases had the highest recovery rate (37%) of any other type of case.

Given these findings, it is plausible that the causal effect of settlement on financial distress among worse-off non-litigated consumers is even larger than our estimated effect for litigated consumers, for two reasons. First, non-litigated consumers may incur more financial distress upon settling due to even lower liquidity or ability-to-pay. Second, debt collectors who litigate likely engage in fewer questionable practices than others, given that they operate through the legal system. Consumers who settle with less-scrupulous collectors that do not operate through the legal system may experience even more financial distress.

7.2. Local average treatment effects and monotonicity

Since we employ an instrumental variables strategy, we can only identify a local average treatment effect (LATE), or the causal effect of settlement among those for whom drawing a high settlement propensity judge is pivotal in the settlement decision (Imbens and Angrist, 1994). Therefore, one possible concern is that the effect of settlements for the average litigated consumer more broadly differs from the effect of settlements induced by the variation in our instrument, the judge's settlement propensity.

Specifically, one concern is that our estimated effect is a "judge effect" rather than a "settlement effect." Perhaps specific consumers in our sample become confused by a judge's negotiation prompt, and thus settle on potentially unfavorable terms, while consumers more broadly are well-informed and would settle on better terms. This would imply that the true effect of settlement on financial distress is either zero or weakly negative in the broader population, whereas we find a positive effect on distress in our sample. For that to hold, it would have to be that consumers are well-informed ex-ante but become ill-informed upon drawing a high settlement propensity judge. Perhaps one reason that may occur is if consumers misinterpret a judge's prompt to negotiate as a signal that they are less protected under the legal system than they thought (and, in fact, are). However, if consumers' beliefs about their options are so easily swayed, they were arguably never that well-informed to begin with and potentially could have been convinced by a debt collector to make a disadvantageous deal even without a judge's negotiation prompt.

Second, in order to have identified a local average treatment effect, we must assume monotonicity in addition to the exclusion restriction: drawing a higher settlement propensity judge should weakly increase the likelihood of settlement for everyone. The monotonicity assumption rules out the presence of "defiers" who settle if and only if they draw a low settlement-propensity judge. Equivalently, our assumption is that everyone who drew a low settlement propensity judge but still settled would have also settled if they drew a high settlement propensity judge. We see little reason to doubt this assumption in our context as it simply implies that no consumers go out of their way to defy the judge's negotiation prompt.

7.3. Sorting on gains

One final concern is that, within our sample, consumers who settle are the ones that have the most to gain from settling. For example, consumers may need some encouragement to begin the negotiation, but they may act in their own self-interest once the negotiation begins. This would introduce "sorting on gains," a form of endogeneity even if judge assignment is random (Heckman, Urzua, Vytlacil, 2006). However, sorting on gains would imply that the *best* effect of settlement for consumers is to cause more financial distress.

8. Conclusion

Overall, settlements with collectors cause increased consumer financial distress, as settlements appear to drain liquidity without improving access to credit or lowering total payments. Our results emphasize the importance of liquidity for consumers and have implications for future research. First, future research should focus on identifying any other motives for consumers to settle, including non-pecuniary motives, and whether consumers are making clear and informed decisions during negotiations. Consumers may not understand that garnishment is a payment plan technology that conserves liquidity or may have low financial literacy (Beshears et al., 2018; Lusardi and Mitchell, 2014). Second, future research should focus on the role of frictions in court-based recovery for collectors and how this may lead collectors to substitute toward out-of-court settlements. Given the central role of collection for well-functioning credit markets, as well as the immense scale of debt collection in the U.S., these are likely fruitful areas for future research.

References

Adelino, Manuel, Kristopher Gerardi, and Paul Willen. "Why don't lenders renegotiate more home mortgages? Redefaults, self-cures and securitization." *Journal of Monetary Economics* 60.7 (2013): 835-853.

Aizer, Anna, and Joseph Doyle. "Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges." *Quarterly Journal of Economics* 130.2 (2015): 759-803.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel. "Regulating consumer financial products: Evidence from credit cards." *Quarterly Journal of Economics* 130.1 (2015): 111-164.

Akey, Pat, Rawley Heimer, and Stefan Lewellen. "Politicizing consumer credit." Working paper (2018).

Argyle, Bronson, Taylor Nadauld, and Christopher Palmer. "Monthly Payment Targeting and the Demand for Maturity." Working Paper (2017).

Baker, Scott R. "Debt and the response to household income shocks: Validation and application of linked financial account data." *Journal of Political Economy* 126.4 (2018): 1504-1557.

Bernstein, Asaf. "Negative equity, household debt overhang, and labor supply." Working paper (2018).

Beshears, John, James J. Choi, David Laibson, and Brigitte C. Madrian. "Behavioral household finance." National Bureau of Economic Research Working Paper (2018).

Board of Governors of the Federal Reserve System. "Report on the Economic Well-Being of U.S. Households in 2017." Technical Report (2018).

Chang, Tom, and Antoinette Schoar. "Judge specific differences in Chapter 11 and firm outcomes." Working Paper (2008).

Consumer Financial Protection Bureau. "How to respond when a debt collector contacts you in three easy steps." https://www.consumerfinance.gov/about-us/blog/how-respond-when-debt-collector-contacts-you-three-easy-steps/ (2017) [Last accessed: June 2018].

Consumer Financial Protection Bureau. *Fair Debt Collection Practices Act: Annual Report* (2014).

Dawsey, Amanda E., and Lawrence M. Ausubel. "Informal bankruptcy." Working paper (2004).

Dawsey, Amanda E., Richard M. Hynes, and Lawrence M. Ausubel. "Non-judicial debt collection and the consumer's choice among repayment, bankruptcy, and informal bankruptcy." *American Bankruptcy Law Journal* 87 (2013): 1-26.

Demyanyk, Yulia, Ralph Koijen, and Otto Van Hemert. "Determinants and consequences of mortgage default." Working paper (2011).

Demyanyk, Yulia. "The impact of missed payments and foreclosures on credit scores." *The Quarterly Review of Economics and Finance* 64 (2017): 108-119.

Dobbie, Will, and Jae Song. "Debt relief and debtor outcomes: Measuring the effects of consumer bankruptcy protection." *American Economic Review* 105.3 (2015): 1272-1311.

Dobbie, Will, and Jae Song. "Target debt relief and the origins of financial distress: Experimental evidence from distressed credit card borrowers." Working paper (2018).

Dobbie, Will, Paul Goldsmith-Pinkham, and Crystal Yang. "Consumer bankruptcy and financial health." *The Review of Economics and Statistics* 99.5 (2017): 853-869.

Doyle, Joseph. "Child protection and child outcomes: measuring the effects of foster care." *American Economic Review*, 97(2007): 1583-1610.

Doyle, Joseph. "Child protection and adult crime: using investigator assignment to estimate causal effects of foster care." *Journal of Political Economy*, 116(2008): 746-770.

Dynan, Karen. "Is household debt overhang holding back consumption?" *Brookings Papers on Economic Activity* (2012): 299-362.

Ernst and Young. "The impact of third-party debt collection on the US national and state economies in 2013." Technical report (2013).

Fay, Scott, Erik Hurst, and Michelle White. "The household bankruptcy decision." *American Economic Review* 92.3 (2002): 706-718.

Fedaseyeu, Viktar. "Debt collection agencies and the supply of consumer credit." Working paper (2014).

Fedaseyeu, Viktar, and Robert M. Hunt. "The economics of debt collection: enforcement of consumer credit contracts." Working paper (2014).

Federal Reserve Bank of New York. *Quarterly report on household debt and credit* (2018, Q1).

Fonseca, Julia, Katherine Strair, and Basit Zafar. "Access to credit and financial health: evaluating the impact of debt collection." Working Paper (2018).

Ghent, Andra C. "Securitization and mortgage renegotiation: Evidence from the Great Depression." *Review of Financial Studies* 24.6 (2011): 1814-1847.

Heckman, James. "The common structure of statistical models of truncation, sample selection and limited dependent variables and a simple estimator for such models." *Annals of Economic and Social Measurement* 5 (1976): 475-492.

Heckman, James. "Sample selection bias as a specification error." *Econometrica* 47 (1979): 153-161.

Heckman, James, Sergio Urzua, and Edward Vytlacil. "Understanding instrumental variables in models with essential heterogeneity." *The Review of Economics and Statistics* 88.3 (2006): 389-432.

Herkenhoff, Kyle, Gordon Phillips, and Ethan Cohen-Cole. "The impact of consumer credit access on employment, earnings, and entrepreneurship." National Bureau of Economic Research Working Paper (2016).

Hunt, Robert M. "Collecting consumer debt in America." Federal Reserve Bank of Philadelphia Business Review (2007): 11-24.

Hynes, Richard M. "Bankruptcy and state collections: The case of the missing garnishments." *Cornell Law Review* 91 (2005): 603-652.

Hynes, Richard M. “Broke but not bankrupt: consumer debt collection in state courts.” *Florida Law Review* 60 (2008): 1-62.

Imbens, Guido, and Joshua Angrist. “Identification and estimation of local average treatment effects.” *Econometrica* 62.2 (1994): 467-75.

Kling, Jeffrey. “Incarceration Length, Employment, and Earnings.” *American Economic Review*, 96 (2006): 863-876.

Lee, Donghoon, and Wilbert van der Klaauw. “An introduction to the FRBNY Consumer Credit Panel.” Federal Reserve Bank of New York. Technical report (2010).

Liberman, Andres. “The value of a good credit reputation: Evidence from credit card renegotiations.” *Journal of Financial Economics* 120 (2016): 644-660.

Lusardi, Annamaria, and Olivia Mitchell. “The economic importance of financial literacy: Theory and evidence.” *Journal of Economic Literature* 52.1 (2014): 5-44.

Lusardi, Annamaria, Daniel Schneider, and Peter Tufano. “Financial fragile households: Evidence and implications.” *Brookings Papers on Economic Activity* (2011): 83-134.

Melzer, Brian. “Mortgage debt overhang: Reduced investment by homeowners at risk of default.” *Journal of Finance* 72.2 (2017): 575-612.

Mian, Atif, Kamalesh Rao, and Amir Sufi. “Household balance sheets, consumption, and the economic slump.” *Quarterly Journal of Economics* 128.4 (2013)? 1687-1726.

Mian, Atif, Amir Sufi, and Francesco Trebbi. “Foreclosures, house prices, and the real economy.” *The Journal of Finance* 70.6 (2015): 2587-2634.

Musto, David. “What happens when information leaves a market? Evidence from post-bankruptcy consumers.” *Journal of Business* 77.4 (2004): 725-748.

Piskorski, Tomasz, Amit Seru, and Vikrant Vig. “Securitization and distressed loan renegotiation: Evidence from the subprime mortgage crisis.” *Journal of Financial Economics* 97.3 (2010): 369-397.

Ratcliffe, Caroline, Signe-Mary McKernan, Brett Theodos, Emma Kalish, John Chalekian, Peifang Guo, and Christopher Trepel. “Delinquent debt in America”. *Urban Institute Opportunity and Ownership Initiative Brief* (2014).

Stock, James, and Motohiro Yogo. “Testing for weak instruments in linear IV regression.” *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg*, edited by Donald W.K. Andrews and James H. Stock, Cambridge: Cambridge University Press (2005), chapter 5, 80–108.

White, Michelle J. “Why don’t more households file for bankruptcy?” *Journal of Law, Economics, & Organization* 14.2 (1998): 205-231.

Zinman, Jonathan. “Household debt: facts, puzzles, theories, and policies.” *Annual Review of Economics* 7 (2015): 251-276.

Table 1. Summary statistics.
Panel A reports case-level summary statistics. "Settlement rate" is the fraction of cases that settled, Pr(Dismiss | Court) is the likelihood of being dismissed conditional on going to court, "Total judgment" is the amount owed in a court judgment, "Garnishment paid" is the average amount of garnishment payments made conditional on a judgment, and "Days to disposition" represents the length between filing date and disposition date. Total judgment and garnishment amounts are available together for 62% of cases that end in judgment. Panel B reports the characteristics of borrowers in our sample in the January before the disposition date of a case (column 1) and compares these with the overall population of borrowers (column 2) as well as the population of borrowers who declare bankruptcy (columns 3 and 4). Columns 2-4 are from Dobbie et al. (2017).

Panel A. Case characteristics

	Mean	SD	Median	N
Settlement rate	0.17	0.37	0	82,218
Pr (Dismiss | Court)	0.06	0.23	0	68,516
Total judgment ($)	2,967	3,946	1,573	40,057
Garnishment paid ($)	736	1,367	117	40,057
Day to disposition	88	83	59	82,218
N of cases per person	1.23	0.57	1	82,218

Panel B. Comparison with other samples

	Litigated cases (1)	2% Random Sample of Credit Users (2)	Bankruptcy filers (3)	Chapter 13 bankruptcy filers (4)
Delinquency flag	0.526	0.148	0.413	0.675
Bankruptcy flag	0.075	0.010	0.007	0.048
Foreclosure flag	0.031	0.003	0.010	0.048
Revolving balance	7,325	6,010	13,080	10,010
Collection balance	7,012	600	1,430	2,500
Credit Score	536	740	630	580
Collection flag	0.756	0.137	0.296	0.467
Charge-off flag	0.410	0.065	0.188	0.310
Judgment flag	0.102	0.009	0.034	0.060
Lien flag	0.012	0.004	0.011	0.021
Age	42.7	48.6	43.7	44.8
Homeowner	0.510	0.470	0.520	0.643

Table 2. Borrower characteristics.

The table compares average characteristics of borrowers who settled (column 1) with borrowers who had their cases heard by the court (column 2). The p-value in column 3 is from a test of differences between these two samples, controlling for court-by-year fixed effects, where we cluster standard errors at the judge level. We compute borrower characteristics in the year before case disposition. Columns 4-6 repeat this exercise where we split cases by high or low judge settlement propensity, defined as SP_{ijcs} above or below its sample median. */**/*** denotes coefficients which are statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively.

	Settlement vs Court			Judge Settlement Propensity		
	Settle	Court	p-value	Low	High	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Settlement propensity	0.006	-0.002	(0.004)***	-0.017	0.031	(0.000)***
Household distress						
Delinquency	0.55	0.52	(0.000)***	0.53	0.53	(0.400)
Bankruptcy	0.21	0.05	(0.000)***	0.08	0.07	(0.223)
Foreclosure	0.03	0.03	(0.021)**	0.03	0.03	(0.700)
Debt balances						
Revolving balance	10,164	6,758	(0.000)***	7,337	7,301	(0.534)
Mortgage balance	46,014	28,154	(0.000)***	30,722	31,946	(0.108)
Access to credit						
Credit score	562	530	(0.000)***	535	537	(0.229)
Non mtg. inquiries	1.6	1.9	(0.000)***	1.9	1.8	(0.898)
Mortgage inquiries	0.1	0.1	(0.062)*	0.1	0.1	(0.396)
Number of trade lines	4.1	3.2	(0.000)***	3.4	3.3	(0.606)
Have a collection	0.67	0.77	(0.000)***	0.76	0.76	(0.521)
Collection balance	5,776	7,259	(0.000)***	7,027	6,983	(0.397)
Have a judgment	0.05	0.11	(0.000)***	0.10	0.11	(0.546)
Have a lien	0.01	0.01	(0.005)***	0.01	0.01	(0.567)

Table 3. First-stage estimates.
The table reports the results of estimating Equation 3, where the left-hand side variable is the settlement indicator S_i and the main right-hand side variable of interest is the judge settlement propensity score, SP_{ijcs}. Column 1 reports estimates with only court-by-year fixed effects, while Column 2 reports estimates also controlling for the judge's collector win rate J_{ijcs}. Column 3 includes all controls, including controls for age (binned), credit score (binned), days to disposition (binned), homeownership, pre-period bankruptcy, and the judge's collector win rate J_{ijcs}. We cluster standard errors at the judge level and report them in parentheses. */**/*** denotes coefficients which are statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively.

	Settlement indicator		
	(1)	(2)	(3)
SP_{ijcs}	0.94	0.92	0.82
	(0.04)***	(0.04)***	(0.06)***
J_{ijcs}		0.06	0.27
		(0.03)**	(0.10)***
Controls	Only α_{cs}	Only α_{cs}, J_{ijcs}	Yes (all)
N Obs	82,218	82,218	82,218
R^2	0.051	0.051	0.157
N Clusters	43	43	43
First-stage F-statistic	737	321	94

Table 4. Second-stage estimates.

This table reports results from estimating Equation 4 using ordinary least squares (OLS; Columns 1, 3, and 5) and two-stage least squares (IV; Columns 2, 4, 6). The dependent variables are indicator variables for delinquency on at least one account in the past 12 months, bankruptcy, and foreclosure, all measured in the January after case disposition. We define control variables in Section 3.1 and omit their estimates for brevity. For IV estimates, we instrument for settlement S_i using the judge settlement propensity SP_{ijcs}. We cluster standard errors at the judge level and report them in parentheses. */**/*** denotes coefficients which are statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively.

	Delinquency		Bankruptcy		Foreclosure	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Settlement	0.00	0.13	-0.047	0.11	-0.012	0.04
	(0.005)	(0.08)*	(0.004)***	(0.03)***	(0.001)***	(0.01)***
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N Obs	82,218	82,218	82,218	82,218	82,218	82,218
N Clusters	43	43	43	43	43	43

Table 5. Long-run financial distress.

This table reports IV estimates of the coefficient on settlement in Equation 4 where dependent variables are cumulative measures of financial distress. We cumulate dependent variables over different horizons, from *T*=2 years after disposition to *T*=4 years after disposition. Each cell reports the second-stage IV estimate for a specification where the dependent variable is indicated in the column, for the horizon indicated in the row. The dependent variables are the total number of years for which a borrower is delinquent through year *T* (column 1), the number of bankruptcy declarations through year *T* (column 2), and the number of foreclosures through year *T* (column 3). Within a given horizon, the number of observations and clusters is the same. Observations and clusters decline as horizons increase as later cohorts drop out from our analysis. We define control variables in Section 3.1 and omit their estimates for brevity. For all specifications, we cluster standard errors at the judge level and report them in parentheses. */**/*** denotes coefficients which are statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively.

Horizon:	Delinquency (1)	Bankruptcy (2)	Foreclosure (3)	N obs	N Clusters
T=2	0.28	0.21	0.05	80,525	40
	(0.122)**	(0.084)**	(0.019)**		
T=3	0.27	0.24	0.04	77,572	39
	(0.120)**	(0.128)*	(0.017)**		
T=4	0.45	0.24	0.04	67,237	37
	(0.143)***	(0.161)	(0.022)*		

Table 6. Robustness.

This table reports IV estimates of Equation 4 under alternative assumptions. Panel A Columns 1-3 report estimates where we drop the control for J_{ijcs}. Columns 4-6 report estimates where we include all controls from Equation 4 but also add controls for the gender and political affiliation of the judges. We measure political affiliations by searching for political contributions using followthemoney.org and the Federal Election Commission website. We include indicator variables for Democrat, Republican, and Independent, and the omitted category is "No Contribution Data." Panel B controls for an indicator for whether a given case concluded with default judgment. Panel C replicates the analysis from Table 4 but in a false disposition year equal to the true year minus two. This choice ensures that the time periods in the placebo analysis do not overlap with the analysis in Table 4. We measure outcomes in the false disposition year plus one, borrower characteristics just prior to false disposition, and instrument for the true settlement indicator with the true judge settlement propensity controlling for the true judge in-court collector win rate. We define control variables in Section 3.1 and omit their estimates for brevity. We cluster standard errors at the judge level and report them in parentheses. */**/*** denotes coefficients which are statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively.

Panel A. Judge actions or characteristics

	Delinquency (1)	Bankruptcy (2)	Foreclosure (3)	Delinquency (4)	Bankruptcy (5)	Foreclosure (6)
Settlement	0.08	0.12	0.03	0.12	0.12	0.04
	(0.08)	(0.03)***	(0.01)***	(0.08)	(0.03)***	(0.01)***
Controls	Yes (except J_{ijcs})	Yes (except J_{ijcs})	Yes (except J_{ijcs})	Yes (Extra)	Yes (Extra)	Yes (Extra)
N Obs	82,218	82,218	82,218	82,218	82,218	82,218
N Clusters	43	43	43	43	43	43

Panel B. Default judgments

	Delinquency (1)	Bankruptcy (2)	Foreclosure (3)
Settlement	0.15	0.18	0.06
	(0.11)	(0.06)***	(0.02)***
Default Judgment Indicator	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N Obs	82,218	82,218	82,218
N Clusters	43	43	43

Table 6, continued.

Panel C. Placebo

	Delinquency (1)	Bankruptcy (2)	Foreclosure (3)
Settlement	0.32	0.02	-0.004
	(0.465)	(0.027)	(0.021)
Controls	Y	Y	Y
N Obs	65,467	65,467	65,467
N Clusters	41	41	41

Table 7. Access to credit.

Panel A reports IV estimates of Equation 4 but where the dependent variables are an indicator if a consumer's credit profile would be classified as prime credit using commonly-used industry benchmarks (column 1), the net number of new credit accounts (column 2), the number of credit report inquiries (column 3), and the New account/inquiry ratio (column 4) in the January after the case is disposed. Panel B reports IV estimates of Equation 4 but where we add an indicator for whether the consumer had a credit score below the median of the distribution in the January before case disposition as well as its interaction with settlement ("Settlement x Low credit score"). We employ a similar instrumental variables strategy where we additionally instrument the interaction of "Settlement x Low credit score" with "Judge settlement propensity x Low credit score." We define control variables in Section 3.1 and omit their estimates for brevity. For all specifications, we cluster standard errors at the judge level and report them in parentheses. */**/*** denotes coefficients which are statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively.

Panel A. Access to credit

	Prime Credit	New Accounts	Credit Inquiries	Ratio
	(1)	(2)	(3)	(4)
Settlement	0.15	-0.65	-0.41	-0.41
	(0.11)	(0.77)	(1.40)	(0.31)
Controls	Yes	Yes	Yes	Yes
N Obs	82,218	82,218	82,218	82,218
N Clusters	43	43	43	43

Panel B. Differential effects by credit score group

	Prime Credit	New Accounts	Credit Inquiries	Ratio
	(1)	(2)	(3)	(4)
Settlement	0.17	-0.27	-0.01	-0.32
	(0.140)	(0.529)	(0.733)	(0.250)
Settlement x	0.00	-1.14	-1.04	-0.30
Low Credit Score	(0.128)	(0.751)	(2.419)	(0.245)
Controls	Yes	Yes	Yes	Yes
N Obs	82,218	82,218	82,218	82,218
N Clusters	43	43	43	43

Table 8. Long-run access to credit.

This table reports IV estimates of the coefficient on settlement in Equation 4 where dependent variables are cumulative measures of access to credit. We cumulate dependent variables over different horizons, from *T*=2 years after disposition to *T*=4 years after disposition. Within each panel, each cell reports the second-stage IV estimate for a specification where the dependent variable is indicated in the column, for the horizon indicated in the row. The dependent variables are whether prime credit represents whether the borrower is prime credit as of year *T* (column 1), the cumulative flow of net new accounts and credit inquiries through year *T* (columns 2-3), and the ratio of the cumulative flow of new accounts to inquiries (column 4). Within a given horizon, the number of observations and clusters is the same. Observations and clusters decline as horizons increase as later cohorts drop out from our analysis. We define control variables in Section 3.1 and omit their estimates for brevity. For all specifications, we cluster standard errors at the judge level and report them in parentheses. */**/*** denotes coefficients which are statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively.

Horizon:	Prime Credit (1)	New Accounts (2)	Credit Inquiries (3)	Ratio (4)	N obs	N Clusters
T=2	0.17	-1.14	-0.53	-0.38	80,525	40
	(0.152)	(1.103)	(2.356)	(0.304)		
T=3	0.22	-0.35	-0.51	-0.01	77,572	39
	(0.207)	(1.092)	(2.805)	(0.229)		
T=4	0.19	-0.11	0.54	0.03	67,237	37
	(0.186)	(0.948)	(3.429)	(0.173)		

Table 9. Recovery rates.

This table reports summary statistics for the proportion recovered from borrowers in various subsamples. For the Missouri court data, the recovery rate is defined as the total amount paid in garnishment divided by the total judgment amount for the sample of cases where both garnishment and judgment amounts are recorded by the courts. For data from the large national collector, the recovery rate is defined as the total amount paid by the borrower after the debt was purchased divided by the balance owed when debt was purchased, top coded to 1 (the ratio can be greater than 1 due to payment of interest accrued subsequent to the debt being purchased). "Sued" refers to borrowers who were sued in court by the collector (non-missing filing date), "Settled" refers to borrowers who settled out of court, "Judgment" refers to borrowers for whom the collector obtained a court judgment (non-missing judgment date), and "Payment>0" refers to borrowers who paid a positive amount to the collector, either directly or through garnishment. A borrower is considered to have settled if the collector sued them, never obtained a court judgment, yet changed their account status to "Paid Off." The column labeled "t-Test" reports a t-statistic associated with a two-sample t-test for equality of the means (clustered by state in rows 3 and 4), where the two samples are the row where the t-stat is reported and "Sued ^ Settled." */**/*** denotes differences in means that are statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively.

		Recovery rate				
	Sample	Mean	SD	Median	N	t-Test
	Missouri Court Data (Main Sample)					
(1)	Sued ^ Judgment	0.38	0.45	0.06	40,057	
(2)	Sued ^ Judgment ^ Payment>0	0.71	0.37	1.00	21,680	
	Large National Collector: All States					
(3)	Sued ^ Judgment	0.43	0.45	0.18	12,892	29.76***
(4)	Sued ^ Judgment ^ Payment>0	0.68	0.39	1.00	8,105	14.56***
(5)	Sued ^ Settled	0.84	0.22	1.00	1,368	
	Large National Collector: Missouri					
(6)	Sued ^ Judgment	0.47	0.46	0.29	353	4.80***
(7)	Sued ^ Judgment ^ Payment>0	0.66	0.41	1.00	251	2.50**
(8)	Sued ^ Settled	0.83	0.25	1.00	38	

Table 10: Collector experience, uncertainty over court outcomes.
Panel A reports IV estimates for Equation 4 for delinquency, bankruptcy, foreclosure, but where we include an indicator for whether the collector is highly experienced as well as its interaction with settlement ("Settlement x High collector experience"). We define a collector as highly experienced if it is in the top 1% of case filers in our sample. We employ a similar instrumental variables strategy where we additionally instrument the interaction of "Settlement x High collector experience" with "Judge settlement propensity x High collector experience." Panel B reports IV estimates of Equation 4 for delinquency, bankruptcy, and foreclosure, but where we exclude consumers who went to court and had their case dismissed. We define control variables in Section 3.1 and omit their estimates for brevity. For all specifications, we cluster standard errors at the judge level and report them in parentheses. */**/*** denotes coefficients which are statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively.

Panel A. The effect of collector experience

	Delinquency	Bankruptcy	Foreclosure
	(1)	(2)	(3)
Settlement	0.01	0.06	0.05
	(0.11)	(0.04)	(0.03)*
Settlement	0.20	0.15	-0.04
x High collector experience	(0.10)**	(0.04)***	(0.05)
Controls	Yes	Yes	Yes
N Obs	82,218	82,218	82,218
N Clusters	43	43	43

Panel B. Outcomes excluding court-winners

	Delinquency	Bankruptcy	Foreclosure
	(1)	(2)	(3)
Settlement	0.13	0.11	0.04
	(0.07)*	(0.03)***	(0.01)***
Controls	Yes	Yes	Yes
N Obs	78,302	78,302	78,302
N Clusters	43	43	43

Table 11. Litigated vs non-litigated cases.
This table reports how the large national debt collector acted on several portfolios of debt they purchased from a major national bank between 2009-2011. The data reflects the most recent action and total recovery for each account as of August 2015. The different actions are: "Litigated," "Call or letter," "Warehoused," or "Sold." Standard deviations are in parentheses.

Action:	Litigated	Call or Letter	Warehoused	Sold
N	18,498	11,553	55,349	80,585
Fraction	11%	7%	33%	49%
Recovery fraction	0.37	0.06	0.01	0.00
At the time of most recent action				
Verified borrower has a job	0.30	0.24	0.00	0.04
	(0.46)	(0.43)	(0.07)	(0.19)
Verified borrower has home	0.68	0.77	0.02	0.26
	(0.47)	(0.42)	(0.14)	(0.44)
Successfully pulled credit report	0.89	0.57	0.20	0.78
	(0.31)	(0.50)	(0.40)	(0.41)
At the time of purchase by collector				
Balance at purchase	6,075	6,474	5,003	5,987
	(5,517)	(6,013)	(5,439)	(5,751)
Days since last payment	703	876	944	681
	(362)	(730)	(2,031)	(395)
Borrower age	45	45	44	46
	(1.2)	(1.1)	0.8	(1.1)

Figure 1. Litigation outcomes in Missouri.
This graph plots the frequency distribution for case outcomes in the sample of counties with random judge assignment (N=82,218).

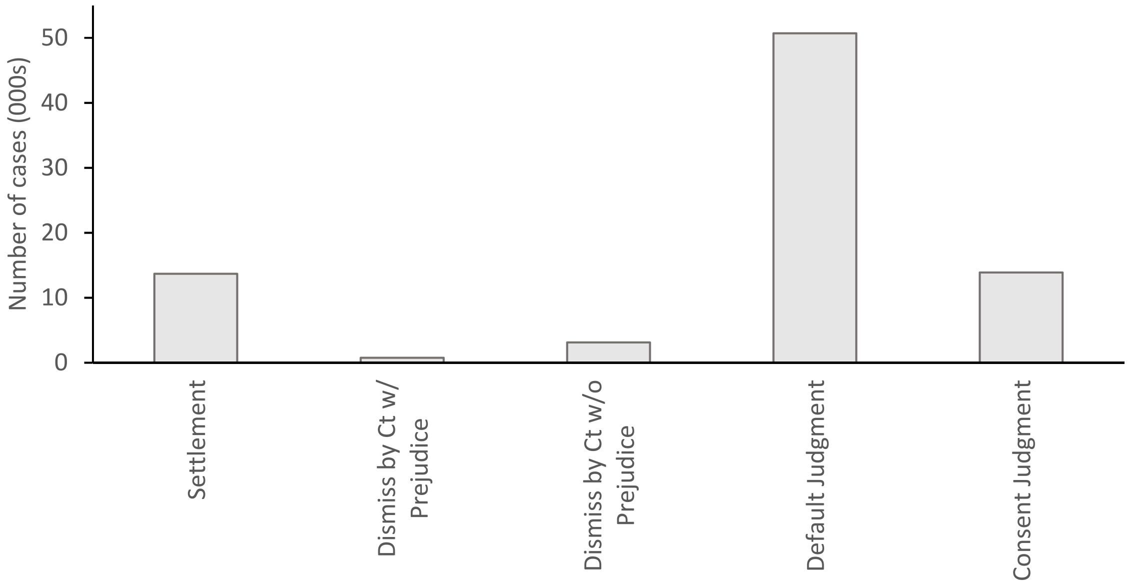


Figure 2. First stage.

This figure plots a settlement indicator vs. our leave-one-out measure of judge settlement propensity. To construct the binned scatter plot, we first regress an indicator for settlement on court-by-year fixed effects and calculate residuals. We then calculate the mean residual in each judge-by-year bin, adding the grand unconditional mean settlement rate to each residual to aid in the interpretation of the plot. The solid line shows the best linear fit estimated on the underlying microdata estimated using OLS. The coefficient shows the estimated slope of the best-fit line including court-disposition year fixed effects, with standard errors clustered at the judge level reported in parentheses.

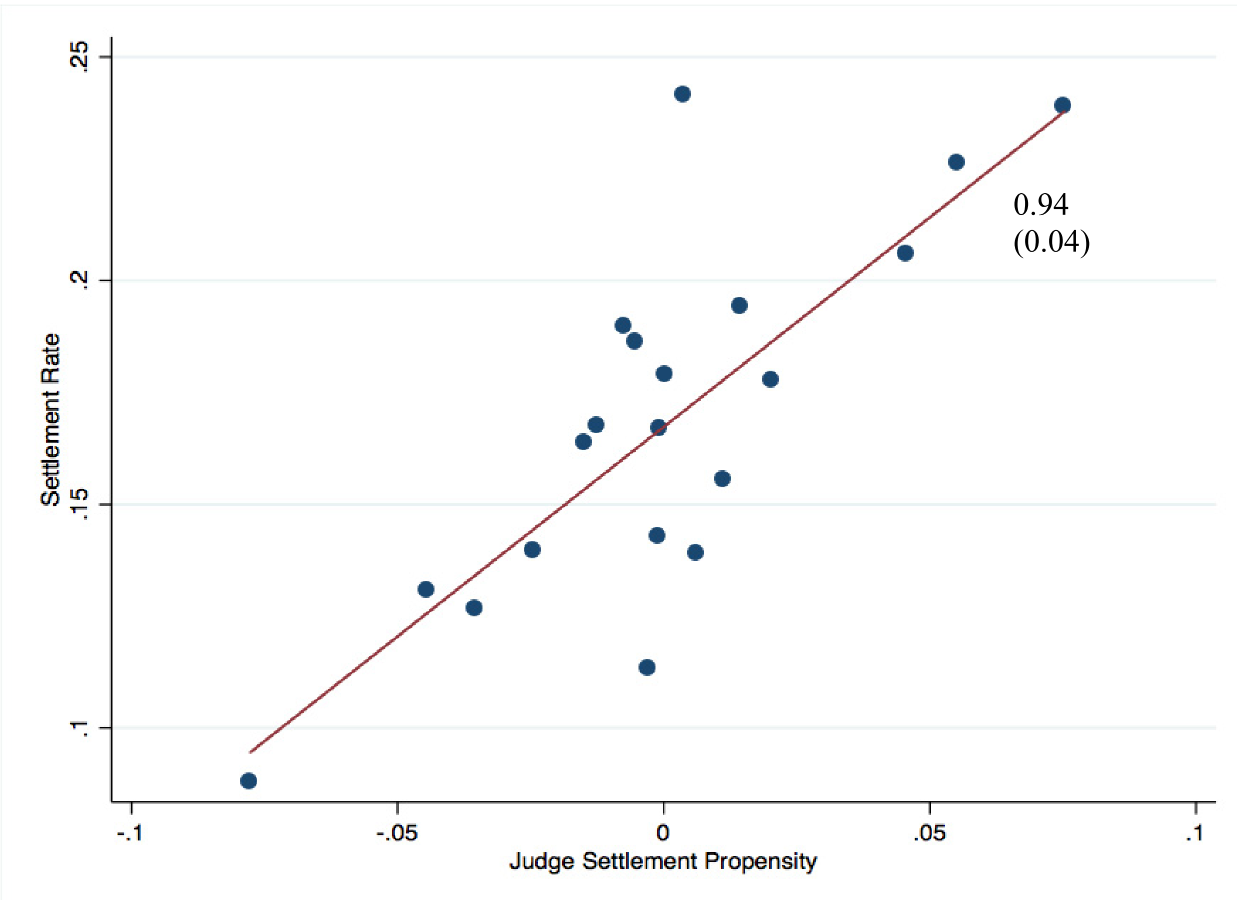


Figure 3. Payment Dynamics.
Panel A plots the mean proportion of the original purchase balance recovered from two subsamples of borrowers at different points following case disposition. The solid lines (point estimate plus/minus 1.96x standard errors) show recovery rates for the subsample that was sued and settled out of court. The dashed lines show recovery rates for the subsample that was sued and lost in court (i.e., the collector obtained a court judgment). A borrower is considered to have settled if the collector sued them, never obtained a court judgment, yet changed their account status to “Paid Off.” The case disposition date is defined as the judgment date for borrowers who lost in court and the first payment date for borrowers who settled and therefore lack a judgment date. Panel B is analogous to Panel A except that it plots the mean proportion of eventual total payments recovered from the two subsamples at different points following case disposition. Only borrowers who eventually make some payments are included in Panel B. Data come from a large national collector as described in Section 2.2.

Panel A: Recovery rates

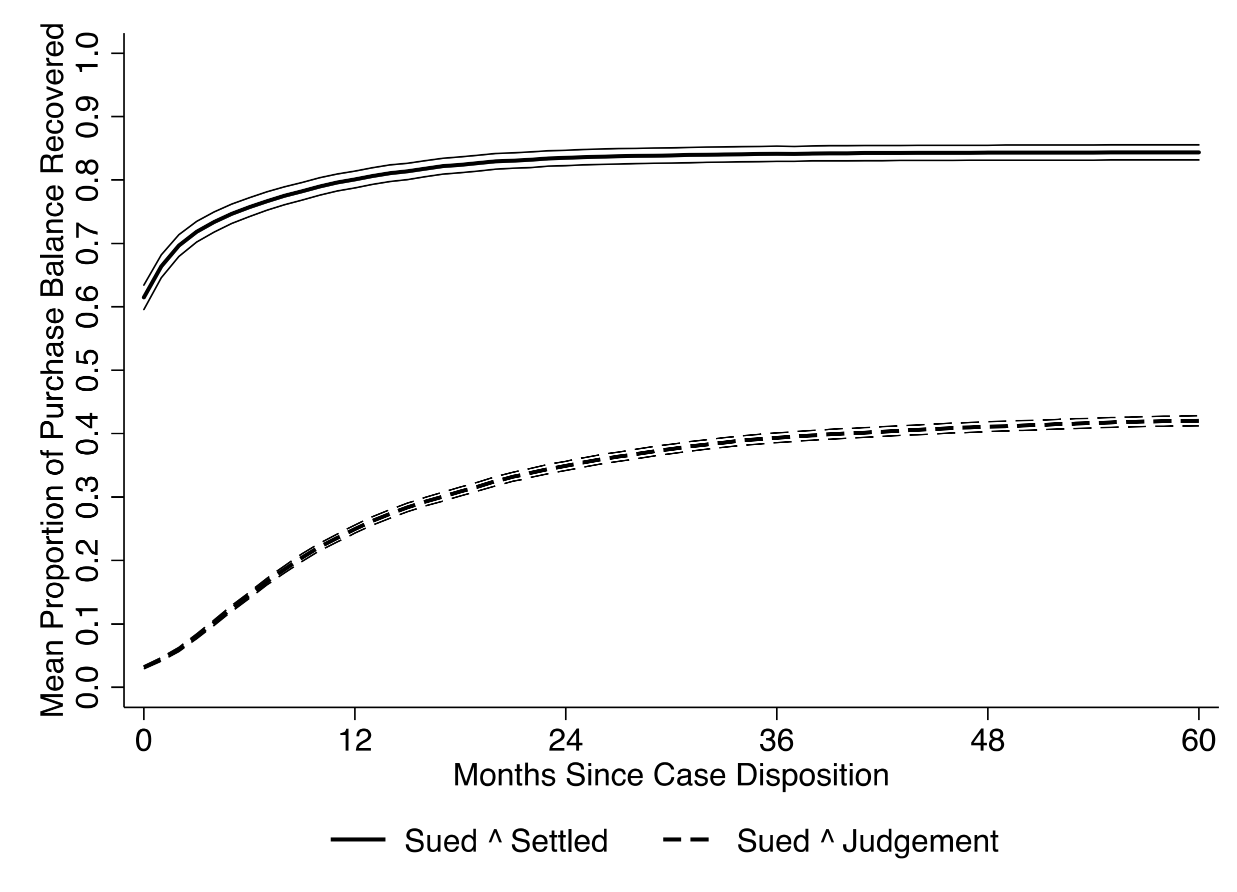
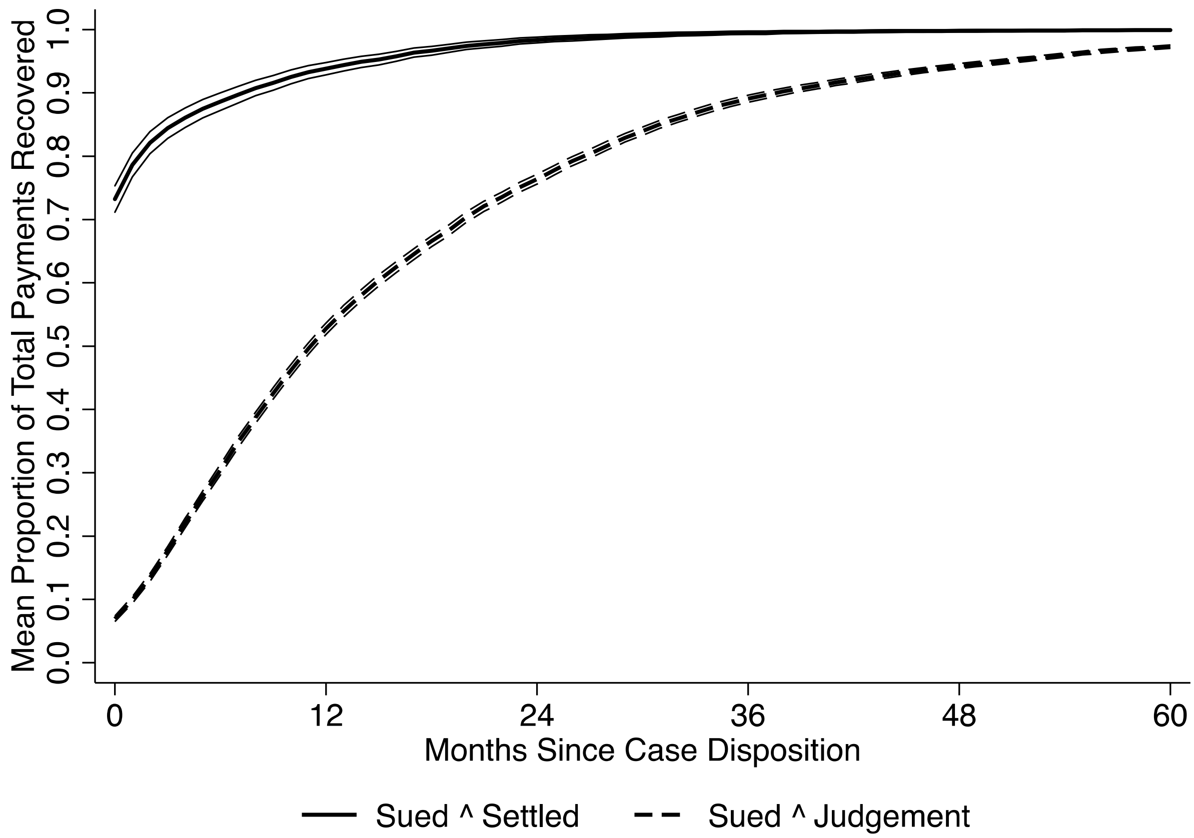


Figure 3, continued.

Panel B: Speed of payments among payers



How do consumers fare when dealing with debt collectors? Evidence from out-of-court settlements

Online Appendix

Table A1. Final sample definition.
This table describes the filters we apply at each stage to arrive at our final sample.

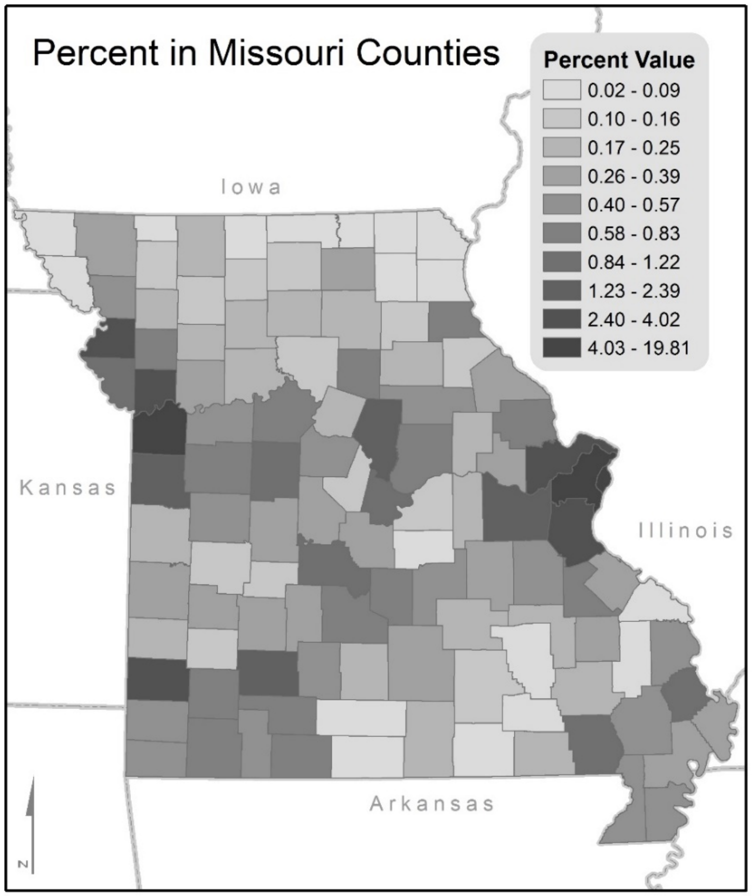
All cases, 2007-2014	667,337
Sample of counties with random judge assignment	203,298
Matched with TransUnion in January before disposition	176,769
…match rate	87.0%
Settlement propensity measure	165,697
Settlement propensity and Matched with TransUnion	143,896
Require t=0 and t=1 presence + Data cleaning	142,038
With lawyer classification	135,989
Cases where borrower was served	82,218
Final matched sample	82,218

Table A2. Persistence of judge settlement propensity.

This table reports estimates of a regression where the dependent variable is the raw judge propensity to settle for a year, and the independent variable is the same propensity in the previous year. Column 1 reports the coefficient with robust standard errors. Column 2 reports the same coefficient but with standard errors clustered at the judge level. Standard errors are reported in parentheses. */**/*** denotes statistically reliably different from zero at the 10%, 5%, and 1% levels, respectively

	Judge Settlement Propensity, t	
	(1)	(2)
Judge Settlement Propensity, t-1	0.662	0.662
	(0.117)***	(0.135)***
N Obs.	141	141
R^2	0.297	0.297
N Clusters		34

Figure A1. Geographical Distribution of Litigation in Missouri Sample
This figure shows the percent of cases that belong to each county in Missouri. The left panel shows the whole sample (N=667,337), while the right one shows the sample matched with TransUnion (N=82,218).



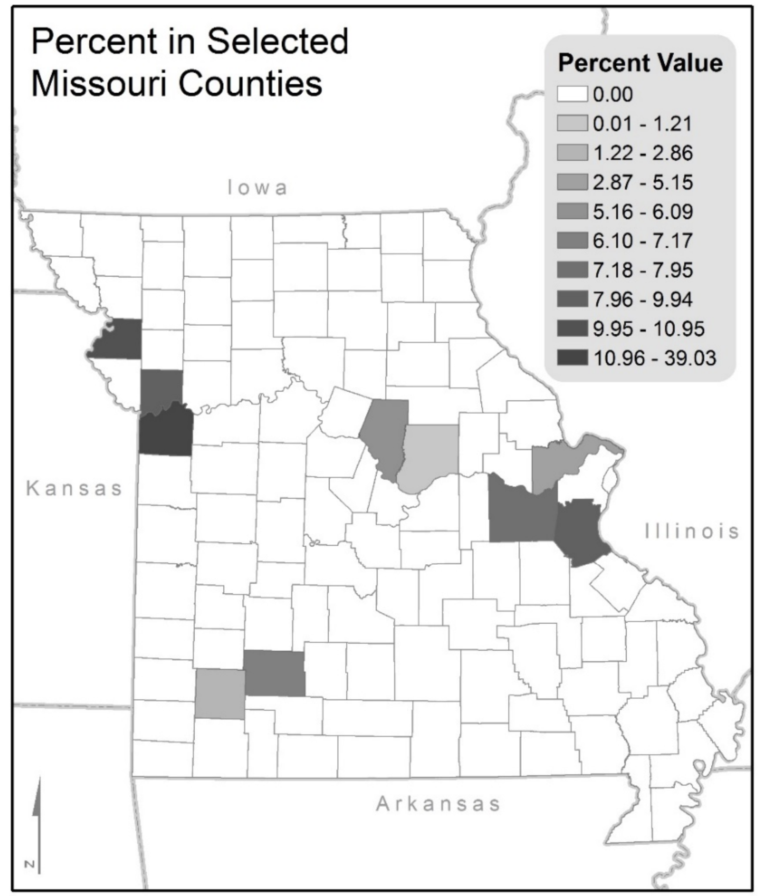
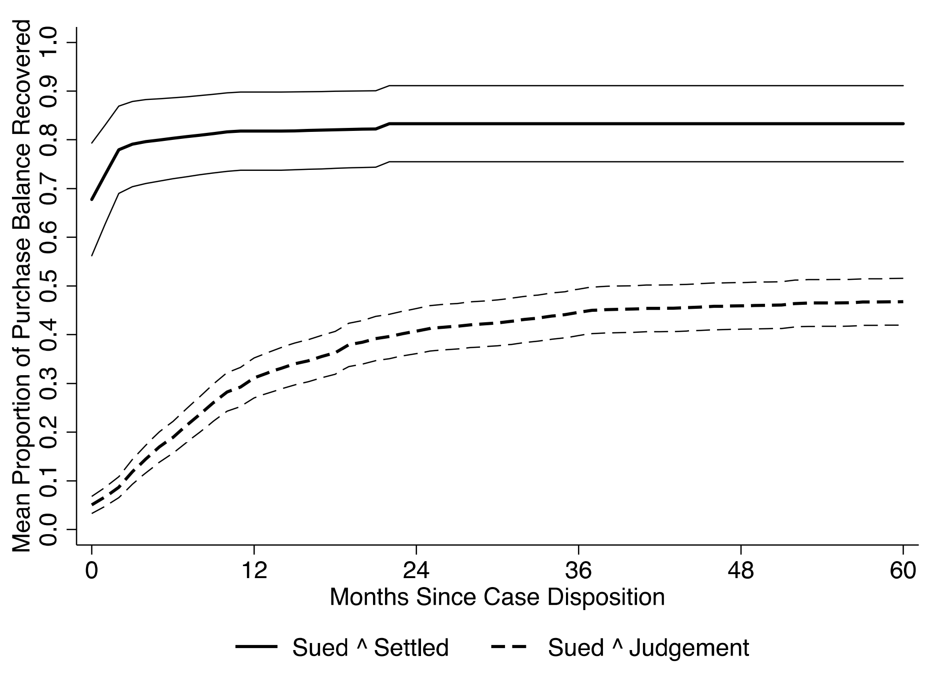


Figure A2. Payment Dynamics--Missouri.
This figure repeats the analysis of Figure 3, limiting the sample to borrowers in Missouri.

Panel A: Recovery Rates



Panel B: Speed of Payments Among Payers