

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

Ing-Haw Cheng, Felipe Severino, and Richard R. Townsend¹

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We test whether consumers experience increased or decreased subsequent financial distress when they settle with debt collectors. We examine new data on civil collection lawsuits where consumers can settle out-of-court. Random assignment of judges with different styles generates exogenous variation in the likelihood of negotiation. Using linked credit registry data, we find evidence that settlements cause increased financial distress, without clearly benefiting consumers through improved access to credit, collector concessions, or avoidance of uncertainty. Consumers experience more distress when making deals with experienced collectors. Overall, the evidence suggests that consumers strike deals that worsen financial distress with unclear benefits.

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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, 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 Consumer Financial Protection Bureau 2018 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, 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. This research received generous financial support from the Tuck School of Business and Alfred P. Sloan Foundation NBER Household Finance small grant program. 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). Collectors often use a combination of litigation and out-of-court negotiation with consumers to resolve delinquent debt. Policymakers have long been concerned about out-of-court negotiations, as consumers may be less sophisticated than collectors or unaware of their legal rights. The Fair Debt Collection Practices Act (FDCPA), Dodd-Frank Act, and Consumer Financial Protection Bureau (CFPB) reflect several of these concerns by regulating the activities of debt collectors.

However, we know little about how negotiated settlements with debt collectors affect consumer financial health. On the one hand, settlements may relieve financial distress by immediately resolving delinquent debt claims and getting consumers back on track. On the other hand, settlements may exacerbate financial distress by requiring large lump sum payments that drain consumers of liquidity, triggering a downward spiral. Ultimately, the question is an empirical one. To fill this gap, this paper studies whether consumers who settle with debt collectors causally experience more or less financial distress compared to consumers who do not. Importantly, if settlements exacerbate financial distress, that might suggest that consumers agreeing to settlements are ill-informed about what would happen to them if they did not settle.

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, on net, consumers who settle causally experience significantly higher short-term 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 it is 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 judges' 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 court 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 among borrowers who draw a high settlement propensity judge versus those who draw a 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 short-term household 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 subsequent delinquency, bankruptcy, and foreclosure by 20%, 160%, and 130% over base rates, respectively. The results are robust across various specifications and sample restrictions. 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 reduce financial distress for consumers? On the one hand, deals typically drain liquidity for consumers, as collectors typically require lump-sum payments in settlements because borrowers lack the ability to credibly commit to a payment plan. Deals may thus worsen distress for liquidity-constrained consumers compared to wage garnishment, which is a payment plan technology that allows collectors to directly extract cash flows using the legal system and conserves consumer liquidity by design. On the other hand, deals may benefit consumers by

reducing distress in two ways: 1) Preserving or increasing access to credit, and 2) Resolving the debt claim at a potential discount. We next consider how deals affect these two potential benefits.

First, a deal might preserve or improve access to credit relative to receiving a court judgment. However, settlement does not significantly affect the probability that a consumer would be classified as prime credit using common industry benchmarks. Furthermore, settlement does not lead to more inquiries on a credit report, suggesting that any such improvement is not significant enough to translate into more realized demand. Finally, the effects of settlement on financial distress and access to credit do not vary with a consumer's pre-case credit score, suggesting that settlement does not reduce distress specifically for consumers who are more distressed ex-ante and who may improve access to credit the most from resolving a debt claim.

Second, consumers may be able to settle the debt at a discount if collectors want to avoid the costs of going forward with a hearing. Our data do not include private settlement amounts, so we instead focus on the recovery rate associated with garnishment. Garnishment involves an implicit haircut for creditors due to several frictions. Collectors may end up garnishing less than the full amount if the garnishment expires, the borrower declares bankruptcy, loses their job, or for other reasons. We estimate that garnishments only recover 15% of judgment amounts, or \$811. Consumers would need to pay less than this amount in a settlement to reduce distress compared to garnishment, assuming a discount rate of zero. We view this possibility as at best unclear considering the large effect of settlement on financial distress.

Overall, our results suggest that settlements with collectors cause increased financial distress, as settlements drain liquidity but have unclear effects on benefits that would reduce distress. Of course, settlements may benefit consumers in ways outside the scope of our credit data, and we caution that we do not reach conclusions about overall consumer welfare. However, given our findings so far, one worry is that unsophisticated or ill-informed consumers agree to deals that are worse than their outside option of having the court hear their case and wages garnished. Consumers may be unaware that garnishment is a court-enforced payment plan that conserves liquidity, and they may deal with collectors who are more informed about the garnishment process.

Two pieces of evidence are consistent with and suggest this worry. First, consumers experience more financial distress when they strike deals with 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 are at a significant disadvantage in negotiations. Second, consumers who settle experience significantly more distress compared with just consumers who lose in court. Higher distress is thus unlikely explained by a benefit of avoiding court-related uncertainty.

We conclude by examining how collectors decide which borrowers to sue, in a bid to better understand the selected population of litigated consumers in our sample. We obtain proprietary data from a large national debt collector that allow us to see which borrowers the debt collector chooses to sue. We find that the collector is more likely to litigate when the collector can successfully verify the consumer has a job and thus a higher ability to pay. Thus, our estimates of the effect of settlement on subsequent financial distress might be even larger among those who were not sued, due to their lower ability to pay. Consumers who are not sued may also deal with other less-scrupulous collectors than those who operate in our legal setting.

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 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; Hynes, 2005, 2008).

Our findings have implications for future research. First, given the scale of collection in the U.S. and the real effects of household financial distress (Dobbie and Song, 2015; Dobbie et al. 2017; Herkenhoff et al., 2016; Musto, 2005; Severino and Brown, 2017), our results point toward

a need for more research in how collection affects household liquidity. Second, our results also point toward a need for more research in how the collection process is plagued by imperfections, both in the settlement process (e.g., potentially reflecting consumer unsophistication, as in Agarwal et al., 2015; Argyle et al. 2018; Beshears et al., 2018; Lusardi and Mitchell, 2014) and frictions in the garnishment process. We comment further on these implications in the conclusion.

1. Hypothesis development

1.1. Collection and settlements

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 each year, 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 (Hynes 2005, 2008).

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).

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 (Fedaseyev, 2015; Fonseca et al., 2018). More recently, the Dodd-Frank Act of 2010 empowered the 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 rights when dealing with collectors (CFPB, 2017). However, concerns persist, largely because a lack of data has limited our understanding of how consumers fare when dealing with collectors (Zinman, 2015).

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 debt 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 with collectors are likely to interact with these frictions in ways that differ from wage garnishment. 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 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. With respect to debt overhang, recent research in mortgages and housing shows that the household debt overhang in the aftermath of the crash in U.S. house prices affected consumption patterns (Dyanan, 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.

Considering these frictions, the effect of settlements on consumer financial distress is ambiguous. Settlements effectively drain liquidity, as debt collectors typically require lump-sum payments due to a lack of the borrower's ability to commit. Wage garnishment, in contrast, effectively enables consumers to commit to a payment plan backed by a court judgment, conserving liquidity. Furthermore, garnishment is limited by law to a percentage of income, and other legal barriers may also restrict garnishment (Hynes, 2005). On the other hand, settlements potentially alleviate debt overhang much more quickly than garnishments, as garnishments prolong the existence of the debt. Settlements could affect financial health through other channels as well, but ultimately the question of how settlements affect financial distress is an empirical one.

Our main contribution is to provide the first micro-level evidence on how out-of-court settlements between consumers and collectors affect subsequent consumer financial distress. Given the scale of collection in the U.S. and the literature suggesting that consumers may be biased or ill-informed in several contexts (see Beshears et al., 2018 and Lusardi and Mitchell, 2014, for reviews), evidence that settlements increase or decrease financial distress informs not only policy concerns but also our understanding of how a significant amount of delinquent debt is resolved.

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 percentage of consumers who are delinquent and the average amount of debt in collections (Ratcliffe et al., 2014). Missouri is also not particularly exceptional with regards to 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 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 court-sanctioned payment plans where a consumer has admitted they owe the debt to the collector. 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 or bank accounts 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 debt holder 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 are often for a lump-sum 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, 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 been settled or heard 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 in 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

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 in the January 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 heard by 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 heard by 43 judges.

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 to, we categorize a case as 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 represent 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 \$737, implying a recovery rate of 25% among cases that go to court. 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

3. Empirical strategy

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

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:

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

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 before 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, and 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 : $E[S_i u_i | X_i, J_{ijcs}] \neq 0$, biasing the OLS estimates. Underlining this concern, Table 2 columns 1-3 suggests 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:

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

where n_{jcs} is the number of cases judge j in court c hears in year s and n_{cs} is the number of cases heard by 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:

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

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:

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

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: $E[SP_{ijcs} u_i | X_i, J_{ijcs}] = 0$. 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 judgement against a consumer, which we compute analogously to 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. First stage

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. The additional columns add controls. As can be seen, we estimate a strong positive relationship between judge settlement propensity and settlement. The F -statistic, shown below the estimated coefficients, is 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, lending credence to the idea that it is a judge-specific effect.

4.2. IV estimation

Table 4 Panel A reports our main 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 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, this could be driven by unobserved differences between borrowers who settle and those who do not. For example, borrowers who settle may be wealthier and less likely to declare bankruptcy, as Table 2 suggests.

Columns 2, 4, and 6 of Table 4 Panel A reports second-stage results from the instrumental variables estimation. 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 base 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.

Figure 3 explores the dynamics of the effects over a longer time horizon by reporting IV estimates of Equation 4 separately in each of the 4 years before and after case disposition. We

primarily find an effect in the first year after case disposition and then point estimates decline each subsequent year. This is consistent with consumers not just making deals that have long-term financial consequences, but deals that have immediate consequences for financial distress.

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 would have on average experienced lower rates of delinquency, bankruptcy, and foreclosure 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.

Considering only these variables, consumers who settle incur more financial distress, leading to higher rates of subsequent bankruptcy and foreclosure. Generally, only consumers who face severe financial distress seek bankruptcy relief or face foreclosure. For example, financial 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 making a deal with a collector increases distress to economically high levels.

4.3. Robustness

4.3.1. 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 it.

Equation 4 and the main results in Table 4 directly control for J_{ijcs} , the rate at which the 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 5 Panel A, Columns 1-3 show that our results remain very 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.³

4.3.2. 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, it would be worrisome if these cases somehow drove our results. To address this, Table 5 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.

4.3.3. Debt Balances. Table 5 Panel C explores how distress manifests itself by estimating Equation 4 with dollar credit balances as left-hand side variables. Settlement causes borrowers' mortgage balances to increase by a point estimate of \$21,032 (reliably different from zero at the 1% level), potentially to finance their settlement or post-settlement expenses. This is consistent with the higher rates of foreclosure in Table 4. Point estimates suggest that settlement leads borrowers to increase their revolving balance by \$854, but this estimate is not statistically reliably different from zero. Note that the increase in credit balances does not necessarily indicate that consumers who settle are accessing more credit, as they may be paying down debt more slowly (e.g., by missing mortgage payments).

4.4. Why don't deals reduce distress?

³ We measure political affiliations by searching for contributions to political parties and election candidates using the online resources such as followthemoney.org and the Federal Election Commission website. We include dummy variables for Democrat, Republican, and Independent, and the omitted category is "No Contribution Data."

Deals may benefit consumers by reducing distress in two ways: 1) Preserving or increasing access to credit, and 2) Resolving the debt claim at a potential discount. We next consider how deals affect these two potential benefits that should reduce distress.

4.4.1. Access to credit. Table 6 examines whether settlements lead to more access to credit. First, we test whether settlement causes consumers to have a credit profile that would be classified as prime credit using common industry benchmarks. We estimate Equation 4 with an indicator for whether a consumer would satisfy such a benchmark as the left-hand side variable, and Column 1 of Panel A reports the results. The coefficient on settlement is 0.15 but with low statistical reliability. Second, we examine whether there is a causal effect of settlement on mortgage and non-mortgage inquiries. The key benefit of a better credit report is that consumers can open accounts on better terms, or at all, as lenders can see a consumer's credit report through inquiries. If settlement led to a large improvement in credit profiles, it is reasonable to expect that settlement should lead to more inquiries to a consumer's credit report compared to consumers who go to court. Columns 2 and 3 of Panel A show that there is no effect of settlement on inquiries.

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 differently. For example, high-score consumers may benefit from settlement more than lower-score consumers if a judgment leads to a larger fall in their credit score. Table 6 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 "Judge settlement propensity x Low credit score." Columns 1-3 show that financial distress affects both high and low credit score groups, while Columns 4-6 show that neither group benefits significantly from settlement in terms of becoming prime credit or having higher realized credit demand.

Overall, these results suggest that consumers who settle do not significantly improve their credit profile. This is perhaps not too surprising given that our sample is largely comprised of consumers with credit scores that are already extremely low. Liberman (2016) finds that

consumers are willing to pay a lot to preserve their credit reputation. Our results would also be consistent with this idea. However, in our context, consumers ultimately do not significantly improve credit profiles, but rather risk further financial distress.

4.4.2. Concessions. Settlements may reduce distress if settlement amounts fall significantly below the present value of garnishment amounts. The Missouri court data tracks garnishment cash flows, but not the dollar amount of private settlements. Instead, we estimate the recovery paid from garnishment. We assume a discount rate of zero so that our estimates provide upper bounds on the present value of recovery amounts. Collectors may end up garnishing less than the full amount if the garnishment expires, the borrower declares bankruptcy, loses their job, or for other reasons.

The summary statistics from Table 1 suggest that the recovery rate from garnishment is 25% from \$711 paid on \$2,967 judgment amounts. However, these quantities apply to an endogenously selected sample of consumers whose cases end up in court and may differ from quantities faced by consumers who settle. To address this selection, we estimate Equation 4 with the judgment amount and garnishment amount as dependent variables, where we instrument for settlement using the judge settlement propensity score as before. We set the values of these amounts equal to zero for cases that settle, and limit the set of court-resolved cases to those with complete data on judgment and garnishment amounts. The estimated coefficients represent the amount of judgment and garnishment that a consumer causally avoids through settlement.

Table 7 Panel A reports the results. Columns 1 and 2 show that a consumer who settles avoids a judgment of \$5,449 and garnishment amount of \$811. These estimates imply a recovery rate of only 15%. For settlements to benefit consumers, consumers would need to settle for less than these amounts.⁴ We view this possibility as at best unclear considering the large effect of settlement on bankruptcy and foreclosure in Table 4. Furthermore, professional collectors who

⁴ The IV approach in Table 7 Panel A is akin to the selection models of Heckman (1976, 1979). Formally following Heckman (1979) yields judgment and garnishment amounts of \$3,645 and \$719, both of which are statistically reliably different from zero at the 1% level. These estimates imply a recovery rate of 20%.

have already incurred fixed litigation costs arguably face low marginal court costs, potentially limiting the amount they may be willing to concede in settlement.

Columns 3 and 4 provide an alternative analysis excluding cases that have zero recorded garnishment cash flows yet do end in a court judgment. Our interpretation is that garnishment failed in these cases, consistent with several barriers to garnishment (Hynes, 2005).⁵ However, one might worry that consumers made unobserved direct payments to creditors in these cases instead. Excluding these cases yields an estimate of the recovery rate conditional on a successful garnishment. Even these aggressive estimates imply a recovery rate of only 46%, again assuming a discount rate of zero.

5. Are consumers ill-informed about their outside options?

Overall, our results so far suggest that settlements with collectors cause increased financial distress, as settlements drain liquidity but have unclear effects on benefits that would reduce distress. Of course, settlements may benefit consumers in ways outside the scope of our credit data, and we caution that we do not reach conclusions about overall consumer welfare. However, given our findings so far, one worry is that unsophisticated or ill-informed consumers agree to “bad deals” that are worse than their outside option of having the court hear their case and wages garnished. 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, or, more broadly, may be unsophisticated negotiators. We tentatively explore this hypothesis in two ways.

5.1. 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 and may use this advantage these in negotiations and settle

⁵ Consistent with barriers to garnishment during our sample period, Missouri enacted garnishment reforms in 2015 that streamline the garnishment process for collectors.

on terms that are less favorable to consumers, leading to more financial distress than when consumers settle with inexperienced collectors. 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. This alternative would predict that consumers who settle with experienced collectors experience less financial distress. Ultimately, the question is an empirical one.⁶

In Table 8 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 at a significant disadvantage in negotiations.

5.2. Reduction in court-related uncertainty

One alternative is that consumers are well-informed about their outside options and strike deals that incur more distress yet benefit them in other ways. Many such benefits outside the scope of our analysis. However, one key benefit of settling may be to avoid court-related uncertainty. For example, risk-averse consumers may be willing to accept the risk of worse outcomes in exchange for a 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 8 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

⁶ 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.

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 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.

6. External validity

6.1. Litigated vs Non-litigated debt collection

As stated earlier, approximately 14% of consumers with a credit file have at least one account in third-party collection each year. These consumers routinely negotiate deals with collectors, often without litigation. Given that our sample consists of litigated consumers, one important question is to what extent litigated consumers differ from non-litigated consumers, and how the effect of settlement might differ across these groups.

To explore this issue, we obtained data from a large national debt collector on how they acted on several portfolios of debt they purchased from a large national bank between 2009-2011. The data describes the most recent action and total recovery for each account as of August 2015. The accounts in the portfolios span several states across the United States.

Table 9 reports summary statistics for these accounts. For each account, the collector either litigated (14%), called or lettered the borrower without subsequent litigation (5%), warehoused the debt (33%), or sold the debt on to another collector (49%).

The overall picture from Table 9 is that the collector litigates cases for “better off” consumers where the collector could successfully verify the consumer had a job, home, and the consumer’s credit report. If the collector could only verify the consumer owned a home, but could not verify a job or credit report, the collector tended to call or letter the consumer but not subsequently 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 \$7,000 for non-litigated “call or letter” cases. Litigated cases had the highest recovery rate (32%) of any other type of case.

If collectors litigate consumers who are better off, 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. Moreover, the selected group of debt collectors in our sample who litigate likely engage in fewer questionable practices than others given that they operate through the legal system. Ill-informed or unsophisticated consumers who settle with less-scrupulous collectors that do not operate through the legal system may experience even more financial distress.

6.2. 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.

First, given that settlements cause costly financial distress but have unclear benefits in our sample, one worry is that litigated consumers in the broader population are well-informed and would refuse these types of deals outside our setting. 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 be the case, it would have to be that consumers are actually well-informed but drawing a high settlement propensity judge causes them to become ill-informed. 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 rights 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 claim that we have estimated the LATE, 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.

6.3. 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 (a treatment assignment in our design) is random (e.g., Heckman, Urzua, Vytlačil, 2006). In our context, however, sorting on gains would imply that the *best* effect of settlement for consumers is to cause more financial distress with unclear benefits.

7. Conclusion

Overall, our results suggest that settlements with collectors cause increased financial distress, as settlements drain liquidity but have unclear effects on benefits. Our results dovetail with a literature emphasizing the importance of liquidity for consumers, and highlight a need for more research in debt collection given the immense scale of collection in the U.S. Furthermore, given the importance of collection for well-functioning credit markets, future research should examine the role of imperfections in the collection process. Our results suggest that there are worries about both consumer financial literacy and sophistication as well as frictions in garnishment for collectors. A fruitful area for future research would be to examine whether smoothing out these imperfections can improve borrower and creditor/creditor outcomes.

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Table 1. Summary statistics.

Panel A reports case-level summary statistics. “Settlement rate” is the fraction of cases per county that settled, $\Pr(\text{Dismiss} \mid \text{Court})$ 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 | 3,947 | 1,573 | 40,063 |
| Garnishment paid (\$) | 737 | 1,367 | 118 | 40,063 |
| 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 (1) | Court (2) | p-value (3) | Low (4) | High (5) | p-value (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 estimation.

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 without including controls, while Column 2 reports estimates 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} . Column 3 reports the first stage but uses continuous controls for to illustrate the impact and directions of the correlations. 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 S_i | | |
|-------------------------|----------------------------|-------------------|---------------------|
| | (1) | (2) | (3) |
| SP_{ijcs} | 0.94 (0.04)*** | 0.82 (0.06)*** | 0.81 (0.06)*** |
| J_{ijcs} | | | 0.15 (0.07)** |
| Credit Score | | | 0.008 (0.007)*** |
| Age | | | -0.063 (0.167) |
| Homeowner | | | 0.046 (0.003)*** |
| Days to disposition | | | 0.66 (0.106)*** |
| Previous bankruptcy | | | 0.25 (0.02)*** |
| Controls | No | Yes | Yes |
| N | 82,218 | 82,218 | 82,218 |
| R ² | 0.051 | 0.157 | 0.145 |
| Clusters | 43 | 43 | 43 |
| First-stage F-statistic | 737 | 94 | 160 |

Table 4. Second-stage estimation.

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, 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 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 (1) | IV (2) | OLS (3) | IV (4) | OLS (5) | IV (6) |
| Settlement | 0.000 (0.005) | 0.13 (0.08)* | -0.047 (0.004)*** | 0.11 (0.03)*** | -0.012 (0.001)*** | 0.04 (0.01)*** |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 82,218 | 82,218 | 82,218 | 82,218 | 82,218 | 82,218 |
| Clusters | 43 | 43 | 43 | 43 | 43 | 43 |

Table 5. 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. Panel B controls for an indicator for whether a given case concluded with default judgment. Panel C replaces the dependent variable in Equation 4 with the dollar balance of revolving and mortgage accounts. We drop individuals with extremely large mortgage balances. 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 (0.08) | 0.12 (0.03)*** | 0.03 (0.01)*** | 0.12 (0.08) | 0.12 (0.03)*** | 0.04 (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 (0.11) | 0.18 (0.06)*** | 0.06 (0.02)*** |
| Controls | Yes | Yes | Yes |
| N Obs | 82,218 | 82,218 | 82,218 |
| N Clusters | 43 | 43 | 43 |

Panel C. Debt Balances

| | Revolving Balance | | Mortgage Balance | |
|------------|-------------------|----------------|-------------------|----------------------|
| | OLS (1) | IV (2) | OLS (3) | IV (4) |
| Settlement | -299 (156)* | 854 (4,492) | 4,920 (553)*** | 21,032 (7,032)*** |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 78,108 | 78,108 | 78,108 | 78,108 |
| N Clusters | 43 | 43 | 43 | 43 |

Table 6. 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) and the number of credit report inquiries (non-mortgage and mortgage) in the January after the case is disposed (columns 2 and 3). Panel B reports IV estimates of Equation 4 for delinquency, bankruptcy, and foreclosure where we include 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 (1) | Non-Mortgage Inquiries (2) | Mortgage Inquiries (3) |
|------------|---------------------|----------------------------------|------------------------------|
| Settlement | 0.15 (0.11) | -0.55 (0.591) | 0.01 (0.017) |
| Controls | Yes | Yes | Yes |
| N Obs | 82,218 | 82,218 | 82,218 |
| N Clusters | 43 | 43 | 43 |

Panel B. Differential effects by credit score group

| | Delinquency (1) | Bankruptcy (2) | Foreclosure (3) | Prime Credit (4) | Non- Mortgage Inquiries (5) | Mortgage Inquiries (6) |
|----------------------------------|--------------------|---------------------|---------------------|------------------------|--------------------------------------|------------------------------|
| Settlement | 0.0824 (0.068) | 0.134 (0.024)*** | 0.0231 (0.011)** | 0.168 (0.140) | -0.52 (0.268)* | 0.0323 (0.033) |
| Settlement x Low Credit Score | 0.0402 (0.244) | -0.0352 (0.076) | 0.0243 (0.042) | -0.001 (0.128) | -0.033 (0.958) | -0.061 (0.060) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| N Obs | 82218 | 82218 | 82218 | 82218 | 82218 | 82218 |
| N Clusters | 43 | 43 | 43 | 43 | 43 | 43 |

Table 7: Concessions

This table reports IV estimates of Equation 4 but where the dependent variables are the judgment amount and total amount garnished. Judgment and garnishment amounts for cases that settled are set equal to zero. Among the cases that were resolved by the courts, we restrict the sample to include only those with complete data on judgment and garnishment amounts. We top-code the judgment amount with the consumer's collection balance from TransUnion (if positive) and top-code the amount garnished with the judgment amount. Columns 1 and 2 report the main estimates while Columns 3 and 4 report estimates where we further restrict the sample of court-resolved cases to those that report a positive total garnishment amount. 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.

| | Judgment Amount (1) | Garnishment Amount (2) | Judgment Amount (3) | Garnishment Amount (4) |
|------------|---------------------------|------------------------------|---------------------------|------------------------------|
| Settlement | -5,449 (1,567)*** | -811 (222)*** | -3,913 (1,344)*** | -1,799 (402)*** |
| Controls | Yes | Yes | Yes | Yes |
| N Obs | 53,765 | 53,765 | 35,360 | 35,360 |
| N Clusters | 43 | 43 | 43 | 43 |

Table 8: Are consumers ill-informed about their outside options?

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. 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 (1) | Bankruptcy (2) | Foreclosure (3) |
|---|--------------------|-------------------|--------------------|
| Settlement | 0.01 (0.11) | 0.06 (0.04) | 0.05 (0.03)* |
| Settlement x High collector experience | 0.20 (0.10)** | 0.15 (0.04)*** | -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 (1) | Bankruptcy (2) | Foreclosure (3) |
|------------|--------------------|-------------------|--------------------|
| Settlement | 0.13 (0.07)* | 0.11 (0.03)*** | 0.04 (0.01)*** |
| Controls | Yes | Yes | Yes |
| N Obs | 78,302 | 78,302 | 78,302 |
| N Clusters | 43 | 43 | 43 |

Table 9. Litigated vs non-litigated cases

This table reports how a 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 | 22,572 | 8,307 | 54,521 | 80,585 |
| Fraction | 14% | 5% | 33% | 49% |
| Recovery (%) | 0.32 | 0.05 | 0.01 | 0.00 |
| At the time of most recent action | | | | |
| Verified borrower has job | 0.31 (0.462) | 0.18 (0.384) | 0.00 (0.0657) | 0.04 (0.197) |
| Verified borrower has home | 0.66 (0.473) | 0.79 (0.409) | 0.02 (0.143) | 0.27 (0.442) |
| Successfully pulled credit report | 0.889 (0.314) | 0.210 (0.407) | 0.478 (0.500) | 0.774 (0.418) |
| At the time of purchase by collector | | | | |
| Balance at purchase | 6056.6 (5562.1) | 6842.1 (6122.8) | 5296.3 (5536.6) | 6069.6 (5830.0) |
| Days since last payment | 703.6 (350.9) | 940.7 (850.4) | 942.5 (2127.2) | 679.5 (382.4) |
| Borrower age | 45.36 (1.158) | 44.45 (1.045) | 44.13 (0.741) | 45.62 (1.053) |

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).

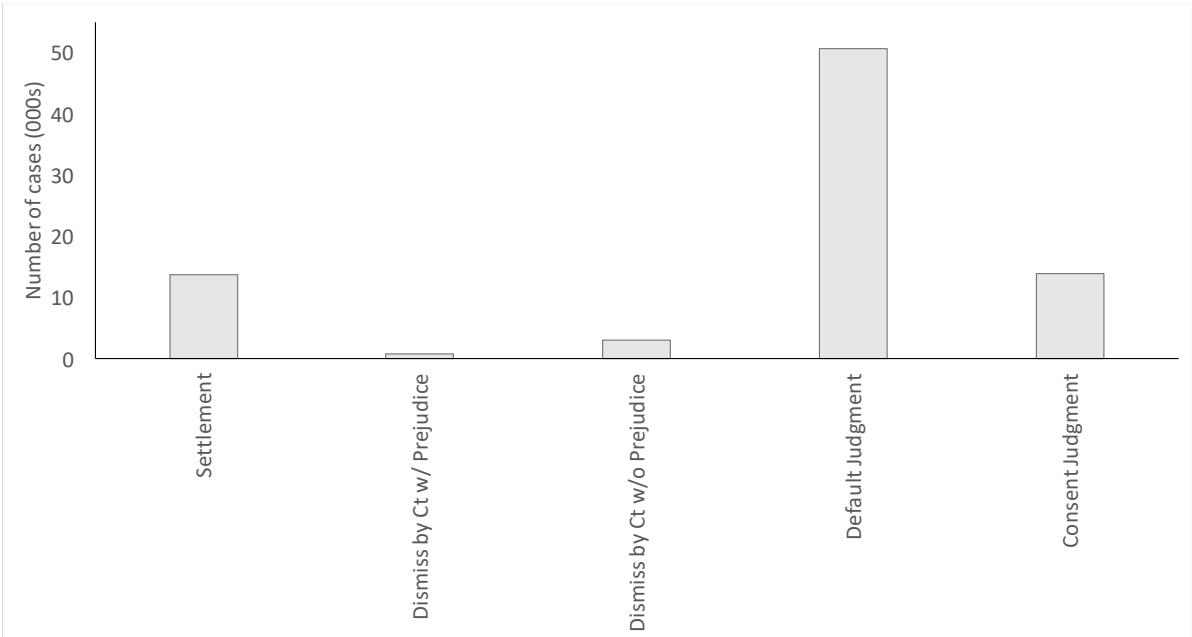


Figure 2. First Stage

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-disposition-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.

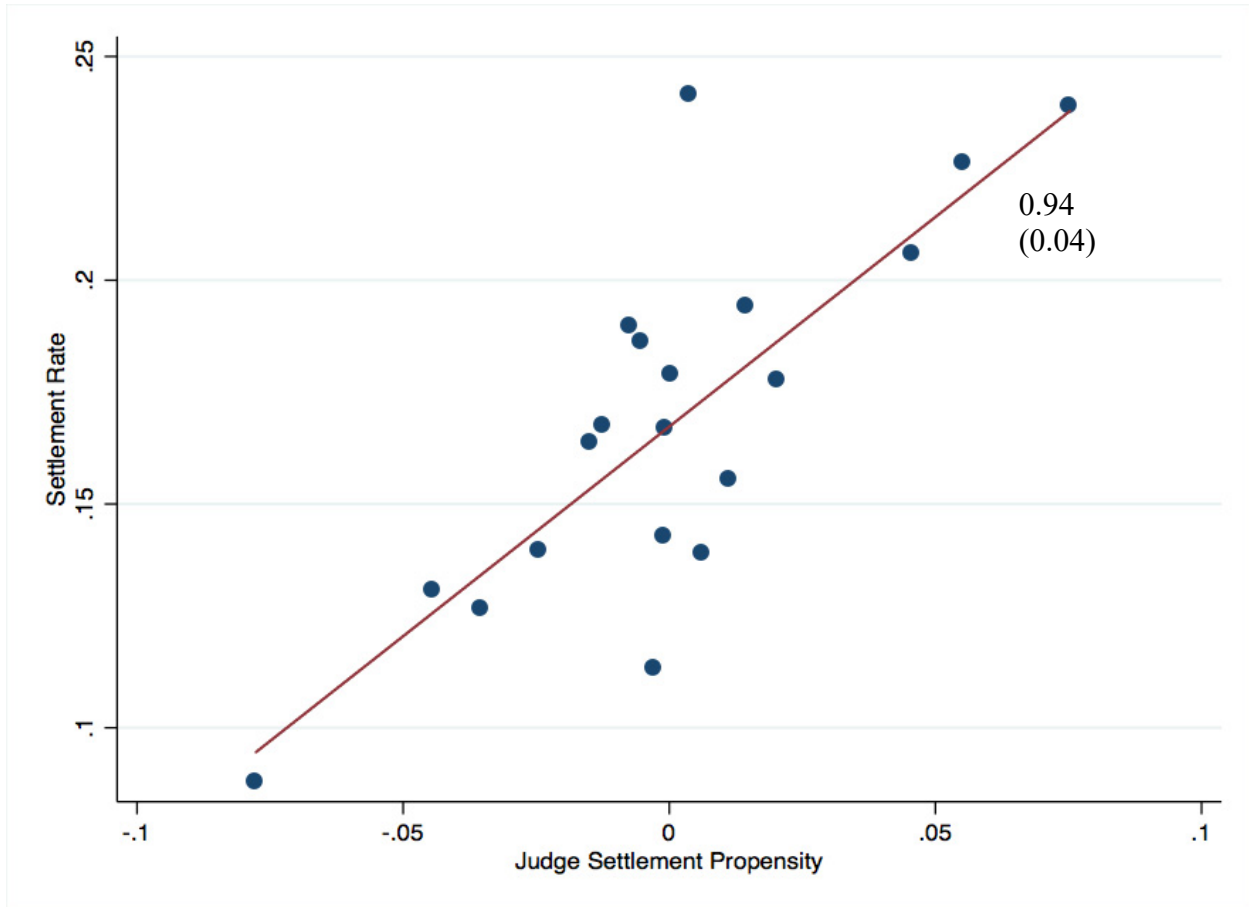
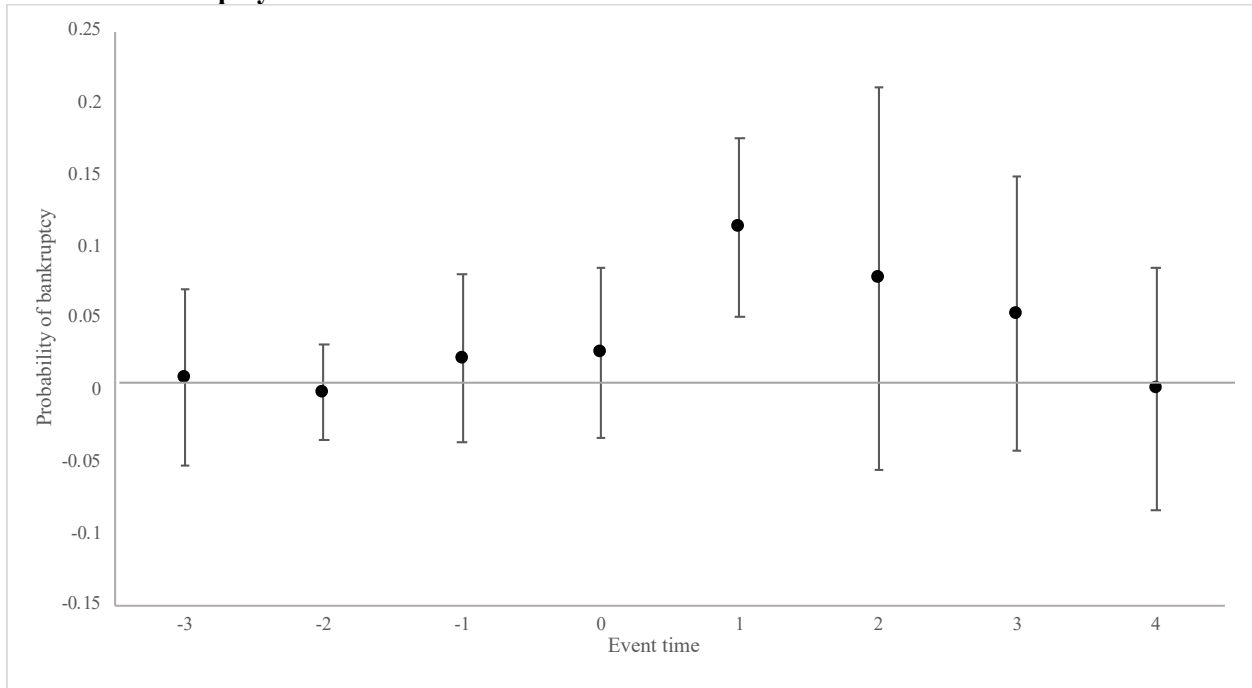


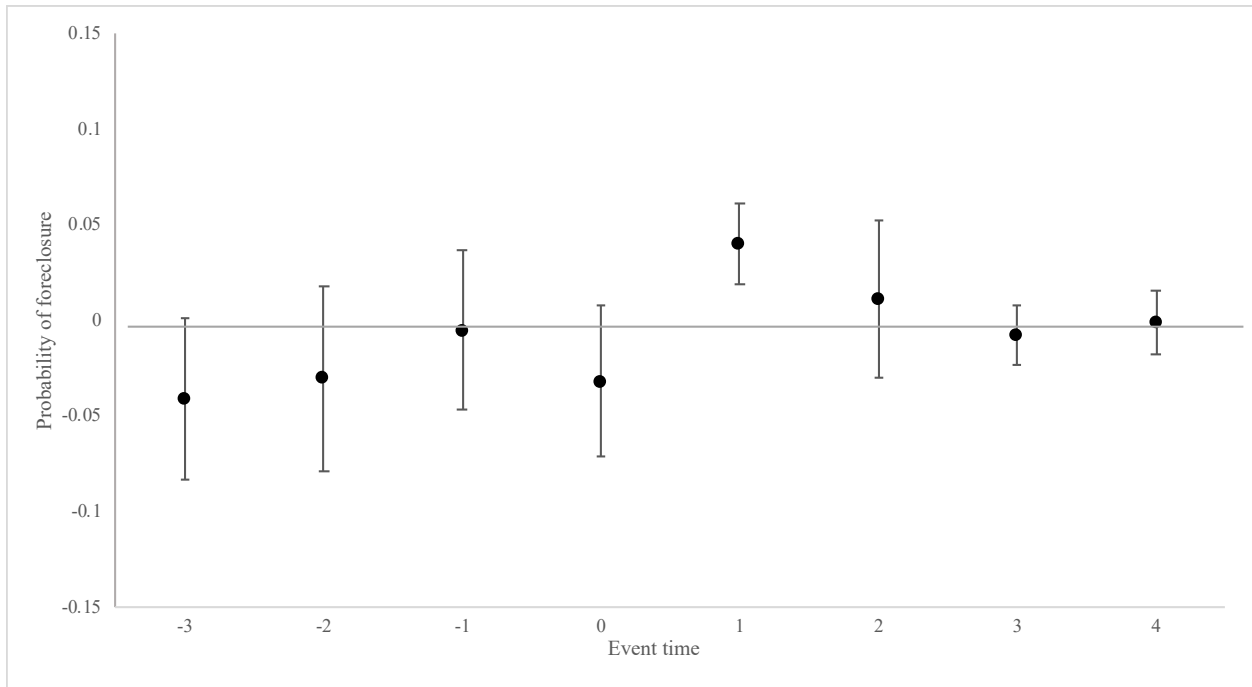
Figure 3. Dynamics

Panel A plots β coefficients from estimating Equation 4 for bankruptcy separately for each event year before and after case disposition. We lag control variables for each year. We measure event years in the January months before and after the case disposition date. Panel B replicates the same analysis for foreclosures. Bars represent 95% confidence intervals, with standard errors clustered at the judge level.

Panel A: Bankruptcy



Panel B: Foreclosure



**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 (0.117)*** | 0.662 (0.135)*** |
| N Obs. | 141 | 141 |
| R | 0.297 | 0.297 |
| Cluster | | 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).

