

Debt Collection and Settlement: Do Borrowers Under-Utilize the Court System?

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Debt contract enforcement is crucial for well-functioning credit markets. For consumer debt, courts allow lenders to garnish wages or foreclose property on severely delinquent borrowers. However, lenders and borrowers also often resolve debt bilaterally, outside of courts. One important function of the debt collection industry is to facilitate this type of resolution. But, there is scant evidence on how consumers fare outside of the court system when they negotiate directly with collectors. To examine this question, we use a unique dataset that links court records from debt collection lawsuits with detailed credit registry data. Randomly assigned judges in our dataset differ in their propensity to preside over cases that end with an out-of-court settlement. Using judge settlement propensity as an instrument, we find that out-of-court settlements on unsecured debt increase the chances of subsequent financial distress for borrowers. The results suggest that the average borrower who settles would be better off going through the court system.

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US households have substantially increased their leverage over the past 20 years, from \$320 billion in 1994 to \$1060 billion in 2010, according to Federal Reserve Flow of Funds data. The Great Recession has brought renewed attention not only to the consequences of this credit expansion, but also to the process of household deleveraging and debt resolution (Dyanan, 2012; Mian and Sufi, 2011; Mian, Rao, and Sufi, 2013). There are several formal mechanisms through which debt resolution can take place within the court system, including wage garnishment, personal bankruptcy, and foreclosure. These mechanisms have been studied extensively in the literature (see, e.g., Dobbie and Song, 2015; Mian, Sufi, and Trebbi, 2015; White, 2005).

However, debt is also often resolved bilaterally, outside of the court system. Indeed, one important function of the debt collection industry is to facilitate this type of resolution. This industry is large and growing. Approximately 14 percent of consumers have been under third-party debt collection in recent years (Federal Reserve Bank of New York, 2015), and the industry collected over \$55 billion in 2013 (Ernst and Young, 2013). Policymakers have long been concerned that the collection process exacerbates financial distress and that households may underutilize available relief. But, there is scant evidence on how consumers fair outside of the court system when they negotiate directly with debt collectors (Zinman, 2015).

This paper aims to fill this gap in our knowledge. There are two major challenges in examining how individuals fair in negotiating deals with debt collectors. The first challenge is one of measurement; it is tough to observe when these deals occur and also to observe subsequent individual-level outcomes. The second challenge is one of identification; individuals who make a deal with a debt collector outside of court may be unobservably different from those who go through the court system. These unobservable differences may then independently effect subsequent outcomes.

To address these issues, we assemble a unique dataset, starting with court records from Missouri debt collection lawsuits from 2007-2014. Most prior research on debt resolution uses court records from Federal bankruptcy courts. However, a much larger volume of debt-related cases are tried in state civil courts. In these cases, a debt holder sues a borrower for breach of contract or some related offense. If the debt holder wins the case, they are then entitled to, among other things, garnish the borrower's wages or bank accounts up to certain statutory limits in order to recover principal and interest owed. We focus on Missouri because, unlike most states, it has a centralized database of cases tried in different circuit courts. It is also a fairly representative state in terms of collection (Ratcliffe et al., 2014).

From the court records, we are able to observe cases that concluded with a ruling in favor of one party or the other, as well as cases that concluded with an out-of-court settlement between the two parties. These out-of-court settlements are typically associated with a lump-sum payment from the borrower to the lender. We are interested in examining whether borrowers end up doing better or worse when they settle than they would have done in court. In order to examine borrower outcomes subsequent to the case, we link the court records with detailed credit registry data from TransUnion. This allows us to observe a rich set of outcomes, including ones associated with financial distress.

Of course, as mentioned before, settlement is endogenous. For example, it may be that individuals who settle have more financial resources and thus appear to do better subsequently than those who go through the court system. Alternatively, it may be that individuals who settle are less sophisticated and thus appear to do worse subsequently. In order to address this issue, we exploit the fact that, in many Missouri state courts, judges are randomly assigned to cases. Empirically, some of these judges are significantly more likely than others to preside over a

settlement. The tendency for a given judge to preside over a settlement is also persistent over time. We attribute this phenomenon to differences in the style of different judges. In particular, 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, some judges suggest the two parties speak in a side room prior to the hearing, while others do not. Given that judges are assigned to cases randomly; this provides random variation in the probability that a case will end in a settlement.

We begin by verifying that judges in our sample do appear to be randomly assigned, as described in the court procedure documents we obtained as well as in our conversations with the court clerks. Consistent with random assignment, we find no significant differences in the year prior the disposition of the case among borrowers that draw a high settlement-prone judge vs those that draw a low settlement-prone judge. These borrowers have very similar credit scores, revolving balances, mortgage balances, etc.

However, after the case is disposed, differences emerge. More precisely, when we instrument for settlement using the settlement-propensity of the judge, we find that settlement has an effect on a variety of outcomes. First, settlement affects some outcomes in the way one would expect, given that it is a form of debt resolution. In particular, settlement increases credit scores and decreases collection balances, repossessions, and liens.

Most interestingly, however, we also find evidence that settlement causes an increase in household financial distress. In particular, it significantly increases subsequent bankruptcy and foreclosure probabilities. We interpret this to mean that the individuals who were randomly induced to settle through the judge they drew, would have been better off going through the court system, on average. This may be, in part, because some may have won their case, but it is also

likely because even for those who would have lost, the legal system would have provided protections. For example, there is a maximum percentage of any given paycheck that can be garnished. Borrowers who settle outside of court may agree to a lump-sum payment that ultimately is more than they can handle financially.

The effects are economically important. In particular, we find that settlement roughly triples the probability of bankruptcy and doubles the probability of foreclosure, with the effects concentrated in the first year after disposition. The results are also robust across various specifications and sample restrictions. For example, we find similar results when we exclude dismissals from the sample as well as when we exclude households that file for bankruptcy within the six months after disposition.

To understand more about the mechanism underlying these results, we explore the effects of settlement on various types of debt balances and inquiries. We find that settlements are linked to increases in mortgage balances and the number of secure trades. This suggests that, on average, borrowers use secured debt and collateralized loans to be able to pay the lump-sum settlement.

Finally, we explore whether there is heterogeneity along various dimension in effects that we document. We find that the effect of settlement on bankruptcy is stronger when the borrower has a lower credit score. This is consistent with the idea that these borrowers may be less able to afford to make a large lump-sum payment than others. We also find that effect is stronger when the debt holder is a bank. This may be because banks are better able to extract settlements.

Of course, the causal effects that we estimate pertain only to the selected population in our sample, namely those who debt collectors chose to take legal action against, rather than simply call and write letters to. To understand how these decisions are made, we obtain proprietary data from a large debt collector. These data allow us to see which borrowers the debt collector chooses

to sue. We find evidence that collectors trade off the fixed cost of litigation versus the expected recovery amount. They sue when balances are large, when they know where to find the borrower, and when they expect borrowers can pay some amount. Thus, if anything, our estimates of the effect of settlement on subsequent financial distress are likely a lower bound, as the effect is likely to be even more significant among those who were not sued due to their lower ability to pay.

This paper relates directly to the existing literature that explores debt collections. Dawsey, Hynes, and Ausubel (2013) and Hynes (2008) examine debt collection from a legal perspective. Fedaseyeu (2014) discusses credit supply, and Fedaseyeu and Hunt (2014) provide stylized facts about the collection industry.

More broadly, this paper contributes to several strands of the literature that focus on debt relief and household outcomes. Previous research has shown that personal bankruptcy can improve earnings, mortality and financial health (Dobbie and Song 2016, Dobbie et al. 2017) and can have real effects on household access to credit (Musto, 2005, Dobbie et al. 2016, Severino and Brown, 2017) as well as entrepreneurship (Herkenhoff et al. 2016). This paper differs in that we study direct negotiation with debt-holders. As far as we are aware, we are the first to examine the effects of out-of-court debt resolution. Given that the number of individuals subject to debt collection in a given year dwarfs the number of individuals resolving debt through the court system, it is essential to understand the consequence of bilateral debt resolution. Our results suggest that the average borrower would be better off going through the court system.

The rest of the paper proceeds as follows. Section 1 describes our data. Section 2 discusses our empirical methodology. Section 3 discusses the results. Section 4 concludes.

1. Data Description and Summary Statistics

To study the effects of direct negotiations with debt collectors, we assemble a unique dataset that contains all court records from Missouri debt collection lawsuits from 2007-2014. In these cases, a debt holder sues a borrower for breach of contract or some related offense. If the debt holder wins the case, they are then entitled to, among other things, garnish the borrower's wages or bank accounts up to certain statutory limits to recover principal and interest owed. We focus on Missouri because, unlike most states, it has a centralized database of cases tried in different circuit courts. It is also a reasonably representative state regarding collection (Ratcliffe et al., 2014). We further limit the sample to only include only cases from courts that randomly assign judges according to their procedures and as confirmed in our interviews with court clerks.

We are interested in examining whether borrowers end up doing better or worse when they settle than they would have done in court. In order to examine borrower 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. To preserve anonymity, the matched data returned to us by TransUnion was stripped of these identifiers. In line with previous research, TransUnion was able to match approximately 70% of borrowers from the court records to their database. The matching sample allows us to observe a rich set of credit-related outcomes, including ones associated with financial distress. We are left with a final sample of 82,218 cases.

From the court records, we can observe cases that concluded with a ruling in favor of one party or the other, as well as cases that ended with an out-of-court settlement between the two parties. Figure 2 shows the type of case rulings included in the analysis: "Settlement" refers to an out-of-court bilateral arrangement and represent about 17% of the outcomes; "Dismiss by Court with

Prejudice,” “Dismissed by Court without Prejudice,” and “Consent Judgment,” represent a small fraction of rulings; “Default Judgement” represents 62% of all rulings.

Table 1, Panel A, shows further summary statistics. Conditional on going to court, the likelihood of a borrower’s case being dismissed is 6%. In case of judgment, 50% of cases receive a garnishment ruling, with the average judgment amount being \$4,149 USD. Missouri’s cap wage garnishment is 10% lower than the federal limit of 25%. The average length of time from filing to disposition is 88 days and the probability of getting another litigation in a year is 9% unconditionally and 8% conditional on settlement.

Out-of-court settlements are typically associated with a lump-sum payment from the borrower to the debt holder. Figure 3 plots the frequency distribution of plaintiff types. The most common plaintiff types are debt buyers and major banks. Debt buyers purchase delinquent debt from credit originators.

Table 1, Panel B, compares characteristics of borrowers in out litigated sample those of borrowers who declare Chapter 13 personal bankruptcy (from Dobbie, Goldsmith-Pinkham, and Yang, 2016) as well as to the population of general population of credit users. Borrowers experiencing debt-related litigation are similar along several dimensions to bankruptcy filers, although their credit scores are lower (536 vs. 580) and, not surprisingly, their likelihood of having a collection flag is higher (76% vs. 47%). Collection balances are also higher than for bankruptcy filers, but revolving balances are smaller. Regarding observables demographic characteristics, their age and homeownership rate look similar to the general population of credit users.

2. Empirical strategy

We are interested in estimating the effect of out-of-court settlement on household distress. Therefore, a naïve empirical design would estimate equations of the form:

$$y_i = \alpha_{cs} + \beta S_i + \Gamma X_i + u_i$$

Where y_i is a household i outcome, S_i is an indicator for whether the case was settled out of court or received a ruling in court, α_{cs} is a court by disposition year fixed effect to account for court specific time-varying trends and X_i is a set of controls that include age bin, credit score bin, days to disposition bin, homeownership status, and previous bankruptcy flags. The problem with the specification above is that it compares borrowers that endogenously chose to settle or not, which can be correlated with other characteristics that affect household financial distress, leading to bias estimates of the effect of out-of-court settlements even with the inclusion of court-disposition year fixed effects.

To overcome this identification challenge, our empirical strategy exploits the fact that specific courts in Missouri assign judges to cases randomly. As stated earlier, we determine courts with random judge assignment by reading court procedure documents that we verify through phone interviews with court clerks. Despite the fact that cases are assigned randomly to judges in these courts, there is significant variation in the probability a judge presides over a case that ends with a settlement (henceforth, “settlement propensity”). Moreover, the settlement propensity is persistent over time. This suggests that judges have different styles and that these styles influence the probability of settlement. Consistent with this idea, Silver-Greenberg (2011) provides several anecdotal examples of how judges can affect settlement. For example, prior to hearing a case, some judges encourage both parties to meet in a side room to negotiate. Our conversations with Missouri debt collection attorneys confirmed that such practices do occur.

In light of these differences in judge style, we estimate judge-year specific settlement propensities following a leave-out estimate methodology, specifically we compute:

$$L_{ijct} = \frac{\sum_{k=1}^{n_{jct}} S_k - S_i}{n_{jct} - 1} - \frac{\sum_{k=1}^{n_{ct}} S_k - S_i}{n_{ct} - 1}$$

This ratio, L_{ijct} , represents the leave-out average settlement rate of judge j in court c in year t minus the rate in court c in year t (this estimate has been used by Kling 2006; Chang and Schoar 2008; Doyle 2007, 2008; Aizer and Doyle, 2013; Dobbie and Song, 2014; and Dobbie et al. 2015). We estimate judge settlement propensities in the full unmatched sample of cases. We also require a minimum of 10 cases and a 5% case share per judge-year for the subsample of cases where we confirmed the defendant was served.

To identify the causal effect of out-of-court settlements on household outcomes, we use an instrumental variables strategy. In the first stage we estimate equations of the form:

$$1^{\text{st}} \text{ Stage:} \quad S_i = \alpha_{cs} + b L_{ijcs} + GX_i + v_i$$

Where S_i is an indicator for whether the case was settled out of court or received a ruling in court, α_{cs} is a court by disposition year fixed effect to account for court-specific time-varying fixed effects and X_i is a set of controls that include age bin, credit score bin, days to disposition bin, homeownership status and previous bankruptcy flags. The variable, L_{ijcs} , is the leave-out settlement rate. The underlying assumption is that the leave-out settlement rate, L_{ijcs} , influences borrower outcomes y_i only through the relevance of own-settlement S_i .

Finally, our coefficient of interest will be estimated using the predicted values of settlement \widehat{S}_i from the first stage regression:

$$2^{\text{nd}} \text{ Stage: } \quad y_i = \alpha_{cs} + \beta \widehat{S}_i + \Gamma X_i + u_i$$

In this case, β represents the causal effect of out-of-court settlement on household outcomes y_i . 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.²

3. Results

The following section discusses the empirical results for the design described above.

3.1. Settlements First Stage

Table 2 shows that the estimated settlement propensity is positively correlated with the likelihood of settlement, a relationship that is robust to the inclusion of controls for credit score bin, age bin, homeowner flags, days to disposition bin, pre-bankruptcy, pre-collection, pre-foreclosure and previous delinquency indicators, and also controlling for the type of plaintiff.

Figure 4 shows the first stage results graphically. The 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 discharge on office-by-filing-month fixed effects and calculate residuals. We then take the mean residual in each judge-by-year bin, adding the mean discharge rate to each residual to aid in the interpretation of the plot. The solid line shows the best linear fit estimated on the underlying micro data estimated using OLS.

Our instrumental variables approach compares outcomes from borrowers with out-of-court settlements with those from borrowers with court rulings using exogenous variation from randomized judges. Table 3 shows that, although borrowers who settle are systematically different from than those who go to court, borrowers who receive low settlement-prone judges are not

² Ideally, we will like to cluster at the court level. However, the final sample has ten court and 43 judges. We run a robustness estimations clustering at the court level correcting for small cluster sample biases and the results are similar.

different on average from those who receive high settlement prone judges, consistent with random assignment.

3.2. Household Outcomes

Table 4 shows the results of the empirical strategy described in Section 2. Panel A Columns 1 and 2 show the results from the endogenous OLS regression. As can be seen borrowers that settle are less likely to file for bankruptcy or to default on their mortgage a year after case disposition. However, as discussed earlier, this could be driven by differences among those who choose to settle. For example, these borrowers may have more resources, and thus may be less likely to face financial distress. Indeed, Table 3 shows that they have higher credit scores, and lower balances.

Columns 3 and 4 of Table 4, Panel A, shows the results from the IV estimation, where settlement is instrument using the judge-specific settle propensity. In this case, we find evidence that settlement causes an increase in household financial distress. In particular, settlement significantly increases subsequent bankruptcy and foreclosure probabilities. We interpret this to mean that the individuals who were randomly induced to settle through the judge they drew, would have been better off going through the court system, on average.

The comparison between the OLS and IV results highlights the severity of the endogeneity problem in this setting, and thus the importance of having a good instrument to estimate the causal effect of out-of-court settlements on household outcomes.

The effects are economically significant as well. The point estimates suggest that settlement nearly triples the probability of bankruptcy (baseline is 7% the estimated effect is 13%) and doubles the probability of foreclosure (baseline is 3% the estimated effect is 4%).

Table 5 shows the dynamics of the effect by estimating the same specification but for different event times with respect to the disposition date. It shows that pre-disposition there are no statistical differences between both groups, but also that the effect on household distress is concentrated in the first year after disposition, with some evidence (although not statistically significant) that the differential effect on distress dissipates in year four.

Table 4, Panel B, also shows how out-of-court settlement affects a variety of other outcomes. Settlement affects some outcomes in the way one would expect, given that it is a form of debt resolution. In particular, settlement weakly increases credit scores, but more importantly decreases collection balances between 6,500 and 7,400 dollars (our estimated of settlement amount), and the likelihood of having any collection balances between 30 and 40 %. The probability of repossession and liens also decline with settlement.³

Table 6, shows that the baseline results are also robust across various specifications and sample restrictions. Panel A shows that we find similar results when we exclude dismissed cases from the sample. Thus, individuals who go through the court process appear to do better, even if their case is not dismissed. In Panel B, we also find similar results when we exclude households that file for bankruptcy within the six months after disposition. This helps alleviate concerns about the timing of the effect.

To understand more about the mechanism underlying these results, we explore the effects of settlement on various types of debt balances and inquiries. In Table 7, Panel A, we find that settlements are linked to increases in mortgage balances and the number of secure trades. This

³ The effect on repossession and liens is not statistically significant. However, these findings are confirmed in Table 7 when focusing on bank plaintiffs that are more likely to report the ruling to the credit bureaus <http://www.latimes.com/business/la-fi-credit-report-20170314-story.html>

suggests that, on average, borrowers use secured debt and collateralized loans to be able to pay the lump-sum settlement.

We also explore whether there is heterogeneity along various dimension in effects that we document. In Table 7, Panel B, we find that the effect of settlement on bankruptcy is stronger when the borrower has a lower credit score. Specifically, the increase in the probability of bankruptcy is 13% for low credit score borrowers and only 4% for high credit score borrowers. This is consistent with the idea that these borrowers may be less able to afford to make a large lump-sum payment than others. We also find that effect is stronger when the debt holder is a bank. This may be because banks are better able to extract settlements. Indeed, banks usually have more reliable documentation with respect to the claims owed by borrowers, which can result in a more credible threat of litigation in court than in the case of debt buyers.

Finally, we note that the causal effects that we have estimated thus far pertain only to the selected population in our sample, namely those who debt collectors chose to take legal action against, rather than simply call and write letters to. In order to understand how these decisions are made, we obtain proprietary data from a large debt collector. These data allow us to see which borrowers the debt collector chooses to sue. Table 8 shows, broadly speaking, that collectors trade off the fixed cost of litigation versus the expected recovery amount. They sue when balances are large, when they know where to find the borrower, and when they expect borrowers can pay some amount. Thus, if anything, our estimates of the effect of settlement on subsequent financial distress are likely a lower bound, as the effect is likely to be even larger among those who were not sued due to their lower ability to pay.

4. Conclusion

Debt contract enforcement is crucial for well-functioning credit markets. For consumer debt, courts allow lenders to garnish wages or foreclose property on severely delinquent borrowers. However, lenders and borrowers also often resolve debt bilaterally, outside of courts. One important function of the debt collection industry is to facilitate this type of resolution. But, there is scant evidence on how consumers fare outside of the court system when they negotiate directly with collectors.

To examine this question, we use a unique dataset that links court records from debt collection lawsuits with detailed credit registry data. Randomly assigned judges in our dataset differ in their propensity to preside over cases that end with an out-of-court settlement. Using judge settlement propensity as an instrument, we find that out-of-court settlements on unsecured debt increase the chances of subsequent financial distress for borrowers. The results suggest that the average borrower who settles would be better off going through the court system.

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Table 1. Litigated Borrowers in Missouri Sample

Panel A shows cases outcomes statistics for the litigation sample, counties where there is random judge assignment. “Settlement rate” is the fraction of cases per county that settled, $\Pr(\text{Dismiss} | \text{Court})$ is the likelihood of being dismissed conditional on going to court, “Total judgment” is the amount owe to the plaintiff after judgement, “Garnishment rate” is the likelihood of cases registering a garnishment conditional on a judgment, “Days to disposition” represents the length between filing date and disposition date, “ $\Pr(\text{Litigation within a year} | \text{Settlement})$ ” is the probability of getting sued again conditional and “ $\Pr(\text{Litigation within a year} | \text{Settlement})$ ” is the probability of getting sued again conditional on being settle in the first case. Panel B shows the comparison of Missouri Sample with the overall population of borrowers. The first three columns are from Dobbie et al. (2015), while the last column is average characteristics the year before disposition from the litigation sample that has a random assignment of judges in Missouri (N=82,218).

Panel A. Litigation Sample case characteristics

	Mean	SD	Median	N
Settlement rate	0.17	0.37	0	82,218
$\Pr(\text{Dismiss} \text{Court})$	0.06	0.23	0	68,516
Total judgment (\$)	4,149	12,514	2,059	42,173
Garnishment rate	0.51	0.5	1	41,752
Day to disposition	88	83	59	82,218
N of cases per person	1.23	0.57	1	82,218
$\Pr(\text{Litigation within a year})$	0.09	0.29	0	82,218
$\Pr(\text{Litigation within a year} \text{Settlement})$	0.08	0.27	0	13,702

Panel B. Comparison with other samples.

	All Credit Users	DGY	DGY
	Random S	B Filers	Ch. 13
Credit Score	740	630	580
Age	48.6	43.7	44.8
Homeowner	0.470	0.520	0.643
Delinquency	0.148	0.413	0.675
Collection	0.137	0.296	0.467
Charge-Off	0.065	0.188	0.310
Bankruptcy	0.010	0.007	0.048
Judgment	0.009	0.034	0.060
Foreclosure	0.003	0.010	0.048
Lien	0.004	0.011	0.021
Repossession	0.003	0.012	0.020
Revolving Balance	6,010	13,080	10,010
Collection Balance	600	1,430	2,500

Table 2. Judge Settlement Propensity. Instrumental Variable First Stage

The table shows the first stage of judge settlement propensity and settlement likelihood, column 1 shows the effect without controls and column 2 the results including age bins, credit score bins, days to disposition bins, and homeownership flag as a control, standard errors are cluster at the judge level.

	Likelihood of Settlement ($St=1$)				
Judge Settlement Propensity	0.94 (0.04)***	0.86 (0.06)***	0.92 (0.07)***	0.84 (0.06)***	0.89 (0.09)***
Credit Score		0.001 (0.00)***		0.001 (0.00)***	
Age		-0.0001 (0.00)		-0.0001 (0.00)	
Homeowner		0.05 (0.00)***		0.04 (0.00)***	
Days to disposition		0.00 (0.0)***		0.00 (0.0)***	
Previous bankruptcy		0.25 (0.02)***		0.25 (0.02)***	
Previous collection				-0.04 (0.00)***	
Previous delinquency				0.03 (0.00)***	
Previous foreclosure				-0.02 (0.00)***	
Plaintiff attorney				0.012 (0.01)	
Control	N	Y	Y	Y	Y
N	82218	82218	82218	82218	82218
R2	0.051	0.144	0.156	0.148	0.159
Clusters	43	43	43	43	43
F-stat	737	198	161	110	104

Table 3. Litigation Sample Test of Random Assignment

The table compares average borrower characteristics in sample of cases that received a judgment (first), and sample of cases that received a settlement (second). The p-values are from tests of differences between the judgment and dismissed sample, controlling for court by year fixed effects. Borrower characteristics are computed at the year before the case decision. Standard errors are clustered at the judge level. The last three columns divide between harsh and lenient judges with respect to their settlements propensity, defined as above and below the median leave-out estimate of judge settlement propensity.

				Settlement Propensity		
	Judgment	Settlement	p-value	Low	High	p-value
Judge Settlement Propensity	-0.002	0.006	(0.004)***	-0.002	0.031	(0.004)***
Mechanical Outcome						
Credit Score	530	562	(0.000)***	535	537	(0.229)
Have a Collection	0.77	0.67	(0.000)***	0.76	0.76	(0.521)
Collection Balance	7,259	5,776	(0.000)***	7,027	6,983	(0.397)
Have a Judgement	0.11	0.05	(0.000)***	0.10	0.11	(0.546)
Have a Repossession	0.02	0.02	(0.000)***	0.02	0.02	(0.124)
Have a Lien	0.01	0.01	(0.005)***	0.01	0.01	(0.567)
Household Distress						
Bankruptcy	0.05	0.21	(0.000)***	0.08	0.07	(0.223)
Foreclosure	0.03	0.03	(0.021)**	0.03	0.03	(0.700)
Secure and Unsecured Debt						
Revolving Balance	6,758	10,164	(0.000)***	7,337	7,301	(0.534)
Mortgage Balance	28,154	46,014	(0.000)***	30,722	31,946	(0.108)
Non Mortgage Inquiries	1.9	1.6	(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	3.2	4.1	(0.000)***	3.4	3.3	(0.606)

Table 4. Effect of Settlements

The table reports coefficients for regressions where the variable is the left-hand side variables on an indicator for whether the borrower's case was settled, the variable is the value since the first January after disposition date, the first two columns are the OLS regression, the last two the instrumental variable regression that uses judge settlement propensity as an instrument for settlement, defined as the average settlement rate for the judge in the year, excluding the own case. Panel A, shows the distress outcomes, and Panel B the mechanical outcomes that should be affected by the settlements. Standard errors are clustered at the judge level and p-values are reported in parentheses. */**/** denotes statistically significant at the 10%, 5%, and 1% levels, respectively.

Panel A. Distress Outcomes

	OLS		2SLS	
Bankruptcy	-0.02 (0.000)***	-0.05 (0.000)***	0.13 (0.001)***	0.12 (0.000)***
Foreclosure	-0.01 (0.000)***	-0.01 (0.000)***	0.04 (0.032)**	0.03 (0.008)***
Controls	No	Yes	No	Yes
N Obs.	82218	82218	82218	82218
N Clusters	43	43	43	43

Panel B. Mechanical Outcomes

	OLS		2SLS	
Credit Score	53 (0.000)***	28 (0.000)***	50 (0.051)*	33 (0.204)
Have a Collection	-0.22 (0.000)***	-0.14 (0.000)***	-0.37 (0.000)***	-0.32 (0.000)***
Collection Balance	-2,871 (0.000)***	-2,179 (0.000)***	-7,391 (0.000)***	-6,557 (0.000)***
Have a Judgement	-0.43 (0.000)***	-0.40 (0.000)***	0.05 (0.886)	0.06 (0.866)
Have a Repossession	-0.01 (0.000)***	-0.01 (0.000)***	-0.02 (0.418)	-0.02 (0.408)
Have a Lien	0.00 (0.022)**	0.00 (0.015)**	0.00 (0.807)	0.00 (0.762)
Controls	No	Yes	No	Yes
N Obs.	82218	82218	82218	82218
N Clusters	43	43	43	43

Table 5. Dynamics of the Effect

The table report coefficient for separate regressions for each event time of the outcome of interest in an indicator for whether the borrower's case was settled, the variable is the value for January of the event time, the instrumental variable regression that uses judge settlement propensity as an instrument for settlement, defined as the average settlement rate for the judge in the year, excluding the own case. Panel A shows the effect on bankruptcy and Panel B for foreclosure. Standard errors are clustered at the judge level. */**/** denotes statistically significant at the 10%, 5%, and 1% levels, respectively

Panel A. Bankruptcy Rates

	-3	-2	-1	0	1	2	3	4
Settlement	0.01 (0.032)	0.00 (0.018)	0.02 (0.031)	0.01 (0.029)	0.12 (0.028)***	0.06 (0.063)	0.04 (0.042)	0.00 (0.038)
N	32,413	42,073	65,467	82,218	82,218	80,525	77,604	67,348
N cluster	33	36	41	43	43	40	39	37
F	53	46	100	159	159	149	161	136
F p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Panel B. Foreclosure Rates

	-3	-2	-1	0	1	2	3	4
Settlement	-0.03 (0.021)	-0.03 (0.025)	-0.01 (0.022)	-0.02 (0.021)	0.03 (0.010)***	0.02 (0.020)	0.01 (0.006)	0.01 (0.008)
N	32413	42073	65467	82218	82218	80525	77604	67348
N cluster	33	36	41	43	43	40	39	37
F	52.5	45.6	100.3	159.4	159.4	148.8	161.0	135.9
F p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Table 6. Robustness of Effect of Settlements

The table reports coefficients for regressions where the variable is the left-hand side variables on an indicator for whether the borrower's case was settled, the variable is the value since the first January after disposition date, the first two columns are the OLS regression, the last two the instrumental variable regression that uses judge settlement propensity as an instrument for settlement, defined as the average settlement rate for the judge in the year, excluding the own case. Panel A, shows the distress outcomes when the analysis sample only includes settlement and judgments. Panel B shows the distress outcomes effect when the sample only includes settlements and consent judgment. Standard errors are clustered at the judge level, and p-values are reported in parentheses. ***/*** denotes statistically significant at the 10%, 5%, and 1% levels, respectively

Panel A. Restricted Litigation Sample to Settlement and Judgement

	OLS		2SLS	
Bankruptcy	-0.02 (0.000)***	-0.05 (0.000)***	0.13 (0.001)***	0.12 (0.000)***
Foreclosure	-0.01 (0.000)***	-0.01 (0.000)***	0.04 (0.063)*	0.03 (0.049)**
Controls	No	Yes	No	Yes
N Obs.	78302	78302	78302	78302
N Clusters	43	43	43	43

Panel B. Timing of bankruptcy, excluding bankruptcy within six months of disposition.

	OLS		2SLS	
Bankruptcy	0.00 (0.988)	-0.02 (0.000)***	0.07 (0.000)***	0.06 (0.000)***
Foreclosure	-0.01 (0.000)***	-0.01 (0.000)***	0.03 (0.046)**	0.02 (0.019)**
Controls	No	Yes	No	Yes
N Obs.	80026	80026	80026	80026
N Clusters	43	43	43	43

Table 7. Potential Mechanism

The table reports coefficients for regressions where the variable is the left-hand side variables on an indicator for whether the borrower's case was settled, the variable is the value since the first January after disposition date, the first two columns are the OLS regression, the last two the instrumental variable regression that uses judge settlement propensity as an instrument for settlement, defined as the average settlement rate for the judge in the year, excluding the own case. Panel A, shows the effect for secured and unsecured debt variables. Panel B column 1 and 2 interacts the main effect with a high credit score dummy defined as credit score greater than 660; column 3 and 4 show the interaction between effect for financial plaintiff focusing on the interaction between debt buyers and major bank. Standard errors are clustered at the judge level and p-values are reported in parentheses. */**/** denotes statistically significant at the 10%, 5%, and 1% levels, respectively

Panel A. Secured and Unsecured Debt

	OLS		2SLS	
Revolving Balance	116 (0.622)	-238 (0.167)	2,617 (0.652)	2,480 (0.559)
Mortgage Balance	11,162 (0.000)***	6,141 (0.000)***	38,008 (0.005)***	19,251 (0.003)***
Non Mortgage Inquiries	-0.22 (0.000)***	-0.15 (0.000)***	-0.85 (0.177)	-0.93 (0.100)
Mortgage Inquiries	0.01 (0.000)***	0.01 (0.005)***	0.03 (0.255)	0.01 (0.571)
Number of Trade Lines	0.66 (0.000)***	0.33 (0.000)***	1.46 (0.015)**	0.81 (0.251)
Controls	No	Yes	No	Yes
N Obs.	82218	82218	82218	82218
N Clusters	43	43	43	43

Panel B. Heterogeneity on Credit Score and Plaintiff Type

	2SLS Credit Score x High		2SLS Plaintiff Type Bank vs x Debt Buyer	
Distress Outcomes				
Bankruptcy	0.13 (0.00)***	-0.09 (0.03)**	0.18 (0.04)***	-0.10 (0.16)
Foreclosure	0.03 (0.01)**	-0.02 (0.45)	0.04 (0.35)	0.01 (0.90)
Mechanical Outcomes				
Credit Score	35.45 (0.20)	-29 (0.52)	66.79 (0.00)***	-46 (0.01)***
Have a Collection	-0.33 (0.00)***	0.10 (0.37)	-0.36 (0.02)**	0.19 (0.12)
Collection Balance	-7,030 (0.00)***	4,343 (0.00)***	-6,389 (0.04)**	2,812 (0.44)
Have a Judgement	0.06 (0.87)	-0.07 (0.85)	-0.46 (0.03)**	0.33 (0.07)*
Have a Repossession	-0.02 (0.41)	0.02 (0.45)	-0.04 (0.18)	0.04 (0.42)
Have a Lien	0.00 (0.83)	0.00 (0.91)	-0.05 (0.02)**	0.07 (0.05)**
Secured and Unsecured Debt				
Revolving Balance	2,283 (0.65)	-2,946 (0.70)	-162 (0.98)	1,273 (0.85)
Mortgage Balance	18,631 (0.00)***	-9,886 (0.38)	-9,744 (0.16)	12,041 (0.21)
Number of Trade Lines	0.67 (0.43)	0.92 (0.67)	1.98 (0.06)*	-2.21 (0.00)***
<hr/>				
Controls	Yes		Yes	
N Obs.	82218		45942	
N Clusters	43		43	

Table 8. Debt collector portfolio allocation

This table shows how a debt collector allocated accounts from a portfolio of charged-off debt into four distinct actions. The debt portfolio was purchased from a top 5 bank between 2009-2011. The different actions are: "Litigated," "Call or letter," "Keep in-house," or "Sold." "Keep in-house" refers to accounts which the collector warehoused.

Action:	Litigated	Call or Letter	Keep in House	Sold
N	22,572	8,307	54,521	80,585
Fraction	14%	5%	33%	49%
At the time of allocation				
Verified borrower has a 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)
Balance	6056.6 (5562.1)	6842.1 (6122.8)	5296.3 (5536.6)	6069.6 (5830.0)
Days since last activity in the account	598.8 (383.7)	937.0 (754.5)	936.1 (2104.1)	672.7 (337.0)
Days since last payment in the account	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)
Aggregate Recovery (%)	0.32	0.05	0.01	0.00
Filed in Court (%)	81.95			

Figure 1. Debt collection and Bankruptcy

This figure plots the proportion of consumer with third party collection from the FRBNY Consumer Credit Panel and the percentage of consumers in bankruptcy from Survey of Consumer Finance and Dobbie et al. (2016).

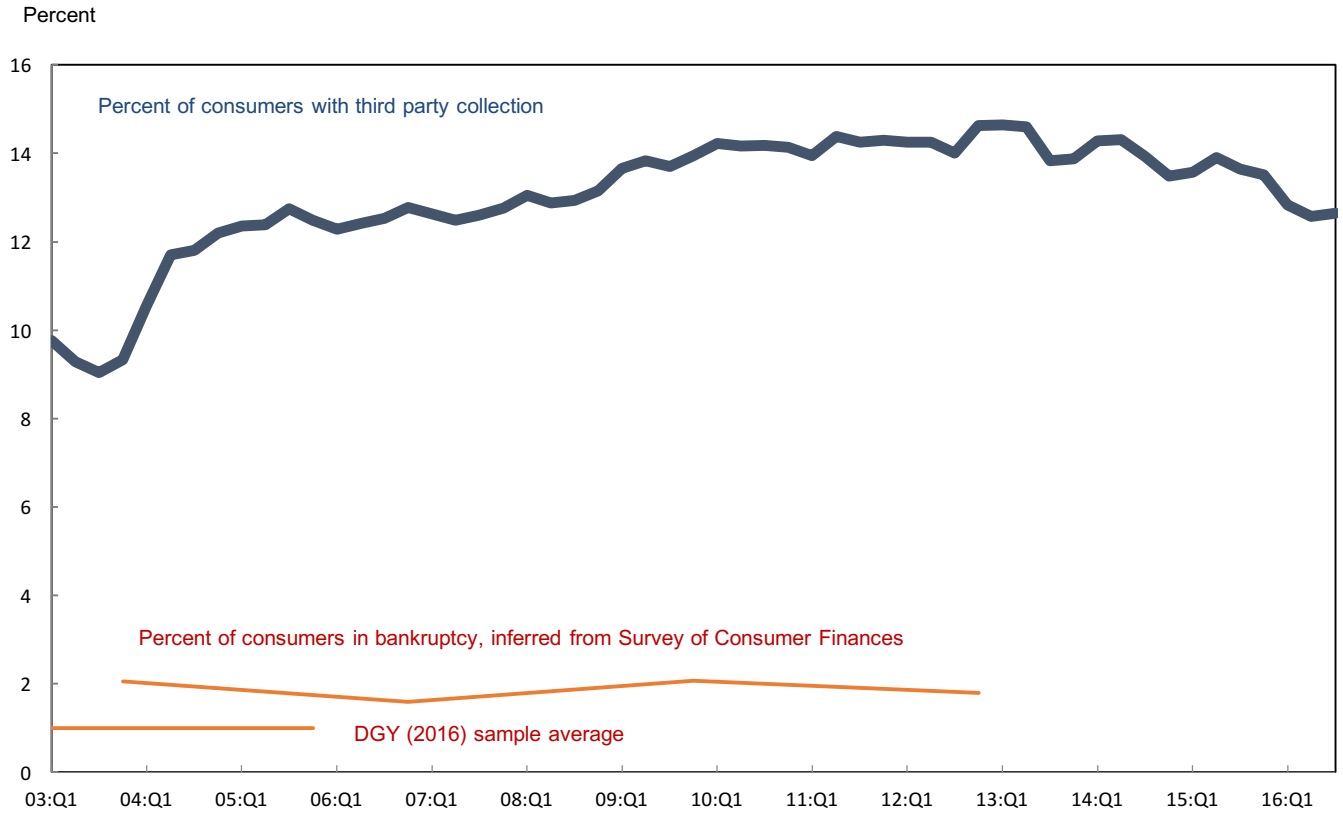


Figure 2. Litigation outcomes in Missouri Litigation Sample

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

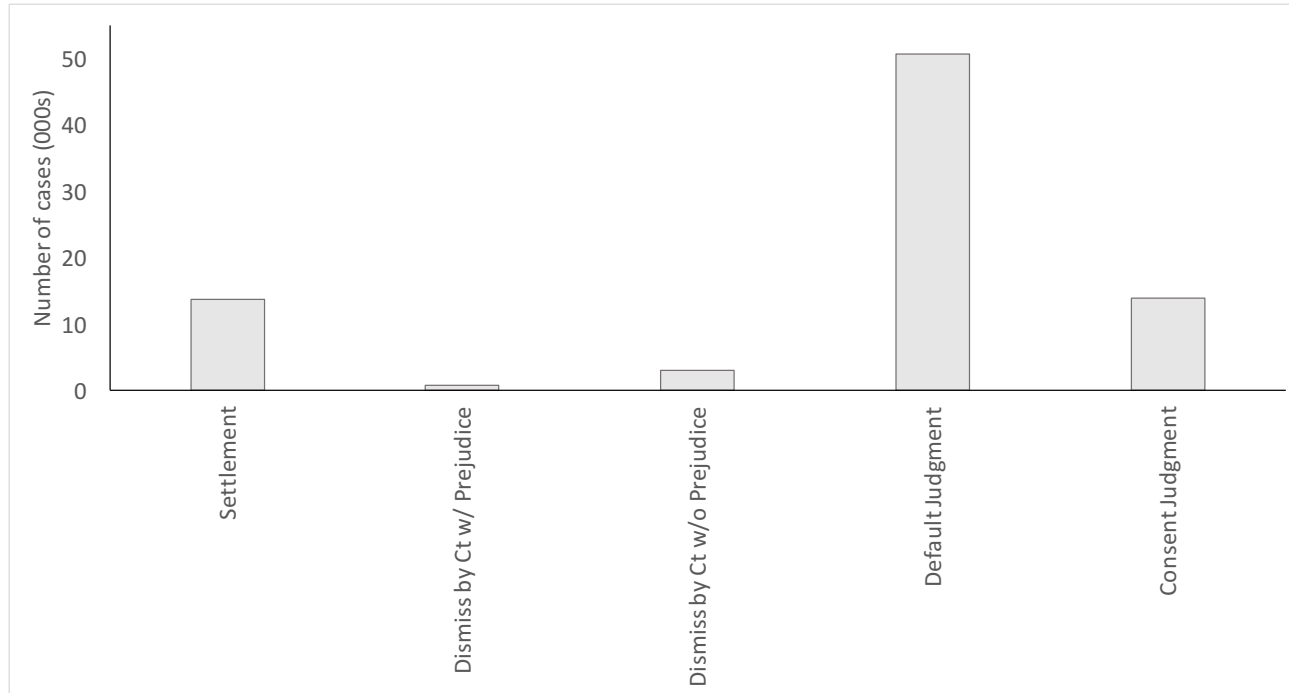


Figure 3. Plaintiff types in Missouri Litigation Sample

This graph plots the frequency distribution for plaintiff types in the sample of counties with random judge assignment (N=82,218).

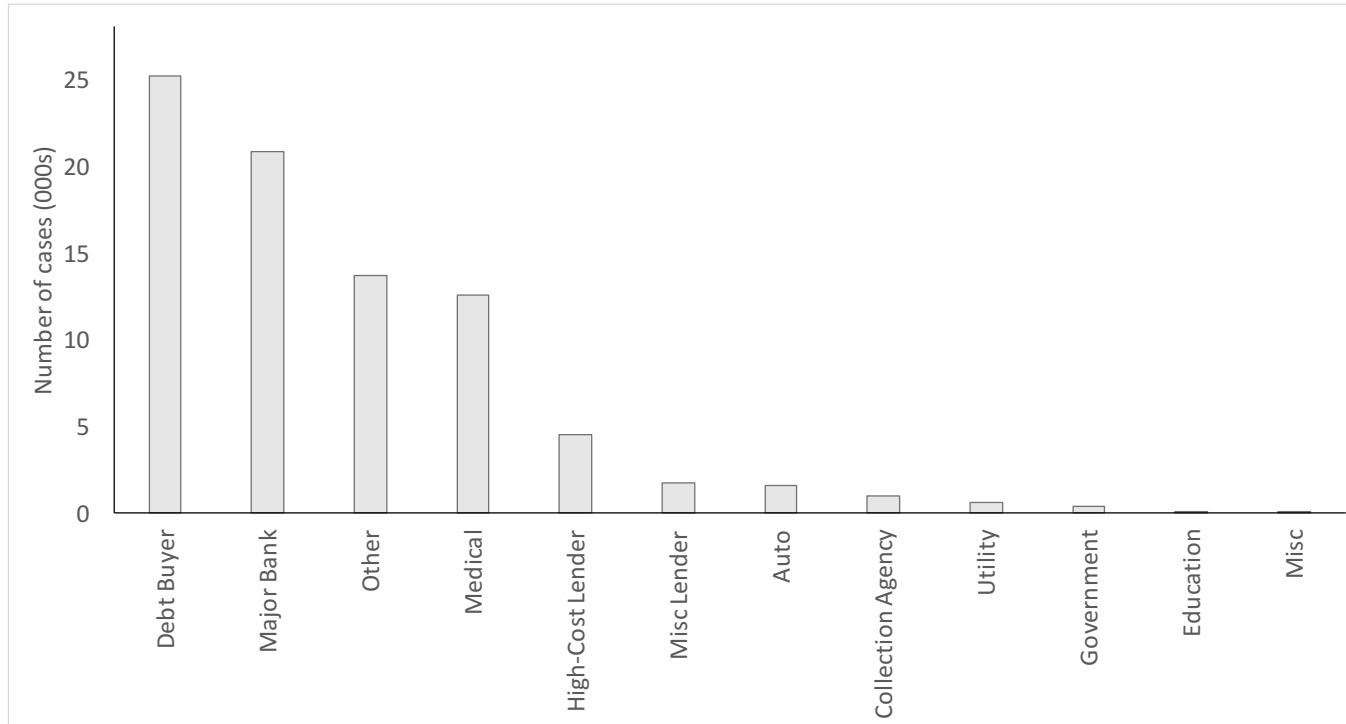
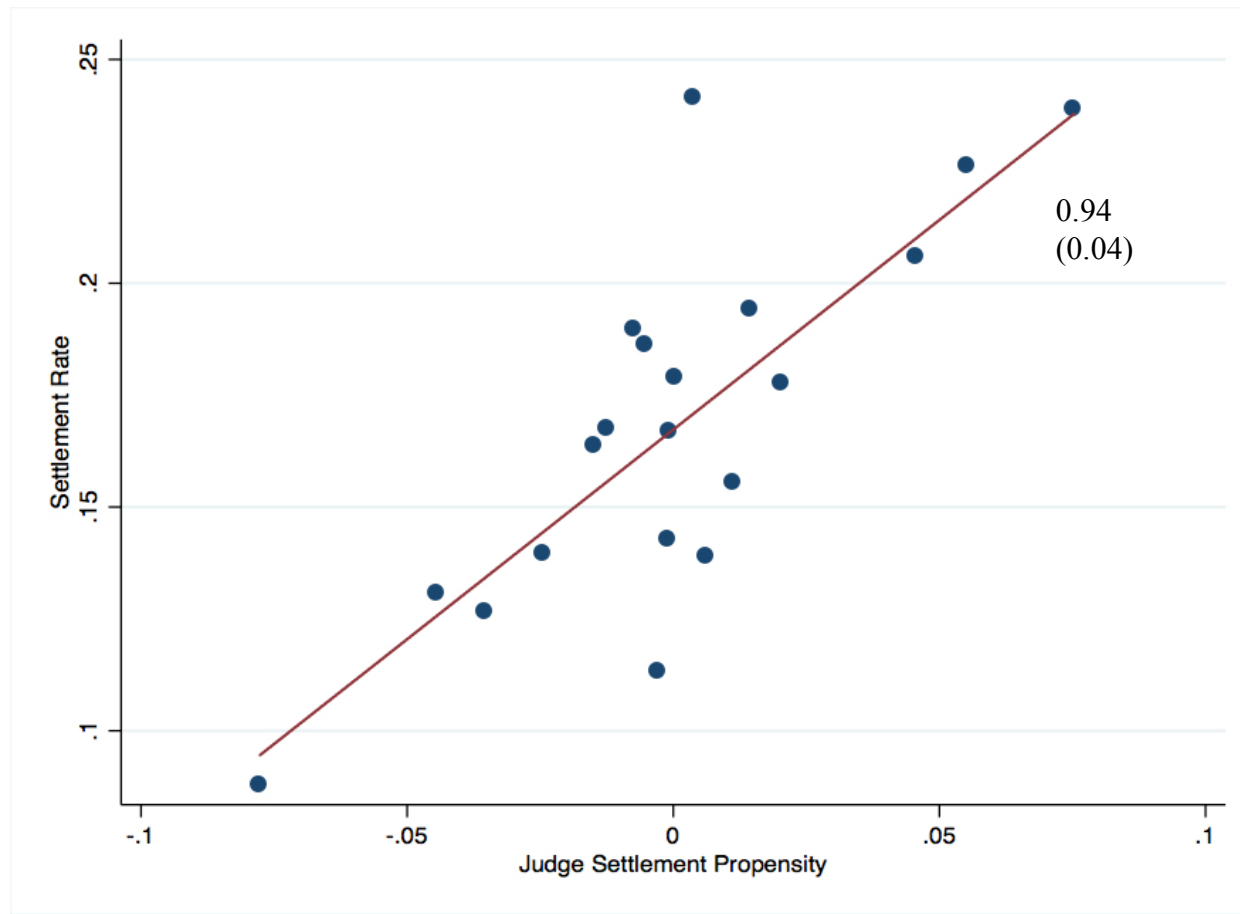


Figure 4. Judge First Stage

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 take the mean residual in each judge-by-year bin, adding the mean discharge 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 coefficients show 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.



Online Appendix

Table A1. Litigation cases in Missouri Sample Definition.

Panel A. describes the final data composition due to the matching process and other refinements, and Panel B compares the litigation sample in the paper with the sample of match cases from Missouri

Panel A. Sample definition of match sample with TransUnion

All cases, 2007-2014	667,337
<hr/>	
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
<hr/>	
Require t=0 and t=1 presence + Data cleaning	142,038
With lawyer classification	135,989
<hr/>	
Cases where borrower was served	82,218
<hr/>	
Final matched sample	82,218

Table A2. Persistency of Judge Settlement Propensity

This table reports the coefficient of a regression on the raw judge propensity to settle for a year and the previous year, to assess how persistent judge settlement propensity. Column 1 shows the coefficient adjusted for robust standard error. Column 2 shows the coefficient when clustering at the judge level. Standard errors are clustered at the judge level and p-values are reported in parentheses. */**/** denotes statistically significant at the 10%, 5%, and 1% levels, respectively

	<u>Judge Settlement Propensity t</u>	
Judge Settlement Propensity t-1	0.662 (0.117)***	0.662 (0.135)***
<hr/>		
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).

