

Does Repeat Play Elicit Cooperation?

Evidence from Federal Civil Litigation

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March 24, 2000

Preliminary Draft – Comments Welcome

Abstract

While some litigants frequently file cases, very few opposing pairs of litigants appear together frequently. Thus, there is little opportunity for litigants to develop trust or reputation. Lawyers, on the other hand, face each other frequently and participate in a community of lawyers that shares information. Lawyers may therefore foster efficient dispute resolution by turning one-shot client interaction into a repeated game. This suggests the following empirical question: Are disputes resolved more quickly by agents (lawyers) who interact frequently, either as individuals or through their firms? We have assembled a dataset on roughly 2000 federal civil cases filed in the Eastern District of Pennsylvania in 1994, supplementing the ICPSR data with attorney, and firm, identity. After accounting for individual attorney speed, we find that cases involving attorney pairs who interact repeatedly in our data are resolved more quickly and are more likely to settle.

Introduction

The past decade has seen the development of a very rich theory of repeated games. (For a survey, see). This theory arose in large part to explain collusion in oligopolies – where there is no legal sanction for the failure to cooperate (see Abreu, Pearce and Stachetti, 1990). It has increasingly been applied to explain how credible extralegal sanctions may substitute for legal sanctions in supporting cooperative behavior in a variety of social and economic settings (see Ellickson, 1991). More generally, the notion that long-run, repeat players may have a strong interest in establishing a reputation for cooperation has become the basis for countless theories of law and social behavior.

Given the pervasiveness of this belief, it is odd to find so little systematic empirical evidence on the relationship between repeat play and cooperation. Perhaps the primary obstacle to such work has been the simple lack of measurable data corresponding to theoretically important behaviors. In the oligopoly setting, for instance, the illegality of price-fixing has made it difficult for investigators to obtain data that might be used to test theories positing that price wars are used to enforce price-fixing agreements (for relatively rare studies of this sort, see Porter (1983) and Ellison (1994)). In the legal setting, while there are wonderfully detailed case studies of close-knit communities and cooperation such as Bernstein (1994) and Ellickson (1991), there is little if any systematic quantitative evidence on the relationship between repeat interaction and cooperation.

In this study, we find such evidence in what is perhaps the most visible American social phenomenon involving conflict and cooperation – the lawsuit. It is well-known that the vast majority of lawsuits are settled sometime after filing, with settlement rates for

some type of cases – such as torts – exceeding 90 per cent. At the same time, the legal system is widely criticized for involving inordinate delay and expense. Indeed, a 1987 study by the National Center for State Courts found that not a single one of the twenty six trial courts studied was meeting American Bar Association standards for the timely resolution of disputes (Goerd, 1989). If the legal discovery process is working as planned, then it should allow the litigants to quickly and cheaply become very well informed regarding each other's case, allowing them to avoid the costly trial stage. The more cooperative are the litigants, the better will the discovery process work, and the earlier will any given case be settled (Gilson and Mnookin, 1994).

On this view, both the failure to settle and delay in resolving legal disputes indicates a failure of cooperation. In this paper, we explore empirically whether cooperation – as measured by speed of case processing and the tendency to settle – is enhanced by repeated interaction of attorneys. Using a dataset of over 2200 civil cases filed in Federal District Court for the Eastern District of Pennsylvania (Philadelphia), we identify the number of repeat interactions between firm and attorney pairs over a six month period of case filings. We find that cases involving attorneys who interact repeatedly in our data are resolved more quickly and with less likelihood of trial than other cases. While we emphasize reputational incentives to cooperate, we recognize that an effect of repeated interaction on case processing could reflect learning as well as reputational incentives. We attempt to distinguish between these mechanisms.

There is little prior empirical work exploring the effect of attorney reputational effects on litigation outcomes. Ashenfelter and Bloom (1990) found that in a variety of contexts – ranging from labor grievance to child custody disputes – clients can get

favorable shifts in expected arbitration awards by hiring a lawyer. They interpret these findings as evidence that hiring an attorney may be an individually dominant but socially inefficient strategy in a game with the classic Prisoner's Dilemma structure. Ashenfelter and Bloom (1990) do not, however, explore the effect of attorney reputation.

Gilson and Mnookin (1994) also posit that litigation has a Prisoner's Dilemma strategic structure (because the parties may disclose their private information, or else refuse to disclose but have the information revealed in any event if and when the opposing party employs a an attorney who invokes the costly evidence discovery process). They then suggest that the litigants may get out of the Prisoner's Dilemma by committing their litigation strategy to lawyers who have an interest in maintaining reputations as cooperative types. Gilson and Mnookin (1994) construct a two-stage game in which the client uses the attorney's reputational type to signal and commit to a cooperative strategy, and Croson and Mnookin (1997) find experimental support for such a commitment strategy.¹

In Section I, we informally review what the theory of repeat games has to say about reputation and cooperation. We explain why the strategic interests of lawyers, rather than their clients, may be expected to determine litigation outcomes, and set forth our empirical measures of attorney cooperation in litigation. Section II describes the data sources and presents some descriptive statistics for the sample. In Section III, we

¹ Neither Gilson and Mnookin (1994) nor Croson and Mnookin (1997) explain how attorneys acquire such reputations. And while their assumption that the attorney must play true to type may be justified in cases where the attorney has been hired by and is under the close supervision of a sophisticated client, it seems unjustified in cases where the client is unable to exercise such control. In such cases, the independent strategic interests of attorneys are likely to be dominant. On our approach, when not subject to tight control by sophisticated repeat-player clients who have their own reputational interests at stake, attorneys pursue strategies which are in *their* interests -- which may or may not coincide with those of their clients'.

document the relationship between repeat play and our measures of cooperation, and we show that the relationship is stronger in contexts where learning and reputational concerns are complementary. In Section IV we attempt to distinguish reputation from learning as explanations for the relationship between repeat play and case processing speed. Section V concludes by discussing possible policy implications.

I. Lawyers and Litigation: Theoretical Background

Most litigants do not appear frequently in court, so that most litigation is between one-shot litigants.² Many attorneys, by contrast, appear frequently enough so that even particular pairs of attorneys oppose each other repeatedly. Siegelman and Waldfogel (1999) calculate the frequency of appearance of underlying plaintiff and defendant clients in federal litigation in the Southern District of New York. They calculate the fraction of plaintiffs and defendants that are institutions (firms or other organizations), as opposed to individuals, by case types, as well as the average frequency of party appearance. Most parties in federal court are institutions, except for plaintiffs in tort, civil rights, and prisoner cases. Except in contract, labor, and intellectual property litigation, plaintiffs are typically one-shotters. Defendants, by contrast, typically appear more once in court.

When litigants have no litigation history, no reputation as litigants, and little probability of reappearing jointly as litigants, lawyers may turn one-shot client interaction into repeat play by attorneys. The question we seek investigate here is how such attorney repeat interaction affects the resolution of federal court litigation. outcomes. This is not a

question which permits an unambiguous a priori theoretical answer. If repeat play by attorneys fosters “cooperation,” this could make case processing either more or less efficient from the client perspective. Consider, for instance, repeat interaction between a plaintiff’s contingent fee attorney and a defense attorney who is compensated on an hourly basis. If the defense attorney is subject to little client control, then that attorney has a credible threat to engage in abusive discovery – dumping truckloads of documents on the plaintiff – and to prolong the dispute even through trial. Such uncooperative behavior may well be a strictly dominant strategy for the defense attorney in the stage game. It may also be stage game dominant for the plaintiff’s lawyer to make abusive discovery requests – asking the defense attorney to produce documents which the plaintiff’s attorney knows she doesn’t need, but which take time away from more valuable defense attorney activities.³ Repeat play may sustain an equilibrium in which both attorneys eschew abusive discovery, to the benefit of both themselves and their clients. On the other hand, a risk averse plaintiff may well prefer to settle when her risk neutral attorney would prefer going to trial. If trial really doesn’t cost the defense lawyer anything – but simply balloons her billable hours – then both attorneys may have an interest in going to trial which conflicts with the interests of their clients.

By reputational incentives, we have in mind an attorney’s concern that uncooperative behavior toward an opposing attorney will be sanctioned in the future. The sanction might operate through reciprocation by the same attorney in future cases, or

² Even if parties had interacted, or expected to interact, multiple times in court, it is not clear that they could dispute cooperatively. Had the parties been able to cooperate, they would not have arrived in court as dispute parties in the first place.

³ The required condition for dominance is that the cost to the plaintiff’s attorney of making an abusive request be less than the cost of making a cooperative request.

it might take the form of the opposing attorney disseminating information on the lack of cooperation to the community. We recognize, however, that reputation is not the only factor influencing the way repeat player attorneys resolve cases. While the prospect of repeat interaction might create incentives to avoid future punishment, at the same time, repeat interaction allows attorneys to learn about each other's styles and strategies. Such learning may reduce transaction costs and facilitate cooperation even without concerns about future interactions.

In characterizing the reputational interests of attorneys in litigation, we look to the theory of repeat games. Perhaps the best known result in this literature is that for sufficiently high discount rates (sufficient weight attached to the future), any feasible payoff can be sustained as a (subgame perfect) equilibrium in a two-player repeat game with an indefinite horizon. (Fudenberg and Maskin, 1986). While this theory does not uniquely identify equilibrium outcomes in repeat games (see, for instance, the discussion in Kreps (1990, pp. 507-512), the Folk Theorem is routinely invoked in the law and economics literature as supporting the hypothesis that repeat play induces cooperation. Our reliance on the alternative "learning" hypothesis is intuitive. Whatever the mechanism, we attempt initially to simply test whether repeat play affects case processing. Later, at section IV, we attempt to distinguish between mechanisms.

As a theoretical matter, it is possible for both learning and reputation effects to be communicated through individual attorneys and/or their firms. For example, if attorneys i and j dispute against one another often, their experience and expected future interaction might affect their case processing only when they are disputing against each other. Or the reputational and learning effects might also operate through, say, attorney i or j 's firm

(call them firms *I* and *J*), so that repeat interaction among attorneys from firms *I* and *J* fosters cooperation, even if the particular attorneys have no joint history and expect no joint future. Or, at another extreme, attorneys might communicate how their opponents behave, so that a) all members of the legal community learn from every attorney interaction, and b) attorneys can expect their uncooperative behavior to be punished by any opponent, not just the particular attorney with whom they failed to cooperate.

These different possibilities have different implications that we can explore empirically. Rather than developing a specific set of hypotheses regarding firm versus individual attorney effects, we use the distinction between firms and attorneys to test our the underlying hypothesis that it is only when client control is relatively weak that independent attorney (or firm) effects will be observed. We hypothesize that institutional parties to litigation exercise greater control over their lawyers than do individual (typically one shot) litigants. On this hypothesis, we would expect that however big it may be, the effect of repeat law firm interaction will be less in cases involving institutional clients than in other cases.

To develop the specific hypotheses, it is helpful to first describe our data in detail.

III. Data and Hypotheses

The underlying data for this study consists of information on 2236 civil cases filed in the federal court in Philadelphia in the first five months of 1994. The data come from three sources. The basic data source is the Administrative Office of the U.S. Courts data set, “Federal Court Cases: Integrated Data Base, 1970-1997” (ICPSR 8429), available from the Inter-University Consortium for Political and Social Research. This sources

includes information such as docket number, nature of suit, filing date, termination date, how the case is resolved (whether adjudicated or, we infer, settled), and whether the plaintiff wins. The most recent AO data available at the time of the study include terminations during 1997, so all but 22 of the cases are terminated. Our second data source is the online records available at the courthouse, from which we gather judge name and plaintiff and defendant attorneys' names and zip codes and law firm names. We ascertain attorneys' genders from their first names. We choose filed cases for 1994 because those are the earliest filed cases available online at the start of our data collection (summer/fall 1998). The third data source is the Martindale-Hubbell Law Directory, which we use to obtain information on attorney background, including age and locations of college and law school attendance.

We include in the dataset only civil cases types that involve non-federal parties. For example, we exclude contracts cases brought by the government to recover overpayment of veteran's benefits or student loans. We exclude prisoner cases. Table 1 shows the distribution of sample cases among included case types. We exclude cases from the basic sample if they are missing attorney identification information (excluding xxx cases), or when multiple cases involving the same attorneys appear to be related (excluding xxx cases), or if cases are not terminated (excluding another 22). Our resulting analysis sample includes 1404 cases.

We have five types of variables:

- 1) cases processing measures – three variables measuring how speedily cases are resolved: the *adjudication rate* (using the AO's "judgment for" variable, we deem a case "adjudicated" if it has a judgment for either the plaintiff, defendant, or both), the

duration between filing and termination, and the *trial rate*, the fraction of cases that proceed beyond pre-trial discovery to the trial phase (using the AO's "procedural progress" variable, we deem a case to have gone to trial if it ends during trial, whether settled or adjudicated);

- 2) repeat play measures - we calculate the number of number of times that each firm, and that each attorney, appears on the plaintiff or defendant side in our data. In particular, we calculate the number of times that each pair of firms, and each pair of lawyers, appears together in the sample. From these variables we create dummies for pairs of firms and lawyers that appear together more than once during the sample. "control" variables possibly affecting case processing for reasons outside our explanation – these variables include case type dummies, individual attorney frequency of appearance measures, and attorney gender;
- 3) attorney acquaintance variables – whether the attorneys attended the same college or law school and, separately, whether they were contemporaries, whether one or both of the attorneys is from neither New Jersey nor Pennsylvania, and attorney age; and
- 4) variable relevant to the expected strength of the repeat interaction effect – whether the plaintiff or defendant are institutions, as opposed to individuals; some variables listed above, such as possibly case type or attorney age, also enter here.

These variables allow the development of the following specific hypotheses:

Repeat Play and Case Processing

After accounting for other determinants of case processing, case processing will proceed more quickly, and with lower probability of trial, if repeat play by attorneys

fosters cooperation *and* attorneys internalize client litigation cost incentives. On the other hand, if repeat play fosters cooperation and cooperation takes the form of collusion against clients, we will find that case processing proceeds less quickly, and with greater probability of trial, when the lawyers repeatedly appear jointly. We are well aware that these measures understate the full extent of prior attorney appearances generally, since the attorneys practicing in federal court also practice in state court, and many have practiced in federal court prior to 1994. We view our measures of the frequencies of appearance and interaction not literally, but rather as noisy measures of the frequencies of joint appearance. Measurement error will, of course, bias us against finding any effect of our repeat play measures on cooperation in case processing.

Transmission of the Repeat Play Effect

If learning or reputation are localized to the interacting pair, then only the number of interactions by the pair of attorneys on the case will affect case processing. On the other hand, if information about past practice, or threat of future punishment, can also be passed to lawyers' law firm colleagues, then the effect of repeat play may depend on whether the pair of firms, not attorneys, on the case are repeat interactors. To test this we can include both firm and attorney repeat interaction measures in the same regression. If transmission operates through the firm, then the firm measure will pick up the effect, leaving no effect for the attorney interaction measure. If past behavior is common knowledge, then there should be no effect of repeat play on case processing.

Prior Acquaintance and the Strength of Effects

We expect prior acquaintance, like realized measures of repeat interaction, to promote cooperation. We expect less cooperation when at least one of the attorneys is non-local. We expect more cooperation when the attorneys are former college or law school classmates. Age is presumably related to prior acquaintance. If so, then older attorneys are more likely than younger attorneys to know each other. At the same time, they are less likely to interact in the future.

Is the Mechanism Learning or Reputation?

We have some strategies for trying to distinguish reputation from learning. First, we can distinguish effects of repeat interaction in cases with institutional, as opposed to individual, parties. Institutional clients presumably control their attorneys' behavior in litigation, so that attorney reputational concerns would not dictate strategic choice. Involved attorneys understand that tobacco companies' defense attorneys are not themselves uncooperative. Rather the tobacco attorneys are understood to be carrying out a litigation strategy determined by their client. We expect, therefore, that any effect of repeat interaction on "cooperativeness" in litigation involving institutional clients reflects only learning and not reputation. We will test this by comparing the effect of repeat interaction in cases involving institutional, as opposed to individual, clients.

Before we can determine whether repeat interaction affects case processing, we must first determine whether there is an "experiment" to examine in our data. Table 2 asks how many repeat appearances of firm or attorney pairs occur in our data. The answer is comforting. Of 1404 cases, 304 cases involve pairs of firms appearing jointly more than once in our data. Of these 304 involving pairs of firms repeatedly appearing

jointly, 258 cases involve pairs of attorneys repeatedly appearing jointly. The number of cases involving pairs of attorneys repeatedly interacting is conservatively estimated in the sense that we have discarded cases that appear to be related (for example, in which the same attorney files many cases against a single defendant within a short span of time).

Since our interest is in measuring the effect of repeat attorney interaction on case outcomes, it is important that the variation in repeat interactions be exogenous. This would be violated if a defendant chose her attorney on the basis of a determinant of attorney cooperation *other than* whether the attorneys were repeat interactors. For example, if the defendant chose an attorney who was a close personal or social acquaintance of the plaintiff's attorney, then cooperation would not be the effect of repeat interaction, but rather repeat interaction would be generated by the choice of presumptively cooperative attorneys. We can test for this sort of behavior by examining whether defense attorney characteristics are independent of plaintiff attorney characteristics. We cannot reject the hypothesis that defense attorney genders are independent of plaintiff attorney genders, overall or by four broad case categories. Similarly, we cannot reject independence of plaintiff and defendant ages (grouped as <40, 40-50, 50-60, 60+) within each of four broad case categories. These results imply that attorney selection processes will generate exogenous variation in the amount of repeat interaction that we observe.

Table 3 shows various how our measures of cooperation in case processing vary across characteristics of attorneys appearing on cases. In the full sample of 1404 cases, 16.5 percent of cases are adjudicated, 5.8 percent actually go to trial, and the average amount of time elapsing between filing and disposition is 9.8 months. In addition to case

type information described in table 1, we have the following variables: attorney ages and genders, whether the attorneys are local or “foreign” (nonlocal), whether the attorneys attended the same college or law school and, if so, whether they were classmates. For the 650 cases that involve diversity jurisdiction, we also observe whether the dispute parties are institutions or individuals.

Here we discuss how our measures of case processing vary across characteristics of disputing attorneys with an eye to our theory. We recognize, of course, that more convincing tests await regressions in which we simultaneously control for a variety of influences on case processing. Still, particularly because they are new, our raw data have some interest.

Of the 1404 cases in the full analysis sample, 139 involve pairs of attorneys sharing college or law school *alma maters*. Of these cases, 28 involve pairs of attorneys who were (contemporary) classmates. Interestingly, these cases are processed less quickly, and with higher probability of trial, than other cases. These attorneys presumably know each other, so we would expect them to be able, as much as any pairs, to be able to cooperate. Their higher trial rates and case durations may reflect cooperation taking the form of lawyer collusion against their clients.

Roughly 20 percent of cases (289) involve at least one attorney from neither New Jersey nor Pennsylvania. Non-local attorneys – and local attorneys dealing with foreign attorneys – are likely to have little prior knowledge of their opponents. Moreover, they expect little future interaction. We would consequently expect cases involving a non-local attorney to be processed less cooperatively, and the raw data show that these cases are processed less quickly.

We have no expectation about gender but report these data because we have them. It is not clear how gender relates to our theory. If men and women communicate differently, then one might argue that men, by virtue of being male, have “learned” how to deal with other male attorneys. Given the relative infrequency of female-female attorney interactions, by contrast, female attorneys will have had less opportunity for such learning. In addition, it may be that all attorneys, both male and female, employ a heuristic that labels all female attorneys as non-repeat players, due to the relative infrequency and inexperience of female attorneys (among attorneys older than age 50, females are very rare). Of course, attorneys are not a random sample of persons, and in particular, may be nonrandom in different ways across gender. For whatever reason, female vs. female cases are substantially more likely than to be adjudicated, and are somewhat longer in duration, than other cases.

Table 3 reports case processing information by age combinations of plaintiff and defendant attorney pairs. If some sort of general learning, say with experience, affects case processing, there should be patterns in age. No obvious patterns emerge here, however.

Table 4 compares trial rates and case duration between cases involving: 1) pairs of attorneys whose law firms – and therefore who themselves – do not interact repeatedly; 2) pairs whose firms interact repeatedly but who do not themselves interact repeatedly; and 3) pairs of attorneys who themselves interact repeatedly in our data. Using this breakdown we can, first, check in the raw data for an effect of repeat interaction on case processing. Comparing columns (1) and (3), the effect is large and significant for all case types except contract (where there is no difference for adjudication rates). Contract cases

are likely to have institutional parties on both sides (in our data, 49 percent of contract plaintiffs, and 78 percent of contract defendants, are institutions). If institutional clients control the litigation process, this could explain the smaller repeat play effects for contract cases.

Second, we can use these data to draw some inferences about whether the effect operates through the specific attorney pair, or is transmitted through the attorneys' firms. For every case type but tort, column (2) shows that quicker case processing than column (1). Some of the repeat play effect appears to operate through the firm, not simply the attorney pair.

IV. Regression Results

1. Repeat Interaction and Case Processing

Our goal is to measure the effect of repeat play on speed – and tendency for settlement – in case processing outcomes. To do this we must control for other determinants of case processing speed apart from the measure of repeat interaction. Presumably, one reason that attorney *pairs* appear frequently in our data is that the individual plaintiff and defendant attorneys appear frequently in the data *separately*. We want to distinguish repeat interaction effects from effects of individual attorney speed on case processing, so we control for individual attorney case processing speed using a series of dummies for the numbers of appearances by the plaintiff and defendant attorneys in our sample.⁴ Case category, furthermore, may be systematically related to both case processing and, if bars are specialized, the tendency for attorneys (or firms) to interact

repeatedly. Hence, we control for case category.⁵ Table 5 reports linear probability model (LPM) results explaining the tendency of cases to be “adjudicated.”⁶

Table 5 explores the relationship between repeat attorney interaction and the tendency for cases to be adjudicated. The first four columns of table 5 include only attorney speed and case type controls, as well as various measures of whether the case involves attorneys interacting repeatedly in our data (“repeat attorney pairs”). The first column shows that cases involving repeat attorney pairs are 6.5 percentage points less likely to be adjudicated (a difference that is significant at the 10 percent level). Column 2 shows that, while the effect is slightly larger for case involving pairs interacting three or more times, the difference is not significant.⁷ The third column breaks down the effect of repeat attorney pairs by case type. The estimates are negative for all but contract cases, although significance is only marginal in each individual case type.

The latter three columns add controls for whether one or more attorney is nonlocal (to measure whether case processing is quicker when attorneys are less likely to be familiar), attorney age and gender variables, judge dummies, and dummies for whether the attorneys attended the same college or law school. We also control for whether the attorneys are close in age (within 3 years), and whether the attorneys were (contemporary) classmates at college or law school. These regressions are based on 951 observations with valid data on included variables. Effects of repeat attorney interaction and adjudication

⁴ These coefficients on these variables do not display systematic patterns, nor are they significant. Still, because of their conceptual importance, we include them in all specifications.

⁵ We include separate dummies for each suit type (see table 1) with over 25 cases in our data.

⁶ The reason we use the linear probability model, rather than probit or logit, is that there is sometimes no variation in the dependent variable among observations for which some dummy explanatory variable is 1. The probit model requires omission of either the explanatory variable or the observations.

⁷ Recall that our measure of repeat attorney interaction surely contains measurement error, so that it is not surprising that the effect of three or more does not differ from the effect of two.

remain negative, although generally not significant. The only control that is significant is the classmate variable, showing that cases involving college or law school contemporaries are significantly more likely to be adjudicated.

This latter finding may strike one as surprising. It is likely, however, that school contemporaries have already learned about each other's litigation styles and propensity for cooperation prior to litigating against one another. This mitigates what Ellickson (1991) has called "third party" sanctions: there is nothing left to tell others about the opponent that has not already been told. It may also mitigate "second party" sanctions – threats to retaliate with uncooperative play in future litigation – because in a sense too much may already be known.. A reputation for cooperation acquired over a very long period in a variety of settings may not be forfeited by behavior in a single litigation. A final possibility is that what we are observing is school contemporaries colluding to run up costs and billable hours to the detriment of their clients' interest, and that it is the very fact that the attorneys do interact repeatedly in a variety of settings that sustains such collusion.

Tables 6 and 7 perform the same exercises using trial rates and durations as dependent variables. Many results are similar to those in table 5. Cases involving attorneys interacting repeatedly are less likely to be tried, and tend to be speedier, than cases involving attorneys interacting only once in our data. For trial rates most of the effect operates through attorneys rather than firms, while for duration (as with adjudication rates) the effect is divided between firms and attorneys.

For trial rates and duration, unlike adjudication rates, cases involving at least one non-local attorney are more likely to be tried, and tend to last longer, than cases involving two local attorneys. In cases involving one or more non-local attorney, the attorneys are

less likely to be familiar with one another and thus may find it more difficult to coordinate on a cooperative solution. They also likely perceive a lower probability of future interaction, and thus are less responsive to potential future retaliation for failure to cooperate. As with adjudication rates, cases involving (contemporary) classmates are slower, and more likely to be tried, than cases involving non-classmate attorneys. As explained earlier, this may represent the peculiar strategic incentives facing attorneys who have established reputations long before they litigated against one another. It may indicate that these attorneys are “cooperating” by slowing down case resolution, against the interests of their clients.⁸

V. Distinguishing Reputation from Learning

We have two basic strategies for distinguishing reputation from learning. First, we can see whether the effect of repeat interaction operates through the firm or the individual. The fourth and seventh columns of tables 5-7 include dummies for whether the attorneys’ *firms* interact repeatedly, as well as the basic repeat attorney pair variable. By including both we can attempt to determine whether the information about the nature of the attorney pair interaction goes beyond the two attorneys involved. When included together to explain adjudication (table 5), however, neither variable is significant, although both are negative. We cannot distinguish whether multicollinearity obscures separate effects. In table 6 the repeat attorney effect is dominant, and in table 7 the effect is roughly evenly divided between attorney and firm. That some of the overall effect (attributed solely to attorney in column 1 of tables 5-7) arises with firm repeat interaction that is not attorney

⁸ Curiously, this result also holds for tort cases, where at least plaintiff attorneys (on contingency fees)

repeat interaction suggests that attorney reputation is transmitted among lawyers at opposing firms. On the other hand, this could reflect learning about a firm's style.

Second, we can see how the effect of repeat interaction varies according to whether clients are institutions or individuals. We assume that institutional parties control their own litigation strategy for reasons relating to organizational strategy. Consequently, we expect little scope for repeat play to promote speedy processing through an attorney reputation mechanism for these cases. A repeat effect here might instead reflect learning alone.

We have data on whether parties are institutions or individuals for 650 diversity (tort and contract) cases in our data. Table 8 reports repeat interaction effects for these cases overall (panel 1), and disaggregated by whether both parties are institutional ("Inst. vs. Inst") and a remainder ("non-Inst. vs. Inst.");" and whether both parties are individual ("Ind. vs. Ind.") and a remainder ("non-Ind. vs. Ind.")). In contrast to baseline results in column (1) of tables 5-7, the panel 1 result here is weaker because it includes only contract cases (where repeat effects are weak, as documented above) and tort. Still, the duration effect is significant and negative. When disaggregated in panels 2 and 3, repeat attorney interaction has a larger and more significant effect for cases involving non-institutional clients. The difference is starkest in panel 3. Cases with individuals parties on both sides are substantially shorter (4 months) with repeat attorney interaction. Remaining cases are insignificantly shorter.

VI. Implications

have incentives to process cases quickly.

Our findings confirm the theoretical prediction that attorneys are more likely to pursue cooperative litigation strategies when they frequently litigate against each other (and therefore expect to litigate against each other in the future with a high probability). While our data does not permit us to significantly distinguish degrees of repeat interaction, a history of attorney repeat interaction does have a significant negative effect on both the trial rate and the duration of legal disputes. Also as predicted, the trial rate is significantly lower for attorneys with repeat interactions. This suggests that cooperative attorneys are using discovery as it was intended, as a tool to eliminate information asymmetries and facilitate settlement once common knowledge of the litigants' evidence has been obtained.

As predicted, we found that a history of repeat interaction between attorneys has a much larger impact on outcomes in cases involving one-shot, individual clients than in cases involving institutional clients. We interpret this as evidence in favor of the hypothesis that it is not just that repeat player attorneys learn how to cooperate with one another, but that they have strong reputational interests in cooperating with attorneys they expect to soon encounter again. Institutional parties still get the advantage of attorney familiarity. But because their own reputational interest is at stake, such clients effectively control and override the independent strategic interests of their attorneys.

That results in cases involving individual, one-shot clients seem so strongly affected by the strategic, reputational interests of *attorneys* is normatively ambiguous. It may well be that repeat player attorneys serve their clients well by avoiding unproductive, predatory discovery and costly trials. Given that the typical contingency fee plaintiff's attorney receives only 33% of any court award but bears 100% of the expenses of trial,

there exists a range of settlement amounts which make both the plaintiff's attorney and the defendant better off, while leaving the plaintiff worse off, than under the expected trial outcome.

Our results have a number of potential practical and policy implications. Local counsel laws – laws which require the use of local counsel in litigation – may further the cooperative resolution of legal disputes by increasing the probability that repeat player attorneys interact. As a practical matter, the effectiveness of non-local counsel as way for institutional defendants to signal an intent to play a tough, non-cooperative strategy may hinge upon the fact that non-local counsel is also not a repeat player in particular court or case type.

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Table 1: Types of Litigation in the Sample

Description	Nature of Suit Code	N
Other Contract Actions	190	259
Airplane	310	11
Airplane Product Liability	315	1
Assault, Libel and Slander	320	10
Federal Employers' Liability	330	100
Marine	340	30
Motor Vehicle	350	157
Motor Vehicle - Product Liability	355	5
Other Personal Injury	360	115
Personal Injury - Medical Malpractice	362	20
Personal Injury - Product Liability	365	98
Asbestos Personal Injury - Product Liability	368	1
Other Fraud	370	14
Truth in Lending	371	1
Other Personal Property Damage	380	10
Property Damage - Product Liability	385	5
Antitrust	410	6
Banks and Banking	430	12
Other Civil Rights	440	220
Jobs	442	100
Fair Labor Standards Act	710	14
Labor/Management Relations Act	720	25
Labor/Management Reporting and Disclosur	730	1
Railway Labor Act	740	2
Other Labor Litigation	790	5
Employee Retirement Income Security Act	791	89
Copyright	820	10
Patent	830	9
Trademark	840	29
Securities, Commodities, Exchange	850	26
Environmental Matters	893	19
		1,404

Table 2: Is There an Experiment?

Number of Joint Appearances	Number of Cases in which a Firm Pair Appears this Number of Times ⁹	Number of Cases in which an Attorney Pair Appears this Number of Times
1	1072	1146
2	165	148
3	40	43
4	23	15
5	30	40
6	12	12
7	20	
8	0	
9	0	
10+	42	
Total	1404	1404

⁹ In principle, the numbers in this column should be multiples of the numbers in the first column. That is, if only two pairs of firms have three joint appearances per firm pair, there should be 6 cases involving firm pairs appearing three times ($2*3=6$). However, we calculate our number of joint by firm and attorney pairs prior to excluding cases from the sample for other reasons such as missing data. Hence the apparent, but not real, inconsistency.

Table 3: Case Processing, by Various Characteristics of Attorneys

Description	Number of Observations	Percent adjudicated	Percent Tried	Elapsed Time (months)
Full Sample	1404	16.5	5.8	9.8
Attended Same College or Law School	139	15.8	7.9	9.3
Classmate at College or Law School ¹⁰	28	35.7	21.4	13.0
Both Attorneys Local ¹¹	1111	15.9	5.0	9.6
Non-local Attorney Involvement	289	18.3	9.0	10.7
Male vs. Male	1050	15.9	6.0	9.8
Male vs. Female	190	15.3	3.7	9.1
Female vs. Male	123	18.7	4.9	10.3
Female vs. Female	26	34.6	3.8	10.6
Civil Rights	338	17.8	8.0	10.8
Contract	259	21.6	5.0	9.1
IP, Labor	229	20.5	3.9	9.8
Tort	578	11.9	5.7	9.6
Single Joint Firm Appearance	1072	18.2	6.9	10.6
Repeat Firm but not Attorney Interaction	74	13.5	6.8	9.0
Repeat (Firm and) Attorney Interaction	258	10.5	1.2	7.0
Both Parties Institutional	99	23.2	10.1	10.1
Inst. vs. Ind.	34	23.5	5.9	8.5
Ind. Vs. Inst.	363	13.2	5.2	9.7
Ind. Vs. Ind.	154	11.7	4.5	9.6
(Ages of Pltf, Deft. Att.s)				
Under 40, Under 40	62	19.4	4.8	9.6
Under 40, 40-60	120	15.0	5.0	9.1
Under 40, 60+	44	20.5	2.3	8.9
40-60, Under 40	121	17.4	6.6	9.6
40-60, 40-60	450	15.1	6.9	10.4
40-60, 60+	161	20.5	6.8	10.1
60+, under 40	78	15.4	3.8	9.9
60+, 40-60	229	15.3	6.1	9.9
60+, 60+	139	17.3	3.6	8.7

¹⁰ Attorneys are deemed classmates if their birth years are less than three years apart and they attended the same college or law schools.

¹¹ An attorney is deemed “local” if she has a Pennsylvania or New Jersey zip code.

Table 4: Case Processing by Repeat Firm and Individual Attorney Interaction

Broad Case Type	Variable	Firm (and Att.) Pairs Appearing Once	Firm, not Individual Att. Pairs, Appearing Repeatedly	Attorney Pairs Appearing Repeatedly	Total
		(1)	(2)	(3)	(4)
Civil Rights	Adjudication rate	20.00%	10.00%	11.11%	17.75%
	Trial rate	9.80%	5.00%	1.59%	7.99%
	Duration (months)	11.82	9.30	7.33	10.84
	N	255	20	63	338
Contract	Adjudication rate	21.79%	18.18%	21.74%	21.62%
	Trial rate	7.26%	0.00%	0.00%	5.02%
	Duration (months)	10.30	5.45	6.58	9.10
	N	179	11	69	259
IP, Labor	Adjudication rate	21.78%	0.00%	13.64%	20.52%
	Trial rate	4.46%	0.00%	0.00%	3.93%
	Duration (months)	10.44	9.00	4.50	9.84
	N	202	5	22	229
Tort	Adjudication rate	13.99%	15.79%	1.92%	11.94%
	Trial rate	6.19%	10.53%	1.92%	5.71%
	Duration (months)	10.02	9.82	7.53	9.56
	N	436	38	104	578
Total	Adjudication rate	18.19%	13.51%	10.47%	16.52%
	Trial rate	6.90%	6.76%	1.16%	5.84%
	Duration (months)	10.57	8.97	6.97	9.826211
	N	1,072	74	258	1,404

Table 5: Repeat Interaction and Adjudication Rates (Linear Probability Models)

	Minimal Controls				All Controls		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Constant	0.239	0.239	0.222	0.240	0.041	-0.014	0.042
	0.026	0.026	0.028	0.026	0.149	0.150	0.149
Repeat Firm Interaction				-0.038			-0.014
				0.047			0.056
Repeat Attorney Interaction	-0.065			-0.034	-0.049		-0.038
	0.034			0.051	0.042		0.062
Two Attorney Interactions		-0.061					
		0.038					
Three or More Att. Ints.		-0.072					
		0.049					
Repeat Att Int*Contract			0.018			0.083	
			0.060			0.071	
Repeat Att Int*Tort			-0.089			-0.106	
			0.048			0.055	
Repeat Att Int*Civil Rights			-0.062			0.024	
			0.056			0.095	
Repeat Att Int*other (IP, labor)			-0.137			-0.137	
			0.091			0.098	
One or More Nonlocal Att					-0.008	-0.004	-0.008
					0.033	0.033	0.033
Male vs. Female Att.					-0.036	-0.044	-0.036
					0.039	0.039	0.039
Female vs. Male Att.					0.037	0.034	0.038
					0.049	0.049	0.049
Female vs. Female Att.					0.073	0.063	0.074
					0.099	0.099	0.099
Pltf. Att. Age					0.000	0.000	0.000
					0.001	0.001	0.001
Def. Att. Age					0.001	0.001	0.001
					0.001	0.001	0.001
Same College or Law School					-0.034	-0.039	-0.033
					0.039	0.039	0.039
Contemporary in Age (w/3 yrs.)					-0.011	-0.012	-0.012
					0.031	0.031	0.031
College or Law School Classmates					0.195	0.193	0.195
					0.085	0.085	0.085
Judge Dummies	No	No	No	No	Yes	Yes	Yes
N	1404	1404	1404	1404	951	951	951

Notes: All specifications include flexible plaintiff and defendant attorney speed parameterization and detailed case category dummies. Standard errors appear below coefficients.

Table 6: Repeat Interaction and Trial Rates (Linear Probability Models)

	Minimal Controls				All Controls		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Constant	0.071	0.070	0.068	0.071	0.187	0.181	0.186
	0.017	0.017	0.018	0.017	0.100	0.101	0.100
Repeat Firm Interaction	-0.058			-0.015			0.013
	0.022			0.030			0.038
Repeat Attorney Interaction				-0.045	-0.058		-0.068
				0.032	0.028		0.042
Two Attorney Interactions		-0.072					
		0.024					
Three or More Att. Ints.		-0.028					
		0.031					
Repeat Att Int*Contract			-0.043			-0.040	
			0.038			0.048	
Repeat Att Int*Tort			-0.051			-0.069	
			0.031			0.037	
Repeat Att Int*Civil Rights			-0.082			-0.085	
			0.035			0.064	
Repeat Att Int*other (IP, labor)			-0.040			-0.025	
			0.058			0.066	
One or More Nonlocal Att					0.061	0.062	0.061
					0.022	0.022	0.022
Male vs. Female Att.					-0.008	-0.008	-0.008
					0.026	0.026	0.026
Female vs. Male Att.					-0.033	-0.033	-0.033
					0.033	0.033	0.033
Female vs. Female Att.					-0.047	-0.049	-0.048
					0.066	0.066	0.066
Pltf. Att. Age					-0.001	-0.001	-0.001
					0.001	0.001	0.001
Def. Att. Age					0.000	0.000	0.000
					0.001	0.001	0.001
Same College or Law School					-0.002	-0.005	-0.003
					0.026	0.027	0.026
Contemporary in Age (w/3 yrs.)					-0.017	-0.017	-0.017
					0.021	0.021	0.021
College or Law School Classmates					0.157	0.159	0.157
					0.057	0.057	0.057
Judge Dummies	No	No	No	No	Yes	Yes	Yes
N	1404	1404	1404	1404	951	951	951

Notes: All specifications include flexible plaintiff and defendant attorney speed parameterization and detailed case category dummies. Standard errors appear below coefficients.

Table 7: Repeat Interaction and Case Duration

	Minimal Controls				All Controls		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Constant	10.459	10.460	10.370	10.496	14.031	13.715	14.214
	0.502	0.502	0.539	0.502	2.885	2.921	2.883
Repeat Firm Interaction				-1.541			-1.934
				0.905			1.092
Repeat Attorney Interaction	-2.813			-1.565	-3.131		-1.573
	0.656			0.983	0.818		1.200
Two Attorney Interactions		-2.797					
		0.741					
Three or More Att. Ints.		-2.844					
		0.953					
Repeat Att Int*Contract			-2.431			-2.553	
			1.165			1.382	
Repeat Att Int*Tort			-2.238			-2.264	
			0.931			1.071	
Repeat Att Int*Civil Rights			-3.484			-6.405	
			1.073			1.844	
Repeat Att Int*other (IP, labor)			-3.886			-3.769	
			1.757			1.906	
One or More Nonlocal Att					1.123	1.226	1.043
					0.645	0.647	0.646
Male vs. Female Att.					-1.591	-1.594	-1.552
					0.747	0.749	0.747
Female vs. Male Att.					0.787	0.899	0.859
					0.955	0.956	0.954
Female vs. Female Att.					-0.334	-0.449	-0.252
					1.914	1.916	1.913
Pltf. Att. Age					0.015	0.016	0.012
					0.025	0.025	0.025
Def. Att. Age					-0.030	-0.028	-0.031
					0.027	0.027	0.027
Same College or Law School					-0.993	-1.013	-0.940
					0.765	0.768	0.764
Contemporary in Age (w/3 yrs.)					0.770	0.759	0.708
					0.600	0.599	0.600
College or Law School Classmates					2.429	2.405	2.335
					1.648	1.650	1.647
Judge Dummies	No	No	No	No	Yes	Yes	Yes
N	1404	1404	1404	1404	951	951	951

Notes: All specifications include flexible plaintiff and defendant attorney speed parameterization and detailed case category dummies. Standard errors appear below coefficients.

Table 8: Effects of Repeat Interaction with Institutional and Individual Clients

	adjudication	trial	duration
Repeat Attorney Interaction	-0.015 0.055	-0.038 0.036	-2.050* 1.047
Repeat*non Inst vs. Inst	-0.027 0.057	-0.031 0.038	-2.149* 1.095
Repeat*(Inst vs. Inst)	0.093 0.140	-0.078 0.094	-1.149 2.690
Repeat*(Ind vs. Ind)	0.030 0.108	-0.059 0.072	-4.409* 2.064
Repeat*(non-Ind vs. Ind)	-0.027 0.059	-0.032 0.039	-1.572 1.121

Note: In this table we examine whether the effect of repeat attorney interactions vary between cases involving institutional vs. individual clients. The three panels show selected coefficients from regressions including flexible plaintiff and defendant attorney speed parameterization and detailed case category dummies. Regressions also include disaggregated effects of, say, Inst vs. Inst on the levels, as well as in the repeat play interaction. Standard errors appear below coefficients. The top panel shows the baseline effect for the 650 cases with insitutional vs individual information (available for contract and tort cases). In the second panel, we include cases with institutional parties on both sides as “Inst vs. Inst.” In the third panel, we include cases with an institutional party on either side as “non-Ind vs. Ind.”