Comment

A Classic at 25: Reflections on Galanter's "Haves" Article and Work It Has Inspired

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This piece comments on Galanter's article "Why the 'Haves' Come Out Ahead" and on articles in this symposium that extend Galanter's thesis or put it to an empirical test. It suggests, first, that none of this work goes very far in explaining why the "haves" do better than the "have nots" in social or even legal life; second, that it has yet to be shown that repeat playing is important to litigation; and third, that despite the passage of 25 years, it is still not clear that in litigation the "haves" come out ahead.

Join me in a thought experiment. Imagine you are teaching a graduate seminar on law and American society and you ask your students to write papers on "Why the 'Haves' Come Out Ahead." What would those papers look like?

I see one of my pretend students turning in a paper on such institutions as rules of inheritance that allow the "haves" to transfer their wealth across generations and an educational system, beginning with the home, which ensures that the children of the "haves" will, for the most part, acquire substantially more in the way of social capital as they grow up than the children of the "have nots." Another student would focus on the invention of the corporation, the ways in which the corporate form allows wealth to be amassed, and the power this form gives to those who control it. A third would write generally on the power of money in society, creatively focusing on the health implications of being richer rather than poorer and citing research that relates personal wealth to nutrition, stress levels, blood pressure, and even to the length of the efforts made to resuscitate those whose hearts have stopped.

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Law & Society Review, Volume 33, Number 4 (1999)

I, of course, would be disappointed, for I would be aware I had not communicated the assignment I intended. "No, no," I would say, "I didn't want to know why the 'haves' are better off generally, I just wanted you to tell me why the 'haves' come out ahead when the law is involved." My students, barely hiding their annoyance at having to repeat the assignment, would respond with papers focusing on the leverage that campaign contribution laws give the wealthy with Congress and the executive branch, on the influence of lobbyists, on the ability of high-priced lawyers to find loopholes in statutes, and on the way the federal system allows corporations to play one state against another as they seek tax breaks and favorable labor laws. I would not have the heart to tell my students to again rewrite because what I was really interested in was why the "haves" do well in court.

The point of the thought experiment is that doing well in court, the core of what Marc Galanter sought to explain in his classic article "Why the 'Haves' Come Out Ahead: Speculations on the Limits of Legal Change" (1974) has relatively little to do with the power of the "haves" and why they do so well in social life. Twenty-five years later, we can see that in some ways, Galanter's article was a period piece. The article was written at a time when much of the most interesting law and society work was court-centered or, otherwise, focused on dispute resolution, and it drew much of its power from the way Galanter masterfully assembled the extant literature and brought to the fore, in explaining who prevails in court, a dimension that previously had not been salient: whether the parties in litigation were repeat players in the litigation game or only involved on a one-shot basis. It was a propitious time to write about the limits of lawsuits as vehicles for legal change. The Supreme Court's transformation from the Warren Court to the Burger Court was a recent accomplishment, and liberal social scientists, who dominated among students of law and society, were becoming painfully aware that not only did litigation often fail to empower the impoverished, but also that many Warren Court decisions that had seemed to help the "have nots" had, in fact, had little impact on the quality of their day to day lives. The time was ripe for someone to explain why, in the long run, litigation might actually reinforce the power of the "haves" or, at least, work no dramatic changes in the status quo.

Galanter was concerned primarily with how parties and groups fared in litigation, but the seminal influence of his article is not limited to those whose interests center on courts. Several papers in this symposium recognize that the analysis of who comes out ahead in legal contests between "haves" and "have nots" requires us to look beyond who prevails in legal disputes heard by courts. Harris (1999) starts with judicial decisions affecting the legal rights of the homeless. She emphasizes, however, that to determine who comes out ahead in welfare law disputes

and by how much, we must look not just at court decisions but also at the responses and adaptations that the actors most directly interested in the implementation of welfare policy make to those decisions. The most important actor in implementing these decisions is ordinarily the welfare agency, which will usually have been, at least nominally, the defendant in the litigation. Also potentially important are the lawyers who brought suit; the agency's clients, some of whom were probably plaintiffs in the litigation; the court, if it has maintained supervisory responsibility over implementation; and political actors such as legislators and governors. Thus, understanding whether the "have nots" have come out ahead in litigations depends not only on whether they can be labeled the winners in court but also, and more importantly, on the litigation's aftermath. How this proceeds turns less on the resources the plaintiff and defendant have brought to the legal battle and more on the resources and organizational interests of the parties involved in implementing the court's decision and on the pressures that political and other actors can bring to bear on them. Whether a party is a repeat player or not in litigation makes no obvious difference, but whether the court maintains supervisory authority over how its decision is implemented can be quite important.

Edelman and Suchman's (1999) fine article moves us yet further from Galanter's central concerns toward an organizational focus. Galanter, of course, recognized the importance of organizational actors in understanding why the "haves" come out ahead. Not only were organizational actors an important subset of Galanter's "haves," but Galanter recognized that it was the fact of their organization and the size, wealth, and power that organizations can amass that gave organizational actors many of the advantages they enjoyed in litigation, including both the need to be and the capacity to perform as repeat players. For Galanter, however, law was in the courts. What was in the informal sector was not exactly law, but was either appended to it (as negotiations in the shadow of the law) or private systems of regulation. These private systems were not only not enforcing legal norms, but might, like the mafia, enforce norms oppositional to law. By contrast, for Edelman and Suchman, law, in the sense of state law, is *in the organization.* Legal norms are incorporated in organizational practices, and legal models—like ideas of due process, police responsibility, and appeal systems—are borrowed from state law and given an organizational aspect. What I find most interesting about the world Edelman and Suchman make visible is the degree to which organizations appear to be prevailing over public authorities in the contest of jurisdictions, that is, the right to authoritatively resolve issues that both the public legal system and the organization's *legal system* have some claim to being able to decide. Something new is happening.

Parties, including organizations on one side and individuals on the other, have long been able to resolve legally cognizable differences privately, by agreement—including agreement in advance of a dispute—as in labor-management grievance aberration. Most private settlement arrangements, however, were reached in the shadow of the law in that if negotiations broke down, the parties could always take the matter to court. The situation with labor-management grievance arbitration was different, but here company and union, two powerful repeat players, would have made their own law in the form of a contract, and there is a rough equality when issues are contested.

Now, however, organizations through contracts of adhesion—rule systems that have not been bargained over, have often not been explicitly agreed to, and sometimes have not even been noticed by those whose lives are regulated—can preclude courts, including appeals courts, from hearing matters otherwise clearly within their jurisdictions, such as the alleged malfeasance of security brokers and even, perhaps, civil rights claims. Yet before organizations could oust the jurisdictions of courts through adhesion contracts, courts had to approve, because in most areas where organizational jurisdiction has replaced court jurisdiction, organizations have not had a clear right to do so. Rather, the courts—through by no means preordained interpretations of common-law contract norms or statutory language—have acquiesced in their own replacement. One wonders whether a court system that did not feel overwhelmed by docket pressure would have so readily acquiesced in this transfer of power from the public to the private sphere. Perhaps, though, courts do not see the transfer of power in these terms. As a condition of ceding jurisdiction, courts usually (at least purport to) require organizations to incorporate substantive norms that are either the law's norms or close enough to these norms to be reasonable in the eyes of the court and to provide procedures for resolving issues that seem fair from a legal perspective. Thus, the organizational judiciaries that Edelman and Suchman discuss are not *appended* systems, as Galanter used that term, nor are they fully private systems; they are something else. I would describe them as *partially mimetic systems;* Edelman and Suchman call them *organizational courts.*

Other articles in this symposium do not look beyond the courts, but focus on the context that most concerned Galanter—litigation—and on repeat playing, the variable that he saw as central to the success of the "haves" (Albiston 1999; Dotan 1999; Farole 1999; Songer et al. 1999). Albiston, looking at litigation under the Family and Medical Leave Act of 1993 (FMLA), Farole, examining litigant success in state supreme courts, and Songer and his colleagues looking at decisions in the U.S. Court of Appeals from 1925 through 1988 all reach conclusions that support

Galanter's repeat player hypothesis. None of these studies, however, escapes the fundamental shortcoming of Galanter's original analysis. Although Galanter opened our eyes to the potential importance of repeat player status, he never *proved* that it played the crucial part he gave it in explaining litigant success. There is good reason for this failure. Repeat player status, as characterized by Galanter and as operationalized by the empirical studies in this symposium, is highly correlated with advantages of wealth and power and, in the case of governments, moral status and control of judicial appointments. This situation makes it difficult if not impossible to identify empirically systematic advantages in litigation enjoyed by repeat players because they are repeat players rather than because of their wealth or power or special status as a government litigant. Indeed, it is clear from what we know about criminal cases that a pure repeat player effect does not generally exist. At the most, there seems to be an interaction effect such that those who have power and are repeat players do better than those who are not powerful, whether or not the latter are repeat players. We cannot be sure from the empirical work that there is even an interaction effect because we are missing the crucial comparison between powerful parties who are repeat players and those who are not. In this symposium, only Hendley et al. in their study of Russian enterprises draw a comparison along these lines, and they do not find a consistent repeat player effect.

Galanter recognized from the start that not all repeat players do well when in court. Some, like the habitual drunk or prostitute, lose again and again. Indeed, the criminal justice system is designed so that defendants who are repeat players will fare worse than those in court for the first time. Not only do those with records that imply previous court experience receive stiffer sentences than first-time offenders, but their vulnerability to impeachment by prior convictions means that they do worse in jury trials than those being tried for the first time; they are either impeached with their prior convictions if they testify or they refrain from taking the stand for fear of impeachment (Kalven & Zeisel 1966). On the civil side, the evidence is more anecdotal, but one hears stories of litigants who file a series of claims that are dismissed as frivolous; indeed some file insubstantial claims so often that they become known offenders, and there is no evidence that the court experience of some debtors helps them when the next creditor sues them in small claims court. None of this information would be news to Galanter, for, as I have mentioned, he noted the existence of repeat losers from the start. Galanter's focus on repeat player status treats this variable, however, as a main effect rather than as a component of an interaction effect. In doing so, it draws attention away from the impor-

tance of wealth and power as the key advantages the "haves" have, in courts and elsewhere.

Albiston and Songer et al. attempt to show that repeat player status by itself is important to explaining litigant success in the arenas they are studying. I like their papers and admire their attempts, but I do not think either completely succeeds.

Albiston makes an important contribution: she identifies a mechanism—namely the summary judgment process and the control over appeals from it—by which repeat players may generate a favorable jurisprudence even if they are losing most cases brought against them. Yet there is much her data do not tell us. We do not, for example, know whether those defendants who prevail in summary judgments are, in fact, repeat FMLA litigants or the extent to which they foresee this possibility. We do not know if summary judgment motions are made strategically, as the theory suggests, or are a routine part of any FMLA defense. If they are the latter, they may still skew precedent as Albiston suggests, but they are a defendant advantage rather than a repeat player advantage. Most important is that we do not know if the precedents generated by employer success at the summary judgment stage, and on appeals from these judgments, has meant that cases that succeeded when the FMLA was new no longer succeed. It could be that the precedents created simply make visible the legal contours that from the start have guided most plaintiff decisions whether to file claims as well as the dispositions of most claims that never reached court. We also do not know if the pattern of decisions that has been generated in FMLA cases reflects not the planned impact of strategic motions and appeals but rather that summary judgment motions studied have been made in and appealed to courts dominated by Reagan and Bush judicial appointees. Perhaps if the appellate courts were dominated by liberal judges, defendant's losing summary judgment motions would generate more published lower court opinions and plaintiffs would win when appealing summary judgements against them. In such a case, defense efforts to secure summary judgment might generate precedents that hurt them in the long run. Finally, we do not know to what extent defendants' success in generating favorable summary judgment decisions and appellate decisions reflects their wealth and power (as manifested, for example, in their ability to hire excellent lawyers) rather than advantages that accrue to them only because they are repeat players.

Songer et al. believe that their data suggest an advantage for repeat players that is more than a wealth effect because in the federal courts of appeals, the repeat player status of appellants is more closely linked to success than the repeat player status of respondents. They argue that this difference is consistent with a unique repeat player advantage because although wealth and

prestige should benefit both appellants and respondents equally, only potential appellants can take full advantage of the sophisticated litigation strategies that Galanter suggests are characteristic of repeat players. Yet even though Songer et al.'s data are consistent with the claim that repeat player status contributes importantly to litigant success, they by no means prove it. The repeat player advantage in their multivariate analysis seems largely to reflect the fact that the win rates on appeal of state and local governments (45.5%) and the federal government (51.3%) are considerably higher than the win rates of businesses (30.8%) or individuals (26.1%). Reasons other than different degrees of repeat playerness can explain these rates, however. For example, it may be that, rather than appealing with an eye to creating winning precedent, government attorneys appeal cases in which they think an appeal is particularly in the public interest. Appellate judges, as part of the governing establishment, may be prone, regardless of party affiliation, to share these sensibilities, and so the government may do particularly well in the cases it chooses to appeal. Thus, regardless of the court's composition, the solicitor general's office is particularly influential when it urges the Supreme Court to take an appeal or argues a case as an amicus on behalf of a state or private party. Farole (1999), looking at five state supreme courts, finds government win rates consistent with those reported by Songer et al., but Farole believes that it may be because governments are special litigants. It is *government,* he notes—not necessarily all repeat players—that come out ahead in five state supreme courts.

Moreover, even if governmental litigants have no special influence with courts and do not benefit from their wealth or repeat playing, they might still win more of their appeals because incentives on attorneys may affect which cases are appealed. Private attorneys have financial incentives, regardless of case strength, to encourage their clients to appeal, or at least to not discourage clients who want to appeal; they usually get paid the same regardless of whether the appeal succeeds. Government attorneys, on the other hand, get paid the same regardless of whether cases are appealed, but their success rates on appeal may affect how they are regarded by their peers as well as their chances for raises or promotions. If so, they have incentives to avoid appealing cases that are likely losers. Moreover, often heavy caseloads mean that government attorneys have an array of cases that might be appealed but do not have the time and resources to appeal them all. Hence, government attorneys may have to make choices among candidate cases for appeal that private litigants and their attorneys do not have to make. If so, what is important about the government's repeat player status may not be that it gives rise to sophisticated precedent-oriented litigation strategies but rather that it gives rise to enough cases to appeal

that attorneys triage cases and naturally refrain from appealing the weaker cases that less frequent repeat players, or the government itself (if it had fewer cases per attorney), would bring.

Another reason to question whether Songer et al.'s findings support their suggestion that repeat player status is important, independent of power, is that they only report a small difference between the success that businesses and individuals enjoy as appellants. The implication of these data are ambiguous because businesses more than individuals may be involved in appeals against governments or other businesses. Moreover, the data do not tell us what proportion of their business cases involve businesses that are repeat players. A similar ambiguity exists when governments are litigant. Although governments are involved in considerable litigation, this does not mean that in every case the government acts as a repeat player. For example, in defending civil actions for damages, the government may appeal because of alleged case-specific evidentiary errors at trials. Such appeals are likely to stem not from a desire to establish precedent but from the same kind of calculations individual litigants make, namely, given what is at stake and the likelihood of prevailing on appeal, an appeal is likely to be worth the cost.

A general weakness shared by the empirical studies investigating Galanter's repeat player hypothesis is that they assume that some classes of litigants (especially governments and businesses) are repeat players whereas others (usually individuals) are not. The assumption is not always true. Cases supposedly involving repeat players may involve organizations that are not frequently in court or cases of types that are not often litigated, although the party frequently litigates other kinds of cases in which it appears. In the first situation, the party is not a repeat player, although the research codes the party that way. In the second situation, the party in court does not have the case-specific experience and incentives to play for precedent associated with repeat playing, and we should not expect a repeat player advantage even though we would expect advantages that accrue to size and power. Hendley et al. (1999), in their contribution to the symposium, control for the first of these problems, for they look at the amount of repeat playing by the enterprises they study. They do not find a consistent repeat player effect. One cannot generalize from their work to U.S. courts, however. Hendley et al. are investigating businesses in the transitioning Russian economy, and they are not looking at success in litigations but at various legal devices these enterprises use to manage disagreements and conflict.

Although Hendley et al. do not find a consistent repeat player effect, they do find that using lawyers matters. Dotan (1999), in his study of litigation before the Israeli Supreme Court, also finds that lawyers are important, as does Harris in her study of litigation involving the homeless. These studies call at-

tention to a major omission in Galanter's "Haves" article and much of the empirical research that builds on it. Neither Galanter's original essay nor much of the work that followed adequately recognizes that where individual one shotters can call on repeat playing lawyers, they may enjoy many of the advantages that repeat players enjoy; in addition, their attorneys often have incentives to act as repeat players, regardless of their individual clients' interests. The use of lawyers by one shotters and, in particular, the use of lawyers who are specialists diminishes the degree to which it is repeat playing rather than wealth and power that gives the "haves" what advantages they enjoy in litigation. The interests of lawyers as repeat players, however, can also disadvantage one-shot clients, because a lawyer may have an incentive to reach a civil settlement or plea agreement that is not in a client's interest. Lawyers are less likely to "sell out" wealthy repeat playing clients because it might cost them the client's future custom.

To summarize, the originality of Galanter's insight about the potential importance of repeat playing to litigants has led some scholars to treat repeat playing as more important than I expect it is. Although I do not doubt that repeat player status can matter or that it has on occasion mattered in the ways Galanter suggests, I think it generally matters only in interaction with other attributes of wealth and power. As such, it is part of a constellation of attributes the "haves" are likely to possess that increases the likelihood that they will prevail in litigation. I expect, however, that it is a less important explanatory variable than several other aspects of that constellation. To evaluate this possibility, we need research that looks at the importance of repeat playing when litigants are equally situated with regard to other attributes of power. Most research that seeks to evaluate the importance of repeat playing, however, focuses on litigation between unequals. From this work, including several studies in this symposium, we cannot specify the degree of advantage uniquely associated with actual or contemplated repeat playing of the litigation game. Nor can we learn whether any advantages repeat player status confers vary with the general strength of litigants or with the matters litigated. For example, there may be a negative or curvilinear relationship between repeat playing and other aspects of litigant strength such that repeat playing may affect litigation outcomes more when both litigants are weak. Or, it may have its peak effects when litigants are of moderate strength rather than very strong or very weak. The first possibility might be true because experience with courts might be more important when litigants do not have the resources to hire top attorneys or develop elaborate cases than when they have the resources to do these things. The second possibility might exist if, when litigating with moderately strong one shotters, repeat players of similar strength add to

the advantages of experience, the advantage of playing (or having in the past played) for precedent; whereas weak repeat players have only an experience advantage over weak one shotters (they cannot afford to play for precedent) and strong repeat players have only a "playing for precedent" advantage over strong one shotters (the latter's ability to hire top-flight lawyers negates the former's experience advantage).

Galanter's "Haves" article assumes that the "haves" come out ahead. If we mean "ahead in life" and we equate "ahead" with material well being, this is necessarily true, for it is coming out ahead that makes people "haves". But it is not necessarily true that the "haves" come out ahead in litigation, so Galanter's assumption is not a tautology. Dotan (ibid.) challenges this conclusion in his study of litigation in the Israeli High Court. Dotan's work, however, does not directly test Galanter's expectation that the "haves" will come out ahead in litigation, for Dotan's "haves" and "have nots" are not litigating against each other; each is opposing a municipal or state public agency. Thus, each is, relative to its opponents, a "have not" as Songer et al. operationalize the concept. Still, one would expect that Dotan's "haves" would have less of a relative disadvantage than his "have nots," but differences are small and disappear when the "have nots" have counsel to represent them.

Yet even if Doton's data came from litigation where "haves" opposed "haves nots," they would not necessarily disprove Galanter's contention that in litigation the "haves" come out ahead. Galanter does not assume that the "haves" will have more recorded court victories than the "have nots," but, rather, he claims that "haves" generally do better than "have nots" in legal contests between them, regardless of whether or not the contests end in a formal decision and written opinion. Moreover, Galanter sees the "haves'" success as rooted not as much in immediate court victories as in their ability to generate rules that advantage them in future bargaining or litigation with "have nots." Only over the long run does repeat playing by the "haves" necessarily give them the advantage in contests with "have nots." This implication of Galanter's work has not been tested. Although it is no doubt true that capitalist law often favors the interests of the "haves," there is little reason to believe that the primary, or even an important, reason for this tilt lies in the clever litigation strategies of the "haves." To the extent that "haves" continue to acquire law that favors their interests in contests with "have nots"—as in recent, partially successful efforts to rein in punitive damage awards—the returns to dollars spent on lawyer lobbyists or public relations firms are probably far greater than the returns to dollars spent on lawyer litigators.

Because of the difficulty of linking the advantages "haves" enjoy in their dealings with "have nots" to rules arising out of prior

litigation, most research that seeks to test Galanter's theory compares the relative success of "haves" and "have nots" when they confront each other in court and uses court records and published decisions to operationalize success. The research of Albiston, Farole, and Songer et al. is in this tradition. Like similar prior research (Wheeler et al. 1987; Songer & Sheehan 1992), these scholars find that when "haves" and "have nots" confront each other in court, the "haves" win disproportionately often. This consistent finding, however, does not prove that the "haves" come out ahead when they go to court.

Albiston, Farole, and Songer et al. all see their results as largely explained by how cases are selected for appeal. Sophisticated "haves" presumably only appeal cases they are likely to win, whereas "have nots" often cannot afford to appeal and are presumably more haphazard in their selections of cases when they can. The result theory tells us that the mix of cases heard by appellate courts is dominated by cases that "haves" have selected because they are likely to win. Missing from the mix are potentially important, precedent-setting cases that "haves nots" are likely to win. When a "have not" threatens to appeal such a case, the "have," motivated by his or her interest as a repeat player, typically settles, on the plaintiff's terms if necessary, to prevent the appellate court from setting an adverse precedent. "Haves" will even settle cases they stand a good chance of winning if a likely victory promises no new favorable precedent but a possible loss threatens to create a costly adverse precedent. Indeed, if the matter is important enough, "haves" will settle if their already good chances of winning will be enhanced by waiting for a case with a stronger fact pattern. But if the "haves" are settling with the "have nots" cases they are likely to lose, and even some they might win, they may not be prevailing in most of their litigation with "have nots," even if trial or appellate court verdicts suggest that they are. They may, on the other hand, be more often the victors, even when settlements are taken into account. We cannot determine from appellate decisions or trial court verdicts alone who fares better in court, "haves" or "have nots." Not only must settlement data be considered, but to the extent winning in court also involves securing important precedents, we must look at the quality of decisions in appealed cases. At least in theory, it is possible that although the "haves" win often on appeal, their victories set few important precedents, and that the "have nots'" less frequent appellate court victories are precedents that skew the law in their favor.

Moreover, it is not always clear what it means to come out ahead in litigation, for winning does not necessarily make the victor better off. Consider outcomes at the other end of the judicial hierarchy from appeals to higher courts: actions to collect debts in small claims courts. Often such actions involve organiza-

tions on one side and individuals on the other. When they do, the organization is usually the creditor-plaintiff, a repeat player, and a "have," whereas the individual is typically the debtor-defendant, a one shotter, and a "have not" (Vidmar 1984). If we count total victories, including default judgments, "haves" are overwhelmingly successful in securing verdicts, but when they do win, they often collect only a fraction of the judgments they have secured, if they collect at all. But when individual "have not" plaintiffs sue organizational "have" defendants and they win, they are likely to collect fully on their judgments (McEwen & Maiman 1984).

So, despite court victory statistics that show that "haves" win far more often than "have nots," who comes out ahead in small claims court litigation? Moreover, in deciding who comes out ahead, should not justice norms figure in the assessment? If businesses win 90% of the debt collection cases they bring, have they come out ahead in court? Suppose 95% of the claims they bring are valid; surely this is relevant in assessing how well they have done. If 95% of the cases the "haves" bring are valid, even if they win every case they bring, are they coming out ahead when lawyers' fees, collection costs, and deadbeat, hard-to-find, or judgment-proof defendants reduce their recoveries to only a fraction of what they are owed? Certainly they are not ahead compared with where they would be had the "have nots" lived up to their legal obligations. Indeed, Vidmar (1984) found that if instead of counting all small claims court judgments secured by businesses litigating with individuals as "have" victories one counted only cases in which the business secured at least half of the amount sought, there was no tendency for business to come out ahead even though they were more likely than individuals to be repeat players and be represented by counsel.

In tort cases, unlike small claims court debt collection cases, repeat playing "haves" are usually defendants and one-shot "have nots" are plaintiffs. Depending on the tort in question, either plaintiffs or defendants may secure the greater number of recorded wins. But even if defendants win more than 50% of the tort cases that reach verdict, as they do in medical malpractice trials, they do not necessarily come out ahead in the litigation process. Many claims are settled before trial, some for their nuisance value and others for large sums of money. Repeat playing "haves" also lose cases to one-shot plaintiffs at trial, and even when they win, their court costs can be enormous. Most important is that we again do not know the justice baseline. If we cannot say how much defendants in litigated tort cases *should* be paying in damages, how can we say who comes out ahead in disputes that are taken to court? From another perspective, that of the status quo ante, plaintiff "have nots," as a group, clearly come out ahead. Given the contingent fee, they cannot end up much worse

off than they were before bringing suit, and sometimes they end up far wealthier.

Even in the situation Edelman and Suchman describe—where the organization becomes the court—we cannot be sure that the organization, in its decisionmaking, comes out ahead. Again the question is compared with what and to whom? The price of becoming the court is often incorporating the legal system's rules in the organization. Even if these rules are watered down, both as abstract norms and as law in action, because they have been institutionalized within the organization they may still offer more protection to individuals at the expense of the organization than they would if matters were left to the reactive mobilization of courts or government agencies by individuals claiming harm. Moreover, the prior legal regime may have offered little protection because legally or as a practical matter recourse to the courts may not have been available. A company court that provides minimal review and only slight due process before a worker is fired may, for example, still advantage the worker more than the "at will" employment regime that might exist if there were no company court.

In short, even though it is 25 years since Galanter wrote, I think we still cannot safely conclude, based on the empirical research that has been done to date, that the "haves" come out ahead in court. We certainly cannot conclude that they come out ahead mainly because they are repeat players and not because of other advantages they possess. My guess is that "haves" do fare better than "have nots" in court, but I doubt if repeat playing is the main reason. Granted, legal rules are important, but here I expect that the "haves" have advanced their interests more through influencing legislation than through playing the litigation game for precedent. Moreover, even if "have nots" tend to lose court contests to "haves" more often than they win, I expect that they are relatively more successful in litigation than they are in other spheres where their interests and those of the "haves" clash. When it comes to success in life, the imaginary students with whom I began this paper got it right. The capacity to litigate well does far less than other variables to explain the vast and increasing inequality that separates "haves" from "have nots" in the United States.

Twenty-five years ago, Galanter's "Why the Haves Come Out Ahead" neatly organized much of the then-current knowledge on the associations between party characteristics and success in litigation. In its focus on repeat player status, it identified an interesting and to that point seldom recognized variable that might figure not just in litigation victories but also in the development of legal regimes that advantage "haves" both in court and out. We can see from the articles in this symposium and the work they reference that Galanter's paper was indeed seminal. Work is sem-

inal, however, when it begins a process of investigation, not because it answers most questions. If we look only at litigation, many questions raised by Galanter's analysis remain unanswered. If we are committed empiricists, we still must wonder about the importance of repeat player status in determining courtroom victories, and even whether, when they litigate, the "haves" really come out ahead.

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