

A New Look at Judicial Impact: Attorneys' Fees in
Securities Class Actions After *Goldberger v.*
Integrated Resources, Inc.

Theodore Eisenberg^{*}
Geoffrey Miller^{**}
Michael A. Perino^{***}

INTRODUCTION

Political scientists studying the judiciary have long been interested in what, if any, impact judicial decisions have on their intended audiences, particularly the lower courts that must comply with them. Compliance in this sense has been defined as the lower court's proper application of standards the superior court has enunciated in deciding all cases raising similar or related questions.¹ Most studies find widespread compliance in lower courts,² with only rare instances of overt defiance.³

This Article attempts to address three questions in the extant judicial impact literature. First, the existing studies use rather insensitive measures of compliance and thus may fail to identify instances of subtle resistance to higher court rulings. Justice

* Henry Allen Mark Professor of Law and Adjunct Professor of Statistical Sciences, Cornell University.

** Stuyvesant Comfort Professor of Law, New York University.

*** Dean George W. Matheson Professor of Law, St. John's University.

1. This definition of compliance is from G. ALAN TARR, *JUDICIAL IMPACT AND STATE SUPREME COURTS* 35 (1977).

2. See, e.g., LAWRENCE BAUM, *THE PUZZLE OF JUDICIAL BEHAVIOR* 115–19 (1998); Sarah C. Benesh & Malia Reddick, *Overruled: An Event History Analysis of Lower Court Reaction to Supreme Court Alteration of Precedent*, 64 J. POLITICS 534, 536 (2002); Donald R. Songer & Reginald S. Sheehan, *Supreme Court Impact on Compliance and Outcomes: Miranda and New York Times in the United States Courts of Appeals*, 43 W. POL. Q. 297 (1990).

3. BAUM, *supra* note 2, at 116 (citing studies).

O'Connor once noted that judges "know how to mouth the correct legal rules with ironic solemnity while avoiding those rules' logical consequences."⁴ In many, if not most cases, lower court judges that do not like a controlling precedent have a number of strategic options open to them to avoid applying that precedent, including interpreting the precedent narrowly, distinguishing it factually, or disposing of the case on procedural grounds.⁵

Equally problematic for the study of judicial compliance is the malleability of *stare decisis*. Under a strict view of precedent, lower courts are bound to follow the legal principles articulated by courts superior to them in the judicial hierarchy.⁶ But this doctrine can bend in practical application. Courts may exercise discretion in determining whether to adhere to *stare decisis* and may consider, among other things, social or economic changes that render a precedent no longer applicable.⁷ This raises an issue of classification: which decisions not to apply precedent constitute the appropriate exercise of judicial discretion and which are simply noncompliant?

The ability of lower court judges to avoid precedents they do not like and the flexibility of *stare decisis* confound judicial impact studies, particularly because the variables used are often only weak proxies for compliance—for example, the proportion of liberal or conservative decisions following a liberal or conservative Supreme Court decision.⁸ With these kinds of dependent variables, judicial politics scholars recognize that researchers may often be unable to identify instances of noncompliance.⁹

4. *TXO Prod. Corp. v. Alliance Res. Corp.*, 509 U.S. 443, 500 (1993) (O'Connor, J. dissenting) (quoting *Garnes v. Fleming Landfill, Inc.*, 413 S.E.2d 897, 907 (1991)).

5. Benesh & Reddick, *supra* note 2, at 536.

6. RONALD DWORKIN, *LAW'S EMPIRE* 24–26 (1986).

7. Kenji Yoshino, Note, *What's Past Is Prologue: Precedent in Literature and Law*, 104 *YALE L.J.* 471, 476 (1994).

8. See generally Donald R. Songer, *The Impact of the Supreme Court on Trends in Economic Policy Making in the United States Courts of Appeals*, 49 *J. POL.* 830 (1987); Ronald Stidham & Robert A. Carp, *Trial Courts' Responses to Supreme Court Policy Changes: Three Case Studies*, 4 *L. & POL'Y Q.* 215 (1982).

9. See Donald R. Songer, *The Circuit Courts of Appeals*, in *THE AMERICAN COURTS: A CRITICAL ASSESSMENT* 35, 42–46 (John B. Gates & Charles A. Johnson eds., 1991) (noting that existing studies have not found areas in which courts of appeals are "clearly defiant or overtly noncompliant," but recognizing the fact that instances of noncompliance may have been too subtle for the empirical tests employed).

A second limitation in the judicial impact literature lies in its restrained focus. Judicial politics in general and the judicial impact literature in particular tend to have a “high court” bias. Scholars typically devote most of their attention to whether the United States courts of appeals or state supreme courts comply with the decisions of the United States Supreme Court.¹⁰ Comparatively few studies¹¹ examine whether United States district courts comply with the precedents of their circuits, even though most judicial activity occurs at the trial court level. Over the last two years, there was an average of nearly 326,000 civil and criminal cases commenced annually in the United States district courts, compared to about 63,000 appeals commenced—about 19% of the district total.¹² During the same time period, the Supreme Court on average granted review in less than one hundred cases.¹³ A focus on high court precedents misses most instances where a court must decide whether or not to comply with controlling precedent.

In addition to devoting most of their attention to the tip of the judicial iceberg, scholars studying judicial politics less frequently examine the ultimate consumers of judicial policies—the members of society who are subject to the rule the court has announced.¹⁴ This limitation in the literature is understandable; consumer behavior is typically much less visible than the behavior of the implementing courts and therefore much more difficult to study.¹⁵ Yet judicial decisions are only words on paper; the real significance of those

10. See Nancy Maveety, *The Study of Judicial Behavior and the Discipline of Political Science*, in *THE PIONEERS OF JUDICIAL BEHAVIOR* 18 (Nancy Maveety ed., 2003).

11. See, e.g., Lawrence Baum, *Responses of Federal District Judges to Court of Appeals Policies: An Exploration*, 33 *W. POL. Q.* 217 (1980); Stidham & Carp, *supra* note 8 (finding that district court opinions changed in a manner consistent with Supreme Court’s new direction).

12. ADMIN. OFFICE OF THE U.S. COURTS, *JUDICIAL BUSINESS OF THE UNITED STATES COURTS: 2007 ANNUAL REPORT OF THE DIRECTOR* 85, 139, 208 (2008). Of course, the numbers are even more skewed than this. Within each case commenced in the district court, it will not be unusual for the judge to write numerous decisions, some of which may be subject to appeal and some of which will be effectively irreversible.

13. *Id.*

14. See BRADLEY C. CANON & CHARLES A. JOHNSON, *JUDICIAL POLICIES: IMPLEMENTATION AND IMPACT* 92–114 (2d ed. 1998).

15. *Id.* at 95.

decisions can be measured only by examining how they affect litigants and other impacted parties.

The data on comparative caseloads at the various levels of the judicial hierarchy raise a third question. If large scale compliance exists, what mechanisms drive it? It has become common to view the judicial hierarchy as a principal-agent system.¹⁶ Lower court agents subject to light monitoring have the ability to shirk, which in the case of judging may involve the district court advancing its own policies rather than those the appellate court prefers. The caseload data suggest a relatively small likelihood that any individual decision will be heard on appeal (much less reversed), a situation which might create prime conditions for non-compliance.¹⁷ Nonetheless, many scholars assume that, even when the likelihood of reversal is remote, fear of reversal plays an important role in keeping lower courts in line.¹⁸ All else being equal, a judge who is reversed more often may suffer a loss of reputational capital or reduced prospects for promotion. If these incentives exist, then one would expect that where the probability of reversal is higher, compliance will be higher as well. Yet some recent evidence raises questions about this argument, finding that there is actually little correlation between the likelihood of Supreme Court review and compliance.¹⁹

The data analyzed in this Article allow us to address each of these questions (the appropriate measure of compliance, the impact of stare decisis at the trial court level and in the consumer population, and the mechanisms driving lower courts to comply with or resist controlling precedent). Specifically, we examine how the district courts in the Second Circuit responded to the decision of the Court of Appeals in

16. See, e.g., Donald R. Songer, Jeffrey A. Segal & Charles M. Cameron, *The Hierarchy of Justice: Testing a Principal-Agent Model of Supreme Court-Circuit Court Interactions*, 38 AM. J. POL. SCI. 673 (1994).

17. Consider that, in 2007, of the 62,846 appeals that were terminated in the U.S. Courts of Appeals, there were only 2,393 reversals (about 4% of the total). In the same year, about 326,000 civil and criminal cases were commenced in the district courts. Taking this latter figure as a rough approximation of cases available for appeal yields a reversal rate of less than 1%, a figure that would drop even further given that a single case may yield multiple decisions capable of appeal. ADMIN. OFFICE OF THE U.S. COURTS, *supra* note 12, at 85, 113, 139, 208.

18. David E. Klein & Robert J. Hume, *Fear of Reversal as an Explanation of Lower Court Compliance*, 37 L. & SOC'Y REV. 579, 582 (2003) (citing studies).

19. *Id.*

Goldberger v. Integrated Resources, Inc.,²⁰ a 2000 case that mandated strict scrutiny by trial court judges of attorneys' fee applications in class actions and admonished trial courts to seek "moderation" in awarding fees. *Goldberger* strongly suggested that excessively high fee awards had a much greater chance of reversal than excessively low ones. If federal district courts complied with *Goldberger*, we would expect to see lower fee awards and greater scrutiny of fee requests. We would also expect that plaintiffs' attorneys would moderate their fee requests.

How much can this analysis tell us about district court compliance generally? After all, fee setting in securities class actions is just one narrow legal issue arising in a specific litigation context. Nonetheless, studying this setting also has several advantages. First, the variables relevant to fee requests and awards have been thoroughly studied and thus we know a great deal about how they are determined.²¹ When combined with a large database of fee awards (approximately seven hundred), we have the potential for a much more precise instrument for studying compliance than past studies have been able to exploit, and therefore we have a much better chance to identify more subtle forms of noncompliance.

Second, we have information about the ultimate consumer population—the plaintiffs' attorneys in securities class actions. Civil procedure rules require those attorneys to publish a notice of any settlement and to specify the fee request they intend to make to the court. The availability of this information allows us to examine whether *Goldberger* affected their behavior as well.

Third, the dynamics of this particular setting shed new light on the hypothesis that judges comply with precedent in order to avoid the chance of reversal on appeal. The *Goldberger* court suggested that reversal on appeal will be more common when a judge awards high fees than when the judge awards low fees. But, as is explained in

20. 209 F.3d 43, 45 (2d Cir. 2000).

21. See Theodore Eisenberg & Geoffrey P. Miller, *Attorney Fees in Class Action Settlements: An Empirical Study*, 1 J. EMPIRICAL LEG. STUD. 27 (2004); Michael A. Perino, *Institutional Activism Through Litigation: An Empirical Analysis of Public Pension Fund Participation in Securities Class Actions* (St. John's Legal Studies, Paper No. 06-0055, 2006), available at, <http://papers.ssrn.com/abstract=938722>.

more detail below,²² in the average securities case, it is the plaintiffs' attorneys, not the members of the class, who are the most likely to appeal a fee award. Since they are only likely to do so if the court awards a low fee, district judges looking to avoid appeals and the potential for reversal have an incentive to give the attorneys precisely what they asked for—the exact opposite of what the *Goldberger* court wanted. It is only where settlements are very large that it may be worthwhile for a class member to undertake the costs and burdens of appeal. These dynamic processes yield a testable hypothesis: if compliance is tied to fear of reversal, we may observe greater compliance as settlement size increases. If on the other hand we observe compliance remaining constant or decreasing with settlement size, we might doubt that district court behavior in fee-setting is driven principally by reversal concerns.

Our empirical analysis yields three primary results. First, contrary to what might be expected, *Goldberger* is not correlated with a general decline either in fee awards or in fee requests. On average, fees demanded and fees received by attorneys in the Second Circuit post-*Goldberger* are no lower than the fees they demanded and received earlier, or the fees in other circuits.

Second, we find that, although *Goldberger* did not result in a wholesale lowering of fees, it did have an impact on fee-setting practices. Specifically, there appears to be an interaction between *Goldberger* and settlement size. As settlement size increases, both fee requests and fee awards rise at a slower rate in the *Goldberger* cases (later cases in the Second Circuit) than in the non-*Goldberger* cases (cases in other circuits and pre-*Goldberger* cases in the Second Circuit). The moderating effect of *Goldberger* in larger cases suggests support for the proposition that judges are responding to fear of reversal.

Third, we examine the ratio of the award to the request as a measure of the scrutiny with which courts review fee requests. Here we observe the same general pattern. In some of the models, the interaction term is again negative and significant, meaning that increases in settlement size are associated with judges reducing

22. See *infra* Part II.

requests to a greater degree in *Goldberger* cases than in non-*Goldberger* cases. These findings are consistent with the hypothesis that compliance is tied to the probability of appeal and reversal: judges knock fees down more frequently in big cases, where they can anticipate an objector's appeal based on the argument that the fee award is too high, than in small cases, where any appeal is more likely to come from an attorney who argues that the fee is too low.

Overall, our findings might suggest that district courts have complied only imperfectly with the admonitions of the *Goldberger* opinion: fee-setting practices, post-*Goldberger*, are not markedly different from those that prevailed before. Yet we would not necessarily interpret these findings as evidence of disobedience. Rather, our study suggests limitations in the simple principal-agent model of judicial hierarchy. Appeals courts like the Second Circuit see attorneys' fees issues in securities class action cases only rarely; district courts—especially in New York City—face them on a regular basis.²³ Our data are consistent with a view of *Goldberger*, not as conveying definitive orders from higher authority, but rather as an invitation to a dialogue: a request that the district courts which have regular exposure to the issues think harder when awarding attorneys' fees and report back (through subsequent appeals) on the results of that reconsideration. So viewed, our study suggests that the district courts may be complying with that broader mandate to reconsider and report, and that their consensus view, after due deliberation, is that the pre-*Goldberger* approach to the determination of fees in securities class action cases reflected a reasonable accommodation of the competing policies of incentivizing class counsel and protecting the class against excessive awards.

The remainder of the Article is structured as follows. Part I describes fee awards generally and the *Goldberger* decision. The section also articulates a number of testable hypotheses concerning the impact of *Goldberger*. Part II describes how the dataset used in the analysis was constructed and specifies how the variables were defined. Part III contains the empirical analysis. Part IV discusses these results. A brief conclusion follows.

23. See *Harman v. Lyphomed, Inc.*, 945 F.2d 969, 973 (7th Cir. 1991) (“District courts are far better suited than appellate courts to assess a reasonable fee in light of the case’s history.”).

I. ATTORNEYS' FEE AWARDS AND THE DECISION IN *GOLDBERGER V. INTEGRATED RESOURCES, INC.*

Goldberger was a securities class action growing out of the junk bond scandal involving Drexel Burnham Lambert Group, Inc., and Michael Milken.²⁴ The defendant, Integrated Resources, Inc., was a diversified financial services company that allegedly participated in fraudulent transactions Drexel and Milken orchestrated, resulting in millions of dollars of losses to its shareholders.²⁵ After complicated litigation, plaintiffs' lead counsel reached settlements with several defendants totaling approximately \$54.1 million.²⁶ Because fees come out of the common fund created for the benefit of the class,²⁷ and because lawyers are invariably paid on a contingency basis, fee determinations put the class and the lawyers in a potentially adversarial relationship. For this reason, the Federal Rules of Civil Procedure require courts to approve any fee award to counsel.²⁸

The plaintiffs' law firm in the case requested an attorneys' fee award equal to 25% of this amount, a bit higher than fees in similarly sized cases,²⁹ but not an unusual figure for a securities class action at that time. The district court judge referred the matter to a special master who concluded that the proposed fee was reasonable as a percent of the recovery and recommended that the request be granted in full. The court, however, returned the issue to the special master with directions that the fee be recalculated according to what is known as the "lodestar approach."³⁰ Applying this methodology, the special master reviewed counsel's records, cited numerous instances in which those records were incomplete or conflicting, rejected charges it found to be excessive, and recommended a substantially

24. For a general overview, see JAMES B. STEWART, *DEN OF THIEVES* (1991).

25. *Goldberger*, 209 F.3d at 45.

26. *Id.* at 45–46.

27. *Boeing Co. v. Van Gemert*, 444 U.S. 472, 478 (1980).

28. FED. R. CIV. P. 23.

29. *Goldberger*, 209 F.3d at 53.

30. The lodestar method requires the court to calculate the product of counsel's reasonable hours and reasonable hourly rate and then to adjust this figure (the "lodestar") to account for other factors, including (in non-fee-shifting cases) counsel's perceived contingency risk. The adjustment factor is commonly referred to as a "multiplier" because it represents a multiple (sometimes a fractional multiple) of the lodestar fee. *See id.*

lower award. Ultimately, class counsel received \$2,150,030, equal to just 3.97% of the total recovery and only about 16% of what the law firm had requested.

Counsel appealed the fee award to the Second Circuit, which affirmed the district court's order. Reaching far beyond the analysis needed to resolve the case before it, the appeals court issued a sweeping opinion that seemed to rewrite the rulebook on attorneys' fees in the circuit. The court started at first principles. Class members are uninformed and disorganized, and thus are unable either to negotiate fees with counsel on the basis of equal bargaining power or effectively to resist exorbitant fee demands at settlement.³¹ Accordingly, they are vulnerable to having an excessive share of their recoveries expropriated by counsel. These structural problems require the court to act as "guardian" or "fiduciary" of the class with respect to fees.³²

In the exercise of this "jealous regard" for class interests,³³ the trial court's "overarching" goal³⁴ should be "moderation."³⁵ The court used the term "moderation" three times—once even in italics. Given the court's approval in this case of a less than 4% fee, the term "moderation" seemed to be a clear signal that fees should generally be lower than those that district courts had been granting. This interpretation seems particularly strong given the concerns the court raised regarding the issue of contingency risk. The court confessed to a "nagging suspicion" that attorneys are "routinely overcompensated" for this factor in securities class actions.³⁶ Because most such cases settle, counsel has a high probability of obtaining a fee.³⁷ Accordingly, while courts are not precluded from taking contingency risk into account in awarding fees, the *Goldberger* court declared that

31. *Id.* at 52.

32. *Id.*

33. *Id.* at 53.

34. *Id.*

35. *Id.* at 52–53.

36. *Id.* at 57.

37. *Id.* at 52 (citing Janet Cooper Alexander, *Do the Merits Matter? A Study of Settlements in Securities Class Actions*, 43 STAN. L. REV. 497, 578 (1991)).

it is not appropriate to start from the assumption that such risk is present.³⁸

The court's directive to moderate fees was not merely hortatory. The Second Circuit warned that judges who fail to limit fees—who instead passively grant exorbitant fee requests from counsel—face significant prospects of reversal on appeal: “we have not hesitated to reverse where we felt an improper appraisal of these factors led to overcompensation.”³⁹ On the other hand, judges who erred by slashing fees faced little risk of reversal: “this Court has never found that a district court abused its discretion by awarding in a common fund case a fee that counsel assailed as too stingy.”⁴⁰

It is not clear how effective this warning would be in convincing district judges to reduce fees. To see this, think about the economics of the typical securities class action. Since the determination of the award is simply about the allocation of the settlement between the class and the attorney, defendants can be expected to be indifferent to the outcome of the fee question and therefore extremely unlikely to appeal any such award (indeed, it is far from clear that they would have standing to appeal given their lack of a concrete stake in the question).⁴¹ To the extent that there is any adversarial testing of the fee award, it typically comes from members of the class who object to the requested fee. Objectors, however, are relatively rare in securities class actions.⁴² Rarer still are appeals from class members

38. *Goldberger*, 209 F.3d at 52.

39. *Id.* at 53.

40. *Id.*

41. See REPORT OF THE THIRD CIRCUIT TASK FORCE, COURT AWARDED ATTORNEY FEES, 108 F.R.D. 237, 266 (1985) (“Since the defendant is interested only in the total size of its liability, so long as the settlement is accepted, it often will be indifferent as to the division of the fund between plaintiffs’ recovery and the attorneys’ fees.”).

42. See Elliott J. Weiss & John S. Beckerman, *Let the Money Do the Monitoring: How Institutional Investors Can Reduce Agency Costs in Securities Class Actions*, 104 YALE L.J. 2053, 2066 (1995). Eisenberg and Miller’s empirical study of all reported class action settlements over a ten-year period found that the median number of objectors in securities cases was zero. Theodore Eisenberg & Geoffrey Miller, *The Role of Opt-Outs and Objectors in Class Action Litigation: Theoretical and Empirical Issues*, 57 VAND. L. REV. 1529, 1550 (2004). In an empirical study of class actions in four district courts, researchers at the Federal Judicial Center found that in 42% to 64% of the cases analyzed there were no objections to settlements. THOMAS E. WILLGING, LAURAL L. HOOPER & ROBERT J. NIEMIC, EMPIRICAL STUDY OF CLASS ACTIONS IN FOUR DISTRICT COURTS: FINAL REPORT TO THE ADVISORY COMMITTEE ON CIVIL RULES 57 (1996).

because, here too, they would have to bear the costs of appeal but would in most instances only reap a small benefit from a reduction in fees.⁴³

In the typical case, the most likely appellant would seem to be, as in *Goldberger*, the disappointed law firm that received a lower fee than it asked for. A district court bent on avoiding reversal of its fee awards has a simple strategy to avoid appeals and thus the possibility of reversal: give the law firm what it wants. These dynamics would only seem to change as the size of the settlement at issue increases. With larger settlements, any percentage decrease in the fee awarded represents a much more substantial amount of money for the class. For very large cases, the prospect of a fee reduction may be sufficiently attractive that it substantially increases the likelihood that a class member will be willing to undertake the costs and risks of appeal. This increased likelihood of appeal should make the court's warnings in *Goldberger* about the risk of reversal more potent to both the lawyers asking for a fee and the court deciding what that fee should be.

The language of *Goldberger* and the dynamics of securities class actions thus provide a useful setting for testing how closely lower courts comply with the wishes of superior courts, whether individuals outside the judiciary modify their behavior in light of such pronouncements, and how much if any role risk of reversal plays in such compliance. If courts or attorneys conformed their behavior to the policies the Court of Appeals for the Second Circuit dictated even when the possibility of reversal was low, then, all else being equal, we should see both fee awards and requests that are consistently lower across cases. In other words, *Goldberger* should be negatively correlated with both fee awards and fee requests. If, by contrast, the impact of *Goldberger* on awards or requests varies with settlement size, then this would suggest that compliance is linked to the probability of appeal and possibility of reversal.

We can also test lower court compliance with *Goldberger* by examining whether *Goldberger* is correlated with the scrutiny with which the district judge reviews the fee request, here measured by the

43. WILLGING ET AL., *supra* note 42, at 81–82.

ratio of the fee awarded to the fee requested. We should observe the same pattern. If compliance is unrelated to fear of reversal, then *Goldberger* should be negative and significant. A finding that the ratio varies with settlement size would be consistent with the hypothesis that fear of reversal plays a role in compliance.

II. THE DATA

Analysis of these hypotheses began with a dataset of 717 settlements in federal securities class actions filed from 1984 through 2005 and settled from 1991 through 2007. Settlements were identified using Institutional Shareholder Service's Securities Class Action Services' database ("ISS"), a preexisting database of securities class action settlements, and from two newsletters, *Securities Class Action Alert* and *Class Action Reports*, that provide information on legal decisions and settlements in class action lawsuits. Collectively, these sources appear to provide comprehensive coverage of securities class action settlements.

For these cases, data were collected on variables that past studies have shown are correlated with fee amounts⁴⁴ or that have been studied in the judicial politics literature on compliance. These data fall into two broad categories: (1) data on case settlement characteristics, and (2) data on case characteristics.

Settlement characteristics were coded using data from ISS, the Stanford Securities Class Action Clearinghouse ("SSCAC"), *Securities Class Action Alert*, *Class Action Reports*, and the federal courts' Case Management/Electronic Case Filing ("CM/ECF") and PACER systems. For each case, data were collected on the size of the settlement, the attorneys' fee request (*Fee Request*), and the district court's fee award (*Fee*). These were measured both as a percentage of the settlement and in inflation-adjusted 2005 dollars. Cases were coded "1" if they were decided in the Second Circuit after the *Goldberger* decision and "0" otherwise (*Goldberger*).

Data on case characteristics came from a variety of sources, including published judicial decisions, settlement notices, media articles, SSCAC, and docket sheets and court filings available

44. These studies are discussed in more detail in Perino, *supra* note 21.

through the CM/ECF and PACER systems. We coded the number of docket entries in the case (*Docket Entries*) and the age of the case (in years) from first filing until settlement (*Age*), both of which serve as proxies for case complexity and litigation effort. Data were also collected on the presence of an SEC or other governmental action (*Government Action*) involving the same allegations at issue in the securities class action, which may serve as a proxy for case quality. A *Government Action* may also suggest that less litigation effort was necessary to achieve the settlement, possibly resulting in lower fees.

Past research has shown that the presence of a public pension fund as a lead plaintiff (*Public Pension*) is correlated with lower fees and fee requests.⁴⁵ Research has also shown that cases involving a particular firm, Milberg Weiss (*Milberg Weiss*), had significantly higher fee requests and fee awards. High profile class actions may result in lower fee awards, all else being equal, because they may involve relatively obvious cases of fraud that require less litigation effort. The Article therefore defines an indicator variable (*High Profile*) that takes a value of “1” if the case is in the top quartile of estimated damages in the sample,⁴⁶ contains an allegation of accounting fraud, and involves a parallel government action.

A few courts have experimented with auctioning off the role of lead counsel (*Auction*).⁴⁷ Although such auctions are rare and have been subject to both academic⁴⁸ and judicial⁴⁹ criticism, research suggests that they are correlated with lower fee requests and fee

45. Perino, *supra* note 21, at 12–13.

46. As a proxy for estimated damages, the Article uses the Maximum Dollar Loss (“MDL”). MDL is the maximum dollar loss during the class period alleged in the complaint and is defined as the dollar value decrease (in constant 2005 dollars) of the defendant issuer’s market capitalization from its peak market capitalization during the class period to the first trading day after the end of the class period. Past studies have shown that MDL is highly correlated with potential damages. Mukesh Bajaj, Sumon C. Mazumdar & Atulya Sarin, *Securities Class Action Settlements*, 43 SANTA CLARA L. REV. 1001, 1014 (2002–2003).

47. An early discussion, and qualified endorsement, of the auction idea is found in Jonathan R. Macey & Geoffrey P. Miller, *The Plaintiffs’ Attorney’s Role in Class Action and Derivative Litigation: Economic Analysis and Recommendations for Reform*, 58 U. CHI. L. REV. 1 (1991).

48. See, e.g., Jill E. Fisch, *Lawyers on the Auction Block: Evaluating the Selection of Class Counsel by Auction*, 102 COLUM. L. REV. 650, 727–28 (2002).

49. REPORT OF THE THIRD CIRCUIT TASK FORCE, SELECTION OF CLASS COUNSEL, 208 F.R.D. 340, 372–85 (2002).

awards.⁵⁰ Prior research has also shown that judicial experience (*Experience*) with securities class actions (measured here by the proportion of securities class actions filed in a district over a five-year study period) is negatively correlated with fee awards.⁵¹

To control for inter-circuit variation in the treatment of fees, the regressions include indicator variables for circuits, with the Second Circuit as the reference category. To control for potential changes in fee awards over time, indicator variables were created for each year in which a settlement was approved and fees were awarded. The regressions use 1991, the earliest settlement year in the dataset, as the reference category.

Descriptive statistics for the dataset appear in Table 1 below.

III. EMPIRICAL ANALYSIS

A. Are Attorneys Fees Lower After *Goldberger*?

We begin with a simple comparison of fees awarded in cases in the Second Circuit in which *Goldberger* was a controlling precedent and those in which it was not. As shown in Figure 1, mean and median fees in the non-*Goldberger* cases are 28.35% and 30%, respectively (measured as a percentage of the settlement in the case). These figures are significantly higher than those in the cases in which *Goldberger* was a controlling precedent (26.03% and 27.25%).⁵² While this finding suggests some compliance with *Goldberger*, it appears that fees were generally declining during this same time period. Mean (median) fees in the cases decided across all circuits in the years prior to 2000 (the year *Goldberger* was decided) were 27.63% (30%), significantly higher than the fees in the cases decided in the later part of the sample period, 26.06% (26.94%).⁵³ Indeed,

50. Perino, *supra* note 21, at 29.

51. Michael A. Perino, *Markets and Monitors: The Impact of Competition and Experience on Attorneys' Fees in Securities Class Actions* (St. John's Legal Studies, Paper No. 06-0034, 2006), available at Jan. 2006, http://papers.ssrn.com/sol3/papers.cfm?abstract_id=870577.

52. In a t test of the means, the t statistic was 2.332 (probability = 0.021). A Mann-Whitney rank-sum test comparing the medians yielded a z statistic of 2.777 (probability = 0.006).

53. These differences are statistically significant. In a t test of the means, the t statistic was 3.0407 (probability = 0.0025). A Mann-Whitney rank-sum test comparing the medians

there is no significant difference between post-*Goldberger* fees in the Second Circuit and post-*Goldberger* fees elsewhere.⁵⁴ So it may be that the lower fees in the Second Circuit following *Goldberger* were due to a secular decline in fees and not to compliance with *Goldberger*.⁵⁵ Even if that were not the case, if we define compliance with *Goldberger* to require the kind of dramatically lower fees that were approved in that case (4%), it is hard to say that lower courts did in fact comply with an interpretation of the *Goldberger* decision as mandating lower fees across the board.

Is there any evidence that the size of fee awards is linked to the likelihood of appeal? Figure 2A contains a scatter plot of the log-transformed size of the settlement and the log-transformed size of the fee award (both of which are measured in constant 2005 dollars). Figure 2A suggests that the relationship between settlement size and fee awards is not constant in the *Goldberger* versus non-*Goldberger* cases. We can clearly see this in the two prediction lines in the figure. As settlement size increases, the fee awards in the *Goldberger* cases appear to grow at a slower rate than fee awards in other cases. It is possible that the few very large settlements in the database are unduly influencing the slope of this line, so Figure 2B excludes settlements in excess of twenty on the natural log scale. Although the differential relationship between settlement and fee awards in the two sets of cases is less dramatic in Figure 2B, it remains. These data suggest the possibility that judges in response to *Goldberger* reduce their fee awards, but only in the largest cases, perhaps because they expected those cases to be subject to greater appellate scrutiny and thus faced a higher risk of reversal.

Of course, other variables might be driving that result as well. For example, if plaintiffs' attorneys were concerned about the higher risks of reversal that *Goldberger* may have created, perhaps they moderated their fee requests in the largest cases. In other words, perhaps the apparent interaction between awards and settlement size

yielded a z statistic of 2.874 (probability = 0.0041).

54. Mean fees were 26.07% in the Second Circuit and 26.17% in other circuits ($t = 0.156$, probability = 0.876).

55. Because fees decline with settlement size, *see* Eisenberg & Miller, *supra* note 21, the observed decline in percentage fees might also be attributable to the recent increase in average settlement size.

is a function not of the judges complying with *Goldberger*, but of attorneys complying with *Goldberger*. To see if this might be the case, Figures 3A and 3B recreate the scatter plots using logged fee requests instead of logged fee awards. The same interaction exists here, suggesting that attorneys may have modified their behavior in light of *Goldberger*.

To fully test the relationship between *Goldberger* and fee awards and fee requests, we ran linear regressions, with either the log-transformed fee award or fee request (in constant 2005 dollars) as the dependent variables. The explanatory variables are settlement size and the other previously identified variables that are correlated with fee awards and requests. The regressions include year- and circuit-fixed effects. Standard errors are clustered by circuit.

Models 1 through 3 are for fee awards. Model 1 contains an indicator variable for *Goldberger*, which is positive but insignificant. The hypothesized relationship was negative, so there seems to be little reason to believe that *Goldberger* generally led to lower fees. Model 2 includes an interaction (*Goldberger_Settle*) that is the product of *Goldberger* and *Settlement*. The coefficient of this variable is negative and statistically significant. All else being equal, in the *Goldberger* cases a 1% change in the settlement amount yields a 0.07% smaller increase in fee award than in the non-*Goldberger* cases. In other words, there is significant evidence that as settlements grow larger the fee awards in *Goldberger* cases grow at a slightly slower rate than the fee awards in the other cases. As a robustness check, Model 3 re-runs the regression on a sample that excludes log settlements of twenty or greater with nearly identical results.

While these findings suggest some kind of compliance with the dictates of *Goldberger*, two features are notable. First, *Goldberger* seemed to call for a dramatic reduction in the fees attorneys received—the case, after all, approved a less-than-4% fee. The small size of the coefficient for the interaction suggests that courts' fee awarding practices did not change nearly so much as a broad reading of *Goldberger* would contemplate. Second, a logical reading of *Goldberger* would seem to require those cuts to occur across the board. But rather than doing that, judges seem to award the lower fees primarily in the largest cases, which at least suggests the possibility that they were motivated to demonstrate compliance with

Goldberger primarily in the cases that had the greatest chance of appeal.

It remains unclear from these regressions, however, whether *Goldberger* led courts to change their fee awarding behavior. Models 4 through 6 thus use the same independent variables but substitute the log fee request as the dependent variable. Again we see in Model 4 that *Goldberger* is insignificant, suggesting no overall effect on fee requests. But, as with fee awards, the interaction term is negative and significant. In Model 5, all else being equal, in the *Goldberger* cases a 1% change in the settlement amount on average yields a 0.06% smaller increase in fee request than in the non-*Goldberger* cases. As this coefficient is nearly identical to the one reported in Model 2, it is possible that it was the attorneys who changed their behavior in response to *Goldberger*, not the courts. The interaction is again consistent with the hypothesis that attorneys reduced their fee requests primarily in the largest cases—those that represented the highest risk of appeal.

A somewhat different picture emerges from Model 6. Here, the interaction coefficient remains negative and significant, but is much smaller than for the equivalent model that tests fee awards (Model 3). These results are consistent with the hypothesis that both attorneys and courts changed their behavior in response to *Goldberger*. Attorneys reduced their fee requests as settlement size increased and courts cut these requests slightly more.

One final interesting result in Table 2 is with respect to the year-fixed effects. Starting in 1998, the coefficient for each year in the fee award regressions is negative and significant. All else being equal, fee awards in these years were notably lower than the reference year of 1991. It is impossible to attribute the drop in fee awards to *Goldberger*, which was not decided until 2000. A more plausible explanation is the impact of public pension funds, which became increasingly active as lead plaintiffs in securities class actions starting at around that time and whose presence has been shown to be correlated with lower fee awards.⁵⁶ Fee requests are significantly lower as well, although there the effect is not consistently significant

56. See Perino, *supra* note 21.

until 2000. While it is possible that the decline in fee requests could be linked to *Goldberger*, it seems equally if not more likely that it was the result of the overall downward trend in fee awards.

*B. Do Courts Scrutinize Fee Requests More Closely After
Goldberger?*

To assess changes in the level of scrutiny judges give attorneys' fee requests after *Goldberger*, we calculate the ratio of fee award to the fee request. Figure 4 shows the distribution of these ratios in the *Goldberger* and non-*Goldberger* cases. It shows that overall judges take a light touch when it comes to reviewing fee requests. In the non-*Goldberger* cases, attorneys received a mean fee equaling 91.8% of their request. In the median case, the attorneys received precisely what they requested. By contrast, in the *Goldberger* cases the mean ratio was 85.7%, with a median of 90%. These differences in means and medians are statistically significant,⁵⁷ suggesting that judges did comply with *Goldberger* by more vigorously reducing attorneys' fee requests. Here too, however, the degree of change is smaller than what the appellate judges in *Goldberger* seemed to contemplate.

We look also at the mean and median ratios just in the Second Circuit before and after *Goldberger*. As shown in Figure 5, there is a slight decline in means, from 87.64% to 85.54%, although this difference is statistically insignificant. There is a steeper drop in medians, from 99.01% to 90.01%, although this difference is only significant at the 10% level.

To better test the relationship between *Goldberger* and the ratio of award to request, we ran linear regressions, with the ratio of award to request as the dependent variable. The explanatory variables are settlement size and the other previously identified variables that are correlated with fee awards and requests. The regressions include year- and circuit-fixed effects. Standard errors are again clustered by circuit.

57. In the t test used to compare means, the t statistic was 4.1763 (probability < 0.0001). The Mann-Whitney rank-sum test was used to compare the medians. The test yielded a z statistic of 3.460 (probability = 0.0005).

The results of these regressions are reported in Table 3. Models 1 through 3 are for the full sample of ratios. Model 1 includes *Goldberger* without any interaction. The coefficient is negative but insignificant so there is no evidence that judges subject to *Goldberger* were generally more rigorous in their review of attorneys' fee requests. Model 2 includes an interaction term to test again whether judges reduce fees more in the cases that are more likely to be appealed. Although the interaction term is negative, it too is insignificant. Model 3 excludes from the analysis logged settlements in excess of twenty.⁵⁸ Here, the interaction term is negative and significant, which is consistent with the hypothesis that judges subject to *Goldberger* increase the scrutiny they give fee requests in those cases in which the likelihood of appeal is greater.

As we saw with fee awards, there is some evidence that courts' scrutiny of fee requests became more vigorous over time. In Models 1 through 3 the year-fixed effects are generally negative and many are significant, although not nearly so consistently as in Table 2. This finding suggests greater judicial scrutiny of fees over the reference time period of 1991. But again, this increased scrutiny can hardly be the product of the decision in *Goldberger* because it begins before the decision, not after it.

Next, we wanted to focus on the cases in which the court actually reduced the fee request, i.e. those cases in which the ratio was less than one. Perhaps among the courts willing to reduce fee requests, those subject to *Goldberger* gave even greater scrutiny to the proposed fees. Models 4 through 6 run the regressions on this subsample. The results are largely consistent with the results for the full sample. Both *Goldberger* and the interaction term are insignificant in the first two models, but are significant in Model 6, which excludes logged settlements in excess of twenty. So this too provides additional support for the hypothesis that *Goldberger's* impact was not universal but varied with the likelihood of appeal. The year-fixed effects are negative and become consistently significant starting in 1996, well before the decision in *Goldberger*.

58. This is consistent with the analysis of fee awards and fee requests. As an alternative specification, the model was run excluding just six outlying settlements, those in excess of \$1 billion. The results are consistent with those reported in Model 3.

Taken together, these data provide some support for the hypothesis that courts in the Second Circuit complied with the admonition in *Goldberger* that they scrutinize fee requests more vigorously. Again, however, the increase in scrutiny is much smaller than what the Second Circuit may have hoped. Compliance seems to increase as the likelihood of appeal increases, suggesting that compliance is linked to risk of reversal.

IV. DISCUSSION

The results reported here are consistent with the hypothesis that judges' compliance is tied to fear of reversal. Of course, other explanations for the pattern we observe in the data are possible. For example, *Goldberger* was decided in 2000, shortly after large institutional investors started to become increasingly active as lead plaintiffs in these cases. Large institutions, particularly public pension funds, appear to engage in negotiations over fees, and prior research has shown that their participation in class actions is correlated with lower fee requests and awards.⁵⁹ While we control for the presence of such plaintiffs in the case, it is possible that institutional investor activism began to influence fee requests and awards in other cases as well. Indeed, we noted earlier that their activism might explain the general decline in fees we observe since 1998. As institutions have been most active in larger cases, perhaps their influence is felt most strongly there, explaining why requests, fees, and scrutiny vary with case size.

Another potential explanation for the pattern we observe is that judges may have read *Goldberger* more narrowly than we do. *Goldberger* was a large securities class action settlement of over \$50 million. Many of the Second Circuit's pronouncements about over-rewarding attorneys for contingency risk were focused on these mega cases. Perhaps judges took this language to mean that they could focus their efforts on the large cases, thereby explaining the pattern we observe. In other words, maybe *Goldberger* stands for the proposition that judges should rigorously scrutinize fees in big cases, exactly as they ended up doing.

59. Perino, *supra* note 21.

Our data are also consistent with an interpretation of *Goldberger*, not as mandating any particular behavior with respect to fees, but rather as instructing the district courts to exercise greater scrutiny and more deliberation. This interpretation is consistent with *Goldberger*'s rejection of "benchmark" approaches to fee-setting. In this view, *Goldberger* was a command invitation to trial courts to engage in a dialogue as to the proper measure of fees in securities class action cases. The district courts were, in effect, instructed to reconsider their prior practices and to report back (through appeals) on the results. Preliminary results of that process of reconsideration are now in. The consensus of the trial court judges appears to be that the preexisting practice was generally appropriate, even when viewed in light of the considerations set forth in *Goldberger*, but that fees in larger cases required some (albeit relatively slight) downward adjustment. This view of the data suggests that a simple principal-agent model may not always capture the full details of the relationship between trial and appellate courts: perhaps appellate courts sometimes behave more like senior partners in law firms than military commanders issuing orders to the troops.

CONCLUSION

Appeals court rulings should always be of interest to trial courts, but the *Goldberger* decision was especially noteworthy. The court's sweeping analysis and broad generalizations, its approval of a fee which many in the plaintiff's bar undoubtedly considered shockingly low, its rhetoric (which contained strong disapproving overtones about class counsel's greed), and its repeated intimations that the fee demanded in the case was grossly excessive, all demanded attention. But how would the trial courts and the plaintiffs' attorneys respond?

Our study finds that fees have declined somewhat in recent years, but these declines appear to be unrelated to the Second Circuit's decision. Indeed, we find that *Goldberger* is not correlated with a general decline in fee awards and fee requests. Instead, there appears to be an interaction between *Goldberger* and settlement size. As settlement size increases both fee requests and fee awards rise at a slower rate in the *Goldberger* cases than in the non-*Goldberger* cases. In addition, we examine the ratio of the award to the request as

a measure of the scrutiny with which courts review fee requests, and we observe the same general pattern. In at least some of the models, the interaction term is again negative and significant, meaning that increases in settlement size are associated with judges reducing requests to a greater degree in *Goldberger* versus non-*Goldberger* cases. These findings are consistent with the hypothesis that compliance is tied to the probability of appeal and reversal.

These findings are subject to differing interpretations. If compliance with *Goldberger* required plaintiffs' lawyers to ask for drastically lower fees or district judges to slash fees dramatically, then it seems that neither attorneys nor courts complied in any meaningful sense. If, on the other hand, the case is interpreted in a more limited sense, as demanding that trial courts and class counsel carefully rethink prevailing practices, then the evidence we observe can be seen as consistent with the proposition that the relevant actors are complying with *Goldberger* by taking greater care in the matter of fees, even if the result of that process is largely consistent with prior practice.

APPENDIX[†]

TABLE 1: DESCRIPTIVE STATISTICS

	N	Mean	Median	Std. Dev.
<i>Goldberger</i>	717	0.215	0.000	0.411
<i>Settlement</i>	717	49.079	7.029	271.143
<i>Fee Request (%)</i>	702	0.295	0.300	0.056
<i>Fee (%)</i>	674	0.266	0.280	0.067
<i>Gov't Action</i>	717	0.351	0.000	0.478
<i>Docket Entries</i>	716	190.473	111.500	334.152
<i>Case Age</i>	717	2.940	2.597	1.624
<i>Experience</i>	717	0.060	0.049	0.041
<i>Public Pension</i>	717	0.109	0.000	0.312
<i>High Profile</i>	717	0.082	0.000	0.275
<i>Auction</i>	717	0.018	0.000	0.134
<i>Milberg</i>	717	0.490	0.000	0.500
<i>1st Circuit</i>	717	0.056	0.000	0.230
<i>2nd Circuit</i>	717	0.346	0.000	0.476
<i>3rd Circuit</i>	717	0.064	0.000	0.245
<i>4th Circuit</i>	717	0.025	0.000	0.157
<i>5th Circuit</i>	717	0.052	0.000	0.221
<i>6th Circuit</i>	717	0.046	0.000	0.210
<i>7th Circuit</i>	717	0.038	0.000	0.190
<i>8th Circuit</i>	717	0.022	0.000	0.148
<i>9th Circuit</i>	717	0.264	0.000	0.441
<i>10th Circuit</i>	717	0.024	0.000	0.152
<i>11th Circuit</i>	717	0.063	0.000	0.243
<i>D.C. Circuit</i>	717	0.001	0.000	0.037
<i>Year 1991</i>	717	0.011	0.000	0.105
<i>Year 1992</i>	717	0.017	0.000	0.128
<i>Year 1993</i>	717	0.018	0.000	0.134
<i>Year 1994</i>	717	0.021	0.000	0.143
<i>Year 1995</i>	717	0.017	0.000	0.128
<i>Year 1996</i>	717	0.061	0.000	0.240
<i>Year 1997</i>	717	0.039	0.000	0.194
<i>Year 1998</i>	717	0.047	0.000	0.213
<i>Year 1999</i>	717	0.061	0.000	0.240
<i>Year 2000</i>	717	0.103	0.000	0.304
<i>Year 2001</i>	717	0.109	0.000	0.312
<i>Year 2002</i>	717	0.138	0.000	0.345
<i>Year 2003</i>	717	0.132	0.000	0.339
<i>Year 2004</i>	717	0.103	0.000	0.304
<i>Year 2005</i>	717	0.071	0.000	0.257
<i>Year 2006</i>	717	0.031	0.000	0.173
<i>Year 2007</i>	717	0.020	0.000	0.138

Note: *Settlement* is in millions of constant 2005 dollars. *Fee Request* and *Fee* are percentages of the relevant *Settlement*. *Class Period* and *Case Age* are in years.

Sources: Institutional Shareholder Services, *Securities Class Action Database*; Stanford Law School, *Securities Class Action Clearinghouse*; CRSP; COMPUSTAT; CM/ECF; PACER; *Securities Class Action Alert*; *Class Action Reports*.

[†] Appendix does not conform to THE BLUEBOOK: A UNIFORM SYSTEM OF CITATION (Columbia Law Review Ass'n et al. eds., 18th ed. 2005).

TABLE 2: REGRESSIONS FOR FEE AWARDS AND FEE REQUESTS

	Logged Fee Awards			Logged Fee Requests		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Goldberger</i>	0.027 (1.17)	1.111 (4.40)**	1.031 (4.36)**	0.056 (1.36)	0.930 (6.43)**	0.352 (2.26)*
<i>Milberg</i>	0.061 (2.61)*	0.056 (2.36)*	0.049 (2.24)*	0.059 (2.28)*	0.055 (2.18)	0.042 (1.82)
<i>Public Pension</i>	-0.173 (5.41)**	-0.181 (5.35)**	-0.170 (6.50)**	-0.248 (5.72)**	-0.255 (5.77)**	-0.227 (7.70)**
<i>Settlement</i>	0.936 (29.08)**	0.955 (37.10)**	0.970 (41.71)**	0.972 (80.46)**	0.988 (92.45)**	0.986 (83.97)**
<i>Gov't Action</i>	-0.001 (0.05)	-0.008 (0.44)	-0.003 (0.17)	-0.003 (0.22)	-0.007 (0.70)	-0.015 (1.20)
<i>High Profile</i>	-0.127 (2.18)	-0.126 (2.08)	-0.115 (1.83)	-0.100 (2.19)	-0.100 (2.08)	-0.058 (0.84)
<i>Auction</i>	-0.586 (6.19)**	-0.616 (6.21)**	-0.540 (6.26)**	-0.575 (6.57)**	-0.599 (7.85)**	-0.632 (6.65)**
<i>Docket Entries</i>	-0.040 (1.71)	-0.040 (1.75)	-0.019 (0.69)	-0.044 (2.74)*	-0.044 (2.82)*	-0.026 (1.46)
<i>Experience</i>	-0.482 (8.72)**	-0.395 (4.73)**	-0.370 (2.73)*	-0.473 (6.43)**	-0.396 (3.91)**	-0.270 (2.14)
<i>Age</i>	0.119 (4.83)**	0.114 (4.58)**	0.102 (7.05)**	0.071 (4.17)**	0.067 (4.01)**	0.055 (3.06)*
<i>1st Circuit</i>	0.133 (4.84)**	0.130 (4.96)**	0.060 (3.53)**	0.023 (0.52)	0.022 (0.49)	0.020 (0.45)
<i>3d Circuit</i>	0.152 (3.02)*	0.140 (2.94)*	0.108 (3.30)**	0.084 (2.61)*	0.076 (2.32)*	0.077 (2.77)*
<i>4th Circuit</i>	0.187 (6.55)**	0.174 (6.35)**	0.113 (4.37)**	0.075 (1.53)	0.065 (1.35)	0.062 (1.14)
<i>5th Circuit</i>	0.068 (2.11)	0.060 (2.03)	0.017 (0.89)	-0.033 (0.92)	-0.038 (1.05)	-0.016 (0.42)
<i>6th Circuit</i>	0.161 (4.27)**	0.151 (4.14)**	0.078 (2.91)*	0.071 (1.62)	0.065 (1.49)	0.061 (1.52)
<i>7th Circuit</i>	0.188 (5.92)**	0.183 (5.57)**	0.101 (6.13)**	0.076 (1.58)	0.073 (1.59)	0.070 (1.50)
<i>8th Circuit</i>	0.178 (4.55)**	0.168 (4.61)**	0.117 (4.86)**	0.031 (0.75)	0.024 (0.57)	0.038 (0.91)
<i>9th Circuit</i>	0.035 (1.33)	0.028 (1.17)	-0.036 (2.33)*	-0.025 (0.62)	-0.030 (0.73)	-0.032 (0.75)
<i>10th Circuit</i>	0.085 (5.20)**	0.090 (5.20)**	0.035 (1.61)	-0.089 (1.68)	-0.084 (1.69)	-0.082 (1.67)
<i>11th Circuit</i>	0.133 (4.92)**	0.134 (4.90)**	0.076 (3.90)**	0.041 (1.02)	0.044 (1.13)	0.044 (1.08)
<i>DC Circuit</i>	0.246 (4.84)**	0.230 (4.41)**	0.116 (1.64)	0.167 (2.06)	0.156 (1.82)	0.129 (1.33)
<i>Year_1992</i>	-0.111 (2.42)*	-0.110 (2.65)*	-0.116 (3.11)**	-0.124 (3.68)**	-0.120 (3.85)**	-0.117 (3.92)**
<i>Year_1993</i>	-0.286 (1.51)	-0.289 (1.47)	-0.125 (1.19)	-0.056 (1.07)	-0.058 (1.34)	-0.068 (2.00)
<i>Year_1994</i>	-0.151 (1.81)	-0.135 (1.64)	-0.121 (1.48)	-0.059 (1.16)	-0.050 (1.05)	-0.045 (0.90)
<i>Year_1995</i>	-0.040 (0.95)	-0.030 (0.65)	-0.031 (0.68)	-0.049 (1.10)	-0.041 (0.88)	-0.033 (0.63)
<i>Year_1996</i>	-0.117 (2.23)*	-0.113 (2.27)*	-0.119 (3.37)**	-0.025 (2.13)	-0.021 (1.50)	-0.014 (0.75)

	Logged Fee Awards			Logged Fee Requests		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Year_1997</i>	-0.178 (2.01)	-0.176 (2.08)	-0.197 (2.72)*	-0.014 (0.94)	-0.013 (1.01)	-0.009 (0.69)
<i>Year_1998</i>	-0.116 (5.01)**	-0.116 (7.05)**	-0.131 (8.02)**	-0.052 (1.53)	-0.052 (1.87)	-0.045 (1.76)
<i>Year_1999</i>	-0.153 (2.96)*	-0.158 (3.62)**	-0.171 (5.15)**	-0.046 (1.25)	-0.050 (1.57)	-0.051 (1.45)
<i>Year_2000</i>	-0.200 (4.13)**	-0.195 (4.21)**	-0.181 (3.76)**	-0.115 (2.34)*	-0.111 (2.34)*	-0.108 (2.76)*
<i>Year_2001</i>	-0.202 (4.17)**	-0.197 (4.45)**	-0.179 (3.68)**	-0.109 (2.21)*	-0.104 (2.27)*	-0.101 (2.54)*
<i>Year_2002</i>	-0.203 (7.84)**	-0.200 (8.38)**	-0.175 (7.78)**	-0.129 (2.65)*	-0.127 (2.81)*	-0.119 (3.10)*
<i>Year_2003</i>	-0.201 (6.10)**	-0.200 (6.68)**	-0.171 (6.09)**	-0.126 (2.42)*	-0.127 (2.62)*	-0.123 (3.12)**
<i>Year_2004</i>	-0.216 (4.04)**	-0.206 (4.04)**	-0.204 (4.01)**	-0.161 (2.48)*	-0.152 (2.42)*	-0.163 (3.10)*
<i>Year_2005</i>	-0.273 (9.48)**	-0.267 (9.81)**	-0.265 (6.70)**	-0.215 (5.89)**	-0.208 (5.93)**	-0.214 (6.17)**
<i>Year_2006</i>	-0.431 (6.39)**	-0.413 (5.90)**	-0.353 (5.20)**	-0.320 (2.93)*	-0.306 (2.74)*	-0.245 (2.53)*
<i>Year_2007</i>	-0.396 (3.89)**	-0.392 (4.22)**	-0.408 (4.32)**	-0.345 (2.79)*	-0.343 (2.97)*	-0.382 (3.85)**
<i>Goldberger_Settle</i>		-0.069 (4.06)**	-0.067 (4.43)**		-0.056 (7.07)**	-0.018 (2.21)*
<i>Constant</i>	-0.094 (0.26)	-0.396 (1.51)	-0.682 (3.42)**	-0.516 (3.26)**	-0.761 (5.67)**	-0.806 (5.78)**
Observations	686	686	674	702	702	690
R-squared	0.96	0.96	0.96	0.98	0.99	0.99

Robust t statistics in parentheses.

* significant at 5%; ** significant at 1%

Note: All models cluster standard errors by circuit with t statistics reported in parentheses. Models 1-3 use the log-transformed fee request measured in constant 2005 dollars as the dependent variable. Models 4-6 use the fee request as a proportion of the settlement as the dependent variable. As a robustness check, Models 3 and 6 omit logged settlements in excess of 20. *Settlement*, *Docket Entries*, and *Age* are log-transformed. *Settlement* is in constant 2005 dollars.

Sources: Institutional Shareholder Services, *Securities Class Action Database*; Stanford Law School, *Securities Class Action Clearinghouse*; CRSP; COMPUSTAT; CM/ECF; PACER; *Securities Class Action Alert*; *Class Action Reports*.

TABLE 3: REGRESSIONS FOR RATIO OF AWARD TO REQUEST

	Full Sample			Ratios < 1		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Goldberger</i>	-0.035 (1.04)	0.113 (1.13)	0.406 (5.30)**	-0.034 (1.74)	0.085 (0.51)	0.416 (3.20)**
<i>Milberg</i>	0.001 (0.19)	0.001 (0.08)	0.003 (0.45)	0.007 (0.64)	0.006 (0.57)	0.008 (0.97)
<i>Public Pension</i>	0.059 (2.95)*	0.057 (2.79)*	0.048 (3.20)**	0.090 (2.21)*	0.090 (2.17)†	0.075 (2.51)*
<i>Settlement</i>	-0.021 (1.78)	-0.018 (1.51)	-0.009 (1.16)	-0.027 (1.99)†	-0.024 (1.57)	-0.012 (1.30)
<i>Gov't Action</i>	-0.011 (1.09)	-0.012 (1.18)	-0.005 (0.44)	-0.015 (0.81)	-0.015 (0.83)	-0.001 (0.04)
<i>High Profile</i>	-0.015 (1.07)	-0.015 (1.07)	-0.033 (2.28)*	0.012 (0.62)	0.012 (0.61)	-0.014 (0.54)
<i>Auction</i>	0.023 (0.40)	0.019 (0.33)	0.077 (6.01)**	-0.086 (0.75)	-0.092 (0.81)	0.093 (1.48)
<i>Docket Entries</i>	0.002 (0.13)	0.002 (0.14)	0.003 (0.24)	0.009 (0.49)	0.008 (0.45)	0.010 (0.95)
<i>Experience</i>	0.015 (0.59)	0.025 (0.96)	-0.050 (1.11)	0.144 (0.31)	0.152 (0.33)	0.032 (0.08)
<i>Age</i>	0.031 (2.31)*	0.030 (2.22)*	0.029 (4.36)**	0.021 (1.31)	0.021 (1.30)	0.018 (2.30)*
<i>1st Circuit</i>	0.062 (1.77)	0.061 (1.73)	0.033 (1.03)	0.025 (1.44)	0.026 (1.54)	-0.029 (1.15)
<i>3^d Circuit</i>	0.035 (0.81)	0.033 (0.76)	0.020 (0.55)	0.075 (3.03)*	0.072 (2.71)*	0.040 (2.55)*
<i>4th Circuit</i>	0.063 (1.66)	0.061 (1.56)	0.039 (1.08)	0.012 (0.38)	0.010 (0.32)	-0.050 (1.04)
<i>5th Circuit</i>	0.054 (1.59)	0.052 (1.53)	0.023 (0.73)	0.060 (4.22)**	0.059 (4.14)**	0.012 (0.58)
<i>6th Circuit</i>	0.049 (1.16)	0.048 (1.10)	0.020 (0.51)	0.041 (2.84)*	0.040 (2.83)*	-0.013 (0.37)
<i>7th Circuit</i>	0.055 (1.43)	0.054 (1.38)	0.019 (0.57)	0.103 (4.63)**	0.103 (4.64)**	0.044 (1.31)
<i>8th Circuit</i>	0.093 (2.43)*	0.091 (2.33)*	0.070 (2.04)	0.037 (1.25)	0.035 (1.23)	-0.031 (0.48)
<i>9th Circuit</i>	0.018 (0.58)	0.017 (0.54)	-0.008 (0.31)	0.013 (0.82)	0.012 (0.77)	-0.030 (4.80)**
<i>10th Circuit</i>	0.086 (2.67)*	0.086 (2.64)*	0.061 (1.96)†	0.077 (4.17)**	0.076 (4.43)**	0.031 (1.51)
<i>11th Circuit</i>	0.043 (1.24)	0.043 (1.23)	0.017 (0.54)	0.093 (5.19)**	0.093 (5.20)**	0.046 (1.95)†
<i>DC Circuit</i>	0.027 (0.51)	0.025 (0.46)	-0.013 (0.25)	0.112 (3.50)**	0.110 (3.40)**	0.040 (1.00)
<i>Year_1992</i>	-0.020 (0.70)	-0.019 (0.69)	-0.020 (0.65)	-0.174 (6.17)**	-0.171 (5.55)**	-0.163 (6.42)**
<i>Year_1993</i>	-0.096 (1.00)	-0.097 (1.00)	-0.035 (0.58)	-0.147 (0.91)	-0.148 (0.92)	-0.051 (0.49)
<i>Year_1994</i>	-0.050 (1.06)	-0.049 (1.02)	-0.047 (1.02)	-0.136 (1.36)	-0.133 (1.33)	-0.120 (1.33)
<i>Year_1995</i>	0.029 (2.32)*	0.030 (2.32)*	0.026 (3.14)**	-0.025 (1.40)	-0.023 (1.24)	-0.010 (0.50)
<i>Year_1996</i>	-0.075 (1.84)†	-0.075 (1.86)†	-0.080 (2.20)*	-0.095 (3.67)**	-0.094 (3.71)**	-0.100 (7.28)**
<i>Year_1997</i>	-0.131 (3.18)**	-0.130 (3.20)**	-0.139 (3.46)**	-0.247 (7.74)**	-0.246 (7.85)**	-0.260 (13.84)**
<i>Year_1998</i>	-0.044 (1.69)	-0.045 (1.63)	-0.055 (1.64)	-0.203 (13.45)**	-0.204 (13.66)**	-0.228 (6.40)**

	Full Sample			Ratios < 1		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Year_1999</i>	-0.074 (2.54)*	-0.075 (2.59)*	-0.079 (2.83)*	-0.169 (10.57)**	-0.170 (10.64)**	-0.177 (6.61)**
<i>Year_2000</i>	-0.062 (5.20)**	-0.061 (5.17)**	-0.057 (5.69)**	-0.133 (5.91)**	-0.131 (5.99)**	-0.119 (4.69)**
<i>Year_2001</i>	-0.057 (3.08)*	-0.056 (3.18)**	-0.051 (2.33)*	-0.143 (6.99)**	-0.142 (7.21)**	-0.134 (5.54)**
<i>Year_2002</i>	-0.041 (1.73)	-0.041 (1.66)	-0.034 (1.59)	-0.158 (4.01)**	-0.159 (4.05)**	-0.145 (4.38)**
<i>Year_2003</i>	-0.054 (2.06)†	-0.054 (2.05)†	-0.043 (1.95)†	-0.136 (7.28)**	-0.135 (7.19)**	-0.106 (5.43)**
<i>Year_2004</i>	-0.044 (2.05)†	-0.042 (1.91)†	-0.038 (1.89)†	-0.102 (4.44)**	-0.102 (4.38)**	-0.093 (3.39)**
<i>Year_2005</i>	-0.046 (3.96)**	-0.045 (3.63)**	-0.043 (5.31)**	-0.121 (3.61)**	-0.120 (3.63)**	-0.123 (3.20)**
<i>Year_2006</i>	-0.079 (1.89)†	-0.077 (1.80)†	-0.076 (2.07)†	-0.149 (3.79)**	-0.147 (3.70)**	-0.173 (4.41)**
<i>Year_2007</i>	-0.033 (1.24)	-0.033 (1.14)	-0.016 (0.69)	-0.097 (3.60)**	-0.096 (3.21)**	-0.043 (1.75)
<i>Goldberger_Settle</i>		-0.009 (1.26)	-0.030 (5.38)**		-0.008 (0.68)	-0.031 (4.09)**
<i>Constant</i>	1.229 (11.58)**	1.187 (10.57)**	1.062 (18.38)**	1.273 (7.06)**	1.235 (5.90)**	1.076 (7.04)**
Observations	674	674	662	335	335	326
R-squared	0.12	0.12	0.14	0.16	0.16	0.20

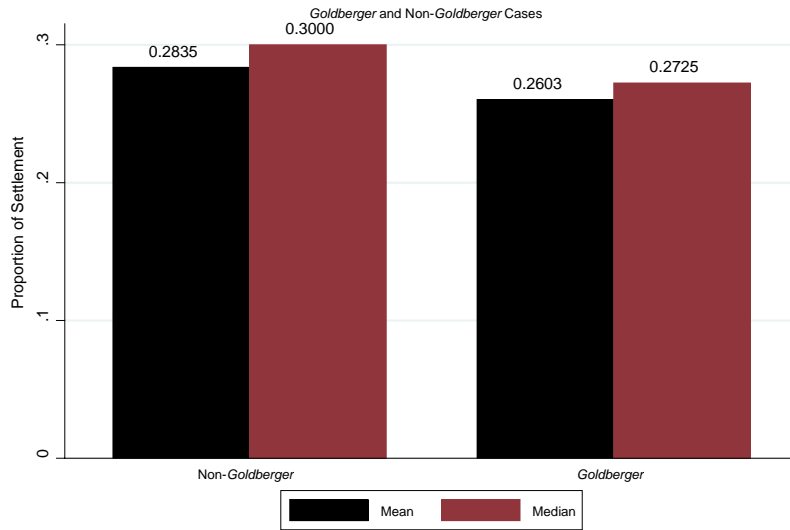
Robust t statistics in parentheses.

† significant at 10%; * significant at 5%; ** significant at 1%

Note: All models cluster standard errors by circuit with t statistics reported in parentheses. As a robustness check, Models 3 and 6 omit logged settlements in excess of 20. *Settlement*, *Docket Entries*, and *Age* are log-transformed. *Settlement* is in constant 2005 dollars.

Sources: Institutional Shareholder Services, *Securities Class Action Database*; Stanford Law School, *Securities Class Action Clearinghouse*; CRSP; COMPUSTAT; CM/ECF; PACER; *Securities Class Action Alert*; *Class Action Reports*.

Figure 1: Mean and Median Fee Awards (Second Circuit Only)



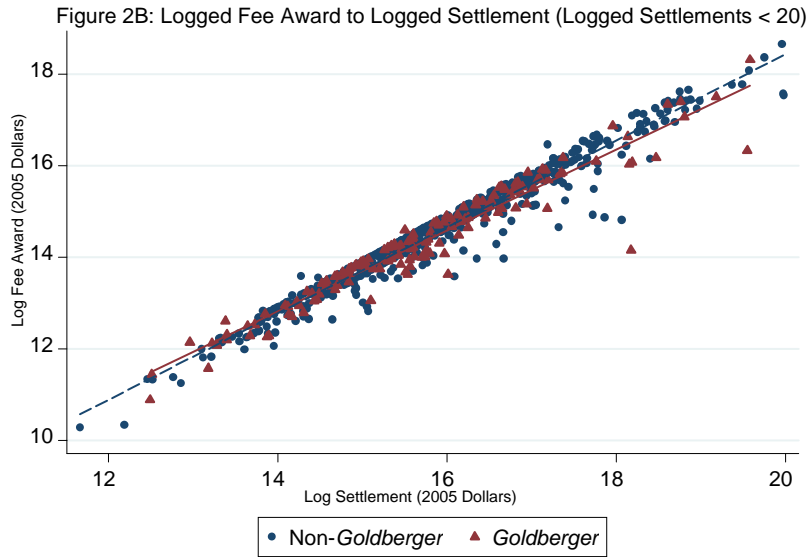
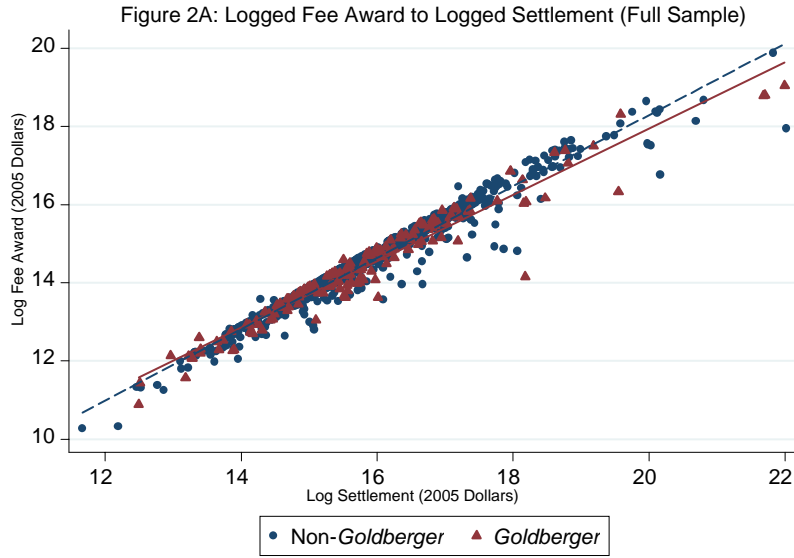


Figure 3A: Logged Fee Request to Logged Settlement (Full Sample)

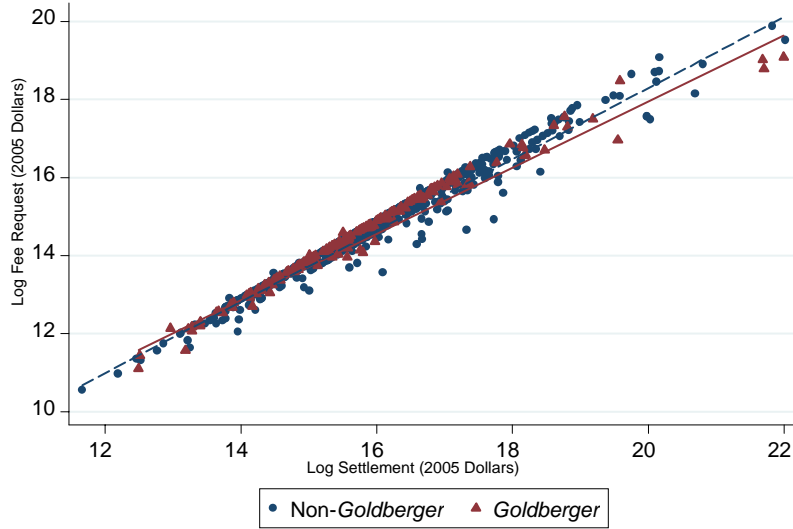


Figure 3B: Logged Fee Request to Logged Settlement (Logged Settlements < 20)

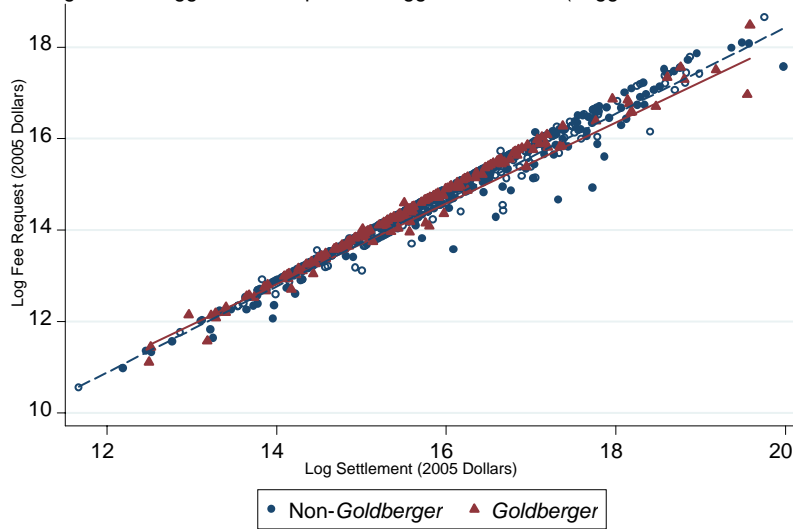


Figure 4: Mean and Median Ratio Award to Request

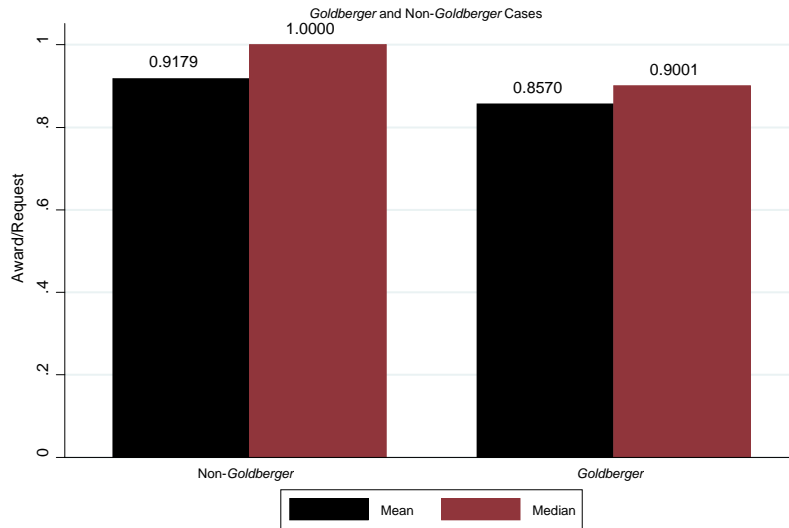


Figure 5: Mean and Median Ratio Award to Request (2d Circuit Only)

