New Evidence on the Frequency and Severity of Medical Malpractice Claims

Patricia M. Danzon
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New Evidence on the Frequency and Severity of Medical Malpractice Claims

Patricia M. Danzon

1986
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Foreword

The rules that govern injury compensation and liability insurance are receiving greater scrutiny today than ever before in American history. Each of the three previous bursts of legislative energy devoted to these issues—the enactment of Workers' Compensation laws, the later and much less uniform adoption of no-fault auto liability statutes, and the passage of medical malpractice reforms—was highly specialized in scope. Now legislative and executive authorities in most states and at the federal level are actively exploring, and sometimes enacting, changes in the law designed to achieve much broader alterations in the civil liability system.

This activity has greatly increased the demand for empirical evidence of the consequences of the current system and statistical predictions of the effects of proposed amendments in tort law. Lawmakers want to know as much as possible about what difference a proposed change is likely to make in the frequency of lawsuits, the compensation paid, and the costs of processing transactions. Judging by the number of inquiries that we receive at the Institute, the interest in such data is both widespread and intense.

The best evidence currently available for answering these questions derives from the period following the wave of revisions in medical malpractice liability law that occurred in many states during the mid-1970s. Indeed, most of the tort law changes that are now being considered would do little more than extend these medical malpractice revisions to the full range of tort cases. Although the specialized characteristics of medical cases must be recognized and accounted for, analysis of data indicating the effects of these measures should be extremely useful to policymakers currently considering general changes.

This report presents the results of such an analysis. It follows an earlier ICJ report of the outcomes of changes in medical malpractice law through 1978. Though that study was useful in indicating the early consequences of statutory changes, the data then available did not record enough experience after legislative actions to provide a solid indication of the long-term effects of those actions. This problem was
exacerbated by the particularly long “tail” (i.e., the time elapsed between the date of injury and the date of claim resolution) that is typical of medical malpractice cases and the fact that some statutory modifications of the tort law were held in abeyance pending the outcomes of legal challenges.

The present study analyzes a new closed claim database, covering the years 1975–84. It was compiled through the cooperation of insurers—both commercial and physician-owned—that covered about 100,000 physicians in 49 states during some or all of that period. Though differences among states and changes in the malpractice insurance market preclude perfect representation of the national scene, we believe this body of data enables development of the most comprehensive and accurate picture possible of the consequences to date of medical malpractice reforms.

This analysis is focused particularly on the effects of tort revisions and other factors on two key characteristics of malpractice claims: the frequency with which such claims are filed and their average severity, as measured in dollar terms. As a result, it is possible for the first time to provide quantitative answers to such questions as: What difference does it make if a ceiling is placed on tort awards; or if collateral sources of compensation are deducted from judgments; or if the statute of limitations is altered; or if binding arbitration is permitted? And, how much does it matter if lawyers proliferate relative to population, or if the jurisdiction is more or less urban? The work reported here takes these questions out of the purely rhetorical context in which they so often appear and subjects them to objective empirical analysis.

We make no claim that this report presents conclusive answers to these questions. But the analysis clearly represents an important step forward in increasing our knowledge of the civil justice system. Some of the findings will surprise readers; some may displease. But I believe all deserve the close attention of policymakers who are now involved in considering major changes in the tort liability rules in their jurisdictions.

Gustave H. Shubert
Director, The Institute for Civil Justice
Executive Summary

An earlier report analyzed the contribution of demographic, medical, and legal factors to trends in malpractice claims over the period 1970–1978.\(^1\) In order to update these early estimates, this report analyzes the effect of the tort reforms and other factors on trends in malpractice claim frequency and severity, using claim experience over the full decade 1975–1984. The length of time since the enactment of the 1975–1977 tort reforms should now, in principle, be long enough to estimate their long-run effects. However, in practice, there are several difficulties. First, the reforms have been subject to legal challenge in many states, and final rulings either upholding or denying their constitutionality have been long delayed. To the extent that the disposition of claims over the last decade has been influenced by uncertainty as to the ultimate outcome of these legal challenges, it may still be too early to estimate full long-run effects of those that have been upheld.

A second practical difficulty in estimating the effects of tort reforms is lack of a consistent, comprehensive database. The medical malpractice insurance market has undergone substantial changes since 1975 in the identity of carriers and types of coverages. Several major stock insurers have withdrawn, to be replaced by physician-owned mutuals and hospital "captives" and by joint underwriting associations (JUAs). A substantial fraction of the market has also changed from an "occurrence" policy (which covers all claims arising out of medical incidents occurring in the policy year regardless of the date of claim filing) to "claims-made" coverage (which covers only claims filed in the policy year regardless of the date of injury, provided that the physician was covered by a claims-made policy with that company at the time of injury). Although these changes in liability insurance markets are not expected to affect the behavior of patients and courts with regard to filing and disposing of claims, the large number of insurers who have had a significant market share for at least some fraction of the period under study makes collection of a

comprehensive database on claim experience very costly. The switch from occurrence to claims-made coverage further complicates the estimation of trends, particularly for claim frequency, because the number of claims reported tends to be low in the early years of claims-made coverage, rising as the policy "matures"—even with no change in the underlying frequency of claims filed.

Data for this study were requested from most of the insurers who had a significant market share at any time from 1975 through 1984. Several of the largest stock insurers and physician-owned companies, covering roughly 100,000 physicians, provided data in the format and detail requested. Forty-nine states are represented for at least some years. However, the database may not be fully representative of the situation nationwide. The units of observation for the analysis are not individual claims, but individual states in each year. The variables to be explained are claim frequency per 100 physicians and average severity per paid claim (including verdicts and out-of-court settlements), by state.

To estimate the effect of a particular tort reform, one cannot simply compare the experience of State X before and after enactment, because other factors may have changed over the same time span. Similarly, one cannot simply compare, at a particular point in time, states that have enacted a particular reform and states that have not, because other factors may contribute to any observed differences between states. For example, assume that State A, which enacted a cap on awards, had 20 percent higher claim severity in 1984 than State B in 1984. One should not infer that the cap had no effect because it may be that claim severity would have been 50 percent higher in A than in B had the cap not been enacted. Thus, to estimate the net effect of the cap—or any other factor—multivariate statistical methods are used to attempt to "control" for other factors.

Further, it is not possible to measure the effect of each variant of each reform—for example, a cap on pain and suffering at $250,000, at $100,000, and so on—because the number of observations on each variant is too small. For most reforms, the average effect for each type of change has therefore been estimated. Two exceptions are that separate estimates were made for the effects of mandatory and discretionary collateral source offset and for different types of legislation regarding screening panels. The results are to be interpreted as showing how much a particular reform affected experience, relative to what the experience would have been had the law not been enacted. For example, the statement that reform X reduced claim frequency by Y percent does not mean that claim frequency fell by Y percent but that it was Y percent lower than it would have been. Because experience in individual states in successive years is not fully independent, estimates of statistical significance are rough (see the appendix).
With these caveats, the evidence from this analysis suggests that the last round of tort reforms affected the frequency and severity of malpractice claims over the decade 1975–1984 in a manner broadly consistent with theory and previous evidence. Claim frequency per physician has grown at roughly 10 percent a year and severity has increased at twice the rate of inflation of consumer prices. Nevertheless, this does not mean that the tort changes have had no effect. States that enacted shorter statutes of limitations and set outer limits on discovery rules have had less growth in claim frequency than states with statutes more lenient to plaintiffs. On average, cutting one year off the statute of limitations for adults appears to have reduced claim frequency by 8 percent; the effect would presumably be greater for a reduction from, say, four to three years than from ten to nine years.

Statutes permitting or mandating the offset of collateral benefits have apparently reduced malpractice claim severity by 11–18 percent and claim frequency by 14 percent, relative to comparable states without collateral source offset. The feedback from a reduction in severity to a reduction in frequency is not surprising, since collateral source offset reduces the potential recovery for a large number of claims, so reduces incentives to file.

Caps on awards have reduced severity by 23 percent. This is the average effect of the various forms of caps over the period 1975–1984. If the dollar thresholds are not revised periodically to keep pace with inflation, the future effect will presumably be greater, unless juries find ways of implicitly circumventing the limits by increased allowances for uncapped components of the award.

Arbitration statutes appear to have increased claim frequency and reduced average severity. Disaggregated (i.e., individual claim) data are necessary to determine whether the reduction in observed average severity results from a reduction in awards per case or simply reflects the filing of more small claims. The net effect appears to be an increase in total claim costs but compensation to more patients.

None of the other reforms analyzed, including screening panels and limits on contingent fees, appear to have had any systematic effect on claim frequency or severity. Again, the overall average may mask different effects in individual states.

Among the other factors affecting claims, urbanization remains a highly significant factor that explains much of the observed difference among states in claim frequency and severity. Per capita income, the unemployment rate, and the number of attorneys per capita have no statistically significant effects after controlling for urbanization. The surgery rate in a state increases claim frequency; the ratio of surgeons to medical specialists increases claim severity. Unfortunately, because
there are no data on trends in the underlying incidence of injury due to medical negligence, it is not possible to compare these trends in malpractice claims with trends in malpractice injuries. Because only a low percentage of malpractice claims were filed ten years ago, some continued growth in malpractice claims is perhaps not surprising. However, the continued growth in malpractice awards in real (constant) dollars suggests that the optimal structure of tort awards may warrant further attention.

This study did not attempt to measure the effect of tort reforms on malpractice insurance premiums. That would depend not only on the effect on expected claim costs, but also on the effect on investment income and on predictability of losses. Reforms that reduce uncertainty of tort losses should reduce the volatility of both price and availability of malpractice insurance.

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This research was undertaken while the author was Associate Professor at Duke University. She is now Associate Professor of Health Care Systems and Insurance at the University of Pennsylvania.

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The author would also like to thank the insurance companies that provided data on malpractice claims, and the American Bar Association for providing data on numbers of attorneys. The views expressed are the author's, and are not necessarily endorsed by the research sponsors. The material reported here also appears in Law and Contemporary Problems, Vol. 49, No. 2, Spring 1986, under the title "Medical Malpractice: Can the Private Sector Find Relief?"
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I. INTRODUCTION

Physicians have been liable for medical malpractice in the United States since the eighteenth century, but malpractice claims were rare until recently. In the late 1960s the frequency of claims per physician and claim severity (size of award per paid claim) began to increase at unprecedented rates, culminating in the medical malpractice insurance crisis of the mid-1970s. In response to this crisis, legislatures in almost every state enacted tort reforms intended to curb the rise in claims, in addition to other changes designed to assure the availability of malpractice insurance.\(^1\)

Between 1975 and 1978, claim frequency per physician slowed or even decreased in many states, but since 1978 claim frequency has resumed an upward trend. The St. Paul Fire and Marine Insurance Company, which has been the leading writer of malpractice insurance for many years, reports a 55 percent increase in claim frequency since 1980—from 10.5 claims per 100 physicians in 1980 to 16.3 in 1984.\(^2\) Claim severity increased faster than the rate of inflation throughout the 1970s, and this trend appears to have continued into the 1980s. Claim severity increased 95 percent during the five year period 1979–1983, from $27,408 in 1979 to $53,482 in 1983.\(^3\) The average malpractice jury award is reported to have risen from $404,726 in 1980 to $854,858 in 1984.\(^4\) This upward trend in claim costs, together with the recent spate of large malpractice premium increases, has revived interest in tort reform and hence in the effect of the reforms enacted after the 1975 crisis.

To date, there have been only two published statistical analyses of the effect of tort reforms and other factors on malpractice claims. My earlier analysis of determinants of trends in frequency and severity between 1970 and 1978 concluded that the increase in claims over time and the persistent diversity in experience among states could be only partly explained by such factors as the increase in the number and

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\(^3\) Ibid.

\(^4\) Jury Verdict Research, Inc., cited ibid. This average excludes claims with zero award to the plaintiff, and may be subject to reporting bias.
complexity of medical treatments and concomitant increase in exposure to the risk of iatrogenic injury. The pro-plaintiff trend in common law during the fifties and sixties also appears to have contributed significantly to the rise in claim frequency and severity. The other major factor contributing to the diversity among states was urbanization; however, it was not possible to identify the specific characteristics of urban environments that generate higher claim frequency and severity. Differences in the number of attorneys per capita, the cost of medical services, per capita income, and unemployment rates did not appear to play a significant role.

This early analysis found mixed effects of the tort reforms enacted in response to the 1975 crisis. By 1978, limitations on the plaintiff’s recovery (caps on awards) and mandatory offset of collateral benefits appeared to have significantly slowed the growth in claim severity in states that enacted such changes. None of the other changes, such as pretrial screening panels or shorter statutes of limitations, showed any effect on frequency or severity. Moreover, none of the reforms could explain, in a statistical sense, the lull in growth of claim frequency that occurred between 1975 and 1978. However, this early analysis, using data on claims closed through 1978, obviously did not purport to measure the long run effect of the tort reforms enacted since 1975. In particular, the effect of shorter statutes of limitations on the “long tail” of claims would not then be evident. Even the estimates of the apparent short run effects might have been contaminated by other unmeasured factors related to the crisis, such as changes in public attitudes, which might prove shortlived.

The only other broad-based statistical analysis of the effects of the 1970s tort reforms is Sloan’s analysis of malpractice insurance premiums paid by physicians over the years 1974–1978. Any effects of the tort reforms were even less likely to be detectable in this study because the data refer to malpractice premiums paid over the years 1974–1978. Many of the reforms became effective in or after January 1976, and it is reasonable to expect a lag in effect on claims filed and closed, and an even longer lag in effect on premiums, because insurers would require some experience under the new laws before adjusting premiums. Thus, it is not surprising that Sloan found little impact of the tort reforms. Specifically, Sloan concluded that the only reforms with a statistically significant effect on premiums were (1) screening panels, which

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appeared to reduce premiums, and (2) statutes permitting binding arbitration of malpractice claims, which appeared to increase premiums. The only significant nonlegislative variable was the surgery rate.

The Sloan and Danzon analyses appear to differ in their estimates of the effect of attorney availability. Sloan found that the number of attorneys per capita appeared to have a positive effect, with a rather low level of statistical significance that depended on the other variables included in the regressions, whereas Danzon found that attorney density did not increase malpractice claim costs. Sloan concludes, "Viewing the empirical evidence in its entirety, the notion that a 10 percent increase in a state's lawyer/population ratio leads to an almost like percentage increase in premiums ... is a distinct possibility."

A plausible reason for the apparent difference in findings is that Sloan's analysis did not control for urbanization. Urban areas tend to have a high density of lawyers per capita, but other characteristics of urban environments may also raise claim frequency and severity. Failure to control for urbanization would then lead to an upward-biased estimate of the effect of attorney density on claims, because the attorney variable would "pick up" the effect of the other, unmeasured characteristics of urban areas that are positively correlated with attorney density. Danzon also found a positive relation between attorney density and claim frequency and severity, if the urbanization variable was omitted, but the attorney effect became insignificant, once urbanization was included.7

This study updates the earlier estimates of the effects of the tort reforms and other factors on trends in malpractice claim frequency and severity, using claim experience over the full decade 1975 through 1984. The length of time since the enactment of the 1975–1976 tort reforms should in principle now be long enough to estimate their long run effects, but in practice several difficulties remain. First, the reforms have been subject to legal challenge in many states, and final rulings on their constitutionality have been long delayed. For example, the California cap on awards for pain and suffering was not finally upheld in state court until early 1985, and the U.S. Supreme Court refused to hear the case only in October 1985.8 To the extent that the disposition of claims over the last decade has been influenced by uncertainty regarding the outcome of these legal challenges, it may still be too early to estimate full long run effects of those reforms that have been upheld.

7Although it is difficult to distinguish empirically the net effects of two highly correlated variables, I infer that urbanization is more important because it has higher explanatory power, if included alone, and it alone is significant when both variables are added.

A second practical difficulty in estimating the effect of tort reforms is lack of a consistent, comprehensive database. The medical malpractice insurance market has undergone substantial changes since 1975 in the identity of carriers and types of coverages. Several major stock insurers have withdrawn and have been replaced by physician-owned mutuals, hospital "captive," and joint underwriting associations (JUAs). A substantial fraction of the market has also changed from an "occurrence" policy—which covers all claims arising out of medical incidents occurring in the policy year, regardless of the date of claim filing—to "claims-made" coverage—which covers only claims filed in the policy year, regardless of the date of injury, provided the physician was covered by a claims-made policy with that company at the time of the injury. Although these changes in liability insurance markets are not expected to affect the behavior of patients and courts with regard to filing and disposing of claims, the large number of insurers who have had a considerable market share for at least some fraction of the period under study makes collection of a comprehensive database on claim experience very costly. The switch from occurrence to claims-made coverage further complicates the estimation of trends, particularly for claim frequency, because the number of claims reported tends to be low in the early years of claims-made coverage, rising as the policy "matures," even with no change in the underlying frequency of claims filed.
II. DATA AND METHODS

DATA

My earlier analysis of 1970s malpractice claim trends drew on a virtual census of all claims closed during 1975 through 1978, collected in a special survey by the National Association of Insurance Commissioners (NAIC).\(^1\) Since 1978, there has been no national compilation of malpractice claims. Data for this study were requested from most of the insurers that have had a large market share at any time from 1975 through 1984. Several major stock insurers and physician-owned companies, covering roughly 100,000 physicians, provided data in the format and detail requested—49 states are represented for at least some years. It remains possible that the database may not be fully representative of the situation nationwide; where relevant, that is noted in the discussion of empirical findings below.\(^2\)

METHODOLOGY

In attempting to measure the effects of various factors, including tort reform, on the frequency and severity of malpractice claims, statistical methods compare the average experience of physicians in different states and at different points in time over the ten years 1975–1984. The units of observation are not individual claims, but individual states in each year. The variables to be explained are claim frequency per 100 physicians and average severity per paid claim, by state. To estimate the effect of a particular tort reform, one cannot simply compare the experience of state X before and after enactment,

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\(^1\)The NAIC, an organization composed of all current heads of state insurance regulatory agencies, collected claim experience from all companies with more than $1 million in malpractice premium volume in any year since 1970. See National Association of Insurance Commissioners, Malpractice Claims 1975–1978, Brookfield, Wisconsin, September 1980.

\(^2\)For example, data published by the American Medical Association show significant differences in claim frequency for 1983, by type of insirer. Physician-owned companies linked to medical societies report 24 claims per 100 insured physicians in that year, compared with only 10 per 100 physicians for commercial companies and 11 per 100 for independent provider-owned insurers. American Medical Association Task Force on Professional Liability and Insurance 1984–85, Professional Liability in the ‘80s, Report 1, p. 10, 1985. These differences could reflect such factors as differences in states in which the companies do business; differences in underwriting stringency; and simple differences in claim reporting practices.
because other factors may have changed over the same timespan. Similarly, one cannot simply compare, at a particular point, states that have enacted a particular reform and states that have not, because other factors may contribute to any observed differences between states. For example, assume that in 1984 state A, which enacted a cap on awards, had 20 percent higher claim severity than did state B, without a cap. One should not infer that the cap had no effect, because claim severity might have been 50 percent higher in A than in B had the cap not been enacted. Thus, to estimate the net effect of the cap—or any other factor—statistical methods are used to "control" for other factors.

The number of factors that can be controlled for depends on the number of independent observations in the sample (which determines the "degrees of freedom") and on the subset of the observations that have the characteristic of interest. This database contains observations on over 45 states in each of 10 years, but the effective degrees of freedom are less than 450 (45 times 10) to the extent that experience in successive years in any state is not fully independent of experience in the same state in previous years. This complicates measurement of statistical significance. Further, it is not possible to measure the effect of each different variant of each reform—for example, a cap on pain and suffering at $250,000, at $100,000, and so on—because the number of observations on each variant is too small. For most reforms, therefore, I have estimated the average effect for each type of change. Two exceptions are that separate estimates were made for the effects of mandatory and discretionary collateral source offset, and for different types of legislation regarding screening panels. Table 1 gives definitions and means of all variables.

To control for the possibility that tort reforms were more likely to be adopted in states with a high underlying propensity for litigation, two-stage least squares (TSLs) estimates are reported and are probably more reliable than ordinary least squares (OLS) estimates, which are also reported. Further detail is given in the appendix.
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<td>Arbitration</td>
<td>.234</td>
<td>.419</td>
<td>– 1 if statute permitting binding arbitration</td>
</tr>
<tr>
<td>CPI (log$_e$)</td>
<td>5.434</td>
<td>.227</td>
<td>Consumer price index</td>
</tr>
<tr>
<td>% over 65</td>
<td>.108</td>
<td>.018</td>
<td>Fraction of state population &gt; 65</td>
</tr>
<tr>
<td>Surgeons per M.D.</td>
<td>.312</td>
<td>.024</td>
<td>Surgical specialists/total physicians in patient care</td>
</tr>
<tr>
<td>Cap</td>
<td>.204</td>
<td>.395</td>
<td>– 1 if any limit on plaintiff recovery</td>
</tr>
<tr>
<td>Claims per physician (CM policies)</td>
<td>.165</td>
<td>.219</td>
<td>Claims filed/earned exposure</td>
</tr>
<tr>
<td>Paid claims per physician (CM policies)</td>
<td>.056</td>
<td>.114</td>
<td>Claims closed with payment/earned exposure</td>
</tr>
<tr>
<td>Severity (log$_e$)</td>
<td>10.349</td>
<td>1.079</td>
<td>Total indemnity/paid claims</td>
</tr>
</tbody>
</table>
III. DETERMINANTS OF MALPRACTICE CLAIM FREQUENCY

THEORETICAL MODEL OF CLAIM FREQUENCY

A rigorous statement of the theoretical model underlying the empirical estimates is given elsewhere.\(^1\) Summarizing, the frequency of malpractice claims per physician is expected to depend on the rate of iatrogenic injury in current and prior years. The rate of iatrogenic injury depends on the number and types of patient contacts per physician and on the rate of injury per patient contact, which may in part depend upon physicians' incentives to practice with care. Under a negligence rule of liability, in principle only those iatrogenic injuries resulting from violation of the standard of due care are actionable. Thus, given a flow rate of iatrogenic injuries per year, the number of potential malpractice claims depends on the standard of care being applied by the courts. The stock of potential claims in any year then depends on the flow of potential claims in prior years and on the statute of limitations, which limits the time during which a potential claim must be filed.

The number of claims actually filed in any year from the stock of potential claims depends on the expected costs and benefits of suit to plaintiffs and plaintiffs' attorneys, which in turn depend on legal rules and norms that determine the probability of winning, the potential award or settlement if successful, and the costs of bringing suit. The number of claims closed with payment in any year depends on the frequency of filing in prior years, on lags in claim disposition, and on the proportion of filed claims that are either dropped or dismissed without payment. Because legal changes and other factors are expected to affect claim filings and disposition by calendar year, the analysis here is on a calendar year basis, rather than on an insurance policy year.

The frequency experience under claims-made and occurrence coverages were analyzed separately, for practical rather than theoretical reasons. In the database available for this study the occurrence experience consists largely of the "tail" of late claims filed more than two years after the policy period, whereas the claims-made data are more representative of all types of claims, at least for the later years. The states represented also differ slightly between the occurrence and claims-made samples.

\(^{1}\)Danzon, Frequency and Severity of Medical Malpractice Claims.
In order to measure the net effect of tort reforms, controlling for demographic factors, estimates of the effect of demographic variables on claim frequency are reported first, in Table 2 for claims filed and Table 3 for claims closed with payment. Estimates of the effect of tort reforms on claims filed and claims paid are reported in Tables 4 and 5, respectively. Each of the law variables was added separately to the basic equation including all demographic variables reported in Tables 2 and 3.²

FINDINGS

Time Trends

The St. Paul Fire and Marine Insurance Company reports an average 10 percent annual growth rate of claim frequency per physician over the decade 1975–1984, with a slower rate in the first four years followed by some acceleration since 1979. This estimate is based on their combined occurrence and claims-made coverage so is not seriously biased by the change in policy form.³ By contrast, the growth rate estimated here from the claims-made sample alone (Table 2) is 14 percent per year, which overstates the true trend in frequency because it reflects in part the maturing of new claims-made policies, particularly for the first half of the decade. This illustrates the potential for upward bias in estimates of true growth in claim frequency based on the experience of new companies offering claims-made coverage. The trend estimated from the occurrence sample of −37 percent per year is best interpreted as the annual rate of “decay” of claim frequency in successive years after the year of practice.

The annual rate of growth of paid claims (21 percent) estimated from the claims-made sample (Table 4) appears to be substantially higher than for all claims filed (14 percent). It might be tempting to infer from the faster growth rate of paid claims either that the proportion of frivolous claims has declined or that insurers are becoming

²Because the coefficients of the TSLS equations changed very little depending on which law was added, only one illustrative set is reported, to show the effect on the demographic coefficients of the two-stage procedure.

³Perfect lack of bias would require that the exposure base (number and specialty mix of physicians) remained constant throughout the period. Claims filed in the first year of a claims-made policy include only claims filed within a year of an incident. The second year of coverage includes first-year claims for that year and second-year claims from the first year of coverage, and so on. A claims-made policy is considered mature by the fifth year. This estimate of growth in claims filed per physician may overstate the growth in incidents filed, to the extent there has been an increase in the number of defendants named per incident.
Table 2
EFFECT OF DEMOGRAPHIC VARIABLES ON FREQUENCY OF CLAIMS FILED
1975–1984

<table>
<thead>
<tr>
<th>Explanatory Variable</th>
<th>Claims per Physician Log ( \frac{F}{1-F} )</th>
<th>Total Claims Filed (Log,( \text{OLS} ))</th>
<th>Total Claims Filed (Log,( \text{TSLS} ))</th>
<th>Occurrence Policies</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
<td>TSLS</td>
<td>OLS</td>
<td>TSLS</td>
</tr>
<tr>
<td>Intercept</td>
<td>-4.813**</td>
<td>-5.311**</td>
<td>-5.064**</td>
<td>-5.133**</td>
</tr>
<tr>
<td></td>
<td>(2.69)</td>
<td>(2.43)</td>
<td>(2.81)</td>
<td>(2.34)</td>
</tr>
<tr>
<td>Time</td>
<td>.141**</td>
<td>.146**</td>
<td>.131**</td>
<td>.133**</td>
</tr>
<tr>
<td></td>
<td>(7.32)</td>
<td>(6.19)</td>
<td>(6.90)</td>
<td>(5.83)</td>
</tr>
<tr>
<td>Exposure (log,( x ))</td>
<td>—</td>
<td>.1039**</td>
<td>1.010**</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(30.40)</td>
<td>(23.71)</td>
<td></td>
</tr>
<tr>
<td>Prior exposure (log,( x ))</td>
<td>.069**</td>
<td>.063**</td>
<td>.061**</td>
<td>.061**</td>
</tr>
<tr>
<td></td>
<td>(6.44)</td>
<td>(4.76)</td>
<td>(4.88)</td>
<td>(4.09)</td>
</tr>
<tr>
<td>Surgical procedures per capita</td>
<td>.011**</td>
<td>.011**</td>
<td>.012**</td>
<td>.012**</td>
</tr>
<tr>
<td></td>
<td>(2.61)</td>
<td>(2.19)</td>
<td>(2.97)</td>
<td>(2.40)</td>
</tr>
<tr>
<td>% Urban</td>
<td>.050**</td>
<td>.285</td>
<td>.365**</td>
<td>.226</td>
</tr>
<tr>
<td></td>
<td>(2.83)</td>
<td>(1.18)</td>
<td>(1.87)</td>
<td>(.94)</td>
</tr>
<tr>
<td>% Migrant</td>
<td>.013*</td>
<td>.010</td>
<td>.012*</td>
<td>.009</td>
</tr>
<tr>
<td></td>
<td>(1.91)</td>
<td>(1.13)</td>
<td>(1.71)</td>
<td>(1.09)</td>
</tr>
<tr>
<td>Unemployment rate</td>
<td>.008</td>
<td>-.002</td>
<td>.012</td>
<td>-.001</td>
</tr>
<tr>
<td></td>
<td>(.35)</td>
<td>(.08)</td>
<td>(.56)</td>
<td>(.02)</td>
</tr>
<tr>
<td>Income per capita (log,( x ))</td>
<td>-.070</td>
<td>-.048</td>
<td>-.079</td>
<td>-.077</td>
</tr>
<tr>
<td></td>
<td>(.32)</td>
<td>(.18)</td>
<td>(.37)</td>
<td>(.30)</td>
</tr>
<tr>
<td></td>
<td>(.50)</td>
<td>(.16)</td>
<td>(.27)</td>
<td>(.01)</td>
</tr>
<tr>
<td>California</td>
<td>.977**</td>
<td>1.239**</td>
<td>.865**</td>
<td>1.117**</td>
</tr>
<tr>
<td></td>
<td>(7.78)</td>
<td>(6.92)</td>
<td>(7.03)</td>
<td>(6.34)</td>
</tr>
<tr>
<td>Explanatory Variable</td>
<td>Claims per Physician Log (1 - P)</td>
<td>OLS</td>
<td>TSLS</td>
<td>OLS</td>
</tr>
<tr>
<td>----------------------</td>
<td>---------------------------------</td>
<td>-----</td>
<td>------</td>
<td>-----</td>
</tr>
<tr>
<td>Illinois</td>
<td>920</td>
<td>1.01</td>
<td>1.01</td>
<td>920</td>
</tr>
<tr>
<td>Maryland</td>
<td>(1.39)</td>
<td>(1.47)</td>
<td>(1.39)</td>
<td>(1.47)</td>
</tr>
<tr>
<td>New Jersey</td>
<td>(3.61)</td>
<td>(2.85)</td>
<td>(3.61)</td>
<td>(2.85)</td>
</tr>
<tr>
<td>New York</td>
<td>(3.39)</td>
<td>(2.38)</td>
<td>(3.39)</td>
<td>(2.38)</td>
</tr>
<tr>
<td>Arizona</td>
<td>872</td>
<td>0.90</td>
<td>0.90</td>
<td>872</td>
</tr>
<tr>
<td>Durbin-Watson</td>
<td>1.34</td>
<td>1.34</td>
<td>1.34</td>
<td>1.34</td>
</tr>
<tr>
<td>ρ statistic</td>
<td>.27</td>
<td>.27</td>
<td>.27</td>
<td>.27</td>
</tr>
</tbody>
</table>

* = p < .10, ** = p < .05 (using a two-tailed test). See appendix for discussion of possible bias in t-statistics.
<table>
<thead>
<tr>
<th>Explanatory Variable</th>
<th>Claims per Physician Log (F)</th>
<th>Total Claims Filed (Log)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
<td>TLS</td>
</tr>
<tr>
<td>Statute of limitations</td>
<td>-0.09 **</td>
<td>0.02 **</td>
</tr>
<tr>
<td>Collateral source</td>
<td>(2.69)</td>
<td>(2.63)</td>
</tr>
<tr>
<td>Offset</td>
<td>-0.19 **</td>
<td>0.18 **</td>
</tr>
<tr>
<td>Mandatory collateral</td>
<td>(2.49)</td>
<td>(2.48)</td>
</tr>
<tr>
<td>Mandatory panel</td>
<td>(1.87)</td>
<td>(1.84)</td>
</tr>
<tr>
<td>Fee limit</td>
<td>(0.97)</td>
<td>(0.96)</td>
</tr>
<tr>
<td>Arbitration</td>
<td>(0.31)</td>
<td>(0.30)</td>
</tr>
</tbody>
</table>

* = p < 0.1, ** = p < 0.05

Table 3

EFFECT OF TORT REFORMS ON FREQUENCY OF CLAIMS FILED 1976–1984

Claims-Made Policies

Occurrence Policies

See appendix for discussion of possible bias in t-statistics.
### Table 4

**Effect of Demographic Variables on Frequency of Claims Closed with Payment, 1975–1984**

<table>
<thead>
<tr>
<th>Explanatory Variable</th>
<th>Claims-Made Policies</th>
<th>Occurrence Policies</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
<td>TSL</td>
</tr>
<tr>
<td></td>
<td>(3.99)</td>
<td>(3.87)</td>
</tr>
<tr>
<td>Time</td>
<td>.213**</td>
<td>.217**</td>
</tr>
<tr>
<td></td>
<td>(8.76)</td>
<td>(8.34)</td>
</tr>
<tr>
<td>Exposure (log)</td>
<td>-.964**</td>
<td>-.940**</td>
</tr>
<tr>
<td>Prior exposure (log)</td>
<td>.130**</td>
<td>.128**</td>
</tr>
<tr>
<td>Surgical procedures</td>
<td>.029**</td>
<td>.029**</td>
</tr>
<tr>
<td>per capita (5.43)</td>
<td>(5.13)</td>
<td>(5.23)</td>
</tr>
<tr>
<td>% Urban</td>
<td>-.647**</td>
<td>-.782**</td>
</tr>
<tr>
<td></td>
<td>(2.71)</td>
<td>(2.93)</td>
</tr>
<tr>
<td>% Migrant</td>
<td>.005</td>
<td>.003</td>
</tr>
<tr>
<td></td>
<td>(.58)</td>
<td>(.30)</td>
</tr>
<tr>
<td>Unemployment rate</td>
<td>-.014</td>
<td>-.021</td>
</tr>
<tr>
<td></td>
<td>(.61)</td>
<td>(.68)</td>
</tr>
<tr>
<td>Income per capita</td>
<td>.034</td>
<td>.048</td>
</tr>
<tr>
<td>(log)</td>
<td>(.13)</td>
<td>(.17)</td>
</tr>
<tr>
<td>Attorneys per capita</td>
<td>-.2202</td>
<td>-.125</td>
</tr>
<tr>
<td>(capita)</td>
<td>(.35)</td>
<td>(.18)</td>
</tr>
<tr>
<td>California</td>
<td>1.154**</td>
<td>1.323**</td>
</tr>
<tr>
<td></td>
<td>(7.26)</td>
<td>(6.69)</td>
</tr>
</tbody>
</table>
Table 4—continued

<table>
<thead>
<tr>
<th>Explanatory Variable</th>
<th>Claims per Physician Log ( \frac{F}{1-F} )</th>
<th>Total Claims Paid (Log.)</th>
<th>Occurrence Policies</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Claims-Made Policies</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>OLS</td>
<td>TSLS</td>
<td>OLS</td>
</tr>
<tr>
<td>Illinois</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>Maryland</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>New Jersey</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>New York</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>Arizona</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>R²</td>
<td>.74</td>
<td>.72</td>
<td>.85</td>
</tr>
<tr>
<td>Durbin-Watson statistic</td>
<td>1.30</td>
<td>1.22</td>
<td>1.60</td>
</tr>
<tr>
<td>ρ</td>
<td>.35</td>
<td>.39</td>
<td>.20</td>
</tr>
</tbody>
</table>

* = p ≥ .10
** = p ≥ .05

using a two-tailed test. See appendix for discussion of possible bias in t-statistics.
Table 5
EFFECT OF TORT REFORMS ON FREQUENCY OF CLAIMS CLOSED WITH PAYMENT, 1975–1984

<table>
<thead>
<tr>
<th>Explanatory Variable</th>
<th>Claims-Made Policies</th>
<th>Occurrence Policies</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
<td>TSLS</td>
</tr>
<tr>
<td>Statute of limitations</td>
<td>-.006 (.62)</td>
<td>.057* (.65)</td>
</tr>
<tr>
<td>Collateral source offset</td>
<td>.113 (.36)</td>
<td>-.038 (.29)</td>
</tr>
<tr>
<td>Mandatory collateral offset</td>
<td>.073 (.78)</td>
<td>.106 (.97)</td>
</tr>
<tr>
<td>Panel</td>
<td>.045 (.63)</td>
<td>.066 (.16)</td>
</tr>
<tr>
<td>Mandatory panel</td>
<td>.041 (.29)</td>
<td>.087 (.80)</td>
</tr>
<tr>
<td>Fee limit</td>
<td>.117 (.40)</td>
<td>.125 (.64)</td>
</tr>
<tr>
<td>Arbitration</td>
<td>.183** (2.27)</td>
<td>.505** (2.46)</td>
</tr>
</tbody>
</table>

* = p ≥ .10  
** = p ≥ .05  
using a two-tailed test. See appendix for discussion of possible bias in t-statistics.
more willing to "buy out" of frivolous claims. No such inference is valid, however, because of the bias due to the maturing of new claims-made policies. Since claims closed with payment take longer to settle than claims closed without payment, the proportion of claims closed with payment will be relatively low in the early years of new policies but will rise as the policies mature, implying a greater apparent growth in paid claims than in total claims, until the insurer's book of business is fully "mature"—that is, the number of new policyholders is equal to the number of terminations each year.

Claims vs. Injuries

Earlier analyses indicated that in the mid 1970s, at most one in ten incidents of malpractice gave rise to a claim.\(^4\) Since then, the total number of claims has roughly doubled. It would be of great interest to know how much of this growth in malpractice claims reflects a growth in iatrogenic injury and how much reflects an increase in the proportion of potential claims that are actually filed. Unfortunately, this is not possible because there are no data on the actual number of iatrogenic injuries.

Several variables were included in this analysis as proxies for exposure to iatrogenic injury. Most consistently significant was the number of surgical procedures per capita, which is positively related to the frequency of claims filed per physician. Because it is unlikely that surgeons are systematically more negligent than other medical specialties, this suggests that the probability that a claim is filed, given an iatrogenic injury, is higher for surgical procedures, which is plausible if iatrogenic origin is more obvious for adverse outcomes of surgical procedures than for other medical treatments, or if surgical mishaps are more serious. Consistent with these hypotheses, the number of paid claims is more sensitive to the number of surgical procedures than is the total number of claims.\(^5\) The frequency of late filed claims (occurrence sample) is not significantly related to the frequency of surgical procedures, suggesting that most surgery-related claims are filed fairly early. Again, this is not surprising because surgical mishaps are likely to be obvious.\(^6\)

\(^4\)For example, Danzon, Medical Malpractice: Theory, Evidence and Public Policy (1985).

\(^5\)The estimated elasticity of claims filed with respect to number of surgical procedures per 10,000 population is 0.01, and the elasticity of claims paid is 0.03, implying a slightly higher conditional probability of payment, given that a claim has been filed, for claims involving surgical mishaps.

\(^6\)In equations not reported here, claim frequency was found to be unrelated to the number of hospital admissions per capita, although most malpractice claims arise out of hospital treatment.
Nontort Sanctions

Since 1975 many states have strengthened their procedures for quality control through Boards of Medical Quality Assurance, partly as a quid pro quo for tort reform. In theory, the frequency of malpractice and therefore the frequency of malpractice claims should be lower in states with active disciplinary boards. Contrary to this expectation, claim frequency tended to be positively related to the number of disciplinary procedures per 1,000 physicians, but statistical significance levels were low.7 This is probably best explained as a reverse effect, that disciplinary procedures are tightened in states experiencing high claim frequency. If so, malpractice liability may indirectly affect the quality of care by creating incentives to strengthen other monitoring mechanisms, as well as a direct effect on physicians' incentives. Note that failure to find a negative relation between disciplinary activity and claim frequency does not imply such activity has no effect on the rate of iatrogenic injury. Because a substantial fraction of negligent iatrogenic injuries did not result in a claim in the mid 1970s (as many as nine out of ten)6 there could be a substantial reduction in injuries with little visible effect on claims.

Urbanization

My earlier analysis concluded that urbanization was the single most important factor contributing to interstate differences in malpractice claims. The results here tend to confirm that conclusion, but with two qualifications. First, urbanization is positively related to the total number of claims filed but negatively related to the number of claims paid. This suggests that urban areas may have a disproportionate number of "frivolous" or nonmeritorious claims. Second, the fact that the apparent effect is much higher for the occurrence sample than for the claims-made sample suggests that urban areas have a disproportionate number of late filed claims, even after controlling for the statute of limitations. These conclusions are somewhat tentative, however, because the major urban states may be underrepresented in this database, particularly in the claims-made sample.9
The Business Cycle

It is often argued that personal injury and disability claims are inversely related to the business cycle. A plausible reason might be that when business activity is low and unemployment rates are high, the opportunity cost of time for attorneys and for patients is low; moreover, plaintiffs are less likely to have first party insurance coverages through employment, so they may be more willing to seek compensation through the tort system. The evidence here is inconsistent with this hypothesis. The frequency of total claims and paid claims is unrelated to the unemployment rate, and the frequency of late claims (occurrence sample) is negatively related to the unemployment rate. There is also no systematic relationship between the frequency of malpractice claims and average per capita income in a state.

The Doctor-Patient Relationship

The rise in malpractice claims and the high incidence of claims in urban areas are often attributed to erosion of the tradition of long-standing physician-patient relationships with a family physician, which are said to have created a psychological barrier to suit. I attempted to measure this phenomenon by including as an explanatory variable the percentage of the state's population that had moved within the last five years (Percent migrant). A mobile population may also have less information about the quality of individual physicians' services, hence physicians' incentives for care may be lower—another reason to expect a positive relation between population turnover and claim frequency. Contrary to these hypotheses, population turnover has no systematic effect, except for late filed claims where the effect is negative. This may reflect a higher cost of filing suit after moving from the area where the injury occurred.

The Elderly

Consistent with my earlier analysis, these data show no relationship between claim frequency and the percentage of the population over 65, so this variable is omitted from the reported equations. Since hospital admission rates for the elderly are roughly twice as high as for persons under 65 and a 1974 California study\(^\text{10}\) showed a higher rate of negligent injury per admission for the elderly, the absence of any significant difference in claim frequency implies that the probability of filing a

claim, given a potentially actionable injury, is less than half that of persons under 65. A plausible explanation is that the elderly have lower compensable damages.

Number of Lawyers

Also consistent with my earlier analysis, there is no evidence that a high density of lawyers per capita has any systematic effect on the frequency of claims filed, after controlling for other characteristics of areas with high lawyer density. Thus, although there is a high simple correlation between number of claims per physician and number of attorneys per capita, this appears to reflect the tendency of attorneys to migrate to areas where litigation rates are high (probably for other areas of law in addition to medical malpractice), rather than an independent effect of attorneys on litigation rates.

TORT REFORMS

In evaluating the evidence of the impact of tort reforms, it must be emphasized that because of the limitations of the data, there is some uncertainty as to the true levels of statistical significance.\textsuperscript{11} Conventional tests of significance are applied here, using unadjusted standard errors and a two-tailed test. In cases where theory yields a clear prediction as to the effect of a particular law—for example, that the effect, if any, of a cap on recoveries should be negative—it may be more appropriate to apply a one-tailed test, in which case significance levels are twice those reported. Where claim frequency and severity are measured in logs, the percentage effect of a reform is obtained by exponentiating the coefficients reported.\textsuperscript{12} In general, the OLS coefficients tend to be less negative than the TSLS coefficients, which is consistent with the hypothesis that reforms were more likely to be enacted in states with high claim costs, so OLS coefficients are biased against observing any negative effect of reforms. The subsequent discussion is therefore based on the TSLS estimates.

\textsuperscript{11}Estimates of standard errors may be downward biased and significance levels may be upward biased because of serial correlation of residuals (see the appendix). On the other hand, the inevitable measurement error involved in collapsing multidimensional laws into simple binary variables probably introduces bias against finding any significant impact even when such an impact may exist. These two sources of bias operate in offsetting directions, but since their relative magnitudes are unknown, the net direction of bias in t-statistics is unknown.

\textsuperscript{12}For example, if the coefficient in the table is \(-.15\), the percentage change in the dependent variable is \(1 - e^{-0.15}\) or \(- \approx -14\%\) percent.
The following results should be interpreted as showing how much a particular reform affected experience, relative to what the experience would have had the law not been enacted. For example, the statement that reform X reduced claim frequency by Y percent does not mean that frequency fell by Y percent but that it was Y percent lower than it would otherwise have been. The percentage difference noted is the average differential in a single year if a particular change had been enacted and had not been overturned. As discussed earlier, some of the reforms were under challenge and therefore might not have been enforced in all cases. The estimates reported here may therefore understate the full long-run effect of a reform, once it has been declared constitutional. Note that if there is an upward underlying trend over time, a given percentage difference implies an increasing absolute difference.

Statute of Limitations

States that have enacted shorter statutes of limitations have experienced some reduction in claim frequency. Specifically, reducing the statute of limitations for adults by one year reduces total claim frequency by 8 percent and frequency of paid claims by 6–7 percent.\(^{13}\) This is the average effect of a one-year reduction, measured at the sample mean of roughly five years; this mean effect cannot be extrapolated indefinitely.\(^{14}\) For example, because the number of claims filed declines with years elapsed from the date of injury, reducing a statute from ten years to nine years probably reduces claims by less than 8 percent, whereas reducing a statute from four years to three years may reduce claims by more than 8 percent.

Collateral Benefits

The only other reforms that show any evidence of reducing claim frequency are laws permitting or mandating awards to be reduced by the amount of insurance coverage from other sources. Collateral source offset is estimated to reduce claim frequency by 14 percent. The effect appears to be greater on claims filed late, but how much greater cannot be stated with precision.\(^{15}\) For paid claims, the negative

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\(^{13}\)The occurrence sample shows a similar estimate (5–6 percent for all claims and paid claims) but only the OLS coefficients are statistically significant.

\(^{14}\)The estimate of the mean for the statute of limitations is based on the arbitrary assignment of 10 years for states with an unlimited discovery period.

\(^{15}\)The TSLS coefficient implies a 77 percent reduction in late filed claims, which seems unreasonably large.
effect appears to be confined to claims filed late (occurrence sample). Laws that simply admit evidence of collateral coverage seem to be as effective in reducing claim frequency as laws mandating offset.

Panels

Screening panels do not appear to have had any systematic effect on the total number of claims filed or paid. Evidence from case studies of the operation of panels in individual states indicates that although the effects of screening panels differ substantially among states, depending on the specific design of the panels, in general they may even encourage filings by reducing the cost to the plaintiff of obtaining expert testimony.\(^\text{16}\)

Arbitration

Laws permitting binding arbitration apparently increased the frequency of claims filed and claims paid.\(^\text{17}\) The effect appears to be greater for small claims than for claims involving serious injury. This inference is suggested by the fact that the measured effect of arbitration is smaller for the occurrence sample, which consists disproportionately of late-filed claims that typically involve more serious injuries, than for the claims-made sample, which includes a disproportionate number of minor claims. This finding is entirely consistent with one objective of the proponents of arbitration, which is to provide a less expensive course for small claims, which may be barred by the costs of the tort system.

\(^{16}\)Danzon, Medical Malpractice and sources cited therein.

\(^{17}\)The coefficients imply as much as a 60 percent increase, which seems implausibly large, because arbitration would probably only be adopted by a minority of patients and providers even in states with enabling legislation.
IV. TRENDS IN MALPRACTICE CLAIMS SEVERITY

THEORETICAL MODEL OF CLAIM SEVERITY

Claim severity is measured as the average indemnity paid on claims closed with payment, including verdicts and out-of-court settlements. For some companies, reported indemnity includes loss adjustment expense, which averages roughly 30 percent of total indemnity.\(^1\)

It is important to note that trends and interstate differences in observed severity (the average amount actually paid) may understate real differences in “potential” severity (the expected payment at verdict or settlement for a specific injury), which I shall call “jury generosity.” The reason is that an increase in jury generosity not only raises actual payments on claims that would have been filed prior to the change, but also raises the expected net payoff on all potential claims. An increase in expected net payoff leads to the filing of more small claims and claims with low probability of winning, which are not worth filing if juries are less generous. As the mix of paid claims becomes more heavily weighted with small claims, observed average severity may fall, even though each individual claim receives a larger award. To measure interstate differences in jury generosity would require data on individual claims, not just statewide average severity available in this database.

In theory, average severity is expected to depend on the “true” damages incurred on claims closed with payment, and on the valuation of these damages by the courts. “True” damages depend on the mix of injury severity in the sample of claims receiving payment, and on the plaintiffs’ actual or potential wage loss, medical expenses, and noneconomic loss. Rules of compensable damages in principle determine the valuation of these damages by the courts, subject to interpretation by judge and jury. Whether changes in these rules have any effect in practice is one of the empirical questions being addressed here.

Of the post-1975 tort reforms, those most directly aimed at reducing severity are caps on awards (either on the total award or, more commonly, on the component for pain and suffering); modification of the collateral source rule, to admit evidence or mandate offset of

\(^1\)Personal communication from the St. Paul. The company-specific dummy variables should control for this difference in reporting practice.
compensation from other sources; and provisions for periodic payment of future damages. Ceilings on schedules for contingent fees may also reduce awards, to the extent that they are enforceable and reduce the incentives of plaintiff attorneys to pursue claims. Although caps on awards and sliding scale fee ceilings are likely to affect only very large potential awards, which are a small fraction of all claims, these few cases account for a very large fraction of dollars paid so they can substantially influence average severity.

Reducing awards is also one objective of proponents of arbitration, since eliminating the role of the supposedly overgenerous jury is one of the major differences between arbitration and tort procedures. Finally, screening panels may affect potential severity to the extent that panels change the cost of litigation or have direct power to determine damages; panels may also affect observed severity indirectly to the extent they screen out "frivolous" claims that might otherwise have been settled with a small payment, thereby changing the mix of claims paid and raising the average amount actually received.

For the analysis of claim severity, the claims-made and occurrence databases were merged, since there are no strong theoretical or practical reasons for distinguishing them; on the contrary, combining them eliminates potential biases that would result from separate analysis. Table 6 reports estimates of the effect of demographic factors on average severity per paid claim; Table 7 reports OLS and TSLS estimates of the effect of tort reforms, obtained by adding each law separately to the basic equation in Table 6.

FINDINGS

Time Trend

Malpractice claim severity has risen roughly twice as fast as the Consumer Price Index (CPI). This can only partially be explained by the fact that medical care prices have risen more rapidly than consumer prices in general, because medical expense accounts for less than one-quarter of reported economic loss in malpractice cases closed with payment. As noted above, this growth in average severity may understate the growth in the "generosity" of the tort system, to the extent that the potential for more generous awards induces the filing of more minor claims. Unfortunately, with these aggregated data it is not possible to tell whether the increase has been uniform across all cases or

\footnote{The elasticity of severity with respect to the CPI is 1.9.}

\footnote{NAIC, Malpractice Claims, p. 51.}
Table 6
EFFECT OF DEMOGRAPHIC VARIABLES ON AVERAGE SEVERITY
PER PAID CLAIM (log.) 1976–1984, CLAIMS-MADE
AND OCCURRENCE POLICIES

<table>
<thead>
<tr>
<th>Explanatory Variable</th>
<th>OLS</th>
<th>TSLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td>-1.082*</td>
<td>-1.031*</td>
</tr>
<tr>
<td>(2.02)</td>
<td>(1.90)</td>
<td></td>
</tr>
<tr>
<td>CPI (log)</td>
<td>1.940**</td>
<td>1.918**</td>
</tr>
<tr>
<td>(25.18)</td>
<td>(24.41)</td>
<td></td>
</tr>
<tr>
<td>% Urban</td>
<td>.679**</td>
<td>.766**</td>
</tr>
<tr>
<td>(5.56)</td>
<td>(5.81)</td>
<td></td>
</tr>
<tr>
<td>% over 65</td>
<td>-2.185*</td>
<td>-1.238</td>
</tr>
<tr>
<td>(1.77)</td>
<td>(.92)</td>
<td></td>
</tr>
<tr>
<td>Surgeons per M.D.</td>
<td>2.654**</td>
<td>2.563**</td>
</tr>
<tr>
<td>(3.22)</td>
<td>(3.18)</td>
<td></td>
</tr>
<tr>
<td>Attorneys per capita</td>
<td>16.370</td>
<td>5.789</td>
</tr>
<tr>
<td>(.78)</td>
<td>(.26)</td>
<td></td>
</tr>
<tr>
<td>Illinois</td>
<td>.599**</td>
<td>.632**</td>
</tr>
<tr>
<td>(5.38)</td>
<td>(5.50)</td>
<td></td>
</tr>
<tr>
<td>Maryland</td>
<td>.251*</td>
<td>.202*</td>
</tr>
<tr>
<td>(2.00)</td>
<td>(1.56)</td>
<td></td>
</tr>
<tr>
<td>New Jersey</td>
<td>-.262**</td>
<td>-.257**</td>
</tr>
<tr>
<td>(4.50)</td>
<td>(4.38)</td>
<td></td>
</tr>
<tr>
<td>New York</td>
<td>.268**</td>
<td>.315**</td>
</tr>
<tr>
<td>(3.70)</td>
<td>(4.07)</td>
<td></td>
</tr>
<tr>
<td>Arizona</td>
<td>-.504**</td>
<td>-.446**</td>
</tr>
<tr>
<td>(4.07)</td>
<td>(3.46)</td>
<td></td>
</tr>
<tr>
<td>California claims-made policies</td>
<td>.253**</td>
<td>.387**</td>
</tr>
<tr>
<td>(2.83)</td>
<td>(3.34)</td>
<td></td>
</tr>
<tr>
<td>California occurrence policies</td>
<td>-.353**</td>
<td>-.219**</td>
</tr>
<tr>
<td>(5.79)</td>
<td>(2.31)</td>
<td></td>
</tr>
<tr>
<td>R²</td>
<td>.627</td>
<td>.624*</td>
</tr>
<tr>
<td>Durbin-Watson statistic</td>
<td>1.387</td>
<td>1.382*</td>
</tr>
<tr>
<td>ρ</td>
<td>.306</td>
<td>.306*</td>
</tr>
</tbody>
</table>

*aFrom equation with statute of limitations added. Values for equations including other laws were very similar.
** = p < .10, * = p < .05 using a two-tailed test. See appendix.

whether the largest awards have grown proportionately faster, as other studies of jury verdicts have found.4

Table 7

EFFECT OF TORT REFORMS ON AVERAGE SEVERITY PER PAID CLAIM (log) 1975-1984, CLAIMS-MADE AND OCCURRENCE POLICIES

<table>
<thead>
<tr>
<th>Explanatory Variable</th>
<th>OLS</th>
<th>TSLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Collateral source offset</td>
<td>-.102*</td>
<td>-.198*</td>
</tr>
<tr>
<td>(1.72)</td>
<td>(1.84)</td>
<td></td>
</tr>
<tr>
<td>Mandatory collateral offset</td>
<td>-.171**</td>
<td>-.114**</td>
</tr>
<tr>
<td>(2.68)</td>
<td>(2.05)</td>
<td></td>
</tr>
<tr>
<td>Cap</td>
<td>-.161**</td>
<td>-.263**</td>
</tr>
<tr>
<td>(2.83)</td>
<td>(2.04)</td>
<td></td>
</tr>
<tr>
<td>Panel</td>
<td>-.005</td>
<td>.185</td>
</tr>
<tr>
<td>(.10)</td>
<td>(1.70)</td>
<td></td>
</tr>
<tr>
<td>Mandatory panel</td>
<td>-.015</td>
<td>-.054</td>
</tr>
<tr>
<td>(.89)</td>
<td>(1.51)</td>
<td></td>
</tr>
<tr>
<td>Arbitration</td>
<td>-.126**</td>
<td>-.212**</td>
</tr>
<tr>
<td>(2.22)</td>
<td>(2.13)</td>
<td></td>
</tr>
<tr>
<td>Fee limit</td>
<td>-.027</td>
<td>-.036</td>
</tr>
<tr>
<td>(.49)</td>
<td>(.34)</td>
<td></td>
</tr>
</tbody>
</table>

* = p ≥ .10
** = p ≥ .05

using a two-tailed test. See appendix.

Urbanization

Consistent with the findings of my earlier study, claim severity is significantly higher in urbanized states. The estimates imply that a ten percentage point increase in the fraction of a state's population living in urban areas is associated with roughly a 7 percent increase in malpractice claim severity. However, the precise magnitude of the urban effect may be influenced by the particular mix of states in this database. Moreover, to the extent the company dummy variables capture state effects as well as company effects, and the company dummies occur disproportionately in large urban states, the measured urban effect may underestimate the true urban effect.

Claim severity is also significantly higher in states with a high ratio of surgical specialists relative to medical specialists. This variable may capture the effect of more complex medical practice in general, in addition to the fact that surgical mishaps are likely to involve more serious injury and be easier to prove.5

5A high probability of proving negligence tends to increase the amount paid in cases settled out of court, which are the great majority of paid claims. Patricia M. Danzon and
The Elderly

Average severity is lower in states with a relatively large elderly population, as expected in view of the lower compensable damages for the elderly, but the level of significance is low. Since the population over 65 accounts for only 11 percent of the total population on average and the evidence above suggests that they do not have a disproportionately high claim frequency, even a small effect on average claim severity would imply that the average claim for persons over 65 receives substantially less than that for persons under 65, which is not surprising in view of the low wage loss of the elderly. Low potential compensable damages presumably contribute to the low propensity to sue noted earlier.

Number of Lawyers

There is no evidence that the number of lawyers per capita has any effect on claim severity, consistent with the findings of my earlier study and with simple theory. To the extent a high density of lawyers depresses the price of legal services, this should affect both the plaintiff and defense sides of the case, and there is no reason to expect any net effect on the outcome. If anything, claim frequency might be affected but there was no evidence for that either.

Tort Reforms

The estimates of the effects of tort reforms are reasonably consistent with theory and with the earlier findings.

Cap on Awards

The average effect of the various statutes to cap all or part of the plaintiff's recovery has been to reduce average severity by 23 percent. This observed average effect obviously masks great differences among cases: The majority of cases would be unaffected by most of the caps, so the effect on the few large awards that are affected must be substantially greater than the average over all cases. Because large awards account for a disproportionate fraction of total dollars—over 50 percent of dollars are paid on 5 percent of cases6—caps that severely reduce the few very large dollar awards can greatly influence the average and the total payout.


6Ibid.
Collateral Source Offset

Laws providing for collateral source offset appear to reduce awards by 11 to 18 percent. Coefficients are similar for laws mandating offset and laws permitting offset at the discretion of the court, but significance levels are slightly higher for mandatory offset. This differs from the earlier analysis based on claims closed between 1975 and 1978, which found a 50 percent reduction for mandatory collateral source offset and no effect of discretionary offset. The lower estimate obtained here is more consistent with a rough estimate I derived, based on estimates of the fraction of awards required to cover economic loss and the fraction of economic loss that might be covered by other sources of insurance. This yields a rough estimate that mandatory offset of all collateral benefits might reduce average severity by 30 to 40 percent. Because some statutes apply only to a subset of collateral sources and offset is sometimes discretionary, the estimate here of 11 to 18 percent reduction from the implementation of these statutes is quite plausible.

Arbitration

States that have enacted special statutes permitting voluntary binding arbitration have average claim severity roughly 20 percent lower than other states. Taken at face value, this suggests that the effect of arbitration on cases actually arbitrated could be even greater, because such cases are probably still only a small fraction of total claims in most states with arbitration statutes. However, the decrease in observed severity may overstate the reduction in award for a specified type of case, if arbitration increases the number of minor claims filed, as suggested by the findings in Table 3. With respect to the effect of arbitration on total claim costs, taken at face value the coefficients imply that the 20 percent decrease in claim severity is more than offset by the positive effect on frequency of claims filed and paid. Thus overall, arbitration probably increases total claim costs, although it does appear to spread the total dollars over a larger number of plaintiffs. However, without more disaggregated data on the effects on individual cases and on litigation cost per case, these conclusions are tentative.

7Danzon, Frequency and Severity of Medical Malpractice Claims.
8Danzon, Medical Malpractice.
Panels

The evidence on the effect of panels severity is not consistent across the different equations. A safe conclusion is that there is no evidence screening panels consistently reduce claim severity.

CONCLUSION

The last round of tort reforms affected the frequency and severity of malpractice claims over the decade 1975–1984 in a manner broadly consistent with theory and with previous evidence. Although claim frequency and severity have continued to rise despite reforms, that does not mean tort changes have had no effect. States that enacted shorter statutes of limitations and set outer limits on discovery rules have had less growth in claim frequency than states with statutes more lenient to plaintiffs. On average, cutting one year off the statute of limitations for adults reduces claim frequency by 8 percent; the effect would presumably be greater for a reduction from, say, four to three years than from ten to nine years.

Statutes permitting or mandating the offset of collateral benefits have apparently reduced malpractice claim severity by 11 to 18 percent and claim frequency by 14 percent, relative to comparable states without collateral source offset. The feedback from a reduction in severity to a reduction in frequency is not surprising, because collateral source offset reduces the potential recovery for a large number of claims, so it reduces incentives to file.

Caps on awards have reduced severity by 23 percent. This is the average effect of the various forms of cap, over the period 1975–1984, during which time some statutes were still under challenge. If the dollar thresholds are not revised periodically to keep pace with inflation, the future effect will presumably be greater, unless juries find ways of implicitly circumventing the limits by increased allowances for uncapped components of the award.

Arbitration statutes appear to have increased claim frequency but reduced average severity. Disaggregated data are necessary to determine whether the reduction in observed average severity results from a reduction in awards per case or simply reflects the filing of more small claims. The net effect appears to be to increase total claim costs but compensate more patients.

None of the other reforms analyzed, including screening panels and limits on contingent fees appear to have had any systematic effect on claim frequency or severity.
Among the other factors affecting claims, urbanization remains a highly significant factor that explains much of the observed difference among states in claim frequency and severity. The evidence suggests that urban areas have a particularly high frequency of nonmeritorious claims (those closed without payment) and claims filed several years after the alleged injury. Per capita income, the unemployment rate, and the number of attorneys per capita have no statistically significant effects. The surgery rate in a state increases claim frequency, and the ratio of surgeons to medical specialists increases claim severity.

A full evaluation of the merits of the various tort reforms from a public policy perspective is beyond the scope of this report and has been done elsewhere. However, it is worth noting that severity has increased on average at almost twice the rate of inflation of consumer prices over the last decade. In the absence of further statutory controls, it appears that the income of successful malpractice claimants—or at least some subset of them—will continue to rise relative to the income of the population as a whole, and relative to the income of other accident victims who are not compensated through the tort system. The optimal structure of tort awards therefore warrants further attention.

Also beyond the scope of this report is the actual and potential effect of tort reforms on malpractice insurance premiums. The analysis here of effect on claim frequency and severity, separately, should not be automatically translated into an effect on premiums for several reasons. First, the net potential effect on premiums depends also on changes in the timing of disbursement of loss reserves and hence investment income, and on litigation expense. Second, reforms that reduce the uncertainty in estimating malpractice claim costs—notably caps on awards, periodic payment of amounts for future damages, and shorter statutes of repose (running from date of incident, not date of discovery)—may be expected to reduce premiums by a modest amount, over and above the reduction in mean expected losses, because of the reduction in insurers' risk. Perhaps more important, by reducing uncertainty such reforms should reduce the volatility in price and availability of malpractice insurance, which is a major inefficiency of the present malpractice system.

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Danzon, Medical Malpractice.
Appendix

DATA AND METHODOLOGY

DATA

The 1982 Danzon analysis of claim frequency and severity used data from the National Association of Insurance Commissioners (NAIC) Survey of Claims Closed 1975–1978. Since the termination of that survey, there has been no comprehensive collection of data on malpractice claims, other than by individual insurance companies for their internal purposes. Data for this analysis were requested from most of the individual companies and joint underwriting associations that have had a significant market share at any time during 1975 through 1984. Many of the companies did not respond and others could not readily provide their data in the format requested, on a calendar and policy year basis. Theoretical considerations suggest analyzing claim frequency and severity by calendar year rather than insurance policy year, because tort reforms are likely to affect the filing and disposition of claims during the calendar years in which the laws are in effect. For occurrence coverage, claim frequency per insured physician in any calendar year depends on the number of physicians insured in previous policy years; the relevant number of policy years depends on the statute of limitations. Similarly, with claims-made coverage, reported claim frequency depends on the number of years of previous coverage with the same company for physicians currently insured. Data were requested on exposure (number of physician years) by policy year and on claims filed, closed, and paid by calendar year and corresponding policy year.

The database used here includes the experience of the St. Paul Fire and Marine Insurance Company, which insures over 55,000 physicians in over 40 states, The Travelers (California), Medical Mutual Liability Insurance Society of Maryland, Southern California Physicians Insurance Exchange, Illinois State Medical Inter-Insurance Exchange, Medical Inter-Insurance Exchange of New Jersey, Medical Liability Mutual Insurance Company of New York, and Mutual Insurance Company of Arizona. Forty-nine states are represented for at least some years, but sample size is small in some states for some years. State year observations with fewer than five insured physicians were deleted from the sample.
Use of these data is further complicated by the fact that the St. Paul began to switch to claims-made coverage in 1975, and 1976 was the last year in which it wrote new occurrence policies. Claims against the earlier occurrence coverage continued to be filed and closed throughout the subsequent decade. Because the database on occurrence average is heavily weighted by the run off on old policies, the analysis of claim frequency is performed separately for claims-made and occurrence coverages.

Data on tort reforms were compiled from several surveys of state statutes. Original statutes were checked where there were discrepancies between the surveys. Most of the laws are measured as binary variables, taking the value of 1 in each year in which the law was in effect, 0 if no change had been enacted or if a law had been enacted but had subsequently been overturned. In the case of collateral source offset and screening panels, separate variables were used to identify mandatory and discretionary requirements. The statute of limitations is measured in years for filing for adults. In states with a discovery rule with no outer limit, the statute of limitations is arbitrarily assigned a value of 10.2

METHODOLOGY

System Issues

The data consist of ten annual cross-sections of over 40 states; therefore, serial correlation of residuals is expected, reflecting unmeasured state-specific and possibly company-specific effects. State effects could include omitted legal and demographic factors that affect the filing and disposition of claims. Company effects could reflect specific claim handling or reporting practices of individual insurers. To control for company effects, I included dummy variables for individual companies other than the St. Paul. It was not feasible to include dummy variables for each state to capture omitted state effects, because of collinearity between these state dummies and the other independent variables that show little variation over the time span. I tried including measures of the frequency and severity of claims closed in 1975 to capture these state effects, but these variables were not statistically

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1The main surveys are Danzon, Frequency and Severity of Medical Malpractice Claims; unpublished survey conducted by the Alliance of American Insurers; American Insurance Association, Medical Malpractice Insurance Reports (draft, November 1981); and American Medical Association, State Health Legislation Reports (various dates).

2A discovery rule tolls the running of the statute of limitations until the injury has, or with due diligence should have been discovered.
significant and did not reduce the serial correlation. Serial correlation of the residuals should not bias coefficient estimates, but standard errors may be downward biased and t-statistics may be upward biased.

The bias in standard errors depends on the correlation across years for each state (\( \rho \)), the length of the time period (\( T \)) and the fraction of the total variance in explanatory variables (\( X \)) attributable to variance between rather than within states.\(^3\) In the extreme, if the correlation of residuals within states across all years is a constant (i.e., between 1975 and 1976, as well as between 1975 and 1984), the corrections are fairly severe: If a variable is constant within states across all years, t-statistics must be divided by \( \sqrt{1 + \rho (T - 1)} \). For example, if \( \rho = .4 \) and \( T = 10 \), the correction is \( \sqrt{1 + .4(9)} = \sqrt{4.6} = 2.2 \). For variables that change within states over the sample period, the correction is smaller and is given by (assuming constant \( \rho \)) \( \sqrt{1 - \rho + (\rho T) \sigma_{X}^{2} / \sigma_{W}^{2}} \), where \( \sigma_{X}^{2} \) is the total variance in \( X \), \( \sigma_{W}^{2} \) is the between-states variance in \( X \) and \( \sigma_{W}^{2} - \sigma_{X}^{2} = \sigma_{W}^{2} \), where \( \sigma_{W}^{2} \) is the within-states variance in \( X \).

This upper bound of 2.2 probably overstates the appropriate correction for several reasons. First, all variables change at least once for at least some states. For background variables such as urbanization, 1975 values were used for the first five years and 1980 values for the last ten years, but true values presumably changed slightly in each year. For tort reforms, some states changed once and some twice, if a reform was overturned. Second, \( \rho \) is presumably not constant over time but diminishes across years, with \( \rho_{1,10} < \rho_{1,2} \). Such diminishing serial correlation was observed in similar data analyzed elsewhere.\(^4\) For example, if the appropriate correction to reported t-statistics is 1.5, then an unadjusted t-statistic of 1.96 (significant at \( p = .05 \) by a two-tailed test) yields an adjusted t-statistic of 1.31, which would be significant at \( p = .1 \) by a one-tailed test, the appropriate null hypothesis for most variables. The unadjusted t-statistic necessary for significance by a one-tailed test would be 2.47 for \( p = .05 \), 1.92 for \( p = .1 \). If the estimated, unadjusted t-statistics are also subject to downward bias because of measurement error in the explanatory variables, then using conventional significance levels for a two-tailed test should not seriously overstate true levels of significance.

To the extent tort reforms were more likely in states with an underlying propensity for high claim frequency and severity not attributable to other measured variables, ordinary least squares (OLS) estimates of the effects of those reforms may be biased. For laws expected to

\(^3\) I am indebted to Rand colleagues Emmett Keeler and Will Manning for this derivation.

\(^4\) Danzon, Frequency and Severity of Medical Malpractice Claims.
reduce claim frequency or severity, the bias from ignoring simultaneity is toward finding no effect even when the true effect is negative. Both OLS and two-stage least squares (TSLS) estimates are therefore reported. Instruments used in the first-stage estimating equations for the laws were: total claim frequency, paid claim frequency, severity per paid claim, number of claims paid more than $100,000, attorneys per capita, and physicians per capita. All values were for 1975.

Functional Form

Logarithmic transformation of the dependent variables is used, on the assumption that changes in law are likely to have a proportional effect on claim frequency and severity. For claim frequency, equations using the logistic transformation of claims per physician are also reported for the claims-made experience. This transformation is not used for the occurrence experience because of difficulty in constructing a meaningful measure of the physician exposure base, claims per physician, on a calendar year basis, for occurrence coverage.

Weighting

Each observation represents the average experience of the physicians insured by the participating companies in each state. Heteroscedasticity due to differences in the number of insureds is expected. For severity per paid claim, each state-year observation is therefore weighted by the number of paid claims in that state in that year. For frequency of claims filed or closed, the appropriate weight is some function of the number of physician-years in the exposure base. However, measuring exposure is not simple.

In the case of claims-made coverage, exposure in theory depends on the number of physicians written in the current policy year and the maturity of their claims-made coverage, because the policy covers claims filed in the policy year arising out of practice in previous years in which the physician was insured. In practice, because the average number of previous years of coverage was not known, physician exposure in the policy year is used as the weight, and the number of physicians insured in previous years is included as an explanatory variable. For occurrence coverage, the ideal weight is again a weighted average of exposure in several prior years. The available database consists primarily of the development of the pre-1977 St. Paul database occurrence coverage, with no new exposure in later calendar years, but it also

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5Ibid., for the rationale for this transformation.
includes a few other companies with growing exposure over time. Thus, there is no simple measure of exposure in each calendar year. Occurrence frequency equations are therefore unweighted, and exposure in current and previous years are included as explanatory variables.
Other ICJ Publications

R-2716-ICJ
The Law and Economics of Workers' Compensation
Policy Issues and Research Needs
L. Darling-Hammond and T. J. Knieper
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