THE FREQUENCY AND SEVERITY OF MEDICAL MALPRACTICE CLAIMS: NEW EVIDENCE

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I

INTRODUCTION

Physicians have been liable for medical malpractice since the eighteenth century in the United States, but malpractice claims were rare until recently. In the late 1960's 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-1970's. 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 (The St. Paul), which has been the leading writer of malpractice insurance for many years, reports a fifty-five 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 1970's, and this trend appears to have continued into the 1980's. The St. Paul reports that paid claim severity increased ninety-five 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

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1. *See e.g.*, P. DANZON, MEDICAL MALPRACTICE: THEORY, EVIDENCE, AND PUBLIC POLICY 97-117 (1985); S. LAW & S. POLAN, PAIN AND PROFIT: THE POLITICS OF MALPRACTICE 161-94 (1978). Most of the changes were enacted during 1975-1976, with effective dates shortly following enactment.

2. ST. PAUL FIRE & MARINE INS. CO., PHYSICIANS' AND SURGEONS' UPDATE: A SPECIAL REPORT 3 (1985).

3. *Id.*

$954,858 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 impact of the reforms enacted after the 1975 crisis.

To date, there have been only two published statistical analyses of the impact of tort reforms and other factors on malpractice claims. My earlier analysis of tort claims during the 1970's concluded that the increase in claims over time and the persistent diversity in experience among states could only partly be explained by such factors as the increase in the number and complexity of medical treatments and the concomitant increase in exposure to the risk of iatrogenic injury.[5] The pro-plaintiff trend in common law during the 1950's and 1960's 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, the specific characteristics of urban environments that generate higher frequency and severity could not be identified. 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. Limitations on the plaintiff's recovery (caps on awards) and mandatory offset of collateral benefits appeared, by 1978, to have significantly slowed the growth in claim severity in states that enacted such changes. However, none of the other changes, such as pre-trial screening panels or shorter statutes of limitations showed any impact 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 impact of the tort reforms enacted since 1975. In particular, any impact of shorter statutes of limitations on the "long tail" of claims would not have been 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 short-lived.

The only other broad-based statistical analysis of the effects of the 1970's tort reforms is Frank Sloan's analysis of malpractice insurance premiums paid by physicians from 1974 to 1978.[6] Any effects of the tort reforms were even less likely to be detected in this study than in mine because many of the reforms only became effective on or after January 1, 1976, and it is reasonable to anticipate a lag in their effect on claims filed and closed. One could expect an even longer lag in effect on premiums, since insurers would require some

4. JURY VERDICT RESEARCH, INC., INJURY VALUATION REPORTS, NO. 292, CURRENT AWARD TRENDS 18-19 (1985). This average is based on non-zero awards.

5. This study analyzed the determinants of trends in the frequency and severity of claims between 1970 and 1978. P. DANZON, THE FREQUENCY AND SEVERITY OF MEDICAL MALPRACTICE CLAIMS (The Rand Corp., Report No. R-2870-ICJ/HCFA, 1982).

6. Sloan, *State Responses to the Malpractice Insurance "Crisis" of the 1970's: An Empirical Assessment*, 9 J. HEALTH POL., POL'Y & L. 629 (1985).

experience under the new laws before adjusting premiums. Thus, it is not surprising that Sloan found little impact from the tort reforms. Specifically, he concluded that the only reforms having a statistically significant impact on premiums were screening panels, which appeared to reduce premiums, and statutes permitting binding arbitration of malpractice claims, which appeared to increase premiums.[7] The only significant non-legislative variable was the surgery rate.[8]

The Sloan and Danzon analyses appear to differ in their estimates of the impact of attorney availability. Sloan found that the number of attorneys per capita appeared to have a positive impact, with a rather low level of statistical significance dependent on the other variables included in the regressions. In contrast, Danzon found that attorney density did not increase malpractice claim costs. Sloan concludes that "[v]iewing the empirical evidence in its entirety, the notion that a ten percent increase in a state's lawyer/population ratio leads to almost a like percentage increase in premiums . . . is a distinct possibility."[9]

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 impact of attorney density on claims. The reason is that 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.[10]

Given the recent rise in claims and severity as well as the necessarily less-than-definitive nature of previous analyses, the time is ripe for additional information. The study reported here updates the earlier estimates of how tort reforms and other factors have affected trends in malpractice claim frequency and severity, using nationwide claims 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 impact. However, in practice several difficulties remain. First, the reforms have been subject to legal challenge in many states, and final rulings on their validity 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 it was not until October 1985 that the U.S. Supreme Court

7. *Id.* at 636, 639-40.
8. *Id.* at 641.
9. *Id.* at 643.
10. Although it is difficult to distinguish empirically the net impact of two highly correlated variables, urbanization appears to be a more important variable than attorney density because urbanization, if included alone, has higher explanatory power; when both variables are included, it alone is significant.

refused to hear the case.[11] To the extent that the disposition of malpractice claims over the last decade has been influenced by uncertainty as to the ultimate judicial outcomes, it may still be too early to estimate the full long-run effects of those reforms that have been upheld.

A second practical difficulty is the lack of a consistent and comprehensive data base. 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 from the market, while the market shares of new entrants—physician-owned mutuals, hospital "captives," and joint underwriting associations (JUA's)—have grown rapidly.[12] A substantial fraction of the market has also changed from an "occurrence" policy—to a "claims-made" policy. The occurrence policy covers all claims arising out of medical incidents occurring in the policy year, regardless of the date of claim filing. In contrast, the claims-made policy 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 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 significant market share for at least some fraction of the period under study makes collection of a comprehensive data base on claims experience very costly. The switch from occurrence to claims-made coverage further complicates the estimation of trends. It particularly complicates the estimation of trends in claim frequency, because the number of claims reported tends to be low in the early years of claims-made coverage, and rises as the policy "matures"—even when there is no change in the underlying frequency of claims filed.

The following report on malpractice claims begins with a brief description of the data base and methodological issues which are covered in more detail in the appendix. The next section analyzes the frequency of claims filed and claims closed with payment. Trends in malpractice claims severity are then analyzed. The concluding section summarizes the findings of the report.

II

DATA AND METHODS[13]

A. Data

My earlier analysis of malpractice claim trends in the 1970's drew on a database of virtually all claims closed from July 1975 through 1978, collected in a special survey by the National Association of Insurance Commissioners

11. Fein v. Permanente, 38 Cal. 3d 137, 695 P.2d 665, 211 Cal. Rptr. 368, *cert. denied*, — U.S. —, 106 S. Ct. 214 (1985).

12. *See* Posner, *Trends in Medical Malpractice Insurance, 1970-1985*, LAW & CONTEMP. PROBS., Spring 1986, at 37, 39-47.

13. For details, see appendix, *infra* at 80-84.

(NAIC).[14] 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 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. It remains possible that the data base may not be fully representative of the situation nationwide. Where relevant, this possibility is noted in the discussion of empirical findings below.[15]

B. Methodology

In attempting to measure the effects of various factors, including tort reform, on malpractice claim frequency and severity, statistical methods compare the average experience of physicians in different states and at different points in time over the ten years between 1975 and 1984. The units of observation are not individual claims, but individual states in each year. The variables requiring explanation are claim frequency per 100 physicians and average severity per paid claim in each state. To estimate the impact of a particular tort reform, one cannot simply compare the experience of state X before and after enactment of the reform, because other factors may also 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 a twenty percent higher claim severity in 1984 than did state B, which did not have a cap. One should not infer that A's cap had no effect, because claim severity might have been fifty percent higher in A than in B had the cap not been enacted. Thus, to estimate the net impact of the cap, or any other factor, a study must employ statistical methods that attempt to "control" for other factors.[16]

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 depends on the subset of the observations that have the

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14. The NAIC, an organization composed of all current heads of state insurance regulatory agencies, collected claim experience data from all companies with more than one million dollars in malpractice premium volume in any year since 1970. *See* NAT'L ASS'N OF INS. COMM'RS, MALPRACTICE CLAIMS: FINAL COMPILATION 3-6 (M. Sowka ed. 1980) [hereinafter cited as NAIC STUDY].

15. For example, data published by the American Medical Association show significant differences in claim frequency for 1983, by type of insurer. 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. SPECIAL TASK FORCE ON PROF. LIAB. AND INS., AM. MEDICAL ASS'N, PROFESSIONAL LIABILITY IN THE '80S, REPORT 1, at 10 (1984) (originally published as a supplement to AM. MED. NEWS, Oct. 1984). These differences could reflect such factors as differences in states in which the companies do business, difference in underwriting stringency, and simple differences in claim reporting practices or policy maturity.

16. On the use of statistical regression analysis, see generally Rubinfeld & Steiner, *Quantitative Methods in Antitrust Litigation,* 46 LAW & CONTEMP. PROBS., Autumn 1983, at 69, 88-104.

characteristic of interest. In this data base there are observations on over forty-five states in each of ten years, but the effective degrees of freedom are less than 450 (forty-five times ten), to the extent that experience in successive years in any state is not fully independent of experience in the same state in prior 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 damages at $250,000, another cap at $100,000, and so on—because the number of observations on each variant is too small. Therefore, for most reforms, the average impact for each general type of change has been estimated. There are two exceptions: separate estimates were made for the effects of mandatory and discretionary collateral source offset, and for different types of legislation regarding screening panels. In the appendix, Table A1 provides definitions, the mean values, and standard deviations of all variables.[17]

It is possible that post-reform trends in the various states are influenced not only by the reforms themselves, but also by the underlying factors in each state that led them to enact or not enact various reforms. For example, tort reforms were probably more likely to be adopted in states with a high underlying propensity for litigation. To control for this possibility, both two-stage least squares (TSLS) estimates and ordinary least squares (OLS) estimates are reported. The TSLS results are probably more reliable. Further detail appears in the appendix.[18]

III

DETERMINANTS OF MALPRACTICE CLAIM FREQUENCY

A. Theoretical Model of Claim Frequency

A rigorous statement of the theoretical model underlying the empirical estimates is given elsewhere.[19] 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 in turn may partially depend upon physicians' incentives to practice with care. Under a negligence rule of liability, in principle only those iatrogenic injuries that result from violation of the standard of due care are actionable. Thus, for any given *flow* rate of iatrogenic injuries per year, the number of potential malpractice claims depends on the standard of care 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 period of time within 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

17. *See infra* at 81.
18. *See infra* at 82-83.
19. P. DANZON, *supra* note 5, at 10-15.

plaintiffs' attorneys. The costs and benefits, 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 which 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 an insurance policy year. For practical reasons, the frequency experience under claims-made and occurrence coverages was analyzed separately. In the available data base, 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.

In order to measure the net impact of tort reforms, controlling for demographic factors, estimates of the impact of demographic variables on claim frequency are reported first. Table 1A estimates the impact for all claims filed, and Table 2A estimates it for those claims closed with payment. Estimates of the impact of tort reforms on claims filed and claims paid are reported in Tables 1B and 2B, respectively. Each of the law-reform variables was added separately to the basic equation that includes all demographic variables reported in Tables 1A and 2A.[20]

B. Findings

1. *Time Trends.* The St. Paul reports an average annual growth rate of claim frequency per physician of ten percent over the decade 1975-1984, with a slower rate in the first four years followed by some acceleration since 1979. Because this estimate is based on their combined occurrence and claims-made coverage, it is not seriously biased by the change in policy form.[21] By contrast, the growth rate estimated here from the claims-made sample alone (Table 1A) is fourteen percent per annum, 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 phenomenon 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 negative thirty-

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

21. Perfect lack of bias would require that the exposure base (number and specialty mix of physicians) remain 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.

TABLE 1A

MALPRACTICE CLAIM FREQUENCY: CLAIMS FILED 1975-1984

Explanatory Variable	Claims-Made Policies				Occurrence Policies	
	Claims per Physician $\text{Log}\left(\frac{F}{1-F}\right)$		Total Claims Filed (Log_e)		Total Claims Filed (Log_e)	
	OLS	TSLS	OLS	TSLS	OLS	TSLS
Intercept	−4.813**	−5.311**	−5.064**	−5.133**	8.412**	7.866*
	(2.69)	(2.43)	(2.81)	(2.34)	(2.08)	(1.89)
Time	.141**	.146**	.131**	.133**	−.369**	−.370**
	(7.32)	(6.19)	(6.90)	(5.83)	(7.50)	(7.54)
Exposure ($\log_e$)	—	—	1.039**	1.010**	.419**	.420**
			(30.40)	(23.71)	(9.47)	(9.50)
Prior exposure ($\log_e$)	.069**	.063**	.061**	.061**	.086**	.087**
	(6.44)	(4.76)	(4.88)	(4.09)	(3.21)	(3.26)
Surgical procedures per cap.	.011**	.011**	.012**	.012*	.006	.006
	(2.61)	(2.19)	(2.97)	(2.40)	(.86)	(.95)
% Urban	.500**	.285	.365**	.226	1.896**	1.851**
	(2.63)	(1.18)	(1.87)	(.94)	(5.11)	(4.87)
% Migrant	.013*	.010	.012*	.009	−.060**	−.060**
	(1.91)	(1.13)	(1.71)	(1.09)	(4.50)	(4.51)
Unemployment rate	.008	−.002	.012	−.001	−.092**	−.090**
	(.35)	(.08)	(.56)	(.02)	(2.16)	(2.11)
Income per cap. ($\log_e$)	−.070	−.048	−.079	−.077	−.597	−.579
	(.32)	(.18)	(.37)	(.30)	(1.26)	(.122)
Attorneys per cap.	−25.183	−10.14	−13.149	.414	4.738	−2.073
	(.50)	(.16)	(.27)	(.01)	(.20)	(.07)
California	.977**	1.239**	.865**	1.117**	5.284**	5.307**
	(7.78)	(6.92)	(7.03)	(6.34)	(9.85)	(9.89)
Illinois	—	—	—	—	.920	1.001
					(1.39)	(1.47)
Maryland	—	—	—	—	.373	.285
					(.61)	(.45)
New Jersey	—	—	—	—	2.353**	2.336**
					(3.39)	(3.37)
New York	—	—	—	—	1.207	1.207*
					(1.78)*	(1.68)
Arizona	—	—	—	—	.968	.906
					(1.47)	(1.36)
R^2	.672	.584	.901	.864	.693	.695
DW	1.46	1.34	1.47	1.34	1.08	1.08
ρ	.27	.33	.26	.33	.46	.46

* $p > .10$
** $p > .05$

Using a two-tailed test. See appendix for discussion of possible bias in t-statistics.

TABLE 1B

MALPRACTICE CLAIM FREQUENCY: CLAIMS FILED 1975-1984

Explanatory Variable	Claims-Made Policies: Claims per Physician Log $\left(\frac{F}{1-F}\right)$		Claims-Made Policies: Total Claims Filed (Log_e)		Occurrence Policies: Total Claims Filed (Log_e)	
	OLS	TSLS	OLS	TSLS	OLS	TSLS
Statute of limitations	−.008	.089**	.009	.082**	.052**	.060
	(.99)	(2.83)	(1.10)	(2.63)	(2.33)	(.52)
Collateral source offset	.137**	−.194*	.148*	−.155*	−.294*	−1.812**
	(2.09)	(1.79)	(2.28)	(1.37)	(1.81)	(2.90)
Mandatory collat. offset	.149**	−.013	.153**	(.037)	−.399**	−1.468**
	(2.03)	(.15)	(2.12)	(.40)	(2.16)	(2.60)
Panel	.102	−.050	.109*	−.009	−.074	.423
	(1.82)	(.17)	(1.94)	(.03)	(.48)	(.77)
Mandatory panel	.049**	−.045	.055**	−.013	.049	.077
	(1.97)	(.52)	(2.16)	(.15)	(.88)	(.35)
Fee limit	.102	−.110	.124*	−.030	−.270*	−.465
	(1.54)	(.71)	(.186)	(.18)	(1.64)	(1.05)
Arbitration	.221**	.534**	.226**	.594**	.103	−.239
	(3.51)	(3.28)	(3.66)	(3.57)	(.55)	(.43)

* $p > .10$
** $p > .05$
} Using a two-tailed test. See appendix for discussion of possible bias in t-statistics.

TABLE 2A

MALPRACTICE CLAIM FREQUENCY: CLAIMS PAID 1975-1984

	Claims-Made Policies				Occurrence Policies	
	Claims per Physician $\text{Log}\left(\frac{F}{1-F}\right)$		Total Claims Paid (Log_e)		Total Claims Paid (Log_e)	
	OLS	TSLS	OLS	TSLS	OLS	TSLS
Intercept	−9.045**	−9.365**	−8.168**	−8.225**	9.817**	9.721**
	(3.99)	(3.87)	(3.09)	(2.92)	(2.41)	(2.32)
Time	.213**	.217**	.206**	.208**	−.254**	−.254**
	(8.76)	(8.34)	(7.42)	(7.01)	(5.16)	(5.16)
Exposure ($\log_e$)	—	—	.964**	.940**	.265**	.265**
			(8.84)	(16.98)	(5.92)	(5.92)
Prior exposure ($\log_e$)	.130**	.126**	.164**	.164**	.104	.104
	(9.54)	(8.59)	(5.23)	(8.39)	(3.95)**	(3.94)**
Surgical procedures per cap.	.028**	.028**	.031**	.031**	.011	.011
	(5.43)	(5.13)	(5.23)	(4.85)	(1.75)*	(1.74)*
% Urban	−.647**	−.782**	−.501**	−.615**	2.714**	2.706**
	(2.71)	(2.93)	(1.75)	(1.97)	(7.25)	(7.03)
% Migrant	.005	.003	.009	.007	−.068**	−.068**
	(.58)	(.30)	(.89)	(.63)	(4.99)	(4.99)
Unemployment rate	−.014	−.021	−.020	−.031	−.097**	−.097**
	(.51)	(.68)	(.63)	(.88)	(2.27)	(2.26)
Income per cap. ($\log_e$)	.034	.048	−.109	−.108	−.923*	−.920*
	(.13)	(.17)	(.14)	(.33)	(1.93)	(1.92)
Attorneys per cap.	−22.020	−12.35	−10.054	1.120	−52.91**	−54.09
	(.35)	(.18)	(.14)	(.01)	(2.05)	(1.88)
California	1.154**	1.323**	1.217**	1.424**	3.582**	3.585**
	(7.26)	(6.69)	(6.76)	(6.22)	(6.64)	(6.63)
Illinois	—	—	—	—	−.990	−.974
					(1.48)	(1.42)
Maryland	—	—	—	—	−1.400**	−1.416
					(2.28)	(2.23)
New Jersey	—	—	—	—	1.602**	1.598**
					(2.29)	(2.28)
New York	—	—	—	—	.312	.299
					(.44)	(.41)
Arizona	—	—	—	—	.382	.371
					(.58)	(.55)
R^2	.74	.72	.85	.84	.53	.53
DW	1.30	1.22	1.60	1.49	1.05	1.05
ρ	.35	.39	.20	.26	.48	.48

* $p > .10$
** $p > .05$
} Using a two-tailed test. See appendix for discussion of possible bias in t-statistics.

TABLE 2B

MALPRACTICE CLAIM FREQUENCY: CLAIMS PAID 1975-1984

	Claims-Made Policies				Occurrence Policies	
	Paid Claims per Physician Log$\left(\frac{F}{1-F}\right)$		Total Claims Paid (Log_e)		Total Claims Paid (Log_e)	
	OLS	TSLS	OLS	TSLS	OLS	TSLS
Statute of limitations	−.006	.057*	−.006	.068*	.047**	.011
	(.62)	(1.65)	(.49)	(1.66)	(2.10)	(.09)
Collateral source offset	.113	−.038	.141	−.080	−.421**	−1.403**
	(1.36)	(.29)	(1.47)	(.49)	(2.60)	(2.25)
Mandatory collat. offset	.073	.106	.082	.081	−.331*	−.956*
	(.78)	(.97)	(.77)	(.59)	(1.75)	(1.75)
Panel	.045	.056	.035	.015	.017	.135
	(.63)	(.16)	(.43)	(.04)	(.11)	(.24)
Mandatory panel	.041	.087	.026	.092	.056	−.005
	(1.29)	(.80)	(.68)	(.73)	(.98)	(.02)
Fee limit	.117	.125	.125	.073	−.033*	−.408
	(1.40)	(.64)	(1.28)	(.29)	(1.82)	(.91)
Arbitration	.183**	.505**	.205**	.496**	.270	.093
	(2.27)	(2.46)	(2.25)	(2.10)	(1.42)	(.16)

* $p > .10$
** $p > .05$
} Using a two-tailed test. See appendix for discussion of possible bias in t-statistics.

seven percent per annum 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 estimated from the claims-made sample, twenty-one percent, appears to be substantially higher than the fourteen percent for all claims filed. It might seem 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 more willing to "buy out" of frivolous claims. No such inference is valid, however, because of the bias due to the maturing of the new claims-made policies. Because 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. This trend implies a greater *apparent* growth in paid claims than in total claims, until the insurer's book of business is fully "mature," meaning that the number of new policyholders is equal to the number of terminations each year.

2. *Claims vs. Injuries.* Earlier analyses indicated that, at most, one in ten incidents of malpractice gave rise to a claim in the mid-1970's.[22] 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 as opposed to an increase in the proportion of potential claims that are actually filed. Unfortunately, this comparison 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 result suggests that, for any given iatrogenic injury, the probability that a claim is filed is higher for surgical procedures. This hypothesis is plausible if iatrogenic origin is more obvious for adverse outcomes of surgical procedures than for other medical treatments, or if surgical mishaps tend to be 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.[23] The frequency of late-filed claims[24] is not significantly related to the frequency of surgical procedures, suggesting that most surgery-related claims are filed relatively

22. *See, e.g.*, P. DANZON, *supra* note 1, at 20-25.

23. The estimated elasticity of claims filed with respect to number of surgical procedures per 10,000 population is .01, and the elasticity of claims paid is .03, implying a higher conditional probability of payment, given that a claim has been filed, for claims involving surgical mishaps.

24. Late-filed claims are those in the occurrence sample that consist disproportionately of claims filed more than two years after the policy period in which the injury occurred. *See supra* text accompanying notes 11-12.

early. Again, this finding is not surprising, since surgical mishaps are more likely to be obvious.[25]

3. *Non-Tort Sanctions.* Since 1975 many states have strengthened their procedures for quality control through medical quality assurance boards, partly as a quid pro quo for tort reform. In theory, the amount 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.[26] This finding suggests that disciplinary procedures are tightened in states experiencing high claim frequency. If so, in addition to having a direct effect on physicians' incentives, malpractice liability may have an indirect effect on the quality of care by creating incentives to strengthen other monitoring mechanisms. Failure to find a negative relation between disciplinary activity and claim frequency does not imply such activity has no impact on the rate of iatrogenic injury. Because a substantial fraction of negligent iatrogenic injuries did not result in claims in the mid-1970's—as many as nine out of ten[27]—there could be a substantial reduction in injuries with little visible impact on claims.

4. *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, with qualifications. First, while urbanization is positively related to the total number of claims filed, urbanization is negatively related to the number of paid claims. This suggests that urban areas may have a disproportionate number of "frivolous" or nonmeritorious claims. Second, the fact that the apparent impact of urbanization 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 some major urban states may be underrepresented in this data base, particularly in the claims-made sample, and the state dummy variables may affect the urbanization coefficient.[28]

5. *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 patients is low. Moreover, plaintiffs are less likely to have first-party insurance coverage through their employers,

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25. In equations not reported here, claim frequency was found to be unrelated to the number of hospital admissions per capita, despite the fact that most malpractice claims arise out of hospital treatment.

26. In order to preserve degrees of freedom, this variable was not included in the final equations reported here.

27. *See* P. DANZON, *supra* note 1, at 20-25.

28. The state dummy variables in the tables indicate the states added to The St. Paul data base.

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-policy 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.

6. *The Doctor-Patient Relationship.* The rise in malpractice claims and the high incidence of claims in urban areas are often attributed to the erosion of traditional long-standing physician-patient relationships with a family physician, which, it is argued, constituted a psychological barrier to suit. This study 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 relatively mobile population probably also tends to have less information about the quality of individual physicians' services. Hence physicians' incentives for care may be lower, which is another reason to expect a positive relation between population turnover and claim frequency. Contrary to these hypotheses, population turnover has no systematic impact, except for late-filed claims, where the impact is negative. This may reflect a higher cost of filing suit after moving from the area where the injury occurred.

7. *The Elderly.* Consistent with my earlier analysis, these data show no relationship between claim frequency and the percentage of the population over sixty-five, so this variable is omitted from the reported equations. Hospital admission rates for the elderly are roughly twice as high as for persons under sixty-five and a 1974 California study[29] showed a higher rate of negligent injury per admission for the elderly. Therefore, 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 sixty-five. A plausible explanation is that the elderly have lower compensable damages.

8. *Number of Lawyers.* Also consistent with my earlier analysis, there is no evidence that a high density of lawyers per capita has any systematic impact on the frequency of claims filed, after controlling for other characteristics of areas with high lawyer density. Thus, that there is a high simple correlation between number of claims per physician and number of attorneys per capita 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 reflecting an independent effect of attorneys on litigation rates.

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29. CAL. MEDICAL ASS'N AND CAL. HOSP. ASS'N, REPORT ON THE MEDICAL INSURANCE FEASIBILITY STUDY (D. Mills ed. 1977), *discussed in* P. DANZON, *supra* note 1, at 19-25.

C. Tort Reforms

In evaluating the evidence on the impact of tort reforms, it must be emphasized that there is some uncertainty as to the true levels of statistical significance because of the limitations of the data.[30] Conventional tests of significance were applied, using unadjusted standard errors and a two-tailed test. In cases where theory yields a clear prediction as to the impact 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 expressed in logarithmic form, the percentage impact of a reform is obtained by an exponentiation of the coefficients reported.[31] In general, the OLS coefficients tend to be less negative than the TSLS coefficients. This result 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 impact of reforms. The subsequent discussion is therefore based on the TSLS estimates.

The following results only show how much a particular reform affected experience relative to what that 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 frequency fell by Y percent but that it was Y percent lower than it otherwise would 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 above, since some of the reforms were under challenge and therefore might not have been enforced in all cases, the estimates reported here may understate the full long-run impact of a reform once it has been declared constitutional.[32]

1. *Statute of Limitations.* States that have enacted shorter statutes of limitations have experienced some reduction in claim frequency. Reducing the statute of limitations for adults by one year reduces total claim frequency by eight percent and frequency of paid claims by six to seven percent.[33] This estimate is the *average* effect of a one-year reduction, measured at the sample mean of roughly five years. The mean effect cannot be extrapolated

30. Estimates of standard errors may be downward biased and significance levels may be upward biased due to serial correlation of residuals. *See appendix, infra* at text accompanying notes 50-54. 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 where 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.

31. For example, if the coefficient in the table is $-.15$, the percentage change in the dependent variable is $(1\text{-}e^{-.15})$ or -14%.

32. Note that if severity is increasing over time, a given *percentage* differential implies an increasing absolute dollar difference.

33. The occurrence sample shows a similar estimate (five to six percent for all claims and paid claims) but only the OLS coefficients are statistically significant.

indefinitely.[34] For example, since 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 eight percent whereas reducing a statute from four years to three years may reduce claims by more than eight percent.

2. *Collateral Benefits.* The only other reforms that show any evidence of reducing claim frequency are laws that permit or mandate reducing awards by the amount of insurance coverage from other sources. Collateral source offset is estimated to reduce claim frequency by fourteen percent. The impact appears to be greater on claims filed late, but the magnitude of the effect on late-filed claims cannot be stated with precision.[35] Laws that simply admit evidence of collateral coverage seem to be as effective in reducing claim frequency as laws which mandate offset.

3. *Panels.* Screening panels do not appear to have had any systematic effect on either the total number of claims filed or paid. This finding is consistent with evidence from case studies of the operation of panels in individual states. Although the impact of screening panels varies substantially among states depending on the specific design of the panels, the evidence suggests that screening panels may even encourage filings by reducing the cost to the plaintiff of obtaining expert testimony.[36]

4. *Arbitration.* In contrast to screening panels, the evidence suggests that laws permitting binding arbitration increased the frequency of claims filed and claims paid.[37] The impact 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 than for the claims-made sample. The former sample consists of disproportionate numbers of late-filed claims that typically involve more serious injuries. The latter includes a disproportionate number of minor claims. This finding is entirely consistent with one objective of arbitration proponents, which is to provide a less expensive recourse for small claims that might be barred by the costs of the tort system.

34. 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.

35. The TSLS coefficient implies a 77% reduction in late-filed claims, which seems implausibly large.

36. *See* P. DANZON, *supra* note 1, at 198-202, and sources cited therein.

37. The coefficients imply as much as a 60% increase, which seems implausibly large, since 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

A. Theoretical Model of Claim Severity

Claim severity is measured by averaging the indemnity paid on claims closed with payment, including verdicts and out-of-court settlements. For some companies, reported indemnity includes loss adjustment expense, averaging roughly thirty percent of total indemnity.[38]

It is important to note that trends and interstate differences in observed severity (the average amount actually paid in all cases) may understate real differences in "potential" severity (the expected payment at verdict or settlement for a specific class of injury), which can be called "jury generosity." The reason is that an increase in jury generosity not only raises actual payments on claims that would otherwise have been filed, but also encourages the filing of claims that would not be brought at lower levels of jury generosity. Specifically, higher generosity promotes the filing of more small claims and more claims with a low probability of winning. As the mix of paid claims becomes more heavily weighted with small claims, observed average severity per claim may fall even though each individual claim receives a larger award than it otherwise would. To measure interstate differences in jury generosity would require data on individual claims in order to control for "case mix" of claims. It cannot be measured by data available in this study indicating statewide average severity.

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. In principle, rules of compensable damages determine the valuation of these damages by the courts, subject to interpretation by judge and jury. Whether changes in these rules have any impact 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 compensation from other sources), and provisions for periodic payment of future damages. Ceilings or 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 most likely to affect very large potential awards, which are a small fraction of all claims,

38. Based on information received in a telephone communication with The St. Paul (Summer 1985). The company-specific dummy variables should control for differences in reporting practice.

these few cases account for a very large fraction of dollars paid. Therefore, they can substantially influence average severity.

Reducing awards is also one objective of arbitration proponents, since eliminating the role of the supposedly overgenerous jury is one of the major differences between arbitration and tort procedure. 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 indirectly affect observed severity to the extent that they screen out "frivolous" claims that might otherwise have been settled with a small payment. By so doing, panels may change the mix of claims paid and raise the average amount actually received.

TABLE 3A

SEVERITY PER PAID CLAIM (LOG_e) 1975-1984
CLAIMS-MADE AND OCCURRENCE POLICIES

	OLS	TSLS
Intercept	−1.082**	−1.031*
	(2.02)	(1.90)
CIP (log_e)	1.940**	1.918**
	(25.18)	(24.41)
% Urban	.679**	.766**
	(5.56)	(5.81)
% Over 65	−2.185*	−1.238
	(1.77)	(.92)
Surgeons per MD	2.654**	2.563**
	(3.32)	(3.18)
Attorneys per capita	16.370	5.789
	(.78)	(.26)
Illinois	.520**	.632**
	(5.38)	(5.50)
Maryland	.251*	.202*
	(2.00)	(1.56)
New Jersey	−.262	−.257**
	(4.50)	(4.38)
New York	.268**	.315**
	(3.70)	(4.07)
Arizona	−.504**	−.446**
	(4.07)	(3.46)
California CM	.253**	.387**
	(2.83)	(3.34)
California OC	−.353**	−.219**
	(5.79)	(2.31)
R^2	.627	.624[a]
DW	1.387	1.382[a]
ρ	.306	.308[a]

* p≥.10 ⎫ Using a two-tailed test. See appendix for discussion of possible bias in
** p≥.05 ⎭ t-statistics.

[a] From equation with statute of limitations added. Values for equations including other laws were very similar.

TABLE 3B

SEVERITY PER PAID CLAIM (LOG_e) 1975-1984
Claims-Made and Occurrence Policies

	OLS	TSLS
Collateral source offset	−.102*	−.198*
	(1.72)	(1.84)
Mandatory collat. offset	−.171**	−.114**
	(2.68)	(2.05)
Cap	−.161**	−.263**
	(2.83)	(2.04)
Panel	−.005	.185
	(.10)	(1.70)
Mandatory Panel	−.015	.054
	(.89)	(1.51)
Arbitration	−.126**	−.212**
	(2.22)	(2.13)
Fee Limit	−.027	−.036
	(.49)	(.34)

In order to analyze claim severity, the claims-made and occurrence data bases were merged because there are no strong theoretical or practical reasons for distinguishing them. In fact, combining them eliminates potential biases that would result from separate analysis. Table 3A reports estimates of the impact of demographic factors on average severity per paid claim. Table 3B reports OLS and TSLS estimates of the impact of tort reforms, obtained by adding each law separately to the basic equation in Table 3A.

B. Findings

1. *Time Trend.* Malpractice claim severity has risen roughly twice as fast as the Consumer Price Index (CPI).[39] This trend can only be explained partially by the fact that medical care prices have risen more rapidly than consumer prices in general, because medical expenses account for less than one-quarter of reported economic loss in malpractice cases closed with payment.[40] 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 in all cases or whether the largest awards have grown proportionately faster, as other studies of jury verdicts have found.[41]

2. *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 seven percent increase in malpractice

39. The elasticity of severity with respect to the CPI is 1.9.
40. NAIC STUDY, *supra* note 14, at 51.
41. M. SHANLEY & M. PETERSON, COMPARATIVE JUSTICE: CIVIL JURY VERDICTS IN SAN FRANCISCO AND COOK COUNTIES, 1959-1980 (The Rand Corp., Report No. R-3006-ICJ, 1983).

claim severity. However, the precise magnitude of the urban effect may be influenced by the particular mix of states in this data base. Moreover, company dummies occur disproportionately in large urban states. Therefore, to the extent that the company dummy variables capture state effects as well as company effects, the measured urban effect may understate the true urban effect.

Claim severity also is significantly higher in states with a high ratio of surgical specialists relative to medical specialists. This variable may capture the general effect of more complex medical practice, as well as the likelihood that surgical mishaps involve more serious injuries and are easier to prove.[42]

3. *The Elderly.* As expected, average severity is lower in states with a relatively large elderly population in view of the lower compensable damages for the elderly. However, the level of statistical significance is low. The population over sixty-five accounts for only eleven percent of the total population on average and the evidence above suggests that it does not have a disproportionately high claim frequency. Therefore, even a small impact on overall average claim severity would imply that the average claim for persons over sixty-five is substantially less than a claim for persons under sixty-five. A substantial difference is not surprising in view of the low wage loss of the elderly. Low potential compensable damages presumably contributes to the low propensity to sue.

4. *Number of Lawyers.* There is no evidence that the number of lawyers per capita has any impact on claim severity. This is consistent with the findings of my earlier study and with simple economic theory. A high density of lawyers could depress the price of legal services, so that both the plaintiff and defendant would use more legal services. However, there is no reason to expect any net impact on the outcome from increased legal effort by both sides. It is more plausible that lower-priced legal services would encourage higher claims frequency, because the cost of filings could drop, but, as noted above, there was no evidence of this phenomenon.

C. Tort Reforms

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

1. *Caps on Awards.* The average impact of the various statutes to cap all or part of the plaintiff's recovery has been to reduce average severity by twenty-three percent.[43] This observed average impact obviously masks great differences among cases. The majority of cases would be unaffected by most

42. A high probability of proving negligence tends to increase the amount paid in cases settled out of court, which represent the great majority of paid claims. P. DANZON & L. LILLARD, THE RESOLUTION OF MEDICAL MALPRACTICE CLAIMS: RESEARCH RESULTS AND POLICY IMPLICATIONS vi (The Rand Corp., Report No. R-2793-ICJ, 1982).

43. *See supra* notes 31-32 and accompanying text.

of the caps. Therefore, the impact 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 fifty percent of dollars are paid on five percent of cases)[44] caps that severely reduce the few very large dollar awards can have a significant impact on the average and on the total payout.

2. *Collateral Source Offset.* Laws providing for collateral source offset appear to reduce awards by between eleven and eighteen percent. Coefficients are similar for laws mandating offset and laws permitting offset at the discretion of the court or jury, but significance levels are slightly higher for mandatory offset. This differs from the earlier analysis based on claims closed from 1975 to 1978,[45] which found a larger impact (fifty percent reduction) for mandatory collateral source offset and no effect for discretionary offset. The lower estimate obtained here is more consistent with a rough estimate based on estimates of the fraction of awards required to cover reported economic loss and the fraction of economic loss that might be covered by other sources of insurance. The latter approach yields a rough conclusion that mandatory offset of all collateral benefits might reduce average severity by thirty to forty percent.[46] Since some statutes apply only to a subset of collateral sources and offset is sometimes discretionary, the estimate here of an eleven to eighteen percent reduction resulting from the implementation of these statutes is quite plausible.

3. *Arbitration.* States that have enacted special statutes permitting voluntary binding arbitration have an average claim severity roughly twenty percent lower than other states. Taken at face value, this suggests that the impact of arbitration on cases actually arbitrated could be even greater, since cases arbitrated are probably still only a small fraction of total claims in most states with arbitration statutes. On the other hand, 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 that are filed, as is suggested by the findings in Table 2B. With respect to the impact of arbitration on total claim costs, when taken at face value the coefficients imply that the twenty percent decrease in claim severity is more than offset by the increase in frequency of claims filed and claims paid. Thus, overall arbitration probably increases total claims 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.

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44. P. DANZON & L. LILLARD, *supra* note 42, at xv.
45. P. DANZON, *supra* note 5, at 30.
46. P. DANZON, *supra* note 1, at 169-70.

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

V

CONCLUSION

The tort reforms enacted since the mid-1970's malpractice "crisis" affected the frequency and severity of malpractice claims over the decade from 1975 to 1984 in a manner broadly consistent with economic theory and with previous evidence. Although claim frequency and severity have continued to rise despite reforms, this trend does not indicate 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 reduces claim frequency by eight percent. The effect would presumably be greater for a reduction from, say, four to three years than from ten to nine years. (Percentage changes are the average differential in a single year, relative to what the situation would have been without enactment of the reform.).[47]

Statutes permitting or mandating the offset of collateral benefits have apparently reduced malpractice claim severity by eleven to eighteen percent and claim frequency by fourteen 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, thereby reducing incentives to file.

Caps on awards have reduced severity by twenty-three percent. This percentage represents the average impact of the various forms of cap, over the period between 1975 and 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 increasing allowances for uncapped components of the award.

Arbitration statutes apparently increased claim frequency, but reduced overall average severity. Disaggregated data would be 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 of more claimants.

None of the other reforms analyzed, including screening panels and limits on contingent fees, appears to have had any systematic impact on claim frequency or severity.

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47. *See supra* note 33.

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 more than two 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.

An overall evaluation of the merits of the various tort reforms from a public policy perspective is beyond the scope of this paper and has been done elsewhere.[48] However, it is worth noting that on average, severity has increased at almost twice the rate of inflation of consumer prices over the last decade. Thus, in the absence of further statutory controls, 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 paper is the impact—actual and potential—of tort reforms on malpractice insurance premiums. The analysis here of impact on claim frequency and severity should not automatically be translated into an effect on premiums for several reasons. First, the net potential impact on premiums also depends on litigation expenses and changes in the timing of disbursement of loss reserves, and hence investment income. Second, reforms that reduce the uncertainty in estimating malpractice claim costs—namely, 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. One can expect this result because of the reduction in insurers' risk. Perhaps more importantly, 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.

48. *See, e.g.,* P. DANZON, *supra* note 1.

APPENDIX

A. Data

My 1982 analysis of claim frequency and severity used data from the National Association of Insurance Commissioners (NAIC) survey of claims closed between 1975 and 1978.[49] 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 the period from 1975 through 1984. Many of the companies did not respond and others could not readily provide their data on a calendar and policy year basis. Theoretical considerations suggest analyzing claim frequency and severity by calendar year rather than insurance policy year, since 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 prior 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 prior coverage with the same company for physicians currently insured. Thus, data on exposure (number of insured physician years) by policy year and on claims filed, claims closed and paid, by calendar year and corresponding policy year, were requested.

The data base used here includes the experience of The St. Paul, which insures over 55,000 physicians in over forty 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 were represented for at least some years, but the 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 new occurrence policies were written. Claims against the earlier occurrence coverage continued to be filed and closed throughout the subsequent decade. Because the data base on occurrence coverage was heavily weighted by the "run off" on old policies, the analysis of claim frequency was performed separately for claims-made and occurrence coverages.

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49. NAIC STUDY, *supra* note 14.

TABLE A1

VARIABLE DEFINITIONS, MEANS, AND STANDARD DEVIATIONS

Variable	Mean	Standard Deviation	Definition
Time	5.71	2.847	Linear time trend, 1975 = 1
Surgical procedures per capita	.840	.092	Surgical procedures per 10,000 population
% Urban	.566	.243	Fraction of state population in urban areas
% Migrant	.212	.058	Fraction of state population changed county within 5 years
Unemployment rate	6.999	1.963	Unemployment rate
Income per cap ($\log_e$)	8.922	.271	Personal income, mean per capita ($\log_e$)
Attorneys per capita	.002	.003	Attorneys in active practice/population
Collateral source offset	.344	.469	= 1 if statute permitting or mandating collateral source offset
Panel	.419	.484	= 1 if statute establishing screening panel
Mandatory panel	1.057	1.33	= 1 if voluntary; 2 if mandatory, findings not admissable; 3 if mandatory, findings admissable
Mandatory collat. offset	.240	.484	= 1 if mandatory offset of any collateral coverages
Fee limit	.337	.467	= 1 if limit on plaintiff attorney fee
Arbitration	.234	.419	= 1 if statute permitting binding arbitration
CPI ($\log_e$)	5.434	.227	Consumer price index
% Over 65	.108	.018	Fraction of state population > 65
Surgeons per MD	.312	.024	Surgical specialists/total physicians in patient care
Cap	.204	.395	= 1 if any limit on plaintiff's recovery
Claims per physician (CM policies)	.165	.219	Claims filed/earned exposure
Paid claims per physician (CM policies)	.056	.114	Claims closed with payment/earned exposure
Severity ($\log_e$)	10.349	1.079	Total indemnity/paid claims

Data on tort reforms were compiled from several surveys of state statutes.[50] Original statutes were checked where there were discrepancies between the surveys. Most of the laws were measured as binary variables, taking the value of one in each year in which the law was in effect (even if under challenge), or zero if no change had been enacted or if a law had been

50. The main surveys are P. DANZON, *supra* note 5; AM. MEDICAL ASS'N, STATE HEALTH LEGISLATION REPORTS (1973-present); unpublished survey by Alliance of American Insurers (1985); Am. Ins. Ass'n, Medical Malpractice Insurance Reports (draft, Nov. 1981).

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 was measured in years for filing for adults. In states with a discovery rule with no outer limit, the statute of limitations was arbitrarily assigned a value of ten.[51] Table A1 lists all variables together with means and standard deviations.

B. Methodology

1. *System Issues.* Since the data consist of ten annual cross sections of over forty states, 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, dummy variables for individual companies other than The St. Paul were included. 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 which show little variation over the decade. Inclusion of measures of the frequency and severity of claims closed in 1975 to capture these state effects was attempted, but these variables were not statistically 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 (ρ), 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.[52] 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 $\sqrt{1 + .4(9)} = 4.6 = 2.2$. For variables that change within states over the sample period, the correction is smaller and is given by (assuming constant ρ) $\sqrt{1 - \rho + (\rho T)\,(\sigma^2_{jx}/\sigma^2_x)}$, where σ^2_x is the total variance in X, σ^2_{jx} is the between-states variance in X and $\sigma^2_{jx} = \sigma^2_x - \sigma^2_{ix}$, where σ^2_{ix} 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

51. A discovery rule tolls the running of the statute of limitations until the injury has been, or with due diligence should have been, discovered.

52. I am indebted to Emmett Keeler and Will Manning for this derivation.

changed once and some twice, if a reform was overturned. Second, ρ 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.[53] 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. An adjusted t-statistic of 1.31 would be significant at $p = .1$ by a one-tailed test, which is the appropriate test 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 the null hypothesis for most variables 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 that is not attributable to other measured variables, ordinary least squares (OLS) estimates of the impact of those reforms may be biased. For laws expected to reduce claim frequency or severity, the bias from ignoring simultaneity leans toward finding no effect even when the true effect is negative. Both OLS and two-stage least squares (TSLS) estimates were 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.

2. *Functional Form.* Logarithmic transformation of the dependent variables are 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 were also reported for the claims-made experience.[54] This transformation was 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.

2. *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 was anticipated. For severity per paid claim, each state-year observation was 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

53. P. DANZON, *supra* note 5, at 21.
54. The rationale for this transformation is given in P. DANZON, *supra* note 5, at 17.

theory depends on the number of physicians written in the current policy year and the maturity of their claims-made coverage, since the policy covers claims filed in the policy year arising out of practice in prior years in which the physician was insured. In practice, since the average number of prior 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 occurrency coverage, the ideal weight is again a weighted average of exposure in several prior years. The available data base consists primarily of the development of the pre-1977 St. Paul occurrence coverage, with no new exposure in later calendar years, but also 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 were therefore unweighted and exposure in current and prior years were included as explanatory variables.