CHANGING THE COMMON LAW: EFFECTS OF THE DECLINE OF CHARITABLE IMMUNITY

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This paper examines the effects of the abrogation of one common-law doctrine—"charitable immunity"—on the operation and viability of one charitable institution—the hospital—in 24 states over the period 1951-1971. Four models of the effects of abrogation are considered. To test these models, I conceptualize the termination of immunity as an experimental treatment in an interrupted time-series design. I conclude that abrogation led to changes of quite varied magnitude in different states. Apart from abrogation, the onset of Medicare and Medicaid, and changes in the economic environment had considerable effects on hospital costs. In states abrogating after the beginning of federal intervention in medical care, judicial doctrine had no discernible impact on the costs of hospitals.

I. INTRODUCTION

We know a good deal about the effects of judicial decisions on abortion, the rights of the accused, school prayer, racial discrimination, and similar issues of great public visibility (Wasby, 1970; Baum, 1978; Johnson, 1979a; 1979b). Most research has focused on doctrinal change in public, as opposed to private, law and on the Supreme Court of the United States rather than on state high courts (but see Harris, 1979; 1980; Canon and Baum, 1981; Kagan, Cartwright, Friedman, and Wheeler, 1977; 1978). Yet, apart from three recent studies (Croyle, 1979; 1980; Canon and Jaros, 1979), we have little systematic knowledge of the impact of common law produced by the state supreme courts (see also Calabresi, 1970; McLauchlan, 1978). Social scientists have "failed to see what nearly all the real participants in court-oriented activity are

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looking at constantly, the vast central areas of court-applied law—tort, agency, contract, real property, inheritance—that shape in a very fundamental way the control, flow, and employment of the basic resources of our society" (Shapiro, 1970: 45; see also Shapiro, 1972; Keeton, 1969; and White, 1979).

This paper examines the effect of the abrogation of one common-law doctrine—"charitable immunity"—on the operation and viability of one charitable institution—the hospital—that it protected for many years. Specifically it focuses on the impact of the decline of charitable immunity on the price of hospitalization in states that moved from protection to liability during the period 1951 to 1971. During much of the first half of the twentieth century, the doctrine of charitable immunity enjoyed considerable currency. Since World War II, however, it has fallen on hard times:[1] abrogated in some states, modified in others, and remaining in only a few. Variation in the state judicial response to this doctrine makes it an especially appropriate subject for a study of the consequences of changes in the common law.

II. THE DOCTRINE OF CHARITABLE IMMUNITY[2]

"There are a number of classes of defendants upon whom the law, for various reasons of policy, has in the past conferred immunity from tort liability to a greater or less extent" (Prosser, 1971: 970). A defendant so immunized "avoids liability in tort under all circumstances. [Immunity] is conferred, not because of the particular facts, but because of status or position of the favored defendant; and it does not deny the tort, but the resulting liability" (Prosser, 1971: 970). The doctrine of charitable immunity protects charitable organizations and enterprises; persons injured by a negligent employee of such an organization cannot collect damages. This immunity covers charities, especially charitable hospitals, but also religious, educational, and similar eleemosynary institutions. In most states retaining the doctrine, immunity from damage suits is complete.[3]

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[1] Thirty-one states, through either the legislature or the state supreme court, have removed the immunity of charitable organizations.

[2] I have, in the early segment of this section, relied very heavily upon Professor Prosser's treatise on the law of torts (1971). Keeton and Keeton (1977), Prosser and Wade (1971), and Green *et al.* (1977) proved quite helpful as well.

[3] But certain jurisdictions provided only partial or very limited immunity. "Thus the charity is held liable where the negligence is that of an officer . . . in selecting the charity's servants, or is in the course of raising money, or the

The doctrine of charitable immunity has its roots in the common law of England. In 1846, the House of Lords adopted the proposition that charitable institutions should not be held liable for the torts of employees (*Feofees of Heriot's Hospital* v. *Ross* [1508]). That principle traveled across the ocean to the United States in 1871. For the first time in an American jurisdiction, the Supreme Judicial Court of Massachusetts applied the doctrine of charitable immunity in that year in *McDonald* v. *Massachusetts General Hospital* (1871). Meanwhile, the English—apparently unbeknownst to the American courts—had already repudiated the doctrine (*Mersey Docks Trustees* v. *Gibbs* [1866]). The doctrine of charitable immunity spread very rapidly among the various American state jurisdictions (see, for instance, *Perry* v. *House of Refuge* [1885]). In adopting and retaining the doctrine, judges have taken into account a number of considerations (see Prosser, 1971: 993). A number of judges, for example, have argued that without such immunity, fear of claims will discourage potential donors and so stifle charitable institutions (e.g., *Vermillion* v. *Women's College of Due West* [1916]). More generally, judges have articulated a concern that liability would lead to large judgments that might destroy charitable organizations.

Judge Wiley Rutledge of the United States Court of Appeals for the District of Columbia led the retreat from charitable immunity in *Georgetown College* v. *Hughes* (1942). Rutledge's opinion considered various arguments for and against charitable immunity. For our purposes it is important to note only that he dismissed the claim that abrogation would have measurable effects on charitable institutions. Since 1942, the high courts of many states have either rejected or abrogated charitable immunity.

Lawyers, judges, and commentators have speculated for years about the impact of abrogation on the economic vitality of charitable institutions generally, and of hospitals specifically. Unfortunately, that speculation has had only the most tenuous empirical basis. Thus Canon and Jaros' recent investigation of the "relationship between abrogation of the charitable immunity doctrine and increased hospital room rates" (1979: 977) comes as a welcome empirical assessment. They used room rate as the dependent variable because it is a good summary indicator of the costs of hospitals. If abrogation resulted in more lawsuits and thus more expenses, surely the

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management of property, or the charity is found to have created a nuisance" (Prosser, 1971: 995).

cost of a bed would be among the first services affected. Canon and Jaros also controlled for economic variables such as inflation by adjusting each figure for per capita income—the closest, on the state level, one can come to the Consumer Price Index—for each year and for all states in the 1947-1974 series. They predicted that "increased costs of insurance protection were in fact passed on to patients in the form of higher room rates" (1979: 976).

Canon and Jaros used two methods to test this prediction. First, they performed an analysis of covariance, comparing in each year the average cost of a bed in states that retained the doctrine. This "static analysis" showed that "in 17 out of the 26 years measured" abrogation did not increase the average cost of a bed (Canon and Jaros, 1979: 978-980), leading to the conclusion that "the static analysis does not lend support to our thesis" (1979: 980). Second, Canon and Jaros pursued a more dynamic analysis by incorporating a two-year lag into the analysis of variance. They found that "in 13 of [the years in which at least one state abrogated immunity] there was a greater increase in hospital room rates in abrogating states than in stable states over the ensuing two-year period" (Canon and Jaros, 1979: 980). On the basis of this analysis they concluded that "the abrogation of charitable immunity has made a visible contribution to increases in hospital room rates" (1979: 981).

Canon and Jaros confronted an important problem and presented results that are quite plausible. It is indeed important to have comparisons between abrogators and non-abrogators. Yet, for several reasons, I find that report unpersuasive. First, the statistical model seems ill-adapted to the purpose of determining whether a doctrinal change has increased hospital costs. Doctrinal change did not occur cross-sectionally (i.e., across states); it happened cross-temporally (i.e., within states). To monitor the effects of a change of policy within a jurisdiction, one needs to use some variant of longitudinal analysis. Canon and Jaros mix together very disparate states in the belief that idiosyncracies or state-specific differences will even out. But in doing so they discard much important information. In *which* states did abrogation increase costs? In *which* did abrogation have no effect? If abrogation did have significant effects, were these long-term or short-term in duration? Did economic conditions have effects on costs over and above the impact of the decline of immunity?

Second, because of their research design, Canon and Jaros could not account and adjust for serial correlation in hospital costs.[4] On the basis of their report alone, one cannot tell whether these data contain substantial degrees of serial correlation; but virtually every time series of financial statistics is laced with this malady. Presence of serial correlation results in inflated tests of statistical significance; Canon and Jaros may thus have reported exaggerated effects of abrogation (Kmenta, 1971; Pindyck and Rubinfeld, 1976; Johnston, 1972).

Third, apart from inflation, one of the most pervasive stimuli of increased costs has come from the introduction of Medicare and Medicaid. The cost of hospital care and hospitalization has skyrocketed since these programs began in 1965, and it is quite possible that the increases Canon and Jaros attributed to the abrogation of charitable immunity are instead merely the result of governmental intervention in the health business. Some five years before the introduction of Medicare and Medicaid the general rise in prices of medical care slowed substantially (Horowitz and Rice, 1967). But "[t]he advent of Medicare and Medicaid tended to accelerate already evident upward trends in use of service, factor inputs, expenses, revenue, income, and rates of return . . ." (Donabedian, 1976: 236). Donabedian suggests that "the institution and administration of third-party payments . . . adds a cost which has to be reflected in higher fees, premiums, or taxes, in lower wages, or in some combination of these . . ." (1976: 259).

To obtain a more accurate estimate of the effects of abrogation, I have (1) selected a simple interrupted time-series design, a choice that permits an assessment of the impact of a single policy intervention on a series of observations (see Campbell, 1969; Campbell and Stanley, 1963; Campbell and Cook, 1979); (2) specified the onset of Medicare and Medicaid as possible influences on the rise of hospital prices; and (3) made adjustments for the presence of serial correlation of errors.

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[4] The use of regression and related forms of analysis in both time-series and cross-sectional studies assumes that "errors corresponding to different observations are uncorrelated. . . . When the error terms from different observations are correlated, we say that the error process is serially correlated or autocorrelated." Normally, the "presence of serial correlation will not affect the unbiasedness or consistency of the ordinary least-squares regression estimators, but it does tend to affect their efficiency" (Pindyck and Rubinfeld, 1976: 18, 109). Thus one tends to reject the null hypothesis when in fact one should not do so. Substantively, serial correlation indicates an error of specification in the model; a variable or variables not included or perhaps improperly included threatens the integrity of the results.

III. FOUR MODELS OF EFFECTS

Changes in public policy can be conceptualized as natural experiments, or quasi-experiments (Campbell, 1969; see, for examples, Caldeira and McCrone, 1982; Albritton, 1979; Lewis-Beck, 1979; McCrone and Hardy, 1978; Aaronson *et al.*, 1978; Ross, 1975; Glass, 1968). The states are the laboratories; and doctrinal changes may or may not have had an impact on the behavior of organizations or individuals. One can derive precise statistical estimates of effect, and one can test numerous alternative models of impact (Lewis-Beck, 1979: 1128).

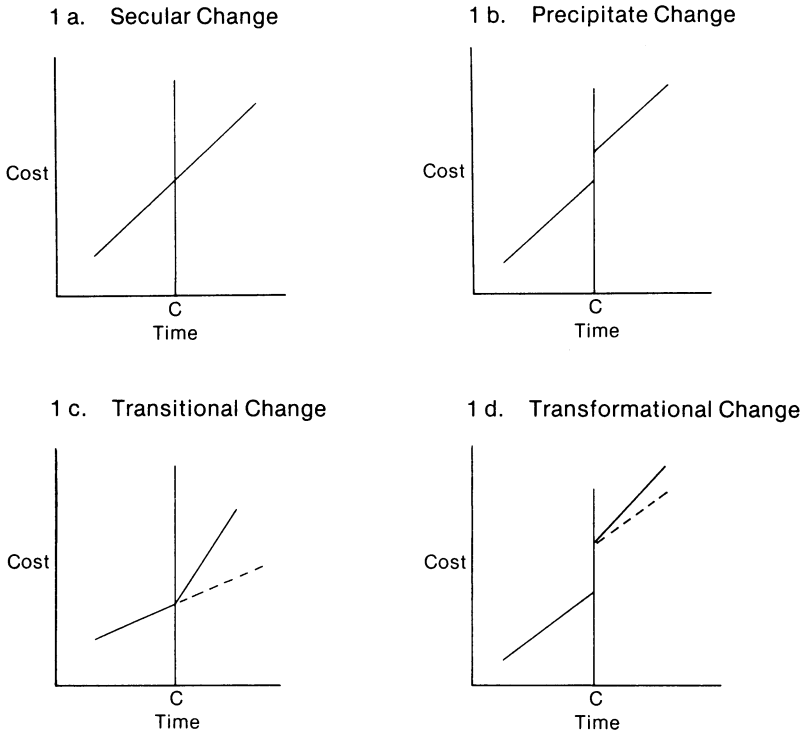
The models presented and tested here have been constructed in large part from obiter dicta in the opinions of state supreme courts that have abrogated charitable immunity. These conceptions of outcomes are not, of course, the only possible ones (see Campbell and Stanley, 1963: 38-39), but I think they rank among the most important.[5]

Secular Change

It is entirely possible, of course, that abrogation did *not* increase the cost of hospital beds, a pattern depicted in Figure 1a. There the x-axis represents time, before and after the doctrinal change; C, the change from immunity to liability; and the y-axis, the average cost of a hospital bed for a particular state in a particular year. This model reflects Judge Rutledge's statement that no "statistical evidence has been presented to show that the mortality or crippling of charities has been greater in states which impose full or partial liability than where complete or substantially full immunity is given. . . . Charities seem to survive and increase in both with little apparent heed to whether they are liable for torts . . ." (*Georgetown College* v. *Hughes* [1942]). Figure 1a is based on the assumption that the cost of hospitalization was growing in a more or less linear fashion—quite apart from any effect of abrogation (see, e.g., Justice Musmanno's opinion, *Flagiello* v. *Pennsylvania Hospital* [1965]). To argue, then, that the abrogation of charitable immunity caused or facilitated an

[5] In a classic study, Campbell and Stanley (1963: 5) assess some twelve internal and external threats to the validity of quasi-experimental designs. The threat due to "history"—i.e., "specific events occurring between the first and second measurement in addition to the experimental variable"—looms as the most serious in the current research. Having taken into account the implementation of Medicare and Medicaid and the economic context, I am reasonably sure that I have ruled out the greatest threats.

Figure 1. Four Possible Effects of the Abrogation of Charitable Immunity



increase in the cost of a hospital bed would be fallacious. That pattern represents instead, long-term, persistent change, and no abrupt change as a result of abrogation. Increases in the cost of hospitalization might well have resulted from urbanization, industrialization, increases in population, improvements in medical technology, and expansion of services.

I have labeled this first model "Secular Change" in cost. Its equation is as follows:

$$\text{PRICE}_t = \beta_0 + \beta_1 \text{BEFORE}_t + \beta_2 \text{CHANGE}_t + \beta_3 \text{AFTER}_t + \beta_4 \text{INCOME}_t + \beta_5 \text{MEDICARE}_t + \epsilon_t$$

where PRICE_t is the average price of a hospital bed for a particular state in a year; BEFORE_t is a counter for the trend of time prior to abrogation; AFTER_t is a counter for the trend from the year of abrogation until the present; CHANGE_t is a variable that takes on the value of zero before the change and one afterward; INCOME_t represents fluctuations in the economic conditions of the state; MEDICARE_t is a variable that takes on the value of one in 1966, the first year of the program, and zero in all other years; and ϵ_t is a term for errors. The

coefficients β_0, β_1, β_2, β_3, β_4, and, β_5 consist of parameters to be estimated. Under conditions of secular change, I expect that $\beta_1 > 0$, $\beta_2 = 0$, $\beta_3 > 0$, $\beta_4 > 0$, and $\beta_5 > 0$—significant trends before and after abrogation, no effect from the policy shift itself, and significant effects from Medicare and from economic fluctuations. Furthermore, because this model predicts no significant alteration in the continuous movement toward the higher cost of a hospital bed, $\beta_1 = \beta_3$—the coefficients for trends before and after abrogation should not be significantly different. For states that abrogated in the early 1950s, a time of relative economic stability, we may well find that an alternative version of the first model is more suitable: no trends before the change, no effect from the change, and a significant trend upward after the shift. Such a pattern indicates that prior to abrogation, the cost of a bed was exceptionally stable over time, so that one would observe no upward trend. After abrogation, one might find significant upward movement.

Precipitate Change

One might, with equal plausibility, also argue that the abrogation of charitable immunity produced a very abrupt increase in the overall cost of a hospital bed, but did not, on a permanent basis, accelerate the rate of increase of cost. Figure 1b represents that situation. The obvious costs of liability for the torts of employees—such as large settlements or insurance premiums—might have been translated into immediate increases in the costs of hospitals. Increase in legal expenses might have forced hospitals either to increase the price of care or to reduce the quality or quantity of services. But, as Canon and Jaros claim, "reduction of services to patients is unlikely, particularly in a period of expanded medical technology and rising public expectations about medical care . . ." (1979: 976). One would expect an initial burst of lawsuits. After that, if the officials of hospitals changed the conditions or if courts, judges, and juries were not favorable to claims, one might find costs returning to a "normal" rate of growth. That temporary burst of increased expenses would not, however, alter the basic pace of the growth of the average cost of a hospital bed. The cost of hospitals, under these assumptions, should return after a time to the pre-abrogation rate of cost increases. This indicates that $\beta_1 > 0$, $\beta_2 > 0$, $\beta_3 > 0$, $\beta_4 > 0$, and $\beta_5 > 0$—that trends exist prior to and after abrogation, that abrogation has an immediate and positive effect on cost, and that Medicare and economic fluctuations increased cost. But, in addition, I expect that $\beta_1 =$

β_3—that trends before and after abrogation do not differ in a statistically significant fashion.

Transitional Change

Change in the average cost of a hospital bed will not occur instantly. A fundamental shift in policy such as abrogation could produce higher cost, but might do so at a much more measured pace than the second model implies. One would then observe few if any direct and immediate increases in cost because of abrogation. The rate of increase would accelerate gradually as the result of fundamental alterations in the structure of hospitals and medical finance wrought by the disappearance of immunities (see *Collopy* v. *Newark Eye and Ear Infirmary* [1958]). Figure 1c shows such a pattern of transitional change, i.e., no short-term change but a new degree of movement toward greater cost. Its essence is a steepening in the slope of average cost per bed as a result of abrogation. Thus, from the third model of change, one can predict that trends exist prior to and after abrogation and thus that abrogation had no immediate impact on cost—$\beta_1 > 0$, $\beta_2 = 0$, $\beta_3 > 0$, $\beta_4 > 0$, and $\beta_5 > 0$. I do, however, expect that the slope of the trend after abrogation will be steeper than before the demise of charitable immunity—$\beta_1 < \beta_3$.

Transformational Change

Opponents of abrogation have often argued that it would have quite dramatic effects on the cost of hospital care. "These institutions will have to recognize that [without immunity] they will either have to increase their rates or solicit larger charitable subscriptions in order to provide funds with which to pay the heavy insurance premiums which will be made necessary thereby" (*Pierce* v. *Yakima Valley Memorial Hospital Association* [1953]). In this view, abrogation could have yielded both short-term and long-term increases in the cost of a hospital bed. So, as in the second model, doctrinal change might have generated a short-term increase in the cost of a bed, perhaps because of litigation or increases in insurance; but, in addition, as in the third model, fundamental changes in social, political, and economic conditions that abrogation caused, or coincided with, result in long-term increases. There is, then, an increase in the rate of increase as well as in the level of cost, as shown in Figure 1d. From this model of transformational change, one can deduce that trends exist before and after abrogation and that the demise of

immunity does indeed increase the cost of a bed, at least in the short run—$\beta_1 > 0$, $\beta_2 > 0$, $\beta_3 > 0$, $\beta_5 > 0$. Further on the basis of this fourth model, I predict that the rate of increase in the cost of a bed increases after the change; i.e., the slope becomes steeper after abrogation—$\beta_1 < \beta_3$.

IV. EVIDENCE, MEASUREMENT, AND ESTIMATION

To estimate the effects of the abrogation of charitable immunity, I draw upon three primary sources of data. First, I use the average annual price of a hospital bed for each state, from 1947 to 1974, as a measure of the cost of hospitalization (American Hospital Association, 1950-1975; Canon and Jaros, 1979).[6] Second, Canon and Jaros have reported the status of the doctrine of charitable immunity in each of the American states (1979: 973), and I have used that information in the formulation of models.[7] The sample includes all states that have abrogated, either partially or completely, charitable immunity from 1951 through 1971—some 24 states. States that abrogated before 1951 or after 1971 are excluded because these states do not produce a sufficient number of observations either before or after the change to permit proper analysis. Third, I have relied upon annual per capita income as a control for fluctuations in the economic conditions of the state (U.S. Department of Commerce, 1975). I would have preferred a more precise measure of annual statewide medical care or, at the least, an indicator of inflation. But neither of these measures exists. I do believe that per capita income and rates of inflation track reasonably well together within states over time.

To estimate the models set out in Section III, I have performed ordinary least-squares regression on the data for each of the 24 states.[8] In series of the sort under examination

[6] These numbers are available for almost all years and for all states. In the few cases where it proved necessary, I have made linear interpolations in order to generate estimates of the missing data.

[7] For states in which more than one change in the common law occurred, I have assessed the first one, working on the assumption that the first one would have the greatest and most detectable impact.

[8] In assessing the coefficients, I have applied two criteria. First, I have required consistency in the signs of coefficients. If one has any pretensions of testing theoretical notions, a careful scrutiny of, and clear expectations about, the sign of coefficients becomes necessary. For example, what if estimates indicate that the abrogation of charitable immunity, contrary to predictions, in fact decreased the cost of a bed? That finding seems so implausible that I would discount it—expecting, instead, that such a result is an indication of a misspecified model. Second, I have used conventional tests of significance at the .05 level as a convenient means of evaluating the significance of estimated parameters.

here, considerable serial correlation is often present. The coefficients that appear in Section V have therefore been corrected for autocorrelation.[9]

V. FINDINGS

Table 1 presents estimates of the effects of abrogation, of economic fluctuations, and of Medicare and Medicaid on the average cost of a hospital bed for the 24 states in my sample.

For all but one of these 24 states, the institution of Medicare and Medicaid in 1966 outweighed other factors. The price of a bed increased far more because of those programs than because of either abrogation or economic circumstances. These results are all the more impressive if one considers the quite different social, political, and economic structures in these states. This pattern is not at all obvious, for neither the judges who wrote the opinions in the late 1960s and early 1970s, nor commentators such as Prosser, Keeton and Keeton, and Canon and Jaros, indicated this relationship. Quite clearly, Medicaid and Medicare revolutionized the cost of medical care. Of course, longitudinal regression normally produces a good statistical fit; but these fits are uniformly excellent. These equations explain over 90 percent of the variance in all of the cases; all of the coefficients have the correct signs; and the correction has reduced the remaining serial correlation to trivial levels. There are, then, good reasons to believe that these models constitute a correct specification of the forces bearing upon the rising prices of hospital beds.

For 12 states, the abrogation of charitable immunity had a positive and long-term, but no short-term, effect. For these states, a model of transitional change best describes the growth of the price of hospitalization. Figure 2a, which traces the price of a bed over time for the State of Washington, exemplifies this pattern of transitional change.[10] Prices went up, steadily but very slowly, until the Washington Supreme Court abrogated the doctrine in 1953; after that, the slope did not move upward immediately, but rather very gradually. Prior to abrogation, the price of a bed increased from $10.00 in 1948 to $15.00 in 1953.

[9] I have made the adjustments based upon the magnitude of rho, and estimate of the degree of serial correlation obtained with the Cochrane-Orcutt procedure (Cochrane and Orcutt, 1949; Johnston, 1972: 262-265; Kmenta, 1971; Pindyck and Rubinfeld, 1976). Then the Durbin-Watson statistic provides a basis for forming a judgment as to whether that correction has succeeded.

[10] To avoid redundancy, I shall present figures for only a few of the 24 states; the chosen jurisdictions exemplify the basic patterns of effects.

Table 1. Effects of Abrogation: Interrupted Time Series, Corrected for Autocorrelation

$$PRICE_t = \alpha_0 + \alpha_1\ BEFORE_t + \alpha_2\ ABROGATION_t + \alpha_3\ AFTER_t + \alpha_4\ INCOME_t + \alpha_5\ MEDICARE_t + \epsilon_t$$

	α_0	α_1	α_2	α_3	α_4	α_5	R^2	D.W.	N	ρ
Arizona (1951)	6.832	−.137	−.628	1.479*	.003	2.282*	.993	1.90	27	.417
	(1.097)	(−.957)	(−.310)	(7.613)	(1.565)	(4.601)				
California (1951)	3.675	1.478*	−2.521*	1.398*	.003*	3.778*	.997	1.85	27	−.017
	(1.493)	(2.460)	(−1.880)	(12.788)	(3.620)	(19.315)				
Delaware (1951)	10.924	.068	−1.432	1.504*	.001	5.453*	.988	1.62	27	.463
	(.931)	(.026)	(−.415)	(4.832)	(.064)	(7.315)				
Mississippi (1951)	6.422*	.263	−.114	.612*	.000	2.081*	.994	1.51	27	.320
	(2.654)	(.471)	(−.120)	(7.306)	(.248)	(9.001)				
Washington (1953)	4.966*	1.104*	−1.474	1.707*	.002	2.610*	.997	1.79	27	.095
	(2.177)	(3.018)	(−1.128)	(12.564)	(1.295)	(8.208)				
Florida (1953)	4.616*	.403	−.535	.788*	.004*	2.679*	.997	1.72	27	.004
	(2.405)	(1.479)	(−.515)	(6.781)	(2.551)	(7.210)				
Kansas (1954)	3.767*	.563*	−.021	.725*	.001*	2.518*	.998	2.18	27	−.748
	(5.279)	(6.320)	(−.046)	(12.849)	(2.754)	(19.783)				
Ohio (1956)	7.281*	.838*	−.064	1.176*	.001	3.498*	.996	1.87	27	−.036
	(4.118)	(4.470)	(−.055)	(7.610)	(.615)	(11.088)				
Nevada (1957)	−2.977	.591*	3.770*	.797*	.006*	2.231*	.994	2.13	26	−.339
	(−1.332)	(2.982)	(2.811)	(3.857)	(4.880)	(6.664)				
New York (1957)	5.188*	.923*	.773	1.644*	.002*	5.559*	.998	1.67	26	.220
	(2.310)	(4.166)	(.496)	(6.412)	(1.882)	(13.731)				
Michigan (1960)	4.841*	1.138*	.558	.733*	.002	4.855*	.997	1.84	27	−.145
	(3.147)	(10.685)	(.407)	(2.215)	(1.245)	(11.592)				

*Statistically significant at the .05 level. T-ratios appear in parentheses under relevant coefficients.

Table 1 continued.

	α_0	α_1	α_2	α_3	α_4	α_5	R^2	D.W.	N	ρ
Kentucky (1961)	3.793*	.852*	−.965	1.292*	.001	1.949*	.995	1.89	27	−.217
	(3.560)	(9.837)	(−.796)	(3.750)	(.859)	(4.279)				
Wisconsin (1961)	3.477*	.882*	−200	1.627*	.001	2.428*	.997	1.613	27	.396
	(1.959)	(6.528)	(−.133)	(3.326)	(.851)	(4.036)				
Montana (1961)	4.250*	.899*	−1.166	1.577*	.001	2.241*	.994	2.03	27	−.192
	(2.386)	(10.838)	(−.730)	(3.534)	(.591)	(3.963)				
Oregon (1963)	5.831*	1.090*	−1.729	3.695*	.001	.502	.996	2.06	27	−.010
	(3.006)	(9.111)	(−.605)	(2.406)	(.764)	(.301)				
Idaho (1966)	5.030*	1.017*	−1.813	.604	.001	3.755*	.985	1.82	27	.199
	(3.038)	(9.398)	(−1.057)	(.399)	(.085)	(2.568)				
Maryland (1966)	1.554	1.212*	−1.403	2.633	.002	4.901*	.996	1.82	27	.265
	(.525)	(5.424)	(−.575)	(1.163)	(.836)	(2.238)				
Nebraska (1966)	6.425*	.939*	.431	−.069	−.001	4.355*	.995	1.51	27	.255
	(5.372)	(10.817)	(.294)	(−.051)	(−1.759)	(3.289)				
Texas (1966)	3.608*	.531*	1.199	−.441	.002	3.146*	.992	1.89	27	−.404
	(2.228)	(5.460)	(.738)	(.328)	(1.523)	(2.175)				
Connecticut (1967)	11.685*	1.232*	4.708*	−.672	−.001	7.142*	.998	1.59	27	.330
	(6.166)	(9.013)	(2.635)	(−.664)	(−.892)	(7.959)				
North Carolina (1967)	5.306*	.585*	2.507*	−1.573*	.001*	4.080*	.999	1.89	27	−.194
	(9.957)	(12.084)	(3.448)	(−5.397)	(1.614)	(12.600)				
Indiana (1968)	5.726*	.924*	.468	−.266	.000	3.784*	.997	1.65	27	−.046
	(4.346)	(10.348)	(.336)	(−.604)	(.201)	(9.900)				
Massachusetts (1969)	4.903*	1.591*	4.234*	−.891	.000	5.807*	.999	2.43	26	.637
	(2.820)	(11.467)	(3.238)	(−1.187)	(.351)	(13.588)				
Missouri (1969)	3.260*	.994*	−.249	.663	.001	3.159*	.997	1.96	27	−.110
	(2.388)	(10.048)	(−.186)	(1.301)	(.576)	(11.554)				

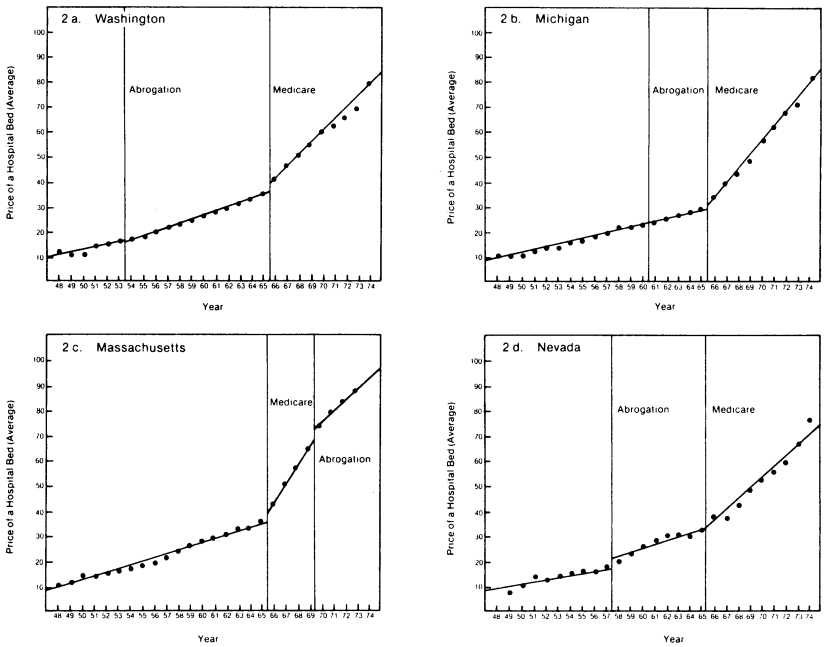
One year after abrogation, the price had increased only $1.00—evidence that no short-term, immediate change had occurred. Between 1953 and 1965, prior to Medicare and Medicaid, the price rose from $17.00 to $35.00—quite apart from inflation. Even after abrogation the slope became only marginally steeper. Before abrogation, the price rose about $1.10 a year; after the change, it climbed approximately $1.70 per year.

In one-third of the states, abrogation had no effect at all on the cost of a bed. For these eight states, neither long-term nor short-term effects emerged, and a model of secular change best describes the movement of price over time. For six of these states there was no increase as a result of abrogation, and no significant trend after the change in judicial policy. There is a significant trend after abrogation in California and Michigan; but the slope actually became less steep. The price in California increased at a rate of $1.47 a year before 1951; after abrogation, that price increased only $1.39 a year. The drop in the State of Michigan registered even more strongly. Figure 2b charts the growth of the average price of a hospital bed for Michigan. It shows, quite clearly, that abrogation had negligible effects on the costs of hospitals in Michigan; graphics for the remaining seven states are very similar.

Decline of the doctrine of charitable immunity increased the price in the short run but not over the long term in three states—Connecticut, North Carolina, and Massachusetts. For one-eighth of the abrogating states, then, the movement in costs over time matches best with a model of precipitate change. Figure 2c presents the average cost of a bed for the State of Massachusetts from 1947 to 1974. Abrogation occurred in 1971, and obviously had no long-term effect after an initial increase of $4.00 the first year. Indeed, it is apparent that the beginning of Medicare and Medicaid in 1966 accounts for the increased rate of increase in price. Even the increase in the first year may have been a product of the upward pressure on prices from Medicare and Medicaid.

Of the states that abrogated charitable immunity, in only one—Nevada—did that change induce *both* long-term and short-term effects. Thus we have rather strong evidence contrary to the fears of judges, lawyers, and hospital administrators. Figure 2d presents the average cost of a hospital bed for 1948-1974 for Nevada. That figure shows that the price does jump up immediately and does increase at a steeper rate after abrogation. Before abrogation, the price

Figure 2. Average Price of a Hospital Bed



increase in Nevada averaged $0.59 each year; in the year after, it increased $3.77; and, then it increased at a rate of $0.79 a year.

Economic fluctuations, as measured by per capita income, had a significant effect on the average price of a bed in 10 of the 24 states—over and above change due to abrogation or Medicare and Medicaid. Per capita income roughly captures such phenomena as inflation, pressures from wage and labor laws, more sophisticated technologies, and so on—forces, in other words, that make it expensive to do business. Though the coefficients are small, the impact of economic conditions on the price of a bed is quite dramatic. For instance, in Florida, for each increase of $1.00 in per capita income, the price of a hospital bed increased less than a penny. Yet, after World War II, income in Florida and elsewhere skyrocketed; and these increases no doubt contributed to rises in hospital costs. For example, an increase in individual income of $100.00 would have produced an increase of almost $1.00 in the cost of a bed in Florida.

In states that abrogated charitable immunity before the mid-1960s, its disappearance in a number of instances had an immediate effect on the price of a bed; in states that did so

later, abolition resulted in little additional expense. That contrast between pre- and post-Medicare/Medicaid abrogators is perhaps the most revealing pattern of all. Of the nine states that moved from partial or full immunity after Medicare and Medicaid began, none manifested long-term increases as a result of abrogation; and only three—Connecticut, Massachusetts, and North Carolina—demonstrated even a short-term increase. Of the 15 that discarded the doctrine of charitable immunity before 1966, one state suffered a short-term increase, and 13 had a long-term increase in price. Clearly, the earlier a state abrogated charitable immunity, the more likely it was that the reform would increase the price of a hospital bed—and, by inference, the costs of hospitals. Just as clearly, the institution of national health insurance programs wrought changes in the hospital business so fundamental as to overshadow the effects of other, less radical shifts in public policy. Abrogation of charitable immunity may well have induced as much or even more increased expense in the 1960s and 1970s than it had in the 1950s, but the effects of Medicare and Medicaid would have obscured that increase.

This set of results seems plausible; after all, most agree that Medicare and Medicaid rather drastically changed the practice and finance of American hospitals. Yet some might, quite reasonably, argue that my models are misspecified, and that abrogation contributed more to hospital cost increases than appears to be the case. A more parsimonious model, these critics might claim, should exclude such global influences as Medicare and Medicaid. What happens if one does not take Medicare and Medicaid into account?

Table 2 presents an estimate of the effects of abrogation and economic conditions alone on the price of a hospital bed; it excludes Medicare and Medicaid. These results, at first glance, seem to support the claim of numerous judges, lawyers, and commentators that abrogation results in large price increases, which, at least by inference, are products of the higher premiums that hospitals must pay to offset abrogation and which are then passed on to patients. In some 15 of the abrogating states, the change in policy yielded significant long-term increases in price. Yet there are a number of anomalies here. First, it is suspicious that all instances of apparent increases in price occur in states that abrogated immunity after 1960. This suggests that perhaps increases from federal medical policies, not included in these equations, make the increase in slope due to abrogation higher than it is in the real

Table 2. Effects of Abrogation: Interrupted Time Series, Corrected for Autocorrelation, Estimated Excluding Medicare/Medicaid

$$PRICE_t = \alpha_0 + \alpha_1 BEFORE_t + \alpha_2 ABROGATION_t + \alpha_3 AFTER_t + \alpha_4 INCOME_t + \epsilon_t$$

	α_0	α_1	α_2	α_3	α_4	R^2	D.W.	N	ρ
Arizona (1951)	−31.906	4.043	1.313	2.634*	.004*	.989	2.26	27	.83
	(−1.392)	(1.306)	(.473)	(4.887)	(2.504)				
California (1951)	−154.660*	10.640*	1.970	6.780*	.003*	.993	2.01	27	.93
	(−3.831)	(3.957)	(.803)	(6.377)	(2.949)				
Delaware (1951)	−114.540*	12.480*	3.230	5.750*	.001	.980	1.70	27	.87
	(−2.580)	(2.445)	(.658)	(5.500)	(.621)				
Mississippi (1951)	−40.285*	4.466*	.933	2.339*	.001	.988	1.10	27	.88
	(−2.631)	(2.758)	(.607)	(5.668)	(.766)				
Washington (1953)	−70.060*	6.850*	−.097	4.300*	.001	.990	1.73	27	.90
	(−2.285)	(2.615)	(−.042)	(5.717)	(.977)				
Florida (1953)	−4.474	−.097	−3.160*	.482*	.013*	.988	2.01	27	.09
	(−1.597)	(−.189)	(−1.706)	(2.373)	(10.864)				
Kansas (1954)	−77.880*	6.480*	.076	3.800*	−.000	.990	1.13	27	.90
	(−3.060)	(3.286)	(.042)	(6.770)	(−.062)				
Ohio (1956)	−59.119	4.215	−2.480	4.286*	.001	.988	1.35	27	.93
	(−1.186)	(1.459)	(−1.002)	(3.898)	(1.004)				
Nevada (1957)	−60.407	4.280	−.195	4.154*	.002*	.988	1.59	26	.92
	(−1.159)	(1.438)	(−.073)	(3.854)	(1.780)				
New York (1957)	−95.670*	6.080*	−1.250	6.190*	.003*	.992	.99	26	.92
	(−1.677)	(1.904)	(−.432)	(5.082)	(2.077)				
Michigan (1960)	−54.918	3.913*	−3.230	5.600*	.001	.990	1.36	27	.93
	(−1.212)	(1.771)	(−1.273)	(5.834)	(.085)				

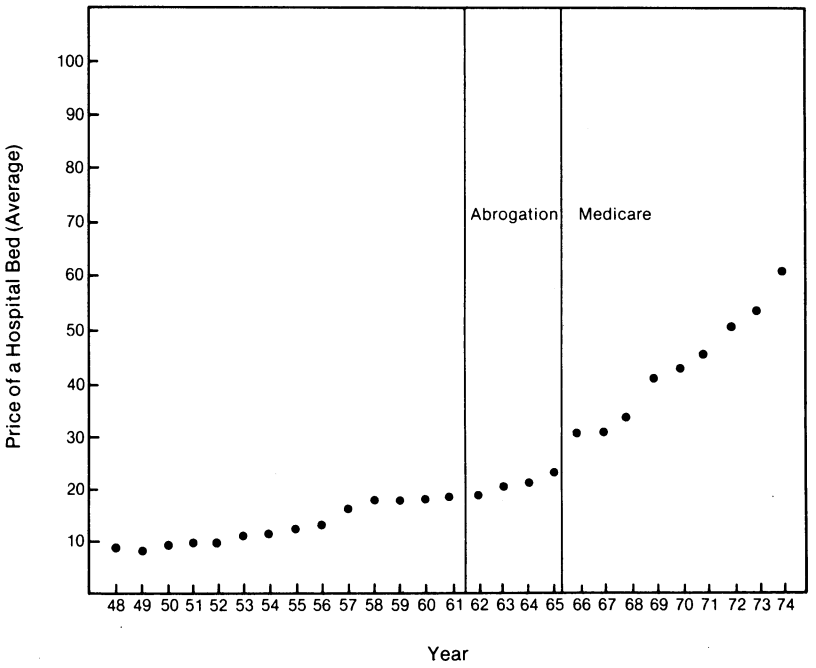
*Statistically significant at the .05 level. T-ratios appear in parentheses under relevant coefficients.

Table 2 continued.

	α_0	α_1	α_2	α_3	α_4	R^2	D.W.	N	ρ
Kentucky (1961)	2.132 (1.539)	.743* (6.233)	−4.870* (−4.125)	2.479* (7.978)	.003* (2.201)	.990	1.89	27	−.04
Wisconsin (1961)	2.658 (1.100)	.735* (4.001)	−3.917* (−2.570)	3.319* (10.119)	.002 (1.518)	.994	1.59	27	.46
Montana (1961)	2.200 (.993)	.871* (7.619)	−6.163* (−4.600)	3.178* (11.162)	.002 (1.487)	.989	1.93	27	−.10
Oregon (1963)	5.679* (3.126)	1.081* (9.577)	−2.509* (−2.351)	4.141* (12.006)	.001 (.914)	.996	2.06	27	−.03
Idaho (1966)	3.947* (2.364)	1.024* (9.537)	1.783 (1.291)	4.068* (7.969)	.001 (.568)	.993	2.02	27	.01
Maryland (1966)	−.273 (−.087)	1.275* (5.213)	1.234 (.539)	7.457* (9.139)	.002 (1.092)	.994	1.79	27	.33
Nebraska (1966)	4.629* (2.203)	1.108* (7.438)	.683 (.421)	4.622* (11.846)	−.001 (−1.561)	.992	1.49	27	.58
Texas (1966)	2.000 (1.270)	.484* (4.754)	4.209* (4.501)	2.290* (5.441)	.003* (2.415)	.991	2.07	27	−.46
Connecticut (1967)	−16.925 (−.948)	2.874* (3.347)	4.000* (1.685)	6.672* (7.315)	−.000 (−.055)	.994	1.40	27	.91
North Carolina (1967)	−12.342 (−1.351)	1.722* (3.725)	2.059 (1.492)	3.190* (5.610)	.001 (.529)	.993	1.30	27	.90
Indiana (1968)	−16.195 (−1.588)	2.282* (4.295)	.339 (.160)	4.180* (4.737)	.000 (.146)	.991	1.25	27	.89
Massachusetts (1969)	−60.089* (−3.264)	4.979* (6.338)	4.384* (1.720)	5.745* (4.778)	.000 (.813)	.995	.909	26	.92
Missouri (1968)	−22.733* (−2.073)	2.741* (5.292)	1.053 (.511)	4.974* (5.632)	−.001 (−.812)	.991	1.23	27	.89

world. Second, the signs and the sizes of the intercepts for all but a few states are not at all plausible. It is, for instance, quite unlikely that the price of a bed would have decreased $110.00 each year in Delaware had neither abrogation nor an increase in per capita income occurred. These intercepts, I believe, indicate a misspecified model. Third, even after adjustments for the presence of serial correlation, nontrivial amounts of correlation between successive errors remain. These high levels of autocorrelation result in unacceptable Durbin-Watson statistics.[11] Substantively, of course, the problem of autocorrelation is that it often implies the existence of a nonexistent relationship. Fourth, for a number of the states, the coefficient that monitors the short-term effects of abrogation has a significant negative sign. If read literally, that would mean that in these states, abrogation—although it may have increased price over the longer haul—actually decreased the average price of a bed for a short time. This is highly implausible. The most optimistic expectation one might reasonably entertain is that abrogation had no effect on price.

Figure 3. Average Price of a Hospital Bed, 1948-1974, Montana



[11] The Durbin-Watson statistic varies from 0 to 4. A statistic below 2 implies the presence of positive serial correlation; above 2, negative. If there is no serial correlation, the Durbin-Watson statistic will be close to 2.

Figure 3 shows the average annual price of a bed for Montana from 1947 to 1974, and demonstrates the inadequacy of the specification in Table 2. For Montana, this specification yields a reasonable set of estimates for the intercept and trends before and after abrogation. Unfortunately, we observe a negative coefficient for the immediate effect of abrogation. From Figure 3, it is apparent that in excluding Medicare and Medicaid, one forces the regression line of the trend after abrogation through a number of earlier observations (1962-1965) that clearly are not on the same plane as later ones. This, in turn, forces down the line for abrogation—producing a negative and significant estimate. Thus after visual inspection, it is obvious that specification of Medicare and Medicaid in the model will clear up these results—which is exactly what happens in Table 1. The message of Table 1, taken together with Figure 3, should be apparent: any model of the effects of abrogation on hospital prices that excludes Medicare and Medicaid is not properly specified.

We have seen so far that states abrogating charitable immunity before the onset of Medicare and Medicaid more often registered increases in price as a result of that doctrinal change. Quite aside from the difference between pre- and post-1960s abrogators, do states in which judicial abolition of immunity produced an increase in price differ, in systematic ways, from states in which abrogation made little or no difference? Perhaps certain kinds of states—less industrialized, less urbanized, less populous, and with more "professional" judiciaries—offer more favorable circumstances for effective judicial policymaking.[12] There is some very slight evidence

[12] For instance, it could well be that in particular states—urbanized, industrialized, affluent—because the hospital business is so institutionalized and has so little "slack" in resources, judge-made policies cannot make a dent in price. In a state such as New York, which has strong unions, high taxes, and complex regulations, how much of an impact can a change in the common law make? It is plausible that in rural, less industrialized states, law, politics, and business are more "wide open" or so unsettled that changes in the common law could have real effects. For instance, in a state such as Mississippi—which has no unions to speak of, few legislative restrictions, and a low cost of doing business—a judge-made change in public policy may loom large indeed. Furthermore, the judiciaries of particular states have greater institutional influence and prestige than do certain others (for the idea of "judicial professionalism," see Glick and Vines, 1973). The more "professional" judiciaries may have greater capacity to make their doctrinal changes effective.

To measure the effectiveness of abrogation in each of these states, I have used the difference between the slopes (i.e., unstandardized regression coefficients) of the increase in the price of a bed before and after—an indicator of the increment in price due to the change in policy. Where the slope did not change after abrogation, a state received zero. Where the slope increased, for instance, $0.56, a state received a score of .56. To operationalize industrialization, population, affluence, and judicial professionalism, I have borrowed from Hofferbert (1974) and Glick and Vines (1973).

that in less populous (r = -0.17), less industrialized (r = -0.17), more professionalized (r = 0.23) jurisdictions, abrogation of charitable immunity had a greater impact. But these relationships—already exceedingly weak—disappear when one controls for the beginning of Medicare and Medicaid.[13] In a recent study of the spread of new policies in the law of torts, Canon and Baum (1981) find little evidence of consistently "innovative" state supreme courts and report weak relationships between scores on innovation and characteristics of the states and judiciaries. This occurs, Canon and Baum argue, because courts cannot take the initiative; the dependence of state supreme courts upon litigants' demands leads to a large element of idiosyncracy in the diffusion of innovations in the law of torts. Perhaps, then, one should not expect systematic differences in the ways in which various state supreme courts affect prices, since judges have no control over the timing of decisions.

These cross-sectional results lend strong support to the conclusions I have drawn from the longitudinal analyses. If one wishes to predict whether the abrogation of charitable immunity had an effect in a state, the most important fact to know would be when—before or after 1966—its supreme court made the doctrinal change.

VI. SUMMARY

Lawyers, judges, and social scientists who have written about the impact of the decline of charitable immunity wish, in essence, to discover the specific effects of abrogation within states over time—not across jurisdictions in one year. To know about the cross-sectional effects of this change would, of course, be a happy bonus, but study across states does not go to the heart of the problem of the policy-maker: how can one assess the consequences of one's actions?[14] This set of

[13] The relationship between the presence of Medicare and Medicaid and the effectiveness of abrogation is very strong and significant (r = -0.68).

[14] Gray's (1976) study of comparative state politics and policy makes very clear some of the severe pitfalls of cross-sectional research. Professor Gray accepts "an argument made by others . . . on theoretical grounds that policymaking is a process. It occurs over time within a governmental system. It does not occur across states or nations; hence, cross-sectional correlational analysis or cross-sectional regression analysis does not usually reflect the process from which the data are generated. A longitudinal research design is appropriate because it more truly reflects our theoretical focus—explaining differences across time" (1976: 255). She finds that a good many of the relationships supposedly established in the literature of comparative state policy and politics are, in fact, spurious—if one subjects them to a proper test over time.

considerations militates in favor of studying changes over time inside individual states. Use of longitudinal data here, however, means that we have excluded about half of the states in the union. But the analytical power gained makes it an acceptable loss. There are in addition a number of threats to the validity of inferences from a quasi-experimental design, and I have tried to rule out the most serious by the introduction of influences such as economic conditions and Medicare.

Change in judge-made private law does matter (Croyle, 1979: 964-965)—under certain circumstances and for certain people. This study of the effects of the decline of charitable immunity in the American states suggest several generalizations. First, the onset of Medicare/Medicaid caused a huge increase in the cost of running hospitals and thus in hospital room rates. Second, economic circumstances—i.e., income—structured the rise of price; as residents of a state became more affluent, and the cost of living rose, the price of a bed increased. Third, only in Nevada did abrogation increase the price of a bed both immediately and over the longer run. Thus, abrogation brought about radical change in only one state. Fourth, abrogation induced significant changes in half of the states—but only over a long period of time. Fifth, in about a third of the states, the demise of charitable immunity had no appreciable effect—in the short or long term. Sixth, in states that abrogated charitable immunity in the middle to late 1960s, the impact of Medicare and Medicaid simply overwhelmed other potential contributors to hospital cost increases. So momentous were the consequent changes in the delivery of medical services and the structure of hospitals that incremental shifts in price from changes in the common-law rule—as occurred in the 1950s and early-1960s—just did not register.

These results offer several lessons—some substantive, others methodological—about the impact of private-law doctrine. First, legal change can lead to societal change of quite varied magnitude and pace—obvious, perhaps, but too often ignored. This study indicates that in most states change in price as a result of abrogation came, if at all, rather gradually. Further, the demise of immunity had very different effects in different states. Second, common-law judges, in this area of doctrine, have had a modest impact on the costs of hospitals but perhaps more substantial effects on the society around them. Congress' domination of the field of health

through Medicare and Medicaid has been so pervasive as to reduce very considerably the impact of judicial doctrine. These doctrines may not have increased costs substantially, but they might have increased the rate of recovery by patients for damages. A state supreme court which abolished immunity after the onset of Medicare found itself in the ideal situation of articulating a policy change whose visible cost effects were coincidentally absorbed by the much greater impact of a new national policy; these courts, in a sense, received a "free ride" on the back of Congress.

There is a clear need to study further the effects of changes in the common law, even though those changes are likely to be slow if not glacial in pace; as Landes and Posner (1976) demonstrate, the value of the common law "depreciates" slowly over time. The gradual arrival of new judicial rules might constitute a formidable barrier to precise estimates of their impact; social and economic consequences, happening over a period of years, might be sufficiently diffused to defy the measures of social scientists.

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