DIVORCE RATES AND THE FAULT REQUIREMENT

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No-fault divorce legislation fundamentally changed the formal legal requirements for divorce from grounds based on moral concerns to those showing that the marriage has broken down without attributing blame. The old laws, which required that the plaintiff be blameless and prove malfeasance by the spouse, would have greatly restricted the number of divorces if they had not been widely subverted through collusion and perjury. The no-fault legislation gives an opportunity to test whether the attempts to legislate morality had any impact on divorce rates. Many reformers and researchers have claimed that the fault provisions were so thoroughly subverted that their removal did not lead to more divorces. The present study explores the impact of no-fault laws in thirty-eight states by applying a time-series–cross-sectional design. When grouped into one variable the laws did increase divorces, but when individual laws are analyzed, only a minority, primarily in eastern states, evidence significant impacts. Finally, I argue that it is probably beyond the limits of feasible social science research to determine why some laws had impacts but others did not.

I. THE ISSUE

A. Background

Religious and moral forces have long attempted to restrict divorce in Western society (Halem, 1980; Phillips, 1988). Divorce was readily obtained in Rome, but the Christian doctrine of the indissolubility of marriage nearly eliminated legal divorce in Europe until the last century. After the Revolution, the American states transferred the divorce function from the legislatures to the courts, and many liberalized divorce grounds during the nineteenth century. However, a backlash led by religious leaders developed, and the states adopted laws that limited the grounds to misconduct. In the past quarter century, these laws in turn were replaced by the present no-fault laws.

With few exceptions, the previous laws required plaintiffs to show malfeasance by their spouses. The specific grounds generally included adultery, desertion, drunkenness, impotency, conviction

of a felony, and cruelty (Pluscowe and Freed, 1963: 657–675; Rhein-stein, 1972: 52–53; Plateris, 1973: 49). Other common grounds were non-support, drug addiction, and commitment to a mental institu-tion. The only "neutral" provision, found in approximately ten states, was separation for a specified number of years, usually five or more. Under most state laws, moreover, petitioners had to be blameless; they could not be guilty of transgressions that could be grounds for divorce. Finally, the laws forbade collusion between the husband and wife to evade these requirements (Rheinstein, 1972: 54–55).

During the 1960s a few states amended their divorce statutes to permit divorce without showing malfeasance, and then from 1970 to 1975 a landslide of liberalizing legislation was passed, which usually permitted divorce upon a showing of the "irretriev-able breakdown" of the marriage. One might expect that such changes would prompt more divorces, and indeed nationwide di-vorce rates did increase substantially, from 2.5 per 1,000 population in 1965 to 3.5 in 1970 and to 4.8 in 1975 (National Center for Health Statistics, 1988: 2–5). But researchers and commentators have gen-erally claimed that the laws had no impact on this growth, largely because the increase appeared to be no greater after the laws than before.

B. *Statutory Language Versus Actual Practices*

How could the no-fault laws, which "fundamentally altered the face of divorce proceedings" (Jacob, 1988: 166), have had no im-pact on divorce rates? The answer generally given is that the legal change did not affect the practical ability to obtain divorces, be-cause the practice under the fault laws differed radically from the formal legal requirements (see, e.g., O'Gorman, 1963; Rosenblatt, 1969; Rheinstein, 1972: 51–105; Frank *et al.*, 1978: 37–45; Friedman, 1984). The vast majority of divorce proceedings were uncontested, staged hearings during which the plaintiff alleged sufficient acts to constitute a divorce, and the defendant admitted the charges. The judges rarely reviewed the accuracy of the charges or investi-gated whether the plaintiff was blameless (see, e.g., Virtue, 1956; Alexander, 1950). Many parties accordingly committed perjury and conducted illegal collusion under the direction of their lawyers and with the acquiescence of the judges (see Jacob, 1988: 33–36, 47–51, 67).

Rheinstein (1972: 49–50, 254–260) and Friedman (1984) inter-preted this situation as a standoff between religious and moral forces wishing to uphold the moral basis of marriages on the one hand and the practical demands of those whose marriages had bro-ken down on the other. The result, in Rheinstein's (1972: 387) words, was a typical "sub rosa institution," reminiscent of Prohibi-tion, and he contended that adoption of no-fault statutes would

mean "hardly more than concordance of the law of the books with the law in action."

Similarly, divorce reform advocates argued that the old laws should be replaced because they had been subverted (Goldstein and Gitter, 1969; Halem, 1980; Jacob, 1988). The reformers disliked the assumptions that one spouse was a wrongdoer and the other was innocent because they were often unrealistic and led to collusion and perjury, which threatened citizens' respect for the legal systems. The law was so widely circumvented, they claimed, that it did not stop those wanting divorces from obtaining them, and thus the no-fault laws would not cause more divorces. Jacob (1988: 55, 167–168) used this argument as an illustration of attempts to underplay the importance of the proposed laws so that they would be handled as routine legislation. Hence it is possible that the reformers consciously or unconsciously understated their expected impact.

Several of the reformers' arguments, on the other hand, did imply that the no-fault laws would lead to more divorces. They often said that some seeking divorce may have been deterred because the old laws required excessive legal expense and because the proceedings were designed to produce "evidence" of malfeasance. A more recent argument is that no-fault laws legitimized temporary marriages, undermining the traditional view of marriage as a life-long relationship (Weitzman, 1985: 366–371). On the other hand, some have claimed that no-fault laws might prevent divorces because the acrimonious proceedings under the old laws hindered reconciliation and because the new laws could provide for mediation services (which apparently was seldom done; see Zuckman, 1975: 8–9).

The question of whether the no-fault divorce laws cause more divorces is largely the same as whether the earlier fault provisions were able to *prevent* divorces. If the new laws did not affect divorce rates, the old laws had been nullified even before the adoption of no-fault (Wheeler, 1973: 611). Were the fault provisions, established and maintained for moral and religious reasons, actually able to regulate behavior? This is but one element of the larger question of the efficacy of attempts to legislate morality. The enactment of no-fault laws provides a natural experiment to determine whether moral restrictions enacted into law are purely symbolic. In fact this may be the only opportunity for such a determination with respect to any law, because it is extremely difficult to obtain detailed information about regulated behavior both under moral laws and in their absence. There is no doubt that such laws are widely circumvented; the interesting question is whether they are *completely* circumvented.

The prevailing view, which corresponds with reformers' predictions, is that the no-fault laws did not lead to more divorces (see, e.g., South, 1985: 37; Jacob, 1988: 162). The implication is that

because the fault provisions were completely ineffectual, the litigants and lawyers had totally subverted the law in full view of the judiciary. The purpose of the present research is to subject this implication to further scrutiny.[1]

II. PRIOR RESEARCH

At least ten studies have explored the impact of no-fault laws on divorce rates, with most concluding that there is none. Five are broad studies of many states that had adopted the laws (Wright and Stetson, 1978; Sepler, 1981; Peters, 1986; Johnson and Skinner, 1986; Weiss and Willis, 1989), and five are studies of individual states: California (Goddard, 1972; Schoen *et al.*, 1975; Becker, 1981); Delaware (Gallagher, 1973); and Nebraska (Frank *et al.*, 1978). Only the latest (Weiss and Willis, 1989) and the two earliest, which were not by social scientists (Goddard, 1972; Gallagher, 1973), concluded that the laws had substantially increased the number of divorces.

Wright and Stetson (1978) studied twenty-five states with no-fault laws as of 1974, comparing average divorce rates before and after the laws went into effect. They found significant increases for the 1969–71 laws but not for the later ones, and upon viewing the trends in individual states concluded that there was little evidence of any impact, with the possible exceptions of California and Florida.[2] Sepler (1981) studied the impact of thirty-one state no-fault laws by visually inspecting graphs of divorce trends. He determined that divorce rates did not increase noticeably beyond what prior trends would suggest. The laws in California, Delaware, Florida, and New Mexico were possible exceptions.

Three economic studies, using large survey research data sets, include no-fault divorce laws in much broader examinations of the causes and impact of divorces. Peters (1986: 446–448), using a 1979 survey, and Johnson and Skinner (1986: 459–462), using a 1972 survey, found that the probability of divorce was no greater in no-fault states.[3] Weiss and Willis (1989: 46–48) conducted a similar analysis, but with six-wave panel data such that the analysis contained a time-series element. They consistently found positive re-

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[1] This study is limited to the issue of whether no-fault laws affected divorce rates. It does not address their impacts on other facets of divorce, such as who files for divorce, financial arrangements, and child custody (see, e.g., Welch and Price-Bonham, 1983; Weitzman, 1985; Hoffman and Duncan, 1988; Jacob, 1989). Furthermore, it only addresses formal divorce decrees, which may not mirror trends in actual marriage breakdowns.

[2] On the other hand, Stetson and Wright (1975), in a cross-sectional study with 1960 state data, found a strong relationship between permissiveness of divorce laws and divorce rates, although few states had no-fault laws then.

[3] These studies classify the laws as those that require mutual consent for divorce and those that permit unilateral divorce. The latter are the traditional no-fault laws, the first three categories in Table 1 on page 553.

lationships, although sometimes not statistically significant, between various types of no-fault laws and the probability of divorce.

The individual state studies also reached differing results. Frank and his colleagues (1978) conducted a time-series analysis of the May 1972 Nebraska law, using data for forty-one months before the law and fifty-three months afterward, and found no evidence of an impact. Gallagher (1973) evaluated the impact of Delaware's 1968 law by simply presenting county divorce statistics for 1966 to 1972, attributing the large increases in 1968 and 1969 to the law. The first of three California studies tentatively claimed that the no-fault law, effective January 1, 1970, increased the number of divorce petitions because the statewide volume in 1970 and 1971 exceeded projections based on filings in the three years before the law (Goddard, 1972). But then filings declined in 1972, and Schoen and his co-workers (1975: 331) argued that the initial increase was artificial because the law reduced the waiting period for a final decree from twelve to six months. Becker (1981: 228–229) estimated the expected growth of the California divorce rate after 1970 by calculating the ratio of its growth to the national divorce rate growth in the 1960s, and applying it to the national growth in 1971 to 1976. He found that California divorce rates were higher than predicted by this formula in 1971–73 but slightly lower in later years, concluding that the no-fault law did not have a lasting effect on divorce rates.[4]

Although the overwhelming evidence is that the no-fault laws have little or no effect on divorce rates, the research can be faulted on various technical grounds. Because Johnson and Skinner (1986), Peters (1986), and Weiss and Willis (1989) conducted wholly or mainly cross-sectional analyses, they encountered the likely problem (which the latter two acknowledged) that states with high divorce rates tended to be the first to adopt no-fault. Also, in general, the cross-sectional design is not well suited for causal analysis (Lieberson, 1985: 179–183).

The remaining studies are time-series analyses, but with the exception of the work of Frank and his colleagues (1978), the number of time units usually falls well short of the thirty to fifty generally recommended for such analyses (see, e.g., Cook and Campbell, 1979: 228). Moreover, many of the time series extend for only a few years after the new laws, although the full impact might not become evident for several years (see Casper and Brereton, 1984). The conclusions are often based simply on the visual observation of graphs, which is unlikely to uncover subtle effects.[5]

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[4] The California divorce rates continued this trend; in 1977 through 1983 divorces per capita in the state declined by an average of 2.6% a year, whereas the national decline averaged 0.3%. As Becker (1981) explains, however, one cannot conclude from these trends that the California no-fault law did not increase divorces but that any impact was limited to the first few years.

[5] For example, Rheinstein (1972: 293–304) concluded, on the basis of the

Largely because of such crude designs, these studies rarely controlled for other factors that might affect divorce trends.

The two multi-state time-series studies (Wright and Stetson, 1978; Sepler, 1981) have severe data quality problems. First, they use divorce statistics that are inaccurate in several states because the data are incomplete, estimated, or based on petitions filed rather than divorces granted.[6] The clearest problem occurred in Arizona: Its no-fault law became effective in 1973, and through 1972 the divorce statistics in some counties are petitions filed in court, whereas starting in 1973 all statistics are for divorce decrees. Since petitions outnumber actual divorces, this data quirk may be responsible for the conclusion that the state's no-fault laws did not lead to higher divorce rates.

Finally, Wright and Stetson (1978) and Sepler (1981) had the wrong years for many of the laws studied.[7] Wright and Stetson relied on secondary sources for information about effective dates. Sepler (ibid., p. 67) claimed that these sources are incomplete; he thus obtained effective dates through "direct perusal of state law digests" and arrived at years that differed from those given by Wright and Stetson in eleven of the twenty-five states they studied. To iron out these inconsistencies, I researched the legislative history of each law. Sepler, it turned out, had twelve incorrect years, twice as many as Wright and Stetson, mainly for two reasons: (1) he apparently assumed that the latest legislation affecting the code section giving grounds for divorce created the no-fault law, whereas in several states the latest legislation only amended an earlier no-fault law; and (2) he assumed that the laws went into effect in the year they were enacted, whereas the effective date was often the next year.

III.   RESEARCH DESIGN

Lempert (1966: 130–131), in the first volume of this *Review*, surveyed research designs for studying the impact of laws, and he concluded that the best design, other than the pure random experiment, is the multiple time-series design. He considered it "the design *par excellence* for impact theory experimentation," largely be-

visual inspection of projections, that a 1900 German law establishing uniform strict divorce grounds did not affect the long-term divorce rate; but Glass and his co-workers (1971) applied time-series regression procedures to the same data and found evidence that the divorce rate was indeed dampened.

[6] These are indicated in footnotes in the published data tables (see, e.g., National Center for Health Statistics, 1988: 2–6). See n. 12 below for a list of states with inadequate data.

[7] These inconsistencies can be seen by comparing Table 1 with Sepler (1981: 90–102) and Wright and Stetson (1978: 576). Interestingly, at least 1 of these 2 works had the correct year for all but 1 of the states. Neither Sepler nor Wright and Stetson consider "separate living" provisions as no-fault laws; in contrast to the other no-fault laws, these are subject to debate concerning whether and when they should be classified as no-fault laws.

cause it provides for controls not available in a single time series (see also Cook and Campbell, 1979: 214–227; Berk *et al.*, 1979; Stimson, 1985). The application of the design, however, has been largely limited to economists, who call it the "pooled time-series–cross-sectional design," and they have developed the statistical techniques for dealing with pooled data. One of its important applications in the law and society field is estimating the impact of changes in minimum alcohol purchasing age on automobile accident deaths, using data from all states over periods of eight to ten years (Cook and Tauchen, 1984; DuMouchel *et al.*, 1987). This is analogous to the present application of the pooled model to estimate the impact of divorce law reform on divorce rates.

The major benefits of the pooled model over a single time series are a larger sample size and better control variables. By far the most important such variables are year and state dummies, which control for nationwide trends and for overall differences in divorce rates between states. These are discussed below.

A. Description of the Pooled Design

The pooled design combines data from several jurisdictions over a number of years, the more jurisdictions and the more years the better. The most common statistical model is an analysis of covariance, known as the fixed effects model (Mundlak, 1978; Pindyck and Rubinfeld, 1981; Hsiao, 1986).[8] A dummy variable is created for each state (except the first) and each year (except the first), and the coefficient associated with the variable is an estimate of the influence of specific factors ("fixed effects") unique to a state or year. Omission of the state and year dummies, since they are highly significant, would cause estimates of the other variables to be biased. The fixed effects reduce the degrees of freedom by the number of states and years included (and an additional degree of freedom for each state is lost in the correction for autocorrelation), but the analysis still has nearly a thousand degrees of freedom.[9] In practice, the fixed effect model transfers each variable into the difference from the state mean for that variable. It is also essentially a time-series analysis only; it combines

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[8] A second common model, the random effects model, is less useful in this application (see Mundlak, 1978) because it assumes that state-specific effects are not correlated with exogenous variables, because it requires the same time series for each state, and because correction for autocorrelation is difficult.

[9] Specifically, the form of the fixed effect model is as follows:
$$Y_{it} = a + bX_{it} + cV_{it} + g_2W_{2t} + g_3W_{3t} + \ldots + g_NW_{Nt} + d_2Z_{i2} + d_3Z_{i3} + \ldots + d_rZ_{iT} + e_{it}$$
where Y_{it} is the number of divorces in the state i and year t, X_{it} is a variable representing the no-fault divorce law, and V_{1t} represents age structure, a control variable. For the year and state dummies:
$$W_{1t} = 1 \text{ for the } i\text{th state } i = 2, \ldots, N; \text{ otherwise } W_{it} = 0;$$
$$\text{and } Z_{it} = 1 \text{ for the } t\text{th year, } t = 2, \ldots, T; \text{ otherwise } Z_{it} = 0.$$

the time-series data from the several states into one regression, and the state dummies control for the across-state variations.

B.   *Statistical Problems*

Pooled regressions are subject to many of the problems faced in time-series or cross-sectional regressions, plus some unique to the design. The Durbin-Watson test indicated autocorrelation problems, which were addressed by using the standard correction using state-specific autocorrelation coefficients (Pindyck and Rubinfeld, 1981: 258–259). Collinearity diagnostics indicated no problems except as noted below. Per capita divorce data are used because otherwise the year-to-year variation in larger states is some two orders of magnitude greater than in smaller states, which means that dummy variable coefficients would be much greater in large states and that the results would probably be dominated by just a few states.[10] Finally, the Bruesch-Pagan test revealed no heteroscedasticity problems.

C.   *Fragility*

As is typical in quantitative analysis, one could have selected from a wide variety of statistical models and variable configurations, and the results may depend on the choices made. Other reasonable variations were applied, especially using logged variables and first differences, entering the lagged dependent variable as an independent variable, and conducting separate time series for each state. These produced results very similar to those obtained with the standard model for Tables 2 through 4 below, but as discussed later the results for individual states (Table 5) vary somewhat from one type of analysis to another.

D.   *Simultaneity*

A finding that there is a relationship between divorce rates and the passage of no-fault laws is not readily interpretable because one does not know which caused which. Commentators often suggest that the increased demand for divorce was a major factor behind divorce law liberalization and the enactment of no-fault laws (see, e.g., Rheinstein, 1972: 469; Sepler, 1981: 77–80; Friedman, 1984: 667; Jacob, 1988: 92–93). On the other hand, it can be argued that increased divorce rates might produce attempts to deter divorce through stricter laws, as happened at the turn of the century (Rheinstein, 1972: 49–50). In any event, one must be aware that a correlation between divorce rates and no-fault laws does not necessarily indicate an impact by the latter.

The issue is addressed by using the Granger test, the standard

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[10] This cannot be cured by using a weighed regression; hence, in the debate between Firebaugh and Gibbs (1985) and Bradshaw and Radbill (1987), the former are correct.

econometric procedure for determining causal direction in a time-series analysis (Judge *et al.*, 1982: 720–723). The test works as follows: Suppose one has reason to believe that two variables, y and x, are simultaneously determined. If this were true, a regression of y on lagged y and lagged x would reveal significance with respect to lagged x variables. That is, in the regression,

$$y_t = a_1 y_{t-1} + \ldots + a_n y_{t-n} + b_1 x_{t-1} + \ldots + b_n x_{t-n} + u_t$$

the coefficient $b_1, \ldots, b_n$ can be expected to be jointly significant using an F test. If not, then x does not cause y. Similarly, if one regresses x on itself lagged and lagged values of y, the coefficients of the lagged y will be significant if y causes x; otherwise y does not cause x.

The Granger test was used with lags of one through three years, and the results suggest that the no-fault laws affected divorce rates ($F = 4.29$; $p = .01$, for the three lags of no-fault laws), but divorce rates did not affect the existence of the laws ($F = .58$; $p = .63$, for the three lags of divorce rates). This finding, however, is not a definitive answer to the simultaneity issue because of the outside chance that lawmakers acted on the basis of (correct) forecasts of rising divorce rates in their states.

IV. VARIABLES

A. Divorces

The dependent variable is the state divorce rate, the number of divorces per 100,000 state population.[11] The divorce statistics are the final versions issued by the National Center for Health Statistics, which are available through 1986 at the time of writing. The initial year is 1960, selected largely because earlier data are probably less accurate.[12] This time span provides a lengthy period before and after the no-fault laws went into effect, at least a decade for most states. Nine states were eliminated from the analysis because of data problems, and a few years were dropped from eight more states.[13] The time-series–cross-sectional analysis, then,

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[11] The data used in this analysis are available on floppy disks from the author at Box 3002, Williamsburg, VA 23185.

[12] In 1958 the National Center for Health Statistics established divorce registration areas (DRAs) to improve the quality of divorce statistics. To qualify for designation as a DRA (and to receive federal funds for record keeping), states must: (1) satisfy certain requirements concerning divorce record keeping; (2) have a central state agency that compiles divorce statistics; and (3) collect all divorce certificates. The certificates are checked by the state agency and the National Center to verify the divorce statistics compiled (see National Center for Health Statistics, 1988). Hence, data from DRA states are probably much better than data from other states. Eighteen states were admitted as DRAs by 1960, 28 by 1970, and 30 by 1980 (ibid., Table 4-2 p. 4-14). Of the states in this study, 15 were not DRAs during the years surrounding the initiation of new laws: Colorado, Delaware, Florida, Maine, Massachusetts, Minnesota, Mississippi, New Hampshire, New Jersey, New York, North Carolina, North Dakota, Texas, Washington, and West Virginia.

[13] The problems are indicated in the source tables (see n. 6 above). The

contains data from forty-one states, in most for a period of twenty-seven years.

B.   No-Fault Laws

The analysis encompasses thirty-eight no-fault laws that went into effect in 1965 to 1985. Two pre-1965 laws and one post-1985 law are excluded (see Table 1). The Illinois and South Dakota laws went into effect in 1984 and 1985, late in the study period, and findings concerning their impact are tentative.

Drawing upon standard definitions (see, e.g., Frank *et al.*, 1978: 1–5; Freed and Foster, 1981), this study organizes the no-fault laws[14] into five categories. They are, in descending order of "liberalness," as follows: (1) incompatibility; (2) irretrievable breakdown (or similar language) tacked on to the earlier law, thus retaining the standard fault grounds; (3) irretrievable breakdown of the marriage as a replacement for the earlier fault grounds (usually as the sole ground but occasionally with one or two others); (4) no-fault on one of the above grounds, except that mutual consent is required; and (5) living separately for a specified period (these states also have the regular fault provisions). In a few states laws fall in more than one type or successive laws fall in different types. They are categorized in the order listed above (for example, the laws in category 3 did not fit in categories 1 or 2, but might fit in categories 4 or 5). Table 1 lists the types of laws for the individual states.

The fifth type is difficult to operationalize, and placement must be somewhat arbitrary. Studies differ concerning whether separate living provisions are actually no-fault laws; for example, Jacob (1989) includes them, but Sepler (1981) does not. Separation alone, without showing fault, is sufficient, but the waiting period might be a substantial hinderance. The present study includes these laws, but only if the waiting period is fairly short—two years or less.[15] Under this criterion, only New Jersey and New York added separate living as new grounds during the period under study. The separation time in New Jersey remained eighteen months throughout; the time in New York was two years, reduced to one

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most common is the absence of data from some counties, and observations were deleted if the non-reporting areas account for at least 2% of the state population. The states deleted were Arizona, Arkansas, Indiana, Kentucky, Louisiana, Nevada, New Mexico, Ohio, and Oklahoma (Nevada is deleted because it is a "divorce mill"). The following observations were also deleted because of data problems: Massachusetts, Missouri, and Washington for 1960; Texas before 1962; Colorado before 1965; Rhode Island before 1968; and California and Mississippi after 1983.

[14] The study encompasses only legislation. It is likely that the appellate courts loosened the fault laws over the years; for example, in Utah the traditional fault defenses were eliminated by case law (Freed and Foster, 1981: 246).

[15] Jacob (1989) includes laws with longer periods, such as Utah's three-year law.

Table 1. Type, Effective Date, and Immediate Impact of No-Fault Divorce Laws, 1965–86[a]

State	No-Fault Divorce Law		Average Annual Percent of Growth of Divorces	
	Type of Grounds[b]	Date Effective	Year Before to Year After	1960–80
Alabama	IC	8/71	12.0	2.7
Alaska	MC	c	—	—
California	IRB	1/70	17.2	5.4
Colorado	IRB	1/72	10.4	7.3
Connecticut	IBA	10/73	15.1	9.1
Delaware	IC	6/68	33.2	7.3
Florida	IRB	7/71	17.9	6.8
Georgia	IBA	7/73	10.6	7.1
Hawaii	IRB	7/72	6.3	8.4
Idaho	IBA	7/71	3.2	4.9
Illinois	MC	7/84	−2.5	—
Iowa	IRB	7/70	5.7	4.9
Kansas	IC	7/69	10.7	5.3
Maine	IBA	10/73	8.3	5.5
Maryland	LS	c	—	—
Massachusetts	IBA	1/76	−0.2	7.0
Michigan	IRB	1/72	11.4	5.3
Minnesota	IRB	3/74	8.7	6.9
Mississippi	MC	7/76	1.6	5.1
Missouri	MC	1/74	6.2	4.7
Montana	IRB	7/73	4.5	4.7
Nebraska	IRB	7/72	10.1	5.7
New Hampshire	IBA	8/71	14.8	8.2
New Jersey	LS	9/71	27.3	10.3
New York	LS	9/67	52.9	13.6
North Carolina	LS	6/65	28.3	8.4
North Dakota	IBA	7/71	14.9	6.8
Oregon	IRB	10/71	10.5	5.9
Pennsylvania	MC	7/80	−.5	—
Rhode Island	IBA	4/75	14.3	7.8
South Carolina	LS	2/79	7.2	8.2
South Dakota	IBA	4/85	1.7	—
Tennessee	MC	4/77	2.6	6.3
Texas	IBA	1/70	8.8	5.6
Utah	IRB	4/87	—	—
Vermont	LS	4/72	16.6	9.5
Virginia	LS	7/75	13.2	6.1
Washington	IRB	7/73	8.0	6.3
West Virginia	MC	6/77	4.5	5.6
Wisconsin	IRB	2/78	8.3	8.3
Wyoming	IRB	5/77	4.8	5.8

[a] The sample was 41 states with adequate divorce data.
[b] The grounds are:
IC–incompatibility (usually with fault grounds)
IBA–irretrievable breakdown added to fault grounds
IRB–irretrievable breakdown only
MC–mutual consent required (also fault grounds)
LS–living separately (also fault grounds)
[c] A no-fault law existed before 1965.

in 1972. New York is a marginal state; one can argue that it should not be included because there the separate living must be under a formal agreement or court decree (Gitlitz [1986], for example, claims it is not a no-fault state), and because the 1972 change might perhaps be used instead of the 1967 change.

Several other states previously had separate living grounds but with longer waiting periods. They are considered to have adopted no-fault when they reduced the time periods by at least half (thus excluding Maryland, which reduced the period from eighteen to twelve months in 1973). These states are North Carolina, South Carolina, and Virginia, with one-year provisions; and Vermont with a six-month provision.

The no-fault laws are represented by a variable that equals zero before the law and one afterward. For the year in which the law went into effect, the variable is the portion of the year (taken to one decimal point) in which the law was in operation. Table 1 gives the month in which each law went into effect.[16]

The new laws might not have immediately affected divorce volume. Couples prompted to seek divorce by a new law would not show up in the divorce statistics for several months, the time required for the court to process petitions and, in some states, for a statutory waiting period to run. Also, potential petitioners may not become aware of the new law right away (Glendon, 1987: 107–108; Schoen *et al.*, 1975: 233; Jacob, 1988: 147). On the other hand, a strong immediate impact is possible. Divorce cases already filed might be processed more quickly due to procedural simplification, and in California the new law reduced the waiting period between filing and decree (Schoen *et al.*, 1975). Because available information is insufficient to stipulate a priori the appropriate lag structure, the no-fault variable was initially entered for the current year and lags of one and two years. The two-year lag was far from significant and was dropped.

C.   Control Variables

A key concern in causal analysis is ruling out other likely explanations for the relationships found. The pooled design is excellent for this purpose (Lempert, 1966), and the major control variables are the year and state dummies. The state dummies control for the combined impact of factors in a state that raise or lower its mean divorce rate in relation to the rates of other states. The year dummies control for factors that raise or lower the mean of the

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16 Obtaining the effective dates of the legislation required extensive research into the legislative history of the statutes and into the states' procedure for establishing effective dates. The original no-fault enactments are available from the author. Delaware (1974), Montana (1975), and Wisconsin (1972) had second no-fault laws not selected because of the criteria set forth in the text. The 1973 Montana law was an IBA law (see Table 1), but it is classified as an IRB state because the second law came only 30 months after the first.

state divorce rates in a year. The year and state dummies do not control for variables that have impacts that are independent of nationwide trends and state differences, but overall they provide substantial controls for variables that cannot be entered into the analysis.

There are, of course, innumerable likely influences on divorce rates other than no-fault laws (see, e.g., Martin and Bumpass, 1989; Trent and South, 1989; South, 1985; Frank *et al.*, 1978: 25–30; Carlson, 1980). Broad demographic factors include trends in the number of marriages, age structure, age at marriage, number of second and subsequent marriages, education levels, and urbanization. Economic trends include the general health of the economy, unemployment rates, and the number of women in the labor force. Changes in marriage style include the propensity to wait before having children, contraceptive use, the extent that marriages follow pregnancies, the extent of separations without divorce, and the propensity to cohabit rather than marry so that terminating relationships are not divorces. Changes on broader culture include those pertaining to women's liberation, sexual freedom, change in stigmas associated with divorce, and welfare policies. Finally, perhaps the best established influence on divorce rates is major military conflicts.

As Phillips (1988: 582–634) stresses, the state of theory about the causes of divorce, the measurement of likely factors, and our ability to untangle reciprocal causation are so incomplete that we have little hard information about what influences divorce trends. In this situation any attempt to control for the other forces affecting divorce by entering specific variables would be largely fruitless, and the use of state and year dummies, which admit our ignorance, is the best research strategy.

The only control variable entered and maintained in the analysis conducted here is age structure, or the percentage of persons between eighteen and forty-four years old. Several demographic factors suggest that age structure is important: The majority of divorces are by young adults, younger spouses are more likely to divorce, and the age at divorce has increased much less than the age at marriage (Carlson, 1979; National Center for Health Statistics, 1985: 2–11, 2–13). Divorces are concentrated in the twenty to thirty-four age group, which accounted for 55 to 59 percent of the divorced husbands and 61 to 65 percent of the wives in the 1970s. The only relevant age structure data for the period under study, however, is the eighteen to forty-four age group, and the percentage in this bracket is entered as a variable.[17] It accounts for more

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[17] The state age structure data to 1980 were obtained from the United States Census Bureau (1968, 1969, 1970, and 1986). The latest revised data after 1980 were not published at the time of research and were obtained directly from the United States Census Bureau. The earlier data have substantial problems. The estimates for the 1960s were based on the 1960 census and were

than 80 percent of the divorces, and it grew from 35 percent of the population in 1970 to 42 percent in 1980, although it leveled off thereafter. More detailed age structure data, available since 1970, are used in a separate analysis of the later no-fault laws (Table 3).

Several other variables used in prior research were tried, but then abandoned because they did not affect the results and their inclusion reduced the number of observations in the analysis. Perhaps the most obvious control variable is marriages: Because marriage is a requirement for divorce, one might expect the number of marriages in prior years to affect the number of divorces. But there was no evidence of such an impact,[18] probably because the length of marriage before divorce varies so much that the relationship between marriage and divorce is too diffuse. In 1984, for example, the four most common lengths of marriage, one through four years, each accounted for less than 10 percent of divorces, and percentages taper off for longer marriage lengths (National Center for Health Statistics, 1988: 2–10).

A second control variable candidate is economic trends, which might affect divorce rates either way (see Becker *et al.*, 1977; South, 1985). Economic hardships can foster discord and, thus, divorce. Better economic conditions might facilitate separate living because the husband can afford the cost of maintenance or the wife can more easily obtain employment. Economic conditions are represented by state per capita real income, the only state-level economic variable for which useable data were available during the time period under study. Both current and prior year personal income showed positive relationships to divorce rates, but influence analysis suggested that these results might be due to a few observations where small states experienced severe economic dislocations. Entering the economic variable did not affect the results concerning the impact of no-fault laws.

The portion of women in the labor force is commonly considered an important cause of increased divorce (see, e.g., South, 1985), but the divergence of divorce and female employment trends since 1980 has cast some doubt on this relationship. Simultaneity problems must also be resolved before the variable is entered; it is likely that divorce rates affect the proportion of women in the work force because separation probably forces more women

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not re-estimated after the 1970 census. The 1969 estimates did not include the 18 to 44 age category, and the figures used for that year are the average of the 1968 and 1970 figures. The 1960 and 1970 data are for April 1, whereas the remaining data are for July 1.

[18] This involved two analyses, one with marriages lagged 1 through 4 years and the other with the average over the prior 8 years. The impacts of both on divorce rates were far from significant, and entering the variables did not affect the results concerning the impact of the no-fault laws, although this determination is uncertain due to the substantial reduction in years encompassed by the analyses. These analyses did not include current-year marriage rates, which have a significant positive relationship to divorce rates, probably because more divorces cause more marriages.

to work as a matter of financial necessity, and the increased threat of divorce may lead more wives to work as a contingency measure (Michael, 1985; Johnson and Skinner, 1986; Phillips, 1988: 622).

V. RESULTS

An initial and superficial indication of the impact of no-fault divorce laws is whether divorce rates rose more than prevailing trends would suggest after the laws went into effect. Table 1 compares the growth rate for the first two years of the laws to the average change for 1960 through 1980 (a period that excludes the later years when divorces leveled, but does include the relatively slow growth periods of the early 1960s and late 1970s). Of the thirty-five states with new laws effective before 1980, twenty-five experienced higher than average growth rates when the laws went into effect, and the growth rate was more than twice the average in eleven states. This is a superficial indicator, however, because among other things the laws may simply have facilitated the processing of pending cases (Zuckman, 1975; Schoen *et al.*, 1975).

The long-term implications of the laws can best be estimated through regression analysis, which is conducted here with the no-fault laws represented as: (1) a single combined variable, (2) separate variables for the five types of laws, and (3) separate variables for the thirty-eight no-fault laws.

Using a single variable is a rather crude procedure because both the old and the new divorce laws differ from state to state, but it does provide a useful estimate of the average impact of the laws. The results, in Table 2, suggest that the laws do lead to higher divorces rates. Only the t ratio for the current year variable is significant (see Table 2a), and the significance level is low given the large sample size; however, the more important significance test is the F value for the combined impact of the two time periods, and it is significant to the .0001 level. The dominance of the current year variable is due to California, where the no-fault law had a large positive coefficient for the current year and a negative coefficient for the lag (see Table 5). When California is excluded, the lagged value dominates, but otherwise the results remain nearly the same.

The year and state dummy variables are highly significant (see Table 2c), suggesting that demographic trends and other factors are far more important than laws in determining divorce rate trends. Table 2b lists the year dummy coefficients, which indicate that factors operating nationwide pushed divorces up through 1981 and then down over the next five years.

The percent of population between eighteen and forty-four years is not significant in Table 2, although it is in later tables. As discussed, this age span is not the best, but more detailed data are not available until 1970. Table 3 gives the results of an analysis us-

Table 2:   Divorce Regressed on the Single No-Fault Variable, 1961–86

a)   Basic Regression Results

Variable	Coefficient	t Ratio
No-fault laws after 1964 (38 states)		
Current year	15.7	3.26*
Lagged	7.8	1.63
Percent population 18 to 44 years	2.9	1.60

b)   Year Dummy Coefficients

1962	−5
1963	2
1964	10
1965	17
1066	25
1967	42
1968	66
1969	89
1970	112
1971	135
1972	162
1973	187
1974	208
1975	236
1976	253
1977	253
1978	258
1979	269
1980	267
1981	270
1982	243
1983	234
1984	222
1985	224
1986	214

c)   Other Regression Values

F value for no-fault laws	9.8**
F value for year dummies	36.0**
F value for state dummies (state dummies are not shown)	49.1**
Dependent variable mean	387
Degrees of freedom	971
Adjusted R-square	.94
Durbin-Watson statistic	2.08

  * $p < .01$
** $p < .0001$

ing that data; here age structure, mainly the eighteen to twenty-four age group, is clearly significant, and the impact of the year dummies is greatly reduced (Table 3c). The findings concerning no-fault laws are similar to those in Table 2, even though only nineteen laws are included (those becoming law after 1972).

In Table 4a the no-fault laws are organized into the five categories (see Table 1), and the variables are again entered in their current and prior year versions. The results suggest that all types of laws lead to more divorces but that the degree of impact varies greatly. Laws that add irretrievable breakdown and those that require mutual consent are not significant at the .05 level (but the latter are significant at the .10 level). The archetypical no-fault laws, with irretrievable breakdown grounds alone, are highly significant, but this results in part from the negative impact for the prior year variable (due to California), which means that the F value overstates the likelihood of a positive impact. Two of the less traditional types of no-fault grounds, incompatibility and separate living, have the greatest impact.

The next analysis contains individual variables for the thirty-eight state no-fault laws.[19] Because of likely collinearity problems,[20] there are separate regressions for the current and lagged versions. Table 5 gives the t ratios for the no-fault laws in these parallel regressions, and there is more evidence for a positive relationship than a negative one. The larger coefficient is positive in some two-thirds of the states, fifteen of which are significant at the .10 level (three are significant in the negative direction, but one would expect nearly that many simply by chance). The current and prior year variables produced consistent results in most states. The most obvious exceptions are California, North Carolina, and Pennsylvania. The first two showed significant positive current year relationships but negative lagged relationships, suggesting that the results for the current year are due to a temporary surge of divorces.[21]

Some of the significant results in Table 5 were not obtained in alternate analyses,[22] but overall there is very strong evidence that

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[19] Collinearity diagnostics are not available in this analysis because the SAS system diagnostics cannot handle this many variables.

[20] Collinearity is likely because the current year and lagged year versions differ by only 1 observation out of the more than 1,000 in the analysis. This suspicion could not be tested for the reasons given in n. 19 above.

[21] In the individual state regressions described in n. 22 below, the current year variables for California and North Carolina have much larger coefficients than the prior year variables.

[22] The results in Table 5 were tested for fragility by using different regression procedures (not reported here, but which will be supplied by the author on request). They were: (1) regressions using first differences with logged versions of non-dummy variables; (2) separate ARIMA regressions for each state using levels; and (3) ARIMA regressions using first differences of logs. The 27 years in the study, however, are fewer than the number usually recommended for time-series analysis. The independent variables in the

Table 3: Divorce Regressed on the Single No-Fault Variable, 1971–86

a)   Basic Regression Results

Variable	Coefficient	t Ratio
No-fault laws after 1972 (19 states)		
Current year	5.6	.83
Lagged	15.9	2.44*
Percent population		
18 to 24 years	21.9	5.10***
25 to 34 years	8.9	2.59*
35 to 44 years	−10.8	−1.85

b)   Year Dummy Coefficients

1972	35
1973	55
1974	71
1975	91
1976	102
1977	99
1978	106
1979	115
1980	113
1981	118
1982	104
1983	105
1984	104
1985	119
1986	122

c)   Other Regression Values

F value for no-fault laws	6.3**
F value for age groups	10.2***
F value for year dummies	6.4***
F value for state dummies (state dummies are not shown)	85.0***
Dependent variable mean	466
Degrees of freedom	588
Adjusted R-square	.98
Durbin-Watson statistic	2.02

   * $p < .05$
  ** $p < .01$
*** $p < .0001$

Table 4: Divorce Regressed on No-Fault Law Types, 1961–86

a) Basic Regression Results

Number of States	No-Fault Law Type	Coefficient	T Ratio	F Value
3	Incompatibility			18.25***
	Current year	34.1	1.73	
	Lagged	38.9	2.02*	
10	Irretrievable breakdown added			1.69
	Current year	−3.1	−.37	
	Lagged	14.6	1.74	
13	Irretrievable breakdown alone			6.80**
	Current year	27.5	3.64***	
	Lagged	−15.5	−2.08*	
6	Mutual consent			2.93
	Current year	−14.0	−1.22	
	Lagged	28.0	2.42*	
6	Living separately			12.96***
	Current year	37.1	3.15**	
	Lagged	8.5	.72	
	Percent population 18 to 44	3.6	2.01*	

b) Other Regression Values

F value for all no-fault law	7.0***
F value for year dummies (year dummies not shown)	32.6***
F value for state dummies (state dummies not shown)	47.4***
Dependent variable mean	386
Degrees of freedom	963
Adjusted R-square	.94
Durbin-Watson statistic	2.06

  * $p < .05$
 ** $p < .01$
*** $p < .0001$

laws in eight states did lead to more divorces. They are California, Delaware, Florida, New Jersey, North Carolina, Pennsylvania, Rhode Island, and South Carolina. There is some but lesser evidence of the laws' impacts in eight more: Alabama, Connecticut,

ARIMA regressions are: (1) the current and prior year no-fault law variables, and (2) the coefficients for the year effects derived from the analysis in Table 2 without the population variable. Roughly two-thirds of the coefficients are positive in each analysis, but the significant coefficients differed somewhat. The states listed in the text have positive coefficients in all 4 analyses; the first group of 8 are significant in at least 3 analyses, and the second group in 1 or 2 (Georgia and Wyoming, significant in Table 5, have negative coefficients in most other analyses).

Table 5:   Divorce Regressed on Individual State Law Variables, 1961–86[a]

	T ratios for the no-fault law variable	
	Current Year	Prior Year
Alabama	3.43*	3.03*
California	4.74*	−2.88*
Colorado	−.29	.46
Connecticut	2.44*	2.50*
Delaware	7.00*	7.28*
Florida	2.81*	1.20
Georgia	1.73*	1.51
Hawaii	−.08	−.86
Idaho	−2.01*	−.69
Illinois	−.89	
Iowa	−3.17*	−3.46*
Kansas	1.28	1.09
Maine	.83	1.23
Massachusetts	−2.14*	−1.23
Michigan	−.44	−.93
Minnesota	−1.16	−.68
Mississippi	.15	.52
Missouri	−1.24	−.29
Montana	−.63	−.04
Nebraska	−.57	−.29
New Hampshire	1.99*	2.01*
New Jersey	1.78*	1.90*
New York	.40	.66
North Carolina	2.06*	−.91
North Dakota	−.57	−.76
Oregon	.48	.06
Pennsylvania	−.31	2.40*
Rhode Island	2.94*	3.32*
South Carolina	3.44*	3.17*
South Dakota	.50	
Tennessee	1.38	1.32
Texas	−.61	−.61
Vermont	2.79*	3.57*
Virginia	1.06	.41
Washington	.44	.81
West Virginia	4.17*	4.11*
Wisconsin	1.13	.42
Wyoming	1.28	1.70*

[a]   This table presents the results for two separate regressions using current year and lagged versions of the no-fault law variables for individual states. The F value for all current year laws is 4.48, and that for the prior year laws is 4.18, both significant to the .0001 level. The percent population 18 to 44 was significant in both analyses (T ratios of 2.94 and 2.29). Year and state dummies were also entered. The Illinois and South Dakota laws were passed so recently that analyses with the lagged variable are not feasible.

*   Significant to at least the .10 level.

New Hampshire, New York, Vermont, Virginia, West Virginia, and Wisconsin. A rough estimate of the magnitude of the effect in these states is that, on the average, the no-fault laws increased divorces by some 20 percent to 25 percent.[23] Overall, however, the results strongly suggest that no-fault laws in the remaining states, 58 percent of those studied, did not appreciably increase divorce rates.

VI. SUMMARY AND DISCUSSION

No-fault laws, operationalized as a single variable, had a significant impact on divorce rates, with the major thrust delayed for a year. There is no evidence of reverse causation; that is, divorce rate growth leading to new laws. The results become cloudier the more the no-fault variable is disaggregated. The impact is comparatively weak for the standard no-fault laws, which permit divorce for irretrievable breakdown of the marriage. Laws with separate living or incompatibility grounds appear to have much more impact. Results concerning individual state no-fault laws are far from uniform, and only a minority of the laws substantially increased divorces.

Logically, the next step is to determine what accounts for the different impacts of the laws, but this is a very difficult task. What about the setting and laws produces more divorces in the eight states in which the impact is clear and perhaps in the eight others in which an impact is also likely? The most striking association is that the no-fault laws in eastern states are more likely to affect divorce rates. The study includes laws in seventeen states east of the Appalachians (including Alabama and West Virginia), and fourteen are among the sixteen that show some evidence of an impact.[24] But I can see no reasonable explanation for this geographic distribution, for these eastern states are very diverse with respect

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[23] This estimate is derived as follows: (1) adding the coefficients for the current and prior year no-fault variables in the second regression discussed in n.22 above (ARIMA with levels); and (2) dividing this figure by the divorce rate when the no-fault laws went into effect. The percentages of increase in the divorce rate for the 16 states are: Alabama, 7%; California, 21%; Connecticut, 23%; Delaware, 26%; Florida, 15%; New Hampshire, 8%; New Jersey, 62%; New York, 39%; North Carolina, 24%; Pennsylvania, 11%; Rhode Island, 28%; South Carolina, 13%; Vermont, 27%; Virginia, 17%; West Virginia, 10%; and Wisconsin 15%. The margin of error, however, is typically very large.

The 20% to 25% estimate is consistent with the overall results obtained in Table 2a, where the current and prior year coefficients sum to 6% of the dependant variable mean. Weiss and Willis (1989: 48) estimated a greater impact of 30% to 40%.

[24] In an analysis similar to that in Table 4, but in which laws are organized by region, the 17 laws in states east of the Appalachians are highly significant ($F = 23.3$), and the remaining 21 laws are far from significant ($F = .9$). Moreover, one could explain away the two "western" state laws that may have an impact: The higher divorce rate after the California law may have resulted largely from fewer migratory divorces in Nevada, and the impact of the Wisconsin law is significant in only 1 of the 4 regressions (see n. 23 above).

to other dimensions that one might associate with divorce practices. It is feasible that states showing an impact would bunch somewhere on the map simply by chance, and thus *post hoc* boundary drawing may be misleading.

Another possible explanation, suggested in Table 4, is that the timing of rather than the criteria for divorce is important in determining the impact of the laws. All six of the separate living laws are among the sixteen having some evidence of an impact, and the California statute also changed the waiting time. But then all of the separate living laws are in eastern states, and their impact might be only a manifestation of a regional effect. In any event, the regression results indicate that the change in timing had a long-term effect on divorces, and not just a temporary jump when the laws went into effect. This implies that longer waiting periods might reduce the number of divorces in a state, perhaps because they facilitate reconciliations or prompt more migratory divorces.

Beyond this speculation, I believe that it is not feasible to discern why some no-fault laws did and others did not affect the divorce rate; this is what Lieberson (1985) calls "undoable" research. It would require cross-sectional analysis, which is generally incapable of determining short-term causation (ibid., pp. 179–183). There is a sizable gray area in which individual laws may or may not have impacts. More importantly, the thirty-eight states provide a meager sample size; the number of observations is nearly matched by the important independent variables. These include the particular statutory grounds for divorce, the grounds as applied in practice, variations in child custody and financial provisions, and the availability of mediation services. It is possible that some early laws produced more divorces because they were used by citizens of neighboring states. The impact is further influenced by provisions in the old fault law as well as the no-fault law.[25] Finally, political, social, and cultural aspects of the states also need to be considered as background factors.

In sum, the results do not provide tidy answers to the questions posed. Although there is clear evidence against the common blanket contention that the no-fault laws did not affect divorce rates, the findings only mildly contradict the earlier studies finding no such impact. Because the analysis has a large number of degrees of freedom, slight relationships can produce significant results. Also, many of the laws that had impacts changed the timing of divorce instead of removing the fault provisions. Sepler (1981), Wright and Stetson (1978), and Peters (1986) do not include these separate living laws in their analyses.

Likewise, the reformers' contentions that the no-fault laws

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[25] There is no apparent relationship between whether the no-fault laws had an impact and the "permissiveness" of laws in 1960 as defined by Stetson and Wright (1975).

merely enacted current practices are perhaps an exaggeration, but they are not so far from the mark that one can accuse them of making irresponsible statements to downplay the magnitude of the reforms they proposed. Weitzman's (1985) contention that the no-fault laws have caused more dissolutions by legitimizing temporary unions receives weak support at best.

The major issue is whether attempts to legislate morality with the old laws were completely futile, as evidenced by whether divorce rates rose when the fault provisions were removed. The answer is that the attempts were not always futile, although they apparently had become so in most states and in a sizable majority if one excludes the separate living laws. Also, there is almost no sign of an impact outside the eastern fringe. In all, the findings thus support the proposition that prevailing customs and peoples' immediate wants can totally thwart and not just partly circumvent laws enacting morality. Moreover, the legal system itself apparently subverted the laws; by all accounts, the lawyers actively participated in circumventing the fault requirements, and judges knowingly presided over sham proceedings. As one lawyer noted, the fault provisions meant that "the courts must often sanction clear violation of the law upon the thinnest of pretexts in order to avert the ruin of many lives" (Adell, 1965: 383–384). If this can happen in the legal system, the implication is that the complete nullification of laws legislating morals is likely elsewhere, but the difficulty of obtaining evidence may prevent any test of this hypothesis.

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