

# ANOTHER LOOK AT NO-FAULT DIVORCE AND THE POST-DIVORCE FINANCES OF WOMEN

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Recent studies by Weitzman (1985) and Peters (1986) suggest that no-fault divorce has harmed women. The research presented here tests these findings by examining the effects of no-fault divorce on the financial situation of women using data on salary and wage income, home ownership, and child support from the National Longitudinal Surveys (NLS) of Labor Market Experience (Ohio State University Center for Human Resource Research [OSU], 1986) young women's cohort. The analyses do not support the hypothesis that no-fault divorce produces adverse financial effects. Rather, it appears that no-fault has had little effect on women's finances. Alternative models that might explain how divorce law affects the financial condition of women are suggested.

Americans have long combined a passion for passing new laws to fix social problems with a concern that public intervention might produce unanticipated adverse consequences. Systematic research on the effects of legal change has yielded ambiguous results. Intended effects often elude administrators. Much of the criticism of economic regulation rests on its unanticipated distortions of investment decisions and other unwanted consequences (see, e.g., Stigler, 1975; McNeil *et al.*, 1979). Likewise, new laws imposing harsh sanctions against drunken driving are often evaded (Ross and Foley, 1987), and the deterrent effect of criminal sanctions in general is largely unproven despite many efforts to

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demonstrate it (Zimring and Hawkins, 1973; Blumstein *et al.*, 1978). Affirmative action laws have had questionable results, and other antidiscrimination laws have produced only sporadically measurable effects (Burstein and Monaghan, 1986).

Recent alterations in divorce laws have invited similar inquiries. Two studies (Weitzman, 1985; Peters, 1986) have alleged that the change from fault to no-fault divorce has had unanticipated and seriously adverse impacts upon women. We shall examine that claim using data from the National Longitudinal Surveys of Labor Market Experience (OSU, 1986) and suggest an alternative description of the impact of legal change.

### I. CHANGE IN DIVORCE LAW AND ITS PURPORTED EFFECTS

State legislatures, courts, and Congress have fundamentally changed divorce law since 1970. In the most publicized change, every state adopted a no-fault procedure for obtaining a divorce; it permits either spouse to receive a divorce upon claiming that his or her marriage is irreparably broken as a consequence of irreconcilable differences. But other changes in the law have perhaps had more important consequences. Most states abandoned common law rules about property division in favor of a quasicommunity property system in which all property accumulated during a marriage is to be divided between the parting spouses (Cheadle, 1981). In addition, states began to enforce new and more stringent collection of child support rules as a result of federal legislation in the early 1980s, about ten years after most states had adopted no-fault divorce (Congressional Almanac, 1984).<sup>1</sup>

Weitzman (1985: 366) asserts that no-fault divorce adversely affects women for the following reason:

Even though many divorced women were not very well off after divorce under the old law, the levers of fault and consent gave them some power to bargain for a better financial settlement. The reformers did not realize that without these levers women would need alternative provisions in the law to enable them to negotiate adequate financial settlements.

She supports her claim that the no-fault statutes of 1970 are at least partially responsible for the impoverished situation of divorced women through a series of comparisons between pre-reform and post-reform property division (*ibid.*, pp. 74, 78) and alimony awards (*ibid.*, pp. 169, 177, and 179). She concludes that “the new divorce laws—and the way these laws are being applied—have exacerbated the effects of the high divorce rate by assuring that

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<sup>1</sup> There have also been significant changes in child custody law, but these are not addressed in this paper because of the absence of pertinent data.

ever greater numbers of women and children are being shunted out of the economic mainstream" (ibid., p. 356).

Weitzman does not distinguish between the effects of no-fault and the new property division rules because an equal division rule was adopted along with no-fault in California, where she obtained most of her data. In few other states, however, was no-fault divorce accompanied by an equal division rule.<sup>2</sup> Moreover, only fourteen states followed California's example in making no-fault the exclusive procedure for divorce; the remainder retained fault grounds as an option (Freed and Walker, 1985). Thus the *legal* change in most states was substantially different from the California situation, and there is therefore a serious question about whether one can generalize from Weitzman's conclusions to the remainder of the nation.

Although Peters (1986) does use a national sample, she applies an idiosyncratic classification scheme. Peters (ibid., pp. 445–446) classifies as no-fault, or "unilateral," many of the divorces in states where couples could select either a conventional fault or a no-fault standard. While no one has systematically collected evidence on the use of fault and no-fault procedures where both are available, anecdotal information suggests that both are used in states that make both available. Many no-fault statutes require a one- or two-year separation, while a mutually accepted fault ground permits an immediate divorce. The ability to avoid delay may make fault divorces more attractive to some couples. Consequently, Peters may misclassify some divorces as "unilateral" (no-fault) when in fact they were what Peters calls "mutual" (fault).<sup>3</sup>

Another problem is that both the Weitzman and Peters analyses focus almost entirely on asset division, alimony, and child support. These issues are surely important, and we shall examine them here to the extent the data permit. There are good reasons, however, to surround a discussion of these resources with caveats, because they may reflect changes in the property division and child support statutes as well as the impact of no-fault.

In addition, exclusive focus on property division, alimony, and child support distorts the situation confronting most divorced women. Most of the families Weitzman studied had less than \$20,000 in assets, and almost no division of assets would substantially affect the long-term welfare of the divorcing parties. As Weitzman (1985: 68) herself acknowledges, "It would take the average divorcing family only six months to earn as much as the net value of

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<sup>2</sup> Those states were Arkansas, Florida, Idaho, and Louisiana.

<sup>3</sup> Peters examines divorces that occurred between 1975 and 1978. The ambiguities of her classification are illustrated by Wisconsin, which she classifies as a fault (mutual) state, but which had optional no-fault since 1969 and mandated no-fault in 1977; likewise, Connecticut and Kansas, which she classifies as no-fault (unilateral), permitted both fault and no-fault divorces during those years.

their assets." On the national level, fewer than 10 percent of divorced women ever received alimony (U.S. Bureau of the Census, 1986). While child support has been more widely awarded to the custodial parent (usually the mother), in 1978 only 49 percent of mothers awarded child support actually received the full amount; the mean amount they received was only \$1,799 (in 1978 dollars) (U.S. Bureau of the Census: 2, 5). Consequently, even if an analysis showed that no-fault adversely affected divorced women by giving the larger share of a couple's property to husbands and yielding stingy child support awards, most divorced women would not be affected substantially since they receive little or nothing from these sources even under the best of circumstances.

Instead, most divorced women, unless they remarry,<sup>4</sup> must earn their own income. Because women generally earn less than men and because divorce may push them into the labor market under unfavorable circumstances, every study has shown that the post-divorce income of women is substantially less than their family's pre-divorce income. However, neither Weitzman nor Peters examines the effect of legal changes on the income of divorced women.

There are several additional reasons for analyzing the income stream. One pertains to the reliability of the information. While many survey respondents find it difficult to provide full and accurate information about past assets, their recall about income is likely to be more reliable. Assets are easily overlooked, have fluctuating value, and are often complex, whereas people who work regularly see pay stubs and are reminded of the annual total by their income tax filing.

Another reason for looking at income flow is that there is a plausible connection between the grounds for divorce and income stream, which lies in the ways in which the divorce process structures a woman's entry into or continuation in the labor market. No-fault may be held accountable for an unfavorable effect if it facilitates divorce (Weitzman, 1985: 27, 37-41), thereby forcing women to go to work although they are unprepared to do so (*ibid.*, pp. 204-207). Such a sequence of events might reduce the post-divorce salary and wage income of divorced women more than if the law allowed the breakup of marriages to take a slower course.

Yet existing evidence suggests that salaries and wages may not be affected by no-fault. It is unclear whether ordinary divorce proceedings have been substantially streamlined by no-fault. The old fault grounds were rarely an important obstacle to divorce; rather, difficulties traditionally centered around questions of property settlements, alimony, child support, and custody, matters that were not centrally altered by no-fault *per se*. If a couple has property or

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<sup>4</sup> In the NLS sample we used, only one-quarter remarried within a year of their divorce.

children, even no-fault divorces take time and money. Moreover, it has been shown that the higher divorce rates of the 1970s were not the consequence of no-fault (Peters, 1986; Bahr, 1983; Sepler, 1981; and Schoen *et al.*, 1975). Thus there is no strong evidence that no-fault by itself pushed women into economic independence more rapidly or more often than fault divorce had.

Changing economic circumstances during the period in which the states adopted no-fault laws may have been more important in altering the economic fate of divorced women than the laws themselves. If ignored, their effects can be mistakenly attributed to no-fault. As no-fault laws were being adopted during the 1970s, the proportion of married women in the paid work force grew to over half, and many women with young children took jobs (Mott and Shapiro, 1982; Gerson, 1985: 3–9; Bianchi and Spain, 1986: 141–59). A much larger array of employment choices became available to women, and paid employment outside the home became the norm for the married as well as the divorced. On the other hand, the unemployment rate fluctuated substantially during those years. Simultaneously, the new feminist movement urged women toward higher career aspirations. Consequently, controls for the changing labor market that women encountered in the 1970s must be introduced to hold constant societal changes while examining the impact of no-fault divorce.

We turn now to a test of the hypothesis that no-fault divorce had no independent effect on the post-divorce finances of women. The Weitzman/Peters hypothesis would predict a rejection of the null hypothesis based on evidence that the financial position of women deteriorated with the spread of no-fault divorce net of simultaneous economic and social changes. We shall test the hypothesis first with respect to the salary and wage income position of divorcing women and then with respect to income transfers such as child support and post-divorce home ownership. Finally, in interpreting our results, we shall examine their implications for specifying the impact of legal change.

## II. THE DATA

Unfortunately, no one thought to examine the impact of no-fault divorce as it was being adopted across the United States during the 1970s. Consequently, no large national data set in which women were directly asked about the kind of divorce they obtained is available.<sup>5</sup> However, it is possible to distinguish between fault and no-fault divorces for many of the respondents in the young women's cohort of the National Longitudinal Surveys of Labor Market Experience conducted for the Department of Labor by the United States Bureau of the Census in repeated interviews since 1968 (OSU, 1986). This paper uses the 587 respondents who

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<sup>5</sup> This is true even for the Panel Study of Income Dynamics.

were divorced and did not immediately remarry<sup>6</sup> from the panel of 5,159 women who were between the ages of fourteen and twenty-four in 1968 and between twenty-nine and thirty-nine by 1983.<sup>7</sup> The respondents were interviewed annually between 1968 and 1973 and then again in 1975, 1977, 1978, 1980, 1982, and 1983. All interviews were in person except in 1975, 1977, 1980 and 1982, when the questions were asked by telephone. The panel provides an exceptionally rich body of data for young women, the cohort most likely to experience divorce (Glick and Lin, 1986). The survey asked nearly identical questions at each interval for most of the indicators, which are shown in Table 1.

Not all data are available for all respondents. The largest gaps exist in home ownership data, which are consistently available for only five points in the survey, and in child support and alimony data, which are available for only 1983. All data were obtained from women.<sup>8</sup>

Several issues surround the variables of wage and salary income. First, we utilize post-divorce income, one or two years after the divorce. This comes closest to replicating the data Weitzman and Peters used. It is substantially higher than the income during the year of the divorce, when most women reach the nadir of their economic troubles. Second, when we use post-divorce income as the dependent variable, we utilize the wage and salary income of the divorced woman only. However, when we employ pre-divorce income as a control variable, we include the husband's wages and salary, since the total spousal income best represents the position from which a woman embarks on her postdivorce existence. Although the two measures are separated in this sample by only two or three years, they are not highly correlated with each other. (Their correlation is .32 for the entire sample and .38 for those not remarried.) Finally, while not encompassing a person's total income, salary and wages do represent a very large portion—usually about 80 percent—of all income (Ryscavage, 1986), especially among young people. Other measures of income flow from this data set would prove to be less reliable; however, our findings cannot be extrapolated to affluent couples who may enjoy substantial investment income.

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<sup>6</sup> Remarried women are excluded because their post-divorce incomes are substantially affected by their new spouses. As shown below, their exclusion does not skew the results of the analysis in favor of the null hypothesis.

<sup>7</sup> This is the date of the last interview on which this analysis is based. Further telephone and personal interviews were conducted in 1985 but were not available for this research. Additional contacts occurred in 1987 and 1988. Previous analyses of the divorces of these women were based only on the early years of the study; see Mott and Moore (1978) and Spitze and South (1985; 1986).

<sup>8</sup> While this controls for any male-female bias in knowledge of family finances, overall reported income levels in both fault and no-fault states may be affected if some women lack pertinent information.



**Table 1.** Variables and Indicators Used

Variable	Indicator
Age	Age at divorce
Race	White/black and others
Length of first marriage	Interval from last reported unmarried status to first reported divorced status
Remarriage	Remarriage at first contact after first divorce
Length of first divorce	Interval between first divorce and remarriage or end of study period
Pre-divorce employment	Reported working at last contact before first divorce
Pre-divorce education	Completed school years at last contact before divorce
Pre-divorce income	Salary and wage income of respondent and husband at last contact before first divorce (in 1967 constant dollars)
Post-divorce income	Salary and wage income of respondent at first contact after divorce (in 1967 constant dollars)
No-fault divorce	Location at time of first divorce in state that permitted only no-fault divorce
Fault divorce	Location at time of first divorce in state that permitted only fault divorce
Mixed divorce	Location at time of first divorce in a state that permitted both fault and no-fault divorce
Unilateral divorce	Peters's (1986) classification
Home ownership	Owning (rather than renting) her home in 1971, 1972, 1973, 1978, or 1983
Child support	Reported receiving child support in 1983 (only those women having children at time of first divorce are used)
Number of children at divorce	Reported number of children before divorce
Alimony	Reported receiving alimony in 1983 (only those women still divorced in 1983 are used)
Unemployment rate	Unemployment rate for women in year of divorce (state rate applied when available; otherwise, national rate applied)
Women in labor force	Women in labor force as percent of total female noninstitutionalized population in year of divorce

Three comparisons examine the effects of the type of divorce experienced by these respondents, which in each case is determined by the legal provisions that governed when the divorce occurred.

#### A. *Pure Fault/No-Fault*

Because most states allow both fault and no-fault divorce, the first comparison uses data from only those respondents in states that permitted either no-fault divorces or fault divorces *exclusively*. For instance, a women who divorced in California in 1969 would be classified as having obtained a fault divorce; those Californians who divorced in 1970 or after are classified as no-fault. However, no New York respondents are used because both fault and no-fault procedures were available to them throughout the study period. Table 2 shows the exact assignment of states and years to the two categories. The procedure used to identify the state of residence depends upon an earlier analysis by Mott and Moore (1978) and extends their geographical code through 1983 for those respondents for whom sufficient data exist.

#### B. *No-Fault/Fault/Mixed*

The second comparison of divorce utilizes information on all respondents whose residence at time of divorce could be ascertained. Three categories were constructed: pure fault, pure no-fault, and mixed. The mixed category includes those respondents living in states where they could use either fault or no-fault procedures (see Table 2). Two dummy variables were created for this comparison, the first between divorces in states where only no-fault divorce was permitted and all other divorces, and the second between divorces in states in the mixed category and all other divorces.

#### C. *Unilateral/Mutual*

The third comparison replicates Peters's (1986: 446) distinctive classification of states as fault and no-fault to examine whether the difference in results between her paper and the present one is an artifact of contrasting categorizations. Fewer respondents were used for this comparison than for the previous one because some respondents who could be categorized as having received their divorce in a "mixed" state could not be assigned to one of Peters's types due to missing data.

#### D. *Control Variables*

One set of control variables reflects global conditions that might affect the economic circumstances of women, quite independent of the type of divorce they obtained. There are two variables in this set: the unemployment rate for women in the



**Table 2.** Classification of States by Grounds for Divorce

Grounds	Years	States	
Fault grounds only	1969–82	Illinois, South Dakota	
	1969–78	Tennessee, Wyoming, West Virginia	
	1969–75	Massachusetts, Montana, Rhode Island, Mississippi	
	1969–73	Kansas, Minnesota, Ohio	
	1969–72	Arizona, Connecticut, Georgia, Indiana, Maine, Missouri, Nevada, New Mexico, Washington	
	1969–71	Hawaii, Kentucky, Nebraska	
	1969–70	Idaho, Colorado, Florida, Michigan, New Hampshire, New Jersey, Oregon, Utah	
	1969	California, Iowa	
	No-fault only	1970–82	California, Iowa
		1971–82	Florida, Michigan, Oregon, Colorado
1972–82		Kentucky, Nebraska	
1973–82		Arizona, Missouri, Washington	
1975–82		Montana	
1977–82		Wisconsin	
Mixed grounds	Pre-1969–82	Arkansas, Oklahoma, Delaware, Texas, Louisiana	
	1969–82	Maryland, South Carolina, Vermont	
	1969–77	Wisconsin	
	1970–82	New York	
	1971–82	Alabama, Idaho, New Hampshire, New Jersey, North Dakota, Utah	
	1972–82	Hawaii, Virginia	
	1973–82	Connecticut, Georgia, Indiana, Maine, Nevada, New Mexico	
	1974–82	Alaska, Kansas, Ohio	
	1975–82	Massachusetts, Rhode Island	
	1976–82	Mississippi	
	1977–82	Tennessee, Wyoming	
	1980–82	Pennsylvania, West Virginia, North Carolina	
1974–82	Minnesota		

year of their divorce and the proportion of women in the labor force during that year. The second set describes personal characteristics of these women that might affect their economic well-being, independent of the kind of divorce they obtained. This set includes a respondent's pre-divorce family income (as measured by her and her husband's salaries and wages), her work status before divorce, her education, and her age at divorce.

### III. ANALYSES

Two adjustments have been made to the raw data. First, the analysis uses 1968 sample weights to adjust principally for the heavy oversampling of blacks in the unweighted sample. Second, all financial data have been converted into constant 1967 dollars.

The analysis employs ordinary least squares (OLS) regression and logit.<sup>9</sup> It utilizes the full longitudinal scope of the data by hinging the analysis around the time of a woman's divorce. Each respondent's age, salary and wages, education, work experience, children, and the like are measured as of the interviews before and after her divorce.<sup>10</sup> For some respondents this might be 1969; for others, it is one of the other years.

Table 3 reports mean values for selected characteristics of the women used in this study. Except for the child support analysis,<sup>11</sup> we use only those who had obtained a divorce and who had not remarried at the time of their first contact after divorce ( $N = 587$ ).

Table 3 shows that this group was in their middle to late twenties at the time of divorce and had been married an average of 6.7 years. This is considerably younger than Peters's (1986: 445) sample, whose mean age was 35.8, or Weitzman's (1985: 408) respondents, most of whom were married much longer than the young women in the NLS sample. We will return to these differences, since they may be important in understanding differences between the conclusions drawn by them and by us. However, it is worth noting that 65 percent of all divorced women were under thirty in 1975 (Glick and Lin, 1986: 740), and approximately half of all divorces take place within the first ten years of marriage (Glick and Norton, 1971: 310).

Nearly one-third of our sample had more than a high school education, and almost three-fifths were employed when interviewed prior to their divorce. Average salary and wage income after divorce was about three-quarters of pre-divorce income (even though pre-divorce income included the salaries and wages of their husbands). Their income in the year of their divorce drops substantially for all except those who had obtained no-fault divorces. Because this drop is temporary, and to be comparable to Weitzman and Peters, we utilize the later (and higher) figures.

Table 3 also shows that because fault alone was available only during the first years of this cohort study, the women who obtained fault divorces were different from all the others. On the average, fault divorces took place in 1974, whereas the other divorces

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<sup>9</sup> The regression analysis used SPSS/PC+; the logit analysis used the Systat logit module.

<sup>10</sup> The irregular timing of the later surveys forces us to utilize data for somewhat noncomparable periods for some respondents.

<sup>11</sup> For that analysis, we use all divorced women who had children at the time of their divorce. We include remarried women because remarriage does not affect the child support obligation.

**Table 3.** Selected Characteristics of Divorced Women not Remarried at First Contact After Divorce

Variable	Total	Fault	Mixed	No-Fault	Mutual	Unilateral
Mean age at divorce	27.7 (587)*	25.3 (82)	28.0 (400)	28.3 (105)	27.3 (177)	27.6 (192)
Mean year of divorce	1977 (587)	1974 (82)	1977 (400)	1977 (105)	1976 (177)	1977 (192)
Mean length of first marriage (years)	6.7 (572)	3.8 (81)	7.1 (388)	7.3 (103)	6.0 (174)	6.8 (187)
Mean length of first divorce (years)	5.1 (587)	6.8 (82)	4.8 (400)	5.0 (105)	5.2 (177)	5.2 (192)
Mean salary and wage income before divorce (in constant dollars)	5,392 (553)	4,884 (80)	5,373 (375)	5,886 (103)	4,698 (173)	5,619 (189)
Mean salary and wage income at divorce (in constant dollars)	3,190 (574)	2,723 (80)	2,983 (391)	4,329 (104)	2,892 (173)	3,633 (189)
Mean salary and wage income after divorce (in constant dollars)	4,053 (554)	3,360 (77)	4,081 (376)	4,472 (102)	3,871 (166)	4,029 (183)
Percent black	12.7 (587)	14.0 (82)	14.5 (400)	14.0 (105)	15.9 (177)	14.6 (192)
Percent working before first divorce	58.2 (563)	50.2 (82)	62.1 (377)	64.1 (104)	53.5 (176)	63.0 (190)
Percent with more than a high school education before divorce	31.7 (579)	17.1 (82)	34.1 (392)	34.2 (105)	22.1 (177)	28.7 (192)

\* Numbers in parenthesis are totals; they vary because of missing data. The 587 represents all respondents in the sample who had experienced divorce and were not remarried at the first contact after divorce. All data here and elsewhere pertain only to a respondent's first divorce.

took place two or three years later; on the average the women with fault divorces were also younger and had experienced shorter marriages than the others. As we shall see, however, these differences do not seem to affect their income streams; neither age nor educational level at divorce is significantly related to post-divorce

salary and wage income for the women whose divorce can be identified as fault or no-fault.

Our first analysis examines the correlates of postdivorce salary and wage income. We chose this as our dependent variable because, as we have already urged, their own income stream is the principal resource for most women. Alternatively, we might have focused on changes in salaries and wages between pre- and post-divorce interviews. Such a difference measure, however, is more difficult to interpret because it is highly sensitive to pre-divorce levels. For those with very low salaries and wages, any improvement would produce a large positive difference, while those who enjoyed large pre-divorce salaries and wages would show negative differences with small drops in income even though the absolute level of their income remained high.

Table 4 shows the effect of the type of divorce upon post-divorce salary and wage income as estimated in the context of labor market conditions and the characteristics of these women around the time of their divorce. Each of the three columns represents one of the three classifications of divorce type discussed above.

The effects of global forces (the unemployment rate and labor force participation of women) and of personal characteristics (age, education, work experience, race, and pre-divorce income) are generally consistent across the sets of respondents classified by type of divorce. Thus, whether we look at the women whose divorces we could identify as fault or no-fault ( $N = 174$ ), include the mixed category of women who obtained their divorce in states that had both fault and no-fault ( $N = 442$ ), or use Peters's classification ( $N = 340$ ), the effects of these variables are generally similar.

The effect of divorce type and its estimated strength depend upon how one classifies type of divorce. In Table 4 the column labeled "Pure Fault/Pure No-Fault" includes only those women who divorced in states that provided no choice; all divorces had to be either fault or no-fault. The effect of the type of divorce proceeding for these women is the opposite of that shown by Weitzman and Peters. Women who used no-fault proceedings did significantly better financially after divorce than those who used fault proceedings. The "Fault/No-Fault/Mixed" column includes women who lived in states where they could use either fault or no-fault rules; here the effects of type of divorce are not statistically significant, although the direction of the coefficient is consistent with the pure fault/no-fault results. Finally, the "Mutual/Unilateral" column replicates Peters's classification that uses the degree of mutuality required in a state with unilateral divorce approximating no-fault. Here the results are not statistically significant even at the .10 level that Peters used, and in fact the sign is negative; unilateral divorce is associated with lower post-divorce salary and wage income.

These results indicate that the effect of no-fault divorce laws

**Table 4.** OLS Regression of Postdivorce Income for Women Not Remarried, Using Various Divorce Law Classifications (Regression Coefficient, *t*-Statistic, and Statistical Significance Shown)

Variable	Divorce Law Classifications		
	Pure Fault/ Pure No-Fault	Fault/ No-Fault/Mixed	Mutual/ Unilateral
Intercept	10184.87** (2.511)	4804.79* (1.896)	4209.26 (1.581)
Unemployment rate	330.04** (2.228)	359.95** (3.731)	364.64** (3.494)
Percent of women in labor force	-254.42** (-2.025)	-112.36 (-1.523)	-112.69 (-1.425)
Age at divorce	12.86 (.172)	-25.25 (-.530)	2.292 (.057)
Education before divorce	472.79* (1.879)	487.66** (3.094)	674.31** (3.670)
Black	-568.35 (-.955)	-591.28 (-1.424)	-409.21 (-.947)
Employment before divorce	967.46* (2.165)	1315.57** (4.494)	1449.70** (4.466)
Income before divorce	.23** (4.542)	.22** (6.790)	.22** (6.176)
Type of divorce			
No-fault	967.25* (2.078)	699.34 (1.502)	—
Mixed	—	431.06 (1.093)	—
Unilateral	—	—	-300.13 (-.987)
Adjusted <i>R</i> -square	.23	.24	.28
Sample size	174	442	340

\* *p* < .05 (one-tail)  
 \*\* *p* < .01 (one-tail)

upon the salary and wage income flow of divorced women that an analyst imputes is quite sensitive to the manner in which the legal situation is characterized. Because a law's effects are rarely straightforward and simple, those characterizations must be drawn with great care and read with considerable skepticism. Probably the most that can be said from the statistical results in Table 4 is that the effect of no-fault laws upon post-divorce salary and wage income flow is in all likelihood either weak or nonexistent. Both aggregate economic forces and individual social characteristics have larger and more certain effects.

These analyses excluded women who immediately remarried after divorce. It is possible that women who suffered most

**Table 5.** Mean Salary and Wage Income at Divorce by Remarriage and Type of Divorce\*

Immediate Remarriage	Fault	No-Fault	Mixed	Mutual (Fault)	Unilateral (No-Fault)
Yes	\$2,364 [1,590] (31)	\$3,503 [2,627] (37)	\$2,608 [2,055] (116)	\$2,668 [1,897] (46)	\$3,083 [3,997] (65)
No	\$2,723 [2,666] (80)	\$4,328 [4,632] (104)	\$2,893 [2,508] (391)	\$2,892 [2,551] (173)	\$3,632 [3,977] (189)

\* Standard deviation is shown in brackets; the number of respondents in each category is shown in parentheses

through the no-fault proceedings found their economic relief by remarriage within the year of their divorce. Table 5 tests this hypothesis by comparing for each divorce law category the mean salary and wage income at the year of the divorce for both those women who remarried immediately and those who did not. The data show that those who remarried immediately had lower at-divorce incomes than those who did not under every divorce law regime. However, when we compare women who remarried quickly and who obtained their divorces under fault laws with those who did so under no-fault statutes (row 1, columns 1 and 2), we find that those who divorced under no-fault statutes had higher incomes than those who divorced under fault statutes. The same is true when Peters's classification is used to compare unilateral divorces with mutual divorces (row 1, columns 4 and 5). Moreover, it is not clear that quick remarriages should be counted as an adverse effect. Our conclusion, therefore, is that excluding the remarrieds has not confounded the analysis of the effect of the type of divorce law.

When we turn our attention to transfers of income and resources such as child support, alimony, and home ownership, the evidence is weaker because there are more cases with missing data. However, our analyses here too show no evidence that the type of divorce strongly affects asset transfer.

Child support is an important source of income for some divorced women. In this sample 493 women had children at the time of their first divorce; 323 of them received no child support payments in 1983,<sup>12</sup> and the 170 who received anything at all collected an average payment in that year of \$2,400 (in 1983 dollars). Because we do not have complete data on many of the 170 women

<sup>12</sup> A few of these women may have lost eligibility for child support between the time of their divorce and 1983 because their youngest child had reached maturity. However, given the youth of this sample, this factor would not have affected more than a handful of respondents.

**Table 6.** Logit Coefficients Describing Effects of Type of Divorce and Personal Characteristics of Women with Children at Time of Divorce on the Log Odds for Receiving Child Support in 1983 (Logistic Coefficient Estimates, First Derivatives, and *t*-Statistic Shown)

Variable	Divorce Law Classifications		
	Pure Fault/ Pure No-Fault	Fault/No-Fault/ Mixed	Mutual/ Unilateral
Intercept	-2.39 [-.481] (-1.493)	-3.92** [-.877] (-4.266)	-2.68** [-.561] (-2.517)
Age at divorce	.10* [.021] (1.827)	.14** [.031] (5.131)	.12** [.024] (3.344)
Black	-1.04* [-.209] (-1.923)	-.51 [-.115] (-1.357)	-.75** [-.158] (-2.274)
Employment before divorce	-.87* [-.176] (-1.838)	-.47* [-.105] (-2.102)	-.02 [-.005] (-.077)
Income before divorce	-.00* [-.000] (-1.891)	-.00 [-.000] (-.313)	-.00* [-.000] (-1.785)
Number of children (1-8)	.20 [.041] (.865)	-.14 [-.032] (-1.483)	-.08 [-.158] (-.644)
Remarriage at first contact after divorce	1.07* [.215] (2.00)	.69** [.155] (2.674)	.83** [.174] (2.380)
Type of divorce			
No-fault	.75 [.150] (1.471)	.44 [.100] (.955)	
Mixed		.40 [.091] (.989)	
Unilateral			-.14 [-.023] (-.486)
Sample size	119	421	265
Proportion receiving child support	.311	.342	.313

\* *p* < .05  
 \*\* *p* < .01



who did receive child support, we cannot analyze the correlates of the level of support received.

We can, however, take a closer look at the relationship between the type of divorce and the likelihood of obtaining any support at all. Table 6 displays a logit analysis that examines the predictors of whether these women received child support in 1983. The logistic coefficients indicate the incremental impact of the independent variable on the logarithm of the odds of receiving child support. The first derivatives, shown in brackets in Table 6, indicate the increment to the actual probability of child support (here the sample proportion) by a one unit increase in the independent variable. In this analysis we include remarried women because remarriage does not affect the child support obligation of either parent.

While the social circumstances of the divorced woman have some impact on child support, the type of divorce is not significantly related to the likelihood of receiving child support. However, some of the other variables show a statistically significant relationship with receipt of child support. Women who were older at the time of their divorce were more likely to receive child support, with each year adding approximately 2 percent to the probability of support. Likewise, women who remarried almost immediately after divorce had between a 10 and 20 percent higher probability of receiving child support. Blacks were between 10 and 20 percent *less* likely to collect child support than whites. Moreover, there is some indication that the higher a woman's pre-divorce family income (as measured in salary and wages), the less likely it was that she received child support, but the magnitude of this effect seems to be small.

This failure of divorce type to affect child support is consistent with the content of divorce law reform during this period. No-fault provisions had no bearing on the question of imposing a child support liability. Under fault proceedings, being at fault did not routinely disqualify a parent from receiving custody or burden him or her with paying child support; such issues were handled under different rules. Moreover, the small minority of women receiving child support in the present study is consistent with census data indicating that in 1983 only a minority of potentially eligible women (3 million of 8.7 million) received it (U.S. Bureau of the Census, 1986: 2).

Alimony was rare among these divorced women; only 3.6 percent of the women in this sample recorded as divorced in 1983 received alimony. This is entirely consistent with national data. Census bureau data for 1983 reported that only 608,000, or 3.5 percent, of the 17.4 million ever divorced or currently separated women received alimony in that year (*ibid.*). Most of the remainder were not even awarded alimony. Although alimony had conventionally been awarded only to the innocent party in a fault pro-

ceeding, these census figures suggest that the spread of no-fault did not change the overall incidence of alimony awards. However, it may have changed its distribution, as suggested by Weitzman and Dixon (1980: 167), who reported that in California, no-fault led to more frequent awards of alimony to women after long marriages, while women ending short marriages were less likely to receive it. Peters (1986: 449) finds otherwise, concluding that alimony was lower in unilateral (no-fault) states, although her analysis produces an *R*-square of only .048 and thus does not account for 95 percent of the variance in alimony payments. Our data are too skimpy to shed further light on these assertions.

Finally, we conducted a tentative analysis of the effects of type of divorce on home ownership among women who had not remarried the year after divorce. Only 82 cases had sufficient data for inclusion in the fault/no-fault comparison, 150 for inclusion in the fault/no-fault/mixed comparison, and 152 for inclusion in the Peters's classification.<sup>13</sup> While the tests are of relatively lower power because of the limited sample size, they suggest no substantial effect of type of divorce on post-divorce home ownership. Regardless of how we classified type of divorce, logit analyses show no significant relation with post-divorce home ownership in this sample, net of the social characteristics of the divorced women. Indeed, only age at divorce and employment before divorce were statistically significantly related to home ownership. Older women were more likely to own their home immediately after their divorce than younger women (probably because home ownership in general increases with age), and women who had worked before their divorce were less likely to own their home after divorce. While these results do not support the conclusions of Weitzman and Peters, the limitations of the data make a weak test.

#### IV. DISCUSSION

The no-fault divorce statutes contain no provisions that explicitly disadvantage women, and there is no indication that those who advocated and drafted the statutes had any intention to hurt or harm the economic position of women (Kay, 1987). Intentions, of course, do not exclude the possibility of unanticipated consequences, and Weitzman and Peters have claimed that unanticipated negative consequences did indeed occur, thereby disadvantaging women.

Our analyses, however, suggest that the effects of no-fault were either modestly benign or neutral to the economic interests of divorcing women. Regardless of how we categorized respondents, no-fault had little impact. Of the six equations presented in Tables 4 and 6, only one shows a statistically significant effect of no-fault, and it is benign rather than adverse. The other coeffi-

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<sup>13</sup> The varying numbers in each category are due to missing data.

cients for divorce type do not even come close to statistical significance.

These results may well be the product of our focus on younger women in contrast to the older cohorts examined by Weitzman and Peters. Younger women may in fact be less disadvantaged by divorce, because they enter the labor force at a younger age in a marketplace more hospitable to women. Moreover, they experienced less time out of the labor force than the older women in the other studies. However, in so far as they represent both the majority of women divorcing in the United States and the labor market conditions in which these women must compete, our findings are perhaps more pertinent to extrapolations to the future than those of Weitzman or Peters.

The absence of a discernible impact for no-fault should not surprise an observer of legal innovations. As we indicated at the outset, studies of the impact of legal change in a variety of settings have shown that laws often have a delayed and diffuse impact (Johnson and Canon, 1984). As Macaulay (1979) has explained with respect to Wisconsin consumer protection laws, and as Sarat and Felstiner (1986) have suggested with respect to the consultations between divorce clients and their attorneys, much "slippage" occurs between statutory language and actual practice. Knowledge of new rules tends to be limited to a small segment of the legal profession. The law's effect flows indirectly through the intervening activities of attorneys and judges, whose perceptions and values expand or contract the meaning given to statutory language.

We need, therefore, to conceptualize the impact of divorce law with a much richer paradigm. The model we inferred from Weitzman and Peters and used in our analysis assumed that law exerts direct effects on divorce outcomes in much the same manner as the socioeconomic characteristics of women and the macroeconomic conditions of society as a whole. In this model there are no intervening variables and no interaction effects.

Our analysis as well as the testimony of other observers of the divorce process suggests that such a paradigm misspecifies the divorce process. Since couples contemplating divorce almost never read the statutes themselves but rather rely on interpretations provided by lawyers and other advisors, we need to replace the statutory characteristics of the law with the interpretations given to couples by their advisors. Support for such a respecification comes from Erlanger *et al.* (1987: 1599), who vividly report the range of variation in attorney interpretations of divorce law in speaking of child support:

Some lawyers attempt to "divide hardship," that is, to make each parent absorb equal deficiencies of income. Others measure the adequacy of support by looking at the custodial parent's budget, trying to make sure the custo-

dian can make ends meet, or by looking at the supporting parent's ability to pay. Still others focus on a flat amount of support per child. Many lawyers also stress that their settlement strategy in any given case depends heavily on who is representing the other spouse. Thus, it is doubtful that parties receive consistent legal information and advice.

In addition, a model that seeks to explain the impact of the law upon a population needs to introduce interaction effects that can capture the differential experiences of various segments of the population. Social circumstances such as the wealth of clients will affect the settlement of property division and alimony, while a combination of wealth, motherhood, and education will influence child custody or child support. The affluent are likely to respond to legal provisions in a different manner than working class couples because they have different kinds of attorneys and different issues. The same may be true of childless couples and those with children. At the same time, women who are secure in their non-household occupation may experience different effects than full-time homemakers.

Our data do not permit a test of this model because we have no data on the key mediating forces: the kind of attorneys representing the divorcing spouses and the interpretations of divorce law they provide. Nor do the data provide sufficient numbers of affluent divorced women, since the sample is composed entirely of young women. The absence of these kinds of data may explain why our analyses have accounted for only a quarter of the variation in post-divorce income. To understand the impact of alternative legal provisions more fully, we shall need a data set specifically designed to reflect the social context in which divorce occurs.

Finally, the findings reported here should not divert our attention from the undisputed fact that divorce per se has enormously adverse economic consequences for many women. This conclusion is reported in numerous studies, as summarized by Bianchi and Spain (1986), and remains untouched by the findings in this paper. Indeed, for those concerned with the policy implications of divorce and divorce law, this paper indicates that no-fault proceedings have left the adverse effects of divorce largely unimpaired. To determine whether reforms of child support collection improve the financial condition of women we must wait until enough time has elapsed to discern whether changes, put in place only in 1984, have had their intended effect. Likewise, to examine the impact of changes in property settlement law, we need more carefully honed data focusing on those divorcing families that have assets to divide. Only then can we determine whether increasing the pool of assets available for division and the choice between the equitable and equal division rule have a discernible impact.

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