Did Unilateral Divorce Laws and No-Fault Divorce Laws Raise Divorce Rates?

By
Michael Joseph Horowitz[1]

Duke University

[1] Michael Horowitz is a 2006 Duke University graduate. He graduated with a B.S. in Economics and will be employed by Citigroup Corporate Banking as a Financial Analyst beginning in the Summer of 2006. His email is mjh18@duke.edu. He would like to thank Marjorie McElroy for assisting him in working through this paper, for authoring his appendix, and for serving as an excellent mentor overall. He would also like to thank Professor McElroy for proposing the examination of the independent effects of no-fault and unilateral laws on divorce rates. He would like to thank Connel Fullenkamp for providing helpful, informative, and clear feedback on his analyses and for discussing strategies to improve his paper. He would like to thank Jonathan Gruber and Justin Wolfers for supplying him with well organized and recently updated data. He would also like to thank Wolfers for his helpful and frequent, email correspondence. He would like to thank Leora Friedberg for her email correspondence and her advice on potential contacts and resources. He would also like to thank Margaret Brinig for her clarifying email. On a more technical note, he would like to thank Paul Ellickson for taking the time to help him work through a STATA problem. Finally, he would like to thank his Econ 193 colleagues who offered significant clarification, support, and positive feedback throughout the numerous stages of the development of this paper.

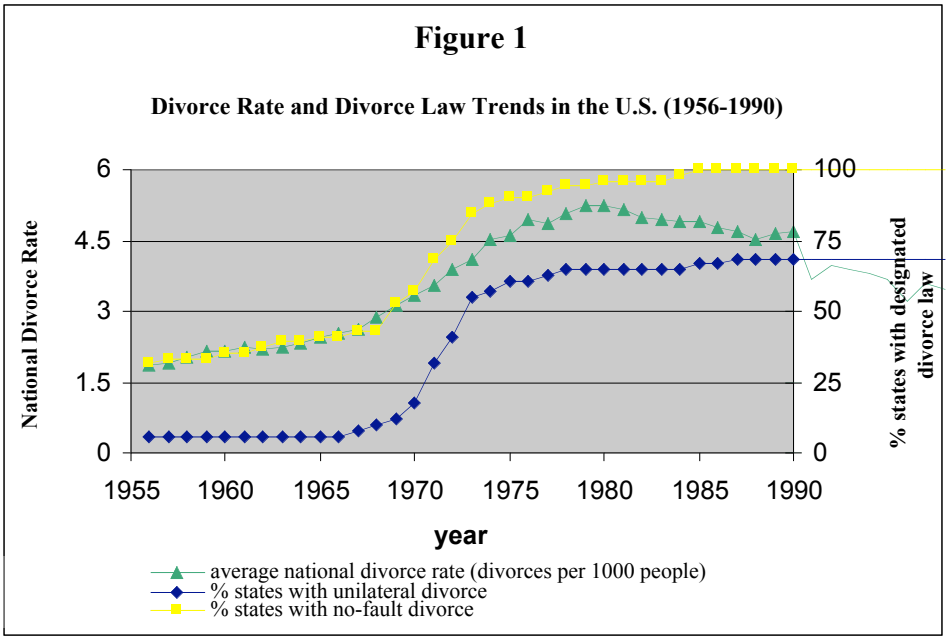
Abstract

This paper examines the independent and joint effects of no-fault and unilateral laws on divorce rates in the U.S. Past analyses have primarily focused on the effects of unilateral laws on divorce rates and have ignored the independent significance of no-fault laws in this relationship. Through the introduction of a new model, I find that no-fault laws, independently, have highly significant and positive effects on divorce rates and unilateral laws do not. This result shows that ignoring the independent significance of no-fault laws is detrimental to divorce law/divorce rate analyses and has implications for social and economic theory.

Introduction

The initial unilateral and no-fault divorce laws were enacted in the U.S. circa 1968, in response to systemic frustration among legal practitioners, and the general public, regarding prevalent fabricated legal marital controversies and divorce difficulties. Since that time, there has been a major liberalizing movement in divorce law as an increasing number of U.S. states have legislated both unilateral and no-fault laws. In practice and by definition, unilateral divorce laws ended the requirement that both spouses must commit to divorce and no-fault divorce laws ended the requirement of showing grounds for divorce. By 1985, a majority of the fifty states had successfully ratified some version of unilateral and no-fault divorce legislation. Over the same period, during which the majority of the United States implemented no-fault and unilateral laws, marital data from 1956 to 1998 plainly indicates that divorce rates dramatically increased (nearly doubling) from the late 1960s to the late 1970s. Soon after, the divorce rates began to level off and then decrease slightly (as illustrated in Figure 1, for the years 1955-1990). This phenomenon led most commentators to believe that the dramatic increase in divorce rates was largely a result of the introduction of the two categories of divorce laws. Today, many states are contemplating rolling back divorce legislation with the intent of reversing the substantial increase in divorce rates that took place during, and shortly after, the implementation of unilateral and no-fault divorce laws. This trend has brought to light several significant economic, statistical, and social questions. Of primary importance is the question of whether the rapid increase in divorce rates, which occurred throughout the late 1960s and the 1970s, can be attributed to the enactment of unilateral and no-fault divorce laws.

Figure 1



Notes: The national divorce rate is calculated using individual state divorce rates which are weighted by state population. Data on state specific divorce rates and state populations are taken from the most recent update (2006) of Justin Wolfers's research data for his paper, *Did Unilateral Divorce Raise Divorce Rates? A Reconciliation and New Results,* which can be obtained from his personal homepage (http://bpp.wharton.upenn.edu/jwolfers/index.shtml).
Data on when each state adopted unilateral and no-fault divorce is taken from Jonathan Gruber's (2004) paper, *Is Making Divorce Easier Bad for Children? The Long Run Implications Of Unilateral Divorce.*

The current U.S. literature on the topic of divorce law and divorce rates has primarily focused on the relationship between the rise in divorce rates and what economists often refer to as "unilateral" divorce laws. By the term "unilateral" economists have historically meant either unilateral laws in isolation (no-fault laws are absent), or unilateral laws with no-fault laws as a criterion (thus blurring the two distinct categories of laws into a single variable). For the remainder of this paper I will refer to both of these specifications of unilateral divorce laws as unilateral$_{cd}$; "cd" for common definition. Further, unilateral$_{cd}$ is most often treated, as its label would suggest, solely as a unilateral variable as if it encompassed laws specifically matching its conceptual definition and as if it solely explained the statistical significance of this definition; the independent significance of its no-fault component, or lack there of, is consequently ignored (many times the only mention of no-fault laws is in the fine print of the

footnotes, a fact I learned the hard way). Therefore, in both specifications of $unilateral_{cd}$, economists run the risk of misinterpreting the effects of unilateral laws by ignoring an obvious and independent counterpart, no-fault laws. Noteworthy authors such as Justin Wolfers, Leora Friedberg, Douglas Allen, and Elizabeth Peters have empirically tested the relationship between $unilateral_{cd}$ and divorce rates and have found mixed results supporting both sides of the issue; there have been no conclusions without significant debate. The debate has focused on definitions of the legislation, the time period of their testing, and whether geographical differences ought to be controlled. Unfortunately, the existence and importance of unilateral and no-fault divorce laws as independent variables and independent laws have been overlooked, constituting a potentially biasing oversight. The likely result of such an oversight is a misleading analysis of the explanatory power of unilateral and no-fault divorce laws. In such an analysis, the coefficient found on $unilateral_{cd}$ divorce will potentially attempt to explain the effects of unilateral laws, the effects of no-fault laws, and the effects of the interaction between the two categories of divorce law on divorce rates, a clearly misleading result. Overall, there is an apparent need to, at a minimum, examine the effects of unilateral and no-fault divorce laws simultaneously, yet independently, to determine whether the separate variables show statistical significance and can contribute to the debate over what led to the rapid increase in divorce rates from the mid 1960's to the late 1970's.

In the following paper I examine the distinct effects of unilateral and no-fault divorce laws on divorce rates. Economic theory suggests that unilateral laws should have no statistically significant effect on divorce rates and that no-fault laws should have a positive relationship with divorce rates (appearing to further compel an independent

analysis of the two categories of laws). By introducing a new model into the current literature, I illustrate that the effects of no-fault and unilateral divorce laws are consistent with the standard predictions of economic theory. This result will potentially put an end to the existing debate over the effects of unilateral divorce laws by illustrating that such laws have little to no statistically significant correlation with divorce rates. Furthermore, no-fault laws will finally be represented in a model as a distinct variable and will be shown to have an extremely statistically significant and positive relationship with divorce rates, in both the short-run (1-15 years after implementation) and the long-run (16-25 years after implementation). Specifically, no-fault laws are shown to bear responsibility for increasing the national divorce rate by an average of approximately 7% in the short-run and an average of approximately 13% in the long-run, following their state specific implementations. This result should redirect the focus of the current literature towards taking a greater, and rightfully warranted, emphasis on the effects of no-fault laws on divorce rates and attributing less explanatory power to unilateral laws.

This paper, in addition to redirecting the focus of current literature and debate, holds important implications for economic theory and public policy. My results support relevant economic theory regarding no-fault and unilateral divorce laws, and therefore champion the extensive application of such theory to marriage, marital bargaining, and divorce, an association frequently challenged by much of the past and current literature. My conclusions also prove significant in providing a more extensive (compared to previous literature) basis for beginning to address major public policy issues, such as the likely effects of repealing each category of divorce laws and whether these laws have affected the strength and structure of U.S. families.

In developing the model presented in this paper, I employ panel data including the following: data provided by Justin Wolfers of state-specific divorce rates, from his paper *Did Unilateral Divorce Raise Divorce Rates? A Reconciliation and New Results*, originally taken from the *Vital Statistics of the United States* for 1956-1998; in addition to data indicating when these states enacted unilateral and no-fault divorce laws obtained from a paper and excel spreadsheet authored by Jonathan Gruber, titled, *Is Making Divorce Easier Bad for Children? The Long Run Implications Of Unilateral Divorce* (it should be noted that this data and the analyses that follow focus exclusively on heterosexual couples). I subsequently create my model by utilizing this data to regress independent dummy variables, which capture whether a state adopted unilateral and/or no-fault divorce laws and for how long it has had these laws, on a dependent variable, "divrate," which captures state specific divorce rates. Following a model first developed by Leora Friedberg, I also include state- and time-specific fixed effects and state-specific linear and quadratic time trends in order to control for geographic trends, as well as nationally evolving trends, correlated with the dependent variable "divrate."

In the first section of my paper, I briefly discuss the history of unilateral divorce laws, no-fault divorce laws, and divorce rate trends. In Section 2, I briefly discuss the past and current literature relevant to my topic. In Section 3, I outline basic economic theories, the "Coase Theorem" and "costs to divorce," which warrant important consideration for my analyses. In Section 4, I present the data and model upon which I base my analyses. In Section 5, I discuss the results of running the model, and the statistical significance of the variables' coefficients. In Section 6, I discuss the results' implications for economic theory. In Section 7, I discuss the results' implications for

public policy related to divorce laws. In Section 8, I briefly discuss the topic of endogeneity and how it relates to my variables. In Section 9, I formulate my conclusions.

1. History of Unilateral Laws, No-Fault Laws, & Divorce Rates

Prior to unilateral and no-fault divorce laws, the U.S. had a legal system requiring mutual and fault divorce. Under this system, both partners, the married man and the married woman, had to jointly decide to end a marriage and/or they had to legally show fault as grounds for a divorce. Consequently, divorce was a somewhat grueling process, and difficult to achieve. As a result, many married individuals were forced to fabricate reasons, or grounds, for divorce, and subsequently persuade, convince, and/or manipulate their spouse into acceptance of these rationales for divorce (Wright & Stetson, 1978). The fabrications and manipulations involved with mutual consent and fault divorce inevitably spilled over into courts. Married couples would commit perjury under oath, insisting that they or their spouse had been adulterous, or they would invent fictitious scenarios showing some other grounds for fault (e.g. abandonment or abuse) (Katz, 1994). An individual who wanted to terminate a marriage without the consent of their spouse was forced to either negotiate with their spouse, or to seek assistance from the courts. This resulted in unhappy marriages whose partners could not achieve termination, an inordinate amount of court time devoted to divorce cases, "expensive divorce proceedings," and a legal system in which individuals told lies and behaved disingenuously (Wright & Stetson, 1978, p. 575). One California Supreme Court Justice described the scene as follows:

> Every day, in every superior court in the state, the same melancholy charade was played: the "innocent" spouse, generally the wife, would take the stand and, to the accompanying cacophony of sobbing and nose-

> blowing, testify under the deft guidance of an attorney to the spousal conduct that she deemed "cruel." (Mosk, 1972, para. 21)

In response, around 1970, the public and the courts decided the system was in dire need of revision, and adopted unilateral and no-fault divorce legislation. Typically, most states enacted no-fault divorce laws prior to unilateral laws, especially in the case of a handful of early movers who implemented no-fault divorce in the early 1900s (these states are discussed and controlled for in my later analyses). These divorce laws stated that married couples were no longer required to show fault as grounds for a divorce. Unilateral divorce laws were generally passed soon after, or simultaneous to, no-fault laws. Unilateral laws ended the requirement that both spouses had to commit to a divorce. The conceptual view for passing both categories of divorce legislation was to "bring law books into alignment with actual practice, and to eliminate the need to assign blame during a divorce, *but not to make divorce easier*" (Friedberg, 1998, p. 609; my emphasis). Currently, all fifty states have enacted some version of no-fault divorce laws, and roughly 65% of the states have enacted unilateral divorce laws, over the same time span (Gruber, 2004). Simultaneous to this systemic legal change, the divorce rate, although already gradually increasing, immediately rose dramatically more than doubling (from approximately 2.2 individuals per 1000 in the 1960s to 5.2 per 1000 in the late 1970's, as illustrated by Figure 1). Since that historic period of time, economists, legal practitioners, and sociologists alike have debated the extent to which the increase in divorce rates can be attributed to unilateral and no-fault laws, but no consensus has emerged.

2. Past & Current Literature

Historic and current literature relating the effects of divorce laws on divorce rates has focused primarily on the correlation between unilateral divorce laws in isolation, or with no-fault grounds, and divorce rates. The debate regarding this statistical relationship notably began with a paper written by Peters (1986). In this paper Peters employed a "contract-theoretic framework" to compare two marital bargaining structures: one assuming asymmetric information in marriage, and the other assuming symmetric information a.k.a. the Coase Theorem (p. 438). Peters empirically argued that the symmetric framework most effectively described marital bargaining schemes, and she subsequently concluded that unilateral_{cd} divorce laws do not have an effect on divorce rates. Although Allen (1992), utilizing the same data as Peters, countered Peters's paper by arguing that unilateral_{cd} laws shared a direct relationship with divorce rates, Peters (1992) was quick to issue a rebuttal. Their debate revolved around the specification of what constitutes a unilateral state, and whether dummy variable controls for geographical discrepancies, in state specific divorce tendencies, are necessary for analyzing the data (Allen 1992).

More recently, Friedberg (1998) presented a new model for analyzing the relationship between divorce law and divorce rates. Using time-series longitudinal data, Friedberg examined the effect of several specifications of unilateral_{cd} laws on divorce rates by employing a model that incorporates state- and year-specific fixed effects, in addition to linear and quadratic time trends. These specifications allowed Friedberg to dispel issues involving geographic difference across states and issues of endogeneity, which were raised in earlier analyses (Wolfers, 2005). As a result, Friedberg's method

became "a seemingly appealing alternative to earlier studies…[and] has since been widely accepted" (Wolfers, 2005, p. 1). With her now generally acclaimed model, Friedberg (1998) concluded that $unilateral_{cd}$ divorce laws were responsible for approximately 17% of the rise in divorce rates between 1968 and 1988.

One of the most current pieces of literature on the topic of divorce law and divorce rates is that of Justin Wolfers. By incorporating additional variables into Friedberg's model, Wolfers (2005) subsequently improved upon the model produced by Friedberg. Using a series of dummy variables to account for the number of years a state has had $unilateral_{cd}$ laws, in contrast to Friedberg's single $unilateral_{cd}$ dummy variable, Wolfers emphasized and observed more of the dynamic effect of $unilateral_{cd}$ laws (and consequently no-fault laws, due to variable criteria) on divorce rates. Further, Wolfers analyzed a significantly larger time range than Friedberg did in an attempt to decrease the potential for biased conclusions founded on a too narrowly focused time period. It is for these reasons that I believe Wolfers's model and methodology represent the best current approach to analyzing the effects of divorce laws on divorce rates, and I elect to base my model upon his in significant manners. With his "dynamic" model, Wolfers (2005) concluded that $unilateral_{cd}$ laws raised divorce rates for roughly a decade but thereafter, their effects on divorce rates disappeared.

3. Basic Economic Theories

- Coase Theorem -

The relevant economic theory used to relate unilateral laws to divorce rates is the Coase theorem. The Coase theorem suggests that all possible allocations of property, between parties, are equally efficient, provided there are no transaction costs. The basic

underlying theoretical construct is that parties will barter with each other, based on individual preferences, until the true/best allocation of property is met. The application of this theory to divorce is addressed in *A Treatise on the Family*, in which the author, Gary Becker (1991), relates the Coase theorem to marriage and divorce. Becker delineates a hypothetical model in which he states that, given mutual divorce laws, two married individuals, devoid of transaction costs and imperfect information, will divorce only when they find themselves better off in doing so. Becker describes this model as "a special case [or application,]…of the Coase theorem" and suggests that in a marriage a couple will engage in a bargaining process and will only divorce if separation is the most efficient outcome of the process (p. 331). Accordingly, Becker suggests that the Coase theorem, when applied to marriage, implies that the move from mutual divorce to unilateral divorce should have no effect on divorce rates. The basic assumption is that the change in divorce laws should only affect the bargaining and distribution of property within marriage, not make divorce a more likely/efficient outcome. Essentially, there would be a role reversal where in mutual divorce the individual who wanted to end the marriage would have to compensate the other to do so, whereas in unilateral divorce, the individual seeking to end the marriage would, conceptually, have to be compensated by the spouse who wished to maintain the marriage. However, whether divorce is considered an efficient outcome would remain unchanged.

- Costs to divorce -

The relevant economic theory used to relate no-fault laws to divorce rates involves costs to divorce. Theoretically, no-fault divorce laws lower the cost to divorce. This result emanates from the fact that showing fault is often expensive and not an easy

task (involves time, money, and energy), especially if "true fault" doesn't exist (Wright & Stetson, 1978). In order to show fault, couples often spend a significant amount of time in divorce courts, hire lawyers, and sometimes even devise and act out elaborate schemes to fabricate fault in their divorce (Katz, 1994). By instituting no-fault laws, the costs to divorcing are substantially reduced as the costs associated with fault are eliminated. Thus, divorce can be seen as easier to obtain and potentially more attractive. As a result, we can expect to see a rise in divorce rates as a greater number of individuals take advantage of the reduced costs to divorce. Theory therefore predicts that not only should no-fault divorce laws have a statistically significant effect on divorce rates, but also a positive one, leading to a greater number of individuals seeking and obtaining divorce.

- Combined Effect -

The "combined effect," or interaction effect, refers to whether, together, unilateral and no-fault laws existing in a given state, have a greater, or lesser, effect on divorce rates than their individual direct effects. The fundamental question is, does either type of divorce law mitigate, or enhance, the other and subsequently cause so-called combined effects on divorce rates? Otherwise stated, does 1+1= 1 or = 3? Relevant theory for this question relates the independent theories for unilateral laws (Coase Theorem) and for no-fault laws (costs to divorce). Pure theory appears to suggest that there should be no combined effect, beyond the independent effects of unilateral and no-fault laws, on divorce rates. The Coase Theorem and costs to divorce do not seem to be directly related, nor do they suggest significant interaction (i.e. interference or support from one to the other). Therefore, we can expect that there will be no combined effect from the two types of divorce laws on divorce rates.

- Overall Theory -

As discussed above, theory suggests results illustrated by the following Table:

Table 1: Theoretical Outcomes of Divorce Law Implementation on the National Divorce Rate

Expected effect on the national divorce rate:	**No Unilateral**	**Unilateral**
No No-Fault	0	0
No-Fault	+ (from no-fault)	+ (from no-fault, no combined effect)

As illustrated by Table 1, theory suggests that if a state has neither unilateral nor no-fault divorce laws, there should unarguably be no effect on that state's divorce rate. If a state has unilateral divorce laws and no no-fault laws, again, there should be no effect on its divorce rate. Finally, if a state has no-fault divorce laws alone, or in combination with unilateral divorce laws, the state should experience a rise in divorce rates due solely to the effect of no-fault laws on the probability of divorce.

3. Model & Data

The fundamental question that I will set out to answer is what is the effect of the shift from mutual and fault divorce to unilateral and no-fault divorce on the national divorce rate, and therefore the likelihood of divorce? To do so, I must construct a model that correlates divorce rates with the legal changes. Using state-specific panel data to best capture cross-state trends over time, my model is defined by Equation 1.

(Eq. 1) $\text{divrate}_{i,t} = \_{q\geq 1}\, \_q * (\text{unilateral + interaction have existed for q years})_{it} +$

$\_{s\geq 1}\, \_s * (\text{no-fault has existed for s years})_{i,t} + \_i\,(\text{state})_i + \_t\,(\text{year})_t + \_{i,t}$

$(\text{ttrend})_{i,t} + \_{i,t}\,(\text{qtrend})_{i,t} + \_{i,t}$

The model I employ is largely based on one utilized by Wolfers (2005). For this reason, I will on occasion reference his findings, and use some of his work as a

benchmark for several of my conclusions throughout this paper. My model differs from Wolfers's solely in my simple, but important, separation of unilateral and no-fault divorce laws into two distinct series of variables (explained below). Instead of the two series of variables represented in my model by the terms "unilateral + interaction have existed for q years" and "no-fault has existed for s years," Wolfers has a single variable series labeled "unilateral divorce has been in effect for k periods" which has as its criteria both unilateral laws and no-fault grounds. To implement this separation, I use data coded by Jonathan Gruber. These data are the only I am aware of which separate the two categories of divorce laws, are accompanied by legal documentation references, and were recently updated.

Equation 1 describes a weighted least squares regression. The regression is weighted using analytic weights of the state populations for the relevant years analyzed. "$Divrate_{i,t}$" is the dependent variable, and it delineates the empirical divorce rate (measured in number of divorced individuals per 1000 people) for each state given a specific year (data provided by Wolfers, currently available on his personal webpage). The subscripts on divrate, and those found on all other coefficients within the model, designate, "i," the state which is being observed and, "t," the year in which the variable is being observed; observations readily made thanks to the use of cross-section time series data.

"Unilateral + interaction have existed for q years," which uses data from the paper, *Is Making Divorce Easier Bad for Children? The Long Run Implications Of Unilateral Divorce?,* by Gruber (2004), is a series of dummy variables that signify the number of years (using two year increments, i.e. 1-2 yrs, 3-4 yrs, ...) a state has had

unilateral divorce laws, and when the state enacted these laws. Additionally, because unilateral laws are always implemented at the same time or following no-fault laws (according to Gruber's data), the variable series "unilateral + interaction have existed for q years," also describes the number of years a state has undergone the combined/interaction effect of unilateral and no-fault divorce laws (for a mathematical delineation of this combination, and its lack of bias for my analyses, see Appendix A). "No-fault has existed for s years," which uses data from the same Gruber paper, is another series of dummy variables that signify the number of years (using the same two-year increments) a state has had no-fault laws, and when the state enacted these laws. Of primary importance are $\_q$ and $\_s$, the coefficients on the variable series "unilateral + interaction have existed for q years" and "no-fault has existed for s years." The values of these coefficients describe the magnitude of, and the relationship between, the independent and direct effects (or marginal effects) of these laws, and their interaction, on divorce rates. By the term "direct effects," I am referring to the marginal effect of these variables on divorce rates, beyond existing state, time, and divorce rate trends (discussed below), which are continually affecting divorce rates across the United States. Otherwise stated, the direct effects of the laws are their impact on divorce rates, all else being constant. Therefore, specifically, values for $\_q$ explain the direct effect of unilateral laws and the direct effect of the combination of unilateral and no-fault laws on divorce rates, and the magnitude of these effects, given q years following unilateral implementation. Values for $\_s$, specifically, explain the independent direct effects of no-fault laws on divorce rates, and the magnitude of these effects, given s years post no-fault implementation.

The model also includes several series of control variables. These variables are designed to isolate the direct effects of unilateral and no-fault laws on divorce rates by controlling for all other trends, between 1956 and 1998, which potentially influence divorce rates. In the model, "state" represents state-specific fixed effects, a set of binary variables. The state fixed effects embody all factors that vary among states, but are constant over time. These fixed effects are crucially important to the regression because they are responsible for controlling omitted variable bias, which may arise from variables that are correlated with divorce rates, and are different among states, yet relatively constant over time; examples include religious orientation, substance abuse (e.g. alcohol) legislation, average income, and all other state-specific characteristics which potentially affect divorce rates. "Year" indicates time-specific fixed effects. Time effects, similar to state effects, are responsible for encompassing and controlling for all variables that vary with respect to time but are constant among states. Examples include the women's liberation movement and society's attitudes toward divorce. Thus, the fixed effects (state and time) variables are important for ensuring that the regression covers geographical and time-specific trends, which may influence the dependent variable and cause a biased estimate of the coefficients found on the unilateral + interaction and no-fault series of variables.

The model also includes variables controlling for linear and quadratic time trends. A major motivation for including these trends, as illustrated in Figure 1, is the already increasing trend in the national divorce rate that clearly exists between 1956 and the mid-1960s. This trend is unlikely an effect of unilateral or no-fault laws, as very few states had these laws during this period, however, in the absence of control variables the

increasing trend is potentially statistically attributable to these laws. Further, in the absence of the legal changes, or "behind the scenes" of the influence on divorce rates posed by the legal changes, it is likely that the increasing divorce rate trend would persist; it is therefore important to identify how the legal changes are affecting divorce rates and what is already an ongoing trend of these rates. Linear and quadratic time trends are reliable tools for ensuring that trends, which are potentially linear or quadratic, such as the increasing divorce rate trend, are successfully controlled for and explained. Specifically, in the model, "ttrend" (which takes on the value of state*(a normalization of the year i.e. 1956 = = 1, 1957 = = 2, 1958 = = 3, ...) allows for the slight loosening of the restrictions posed by the fixed effects variables by relaxing the constraints that these unobservable factors of divorce are constant over time or state. Ttrend, therefore, allows for, and captures, unobserved factors influencing divorce that take on a linear trend and may vary across states. Finally, "qtrend" (which takes on the value of state*[(a normalization of the year)^2] further loosens the restrictions posed by the fixed effects variables by capturing unobserved factors influencing divorce which take on a quadratic trend and may vary across states.

There are several additional arguments for including linear and quadratic time trends. As Friedberg (1998) points out, and as found in this paper, linear and quadratic time trends are jointly statistically significant throughout all regressions in which the relationship between unilateral and no-fault laws and divorce rates is estimated. Consequently, their inclusion is in part necessitated by their statistically significant role in this relationship. This statistical significance suggests the existence of linear and quadratic trends that must be controlled for, otherwise risking potential bias on findings

related to other coefficients. Further, this statistical significance also illustrates that these variables are not added merely to boost adjusted R^2 values. Additionally, as Wolfers (2005) noted, the inclusion of linear and quadratic time trends has become standard practice when employing "difference-in-difference" estimation methods, such as those used in this paper (p. 7). Thus, through my analyses, I have adhered to the rules and methods used in past studies. Finally, the low contribution of these variables to the adjusted R^2 values presented in this paper, as illustrated in Table 9 and discussed below, illustrates that these variables are not significantly driving the results of my analyses and do not likely pose a major threat of inducing spurious regressions or issues of multicollinearity.

Thus, overall, the control variables are a vital part of the model as they are designed to "flush out" (and capture) the statistical significance of all potential predictors of divorce rates, excluding no-fault and unilateral laws. As a result, the control variables ensure that the coefficients, which I find for the unilateral + interaction and no-fault series, are an estimation of their true direct effects, or marginal effects above all other statistically significant predictors, on divorce rates. Further, by controlling for all potential influencers of divorce rates, the multitude of control variables included in the model ensure that the regressions run do not suffer from omitted variable bias, which may stem from variables that are correlated with divorce rates, yet are absent from the model.

4. Results & Coefficients

- Replicating Wolfers -

Wolfers (2005) measured the dynamic effect of the change from mutual to unilateral$_{cd}$ divorce (in Wolfers's case, unilateral with no-fault grounds) by tracing out the path of unilateral's effect on divorce rates between 1956 and 1998. One of Wolfers (2005) main criticisms of Friedberg revolved around the sensitivity of her results to the inclusion of state-specific controls. As a result, using Friedberg's basic model setup, Wolfers included a series of dummy variables labeled "unilateral divorce has been in effect for k periods," to capture the trending effect of unilateral$_{cd}$ divorce in an attempt to stop state-specific control variables and a single unilateral variable from explaining this dynamic change (Wolfers, 2005, p. 8). Using data from the *Vital statistics of the U.S.*, Wolfers estimated:

(Eq. 2) Divorce Rate$_{i,t}$ = $\_{k\geq1}$ $\_k$*(unilateral divorce has been in effect for k periods)$_{i,t}$ + $\_i$ (state) $_i$ + $\_t$ (year) $_t$ + $\_{i,t}$ (ttrend)$_{i,t}$ + $\_{i,t}$ (qtrend)$_{i,t}$ + $\_{i,t}$

Equation 2 is essentially a restated Equation 1 without separate variables for the length of time a state has had no-fault divorce. Instead, in equation 2, Wolfers makes no-fault laws a criterion of unilateral$_{cd}$ divorce. Because my model is closely related to Wolfers's, and because Wolfers was professionally generous in sharing a significant portion of his data with me, it is important to replicate his analyses as a way of ensuring that my model is correctly specified and applied. Running my model for Equation 2, using Wolfers's data, returns the results found in Table 2. The numbers found in Table 2 differ slightly from those reported in Wolfers's (2005) paper; however, upon further analysis, investigation, and discourse with Wolfers, the numbers in Table 2 were determined to be the correct

coefficients; the numbers reported in Wolfers's (2005) paper are not updated to his most recent dataset (they are correctly specified in Wolfers's separate STATA.do file analyses and a more recent version of his paper currently in press with the AER, which are both available on his personal homepage).

Table 2: Replicating Justin Wolfers (15+ specification 1956-1998)

Dependent Variable: Divorce Rate (individuals per 1000)			
Specification:	**With Fixed Effects**	**With Linear Time Trends**	**With Quadratic Time Trends**
Unilateral Years 1-2	.27*	.34*	.30*
	(.08)	(.06)	(.05)
Unilateral Years 3-4	.21**	.32*	. 29*
	(.09)	(.07)	(.07)
Unilateral Years 5-6	.16***	.30*	.29*
	(.08)	(.08)	(.10)
Unilateral Years 7-8	.16***	.32*	.35*
	(.08)	(.08)	(.10)
Unilateral Years 9-10	-.12	.08	.16
	(.08)	(.09)	(.12)
Unilateral Years 11-12	-.32*	-.10	.05
	(.08)	(.10)	(.14)
Unilateral Years 13-14	-.46*	-.20***	.03
	(.08)	(.11)	(.17)
Unilateral Years 15+	-.51*	-.21***	.25
	(.08)	(.12)	(.21)
Year Fixed Effects	Yes F=145.04	Yes F=52.87	Yes F=72.41
State Fixed Effects	Yes F=220.29	Yes F=214.81	Yes F=135.02
Linear Time Trends	No	Yes F=70.13	Yes F=37.24
Quadratic Time Trends	No	No	Yes F=19.10
Adjusted R^2	.9310	.9732	.9822

* Statistical significance at 1% level ** Statistical significance at 5% level *** Statistical significance at 10%

Notes: Standard errors are in parentheses. Weighted using state populations.

From Table 2 it is easy to ascertain why Wolfers (2005) concluded that, contrary to economic predictions, unilateral laws increased the divorce rate for roughly a decade, specifically eight years. Throughout each of his trials (each trial is represented by a column), his unilateral coefficients for the first eight years after implementation are highly significant and robust. Additionally, his Adjusted R^2 values increase with the

inclusion of his control variables, all of which have F-statistics which are statistically significant at the 1% level.

In an attempt to describe a greater portion of the long-run dynamic effects of unilateral laws on divorce rates, Wolfers (2005) ran a second regression which included dummy variables for years since unilateral implementation up to a 25+ year specification (as compared to the 15+ year specification in the first regression) for the years 1956-1998. After running Wolfers's numbers and my model to replicate this regression, I receive the data found in column 2 of Table 3. Again, the numbers found in Table 3 differ slightly from those reported in Wolfers's (2005) paper; however, it was again determined that the numbers found in Table 3 represent the correct statistics for the variables; the numbers exactly match those specified in Wolfers's STATA.do file analyses and a recent update of his paper which can again be found on his website.

Table 3: Replication of Justin Wolfers (25+ specification 1956-1998)

Dependent Variable: Divorce Rate (individuals per 1000)		
Column:	**1**	**2**
Variable:	**Unilateral short-term**	**Unilateral long-term**
Years 1-2	.30* (.05)	.29* (.06)
Years 3-4	. 29* (.07)	.26* (.06)
Years 5-6	.29* (.10)	.25* (.08)
Years 7-8	.35* (.10)	.28* (.09)
Years 9-10	.16 (.12)	.06 (.11)
Years 11-12	.05 (.14)	-.09 (.13)
Years 13-14	.03 (.17)	-.15 (.15)
Years 15-16	.25 (.21)	-.04 (.17)
Years 17-18		-.017 (.20)
Years 19-20		-.03 (.23)
Years 21-22		-.05 (.26)

Years 23-24		-.02 (.30)
Years 25+		-.15 (.34)
Adjusted R^2	.9822	.9847
Includes: State and Year Fixed Effects, Linear Time Trends, and Quadratic Time Trends		
All have prob>F = .000		
* Statistical significance at 1% level, ** Statistical significance at 5% level, *** Statistical significance at 10%		

Notes: See notes to Table 2

From Table 3, Wolfers had a better sense of the overall short- , and now long-run effects, of unilateral divorce laws. Again, it is clear that unilateral laws remain highly significant at the 1% level, over the first eight years following implementation, further illustrating their robustness. Further, these variables are somewhat sizeable when compared to the weighted average divorce rate over the period 1956-1998 of 4.07 individuals per 1000. On the other hand, the long-term effects of unilateral laws are shown to be statistically insignificant beginning in year nine and continuing beyond the 15+ years specified in Table 2 to 25+ years illustrated in Table 3. This long period of insignificance in the latter years of having unilateral laws suggests that the statistically significant relationship between unilateral laws and divorce rates is limited to the decade following unilateral implementation and dissolves thereafter.

From the two replications described above, it appears clear that my model is correctly coded and applied. However, returning to the matter at hand, for both the analyses above, the importance of no-fault laws as an independent variable, or variable series, has been ignored. The laws are included as a criterion of unilateral divorce, yet, are not analyzed as a potential rationale for the explanatory power attributed to unilateral laws. This is particularly surprising since it appears that theory suggests no-fault laws would be the primary cause for any explanatory power found on the joint unilateral variable. As a result of this oversight, we now turn to my preferred specification.

- Preferred Specifications -

My preferred model is that described by Equation 1. This model includes an analysis of no-fault laws in combination with unilateral laws and the interaction of the two categories of divorce legislation. The data I use from this point on diverge from Wolfers's data (mine is coded by Gruber (2004) as opposed to the Friedberg (1998) data which Wolfers uses); as a result, I cannot directly compare my results to his, although I can compare general trends. Further, for all regressions run for the remainder of my paper, I exclude no-fault data on a collection of states which I refer to as "early movers" (or simply anomalies). These states all implemented no-fault divorce prior to 1932, which is not characteristic of the majority of the United States, and constitute potential biases for my analysis, which focuses on the period 1956-1998. Excluding these states' no-fault data does not significantly affect any of my results (which I have tested), but, instead, allows me to paint a clearer picture of the dynamic effects of unilateral and no-fault divorce laws. Thus, all trends I find and report are robust to this omission, a fact about which I have remained vigilant.

Running my preferred model solely using unilateral data (emulating a unilateral_{cd} specification of unilateral laws) returns the results found in Table 4. These results show the same general trend as those found by Wolfers, yet they are slightly different in magnitude. Differences in magnitude can be attributed to the different data sources and legal criteria used. Nonetheless, it would appear that unilateral laws share a statistically significant and positive relationship with divorce rates for roughly a decade. However, in this analysis, like Wolfers's, unilateral_{cd} laws are potentially explaining not only their own direct effect on divorce rates, but also the direct effects of no-fault laws and the

interaction between the two categories of divorce laws on divorce rates. To end my analysis here, and suggest that unilateral laws caused a statistically significant increase in divorce rates each year for roughly a decade, is potentially misleading.

Table 4: Preferred Specification for Unilateral Alone (15+ specification 1956-1998)

Dependent Variable: Divorce Rate (individuals per 1000)	
Variable:	**Unilateral**
Years 1-2	.20* (.05)
Years 3-4	.15* (.06)
Years 5-6	.15** (.06)
Years 7-8	.13*** (.07)
Years 9-10	.01 (.07)
Years 11-12	-.08 (.08)
Years 13-14	-.12 (.09)
Years 15+	.05 (.10)
Adjusted R^2 = .98	
Includes: State and Year Fixed Effects, Linear Time Trends, and Quadratic Time Trends **All have prob>F = .000** * Statistical significance at 1% level, ** Statistical significance at 5% level, *** Statistical significance at 10%	

More descriptive results are presented in Table 5. In Table 5, my preferred model (Equation 1) is run for the short term 15+ specification, using both no-fault and unilateral data for each individual state. This specification of my model is also determined by an F-test to be significantly better than that depicted in Table 4 which can be rejected at the 1% level.

Table 5: Preferred Specification (15+ specification 1956-1998)

Dependent Variable: Divorce Rate (individuals per 1000)		
Variable:	**Unilateral + Interaction**	**No-fault**
Years 1-2	.30* (.07)	-.06 (.05)
Years 3-4	. 11 (.07)	.10*** (.05)
Years 5-6	.08 (.08)	.15* (.06)
Years 7-8	-.01 (.08)	.24* (.06)

Years 9-10	-.08 (.08)	.15** (.07)
Years 11-12	-.26* (.09)	.18** (.07)
Years 13-14	-.35* (.10)	.25* (.08)
Years 15+	-.19*** (.11)	.33* (.09)
Adjusted R^2 = .98		
Includes: State and Year Fixed Effects, Linear Time Trends, and Quadratic Time Trends **All have prob>F = .000** * Statistical significance at 1% level, ** Statistical significance at 5% level, *** Statistical significance at 10%		

Notes: See notes to Table 2

From Table 5 it becomes clear that unilateral laws, and the interaction between these laws and no-fault laws, are not highly significant for the period of roughly a decade, but instead appear significant solely for the two years following unilateral implementation (in the first decade). Further, excluding the first two statistically significant years, the unilateral + interaction coefficients are sizably reduced, as compared to the coefficients reported in Table 4. Immediately, this seems to suggest that observing unilateral laws in the absence of no-fault laws, or with no-fault laws as a criterion for unilateral divorce, leads to the attribution of statistical significance and coefficient magnitude that is unmerited to unilateral laws; this statistical significance and coefficient magnitude more likely primarily belongs to no-fault laws. The statistical significance of unilateral laws and the interaction in select latter years (11-15+), although seemingly interesting, will later be shown to not be reminiscent of the long-run trend of the variable series. However, the negative coefficients found on unilateral + interaction for the latter years of the regression are reminiscent of the long-run effects of unilateral divorce on divorce rates. This phenomenon was also observed by Wolfers (see table 3 "Unilateral Long-Term") and appears to suggest that states that enacted unilateral divorce laws underwent

a reduction in divorce rates beginning approximately the decade following implementation which continued on thereafter.

No-fault laws, on the other hand, are shown to be highly significant and positive for all years, excluding years one and two, following no-fault enactment (see Table 5). This result suggests that in each year following the implementation of no-fault laws, in a given state, the divorce rate raises as a result of these laws. The magnitudes of the coefficients on the number of years a state has no-fault laws are also quite notable. Of the statistically significant coefficients, they average .2 individuals per 1000. Comparing these coefficients to an average national divorce rate of 4.07 individuals per 1000, in the United States over the period 1956-1998, no-fault laws are shown to increase the national divorce rate by an average of 5% in the short run (1-15 years) (illustrated by Equation 3 below).

(Eq. 3) $[(.10 + .15 + .24 + .15 + .18 + .25 + .33)/7]/4.07 \approx .05$ or 5%

To fully understand the statistical insignificance of the initial two years following no-fault implementation on divorce rates, which will later be shown to be robust throughout all regressions, I first explain the meaning of these years in my specification and my data. It is important to note that, "years 1-2" found in Table 5 and all other tables are not a full two years. The manner in which the Gruber data, which delineates when each state enacted unilateral and no-fault divorce laws, is specified in such a way that the year in which the laws are implemented becomes the first year of that category of laws' life. For example whether a state enacted no-fault laws on January 1, 1970 or December 31, 1970, the legal change is coded the exact same way, the first year of having no-fault laws as being 1970. Thus, it is safest to say that the lag on the statistical significance of

no-fault laws on divorce rates is one and a half years (assuming each day of the year has an equal chance for being the one on which enactment of a given category of divorce law occurs). With that said, it appears that there is a lag relating to the statistically significant effect of no-fault divorce laws on divorce rates. One possible explanation for this lag is simply word of mouth; it may be that it takes time to get the word out regarding the legal change. Another possible rationale is that there is some sort of adjusting time required for the legal change; this adjustment may be in the legal, social, or public realm. Nonetheless, there seems to be a realistic period of just over a year after implementation, in which no-fault laws do not significantly affect divorce rates.

In an attempt to capture more of the long-term dynamic effects of unilateral and no-fault divorce laws, I regress my preferred model for a 25+ specification. The results of this regression are found in Table 6.

Table 6: Preferred Specification (25+ specification 1956-1998)

Dependent Variable: Divorce Rate (individuals per 1000)		
Variable:	**Unilateral + Interaction**	**No-Fault**
Years 1-2	.31* (.07)	-.04 (.05)
Years 3-4	. 11 (.08)	.12** (.05)
Years 5-6	.09 (.09)	.19** (.06)
Years 7-8	.00 (.10)	.30* (.07)
Years 9-10	-.07 (.12)	.22* (.08)
Years 11-12	-.24*** (.14)	.27* (.09)
Years 13-14	-.32** (.16)	.35* (.10)
Years 15-16	-.13 (.18)	.39* (.11)
Years 17-18	-.11 (.21)	.44* (.12)
Years 19-20	-.14 (.24)	.52* (.13)
Years 21-22	-.19 (.28)	.57* (.14)

Years 23-24	-.04 (.31)	.51* (.15)
Years 25+	-.13 (.36)	.53* (.17)
Adjusted R^2 = .98		
Includes: State and Year Fixed Effects, Linear Time Trends, and Quadratic Time Trends **All have prob>F = .000** * Statistical significance at 1% level, ** Statistical significance at 5% level, *** Statistical significance at 10%		

Notes: See notes to Table 2

Table 6 presents a substantially clearer picture of the overall effects of unilateral and no-fault laws on divorce rates. Examining the unilateral + interaction series of variables, it appears that their statistical significance is primarily isolated to the first two years following unilateral implementation. These significant years also maintain a magnitude of roughly .3 and are robust to all regressions run. A magnitude of .3 is quite notable as it represents approximately a 7% average increase in the national divorce rate for the first two years following unilateral implementation (again compared to an average national divorce rate of 4.07). Whether the statistical significance and magnitude of the coefficient found on the first two years of the unilateral + interaction series of variables can be attributed to unilateral laws, the interaction between unilateral laws and no-fault laws, or both is unknown. However, it seems more likely that the significance and magnitude of this coefficient stems from some type of interaction existing between the two categories of divorce laws (potential reasons for the interaction are discussed in section 5, Economic Theory Implications). There are two rationales for this conjecture, both of which are based on the dynamic effects of no-fault laws. First, the significance of unilateral laws and the interaction is not representative of an ongoing trend. The significance comes in the first two years post unilateral implementation and then ends. I would hypothesize that any significance of unilateral laws would act similarly to those of other statistically significant divorce laws such as no-fault laws. Because no-fault laws

are shown to maintain a statistically significant trend for many years, and the significance on the unilateral + interaction coefficients instead exists solely for two years (or even less if you consider that the first is not a full year) and then disappears, I am led to believe that the significance of the unilateral + interaction series is not emanating from the divorce law (unilateral) but the interaction part of the series of variables. Second, because no-fault divorce laws exhibit a slightly less than two-year lag, likely due to word of mouth delays, I would expect other divorce laws, such as unilateral laws, to do the same. Accordingly, because the coefficients on the unilateral + interaction series of dummy variables are significant solely for the first two years (ignoring a few later years that appear to be irregularities in the overall trend of the variable series), this significance is likely the result of the interaction aspect of the series. Otherwise stated, the two years of significance found on the unilateral + interaction series of variables, is likely attributable to the effect on divorce rates caused by a state simultaneously having both unilateral and no-fault divorce laws, not unilateral laws alone. Again, it is important to remember that the discussion above is solely my conjecture, and a definite answer requires additional analysis; however, the significance and trend of the unilateral + interaction series of dummies does not seem reminiscent of what one would expect from a statistically significant divorce law (based on observations of no-fault laws' significance and effect on divorce rates). For this reason, I will often refer to this significance as belonging to the interaction of unilateral and no-fault laws.

The remaining unilateral + interaction coefficients are primarily statistically insignificant. Further, they appear to trend insignificantly leading one to expect that even further reaching specifications (i.e. 35+, 45+, ...) will also show insignificance for added

latter years (a fact which will be confirmed in the next specification, Table 7). Also, the results again suggest a negative trend for the series beginning approximately a decade following unilateral enactment.

The trends and effects of no-fault laws on divorce rates, found in Table 6, are also quite informative and notable. Again, there seems to be a less than two year lag on the significant effects of no fault laws on the divorce rate. However, no-fault laws are highly significant for all years beyond the initial two years. Further, the statistical significance and the magnitude of the coefficients found on the no-fault series have both increased from the initial 15+ specification found in Table 5; a direct comparison can be found in Table 7.

Table 7: No-Fault Comparison (Short Term 15+ versus Long Term 25+ 1956-1988)

Dependent Variable: Divorce Rate (individuals per 1000)		
Variable:	**No-Fault 15+ Specification**	**No-Fault 25+ Specification**
Years 1-2	-.06 (.05)	-.04 (.05)
Years 3-4	.10*** (.05)	.12** (.05)
Years 5-6	.15* (.06)	.19** (.06)
Years 7-8	.24* (.06)	.30* (.07)
Years 9-10	.15** (.07)	.22* (.08)
Years 11-12	.18** (.07)	.27* (.09)
Years 13-14	.25* (.08)	.35* (.10)
Years 15-16 (15+)	.33* (.09)	.39* (.11)
Years 17-18		.44* (.12)
Years 19-20		.52* (.13)
Years 21-22		.57* (.14)
Years 23-24		.51* (.15)
Years 25+		.53* (.17)
Taken from:	Table 5 Column 2	Table 6 Column 2

* Statistical significance at 1% level, ** Statistical significance at 5% level, *** Statistical significance at 10%

Notes: See notes to Table 2

Table 7 clearly illustrates that no-fault laws are, in this analysis, primarily significant at the 1% level, and have short-term (1-15 years post implementation) effects averaging a 6% increase in national divorce rates, as compared to the 5% average found in table 5. No-fault laws also appear to have a substantial long-term (16-25 years) impact on the national divorce rate, increasing it by an average of 12%. Interestingly, but not ironically, the average proportional effects of no-fault divorce laws on divorce rates, which I have been reporting, are highly in the realm of the statistically significant effects often attributed to unilateral laws by previous studies (example; Friedberg, 1998). Again this result relates to many economists' lack of attention to the independent effects of no-fault laws.

What I find most surprising among the results presented in Table 6 is the overall trend of no-fault laws' effects on divorce rates. Most notably, the trend shows no signs of terminating. Upon examining the dynamic effects of no-fault laws in Table 6 three things become clear. First, the statistical significance of the coefficients strengthens as time goes on post no-fault enactment. Second, the magnitudes of the no-fault coefficients also take on an increasing trend as time passes post no-fault enactment. Specifically, the magnitudes of the statistically significant coefficients in the no-fault series of variables begin at .12 and increase to a maximum of .57. Third, relating to the first two points, the increasing positive trend of no-fault's dynamic effect shows no blatant, or even notable, signs of stopping or slowing. These facts prompted me to investigate even further reaching long-run trends of no-fault laws' effects on divorce rates.

In order to further investigate the long-run effects of no-fault divorce laws, I simply loosen the restrictions posed on the two previous specifications of my preferred model. Otherwise stated, I increase the 25+ specification for no-fault laws to a 45+ specification, thereby allowing no-fault laws to trend without restriction for 45+ years post legal implementation. I allow unilateral laws to trend for 35+ years because only a couple of states trend beyond this point, they are not the series I am investigating, and I wish not to risk any biases arising from the few states that trend past 35+ years. Table 8, below, presents the results of this regression; however, please note that the coefficients for the years 37-45+, for no-fault laws, have been omitted for spatial considerations (for more specifics refer to the notes located below Table 8).

Table 8: Preferred Specification (45+ specification 1956-1998)

Dependent Variable: Divorce Rate (individuals per 1000)		
Variable:	**Unilateral + Interaction**	**No-Fault**
Years 1-2	.31*	-.03
	(.07)	(.05)
Years 3-4	. 10	.14**
	(.08)	(.05)
Years 5-6	.06	.21**
	(.09)	(.06)
Years 7-8	-.03	.33*
	(.11)	(.07)
Years 9-10	-.11	.26*
	(.13)	(.08)
Years 11-12	-.29***	.31*
	(.15)	(.09)
Years 13-14	-.36**	.39*
	(.18)	(.10)
Years 15-16	-.16	.42*
	(.21)	(.11)
Years 17-18	-.15	.46*
	(.25)	(.13)
Years 19-20	-.18	.54*
	(.28)	(.14)
Years 21-22	-.23	.58*
	(.32)	(.16)
Years 23-24	-.08	.50*
	(.37)	(.17)
Years 25-26	-.14	.45*
	(.41)	(.19)
Years 27-28	-.09	.45*
	(.46)	(.21)

Years 29-30	-.23 (.50)	.46* (.23)
Years 31-32	-.44 (.53)	.40 (.25)
Years 33-34	-.05 (.56)	.25 (.27)
Years 35-36 (35+)	-.26 (.61)	-.01 (.28)
Adjusted R^2 = .98		
Includes: State and Year Fixed Effects, Linear Time Trends, and Quadratic Time Trends **All have prob>F = .00** * Statistical significance at 1% level, ** Statistical significance at 5% level, *** Statistical significance at 10%		

Notes: See notes to Table 2
Coefficients on the variables for years 37-45+, for no-fault laws, are omitted solely for spatial reasons. These omitted coefficients continue the ending trends found in the table. Specifically, the coefficients for no-fault laws for the omitted years are all statistically insignificant and generally decreasing.

Table 8 is the final and most far reaching specification of my preferred model, which I will be analyzing. The regression it represents does a competent job of capturing the overall effects of no-fault laws, unilateral laws, and the interaction between the two categories of laws. The trend exhibited for the dynamic effects of unilateral laws and the interaction of unilateral and no-fault laws are almost identical to the earlier specifications (Tables 5, 6). Overall, they show little statistical significance on increasing national divorce rates (excluding years 1 and 2 post unilateral implementation).

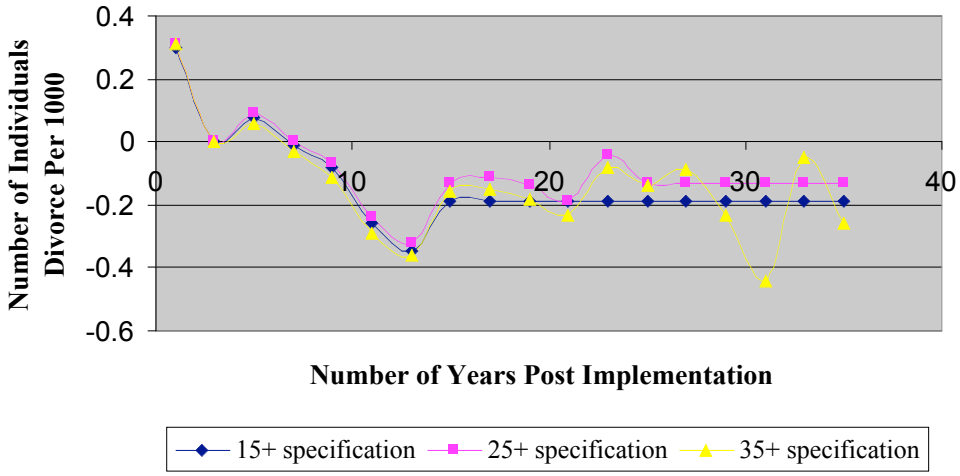
What is most interesting about the specification of the model found in Table 8 are the results found for the latter years of the no-fault series of dummy variables. It becomes clear that the statistically significant effects of no-fault laws are not never-ending. On the contrary, although they do exhibit a positive, significant, and increasing trend over the first two decades following enactment, the effects of no-fault laws eventually reach a maximum magnitude, of .58, at which point the magnitude on the subsequent coefficients begin to decrease and lose significance. By the third decade following enactment, the effects of no-fault laws on divorce rates are no longer statistically significant. The results of the regression found in Table 8 therefore paint a

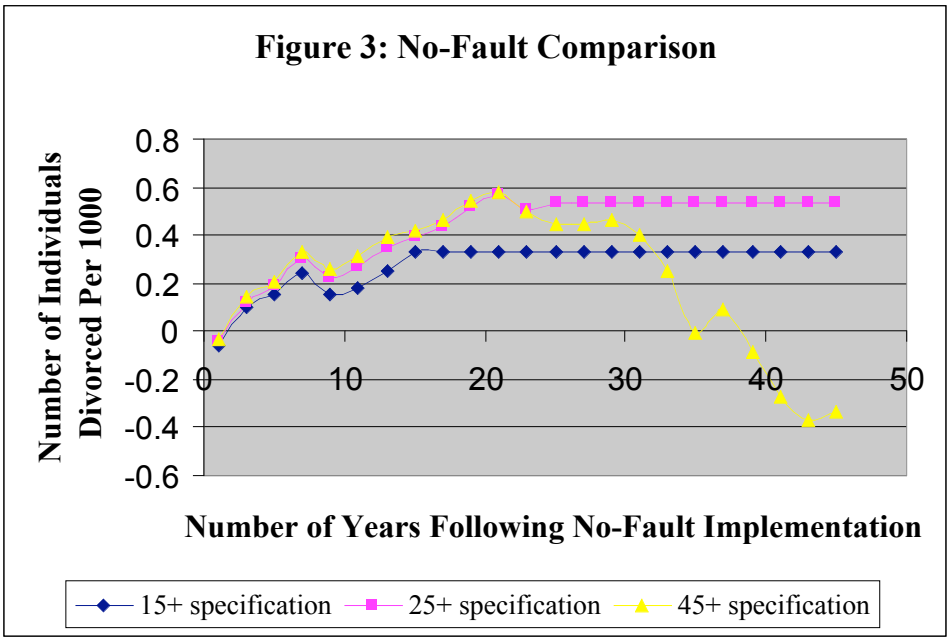
more realistic picture of the divorce laws, specifically no-fault laws, which increase for a period and then decrease and fade slowly away. With this regression, no-fault laws average roughly a 7% increase in divorce rates in the short run and a 13% increase in divorce rates in the long run (16-25 years post implementation).

- Which Specification Is Best? -

Now the question becomes which specification of those discussed above is best for describing the divorce rate – divorce law data. Specifically, can we accept the final specification of the model described in Table 8 as the best of all three? All three specifications are presented in figures below. The first figure compares the coefficients found on the different unilateral specifications, and the second compares the coefficients found on the different no-fault specifications. The two figures clearly show that each specification returns results considerably close to each other.

Figure 2: Unilateral + Interaction Comparison





Although the results of each specification are graphically and numerically closely related, I choose to test whether any one specification is best. To do so, I use an F-test to determine whether we can reject any of the specifications in the face of the others. To test the final specification against the first two, using an F-test, I must first specify the restrictions posed by the first two specifications (Table 5, 6). The restrictions of the first (the 15+) specification, as compared to the final (the 45+) specification in Table 8, are that the coefficients on unilateral + interaction are the same for the years 15 through 35 (by twos) and the coefficients on no-fault are the same for the years 15 through 45 (by twos). Otherwise stated, in terms of an F-test, these restrictions represent the null hypothesis totaling 25 separate restrictions, to be tested and potentially rejected. The F-test for the final specification versus the first is presented in Equation 4, where q is the number of restrictions described under the null hypothesis, SSR stands for the sum of squared residuals, n is the number of observations, and $k_{unrestricted}$ is the number of regressors in the unrestricted regression.

(Eq. 4) H_0: for $\_q \rightarrow \_15 = \_17 = \_19 = \_21 = \_23 = \_25 = \_27 = \_29 = \_31 = \_33 = \_35$
for $\_s \rightarrow \_15 = \_17 = \_19 = \_21 = \_23 = \_25 = \_27 = \_29 = \_31 = \_33 = \_35 = \_37 = \_39 = \_41 = \_43 = \_45$

$F = [(SSR_{restricted\ (15+)} - SSR_{unrestricted\ (45+)}) / q] / [SSR_{unrestricted\ (45+)} / (n - k_{unrestricted\ (45+)} -1)]$

$= [(109.30 - 106.42) / 25] / [106.42 / (1842 - 258 -1)] = 1.71$, which has a p-value of approximately .02

With a p-value of .02 we can reject the null hypothesis at the 5% level. Thus, an F-test suggests that the final specification, found in Table 8, is more adept at explaining the divorce rate – divorce law data, statistically speaking, than the 15+ specification. Using the same method, an F-test, to compare the final specification with the second (the 25+) specification, returns the results described by Equation 5. Here, the null hypothesis contains a total of 15 restrictions; namely that the coefficients on the variables in the unilateral series for the years 25 to 35 (by increments of two) are equal, and the coefficients on the variables in the no-fault series for the years 25 to 45 (by increments of two) are equal.

(Eq. 5) H_0: for $\_q \rightarrow \_25 = \_27 = \_29 = \_31 = \_33 = \_35$
for $\_s \rightarrow \_25 = \_27 = \_29 = \_31 = \_33 = \_35 = \_37 = \_39 = \_41 = \_43 = \_45$

$F = [(SSR_{restricted\ (25+)} - SSR_{unrestricted\ (45+)}) / q] / [SSR_{unrestricted\ (45+)} / (n - k_{unrestricted\ (45+)} -1)]$

$= [(108.43 - 106.42) / 15] / [106.42 / (1842 - 258 -1)] = 1.99$, which has a p-value of approximately .01

With a p-value of .01 we can reject the null hypothesis at the 1% level. Thus, an F-test again suggests that the final specification, found in Table 8, is better at explaining the divorce rate – divorce law data, statistically speaking, than, in this case, the 25+ specification. Therefore, from the results obtained in Equations 4 and 5, it can be concluded that the third and final specification (Table 8), which appeared to offer the

most descriptive account of the overall effects of no-fault and unilateral laws on divorce rates, is in fact the best at explaining the data examined in this paper.

To obtain a sense of how well the specification found in Table 8 estimates the empirical divorce rate data, two figures (4 and 5) are presented below. Each figure represents a state randomly chosen by a random number generator. Both graphs illustrate the empirical divorce rate, the estimated divorce rate, and the residual difference between the two, for the given state over the period 1956-1998. As one can clearly see, the predicted values are extremely close to the empirical data, which suggests that the observed specification of my model is, at a minimum, a reasonably good predictor of divorce rates. Therefore, not only is the final specification of my model the best among those examined in this paper, it is also a good predictor for actual divorce rates.

Figure 4

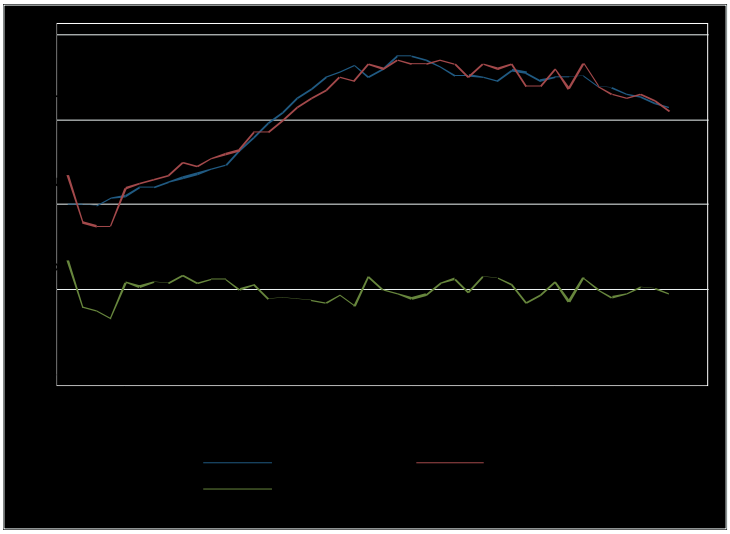
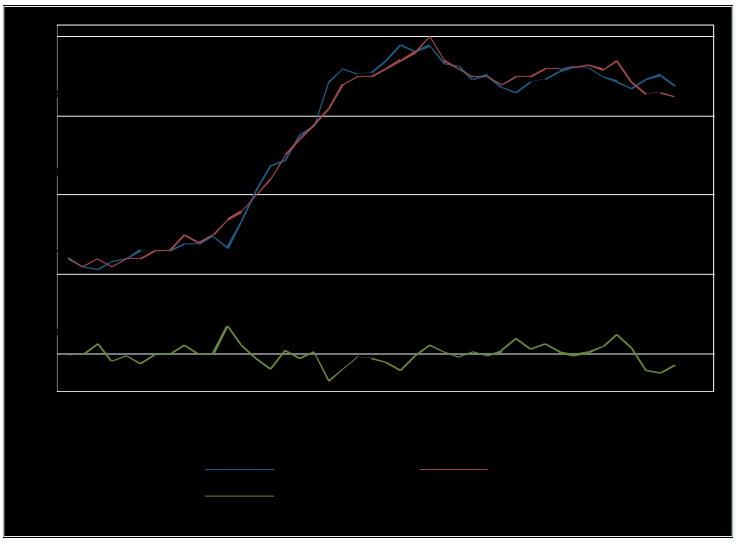


Figure 5



- Control Variables -

The overall impact of the fixed effects and the state-specific time trends on divorce rates are largely significant, and have a noteworthy amount of explanatory power in the divorce rates analyses. This explanatory power is also found to be robust throughout all specifications. As illustrated in Table 9, which uses the specification of my preferred model found in Table 8 as an example, the control variables help to raise the adjusted R^2 from approximately .42 to near perfect. This result is to be expected as there are many variables, such as religion, family size, alcohol use, and existing divorce rate trends which likely affect divorce rates and are encompassed in the fixed effects and time trend variables. Further, as illustrated in Table 9, state fixed effects are most notable in increasing the explanatory power of the regression (a result also found by Friedberg (1998)), which again is an anticipated result as these variables represent fixed state characteristics, which are likely most significant in estimating that states' divorce rate.

Table 9: Preferred Specification (45+ specification 1956-1998)

Dependent Variable: Divorce Rate (individuals per 1000)				
Specification:	This table reports adjusted R^2 values related to the 45+ specification of my preferred model found in Table 8.			
Adjusted R^2	.41	.53	.95	.98
Year Fixed Effects	No	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes
Linear and Quadratic Time Trends	No	No	No	Yes

All control variables also play a significant role in isolating the true effects of the variables of interest (the divorce law variables). This effect, although not depicted in this paper, is evident from the fact that when my preferred model is regressed without control variables, almost all variables are highly statistically significant and most have magnitudes greater than one, which is extremely large compared to a 4.07 average divorce rate. Thus, as illustrated by all regressions depicted in this paper, all control mechanisms are successful in isolating the true, and certainly more realistic, effects of no-fault and unilateral laws on divorce rates. Specifically the control variables help to show the lack of significance of the unilateral + interaction series of variables and substantially reduce the magnitudes of the coefficients found on the unilateral + interaction and no-fault series of variables.

5. Economic Theory Implications

- Coase Theorem -

Are the implications of the model aligned with economic theory? To respond to this question, I begin by relating back to the Coase Theorem. The model, overall, appears to directly support the theories presented by the Coase Theorem. The results of all regressions examined suggest that the move to unilateral divorce laws had little to no statistically significant effect on divorce rates. Specifically, the magnitude of each

coefficient found on the unilateral series was most often small, many even taking on negative values, and highly insignificant. The only potential effect of these laws on divorce rates is limited to the less than two year period post implementation, which demonstrated significance that is debatably attributable to the interaction feature of the unilateral + interaction series of variables; nonetheless, the remaining 33+ years examined throughout this paper, are almost entirely insignificant. This directly supports the prediction of the Coase Theorem, as described by Becker, which suggests that unilateral divorce laws will solely shift the bargaining power within a marriage and not make divorce a more likely outcome. These results illustrate the validity and potential extensive and successful application of the Coase Theorem to marriage and divorce. Because the Coase Theorem can be successfully applied to marriage and divorce, and consequently is a good indicator of the effects of unilateral divorce laws on divorce rates, we can make some hypotheses concerning the bargaining system/structure in marriage. Specifically, the assumptions that underlie the Coase Theorem can be applied to marital bargaining. Therefore, the data and analyses in this paper suggest that marital bargaining has negligible transaction costs and relatively perfect information, a fact often challenged in any realm. This result, and the overall statistical insignificance of unilateral divorce laws, is in stark contrast to many of the empirical studies and findings carried out in past and current literature related to this topic (such as that of Wolfers (2005) and Friedberg(1998)). Because many economists have analyzed unilateral with no-fault grounds variables or unilateral laws in isolation, they often attribute statistical significance to unilateral_{cd} laws, which is unwarranted. This leads to a misinterpretation of the statistical significance of unilateral laws, a lack of acknowledgment of no-fault

laws, and an unfair investigation of the application of the Coase Theorem. The results above, therefore, help to salvage the extensive application of the Coase Theorem, specifically in the realm of marriage

- Costs to Divorce -

The many specifications of my model also unanimously confirm economic theory concerning no-fault laws and costs to divorce. As theory predicted, no-fault laws are empirically found to have highly significant and positive effects on divorce rates. Further, not only are these laws found to be significant in the short-run, but also the long-run (see Table 5 and 8). It can therefore be suggested with confidence that there are costs to divorce in marriage and that no-fault laws, when implemented, assume a role in reducing these costs. Additionally, the reduction in divorce costs is shown to lead to an increase in divorce rates as divorce becomes a more attractive (less costly) outcome of marital bargaining, also a theoretically predicted result. Again, these findings call for increased focus on no-fault laws as an independent variable in divorce law – divorce rate analyses.

- Combined Effect -

The results of the model conflict with theory regarding the effect of the interaction between unilateral and no-fault divorce laws on divorce rates. The model reveals that the interaction aspect of the unilateral + interaction series of variables is likely responsible for a sizable, positive, and statistically significant correlation with divorce rates over the first two years post unilateral implementation. Specifically, the interaction between the two categories of divorce law may be responsible for a 7% increase in the national divorce rate, per year, for the first two years post unilateral

implementation. This is in direct contradiction to the theoretical prediction that the interaction between no-fault and unilateral laws will have no significant impact on divorce rates. There are several potential rationales for why we might find this positive relationship. One line of reasoning suggests that states that have both types of divorce laws also have additional so-called unobserved (i.e. not in my data set) characteristics that may influence divorce rates. One such unobserved characteristic may be that states that have both unilateral and no-fault divorce laws are more likely to have other laws related to marriage that could increase the divorce rate further. These laws may include specific child custody, alimony, or property laws, which are positively correlated with divorce rates. However, this interpretation is again unlikely as the statistical significance of the interaction between no-fault and unilateral laws is not reminiscent of other divorce law effects on divorce rates (such as no-fault laws). Another potential line of reasoning is that no-fault and unilateral divorce laws indirectly affect divorce rates via a reduction in stigma regarding divorce (Wolfers, 2005). This idea, briefly delineated by Wolfers (2005) suggests that increased access to, and increased rates of divorce law, designed to make the divorce process more manageable, consequently reduces negative stigma towards divorce. A reduction of stigma should almost certainly lead to an increase in divorce rates. Further, this stigma reduction is likely most pronounced and prevalent when states have both major divorce laws, unilateral and no-fault. As a result, states that have unilateral and no-fault laws may be expected to have significant increases in divorce rates early on as stigma is reduced.

- Overall Empirical Findings -

As discussed above, empirical analysis suggests the results illustrated by the following table:

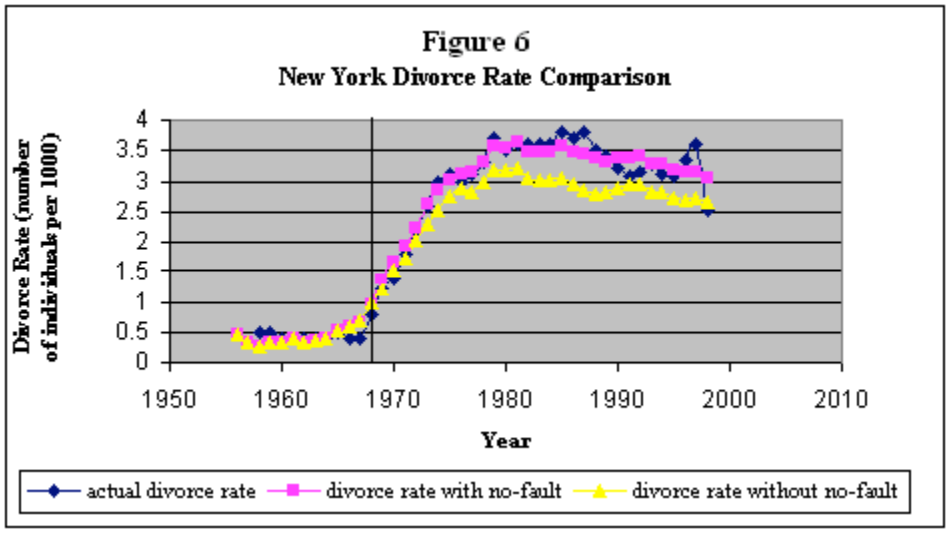
Table 10: Empirical Outcomes of Divorce Law Implementation on the National Divorce Rate

Expected effect on the national divorce rate:	**No Unilateral**	**Unilateral**
No No-Fault	0	0
No-Fault	+ (from no-fault)	+ + (from no-fault and from combined effect)

Notes: In the above table the two year significance of the unilateral + interaction series of variables, post unilateral implementation, is assumed to be attributable to the interaction component of the series.

A graphical representation of these findings is illustrated in Figure 6 below, for New York State. Figure 6 shows the actual divorce rate, the divorce rate as predicted by the specification of my model found in Table 8, and the divorce rate as predicted by the specification found in Table 8 assuming the coefficients on the no-fault law series to be zero (all other coefficients remain unchanged) for New York state. The vertical line in the figure represents the year of no-fault law implementation. It is clear from the figure that almost immediately following no-fault implementation, the divorce rate begins to undergo large increases and the specification which includes no-fault coefficients takes on higher divorce rates than the specification which restricts the effect of no-fault laws, on divorce rates, to zero ("divorce rate without no-fault"). Additionally, it is evident that the specification which includes no-fault coefficients ("divorce rate with no-fault") is a relatively good predictor of the actual divorce rate in New York for 1956-1998. Thus, no-fault laws appear to significantly and positively contribute to the rise in divorce rates, specifically in New York, for the period examined (1956-1998). It is also important to note that the difference between the lines labeled "divorce rate with no-fault" and

"divorce rate without no-fault," represents the marginal effect of having no-fault laws on divorce rates, for New York. Thus, it appears that without no-fault laws, the divorce rate would be significantly lower and, in New York, would be reminiscent of the "divorce rate without no-fault" line. Unilateral laws on the other hand, do not appear to be driving a significant portion of the increase in divorce rates and do not seem to be a good predictor of actual divorce laws in the absence of the clearly significant no-fault divorce coefficients.



6. Public Policy Implications

Many states have recently considered rolling back their unilateral and no-fault divorce laws, stating that these laws have facilitated, and made it more attractive to, divorce (Peters, 1986, Friedberg, 1998). These states assert that the laws were enacted to support struggling couples, not to encourage and facilitate the divorce process (Peters, 1986, Friedberg, 1998). According to the findings of the model, the claims made by states trying to tighten divorce laws have some merit. The result of rolling back divorce laws ultimately depends on which divorce laws are currently in place in the given state. For example, if a state only has no-fault laws, which the model suggests causes a

statistically significant increase in divorce rates, repealing these laws will, consequently, reduce divorce (as suggested by Figure 6). The amount by which this change is expected to reduce the divorce rate is roughly 10% (average of all statistically significant effects of no-fault laws) each year for over two decades. This result, and those that follow, assume that divorce trends behave the same for a legal repeal as they do for a legal implementation, a fact which needs further investigation. Nonetheless, a repeal of no-fault laws can certainly be expected to reduce divorce rates. If a state has only unilateral laws, however, the model suggests that removing these laws will have no effect on divorce rates, and therefore states cannot be expected to change divorce rates in this manner. If a state begins with both unilateral and no-fault divorce laws, there are two possible outcomes, on divorce rates, as a result of rolling back divorce law. One, if solely no-fault divorce laws are repealed, or both unilateral and no-fault laws are repealed, the state can be expected to reduce divorce rates by the same degree as discussed above in addition to 7% for each of the first two years as a result of eliminating the interaction between the two categories of divorce laws. Two, if unilateral laws are repealed, the divorce rate can be expected to reduce by 7% each year for the first two years, again as a result of eliminating the interaction between the divorce laws.

Many individuals also argue that unilateral and no-fault divorce laws are detrimental to the structure of American families (Friedberg, 1998). The assertion is that unilateral and no-fault divorce laws are responsible for decreasing investments in marital specific capital (i.e. children or other shared resources), and creating greater volatility within the marriage market. The underlying hypothesis assumes the following: if divorce is easier and more attractive people will enter into marriages more casually, they will rely

on divorce as a "get-out-of-marriage-free card," invest less in marriages, expect shorter-term marriages, and marry and divorce more frequently, thus creating a less stable family structure. The model suggests that this argument has potential merit. Again, the expected effect of no-fault laws and the effect of the interaction between unilateral and no-fault laws on divorce rates are statistically significant and positive. Thus, as a result of unilateral and no-fault divorce laws, the national rate of divorce has increased and accordingly, divorce has become a more likely outcome of marriage. However, as this result directly relates to decreased investment in marital-specific capital, marriages more casual in nature, and the quality of domestic life, are beyond the scope of this paper and require further empirical analysis. However, the model does suggest specific divorce laws and relationships as a potential focus for further and more specific empirical investigation, and the specific effects of no-fault divorce laws and the interaction between these laws and unilateral laws on divorce rates, as an appropriate starting place to begin additional empirical analysis.

7. Endogeneity

When dealing with legal changes and social behavior, in combination, endogeneity becomes a significant, and frequent, topic of debate. In the case of my model, it is the endogeneity of no-fault and unilateral laws to divorce rates that would be central to the discussion and, subsequently, to the legitimacy of using longitudinal data to test my model. The question becomes, are these laws, and their interaction, responsible for causing a rise in divorce rates, or are greater divorce rates and/or a greater tolerance towards divorce, which has led to a rise in divorce rates, responsible for bringing about these laws (Friedberg, 1998)? Statistically speaking, it is difficult to develop a definitive

and indisputable response to this question. Doing so, for my purposes, requires discovering and testing a variable that is correlated with the divorce laws, yet, is not correlated with divorce rates, alone, and varies across time and states (Friedberg, 1998). However, there are a few studies currently published that suggest that unilateral, and likely no-fault, divorce laws are endogenous to divorce rates (Friedberg, 1998, Gruber & Hanratty, 1995, Wolfers, 2005). Friedberg's (1998) study involves testing the correlation between a state's initial divorce rate and the probability and timing of unilateral divorce law implementation. If this correlation is found to be highly significant, it would suggest that the divorce laws are endogenous. The results of the study found a relatively high correlation between a state's initial divorce rate (Friedberg used each state's divorce rate for the year 1968 as it's initial rate) and the state adoption of unilateral divorce laws, but, the initial divorce rates were not correlated with the timing of divorce. In Table 11 below, I conclude similar results for no-fault divorce laws. The table illustrates that the year no-fault is implemented, represented by "yearimp," has no statistically significant correlation (p-value = .534) with a state's initial divorce rate, "initial," for our purposes, to emulate Friedberg, assumed to be 1968. These conclusions support the hypothesis that unilateral and no-fault divorce laws are not endogenous to divorce rates.

Table 11: Endogeneity Test 1 (1956-1998)

Dependent Variable: Divorce Rate in 1968 (individuals per 1000)	
Variable:	**Unilateral**
Yearimp	.02 (.03)
P-value	.54
* Statistical significance at 1% level, ** Statistical significance at 5% level, *** Statistical significance at 10%	

Another study conducted by Friedberg (1998) produces a similar conclusion. In this study, Friedberg employs a method used by Gruber and Hanratty (1995). In their regression, Gruber and Hanratty add a lead dummy variable representing a law change

occurring in the subsequent year. The hypothesis underlining this strategy is that, if the lead variable is found to be statistically insignificant, then the law change being examined can be said to be not endogenous to the dependent variable. Friedberg (1998) employed this strategy to determine the endogeneity of unilateral laws to divorce rates. She found that the coefficient on the lead variable took on a very low value, which was not statistically different from zero and therefore, concluded that unilateral laws are likely not endogenous to divorce rates. I have conducted the same study for no-fault divorce, and the results are given below in Table 12. The regression includes all fixed effects, time trends, and variable series from the 45+ specification of my model, although not depicted in the table. Endog, the dummy representing whether a state enacted unilateral divorce laws in the subsequent year, is not statistically significant or different from 0, with a P-value of .157. Thus no-fault divorce laws, again, can be said to be not endogenous to divorce rates.

Table 12: Preferred Specification (45+ specification 1956-1998)

Dependent Variable: Divorce Rate (individuals per 1000)	
Specification:	This table uses the 45+ specification, found in Table 8, with the addition of a variable to test for endogeneity.
Endog	-.07 (.05)
P-value	.142
Includes: State and Year Fixed Effects, Linear Time Trends, and Quadratic Time Trends **All have prob>F = .000** * Statistical significance at 1% level, ** Statistical significance at 5% level, *** Statistical significance at 10%	

Notes: See notes to Table 2

Therefore, although there seems to be some correlation between a state's divorce rate and likelihood of enacting divorce laws, studies illustrate that the timing of divorce law implementation is not related. Thus, the evidence supports the fact that unilateral and no-fault divorce laws are likely not endogenous to divorce rates. However, because there is no perfectly concrete evidence, endogeneity remains a potential, although unlikely,

limitation of the analysis.

8. Conclusions

The model and data presented in this paper shed new light on the topic of the relation between divorce law and divorce rates in the U.S. Current literature on this topic has focused primarily on $unilateral_{cd}$ divorce laws, either defined alone or with no-fault criteria, and their statistical significance in regards to divorce rates. However, as illustrated by the results of the many specifications of my model, ignoring the independent effects of no-fault divorce laws when examining the effects of unilateral divorce is detrimental to the analysis. Instead, there should be increased focus on the manner in which no-fault divorce laws fit into the relationship regarding unilateral divorce law and divorce rates, specifically as a separate variable. The model suggests that unilateral divorce laws have primarily statistically insignificant effects on divorce rates. This result, as compared to that of others such as Friedberg and Wolfers, illustrates that unilateral laws are often attributed unwarranted explanatory power when examined in the absence of no-fault divorce laws, or with no-fault laws as a criterion for unilateral divorce. Additionally, the many specifications of the model illustrate that no-fault laws have highly statistically significant and enduring effects on divorce rates. Specifically, no-fault laws are found to be responsible for increasing the national divorce rate by approximately 7% in the short-run (1-15 years) and approximately 13% in the long-run (16-25 years). This result mandates a greater focus on no-fault laws' large impact on divorce rates, and the importance of isolating these laws as an independent variable in divorce law-divorce rate analyses. Additionally, the analyses in this paper also suggest the possibility that the interaction between no-fault and unilateral laws has a statistically

significant and positive effect on divorce rates of roughly 7% for slightly less than two years post unilateral implementation. This result necessitates a more thorough examination of this phenomenon in order to discover whether it is in fact attributable to the interaction between no-fault and unilateral laws and what are its more specific causes (i.e. reduced stigma and other divorce related laws). Finally, although many studies have found that unilateral laws are not endogenous to divorce rates, the above analysis is the only I am aware of which tests, independently, no-fault laws' endogeneity in relation to divorce rates. No-fault laws, like $unilateral_{cd}$ laws, are found to not be endogenous to divorce rates which, again, I believe is a novel finding in the field.

The overall importance of these findings is substantial. First, findings on the statistical significance of unilateral and no-fault divorce laws illustrate that unilateral laws do not appear to be a significant, or even marginally impactful, cause of increases in divorce rates. This result should end the debate over the significance of these laws, and shift the focus of the literature to no-fault laws. Further, this result is especially important for those states considering repealing either no-fault or unilateral divorce laws. My findings suggest that repealing unilateral laws would likely prove ineffective in reducing divorce rates (a potential mistake encouraged by many previous analyses), and only when repealing no-fault laws can a state likely significantly reduce divorce rates. Additionally, the findings in this paper reveal that economic theory related to unilateral and no-fault divorce laws is applicable to marriage, marital bargaining, and divorce. This result has been significantly challenged by past studies related to this topic, which have cited misleading analyses as a means of determining this relationship. Thus, the results in this paper appear to be successful in salvaging the economic theories (specifically, the

Coase Theorem and costs to divorce) and championing their extensive application to the realm of marriage and divorce law. The results in this paper also provide a broader and more informed divorce law knowledge base (as compared to unilateral variables, which are examined with or without no-fault criteria) for responding to, or directing, major public policy questions such as which laws, if repealed, are most likely to strengthen family structure vis-à-vis marriages. Further empirical study is required, however, to determine definitive answers to public policy issues as the specifics of how the divorce laws affect divorce rates has not been discussed. Finally, this paper has illustrated the benefit of using panel data, and has confirmed the view held by those participating in the divorce law debate that an analysis of divorce law requires controlling for geographical differences and time trends (both state-specific and nationwide). This result is illustrated by the ability of these variables to tease apart the explanatory power of our focus variables and the magnitudes of their coefficients, in addition to significantly raising my model's adjusted R^2.

Works Cited

Allen, D. (1992). Marriage and divorce: Comment. *American Economic Review, 82* (3), 679-685.

Becker, G. (1991). *A treatise on the family*. Massachusetts: Harvard University Press.

Friedberg, L. (1998) Did unilateral divorce raise divorce rates? Evidence from panel data. *The American Economic Review*, *88* (3), 608-627.

Gruber, J. (2004). Is making divorce easier bad for children? The long run implications of unilateral divorce. *The Journal of Labor Economics, 22* (4), 799-833.

Gruber, J., & Hanratty, M. (1995). The labor market effects of introducing national health care: Evidence from Canada. *Journal of Business and Economic Statistics*, *13* (2), 163-173.

Katz, S. N. (1994). Historical perspective and current trends in the legal process of divorce. *Future Child, 4* (1), 44-62.

Mosk, S. J. (1972). *In re marriage of McKim, 6 Cal. 3d 673*. Retrieved December 16, 2005 from http://online.ceb.com/calcases/C3/6C3d673.htm

Peters, H. E. (1986). Marriage and divorce: Informational constraints and private contracting. *The American Economic Review, 76* (3), 437-454.

Peters, H. E. (1992). Marriage and divorce: Reply. *American Economic Review, 82* (3), 686-693.

Wolfers, Justin. (2005). Did unilateral divorce laws raise divorce rates? A reconciliation and new results. *University of Pennsylvania, CEPR, IZA, and NBER*. Retrieved October 3, 2005, from http://www.nber.org/~confer/2005/si2005/le/wolfers.pdf

Wright, G. C. & Stetson, D. M. (1978). The impact of no-fault divorce law reform on divorce in American states. *Journal of Marriage and the Family, 40* (3), 575-580.

APPENDIX A

April 13, 2006 Marjorie B. McElroy

This appendix derives the expected value of the estimator for the coefficient on the N*u dummies for a sample where there are no observations where unilateral laws apply but no fault laws do not apply.

Let all variables be written as deviations from their means. Assume that the true model is

$$y = \beta_{NU}(N * u) + \beta_u u + \in \quad (1)$$

where (N*u) represents the vector of interactions of the no fault and unilateral dummies and u is the vector of unilateral divorce dummies. Suppose instead of (1), one estimates a model that omits unilateral dummy, u. So the the estimated model is

$$y = \beta_{Nu}(N * u) + v. \quad (2)$$

The ordinary least squares estimator of (2) is:

$$\begin{aligned}
\hat{\beta}_{Nu} &= \frac{(N*u)'y}{(N*u)'(N*v)} \\
&= \frac{(N*u)'}{(N*u)'N*u}\{\beta_{Nu}(N*u) + \beta_u u + \in\} \\
&= \beta_{Nu} + \beta_u \frac{(N*u)'u}{(N*u)'(N*u)} + \frac{(N*u)'\in}{(N*u)'(N*u)} \\
&= \beta_{Nu} + \beta_u \frac{r_{N*u,u}}{r_{N*u,N*u}} + \frac{(N*u)'\in}{(N*u)'(N*u)},
\end{aligned}$$

where $r_{N*u,u}$ *and* $r_{N*u,N*u}$ *are* the obvious sample covariances. Now in the actual sample $\{(N*u)_{it} = 1\} \Rightarrow \{u_{it} = 1\}$ so these two covariances are equal ($r_{N*u,u} = r_{N*u,N*u}$) and the middle term reduces to β_u. For the usual reasons, the expectation (or plim) of the last term is zero. Hence,

$$E(\hat{\beta}_{Nu}) = \beta_{Nu} + \beta_u.$$

This result generalizes to the case where there are additional regressors in (1) and (2). The generalization is easy to see. In the case above, the "bias term" is OLS estimator of the so-called auxiliary regression of u on the included right hand side variable, $N*u$. Since, in the sample u and $N*u$ are the same, it is not surprising that the estimated coefficient for the auxiliary regression is 1. In the generalization, the bias term is a vector of OLS coefficients from the auxiliary regresion of u on multipe included regressors, one of which is $N*u$. Again, it is not surprising that the estimated coefficent on $N*u$ is 1 and, in addition, that all of the remaining estimated coefficients are zero. In other words, when there are multiple regressors in (1) and (2), the scalor relationship (4) still holds and all other coefficient estimators are unbiased.