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## Do Joint Parenting Laws Make Any Difference?

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## Do Joint Parenting Laws Make Any Difference?

*Douglas W. Allen and Margaret Brinig\**

Using a unique data set on divorcing couples, we analyze the effects of a change in legal entitlement on the outcomes for divorcing couples. In particular, we analyze the 1997 change to custody provisions in the State of Oregon. Prior to 1997, Oregon assigned custody, based on the discretion of the court, in the best interests of the child. This was changed to a “presumption” of joint parenting, which manifests in the courts encouraging and imposing joint (or shared) custody in cases that otherwise would have had sole custody arrangements. We find that the law had several implications for divorce behavior: different custody outcomes (less sole custody to mothers, more sole custody to fathers), more mediation, longer times until the final divorce, and more acrimonious divorces (more abuse actions filed). We find no evidence that spouses attempted to bargain around the change of legal entitlement.

### I. INTRODUCTION

Changing family laws continue to be at the heart of many social policy debates, and yet very little is known about the effects of these changes. Over the past 25 years, many states have gone through a “joint parenting” exercise. With this debate taking place in local and state media, the sides are almost as well defined, if less broadly known, as those regarding same-sex marriage or abortion. Joint parenting agreements are used within the context of joint custody laws. They are generally supported by men’s groups and pro-family advocates, who argue that “children need both parents to be successful.”<sup>1</sup> Not surprisingly, some women’s groups see joint parenting as a threat to the long-standing presumption that they would be given custody under a “best interests of the child” test. They claim that children are victimized by agreements that force both parents to deal with the logistics of child

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<sup>1</sup>As an example, Mathis, in the *Bismarck Tribune* states: “If children could vote, they would vote for both parents” (2007:6A). When he signed the joint parenting bill into law, Governor Vilsak (Iowa) said, “I hope it sends a message to fathers and mothers across the state that we want them to be engaged in the raising of their children and we think if they are, the outcomes for their children will be better” (Pitt 2004).

rearing, and that joint parenting rules are unable to protect children from abusive parents.<sup>2</sup> Like other family policy debates, both sides tend to talk past each other and policymakers have very few facts at their disposal. Our goal here is to be the first to shed some actual empirical light on the question: What effect do joint parenting laws actually have on custody, support, and the divorce process?

There is a good reason why so little is known about the actual effects of joint parenting laws: the relevant data are buried in court records and are difficult to obtain. In our case, we have compiled a unique data set on divorcing couples to analyze the 1997 change to custody provisions in the State of Oregon. Prior to 1997, Oregon, like most states, assigned custody, based on the discretion of the court, in the best interests of the child. This was changed to a “presumption” of joint parenting, which manifests in the courts imposing joint (also called shared) custody in cases that otherwise would have had sole custody arrangements.<sup>3</sup> By “presumption” is meant the court’s “starting point” or anticipated award and thus, unless the case is settled, arguments must be made to alter the court’s position. We chose the State of Oregon for reasons of data availability and because there was an actual change in law. However, joint parenting laws are not restricted to one state. In fact, Iowa and Maine have also changed their law and there is legislative activity regarding joint parenting in most states. More than 40 states (and the District of Columbia) have class action suits over equal treatment in custody awards, though as of September 2010, none of these has been successful.<sup>4</sup>

Changes in child custody laws may influence the bargaining that takes place within marriage and, as a result, may change the relative bargaining strengths and resource allocation within marriage.<sup>5</sup> A critical feature of all bargaining frameworks is the threat

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<sup>2</sup>Omdahl (2006), in the *Grand Forks Herald*, states that “the measure would substitute judicial neutrality with bitter grudges in the determination of joint custody arrangements. . . . [T]he court grant[s] both parents—the good and the bad—equal custodial rights even when it isn’t in the best interests of the children.”

<sup>3</sup>Proponents of the change would hasten to argue that “best interests of the child” is still the standard. What has changed is the growing equating of joint parenting with “best interests.” We mean by sole custody that one parent has physical custody most of the time, while the other has visitation (called “access” in some jurisdictions and “parenting time” in Oregon). For differential child support purposes, it means that neither parent spends less than 35 percent of the time with the child.

<sup>4</sup>See, e.g., *Urso v. Illinois*, #04-CV-6056 (N.D. Ill. Kennelly, J.) (42 U.S.C. § 1331), *dismissed for lack of subject matter jurisdiction*, Oct. 7, 2004; *Creed v. Wisconsin*, 04-00917 (E.D. Wis. Curran, J.), *dismissed with prejudice*, Sept. 24, 2004 (same); *Ward v. Louisiana*, 04-CV-2697 (E.D. La. Fulton, J.); *Martin v. Florida*, 04-CV-22385 (S.D. Fla. Jordan, J.) (42 U.S.C. § 1985). In a startling decision, the Iowa Supreme Court said that the law could not possibly have meant joint custody because this would be against the best interests of the child. *Marriage of Hansen*, 733 N.W.2d 683 (Iowa 2007).

<sup>5</sup>The bargaining literature no doubt starts with Manser and Brown (1980) and McElroy and Horney (1981), who, in the context of consumer theory, first challenged the notion the household could be represented by a single utility function. Bargaining models of one type or another examining intrahousehold allocations of resources during marriage have also been modeled by Woolley (1988), Lundberg and Pollak (1993, 1994), Sayer and Bianchi (2000), Chiappori et al. (2002), and Gray (1998), to name just a few. Chiappori (1992) developed a “collective” approach to household allocation to examine shares in marriage and labor supply issues. Allen (1992a) examines how noncooperative bargaining in marriage influences the choice of spouse.

point in case of bargaining failure.<sup>6</sup> Threat points *while* married are often assumed to be the anticipated utility when divorced, and may depend on wages, nonlabor incomes, sex ratios, unemployment rates, government social programs, and, of course, divorce laws regulating property, child support, and custody.<sup>7</sup> With the exception of Gray (1998) and the voluminous work on grounds for divorce, however, little empirical attention has been given to legal details *at* divorce and the effect these might have on custody outcomes.<sup>8</sup> No doubt this dearth of literature stems from two sources. First, for an average married couple, far from the margin of divorce, changes in legal rules regulating custody likely have little impact on behavior.<sup>9</sup> Second, bargaining models generally make no specific predictions about the exact terms under which household gains are to be divided. To say that one side benefits from a change in threat point begs the question: In what specific way does he or she benefit? This shortcoming makes empirical testing more difficult. This latter issue is important within the context of divorce negotiations where financial and liquid assets are being split along with nonfinancial and illiquid assets, and many outcomes are possible. Our Oregon data addresses both these shortcomings.

Of particular interest to us (and economists, lawyers, and family advocates) is whether changes in the legal rules regulating families will have any real effect on divorce outcomes. At first glance, our question seems to have an obvious answer, but three results are plausible. First, if family life is teeming with transaction costs and if the legislated change was a surprise, then any legal changes may not be bargained away or anticipated, and may have real consequences. Second, legal changes might simply lead to offsetting financial changes. For example, a change to legal rules regulating child custody might lead to the same custody allocation, but with a transfer of dollars to the party placed in the better bargaining position.<sup>10</sup> Finally, legislated changes in threat points might be a confirmation

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<sup>6</sup>There have been many empirical studies showing that differences in nonlabor income between parents, shares of household wealth, and assets brought into the marriage all influence child outcomes, labor supply, and other economic choices during marriage. See, e.g., Thomas (1990), Hodinott and Haddad (1995), or Fortin and Lacroix (1997). An endogeneity problem often exists in testing bargaining models because the variables used to measure differences in bargaining positions (like wages) are themselves often the product of internal bargaining.

<sup>7</sup>Lundberg and Pollack (1993) consider noncooperative behavior within the household as a more credible threat point than divorce.

<sup>8</sup>On the no-fault divorce literature, see Peters (1986), Allen (1992b), Friedberg (1998), Brinig and Buckley (1998), and Wolfers (2006).

<sup>9</sup>In this context, the Lundberg and Pollack (1993) threat point of noncooperative behavior within the family seems more plausible. Indeed, most of the bargaining literature focuses on intrahousehold bargaining, rather than on couples who have already separated and who are in the process of negotiating the terms of their permanent dissolution of marriage. However, this latter group may be quite sensitive to changes in legal rules that directly and immediately influence their bargaining position and, as a result, the likely allocations of property and custody by divorce courts is more relevant for them.

<sup>10</sup>Mnookin and Kornhauser (1979) were perhaps the first to point this out. The reason for this anticipated effect is that divorcing couples should recognize the optimal allocation of custody and time allocation of children, and bargaining over this with side payments should be relatively straightforward—a simple application of the Coase theorem.

of changes that have already taken place at the court or societal level. In this situation, the law will be neutral with respect to the legislated behavior, and there will be no compensating side payments because there has been no *real* change in threat point. Thus, the effects of the legal change in custody can be summarized in three hypotheses.

- H<sub>1</sub>: Effect on custody allocation, and no dollar transfers.
- H<sub>2</sub>: No effect on allocation, but dollar transfers from one spouse to the other.
- H<sub>3</sub>: No effect on allocation, and no dollar transfers.

These three different hypotheses are found in the day-to-day debate over joint parenting laws.

To try to distinguish these three different outcomes to a legislated change in child custody we have an empirical emphasis in this article. Within the context of the tests we conduct, we show that changes to the legal presumption of custody did impact almost every margin we examine, and sometimes in ways that are unexpected. That is, we find that the move to legal joint parenting led to changes in the custody of children (but, surprisingly, not toward joint custody), the use of mediators, and the length of divorce proceedings. We find no evidence of changed dollar transfers between spouses.

However, the story does not stop there. We also show that the simple change in law led to other responses in behavior. The Oregon legislation contained a protection clause for an exemption from the presumption for cases of child abuse. Divorcing couples used this part of the legislation to improve their custody outcomes by accusing the spouse of abuse. The end result was longer, more costly, more acrimonious divorces. Generally speaking then, we find evidence that supports Hypothesis H<sub>1</sub>.

## II. LEGAL CHANGES IN OREGON

Joint parenting legislation is the result of lobbying efforts by father's rights groups who want fathers to gain more access to their children after divorce. In its historical context, it is part of the evolving family law changes brought on by the wave of no-fault divorce legislation in the 1970s and the growing number of divorced households. Prior to these changes, divorce was less common and typically mutual. Since the introduction of various degrees of unilateral divorce, the state has modified property laws, support laws, and custody laws in an effort to balance the interests of the family stakeholders. Since state rules can never mimic the details of mutual agreements, such legal changes often leave the door open for opportunistic behavior.

Before 1997, Oregon's divorce law regarding custody was fairly typical. It provided for joint legal custody (shared decision making) in the vast majority of cases and physical custody awards "in the best interests of the child." Although joint physical placement was a possibility, it was not favored. The legislation amending the statute, 1997 Oregon Laws Ch. 707 (S.B. 243), shows the change in emphasis.

*The court may hold a hearing to decide the custody issue prior to any other issues. <+When appropriate,+> the court <-may> <+shall+> recognize the value of close contact with both parents and encourage<-, where practicable,+> joint parental custody and joint responsibility for the welfare of the children.<sup>11</sup>*

After the statute went into effect, Oregon courts were required to recognize the value of close contact with both parents and encourage joint parental custody. In addition, 1997 legislation noted in its very first section that it was state policy to “[a]ssure minor children of frequent and continuing contact with parents who have shown the ability to act in the best interests of the child.”<sup>12</sup>

Consistent with this was the change in vocabulary throughout the statute from “visitation” to “parenting time.” The legislation also mandated several procedural devices to encourage this shift in emphasis. The first was a requirement that divorcing parents file a “parenting plan.”<sup>13</sup> The second was a rule mandating that if one party wanted joint custody and the other objected, they be sent to mediation unless “participation in mediation will subject the party to severe emotional distress.”<sup>14</sup> In addition, custody would not be given to a parent who had been found to have committed any spousal or child abuse under Oregon law.<sup>15</sup> Spouses alleging domestic violence were excepted from mediation procedures as well as from the joint custody presumptions. If they still could not agree after 90 days, the court would determine custody, still with the goal of “ensuring the non-custodial parent sufficient access to the child to provide for appropriate quality parenting time.” Third, enforcement of the custody and “parenting time” order was designed to be more effective. Theoretically, motions to enforce could be brought without attorneys and with the same remedies available for enforcement of child support, including specific schedules of visitation, fines, jail time, “make-up” visitation, and even elimination of spousal support.<sup>16</sup>

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<sup>11</sup><+> represents added language, and <-> indicates deleted language.

<sup>12</sup>1997 Oregon Laws c. 707 (S.B. 243).

<sup>13</sup>The basic idea was that if the parents could focus on a common task and work out a concrete arrangement, it would work better over the long run than something as vague as “liberal visitation.”

<sup>14</sup>Or. Rev. Stat. § 105.179 (2003). The Oregon Task Force Report that spawned the legislation specifically mentions domestic violence.

<sup>15</sup>Or. Rev. Stat. § 105.137 makes abusive relationships exceptions to joint parenting awards: “However, if a parent has committed abuse, as defined in ORS § 107.705, there is a rebuttable presumption that it is not in the best interests and welfare of the child to award sole or joint custody of the child to the parent who committed the abuse.” Section 107.105 defines abuse as:

- (1) “Abuse” means the occurrence of one or more of the following acts between family or household members:
  - (a) Attempting to cause or intentionally, knowingly or recklessly causing bodily injury.
  - (b) Intentionally, knowingly or recklessly placing another in fear of imminent bodily injury.
  - (c) Causing another to engage in involuntary sexual relations by force or threat of force.

<sup>16</sup>Or. Rev. Stat. § 107.135.

### III. ANALYTICAL FRAMEWORK

The fundamental prediction of bargaining theory is that changes in threat points that improve bargaining positions of Party A improve the share of wealth going to Party A.<sup>17</sup> This simple prediction provides a benchmark for interpreting the Oregon joint custody changes. If the Oregon law had been unanticipated by any of the parties, then the change in custody law can be seen as a change in a negotiation threat point. That is, if a spouse had previously not agreed to the other having sole or joint custody, then the latter now has a credible threat to go to court and obtain at least joint custody. In most cases, under this benchmark, the direction of the change favored the traditional noncustodial parent (the father), and likely led to reduced offers to settle the divorce proceedings. In terms of the settlements, such a change should improve the offers made to husbands. If the switch to joint parenting was an *unanticipated* clear transfer of legal entitlement to the historical noncustodial parent (the father), then under this assumption we expect (1) fathers to obtain custody more often, and (2) for more mediators to be used.

However, the Oregon legal change was more complicated than a simple transfer of power from wives to husbands. Separated couples often enter into divorce negotiations with conflicting views of their marital histories and relative contributions. They often have strong preconceived views on what a “fair” split would entail, and often view deviations from this as a form of “divorce theft,” even if these deviations result from legislation. The complicated nature of marital assets and their measurement difficulties, when combined with the motivation of the individuals, allows for the possibility of changes in behavior on many margins.

A loser from a legislated reassignment of rights is unlikely to sit back and do nothing, particularly when these rights are as important as child custody. As a first strategy, the loser is likely to fight back and make arguments against the change in rules. At the same time, the winner from a legal change in custody rules may use the new threat point as a launching pad for increased efforts at marital property rights. In the Oregon case, this is particularly likely because the legislation contains an “abuse safety valve” making abusive parents ineligible for joint custody. Thus, with imposed joint custody, the party negatively affected (usually the wife) is likely to argue that the other spouse is not fit for joint custody, or make other claims to support sole custody. The party positively affected may do the same thing in an effort to achieve sole custody, especially if (usually) he does not want to have frequent dealings with his former wife. Since the law allows for movements away from joint parenting in the case of abuse, we should expect attempts to use this strategy by both sides.

Thus, if the legal change was unanticipated, there should be evidence that both sides used the new law to maximize their own welfare at the expense of the former spouse. Since the law allowed an option to avoid the transfer of rights through the use of abuse provisions, there should be (1) an increase in the time it takes to settle a divorce, (2) an increase in the number of abuse complaints made, and (3) an increase in the number of custody motions made.

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<sup>17</sup>See Cooter et al. (1982) for a seminal exposition.

However, these predictions must be tested against two alternatives. A second alternative hypothesis, à la Becker, is that if the costs of bargaining are very low, then the switch to joint parenting might have no effect on custody or anything else, since the husband and wife merely bargain around the law. The loser from the legal switch may decide to “buy” custody back through agreeing to lower support (even, perhaps, lower child support) or a smaller share of marital property. We call this Hypothesis H<sub>2</sub>. Yet another alternative hypothesis is that the switch to joint parenting could have been *anticipated* by the divorce lawyers and others following the divorce situation closely. Although the more subtle aspects of the law dealing with abuse options seem unlikely to have been anticipated, the general thrust of the law may have been. If so, then there would be a trend in father’s custody over time, with no sudden change with the 1997 joint parenting enactment. As mentioned above, we call this Hypothesis H<sub>3</sub>. The distinguishing test between H<sub>2</sub> and H<sub>3</sub> is that if H<sub>2</sub> were true, we would expect to observe a transfer of wealth from mothers to fathers to compensate for the transfer of legal rights. The remainder of this article attempts to sort through these three hypotheses.

## IV. EMPIRICAL RESULTS

### A. Data Sources

Between 1995 and 2002, there were approximately 125,000 divorces in Oregon.<sup>18</sup> As each Oregon divorce is granted, the circuit court clerk sends information to the Division of Vital Records of the Department of Health and Human Services. This information, more extensive than that collected in most states, includes the names, counties of birth and residence of each spouse, their ages, the date of marriage, separation, and divorce, the identity of the plaintiff in the divorce action, the number of marriages for each spouse, the date and way any previous marriage ended, the education and race of each spouse, the number of minor children in the household, and the custody awarded for each child.<sup>19</sup> Brinig obtained electronic files containing all this administrative information. In addition, for a random subset of divorces with children (since these changes only affect those with children), she

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<sup>18</sup>Oregon records show the following divorce numbers:

1995	15,329
1996	14,973
1997	14,880
1998	15,265
1999	15,647
2000	16,583
2001	16,569
2002	16,151

The total is 125,397. Note that there is no apparent decrease in the crude divorce rate.

<sup>19</sup>The only other states to continue to collect as much data, since the National Center for Health Statistics stopped compiling individual divorce data in 1995, are Connecticut, Montana, and Virginia. None of these states has judicial records online. We could obtain custody trends in these states, but very little else.

matched each divorce to the Oregon Online Judicial Information Network (OJIN) to obtain specific information about the court proceedings surrounding the divorce.<sup>20</sup> Since 1991, OJIN has collected case information from each county's circuit courts, which it makes available free of charge at various sites in Oregon and, for a set-up and hourly fee, to online users elsewhere.<sup>21</sup> The OJIN information allowed her to collect data for each case on attorney representation, the number of court incidents (including motions), the amount of fees charged, whether or not a party alleged domestic violence (and whether a protective order issued), whether or not one alleged failure to pay child support or sought to change visitation or custody, and so forth.

From this information we first randomly selected 500 cases involving children for each of the eight years involved (nearly three years before the statute went into effect in late 1997, and slightly more than five years thereafter).<sup>22</sup> After matching the two electronic databases for the 4,000 cases, identifying information was deleted from the files. This led to the creation of our data set of 3,806 divorces over the eight-year period.<sup>23</sup> The extraordinary detail of this data set allows us to examine the effects of the change in Oregon's switch to a presumption of joint parenting on many margins. We assume the switch in the Oregon divorce law is exogenous to these divorcing couples.<sup>24</sup>

Before plunging into the regression results, a simple figure might help as an aid to interpretation. Consider the decision to grant the wife sole custody of the children. Figure 1 plots the raw custody fractions from our data set. These raw data show that custody to wives was essentially constant prior to the change in legal regime, and began to fall continuously afterward. Custody to wives was highest, at 68 percent, just prior to the legal change, and fell to a low of 51 percent by the end of our data period. Similar graphs, with equally striking results, can be drawn for each of the dependant variables we are interested in. The relevant

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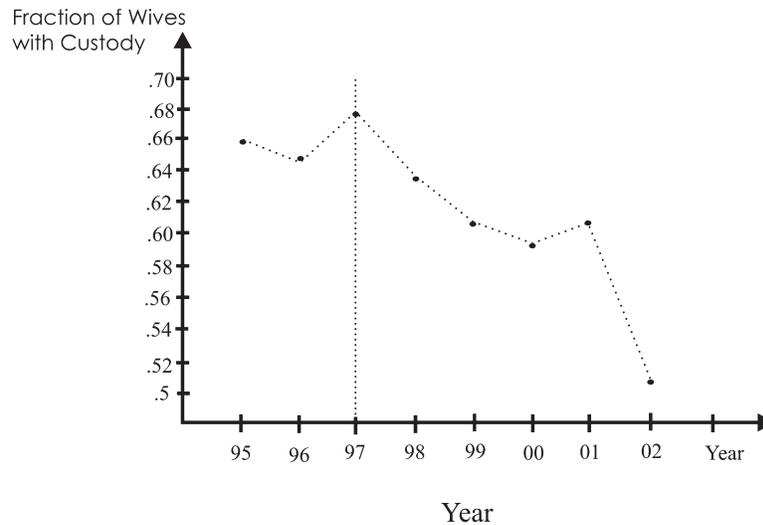
<sup>20</sup>Brinig was able to match all divorce cases with OJIN. There was no issue of multiple records, mismatches, or other complications.

<sup>21</sup>Human subjects review board permission was given for the matching, based on names and type of actions, required by the process.

<sup>22</sup>SPSS, the statistical program, allows a random selection of any given number of cases. There were in excess of 7,000 with children each year. We chose 500 cases per year to keep data-collection costs reasonable while at the same time having a data set large enough to produce reasonable estimates.

<sup>23</sup>We eventually excluded cases in which custody of minor children went to neither father nor mother, or where a diligent search revealed no court records to match. This resulted in deletion of less than 200 cases.

<sup>24</sup>In a perfect world, we would like to test this assumption. This would require data on all those separated and divorced. Unfortunately, those who separate, but never divorce, are not in our data set, nor are the data available. Instead, we report the means of our variables for before and after the change in legal regime in Table 1. For dichotomous variables, we use a chi-squared test for a difference in means, while for the continuous variables we use a simple *t* test. From a statistical point of view, there is little difference between the two samples, although at the 5 percent level the average age of the wife is slightly higher, there were more nonwhites, and fewer welfare recipients in the post-legal-change group. However, looking at the actual differences of magnitude, there would appear to be little difference between the two groups: the before and after means are all similar in size. This suggests it is reasonable to treat the legal change as exogenous.

*Figure 1:* The unconditional fractions of child custody for wives.

question is: How much of the decline after 1997 is attributable to the legal change? We determine this using three regression coefficients to calculate the total marginal impact of the law, holding constant the various control variables listed in Table 1.

#### *B. Note on Regressions*

Within six tables, this article reports on 18 regressions. Because the dependent variables are binary, most of the regressions are logit regressions, with logit coefficients and their standard errors reported in the table. Each logit regression reports the mean of the dependent variable, and indicates with an \* whether a variable is statistically significant. When OLS and tobit regressions are used, *t* statistics are reported in parentheses rather than standard errors. Our testing methodology is to examine three variables (TREND, TREND-AFTER-97, and POST-1997), while holding constant a number of marriage and individual characteristics. Due to the large number of control variables and our interest in the three test variables, the tables include the regression results only for the latter in order to make the tables easier to read and fit on the page.<sup>25</sup> Table 1 defines all the variables used in the regressions, but POST-1997 is a dummy variable that equals 1 after November 1997 when the joint parenting law was enacted. TREND is a straightforward trend variable equal to 1 in 1995, 2 in 1996, and so forth. TREND-AFTER-97 interacts these two. Generally

<sup>25</sup>The full results are available from the authors.

Table 1: Definition of Variables

		<i>Mean Prelaw</i>	<i>Mean Postlaw</i>
<i>Independent Variables</i>			
Length of separation	= Number of years between separation and divorce	1.483	1.482
Length of marriage	= Length of marriage in years	10.675	10.831
Wife's age	= Age of wife in reference year	33.90*	34.77*
Husband's age	= Age of husband in reference year	36.76	37.16
Wife's race	= 1 if wife is white	0.873*	0.842*
Husband's race	= 1 if husband is white	0.863*	0.835*
Wife's education	= Number of years of wife's education	12.79	12.95
Husband's education	= Number of years of husband's education	12.86	12.99
Wife out of state	= 1 if wife lives out of Oregon	0.037	0.042
Husband out of state	= 1 if husband lives out of Oregon	0.084	0.079
Welfare	= 1 if wife is on welfare	0.14*	0.10*
Number of wife marriages	= Number of marriages for wife	1.29	1.29
Number of husband marriages	= Number of marriages for husband	1.29	1.26
Number of children	= Number of children in household	1.88	1.85
County per-capita income	= Per-capita income in county. (1,000s of dollars)	25,324*	26,873*
Husband lawyer	= 1 if husband represented by a lawyer	0.36	0.36
Wife Lawyer	= 1 if wife represented by a lawyer	0.48	0.47
Trend	= 1 in 1995, 2 in 1996, etc.		
Guideline	= The child support guideline for two children at the average income level	\$828	\$799
Trend-after-97	= TREND times POST-1997		
Post-1997	= 1 if reference year was November 1997 or later		
<i>Dependent Variables</i>			
Wife custody	= 1 if wife obtains sole custody	0.66	0.59
Husband custody	= 1 if husband obtains sole custody	0.08	0.10
Joint custody	= 1 if custody shared	0.24	0.28
Split custody	= 1 if husband and wife split custody	0.02	0.03
Mediator	= 1 if the parties used a mediator	0.035	0.112
Spouse support change	= 1 if any change was made to spousal support after divorce	0.081	0.088
Spouse support	= Actual spousal support amount per month	97.95	160.84
Child support	= Actual child support amount per month	359.97	348.38
Divorce timing	= 1 if divorce took place within a given year	0.60	0.40
Abuse allegation	= 1 if either spouse filed domestic violence allegation	0.26	0.21
Court order	= 1 if protective domestic violence order issued	0.23	0.21
False allegation	= 1 if there is an allegation with no order	0.03	0.05
Wife makes allegation	= 1 if wife makes the domestic violence allegation	0.23	0.19
Wife makes false allegation	= 1 if no order is issued after allegation	0.005	0.01
Multiple allegations	= 1 if either the husband or wife made more than one allegation	0.19	0.29

\*Indicates the difference in means is significant at the 5 percent level.

Table 2: Custody Outcomes (Including a Trend Variable); Dependent Variable = 1 if Custody is to (1) Wife, (2) Husband, (3) Joint, or (4) Split

	<i>Wife Custody (1)</i>	<i>Husband Custody (2)</i>	<i>Joint Custody (3)</i>	<i>Split Custody (4)</i>
TREND	0.105 (0.092)	-0.770* (0.203)	0.147 (0.099)	-0.853 (0.439)
TREND-AFTER-97	-0.206* (0.098)	0.853* (0.212)	-0.089 (0.105)	1.087* (0.454)
POST-1997	0.505 (0.276)	-1.464* (0.491)	0.110 (0.301)	-2.293* (1.011)
Chi-square	186.60	282.678	116.755	63.943
% correct	64.6	91.3	72.3	98
<i>N</i>	2,958	2,958	2,958	2,958
Mean dep. var.	0.62	0.08	0.28	0.02

\*Significant at the 5 percent level.

NOTE: Logit coefficients with standard errors in parentheses. See Sections IV.B and IV.C for an explanation of Table 2.

speaking, we are interested in whether there is a change in the trend of our dependent variables after the legal change.<sup>26</sup>

### C. Custody Results

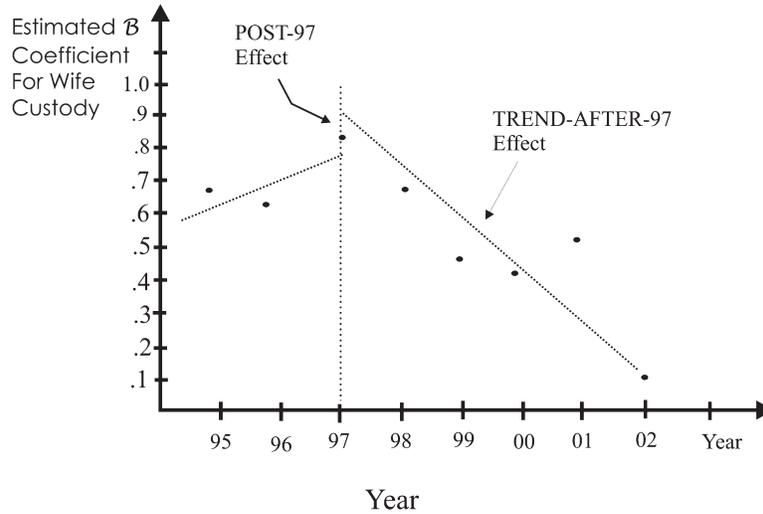
Presumably, the purpose of a joint parenting law is to increase the amount of joint parenting. In terms of gross custody numbers, for those divorcing before the November 1997 cut-off date, 66 percent of the time sole custody was awarded to the wife. After November 1997, sole custody is awarded to the wife on average only 59 percent of the time.<sup>27</sup> This result is confirmed when individual and marriage characteristics are controlled for. Table 2 shows a series of logit regressions, where the dependent variable is 1 for the

<sup>26</sup>Unfortunately, our data are for only one state, and within the state we do not have a control group of undivorced couples. Hence we are unable to do a difference-in-difference experiment across the regime shift. Data from other states are not as complete, and there is no control group within the Oregon data. We did experiment with regression discontinuity techniques by looking at the outcomes in the neighborhood of the legal change. Although this predictably raised the standard errors, the point estimates remained similar and none of our conclusions were affected. Because our different tests all are before/after experiments within a single state, we can only use a time trend to control for time effects. Wolfers (2006) has shown how a time trend might contaminate a trend with dynamic adjustments. He argues for using year dummy variables. We did this, but our data do not allow for very precise estimates. Given our short panel, the contamination problem seems minimal, and we therefore used trend variables.

<sup>27</sup>Local newspapers even noted the increase in father-headed households:

In Marion and Polk counties, households headed by single fathers rose from 1,921 in 1990 to 3,166 in 2000. And the rate of increase for single-dad households is outpacing the rise in households headed by single moms, which increased 31.9 percent (6,574 to 8,674) in Marion and Polk counties. Nationwide, the increase for single moms was 25 percent." (Statesman Journal 2001:1A)

Figure 2: Estimated custody coefficients for wife custody.



various types of custody arrangements.<sup>28</sup> The variables of interest are *TREND*, *TREND-AFTER-97*, and *POST-1997*—the variables measuring a change in legal regime.

Figure 1 showed that the law looks like it contributed to a decline in sole custody for the wife. This is confirmed by the three test regression coefficients in Table 2. According to Regression (1) in Table 2, at the margin, custody for wives had (statistically) not been changing before the law. After the change in custody rules, there is a decline in awards to wives attributable to the legal change. Plotting the sum of the marginal impact of the law yields Figure 2.

The dots in Figure 2 are the estimated custody coefficients for a given year based on the coefficients from the regression. The positive value of *POST-1997* in Regression (1) of Table 2 shows up as a shift up in the *intercept* in 1997, while the large negative coefficient on *TREND-AFTER-97* is reflected in the downward slope of the second trend line. If this graph were drawn for the other regressions in the article, this basic pattern would appear over and over. The legal change to joint parenting often reverses the prior trend, although not always significantly.

In a similar fashion, Regression (2) of Table 2 shows a very strong marginal effect of the joint parenting law on sole custody for fathers. Other things constant, father sole custody was falling over time (*TREND* is negative) until the legal change and then this effect is reversed after the law (*TREND-AFTER-97* is positive). If drawn, the total estimated

<sup>28</sup>We also ran the regressions in Table 2 under one large multinomial regression. The signs and magnitudes of the coefficients remained stable, although their statistical significance improved. We present the logit coefficients because they are easier to interpret, do not depend on a specific reference category, and because the four custodial categories violate the independence assumption to run the multinomial logit.

coefficients would look like an inverted Figure 2. Moving to Regression (4), we see there was a strong reversal in split custody. Split custody is where each spouse receives custody over different children in the marriage. Whereas split custody decisions were declining over time, they start to increase after the legal change. Ironically, and perhaps most surprising, the effect on joint parenting shown in Regression (3) shows there was effectively *no* change that can be attributed to the law. Thus, the joint parenting law altered custody, but this came through changes in sole and split custody. This result is quite surprising.

As is often found in regressions that include legal changes, the three variables testing for the significance of legal change are not the most economically meaningful ones.<sup>29</sup> These regressions control for those variables listed in Table 1 and the two variables controlling for whether the husband or wife lives out of state are quite strong. Each has a large negative effect on gaining custody.<sup>30</sup> Otherwise, the number of children and the presence of a lawyer are the most important variables in explaining custody.

#### *D. Mediation*

Divorces are legally resolved in one of four nonmutually exclusive ways. Couples can resolve the issues themselves by affidavit, with the court only approving the final agreement. They can have their lawyers resolve the issues by stipulation, again with only formal court approval. They can use mediators to reach agreements (ratified by the court), and, finally, they can go to trial (with or without first attempting to mediate). Prior to 1997, private settlement by affidavit accounted for 60.1 percent of settlements. Mediation accounted for only 3.5 percent of settlements. After 1997, mediated settlements account for 11.2 percent of settlements, with most of this increase coming from settlements by affidavit, and some reduction in litigated cases. This increase is understandable, given the directive of the statute to refer disputed custody cases to a mediator.<sup>31</sup> There is virtually no change in settlements by stipulation.

Table 3 shows a logit regression where the dependent variable is 1 if settlement was by mediation. Holding other factors constant, the three test variables show that prior to the joint parenting act, there was no growth in the use of mediators, but after the legal change there was a significant increase in their use.

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<sup>29</sup>Indeed, the explanatory power of these four regressions, and the one on mediation found in Table 3, is not very strong. Comparing the percentage of predicted correct outcomes with the mean of the dependent variable shows only a small improvement over a naïve model that always predicted 1 as the outcome. The effect of the law on custody is quite robust to various specifications, but the legal variables, along with all the other controls, do not explain a lot of the variation in custody.

<sup>30</sup>If we select only those divorces where both spouses remain in state, our results remain unchanged in terms of magnitude, sign, and significance.

<sup>31</sup>A spouse is allowed to decline mediation if it is too stressful, however (typically because of abuse during the marriage).

Table 3: Mediation Resolved Dispute; Dependent Variable = 1 Divorce Was Resolved with Mediation

	<i>Mediation</i>
TREND	-0.071 (0.245)
TREND-AFTER-97	0.514* (0.253)
POST-1997	-1.537* (0.641)
Chi-square	131.337
% correct	91.5
N	2,454
Mean dep. var.	0.08

\*Significant at the 5 percent level.  
 NOTE: Logit coefficients with standard errors in parentheses. See Sections IV.B and IV.D for an explanation of Table 3.

Table 4: Regressions: Child Support and Spousal Support

	<i>Logit</i> (1) <i>Change to Spousal Support</i>	<i>Tobit</i> (2) <i>Spousal Support Full Sample</i>	<i>Tobit</i> (3) <i>Child Support Full Sample</i>	<i>Tobit</i> (4) <i>Spousal Support Wife Custody</i>	<i>Tobit</i> (5) <i>Child Support Wife Custody</i>
Guideline			-0.051 (-4.23)**		-0.052 (-3.66)
TREND	0.050 (0.162)	0.172 (0.20)	-0.558 (-2.83)**	0.588 (0.58)	-0.586 (-2.65)
TREND-AFTER-97	-0.009 (0.173)	0.107 (0.12)	-0.114 (-0.47)	-0.112 (-0.10)	0.032 (0.11)
POST-1997	-0.124 (0.497)	-0.965 (-0.37)	1.530 (1.92)*	-1.377 (-0.42)	0.685 (0.72)
Chi-square	223.87	245.46	138.98	148.73	82.4
N	2,739	3,702	2,046	1,691	1,263

\*\*Significant at the 5 percent level; \*significant at the 10 percent level.  
 NOTE: Tobits have *t* statistics in parentheses. See Sections IV.B. and IV.E for an explanation of Table 4.

*E. Child and Spousal Support*

Generally speaking, the results on custody and mediation reported so far would support Hypothesis H<sub>1</sub> and reject H<sub>3</sub>; however, one might have expected an increase in joint parenting. This latter result might have occurred because the wife merely purchased the custody rights back through compensating changes in spousal or child support awards in cases where joint parenting was awarded. To investigate this, we present five regressions in Table 4 that examine child and spousal support before and after the law change. The first is a logit regression where the dependent variable equals 1 if there was *any* change to spousal support after the divorce. From the estimates in Regression (1) there clearly is no significant change in this variable after 1997—most spousal awards remained the same.

Column (2) shows a tobit regression that uses the actual spousal support values rather than a 0/1 dummy for whether support was made, using the entire sample. Column (3) shows a tobit regression where the dependent variable is the actual amount of child support paid.<sup>32</sup> In both Columns (2) and (3) the sample contains all custody arrangements. Columns (4) and (5) repeat the Tobit regressions, but look at the subsample of wife custody only.<sup>33</sup>

The results from Regressions (2) and (5) in Table 4 confirm the logit results, and find no change in either spouse or child custody awards. This holds for both the full and subsamples. This is a strong refutation of Hypothesis H<sub>2</sub>. If wives had lost custody rights, they might have gained them back by asking for less child or spousal support. This does not appear to have happened.

#### *F. Timing of Divorce*

Most separated couples resolve their divorces quickly. In the Oregon sample, approximately 70 percent of separated couples complete their divorce within one year. Still, that leaves a substantial number who have a delayed divorce. Some of these couples no doubt have difficulty bargaining over child custody. Both may want custody, both may want the other parent not to have it, and the discrete nature of parenthood may make neither satisfied with potential agreements.<sup>34</sup> On the one hand, a law that announces a presumption of joint custody with encouraged mediation might help speed up the bargaining process. On the other hand, if the losing party individually fights against the transfer of legal rights, the time it takes to divorce could increase.

Our data provide an eight-year window on separated couples who end up divorcing. Since our data span eight years, we can use them to create a panel where couples enter in 1995, separate, and remain in the data set until they obtain a divorce. Unlike most panels of married individuals, everyone in the sample actually obtains a divorce. Thus we can think of divorce in this context as a hazard, with no right-side censoring. In Table 5 we use these panel data in the context of a discrete time logit estimator. The regressions in Table 5 have a limited dependent variable. This variable equals 1 if the couple divorce in the reference

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<sup>32</sup>Tobit regressions are used to control for the left-hand-side censoring that results when zero support is paid. Tobit solves a latent variable problem. Given that our data are the *actual* support payments, it is likely that cases exist where custody is granted to the wife, but the child spends enough time with the father that the net payment (in the father's eyes) should be negative, and is, in practice, set at zero. This seems even more likely for spousal support. Hence the tobit procedure seems appropriate.

<sup>33</sup>The regressions on child support include a variable that gives the child support guideline amounts per year for a family with two children at the average income level.

<sup>34</sup>Children have some characteristics that make them public goods within the marriage (Weiss & Willis 1985). When the marriage is going well, one spouse's role as parent does not significantly change when the children are with the other parent. Divorce often changes this. Time with children is often a zero-sum game, especially when one spouse is suspicious of the parenting actions of the other parent.

Table 5: Timing of Divorce; Dependent Variable = 1 if Divorced in Any Given Year

	<i>Unrestricted Regression (1)</i>	<i>Separated Less Than 1 Year and Before 2002 (2)</i>
TREND	1.047* (0.084)	0.762* (0.126)
TREND-AFTER-97	-0.456* (0.088)	-0.119 (0.140)
POST-1997	-1.921* (0.193)	-2.137* (0.368)
Chi-square	1696.670	282.331
% correct	68.5	70.1
N	6,078	1,862
Mean dep. var.	0.47	0.474

\*Significant at the 5 percent level.

NOTE: Logit coefficients with standard errors in parentheses. See Sections IV.B and IV.F for an explanation of Table 5.

year, and equals 0 if they remain separated. Thus a couple who divorces in 1998 would show up in the data for 1995, 1996, 1997, and 1998. The dependent variable for this couple would be 0, 0, 0, 1. If we run a logit regression on these panel data, a positive coefficient on the time test variables would mean the probability of a divorce increased. That is, it would signal that the divorce happened sooner after separation.

Given the structural nature of this test, there is a built-in positive bias on the coefficients of interest. Since our window of data is rather short, couples who separate in, say 1998, but do not divorce until 2003, are not in the data. The divorces near the end of our sample will tend to be short because the very long and recent separations are not selected. Indeed, looking at the raw data, this is clearly a problem for separations longer than two years. However, this will bias the TREND-AFTER-97 and POST-1997 variables in the positive direction, and we find the opposite result. The first column of Table 5 shows the results when the full unrestricted sample is used. Here, where the bias is strongest in the positive direction, we see that the coefficients for TREND-AFTER-97 and POST-1997 are large, statistically significant, and *negative*. In other words, the move to joint parenting increased the length of time to divorce, even in the case where the effect is biased in the opposite direction by sample selection.

The other regression in Table 5 restricts the sample of data to avoid the bias. Column (2), looks at the case where the couples were separated for less than one year before the divorce, and were separated no later than 2001. In this case, there should be no bias at all.<sup>35</sup> In both cases, we see that the move to joint parenting increased the length of separation.

<sup>35</sup>We looked at several different subsamples to test for sensitivity of the selection bias. In the cases for individuals separated for three, two, or one year, the coefficient for POST-1997 was -2.77, -2.59, and -2.26—very similar to our unbiased result.

For those separated less than two years, the increase in time was four months, an increase of 31 percent. In the reduced sample, the coefficient on *POST-1997* is very large relative to the other variables in the regressions and significant. The coefficient on *TREND-AFTER-97* is, not surprisingly, insignificant. These results suggest that, at the margin, the legal change had a large one-time shock on the length of time to divorce.<sup>36</sup>

The results to this point pose an interesting puzzle. Oregon passed a law imposing joint parenting and the use of mediators. However, the change in custody was only in terms of sole custody, and the divorces took considerably longer. Are these two findings related?

### *G. Divorce Resolutions*

One source of increased divorce time is additional predivorce litigation. In Oregon, the joint parenting law included an abuse “safety valve” in order to prevent parents who had engaged in domestic violence from gaining joint custody. Finding someone abusive, however, has to start with an accusation and evidence in court. Parents might even use the threat of an accusation to make the other parent back down on custody claims. Either way, accusations and defense take time, and may explain the increased length of divorce.<sup>37</sup> This type of behavior is likely to take place by both parties: mothers in defense of what they had prior to the legal change, and fathers in an attempt to gain sole custody.

In addition to data on mediation, the divorce records contain information on whether accusations of domestic violence (abuse) were made and whether or not the court issued protective orders based on these accusations. We call cases where an abuse claim was made but no order issued a “false claim.”<sup>38</sup> Prior to 1997 in Oregon, about 3 percent of divorce files had false claims. After 1997, this rises to about 6 percent.

Table 6 presents some regression results on various measures of “trouble” in the divorce proceedings. The logit regressions in Table 6 all examine the issue of accusations of domestic violence abuse. Regression (1) looks at whether or not a domestic violence allegation was made, Regression (2) at whether the court issued a protective order, and Regression (3) at whether the accusation was false or not. In all three of these logit regressions, the dependent variables of interest are *ACCUSE 97* and *TREND-AFTER-97*. *ACCUSE 97* is a dummy variable equal to 1 if the accusation was made after the change in custody law. Regression (1) shows that there was a very large increase in abuse accusations following the change in the law, and then a movement back to the earlier trend. Although the number

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<sup>36</sup>In an alternative test we ran a simple OLS regression with *LENGTH OF SEPARATION* as our dependent variable. The results were almost the same. The joint parenting law increased the length of time to divorce; however, there was no previous significant trend of shorter divorces.

<sup>37</sup>Some have found that joint custody laws have little impact on actual time in the home. See Maldonada (2005:998–99); Scott (2000:1969 n190): “[R]eforms favoring joint physical custody failed to influence behavior because they were apparently inconsistent with the private preferences of parents regarding custodial arrangements.”

<sup>38</sup>This does not necessarily mean there was no abuse. There could also have been a withdrawing of the complaint because of reconciliation or a failure of proof. Since these domestic violence petitions were separate actions, many of which had criminal consequences, the standard of proof was a heightened one.

Table 6: Conflicts and Accusations of Abuse; Dependent Variable = (1) 1 if Accusation (2) 1 if an Order Issued; (3–5) 1 if “False” Accusation; (6) 1 if Multiple Accusations

	(1) Abuse Claim Post-1997	(2) Court Protective Order	(3) False Accusation	(4) Wife Makes Accusation	(5) Wife False Accusation	(6) Multiple Accusation
TREND	0.004 (0.025)	-0.063* (0.027)	0.227* (0.053)	-0.487* (0.044)	0.221 (0.114)	-0.175* (0.161)
TREND-AFTER-97	-1.152* (0.122)	-1.041* (0.118)	-0.288 (0.135)	0.421* (0.097)	0.514 (0.516)	0.345 (0.125)*
ACCUSE 97	8.370* (0.746)	7.584* (0.692)	2.022* (0.767)	2.480* (0.550)	-3.678 (3.613)	-1.334 (0.291)*
Chi-square	493.472	452.220	78.646	1001.643	38.023	38.330
% correct	80.1	82.6	95.5	88.2	99.1	74.8
N	2,473	2,473	2,473	2,473	2,473	658
Mean dep. var.	0.25	0.22	0.04	0.205	0.009	0.256

\*Significant at the 5 percent level.

NOTE: Logit coefficients with standard errors in parentheses. See Sections IV.B and IV.G for an explanation of Table 6.

of accusations starts to fall after the initial jump, the number is still higher in 2002 than in 1996 after controlling for the other variables. Regression (2) shows that courts responded to the increase in accusations by dramatically increasing the number of protective orders after the change to joint parenting. The number of protective orders also falls over time after 1997 as well. Finally, Regression (3) shows that a number of these accusations turned out to be false, and that these false accusations do not fall over time. All these results are statistically significant. Thus one of the possible sources for the increased time to divorce looks like an increase in the number of domestic violence claims. Interestingly, more of these were successful than unsuccessful.

The last three regressions in Table 6 look at two different aspects of these abuse claims. The dependent variable in Regression (4) is 1 if the wife made the accusation. Most accusations of domestic violence were made by wives: 91 percent prior to 1997, and 82 percent after 1997. The coefficient on ACCUSE 97 is large and significant, but then the number of wife claims begins to fall. Regression (5) has a dependent variable equal to 1 if the wife made a false accusation. From this regression we see that wives are less likely to make false accusations after the legal change, but that this effect is not statistically significant. Looking at the raw numbers of the false claims, 21 percent prior to 1997 are made by wives, but after 1997 20 percent still are. Thus, husbands remain more likely to make “false” claims than wives. In examining the divorce files, we discover that false abuse claims made by wives occur mostly because the plaintiff failed to appear at court, while most false claims made by husbands are dismissed by the court for lack of evidence. Finally, Regression (6) in Table 6 examines what happened to the number of multiple accusations after the change in law. This regression shows that the number of cases with multiple accusations increases quite dramatically over time. The bottom line is more divorce cases contained more accusations of domestic violence abuse, and these cases were revisited more often, after the introduction of joint parenting.

## V. CONCLUSION AND DISCUSSION

In the academic and policy arenas of the family, there is an ongoing debate over the neutrality of legal institutions on family behavior. Becker (1981) provided a theoretical argument for legal neutrality in divorce based on the Coase theorem.<sup>39</sup> In addition, advocates for family law reform often have implicit theories of family formation in which changes in behavior in response to a legal change are not an option.<sup>40</sup> Empirical studies, however, have generally shown that changes in family law matter, although these changes are not always large, and not always in the directions Becker would have predicted. This article has empirically examined a change in custody rules, and has shown that this legislation did influence behavior on several margins, mostly in ways one might expect. In particular, the movement toward joint custody had real effects on custody awards, the use of mediators, and divorce proceedings. Paradoxically, many changes were not those sought by the legislation's proponents, who sought more equal parenting in the context of less acrimonious and less costly divorces and mediated solutions. Rather, the legislation created incentives that led to more dragged out and acrimonious divorces and no more equal parenting.

The 1997 change to Oregon's child custody law introduced what many would consider a "minor" change: a move to a presumption of joint custody, or "joint parenting." This change was lobbied for by father's rights groups and, presumably, was intended to benefit children through greater parenting by both parents. However, the inclusion of an "abuse" escape clause led to some surprising changes in divorce outcomes. To summarize, we found that the change in custody presumption led to:

1. A fall in sole custody to mothers, a rise in sole custody to fathers, and surprisingly, little change in joint custody.
2. A strong and significant change in the percentage of mediated divorces.
3. No significant change in the raw number of spousal support motions or in the dollar awards of spousal or child support.
4. A decrease in the speed of divorce. That is, divorces happened later after separation.
5. An increase in the number of abuse actions filed, particularly by wives. These abuse actions led to an increase in court no-contact orders, though not by as much as the increase in claims. The accusations were made by both the husband and wife, and were more likely to be repeat allegations.

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<sup>39</sup>Despite empirical evidence to the contrary, this view is becoming a creeping stylized fact among economists. McCloskey, in an otherwise delightful piece encouraging economists, among other things, to get their history correct, states: "For example, economists 'predict' . . . that, surprisingly, no-fault divorce should have no long-term effect on the prevalence of divorce. . . . And such a surprising claim on the basis of Prudence alone seems to be factually true in the world" (2002:23–24).

<sup>40</sup>Consider, e.g., a theory often touted in the same-sex marriage debate. "Love"-based theories of marriage suggest that loving couples should marry, nonloving couples should divorce, and that "love" is exogenous to the couple and independent of sex.

Taken together, the results paint the following picture. The change in joint parenting law transferred custody rights to fathers who were able to use them to increase sole and split custody. There is no evidence that mothers bought these rights back through reduced support. In addition, the “abuse” escape clause, although utilized by both parents, was mostly wielded by wives. Thus, at least over the first five years of the law, the formal transfer of legal rights led to more disputes in settling the divorce, leading to longer divorces. Our evidence suggests this increase in the length of trial comes from increased accusations of abuse and battles over custody of children.

Considering that many states are interested in joint parenting laws, our results have relevance for policymakers. The small change in custody rights has real effects, and on margins some might not anticipate. Our results suggest that care should be given in designing custody laws.

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