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Joint custody law and mothers' labor market outcomes: evidence from the USA

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Abstract This paper studies the economic implications for mothers of the changes in child custody law from maternal preference to joint custody using the 1960–2000 Census Public Use Micro Sample (IPUMS). Variation in the timing of the joint custody reform across states provides a natural experimental framework to study the causal effect of shared custody on mothers' economic outcomes. The results show that only single mothers experience a decrease in earnings as a consequence of the adoption of the joint custody law, exposing them to a higher risk of poverty. The paper discusses a possible explanation for these findings, namely that the higher child support payment the mother receives from the non-custodial father in case of joint custody might discourage her from looking for high paid jobs or investing in her career.

Keywords Joint custody law · Bargaining · Difference in difference

JEL Classification J12 · J13

1 Introduction

In the last quarter century, the US has witnessed remarkable changes in the family. The social institution of marriage has been increasingly substituted by cohabitation,

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while divorce rates had already risen sharply from the mid-1960s to the early 1980s (Stevenson and Wolfers 2006). Although many scholars have looked at the consequences of the move from mutual consent to unilateral divorce law (Wolfers 2006; Friedberg 1998; Stevenson 2007; Gruber 2004, among others), only recently has the economic literature started to pay attention to another important aspect of divorce, namely the custody of children after divorce.

The allocation of custody is a crucial aspect of any divorce process since it determines the post-divorce living arrangements and potentially has long-lasting consequences on parents and children's well-being. California was one of the first states in the US to introduce a law favoring joint custody at the end of the 1970s and since then it has been extended to most states (Brinig and Buckley 1998). Rather than awarding sole custody to one parent (traditionally the mother) and limited visiting rights to the other (the father), joint custody is intended to preserve the parental role and status of both parents after separation, enabling continued involvement and ensuring the best interest of the child. Under joint custody, the child is expected to spend time with each parent (joint physical custody) and parents need to agree on important decisions regarding their children (joint legal custody). The financial arrangements of parents also change since they are directly called to contribute child expenses.

This study investigates whether joint custody reform affects labor market outcomes of mothers by examining the reform's impact on poverty, earnings, other income received, probability of relying on welfare, and of being in the labor force. The most obvious way custody reform affects maternal labor market outcomes is through the divorce propensity, but the sign of this effect is unclear a priori. For example, married mothers may be less likely to file for divorce because they expect to see their time with their children reduced, with a negative effect on divorce rate. Conversely, joint custody might lead mothers to invest less in their children and marriage due to a lower expected return in these specific marital investments, generating a positive effect on divorce rates. Despite the ambiguous prediction on divorce, custody reform can also directly affect the labor market outcomes of married mothers by altering the balance of power between spouses within marriage (Chiappori et al. 2002; Allen and Brinig 2011; Chiappori et al. 2002; Nunley and Seals 2011). Joint custody may also affect the behavior of divorced/separated mothers, although with potentially opposite effects. On the one hand, there might be a redistribution of childcare responsibilities among ex-partners since separated partners spend less time on parenting and more time in the labor market. On the other hand, divorced/separated mothers might devote more time to the re-marriage market, thus increasing their likelihood of re-marriage and decreasing the labor force participation.

In order to investigate the possible effect of joint-custody law on mothers' labor market outcomes, I use the exogenous variation provided by the custody reform across the US states as a natural experiment to identify the average treatment effect of the reform. The same identification strategy to study the impact of joint custody on several parental and children outcomes in the US has been used by Halla (2013), Teng (2008), Nunley and Seals (2011), Altindag et al. (2015), and Allen, Nunley and Seals

(2011). Mothers with children who live in states that introduced the joint custody law between 1970 and 2000 constitute the treatment group, while the comparison group is made of mothers who live in states that had yet to adopt joint custody law by 2000. This study provides two important extensions to the existing literature. First, it estimates the effect of the joint custody reform on several economic outcomes of mothers and not only on the labor force participation and hours of work of (married) mothers (see Nunley and Seals 2011; Altindag et al. 2015; Halla 2013). Second, the results for single mothers are interpreted in the light of a new mechanism, the increase in child support payments, that, to the best of knowledge, has yet to be linked to mothers' behavior. Allen et al. (2011) have investigated the effect of joint custody legislation on the child support received by single mothers; Halla (2013), Böheim et al. (2016), and Nunley and Seals (2011) have studied the effect of joint custody reform on the labor force participation of women and married mothers respectively, but none of these studies has linked these three aspects together.

The findings indicate that only divorced/separated/never married (single, henceforth) mothers are negatively affected by the joint custody law in terms of a decrease in earnings, which exposes them to a higher risk of poverty. The probability of being in the labor force is instead unchanged with the passage of the custody law. Importantly, the results are not driven by changes in the composition of family structure as a consequence of the introduction of the joint custody law: on aggregate, the proportion of children living with a single parent or in intact families does not change. This is extremely relevant for the analysis because it rules out that selection effects due to a changing composition in the stock of married and unmarried population might affect the estimates and allows us to focus exclusively on single mothers, for whom the joint custody reform turns out to have an impact. The results are robust to controlling for unilateral divorce, specific property division laws and child support legislation. No evidence is found that differences in the exposure to the reform matter for mothers' labor market outcomes. Child's age at the reform does not matter, except for the odds of living in poverty, which increases as the child ages, and for the probability of receiving "other income," which is higher when the child is young.

The findings can be explained by the higher child support payments a mother receives from the non-custodial father in case of joint custody, which might discourage her from looking for a high paid job or investing in her career. Since there is no effect on mother's labor force participation, it can be argued that the drop in earnings might be due to a reduction in the number of hours worked. Moreover, since the probability of living in poverty increases, the increase in compliance with child support payment seems not to be enough to compensate the mother's reduction in earnings - especially for low income mothers.

The rest of the paper is organized as follows: Section 2 describes the evolution of the law concerning the custody decision over time; Section 3 briefly discusses the theoretical framework and related work on this topic; Section 4 describes the data employed in the analysis and the sample selection; Section 5 illustrates the identification and empirical strategy; Section 6 presents the estimation results and some robustness checks; while Section 7 provides possible interpretations of the results.

2 Legal background

Looking at the custody decision making in historical context, the determination of which parent should have custody of children in a divorce proceeding has gone through several changes in the US (Brinig and Buckley 1998). In case of divorce, until the mid-nineteenth century, fathers had a near absolute right to custody. In the late 1800s, following the increasing specialization within the family with the father acting as the breadwinner and the mother looking after the children and the house, the paternal preference was gradually replaced by the maternal preference. In many states, maternal preference took the form of the "tender years doctrine" (under age 6), that presumes the mother is the more suitable custodian for children in case of parental separation.[1]

By the 1920s, the maternal preference for custody, regardless of the child's age, became firmly fixed and was encoded in the statutes of all 48 states. The maternal presumption for custody remained in this form for many decades, to be challenged only after the divorce rate began to rise in the 1960s. Legal practice changed between the 1970s and the 1990s, after the Family Court of New York invalidated the "tender years doctrine" in New York State (*Watts v. Watts*, 1973) in favor of the doctrine of *the best interest of the child* (BIOC), which makes no reference to the gender of the parent. Since then, other states moved from the explicit maternal preference to the gender-neutral custody assignment (Mason 1999). Despite this legal change, many courts adopted the "primary caretaker presumption," which operated in a gender biased manner since in the vast majority of the cases the primary caretaker was identified as the mother (Cancian and Meyer 1998; Rose and Wong 2014).[2]

Many other changes occurred before the mid-1970s, i.e., women's increasing labor force participation, the feminist movement, fathers' claims of sex discrimination in custody decisions, and constitutional concerns for equal protection (Jacob 1988).

All these changes prepared the path for the next step, the joint custody after divorce. Between the late 1970s and the early 1980s, the joint custody statute was enacted in California, Connecticut, Kansas, Kentucky, and Oregon. By 1990, more than 40 states had shared parenting statuses in which joint custody was either an option or preference, and most other states had recognized the concept of joint custody in case law. Contrary to other family-law reforms, opinions from experts were rare in the legislative discourses on joint custody (Jacob 1988). In the absence of scientific evidence on the consequences of joint custody for children, the reform was passed in relative obscurity (Jacob 1988).

[1] In the "tender years doctrine," children were no longer viewed as economic assets or as workers under the control of their fathers; instead they came to be seen as individuals with their own needs and in the case of young children, they were recognized to have special needs for nurture, normally satisfied by the mother. Current laws do not give special attention to child nurture and do not recognize the importance of developmental stages. In other words, two-year old children are treated the same as children of twelve.

[2] Utah is a typical example of inconsistency between state statutes and case law. Although the explicit maternal presumption was repealed by the legislatures in 1977, the courts continued to practice maternal preference toward mothers in child custody cases until 1986; the judicial preference in favor of the mother was explicitly abolished in the case *Pusey v. Pusey* in 1986 (Chen 2013).

Nowadays, in most US states, the most common arrangement is joint legal custody and sole or primary physical custody to one of the parents.[3] Law documents say that in a typical divorce involving at least one child, permanent physical custody is awarded to the parent with whom the child will live most of the time. Usually, the custodial parent shares joint legal custody with the noncustodial parent. As a matter of fact, the introduction of the joint custody law has not changed the living situation of the families but only the legal aspects of the custody arrangements. Further evidence of this is provided in Section 6.1.

There is evidence that judges have followed the prescriptions of the new custody laws. Halla (2013), using data from divorce certificates provided by the *National Vital Statistics System* of the *National Center for Health Statistics*, shows that the probability of a joint custody award increases by 3 percentage points each year after the introduction of the joint custody. Further support for this finding is provided by Allen and Brinig (2005), Brown et al. (1997), and Maccoby and Mnookin (1997) who suggest that there was a substantial increase in the number of joint custody arrangements after the reform.[4] Evidence on the application of the law is particularly important in the identification strategy (see Section 5) because it proves that the reform did not exist only on papers but it was actually implemented by states.

3 Theoretical considerations

Changes in child custody law may influence the bargaining that takes place within marriage and modify the resource allocation in it. *Exit threat* bargaining models (Manser and Brown 1980; McElroy and Horney 1981) emphasize the role played by each spouse's best outside alternative to marriage as relevant in determining the household distribution. Any policy changes, like custody reform, that alter divorce-threat point potentially affect allocation decisions. In this case, the introduction of joint custody will, ceteris paribus, improve the bargaining position of the spouse who most likely would have been the non-custodial parent, i.e., the father, while the opposite holds true for the mother. By contrast, in collective models, allocation decisions are the result of bargaining over possible decisions and do not assume any specific decision-making process (Browning and Chiappori 1998; Chiappori et al. 2002).

[3]Nearly all the adopted states have distinguished in their legislation between legal and physical custody. Legal custody refers to the parental right to make major decisions regarding child's health, education, welfare etc. Physical custody refers to the living arrangement of the child on a day-to-day basis. In states permitting or encouraging joint legal and physical custody arrangements, it is difficult to determine what percentage of parents have joint legal or physical custody, as the data must be obtained from individual divorce decrees. For this reason no distinction is made between the two in this analysis and the child custody reform that is examined is meant to capture the overall change to both types of joint-custody arrangements.

[4]Allen and Brinig (2011) found that joint parenting in Oregon led to changed custody arrangements, but it came through changes in sole and split custody (some of the children living with one parent, and some of the children living with the other parent) and not in joint custody. However, these results should be interpreted with caution since they only refer to one state and are based on a before/after approach.

Most of the theoretical literature has focused on how changes in family laws predict changes in labor force participation of married couples. Both divorce-threat-bargaining and collective models provide clear predictions concerning the effect of family-law reforms on household labor supply, with men allocating more time to household work and less to market work, and the opposite for women (Altindag et al. 2015). This could have a twofold advantage: raise the mother's bargaining position within marriage by increasing her labor market skills and insure her against increased divorce-threat of fathers who, after the custody reform, can rely on higher expectations of shared child custody. Similarly, Grossbard-Shechtman (1995) shows that laws affecting divorce settlements, like the no-fault divorce, can change the equilibrium outcomes in the marriage market by reducing the demand for spousal labor by men and increasing women's labor market participation. This reasoning can be easily extended to the custody reform. This suggests that bargaining power and labor supply are inversely related. In contrast, Gray (1998) shows that married women reduce their labor supply when they lose bargaining power.

However, Lundberg and Pollak (1993) point out that most of the bargaining literature focuses on intra-household bargaining, rather than on couples who have already separated or are close to dissolving their marriage. For an average married couple, far from the possibility of divorce, changes in legal rules regulating child custody might have a weak impact on individual behavior. On the contrary, (close to) separated couples might be the most sensitive to changes in legal rules that immediately influence their bargaining position; for them, changes in the allocation of property and custody settlements decided by courts at divorce might be especially relevant. Joint custody reform might therefore affect labor market outcomes of single mothers through different channels than the ones of married mothers, with no clear-cut predictions. On one side, under the new custody regime single mothers might spend on average less time on parenting, thus increasing the time devoted to work. On the other side, they might increase the time spent on the re-marriage market, which in turn increases their probability of re-marriage and reduces their labor market participation.

Ascertaining how married and single mothers respond to the joint custody reform is ultimately an empirical question upon which this paper intends to shed some light.

3.1 Related work

In the last decades, there has been an ongoing debate among the supporters and opponents of custody reform. Proponents typically argue that children may benefit from more parental resources, in particular from fathers both before and after divorce, and the benefits might extend to parents as well. According to the *bonding theories*, before divorce a father might be more willing to invest in, or bond more closely with, his children (and possibly with his wife) if he is not going to lose access to his child(ren) in case of divorce (Brinig and Buckley 1998). After divorce, according to the *monitoring theories*, on a move to joint custody a father might be more ready to give child-support payments if joint custody solves the agency-cost problem arising from the fact that the non-custodial parent (usually the father) is not able to monitor how the child-support payments are actually spent (Brinig and Buckley 1998; Weiss and Willis 1985). Divorced parents who share physical custody of their children also

share the time and energy costs of homemaking and parenting, facilitating labor force participation or investment in their careers (Morrow 1991).

Opponents emphasize the countervailing effects of joint custody on families. The shift from the maternal custody regime to the joint custody regime might disadvantage women in their property negotiations if they give more value to sole custody than the spouse gives to joint custody and trade-off property for sole custody (Brinig and Buckley 1998; Rasul 2006). Moreover, joint custody might not work well in high-conflict families (Mason 1999) and eventually might lead to less support for children if the court establishes a reduction in child support payments entitled to a mother (Morrow 1991; Seltzer and Maralani 2001; Mason 1999). Finally, mothers' careers and educational opportunities might be restricted by limitation in their geographical mobility, incorporating a gender-based discrimination that reinforces the economic dependency of women (Bartlett and Stack 1991).

A number of researchers have attempted to provide empirical evidence of the effects of joint custody reform on individual behavior and children's well-being. Research on the relationship between changes in family law and individual behavior has mainly focused on the effect of custody law on divorce, marriage, fertility and employment decisions in the US. Brinig and Buckley (1998) find a negative effect of the introduction of joint custody law on divorce rates. Recently, Halla (2013) has shown that the introduction of joint custody led to a long-run increase in marriage rates and fertility, a decrease in female labor market participation, and a not significant effect on divorce rates probably due to countervailing forces, i.e., costs effects and behavioral effects, which move in opposite directions.[5] Further, he finds that male suicide rates and domestic violence fell in treated states. Qualitatively similar effects are found for marriage and fertility rates in Austria and Germany, but differently from the previous study, the divorce rate decreased after the custody reform (Böheim et al. 2012). Focusing only on married mothers, Nunley and Seals (2011) find an increase in labor force participation in law-adopting states in the US. Similarly, Altindag et al. (2015) show that custody reform encouraged a reallocation of time within marriage, with married mothers increasing the time devoted to work in the market and married fathers increasing the time allocated to work at home.[6,7]

Several studies have looked at the effect of custody arrangement on children's well-being after divorce with mixed results. Consistent with the *bonding theories*, past research has shown that fathers awarded joint custody spend more time with their children (Seltzer 1998) with positive effects on children's behavioral and emotional

[5]Possible reasons for the difference in findings between the two studies could be attributed to differences in the definition of divorce rates, in the time window and in the functional form of their estimation models.

[6]The differences between Halla (2013) and Nunley and Seals (2011) and Altindag et al. (2015) might be explained by the different sample selection and estimation procedure.

[7]Recently, two studies have examined the effect of the introduction of the gender-neutrality in the US, according to which fathers have equal rights to custody at divorce. Chen (2013) shows that gender-neutral laws increases divorce rates in the long run, while Rose and Wong (2014) finds that the gender-neutral law decreases the hazard into marriage. Rose and Wong (2014) also investigate the subsequent move toward marriage neutrality, which extends to unmarried fathers the possibility to gain sole custody if they qualify as a suitable custodian. They find no evidence of a reform effect on marriage rates.

adjustment (Bauserman 2002). Joint custody is found to increase the involvement of nonresident parents in their children's lives, although this effect diminishes over time (Huang et al. 2003). Teng (2008) shows that the educational attainment of children of divorced or separated parents increases in states adopting joint custody law. Other studies have however highlighted the negative effect of joint custody law on (i) children exposed to parental conflict, especially among families who showed a high intensity of inter-parental hostility at an earlier time (Maccoby et al. 1991; Kuehl 1989; Mason 1999), (ii) on children of intact families that experience a decrease in their educational attainment (Teng 2008), and (iii) on children raised in a joint custody regime who reduce their probability of attending a private school (Nunley and Seals 2011) and experience worse labor market outcomes (Maiti 2015).

A few studies have focused on the impact of child-custody on the receipt and level of child-support income, with no clear-cut indications. Confirming the predictions of the *monitoring theories*, some studies find that fathers awarded joint custody are more willing to comply with child support orders than non custodial fathers (Del Boca and Ribero 1998; Huang et al. 2003; Allen et al. 2011). However, other studies find no effect (Seltzer and Maralani 2001; Seltzer 1998; De Blasio and Vuri 2013; Allen and Brinig 2011), or evidence of heterogeneity according to marital status (Allen et al. 2011). In particular, joint custody reform has an effect only on divorced mothers, while separated and never-married mothers are unaffected by the reform.

The present analysis improves upon much of the previous research by providing a broader view of the labor market consequences of joint custody reform for single mothers and by linking their market behavior with the finding of an increase in child support recipiency.

4 The data and the sample selection

The primary data for the 50 US states and the District of Columbia come from the 1960 to 2000 Census Five-Percent Public Use Micro Sample (IPUMS).[8] The focus here is on mothers between 19 and 55 years of age whose youngest child is under 18 years and therefore still potentially involved in child custody disputes.[9]

Several variables are used to measure mothers' labor market outcomes: (i) poverty, (ii) earnings, (iii) other income and its dummy, (iv) labor force participation, and (v) being "on welfare." Poverty is an indicator equal to one if the mother falls below the official "poverty line"; the variable "earnings" reports the mother's total pre-tax wage and salary income, that is, money received as an employee for the previous year; "other income" is computed as an individual's total income, or losses, from sources

[8]The 1960 Census sample is included to allow for a credible identification of *preexisting* state specific trends (Gruber 2004; Wolfers 2006).

[9]Stepmothers are excluded since the census questions identifying this group changed in 1990. Moreover, remarried families have many confounding effects that make the impact of joint custody less clear-cut. Widowed mothers—who represent a very small fraction of the population (only 0.2%) mainly due to the selection of relatively young women in the sample—are excluded. As expected, the inclusion of this group in the sample does not change the results.

not included in the other IPUMS person-record income variables;[10] a dummy variable "being on welfare," equal to one if the mother receives income from any public assistance programs and zero otherwise.[11] Finally, the set of income-related variables is completed with "labor force participation (LFP)," a dummy variable equal to one when the mother is in the labor force.[12] All the income variables are expressed in 2000 dollars. Since some income variables have different top codes over years, consistent values of the top codes are generated by imposing the real values of the 1960s top codes (the most restrictive) in each year, as suggested by Genadek et al. (2007). This procedure makes the variables used in the analysis comparable across years. Summary statistics for the whole sample and separately for married mothers and single mothers are presented in Table 1. Panel A reports the summary statistics for the outcomes, Panel B for the control variables (age, level of education, and race of the mother, total number of children and age, gender and year of birth of the youngest child in the household).

During the period of analysis, many states have changed their child custody law, as shown in column 1 of Appendix Table 8. The coding of the custody law for each state is from Teng (2008), who distinguishes states with custody law awarding joint custody only if parents are in agreement from states which allow application by only one parent.[13] This distinction is important because if agreement is necessary, the threat of opposing is credible and may affect intra-household bargaining power. For six states (Colorado, Nebraska, North Carolina, Oregon, Vermont, and Wisconsin) both the year of the more restrictive custody rule and the year of the more permissive one are reported.[14]

Appendix Table 8 also provides a summary of the rules in place over the period of analysis by state. The unilateral divorce coding in column 2 is from Gruber (2004), while the data on property division types (columns 3 and 4) are from Gray (1998) and Stevenson (2007). Columns 5 to 12 report the year of introduction of eight child support laws which measure the vigor of state child support legislation (from Huang 2002 and Huang et al. 2002). The legislative activity covers two steps of the enforcement process: obtaining adequate child support orders and collecting child support

[10]Results related to the "other income" variable should be interpreted with caution since this variable is not perfectly comparable across years. As reported in the IPUMS documentation (see Ruggles and Sobek (1997)), in 1960 respondents were simply asked to declare the amount of money income received from sources other than wages and self-employment. Beginning in 1970, other possible sources of additional income, such as the sale of personal property, lump-sum insurance and inheritance payments, payments "in kind", veterans' payments, unemployment compensation, child support, and alimony were added to the list of excludable variables. The exclusion of the year 1960 does not change the results.

[11]The variable "being on welfare" includes federal/state Supplemental Security Income (SSI) payments to elderly (age 65+), blind, or disabled persons with low incomes, Aid to Families with Dependent Children (AFDC) and General Assistance (GA).

[12]Unfortunately, it was not possible to use the number of hours worked because this variable is not collected throughout the Censuses.

[13]Brinig and Buckley (1998) provided another code for joint custody but it is update only up to 1993. For this reason this paper prefers the coding proposed by Teng (2008), who has nevertheless proved his results to be robust to the use of the Brinig and Buckley's coding.

[14]The analysis uses the more restrictive coding but the results are unchanged if the other rule is considered. Results are available on request.

Table 1 Sample means for mothers with children under 18 years and by marital status

	All sample		Single mothers		Married mothers	
	Mean (1)	Std. Dev. (2)	Mean (3)	Std. Dev. (4)	Mean (5)	Std. Dev. (6)
Panel A: Outcomes						
Poverty	0.134	(0.341)	0.381	(0.486)	0.088	(0.283)
Log earnings	9.33	(11.93)	9.48	(10.91)	9.29	(12.15)
Log other income	8.08	(11.93)	7.96	(14.05)	8.11	(11.25)
Other income (0/1)	0.258	(0.438)	0.357	(0.479)	0.240	(0.427)
On welfare	0.054	(0.227)	0.224	(0.417)	0.017	(0.130)
LFP	0.550	(0.497	0.732	(0.443)	0.516	(0.499)
Panel B: Controls						
Mother's variables						
Married	0.842	(0.365)	–	–	–	–
Divorced	0.078	(0.268)	0.492	(0.500)	–	–
Separated	0.035	(0.185)	0.224	(0.417)	–	–
Never married	0.045	(0.207)	0.284	(0.451)	–	–
Age	35.46	(8.12)	34.08	(8.16)	35.72	(8.08)
Less than high school	0.238	(0.426)	0.259	(0.438)	0.234	(0.424)
High school	0.401	(0.490)	0.391	(0.488)	0.403	(0.490)
College graduate	0.361	(0.480)	0.350	(0.477)	0.363	(0.481)
White	0.836	(0.370)	0.624	(0.484)	0.876	(0.329)
Black	0.110	(0.313)	0.307	(0.461)	0.073	(0.261)
Other race	0.053	(0.224)	0.069	(0.253)	0.051	(0.218)
Hispanic origin	0.074	(0.262)	0.099	(0.299)	0.069	(0.254)
Tot n. of children	2.08	(1.18)	1.85	(1.10)	2.12	(1.19)
Youngest child's variables						
Female	0.490	(0.499)	0.497	(0.499)	0.489	(0.499)
Age	7.00	(5.43)	7.65	(5.28)	6.88	(5.45)
N	1,441,684		228,089		1,213,595	

Source. The 1960, 1970, 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS).

Notes: Log earnings is computed on 842,615 observations for the whole sample, on 170,135 observations for the sample of single mothers and on 672,480 observations for the sample of married mothers; log of other income is computed on 372,553 observations for the whole sample, on 81,340 observations for the sample of single mothers and on 291,213 observations for the sample of married mothers; on welfare is not available for year 1960 and it is computed on 1,194,473 observations for the whole sample, on 215,170 observations for the sample of single mothers and on 979,303 observations for the sample of married mothers.

payments. The eight child support laws considered are paternity and genetic tests, numerical guidelines, presumptive guidelines, wage withholding under delinquency, immediate wage withholding for a new case, universal wage withholding, and state

income tax interception. Numerical guidelines advise judges concerning child support laws and are non-binding. Presumptive guidelines require that these numerical guidelines are used unless the judge can cite good reasons to deviate from them. The wage withholding under delinquency measure indicates that the state has developed a system, similar to income tax withholding, that allows the deduction of child support obligations and any arrearages accrued from the obligor's pay-check. Immediate withholding laws require all new or modified support orders for welfare recipients to have immediate withholding of support. Finally, the state income tax interception measure indicates that the state has procedures available to garnish state income tax refunds, up to the amount of overdue child support. A 2-year lag between legislative enactment and implementation is assumed.

5 Identification and estimation strategy

As shown in Appendix Table 8, states vary substantially in the year they enacted joint custody law and there is convincing evidence that the introduction of joint custody can be considered random. For instance, Halla (2013) has shown that the introduction of joint custody does not follow any systematic geographical patterns, there is no relationship between the size of the state and the timing of the reform and there are no links with political ideology. A similar conclusion has been reached by Altindag et al. (2015), who show that trends in married mothers' and fathers' time allocated to market work and household work in early-adopting states are similar to those in late-adopting states. Moreover, they show that labor market participation, weeks worked, and hours of work of married mothers follow similar patterns in states that will adopt and will not adopt the joint custody in the years preceding its introduction. This apparent randomness in the introduction of joint custody law across states and time is also supported by historical accounts. Jacob (1988, ch.8) reports that joint-custody reform was discussed by a small group of proponents and passed legislatures in relative obscurity.

Taking advantage of the randomness in the introduction of joint custody law and the evidence on the change in joint custody arrangements following custody reform (mentioned in Section 2), the impact of the reform on mothers' labor market outcomes can be estimated using a standard Dif-in-Dif approach. The different timing in the introduction of joint custody across US states provides a useful quasi-experimental setting to study the causal effect of joint custody on mothers' outcomes. Identification is thus obtained through variation across states and over years in which states implemented the joint custody reform.[15] Mothers with dependent children who live in states that change their laws to favor joint custody by year 2000 constitute

[15] This approach has been used extensively in the literature investigating the consequences of introducing the unilateral divorce law (see Gruber 2004; Friedberg 1998; Stevenson 2007 and 2008; Stevenson and Wolfers 2006, Wolfers 2006, among others). The same approach has been used to study the consequences of adopting the joint custody law on several outcomes (see Halla 2013; Teng 2008; Böheim et al. 2012 and 2016; Altindag et al. 2015; Nunley and Seals 2011; Allen et al. 2011; Rose and Wong 2014; Chen 2013). For a general overview of the difference-in-difference models see Angrist and Pischke (2009) and Wooldridge (2003).

the treatment group, while those who live in states that did not institute joint-custody reform before 2000 represent the control group.

In the first part of the analysis, I estimate the impact of legal changes on mothers' outcomes in the same time period , i.e., the *contemporaneous* effect of legal changes. This approach is similar to Friedberg (1998) and Wolfers (2006). Specifically, I estimate a Dif-in-Dif model of the form:

$$y_{ist} = \beta \times JC_{st} + \mu_s + \tau_t + \mu_s \times t_t + \gamma X_{ist} + \varepsilon_{ist} \quad (1)$$

where y_{ist} represents the outcome of interest for mother i resident in state s and observed in year t. The variable JC_{st} is dummy variable equals to one if the law is in place in state s at year t and zero otherwise. All estimates include a vector of state dummies μ_s and Census year dummies τ_t.

The focus of the analysis is on the parameter β, which captures the difference in the outcome variables between mothers in joint-custody versus sole-custody states. X_{ist} represents a vector of mother characteristics: age, race, ethnicity (a dummy variable for Hispanic origin), family structure indicator variables, number of children at home, educational attainment indicator variables (high school graduate, some college and college). Some child characteristics are also included: sex, age, and year of birth of the youngest child in the household, i.e., the one for whom the custody agreements are more binding. Census year dummies τ_t control for mothers' outcomes growth common to all states; state fixed effects μ_s control for unobserved influences on mothers' outcomes that vary across states. To take into account the fact that some factors influencing individual outcomes may vary within a state over time, state-specific linear time trends are included, indicated by the interaction $\mu_s \times t$ in Eq. 1, which capture the influence of any omitted time-varying state-level variables.[16] All the regressions include a full set of child age by year, by state of residence and by state of birth interactions to allow for differential time or state patterns by age of the child. Finally, ε_{ist} captures idiosyncratic variation by individual. Recent analyses demonstrate that pervasive serial correlation in state level difference-in-difference models may produce downward biased standard errors (Bertrand, Duflo and Mullainathan, 2004). For this reason, Huber-White SEs clustered at state level are used throughout, which are robust to arbitrary forms of error correlation within a state.

Ideally, the relevant variables would be measured at the year of divorce when joint custody is usually decided instead of the survey year; unfortunately, census data do not provide this information. However, given that the custody arrangement is decided according to the prevalent law in the individual state of residence, it can be assumed that mothers make their decisions based on the prevailing custody law in their current

[16] As pointed out by Nunley and Seals (2011) for example, the increase in female labor force participation rate and fathers' involvement in childrearing could be correlated with the passage of joint-custody reform. Failure to account for these state-level covariates could bias the estimates. However, fixed trend models are quite demanding when using census data because there are few time-series observations (in this case only five). Moreover, trends measuring decade time windows will not be able to capture the short-run endogeneity, but only the long-term one (Gruber 2004; Angrist and Pischke 2009).

state of residence (Halla 2013). The issue of endogeneity in moving will be explored in Section 6.4.

It is important to mention that year, state, state-specific linear trends, and interaction effects capture the differences in mother's outcomes across states and their changes *only* under the hypothesis that there are no selection effects into different family structures, like marriage or divorce. In absence of selection, the coefficient on JC_{st} identifies the net effect of joint custody, otherwise it only captures a gross effect (Teng 2008). Evidence on the absence of selection effects will be provided in Section 6.1.

The *contemporaneous* DID estimator used in Eq. 1 imposes the assumption of immediate and constant response to the reform. However, labor market outcomes might respond to the adoption of the custody law via a dynamic process. To take into account this possibility, this paper uses the empirical strategy suggested by Wolfers (2006) and adopted by Halla (2013) and Chen (2013) who introduce a dynamic structure of the policy intervention through dummy variables for the years after the adoption of the joint custody law. Specifically:

$$y_{ist} = \sum_{r=0}^{m} \beta_{-r} JC_{s,t-r} + \mu_s + \tau_t + \mu_s \times t_t + \gamma X_{ist} + \varepsilon_{i,s,t} \tag{2}$$

where $JC_{s,t-r}$ denotes a series of binary variables equal to one if a state s has introduced joint custody r years ago. Lags up to 15 years after the reform are included. This flexible specification allows us to trace out the outcomes' full adjustment path.

The validity of the DID approach is based on the common trends assumption which requires that in the period before the treatment the trend in the outcome(s) in the treated and control groups is the same or very similar (Angrist and Pischke 2009). This is equivalent to saying that the unobserved determinants of the mothers' outcomes for the treated and controls would have followed parallel trends had there been no exposure to the treatment. While the common trend assumption is no testable by assumption, it is possible to assess its plausibility using the "Granger Causality" approach taken in Autor (2003) and described in Angrist and Pischke (2009). The Granger causality testing means checking whether, conditional on state and year effects, past policy variable JC_{st} predicts y_{ist} while future JC_{st} does not. Therefore, a model of the following form is estimated:

$$y_{ist} = \sum_{r=0}^{m} \beta_{-r} JC_{s,t-r} + \sum_{r=1}^{q} \beta_{+r} JC_{s,t+r} + \mu_s + \tau_t + \mu_s \times t_t + \gamma X_{ist} + \varepsilon_{i,s,t} \tag{3}$$

which allows for q leads (r_{+1}, r_{+2},, r_{+q}) or anticipatory effects and m lags (r_0, r_{-1},, r_{-m}). In absence of anticipatory effects, the coefficients for future policy changes should be zero and this could be interpreted as evidence of absence of a treatment effect before the treatment. Results from estimation of Eqs. 1, 2 and 3 are reported in Section 6.

6 Impact of joint custody laws on mother's outcomes

6.1 Compositional changes in family structures

Before turning to the estimates of the effect of joint custody reform on mother's labor market outcomes, it is important to rule out possible selection effects into and out of marriage and fertility that might bias our estimates. Recently, Halla (2013) has shown that the introduction of joint custody in the US led to a long-run increase in marriage rates but had no effect on divorce rates, which he attributes to countervailing forces. Looking at children aged between 15 and 18 years, Teng (2008) shows that the adoption of unilateral divorce and joint custody laws do not change the probability for a child of living in an intact family or in a single parent family. Finally, Böheim et al. (2016) find that the introduction of a custody reform in Austria has a negative effect on marital birth rates but it vanishes when restricting the sample to more comparable districts.

To assess the impact of joint custody regulation on family structure in my sample, regressions of joint custody on marital status of parents of children below 18 years of age are run, as in Gruber (2004). Specifically, the sample is made of all children aged 0–18 living with a least one biological parent (stepparents have been excluded) and the dependent variables are whether the child's mother or father are divorced, separated, never married, married, or widowed. As Gruber (2004) has proved that making divorce easier increases the probability that children are living with a divorced parent, a dummy for adopting unilateral divorce law is included. As additional outcome, the number of siblings the child is living with is considered as a proxy for family size (and therefore of fertility). The regression performed is as follows:

$$f_{ast} = \alpha + \beta_1 JC_{ast} + \beta_2 UD_{ast} + \beta_3 Race_{ast} + \mu_s + \mu_s \times t_t + \tau_t + \lambda_a + \lambda_a \times \tau_t + \mu_s \times t_t + \varepsilon_{ast} \quad (4)$$

where a indexes ages, s states, t years; f_{ast} is the cell mean divorce rates (or separated, married, never married, and widow rates) of the mother/father the child is living with, or the number of siblings. JC is the dummy for joint custody laws as defined in Section 4; UD is a dummy for unilateral divorce law in state s in year t. Race are for % Blacks and % White, respectively in the cell; μ_s, τ_t and λ_a are full set of dummies for state, year, and age group; $\lambda_a \times \tau_t$ is a full set of age*year interactions to control for differential time patterns by age. As in Gruber (2004), this model controls for fixed factors that vary by age, state, or year and it is identified by the passage of joint custody and divorce laws over time. I consider the effect of the two legal reforms on the mother/father's marital status. Three different regressions are performed: (i) the first including only unilateral divorce law control to replicate results in Gruber (2004) and therefore to check the validity of our estimates;[17] (ii) the second including only joint custody law control; (iii) the last including both divorce

[17] Unilateral divorce, which allowed divorce with the consent of just one rather than both spouses, was rare before the late 1960s, but it was in place in most states by the mid-1970s. Of the 50 states, in 2006, five had yet to adopt any form of unilateral divorce.

and joint custody law controls. State-specific trends are controlled for and standard errors are corrected for clustering on state (Bertrand et al. 2004).

As a preliminary step, it is important to establish whether the changes in child custody law and the movement to unilateral divorce law are correlated with each other. If the two movements are jointly determined, disentangling the two effects would be difficult. As proposed by Chen (2013), only states that have completed both legal reforms are considered and the timing of the unilateral divorce law is then regressed on the timing of the child custody law. The correlation between the times of changes in the two laws turns out to be negligible (coeff. 0.0394, s.e. 0.467), suggesting that the adoption of unilateral divorce law does not predict when a state changed its custody law. The independence of the two legal trends allow to estimate the effect of child custody law controlling for the adoption of unilateral divorce in the same regression.

The results in Table 2 (columns 1 and 4) are in line with Gruber (2004). There is evidence of a positive effect of the adoption of unilateral divorce on the proportion of children living with a divorced mother/father (Panel A - columns 1 and 4), while the effect is negative (positive) for the odds of living with a separated mother (father) (Panel B - columns 1 and 4). On the contrary, the adoption of the joint custody law does not affect the probability that a child lives with his/her divorced mother/father (Panel A - columns 2 and 5). There is only some evidence that the adoption of the joint custody law increases the proportion of children living with a separated mother (Panel B - column 2), but not with a separated father (Panel B - column 5). Concerning the other marital status variables, both unilateral divorce and joint custody laws have hardly any effect on the odds of a child of living with a married, never married or widowed parent. Only the odds for a child of living with a married father decreases after the adoption of the unilateral divorce law even after controlling for the introduction of the joint custody law (column 6). No effects are found for the number of siblings in the family the child is living with. Overall, it does not seem to be any significant effect of the adoption of joint custody law on family composition, once unilateral divorce is controlled for (columns 3 and 6), with the exception of living with a separated mother (Panel B, column 3).[18]

Nunley and Seals (2011) and Altindag et al. (2015) propose a different method to address potential selection issues generated by custody reform based on the inverse probability procedure (IPW) suggested by Wooldridge (2002). This procedure gives less weight to observations who are more likely to be in the sample because of custody reform. In their case, the weights are obtained through a two-step procedure. In

[18] Altindag et al. (2015) use a different strategy to control for possible selection effects. They examine a sample of married couples who wed before 1977, i.e., before the widespread adoption of joint custody law across states. For those couples, the decision to marry should not be influenced by the passage of joint custody law because it was not in place. The estimates based on the restricted sample do not differ from those on the full sample confirming that selection is not an issue in the custody reform context. A similar approach has been adopted by Böheim et al. (2016) who restrict the sample only to marriages formed before the reform. Halla (2013) restricts the sample to married women and obtain similar results to the whole sample of women, supporting again the hypothesis that selection into marriage is not an issue. Unfortunately, it is impossible to adopt the same strategy in this paper because the year of marriage is not recorded in the IPUMS data.

Table 2 Compositional effects of joint custody and unilateral divorce laws

	Child is living with mother:			Child is living with father:		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Divorced						
Unilat. Divorce	0.008***		0.008***	0.002		0.002
Joint Custody		−0.001	−0.002		−0.000	−0.001
		(0.002)	(0.002)		(0.001)	(0.001)
Panel B: Separated						
Unilat. Divorce	−0.003*		−0.003*	0.001**		0.001**
	(0.002)		(0.002)	(0.000)		(0.000)
Joint Custody		0.004**	0.004**		0.000	−0.000
		(0.002)	(0.002)		(0.001)	(0.000)
Panel C: Never Married						
Unilat. Divorce	−0.001		−0.001	0.001		0.001
	(0.002)		(0.002)	(0.001)		(0.001)
Joint Custody		−0.001	−0.001		0.001	0.000
		(0.001)	(0.001)		(0.001)	(0.001)
Panel D: Married						
Unilat. Divorce	−0.003		−0.003	−0.005**		−0.005**
	(0.004)		(0.004)	(0.002)		(0.002)
Joint Custody		−0.003	−0.003		−0.000	0.001
		(0.003)	(0.003)		(0.001)	(0.001)
Panel E: Widowed						
Unilat. Divorce	−0.001		−0.001	0.001		0.001
	(0.001)		(0.001)	(0.001)		(0.001)
Joint Custody		0.001	0.001		0.000	−0.000
		(0.001)	(0.001)		(0.001)	(0.001)
Panel F: N. of Siblings						
Unilat. Divorce	−0.025		−0.024	−0.036*		−0.036
	(0.019)		(0.019)	(0.020)		(0.023)
Joint Custody		−0.003	−0.003		−0.007	−0.001
		(0.013)	(0.013)		(0.016)	(0.018)

Source: The 1960, 1970, 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS).

Notes: Dependent variables are the cell means at the state by year by age level. Control variables include a full set of state, year and child age dummies, child age by year interactions, % Black, % White. Standard errors are clustered by state of residence. Symbols: *** significant at 1%; ** significant at 5%, * significant at 10%

the first step, they estimate the probability that a child lives in an intact family using a probit regression on the full sample, controlling for individual and state-level characteristics. In the second step, they estimate the predicted probability of being married. The inverse of the predicted probabilities are used as weights in the regression of

interest. Using a similar approach, in this context, it is investigated if potential selection issues lead to substantial bias in the estimates for married/single women. First, the predicted probability of being married/single is computed on the full sample; then equation (1) is run for married/single mothers using the IPW. Results (not shown) are substantially equal to the estimates shown in Panel B and Panel C of Table 3 (see next Section), confirming that selection bias is not an issue in this context.

Finally, to rule out that our findings are driven by the static DID specification adopted, a specification is employed that controls for the dynamics of the response of divorce rate (and the other marital status rates) and fertility rate to changes in child custody assignment and unilateral divorce. The results (available on request) show that only for separated mothers does the timing of the introduction of joint custody law seems to matter and the effect is increasing over time; however, the other marital statuses and fertility do not show a dynamic response to legal changes.

6.2 Benchmark estimates

The effect of the joint custody reform on the set of mothers' economic outcomes is almost negligible. This is shown in Panel A of Table 3 where the results for mothers are reported after controlling for state fixed- and year-fixed effects and linear state specific time trends.[19] This finding is consistent with the Coase theorem prediction according to which if the costs of bargaining are very low, the switching to joint parenting should have no effect on parental outcomes, since the husband and the wife simply bargain "in the shadow of the law" (Becker 1993; Mnookin and Kornhauser 1979).

Nevertheless, the total effect might hide important heterogeneity. As mentioned by Lundberg and Pollak (1993), for married couples, far from the margin of divorce, changes in legal rules concerning custody arrangements might have little impact on behavior, while divorced or separated couples who have negotiated or are in the process of negotiating the terms of the dissolution of their marriages might be more sensitive to changes in legal rules. Therefore, the effects of the reform might differ according to the family structure.

A separate analysis by marital status might be flawed if the joint-custody reform alters the composition of the married/single population, an effect driven by the potential selection into marriage and divorce (Halla 2013). In this case, it would be difficult to identify the channels through which joint custody operates (Teng 2008). However, evidence provided in Section 6.1 has shown that selection effects into and out of marriage do not represent an issue in this context and therefore a separate analysis of married and single mothers does not confound the causal channels with a selection effect due to a changing composition of population with children.

The results in Panel B of Table 3 show that there is no effect of the legal reform for *married* mothers living in a state with joint custody law as opposed to married mothers living in a non-adoption state for any of the outcomes considered. Although the effect on married women passes most likely through divorce as mentioned before,

[19] In the interest of preserving space, only the coefficients on joint custody are reported.

Table 3 The effect of joint custody law on mother's labor market outcomes

	Poverty	Log earnings	Log other income	Other income (0/1)	On welfare	LFP
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All mothers						
joint custody	0.014*	−0.029	0.062	0.007	0.002	0.009
	(0.007)	(0.019)	(0.060)	(0.004)	(0.005)	(0.011)
N	1,441,684	842,615	372,553	1,441,684	1,194,473	1,441,684
Panel B: Married mothers						
joint custody	0.011	−0.016	0.003	0.006	0.001	0.013
	(0.007)	(0.019)	(0.078)	(0.004)	(0.002)	(0.011)
N	1,213,595	672,480	291,213	1,213,595	979,303	1,213,595
Panel C: Single mothers						
joint custody	0.031**	−0.081***	0.143***	0.009	0.009	−0.007
	(0.012)	(0.024)	(0.039)	(0.008)	(0.019)	(0.014)
N	228,089	170,135	81,340	228,089	215,170	228,089

Source. The 1960, 1970, 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS).

Notes: Control variables include age, race, ethnicity (a dummy variable for Hispanic origin), family structure indicator variables, number of children at home, educational attainment indicator variables (high school graduate, some college and college) for mothers; sex, age, and year of birth of the youngest child in the household. All the regressions include also a full set of state and year dummies, and child age by year interactions. Standard errors are clustered by state of residence. Symbols: *** significant at 1%; ** significant at 5%, * significant at 10%

both Halla (2013) and the analysis presented in Section 6.1 have proved that the joint custody reform does not affect divorce rates; this might explain the absence of any effects for married mothers. Some of these findings are consistent with results in Altindag et al. (2015), who show that custody reform is unrelated to married mothers' LFP, although it increases the time allocated to market work. Other studies show different results: Nunley and Seals (2011) show that joint-custody reform is associated to higher rates of LFP for married mothers, while Halla (2013) finds that married women are less likely to participate in the labor market in states that adopt joint-custody law. The difference among the two studies might be attributed to the choice of the time period and the dataset employed.

Results for *single* mothers reported in Panel C of Table 3 show that they experience a decrease in their living standards. There is a significant deterioration in earnings (−8.1%) and an increase in the probability of living in poverty (3.1 percentage points), while the amount of other income received increases (14.3%). Moreover,

the introduction of joint custody does affect neither the probability of being in the labor force of single mothers nor their probability of relying on welfare with respect to single mothers living in state without joint custody law. These results are consistent with Brinig and Buckley (1998) who suggest that single mothers might exchange property and child support payments for sole custody, thus making them poorer.

Given that the results show that joint custody has influenced only single mothers, the rest of the analysis will focus on this group of mothers (results for married mothers are available on request). It can be argued that single mothers, who can rely on higher "other income," may have reduced the number of hours worked and consequently their earnings but have not changed their labor market participation. However, the increase in the amount received on "other income" is not enough to compensate the decrease in earnings, increasing then the risk of living in poverty.

As mentioned in Section 5, the amount of exposure to joint custody law may matter and a static approach like the one used in Eq. 1 might not capture the dynamic adjustments of the outcomes to the reform. To explore this issue a variable measuring the exposure to the joint custody law and estimating the effect of joint custody adoption by duration of exposure is constructed. Specifically, the adoption states are divided into the following year intervals after the regime shift: 1–2 years, 3–6 years, 7–10 years, 11–14 years, and 15 and more, as in Eq. 2. In this way, it is possible to take advantage of the fact that individuals interviewed in the same year in different states may have different years of exposure to joint child custody law according to when the individual's state of residence enacted the law. The model is augmented including leads, as indicated in Eq. 3, to capture anticipatory effects in the spirit of Granger (1969). The main goal of the Granger test is to exclude that, conditional on state and year effects, future changes in the law may predict the current outcomes (Autor 2003; Angrist and Pischke 2009). Specifically, three leads are included, for years 1, 2, and 3 before the reform.

Results reported in Table 4 show no evidence of anticipatory effects. The only exception is represented by the variables "being on welfare" and "LFP", for which an anticipatory effect at year $t-2$ seems at work. However, there is no effect at year $t-1$, making it harder to believe that the effect at $t-2$ can be considered as an anticipatory response. When looking at the dynamic adjustments after the adoption of the law, it appears that years of exposure do not matter for poverty, earnings, and probability of receiving other income. For the variables "amount of other income" and "being on welfare," the first 2 years after the joint custody reform are the most important but the effect vanishes afterwards (with more persistence for the amount of other income). Years of exposure turn out to be important only for the variable LFP. When allowing the effect of the reform to vary by years of exposure, the LFP turns out to be significant and approximately constant over time (between about -4.0 and -5.0 percentage points) while in the static case it was negative and not significant. The negative sign is compatible with the hypothesis that single mothers might reduce their time spent parenting and use the additional time on the re-marriage market, thus increasing their probability of re-marriage and in turn decreasing the labor market participation. This explanation has been suggested by Halla (2013), who shows that women in all age groups (except women between 45 and 54 years of age) reduce their labor market participation as a consequence of the reform, although in his case the

Table 4 Dynamic DID estimates of the effect of joint custody law on single mothers' labor market outcomes

	Poverty	Log earnings	Log other income	Other income (0/1)	On welfare	LFP
	(1)	(2)	(3)	(4)	(5)	(6)
Joint custody in effect for:						
year −3	−0.012	0.054	0.015	0.003	−0.010	−0.020
	(0.019)	(0.046)	(0.102)	(0.010)	(0.012)	(0.012)
year −2	0.013	−0.081	0.084	−0.005	0.030**	−0.030**
	(0.021)	(0.067)	(0.114)	(0.012)	(0.015)	(0.014)
year −1	−0.015	0.097*	0.135	−0.002	−0.027	−0.007
	(0.019)	(0.053)	(0.084)	(0.012)	(0.021)	(0.012)
years 1−2	0.028	−0.044	0.324***	0.012	0.053***	−0.052***
	(0.017)	(0.051)	(0.098)	(0.011)	(0.015)	(0.010)
years 3−6	0.016	0.005	0.202**	0.010	0.018	−0.043***
	(0.016)	(0.050)	(0.090)	(0.013)	(0.015)	(0.012)
years 7−10	0.016	0.018	0.219**	0.016	0.012	−0.044***
	(0.019)	(0.060)	(0.103)	(0.014)	(0.015)	(0.014)
years 11−14	0.019	0.000	0.184	0.018	0.005	−0.049***
	(0.024)	(0.060)	(0.136)	(0.017)	(0.019)	(0.016)
years 15+	0.002	−0.022	0.120	0.014	0.004	−0.040**
	(0.024)	(0.060)	(0.148)	(0.020)	(0.019)	(0.018)
N	228,089	170,135	81,340	228,089	215,170	228,089

Source. The 1960, 1970, 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS).

Notes: The sample is made of single mothers. Control variables include age, race, ethnicity (a dummy variable for Hispanic origin), family structure indicator variables, number of children at home, educational attainment indicator variables (high school graduate, some college and college) for mothers; sex, age, and year of birth of the youngest child in the household. All the regressions include also a full set of state and year dummies, and child age by year interactions. Standard errors are clustered by state of residence. Symbols: *** significant at 1%; ** significant at 5%, * significant at 10%

effect is increasing (in absolute term) over time.[20] Overall, taking into account the dynamic effect of the policy intervention does not seem to provide additional information with respect to the static approach based on the assumption of an immediate response for most of the outcomes under analysis. For simplicity, in what follows the static approach will continue to be used.

[20]The different pattern of the results between this study and Halla (2013) can be easily reconciled. If one computes the difference between the labor force participation of all women and the labor force participation of married women reported in Table 6 of Halla (2013), this gives the effect for single women which is approximately constant over time.

In addition to investigating the effect of joint custody by duration of exposure, it is also possible to allow the effect of the reform to differ according to the age of the youngest child in the household. The focus is on the youngest child in the household because most of the bargaining between spouses on children custody takes place when children are young and sole custody might be considered as preferred. This analysis might be problematic if joint custody has a direct effect on fertility rates, thus generating a selection effect. However, Section 6.1 has proved that the introduction of joint custody did not have an impact on fertility behavior and therefore the selection problem can be neglected.

The heterogeneity of the effect according to the age of the youngest child in the household at the time of the reform is studied by estimating the following equation:

$$y_{ist} = \mu_s + \tau_t + \mu_s \times t_t + \gamma X_{ist} + \sum_j \beta_j \mathbf{I}\{JC_{st} = 1\} \times \mathbf{I}\left\{a_j \leq (YB_{ist} - YJC_s) \leq b_j\right\} + \varepsilon_{ist} \quad (5)$$

where YB is the year of birth of the child i whose single mother is living in state s at time t, YJC_s is the year of adoption of joint custody law in state s; their difference

Table 5 DID estimates by age of the child at joint custody reform

	Poverty	Log earnings	Log other income	Other income (0/1)	On welfare	LFP
	(1)	(2)	(3)	(4)	(5)	(6)
Joint custody in effect for:						
Born after the reform	0.003	−0.002	0.109	0.005	0.007	0.006
	(0.015)	(0.037)	(0.081)	(0.005)	(0.006)	(0.016)
Age 0−3 at the time of reform	0.013	−0.022	0.081	0.009*	0.004	0.009
	(0.010)	(0.025)	(0.058)	(0.005)	(0.005)	(0.013)
Age 4−7 at the time of reform	0.015*	−0.026	0.071	0.009**	0.004	0.008
	(0.008)	(0.021)	(0.069)	(0.004)	(0.005)	(0.012)
Age 8−11 at the time of reform	0.015**	−0.038*	0.062	0.004	0.002	0.007
	(0.007)	(0.019)	(0.060)	(0.005)	(0.004)	(0.011)
Age 12+ at the time of reform	0.017**	−0.031	0.043	0.007*	−0.000	0.009
	(0.007)	(0.019)	(0.078)	(0.004)	(0.004)	(0.009)
N	228,089	170,135	81,340	228,089	215,170	228,089

Source. The 1960, 1970, 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS).

Notes: The sample is made of single mothers. Control variables include age, race, ethnicity (a dummy variable for Hispanic origin), family structure indicator variables, number of children at home, educational attainment indicator variables (high school graduate, some college and college) for mothers; sex, age, and year of birth of the youngest child in the household. All the regressions include also a full set of state and year dummies, and child age by year interactions. Standard errors are clustered by state of residence. Symbols: *** significant at 1%; ** significant at 5%, * significant at 10%

represents the child's age at the time of the adoption of joint custody law; **I** is an indicator function. The effect of interest is β_j which represents the effect of the move to joint custody for single mothers whose child's age at the time of the reform was in the range $[a_j, b_j]$ with respect to single mothers living in states that did not adopt joint custody (JC=0). β_j is identified using three different sources: the difference in the timing of adoption on joint custody law, the fact that not all the states adopted it, and the difference in child age at time of the reform.

Results are reported in Table 5. All the coefficients for children born after the introduction of the joint custody law are not statistically different from zero. Interestingly, the odds of living in poverty for single mothers increases as the child ages by almost 2 percentage points when the child is 12 years or older. Moreover, a larger fraction of single mothers receive "other income" (0.9 percentage points) when the youngest child is below 7 years of age, although there is no differential effect of the reform by the age of the youngest child on the amount of "other income received."

6.3 The impact of unilateral divorce, property, and child support laws

During the period under analysis, in addition to the unilateral divorce law, many states changed the rules governing property division and removed fault as a consideration in property division (Ellman and Lohr 1998).[21] From a financial point of view, the introduction of new laws regarding property division in a state can be as important as joint custody for a household's wealth after divorce. Moreover, several child support laws to enforce parental payment were passed in the same years. The timing of the introduction of these laws is reported in Appendix Table 8.

In order to identify the net effect of joint custody on single mother's outcomes, changes in family laws concerning unilateral divorce, property division, and child support enforcement are controlled for, as presented in Table 6. The importance of controlling for contemporaneous legal changes has been highlighted by González-Val and Marcén (2012) who have explored the response of the divorce rate to the introduction of the unilateral divorce law once the changes in custody law and Child Support Enforcement program have been controlled for.

For the sake of comparison, column 1 of Table 6 reports the coefficients of Table 3 Panel C; column 2 introduces a dummy variable equal to one if the state s has already

[21] States can be divided into three groups according to the property division regime: common law property, community property, and equitable division. In a common law regime, marital property is divided at divorce according to who has legal ownership of the property; in a community property regime, most property acquired during the marriage (except for gifts or inheritances) is owned jointly by both spouses and division of community property under divorce may take place by item, by splitting all items or by value; finally, in a equitable division regime, judges have discretion in allocating marital property according to what the judge deems as fair. In equitable division states, judges can act more discretionally in property allocation than in common law or community property states (Gray 1998). Moreover, community property states are considered as transferring more assets to the wife after divorce than in common-law jurisdictions since household assets tend to be held in most cases in the husband's name (Gray 1998).

Table 6 Some robustness checks: the effects of property and child support laws

	Poverty					Log earnings				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Joint custody	0.031**	0.030**	0.035**	0.036**	0.054**	−0.081***	−0.079***	−0.083***	−0.083***	−0.110**
	(0.012)	(0.013)	(0.014)	(0.015)	(0.026)	(0.024)	(0.024)	(0.029)	(0.029)	(0.053)
Unilat. divorce		0.017	0.018	0.018	0.199***		−0.053	−0.054	−0.052	−0.554***
		(0.024)	(0.023)	(0.023)	(0.061)		(0.050)	(0.050)	(0.049)	(0.124)
No fault			−0.028	−0.028	−0.073***			0.020	0.020	0.168***
			(0.025)	(0.025)	(0.019)			(0.052)	(0.053)	(0.042)
Equitable div.			−0.091***	−0.086***	0.326***			0.700***	0.672***	−0.341***
			(0.015)	(0.020)	(0.058)			(0.054)	(0.060)	(0.126)
Legislative index				−0.004	−0.001				0.015	0.002
				(0.007)	(0.007)				(0.013)	(0.015)
Child support					0.100					−0.243
					(0.078)					(0.174)
N	228,089	228,089	228,089	228,089	191,120	170,135	170,135	170,135	170,135	144,659
	Log other income					Other income (0/1)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Joint custody	0.143***	0.143***	0.092**	0.092**	0.040	0.009	0.008	0.003	0.003	−0.002
	(0.039)	(0.038)	(0.045)	(0.045)	(0.066)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)
Unilat. divorce		−0.050	−0.055	−0.056	0.084		0.019	0.018	0.018	0.061
		(0.158)	(0.141)	(0.141)	(0.156)		(0.020)	(0.018)	(0.018)	(0.036)
No fault			0.217***	0.217***	−0.032			0.027**	0.027**	−0.013
			(0.080)	(0.079)	(0.076)			(0.012)	(0.012)	(0.011)

Table 6 (continued)

Equitable div.			−1.338***	−1.335***	0.950***			−0.101***	−0.099***	−0.549***
			(0.049)	(0.054)	(0.322)			(0.006)	(0.009)	(0.031)
Legislative index				−0.003	0.009				−0.002	0.002
				(0.021)	(0.026)				(0.004)	(0.004)
Child support					−0.358*					0.066**
					(0.190)					(0.030)
N	81,340	81,340	81,340	81,340	52,620	228,089	228,089	228,089	228,089	191,120
	On welfare					LFP				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Joint custody	0.009	0.010	0.019	0.019	0.046**	−0.007	−0.007	−0.011	−0.011	−0.013
	(0.019)	(0.018)	(0.014)	(0.015)	(0.018)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)
Unilat. divorce		0.020	0.022	0.021	0.015		−0.007	−0.008	−0.007	0.002
		(0.033)	(0.033)	(0.031)	(0.046)		(0.015)	(0.015)	(0.015)	(0.055)
No fault			−0.050**	−0.050**	−0.074***			0.021	0.021	0.027**
			(0.024)	(0.024)	(0.016)			(0.014)	(0.013)	(0.010)
Equitable div.			0.341***	0.354***	0.237***			−0.193***	−0.202***	−0.424***
			(0.017)	(0.021)	(0.049)			(0.012)	(0.016)	(0.041)
Legislative index				−0.007	−0.005				0.007	0.009*
				(0.006)	(0.007)				(0.005)	(0.005)
Child support					0.107*					−0.001
					(0.054)					(0.042)
N	215,170	215,170	215,170	215,170	191,120	228,089	228,089	228,089	228,089	191,120

Source. The 1960, 1970, 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS).

Notes: The sample is made of single mothers. Control variables include age, race, ethnicity (a dummy variable for Hispanic origin), family structure indicator variables, number of children at home, educational attainment indicator variables (high school graduate, some college, and college) for mothers; sex, age, and year of birth of the youngest child in the household. All the regressions include also a full set of state and year dummies, and child age by year interactions. Standard errors are clustered by state of residence. Symbols: *** significant at 1%; ** significant at 5%, * significant at 10%

enacted *unilateral divorce* at t,[22] in column 3 *No-fault Property* and *Equitable Division* dummy variables are added, indicating the presence of specific property division laws.[23] As shown in columns 1–3 of Table 6, adding these controls do not alter the coefficients on joint custody for any of the outcomes considered. Interestingly, living in a equitable property states, where judges have more discretion in allocating marital property, has a direct positive effect on single mothers' earnings, it increases the probability of relying on welfare and reduces the probability of living in poverty, the probability and the amount of income from other sources and the probability of labor market participation.

In the same decades, many states have also been strengthening child support enforcement over time. Since there is a close link between the choice of custody arrangement and child support enforcement, column 4 of Table 6 includes a measure of the child support legislation. Specifically, a single legislative index has been created by summarizing the eight measures of child support legislation mentioned in Section 4, with values ranging from zero for states with no law to eight for states with all eight laws enforced.[24]As before, the results on joint custody are very robust to the inclusion of this measure of child support enforcement.

Similar to González-Val and Marcén (2012), a measure of effectiveness in the collection of child support is built using the Current Population Survey-Child Support Supplement (CPS-CSS) from 1992 to 2000, a nationally representative sample administered biannually by the U.S. Census Bureau. The effectiveness measure is defined as child support collection amount in a state, divided by child support amount owed in the same state. Unfortunately, these data are available only from 1992 and by extrapolating the data back to 1990, the analysis can be run only over the last two decades, 1990 and 2000. For this reason, the results are not fully comparable to the previous ones and must be interpreted with caution. Once the effectiveness in the collection rate is controlled for, the results show that the introduction of the law on joint custody makes matters worse for single mothers since it increases the probability of being below the poverty line and of relying on welfare, it decreases earnings and the probability of being in the labor force (see column 5 of Table 6). Since in most cases, the coefficient on child support effectiveness rate is not significant, the difference in the joint custody coefficient between column 4 and column 5 should be attributed to the different sample size.

[22]The divorce coding proposed by Gruber (2004) has been used, but the results are virtually unchanged if the one proposed by Friedberg (1998) is instead used. The main difference between the two is that Gruber considers as "non-adopting" the states that have unilateral divorce and separation requirements (New Jersey, Ohio, Pennsylvania, South Carolina, and the District of Columbia). Results available upon request.

[23]Three states (Massachusetts, Missouri and Montana) shifted from common law property division to equitable division over the period. Equitable division is included as control, common law is the excluded category and community property is omitted because of collinearity with state fixed effects.

[24]The results are unchanged if all the eight measures are included instead of the single index or if the single index is computed through a factor analysis and taking the first principal component. Results available upon request.

6.4 A threat to the identification strategy

A drawback of the identification strategy is that substantial interstate migration in response to changes in joint custody laws might bias the estimates, as mentioned in Section 5. In other words, individuals might endogenously move in view of states' changes in family law, introducing estimation bias. It can be argued that endogenous migration is not a problem in this context for several reasons.

First, migration of parents during their children's teenage years is not very widespread. In the sample of single mothers, at the survey year only 14.3% of children live in a state different from the state of birth. Second, if parents fully agree on shared custody at the time of divorce they do not need to migrate to another state with a more favorable law on joint custody to enforce the agreement. On the contrary, if parents do not agree on the form of custody arrangement, the primary custodial parent cannot move with the child to another state without the permission of the other parent because child abduction is considered a criminal offence. Finally, when the custody arrangement is settled, the custodial parent, usually the mother, who wishes to relocate might be prohibited by the court from doing so unless she is willing to give up the custody of her child(ren) (Folberg 1991, p.84).

A simple way to assess the severity of bias induced by potential selective migration is to use the change in law in the child's state of birth (SOB) instead of the state of residence (SOR) and evaluate the sensitivity of the results. This check captures the case of a woman who changes residence after divorce to match a more favorable custody law.[25] The estimates using either "change of joint custody law in the SOR" or "change of joint custody law in the SOB" turn out to be similar. As additional robustness check, Eq. 1 is estimated for single mothers excluding those whose child's place of birth is different from the current state of residence to evaluate the importance of potential endogenous movers. The results are again similar to what shown in Table 3 Panel C, confirming that endogenous migration is not affecting the estimates (both set of results are available upon request).

7 Interpretation of the results

The analysis above has shown that the adoption of the joint custody law affects single mothers in terms of reduced earnings, increased probability of living in poverty and increased amount of "other income" received. In this section, a potential explanation for these findings is provided by exploring the link between joint child custody and child support. For this purpose, the analysis uses the Current Population Survey-Child Support Supplement (CPS-CSS) from the years 1994 to 2002. Information

[25] It is assumed that children were born before the divorce took place, which seems a reasonable assumption.

gathered from respondents includes data on custody arrangements, child support payments, and visitation, and detailed individual and family characteristics. The sample of women selected are between 14 and 60 years of age, with their own children below 18 years in the household, who are supposed to receive child support payments. The dependent variable is the collection rate computed as the ratio between the amount of child support actually received last year over the amount of money supposed to be received as child support.[26] Importantly, in the CPS-CSS, the respondent is asked if a court or judge ever gives the joint legal or physical custody of the child(ren). It is here investigated whether being awarded joint (legal or physical) custody has an effect on the child support payment and in particular on the collection rate.

Table 7 first considers the effect of joint legal custody on collection rate (Panel A), then the effect of joint physical custody (Panel B) and finally both types of custody arrangements simultaneously (Panel C). It also controls for race, education, number of children below 18 years old, marital status, personal income, and being below the poverty line (the last two variables are potentially endogenous but the results are robust to their exclusion). The results show that mothers awarded joint legal custody increase the collection rate by between 7 and 10 percentage points. This result is robust to the model specification (columns 1–5) and to the choice of the sample (for example considering only divorced and separated mothers as in column 6). Moreover, mothers with high school education or more increase significantly the collection rate if awarded joint legal custody (columns 7 and 8), while the effect is not significant for mothers with less that high school (column 9). When we look at Panel B, the effect of being awarded joint physical custody has a positive and significant effect on the collection rate (between 6.0 and 7.0 percentage points). As before, there is some heterogeneity according to the level of education. Interestingly if joint legal and physical custody are controlled for simultaneously (Panel C), only the shared legal custody of the child(ren) matters. This result might be due to the high collinearity of these two variables.[27]

[26] All the income variables are expressed in 1999 dollars.

[27] Some unobserved characteristics may affect both joint custody and the financial involvement of non-resident parents. Parents who choose joint custody are different from those who do not in terms of disagreement on post-divorce child-rearing decisions or conflictual relationship between partners. Here a 2SLS strategy is used to adjust for such unobserved characteristics in order to have unbiased estimates of joint custody (Seltzer 1991, 1998). Following Seltzer (1998), the average percentages of joint custody in each state are used as an instrument for actual joint custody. The effect of the percentage of state joint custody (either legal or physical) on observed joint custody is of the expected sign and strongly significant. This represents our first stage regression. As one might expect, parents who live in states with a higher percentage of joint custody are more likely to share child custody. Results from the 2SLS regressions are very similar in size to those reported in Table 7 even if mainly not significantly different from zero due to the high standard errors (results available on request). The Hausman test of equality between OLS and IV does not reject the null hypothesis of no differences between OLS and 2SLS estimates.

Table 7 Effect of joint legal and physical custody on the collection rate

	All (1)	All (2)	All (3)	All (4)	All (5)	only divor/separ (6)	high school (7)	school (8)	high school (9)
Panel A									
Joint legal	0.105***	0.097***	0.084***	0.077***	0.074***	0.079***	0.070***	0.092 ***	0.048
	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.011)	(0.019)	(0.052)
Panel B									
Joint physical	0.078***	0.072***	0.064***	0.058***	0.058***	0.057***	0.056***	0.058**	0.019
	(0.009)	(0.009)	(0.008)	(0.009)	(0.008)	(0.011)	(0.011)	(0.022)	(0.068)
Panel C									
Joint legal	0.099***	0.091***	0.078***	0.071***	0.068***	0.074***	0.063***	0.092***	0.049
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.014)	(0.024)	(0.057)
Joint physical	0.014	0.015	0.016	0.014	0.016	0.012	0.018	−0.000	−0.004
	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.014)	(0.014)	(0.028)	(0.075)
Race	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Children	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes	–	–	–
Marital status	No	No	No	Yes	Yes	–	Yes	Yes	Yes
Income-related variables	No	No	No	No	Yes	No	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	8102	8102	8102	8102	8102	4380	2454	1502	424

Notes: Data from the Current Population Survey-Child Support Supplement (CPS-CSS) from 1994 to 2002. The sample is made by women who were supposed to receive child support payments in the past year. The dependent variable is the collection rate ratio between the amount of child support actually received last year over the amount of money supposed to be received in child support last year. Control variables include race (black, other and white as omitted category), children (one, two, or more than 2 as omitted category), education (less than high school diploma as omitted category, high school diploma and more than high school diploma), marital status (separated, divorced, never married, and married as omitted category), income-related variables (being below the poverty line and individual income), plus a full set of year and state dummies. From column 6 to 9, only divorced/separated mothers. Standard errors are clustered by state of residence. Symbols: *** significant at 1%; ** significant at 5%, * significant at 10%

The results are consistent with Pearson and Thoennes (1988) who find that joint legal custody increases compliance with child support orders and with Allen et al. (2011) who show that joint-custody enactment raises the probability of receiving child support for single mothers, with an effect mainly driven by divorced mothers, while separated and never-married mothers are unaffected by the joint custody reform. Seltzer and Maralani (2001) also look at the effect of the introduction of joint custody on child support payments received by single mothers and come to different conclusions: there is no evidence of an effect of joint custody law on the probability of receiving child support. The discrepancy between the results in this paper and those of Seltzer and Maralani (2001) might be due to the different time period of analysis, longer in this case, which could capture the increased ability of states to collect child support over time.

The results confirm that in the case of joint (legal or physical) custody mothers can rely on stronger child support payment. This is in line with the arguments made by Weiss and Willis (1985) who suggest that the opportunity of exchanging custody for payment support or other transfers could increase the contribution from the non-custodial parent. However, this belief might have consequences for mothers who, relying on the larger financial involvement of fathers, might be less interested in looking for high-paid jobs or advancing personal careers or might simply reduce the number of hours worked. This behavior might lead to a decrease in mothers' living standards in case the payments from fathers do not compensate the mothers' drop in earnings after divorce. Moreover, as shown in Table 7, joint custody increases the collection rate mainly for highly educated mothers; according to the theory of assortative mating, those mothers are more likely to have a highly educated partner, potentially with higher incomes. If only single mothers with high income nonresident fathers benefit from joint custody, as shown by Seltzer and Maralani (2001), this might explain the negative effect of joint custody on mothers' economic outcomes.[28]

8 Summary and Conclusions

The main goal of the adoption of the joint custody law was to make easier the access of the non-custodial parent (usually the father) to their child(ren), to increase the

[28] Potentially, the results might also be explained by the fact that joint legal custody places more restrictions on the residential parent than sole custody does. Courts may restrict the geographic area in which a parent lives as part of the custody order, or they may deny a subsequent request for permission to move if it is viewed as an attempt to hinder the other parent's visitation. To shed some light on this channel, this study investigates if divorced/separated mothers are less able to move under joint custody than under sole custody. Using the CPS-CSS data and the same sample of Table 7, it is found that mothers awarded joint custody do have the same probability of moving than mothers with sole custody. Moreover, there is no evidence that joint custody places restrictions on career investment since divorced/separated mothers with joint custody have a higher probability of being employed, earn more and rely less on welfare.

financial support but also to share child-rearing responsibilities between parents after divorce. This paper investigates the effect of the introduction of the joint custody law on mothers' labor market outcomes. It uses the variation occurring from the different timing of custody law reforms across the US states as a source of identification. The results show that the consequences of the adoption of the joint custody law are very different between single and married mothers. Married mothers are not affected by the reform, while single mothers experience a decrease in earnings and an increase in other income received, which however do not compensate each other. As a consequence, single mothers are more likely to live in poverty. The results are robust to controlling for unilateral divorce, specific property division laws and child support legislation. There is no evidence that the exposure to joint custody law affects mothers' outcomes. When the heterogeneity of the effect depending on the age of the youngest child in the household is investigated, the odds of living in poverty are found to increase as the child ages, when child's economic needs tend to increase. The possibility that the adoption of joint custody law has any compositional effects has also been excluded: on aggregate, there is no significant change in the proportion of children living with a single parent or in an intact family. This helps to rule out that the results are driven by a change in marital choices due to the adoption of joint custody law. Using the Current Population Survey-Child Support Supplement 1994–2002 there is a possible explanation for these results: the higher child support payments a mother receives from the non-custodial father in case of joint custody might discourage her from looking for high paid jobs or investing in her careers. However, the increase in compliance with child support payment is not enough to compensate for the reduction in mothers' earnings if it mainly comes from higher income fathers. Finally, the result of an increase in the amount of other income received by single mothers combined with an increase in the collection rate following the joint custody reform potentially suggest the existence of fewer conflicts among separated partners. This result is compatible with findings by Halla (2013) and Wolfers (2006) who show that suicide rates and domestic violence have decreased after the introduction of joint custody. However, the findings also show a decrease in earnings and an increase in the probability of living in poverty. To ascertain if these changes had an overall positive or a negative effect on single mothers' well-being is however left for future research.

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Compliance with Ethical Standards

Conflict of Interests The author declares that she has no conflict of interest.

Appendix

Table 8 Year of introduction of joint custody, divorce laws, and child support enforcement variables and types of marital property laws

State	Joint custody	Unilateral divorce	No-fault prop. division	Property settl. law	Child support enforcement variables							
					Genetic tests	Paternity test	Numeric. guidelines	Presump. guidelines	Wage withhol.	Imm. withhol.	Univ. withhol.	State interc.
Alabama	1997	1971	Fault	Common law	1984	1984	1987	1989	1984	1989	1993	1985
Alaska	1982	1935	1974	Equi. dist.	1989	1976	1987	1987	1981	1988	1994	
Arizona	1989	1973	1973	Comm. Prop.	1984	1971	1989	1989	1977	1987	1987	1985
Arkansas	2003	0	1979	Equi. dist.	1989	1985	1989	1989	1983	1989	1991	1983
California	1980	1970	1970	Comm. Prop.	1986	1986	1990	1990	1980	1989	1989	1984
Colorado	1987(1983[a])	1972	1971	Equi. dist.	1991	1985	1986	1986	1981	1989	1993	
Connecticut	1980	1973	Fault	Equi. dist.	1989	1985	1989	1989	1955	1983	1983	1991
Delaware	1981	1968	1974	Equi. dist.	1984	1984	1983	1988	1974	1990	1993	1992
D.C.	1996	0	Fault	Equi. dist.	1984	1984	1990	1990	1987	1990	1995	1987
Florida	1982	1971	1986	Common law	1986	1986	1987	1989	1978	1986	1986	
Georgia	1990	1973	Fault	Common law	1991	1980	1989	1989	1981	1989	1993	1980
Hawaii	1980	1972	1960	Equi. dist.	1989	1983	1986	1986	1984	1988	1988	1982
Idaho	1982	1971	1990	Comm. Prop.	1982	1985	1989	1989	1986	1990	1993	1981
Illinois	1986	0	1977	Equi. dist.	1984	1984	1984	1990	1984	1988	1988	1989
Indiana	1983	1973	1973	Equi. dist.	1987	1986	1989	1989	1982	1985	1997	1981
Iowa	1977	1970	1972	Equi. dist.	1989	1990	1984	1989	1984	1990	1993	1980
Kansas	1980	1969	1990	Equi. dist.	1994	1985	1986	1992	1985	1990	1992	1981
Kentucky	1980	1972	Fault	Equi. dist.	1984	1986	1990	1990	1984	1988	1988	1986
Louisiana	1981	0	Fault	Comm. Prop.	1985	1980	1989	1989	1982	1989	1993	1992

Table 8 (continued)

State	Joint custody	Unilateral divorce	No-fault prop. division	Property settl. law	Child support enforcement variables							
					Genetic tests	Paternity test	Numeric. guidelines	Presump. guidelines	Wage withhol.	Imm. withhol.	Univ. withhol.	State interc.
Maine	1981	1973	1985	Equi. dist.	1991	1985	1989	1989	1985	1990	1991	1985
Maryland	1986	0	Fault	Common law	1984	1984	1989	1989	1976	1991	1993	1957
Massachusetts	1982	1975	Fault	Common law[c]	1986	1986	1989	1989	1986	1986	1998	1986
Michigan	1981	1972	Fault	Equi. dist.	1982	1986	1985	1990	1982	1990	1990	1985
Minnesota	1982	1974	1974	Equi. dist.	1980	1980	1983	1983	1978	1990	1993	1980
Mississippi	1983	0	Fault	Common law	1987	1981	1989	1989	1985	1989	1993	1985
Missouri	1983	0	Fault	Common law[c]	1987	1987	1989	1989	1973	1990	1993	1984
Montana	1981	1973	1975	Common law[d]	1989	1985	1989	1989	1985	1989	1991	1985
Nebraska	0(1983[a])	1972	1972	Equi. dist.	1984	1986	1985	1985	1985	1991	1994	1984
Nevada	1981	1967	1973	Comm. Prop.	1989	1983	1987	1989	1985	1989	1993	
New Hampshire	1981	1971	Fault	Equi. dist.	1988	1985	1988	1988	1985	1993	1993	
New Jersey	1991	0	1980	Equi. dist.	1983	1983	1986	1988	1981	1990	1990	1985
New Mexico	1982	1933	1976	Comm. Prop.	1986	1986	1988	1989	1985	1990	1993	1985
New York	0	0	Fault	Common law	1976	1985	1989	1989	1977	1990	1994	1985
North Carolina	1988(1957[a])	0	Fault	Common law	1979	1981	1985	1989	1975	1989	1993	1979
North Dakota	0	1971	Fault	Equi. dist.	1989	1975	1983	1989	1979	1989	1989	1983
Ohio	1981	0	Fault	Common law	1986	1982	1990	1990	1986	1993	1993	1985
Oklahoma	1983	1953	1975	Equi. dist.	1985	1985	1988	1989	1978	1989	1994	1985
Oregon	1987(1977[a])	1971	1971	Equi. dist.	1981	1983	1989	1989	1985	1993	1993	1985
Pennsylvania	1981	0	Fault	Common law	1989	1985	1985	1989	1985	1989	1989	1985

Table 8 (continued)

State	Joint custody	Unilateral divorce	No-fault prop. division	Property settl. law	Child support enforcement variables							
					Genetic tests	Paternity test	Numeric. guidelines	Presump. guidelines	Wage withhol.	Imm. withhol.	Univ. withhol.	State interc.
Rhode Island	0	1975	Fault	Common law	1984	1988	1987	1987	1980	1990	1994	1982
South Carolina	1996	0	Fault	Common law	1984	1984	1989	1989	1985	1989	1994	1984
South Dakota	1989	1985	Fault	Equi. dist.	1989	1986	1989	1989	1986	1990	1990	
Tennessee	1986	0	Fault	Common law	1983	1984	1989	1989	1985	1990	1994	
Texas	1987	1970	Fault	Comm. Prop.	1989	1983	1989	1989	1985	1985		
Utah	1988	1987	1983	Equi. dist.	1992	1990	1989	1989	1977	1990	1993	1985
Vermont	0(1987[a])	0	Fault	Equi. dist.	1983	1983	1985	1985	1983	1989	1989	1989
Virginia	1987	0	Fault	Common law	1985	1988	1988	1988	1982	1988	1995	1950
Washington	1987[b]	1973	1973	Comm. Prop.	1994	1976	1988	1988	1984	1989	1994	
West Virginia	0	0	Fault	Common law	1989	1986	1986	1989	1986	1990	1993	1995
Wisconsin	1988(1977[a])	1978	1977	Equi. dist.	1987	1983	1983	1987	1977	1985	1989	1987
Wyoming	0	1977	Fault	Equi. dist.	1989	1978	1989	1989	1986	1989	1994	

Notes: 0 in columns 1 and 2 denotes that a state legislation was never adopted or acknowledged. Eq. dist stands for equitable distribution, Comm. Prop. stands for community propert, Comm. Law stands for Common Law. Sources: Unilateral Divorce coding: Gruber (2004); Joint Custody: Wah Leo (2008); Property division types: Gray (1998); Year of no-fault divorce: Ellman and Lohr (1998). Genetic tests, paternity, numerical guidelines, presumptive guidelines, wage withholding, immediate withholding, universal withholding, state intercept are from Huang et al. (2003)

[a]Consent of both parents required in joint custody cases

[b]Agreement required, but will not have bearing if found to be unreasonable

[c]Changed to equitable division in 1976

[d]Changed to equitable division in 1974

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