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# The Effect of Joint-Child-Custody Legislation on the Child-Support Receipt of Single Mothers

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**Abstract** Due to the preponderance of single mothers on public assistance, delinquent child support has been a contentious political issue in the U.S. We examine whether joint-child-custody reform affects the child-support receipt of single mothers. We use variation in the timing of joint-custody reforms across states to identify the effect of joint custody on the child-support receipt of single mothers. Joint-custody enactment raises the probability of receiving child support for all single mothers by 8%. The effect on all single mothers is driven by the effect on divorced mothers, as separated and never-married mothers are unaffected by joint-custody reform. We conclude joint-custody reform confers the most benefit on divorced mothers and their children, particularly those who do not receive public assistance.

**Keywords** Child custody · Child support · Divorce · Joint custody

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## Introduction

Lack of financial support from noncustodial fathers places an enormous burden on single mothers in the United States. Since the 1970s, state and federal governments have passed a bevy of child-support-enforcement (CSE) reforms in order to minimize single mothers' dependence on public assistance (Beller and Graham 1993). During this time, many states also passed legislation directing courts to consider shared child custody as the preferred custodial arrangement (Brinig and Buckley 1998). Although public policy has been overwhelmingly focused on obtaining and subsequently enforcing child-support orders for single mothers, joint child custody may provide additional incentive for fathers to pay child support because they are able to both spend more time with their children and monitor how child-support payments are spent (Brinig and Buckley 1998). In fact, for parents with child-support awards, 84.6% of those with joint-custody arrangements receive some child support.[1] By contrast, the child-support receipt rate is 61.5% for those with a sole-custody arrangement.[2]

Several studies suggest joint-child-custody arrangements increase the receipt and level of child-support income (Del Boca and Ribero 1998; Huang et al. 2003; Pearson and Thoennes 1988), while others suggest joint-child-custody arrangements have no effect on the receipt and level of child-support income (Arditti and Keith 1993; Gunnoe and Braver 2001; Seltzer 1991, 1998; Seltzer and

[1] Statistics referenced from the U.S. Office of Child Support Enforcement. Data used to generate the report come from the April 2006 Current Population Survey Child Support Supplement http://www.census.gov/prod/2007pubs/p60-234.pdf.

[2] Ibid.

Maralani 2001). The inconsistencies present in the existing literature are likely the result of differences in socioeconomic status (SES), a reflection of the parents' education, financial resources, and subsequent level of access to the legal system (Seltzer 1991), and unobserved characteristics of the family that determine continued (or lack of) support from fathers following marital dissolution. Because of these issues, it is difficult to estimate the causal effect of joint child custody on the child-support receipt of single mothers.

A way to circumvent these issues is to use variation in the timing of joint-child-custody reforms across states as a natural experiment to identify the average treatment effect (ATE) of the policy change, which provides a causal interpretation (Angrist and Pischke 2009). The timing of joint-child-custody reforms is a proxy for the prevalence of joint-custody arrangements. While the joint-custody laws used in the analysis do not distinguish between joint-legal and joint-physical custody, the prevalence of joint-legal and joint-physical arrangements coincided with joint-custody reforms (Kelly 1994). As such, the estimated impact of joint-custody reform on the child-support receipt of single mothers would capture the large shift in preferences for both joint-legal and joint-physical cases across states. Natural experiments require treatment and comparison groups. The treatment group in this study is single mothers who live in states that adopt joint-custody laws between 1978 and 1993. The comparison group is single mothers who live in states that had yet to adopt joint-custody laws by the last survey date.

We also provide two other important extensions to the existing literature. First, we estimate the effects of joint-custody reform on the probability of receiving child support separately for never-married, divorced, and separated mothers. Second, we estimate the effects of joint-custody reform on the probability of receiving child support for sub-samples of single mothers who receive public assistance and for those who do not receive public assistance. Examining these subsamples of single mothers likely provides a clearer picture of how joint custody affects child-support receipt, as these mothers have different rates of receiving both joint-child-custody arrangements and child-support income.

Data on child-support receipt are from the March Current Population Survey (CPS) from 1978 to 1993. We use a logit specification to estimate the effects of joint-custody reform on the probability of receiving child support for all single mothers, for sub-samples of never-married, divorced, and separated mothers, and for sub-samples of single mothers who receive public assistance and for those who do not receive public assistance. We find a statistically significant, positive effect of joint-custody reform on the probability of receiving child support for all single mothers, which translates into a 7% increase. However, the effect on all single mothers may be driven by the effect on divorced mothers, whose probability of receiving child support increases by 8% when examined separately. We find no statistical evidence linking joint-custody reform to the probability of receiving child-support income for never-married and separated mothers.

The effects of joint-custody reform differ for the partitioned samples based on receipt of public assistance. For single mothers who do not receive public assistance, joint-custody reform raises the probability of receiving child support by 8%. However, divorced mothers benefit the most, as the probability of receiving child support increases by 6% following joint-custody reform. Never-married and separated mothers who do not receive public assistance are unaffected by the joint-custody reform. For the sample of single mothers who receive public assistance, joint-custody reform has no effect on the probability of receiving child support. This finding is robust for sub-samples of never-married, divorced, and separated mothers. We conclude joint-custody reform confers the most benefit on divorced mothers, particularly those who do not receive public assistance.

The remainder of the paper is organized as follows. Section 2 provides background information on child-custody reform, theoretical predictions of how we expect joint-custody reform to affect the child-support receipt of single mothers, and previous research on the effect of joint custody (including actual custodial allocations and state-level reforms) on the child-support receipt of single mothers. Section 3 discusses the data and econometric methodology. Section 4 presents the results. Section 5 offers concluding remarks.

## Background

### Child-Custody Legislation

From the 1920s until the 1960s, states had explicit provisions stating their preference for mothers in child-custody cases (Kelly 1994). By the mid-1970s, the majority of states removed the explicit preference for mothers when allocating custody rights (Cancian and Meyer 1998). With the passage of the federal Uniform Marriage and Divorce Act in 1970, gender-neutral, child-custody laws became the standard by which courts measured the best interests of the child (BIOC).[3] Despite this legal change, courts continued to award sole custody to mothers in the majority of cases (Cancian and Meyer 1998). However, in the 1970s and

[3] See Kelly (1994) and Buehler and Gerard (1995) for a discussion of the BIOC standard.

1980s, many states either developed explicit provisions or set precedent by ruling in favor of joint-child-custody arrangements (Brinig and Buckley 1998; Kelly 1994). Table 1 shows the timing of joint-custody reforms across states.

A number of underlying factors contributed to the widespread adoption of joint-custody laws across states. First, the division of labor between parents began to change. Fathers began participating in child rearing and other household activities at greater rates, while mothers' participation in the labor market rose substantially (Jacob 1988). The redefinition of traditional gender roles provided a political voice to fathers' rights groups who actively sought equality in the division of children following marital dissolution (Jacob 1988). Second, results from child-development research indicated the importance of fathers in the development of children (Kelly 1994). Third, rising welfare participation among single mothers and the preponderance of "dead-beat" dads who were in arrears of child-support payments led states to consider policies aimed at resolving problems associated with the rising number of single-mother-headed households.[4]

**Table 1** Year of introduction of joint-custody laws by state

| State | Joint custody | State | Joint custody |
|---|---|---|---|
| Alabama | – | Montana | 1981 |
| Alaska | 1982 | Nebraska | 1983 |
| Arizona | 1991 | Nevada | 1981 |
| Arkansas | – | New Hampshire | 1974 |
| California | 1979 | New Jersey | 1981 |
| Colorado | 1983 | New Mexico | 1982 |
| Connecticut | 1981 | New York | 1981 |
| Delaware | 1981 | North Carolina | 1979 |
| Florida | 1979 | North Dakota | 1993 |
| Georgia | 1990 | Ohio | 1981 |
| Hawaii | 1980 | Oklahoma | 1990 |
| Idaho | 1982 | Oregon | 1987 |
| Illinois | 1986 | Pennsylvania | 1981 |
| Indiana | 1973 | Rhode Island | 1992 |
| Iowa | 1977 | South Carolina | – |
| Kansas | 1979 | South Dakota | 1989 |
| Kentucky | 1979 | Tennessee | 1986 |
| Louisiana | 1981 | Texas | 1987 |
| Maine | 1981 | Utah | 1988 |
| Maryland | 1984 | Vermont | 1992 |
| Massachusetts | 1983 | Virginia | 1987 |
| Michigan | 1981 | Washington | – |
| Minnesota | 1981 | West Virginia | – |
| Mississippi | 1983 | Wisconsin | 1979 |
| Missouri | 1983 | Wyoming | 1993 |

Data for the child-custody reforms are from Brinig and Buckley (1998). – indicates the state has not passed joint-custody laws

## Theoretical Predictions of Joint-Custody Reform on Child-Support Receipt

Fathers may be more likely to pay child support with joint-custodial arrangements because they spend more time with the child and are able to better monitor the allocation of child-support payments (Hofferth et al. 2010).[5] Thus, we expect joint-custody reform to increase the child-support receipt of single mothers.[6] We also expect the effects of joint-custody reform to vary for never-married, divorced, and separated mothers for three reasons: (i) establishing paternity is an obstacle for never-married mothers to obtain child-support orders but less so for divorced and separated mothers (Beller and Graham 1993), (ii) never-married fathers may have less of a bond with their children compared to separated and divorced fathers (Monna and Gauthier 2008), and (iii) joint-custody arrangements are less common for never-married mothers relative to divorced and separated mothers (Seltzer 1998). As such, we expect joint-custody reform to have a smaller effect on the child-support receipt of never-married mothers. By contrast, we expect joint-custody reform to increase the probability of receiving child support for divorced mothers. We also expect potential differences to arise between divorced and separated mothers for two reasons: (i) separated mothers may not have court settled arrangements for child custody or child support due to the uncertainty of future divorce and (ii) a portion of separated mothers may not divorce because of the high costs. Separated mothers who remained married because of the high cost of divorce are likely to be of lower SES. In fact, 39% of the separated mothers in our sample have less than a high school degree, compared to 41% of never-married mothers and only 23% of divorced mothers. Seltzer (1991) finds joint custody and child support are both positively related to SES. Thus, we

[4] Mimura (2008) reports single head-householders are significantly more likely to experience economic hardship than married head-householders.The policies adopted by federal and state governments to combat the economic harship faced by single mothers include the child-support-enforcement program (Lerman 1993; Freeman and Waldfogel 2001; Sorensen and Hill 2004), the Earned Income Tax Credit (Bok and Simmons 2002; Mammen et al. 2009), and Child-Care-Assistance Programs (Forry 2009).

[5] For example, Garasky and Stewart (2007) find that increased visitation by non-resident fathers decreases the probability that children experience food insecurity, and Eldar-Avidan et al. (2008) find stronger relationships between the noncustodial parent and child reduces negativity from financial contribution on both sides.

[6] Increasing child-support receipt is important for single mothers, as single mothers often suffer financially following divorce. In particular, Sanders and Porterfield (2010) show that single mothers, who are heads of households, accumulate fewer assets.

expect the child-support receipt of separated mothers to be less affected by joint-custody reform.

The effects of joint-custody reform should also differ by the welfare-participation status of single mothers for two primary reasons. First, single mothers who receive public assistance are less likely to have shared child custody, an indication that they may be unaffected by joint-custody reform. By contrast, single mothers who do not receive public assistance are more likely to have joint custody (Seltzer 1991). Second, it is also plausible that fathers of the children whose mothers receive public assistance are of lower SES and unable to pay child support (Roff 2008).[7] Third, mothers who receive public assistance have to relinquish their child-support receipts to the welfare agency. This may decrease fathers' incentives to pay child support and mothers' incentives to seek child support awards (Roff 2008). As a result, we expect joint-custody reform to have a smaller effect on single mothers who receive public assistance relative to those who do not receive public assistance.

Table 2 presents summary statistics for the child-support receipt of single mothers. The statistics show that divorced mothers have the highest child-support-receipt rates. Separated mothers have lower child-support-receipt rates, but never-married mothers have the lowest rates of child-support receipt. Partitioning the sample by welfare participation status, 18% of single mothers who receive public assistance receive child support compared to 48% of single mothers who do not receive public assistance. As was the case for nonpartitioned sample, divorced mothers have the highest child-support-receipt rates followed by separated and never-married mothers, regardless of welfare-participation status. However, the child-support-receipt rates are higher for those who do not receive public assistance relative to those that do receive public assistance.

**Table 2** Summary statistics for the child-support receipt of single mothers

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| Full sample | | | | |
| *Child-Support Receipt* | 0.3651 | 0.1444 | 0.5165 | 0.2822 |
| | (0.4815) | (0.3515) | (0.4997) | (0.4500) |
| Number of observations | 51,274 | 13,251 | 25,756 | 12,267 |
| Partitioned samples by receipt of public assistance | | | | |
| Receives public assistance | | | | |
| *Child-support receipt* | 0.1818 | 0.1271 | 0.2678 | 0.1569 |
| | (0.3857) | (0.3331) | (0.4429) | (0.3637) |
| Number of observations | 19,322 | 7,864 | 6,366 | 5,102 |
| Does not receive public assistance | | | | |
| *Child-support receipt* | 0.4774 | 0.1703 | 0.5996 | 0.3730 |
| | (0.4995) | (0.3758) | (0.4900) | (0.4834) |
| Number of observations | 31,942 | 5,387 | 19,390 | 7,165 |

Standard deviations are in parentheses. *Child-Support Receipt* equals one if the single mother receives child support

## The Relationship between Joint Child Custody and Child Support

A number of studies examine the relationship between joint-child-custody arrangements and child-support outcomes. Several of these find that the receipt and level of child-support income and joint custody are positively related (Del Boca and Ribero 1998; Huang et al. 2003; Pearson and Thoennes 1988), while others fail to detect a statistically significant link between the two variables (Arditti and Keith 1993; Gunnoe and Braver 2001; Seltzer 1991, 1998; Seltzer and Maralani 2001). Individual-specific unobserved heterogeneity, simultaneity bias, the lack of nationally-representative data, and difficulty finding a valid instrument are all common problems when researchers attempt to establish a causal link between joint custody and child-support receipt.

Researchers primarily use an instrumental variables (IV) approach to identify the causal effect of joint custody on child-support receipt. In order for an instrument to be valid, it must be significantly correlated with joint custody but not otherwise affect child-support receipt (Staiger and Stock 1997). Seltzer (1998) uses variation in child-custody laws across states; however a potential problem with this paper is that the sample postdates the majority of joint-custody reforms (See Table 1). Seltzer (1998) uses data from the National Survey of Families and Household (NSFH) for two waves: 1987–1988 and 1992–1994. The majority of states adopted joint custody in early- to mid-1980s. In fact, 34 states adopted joint-custody laws before the sample began. As such, there remains little variation across states, which reduces the statistical power of joint-custody reforms to predict joint custody. We believe the approach by Huang et al. (2003) is the most reliable. They use biennial data from 1992 to 1998 March and April CPSs. Their instrument for the custodial arrangement is the percentage of joint-custody arrangements across states. This instrument predicts joint custody but is statistically unrelated to child-support receipt, indicating it is a valid instrument statistically. Similar to Huang et al. (2003),

[7] For these mothers, child-care subsidies could help raise their standard of living (Forry 2009). Child-care assistance is more likely to be awarded to rural, low-income single mothers who were employed (Mammen et al. 2009). In addition, rural, low-income single mothers receive consistent support from their families (Son and Bauer 2010).

Seltzer and Maralani (2001) use the percentage of child-custody cases as an instrument for joint custody. A limiting factor of their study, however, is that it is only representative for Wisconsin.

An alternative approach to identifying the causal effect of joint custody on child-support receipt is to use the timing of joint-custody reforms across states, as used by Brinig and Buckley (1998). They use state-level, panel data and the timing of the joint-custody reforms across states to achieve identification. Their results indicate a statistically significant, positive effect of joint-custody laws on child-support receipt relative to child-support mandates. Unfortunately, their sample begins in 1986 and ends in 1994, which postdates the majority of child-custody reforms across states (See Table 1). In particular, from 1986 to 1994, 13 states adopt joint custody, while 31 states adopt joint custody prior to 1986. As such, their sample period does not encompass the majority of the variation in joint-custody reforms across states. Another potential limitation is unobserved heterogeneity at the state level, which could bias estimates.

Our study encompasses the work of previous research and provides a number of extensions. Similar to Brinig and Buckley (1998), we use the timing of joint-custody reforms across states as a source of quasi-experimental data with which to examine the impact of joint-custody arrangements on the receipt of child support. Instead of using state-level panel data, we estimate the effects of joint-custody reform on a nationally-representative sample of single mothers, which allows us to differentiate between never-married, separated, and divorced mothers and single mothers who do and do not receive public assistance. Joint-custody reform is likely to affect these single mothers differently, as each receives joint-custody arrangements and child-support income at different rates.

We contend that using variation in the timing of joint-custody reforms across states to identify the causal effect of joint custody on child-support receipt is a better approach than IV because of the many problems associated with the IV approach (Bound et al. 1995; Nelson and Startz 1990a, b; Staiger and Stock 1997). The natural experiment approach we take is generally thought to protect against endogeneity associated with the policy variable; in the case of joint custody, selection bias is a major concern. It is likely that family-level unobserved heterogeneity affects child-support receipt and is also correlated with whether or not the post-dissolution family has a joint-custody arrangement. Hence, we believe the difference-in-difference (DD) approach generates a "cleaner" estimate of the effect of joint custody on child-support receipt because it allows us to control for unobserved heterogeneity through the separation of families into "treatment" and "comparison" groups. However, there are well known problems associated with the DD estimator, most notably that the standard error associated with the policy estimate is often understated (Bertrand et al. 2004). To address this potential problem, we cluster standard errors at the state-time level.[8]

Our analysis extends the existing literature in the following ways. First, we are able to identify the causal effect of joint-child-custody arrangements on the child-support receipt of single mothers by using the variation in the timing of joint-custody reforms across states as quasi-experimental data (Angrist and Pischke 2009). Second, the sample spans from 1978 to 1993, over which time 42 states adopt joint custody. This provides additional variation to identify the causal effect of joint-custody arrangements on the child-support receipt of single mothers than found in the literature. Third, it is unlikely that the joint-custody and CSE reforms are independent of one another, as both were part of the same legislative agenda to address problems associated with the rising incidence of single-parent households (Jacob 1988). We are able to control for these law changes by holding them constant during the time period when both joint-custody and CSE reforms were occurring. Fourth, we estimate the effects of joint-custody reform on the receipt of child support for sub-samples of never-married, divorced, and separated mothers. Fifth, we estimate separately the effects of joint-custody reform on the child-support receipt of single mothers who do not receive public assistance and for those who receive public assistance.

## Data and Econometric Strategy

We use data from the March CPS from 1978 to 1993 to examine the impact of joint-custody reform on the child-support receipt of single mothers. A number of other researchers use the April CPS-Child Support Supplement (CSS) (Beller and Graham 1993; Huang et al. 2003). An advantage of using the April CPS-CSS is the availability of information on child-support awards and custodial allocation, including whether there is a joint-custody arrangement. However, this information is only available for years that postdate the majority of legal reforms directing courts to consider joint custody as the preferred custodial allocation. Despite this advantage, the April CPS-CSS has several critical drawbacks: it is biennial and information on the child-support income of single mothers is collected for fewer years than the March CPS. Another limitation is that the survey begins in 1979, and the next year of survey occurs in 1982. This is important, as 13 states adopt joint-custody laws between 1979 and 1982. The March CPS has

[8] See Bertrand et al. (2004) and Angrist and Pischke (2009) for more information on adjusting standard errors for the DD estimator.

its advantages over the April CPS: annual surveys are provided and information on child-support income is reported in each survey year. The most important reason to use the March CPS is the annual frequency of surveying. Because child-custody reforms occur in almost every year, this provides a way to exploit fully the variation in the timing of these legal changes. Using biennial data, as provided by the April CPS-CSS, does not fully exploit the variation in the timing of joint-custody reforms. In particular, using the April CPS-CSS assumes that between-survey-year law changes affect child-support receipt the same as law changes occurring in the survey year. This could lead to a finding of a spurious relationship between joint-custody reform and child-support receipt.

The one drawback of the March CPS is the lack of information provided on whether child-support awards were granted by courts. This is a potential source of bias. If child-support awards are negatively correlated with joint-custody reform but positively related to child-support receipt, our estimates are understated. A negative correlation between joint-custody reform and child-support awards could arise from cooperation among parents and/or informal child-support agreements. By contrast, if joint-custody reform is positively correlated with child-support awards and child-support awards are positively related to child-support receipt, our estimates are overstated. However, we are able to use the April CPS-CSS to gain insight into the relationship between child-support awards and child-support receipt. During a sample period analogous to ours, 78% of single mothers with a child-support award receive some child support. Over this period, data are not available on the child-support receipt for mothers without awards. However, new variables collected in the 1994 April CPS-CSS show only 9% of mothers without child-support awards receive any child support.

Due to data limitations, we are unable to test the correlation between joint-custody reform and child-support awards. However, we are able to use the 1994 April CPS to examine the correlation between actual joint-custody arrangements and child-support awards. The correlation coefficient between joint-custody arrangements and child-support due (not necessarily mandated by courts) is 0.20, while the correlation coefficient between joint-custody arrangements and child-support awards ordered by courts is −0.06. Both of these correlation coefficients are small, indicating weak relationships between joint-custody arrangements and child-support awards.

Female single-headed households with own children under 18 present are our units of observation. Our full sample contains never-married, divorced, and separated mothers. We eliminate observations that contain subfamilies and those in which the mother is not the head of household. We use this sample because the child-support-income variable is provided at the household level. Therefore, all persons living in the household are given the same value as the head of the household. For example, consider a married couple with four children, all of which are female. Assume the parents have three girls above the age of 18 and one under the age of 18, and that the head of the household reports having one child under 18 and a zero for child-support income received. Since they are reported at the household level, everyone in the household gets a one for children under 18 and a zero for child-support receipt. The parents are not in our sample because they are married. However, the three daughters would each get an observation as a never-married mother with one child who receives no child support. Deleting sub-families circumvents this problem.

We use Brinig and Buckley's (1998) child-custody law coding (See Table 1). Both cross-state and cross-time variation in child-custody reforms provide a source of quasi-experimental data with which to examine how the adoption of laws directing courts to consider joint-custody arrangements as the preferred custodial allocation alter the incentives of noncustodial fathers to pay child support. Single mothers who live in states that adopt joint custody in any year between 1978 and 1993 are the treatment group. The comparison group is comprised of single mothers who live in states that had yet to adopt joint custody by the survey date. In 1978, only 3% of our sample lives in joint-custody states. However, by 1993, 93% of the sample lives in states that have adopted joint custody at some point during the sample period. Hence, our treatment and comparison groups exhibit substantial variation over time.

Our econometric strategy is to compare the child-support receipt of single mothers who live in states that enact joint custody with those who live in states that have yet to enact joint custody. The main covariate of interest is joint-custody reform. The econometric model is

$$\begin{aligned} \mathit{Child\,Support}_{i,s,t} = \beta_0 + \beta_1 \mathit{Joint\,Custody}_{s,t} + \beta_2 \mathbf{CSE}_{s,t} \\ + \beta_3 \mathbf{X}_{i,s,t} + \beta_4 \mathbf{S}_{s,t} + \sum\nolimits_s \beta_s \eta_s + \sum\nolimits_t \beta_t \tau_t + u_{i,s,t}. \end{aligned} \tag{1}$$

The terms $i$, $s$, and $t$ represent single mothers, states, and time, respectively. The variable *Child Support* equals one if the single mother receives child-support income and zero otherwise; *Joint Custody* equals one if a state adopts joint custody and zero otherwise; **CSE** is a vector of Child Support Enforcement variables, including expenditures and various reforms; **X** is vector of single mother controls, including age, race, educational attainment, the number of children under six, and the number of children under 18; **S** is a vector of time-varying, state-level controls, including the contemporaneous and lagged maximum AFDC benefits paid to families of four and the unemployment rate along

**Table 3** Variable definitions and summary statistics for single-mother and state-level controls

| Variable name | Variable description | Mean | Std. Dev. |
|---|---|---|---|
| Single mother controls | | | |
| Divorced | =1 if single mother is divorced | 0.5023 | 0.4999 |
| Separated | =1 if single mother is separated | 0.2392 | 0.4266 |
| Never married | =1 if single mother is never married | 0.2584 | 0.4378 |
| Receives public assistance | =1 if single mother receives public assistance | 0.3799 | 0.4854 |
| Children under 6 | Number of children in household under 6 years of age | 0.5573 | 0.7697 |
| Children under 18 | Number of children in household under 18 years of age | 1.8638 | 1.0276 |
| Age | In years | 33.430 | 7.3872 |
| Age squared | Age in years squared | 1172.1 | 503.65 |
| Black | =1 if single mother is black | 0.2656 | 0.4416 |
| Hispanic | =1 if single mother is Hispanic | 0.1589 | 0.3656 |
| High school | =1 if single mother has only a high-school degree | 0.4057 | 0.4910 |
| Some college | =1 if single mother has attended college with no degree | 0.1957 | 0.3967 |
| Graduate | =1 if single mother is a college graduate | 0.0848 | 0.2785 |
| Metro | =1 if single mother lives in an urban area | 0.7203 | 0.4489 |
| State-level controls | | | |
| AFDC benefit | Dollar amount of the maximum AFDC benefit paid to families of four | 365.66 | 146.17 |
| Unemployment | Percentage of the unemployed population who is searching for employment | 7.0280 | 2.0592 |
| CSE expenditures | Dollar amount spent on child-support enforcement per single-mother family | 61.799 | 32.190 |
| Genetic testing | =1 if state allows genetic testing to be used in establishing paternity | 0.5794 | 0.4937 |
| Wage withholding | =1 if state withholds wages from the paychecks of delinquent parents | 0.7906 | 0.4069 |
| Immediate withholding | =1 if state withholds payments for all new cases of mothers on welfare | 0.3674 | 0.4821 |
| Universal withholding | =1 if state withholds payments from parents regardless of welfare receipt | 0.1769 | 0.3896 |
| Paternity until 18 years | =1 if state allows the establishment of paternity until child reaches age 18 | 0.6535 | 0.4759 |
| Numerical guidelines | =1 if state has guidelines in place for issuing child-support orders | 0.4524 | 0.4977 |
| Presumptive guidelines | =1 if state mandates judges to follow the numerical guidelines | 0.3683 | 0.4823 |
| State intercept | =1 if state intercepts income-tax refunds for child-support orders in arrears | 0.4904 | 0.4999 |

Means and standard deviations are for the full sample, with 51,274 observations for all variables (all single mothers). The variables *AFDC Benefit* and *CSE Expenditures* are measured in 1993 dollars

with two of its lags; $\eta$ and $\tau$ are state and time fixed effects, respectively; and $u$ is the disturbance term. Table 3 presents variable definitions and summary statistics for single-mother and state-level controls.

Angrist and Pischke (2009) suggest that controls at the state level (i.e. **CSE**, **S**, and $\eta$) are the most important covariates to aid in parsing the effect of the policy variable from other influences. This natural experiment approach circumvents problems with unobserved heterogeneity at the individual level but requires additional controls at the state level to ensure identification. In particular, we contend that the variables in **CSE** are most important, as they were part of a parallel legislative agenda. Hence, it is important to estimate the effect of joint-custody reform on the child-support receipt of single mothers holding the variables in **CSE** constant. Failure to include these variables as controls could result in a spurious relationship between joint-custody reform and the child-support receipt of single mothers, as a number of other studies show the importance of various CSE reforms in determining the child-support income received by single mothers (Argys and Peters 2001; Argys et al. 2001; Beller and Graham 1993; Freeman and Waldfogel 2001; Neelakantan 2009; and Sorensen and Hill 2004).[9]

[9] We also estimate models with an additive index of the Child Support Enforcement (CSE) reform variables as in Huang (2002) and Huang et al. (2003). The CSE index is not statistically different from zero in any specification. The inclusion of the additive CSE index does not materially affect the estimated effect of joint-custody reform. In addition, we check the sensitivity of our estimates to the inclusion of additional state-level controls, including real per-capita income, the demographic make-up of the population, Supplemental Security Income (SSI) participation rates, and other family-law reforms, and we find that the estimated effects of joint-custody reform are not materially affected by the inclusion of these variables. As such, we do not report these results. The chosen empirical specification is comparable to recent work by Sorensen and Hill (2004).

**Table 4** Logit estimates for the effects of joint-custody laws on the child-support receipt of single mothers

| Variable | All | Never Married | Divorced | Separated |
|---|---|---|---|---|
| Joint-custody reform | 0.0212* | −0.0007 | 0.0363** | 0.0171 |
| | (0.0090) | (0.0116) | (0.0125) | (0.0164) |
| Divorced | 0.2548*** | – | – | – |
| | (0.0070) | | | |
| Separated | 0.1120*** | – | – | – |
| | (0.0084) | | | |
| Receives public assistance | −0.1819*** | −0.0151* | −0.2880*** | −0.1496*** |
| | (0.0059) | (0.0070) | (0.0085) | (0.0097) |
| Number of observations | 51,274 | 13,251 | 25,756 | 12,267 |

Estimates are reported as marginal effects. Each specification includes state and year fixed effects and the controls from Table 3. We adjust our standard errors by clustering at the state-time level

* $p < .05$; ** $p < .01$; *** $p < .001$

## Results

In Sect. 4.1, we examine the impact of joint-custody reform on the probability of receiving child support for all single mothers and for sub-samples of never-married, divorced, and separated mothers. In Sect. 4.2, we estimate the impact of joint-custody reform on the probability of receiving child support for sub-samples of single mothers based on their welfare-participation status. Estimates for the single-mother and state-level controls are presented in Tables 7–12 in the Appendix.

### Logit Estimates for Single Mothers

Table 4 presents the estimated marginal effects of joint-custody reform on the child-support receipt of all single mothers and for sub-samples of never-married, divorced, and separated mothers. In the model for all single mothers, we present different intercepts for divorced and separated mothers and for those who receive public assistance. Likewise, the models estimated for subsamples of never-married, separated, and divorced mothers include a different intercept for those who receive public assistance. The estimates for the indicator variables in models for all single mothers suggest that divorced and separated mothers are more likely to receive child support than their never-married counterparts. By contrast, single mothers who receive public assistance are less likely to receive child support relative to those who do not receive public assistance, which is also the case for each of the subsamples of never-married, separated, and divorced mothers.

The estimated effect for joint-custody reform corresponds to a 7% (or 2.1 percentage point) increase in the probability of receiving child support for all single mothers.[10] The estimates for the different sub-groups of single mothers show that only divorced mothers are significantly affected by joint-custody reform. Their probability of receiving child support increases by 8% (or 3.6 percentage points) following joint-custody reform. Because never-married and separated mothers are unaffected by child-custody reform, divorced mothers appear to be driving the results for the sample of all single mothers.

Our findings are generally consistent with our initial hypotheses. Joint-custody reform raises the probability of receiving child support for all single mothers. However, the child-support receipt of never-married and separated mothers is unaffected by joint-custody reform, while the probability of receiving child support rises for divorced mothers: the group of single mothers most likely to have shared child custody. A likely reason for the lack of statistically significant finding for never-married mothers is that they are least likely to receive a joint-custody arrangement, primarily because they are often of low SES and establishing paternity is often difficult. There are a number of reasons divorced mothers may be different from separated mothers with respect to joint-custody reform and child-support receipt. Perhaps, the most likely reason for this difference is that child custody and child-support awards are unlikely to be settled for separated couples. This could be due to the possibility that separated mothers are often of low SES, which may make divorce too costly to pursue. By contrast, divorced mothers are the most likely

[10] We calculate the percent change in the probability of receiving child support by using the predicted values for the probability of receiving child support when the variable *Joint-Custody Reform* is set equal to zero and one, while all other right-hand-side variables are held at their mean values.

to receive a joint-custody arrangement, as they are more likely to be of higher SES.

Our estimates for the effect of joint-custody reform on child-support receipt, while similar in sign, differ in magnitude from those found by Brinig and Buckley (1998). They find a ten percentage point increase in child-support receipt relative to child-support orders. We also find a statistically significant, positive effect of joint-custody reform on single mothers' child-support receipt, but our estimate is much smaller. Specifically, we find a 2.1 percentage point (or 7%) increase in the child-support receipt rates for all single mothers following joint-custody reform. Our results are similar to those of Huang et al. (2003) who find a positive effect of predicted joint custody on child-support payments to divorced mothers. By contrast, our estimates do not support the conclusion by Seltzer (1998), who finds that joint custody is unrelated to child-support payments received by divorced mothers after conditioning on family characteristics.

We contend that our estimates differ from Brinig and Buckley (1998) for two primary reasons. First, our sample encompasses the dramatic shift from the maternal-preference to the joint-custody standard which began in the late-1970s and continued throughout the 1980s. Brinig and Buckley's (1998) sample period begins in 1986, which postdates the majority of joint-custody reforms. As a result, their estimates could reflect a pre-existing trend rather than the effect of joint-custody reform on child-support receipt, which could overstate the estimated effect. In fact, Sorensen and Hill (2004, Fig. 1) present trends in child-support receipt rates for single mothers, indicating an overall upward trend during the time in which the majority of child-custody reforms occurred (i.e. the early-1980s). Wolfers (2006) shows that failure to account for pre-existing trends can drastically overstate the effects of state-level reforms on the outcome of interest. Second, it could be that child-support-receipt rates and joint-custody reform are simultaneously determined. The adoption of joint-custody laws may have been a low-cost (to the state) incentive for nonresidential parents to pay child support. As such, low rates of child-support receipt could lead to joint-custody reform. The use of household-level data circumvents this potential problem, as it is unlikely that individual child-support receipt caused state-level joint-custody reform.

### Logit Estimates for Single Mothers by Welfare-Participation Status

The next set of models examines the impact of joint-custody reform on the child-support receipt of single mothers by welfare-participation status. Table 5 presents the marginal effects of joint-custody reform on the probability of receiving child support for all single mothers and for subsamples of never-married, separated, and divorced mothers who receive public assistance. We find no statistical evidence linking joint-custody reform to the child-support receipt of all single mothers who receive public assistance. This effect is robust for subsamples of never-married, divorced, and separated mothers. A couple of explanations exist for the lack of statistical significance found for the effect of joint-custody reform on the child-support receipt of single mothers who receive public assistance. First, single mothers who receive public assistance may either have to relinquish their child support to the welfare agency or receive lower welfare benefits. This reduces the incentive for noncustodial fathers to comply with child-support orders, and it also reduces the incentive for single mothers to pursue child-support income from nonresidential fathers (Roff 2008). Second, lower SES mothers are less likely to receive child support or joint custody. Therefore, they should be less affected by joint-custody reform (Seltzer 1991).

**Table 5** Logit estimates for the effects of joint-custody laws on the child-support receipt of single mothers who receive public assistance

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| Joint-custody reform | −0.0035 | −0.0190 | 0.0198 | −0.0100 |
| | (0.0112) | (0.0162) | (0.0200) | (0.0217) |
| Divorced | 0.1150*** | – | – | – |
| | (0.0086) | | | |
| Separated | 0.0461*** | – | – | – |
| | (0.0082) | | | |
| Number of observations | 19,322 | 7,864 | 6,366 | 5,102 |

Estimates are reported as marginal effects. Each specification includes state and year fixed effects and the controls from Table 3. We adjust our standard errors by clustering at the state-time level

* $p < .05$; ** $p < .01$; *** $p < .001$

Table 6 is analogous to Table 5, except that we focus on single mothers who *do not* receive public assistance. It is clear from these estimates that single mothers who do not receive public assistance are affected differently by joint-custody reform than those who receive public assistance. We find an 8% (or 3.4 percentage point) increase in the probability of receiving child support for all single mothers after enactment of joint-custody laws. Similar to the estimates shown in Table 3, joint-custody reform's effect on divorced mothers appears to drive this result, as never-married and separated mothers are unaffected. Divorced mothers' probability of receiving child support rises by 6% (or 3.4 percentage points) following joint-custody reform.

**Table 6** Logit estimates for the effects of joint-custody laws on the child-support receipt of single mothers who do not receive public assistance

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| Joint-custody reform | 0.0337** | 0.0260 | 0.0344* | 0.0320 |
| | (0.0116) | (0.0173) | (0.0134) | (0.0238) |
| Divorced | 0.1900*** | – | – | – |
| | (0.0075) | | | |
| Separated | 0.1802*** | – | – | – |
| | (0.0112) | | | |
| Number of observations | 31,942 | 5,387 | 19,390 | 7,165 |

Estimates are reported as marginal effects. Each specification includes state and year fixed effects and the controls from Table 3. We adjust our standard errors by clustering at the state-time level

* $p < .05$; ** $p < .01$; *** $p < .001$

The estimates shown for the effects of joint-custody reform on the child-support receipt for sub-samples of single mothers who receive public assistance and for those who do not receive public assistance largely support our hypotheses. Consistent with our predictions, the probability of receiving child support for single mothers who do not receive public assistance increases following joint-custody reform, while the probability of receiving child support is unaffected for single mothers who receive public assistance. Similar to the results from Sect. 4.1, the estimated effects on single mothers who do not receive public assistance appear to be driven by divorced mothers, as the child-support-receipt rates of never-married and separated mother are unaffected by joint-custody reform.

## Conclusions

The preponderance of single mothers on public assistance is attributable primarily to lack of child-support payments from noncustodial fathers. Thus, increasing collection of delinquent child support has been a contentious political issue in the U.S. for over 30 years (Freeman and Waldfogel 2001; Rowe 1989; Sorensen and Hill 2004). Because joint-custody reform does not have explicit costs to taxpayers but provides incentives for fathers to pay child support, it could be a low-cost way for states to reduce the welfare dependency of single mothers. We study the impact of joint-child-custody legislation on the child-support receipt of single mothers. We exploit variation in the timing of child-custody reforms across states to identify the effect of joint-custody reform on the probability of receiving child support for single mothers. Using data from the March CPS, we find a statistically significant, positive effect of joint-custody reform on the probability of receiving child-support income for single mothers. This effect translates into a 7% (or two percentage point) increase in the probability of receiving child support. Our results indicate joint-custody reform provides a positive incentive for non-resident fathers to pay child support.

Because never-married mothers are less likely to have joint-custody arrangements than divorced or separated mothers, we partition the data into subsamples of never-married, divorced, and separated mothers. We find that never-married and separated mothers are unaffected by joint-custody reform. By contrast, the probability that divorced mothers receive child support rises by approximately 8% (or four percentage points) following joint-custody reform. This suggests that the effect of joint-custody reform on the child-support receipt of all single mothers is driven primarily by the effect on divorced mothers.

We also consider the effects of joint-custody reform on the child-support receipt of subsamples of single mothers who receive public assistance and for those who do not receive public assistance. These single mothers differ both in terms of child-support-receipt rates and the likelihood of having a joint-child-custody arrangement. Joint-custody reform increases the probability of receiving child support for single mothers who do not receive public assistance, while there is no statistical evidence that joint-custody reform affects the probability of receiving child support for those who receive public assistance.

There is significant debate as to whether joint custody places the more vulnerable party—mothers—in a worse bargaining position following divorce, and whether joint custody increases the involvement of non-resident parents in the lives of their children (Jacob 1988; Seltzer 1991). While our study does not necessarily shed light on these important issues, our overall conclusion is that joint-custody reform does increase child-support receipt rates for those most likely to have joint-custody arrangements: divorced mothers who do not receive public assistance.

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## Appendix

See Tables 7, 8, 9, 10, 11, 12.

**Table 7** Logit estimates for the effects of single-mother controls on the child-support receipt of single mothers

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| Children under 6 | −0.0183*** | −0.0030 | −0.0264*** | −0.0105 |
| | (0.0041) | (0.0045) | (0.0071) | (0.0066) |
| Children under 18 | 0.0306*** | 0.0067 | 0.0433*** | 0.0176*** |
| | (0.0027) | (0.0037) | (0.0044) | (0.0044) |
| Metro | −0.0095 | −0.0320*** | −0.0073 | 0.0060 |
| | (0.0062) | (0.0094) | (0.0086) | (0.0111) |
| Age | 0.0046 | 0.0076* | 0.0035 | 0.0163* |
| | (0.0031) | (0.0035) | (0.0052) | (0.0064) |
| Age-squared | −0.0001* | −0.0001* | −0.0001 | −0.0002*** |
| | (0.0000) | (0.0001) | (0.0001) | (0.0001) |
| Black | −0.1835*** | −0.0392*** | −0.2912*** | −0.1529*** |
| | (0.0061) | (0.0077) | (0.0097) | (0.0090) |
| Hispanic | −0.1240*** | −0.0434*** | −0.1515*** | −0.1116*** |
| | (0.0075) | (0.0092) | (0.0141) | (0.0113) |
| High school graduate | 0.1224*** | 0.0518*** | 0.1527*** | 0.0884*** |
| | (0.0067) | (0.0080) | (0.0101) | (0.0111) |
| Some college | 0.2027*** | 0.1211*** | 0.2156*** | 0.1814*** |
| | (0.0083) | (0.0147) | (0.0104) | (0.0159) |
| College graduate | 0.2522*** | 0.1026*** | 0.2641*** | 0.2134*** |
| | (0.0110) | (0.0223) | (0.0117) | (0.0234) |
| Number of observations | 51,274 | 13,251 | 25,756 | 12,267 |

Estimates are reported as marginal effects. Each specification includes state and year fixed effects and the controls from Table 3. We adjust our standard errors by clustering at the state-time level

* $p < .05$; ** $p < .01$; *** $p < .001$

**Table 8** Logit estimates for the effects of state-level controls on the child-support receipt of single mothers

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| AFDC benefit | 0.0001 | −0.0001 | 0.0001 | 0.0004* |
| | (0.0001) | (0.0001) | (0.0001) | (0.0002) |
| AFDC benefit (−1) | −0.0001 | 0.0000 | 0.0000 | −0.0001 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Unemployment | −0.0029 | −0.0148 | 0.0085 | −0.0215 |
| | (0.0082) | (0.0134) | (0.0113) | (0.0166) |
| Unemployment (−1) | 0.0134 | 0.0185 | −0.0018 | 0.0470* |
| | (0.0110) | (0.0153) | (0.0162) | (0.0218) |
| Unemployment (−2) | −0.0089 | −0.0010 | −0.0072 | −0.0193 |
| | (0.0070) | (0.0066) | (0.0107) | (0.0142) |
| Child-support expenditures | −0.0001 | −0.0002 | 0.0000 | 0.0000 |
| | (0.0002) | (0.0002) | (0.0002) | (0.0003) |
| Genetic testing (−1) | −0.0148 | −0.0151 | −0.0130 | −0.0119 |
| | (0.0091) | (0.0115) | (0.0124) | (0.0153) |
| Wage withholding | 0.0082 | 0.0129 | 0.0089 | 0.0064 |
| | (0.0092) | (0.0117) | (0.0128) | (0.0160) |
| Immediate withholding | −0.0107 | 0.0057 | −0.0130 | −0.0179 |
| | (0.0120) | (0.0141) | (0.0158) | (0.0186) |
| Universal withholding | 0.0275* | 0.0200 | 0.0365* | −0.0032 |
| | (0.0114) | (0.0124) | (0.0153) | (0.0170) |

**Table 8** continued

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| Paternity until 18 years | −0.0091 | 0.0010 | −0.0285* | 0.0242 |
| | (0.0101) | (0.0130) | (0.0136) | (0.0159) |
| Numerical guidelines | −0.0105 | −0.0011 | −0.0121 | −0.0122 |
| | (0.0117) | (0.0135) | (0.0166) | (0.0173) |
| Presumptive guidelines | 0.0154 | 0.0092 | 0.0083 | 0.0202 |
| | (0.0183) | (0.0201) | (0.0206) | (0.0278) |
| State Intercept (−1) | −0.0017 | −0.0111 | 0.0189 | −0.0290 |
| | (0.0089) | (0.0119) | (0.0122) | (0.0156) |
| Number of Observations | 51,274 | 13,251 | 25,756 | 12,267 |

Estimates are reported as marginal effects. (−1) denotes a lag order of one and (−2) denotes a lag of order two. Each specification includes state and year fixed effects and the controls from Table 3. We adjust our standard errors by clustering at the state-time level

* $p < .05$; ** $p < .01$; *** $p < .001$

**Table 9** Logit estimates for the effects of single-mother controls on the child-support receipt of single mothers who receive public assistance

| Variable | All | Never Married | Divorced | Separated |
|---|---|---|---|---|
| Children under 6 | −0.0032 | −0.0028 | −0.0032 | −0.0055 |
| | (0.0040) | (0.0051) | (0.0095) | (0.0068) |
| Children under 18 | 0.0020 | 0.0026 | −0.0023 | 0.0046 |
| | (0.0028) | (0.0041) | (0.0059) | (0.0046) |
| Metro | −0.0177** | −0.0130 | −0.0292** | −0.0117 |
| | (0.0079) | (0.0111) | (0.0146) | (0.0147) |
| Age | 0.0026 | 0.0090** | −0.0064 | 0.0036 |
| | (0.0034) | (0.0045) | (0.0081) | (0.0071) |
| Age-squared | −0.0001 | −0.0002** | 0.0001 | −0.0001 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Black | −0.0622*** | −0.0474*** | −0.0883*** | −0.0656*** |
| | (0.0067) | (0.0093) | (0.0144) | (0.0109) |
| Hispanic | −0.0535*** | −0.0468*** | −0.0769*** | −0.0448*** |
| | (0.0079) | (0.0096) | (0.0186) | (0.0140) |
| High School Graduate | 0.0667*** | 0.0424*** | 0.1044*** | 0.0599*** |
| | (0.0070) | (0.0089) | (0.0141) | (0.0139) |
| Some College | 0.1891*** | 0.1453*** | 0.2505*** | 0.1726*** |
| | (0.0141) | (0.0208) | (0.0229) | (0.0247) |
| College Graduate | 0.2552*** | 0.1232** | 0.3551*** | 0.2085*** |
| | (0.0313) | (0.0511) | (0.0448) | (0.0614) |
| Number of Observations | 19,322 | 7,864 | 6,366 | 5,102 |

Estimates are reported as marginal effects. Each specification includes state and year fixed effects and the controls from Table 3. We adjust our standard errors by clustering at the state-time level

* $p < .05$; ** $p < .01$; *** $p < .001$

**Table 10** Logit estimates for the effects of state-level controls on the child-support receipt of single mothers who receive public assistance

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| AFDC benefit | 0.0000 | −0.0002 | −0.0001 | 0.0004** |
| | (0.0001) | (0.0001) | (0.0002) | (0.0002) |
| AFDC benefit (−1) | 0.0000 | 0.0001 | −0.0001 | −0.0002* |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Unemployment | −0.0056 | −0.0102 | 0.0030 | −0.0025 |
| | (0.0107) | (0.0148) | (0.0181) | (0.0205) |

**Table 10** continued

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| Unemployment (−1) | 0.0181 | 0.0116 | 0.0245 | 0.0182 |
| | (0.0117) | (0.0163) | (0.0221) | (0.0244) |
| Unemployment (−2) | −0.0096 | −0.0011 | −0.0187 | −0.0145 |
| | (0.0070) | (0.0078) | (0.0161) | (0.0135) |
| Child-support expenditures | 0.0000 | −0.0002 | −0.0003 | 0.0008** |
| | (0.0002) | (0.0003) | (0.0004) | (0.0003) |
| Genetic testing (−1) | −0.0158 | −0.0055 | −0.0348 | −0.0105 |
| | (0.0111) | (0.0132) | (0.0213) | (0.0189) |
| Wage withholding | −0.0132 | −0.0043 | −0.0080 | −0.0265 |
| | (0.0116) | (0.0149) | (0.0213) | (0.0221) |
| Immediate withholding | −0.0166 | 0.0116 | −0.0403 | −0.0332 |
| | (0.0140) | (0.0186) | (0.0265) | (0.0215) |
| Universal withholding | 0.0329** | 0.0105 | 0.0924*** | 0.0067 |
| | (0.0141) | (0.0156) | (0.0296) | (0.0227) |
| Paternity until 18 years | 0.0083 | 0.0002 | −0.0063 | 0.0367* |
| | (0.0130) | (0.0159) | (0.0243) | (0.0202) |
| Numerical guidelines | −0.0135 | −0.0032 | −0.0067 | −0.0377* |
| | (0.0141) | (0.0160) | (0.0256) | (0.0212) |
| Presumptive guidelines | 0.0125 | −0.0065 | 0.0061 | 0.0594 |
| | (0.0193) | (0.0243) | (0.0314) | (0.0388) |
| State intercept (−1) | −0.0147 | −0.0178 | 0.0009 | −0.0427** |
| | (0.0107) | (0.0140) | (0.0213) | (0.0194) |
| Number of observations | 19,322 | 7,864 | 6,366 | 5,102 |

Estimates are reported as marginal effects. (−1) denotes a lag order of one and (−2) denotes a lag of order two. Each specification includes state and year fixed effects and the controls from Table 3. We adjust our standard errors by clustering at the state-time level

* $p < .05$; ** $p < .01$; *** $p < .001$

**Table 11** Logit estimates for the effects of single-mother controls on the child-support receipt of single mothers who do not receive public assistance

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| Children under 6 | −0.0264*** | −0.0018 | −0.0306*** | −0.0145 |
| | (0.0063) | (0.0086) | (0.0087) | (0.0108) |
| Children under 18 | 0.0528*** | 0.0168** | 0.0620*** | 0.0310*** |
| | (0.0041) | (0.0066) | (0.0052) | (0.0070) |
| Metro | −0.0025 | −0.0498*** | 0.0002 | 0.0273* |
| | (0.0083) | (0.0150) | (0.0096) | (0.0163) |
| Age | 0.0124*** | 0.0082 | 0.0066 | 0.0292*** |
| | (0.0046) | (0.0066) | (0.0059) | (0.0090) |
| Age-squared | −0.0002*** | −0.0002 | −0.0001* | −0.0004*** |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Black | −0.2601*** | −0.0324** | −0.3500*** | −0.2185*** |
| | (0.0083) | (0.0134) | (0.0113) | (0.0136) |
| Hispanic | −0.1556*** | −0.0334* | −0.1621*** | −0.1495*** |
| | (0.0110) | (0.0181) | (0.0158) | (0.0167) |
| High school graduate | 0.1366*** | 0.0539*** | 0.1480*** | 0.0974*** |
| | (0.0094) | (0.0143) | (0.0116) | (0.0171) |

**Table 11** Logit estimates for the effects of single-mother controls on the child-support receipt of single mothers who do not receive public assistance

| Variable | All | Never married | Divorced | Separated |
|---|---|---|---|---|
| Some college | 0.1937*** | 0.1022*** | 0.1873*** | 0.1805*** |
| | (0.0096) | (0.0198) | (0.0109) | (0.0200) |
| College graduate | 0.2425*** | 0.1028*** | 0.2330*** | 0.2148*** |
| | (0.0111) | (0.0282) | (0.0110) | (0.0250) |
| Number of observations | 31,942 | 5,387 | 19,390 | 7,165 |

Estimates are reported as marginal effects. Each specification includes state and year fixed effects and the controls from Table 3. We adjust our standard errors by clustering at the state-time level

* $p < .05$; ** $p < .01$; *** $p < .001$

**Table 12** Logit estimates for the effects of state-level controls on the child-support receipt of single mothers who do not receive public assistance

| Variable | All | Never Married | Divorced | Separated |
|---|---|---|---|---|
| AFDC benefit | 0.0001 | 0.0001 | 0.0000 | 0.0003 |
| | (0.0001) | (0.0002) | (0.0001) | (0.0002) |
| AFDC benefit (−1) | 0.0000 | −0.0001 | 0.0001 | 0.0000 |
| | (0.0001) | (0.0001) | (0.0001) | (0.0001) |
| Unemployment | −0.0014 | −0.0233 | 0.0082 | −0.0353 |
| | (0.0107) | (0.0238) | (0.0124) | (0.0237) |
| Unemployment (−1) | 0.0078 | 0.0313 | −0.0098 | 0.0596* |
| | (0.0152) | (0.0267) | (0.0183) | (0.0328) |
| Unemployment (−2) | −0.0038 | 0.0008 | −0.0005 | −0.0136 |
| | (0.0103) | (0.0110) | (0.0125) | (0.0227) |
| Child-support expenditures | −0.0003 | −0.0001 | 0.0000 | −0.0008** |
| | (0.0002) | (0.0003) | (0.0003) | (0.0004) |
| Genetic testing (−1) | −0.0031 | −0.0206 | 0.0041 | −0.0057 |
| | (0.0110) | (0.0185) | (0.0125) | (0.0225) |
| Wage withholding | 0.0219* | 0.0347** | 0.0117 | 0.0337 |
| | (0.0118) | (0.0175) | (0.0139) | (0.0218) |
| Immediate withholding | 0.0003 | 0.0022 | 0.0034 | −0.0021 |
| | (0.0133) | (0.0209) | (0.0160) | (0.0258) |
| Universal withholding | 0.0169 | 0.0347* | 0.0138 | −0.0138 |
| | (0.0133) | (0.0206) | (0.0155) | (0.0236) |
| Paternity until 18 years | −0.0233* | −0.0005 | −0.0353*** | 0.0108 |
| | (0.0122) | (0.0199) | (0.0135) | (0.0243) |
| Numerical guidelines | −0.0054 | 0.0041 | −0.0127 | 0.0136 |
| | (0.0132) | (0.0214) | (0.0163) | (0.0262) |
| Presumptive guidelines | 0.0109 | 0.0265 | 0.0043 | −0.0133 |
| | (0.0203) | (0.0274) | (0.0220) | (0.0351) |
| State intercept (−1) | 0.0035 | −0.0115 | 0.0204 | −0.0269 |
| | (0.0113) | (0.0190) | (0.0131) | (0.0226) |
| Number of observations | 31,942 | 5,387 | 19,390 | 7,165 |

Estimates are reported as marginal effects. (−1) denotes a lag order of one and (−2) denotes a lag of order two. Each specification includes state and year fixed effects and the controls from Table 3. We adjust our standard errors by clustering at the state-time level

* $p < .05$; ** $p < .01$; *** $p < .001$

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