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EMPIRICAL ESSAYS ON PUBLIC HEALTH AND FAMILY POLICIES

BY

Brandeanna Dawn Allen

A Dissertation Submitted to the Graduate School at Middle Tennessee State

University in Partial Fulfillment of the Requirement for the Degree

Doctor of Philosophy/Economics

Murfreesboro, TN

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APPROVAL PAGE

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Approved by:

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Dr. Charles Baum, Committee Chair

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Dr. Mark Owens, Committee Member

_____
Dr. Adam Rennhoff, Committee Member

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Dr. Charles Baum, Department Chair, Economics and Finance

_____
Dr. Michael D. Allen, Dean, College of Graduate Studies

DEDICATED TO THE MEMORY OF MY MOM

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ABSTRACT

This dissertation consists of three chapters on the effects of public health and family policies. The first chapter, "The Impact of Mandatory Delay Laws on Fertility Outcomes," examines the effect of waiting periods and associated counseling requirements for abortions on abortion, birth, and pregnancy rates. I find the laws reduce abortion rates, increases birth rates, and have no effect on pregnancies. Further, the effect of the laws varies by the counseling requirements mandated by the state. The second chapter, "The Effects of Mandatory Delay Laws on Women and Children," analyzes secondary consequences of the laws on women and children. I find the laws increase adoption rates, but they have no effect on child maltreatment. The laws are shown to decrease the number of mentally unhealthy days per state, but they have no effect on female suicides. The third chapter (co-authored with John Nunley and Alan Seals), "The Effects of Joint-Child-Custody Legislation on the Child-Support Receipt of Single Mothers," focuses on whether joint custody affects single mother's child support receipt. Our results show joint custody raises the probability that single mothers receive child support. Further, divorced, single mothers who do not receive welfare benefit the most from joint custody.

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CHAPTER I

INTRODUCTION

This dissertation consists of three essays that investigate the impact of public health and family policies. This body of work extends the existing literature by examining secondary consequences of mandatory delay laws for abortion and joint-child custody legislation. Specifically, the essays examine the effects of mandatory delay laws on fertility, the effects of mandatory delay laws on women and children, and the effects of joint custody on the receipt of child support for single mothers.

The first essay empirically examines the effect of mandatory delay laws for abortion on fertility outcomes. In this essay, I use abortions, births, and pregnancies to measure changes in fertility. In 1992, the U.S. Supreme Court reversed a previous ruling and legalized mandatory delay laws. Consequently, 24 states enacted laws that require women to wait a specified period of time before obtaining an abortion. States provide counseling to women during the waiting period, and the type of counseling varies across states. Using state-level panel data, I find mandatory delay laws are associated with almost a six percent reduction in abortion rates. The adoption of mandatory delay laws is positively related to births but has no effect on pregnancy rates. The effect of mandatory delay laws vary by state visit and counseling requirements. My results show in-person counseling has a greater effect on reducing abortion rates and increasing birth rates than counseling received online or over the phone. Furthermore, mandatory delay laws reduce the abortion rate by nearly twenty-five percent in states that require in-person counseling that includes information often criticized as medically ungrounded.

My second essay continues the analysis of mandatory delay laws by estimating the effects of the laws on women and children. The rise in births from mandatory delay laws could affect children by increasing the number of adoptions or, possibly, the number of child maltreatment cases. Using state-level panel data, I find mandatory delay laws increase the number of public adoptions by fourteen percent, but they do not affect child maltreatment. I further my analysis of mandatory delay laws by estimating their effect on the mental health of women. The stated intent of mandatory delay laws is to allow women to make a more informed decision, which could decrease the emotional distress of the decision. My results give some support to this claim. I find mandatory delay laws reduce the number of mentally unhealthy days per year by 6 % percent, but I find no effect of mandatory delay laws on female suicides.

My final essay (co-authored with John Nunley and Alan Seals) moves away from mandatory delay laws to estimate the effects of another policy, joint custody, on women and children. 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 likelihood of child-support receipt for single mothers. Joint-custody enactment raises the probability of receiving child support for all single mothers by six percent. The effect on all single mothers is primarily driven by the effect on divorced mothers because 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.

CHAPTER II

THE IMPACT OF MANDATORY DELAY LAWS ON FERTILITY OUTCOMES

2.1. INTRODUCTION

The U.S. Supreme Court ruling in *Roe v. Wade* (1973) legalized abortion yet still gave states the right to restrict abortion access (Levine, 2004). States began adopting laws restricting abortion access as early as the end of the decade. Prohibiting the use of Medicaid funds to pay for an abortion procedure (1973) and requiring minors to either inform or obtain consent from a guardian prior to obtaining an abortion (1973) were the earliest and most common restrictions passed by states (Levine, 2004). States began adopting mandatory waiting periods for abortion in 1992 following the U.S. Supreme Court decision in *Planned Parenthood of S.E. Pennsylvania v. Casey* (Levine, 2004). Mandatory delay laws vary by state, but typically women receive counseling concerning the procedure and are then required to wait at least 24 hours before they can obtain an abortion (Joyce, Henshaw, Dennis, Finer, & Blanchard, 2009). Between 1992 and 2005, 23 states passed mandatory delay laws, and the overall abortion rate fell by 29 %, from 17.74 to 12.58 abortions per 1,000 women.

Mandatory delay laws have generated substantial controversy in and out of the court system. Opponents of the laws claim waiting periods unnecessarily increase the burden for abortions while proponents claim the laws allow women to make a more informed decision (Richardson & Nash, 2006; New, 2008). A major point of contention among advocacy groups is the visit and counseling requirements associated with the waiting period (Richardson & Nash, 2006; New, 2008). For instance, some states require in-person counseling, thereby increasing the number of visits to the provider. States may also include information which is often criticized as medically ungrounded such as

stating that a link between breast cancer and abortion exists, the fetus can feel pain after a certain amount of weeks of gestation, and/or post-procedure negative emotional distress is associated with abortion (Center for Reproductive Rights, 2006; Richardson & Nash, 2006; NARAL: Pro-Choice America, 2009).

Although debate exists over mandatory delay laws, current literature suggests the laws have little or no effect on abortion rates (Bitler & Zavodny, 2001; Levine, 2002; Medoff, 2007; Wind, 2009). In this paper, I conduct an analysis of the effects of mandatory delay laws on fertility outcomes in the U.S. This research provides a couple of extensions to the literature. First, I estimate the impact of the laws by the specific waiting period requirements such as requiring counseling to be in person or allowing it to be received via phone, internet, fax or mail. Second, I examine the impact of different types of information given to women during counseling. Third, I include data from 1976 to 2005. The samples in the existing literature end during the late 1990s. This allows me to analyze twice the number of state law changes than has been previously studied, in order to better capture the effects of mandatory delay laws on abortions, births, and pregnancy rates.

I construct a state-level panel on abortions, births, pregnancies, and abortion restrictions, in order to identify the effects of mandatory delay laws on fertility outcomes. I find support for several conclusions regarding mandatory delay laws. Panel regressions reveal that mandatory delay laws are associated with a reduction in abortion rates, accounting for less than ten percent of the overall decline in abortion rates since the first law was enacted. A decline in abortion rates is consistent with theory because mandatory

delay laws increase the financial and emotional costs of abortion. Mandatory delay laws also increase birth rates, but they have no effect on pregnancy rates. Furthermore, I find the effect of mandatory delay laws differs by visit and counseling requirements. Mandatory delay laws which require in-person counseling decrease the abortion rate by more than seven percent, and mandatory delay laws that allow counseling to be received online or over the phone reduce abortion rates by only five percent. The effect of mandatory delay laws which include information that opponents claim is medically ungrounded is not significantly different from the effect of mandatory delay laws without this information. However, the most restrictive form of mandatory delay laws that requires in-person counseling that includes this information reduces the abortion rate by almost twenty-five percent.

2.2. BACKGROUND

2.2.1. Legal Background

Waiting periods were ruled unconstitutional in the U.S. Supreme Court decision in *City of Akron v. Akron Center for Reproductive Health* (1983). The court found waiting periods were intended to dissuade women from having abortions when not medically necessary (Levine, 2002; Levine, 2004). However, in a 5 to 4 vote, the U.S. Supreme Court ruled in *Planned Parenthood of S.E. Pennsylvania v. Casey* (1992) that mandatory delay laws do not violate the rights set forth in *Roe v. Wade* (1973) (Levine, 2004; Richardson & Nash, 2006). The ruling legalized any restrictions that did not impose "undue burden," defined as a "substantial obstacle in the path of a woman

seeking an abortion before the fetus attains viability" (Levine, 2004). States began passing mandatory delay laws in August of the same year (Levine, 2004; Joyce, Henshaw, Dennis, Finer, & Blanchard, 2009). To date, 24 states have passed laws requiring women to delay having an abortion for a specified period of time after the woman first contacts the abortion provider (Guttmacher Institute, 2009; Joyce, Henshaw, Dennis, Finer, & Blanchard, 2009).

The debate surrounding mandatory delay laws extends beyond the U.S. Supreme Court. Proponents of the law claim mandatory delay laws and counseling allow women to make a more informed decision regarding their options (New, 2008; National Right to Life Committee, 2009). Opponents of the law claim women are given counseling which is biased and medically ungrounded with the intent to dissuade them from having an abortion (Center for Reproductive Rights, 2006; Richardson & Nash, 2006).

2.2.2. Theoretical Background

Models of fertility follow the work of Becker and assume women make rational decisions about fertility by weighing the costs and benefits.[1] Restrictions to abortion should raise the cost of having an abortion and, therefore, reduce the number of abortions. Raising the cost of abortion increases the cost of unwanted pregnancies. The increase in the cost of pregnancy should lead to increased contraceptive use and a decrease in the number of pregnancies. The effect of mandatory delay laws on births is, therefore, ambiguous. Births could increase because women who are already pregnant

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[1] See Kane and Staiger (1996), Levine (2002), Levine and Staiger (2002), and Medoff (2002) for theoretical models with abortion restrictions. See Montgomery and Trussel (1986) for an overview of fertility models.

may decide not to have an abortion. However, births could decrease due to a decline in pregnancies from changes in pre-pregnancy behavior.

The financial and emotional costs of having an abortion should vary by counseling requirements. For example, an in-person counseling requirement mandates that a woman must visit the provider twice. A woman has to first visit the provider to receive counseling 24 hours in advance of the visit to have the abortion procedure. This requirement raises the financial cost of having an abortion by increasing travel costs and forgone wages (Lupfer & Silber, 1981; Joyce, Henshaw, Dennis, Finer, & Blanchard, 2009). Many states have few providers, which could potentially require out-of-town travel to obtain an abortion. A 24-hour waiting period that requires in-person counseling may force women to pay for overnight accommodations as well. Requiring in-person counseling could also increase the emotional costs associated with abortion because it may be more stressful than obtaining materials online. Also, encountering protestors on multiple occasions could cause additional stress. Thus, states with in-person counseling requirements are expected to have lower abortion rates than states that allow counseling via fax, phone, internet, or mail.

Also, some information may make the decision to have an abortion or give birth more stressful and increase emotional costs. For instance, a woman may experience additional stress in states whose counseling includes information of a possible link between abortion and breast cancer. In addition, a woman may be deterred from having an abortion because of information stating women are more likely to experience depression, guilt, and overall negative emotional distress after an abortion. Including

information about fetal pain is also likely to deter some women from having the procedure. Thus, states with mandatory delay laws that provide these types of information are expected to have lower abortion rates.

2.3. EXISTING LITERATURE

Extensive research exists on the effects of abortion legalization and abortion restrictions on abortion, birth, and pregnancy in the U.S. (Blank et al., 1996; Haas-Wilson, 1996; Kane & Staiger, 1996; Levine et al., 1996; Levine et al., 1999; Bitler & Zavodny, 2001; Levine, 2002; 2003). The majority of the literature on abortion laws focuses on restrictions to Medicaid funding and parental involvement laws. States with legal constraints to Medicaid do not permit the use of Medicaid funds to pay for abortions, thereby increasing the direct financial cost for women of low income. Most studies find that Medicaid funding restrictions decrease the overall abortion rate with estimates ranging from a 9 to 25 % decline (Blank, George, & London, 1996; Haas-Wilson, 1996; Levine, Trainor, & Zimmerman, 1996).[2] Medicaid funding restrictions are found to have either no effect or a slight negative effect on births, which is attributed to a reduction in pregnancies (Kane & Staiger, 1996; Levine, Trainor, & Zimmerman, 1996). States with parental involvement laws require minors to obtain parental consent or to inform a parent of their decision before obtaining an abortion. Some studies show parental involvement laws decrease abortion rates for minors by 13 to 25 % (Haas-Wilson, 1996; Levine, 2002; 2003). Other studies find no effect of parental involvement

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[2] Bitler and Zavodny (2001) find no effect of Medicaid funding restrictions on the timing of abortion and some evidence of an increase in total abortions.

laws on total abortion rates for all women (Blank, George, & London, 1996; Bitler & Zavodny, 2001; Levine, 2003).[3] Studies also find states with parental involvement laws have lower teenage birth and pregnancy rates (Kane & Staiger, 1996; Levine, 2002; 2003).[4]

The effects of Medicaid funding and parental involvement laws are limited to women with low-income or minors. Mandatory delay laws are unique because they change the financial and emotional costs for all women seeking an abortion. The first mandatory delay law passed in 1992, much later than the first Medicaid funding and parental consent laws. Due to the more recent nature of mandatory delay restrictions, the impact of these laws has not been widely studied. Initial research on the effects of mandatory delay laws on fertility focused on Mississippi because it was the first state to adopt the law (Althaus & Henshaw, 1994; Joyce, Henshaw, & Skatrud, 1997; Joyce & Kaestner, 2000; Joyce & Kaestner, 2001). The overall abortion rate declined in Mississippi after the law was passed (Althaus & Henshaw, 1994; Joyce et al, 1997). However, out-of-state abortions and second trimester abortions increased after Mississippi passed the law (Joyce & Kaestner, 2000; Joyce & Kaestner, 2001).[5]

Subsequent research on these restrictions uses state-level panel data with state and year fixed effects and state-specific time trends (Bitler & Zavodny, 2001; Levine, 2002; Medoff, 2007). This literature finds no effect of mandatory delay laws on state abortion,

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[3] Bitler and Zavodny (2001) find no effect on total abortion rates but an increase in later-term abortions. Thus, women delay having an abortion when these laws are in place.
[4] Levine (2002) and Levine (2003) show parental consent laws are associated with increased contraceptive use among minors, which is consistent with the reduction in pregnancies.
[5] Previous research shows no effect of South Carolina's one-hour waiting period on out-of-state or second trimester abortions (Joyce & Kaestner, 2001).

birth, or pregnancy rates (Bitler & Zavodny, 2001; Levine, 2002; Medoff, 2007).[6] However, mandatory delay laws are found to increase the rate of late-term abortions (Bitler & Zavodny, 2001). Mandatory delay restrictions were not the primary focus of the papers by Bitler and Zavodny (2001), Levine (2002), or Medoff (2007). To my knowledge, the current literature does not estimate the effects of waiting period requirements on fertility.

This paper provides a couple of extensions to the literature. First, I show how the effects of mandatory delay laws differ by state visit requirements. For example, I analyze the differences in the impact of mandatory delay laws that require in-person counseling compared to laws that allow counseling to be received online or over-the-phone. Second, I compare the effects of mandatory delay laws with and without additional information during counseling to determine whether the information given is an important determinant in the effect of the law. Third, I am able to better capture the variation in the laws by using 30 years of annual data from 1976 to 2005. The majority of the literature on mandatory delay laws end their samples prior to 1997 (Bitler & Zavodny, 2001; Levine, 2002) or use decennial data (Medoff, 2007).[7] However, twelve additional states adopted mandatory delay laws between 1997 and 2005. By examining data through 2005, my analysis of the effects of mandatory delay laws on abortions, births, and pregnancies exploits twice the variation in state law changes than the current literature. [8]

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[6] Levine (2002) finds births and pregnancies rates decline with state and year fixed effects but no effect of mandatory delay laws on either outcome with state trends included.

[7] Medoff (2007) uses decennial data for 1980, 1990, and 2000. Using 30 years of annual data, I am able to better capture the variation in the laws.

[8] The latest year of data available through the CDC's Abortion Surveillance System is 2005.

2.4. DATA

I construct a state-level panel to analyze the effects of mandatory delay laws. Data on state abortion restrictions including mandatory delay, parental involvement, and Medicaid funding laws were obtained from Bitler and Zavodny (2001), Levine (2004), Americans United for Life (2008), and the Alan Guttmacher Institute (AGI). Table 1 displays the data and sources used in this analysis.

Data on the major legislative actions in reproductive health across states are from AGI. This information allows me to identify counseling visit and information requirements for each state. Visit requirements include in-person counseling or counseling that is received via mail, fax, phone, or internet. Information requirements include information on post-procedure negative emotional distress, fetal pain, or a link between breast cancer and abortion. I chose to examine these requirements because I feel they could potentially have a measurable impact on fertility decisions (Center for Reproductive Rights, 2006; Richardson & Nash, 2006).[9]

The AGI and the Center for Disease Control (CDC) are the two main sources of state abortion rates in the literature. I use state abortion rates from the CDC for the majority of this analysis because the CDC reports annual abortion rates, which allows me to exploit annual variation in law changes. This is important because at least one state adopts a mandatory delay law in almost every year between 1992 and 2005. The AGI only reports abortion rates in half of the years in this period. The CDC collects information on the number of abortions per year from health departments and surveys, while the AGI collects information directly from abortion providers. Unfortunately, the

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[9] See Guttmacher Institute (2009) for a complete list of state counseling requirements.

CDC may not include information from smaller clinics and could understate the number

of abortions (Blank, George, & London, 1996; Center for Disease Control, 2005).[10]

The abortion rate is defined as the number of abortions per 1,000 women of

childbearing age (15 to 44) in the population. State-level rates are preferred for abortion

analysis because they are collected from health departments and abortion providers.

Individual survey data on abortion is potentially measured with error because individuals

underreport the number of abortions, and the rate of underreporting varies by culture and

legal restrictions (Jones & Darroch, 1992; Rossier, 2003).

Figure 1 presents the abortion rate per 1,000 women of childbearing age for all

states in my sample from 1976 to 2005. The figure also includes the abortion rate per

1,000 women of childbearing age in comparison and reform states. A reform state is a

state that adopts a mandatory delay law at any time in the sample. A comparison state is a

state that does not adopt a mandatory delay law at any time during my sample. The graph

shows reform states have lower abortion rates. Wolfers (2006) argues that pre-existing

trends in the outcome variable could produce biased estimates when analyzing policy

changes. Figure 1 shows abortion rates began to decline around 1980, but the first

mandatory delay law did not pass until 1992. Therefore, I begin my sample in 1976, prior

to the downward trend in abortion rates that began around 1980, in order to capture pre-

existing trends in abortion rates.

Birth rates are defined as the number of births per 1,000 women of childbearing

age in each year and state. Following Levine et al. (1996), I define the pregnancy rate as

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[10] In fact for the available years of data from the AGI, the average abortion rate per 1,000 women aged 15 to 44 reported by the CDC is 16.93 compared to 20.25 reported by the AGI.

the sum of abortion and birth rates for each year and state.[11] Time-varying state controls, which may be correlated with abortion rates and mandatory delay laws, are also included. Specifically, I include covariates measuring the number of hospital beds per million people and the number of physicians per 1,000 people in the population to control for abortion access.[12] Other controls include the marriage rate, the female labor force participation rate, the unemployment rate, state per capita income, the percent of women in state legislature, and state maximum benefits for Aid to Families with Dependent Children (AFDC) or Temporary Assistance for Needy Families (TANF). I also control for demographic changes by including the population of females aged 15 to 19, 20 to 24, 25 to 34, and 35 to 44 as well as the percent of females that are white and the percent that are black. I use state population of females aged 15 to 44 to weight the regressions.

2.5. ESTIMATION METHODOLOGY

The estimation relies on cross-state and cross-time variation in the timing of laws to identify the effects of mandatory delay restrictions on fertility outcomes. The timing of mandatory delay reform was likely not in response to changes in fertility but rather in response to a Supreme Court decision because prior to the ruling in *Planned Parenthood of S.E. Pennsylvania v. Casey* (1992) mandatory delay laws were deemed unconstitutional (Levine, 2004).

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[11] It is possible that the rate of miscarriages changes over time. See Levine et al. (1996) for additional discussion on the problem of pregnancy losses.
[12] Blank et al. (1996) show that abortion providers may be endogenous to the abortion rate. Therefore following Levine (1996) and Bitler and Zavodny(2001), I include the physician rate and hospital bed rate to control for abortion access in the state.

Multivariate regression analysis is used to capture the effect of the laws on fertility. The following equation is used:

$$Y_{st} = Mandatory\ Delay_{st}\beta_1 + R_{st}\beta_2 + X_{st}\beta_3 + \gamma_s + \gamma_t + trend * \gamma_s + \epsilon_{st}.$$

Subscripts s and t index states and years, respectively. Y represents the logged values of state-level fertility outcomes: abortion, birth, and pregnancy rates. *Mandatory Delay* is an indicator variable for whether a state has a mandatory delay restriction. *Mandatory Delay* is replaced with interaction terms for mandatory delay laws with certain counseling requirements in subsequent estimations. R is a vector of indicator variables for other abortion restrictions. X is a vector of time-varying state controls such as state population demographics, economic conditions and the state political environment, γ_s is a vector of state fixed effects, γ_t is a vector of year fixed effects, and $\gamma_s*trend$ is a vector of state-specific linear time trends. State fixed effects capture anything specific to a state that does not change over time that is not already accounted for. State-specific linear time trends are included to capture state-specific unobserved changes over time, such as demographic changes, which could be correlated with fertility.

Table 2 provides summary statistics for the key variables. Columns 2 and 3 of Table 2 present the summary statistics for states with and without mandatory delay restrictions. More than 70 % of states with mandatory delay laws allow counseling to be received via mail, phone, fax, or internet whereas fewer than 30 % require counseling to be received in person. Slightly over half of the states with mandatory delay laws also include at least one additional information requirement. The simple averages show that 97 % of states with mandatory delay laws also have parental consent laws, and 95 % of

states with mandatory delay laws have Medicaid funding restrictions. The abortion rate is 10.5 abortions per 1,000 women in states with mandatory delay laws, which is much lower than the 17.3 abortions per 1,000 women in states without mandatory delay laws. Birth rates are similar between states with and without the restriction while pregnancy rates are lower in states with mandatory delay laws.

2.6. RESULTS

2.6.1. Abortion, Birth, and Pregnancy Rates

Table 3 presents the results of mandatory delay laws on abortion, birth, and pregnancy rates using state time-varying controls, state and year fixed effects, and state-specific linear trends.[13],[14] The effect of mandatory delay laws on abortion rates is negative and statistically significant. Mandatory delay laws are shown to reduce abortion rates by 5.88 %.[15] I also present the effects of mandatory delay laws on abortion rates using AGI data for all available years from 1976 to 2005. Using AGI data, I find mandatory delay laws reduce abortion rates by 5.06 %. Because the results are similar, the remainder of the analysis uses abortion data from the CDC.

Recall that theory suggests an ambiguous effect of mandatory delay laws on births and a negative effect on pregnancies. Table 3 shows mandatory delay laws increase the state-level birth rate by 0.81 %. The average birth rate for my sample is 66.55 per 1,000 women of childbearing age. Therefore, mandatory delay laws increase the birth rate by

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[13] The estimates presented in Tables 3 and 4 are for logged outcomes.
[14] Table A1 presents the estimates for state time-varying controls on abortions, births, and pregnancies.
[15] I test the statistical significance of the joint inclusion of state-specific linear trends using an F-test. The F-statistic is 21.30 and 21.56 in abortion and birth models, respectively. Therefore, the joint inclusion of state-specific linear trends is significant.

0.54 births per 1,000 women. The table shows pregnancy rates are unaffected by mandatory delay laws.

Table 3 also presents the estimates for other restrictions on abortion rates. I find no effect of parental involvement laws on abortion rates for all women of child-bearing age. Several studies find a similar result for all women (Blank, George, & London, 1996; Bitler & Zavodny, 2001; Levine, 2002; Levine, 2003).[16] Similarly, I find no effect of restricting Medicaid funding on abortion rates. The literature is mixed on the effects of these laws. Bitler and Zavodny (2001) find some evidence of a positive effect of restricting Medicaid funds on the abortion rate while the research by Blank et al. (1996) and Levine et al. (1996) both find it reduces abortion rates. Differences in the results could be due to differences in data and time frames. Using CDC data, I find an increase in abortion rates from enjoining Medicaid funding, which is similar to that found in the research by Bitler and Zavodny (2001).

2.6.2. Counseling and Visit Requirements

Table 4 presents the effect of mandatory delay laws in conjunction with visit and counseling requirements on abortion and birth rates.[17] Model 1 shows in-person counseling has a greater effect than counseling obtained online or over the phone. This is

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[16] Levine (2002) and Levine (2003) both find evidence that parental involvement laws reduce the teenage abortion rate.

[17] Because mandatory delay laws do not affect pregnancy rates, as shown in the previous section, the estimates are not reported for the impact of mandatory delay laws by visit and counseling type on pregnancy rates. However, mandatory delay laws only significantly affect pregnancy rates in one model specification. Mandatory delay laws with online or over-the-phone counseling that does not include additional information reduce the pregnancy rate by 1.61 percent compared to states without mandatory delay laws.

consistent with predictions that in-person counseling has higher emotional and monetary costs. Specifically, mandatory delay laws that require in-person counseling reduce the abortion rate by 7.31 % and increase the birth rate by 2.05 % compared to states without mandatory delay laws. Mandatory delay laws with counseling that can be obtained by phone, fax, internet, or mail reduce the abortion rate by 5.16 % and have no significant effect on birth rates.

Model 2 presents the results of mandatory delay laws with and without additional information given during counseling. Mandatory delay laws with additional information reduce the abortion rate by 5.39 % while mandatory delay laws without the additional information during counseling reduce the abortion rate by 6.23 %. An F-statistic of 0.02 reveals there is no statistical difference between the effects of mandatory delay laws with and without the counseling information on abortion rates compared to states without mandatory delay laws. However, mandatory delay laws that include additional information at counseling increase the birth rate by 1.37 %. Mandatory delay laws without the additional information have no statistically significant effect on birth rates.

The effects of the information provided may depend on whether the information is given in-person or not. Model 3 of Table 4 includes counseling interaction terms for visit and information requirements. Mandatory delay laws that allow information to be obtained online or over-the-phone but do not include the additional counseling information reduce the abortion rate by 9.8 %. This is important because about 31 % of my sample with mandatory delay laws have this type of waiting period requirement. Model 3 also shows the most stringent form of mandatory delay laws, those which

require in-person counseling that includes additional information, has the greatest impact. This restrictive form of mandatory delay laws decreases the abortion rate by 24.9 % and increases the birth rate by 3.44 %.

2.6.3. Comparison to Previous Work

Bitler and Zavodny (2001), Levine (2002), and Medoff(2007) find no effect of mandatory delay laws on abortion rates. The results presented in this paper show mandatory delay laws decrease abortion rates by 5.9 %. To see whether results differ from previous research due to different controls or extending the time frame to include additional state variation in law changes, I re-estimate my model using data coinciding with years used in the research by Bitler and Zavodny (2001), Levine (2002), and Medoff (2007).[18] Table 5 presents these regression results. Using my data and model and the time frames used in their papers, I also find insignificant effects of the laws. Because my model produces insignificant results using a shorter time frame, I contend that the results in this paper differ from previous research due to the additional variation in the laws gained by using data through 2005.

Levine (2002) finds mandatory delay laws have no effect on birth or pregnancy rates in a model with state and year fixed effects and state-specific linear trends. In this paper, I find mandatory delay laws are associated with an increase in births, but they have no effect on pregnancies.

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[18] I estimate my model for years 1976 to 1997 to compare to Bitler and Zavodny's (2001) research. Their data begins in 1974; however, the first mandatory delay law doesn't pass until 1992.

2.7. CONCLUSION

The first mandatory delay law was passed in 1992 following the ruling in *Planned Parenthood of S.E. Pennsylvania v. Casey* (Levine, 2004). Significant controversy surrounds mandatory delay restrictions to abortion (Richardson & Nash, 2006; New, 2008) despite the fact that empirical research shows little effect of the laws on fertility (Bitler & Zavodny, 2001; Levine, 2002; Medoff, 2007). This paper analyzes the effects of mandatory delay laws on fertility outcomes and investigates the impact of counseling and visit requirements associated with the law.

Mandatory delay laws reduce the abortion rate by 5.88 %, increase the birth rate by 0.81 %, and have no effect on the pregnancy rate. The total number of abortions in the U.S. in 2005 was 870,000, which accounted for approximately 22 % of all pregnancies (Center for Disease Control, 2005; Guttmacher Institute, 2009). Forty-six percent of states had mandatory delay laws in 2005. If mandatory delay laws reduced the abortion rate in these states by 5.88 %, this would be a reduction of about 23,000 from (870,000*0.46*0.0588) abortions annually. If all states adopted mandatory delay laws, the effect would have been over 50,000 (870,000*0.0588) fewer abortions annually. During the time that mandatory delay laws were changing, the abortion rate fell from 18 to 13 abortions per 1,000 women of childbearing age. Mandatory delay laws could explain a reduction in the abortion rate of 0.48 (18*0.46*0.0588) abortions per 1,000 women. This explains less than 10 % (0.48/(18-13)) of the decline in the abortion rate.

The effect of mandatory delay laws varies by visit and counseling requirements given during the waiting period. Counseling received in person has a larger effect on

reducing abortion rates and increasing birth rates than counseling received via fax, phone, internet, or mail. The impact of mandatory delay laws with information that has been criticized as medically ungrounded does not differ from the effects of the laws without this information. The effect of counseling information hinges on whether the information is given in person or not. The most basic form of the law, which allows counseling to be received online or over-the-phone without additional information, reduces the abortion rate by almost 10 %. However, mandatory delay laws that require in-person counseling with additional information requirements have the largest impact. This restrictive form of the law reduces the abortion rates by almost 25 %. If all states in the U.S. adopted this form of mandatory delay law, then these results predict there would be about 216,000 (870,000*0.2486) fewer abortions annually.

States continue to pass mandatory delay laws and alter their counseling requirements. Recently, several states have adopted ultrasound examinations prior to having as abortion as part of their counseling requirements (Guttmacher Institute, 2009). The evidence presented in this paper shows the requirements associated with mandatory delay laws are an important determinant in the effect of the laws on abortion and birth rates.

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Table 1: Variable Definition and Data Source

Variable	Definition	Data Source
Abortion Restrictions		
Mandatory Delay	=1 if state has a mandatory waiting period for abortion	Bitler and Zavodny (2001), Levine (2004), and Americans United for Life; Defending Life 2008, Alan Guttmacher Institute: Monthly State Update Archives
Parental Involvement	=1 if state has a parental involvement law for minors	Bitler and Zavodny (2001), Levine (2004), Alan Guttmacher Institute
Medicaid Funding	=1 if state has Medicaid Funding Restrictions	Bitler and Zavodny (2001), Levine (2004), Alan Guttmacher Institute
Medicaid Funding Enjoined	=1 if state enjoined Medicaid Funding Restriction	Bitler and Zavodny (2001)
Border States with Mandatory Delay	Percent of border states that have mandatory delay laws	Created from Mandatory Delay
Border States with Parental Involvement	Percent of border states that have parental involvement laws	Created from Parental Involvement
Border States with Medicaid Funding	Percent of border states that have Medicaid funding laws	Created from Medicaid Funding
Border States with Medicaid Funding Enjoined	Percent of border states that have enjoined Medicaid funding laws	Created from Medicaid Funding Enjoined
Fertility Outcomes		
Abortion Rates	Abortions per 1,000 women aged 15 to 44	Johnston Archive: CDC and AGI sources
Birth Rates	Births per 1,000 women aged 15 to 44	U.S. Department of Health and Human Services: National Vital Statistics System
Pregnancy Rates	Sum of Abortions and Births per 1,000 women aged 15 to 44	Created from Abortion and Birth Rates
State Controls		
Hospital Bed Rate	Number of hospital beds per million people in the population	U.S. Census Bureau: Statistical Abstracts of the U.S., Health and Nutrition
Physician Rate	Number of physicians per 1,000 people in the population	U.S. Census Bureau: Statistical Abstracts of the U.S., Health and Nutrition
% Women State Legislators	Percent of state legislature that are women (including both parties)	Center for the American Woman and Politics
Marriage Rate	Marriage Rate per 1,000 population per state	U.S. Census Bureau: Statistical Abstracts of the U.S., Vital Statistics
Female Labor Force Participation Rate	Female Labor Force Population Rate	U.S. Census Bureau: Statistical Abstracts of the U.S., Labor Force, Employment
% Population (white)	Percent of female population that is white	U.S. Census Bureau: Current Population Survey
% Population (black)	Percent of female population that is black	U.S. Census Bureau: Current Population Survey
Population (women)	Population of women by ages 15-19, 20-24, 25-34, 35-44	U.S. Census Bureau: Current Population Survey
Unemployment Rate	State Unemployment Rates	U.S. Department of Labor: Bureau of Labor Statistics
Per Capita Income	Real per capita income per state	Regional Economic Information System
Maximum AFDC and TANF Benefits	Maximum AFDC and TANF Benefits paid to families of three with no income	U.S. Department of Health and Human Services: Administration for Children and Families, Welfare Rules Databook

Table 2: Descriptive Statistics for Full Sample and by Mandatory Delay Restrictions

Variable	Full Sample	With *Mandatory Delay*	Without *Mandatory Delay*
Restrictions			
Mandatory Delay	0.120	1	0
Mandatory Delay:			
Requires In-Person Counseling	0.034	0.287	0
Allows Counseling received via mail, phone, fax, or internet	0.086	0.714	0
Includes Additional Counseling Information	0.060	0.506	0
Does not include Additional Counseling Information	0.059	0.494	0
Other Restrictions			
Parental Involvement	0.344	0.966	0.259
Medicaid Funding	0.691	0.949	0.655
Medicaid Funding Enjoined	0.154	0.023	0.172
Percent of Border States with Mandatory Delay	0.119	0.348	0.087
Percent of Border States with Parental Involvement	0.346	0.697	0.298
Percent of Border States with Medicaid Funding	0.697	0.787	0.684
Percent of Border States with Medicaid Funding Enjoined	0.147	0.097	0.154
Fertility Outcomes			
Abortion Rate	16.510	10.528	17.327
Birth Rate	66.547	66.549	66.546
Pregnancy Rate	83.057	77.077	83.873

Notes: Sample: 1976 – 2005, n = 1481 (unbalanced panel). Abortion, birth, and pregnancy rates are numbers of abortions, births, and pregnancies per 1,000 females aged 15 to 44 in each year and state. Probability of Abortion is the abortion rate divided by the pregnancy rate by year and state. Probability of Birth is the birth rate divided by the pregnancy rate for each year and state.

Table 3: Abortion Access Restrictions on State Abortion, Birth, and Pregnancy Rates

Variable	Abortion Rate (CDC data)	Abortion Rate (AGI data)	Birth Rate	Pregnancy Rate
Mandatory Delay	-0.0588***	-0.0506**	0.0081*	-0.0020
	(0.0214)	(0.0195)	(0.0044)	(0.0054)
Parental Involvement	0.0223	-0.0061	0.0066*	0.0034
	(0.0165)	(0.0133)	(0.0034)	(0.0041)
Medicaid Funding	0.0079	-0.0204	0.0121***	0.0098*
	(0.0212)	(0.0160)	(0.0044)	(0.0053)
Medicaid Funding	0.0514**	0.0207	0.0107**	0.0216***
Enjoined	(0.0248)	(0.0175)	(0.0052)	(0.0062)
Adjusted R²	0.9125	0.9617	0.9273	0.9302

Notes: Sample: 1976 – 2005, n = 1481 (unbalanced panel). Standard errors are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively. Regressions are weighted using female population per state and year. Regressions include state time-varying controls, state and year fixed effects and state linear time trends. The abortion rate is the number of abortions per 1,000 women aged 15 to 44 in each year and state. Birth rate is the number of births per 1,000 women aged 15 to 44 by year and state. Pregnancy rate is the sum of births and abortions per 1,000 women aged 15 to 44 by year and state. The outcome variable is logged. Models include time-varying state controls listed in Table 1. The estimates for time-varying state controls are presented in Table A1.

Table 4: Effects of Mandatory Delay Laws by Counseling and Visit Requirements

Mandatory Delay Law includes:	Abortion Rate	Birth Rate
Model 1:		
In-Person Counseling	-0.0731**	0.0205***
(2 Visits)	(0.0347)	(0.0072)
Online or Over-the-Phone Counseling (1 Visit)	-0.0516**	0.0019
	(0.0254)	(0.0053)
Model 2:		
Includes Additional Information at Counseling	-0.0539*	0.0137**
	(0.0318)	(0.0066)
Does not include Additional Information at	-0.0623**	0.0041
Counseling	(0.0272)	(0.0056)
Model 3:		
In-Person Counseling includes Additional	-0.2486***	0.0344**
Information	(0.0751)	(0.0156)
In-Person Counseling does not include Additional	-0.0337	0.0102
Information	(0.0328)	(0.0068)
Online or Over-the-Phone Counseling includes	-0.0262	0.0173**
Additional Information	(0.0387)	(0.0080)
Online or Over-the-Phone Counseling does not	-0.0981***	-0.0076
include Additional Information	(0.0370)	(0.0077)

Notes: Sample: 1976 – 2005, n = 1481 (unbalanced panel). Estimates are reported as marginal effects. Standard errors are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively. Regressions are weighted using female population per state and year. Regressions include state time-varying controls, state and year fixed effects and state linear time trends. The estimates in each model are relative to states without mandatory delay laws. The estimates for time-varying state controls are listed in Table 1.

Table 5: Effects of Mandatory Delay Laws on Abortion Rates by Different Time Frames

Variable	Bitler and Zavodny (2001) 1976-1997	Levine (2002) 1985-1996	Medoff (2007) 1982,1992,2000
Mandatory Delay	0.0107 (0.0394)	0.0602 (0.0437)	0.1010 (0.1496)
Adjusted R^2	0.9182	0.9447	0.9182

Notes: Standard errors are in parentheses. Regressions are weighted using female population per state and year. Regressions include state time-varying controls, state and year fixed effects and state linear time trends. The abortion rate is the number of abortions per 1,000 women aged 15 to 44 in each year and state. Models include time-varying state controls listed in Table 1.
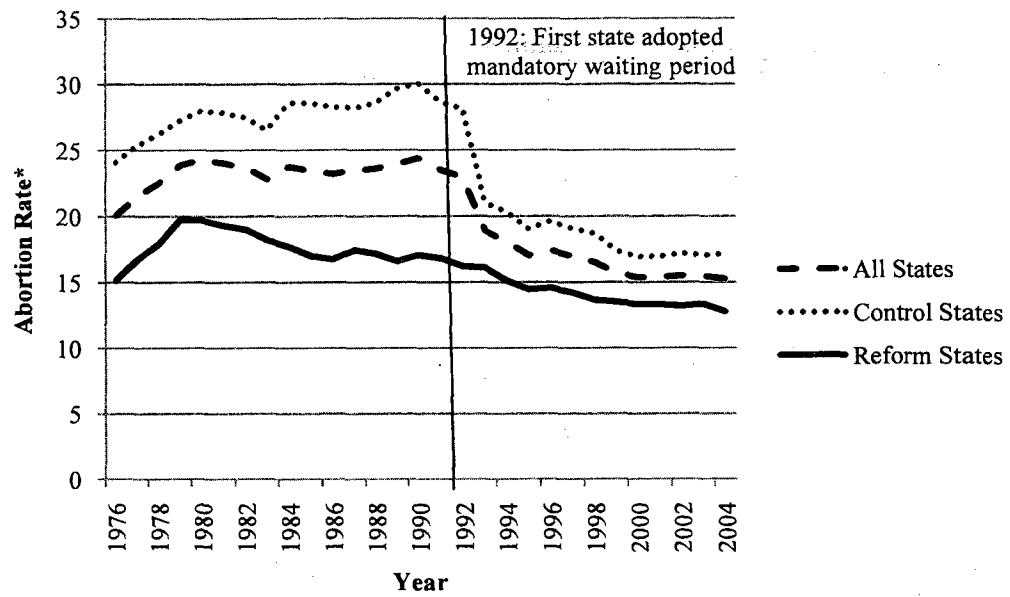
*Number of abortions per 1,000 females aged 15 to 44 in the population

Figure 1: Abortion Rates

APPENDIX

Table A1: Estimates for the Effects of State-Level Controls on Fertility Outcomes

State Controls	Abortion Rate (CDC data)	Abortion Rate (AGI data)	Birth Rate	Pregnancy Rate
Border States with Mandatory Delay	-0.0026 (0.0468)	-0.0022 (0.0389)	0.0050 (0.0089)	0.0005 (0.0122)
Border States with Parental Involvement	0.0227 (0.0356)	0.0245 (0.0284)	-0.0242*** (0.0068)	-0.0080 (0.0093)
Border States with Medicaid Funding	0.0228 (0.0446)	0.1138*** (0.0318)	-0.0379*** (0.0085)	-0.0309*** (0.0116)
Border States with Medicaid Enjoined	0.0667 (0.0483)	0.0550* (0.0326)	0.0198** (0.0092)	0.0333*** (0.0125)
Population of Black Women	-21.244*** (3.2000)	-7.7836*** (2.4800)***	-4.5842*** (0.6110)	-8.9557*** (0.8320)
Population of White Women	-26.797*** (2.4400)	-6.3777 (1.8700)	-6.4815*** (0.4650)	-12.105*** (0.6330)
% Population of Women aged 20 to 24	-1.9197 (1.6800)	-2.4184* (1.4300)	3.3190*** (0.3200)	2.6640*** (0.4360)
% Population of Women aged 25 to 34	-2.5275** (0.9990)	-1.0586 (0.8280)	3.1496*** (0.1900)	2.1673*** (0.2590)
% Population of Women aged 35 to 44	-2.2650 (1.6900)	-1.6331 (1.4100)	1.0527*** (0.3220)	0.5095 (0.4390)
Unemployment Rate	-0.0063 (0.0055)	0.0133*** (0.0042)	-0.0040*** (0.0010)	-0.0072*** (0.0014)
Per Capita Income	1.4e-03 (1.0e-05)	2.7e-05*** (8.0e-06)	8.1e-06*** (1.9e-06)	1.0e-05 (2.6e-06)
Maximum AFDC and TANF Benefits	0.0012*** (0.0001)	0.0002*** (0.0001)	0.0001*** (2.1e-06)	0.0004*** (2.8e-05)
Physician Rate	0.0001* (0.0001)	-0.0001 (0.0001)	3.2e-06 (9.6e-06)	2.4e-05* (1.3e-05)
Hospital Bed Rate	3.25e-05 (4.7e-05)	-3.8e-05 (4.0e-05)	-4.0e-05*** (8.9e-06)	-1.9e-05 (1.2e-05)
Female Labor Force Participation	0.0038 (0.0038)	-0.0064** (0.0030)	0.0004 (0.0007)	0.0017* (0.0010)
% Females in State Legislature	1.0e-06 (5.8e-06)	0.0010 (0.0013)	5.0e-08 (1.1e-06)	1.3e-07 (1.5e-06)
Marriage Rate	0.0002 (0.0008)	0.0001 (0.0006)	0.0002 (0.0002)	0.0001 (0.0002)

CHAPTER III

THE EFFECTS OF MANDATORY DELAY LAWS ON WOMEN AND CHILDREN

3.1. INTRODUCTION

In 1992, the U.S. Supreme Court overturned their previous position on waiting periods for abortion. *Planned Parenthood of S.E. Pennsylvania v. Casey* (1992) legalized delay laws so long as they did not impose substantial burden on women (Levine, 2004). Following this ruling, 24 states adopted mandatory delay laws that require women to receive information from an abortion provider and then wait a specified period of time before obtaining an abortion. These laws have been shown to decrease abortion rates, increase birth rates, but have no effect on the overall number of pregnancies (Allen, Impact of Mandatory Delay Laws on Fertility Outcomes, 2010). Kalist and Molinari (2006) find a 10 % decrease in the abortion rate increases the portion of unwanted pregnancies that result in live births by 3.5 %.[1,2] Allen (2010) finds mandatory delay laws decrease the abortion rate by almost 6 %. Using Kalist and Molinari's (2006) estimates, a 6 % reduction in the abortion rate may lead to a 2.1 % rise in the number of births from unwanted pregnancies.

These previous studies focused on pre-delivery measures, but the impact of mandatory delay laws may extend beyond the decision to have an abortion or give birth. For instance, the rise in births from unwanted pregnancies could lead to more adoptions or, possibly, more child maltreatment. Mandatory delay laws may also impact women

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[1] A one-to-one relationship exists between the reduction in the number of abortions and the increase in the number of births from unwanted pregnancies. Although all abortions are unwanted pregnancies, not all unwanted pregnancies result in abortion. Thus, there is not a one-to-one relationship in the percent decline in abortions and the percent rise in births from unwanted pregnancies.

[2] Kalist and Molinari (2006) use self-reported survey data. In the survey, women stated whether a pregnancy was unwanted at any time during the pregnancy.

beyond changes to fertility. Proponents of mandatory delay laws claim the laws allow women to make a more informed decision. If they are correct, then the counseling and waiting period required by the law could decrease the stress that women experience (National Right to Life Committee, 2010). However, opponents disagree. They claim women are sure of their decision to have an abortion prior to contacting the provider (Wind, 2009). Thus, they claim the laws only serve to prolong a difficult process, which could further increase the emotional distress of an unwanted pregnancy (Wind, 2009). Some of the counseling requirements associated with the laws have also been criticized as biased or medically ungrounded and may be another source of stress for women (Richardson & Nash, 2006).

Given these conflicting assertions, this research empirically tests whether mandatory delay laws have secondary consequences on women and children. Specifically, I test whether mandatory delay laws impact children by analyzing their effect on child maltreatment and adoption. I also test whether mandatory delay laws and their associated requirements affect women's mental health. I use state rates for female suicides and the mean number of mentally unhealthy days per year as proxies for female mental health.

My results support several conclusions regarding mandatory delay laws. I find mandatory delay laws for abortion increase the number of public adoptions by 14 %. I also find the laws have no statistically significant affect on child maltreatment. Further, my results suggest mandatory delay laws may decrease the stress of an unwanted pregnancy because the laws are associated with a decrease in the mean number of

mentally unhealthy days per year for women (3.8 days). However, the laws do not affect female suicide rates, a measure of more drastic effects on mental health.

3.2. BACKGROUND

Mandatory waiting periods were ruled unconstitutional in the 1983 U.S. Supreme Court decision in *City of Akron v. Akron Center for Reproductive Health* (Levine, 2002). Waiting periods remained illegal until 1992 when the U.S. Supreme Court overturned the ruling in *Planned Parenthood of S.E. Pennsylvania v. Casey* (1992) (Levine, 2004). Mississippi was the first state to pass a mandatory delay law in late 1992. Between 1992 and 2010, 23 additional states passed laws requiring women to receive counseling information and wait a specified period of time before obtaining an abortion (Levine, 2004; Guttmacher Institute, 2009). Table 1 presents the year each state passed a mandatory delay law, if at all.

The waiting period and information requirements for mandatory delay laws vary across states. Some states require in-person counseling while others allow the information to be received via mail, phone, fax, or online as long as the information is obtained in advance (Guttmacher Institute, 2009). In-person counseling requires two visits to the provider, which could increase emotional and financial costs (Joyce, Henshaw, Dennis, Finer, & Blanchard, 2009). The type of information provided to women during the waiting period also varies across states (Guttmacher Institute, 2009). Some information given during counseling has been criticized as biased including information that states abortion is linked with breast cancer, that the fetus can feel pain, and that abortion is

linked with post-traumatic stress disorder (Richardson & Nash, 2006). Table 1 also presents state counseling and visit requirements as of 2005.

3.3. EFFECT ON CHILDREN

Mandatory delay laws have been shown to change the fertility decisions of women. Specifically, the laws are shown to decrease the abortion rate and increase the birth rate (Allen, 2010). The births that are impacted by mandatory delay laws are those from unwanted pregnancies because the laws only affect women who are seeking an abortion. An increase in births from unwanted pregnancies could lead to an increase in the number of children adopted. Mandatory delay laws could also lead to an increase in child maltreatment. If the children from unwanted pregnancies are not given up for adoption, then they could be more at risk for abuse within the home.

3.3.1. Adoption

Adoption has been linked with abortion availability. For example, Bitler and Zavodny (2002) find repealing laws that restricted abortion access prior to *Roe v. Wade* led to a 34 to 37 % decline in the adoption of white children. Gennetian (1999) finds that a 10 % increase in abortion providers reduced the ratio of infants relinquished for adoption by up to 13 %.

One way this paper extends the literature is by analyzing the impact of mandatory delay laws on adoption. Although previous research has not examined the effect of mandatory delay laws on adoption rates, research exists on the effects of other laws that restrict abortion access, such as parental involvement and Medicaid funding laws, on

adoption. Parental involvement laws, surprisingly, are found to decrease the number of infants relinquished for adoption (Gennetian, 1999; Medoff, 2008). Medicaid funding restrictions are also found to significantly decrease the number of infants relinquished for adoption in some estimations (Gennetian, 1999) but have no effect in others (Medoff, 2008).

I expect mandatory delay laws to have a greater impact on adoption than other restrictions because most states provide information on adoption during the counseling and waiting period associated with mandatory delay laws (Althaus & Henshaw, 1994; Joyce & Kaestner, 2001; Center for Reproductive Rights, 2009; NARAL Pro-Choice America, 2010).[3] If adoption is explicitly stated as an alternative to abortion during the counseling, then women who choose not to have the abortion may be more likely to give their child up for adoption compared to women who are not given this information. Also, I expect mandatory delay laws to have the opposite effect on adoption as parental involvement laws and Medicaid funding restrictions. The decrease in adoption rates from Medicaid funding and parental involvement laws has been attributed to a decrease in pregnancy rates (Gennetian, 1999; Medoff, 2008). However, mandatory delay laws do not appear to significantly affect pregnancy rates (Allen, 2010). Thus, I expect mandatory delay laws to increase adoption rates due to the increase in births from unwanted pregnancies.

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[3] NARAL Pro-Choice America provides state profiles of abortion laws. In fact, their profiles report that all states with mandatory delay laws, except Missouri, include information on adoption alternatives and/or agencies as part of the state-mandated counseling material (NARAL Pro-Choice America, 2010).

3.3.2. Child Maltreatment

Child maltreatment has been linked with both abortion legalization and restrictions to abortion access. Studies show abortion legalization led to a decrease in infant homicide by as much as 8.5 % and a decline in reports of child abuse and neglect by 10 % (Bitler & Zavodny, 2002; Sorenson, Wieve, & Berk, 2002; Bitler & Zavodny, 2004). Restrictions to abortion access have also been studied in conjunction with child maltreatment; however, the findings are less consistent. Bitler and Zavodny (2002) find no consistent effect of mandatory laws on child abuse reports. However, Sen (2007) finds that mandatory delay laws increased infant homicide deaths by as much as 30 % and unintentional deaths of infants by 9 %. Kalist and Molinari (2006) claim eliminating restrictions to abortion access could reduce infant homicide, but an additional 20,000 abortions would only reduce the number of infant homicides by approximately one infant.

This paper builds on the child maltreatment literature by estimating the effects of mandatory delay laws on child maltreatment over a period with more law changes than what is found in the literature. Bitler and Zavodny (2002) explicitly state their estimates of mandatory waiting period laws on child abuse should be considered with caution because only a few states had adopted the laws by the end of their sample period, 1996. My sample extends to 2005, which captures twice the number of mandatory delay law changes as Bitler and Zavodny's (2002) research on child abuse. I also estimate the effects of mandatory delay laws on infant deaths from violence using a sample period

containing 20 % more state law changes than the period examined in Sen's (2007) research.

3.4. EFFECT ON WOMEN

Mandatory waiting period restrictions could also have an impact on the emotional state of women. However, the effect is ambiguous. Mandatory waiting period restrictions could lower the stress of having an abortion by providing women with materials that aid them in making a more informed decision. However, if the counseling received is biased, as some opponents claim, then it could increase the mental anguish of an unwanted pregnancy.

The literature on the effects of abortion on the mental health of females is mixed.[4] Most studies rely on self-reported surveys and longitudinal analyses of depression and anxiety. The analysis of mental health and abortion is complicated by unobserved characteristics such as prior emotional state. Some studies find women who have abortions have higher rates of depression, anxiety, suicidal thoughts, and substance abuse (Broen, Moum, Bodtker, & Ekeberg, 2005; Fergusson, Horwood, & Ridder, 2006). However, other research reports that unintended pregnancies are the cause of increased anxiety and suicidal ideation (Fergusson, Horwood, & Ridder, 2006; Molina & Duarte, 2006), and there is little to no difference in the emotional well-being of women who have an abortion or give birth for unintended pregnancies (Kero, Hogberg, & Lalos, 2004; Steinberg & Russo, 2008). Rearden et al. (2002) examines 173,279 California Medicaid

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[4] See Charles et al. (2008) for a comprehensive review of the literature on abortion and mental health.

records and finds women who have an abortion had a higher risk of death from all causes, as well as suicide, than women who carried a pregnancy to term (Reardon, Ney, Scheuren, Cougle, Coleman, & Strahan, 2002). To my knowledge, only the research by Klick (2006) analyzes the impact of a state restriction to abortion on suicide rates. Klick (2006) finds mandatory waiting periods reduce female suicide rates between 10 and 30 %.

Two reasons exist for the lack of research examining the relationship between abortion restrictions and mental health. First, the impact of state abortion restrictions on female mental health is difficult to estimate because mental health surveys, such as the National Health Interview Survey and the Behavioral Risk Factor Surveillance System, do not report state identifiers to maintain the anonymity of respondents. Without state identifiers, I am unable to estimate the impact of mandatory delay laws on an individual's mental health status. Instead of relying on mental health surveys, I use the state rate of female suicides and the state average number of mentally unhealthy days to proxy for women's emotional state. Second, most abortion restrictions only affect female mental health by changing the decision to have an abortion or give birth. However, mandatory delay laws differ from other abortion restrictions because the counseling and waiting period associated with the law could directly affect the mental anguish of unwanted pregnancies (Klick 2006). Thus, the effects of mandatory delay laws on female mental health could vary based on the type of counseling given and the visitation requirement. For instance, requiring in-person counseling could be more stressful than allowing information to be received online or over-the-phone. However, if the counseling is

informative and helps a woman feel she has made a more informed decision, then she may experience less stress.

In my research, I am able to provide a clearer picture of the effects of these laws on female mental health. I analyze the impact of delay laws on mental health using the mean number of mentally unhealthy days per year, which has not been estimated in the literature. Second, I test whether controversial counseling laws have an effect on female mental health. Third, unlike Klick's (2006) research that uses women aged 25 to 64, I analyze female suicide rates for women aged 15 to 44 in the population because the laws should only affect the mental health of women who are of child-bearing age. Further, by extending the data to 2005, my analysis of mandatory delay laws on suicide rates includes almost double the number of law changes than the research by Klick (2006).

3.5. DATA

Data on mandatory delay laws were obtained from Bitler and Zavodny (2001), Levine (2004), Americans United for Life (2008), and the Alan Guttmacher Institute. Visit and counseling requirements are obtained from the Alan Guttmacher Institute's state policy reports. Table 2 provides a list of the outcome and explanatory variables, and table A1 in the appendix provides a list of state-level control variables used in this analysis.

State adoption rates from the U.S. Department of Health and Human Services are obtained for the years 1995 to 2005. Adoption outcomes are defined as the number of adoptions per 1,000 women of childbearing age and the number of adoptions per 1,000

pregnancies.[5] Data is restricted to public adoptions because annual data on private adoptions at the state level is unavailable. The National Counsel for Adoption reports that public adoptions accounted for approximately 56.5 % of U.S. adoptions in 2002 (National Council for Adoption, 2007). Rates of maltreatment are defined as the number of substantiated child abuse investigations per 1,000 children, the number of fatalities from violence for infants aged 0 to 4 per 100,000 infants, and the number of homicides for children under 18 per 100,000 children.[6] The number of substantiated child abuse investigations and child homicides is collected from the U.S. Department of Health and Human Services for years 1990 to 2005. The number of infant fatalities from violence is collected from the CDC's WISQARS Injury Mortality Reports for years 1990 to 2005.

Female suicide rates are obtained from the CDC Compressed Mortality Files for 1990 to 2005. Female suicide rates are defined as female deaths from suicide per 100,000 women aged 15 to 44 in the population.[7] The mean number of mentally unhealthy days per year among females is collected from the CDC's Health-Related Quality of Life Data.[8] The CDC asked surveyed respondents to report the number of mentally unhealthy

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[5] The pregnancy rate is defined as the sum of abortions and births per state and year as in the research by Medoff (1993) and Levine et al. (1996).

[6] I use child abuse, infant fatalities from violence and child homicides to proxy for parental maltreatment because parents are responsible for nearly 70 percent of child abuse or neglect fatalities (U.S. Department of Health and Human Services, 2009).

[7] A major distinction in classification of causes of death occurred between 1998 and 1999 in the CDC Compressed Mortality Files. However, a study conducted by the CDC National Vital Statistics Report states the differences between the two classifications "are small enough that it can be concluded that the revision does not substantially affect mortality patterns for suicide or homicide" (Anderson, Minino, Hoyert, & Rosenberg, 2001). A comparability ratio of one means the two classifications are identical. This ratio, in the CDC report, is 0.9962 for suicides (Anderson, Minino, Hoyert, & Rosenberg, 2001). The report states that "...Only a few deaths were not classified consistently in these categories. It is not yet clear whether these inconsistencies are real or whether they are records that were unable to be identified as pending amendment" (Anderson, Minino, Hoyert, & Rosenberg, 2001).

[8] The CDC's Health-Related Quality of Life Data respondents are females who are 18 years of age or older. I am unable to partition the data by age.

days they had in the past 30 days.[9] The CDC does not report state identifiers at the individual level; however, they report the mean values for each year and state.

State-level controls include the number of hospital beds per million people in the population, the number of physicians per 1,000 people in the population, the marriage rate, the percent of female legislators, the female labor force population, the percent of the female population in various age groups, the percent of the female population by race, the unemployment rate, maximum benefits for Aid to Families with Dependent Children (AFDC) and Temporary Aid to Needy Families (TANF), and per capita income.[10] I also include male suicide rates in the estimation of female suicide rates to control for anything that could be correlated with suicide in general.[11]

Table 3 provides summary statistics for the key explanatory variables. Column 2 presents the summary statistics for states with mandatory delay restrictions. Mandatory delay laws were in place for 22.6 % of the observations in my sample from 1990 to 2005. Only 28.7 % of the state observations in my sample require in-person counseling compared to more than 70 % that allow counseling to be received via mail, phone, fax, or internet. The simple averages show about half of the states (51 %) with mandatory delay laws include at least one additional information requirement during counseling.

Table 4 presents the summary statistics for key outcome variables at the beginning and end of the samples for comparison and reform states. A state is in the comparison group if it does not adopt a mandatory delay law at any time during my

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[9] I multiply the monthly average by twelve to report the number of mentally unhealthy days each year.
[10] Appendix tables A2 and A3 present the estimates of state-level controls for all regressions.
[11] It is possible that males also experience increased stress from abortion and abortion restrictions. The results are not materially different when I exclude male suicide rates from estimations.

sample period. A state is in the reform group if it adopts a mandatory delay law at any time in the sample period. The averages show the rate of substantiated child abuse reports declined from 1990 to 2005 for both reform and comparison states. However, the decline in child abuse reports was greater for comparison states (34 %) compared to reform states (16 %). Infant and child fatality rates declined for comparison states (6 % and 25 %, respectively), but they rose for reform states (18 % and 17 %, respectively) over the same time period. The rate of adoptions by public agencies increased dramatically over the sample period. However, the adoption rate increased more for reform states (139 %) than for comparison states (125 %).[12] The rate of female suicides increased by 4.5 % for comparison groups but fell by less than 1 % for reform states. The mean number of mentally unhealthy days per year increased more for reform states (22 %) than for comparison states (16 %).

3.6. ESTIMATION METHODOLOGY

I use the timing of laws across states to capture the effect of mandatory delay restrictions on adoption, child maltreatment, and female mental health. The following equation is used to capture the effect of mandatory delay restrictions to abortion access:

$$Y_{st} = Delay_{st}\beta_1 + R_{st}\beta_2 + X_{st}\beta_3 + \gamma_s + \gamma_t + trend * \gamma_s + \epsilon_{st}$$

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[12] At least part of the large increase in the adoption rate can be attributed to the rise in the number of adoptions that were performed by public agencies. The National Council for Adoption reported that the number of adoptions by public agencies rose from 40 % in 1992 to 56 % in 2002 (2007).

where s indexes states and t indexes years.[13] Y represents the logged values of state-level child maltreatment, adoption, female suicide rates, and average mentally unhealthy days per year. *Delay* is an indicator variable for if a state has a mandatory delay restriction. R is a vector of indicator variables for other restrictions such as parental consent and Medicaid funding restrictions, X is a vector of state time-varying controls, γ_s is a vector of state fixed effects, γ_t is a vector of year fixed effects, and $\gamma_s*trend$ is a vector of state-specific linear time trends. Fixed effects are included to account for state characteristics that do not change over time. State-specific time trends are included to capture anything specific to the state that changes over time that could be correlated with adoption, child maltreatment, or female mental health.

3.7. RESULTS

3.7.1. Child Outcomes

Table 5 reports the results for the effect of mandatory delay laws and counseling requirements on public adoption rates. I use a one-year lag of the law because there is a difference in the time between when a pregnant woman is exposed to a delay law and the time at which she gives the child up for adoption. Women are most likely subject to the law early in their pregnancy since most abortions occur within 9 weeks of gestation (Alan Guttmacher Institute, 2009). However, adoption does not occur until the pregnancy is

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[13] Some previous studies do not weight the outcome variable by the population. Instead, they estimate the effect of mandatory delay laws on the number of cases and control for population changes (Sen, 2007). In order to show that my results do not differ because of the way the outcome variable is specified, I estimate my analysis both ways. Appendix tables A4-A6 report the results for estimations of mandatory delay laws on the number of adoptions, suicides, child abuse, child fatalities and infant fatalities controlling for the population of females, children, and infants. The results from both estimation methods are similar.

full-term and the child is born. The results from model 1 show that mandatory delay laws increase the adoption rate by more than 14 %.[14] Models 2 and 3 show the effects of mandatory delay laws on adoption rates vary by the counseling and visit requirements associated with the laws. Mandatory delay laws that allow counseling to be obtained online or over-the phone increase the adoption rate by over 14.5%. Mandatory delay laws that do not include additional counseling information during the waiting period increase the adoption rate by about 14 %. However, mandatory delay laws that require in-person counseling and those that include additional counseling information do not have a statistically significant effect on adoption rates.

With approximately 51,000 public adoptions annually, a 14% increase in the adoption rate from mandatory delay laws would increase the number of public adoptions by approximately 7,200 annually (51,341*0.14). Public adoptions account for 56.5% of all adoptions. Thus, the number total number of adoptions are approximately 90,869 from (51,341/0.565) and the number of private adoptions are 39,528 (90,869*0.435). If mandatory delay laws affect private adoptions in the same way, then the number of private adoptions would increase by 5,500 (39,528*0.14) and the number of overall adoptions would increase by 12,700 (7,200 + 5,500) annually if all states adopted the law.[15]

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[14] I estimate the effect of mandatory delay laws on adoptions per 1,000 women and per 1,000 pregnancies because both specifications are found in the literature (Gennetian, 1999; Bitler & Zavodny, 2002; Medoff, 2008). I present both outcomes to show that changes in the female population and in the number of pregnancies are not driving the results.
[15] The effect for the 46 % of states with mandatory delay laws would be an increase of about 3,300 public adoptions (7,200*0.46), 2,600 private adoptions (5,600*0.46), and 5,900 total adoptions (12,700*0.46).

Table 6 presents the results of mandatory waiting period laws on child maltreatment. My results show mandatory waiting periods do not have a statistically significant impact on child abuse. Also, the laws do not affect either infant fatalities from violence or child homicides. Further, the counseling and visit requirements associated with mandatory delay laws do not have a statistically significant impact on either measure of child maltreatment.

3.7.2. Female Outcomes

Table 7 presents the effects of mandatory waiting period restrictions on female mental health. Mandatory delay laws do not have a statistically significant effect on female suicide rates. This result differs from the findings by Klick (2006) who estimates that mandatory waiting periods reduce female suicide rates by as much as 30 % among women aged 25 to 64. Several reasons exist for the differences in results. First, I use female suicide rates for women between the ages of 15 to 44. I believe this age group is more appropriate because this coincides with the childbearing age of women. Second, I use seven additional years of data during which time eleven states adopted mandatory delay laws. Finally, I use state-specific time trends, which are jointly significant. I am able to replicate a statistically significant reduction in suicide rates from mandatory delay laws by re-estimating my model using years of data coinciding to Klick's (2006) paper without including state-specific time trends.

Female mental health could be harmed by mandatory delay laws without females committing suicide because suicide is an extreme measure of mental health. Therefore, I

also estimate the impact of mandatory delay laws on female mental health using the number of mentally unhealthy days per year. This variable shows the average number of mentally unhealthy days of surveyed female respondents in each year and state. Mandatory delay laws decrease the number of mentally unhealthy days by 6.3 %. The average number of mentally unhealthy days is 43.20 a year. Thus, mandatory delay laws reduce the number of mentally unhealthy days by about 2.6 days per year (or 5.2 hours per month).

Table 7 also presents the effects of mandatory delay laws on female mental health by the visitation and information requirements associated with the laws. Female suicide rates are not significantly affected by mandatory delay laws, regardless of the visitation or information requirements. However, the effects of mandatory delay laws on mentally unhealthy days per year differ by counseling type. Mandatory delay laws that allow counseling to be received online or over-the-phone and those that do not include additional information reduce the number of mentally unhealthy days by 6.6 % (or 2.9 days a year) and 8.7 % (or 2.9 days a year), respectively. Mandatory delay laws that require in-person counseling and those that include additional counseling information do not significantly affect female mental health. Perhaps, the extra visit and the additional information increase stress, as opponents of the law claim, which counter-balances the reduction in stress from the waiting period.

The effect of the counseling and visit requirements on female mental health is consistent with the stated intent of mandatory delay laws. Recall that proponents of the law claim waiting periods decrease stress by allowing women to make a more informed

decision regarding abortion. The overall effect of mandatory delay laws is consistent with this view as the laws are associated with a reduction in the number of mentally unhealthy days.

3.8. CONCLUSION

This paper analyzes the impact of mandatory delay laws on women and children. Mandatory delay laws have been shown to decrease the abortion rate and increase the birth rate (Allen, 2010). A decrease in the abortion rate is found to increase the number of births from unwanted pregnancies, which could affect the number of children adopted or possibly abused (Kalist & Molinari, 2006). The laws could also affect women from changes in fertility and changes in emotional stress from the waiting period and counseling information.

Mandatory delay laws do not affect child maltreatment. However, mandatory delay laws do increase adoptions. Specifically, this research finds mandatory delay laws that allow counseling to be received online or over-the-phone and do not include additional counseling information increase the number of public adoptions by about 14 %. In 2005, there were 51,341 public adoptions and an estimated 39,528 private adoptions. Using my results, mandatory delay laws would increase the number of public adoptions by about 7,200 adoptions annually (51,341*0.14) if all states adopted the laws. If mandatory delay laws affect private adoptions in the same way they affect public adoptions and all states adopted mandatory delay laws, then the number of private

adoptions would increase by 5,500 (39,528*0.14) and the overall number of adoptions would increase by almost 12,700 (5,500+ 7,200).

Contrary to previous research, which finds between a 10 and 30 % effect of the laws on female suicide rates (Klick, 2006), I find no effect of mandatory delay laws or associated counseling requirements on female suicide rates. Although suicides are unaffected by mandatory delay laws, it may be that suicide is too strong of a measure for female mental health. Therefore, I also use the state average for female mentally unhealthy days to analyze the impact of delay laws on female mental health. I find that mandatory delay laws reduce the number of mentally unhealthy days by 2.6 to 3.8 days per year, depending on the type of counseling given. A decrease in stress is consistent with the stated intent of the laws that counseling and waiting periods allow women to make a more informed decision regarding abortion and reduce the overall stress of the decision.

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Table 1: Summary of State Mandatory Delay Laws as of 2005

State	Year Passed	Online-Counseling*	Controversial Information**	State	Year Passed	Online-Counseling*	Controversial Information**
AL	2002	X		MT	--		
AK	--			NE	1993	X	X
AZ	--			NV	--		
AR	2001	X	X	NH	--		
CA	--			NJ	--		
CO	--			NM	--		
CT	--			NY	--		
DE	--			NC	--		
FL	--			ND	1994	X	
GA	2005	X	X	OH	1994		
HI	--			OK	2005	X	X
ID	1995	X		OR	--		
IL	--			PA	1994	X	
IN	1997			RI	--		
IA	--			SC	1995	X	X
KS	1992	X	X	SD	1994	X	X
KY	2000	X		TN	--		
LA	1995			TX	2003	X	X
ME	--			UT	--		
MD	--			VT	--		
MA	--			VA	2001	X	
MI	1999	X	X	WA	--		
MN	2003	X	X	WV	2003	X	X
MS	1992		X	WI	1996		
MO	2005	X		WY	--		

Notes: X represents whether the state has the counseling requirement in 2005. *Women may receive information online, over-the-phone, fax, or mail as long as it is received in advance. **Controversial information includes stating that a link exists between breast cancer and abortion, that the fetus can feel pain, and that abortion is linked to depression and guilt. -- indicates state does not have a mandatory delay law by 2005.

Table 2: Variable Definition and Data Source for Mandatory Delay Laws and Outcome Variables

Variable	Definition	Data Source
Abortion Restriction		
Mandatory Delay	=1 if state has a mandatory waiting period for abortion	Bitler and Zavodny (2001), Levine (2004), and Americans United for Life; Defending Life 2008, Alan Guttmacher Institute: State Policies in Brief
Outcomes		
Adoption Rates	Adoptions per 1,000 women aged 15 to 44 and per 1,000 pregnancy	U.S. Department of Health and Human Services: Administration for Children and Families, Adoption and Foster Care Statistics
Child Abuse Claims	Child Abuse Investigations per 1,000 children in the population	U.S. Department of Health and Human Services: Administration for Children and Families, National Child Abuse and Neglect Data System (NCANDS)
Child Homicide Rates	Child Homicides per 100,000 children in the population	U.S. Department of Health and Human Services: Administration for Children and Families, National Child Abuse and Neglect Data System (NCANDS)
Infant Fatality Rates	Infant Fatalities per 100,000 infants in the population	Center for Disease Control (CDC): Compressed Mortality Files WISQAR
Female Suicide Rates	Female deaths from suicide per 100,000 women aged 15 to 44	Center for Disease Control (CDC): Compressed Mortality Files
Mentally Unhealthy Days	Mean number of mentally unhealthy days per year	Center for Disease Control (CDC): Health-Related Quality of Life Data

Table 3: Descriptive Statistics for Full Sample and by Mandatory Delay Restrictions

Variable	Full Sample	States With *Mandatory Delay*
Restrictions		
Mandatory Delay	0.226	1
Mandatory Delay:		
Requires In-Person Counseling	0.065	0.287
Allows Counseling received via mail, phone, fax, or internet	0.161	0.714
Includes Additional Counseling Information	0.114	0.506
Does not include Additional Counseling Information	0.112	0.494

Notes: Sample: 1990 – 2005, (unbalanced panel).

Table 4: Descriptive Statistics of Outcome Variables for Comparison and Reform States

Variable	Mean Beginning of Sample	Mean End of Sample	Percentage Change
Comparison States			
Child Abuse Reports	10.43 (4.35)	6.89 (3.09)	-33.9 %
Infant Fatality Rate	3.31 (1.73)	3.11 (1.99)	-6.04 %
Child Homicide Rate	1.92 (1.60)	1.44 (0.81)	-25.0 %
Adoption Rate	4.29 (2.41)	9.67 (3.46)	125.4 %
Female Suicide Rate	5.80 (1.84)	6.06 (2.84)	4.48 %
Mentally Unhealthy Days	39.26 (6.95)	45.51 (3.97)	15.9 %
Reform States			
Child Abuse Reports	8.03 (4.05)	6.78 (3.28)	-15.6 %
Infant Fatality Rate	3.33 (1.43)	3.92 (1.76)	17.7 %
Child Homicide Rate	1.77 (0.85)	2.07 (1.19)	16.9 %
Adoption Rate	3.33 (1.67)	7.97 (3.19)	139.3 %
Female Suicide Rate	5.67 (1.26)	5.62 (0.89)	-0.88 %
Mentally Unhealthy Days	39.10 (5.41)	47.80 (8.15)	22.3%

Notes: Standard deviations are in parenthesis. Data for substantiated child abuse reports are for years 1990 to 2005, adoption rates are for years 1995 to 2005, female suicide rates are for years 1990 to 1998, and mentally unhealthy days are for years 1993 to 2005. Comparison states are states that never adopt mandatory delay laws. Reform states are states that adopt mandatory delay laws at any time during the sample. Substantiated child abuse reports are the number of reports per 1,000 children in each year and state. Child Fatalities are the number of child fatalities per 100,000 children in each year and state. The adoption rate is number of adoptions per 1,000 pregnancies aged 15 to 44 and the female suicide rate is number of female suicides per 100,000 females aged 15 to 44 in each year and state.

Table 5: The Effect of Mandatory Delay Laws on Adoption Rates

Variable	Total adoptions per 1,000 women	Total adoptions per 1,000 pregnancies
Model 1:		
Lag of Delay Law	0.1405**	0.1433**
	(0.0575)	(0.0580)
Model 2: Law Includes:		
In-Person Counseling	0.0041	0.0207
(2 Visits)	(0.1063)	(0.1074)
Online or Over-the-Phone	0.1453**	0.1467**
Counseling	(0.0588)	(0.0594)
(1 Visit)		
Model 3: Law Includes:		
Includes Additional Information at	0.0424	0.0336
Counseling	(0.0878)	(0.0886)
Does not include Additional	0.1383**	0.1461**
Information at Counseling	(0.0577)	(0.0582)
Number of Observations	528	528

Notes: Sample: 1995– 2005 (unbalanced panel). Standard errors are in parentheses. Regressions are weighted using female population per state and year. The dependent variables are natural logs. Models include time-varying state controls listed in Table A1.

Table 6: The Effect of Mandatory Delay Laws on Child Maltreatment

Variable	Child-Abuse Report Rates	Infant Fatality Rates	Child Homicide Rates
Model 1:			
Mandatory Delay	0.0369	-0.0412	-0.0879
	(0.0552)	(0.0843)	(0.1077)
Model 2: Law Includes:			
In-Person Counseling	0.0167	-0.0757	-0.2095
(2 Visits)	(0.0914)	(0.1481)	(0.1864)
Online or Over-the-Phone Counseling	0.0285	-0.0334	-0.0572
(1 Visit)	(0.0552)	(0.0894)	(0.1124)
Model 3: Law Includes:			
Includes Additional Information at	-0.1063	-0.0476	-0.1807
Counseling	(0.0685)	(0.1070)	(0.1346)
Does not include Additional	0.0597	-0.0333	-0.0327
Information at Counseling	(0.0655)	(0.1062)	(0.1336)
Number of Observations	664	664	664

Notes: Sample: 1990- 2005 (unbalanced panel). Standard errors are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively. The dependent variables are logged. Child abuse report rate is the number of substantiated child abuse reports per 1,000 children by year and state. Child homicide and infant fatality rates is the number of child homicides and infant fatalities from violence per 100,000 children and infants. Models include time-varying state controls listed in Table A1.

Table 7: Effects of Requirements of Mandatory Delay Laws on Female Mental Health

Variable	Suicide Rates	Mentally Unhealthy Days
Model 1:		
Mandatory Delay	-0.0220	-0.0630**
	(0.0266)	(0.0319)
Model 2: Law Includes:		
In-Person Counseling	-0.0710	-0.0554
(2 Visits)	(0.0531)	(0.0595)
Online or Over-the-Phone Counseling	-0.0070	-0.0657*
(1 Visit)	(0.0300)	(0.0365)
Model 3: Law Includes:		
Includes Additional Information at Counseling	0.0342	-0.0378
	(0.0367)	(0.0432)
Does not include Additional Information at	0.0084	-0.0872**
Counseling	(0.0345)	(0.0433)
Number of Observations	800	619

Notes: Sample for suicide rates: 1990 - 2005 (balanced panel). Sample for mentally unhealthy days: 1993 - 2005 (unbalanced panel). Standard errors are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively. The dependent variables are logged. Female suicide rate is the number of female suicides per 100,000 women in each year and state. Mentally unhealthy days represent the average number of mentally unhealthy days for each year and state from survey information. Models for suicide include time-varying state controls listed in Table A1 as well as male suicide rates.

APPENDIX

Table A1: Variable Definition and Data Sources for Control Variables

State Controls	Definition	Data Source
Hospital Beds	Number of hospital beds per million people in the population	U.S. Census Bureau: Statistical Abstracts of the U.S., Health and Nutrition
Physician Rate	Number of physicians per 1,000 people in the population	U.S. Census Bureau: Statistical Abstracts of the U.S., Health and Nutrition
% Women State Legislators	Percent of state legislature that are women (including both parties)	Center for the American Woman and Politics
Marriage Rate	Marriage Rate per 1,000 population per state	U.S. Census Bureau: Statistical Abstracts of the U.S., Vital Statistics
Female Labor Force Participation	Female Labor Force Population Rate	U.S. Census Bureau: Statistical Abstracts of the U.S., Labor Force, Employment, and Earnings
Population (white)	Population that is white	U.S. Census Bureau: Current Population Survey
Population (black)	Population that is black	U.S. Census Bureau: Current Population Survey
Unemployment Rates	State Unemployment Rates	U.S. Department of Labor: Bureau of Labor Statistics
Per Capita Income	Real per capita income per state	Regional Economic Information System
Maximum AFDC and TANF Benefits	Maximum AFDC and TANF Benefits paid to families of three with no income	U.S. Department of Health and Human Services: Administration for Children and Families, Welfare Rules Databook
Male Suicide Rates	Male deaths aged 15 to 44 from suicide	Center for Disease Control (CDC): Compressed Mortality Files (This variable is only used in estimation for female suicide rates)
Population (women)	Population of women by ages 15-19, 20-24, 25-34, 35-44	U.S. Census Bureau: Current Population Survey
Abortion Restrictions		
Parental Involvement	=1 if state has a parental involvement law for minors	Bitler and Zavodny (2001), Levine (2004), Alan Guttmacher Institute: State Policies in Brief
Medicaid Funding	=1 if state has Medicaid Funding Restrictions	Bitler and Zavodny (2001), Levine (2004), Alan Guttmacher Institute: State Policies in Brief
Medicaid Funding Enjoined	=1 if state enjoined Medicaid Funding Restriction	Bitler and Zavodny (2001)
Border States with Mandatory Delay	Percent of border states that have mandatory delay laws	Created from Mandatory Delay
Border States with Parental Involvement	Percent of border states that have parental involvement laws	Created from Parental Involvement
Border States with Medicaid Funding	Percent of border states that have Medicaid funding laws	Created from Medicaid Funding
Border States with Medicaid Funding Enjoined	Percent of border states that have enjoined Medicaid funding laws	Created from Medicaid Funding Enjoined

Table A2: Estimates for the Effects of State-Level Controls on Child Outcomes

State Controls	Total Adoptions per 1,000 women	Total Adoptions per 1,000 pregnancies	Child Abuse Reports	Infant Fatality Rates	Child Homicide Rates
Parental Involvement	0.0014	-0.0079	0.2164***	0.0325	-0.0365
	(0.0516)	(0.0521)	(0.0621)	(0.0961)	(0.1210)
Medicaid Funding	-0.0145	-0.0341	0.0404	-0.1690	-0.3897**
	(0.3350)	(0.3380)	(0.0951)	(0.1470)	(0.1850)
Medicaid Funding Enjoined	0.1230	0.1332	-0.0535	-0.3655*	-0.3094
	(0.3240)	(0.3270)	(0.1490)	(0.2310)	(0.2910)
Border States with Mandatory Delay	0.2934**	0.2867**	-0.0340	-0.0062	0.3454
	(0.1050)	(0.1060)	(0.1320)	(0.2050)	(0.2580)
Border States with Parental Involvement	0.0543	0.0762	-0.2505	-0.1664	0.0700
	(0.1420)	(0.1430)	(0.1320)	(0.2040)	(0.2560)
Border States with Medicaid Funding	0.5746	0.6002*	0.3354	0.0131	-0.7208*
	(0.3710)	(0.3740)	(0.2090)	(0.3230)	(0.4070)
Border States with Medicaid Enjoined	0.4283	0.3875	0.9687**	-0.0716	-0.0294
	(0.4280)	(0.4320)	(0.3860)	(0.5980)	(0.7520)
% of Black Women	-58.816**	-65.106***	-14.013	24.629	-14.794
	(26.700)	(27.000)	(15.300)	(23.600)	(29.800)
% of White Women	-40.430	-46.737	-15.419**	-10.662	-37.273***
	(19.900)	(20.100)	(7.1600)	(11.1000)	(14.000)
% Population of Women aged 20 to 24	-0.3584	-1.2350	6.5980	-6.5915	-11.4269
	(6.9000)	(7.0000)	(5.6500)	(8.6600)	(10.9000)
% Population of Women aged 25 to 34	-10.662	-12.903*	6.0330	-4.2836	-6.9187
	(6.6700)	(6.7600)	(3.7300)	(5.7200)	(7.2200)
% Population of Women aged 35 to 44	-12.248	-13.518	18.668***	3.6249	6.4887
	(9.2700)	(9.4000)	(6.0800)	(9.3300)	(11.800)
Unemployment Rate	-0.0187	-0.0213	-0.0488	-0.1035**	-0.0492
	(0.0287)	(0.0290)	(0.0253)	(0.0392)	(0.0494)
Per Capita Income	5.1e-05**	0.0001**	-4.5e-05	-0.0001*	0.0001
	(2.8e-05)	(2.9e-05)	(2.7e-05)	(0.0000)	(0.0001)
Maximum AFDC and TANF Benefits	-0.0004	-0.0004	0.0002	-0.0001	0.0002
	(0.0005)	(0.0005)	(0.0006)	(0.0009)	(0.0011)
Physician Rate	0.0036	0.0040	0.0011	-0.0009	-0.0013
	(0.0030)	(0.0030)	(0.0012)	(0.0019)	(0.0023)
Hospital Bed Rate	0.0087	0.0100	-0.0104	-0.0044	-0.0075
	(0.0195)	(0.0196)	(0.0063)	(0.0098)	(0.0124)
Female Labor Force Participation	0.0605***	0.0607***	-0.0052	0.0067	-0.0052
	(0.0111)	(0.0112)	(0.0108)	(0.0167)	(0.0210)
% Females in State Legislature	2.2e-07	-5.0e-07	-1.4e-06	4.9e-06	-3.7e-06
	(7.3e-06)	(7.4e-06)	(8.3e-06)	(1.3e-05)	(1.6e-05)
Marriage Rate	-0.0244*	-0.0252*	0.0077	0.0153	0.0275
	(0.0158)	(0.0159)	(0.0127)	(0.0196)	(0.0247)

Table A3: Estimates for the Effects of State-Level Controls on Female Outcomes

State Controls	Suicide Rates	Mentally Unhealthy Days
Parental Involvement	-0.0546	0.0713*
	(0.0265)	(0.0400)
Medicaid Funding	0.0956	-0.0317
	(0.0563)	(0.0503)
Medicaid Funding Enjoined	-0.0728	0.0986
	(0.0972)	(0.0874)
Border States with Mandatory Delay	0.0489	-0.1764**
	(0.0554)	(0.0790)
Border States with Parental Involvement	-0.0971	-0.0908
	(0.0645)	(0.0799)
Border States with Medicaid Funding	0.0790	-0.0913
	(0.0978)	(0.0982)
Border States with Medicaid Enjoined	-0.3687**	-0.5936**
	(0.1670)	(0.2610)
% of Black Women	8.3104	1.4297
	(8.7900)	(9.6600)
% of White Women	-3.0933	-4.1585
	(6.3000)	(4.6400)
Population of Women aged 20 to 24	0.4336	2.3924
	(3.3200)	(3.7700)
Population of Women aged 25 to 34	1.4886	1.7703
	(2.2700)	(3.1200)
Population of Women aged 35 to 44	-0.4811	3.8714
	(3.6000)	(4.2800)
Unemployment Rate	0.0008	0.0172
	(0.0129)	(0.0172)
Per Capita Income	-9.4e-06	3.3e-05**
	(1.5e-05)	(1.7e-05)
Maximum AFDC and TANF Benefits	-0.0002	0.0007**
	(0.0003)	(0.0003)
Physician Rate	0.0003	-0.0002
	(0.0005)	(0.0012)
Hospital Bed Rate	-0.0018	-0.0060
	(0.0023)	(0.0077)
Female Labor Force Participation	-0.0013	0.0031
	(0.0058)	(0.0061)
% Females in State Legislature	-5.3e-06	-7.1e-07
	(5.4e-06)	(4.4e-06)
Marriage Rate	-0.0033	0.0014
	(0.0079)	(0.0086)
Male Suicide Rate	0.3419***	
	(0.0777)	

Table A4: The Effect of Mandatory Delay Laws on the Number of Adoptions

Variable	Total adoptions (control for female population)	Total adoptions (control for pregnancies)
Model 1:		
Lag of Delay	710.57***	696.18***
	(147.15)	(145.95)
Model 1: Law Includes:		
In-Person Counseling	37.731	35.173
(2 Visits)	(271.19)	(263.78)
Online or Over-the-Phone	733.72***	795.18***
Counseling	(150.37)	(263.78)
(1 Visit)		
Model 2: Law Includes:		
Includes Additional Information at	335.53	548.25**
Counseling	(225.55)	(224.38)
Does not include Additional	670.68***	674.10***
Information at Counseling	(148.03)	(144.24)
Number of Observations	528	528

Notes: Sample: 1995– 2005 (unbalanced panel). Standard errors are in parentheses. Regressions are weighted using female population per state and year. Models include time-varying state controls listed in Table A1.

Table A5: The Effect of Mandatory Delay Laws on Child Maltreatment

Variable	Child-Abuse Reports	Infant Fatalities	Child Homicides
Model 1:			
Mandatory Delay	-198.72	-1.0064	-2.8134
	(573.4)	(0.8960)	(1.7410)
Model 2: Law Includes:			
In-Person Counseling	375.90	-4.8555	-0.6718
(2 Visits)	(946.13)	(3.0157)	(1.4344)
Online or Over-the-Phone Counseling	410.99	-1.9454	-1.2563
(1 Visit)	(571.42)	(3.0157)	(0.8652)
Model 3: Law Includes:			
Includes Additional Information at	775.81	-1.3624	-2.1569
Counseling	(744.58)	(1.0525)	(2.4089)
Does not include Additional	-302.91	-1.0572	-2.7827
Information at Counseling	(712.02)	(1.0214)	(2.2956)
Number of Observations	664	664	664

Notes: Sample: 1990- 2005 (unbalanced panel). Standard errors are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively. Dependent variables are the count values of child abuse, infant or child fatalities. The models include controls for children or infant population in each year and state. Models include time-varying state controls listed in Table A1

Table A6: Effects of Requirements of Mandatory Delay Laws on the Number of Female Suicides

Variable	Female Suicides
Model 1:	
Mandatory Delay	-0.4996
	(1.0738)
Model 2: Law Includes:	
In-Person Counseling	-2.8733
(2 Visits)	(3.5355)
Online or Over-the-Phone Counseling	0.0594
(1 Visit)	(1.8840)
Model 3: Law Includes:	
Includes Additional Information at Counseling	-0.5074
	(2.3337)
Does not include Additional Information at	-0.4912
Counseling	(2.4159)
Number of Observations	800

Notes: Sample for suicide rates: 1990 - 2005 (balanced panel). Standard errors are in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively. Models for suicide include time-varying state controls listed in Table A1 as well as male suicide rate and female population.

CHAPTER IV

THE EFFECTS OF JOINT-CHILD-CUSTODY LEGISLATION ON THE CHILD-SUPPORT RECEIPT OF SINGLE MOTHERS

(with John Nunley and Alan Seals)

4.1. 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 Maralani 2001). The inconsistencies present in the existing literature are likely the result of differences in socioeconomic

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[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.

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 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.

4.2. BACKGROUND

4.2.1. 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

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

1998). However, in the 1970s and 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]

4.2.2. Theoretical Predictions of Joint-Custody Reform

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

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[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).

of child-support payments (Hofferth, et al. 2010).[5] Thus, we expect joint-custody reform to increase the child-support receipt of single mothers. 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

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[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.

related to SES. Thus, we 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). 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.

4.2.3. The Effects of Joint Child Custody on 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 1992-1998 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), 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; 1990b; 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 differences-in-differences (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.[6]

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.

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

4.3. 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 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

$$Child\ Support_{i,s,t} = \beta_0 + \beta_1\ Joint\ Custody_{s,t} + \beta_2\ \mathbf{CSE}_{s,t}$$
$$+ \beta_3 \mathbf{X}_{i,s,t} + \beta_4\ \mathbf{S}_{s,t} + \sum_{s} \beta_s\ \eta_s + \sum_{t} \beta_t\ \tau_t + u_{i,s,t}. \tag{1}$$

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 with two of its lags; η and τ 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 η) 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).[7]

4.4. RESULTS

In Section 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 Section 4.2, we estimate the impact of joint-custody reform on the probability of receiving child support for sub-samples of single mothers

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[7] 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).

based on their welfare-participation status. Estimates for the single-mother and state-level controls are presented in Tables A1-A6 in the Appendix.

4.4.1. 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.[8] 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-

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[8] 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.

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 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, Figure 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.

4.4.2. 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 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 4, 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.

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 Section 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.

4.5. 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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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

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

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 (0.4815)	0.1444 (0.3515)	0.5165 (0.4997)	0.2822 (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 (0.3857)	0.1271 (0.3331)	0.2678 (0.4429)	0.1569 (0.3637)
Number of Observations	19,322	7,864	6,366	5,102
Does not Receive Public Assistance				
Child-Support Receipt	0.4774 (0.4995)	0.1703 (0.3758)	0.5996 (0.4900)	0.3730 (0.4834)
Number of Observations	31,942	5,387	19,390	7,165

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

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

Notes: 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.

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* (0.0090)	-0.0007 (0.0116)	0.0363** (0.0125)	0.0171 (0.0164)
Divorced	0.2548*** (0.0070)	--	--	--
Separated	0.1120*** (0.0084)	--	--	--
Receives Public Assistance	-0.1819*** (0.0059)	-0.0151* (0.0070)	-0.2880*** (0.0085)	-0.1496*** (0.0097)
Number of Observations	51,274	13,251	25,756	12,267

Notes: 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 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 (0.0112)	-0.0190 (0.0162)	0.0198 (0.0200)	-0.0100 (0.0217)
Divorced	0.1150*** (0.0086)	--	--	--
Separated	0.0461*** (0.0082)	--	--	--
Number of Observations	19,322	7,864	6,366	5,102

Notes: 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: 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** (0.0116)	0.0260 (0.0173)	0.0344* (0.0134)	0.0320 (0.0238)
Divorced	0.1900*** (0.0075)	--	--	--
Separated	0.1802*** (0.0112)	--	--	--
Number of Observations	31,942	5,387	19,390	7,165

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

APPENDIX

Table A1: 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

Notes: 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 A2: 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)
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

Notes: 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 A3: 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 (0.0040)	-0.0028 (0.0051)	-0.0032 (0.0095)	-0.0055 (0.0068)
Children under 18	0.0020 (0.0028)	0.0026 (0.0041)	-0.0023 (0.0059)	0.0046 (0.0046)
Metro	-0.0177** (0.0079)	-0.0130 (0.0111)	-0.0292** (0.0146)	-0.0117 (0.0147)
Age	0.0026 (0.0034)	0.0090** (0.0045)	-0.0064 (0.0081)	0.0036 (0.0071)
Age-squared	-0.0001 (0.0001)	-0.0002** (0.0001)	0.0001 (0.0001)	-0.0001 (0.0001)
Black	-0.0622*** (0.0067)	-0.0474*** (0.0093)	-0.0883*** (0.0144)	-0.0656*** (0.0109)
Hispanic	-0.0535*** (0.0079)	-0.0468*** (0.0096)	-0.0769*** (0.0186)	-0.0448*** (0.0140)
High School Graduate	0.0667*** (0.0070)	0.0424*** (0.0089)	0.1044*** (0.0141)	0.0599*** (0.0139)
Some College	0.1891*** (0.0141)	0.1453*** (0.0208)	0.2505*** (0.0229)	0.1726*** (0.0247)
College Graduate	0.2552*** (0.0313)	0.1232** (0.0511)	0.3551*** (0.0448)	0.2085*** (0.0614)
Number of Observations	19,322	7,864	6,366	5,102

Notes: 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 A4: 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 (0.0001)	-0.0002 (0.0001)	-0.0001 (0.0002)	0.0004** (0.0002)
AFDC Benefit (-1)	0.0000 (0.0001)	0.0001 (0.0001)	-0.0001 (0.0001)	-0.0002* (0.0001)
Unemployment	-0.0056 (0.0107)	-0.0102 (0.0148)	0.0030 (0.0181)	-0.0025 (0.0205)
Unemployment (-1)	0.0181 (0.0117)	0.0116 (0.0163)	0.0245 (0.0221)	0.0182 (0.0244)
Unemployment (-2)	-0.0096 (0.0070)	-0.0011 (0.0078)	-0.0187 (0.0161)	-0.0145 (0.0135)
Child-Support Expenditures	0.0000 (0.0002)	-0.0002 (0.0003)	-0.0003 (0.0004)	0.0008** (0.0003)
Genetic Testing (-1)	-0.0158 (0.0111)	-0.0055 (0.0132)	-0.0348 (0.0213)	-0.0105 (0.0189)
Wage Withholding	-0.0132 (0.0116)	-0.0043 (0.0149)	-0.0080 (0.0213)	-0.0265 (0.0221)
Immediate Withholding	-0.0166 (0.0140)	0.0116 (0.0186)	-0.0403 (0.0265)	-0.0332 (0.0215)
Universal Withholding	0.0329** (0.0141)	0.0105 (0.0156)	0.0924*** (0.0296)	0.0067 (0.0227)
Paternity Until 18 Years	0.0083 (0.0130)	0.0002 (0.0159)	-0.0063 (0.0243)	0.0367* (0.0202)
Numerical Guidelines	-0.0135 (0.0141)	-0.0032 (0.0160)	-0.0067 (0.0256)	-0.0377* (0.0212)
Presumptive Guidelines	0.0125 (0.0193)	-0.0065 (0.0243)	0.0061 (0.0314)	0.0594 (0.0388)
State Intercept (-1)	-0.0147 (0.0107)	-0.0178 (0.0140)	0.0009 (0.0213)	-0.0427** (0.0194)
Number of Observations	19,322	7,864	6,366	5,102

Notes: 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 A5: 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)
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

Notes: 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 A6: 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)	0.0001 (0.0002)	0.0000 (0.0001)	0.0003 (0.0002)
AFDC Benefit (-1)	0.0000 (0.0001)	-0.0001 (0.0001)	0.0001 (0.0001)	0.0000 (0.0001)
Unemployment	-0.0014 (0.0107)	-0.0233 (0.0238)	0.0082 (0.0124)	-0.0353 (0.0237)
Unemployment (-1)	0.0078 (0.0152)	0.0313 (0.0267)	-0.0098 (0.0183)	0.0596* (0.0328)
Unemployment (-2)	-0.0038 (0.0103)	0.0008 (0.0110)	-0.0005 (0.0125)	-0.0136 (0.0227)
Child-Support Expenditures	-0.0003 (0.0002)	-0.0001 (0.0003)	0.0000 (0.0003)	-0.0008** (0.0004)
Genetic Testing (-1)	-0.0031 (0.0110)	-0.0206 (0.0185)	0.0041 (0.0125)	-0.0057 (0.0225)
Wage Withholding	0.0219* (0.0118)	0.0347** (0.0175)	0.0117 (0.0139)	0.0337 (0.0218)
Immediate Withholding	0.0003 (0.0133)	0.0022 (0.0209)	0.0034 (0.0160)	-0.0021 (0.0258)
Universal Withholding	0.0169 (0.0133)	0.0347* (0.0206)	0.0138 (0.0155)	-0.0138 (0.0236)
Paternity Until 18 Years	-0.0233* (0.0122)	-0.0005 (0.0199)	-0.0353*** (0.0135)	0.0108 (0.0243)
Numerical Guidelines	-0.0054 (0.0132)	0.0041 (0.0214)	-0.0127 (0.0163)	0.0136 (0.0262)
Presumptive Guidelines	0.0109 (0.0203)	0.0265 (0.0274)	0.0043 (0.0220)	-0.0133 (0.0351)
State Intercept (-1)	0.0035 (0.0113)	-0.0115 (0.0190)	0.0204 (0.0131)	-0.0269 (0.0226)
Number of Observations	31,942	5,387	19,390	7,165

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

CHAPTER V

CONCLUSION

The first essay is entitled "The Impact of Mandatory Delay Laws on Fertility Outcomes." While research exists on other abortion restrictions, there is little empirical research on the effects of mandatory delay laws due to their more recent nature. These restrictions, unlike parental involvement and Medicaid funding restrictions, were unconstitutional until the U.S. Supreme Court overturned the ruling in 1992. My results show mandatory delay laws, which require a waiting period before abortion, reduce abortion rates, increase birth rates, but have no effect on pregnancy rates. The effects of the laws are also shown to vary by visitation and counseling requirements.

The second essay is entitled "The Effects of Mandatory Delay Laws on Women and Children." This essay examines whether changes in fertility from mandatory delay laws have secondary consequences on women and children. My results show children are affected through an increase in adoption. However, mandatory delay laws do not affect child maltreatment. Further, I find mandatory delay laws impact female mental health. Specifically, the laws reduce the number of mentally unhealthy days per year by six percent. However, the laws do not affect female suicide rates, which I use as a more drastic proxy for female mental health.

My final essay is entitled "The Effects of Joint-Child Custody Legislation on the Child-Support Receipt of Single Mothers," (co-authored with John Nunley and Alan Seals). This essay examines whether joint custody affects the probability that single mothers receive child support. Joint custody could encourage fathers to pay child support

because they are able to spend more time with the child and better able to monitor how the support is being spent. Using a quasi-natural experiment, we find joint custody increases the probability that single mothers receive child support. Furthermore, divorced mothers who do not receive public assistance benefit the most from joint custody.