

**Undue Burdens: The Effect of Abortion Restrictions on Foster Care
Entry Rates**

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Abstract

Since the 1973 ruling of *Roe v Wade*, the Supreme Court has permitted a new abortion law enforced at the state level that requires parental consent or notification for unmarried minors seeking abortion. This paper uses a panel of pooled state-level foster care entry rates over the years 1990 through 2005 and considers the impact of parental involvement restrictions on the foster care entry rates. Adding state and year fixed effects to control for changing unobservable variables, the results suggest a statistically significant positive correlation between enforced parental consent laws and foster care entry rates during these years. The results provide evidence that the presence of enforced parental consent laws can explain some of the increase in foster care entries. In opposition to previous results testing alternative outcomes, these results point to the ineffectiveness of notice laws (as opposed to consent laws) when considering foster care entry as the tested outcome.

*The detriment that the State would impose upon the pregnant woman by denying this choice altogether is apparent. Specific and direct harm medically diagnosable even in early pregnancy may be involved. Maternity, or additional offspring, may force upon the woman a distressful life and future. Psychological harm may be imminent. Mental and physical health may be taxed by child care. There is also the distress, for all concerned, associated with the unwanted child, and there is the problem of bringing a child into a family already unable, psychologically and otherwise, to care for it.*¹

-The United States Supreme Court's Statement in *Roe v. Wade*
January 22, 1973

I. Introduction

This excerpt from the United States Supreme Court's Statement in *Roe v. Wade* demonstrates one of the intended outcomes of the legalization of abortion: to avoid bringing a child into a family unable, psychologically and otherwise, to care for it. Since the 1973 ruling, the Supreme Court has permitted a new abortion law enforced at the state level that requires parental consent or notification for unmarried minors (women under the age of eighteen) seeking abortions.² Several papers have been published questioning the effects of this law on teen pregnancy, birth rates, and abortion demand, but there is very little literature analyzing the possibility of unintended negative effects on the families who are subjected to the law.³ There remains a need to analyze if under the restrictions, a greater number of babies are born into families unable to care for them. This paper will question whether the parental involvement restrictions, which can be considered pro-life legislation, are successful at reducing the demand for abortion, or if instead the laws perpetuate the problems that *Roe v Wade* sought to eradicate. The children in the United States foster care system represent a population of children who were born into families deemed unfit to care for them. This paper will use foster care as a proxy for the outcome that *Roe v Wade* sought to avoid and will analyze the effects of

¹ Donahue and Levitt [2001 pp. 384].

² Haas-Wilson [1996]

³ Kane & Staiger [2006], Haas-Wilson [1996], Levine [2002], Tomal [1996], Oshfeldt & Gohman [1994]

the restrictions on the foster care entry rate. Because the parental involvement restrictions are enforced at the state level and vary in both the timing and degree of enforcement, this paper will use a panel of state-level foster care entry rates between the years 1990-2005 to identify the effects of the restrictions.

The structure of the paper is as follows: Section II provides related background on the parental involvement restrictions on abortion and on the United States foster care population. Section III presents relevant literature about the effects of abortion legislature and specifically the effects of the parental involvement restrictions. Section IV presents the relevant economic theory and the alternative hypotheses tested in this paper. This is followed by Section V, which presents the data and Section VI, which presents the empirical method, the results, and the implications. Section VII concludes, and Section VIII provides comments for future directions. An appendix provides a table of the timing of the parental involvement restrictions by state, type of law, and level of enforcement. All other tables appear in the body of the paper.

II. Related Background

A. Parental Involvement Restrictions on Abortion

The majority of the states enacted parental involvement restrictions on minors' attainment of abortion between the years 1980-1999.⁴ The restrictions were enacted with the desired outcome to reduce minors' abortion and pregnancy by making it more difficult to receive an abortion.⁵ As can be seen in the table in the appendix, some states dictate that minors must obtain parental consent in order to abort a pregnancy, while others require only parental notification. Consent laws require written permission from

⁴ Haas-Wilson [1996], Greenberger & Connor [1991], McNabb [2005]

⁵ "Talking Points" [2007]

one or both parents. Notification laws do not require consent but instead either require the parent to sign a form indicating they have been notified, or a waiting period is imposed between the time of parental notification and the abortion.⁶ Another difference across states is the degree to which the law is enforced, with some states not having any enforcement, others having enjoined enforcement. An enjoined law requires abortion providers to instruct a minor to notify or receive consent from a parent, but the abortion providers are not required to enforce this law.

Between the years 1990 and 2005, only eight states did not enact a law requiring parental consent notification, and an additional fourteen states had either an unenforced law or an enjoined law. Research has found that both consent and notice laws have equally significant effects; however, instances in which the laws exist but are not enforced present statistically insignificant effects.⁷ This suggests the importance of enforcement in restricting minors' abortions. To test the results found in previous literature about the effects of abortion restrictions, my analysis will analyze the difference in effects for *consent* versus *notice* laws as well as *enforced* versus *enjoined* versus *not enforced* laws.⁸

In order to analyze the effects of the restrictions on the incidence of foster care, it is important to first understand the similarities in characteristics between women who decide to have abortions and parents whose children enter foster care. First, consider the characteristics of U.S. women having abortions. Table 1 compares the percentages of women who have abortions to all women age 15-44 in several categories. The

⁶ The American Civil Liberties Union [2008]

⁷ Tomal [1999], Oshfeldt & Gohmann [1994], Haas-Wilson [1996]

⁸ There are two different types of laws, consent and notice, each with three different levels of enforcement. The model will include six dummy variables: notice-not enforced, consent-not enforced, notice-enjoined, consent-enjoined, notice-enforced, and consent-enforced.

disproportionality rates show the degree to which the women who have had an abortion are representative of the population of women as a whole. Women who have had an abortion are disproportionately Non-Hispanic Black (by a rate of 2.31) as compared to the general population of women. They are also disproportionately unmarried, from a metropolitan area, under 200% of the federal poverty level, and of minority status. While the data in Table 1 provide information about the women having abortions, research by Gruber, Levine, and Staiger [1999] takes the analysis a step further by estimating the characteristics of the children who would have been born to the women having abortions had they not been aborted. They estimate the characteristics of the marginal child after the passing of *Roe v Wade*. Their results find that the child on the margin for abortion would have been 70% more likely to live in a single parent family, 40% more likely to live in poverty, and 50% more likely to receive welfare.

Table 1

Percentage distribution of women obtaining abortions in 1994 and of all U.S. women aged 15-44 in 2000*

Characteristic	% of All Women Having Abortions	% of All Women Ages 15-44	Disproportionality Rate
Were Never Married	64.4	40.8	1.57
Live in Metropolitan Area	88.5	78.8	1.12
Poverty Status**			
<100%	25.4	12.8	1.98
100-199%	24.4	17.5	1.39
200-299%	18.9	17.9	1.05
>300%	31.3	51.8	.6
Race			
Non-Hispanic White	40.9	68.2	.59
Non-Hispanic Blacks	31.7	13.7	2.31
Native Americans	.9	.9	1
Asians/PI	6.4	4.4	1.45
Hispanics	20.1	12.8	1.57

* Jones, Darroch, and Henshaw [2002]

** Percentage of the federal poverty level

B. Foster Care

Like women who have abortions, children placed in foster care are disproportionately minority (a disproportionality rate of 2.43 for Non-Hispanic Blacks)⁹, from primarily urban areas, and from homes with poor economic standing.¹⁰ Now, consider the conditions under which a child enters the foster care system, shown in Table 2. Only 26% of children in foster care do not have living and present birth parents. The majority of children placed into substitute care are placed for their own protection, despite the presence of a biological parent.¹¹ Parent-related protective services are deemed necessary for reasons such as drug abuse, alcoholism, parental mental illness, parental incarceration, inadequate housing, or homelessness.¹²

Each of these family characteristics can be related to the theorized characteristics of the homes into which the marginal child was to be born had not been for the woman's selection out of pregnancy.¹³ Research on the subject has demonstrated that children on the margin for abortion would have been born into less optimal environments than the average U.S. child.¹⁴ The population of women who have abortions and the population of children placed into foster care are both 1) disproportionately minority, 2) from primarily urban areas, and 3) from homes with poor economic standing. Considering these shared characteristics, it follows that compared to the general population of women, women who have abortions are at a greater risk to give birth to a future foster care child.

⁹ Hill [2006]

¹⁰ Committee on Ways and Means [2004]

¹¹ This trend is consistent with data from the Committee on Ways and Means [1991] which considers data from the early 1980's and the U.S. Department of Health & Human Services which considers data from the early 1990's to today. Though the exact numbers have changed over time, the trends indicated in this paper are consistent over a nearly thirty year period of time and can be considered accurate in so much as they describe general trends.

¹² Committee on Ways and Means [1991]

¹³ The theory about the marginalized child comes from Gruber, Levine, and Staiger [1997].

¹⁴ Donahue and Levitt [2001], Gruber et al [1997], Gruber, Levine, and Staiger [1997].

Table 2
Reasons for Removing Children from Their Homes

Reasons	% of Foster Care Population
Neglect	42%
Caretaker Absent or Incapacitated	26%
Physical Abuse	12%
Sexual Abuse	8%
Legal Offense	7%
Other	5%

This figure [Lewit 1993] represents California in 1991. Though it would be ideal to have more recent nationwide data, the point is merely to demonstrate rough estimates of the trends.

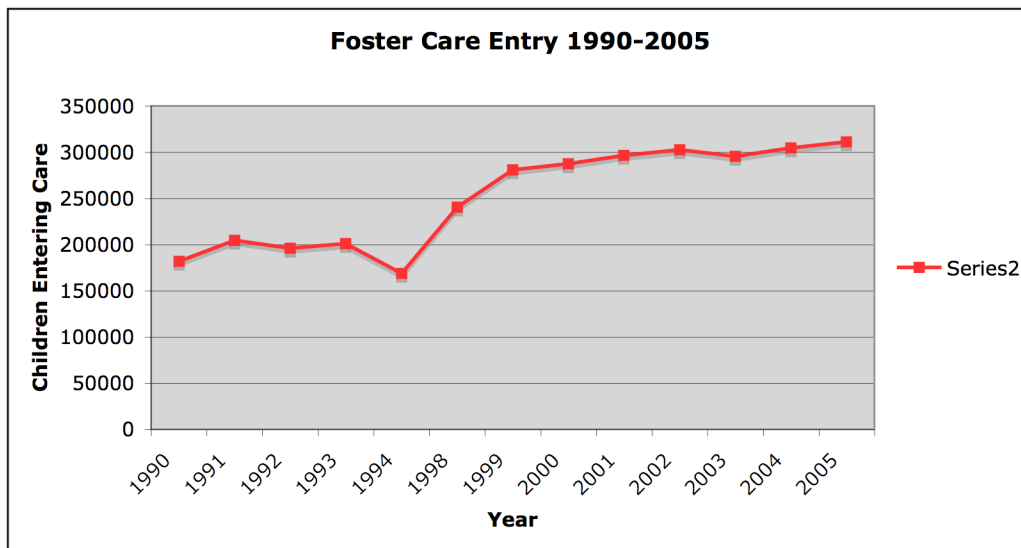
Returning to a discussion of the effects of the parental involvement restrictions on the incidence of foster care, consider again the intended outcome of *Roe v Wade*: to avoid bringing a child into a family unable, psychologically and otherwise, to care for it. This paper will use the foster care population as a proxy for such children because it represents an entire population of children born into families that for reasons shown in Table 2, were determined unfit to care for them.

The number of children in the United States foster care system has increased from 272,000 in 1962 to 513,131 in 2005.¹⁵ There is very little literature analyzing the reasons for this increase. This paper will try to determine the extent to which the increase is associated with the consent and notification laws passed since *Roe v Wade*. I will specifically consider the years 1990-2005 due to the availability of state specific data. Instead of considering the entire foster care population, I will consider foster care entry rates (in each state) because my research question is most concerned with the children who are entering foster care at time t . Considering the entire population would weaken

¹⁵ The numbers for the total foster care population that I use was found in two different sources. For data from 1962-1999, I used numbers presented by the Committee on Ways and Means [2004] which used data compiled by the American Public Welfare Association. For 2000-2005, I used numbers presented by the U.S Department of Health and Human Services which used VCIS survey data.

the benefit of year fixed effects because it would consider children already living in foster care prior to any change in abortion law. Figure 1 depicts the national foster care entry numbers between 1990-2005, demonstrating a clear upward trend. In 1995, foster care data began to be collected by a new organization, and states were not required to report data until 1998. Note that the graph jumps from 1994 to 1998 because data for 1995-1997 only exists for very few states.¹⁶ Between the years 1990-2005, ten states enacted the parental involvement restrictions, and my analysis will investigate whether the enactment of these laws led to an increase in foster care entry. My sample size for each year and for each state will include every child recorded to have entered foster care.

Figure 1
United States Foster Care Entry
1990-2005



The numbers for the total foster care population that I use was found in two different sources. For data from 1962-1999, I used numbers presented by the Committee on Ways and Means [2004] which used data compiled by the American Public Welfare Association. For 2000-2005, I used numbers presented by the U.S Department of Health and Human Services which used VCIS survey data.

¹⁶ When the limited data are added to the dataset, the regression results do not change significantly, so I left the data for 1995-1997 out of my analysis.

III. Literature Survey

A. Past Research on the Effects of the Legalization of Abortion

Though the majority of the literature review will consider past research on the effects of the parental involvement restrictions, existing research that tests the effects of abortion legalization has relevance in that it introduces the economic theory concerning the overall effects of abortion policy on outcomes such as crime, poverty, and welfare. One of the better known arguments on this topic was contributed by John J. Donahue and Steven D. Levitt [2001]. Their paper entitled “The Impact of Legalized Abortion on Crime” suggests that legalized abortion was responsible for as much as 50 percent of the drop in crime during the 1990s. They use an index that is designed to reflect the effect of all previous abortions on crime in any particular year and run panel data regressions of the effect of this abortion index on crime rate per capita. Because five states legalized abortion three years prior to *Roe v Wade*, the authors exploit this timing stagger to the advantage of their argument and demonstrate that these states saw a drop in crime before the other states. Donahue and Levitt [2001] warn that their findings “should not be misinterpreted as either an endorsement of abortion or a call for intervention by the state in the fertility decisions of women.” Levitt’s popular book *Freakonomics*, includes a chapter about the effect of abortion legalization on crime rates, and in it, he points to two important conclusions. The first is that “crime might just as easily be curbed by ‘providing better environments for those children at greatest risk for future crime’.”¹⁷ This conclusion can be easily applied to the provision of better environments for those children at greater risk for foster care placement. His second conclusion proposes that

¹⁷ Levitt and Dubner [2005]

“when the government gives a woman the opportunity to make her own decision about abortion, she generally does a good job of figuring out if she is in a position to raise the baby well.”¹⁸ My analysis of the abortion restrictions’ effect on foster care entry will bear in mind the conclusions of Donahue and Levitt and will extend their research by studying more recent abortion policy as well as introducing a new outcome variable.

B. Past Research on the Effects of Parental Involvement Restrictions

This section will provide a survey of the current economic literature on the effects of the parental involvement restrictions. The articles provide differing results with regard to the effect of the restrictions on birth rates, minors’ demand for abortion, abortion rates, and minors’ contraceptive behavior.

Thomas J. Kane and Douglas Staiger [1996] investigate the effect of abortion access on teen birthrates using county-level panel data accessed from the National Cancer Institute and data on state abortion laws from Blank, George, and London [1994]. Their results suggest that recent restrictions in abortion access were related to small declines among in-wedlock births, with out-of-wedlock births relatively unaffected. They conclude that their results are consistent with “the model in which pregnancy is endogenous and women gain new information about the attractiveness of parenthood only after becoming pregnant”.¹⁹ Kane and Staiger’s [1996] conclusions countered the existing literature in which many authors found that the parental consent laws for teenagers seeking abortions coincided with a *decrease* in birthrates.²⁰ They conclude that there is no strong evidence that the consent laws actually affected teen birthrates.

¹⁸ Levitt and Dubner [2005]

¹⁹ Kane and Staiger [1996].

²⁰ Kane and Staiger [1996] presents Matthews, Ribar, and Wilhelm [1995] and Jackson and Klerman [1994] as two papers with these findings.

Implicit in the argument that birth rates have either fallen or remained stagnant as a result of the parental involvement restrictions is the question of whether or not the restrictions have also decreased the demand for abortion and thus the abortion rate. If the restrictions are thought to cause a decrease in the abortion rate while simultaneously decreasing or unaffected the birth rate for minors, it would appear that the restrictions on abortion are somehow deterring minors from engaging in sexual activities that lead to pregnancy. Deborah Haas-Wilson [1994] used pooled time-series cross-sectional state data for eleven years between 1978-1990 to test the effect of parental involvement laws on minors' demand for abortion. Her results suggest that the laws decrease minors' demand for abortion by 13 to 25 percent. Haas-Wilson critiques past studies—though they also found a negative impact on minors' abortion rates—for three critical reasons: first, they did not provide empirical analyses of the restrictions over a very short period of time despite the continuous nature of the state specific changes; second, they did not distinguish between enforced and unenforced restrictions; third, they did not control for unobserved abortion sentiment differing from state to state.²¹ My analysis will build on the standards set by Haas-Wilson by considering thirteen years between 1990 and 2005, by testing the level of enforcement in each state, and by using state fixed effects to control for the unobserved abortion sentiment.

The decrease in abortion demand suggested by Haas-Wilson and others would seem to be followed by a decrease in abortion rates for minors. Robert L. Ohsfeldt and Stephan F. Gohmann [1994] use state-level data pooled over time for adolescents aged 15-17, comparing them to older teens aged 18-19 and adults aged 20-44, and their results

²¹ Haas-Wilson [1994] points to the results of Donovan [1983], Cartoof and Klerman [1986], Haas-Wilson [1993], Ohsfeldt and Gohmann [1994], and Blank, George, and London [1994]. Each of these authors found the parental involvement restrictions had a negative impact on minors' abortion rates.

“indicate that parental involvement laws reduce adolescent abortion rates and may, to a lesser degree, reduce adolescent pregnancy rates.” Similarly, Philip B. Levine [2003] found that the laws resulted in fewer abortions for minors, resulting in fewer pregnancies; however there was no statistically significant effect on births. Levine used state-level data over the 1985-1996 period as well as microdata from the 1988 and 1995 National Surveys of Family Growth (NSFG). He concludes that the laws decrease pregnancy rates because abortions decrease without an increase in birth rates and he attributes the decrease in pregnancy to an increased use of contraception rather than a reduction in sexual activity.

The current economic literature on the parental involvement restrictions points to the benefits of the legislation by suggesting that it decreases the abortion rate while increasing minors’ use of contraception and decreasing teen pregnancy. However, there is very little research on the effects that these restrictions have on the outcomes for families subjected to the law. This paper will add to the current literature in four ways. First, it will introduce foster care entry rates as a new dependent variable used as a proxy to measure the extent to which the laws affect the number of children born into families unable to care for them. Second, it will consider the more recent effects of changes in abortion policy by using pooled state panel data from 1990 through 2005. Third, it will use state and year fixed effects in order to control for confounding variables. Fourth, it will test the existing hypotheses concerning the type of parental restriction laws and the degree to which the laws are enforced. There are two different types of parental involvement laws: one requires that the minor simply notify a parent prior to having an abortion, and the other requires the minor obtain parental consent. In addition, state laws

differ in levels of legal enforcement. Past literature has found that that both consent and notification laws show equally significant effects; however, in states with enforced laws, the effects are much greater than in states in which the laws are merely enjoined or not enforced at all.²² My analysis will test these hypotheses by treating the different types of laws and the different levels of enforcement as separate independent variables.

IV. Economic Theory: The Hypotheses

This paper asks whether people living under parental involvement restrictions on minors' abortion access are more or less likely to have children who enter the foster care system. In addition, it asks whether the type of law and the level of enforcement of the law have differing effects on the foster care entry rate. The null hypothesis for the first, broader question states that the parental involvement restrictions on minors' abortions access have no statistically significant effect on the rate at which children enter foster care. The two alternative hypotheses considered in this paper point to either a positive correlation (the restrictions lead to an increase in foster care entry rates) or a negative correlation (the restrictions lead to a decrease in foster care entry rates). The economic theory behind each of these two hypotheses will be discussed in this section. The null hypotheses for the second question with respect to the types of law and the level of enforcement states that 1) the effect is both the same and negligible whether or not the presiding restriction is a consent or notification law, and 2) the effect is both the same and negligible whether or not the presiding law is enforced, enjoined, or unenforced. This paper will test the alternative hypotheses that 1) consent and notification laws show

²² Tomal [1999], Ohseldt and Gohmann [1994], Haas-Wilson [1996]

equally significant effects when the law is enforced, and 2) that consent and notification laws show equally insignificant effects when the law is enjoined or unenforced.

A. Alternative Hypothesis I: the Restrictions Increase the Foster Care Entry Rate

The hypothesis that the restrictions have a positive correlation with the foster care entry rate is based on the economic theory presented by Donahue and Levitt [2001].

They argue that the increase in access to abortion caused by the legalization of abortion increased the abortion rate and decreased crime. This demonstrates a negative correlation between access to abortion and crime rates. Based on their arguments, it would follow that a decrease in access to abortion would decrease the abortion rate and would be positively correlated to crime rates. In connecting this theory to my research question, I will review the economic theory established by Donahue and Levitt, replacing crime rates with foster care entry rates.

High abortion rates may lead to reduced foster care incidence either through reductions in cohort sizes or through lower per capita foster care entry rates for affected cohorts. If the higher abortion rate coincides with a lowered birth rate, there will be fewer children in the general population, and thus fewer children entering foster care. The more interesting possibility is that children born during a period in which there is a high abortion rate may on average have lower subsequent rates of foster care entry for either of two reasons.²³ First, as already demonstrated in the background section on foster care, women who have abortions are at a greater risk to give birth to a future foster care child. Second, a woman's decisions to have an abortion may be directly or indirectly made to

²³ The arguments in this paragraph are taken from Donahue and Levitt [2001], in which the authors argue that legalized abortion leads to reduced crime.

provide a better outcome for her child by either 1) optimizing the timing of childbearing or 2) opting out of childbearing altogether and thus preventing an unborn child from being born into a situation in which its parent could not provide sufficient support. Abortion provides a woman the opportunity to delay childbearing if the current conditions are suboptimal. Because children get placed into foster care due to the suboptimal conditions with their birth families, it follows that the abortion rate would be negatively associated of foster care entry rates.

From this theory it would make sense that any outside effect that decreased abortion rates while increasing birth rates could lead to an increase in foster care entry. More children on the whole would be born into families with characteristics such as those held by the biological families of foster children and thus, more children would enter the foster care system. Theoretically the abortion restrictions on minors should decrease the demand for abortions, which would subsequently decrease the number of abortions and increase the birth rate. The related literature presented previously suggests a decrease in demand for abortions; however, it does not suggest an increase in birth rates. Inherent in the alternative hypothesis that the abortion restrictions increase the foster care entry rate is the hypothesis that the restrictions also increase birth rates. Testing this alternative hypothesis will also indirectly test past economists' findings that the restrictions do not increase birth rates.²⁴ If the stated theory applies, the states and years with enforced parental consent and notification abortion laws should have an observed increase in foster care entry.

²⁴ See literature survey section for the presentation of these past findings.

B. Alternative Hypotheses II: the Restrictions Decrease the Foster Care Entry Rate

The relevant literature concerning the restrictions' decrease in the abortion rate yet lack of effect on the birth rate, points to either the null hypothesis or this second hypothesis that the restrictions actually decrease the foster care entry rate. This hypothesis maintains the economic theory presented by Donahue and Levitt; however, it implies that these new restrictions involve an additional mechanism than did the abortion policies in 1973. Some mechanism, unexplained in Donahue and Levitt's paper, seems to have prevented the birth rate from increasing even with an observed decrease in the abortion rate. Levine [2003] and McNabb [2005] present evidence that suggests an increase in the likelihood that a woman uses contraceptives with the presence of an enforced abortion restriction. An increase in the use of contraception would explain findings that the restrictions decrease abortion demand without increasing births. This theory implies that women are opting for contraceptive use at a higher frequency due to the increase in the cost of having an abortion that results from the parental involvement restrictions. Considering this theory, it would follow that the presence of an enforced parental involvement restriction would decrease the number of pregnancies experienced by women who are at a greater risk for producing children who will enter foster care, and hence would decrease the number of children entering foster care. Because foster care entries is observed to be increasing between 1990-2005 (see Table 4), this hypothesis infers that there are many unobserved variables affecting foster care incidence, and that the abortion restrictions are actually producing an effect in opposition to the major trend.

V. Data

Acquiring complete and accurate data on foster care populations presents several complications. Historically the best source for foster care data has been the Voluntary Cooperative Information System (VCIS) which has been operational since 1982. VCIS is operated by the American Public Human Services Association, formerly the American Public Welfare Association (APWA), with funding assistance from the Administration for Children and Families (ACF), U.S. Department of Health and Human Services (HHS). The federal government collected voluntary annual data on substitute care and adoption from the late 1940's until 1975. Because states were not forced to submit accurate data, these data are not completely reliable. The most problematic period for foster care data is between 1975 and the early 1980's, during which time virtually no state-specific data were reported. APWA developed VCIS to fill the continuing need for national information on child welfare programs. Published summaries of VCIS data begin in 1982.²⁵

In this paper, I use pooled state panel data for the numbers depicting entry into the foster care system. The panel data include every child who entered foster care in the years 1990 through 2005. For data from 1990 through 1999, I used numbers presented by the Committee on Ways and Means [2004] which used data compiled by the American Public Welfare Association. For 2000 through 2005, I used numbers presented by the U.S Department of Health and Human Services [2006, 2006, 2007] which used VCIS survey data.

Though the VCIS data is the most complete and accurate data available to me at this time for the years 1990-2005, it is important to note its limitations. “VCIS data are

²⁵ U.S. Department of Health and Human Services [2007]

not completely comparable because states use different reporting periods. Further, the data collected from states that changed reporting periods may include the double counting or omission of some children. Since the VCIS database does not contain sufficient information to correct for these problems, the data has not been altered.”²⁶ The pooled panel data includes fourteen years of foster care entry data for thirty-nine states. The total observations equal 459 because some states are missing data for certain years.²⁷

The dependent variable in my model, foster care entry rate, is the state level entries into foster care divided by the state level population of people age 0-19. This state level rate controls for changes in population size from year to year by including year specific data from the Census U.S. Intercensal County Population Data available at NBER and the U.S. Census Bureau population estimates located on the U.S. Census Bureau website.²⁸ The state population was also used to perform weighted least squares for the right hand variables.

Data on the timing of enactment and enforcement of the parental involvement restrictions by state were compiled from McNabb [2005] and the NARAL pro-choice America website. McNabb collected the specific information from Haas-Wilson [1996] and Greenberger & Connor [1991]. Haas-Wilson obtained the data from the NARAL Foundation and concludes that “the NARAL data are the best available data on the dates of actual enforcement of the restrictions and are based on NARAL’s tracking of court decisions, attorney general opinions, and other published sources.”²⁹

²⁶ U.S. Department of Health and Human Services [2007]

²⁷ Stata controlled for this using the `tsset` command.

²⁸ The reference from NBER can be found in the reference section under Roth [2007]

²⁹ Haas-Wilson [1996]

VI. Empirical Method and Results

A. Unrestricted Model

My empirical method employs the use of panel data in order to explore the role of cross-state heterogeneity in foster care incidence in detail without having to be specific about the sources of heterogeneity.³⁰ State fixed effects control for unobserved influences on foster care entry that vary across state such as abortion sentiment or poverty level. This allows the effect of the parental involvement restrictions to be identified from its variation within a state over time. I assembled a panel of state-level foster care entry rates over a fifteen year period of time from the sources mentioned above. In addition to state fixed effects, I use year fixed effects to control for evolving unobserved national attributes that affect the incidence of foster care.

To estimate the relationship with state-level time-series data, I have adapted Friedberg [1998]’s model for unilateral divorce law’s effect on divorce rates. The use of state effects in this model is advantageous because it explains foster care entry patterns flexibly, without having to include all relevant variables that could explain changes in foster care entry patterns. One risk with using this model is the possibility that factors which influence foster care incidence vary across states and may confound the estimates of the state effects. This will add bias to the estimates of the coefficients on my variables of interest if the changing factors are correlated with the changes in abortion restriction laws and do not change at a national level uniformly (which would allow them to be picked up by the year effects).³¹

³⁰ The empirical method used in my research was modeled directly after the work of Leora Friedberg [1998] in her paper “Did Unilateral Divorce Raise Divorce Rates? Evidence from Panel Data”.

³¹ Friedberg [1998]

Equation (1) represents my unrestricted model for explaining the foster care entry rate as dependent on the parental involvement restrictions, state fixed effects, and year fixed effects.

Unrestricted Model

$$(1) \text{ FCE-rate}_{st} = b_0 + b_1 * X_{1st} + b_2 * X_{2st} + b_3 * X_{3st} + b_4 * X_{4st} + b_5 * X_{5st} + b_6 * X_{6st} + b_{7s} * \text{state}_s + b_{8t} * \text{year}_t + e_{st}$$

The dependent variable FCE-rate_{st}, is the state level foster care entry rate, which is the number of entries that occur within a state each year divided by the state population of people age 0-19 in that year. The right hand side of the equation has been divided by the state population of people age 0-19 as well in order to perform weighted least squares. I ran the regressions first without considering the weights, but the results were sensitive to the addition of the weights. The state and year fixed effects are state_s and year_t. The variables of interest are X_{1st} through X_{6st}, each indicating a different type of parental involvement law, differentiated by the level of enforcement. Each variable is one if the state has the indicated law in year *t* and zero otherwise. Their indicators are listed below.

- X_{1st} = 1 if the state in year *t* has a Notice Law – Not Enforced; 0 otherwise
- X_{2st} = 1 if the state in year *t* has a Consent Law – Not Enforced; 0 otherwise
- X_{3st} = 1 if the state in year *t* has a Notice Law – Enjoined; 0 otherwise
- X_{4st} = 1 if the state in year *t* has a Consent Law – Enjoined; 0 otherwise
- X_{5st} = 1 if the state in year *t* has a Notice Law – Enforced; 0 otherwise
- X_{6st} = 1 if the state in year *t* has a Consent Law – Enforced; 0 otherwise

The null hypothesis tested by this analysis states that b₁= b₂=b₃=b₄=b₅=b₆= 0, indicating that the each of the parental involvement restrictions on minors’ abortion access has no statistically significant effect on the rate at which children enter foster care.

As demonstrated by Friedberg, the coefficients for each of the variables reflecting the presence of a law turned out to be sensitive to the nature of state and time controls

used. To demonstrate the sensitivity, Table 3 presents the results of first estimating the basic regression and then adding state and year effects, as in equation (1). Regression 3.1 in Table 3 has no state or year effects. The estimated coefficients on X_{3st} , at $-.303$, and X_{6st} , at $-.2949$, are statistically significant and negative, indicating a decrease in foster care entry rates resulting from the presence of an enjoined notice law or an enforced consent law, respectively. Regression 3.2 in Table 3 adds year fixed effects and again produces statistically significant negative coefficients for the same variables: X_{3st} , at $-.1947$, and X_{6st} , at $-.1375$. The coefficients on both of these variables are smaller with the addition of the year fixed effects. In addition, this regression produces a statistically significant positive effect for X_{4st} , at $.1756$. These results from regression 3.1 and 3.2 demonstrate that before controlling for state specific variables such as abortion sentiment or poverty level, enjoined notice laws and enforced consent laws are correlated with a *decrease* in foster care entry, while enjoined consent laws are correlated with an *increase* in foster care entry. After controlling for state fixed effects, it will be apparent that the results from these first two regressions are affected by one or more confounding factors due to state specific variables.

Regression 3.3 ignores year effects and adds state effects, which are meant to absorb everything affecting foster care incidence at the state level that is constant over time. Adding state effects produces insignificant p-values for the coefficients of the enjoined laws (X_{3st} and X_{4st}) but continues to produce a statistically significant, negative coefficient for the enforced consent law variable (X_{6st}) at $-.3902$. Based on past research

Table 3: Regression Results				
Dependent Variable: Foster Care Entry Rate				
Independent Variables:	3.1	3.2	3.3	3.4
X ₁ Notice Law <i>Not Enforced</i>	-.1814 (-1.87)	-.0376 (-0.54)	Dropped	Dropped
X ₂ Consent Law <i>Not Enforced</i>	-.1698 (-1.06)	-.0395 (-0.35)	Dropped	Dropped
X ₃ Notice Law <i>Enjoined</i>	-.3030** (-2.69)	-.1947* (-2.44)	-.5069 (-0.67)	.1263 (.26)
X ₄ Consent Law <i>Enjoined</i>	.1019 (1.03)	.1756* (2.50)	.6460 (1.26)	-.0026 (-0.01)
X ₅ Notice Law <i>Enforced</i>	-.0707 (-0.87)	.0546 (0.94)	-.4115 (-1.52)	.0583 (0.33)
X ₆ Consent Law <i>Enforced</i>	-.2948*** (-3.76)	-.1375* (-2.44)	-.3902* (-2.28)	.2928* (2.51)
Adjusted R ²	.0598	.5297	.1191	.6382
Year Fixed Effects	No	Yes	No	Yes
State Fixed Effects	No	No	Yes	Yes
Intercept	.7061 (9.71)	1.0712 (10.53)	.8008 (5.88)	.9464 (8.36)
1990 year effect	–	Dropped	–	Dropped
1991 year effect	–	.1919 (1.50)	–	.1458 (1.30)
1992 year effect	–	.0021 (.02)	–	.0150 (.14)
1993 year effect	–	-.0101 (-.08)	–	-.0125 (-0.12)

Table 3: Regression Results (cont)				
Dependent Variable: Foster Care Entry Rate				
Independent Variables:	3.1	3.2	3.3	3.4
1994 year effect	–	.2216 (1.69)	–	.1746 (1.50)
1998 year effect	–	.2095 (1.67)	–	.1840 (1.66)
1999 year effect	–	.2651* (2.19)	–	.2099 (1.95)
2000 year effect	–	-.6651*** (-6.62)	–	-.7255*** (-8.09)
2001 year effect	–	-.6615*** (-6.59)	–	-.7215*** (-8.05)
2002 year effect	–	-.6575*** (-6.54)	–	-.7169*** (-7.99)
2003 year effect	–	-.6665*** (-6.64)	–	-.7254*** (8.08)
2004 year effect	–	-.6555*** (-6.53)	–	-.7139*** (-7.96)
2005 year effect	–	-.6437*** (-6.41)	–	-.7037*** (-7.84)

Regression of foster care entry rate, 1990-2005, on whether a state has a parental involvement restriction law on abortion for minors. Each if the X independent variables represents a different type of law, varied by level of enforcement as indicated. Coefficients are presented with T-statistics in parentheses. Regressions are weighted by state population for people age 0-19. N=459. Data for all states in the years 1995-1997 is missing. For other years it is incomplete, which is accounted for by Stata using tsset. The stat fixed effects are not included in effort of space.

*Indicates a p-value smaller than .05
**Indicates a p-value smaller than .01
***Indicates a p-value smaller than .001

on the effects of the abortion restrictions on various dependent variables, the *insignificance* of the enjoined laws (X_{3st} and X_{4st}) was expected, and the *significance* of the enforced law follows past findings as well. These results indicate that the values in

regression 3.1 and 3.2 were the result of unobserved state specific variables affecting foster care incidence—such as abortion sentiment or poverty level—that were eliminated by the addition of fixed state effects.

The past three equations are only present in this paper to demonstrate the sensitivity with which the results react to the addition of state and year fixed effects. The results of interest are produced by regression 3.4, which adds both state and year fixed effects, representing the full equation (1). As in regression 3.3, the enforced consent law variable (X_{6st}) has a statistically significant coefficient. More importantly, however, the addition of both state and year fixed effects causes this coefficient to be positive, at 2.929, suggesting support of Alternate Hypothesis (I) that predicts an increase in the foster care entry rate with the addition of an enforced consent law. As mentioned previously, Friedberg's model predicts that the addition of state and year effects will alter the results quite a bit by simultaneously controlling 1) for unobserved influences that vary across state, and 2) for unobserved national attributes that affect the likelihood of foster care entry. Due to the importance of these added controls, the results from equation 3.4 are not only statistically significant but provide convincing evidence that the presence of an enforced consent law can explain some of the increase in foster care entry between 1990-2005.

B. Implications of Regression 3.4

The suggested positive effect of the enforced parental consent laws on foster care entry rates provides reason to believe that these laws produced some unintended outcomes. In addition to providing evidence for a causal relationship between the presence of enforced parental consent laws and the increase in foster care entries, the

results point to the ineffectiveness of notice laws, unenforced laws, and enjoined laws when considering foster care entry as the tested outcome. A more in depth comparison of the different types of laws will follow in the analysis of several restrictive models in the next section.

The positive relationship between the enforced consent restrictions and foster care entry rates provide an interesting story about the effect of the laws on birth rates. Past research by Matthews, Ribar, and Wilhem [1995], Jackson and Klerman [1994], and Kane and Staiger [1996] demonstrate that the abortion restrictions either decreased birth rates or had no significant effect. Inherent in the finding of a positive effect on foster care entry is a counter argument providing suggestive evidence for an increase in birth rates in states in year t with enforced consent laws. To see this, consider Donahue and Levitt's theory that the legalization of abortion (and consequential higher abortion rates) leads to reduced crime either through reductions in cohort sizes or through lower per capita offending rates for affected cohorts.³² In other words, legal restrictions on abortion (and consequential lower abortion rates) may lead to increased foster care entry either through increase in cohort sizes or through higher per capita foster care entry rates for affected cohorts. Logically, if foster care entry did in fact increase due to the enforced consent laws, there must have been an increase in birth rates for the part of the population most likely to give birth to a child who will enter foster care. It is possible that the birth rates of the general population decreased substantially enough to offset any increases that may have resulted from the restrictions on minors' access to abortion. However, at the very least, the evidence provides support for the theory that the restrictions on abortion

³² Donahue and Levitt [2001]

rights leads to an increase in birth rates for the sector of the population with mothers who are most susceptible to being unable to provide care for their children.

C. Restricted Models

In order to test whether the type of law and the level of enforcement of the law have differing effects on the foster care entry rate, I placed seven different restrictions on the original model. The equations and results for five of the restrictions are discussed below and Table 4 provides F-statistics and corresponding p-values. Equation (2) presents Restriction A, the most restrictive of the equations. This restriction provides an additional test of the original null hypothesis that each of the coefficients for the different types of law are equal to each other and equal to zero. It lumps the six different types of laws into one variable with only one coefficient, b_{123456} .

Restriction A

$$(1) \quad \text{FCE -rate}_{st} = b_0 + b_{123456} * (X_{1st} + X_{2st} + X_{3st} + X_{4st} + X_{5st} + X_{6st}) \\ + b_{7s} * \text{state}_s + b_{8t} * \text{year}_t + e_{st}$$

Testing this restriction does not produce a statistically significant p-value and fails to reject the null that states: the mere presence of a restrictive abortion law has an insignificant effect on the foster care entry rate. This result is expected because the model is too restrictive and fails to account for the possibility that some of the laws may have a greater effect than others.

Restriction B tests the null hypothesis that when a law is either not enforced or is just enjoined, there will be no statistically significant effect on the foster care entry rate. It lumps together the four types of laws that are either not enforced or enjoined into one variable with only one coefficient, b_{1234} . The F-statistic and corresponding p value

produced by this test fail to reject the null. This result is also expected because past research supports the hypothesis that if a law is not enforced, it will not create significant effects.

Restriction B

$$(2) \quad \text{FCE -rate}_{st} = b_0 + b_{1234} * (X_{1st} + X_{2st} + X_{3st} + X_{4st}) + b_5 * X_{5st} + b_6 * X_{6st} + b_{7s} * \text{state}_s + b_{8t} * \text{year}_t + e_{st}$$

Restriction C tests the null hypothesis that when a law is enforced, it will have insignificant effects on the foster care entry rate. By combining the two variables for consent law (enforced) and notice law (enforced), X_{5st} and X_{6st} , this restriction tests the significance of the coefficient b_{56} . The F-statistic and corresponding p-value for this test is in fact statistically significant and suggests that both consent laws and notice laws, when enforced, have equal and statistically significant effects on the foster care entry rate. This supports past findings that enforcement of the parental involvement laws is necessary for an effect to occur. While in this paper the effect on the tested dependent variable is undesirable in that an increase in foster care entry can be considered an indicator of an increase in sub optimal outcomes for the United States youth; policy makers hoping for a desirable outcome from the existence of these laws may be interested in the importance of the degree to which the laws are enforced. The effects on different dependent variables will differ, but the relevant research on the effect of parental involvement laws each point to the necessity of enforcement in the production of any effect, regardless of the tested dependent variable. As Haas-Wilson [1996] points out, the law must be enforced in order to increase the actual costs to minors.

Restriction C

$$(3) \quad \text{FCE -rate}_{st} = b_0 + b_1 * X_{1st} + b_2 * X_{2st} + b_3 * X_{3st} + b_4 * X_{4st} \\ + b_{56} * (X_{5st} + X_{6st}) + b_{7s} * \text{state}_s + b_{8t} * \text{year}_t + e_{st}$$

Restriction F tests the null hypothesis that notice laws have equal and insignificant effects, while Restriction G tests the null hypothesis that consent laws have equal and insignificant effects. Both of these null hypotheses have been considered to be true based on past research on the effects of parental involvement laws. The F-statistic and corresponding p-value for Restriction G actually indicates an equal and statistically significant effect when considering the presence of a consent law (regardless of enforcement level) as one variable with one coefficient, b_{246} .

Restriction F

$$(4) \quad \text{FCE -rate}_{st} = b_0 + b_{135} * (X_{1st} + X_{3st} + X_{5st}) + b_2 * X_{2st} + b_4 * X_{4st} + b_6 * X_{6st} \\ + b_{7s} * \text{state}_s + b_{8t} * \text{year}_t + e_{st}$$

Restriction G

$$(5) \quad \text{FCE -rate}_{st} = b_0 + b_1 * X_{1st} + b_{246} * (X_{2st} + X_{4st} + X_{6st}) + b_3 * X_{3st} + b_5 * X_{5st} \\ + b_{7s} * \text{state}_s + b_{8t} * \text{year}_t + e_{st}$$

The results from Restrictions F and G support the alternative hypothesis that consent laws, regardless of the degree to which they are enforced, have a greater impact on foster care entry rates than do notice laws. Equation 3.4 demonstrates that notice laws do not have a statistically significant effect on foster care entry rates even when enforced, and these new restrictions go a step further in distinguishing between consent and notice laws by showing an equal and significant result for all consent laws regardless of enforcement. These results are of particular interest in that they contradict past findings. Tomal [1999], Oshfeldt & Gohmann [1994], Levine [2003] use abortion rates, birth rates, and pregnancy

rates as dependent variables and find statistically significant effects due to the presence of *both* consent *and* notice laws.³³ The data used in this paper for foster care entry rates seem to be more sensitive to the difference between consent and notice laws than the data used for other dependent variables tested previously.

Table 4: Results for Testing Restrictive Models	
Restrictions:	
A	F-stat = 1.59 p-value = .1768
B	F-stat = .03 p-value = .99672
C	F-stat = 3.16 p-value = .0433
F	F-stat = .07 p-value = .9360
G	F-stat = 3.14 p-value = .0433

D. Restricting the Model to Effect only the Proportion of the Population at Risk for Culling in a given State, in a given Year

In the previous sections, I highlighted a strong empirical correlation between the presence of an enforced parental consent law and the increase in foster care entry rates. This section will add a variable to the right hand side of the equation that will allow the magnitude of the law variable to represent the proportion of the children ages 0-19 in each state s , in each year t , who were at risk for culling if the law was passed in year τ . If the enforced parental consent laws were the reason for the increase in foster care entry rates, then it would follow that the increase in entry rates would be best

³³ The authors find differing results depending on the dependent variable. The importance for this section of the paper is not the results that they found, but simply the fact that their results are equally sensitive to notice laws as they are to consent laws.

explained by a variable that took into account the proportion of the population who were born after the law was passed. This variable would more accurately represent the extent to which the law had the opportunity to effect the population of children eligible (by virtue of their age) to enter foster care.

To do this, I use the unrestricted model from equation (1) and allow a new variable to interact with the dummy variable for enforced consent law.

New Model

$$(2) \text{FCE-rate}_{st} = b_0 + b_6 * [P_{6stT} * X_{6st}] + b_{7s} * \text{state}_s + b_{8t} * \text{year}_t + e_{st}$$

The model includes state and year fixed effects while adding a new variable, P_{6stT} , which equals the fraction of the population s who was in the cohort at risk for culling by law passed in year τ in the current year t . To see this, consider the following example.

Colorado passed an enforced consent law in $\tau = 1998$. In year $t = 1990-1998$, $P_{6stT} = 0$ because the law did not have the opportunity to affect any portion of the population simply because it did not exist in those years.³⁴ In year $t = 1999$, P_{6stT} = the number of children in state s in year t age 0, divided by the total number of children in state s in year t ages 0-19. Five years after the passing of the law, when $t = 2003$, P_{6stT} = the number of children in state s in year t ages 0-4, divided by the total number of children in state s in year t ages 0-19. This continues until year $t = 2005$, P_{6stT} = the number of children in state s in year t age 0-6, divided by the total number of children in state s in year t ages 0-19. The null hypothesis tested by this analysis states that $b_6 = 0$ indicating that enforced parental consent restrictions on minors' abortion access have no statistically significant effect on the rate at which children enter foster care.

³⁴ Based on previous methods used in Economic papers testing the effect of abortion restrictions, I assume a one year lag before the presence of the law has time to take effect. So, the dummy variable indicating the presence of a law does not = 1 until one year after the law is passed.

In this model, I am only testing the effect of the enforced parental consent laws. This allows the variables for each of the other types of laws to disappear because the proportion of children at risk for culling by a parental consent law passed in year τ would equal 0 for states and years without a parental consent law.

The coefficient for the variable representing the presence of an enforced parental consent law is sensitive to the addition of the proportion variable, P_{6stT} . Table 5 presents the results from the regression using equation (2), which includes state and year fixed effects. The coefficient on $[P_{6stT} * X_{6st}]$ is positive at .6153 and is highly statistically significant with a p-value of .000. These results provide further support for Hypothesis (I) that predicts an increase in the foster care entry rate with the addition of an enforced consent law.

The addition of the proportion variable, P_{6stT} , allows the right hand side of the equation to be more precise in its explanation of the foster care entry rate by more accurately representing the extent to which the law had the opportunity to effect the population of children eligible (by virtue of their age) to enter foster care. These results are consistent with enforced parental consent laws increasing foster care entry rates with a one year lag.

Table 5: Regression Results	
<i>Restricting the Model to Effect only the Proportion of the Population at Risk for Culling in given a State in a given Year</i>	
Independent Variables:	Dependent Variable: Foster Care Entry Rate
$P_{6stT} * X_{6st}$ Consent Law <i>Enforced</i>	.6153*** (3.55)
Adjusted R ²	.4758
Year Fixed Effects	Yes
State Fixed Effects	Yes

Intercept	.4230 (.008)
1990 year effect	dropped
1991 year effect	.1991 (1.68)
1992 year effect	.1290 (1.09)
1993 year effect	.1461 (1.24)
1994 year effect	.0412 (.35)
1998 year effect	.1441 (1.23)
1999 year effect	.3382** (2.89)
2000 year effect	-.5402*** (-5.61)
2001 year effect	-.5500*** (-5.66)
2002 year effect	-.5533*** (-5.66)
2003 year effect	-.5703*** (-5.79)
2004 year effect	-.5674*** (-5.72)
2005 year effect	-.5655*** (-5.65)

Regression of foster care entry rate, 1990-2005, on whether a state has an enforced parental consent law on abortion for minors. Coefficients are presented with T-statistics in parentheses. Regressions are weighted by state population for people age 0-19. N=507. Data for all states in the years 1995-1997 is missing. For other years it is incomplete, which is accounted for by Stata using tsset.

*Indicates a p-value smaller than .05
**Indicates a p-value smaller than .01
***Indicates a p-value smaller than .001

VII. Conclusion

I have investigated the effect of the varying parental involvement laws on foster care entry rates for the years between 1990 and 2005. Adding both state and year fixed effects suggests a positive correlation between enforced parental consent laws and foster care entry rates. The results from equations (1) and (2) suggest that enforced parental consent laws are a primary explanation for the increase in foster care entry rates and

provide a strong argument for the presence of unintended negative outcomes for families affected by the presence of the abortion restrictions.

In addition, my results support the existing research that parental involvement laws do not have much of an effect on a variety of outcomes, unless they are enforced at the state level. However, my results counter the existing research concerning the difference in effect for consent and notification laws. When testing foster care entry rates as an outcome, my findings suggest that the type of law, whether consent or notice, is important when determining the effect and cannot be considered to have the same effect. Results show consent laws to have a significant effect, while notification laws have an insignificant effect. This divergence from past research, suggests that the data I used for foster care entry is highly sensitive to the specific type of law, when compared to past data used for various other dependent variables.

My research was originally inspired by Donahue and Levitt's [2001] paper about the impact of the legalization of abortion on crime rates. As Kane and Staiger [1996] pointedly state, "Implicit in much of the literature and the public debate [on abortion restrictions] is the presumption that each of these policies—from legalization [of abortion] through the requirements of parental consent for teens—will have similar effects [on previously tested dependent variables such as birth rates, abortion rates, and pregnancy rates]." However, they argue that the more recent access to barriers is modest relative to the barriers faced by women prior to the legalization of abortion. Keeping this in mind, it follows that the legalization of abortion would have had an even greater effect on foster care entry rates than do the recent policies, though admittedly the direction of the effect is unknown at this time. Without proper data for the foster care entry during

the years following *Roe v Wade*, any connection between abortion policy and foster care incidence must be analyzed by using more recent policy such as the parental involvement restrictions. The statistically strong positive effect of these “modest” barriers to abortion that I find in my research suggest that an even stronger effect may be found if I were able to consider the much larger barriers faced by women prior to the legalization of abortion (by looking at the subsequent removal of these barriers after the passing of *Roe v Wade*).

My findings imply the possibility that the abortion restrictions, which can be considered pro-life legislation, have unintended negative effects on the population of children whose mothers are subjected to the law. The evidence suggests that enforced parental consent laws lead to a greater number of babies being born into families unable to care for them, as demonstrated by the positive effect of these laws on foster care entry. Parental involvement laws were deemed constitutional in *Planned Parenthood of S.E. Pennsylvania versus Casey (1992)* so long as they did not impose an “undue burden” on women.³⁵ While the question of the undue burden placed on the woman is a topic to be debated in court, it seems indisputable that the babies born to women who are unable to care for them are not “due” the burden with which they will have to live their lives. The results presented in this paper demonstrate that the enforced parental consent laws do provide an “undue burden” on the babies of women affected by the laws, and the laws seem to be perpetuating the problems that *Roe v Wade* sought to eradicate.

This paper is certainly not intended to endorse abortion; however, it does seek to question the effects of abortion restrictions on the foster care population. Donahue and Levitt’s theory indicates that *if* the enforced consent laws did in fact increase foster care entry, it may be explained due to an increase in birth rates or due to an increase in the

³⁵ Levine [2003]

higher per capita foster care entry rates for women giving birth. The results of this paper suggest an argument against past findings that abortion restrictions do not have an increased effect on birth rates. In addition, the results suggest that the type of woman affected by the abortion restrictions are at a greater risk to give birth to a future foster care child than are the general population. Taking the Donahue and Levitt's theory one step further, the results suggest that the restrictions on abortion are altering the decision power of at-risk women, providing a constraint on the decision to prevent an unborn child from being born into a situation in which his mother cannot provide sufficient support. The most important finding inherent in my results is the need for the provision of better environments for children at greatest risk for foster care entry; this is a necessary improvement that has nothing to do with abortion policy.

VIII. Future Directions

A. Limitations of Data

As mentioned in the Data section of this paper, collecting data in a timely fashion has been the most constraining aspect of the research process. Future research would ideally be able to consider data from a larger time span. It would be largely beneficial to consider foster care entry data beginning before 1980 in order to cover the entire span of time in which the parental involvement laws were enacted. This seems highly improbable, however I am hopeful to at least be able to acquire data for the mid 1980s, which would be more conclusive. With the addition of these critical years in the history of parental involvement laws, I will be able to look more closely at the issue of timing and *how* the laws affect foster care entry. Data on earlier years would provide a better gauge of the permanency of the effects as well as the state-specific time trends. In

addition to acquiring earlier data, my empirical work would benefit from county specific data; however, at this time, I am unaware of any datasets that include such information.

B. State Specific Time Trends

In using Friedberg's econometric methodology, I had hoped to include both linear and quadratic state specific time trends. Relaxing equation (1) by adding the time trends would produce:

$$(3) \text{ FCE-rate}_{st} = b_0 + b_1 * X_{1st} + b_2 * X_{2st} + b_3 * X_{3st} + b_4 * X_{4st} + b_5 * X_{5st} + b_6 * X_{6st} + b_7s * \text{state}_s + b_{8t} * \text{year}_t + b_{9s} * \text{state}_s * \text{year}_t + b_{10s} * (\text{state}_s * \text{year}_t)^2 * e_{st}$$

The state fixed effects used in my regressions controlled for state specific patterns of foster care without having to control for each variable individually. Adding state specific time trends would control for the factors that influence foster care entry that also vary within a state over time. Based on Friedberg's results, I would expect that the addition of state-specific time trends would alter the results of my regressions greatly by eliminating the bias added if changing factors across states are correlated with the law changes without changing at a national level uniformly (which would then already be picked up by the year effects).

I was unable to include state specific time trends in my regressions because the number of observations (459) was too low. At this time, I do not have access to county data for foster care entry but if I were able to use county data, I believe I would have enough observations to include at least linear time trends.

When Friedberg added the time trends, the coefficient of interest increased, the standard error decreased, and the explanatory power increased. Because my empirical method is modeled after hers, I would expect to see similar results with the addition of

time trends. Her research does not provide any reason to predict that the addition of time trends to my regression will change the sign on the coefficient of interest, but rather provides reason to believe the explanatory power of the regression will increase.

C. Age Distribution within Foster Care System

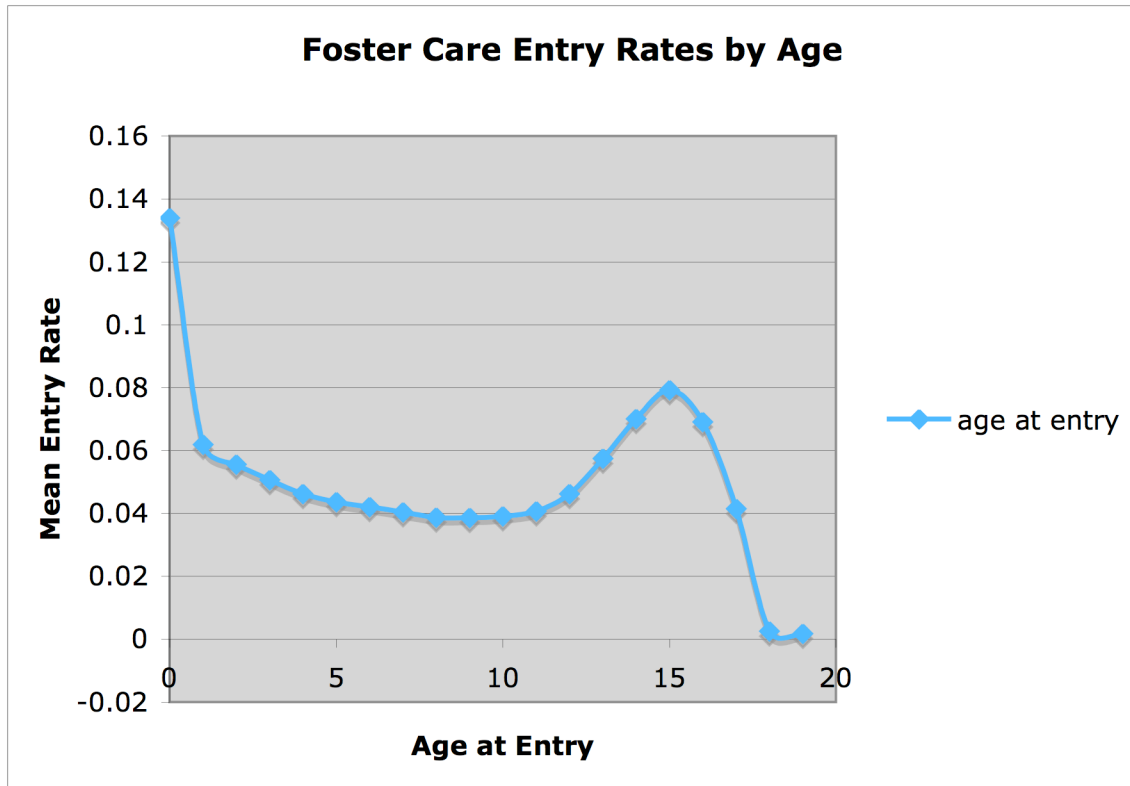
Donahue and Levitt [2001] argue that “if legalized abortion is the reason for the decline in crime, then one would expect that decreases in crime should be concentrated among those cohorts born after abortion is legalized.” They build on their original analysis by allowing their dependent variable (previously a crime rate for all ages) to only represent the crime rate for those who were born after the legalization of abortion. In an effort to follow this methodology, I calculated state specific foster care entry rates for each individual age beginning with zero and ending with nineteen. Figure 2 shows the mean foster care entry rates by age in 2000, calculated using data received in an email message from Michael Dineen at the National Data Archive for Child Abuse and Neglect (NDACAN), Cornell University.

The pattern in 2000 closely matches patterns seen in the years 2000-2005, and as can be seen, the empirical frequency distribution for children entering foster care is not a uniform distribution. The entry rates are highest for children under a year old and jump dramatically for children in their mid teens.³⁶ This lack of uniformity suggests that allowing the dependent variable to only include entry rates for children born after the passing of a parental involvement law could have significant effects. Unfortunately these data were only available for the years 2000 to 2005, so I did not change the dependent

³⁶ I was not able to find research concerning the reason for this jump in the mid teens, but it would be a very interesting topic for future research.

variable. If state and age specific entry rates could be collected for additional years, using them as the dependent variable would produce more conclusive results.

Figure 2
United States Foster Care Entry by Age
2000



SOURCE: National Data Archive for Child Abuse and Neglect (NDACAN), Cornell University

D. Controlling for Non-Minors

The problem of unintended pregnancies is particularly evident for women under the age of eighteen as demonstrated by the fact that by age twenty, approximately 40 percent of all teenage women have been pregnant and of these pregnancies, approximately 84 percent were unintended.³⁷ Despite the relevancy of this age group, the use of parental involvement laws as the explanatory variables in my model is restrictive in that it relies only on the effect the laws have on mothers who are under the age of

³⁷ Haas-Wilson [1996]

eighteen. This cuts out a significant portion of the population of women who have the potential to give birth to future foster care children. If the abortion restrictions on minors effectively increase foster care entry rates, one would expect the increase to be concentrated among those cohorts born to women under the age of eighteen. Testing this hypothesis is impossible with the current available data because information about the mothers of foster care children is very limited; however, if the data were to become available, the use of foster care entry data for only children born to minors would be another interesting empirical method to explore.

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VI. Appendix

Table 1¹
Timing of Parental Involvement Restrictions by
State, Type of Law, and Level of Enforcement

State	No Law	Notice Law Not Enforced	Consent Law Not Enforced	Notice Law Enjoined	Consent Law Enjoined	Notice Law Enforced	Consent Law Enforced
Alabama							1990-2005
Alaska			1990-2005				
Arizona							1990-2005
Arkansas						1990-2005	
California					1990-2005		
Colorado					1990-1998		1999-2005
Connecticut	1990					1991-2005	
Delaware			1990-2005				
District of Columbia							
Florida						1990-2005	
Georgia		1990-2005					
Hawaii							
Idaho							
Illinois				1990-2005			
Indiana							1990-2005
Iowa	1990-1996					1997-2005	
Kansas							
Kentucky							
Louisiana							1990-2005
Maine		1990-2005					
Maryland		1990-2005					
Massachusetts							1990-2005
Michigan	1990-1991						1992-2005
Minnesota						1990-2005	
Mississippi			1990-2005				
Missouri							1990-2005
Montana							
Nebraska				2005		1990-2004	
Nevada							
New Hampshire				1990-2005			
New Jersey							
New York							
North Carolina	1990-1995						1996-2005
North Dakota							1990-2005
Ohio		1990-2005					
Oklahoma	1990-2001						
Oregon	1990-2005					2002-2005	
Pennsylvania					1990-2005		
Rhode Island							1990-2005
South Carolina			1990				1991-2005
South Dakota							
Tennessee						1990-2005	
Texas	1990-1995						1996-2005
Utah						1990-2005	
Vermont							
Virginia							1990-2005
Washington	1990-2005						
West Virginia							
Wisconsin							1990-2005
Wyoming							1990-2005

¹ I apologize that this table could not appear in the body of the paper. It was extremely temperamental, yet crucial to the paper. I thus include it in the appendix. Special thanks to Leland McNabb whose original adaption of these data made my paper possible.