Public Policies and Private Decisions: An Analysis of the Effects of Abortion Restrictions on Minors' Contraceptive Behavior

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Abstract

In this paper, the author adapts a sequential game model from the fertility decision process to estimate the effects of abortion restriction laws on the decision to use contraceptives at first sex. The specific laws incorporated are parental involvement laws, discretely known as parental consent and parental notice laws. Including various controls, the estimated model yields evidence that the presence of an enforced abortion restriction law leads to an overall increase in the likelihood that a woman uses contraceptives at first intercourse. This result implies a long-term decrease in abortions demanded without an offsetting increase in births, due to pregnancy avoidance.

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Introduction

A recent study in *The New York Times* reported that parental involvement laws that restrict abortion access appear to have either a negligible effect—or no effect at all—on the abortion rate (Leland 2006). Researchers immediately began to question this conclusion[1], but it is not entirely out of line with past findings related to these laws. Previous work has found a statistically significant decrease in the abortion rate due to the laws, but the magnitude of the estimated effect has been small, and in some cases has been insignificant. This new research does not eliminate the possibility of any effect, but rather brings into question the true magnitude of the laws' effects.

The small decrease in abortions the *Times* study found contradicts the direction of the effect as estimated in previous studies. According to Phillip Levine (quoted in Leland's article), however, the results are unsurprising. Many models used to measure the effects of such laws rely on broad statistics that could miss the effects such laws have on individual women. Sweeping measures such as abortion rates and birthrates are computed as ratios in an attempt to determine the effect on pregnancies indirectly. The lack of individual detail indicates that the current body of research may estimate the decision process too imprecisely to determine the true effects.

Past studies investigating associations among abortion rates, birthrates, Medicaid payments, restrictiveness of laws across time, and several other variables have produced inconsistent results (Levine 2002). The inconclusiveness of the entire body of work does not necessarily indicate that these factors are not in the explanatory set, but rather that the models are subject to confounding from earlier decisions. In other words, it may be that

[1] Michael New points out some of the shortcomings in Leland's article in "A lesson in data and analysis for the *New York Times*," found on the Heritage Foundation website (<http://www.heritage.org/Research/Family/wm1009.cfm>).

intermediate decisions in the fertility process mask the laws' effects. Specifically, a woman's decision to use contraceptives more or less carefully could mitigate these effects. Sen (2003) mentions the possibility of masked effects, pointing out that more careful contraceptive behavior would decrease the number of pregnancies and, therefore, the number of desired abortions. This decreased demand resulting from fewer pregnancies would affect the measurement of these exogenous changes and the specific elasticity of abortion demand.

This paper investigates decisions about contraceptive use using a method similar to that of Argys (2002), though not as complex. I hypothesize that a woman's decision making about contraceptive use at first sex is responsive to restriction laws on abortion at points before the decision to abort. Therefore, the long-term result of these laws should be a decrease in abortions and a coincident decrease in the number of pregnancies for the affected age groups. This hypothesis has been supported in the past by Levine (2002), and indirectly by Haas-Wilson (1996). I will adapt a multi-stage game from an expanded decision model based on those by Lundberg and Plotnick (1995) and Akerlof et al (1996). I then use the decision process as a model to estimate the effects of various environmental forces on women's decisions to use contraceptives.

The second section provides background on the literature related to the effects of abortion laws and other factors on incidence of abortions. The third section elaborates on the model estimated and the theory behind it. The fourth section contains the results of the analysis and a discussion of their significance. I conclude in section five.

Section II: Background

One highly publicized method of controlling teen pregnancy involved laws dictating parental involvement in the abortion decision. These laws dictate either that teens must obtain parental consent in order to abort a pregnancy, or notify at least one parent of their decision. Kane (1996) looked at the effects of these laws on birthrates, but he admits that his findings, though suggestive, are questionable. The largest observed negative effects for out-of-wedlock births occur in "isolated counties," or counties more than 50 miles from an abortion provider. This finding indicates that other factors not accounted for may affect the results. As a control he evaluated the effects of these laws on older women as well as on minors. The basic assumption is that parental involvement laws should not have any effect on the choices of older women, but Kane's model generated significant coefficients on the parental consent terms, even for older women. However, the effects calculated are greatest for minors between the ages of 15 and 17, indicating that the laws have some effect. The significant coefficients for all age groups may indicate that there is a contributing variable not being considered. Using birthrates to investigate the effects of these laws may, as previously mentioned, be too rough a measure to establish what actually changes the birthrate. Therefore, use of an event that occurs chronologically earlier, such as contraceptive decisions, may remove some of the observed uncertainty.

Haas-Wilson (1996) considers another possible source of confounding that makes evaluation of parental involvement laws difficult. She examines the effects of the laws by accounting for whether laws requiring either consent or notice are enforced. The results suggest that enforcement of these laws indeed does reduce minors' demand for abortions. Surprisingly, when the author removes controls for statewide anti-abortion sentiment, the significant effects disappear. This difference may indicate that the laws make abortion more costly for minors if their parents are more opposed to abortion. The observed effects of unenforced laws were insignificant, further supporting the importance of enforcement.

Haas-Wilson points out that the laws must be enforced to increase the actual costs to minors, so the potential additional effect of parents' sentiment is irrelevant. The necessity of enforcement of these laws may place another cost upon their implementation. However, her results also indicate that unenforceable statutes may have an effect; the variable Haas-Wilson includes to account for unenforced statutes has a positive effect on the ratios "abortions/minors" and "abortions/births." Because either an increase in abortions or a decrease in births may change the ratio, the exact effect of such statutes is ambiguous. Therefore, I include both a law term and an interaction term (law x enforcement) in the present model. Information on laws for the relevant years is contained in Table 1.

Levine (2002) posits that effects from law changes do not require extensive foreknowledge on the part of minors seeking abortions. He incorporates a 1-year lag in his specification, both to account for the length of pre-existing pregnancies and to account for effects of mid-year enactment that might be lost in annual data collection. If the law is not enforced, then the minor does not need to account for the additional costs involved with parental notification or consent, and because of this, the law may no longer have an effect for that individual. However, an enforced law may negatively affect the decision to abort, due in part to parental sentiment. Similar to Levine, I maintain a 1-year "lag," under which newly enacted laws do not "exist" until the following year. This stipulation implies that if a law, for example, were enacted in 1988, its assumed effect does not begin until the following year; therefore, such a law would affect only the 1995 portion of the data.

Table 1: Parental involvement restrictions by state and year

State	Type	Year Enacted	Status
Alabama	Consent	1987	Enforced
Alaska	Consent	1949	Not Enforced
Arizona	Notice	1982[a]	Enforced
Arkansas	Notice	1989	Enforced
California	Consent	1987	Enjoined[b]
Colorado	Consent	1963[c]	Enforced[d]
Connecticut	Notice[c]	1990	Enforced
Delaware	Consent	1953	Not Enforced
District of Columbia	n/a		
Florida	Notice	1973	Enforced
Georgia	Notice	1987	Not Enforced
Hawaii	n/a		
Idaho	Notice	1973	Not Enforced
Illinois	Notice	1977[e]	Enjoined
Indiana	Consent	1974	Enforced
Iowa	Notice	1996	Enforced
Kansas	Notice	1992	Enforced
Kentucky	Consent	1982[f]	Enforced
Louisiana	Consent	1978	Enforced
Maine	Notice	1979	Not Enforced
Maryland	Notice	1983	Not Enforced
Massachusetts	Consent	1974	Enforced
Michigan	Consent	1991	Enforced
Minnesota	Notice	1981	Enforced
Mississippi	Consent	1986	Not Enforced
Missouri	Consent	1979	Enforced
Montana	Notice	1974	Not Enforced
Nebraska	Notice	1981[g]	Enforced
Nevada	Notice	1981	Enjoined
New Hampshire	Notice[g]		Enjoined
New Jersey	Notice	1999	Enjoined
New Mexico	Consent	1969	Not Enforced
New York	n/a		
North Carolina	Consent	1995	Enforced
North Dakota	Consent	1981	Enforced
Ohio	Notice	1974	Not Enforced
Oklahoma	Notice	2001[h]	Enforced
Oregon	Consent	1969	Repealed[j]
Pennsylvania	Consent	1974[k]	Enjoined
Rhode Island	Consent	1982	Enforced
South Carolina	Consent	1974[l]	Not Enforced
South Dakota	Consent	1973	Not Enforced
Tennessee	Notice	1973	Enforced[m]
Texas	Consent	1995	Enforced
Utah	Notice	1974	Enforced
Vermont	n/a		
Virginia	Consent	1973	Enforced
Washington	Consent	1970[n]	Repealed
West Virginia	Notice	1984	Enforced
Wisconsin	Consent	1985	Enforced
Wyoming	Consent	1989	Enforced

a. Statute was replaced in 1989 with a consent law.

b. Law was modified in 1992 and 1995, then enjoined in 1997.

c. Law was enjoined in 1975.

d. The enjoined law was replaced in 1998, and the new statute is enforced.

e. The law was apparently enjoined before 1990, and the new (1995) statute was enjoined in 1996.

f. Some sources report that this statute was enjoined, possibly before 1990. Amended in 1998.

g. No year of enactment could be found, but the law was enjoined in 2004.

h. Additional statutes were enacted in 2005.

j. This law was not enforced, and then a separate 1973 law was repealed.

k. Pennsylvania's 1974 law was enjoined, and a new one was enacted in 1982.

l. South Carolina enacted another law in 1990 that is enforced.

m. The law was partially enforced in the 1980s, and a consent statute was enacted in 1988.

n. This law was apparently enjoined and then repealed at some point before 1990

Adapted from Haas-Wilson (1996), Greenberger & Connor (1991), and the NARAL pro-choice America website <http://www.prochoiceamerica.org/whodecides>

The information one teen obtains in seeking an abortion may spread among peers or remain secret. The spread of information among peers may imply that, even if only one individual discovers new information, it could reach many people, even if the individual initially tells only one person. Alternatively, conversations on the subject may arise which reveal such information to minors, whether they take place among family members or peer groups. If the information is kept secret, then the single individual will account for the costs in future calculations, and the future probability of pregnancy should decrease to mitigate the costs associated with abortion. If the information spreads, then other teens will include it in their calculations, and their decisions will also incorporate these costs. However, changes in decision making for these teens may evidence the time required for information to spread.

Haas-Wilson, however, does not consider the potential confounder related to adjacent states' policies. Teens may leave their home state and travel to neighboring states to have abortions if it is more convenient, creating a possible bias in abortion reports. Levine (2003) points out this shortcoming, but his results do not conflict with hers. Overall, his results suggest that parental involvement laws depress the pregnancy rate, because abortions fell without an increase in births. This finding is consistent with what I hypothesize to be the overall effect of these laws, in that behavioral alterations will prevent any long-term increase in the birthrate due to individual maximization of utility throughout the decision process. This maximization implies that at least some women who did not want to have a child before the law change will find new ways to avoid having a child after the change. Levine's results do not replicate the uncertainty observed in Kane's (1996) results, because estimations of the effects of parental involvement laws for older women are not significant. Although the results do not unequivocally point to the effectiveness of these statutes, they indicate that the statutes have at least some effect on teen pregnancy rates. The model offered here seeks to confirm Levine's hypothesis at the individual level.

Haas-Wilson (1996), like Kane (1996), uses the birthrate in her consideration of the effects, but she uses a calculated ratio of abortions and births. Because a decrease in the number of pregnancies could simultaneously decrease the number of abortions and births, such a measure cannot absolutely predict the effect of a parental involvement law. One might argue that the decrease in pregnancies occurred among a group that would have exclusively aborted anyway. If one accepts this argument, then the birthrate would not change, and pregnancy avoidance would simply be substituted for abortions. To account for the higher cost of obtaining an abortion, women would instead increase contraceptive use until the perceived risk of pregnancy decreases to an optimal level. Such a condition would yield results similar to those from Levine (2003).

Another aspect of availability that has been examined is the proximity and accessibility of an abortion provider, because the costs of transportation to the clinic must enter into the total cost of an abortion. Based on purely economic considerations, an increase in distance to the nearest provider should result in a decrease in the demand, because the transportation costs increase as the distance increases. Brown (2001) uses county-level data from Texas to evaluate the effect of distance to a provider on the demand for abortions. He finds that women who give birth tend to live farther from abortion providers, consistent with the above decision model. He considers the distance variable to be endogenous, which is a good assumption, especially if one assumes that providers that are too close may be undesirable for personal or social reasons. If a provider is located within 5 miles of a woman's home, she may choose not to use it because of her concern about encountering an acquaintance, which would result in additional negative emotional costs. This problem indicates that there may be a range of acceptable distances greater than some critically low value.

Work by Kane and Staiger (1996) yielded similar results to those of Brown (2001), but with one particularly interesting addition. They found that the observed effect on the birthrate arose primarily from a decline in in-wedlock births. This may partially explain the rise in teen births, especially if the observed increase arises from a proportional measure. If the number of out-of-wedlock births was relatively unaffected while the in-wedlock number fell, then the proportion of out-of-wedlock births would rise even though the absolute number remained unchanged. Therefore, the effect of distance on teens is ambiguous, and I do not consider it in the present model.

The authors also found that the changes in the birthrate after the proximity of an abortion provider increased or decreased were concentrated in the few years immediately following the change. Unlike Brown (2001), they assume that distance to a provider is exogenous, which may explain why they failed to find any significant effects for clinics that are within 50 miles. Also, their estimates set the distance to a provider located in the county of residence at zero, which may seriously bias their results toward zero. Though obscure, the chance that a woman living at the opposite side of a county with a provider may choose to travel to the adjacent county to have an abortion is probably greater than zero, so this possibility may also bias the data. Overall, these findings indicate that the effects on out-of-wedlock births are negligible, and the observed differences stem from changes in in-wedlock birthrates. Furthermore, the NARAL foundation website's[2] section "Who Decides?" provides current information regarding the percentage of counties with abortion providers. The unweighted average

[2] <http://www.prochoiceamerica.org/whodecides>, last accessed 04/08/2006. The organization is obviously choice-biased, but the information it provides is indispensable.

across states, excluding Alaska, Hawaii and Washington D.C., indicates that 79% of counties in a given state do not have abortion providers. This statistic indicates that abortion providers in a neighboring state may be closer for a given individual than those in the home state, making estimation of the effects of distance to a provider problematic.

The relative effects of policies on different races must be considered, especially since several studies have observed significant differences among races and ethnicities. Lundberg and Plotnick (1995) lament the difficulty in accurately determining effects for certain race groups due to small sample size, and potential reporting bias. Some studies have suggested, based on statistical evidence, that certain groups categorically underreport pregnancies and abortions (Lundberg and Plotnick 1995). Klerman (1999) finds significant different effects for blacks and whites arising from changes in legality of abortion and funding of abortion. Klick & Stratmann (2005) also find differences among races in safe-sex behavior, discussed below. Tomal (1999) does not use full controls for race in her model, but rather uses the percent of a given county that is white as a proxy for racial differences. This proxy indicates that the incidence of both births and abortions decreases as the percentage of whites in a county increases. To account for differences among races, I include indicators for four racial groups.

Tomal (1999) and Ohsfeldt & Gohmann (1994) compare the effects of laws across teens aged 15-17 and teens aged 18-19. The effects of consent and notice laws in both models are significant, but the specifications of the models prevent full comparison. Ohsfeldt & Gohmann compute the dependent variable as a ratio, whereas Tomal (1999) computes separate regressions. Tomal finds a decrease in abortions and an increase in births, given the presence of a law, for both groups of minors. However, Ohsfeldt & Gohmann (1994) find a decrease in the *ratio* of abortions in the two age groups given a law change, indicating that the restriction affects the younger group more. Though the laws should not affect decisions of non-minor teens, these results yield an ambiguous effect. Unless one assumes some form of habit persistence (Arcidiacono et al.), or, as Tomal points out, a much more significant effect from state sentiment, the results are not fully compatible. However, these differences may be due to the scope of the data in the two papers; Tomal uses county-level data from a few states, while Ohsfeldt & Gohmann use state-level data for the country.

Researchers have attempted to account for the problematic measurement of pregnancies by using other indicators of risk-minimizing behavior. Arguing that illegal abortions may negatively affect regressions using abortion rates and birthrates, Klick & Stratmann (2005) use gonorrhea rates to proxy for safe-sex behavior. They use data on the incidence of gonorrhea among women under 20 from 1981 to 1998, arguing that its short incubation period would decrease the lag effect that might result from use of other sexually transmitted diseases (STDs). Its curability might also be a benefit, because correlation of rates across time periods might be lower than it would for more persistent STDs. The authors do not find a significant effect in the population of black females under 20, while their results for white and Hispanic females are negative and significant. The problematic component of using STD rates as a proxy for safe sex is that the rates underestimate the actual incidence of birth control use, which may differ among racial groups. Therefore, a significant result despite this limitation is important. The authors offer explanations of the insignificant result for blacks that relate exclusively to family characteristics. I add that the difference may result, in some part, from use of contraceptive methods that do not guard against STDs. The present model takes a more direct look at what the above authors have largely investigated by proxy.

Section III: Method, Model, and Results

The fertility decision tree begins with the decision to engage in sexual intercourse, but there are many nodes between the initial decision and a birth. In addition to the nodes on the tree itself, there are hidden decisions that the woman may make—even when unaware—that affect the path followed. Each decision may affect the probability that a specific node is reached. For example, if a woman decides not to engage in sex, then no additional nodes are reached, because the process ends with abstinence. In this example, the probability of pregnancy is zero. Alternatively, if the woman decides to engage in sex and to use contraceptives, and no pregnancy results, then the process again stops, since no more decisions are necessary. The interactions within the tree are complex, and single exogenous environmental changes may affect multiple branches of the tree.

The fertility decision tree may be readily adapted into a multi-stage, one-player game, in which both endogenous and exogenous forces affect the probability of a specific outcome. If one assumes rational behavior and perfect information—or, at least, that participants believe they have perfect information—then the desired outcome in the final stage of the game will be the starting point in a woman's decision making. The model of the game in Figure 1 indicates that probabilities of pregnancy differ, but it does not indicate the inherent assumption that sexual activity generates some utility for the woman.

The assumption of perfect information may seem inappropriate, given the fact that unwanted pregnancies exist. I argue here that these details are unimportant to the overall game and merely affect intra-stage probabilities and errors. For example, risk of pregnancy may vary according to personal propensity to use contraceptives, proper use of contraceptives, and the exogenous statistical efficacy rate of the method. However, since a woman may simply want to decrease her (perceived) risk of pregnancy, the exact probabilities are irrelevant as long as some decrease in the likelihood of pregnancy results from use.

The actual game may take several forms, each constituting a part of the overall tree. If a woman does not want a birth, she may play two discrete games. In the first, she will choose abstinence, and the decision will result in zero pregnancies 100% of the time, and therefore no births. This game ends after one stage, because the final outcome of “no birth” results after her having made only one decision. In the second game, her decisions become more complex. If she chooses sex, the second decision involves contraceptives. The game may either stop at the second stage, as with abstinence, or it may continue to a third stage if a pregnancy results. The woman then must decide in the third stage whether to obtain an abortion, or to carry the pregnancy to term.

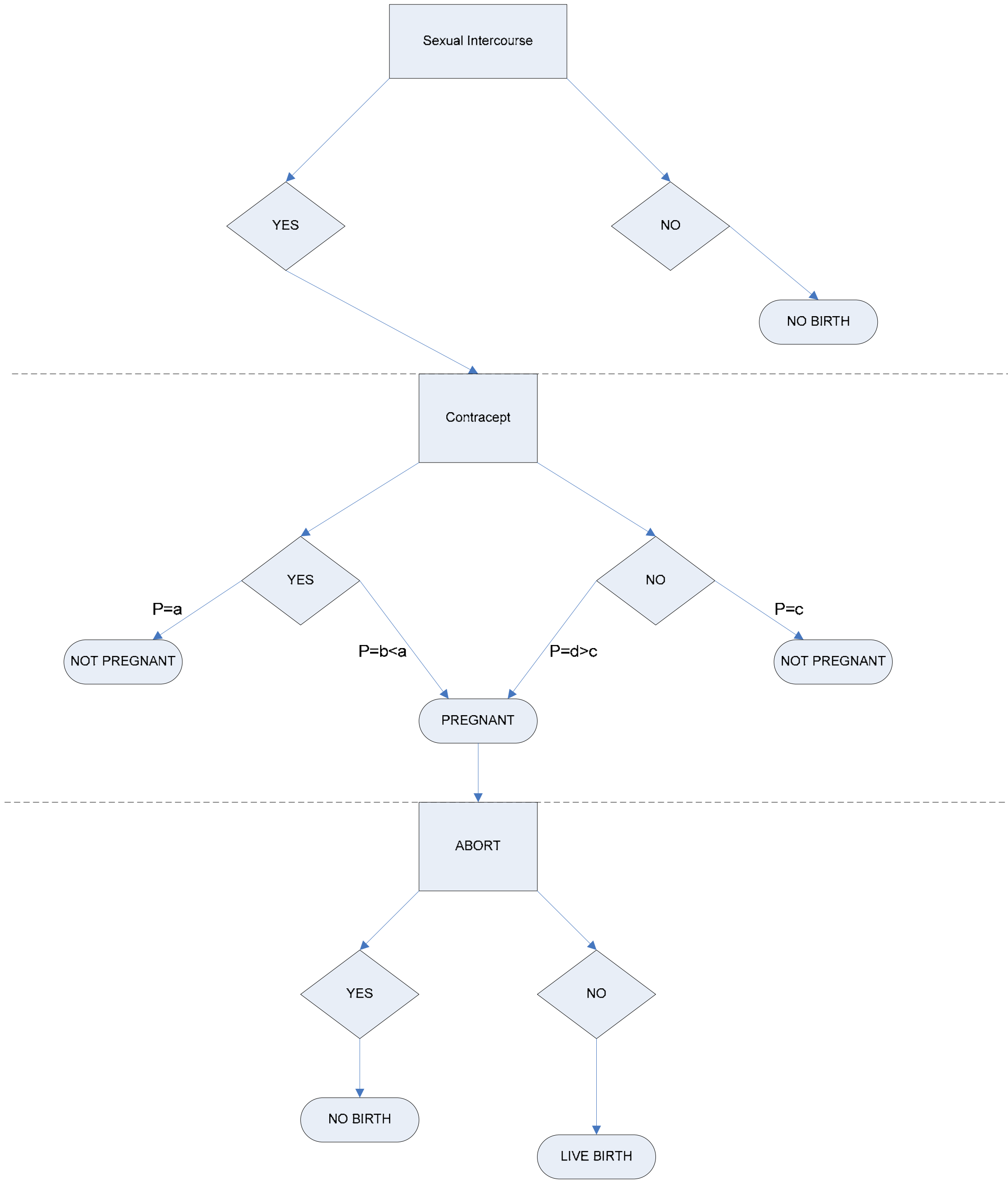

Figure 1: Adapted model of the fertility decision tree, applicable to a sequential, one-player game model

Uncontrollable events such as stillbirths, miscarriages and spontaneous abortions complicate the probabilities of this stage, but they do not figure into the decision making process. One may argue that the above are not completely uncontrollable, but for the purposes of this model I assume that women do not behave in unnecessarily risky ways that might cause permanent harm. Similarly, illegal abortions will not be an option. One may attribute this exclusion to prohibitive costs, primarily due to health risks. By eliminating this possibility, minors seeking an abortion must obey all relevant laws. In addition, Klick & Stratmann (2005) mention past work that does not detect any bias stemming from substitution toward illegal abortions. Specifically, Levine and Staiger (2004) investigated effects on the female death rate to determine whether an increase in obtaining illegal abortions might have resulted from substitution toward illegal abortion, but they find no significant increases, and therefore conclude that this substitution does not occur measurably.

At each stage in the game, there is a chance that the opposite outcome may result, and therefore a woman may change her behavior to maximize her expected payoff. Since contraceptives are not 100% effective, a woman's individual preferences will largely dictate her behavior, but it should be possible to glean some average propensity to contracept, given a set of environmental conditions. This game provides a model to determine which factors most heavily influence a woman's decision.

The data used come from cycles 3 (1982), 4 (1988), and 5 (1995) of the National Survey of Family Growth (NSFG), executed by the National Center for Health Statistics at the Centers for Disease Control (CDC). The NSFG surveys respondents aged 15-44 about fertility behavior and decisions, specifically related to contraceptive use, abortions, and use of family planning services. The respondent populations of cycles 1 and 2 consist only of women who have a child at home, and therefore they are selected from a biased universe of possible respondents. In cycle 3 and later, the survey includes women aged 15-44 regardless of marital status or presence of own children. Cycle 6 (2002) data are not used because privacy restrictions preclude any meaningful estimation that attempts to control for contextual variation. Public use data from cycles 3-5 have contextual information on region of residence, and this regional information is the basis for the estimation of contextual controls that I generate. Since the data is aggregated to the regional level, there is no straightforward way to evaluate the effects of abortion restriction laws at the state level. Regardless, the use of individual responses rather than gross rates may provide greater insight into a woman's decision making.

For use with the NSFG data, I aggregate abortion laws to a regional "index." Two indicator variables, one for the existence of a parental involvement law and the other for an interaction with its enforcement, are weighted by the Census-estimated number of females aged 15-24 in the given state in the year of the cycle data. That is, the presence of the law is multiplied by the state's age 15-24 population, then divided by the age 15-24 population of the entire region. The average for the region in a given year is applied as a fixed value for respondents in that region who would be affected by the laws. Individual analyses were performed with cross-sectional, single-year data first before any pooling across years. I do not include border effects, since Levine (2003) maintains that their inclusion does not significantly alter results.

Individual factors that make one woman more likely than another to contracept may relate to unobservable social or cultural characteristics. However, these characteristics may be systematically related to the woman's religion, race, and living conditions. The dependent variable is contraceptive use at first intercourse. The probit models I estimate are variants of the following general form:

$P(\text{contraceptive use}|X_1,\ldots,X_n) = \Phi[\beta_0 + \beta_1(\text{Ablaw, Abenf}) + \beta_2(\text{Abortion interaction terms}) + \beta_3(\text{Talkany}) + \beta_4(\text{Age}) + \beta_5(\text{Age}^2) + \beta_6(\text{Year fixed-effects}^{\S}) + \beta_7(\text{Live14}) + \beta_8(\text{Religion Dummies}) + \beta_9(\text{Incpov}) + \beta_{10}(\text{Urban-Rural Dummy}) + \beta_{11}(\text{Metro-NonMetro Dummy}) + \beta_{12}(\text{Race Dummies}) + \beta_{13}(\text{Momeduc})]$

§--these variables will enter into the equation only in specifications over time

"Ablaw" and "Abenf" are the primary variables of interest in the regression. "Ablaw" is the index variable that denotes the presence of an abortion law in a given region, while "Abenf" denotes whether the law is enforced.

I use indicator variables in the regression to control for race and religion, and also to account for the following contributory factors: Urban-Rural living, Metro—NonMetro living, "Talkany" (a dummy indicating discussions about fertility decisions with parents), and "Live14" (an indicator of whether a woman lived with both own parents at age 14). The Rural-Urban indicator denotes whether a woman lives in a rural or remote area, whereas the Metro-NonMetro indicator relates to the size of the area. The Live14 indicator serves as a proxy for the unity of the woman's household during formative adolescent years. Talkany is an indicator that has been conflated to proxy for parental sentiment and also the relationship between parents and child. The variable is equal to 1 if the woman ever discussed either STDs, pregnancy, birth control, or the menstrual cycle with one or both parents.

Momeduc serves as an additional proxy for the parent-child relationship. In this case, the variable attempts to capture the role-model aspect of the relationship that may arise from high educational attainment. Such attainment may suggest ambition and a desire for success that affect the upbringing of the child. Incpov (Income Relative to the

Poverty Level) is used as a control to isolate the specific effect that low income might have on contraceptive decisions.

The first three probit estimations use only 1 NSFG cycle each. These regressions are set to include only the respondents aged 18 or younger, so the statistical power of the regressions is weak, as can be seen by the small number of observations. Across the three unpooled regressions, the average n is about 521. In these models, the probit estimation is run using Ablaw and Abenf values for the year of the cycle (i.e., if it is 1988, the abortion law indices for 1988 are used). For consistency, I control for religion, race, metropolitan residence, rural residence, and age at first intercourse. In spite of the small number of observations, the regression yields estimates for several coefficients that are consistent with theory. For example, in the cycle 4 (1988) specification, the coefficients on Age, Talkany, Incpov, and Momeduc are all positive. This indicates that an increase in either age at first intercourse, income relative to the poverty level, or mother's education increase the likelihood of contraceptive use at first intercourse, all other things constant.

After running individual regressions with each set of cross-sectional data, I estimate pooled regressions. The first contains only respondents from cycles 4 and 5, and the second includes respondents from all three cycles. These pooled cross-sections were assembled in an attempt to counteract the weak statistical power that plagues the relatively small number of observations in the NSFG data. I use the calculated indices Ablaw and Abenf in the pooled cross-sectional model to control for regional sentiment related to abortion. Each observation had the proper Ablaw and Abenf index applied to it by matching region_i and year_t across the years.

I then added 6 interaction variables to try to isolate the effects of the laws on decision making. These interaction variables are tripartite. First, an indicator variable for three separate age ranges was created by adjusting age responses from the data to 1995 age (age95). The three variables grouped age95 according to what laws should apply to the respondent, given her age95. Applicability of 1995 laws was set only for women whose age95 was less than or equal to 18. Likewise, the 1988 laws were applied to women who would have been 18 or younger in 1988, or whose age95 was between 19 and 25. The 1982 laws were applied to women between 26 and 31. The second part of the interaction term consisted of the regional index for the separate Ablaw and Abenf. The third part of the term was designed to limit the regression of the indices to appropriate respondents. A second indicator was created equal to 1 if the respondent's first intercourse occurred before age 18. Thereby, the specific law's applicability in decision making becomes dependent on the respondent's minor status at first intercourse.

After the first two pooled regressions, I ran a separate regression that excluded all individuals whose first intercourse was after age 18. Although the coefficients' magnitudes change, all signs are preserved except for that on othrel, which controls for a statistically small part of the sample. This fact may indicate that all of the abortion indices are subject to confounding, due to the crudeness of the estimation.

Sen (2003) mentions potential problems with the NSFG data[3], but it is the best available data for an individual-level regression. Sen used gonorrhea rates as a proxy to measure safe-sex behavior, and indeed, use of STD rates provides a useful instrumental measure of behavioral changes, because pregnancy rates and STD rates may be

[3] The author mentions "concerns about endogeneity between Medicaid restrictions and unobserved state attitudes. However, by including controls for characteristics such as religion and religiosity, I hope to eliminate this potential bias.

correlated. However, using this measure discounts several important facts, some of which were mentioned above. First, summary statistics in the NSFG Cycle 4 data indicate that the most popular method at first sexual intercourse is the birth control pill, followed by the condom. Condoms and diaphragms protect against both pregnancies and STDs, but unfortunately birth control pills do not. Therefore, birth control pill users may be incorrectly assumed into a non-user group associated with STD transmission. Sen briefly mentions that current-period STD rates are not independent of the observed rate in the previous period, and it may be difficult to estimate the extent to which such a dependency would bias results. Therefore, the use of the NSFG data under the current stipulations provides more support for the effects of the laws while mitigating some of the concerns that Sen and others raise.

Section IV: Discussion

Although the model yields estimates of negative coefficients on two of the enforcement terms, the marginal effects of the two negative terms are smaller than that of the positive terms. Overall, this result points to an effect of abortion restriction laws on women's decision making (i.e., probability) in contraception, despite the lack of fine adjustment in the indicators. Considered separately, it appears that these results contradict the implications of the paper by Haas-Wilson (1996), because enforcement of the law (according to the abenf881 coefficient in the pooled regression; see table 4) results in an almost 44% decrease in the probability of contraception. However, one must remember that the law cannot be enforced if it does not exist. Therefore, the overall marginal effect of an enforced abortion restriction law is a 55.4—43.6≈12% *increase* in the probability of contraceptive use at first intercourse. Thus, under this individual-respondent stipulation, the results remain consistent with those from previous pieces of research that use broader measures than individual choice. The presence of an abortion restriction law increases the likelihood of contraceptive use, which would in turn decrease the likelihood of pregnancy. A decrease in the likelihood of pregnancy would decrease the number of abortions demanded, and also decrease the number of live births.

The statistically significant coefficients persist for black and Hispanic subgroups, but this model does not provide an answer to the question of what causes the difference. Rather, the model indirectly poses another question: why, given some set level of income, education, and other such variables, would a black or Hispanic woman be 5-10% less likely to contracept at first intercourse than a white woman? I again refer to potential unobserved cultural differences which, in this specification, lend weight to the arguments of Klick & Stratmann (2005) about differing cultural norms, rather than to my hypothesis regarding divergent contraceptive choices. If divergent contraceptive choices across races were the source of variation, there should be no observed difference in likelihood of contraceptive use; however, these results suggest that it is not a difference in choice, but rather in overall use. Some sort of family or "neighborhood effect" (see Averett et al. 2002) may exist that biases young black or Hispanic women away from contraceptive use.

Age at first intercourse has an effect on the likelihood of contraceptive use that is consistent with the theory underlying the model. The older an individual is at first intercourse, the more likely they should be to be circumspect and cautious. Alternatively, younger individuals might be too embarrassed to purchase contraceptives, or the individuals might be afraid of the consequences if their parents were to find them. These hypotheses are partially confirmed by reported reasons for non-use in Jones et al. (2002). The loss of all significance in the last stipulation is surprising, but since age at first sex must occur before age 18, the spread of values for this variable is substantially reduced; the signs are preserved, however, so the results do not become inconsistent.

The variable Talkany is significant, as hypothesized, implying that parental sentiment and the closeness of the relationship between the parents and daughter may affect the likelihood of contraceptive use. Specifically, the results indicate that having talked with parents about any of the aforementioned topics increases the likelihood of contraceptive use by around 10%. If one infers from this "talk" that the parental relationship is strong, or at least that parental sentiment is known, then these results are consistent with theory.

The marginal effect of income relative to poverty level is positive and strongly significant. This sign is consistent with theory, and the magnitude seems reasonable, given that a unit increase would be a 1% increase relative to the poverty level, and the data record entries up to over 900% of the poverty level. This result suggests that poverty is indeed a contributing factor in the teen pregnancy and childbirth problem.

The sign on the proxy Live14 is consistent with theory, but its small magnitude might have several explanations. The simplest explanation is perhaps that teens' contraceptive choices are not significantly affected by divorce. This conclusion, of course, relies on an average that might be biased downward. If more emotionally taxing separations have a larger negative effect, but there are relatively few in the data, then the true effect might be masked.

Like Live14, the magnitude of the effect of Momeduc is small, but its sign is consistent with theory. As with income, this variable is continuous, so the magnitude, though small, is not unreasonable. More education should, in theory, make the parents value their children's education more. Therefore, if a parent stresses the child's education, the child may be more responsible and circumspect when engaging in activities that could hinder future opportunities.

Unfortunately, under the pooled specification the nature of the data prohibited the inclusion of religiosity controls. Given the potentially long period of time between the survey and the respondent's first intercourse, current religious beliefs may be an inconsistent measure of previous sentiment. Therefore, one cannot glean much information from the estimated effects on contraceptive use. A variable to assess religiosity at age 14 exists, but I omitted it for continuity across the regressions in an attempt to maintain statistical power of the overall model. In addition, religious service attendance at age 14 may not be a personal choice, and it may therefore proxy at the least for parental strictness. Given the evidence that religiosity may have a statistically significant effect on fertility decisions (cf. Arcidiacono et al.), such controls should be included in future research.

Averett et al. (2002) also used NSFG data in their research, but they used only one cycle of the data. The use here of several cycles to create pooled cross-sections improves on the statistical power that Averett et al. had, but the lack of state-level data limits the precision of the estimations. A logical extension to future research would be to combine the two methods and use state-level data with pooled cross-sections to achieve both greater statistical power and precision in variable estimation. Such a combination might provide the level of detail necessary to determine accurately the magnitude of the effects of restriction laws.

The use of such a model with individual results could provide increased insight into the mechanism behind observed changes, as it may with racial differences in contraception. It may be reasonable, once observable cultural variables have been controlled for, to assume that significant differences across race stem from unobservable characteristics.

Section V: Conclusion

As researchers continue to uncover nuances related to fertility behavior, they must include past discoveries that may extend the utility of their work. I have attempted here to develop an effective proxy for individual decision making when fine contextual detail is not available. This model provides a useful platform for future extensions involving more elaborate controls and more stringent measurements of both abortion sentiment and restrictions. In spite of the relatively broad specification, the results appear to be consistent with past findings. This fact may imply that more narrow specification could result in important breakthroughs related to understanding of how women play the "fertility game."

Tree diagrams make the fertility decision process appear straightforward and uncomplicated. However, the models eliminate signs of the feedback that occurs based on changes in payoffs. Changes in these payoffs are what dictate changes in fertility behavior and, consequently, changes in birthrates, pregnancy rates and abortion rates. When analyzing fertility effects on such a broad scale, it is important to remember that, no matter how broad a measure one is using, the ultimate source of observed changes is the individual woman.

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Variable	1982	1988	1995	Pooled 1	Pooled 2	Pooled 3
Observations	6781 obs.	7635 obs.	9881 obs.	17516 obs.	24297 obs.	13503 obs.
Age	28.83 (7.7)	30.64 (7.6)	31.67 (7.7)	31.22 (7.7)	30.55 (7.7)	28.63 (7.9)
Protestant %	69.650	66.274	54.681	59.734	62.502	65.845
Catholic %	21.516	24.545	28.914	27.010	25.476	21.329
Jewish %	1.593	1.244	1.154	1.193	1.305	0.889
Other Religion %	1.593	1.716	3.998	3.003	2.609	2.214
Mother's Ed.	10.86 (3.16)	11.2 (3.2)	11.2 (3.2)	11.21 (3.4)	11.11 (3.4)	11.049 (3.24)
Age at first sex	17.34 (2.8)	17.64 (2.9)	17.64 (2.9)	17.57 (3.0)	17.51 (3.0)	15.521 (1.39)
Am. Indian %	1.032	2.816	3.188	3.026	2.469	2.614
Asian %	1.195	1.847	2.459	2.192	1.914	0.963
Black %	43.017	33.608	23.894	28.129	32.284	39.606
White %	54.712	62.750	70.964	67.384	63.847	57.513
Hispanic %	6.710	7.243	14.229	11.184	9.935	8.576
Rural %	35.791	22.449	12.863	17.042	22.274	22.188
Metro %	78.248	78.533	80.619	79.710	79.302	78.227
Talkany %	72.157	74.499	55.632	63.856	66.173	65.77
Income relative to poverty level	265.638 (193.74)	296.359 (198.56)	316.922 (221.98)	307.959 (212.33)	296.148 (208.17)	262.569 (201.52)
Northeast %	17.829	19.502	18.804	19.108	18.751	17.263
Midwest %	22.552	38.468	24.026	30.321	28.106	28.668
South %	44.713	25.396	34.632	30.606	34.543	36.651
West %	15.072	16.634	22.538	19.965	18.599	17.418

Table 2: Means, variances, and percentages of the data

Year	Age		
1982	<=18		
83	19		
84	20		
85	21		
86	22		
87	23		
1988	<=24	<=18	
89	25	19	
90	26	20	
91	27	21	
92	28	22	
93	29	23	
94	30	24	
1995	<=31	<=25	<=18

Table 3: Derivation of age95, used to calculate the applicability portion of the interaction terms in the pooled cross-sectional regressions

	1982	1988	1995	pooled 1	pooled 2	pooled 3
	611 obs	449 obs	502 obs	17166 obs	23940 obs	13315 obs
ablaw	0.0593	-0.272	-0.0398	-0.0961***	-0.122	-0.117***
abenf	-0.222	0.27	0.0041	0.0449**	0.0663	0.0423
ablaw821					0.197**	0.201**
abenf821					0.0479	0.113
ablaw881				0.53***	0.552***	0.638***
abenf881				-0.441***	-0.436***	-0.508***
ablaw951				0.51***	0.548***	0.557***
abenf951				-0.037	-0.0524	-0.00021
age1stsex	0.364	0.292	0.548***	0.11***	0.107***	0.0876
age1stsex2	-0.0107	-0.00797	-0.0178***	-0.00223***	-0.00217***	-0.00132
talkany	0.013	0.0434	0.0131	0.113***	0.0961***	0.0999***
amind	-0.164	0.0741	0.0405	-0.0172	-0.0281	-0.0187
asian	-0.269*	-0.183	-0.14	-0.0672**	-0.0826***	-0.0123
black	-0.0862*	-0.859	-0.114**	-0.0621***	-0.0667***	-0.0603***
hispanic	-0.1482**	0.000843	-0.134**	-0.133***	-0.119***	-0.0859***
norel	-0.0562	-0.0297	0.012	-0.0238*	-0.0304**	-0.0116
cath	0.0295	-0.123*	0.0559	-0.0277***	-0.0291***	-0.0197
jew	0.317*	Drop	Drop	0.0212	0.0954***	0.146***
othrel	-0.137	0.0838	-0.0523	-0.00726	-0.00755	0.0362
live14	0.0849*	-0.011	0.0149	0.022**	0.0305***	0.0249**
incpov	0.000306**	0.000292*	-0.000019	0.000225***	0.000233***	0.000275***
rural	0.0288	0.127**	0.0217	0.0216*	0.0158	0.0337***
bigcity	-0.0441	-0.0714	0.0555	-0.0141	-0.0213**	-0.00763
momeduc	0.0289***	0.0208*	0.0156**	0.0163***	0.015***	0.0117***
I88				-0.0775***	0.0487***	0.018
I95					0.124***	0.0746***

Table 4: Marginal effects from single-year and pooled-cross-sections regressions

*--significant at the 0.1 level

**--significant at the 0.05 level

***--significant at the 0.01 level