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FREQUENCY OF SEXUAL ACTIVITY AMONG UNMARRIED ADOLESCENT GIRLS:

DO STATE POLICIES PERTAINING TO ABORTION ACCESS MATTER?

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INTRODUCTION

Over the last decade, there has been a marked increase in state laws regulating abortion in this country. For example, whereas in 1988 only 13 states required parental notification before a minor could obtain abortion, at present all but 9 states have that requirement. In 1976 the Congress passed the Hyde Amendment which cut off Federal funding of abortion for Medicaid recipients except in life threatening cases, thus leaving the choice of making such funds available entirely to the states. At present, 16 states fund all abortions sought by Medicaid recipients, 32 states only fund abortions resulting from rape or incest or life-threatening pregnancies, and 2 states only fund abortions in case of life-threatening pregnancies. In addition, 15 states have placed various restrictions on private insurance coverage of abortion procedures.

Each additional regulation increases the cost of an abortion and hence the opportunity cost of engaging in sex when a pregnancy is not desired. A substantial literature in economics has investigated whether the above restrictions affect the number of abortions obtained, particularly by minors, and the results largely indicate that restrictions do reduce such abortions. In contrast, whether the restrictions affect the *antecedents* of pregnancy – namely sexual activity and contraception use among minors – remains a relatively unexplored issue. Yet, this is without doubt an important issue. Adolescent pre-marital sexual activity is among the leading social problems in the USA. Statistics from the Alan Guttmacher Institute indicate that currently about 3 million teenagers – 25 percent of all sexually active adolescents — contract a sexually transmitted disease every year, and about 1 million teenage women – 11 percent of the *total* population of women between 15-19 years – experience an unplanned pregnancy every year. Adolescent childbearing is more common in USA than comparable developed nations like Great Britain, France, Canada and Sweden [Darroch, et al., 2001]. Hence, while policies promoting abstinence or responsible sexual behavior among adolescents are desirable, the question becomes whether state regulations restricting abortion actually contribute towards achieving that end. If the restrictions

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are effective in reducing access to legal abortion but not effective in reducing accidental pregnancies via promoting 'safe' sexual behavior among adolescent women, then there is cause for concern, because this may increase the demand for illegal abortions, or increase incidences of premature and unwanted motherhood with the potential for substantial economic and psychological costs for both mother and child.

To this author's knowledge, there exist only three papers in the current literature [Argys, Averett & Rees, 2002; Levine, 2001;2003] that directly consider the issue of state abortion restrictions on young women's sexual and contraception behavior. This work aims to add to that body of literature. Moreover, this work is first that considers *frequency* of sexual activity, as well as non-contracepted sexual activity, among adolescent women rather than simply the binary choice of being sexually active or not active.

The next section briefly reviews existing literature related to the impact of existing restrictions on abortion, as well as existing literature on adolescent sexual activity. Section 3 describes the model and the data. Section 4 reviews and discusses the results, and suggests some directions for future research.

LITERATURE REVIEW

The issues of interest pertaining to the effects of abortion restrictions on adolescent behavior are whether such restrictions actually reduce the number of abortions performed, and whether they deter the kind of behavior that leads to accidental pregnancies in the first place. The literature on the effect of state abortion restrictions on demand for abortion by adolescents may be divided into two broad categories. The first category considers temporal changes in abortion rates among minors at the state level following a restriction being effected. For example, Oshfeldt and Gohmann [1994] consider the effect of parental involvement laws, Kane and Staiger [1996] consider the effect of Medicaid funding restrictions, and Haas-Wilson [1996] considers the effect of both of the above on reported teen abortion rates in the state. The consensus is that restrictions do reduce teen abortion rates. The second category of studies uses individual level data to investigate whether abortion restrictions in the state of residence affect how young women resolve their pregnancies. Lundberg and Plotnick [1995] and Levine, et al. [1996] use NLSY79 data and find evidence that Medicaid funding restrictions negatively impact the probability of abortion. Joyce and Kaestner [1996] use data from three states between 1986 and 1991, and find that parental involvement laws reduce the probability of abortion among non-black minors in one state, but have no effect in the other states. However, the literature on whether or not the restrictions affect teen *birth rates* (which could provide evidence regarding whether abortion restrictions simultaneously deter pregnancy-risk behavior) yields extremely ambiguous results. The literature on the effects of abortion restrictions on birth rates is summarized in detail by Sen [2002a], and demonstrates that there is no clear consensus across studies regarding whether the restrictions increase, fail to affect, or decrease birth rates.

The next issue, then, is the direct effect of the restrictions on adolescent sexual activity. Much of the literature regarding adolescents comes from disciplines other

than economics. The focus is on age of first initiation into sexual activity and contraception use at first intercourse (rather than contemporary sexual and contraception behavior), and the role played by socio-demographic characteristics and environmental influences in deciding age of sexual initiation. A review of this literature is available in Moore, et al. [1995]. The emphasis is on the process of socialization of attitudes regarding sex, and how familial and environmental factors affect perceptions regarding 'appropriate' sexual behavior.[1]

In contrast to sociological models, the economic viewpoint emphasizes rationality, and decision making using the cost-benefit approach. According to this viewpoint, an adolescent woman's decision to participate in sexual intercourse and non-contracepted sexual intercourse should be made by weighing the *benefits* of such an activity (like physical gratification) against the *costs* of such activity – the most notable being the opportunity cost of an unwanted pregnancy in terms of lost educational, earnings and lifestyle opportunities. Easy access to abortion prevent the consequences of an unwanted pregnancy from being long-lasting, thus lowering the cost of (non-contracepted) sexual intercourse. Accordingly, restriction*s* that make abortion more inaccessible or costly *increase* the opportunity cost of sexual intercourse, and should therefore decrease one or both of participation in intercourse per se and participation in intercourse without contraception. This leads to the conjecture that, other factors equal, adolescent women residing in states with more abortion restrictions should be less prone to engaging in sexual intercourse than their counterparts in states with fewer restrictions. Alternately, there should be reductions in the propensity of adolescents *within* a state to engage in sexual activity following the passage of a new abortion restriction.

As mentioned before, I am aware of only three papers in the literature [Argys, Averett & Rees, 2002; Levine, 2001; 2003] that consider the direct impact of abortion restrictions on adolescent sexual behavior. The papers by Levine adopt the within-state approach. In the first study, the author uses data from the Youth Risk Behavior Survey (YRBS) for 1991, 1993, 1995 and 1997 and estimates binary models with state and year fixed effects for whether the respondent had sexual intercourse in the last three months, and if so, whether contraception was used. The restrictions of interest are availability of Medicaid funding for 'non-emergency' abortions, parental involvement laws, and mandatory waiting periods, and none are found to have a significant effect. The disadvantages are that YRBS includes very few individual level characteristics, and more importantly, that a very limited number of actual changes in restrictions occur within the first and last survey year. In only three, seven and six states are changes observed between 1991 and 1997 in Medicaid funding, parental involvement and mandatory waiting periods, respectively. Such limited within-state variability is, of course, a handicap in a model with state fixed effects and may be one reason why Levine finds no evidence of the restrictions affecting adolescent sexual behavior. In the second study, the author uses data from National Survey of Family Growth (NSFG) from 1988 and 1995. The author reports that the results are "generally weak, with little systematic pattern in the coefficients." However, while there is no evidence that parental involvement reduces sexual intercourse per se, it appears to have a

negative effect, with a p-value of about 0.08, on the probability of not using contraception at time of last intercourse.

Argys, et al. employ a cross sectional model data on 15 -19 year olds from the NSFG 1995, and binary models of sexual intercourse and contraception use at last intercourse. Results show no effect of Medicaid restrictions on either sexual intercourse or contraception use. Parental notification laws have a weak, positive effect on contraception use, but the results are not robust to inclusion of various county level controls.

A shared feature of the above studies is that they consider the discrete choice of whether or not to be sexually active at all over a certain period. Hence, they do not distinguish between adolescents who engaged in sex on a regular basis and those who may have had a single, chance sexual encounter in that period. In the area of contraception, Argys, et al. and Levine [2002] consider the probability of contraception use at the last sexual encounter only, while Levine [2001] separately considers the probability of contraception use at the first intercourse ever and most recent intercourse. It may be that the effects of state abortion restrictions operate in part through the *frequency* of sexual intercourse and/or non-contracepted intercourse that adolescents choose to indulge in. Hence, models that go beyond the binary choice of being sexually active at all, and consider the issue of frequency of sexual intercourse and non-contracepted intercourse, may be useful.

DATA AND MODEL

I employ data from the first round of the National Longitudinal Survey of Youth, 1997 (hereafter NLSY97). The NLSY97 was initiated in 1997, with a sample of 6,748 respondents representative of the U.S. population who were aged 12-16 years on Dec. 31, 1996, and a supplemental over-sample of 2236 Hispanic and black people of the same age group. A self-administered section of the survey asks all respondents who were 14 or older as of December 31, 1996 about their sexual behavior and contraception use history. Respondents are asked whether they have ever had sexual intercourse, whether and, if so, how frequently they have had sexual intercourse in the 12 months preceding the survey, the regularity of contraception use during intercourse, and the most frequently used method of contraception. In addition, respondents 15 and older as of December 31, 1996 are asked about their expectations about the future in terms of educational attainment. I use the sub-sample of never-married female respondents aged 15 or more in this study. The final sample size is 1724 respondents. The NLSY97 provides extensive information on socio-demographic characteristics of all respondents as well as their state of residence. This makes it possible to incorporate state level characteristics, including abortion restrictions that affect the 'cost' of sexual activity. The hypothesis of interest is whether, other things equal, adolescent women residing in states that restrict Medicaid funding for abortions and that have laws requiring parental involvement before a minor can obtain abortion engage in sexual intercourse per se and non-contracepted sexual intercourse less frequently than their counterparts in states without those restrictions.

The appropriate empirical model for sexual intercourse has to take into account the following factors: In the 12 months preceding the survey date, respondents partici-

pate in sexual intercourse a discrete number of times, which includes 'zero times' for those who did not participate in intercourse in that period. Given that the frequency of sexual intercourse can only take non-negative integer values, count-data models seem more appropriate than conventional regression models that implicitly assume that the dependent variable is continuous, though these models were also later estimated as part of specification tests. Sexual behavior is likely to be influenced by a vector of individual, county and state level observables as well as some unobservable characteristics. Furthermore, if respondents report not being sexually active in that designated time (that is, zero sexual encounters), then this can be due to any one of two underlying reasons: (1) that the respondent has previously been, or is prepared to be, sexually active, but simply happened to be celibate in the past 12 months; (2) that the respondent reports abstinence because she *would not choose to engage in sexual activity* at this stage of her life at all. The appropriate model that can incorporate all the above is the zero inflated negative binomial model.

The assumption is that there are underlying dichotomous variables $D_1 \ldots\ldots D_N$ which denote whether or not the respondent is willing to be sexually active, with $p_1, p_2 \ldots p_{N-}$ denoting the probabilities that the respondent is *not* willing to be sexually active at this life-stage (that is, $D_i = 0$). When D_i is 0, the corresponding explained variable Y_i (frequency of sexual intercourse in preceding 12 months) automatically takes the value of zero. When D_i is 1, the corresponding explained Y_i follows an independent negative binomial distribution (though Y_i may still take the value of 0) with parameter λ_i, where

$$\ln \lambda_i = \beta' X_i + \varepsilon. \tag{1}$$

X_i is the vector of observables and ε captures unobservable heterogeneity. Exp(ε) follows a gamma distribution with mean 1 and variance v^2.[2] Therefore

$$\text{Prob}(Y_i = 0 \mid \varepsilon) = p_i + (1 - p_i) R_i(0) \tag{2}$$

$$\text{Prob}(Y_i = j > 0 \mid \varepsilon) = (1 - p_i) R_i(0) \tag{3}$$

$$R_i(y) = \text{the negative binomial probability} = \frac{\exp(-\lambda_i)\exp(\varepsilon)\lambda_i^{y}}{y_i}! \tag{4}$$

It is assumed that the underlying dichotomous variable D_i is normally distributed, and

$$\text{Prob}(D_i = 0) = p_i = \Phi(\tau \beta' X_i). \tag{5}$$

Thus, the assumption is that there is a strong relation between the way in which observables affect the value of Y_i and how they affect the corresponding probability p_i.[3]

The modeling of non-contracepted sex is more challenging. Contraception use has typically been approached via one of three methods in the existing literature. The first method considers only the behavior of the sexually active sub-sample, a method that implicitly assumes that the contraception decision is made *only after* the decision to be sexually active is made and thereby ignores the possible inter-dependency of the two decisions. Studies that generalize from results pertaining to contraception behavior among sub-samples of sexually active teens without considering the possible interdependence of such behavior and the initial sexual activity [for example, Hogan, et al., 1985] implicitly make that assumption. The second method acknowledges that the sexually active sub-sample is a non-random selection of the full sample, and adopts a sample selection correction method to account for this non-randomness [Moore, Morrison & Glei, 1995; Argys et al., 2002]. Two problems with this method are that it requires normally distributed error terms to construct the Mill's ratio, as originally described in Heckman [1979] and is therefore incompatible with a negative binomial model, and also that it requires a valid identification restriction — in that there should be at least one variable that affects the decision to be sexually active but not the contraception decision. In reality, identifying a variable that can feasibly take this role is often problematic. The final method treats the decisions of sexual activity and contraception use as effectively non-separable decisions, and estimates reduced form models. This method is adopted by Brewster [1994], who estimates separate hazard models for the risk of experiencing a *contracepted* first intercourse by a certain age, and a *non-contracepted* first intercourse by a certain age. It is also used by Rees, et al. [2001] and Sen [2002b] when testing the effects of alcohol use on non-contracepted sexual activity. This is the approach that I adopt here. Accordingly, the explained variable, Z_i, now becomes the number of incidences of intercourse *without* any effective form of contraception in the last 12 months. The zero inflated negative binomial model is again adopted to account for the fact that an observation of zero incidences of non-contracepted sexual intercourse could arise from the fact that 1) the respondent is not averse to non-contracepted intercourse but simply did not participate in it during this time period; 2) the respondent would never participate in non-contracepted intercourse.[4] Again, there is an underlying dichotomous variable G_i, which is normally distributed and takes the value 0 with probability q_i. If G_i is 0, then the observed variable Z_i automatically takes the value 0. If G_i is 1, then Z_i follows independent, negative binomial distribution with parameter γ_i. Also,

$$\ln \gamma_i = \alpha' X_i + u, \tag{6}$$

where $\exp(u)$ follows a gamma distribution with mean 1 and variance s^2, and

$$q_i = \text{Prob}(G_i, I = 0) = \Phi(\kappa\alpha' X_i).$$

Precise formulations of likelihood functions are available from the author upon request.

In addition, I run estimations for non-contracepted sex using the sexually active sub-sample only. This is a useful specification test, but it must be kept in mind that due to the potentially non-random nature of the sexually active sub-sample, these results may not be generalized to the full sample.

In accordance with much of the existing literature, I include in X_i measures of physical maturity, familial structure, demographic characteristics, income, religion, environmental characteristics and so forth. In addition, I include measures of the state abortion policies of interest. A problem posed here, as with all cross-sectional studies of this nature, is that state abortion policies are likely to be correlated with unobservable beliefs and attitudes of the state's population, which may in turn influence the sexual behavior of teens residing in the state. Examples are, the degree of conservatism among the state population, their attitudes towards traditional family values, religiosity, sympathy towards women having reproductive 'choice,' and so forth. Barring availability of longitudinal panel data-sets with the required information on teen sexual activity that also have sufficient within-state variation in policies so as to permit using state fixed-effects and perhaps state-specific time trends, there is no 'perfect' way to control for this problem of unobservables and the bias they may cause in estimates of the effects of abortion policies. Though at the time of this study, multiple waves of the NLSY97 data were available, they do not help alleviate the problem, for there were extremely few changes in relevant state abortion policies over this time period. Hence, were the models re-estimated using multiple waves of data and state fixed effects, the policies' effects would be almost entirely subsumed in the state fixed effects.[5] However, one way to partially circumvent this problem in cross-sectional data is to do specification tests where additional state-level observables that can instrument unobservable state attitudes are included, and inspect whether the effects of the state abortion policies are robust to the inclusion of these instruments.

X_i contains the following: race (1 if black); ethnicity (1 if Hispanic, and also 1 if a language other than English is spoken at home); *age*; whether the respondent has *attained menarche*; a *religious dummy* indicates whether the respondent is affiliated to a religion that has published statements condemning abortion;[6] whether the respondent resides with *both biological/adoptive parents*; whether there is a *strict father (or father figure)* in the household; whether there is a *strict mother (or mother figure)* in the household; three binary indicators of family income (whether family income is *greater than 5 times* the poverty level, whether family income is *at or below poverty level*, whether family income information is *missing*); residence in an *urban* area; the respondent's *highest grade completed in school adjusted for age*;[7] the *probability with which the respondent believes she will complete a college degree by age 30*; and state policies. The primary state policies of interest are whether the state allows *Medicaid funding* for abortion procedures (*1 if funding is available*), and whether the state has a law enforcing *parental involvement* before a minor can obtain an abortion (*1 if law exists*). As discussed previously, one way to distinguish the effects of the policies from the effects of state-level unobservables that might influence both the policies and teen sexual behavior is to re-estimate the model with additional state-level controls. Accordingly, in the first robustness test, I also include whether the

state has in place a policy that *restricts insurance coverage* for abortion procedures,[8] whether the state education agency requires *sex and AIDS education in public schools*, the *maximum monthly AFDC payment* available to a family of four, and the *AIDS rate* per 100,000 population. Note that these policies are included primarily to capture state-level attitudes; their direct effect on teen sexual behavior is not of primary interest in this paper. In the second robustness test, I include in addition a binary indicator for whether the state had *liberalized abortion laws prior to Roe vs. Wade*,[9] *state divorce rates* per 1000 population in 1996, and a dummy for whether the state is in the *South*.

EMPIRICAL RESULTS & DISCUSSION

Of the 1724 respondents in my final sample, 456 (approximately 19 percent of all 15 year olds, 29 percent of all 16 year olds, and 33.5 percent of all 17 year olds) report having been sexually active in the 12 months preceding the survey. Table 1 gives the distribution of frequency of sexual intercourse, and Table 2 gives the distribution of the frequency of non-contracepted intercourse.[10] Note that about 50 percent of the sexually active sub-sample report at least one incidence of unprotected sexual intercourse, and about 10 percent of the sexually active sub-sample report more than 25 incidents of unprotected intercourse in the preceding 12 months. This indicates that many adolescents expose themselves to the risk of pre-marital pregnancy with disturbing frequency.

TABLE 1
Distribution of Frequency of Sexual Intercourse in Previous 12 Months.

Frequency Sexual Intercourse	Number of Respondents	Percentage (Full)	Percentage (Sexually Active)
0	1268	73.55	
1 to 5	159	9.23	34.87
6 to 10	80	4.64	17.54
11 to 20	76	4.41	16.67
21-50	67	3.88	14.71
51-100	29	1.7	6.37
More than 100	45	2.62	9.87
Total	1724	100	100

Notes: The sample consists of never-married adolescent females aged 15-17 as of December 31, 1996 from the first wave of the NLSY97. Column 3 gives percentages of full sample. Column 4 gives percentages of sub-sample who were sexually active in last 12 months.

Table 3 presents means and standard deviations of all variables for the full sample and for the sexually active and non-active samples separately. Table 4 presents coefficients, t-statistics, and marginal effects calculated at the mean for the zero inflated negative binomial model of sexual intercourse.[11] The first three columns present results when only the two state restrictions on abortion are included in addition to individual characteristics. Subsequent sets of columns present results after including

the additional state characteristics described earlier. Table 5 presents corresponding results for non-contracepted intercourse. The last set of columns in Table 5 includes results for the sexually active sub-sample only.[12]

TABLE 2
Distribution of Frequency of Non-contracepted Sexual Intercourse in Previous 12 Months.

Frequency Non-contracepted Intercourse	Number of Respondents	Percentage (Full)	Percentage (Sexually Active)
Sexually non-active	1268	73.55	—
0	226	13.11	49.56
1 to 2	61	3.56	13.39
3 to 5	48	2.8	10.53
6 to 10	36	2.09	7.9
11 to 25	35	2.06	7.7
26 to 100	37	2.19	8.14
More than 100	13	0.76	2.85
Total	1724	100	100

Notes: Intercourse with highly ineffective methods like withdrawal are included among non-contracepted intercourse. About 5 percent of the sexually active sample report relying on such methods. Column 3 gives percentages of full sample. Column 4 gives percentages of sub-sample who were sexually active in last 12 months.

Like most of the existing literature on adolescent sexual activity, results in Table 4 indicate that that older respondents have sex more frequently, respondents living with both natural /adopted parents and those with a strict mother figure at home have sex less frequently. Respondents who have strong expectations of completing college by age 30 have sex less frequently. This is consistent with the theory of opportunity costs, and may also indicate a negative relationship between innate intelligence/ability and propensity to have sex during adolescence. Being black or Hispanic does not appear to have any significant impact on frequency of sexual intercourse. This non-effect of race after family income and family structure are controlled for is in keeping with results from other studies like Brewster [1994]. The effects of family income is somewhat surprising, in that while adolescents from wealthy families appear to be less likely to have sexual intercourse than the control group, so do adolescents from poor families – though in the latter case, the effects are smaller and have weaker statistical significance. Religious beliefs and urban residence have the expected signs, but are statistically insignificant.

It is interesting to compare some of the results pertaining to the frequency of sexual intercourse per se with those pertaining to frequency of non-contracepted intercourse (Table 5). For instance, age has insignificantly positive effects when using the full sample and insignificantly negative effects for the sexually active sub-sample, suggesting that while older adolescents are more likely to be sexually active, younger adolescents are no less likely to have non-contracepted sex conditional on being sexually active. Similarly, while a foreign language spoken at home had no effect on sexual intercourse per se, it has a positive effect on non-contracepted intercourse in one of

TABLE 3
Means and Standard Deviations for Independent Variables

Variables	Description	Full Sample (N = 1724)	Not Sexually Active (N = 1268)	Sexually Active (N = 456)
BLACK	Black	0.282	0.256	0.355
		(0.40)	(0.43)	(0.48)
FORLANG	Foreign language at home	0.167	0.188	0.106
		(0.37)	(0.39)	(0.31)
HISP	Hispanic	0.197	0.213	0.151
		(0.39)	(0.41)	(0.36)
AGE	Age	15.78	15.74	15.90
		(0.69)	(0.69)	(0.68)
MENARCHE	Attained menarche	0.974	0.969	0.987
		(0.16)	(0.17)	(0.11)
FUNDRELG	Member of religious denomination opposing abortion	0.634	0.634	0.634
		(0.48)	(0.48)	(0.48)
STRICTM	Strict mother /mother figure in household	0.493	0.521	0.414
		(0.50)	(0.500)	(0.493)
STRICTF	Strict father / father figure in household household	0.493	0.461	0.331
		(0.49)	(0.50)	(0.47)
RICHHH	Household Income >=5 times poverty level	0.101	0.117	0.054
		(0.30)	(0.32)	(0.22)
POORHH	Household Income < poverty level	0.153	0.142	0.184
		(0.36)	(0.34)	(0.38)
TWOPAR	Lives with both natural/adoptive parents	0.497	0.549	0.351
		(0.50)	(0.49)	(0.47)
MISSHH	Household Income missing	0.303	0.305	0.295
		(0.45)	(0.48)	(0.45)
URBAN	Residence in Urban Area	0.592	0.588	0.606
		(0.49)	(0.49)	(0.48)
CHCOLL	Self-reported expected % chance of college degree	77.07	80.63	67.19
		(30.24)	(27.96)	(33.94)
ADJHGC	Highest Grade Completed (adjusted for age)	9.223	9.215	9.247
		(1.03)	(1.02)	(1.07)
MEDFUND	Medicaid funding available for abortion	0.373	0.380	0.353
		(0.48)	(0.48)	(0.47)
PARINVOLV	Enforced Parental involvement laws	0.244	0.238	0.261
		(0.43)	(0.42)	(0.43)
NOINS	Restrictions on private insurance	0.042	0.043	0.036
		(0.19)	(0.20)	(0.18)
AIDS	AIDS per 100,000 of population.	26.19	26.64	24.94
		(18.92)	(19.64)	(16.74)
SEX_ED	Mandated AIDS & Sex education.	0.542	0.540	0.546
		(0.49)	(0.49)	(0.49)
MAXAFDC	State Max monthly AFDC payments to family of 4	455.5	461.8	436.3
		(180.5)	(182.6)	(178.9)
LIBABOR	State liberated Abortion laws prior to Roe vs. Wade	0.398	0.406	0.371
		(0.48)	(0.49)	(0.48)
SOUTH	State is in South	0.373	0.363	0.405
		(0.48)	(0.48)	(0.49)
DIV96	Divorce Rate in State (per 1000 population)	4.908	4.886	5.024
		(3.30)	(2.88)	(3.42)

Notes: Standard deviations in parenthesis.

the models with the full sample, and in the model with only the sexually active subsample, suggesting that adolescents from such households who are sexually active may be less knowledgeable about or less able to access contraception. Belonging to a religious denomination forbidding abortion was not found to significantly decrease frequency of sexual intercourse, but it appears to decrease frequency of non-contracepted intercourse. Family income level or family structure does not by and large affect the frequency of non-contracepted intercourse significantly.

Regarding the two policy variables of primary interest, I find that the presence of Medicaid funding restrictions or parental involvement laws appear to affect neither sexual intercourse per se nor non-contracepted intercourse with statistical significance. In fact, the sign on the effect of Medicaid funding restrictions is sensitive to the inclusion of other state level variables, and it is actually counter-logical before the additional state variables are included. Afterwards, availability of funding has the expected sign, but falls well short of statistical significance. The effect of parental involvement laws has the expected sign, but nonetheless falls somewhat short of the 10 percent level of significance in all model specifications. In lieu of state-fixed-effects, I ran additional specifications where an interaction between Medicaid funding availability and the binary indicator of family income being at/below poverty level was included in all models. Arguably, while state-effects are relevant for all respondents, Medicaid funding restrictions are relevant only for respondents for low income families. Thus this helps show whether there are differences in the effects of Medicaid funding restrictions for respondents from poor families and respondents from all other families, assuming that state-effects are constant across the two groups.[13] However, the interaction term continued to be statistically insignificant (and its sign was sensitive to the model specification). This provided further evidence of the failure of Medicaid funding restrictions to notably affect sexual activity or contraception use. These results are available upon request.

The effects of some of the other state-level controls are noteworthy. Restrictions on insurance coverage on abortion procedures have a negative association with frequency of non-contracepted sex for the full sample (though it is insignificant for the sub-sample only). While this may simply be an artifact of unobserved state attitudes that variable is capturing, it may also be speculated that such a restriction on insurance policies serves as an incentive for parents to speak to their children about sex and contraception so as to avoid accidental pregnancies (an abortion procedure that does not require hospitalization costs about $400-$600 — which, while not prohibitive for non-poor families, might still give added incentives to parents to talk to their children about avoiding unwanted pregnancies). Since this restriction is not of primary interest, I do not delve into this further, but it suggests an interesting direction for future research. Also, interestingly, higher maximum monthly AFDC payments are associated with *less* frequent sexual activity. While the result contradicts conventional economic theory, there are related precedents in the literature. Previous work by Duncan and Hoffman [1990] find that higher AFDC benefits did not increase out-of-wedlock teen births among black women, and Schultz [1994] finds that not only did higher AFDC benefits *not* increase out-of-wedlock births among black women aged 15-

TABLE 4
Zero-Inflated Negative Binomial Model of Frequency of Sexual Intercourse, Full sample

	MODEL 1			MODEL 2			MODEL 3		
	Coeff	**t-stat**	**Marginal Effect**	**Coeff**	**t-stat**	**Marginal Effect**	**Coeff**	**t-stat**	**Marginal Effect**
BLACK	-0.144	-0.650	-1.024	-0.325	-1.410	-2.097	-0.285	-1.240	-1.845
HISP	0.028	0.080	0.210	0.367	1.010	2.848	0.353	0.940	2.712
FORLANG	-0.085	-0.240	-0.625	-0.389	-1.010	-2.677	-0.323	-0.810	-2.215
AGE	0.340***	2.620	2.496	0.443***	3.250	3.054	0.469***	3.380	3.214
MENARCHE	0.827	1.290	6.070	0.820	1.310	5.648	0.715	1.120	4.904
FUNDRELG	-0.220	-1.160	-1.664	-0.256	-1.340	-1.829	-0.258	-1.350	-1.835
TWOPAR	-0.414**	-2.070	-3.056	-0.397**	-1.958	-2.749	-0.431**	-2.080	-2.975
STRICTM	-0.370**	-1.960	-2.720	-0.405**	-2.180	-2.798	-0.439**	-2.260	-3.024
STRICTF	0.158	0.730	1.176	-0.054	-0.240	-0.370	-0.090	-0.400	-0.612
RICHHH	-0.900***	-2.720	-4.780	-0.866**	-2.540	-4.360	-0.859**	-2.490	-4.315
POORHH	-0.475*	-1.710	-2.984	-0.454	-1.640	-2.697	-0.529*	-1.860	-3.056
MISSHH	-0.297	-1.350	-2.050	-0.354	-1.560	-2.268	-0.343	-1.500	-2.195
URBAN	0.338*	1.650	2.423	0.213	1.020	1.444	0.201	0.960	1.360
CHCOLL	-0.011***	-2.890	-0.077	-0.011***	-2.950	-0.075	-0.011***	-2.940	-0.075
ADJHGC	-0.005	-0.050	-0.038	0.015	0.140	0.105	-0.005	-0.050	-0.034
MEDFUND	-0.154	-0.750	-1.106	0.642	1.210	4.887	0.623	1.300	4.701
PARINVOLV	-0.399	-1.560	-3.257	-0.514	-1.540	-4.069	-0.545	-1.430	-4.334
NOINS				-0.552	-1.080	-2.998	-0.561	-1.100	-3.020
AIDS				-0.012	-1.620	-0.082	-0.010	-1.280	-0.069
SEX_ED				-0.229	-1.110	-1.600	-0.218	-1.050	-1.513
MAXAFDC				-0.002***	-2.490	-0.015	-0.003***	-2.790	-0.022
LIBABOR							0.284	1.200	2.021
SOUTH							-0.427	-1.160	-2.800
DIV96							-0.011	-0.350	-0.072
CONSTANT	-2.130	-0.970		-2.474	-1.110		-2.222	-0.980	
Log Alpha	1.657	6.18		1.693	5.98		1.690	5.94	
t	0.016	1.70		-0.060	-1.97		-0.064	-1.93	
Vuong Stat	2.13			2.01			2.03		
Log Likelihood	-2877.15			-2849.66			-2847.45		

Notes: Marginal effects are calculated at sample means. N = 1724.
* Significant at 10 percent or better. ** Significant at 5 percent or better. *** Significant at 1 percent or better.

TABLE 5
Zero-Inflated Negative Binomial Model of Frequency of Non-Contracepted Sexual Intercourse, Full sample and Sexually Active Sub-sample

	MODEL 1			MODEL 2			MODEL 3			MODEL 4 (Sexually Active)		
	Coeff	t-stat	Marginal Effect	Coeff	t-stat	Marginal Effect	Coeff	t-stat	Marginal Effect	Coeff	t-stat	Marginal Effect
BLACK	0.243	0.620	0.531	-0.026	-0.060	-0.049	-0.010	-0.020	-0.019	-0.546	-1.51	-0.433
HISP	0.196	0.390	0.432	0.567	1.060	1.313	0.612	1.100	1.439	0.099	0.21	0.104
FORLANG	1.163**	2.080	2.408	0.752	1.200	1.440	0.693	1.040	1.328	1.101*	1.88	1.008
AGE	0.244	1.110	0.504	0.300	1.260	0.574	0.307	1.270	0.589	-0.003	-0.39	-0.003
MENARCHE	1.331	1.360	2.755	1.389	1.430	2.658	1.302	1.310	2.494	0.788	0.68	1.200
FUNDRELG	-0.785**	-2.510	-1.840	-0.864***	-2.750	-1.902	-0.901***	-2.720	-2.003	-0.863***	-3.01	-0.608
TWOPAR	-0.916***	-2.920	-1.958	-0.803**	-2.580	-1.574	-0.793**	-2.380	-1.555	-0.225	-0.73	-1.241
STRICTM	-0.432	-1.480	-0.898	-0.496*	-1.700	-0.954	-0.509*	-1.690	-0.982	-0.329	-1.15	-0.481
STRICTF	0.241	0.730	0.508	0.021	0.060	0.040	-0.003	-0.010	-0.006	-0.012	-0.04	-0.003
RICHHH	-0.795	-1.550	-1.232	-0.713	-1.340	-1.049	-0.723	-1.340	-1.062	0.141	0.23	1.155
POORHH	-0.782*	-1.700	-1.264	-0.580	-1.260	-0.920	-0.598	-1.260	-0.944	-0.310	-0.72	-1.334
MISSHH	-0.405	-1.190	-0.773	-0.302	-0.890	-0.544	-0.306	-0.900	-0.551	-0.029	-0.20	-0.302
URBAN	0.607*	1.810	1.209	0.501	1.500	0.928	0.527	1.530	0.977	0.591*	1.81	1.840
CHCOLL	-0.029***	-5.000	-0.060	-0.028***	-4.740	-0.053	-0.027***	-4.330	-0.051	-0.015**	-2.87	-0.156
ADJHGC	0.194	0.890	0.401	0.168	0.750	0.322	0.146	0.620	0.279	0.034	0.17	0.352
MEDFUND	-0.119	-0.360	-0.242	0.779	0.910	1.691	0.860	1.100	1.897	0.280	0.61	1.342
PARINVOLV	-0.276	-0.790	-0.614	-0.493	-1.180	-1.077	-0.668	-1.320	-1.539	-0.027	-0.65	-0.174
NOINS				-1.029**	-2.510	-1.807	-1.012**	-2.360	-1.784	-1.009	-1.27	-1.001
AIDS				-0.010	-0.860	-0.019	-0.009	-0.730	-0.018	-0.101	-0.96	-0.002
SEX_ED				-0.228	-0.680	-0.442	-0.266	-0.750	-0.518	-0.298	-1.51	-0.677
MAXAFDC				-0.002*	-1.860	-0.005	-0.003	-1.370	-0.005	0.0002	0.14	0.002
LIBABOR							0.164	0.360	0.320	0.198	0.65	0.221
SOUTH							0.0001	0.000	0.000	0.404	0.83	0.014
DIV96							-0.020	-0.410	-0.038	0.018	0.876	0.025
CONSTANT	-1.283	-0.360		-1.138	-0.290		-1.135	-0.280		3.055	0.81	
Log Alpha	3.285	42.61		3.268	41.99		3.264	41.85		1.800	22.56	
t	-1.80	-1.25		-1.14	-1.20		-1.30	-1.05		-2.83	-0.50	
Vuong Stat	-0.56			-1.02			-0.43			-0.28		
Log Likelihood	-1499.06			-1473.95			-1473.27			-1149.30		

Notes: Marginal effects are calculated at sample means. N = 1724. N=456 for sub-sample.
* Significant at 10 percent or better. ** Significant at 5 percent or better. *** Significant at 1 percent or better.

24, but it inexplicably *decreased* such births among non-black women in the same age group. Along a related line, Levine [2001] finds that adolescents residing in states that have reformed their welfare system to make obtaining benefits *more difficult* are actually more likely to be sexually active than their counterparts in other states. Levine suggests the possibility that welfare reforms are endogenous to the fertility behavior of adolescents in the state. Similarly, it could be conjectured that, in this case, per family welfare payments are endogenous to the size of the current caseload or potential future caseload. States where teen sexual and fertility behavior are conducive to keeping the potential welfare caseload low may be inclined to be more generous.[14] Nonetheless, the result seems unexpected enough to warrant further research in the future.

One issue of concern is whether the two abortion policies of primary interest – Medicaid funding availability and parental involvement laws — are sufficiently strongly correlated so that both appear insignificant due to multicollinearity. If the states which enforce parental involvement systematically fail to provide Medicaid funding and vice versa, then the correlation coefficient between the two policy indicators in my sample is close to -1, which is a potential problem. To inspect this, I re-ran the equations alternately including only the binary indicator parental involvement law and only the availability of Medicaid funding. Even then, neither was found to be statistically significant. Moreover, the correlation coefficient between *presence* of a parental involvement law and *availability* of Medicaid funding in the sample is -0.289. The negative sign is expected, but the magnitude does not seem large enough to cause serious multicollinearity concerns.

I attempted certain other specifications, whose results are available upon request. I experimented with including additional state level controls, like female labor force participation, the ratio of female to male unemployment rates in the state, state poverty rates, percentage of adolescents 'in need' who were able to obtain contraception from publicly funded clinics, and whether the state provided police protection around abortion clinics. This information is available from the Alan Guttmacher Institute. However, including these did not change the lack of statistical significance of Medicaid restrictions and parental involvement. I also re-estimated the frequency of sexual activity and non-contracepted sexual activity using regular negative binomial models (instead of zero-inflated), as well as Tobit models. The results were qualitatively the same, in that in no case did either parental involvement laws or Medicaid restrictions have any statistically significant negative effects. Finally, I ran estimations using standard probit models for binary indicators of 'had any sexual activity or not' and 'had any non-contracepted sexual activity or not', and Tobit models for frequency of sexual activity and frequency of non-contracepted sexual activity. In all cases, the effects of the two abortion restrictions of interest turned out to be statistically insignificant.

In conclusion, I do not find support for the hypothesis that Medicaid funding restrictions reduces sexual intercourse or non-contracepted intercourse among adolescents. Parental involvement laws have a consistently negative sign but fall short of being statistically significant at 10 percent level. Hence, it appears that they also fail to discernibly reduce sexual intercourse or non-contracepted intercourse among adolescents. The question then becomes, how might one interpret these non-effects? One

reason could be the failure to control adequately for unobservable state effects in spite of the inclusion of other state-level policies and characteristics. Bias arising from failure to control for such unobservables may operate in two ways. The first and more plausible case is that the more religiously or socially conservative states have more restrictive abortion policies. In such cases, the estimated effects of the policies would be biased *away* from zero – namely, they would appear to affect sexual activity and unprotected sexual activity more strongly and significantly than is true, hence increasing the risk of type I error. This would be a concern if I found the policies of interest to have significant non-zero effects on teen sexual behavior. However, given that I already fail to reject the null hypothesis of 'no effect' for the restrictions of interest, the presence of this form of bias in my model does not qualitatively change my conclusions. The second and less plausible case is that states with otherwise relatively 'non-traditional' norms and attitudes and a propensity among teens to engage in premarital sexual activity choose to impose restrictive abortion policies as a form of deterrence. In this case, the state unobservables could cause the estimated effects of the policies to be biased towards zero, increasing the risk of type II error. Evidence seems to support the first case, because among the fifteen states that had made abortion accessible even before Roe versus Wade, the great majority did not have a parental involvement law in place, and a higher proportion of this group of states provided Medicaid funding for abortion than did the other states (more specific details are available upon request from the author). Thus, it seems that current permissive (restrictive) abortion policies are positively correlated with historically liberal (conservative) attitudes in the state, especially with regards to sexual activity and reproductive rights. This helps alleviate concerns about whether the paper's main findings are spurious.

Multiple explanations might be given for the non-effects of abortion policies on teen sexual behavior. One explanation could be the tendency among adolescents to discount the future at very high rates, so that the 'price' of abortion and future consequences thereof do not enter present decisions about sexual activity and contraception use. Note that if this is the case, then the desirability of the policies become somewhat debatable, for it indicates that the policies will do little to prevent the initial occurrence of unwanted pregnancies among adolescents. Hence, there is the risk that they will lead to more cases of unplanned teen motherhood or of cases of adolescent women trying to obtain abortion in illegal and health-endangering manners. An alternate explanation is that, on average, the restrictions on abortion access are *non-binding*. If most adolescents are not averse to letting their parents know of an accidental pregnancy and are willing to discuss the option of an abortion with them, and if most adolescents are either not eligible for or do not plan to use Medicaid funding for abortion procedures anyway, then the above restrictions will not be binding constraints and hence will not affect sexual behavior. Nor should they later affect the demand for abortion among adolescents in this generation who experience an unwanted pregnancy. In that case, while the policies do no harm, but at the same time they are essentially meaningless. A final explanation might be the lack of information. It is not clear what mechanisms different states have in place to inform adolescents regarding the presence of the above restrictions. If adolescents are simply unaware of the restrictions, then they will not factor them into the potential cost of sexual activity.

Hence, an initial direction of policy might be to ensure that adolescents are made aware of the existence of the regulations, through school programs or through public service messages on the media. If the regulations serve as binding constraints for most adolescents and if most adolescents are rational enough to consider future consequences of current actions, then lack of information regarding the existence of the restrictions may be what prevents the restrictions from having negative effects on the frequency of sexual intercourse (non-contracepted intercourse). Therefore, better dissemination of information might serve as a useful remedy.

NOTES

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1. The factors systematically found to be correlated with early initiation into sex are structure of parental family, poverty, physical maturity, religiosity and church attendance, use of addictive substances, dating young, and sexual attitudes of friends, siblings and parents. Similar characteristics are found to play an important role in contraception use. However, from the economist's point of view, there are endogeneity issues with many of these variables, and the direction of causality between these variables and sexual activity/contraception behavior is uncertain. The more obvious suspects are substance use, dating young, church attendance, school performance and the sexual attitudes of self-selected friends (for more on this issue, see Levine, [2001]).
2. If $v^2 = 0$, then the negative binomial distribution resorts to the standard Poisson distribution with equal mean and variance. $v^2 \neq 0$ gives rise to the 'overdispersion' in negative binomial models.
3. One referee raised concerns about the relatively restrictive form of the model, where it is assumed that $p_i = \Phi(\tau\beta' X_i)$. A more general non-restrictive form would just be $p_i = \Phi(\zeta' X_2)$. However, in practice, this unrestricted form also requires an identification restriction, with there being at least one variable in X_2 that is not in X_1. It is extremely difficult to find a variable that will feasibly affect willingness to be or not be sexually active (i.e. whether or not $D_i = 0$), but will not thereafter affect the frequency of sexual intercourse. Attempts to estimate an unrestricted ZINB model without the identification restriction resulted in failure of the model to converge both in STATA and LIMDEP. Accordingly, I have chosen to go with the restricted version.
4. An admitted shortcoming is that no further distinction can be made between those who are not averse to being sexually active at this life stage but are averse to non-contracepted sexual intercourse, and those who simply would not participate in sexual intercourse – non-contracepted or otherwise – at this life stage. A structural model that can incorporate these aspects is beyond the scope of this paper.
5. Specifically, four waves of the NLSY97 data were available at on-set of this work, from 1997 to 2000. However, to this author's knowledge, there were no changes to state Medicaid funding restriction laws in this period. Parental involvement laws were imposed in Texas (1999). They were passed in Colorado (1998) and Florida (1999) but not enforced in either state, so are unlikely to have much effect. They were also passed in New Jersey (1998) but almost immediately enjoined by court order.
6. The denominations are obtained from Haas-Wilson [1996]. They include Roman catholic Church, Eastern Orthodox Churches, Churches of Christ, the American Baptist Association, the Lutheran Church-Missouri Synod, African Methodist Churches, Christian Churches, Assemblies of God, and Churches of Jesus Christ of Latter day Saints.

7. Since most respondents are currently in school, their grade depends on their current age. Hence, the variable used is (Highest grade completed + 5 – Age).
8. Idaho, Kentucky, Missouri and North Dakota only allow abortion coverage through optional riders which require separate, additional premiums. Rhode Island enjoins the same. Colorado, Illinois, Kansas, Michigan, Nebraska, North Dakota, Ohio, Pennsylvania, Rhode Island and Virginia explicitly prohibit insurance coverage whenever public funds are used or public employees are insured. Exceptions are made in most cases when abortion is required to preserve the woman's life, and in some cases when the pregnancy is a result of rape or incest. Those details are available upon request. The extent of the direct effect of these policies on adolescent behavior is questionable since adolescents are covered by their parents' insurance, and would not be able to make use of the insurance coverage if they did not wish to let their parents know about the abortion anyway. However, the presence of this restriction should be indicative of the state's public opinion about abortion.
9. Fifteen states had adopted laws making abortion relatively easy to obtain before the Roe vs Wade ruling — CA, NY, HI, WA, KS, NM, OR, CO, VA, MD, SC, NC, DE, AK and AR.
10. Details about choice of contraception method are not presented here. Briefly, condoms were found to be the contraception of choice among more than 60% of the sexually active group, followed by the pill, which was preferred by about 14%. A total of 5% of the sexually active sample reported depending on unsafe methods like withdrawal and rhythm, and I count such encounters among 'non-contracepted intercourse.'
11. Finally, the high values of the Vuong statistics indicate that the zero inflated model is to be favored against the regular (non zero-inflated) model. The negative value of τ indicates that (not surprisingly) variables that *increase* the predicted frequency of engaging in sex also *decrease* the probability that the respondent will prefer to abstain from sex altogether at this life-stage.
12. In the case where only the sexually active sub-sample is considered, the zero values may be either because the respondent would simply not participate in non-contracepted sex, or because she happened not to participate in it during the previous year.
13. I am grateful to one of the referees for this suggestion.
14. It could also be speculated that higher welfare payments lead to stronger negative feelings about welfare recipients among the rest of the population. This may affect adolescents' perceptions about the 'acceptability' of going on welfare, and hence lead them to take precautions against such an eventuality.

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