A Preliminary Investigation of The Effects of Restrictions on Medicaid Funding for Abortions on Female STD Rates

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A PRELIMINARY INVESTIGATION OF THE EFFECTS OF RESTRICTIONS ON MEDICAID FUNDING FOR ABORTIONS ON FEMALE STD RATES.

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Summary[a]

There is evidence in the economic literature that restrictions on Medicaid funding for abortion reduces the demand for abortion. The unresolved question is whether such restrictions also increase safe sex (that is, pregnancy avoidance) behavior among women. This study explores that issue using state-level gonorrhea rates among women for 1975-95. The rationale is that sexual behavior that leads to greater risk of accidental pregnancies is likely to be highly correlated with sexual behavior leading to greater risk of STD infection. Since gonorrhea has an incubation period of about a week, and is transmitted almost exclusively through sexual intercourse, a change in sexual behavior should soon be followed by a change in gonorrhea rates. The study used a partial adjustment model with lagged dependent variables estimated using Arellano-Bond's GMM method. Results fail to find any statistically significant evidence that Medicaid funding restrictions are effective in reducing gonorrhea rates. This finding is robust to a variety of alternate specifications and tests. This suggests that restrictions on Medicaid funding for abortion fail to promote safe sex behavior among women.

1. Introduction:

In 1973, the U.S. Supreme Court case of Roe vs. Wade established the constitutional right of women to abort a pregnancy. The decision made it illegal for states to implement laws prohibiting women from obtaining abortions, but left considerable ambiguity regarding the authority of a state to impose restrictions that could curtail a woman's ability to do so. The issue became particularly contentious with the 1976 Hyde Amendment passed by the Congress, which cut off federal Medicaid funding for most abortion procedures and left Medicaid funding for abortion to the states' discretion. Many states proceeded to restrict on Medicaid funding for abortion procedures, though in some cases this was temporarily over-turned by judicial decisions. However, in 1981 a Supreme Court ruling established the full right of the states to restrict Medicaid funding for abortion procedures if they so chose.

Needless to say, the issue of Medicaid funding restrictions on abortion remains controversial. Therefore, it seems useful to determine from a public policy perspective what *real impacts* the Medicaid funding restriction may have. There is a considerable body of literature offering evidence that such restrictions do, in fact, reduce the demand for legal abortions. However, there is much ambiguity in empirical findings regarding whether such restrictions increase the incidence of unplanned births. The literature that tests this hypothesis using actual birth incidences yields conflicting results, and the studies that attempt to directly explore the effects of such behavior on individual sexual activity are hampered by data restrictions. Hence, this work proposes an alternate angle of investigation, and looks at whether Medicaid restrictions have any effect on the rates of sexually transmitted disease, hereafter referred to as STD, among women. [b]

If Medicaid funding restrictions *do*, indeed, reduce the rates of sexually transmitted diseases, then this implies that such restrictions inhibit the kind of risky sexual activities that cause such diseases (or conversely, the availability of Medicaid funding encourages the kind of risky sexual activities that causes such diseases). Hence, the findings of this study help serve two purposes. First and more importantly, they provide an indirect but reasonably convincing test of whether Medicaid funding restrictions encourage safe sex behavior. Second, they directly test whether Medicaid funding restrictions generate positive externalities by reducing the societal health cost burden of high STD rates. It is worth noting that STD rates have previously been utilized to proxy 'risky' sexual behavior in the health science literature (e.g. Scribner et al, 1998)[1], but to the author's knowledge, they have been used in that capacity in the economic literature by only one study -- Chesson et al, (2000) [2].

The STD used here is gonorrhea rates among women – a disease with a very short incubation period which is transmitted almost entirely through sexual intercourse. The period covered is 1975-95, and the model used is one of partial adjustment with lagged dependent variables. The results uniformly fail to find evidence that Medicaid funding restrictions reduce gonorrhea rates among women with any statistical significance. Hence, this study is unable to offer support for such funding restrictions on either the grounds that they reduce the cost burden of STDs, or the grounds that they seem to promote safe sex behavior.

2. Previous Research:

2.1 Theoretical Discussion:

Conventional wisdom might hold that restrictions on abortion that raise the price of abortion affect a woman's choices *after* a pregnancy has occurred, and might increase the probability of the pregnancy being carried to term. However, a pregnancy is rarely an exogenous event. Rather, it is an outcome of prior decisions made regarding sexual intercourse and contraception use. Thus, increased access to abortion procedures, via lowering the opportunity cost of unwanted pregnancies, could increase the incidence of behaviors that *heighten* the risk of unwanted pregnancies, while reduced access to abortion procedures could *decrease* the incidence of the same. Hence, the net effect of abortion restrictions on childbearing depends on *the stage at which* women factor in the restriction into their decision-making process. If it is only after the pregnancy has already occurred, then the restrictions should negatively affect the number of abortions but not the number of pregnancies -- and thus increase the number of unplanned births. On the other hand, if it is during *initial* decision-making about sexual intercourse and contraception use, then the restrictions could negatively affect both the number of initial pregnancies as well as the number of abortions thereafter, so that the eventual *net* effect on childbearing is ambiguous.

This study suggests an indirect method to gauge whether women factor in the presence of Medicaid funding restrictions in decisions regarding sexual activity and contraception. Behaviors that leads to STD infections and behaviors that leads to accidental pregnancies are, in many ways, positively correlated. Examples are sexual intercourse at relatively young ages, non-monogamous sexual relationships, unprotected sexual intercourse, and so forth. If restrictions on abortion influence women's choices only *after* the occurrence of a pregnancy, then they will not affect any of the above behaviors and should not impact STD rates. However, if the restrictions influence women's choices regarding sex and contraception and encourage safe sex behavior, then they should simultaneously reduce STD rates. Hence, the effect (or lack thereof) of restrictions on Medicaid funding for abortion on STD rates provide indirect but reasonably convincing indications of the effectiveness of those restrictions in promoting safe sex behavior.

2.2. Existing Empirical Work:

Economic studies have considered the impact of Medicaid funding restrictions for abortion on the abortion rates at state or county levels (Blank et al, 1996;[3] Levine et al, 1996;[4] Haas-

Wilson, 1996;[5] Kane & Staiger, 1996; [6] Matthews et al, 1997;[7]) as well as on the probability of individual women getting an abortion (Lundberg & Plotnick. 1995;[8] Levine et. Al, 1996). By an large, the results indicate that the presence of Medicaid restrictions negatively affect state/county abortion rates and probability of abortion for individual respondents. Thus, there is reasonable evidence to support the hypothesis that Medicaid funding restrictions affect pregnancy resolution choices. The next question is, do such restrictions also affect choices pertaining to sexual and contraception behavior? One method is to the effect of such restrictions on actual birth outcomes. If the restrictions reduce the likelihood of abortion but not the likelihood of a pregnancy, then they should be accompanied by an *increase* in birthrates. On the other hand, no increase or a decrease in birthrates (in conjunction with reductions in abortion rates) would imply increases in pregnancy-avoidance behavior. Empirical results on this issue are rather ambiguous. Evans et al (1993)[9] find faster increases in birthrates in a state with Medicaid funding restrictions compared to states without. However, Levine et al and Matthews et al find that Medicaid funding restrictions have either negative or insignificant effects on birth-rates, depending on the model specification. Kane and Staiger find a negative effect on birthrates for white women, but a positive effect for black women. On the other hand, Currie et al. (1996)[10] use individual level data from NLSY79 and find positive effects on birth probabilities for all racial and income groups; Tomal (1999)[11] uses county-level data from 12 states and finds higher birthrates among teens; and Cook et al (1999)[12] find that inadequacy of abortion funds in the state of North Carolina has *positive* and significant effects on birthrates for some groups and insignificant effects for the others in that state. Hence, there is no clear consensus on whether Medicaid funding restrictions actually promote pregnancy-avoidance behavior.

An alternate route is to directly consider the impact of Medicaid funding restrictions on sexual activity and contraception use. Three studies (Argys et al, 1999;[13] Sen, 1999;[14]

Levine, 2001 [15]) investigate the effect of Medicaid restrictions on adolescent sexual activity. The consensus is that Medicaid restrictions *do not* appear to lead to more abstinence or more diligent contraception use. However, studies using individual-level data to investigate determinants of sexual activity are typically plagued by problems of misreporting of actual sexual activity. Furthermore, the cross-sectional nature of the data used by Argys et al (NSFG, year 1995) and Sen (NLSY97, year 1997) lead to concerns about endogeneity between Medicaid restrictions and unobserved state attitudes. Levine uses YRBS data for multiple years between the late 1980s and 1990s, but is hindered by the fact that very few states actually *changed* Medicaid laws within that period. Hence the effects of the restrictions are, for the greater part, subsumed within the state fixed effects. Thus, there remains considerable scope for exploring the effect of Medicaid restrictions on sexual activity from another angle – its effects on state STD rates.

3. Data :

3.1. STD Rates:

The data set covers the 20 year period of 1975 to 1995. Data for gonorrhea rates (calculated per 100,000 female population) are obtained from the Centers for Disease Control and Prevention (CDC), which records state surveillance reports of these cases.[c] Gonorrhea is a bacterial diseases that, besides mother-to-infant, are transmitted only through sexual intercourse. The incubation period is typically less than 2 weeks. Figure 1 shows the over-all female gonorrhea rates in the country over 1975-95. Figure 2 shows the age-specific gonorrhea rates among women in 1995. Note that the rates are the highest among adolescents and young adults, verifying that the age-groups most susceptible to gonorrhea coincide with age-groups most at risk of unplanned, pre-marital pregnancies, and hence potentially most affected by restrictions on abortion access.

The CDC warns that, because of different policies regarding reporting of infectious STDs among private and public clinics, there may be a bias towards reporting of incidences among groups more likely to use public STD clinics. However, this is not a disadvantage in this study. Because there is likely to be a positive correlation between the probability of using a public STD clinic and the probability of Medicaid eligibility, this actually diminishes any concerns about whether Medicaid funding restrictions are relevant to the populations from whom the STD data are obtained. It should be noted, though, that this bias in data reporting could lead to an *over-estimation* of the impact of Medicaid funding restrictions on STD rates for the whole population of women.

3.2. Medicaid Restrictions:

My data on the history of Medicaid funding restrictions for the years 1975-1990 is the same as that utilized by Blank et al (1996) and Levine et al (1996). I am very grateful to Rebecca Blank for sharing this data. The information is updated for the years 1991-1995 using reports by Sollom (1994; 1996)[16,17]. Briefly, the history of Medicaid funding restrictions is as follows: From 1974-76, Medicaid funding for abortions was generally available. Following the passage of the Hyde Amendment in 1976, 36 states also placed Medicaid funding restrictions, but between 1977 and 1980, the status of the laws were unclear, triggered on and off by a variety of judicial decisions. By 1981, however, the Supreme Court's ruling gave states the full power to restrict Medicaid funding, and the majority of states immediately did so (in most cases, exceptions were made when the pregnancy was life-threatening or the result of rape or incest). A few states instituted restrictions in the late 1980s, while a few others instituted restrictions for a few years but then removed them. By 1994, just 17 states funded abortions for low-income women.[d] This legislative history creates a 'natural experiment'. Since state gonorrhea rate data is available both prior to and after the passage (or withdrawal) of the restriction, one can test if the restriction cause a change in the STD rates after unobserved state characteristics are controlled for with ‘fixed effects’.

The one state that is hard to categorize is North Carolina, which makes available funding for abortion to low-income women, but does so from a separate abortion fund. Between 1978-1993, this fund was sometimes depleted before the end of the fiscal year, and public abortion funding was suspended. Cook et al. demonstrate that this affected abortion rates and birth rates in the state. This unique situation – of funding being theoretically available but in practice occasionally suspended – makes it difficult to categorize North Carolina. Thus, I choose to omit the data for North Carolina altogether.

3.3. Other Variables:

The study includes additional variables that may influence state STD rates but may not be entirely captured by state or year ‘fixed effects.’ The choice of such variables is somewhat constrained by the necessity that the data should be available for all of 1975-1995. Four variables are utilized: the percentage of state population aged between 15-19 years (**teenperc**), included because gonorrhea rates are highest among that age-group; the maximum level of monthly AFDC payments available to a family of three (adjusted to 1982-84 dollars) (**maxafdc**), included because greater AFDC generosity might lower the opportunity cost of childbearing and hence promote ‘risky’ sexual behavior; and the minimum drinking age in the state (**dr-age**), included because Chesson et al (2000) find that that lower minimum drinking ages are associated with higher STD rates among youth. Finally, a control is used for the percentage of the state population eligible for Medicaid for reasons other than age and disabilities (**medicaidperc**). This last was included at the suggestion of a referee who pointed out that the passage of Medicaid funding restriction might be influenced by the size of the state population likely to be influenced by the legislation. If states imposed the restriction only if the size of that population was declining, then the restriction would appear to have a smaller effect on over-all female STD rates simply because the share of women in the population whose sexual behavior could potentially be affected by the restriction had declined. This necessitates the inclusion of a control for the proportion of non-elderly, non-disabled Medicaid recipients in the state population. A problem arises because state-by-state data on people eligible for Medicaid funding by reason (age, disability, poverty) is only available after 1984.[e] However, data on the percentage of the population on *AFDC* is available for all necessary years. The correlation between percentage of population on AFDC and percentage eligible for Medicaid for reasons other than age and disability (using post-1984 data) is 0.882, and significant at better than 0.1 percent level. Also, the adjusted R^2 from regressing percentage eligible for Medicaid (for reasons other than age and disability) on percentage population on AFDC, state-specific dummies and a linear time trend is 0.941. Hence, I address the missing data issue in various ways. First, I use actual values of **medicaidperc** for years 1985 onwards, and predicted values based on the regression for the prior years. These are the results presented in the paper. Next, I use actual values of **medicaidperc** for years 1985 onwards, and percentage of population on AFDC for the prior years, and finally, I use percentage of population on AFDC as an instrument for **medicaidperc** for all years. These results are discussed later with other robustness checks. Sample means for all variables are presented in table A in the appendix.

5. Empirical Results:

5.1. The Model.

Since gonorrhea is a communicable disease, its prevalence in any one period should in part be dependent on its prevalence in the previous period. Hence, the model specified is

$$Log\ STD_{jt} = \gamma\, Log\ STD_{jt-1} + \alpha_0 (Mfundrest)_{jt} + X_{jt}\beta + \eta_j + \mu_t + e_{it} \qquad (1)$$

And

$$Log\ STD_{jt} = \gamma\ Log\ STD_{jt-1} + \alpha_0(Mfundrest)_{jt} + X_{jt}\beta + \eta_j + \mu_t + t^*\eta_j + e_{it} \qquad (2)$$

Where

$j = 1….51; \quad t = 75…95.$

This is sometimes referred to as the 'partial adjustment' model. The η_j denote the state dummies and captures all state-specific factors that remain largely invariant over time -- for example, the religious composition of the state population, geographical characteristics, population density etc. Moreover, the state-fixed effects also account for any systematic, time invariant differences between states in their policies for reporting gonorrhea rates. μ_t denote year dummies which capture factors that are common across all states in a particular year (e.g. federal government policies, state of health technology etc). In recognition of the fact that unobservables in a state might change across time in ways that differ from other states, the model also includes the state-specific time trends $t^*\eta_j$. 'Mfundrest' denotes the Medicaid funding restriction variable, 1 if funding restrictions are in place for that state for part or whole of that year, and 0 otherwise. X_{jt} is a vector of the other state level variables.

An OLS estimation of the above models with the state dummies is akin to doing a standard fixed effects model. However, it is now well established in the literature that, because of the correlation of the lagged dependent variable to the transformed error term, standard fixed effects estimators of models with lagged dependent variables result in biased and inconsistent estimates unless the number of time periods are large (see Nickell, 1981;[18] Ridder & Wansbeek, 1990;[19] Kiviet, 1993 [20]) In this model, T = 20, hence the bias may not be negligible. An alternative method that wipes out the state effects is a first difference transformation. Because $\Delta Log\ STD_{jt-1}$ is still correlated with Δe_{it} , appropriate instruments must then be used as estimator of $\Delta Log\ STD_{jt-1}$ to obtain unbiased, consistent estimates. The estimator used here is the well-known GMM estimator by Arellano and Bond (1991) [21], which utilizes the orthogonality conditions that exist between the lagged values of the dependent variable and the disturbance e_{it} , and uses as instruments for each $\Delta Log\ STD_{jt-1}$ the set of variables ($Log\ STD_{jt-2}, Log\ STD_{jt-3}, \ldots\ldots. Log\ STD_{jt-(t-1)}$). Arellano and Bond (1991) clarify that absence of first-order autocorrelation is not a required condition for consistency of the GMM estimators, but it is required that there be no second order autocorrelation.

5.2 Estimation Results and Robustness Checks

Prior to actually estimating the model, I do some tests for the validity of the data on STD rates. First, for the model to be meaningful, it is, of course, necessary that behaviors leading to unwanted pregnancies and to STD infections be positively correlated. One method is to find what the correlation is between female STD rates and abortion rates *in the absence* of Medicaid funding restrictions. I compute the correlation coefficient between the gonorrhea rates and the rate of abortions per 1000 15-45 year old women (based on data from the Alan Guttmacher Institute) using all pooled state-year observations when there are no Medicaid funding restrictions. The correlation coefficient is 0.689, and is significant at better than 0.1 percent level, indicating a strong association between behaviors leading to unwanted pregnancies and those leading to STD infections. Next, for the model with state-fixed effects to be valid, it is also necessary that there be within-state variation in the STD rate data. To test for this, I first adjust each state-year observation of (log) gonorrhea rates by subtracting from it the within-state mean (log) gonorrhea rate.

$$Log\ STD_{jt} - Log\ STD_{j}(mean) \qquad (3)$$

One test for the presence or otherwise of within-state variation in the dependent variable is whether the mean absolute deviations from the mean for each state is equal to zero. Accordingly, I perform the t-test for each state to test the null that

$$\left(\frac{\Sigma \mid Log\ STD_{jt} - Log\ STD_{j}(mean) \mid}{21}\right) = 0. \quad \text{where } t = 75\ldots 95. \qquad (4)$$

For each state I am able to reject that null decisively at better than 1 percent significance level (t-statistics are in table B in the appendix). This verifies that there is variation in the within-state STD rate data, hence it is suited for estimations using fixed effects models.

Estimation results are presented in table 1. Columns 2 to 4 present results using data from all states except North Carolina, estimated both with and without state-specific time trends.[f] The coefficients on Medicaid funding restrictions are found to be negative, but the effects fail to be statistically significant even at the 10 percent level. To account for the possibility that the effects of the restrictions on sexual behavior may manifest gradually over time, I re-estimate the equation after including the *number of years* since the passage of the restriction (**Length_rest**), which is always 0 for states that do not have the restriction. The inclusion of a variable showing the length of restriction might be particularly important in a first difference model, where the imposition of a restriction (change of restriction dummy from 0 to 1) will show up as a one-time shock in the year of the passage of the restriction only. However, I still fail to find any evidence of the restrictions having statistically significant, negative effects. Thus far, therefore, there is little evidence to support the hypothesis that the restrictions reduce risky sexual behavior. I also re-estimate the equations after including an interaction between **Mfundrest** and **medicaidperc** to see if effects of the funding restrictions vary with the percentage of the population potentially eligible for funding. The effects continue to be statistically insignificant (the t-statistic is always less than |0.90| in absolute value).

Regarding the other variables, the high, positive and statistically significant coefficient on lagged (log) gonorrhea rates testify to a strong persistence effect, indicating that current prevalence is, indeed, highly affected by the past prevalence of the disease. even after controlling for state and year effects. Higher monthly AFDC payments are associated with a weakly significant increase in gonorrhea rates, though the *magnitude* of the increase is very small. A greater percentage of adolescents in the state population is associated with higher gonorrhea rates in some of the model specifications. The effects of higher legal drinking ages and percentage of population eligible for Medicaid for reasons other than age and disability are both statistically insignificant in virtually all cases.

It could be argued that a model specification that constrains the Medicaid funding restriction coefficients to be equal for all states, is overly restrictive. It might be hypothesized that the sensitivity of STD rates to Medicaid funding restrictions could vary based on the state's *prior* STD prevalence. To explore this, I divide the states into two halves based on the female gonorrhea rates in the states in 1975, and re-estimate the equations.[9] The results are presented respectively in columns 5-7 and 8-10. Again, in neither group do the effects of the funding restrictions appear to be statistically significant. Finally, it might be argued that, since many states started restricting Medicaid funding as early as 1977 or 1978, and since my data covers 1975-1995 (I do not have access to state specific data on gonorrhea rates among women prior to 1975), the funding restriction effects may largely be subsumed in the state-fixed effects for many states, resulting in their apparent non-effect. A way to test for this is to re-estimate the equations with a sub-sample of states that imposed Medicaid funding restrictions later, so that there is guaranteed within-state variations in restrictions available in the sub-sample. Accordingly, I select DC (restrictions imposed in 1989), Michigan (restrictions imposed in 1989) and Pennsylvania (restrictions imposed in 1985), and re-estimate the equations. The results are in columns 11-13, and continue to show no evidence of Medicaid funding restrictions exerting significantly negative effect on STD rates.

All of the above equations were re-estimated using separate dummies for whether the funding restrictions were in effect for part of the year or the full year, as well as setting the funding restriction to '0' where it was in effect for only part of the year. The equations were also re-estimated using the two alternate measures for **medicaidperc** described earlier in the data section, and after including the data from North Carolina and treating it as a state without funding restrictions. Numerous alternate specifications of the vector X_{it}, were tried -- omitting each variable in the vector alternately, including each variable by itself and omitting the rest, omitting X_{it}, altogether. Equations were also re-estimated after dividing states into four quartiles (rather than two halves) based on 1975 gonorrhea rates. Finally, all of the above were re-estimated using OLS rather than GMM. In all cases, the effects of the Medicaid funding restrictions on gonorrhea rates fell well short of statistical significance even at the 10 percent level.

While Medicaid funding restrictions systematically fail to have statistically significant impacts on gonorrhea rates among women, it might be asked whether the *magnitudes* of the point estimates are noteworthy. For the full sample, the point estimates show declines ranging between 2.4%-4%. It is difficult to make scientific inferences regarding what percentage decline in 'risky' sexual behavior these numbers would imply. If there always existed a constant ratio between the rate of gonorrhea infection per 100,000 women to the rate of unwanted pregnancies per 100,000 (for example, if 200 cases of gonorrhea per 100,000 women per year automatically implied 400 cases of unwanted pregnancies per 100,000 women per year), then it could be inferred that percentage declines in gonorrhea rates are paralleled by the same percentage decline in the rate of unwanted pregnancies – in this case, a 2.4%-4% decline. But if this ratio were time-variant or were to change with the passage of funding restrictions, then such inferences would no longer hold. It may be worthwhile, however, to compare the results here to those of the only other study in the economic literature that has used STD rates to proxy for risky sexual behavior. Chesson et al, when estimating the impact of increased beer taxes on (log) gonorrhea rates using data from all states, obtained point estimates showing declines in gonorrhea rates of 16.7% for women only and 25.4% for the full population. Thus, a simple comparison of point estimates from the two studies suggest that, when it comes to reducing gonorrhea rates, Medicaid funding restrictions perform much more poorly than a $1 per gallon increase in beer taxes!

6. Conclusions:

This study has attempted to investigate whether Medicaid funding restrictions on abortion lead to adoption of safe sex behavior among women by testing whether the passage of this restriction impacts gonorrhea rates – a disease with a short incubation period that is transmitted primarily via sexual intercourse -- among women. The results of find no indication that Medicaid funding restrictions significantly reduce gonorrhea rates. Hence, it seems that such restrictions do not generate positive externalities in form of reduction of the cost-burden of STDs on society. The question of greater interest is, can it be confidently inferred from these results that such restrictions fail to effectively promote safe-sex behavior among women? While such an inference seems logical, some caveats might exist. It could be conjectured that the female population affected by the funding restrictions and those most prone to STD infections do not overlap, hence pregnancy-avoidance behaviors adopted by the former may not translate into observed reduction in STD rates. It might be speculated that Medicaid funding restrictions lead women to adopt pregnancy-risk-reducing behavior *primarily* in form of increased use of a contraception like the pill, which do nothing to prevent STD infections. A final caveat might lie in the nature of the data collection -- if there happens to be a change in the stringency with which states collect STD data that coincides with the passage of the Medicaid funding restrictions in those states, then that may also bias the estimates of the effects of the restriction. While the degree of feasibility of any of the above scenarios is questionable, there does remain scope for further research on the impact of Medicaid funding restrictions on women's sexual behavior using alternative methods.

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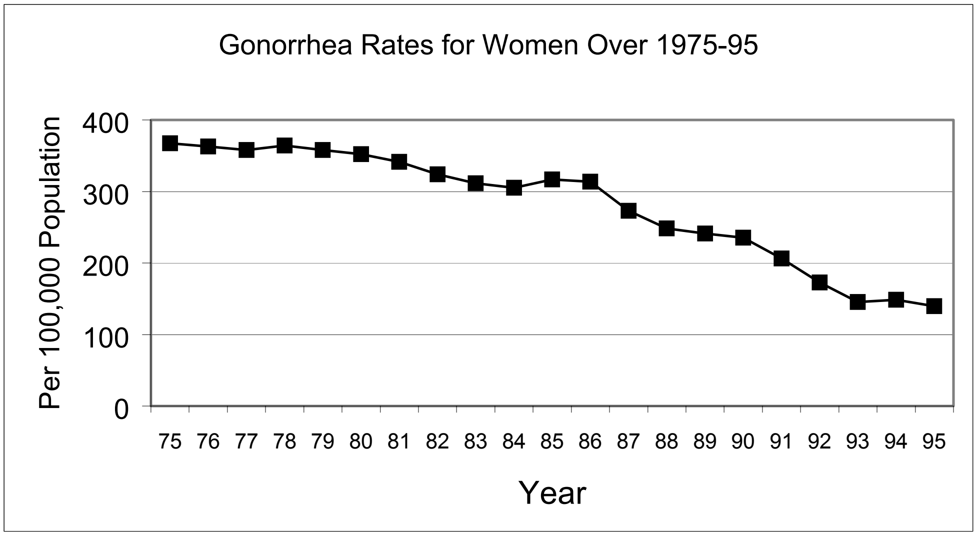
Table 1: GMM Estimation results for Effects of Medicaid Funding Restrictions on (Log) Gonorrhea Rates for Different Groups of States.

	All States (except North Carolina)			States With Lower STD Rates in 1975 [b]			States With Higher STD Rates in 1975 [b]			Michigan, Pennsylvania and DC [c]		
	β (t-stat)	β (t-stat)	β (t-stat)	β (t-stat)	β (t-stat)	β (t-stat)	β (t-stat)	β (t-stat)	β (t-stat)	β (t-stat)	β (t-stat)	β (t-stat)
Lagged (Log) STD Rate	0.870 (35.60)	0.587 (14.65)	0.587 (14.64)	0.831 (23.23)	0.505 (10.22)	0.503 (10.20)	0.827 (21.56)	0.581 (11.01	0.582 (11.08)	0.194 (2.52)	0.164 (2.18)	0.140 (2.03)
Mfundrest	-0.039 (-0.99)	-0.028 (-0.85)	-0.024 (-0.67)	-0.108 (-0.89)	-0.058 (-0.70	-0.056 (-0.10)	-0.023 (-0.64)	-0.018 (-0.52)	-0.021 (-0.62)	-0.010 (-0.17)	-0.009 (-0.11)	-0.009 (-0.09)
Teenperc	0.009 (0.07)	0.024 (2.00)	0.024 (1.92)	0.006 (0.27)	0.031 (1.34)	0.027 (1.14)	0.016 (1.37)	0.006 (2.47)	0.004 (2.36)	0.052 (1.64)	0.044 (1.17)	0.054 (1.76)
Maxafdc	0.0004 (1.68)	0.0005 (1.86)	0.0004 (1.61)	0.0002 (0.47)	0.0007 (1.84)	0.0007 (1.78)	0.0004 (1.47)	0.0004 (1.44)	0.0004 (1.45)	0.0011 (1.74)	0.0013 (1.85)	0.0014 (1.84)
Dr_age	0.013 (0.87)	-0.007 (-0.56)	-0.007 (-0.50)	-0.012 (-0.86)	-0.033 (-1.44)	-0.36 (-1.56)	0.010 (0.87)	-0.004 (-0.02)	-0.0001 (-0.28)	-0.030 (-1.33)	-0.011 (-0.80)	-0.025 (-1.20)
Medicaidperc	0.006 (0.69)	0.016 (1.11)	0.017 (1.51)	0.016 (0.99)	0.023 (1.11)	0.023 (1.00)	-0.007 (-1.08)	-0.006 (-0.44)	-0.005 (-0.36)	0.011 (0.78)	0.022 (1.02)	0.023 (1.05)
Length_rest			-0.019 (-1.22)			-0.023 (-1.26)			-0.021 (-1.26)			-0.005 (-0.25)
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Time Trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Wald chi-sq	2011.3	3392.6	3397.4	1072.5	1824.8	1829.6	973.8	1687.3	1694.3	158.6	162.1	-3.18
Test for Null of No 1st Order Auto-correlation.	-12.09	-11.31	-11.28	-8.91	-8.64	-8.59	-7.15	-6.53	-6.56	-3.12	-3.17	-0.36
Test for Null of No 2nd Order Auto-correlation.	-1.36	-1.51	-1.53	-1.12	-1.40	-1.43	-1.26	-1.29	-1.23	-0.21	-0.30	56
N	947	947	947	475	475	475	472	472	472	56	56	

Notes: All models are estimated using Arellano Bond's GMM estimation method. Consistency of the GMM estimator is not violated by the presence of first order auto-correlation, but would have been had there been second-order auto-correlation (Arellano & Bond, 1991). [b] States are divided into two halves based on gonorrhea rates in 1975. For details of which are in which group, see endnote 'e'. [c] MI, PA and DC are selected because they passed Medicaid funding restrictions in the late 1980s, thus permitting observations on STD rates for a number of years both before and after the passage of the restrictions. All models were estimated using OLS. Other robustness tests performed are described in the text. In all cases, the effects of the Medicaid funding restrictions fell well short of being statistically significant even at the 10 percent level. Those results are available from the author upon request.

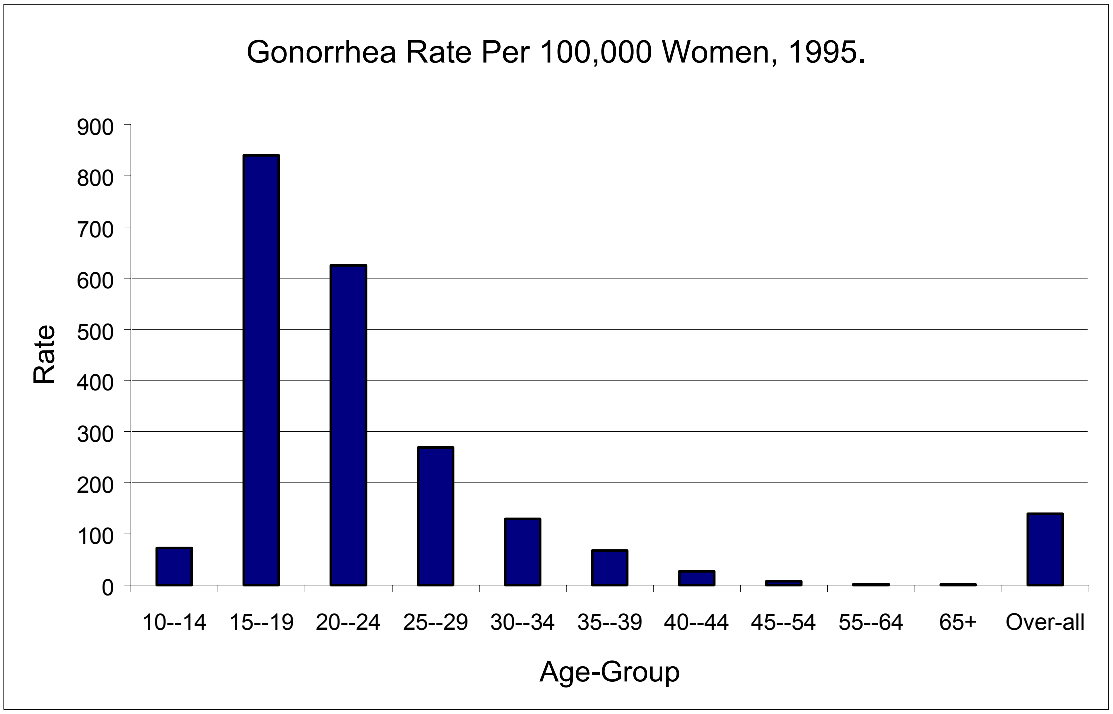
Appendix:

Figure 1:



Source: Centers for Disease Control and Prevention, various STD surveillance reports.

Figure 2:



Source: Centers for Disease Control and Prevention, 1995 Surveillance Report.

Table A: Variable Means Using Pooled State-Year Observations.

Variable	Description	All States except North Carolina (N=1048)[a]		States in Lower Half Based on Gonorrhea Rates in 1975 (N=525)[b]		States in Upper Half Based on Gonorrhea Rates in 1975 (N=523)	
		Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Gonorrhea Rates	Rates per 100,000 population for women	269.65	225.25	145.90	88.01	393.87	251.20
Mfundrest	1 if restriction on Medicaid funding for abortion is in place, 0 otherwise.	0.661	0.474	0.605	0.489	0.716	0.451
Length_rest	Years since restriction has been in place	5.78	6.15	5.436	6.113	6.410	6.201
Teenperc	Percentage of state population 15-19 years old.	9.631	2.317	9.536	2.087	9.726	2.540
Maxafdc	Minimum drinking age	20.05	1.268	20.06	1.240	20.02	1.297
Dr_age	Maximum AFDC payment to family of three in dollars (CPI 1982-84 = 100)	376.464	161.485	442.84	142.03	312.64	150.12
Medicaidperc	Percentage on Medicaid for reasons other than age and disability (actual values for 1985 and after, predicted values prior 1985)	5.331	2.430	5.625	2.195	5.030	2.601
AFDCperc	Percentage of population on AFDC caseload	4.907	1.723	4.861	1.572	4.702	1.861

Notes: Variable means by state are available upon request.
[a] : Sample used for results presented in table 1. [b] Sample used for results presented in table 2.
Predicted values of 'Medicaidperc' for years prior to 1985 are obtained by regressing values on and after 1985 on percentage population on AFDC, state fixed effects, and a time trend. The adjusted R^2 is 0.94.

Table B: Results from testing for the presence of within-state variation in (log) STD rates.

State	T-statistic from testing $\dfrac{(\Sigma \mid \text{Log STD}_{it} - \text{Log STD}_i(\text{mean}) \mid)}{21} = 0.$	State	T-statistic from testing $\dfrac{(\Sigma \mid \text{Log STD}_{it} - \text{Log STD}_i(\text{mean}) \mid)}{21} = 0.$
Alabama	8.428	Missouri	7.527
Alaska	11.425	Montana	8.314
Arizona	8.689	Nebraska	7.468
Arkansas	5.874	Nevada	7.205
California	7.675	New Hamp.	9.002
Colorado	9.463	New Jersey	6.187
Connecticut	5.957	New Mexico	10.298
Delaware	5.344	New York	6.045
DC	8.241	N. Carolina	7.533
Florida	7.289	N. Dakota	6.959
Georgia	4.496	Ohio	6.066
Hawaii	11.509	Oklahoma	10.889
Idaho	10.385	Oregon	7.973
Illinois	6.397	Pennsylvania	5.012
Indiana	8.231	Rhode Island	5.885
Iowa	9.300	S. Carolina	8.645
Kansas	8.918	S. Dakota	8.588
Kentucky	11.365	Tennessee	9.228
Louisiana	10.259	Texas	9.812
Maine	8.537	Utah	9.667
Maryland	7.019	Vermont	9.465
Massachusetts	5.397	Virginia	8.260
Michigan	5.566	Washington	7.175
Minnesota	7.934	West Virginia	7.969
Mississippi	7.130	Wisconsin	5.406
		Wyoming	11.214

Notes: This exercise was done to test that there is within-state variation in the dependent variable.

[a] The author is grateful to Susan Clayton of CDC for help with acquiring data for STD rates, and to Rebecca Blank for generously sharing data on the dates of Medicaid restrictions in different states as well as longitudinal data on various state-specific variables. Junsoo Lee and Traci Mach provided very helpful suggestions regarding data and methodology, Anna Kazandjan and Servet Ciltas provided valuable research assistance. The paper also benefited considerably from comments by two anonymous referees. The responsibility for all opinions and errors belongs to the author.

[b] Women are the population of obvious interest here since they are more directly affected by restrictions on abortion. Parallel results for both genders are available from the author upon request.

[c] The CDC obtains data for the actual number of cases of gonorrhea in total and by age, race and gender from the quarterly and annual reports from STD control programs and health departments from the 50 states and DC. They transform this data into rates per 100,000 population using the intercensal total population estimates and estimates by race, gender and age for each state from the Bureau of Census.

[d] The states where funding was available in 1994 were Alaska, California, Connecticut, Hawaii, Idaho, Illinois, Maryland, Massachusetts, Minnesota, Montana, New Jersey, New Mexico, New York, Oregon, Vermont, Washington and West Virginia. In Vermont, funding was available since 1983. In Idaho, Minnesota, Montana and Illinois, funding became available after 1990 by state judicial court orders. North Carolina funds abortions for low-income women from a separate state abortion fund, but has suspended this if the fund was depleted before the end of the fiscal year.

[e] This data is obtained from various editions of *The Green Book*, and from the on-line state databases maintained by the Urban Institute (http://newfederalism.urban.org/nfdb/index.htm).

[f] Statistical tests verify that state dummies are jointly significant, as are year dummies and the state-specific time trends.

[g] The states in the lower half are Idaho, Iowa, Maine, Massachusetts, Minnesota, New Hampshire, New Jersey, North Dakota, Pennsylvania, Rhode Island, Utah, Vermont, West Virginia, Connecticut, Hawaii, Indiana, Kentucky, Michigan, Montana, Nebraska, New York, South Dakota, Washington, Wisconsin, and Wyoming. The states in the upper half are Arizona, California, Colorado, Delaware, Illinois, Kansas, Louisiana, Missouri, New Mexico, Ohio, Oklahoma, Oregon, Virginia, Alabama, Alaska, D.C., Florida, Georgia, Maryland, Mississippi, Nevada, North Carolina, South Carolina, Tennessee and Texas.