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The effect of Medicaid abortion funding restrictions on abortions, pregnancies and births

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Abstract

This paper considers whether state Medicaid abortion funding restrictions affect the likelihood of getting pregnant, having an abortion and bearing a child. We exploit a natural experiment afforded by Supreme Court decisions and employ more traditional multivariate models with alternative fixed effect specifications. An analysis of 12 years of state-level data indicate that restrictions are associated with a reduction in abortions and either no change or a reduction in births, implying fewer pregnancies. Subsequent analysis of the National Longitudinal Survey of Youth (NLSY) is consistent with these findings and show the response is concentrated among the low-income population.

1. Introduction

In 1973 the U.S. Supreme Court case of Roe vs. Wade established that a woman's constitutional right to privacy included the right to abort a pregnancy. This decision made it illegal for states to implement laws prohibiting a woman from obtaining an abortion. The authority of a state to impose restrictions on abortion that make it more difficult for a woman to exercise this right, however, remained ambiguous. In the two decades following this decision, several states have tried to impose many different forms of abortion restrictions. Since debate about these restrictions is often quite passionate, it seems useful to determine whether, from a public policy perspective, they have any real impact on behavior.

Although some previous research has focused on the impact of such restrictions

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on abortions, other aspects of fertility behavior may also be affected [1]. Becker, 1981 claims that the demand for children, like that of ordinary consumer goods, depends on their relative cost. By reducing the availability of abortion, restrictions increase the cost of terminating an unwanted pregnancy. Women subject to restrictive laws, therefore, could be more likely to engage in behavior reducing the risk of pregnancy and would then be less likely to become pregnant than women who do not face such restrictions. Similarly, the likelihood of ending a pregnancy in abortion should be lower among women facing abortion restrictions. Thus, while restrictions on abortions would likely reduce the number of abortions, the impact on births is ambiguous and depends on the magnitude of the possible impact on pregnancies.

This analysis examines the effect of Medicaid abortion funding restrictions on fertility outcomes in the United States. Following the Roe vs. Wade decision, the Medicaid system, which provides health insurance for certain low-income individuals, paid for all abortions performed on covered women. These payments were eventually eliminated in many states in response to a 1980 Supreme Court decision. To examine the impact of these restrictions, we first examine state-level data on abortions and births over a 12 year period. We exploit the natural experiment afforded by Supreme Court decisions as well as employing more traditional multivariate models with alternative fixed effect specifications in our analysis. The findings suggest that funding restrictions reduce the rate of abortion and either reduce or have no significant effect on birth rates, depending upon specification. These results imply that the fraction of women who become pregnant also declines in response to these restrictions. Subsequent analyses of microdata from the National Longitudinal Survey of Youth (NLSY) support these findings and indicate that the effects of such policies are concentrated among the low income population.

2. Institutional background

The history of state abortion restrictions can be characterized by passage of controversial laws, court battles, injunctions and an eventual high court decision that either invalidates the law or sets up parameters for legal implementation. This pattern holds for Medicaid abortion funding restrictions that are considered in this paper. Since the empirical work to follow will rely on this legislative/judicial history, it will be useful to summarize it here [2].

Medicaid abortion funding restrictions originated with the Hyde Amendment, which was enacted by Congress in 1976 and was initially scheduled to go into

[1] Some examples are Moore and Caldwell, 1977, Medoff, 1988, Lundberg and Plotnick, 1990, Rothstein, 1992, Haas-Wilson. Currie et al., 1993 is a notable exception, examining the effect of Medicaid funding restrictions on birth weight.

[2] Drucker, 1990, Epstein and Jobylka, 1992 provide greater detail on all Supreme Court Decisions related to abortion.

effect in October of that year. This law prevents federal Medicaid funds from being used to pay for most abortions and allows states to impose similar funding restrictions. A court injunction temporarily prevented enforcement of the Hyde Amendment through October of 1977, but then it became effective through February of 1980. At that point, another injunction prohibited its enforcement [3]. In 1980 the Supreme Court finally ruled in Harris vs. McRae and Williams vs. Zbaraz that the Hyde Amendment was constitutional and it has been in effect ever since October of that year.

This history can be roughly characterized by three separate periods. From 1974–76 Medicaid funding for abortions was generally available. Between 1977 and 1980 the status of the law was unclear, triggered on and off by a series of judicial decisions. A woman making fertility decisions would have had difficulty perfectly forecasting the court's behavior and would have to incorporate this uncertainty into her decisions. By 1981, the Supreme Court's rulings eliminated all ambiguity and states that wanted to restrict Medicaid funding clearly could do so. After these rulings, 27 states virtually immediately instituted definitive, enforceable Medicaid funding restrictions and, by 1990, such restrictions were in place in 37 states.

This legislative/judicial history creates a natural experiment that can be used to examine the impact of this form of abortion restriction. Although there exists some other variation in states laws over time, a large fraction appears to come as a direct result of a Supreme Court decision. As such, states can usefully be differentiated into a "treatment group" of states whose laws changed shortly following a court ruling and a "control group" of states that never had a restriction. The effects of the law change may then be observed by examining the difference in women's behavior before and after the change in treatment group states compared to control group states. This approach will be exploited in the empirical work to follow.

3. Theoretical considerations

Conditional upon pregnancy, an increase in the cost of abortion would lead to substitution towards the remaining option of giving birth. Thus, holding pregnancies constant, restrictions in Medicaid funding would cause abortions to fall and births to rise one for one. Abortion laws could also, however, reduce the number of women becoming pregnant. Depending on the size of the possible fall in pregnancies, abortion laws could cause births to rise, remain unchanged, or even fall. This section of the paper will provide some theoretical intuition that can

[3] According to Gold, 1982, five states (North Dakota, South Dakota, Rhode Island, Kentucky and Wyoming) did not abide by the court injunction and would not use Medicaid funds to pay for abortions during this period. Since Arizona did not participate in the federal Medicaid system at this time, they also did not provide any funding for abortions.

illustrate the alternative outcomes. A full model of fertility is beyond the scope of the paper.

Consider a world with no abortion restrictions. Assuming no pregnancy losses, the relationship between births, pregnancies and abortions is given by the identity, $B_u = P_u - A_u$, where B indicates the number of births, P represents the number of pregnancies, A is the number of abortions and the subscript, u, indicates that no restrictions are in place. If abortion restrictions are imposed, the A_u women who would have had an abortion in a world with no restrictions now have three options. First, they could still choose to have an abortion, so that $B_r = B_u$ and $A_r = A_u$ where the subscript, r, indicates that restrictions are in place (i.e. the demand for abortions is price inelastic). Second, they could decide not to abort and give birth instead, so that $A_r < A_u$ and $B_r > B_u$. Third, they could reduce the likelihood of getting pregnant by altering their contraceptive choices or, perhaps, by choosing abstinence, so that $A_r < A_u$, $P_r < P_u$ and $B_r = B_u$ (i.e. birth control and abortion are perfect substitutes). Under each of these scenarios the number of births increases or remains unchanged.

For the number of births to actually fall it must be the case that some of the women who get pregnant and choose to give birth in a world without Medicaid restrictions must not give birth when abortion becomes more costly. A possible reason for this is that the pregnancy decision and the abortion decision occur at different times [4]. At the time a woman "chooses" to try to get pregnant, future events may still be uncertain. With some probability, for instance, news of the pregnancy may lead to the departure of the father. In this framework, abortion provides an "option value" that protects women against the possibility of an undesirable outcome. In the above example, if the father leaves, the woman may choose to abort and avoid the hardships of raising the child by herself, but she would choose to give birth if he stays. If the ability to exercise this option is hindered through abortion restrictions, then, ex-ante, it may be best to delay pregnancy. Thus, some of those women who would have given birth in an unrestricted world choose to delay pregnancy in a world with abortion restrictions.

4. Review of the literature

Although a considerable amount of research has examined the effects of state abortion restrictions on the demand for abortion, cited above, little research has been conducted regarding the potential effects on pregnancies and births. This section will briefly review the few studies that have been conducted.

Two studies have compared the behavior of women in one or two states that changed their laws to that in a neighboring state or two where laws were unchanged. Trussell et al., 1980 report an analysis of fertility behavior among

[4] Akerlof et al., 1994 similarly links an increase in births to a liberalization in abortion policy. A simple extension would predict that abortion restrictions would reduce births.

women eligible for Medicaid in Ohio, Georgia and Michigan between 1977 and 1978. In 1977, Ohio and Georgia implemented Medicaid funding restrictions and Michigan did not [5]. Trussell et al., 1980 report in their table 3 that the number of abortions in Ohio and Georgia fell between roughly one-quarter to one-third, but the number of abortions in Michigan was roughly constant. They also report that the number of live births and the number of pregnancy losses was virtually unchanged in all three states, indicating that the number of pregnancies had to have fallen in Ohio and Georgia relative to Michigan between the two years. Evans et al., 1993 consider the number of births to all women in Michigan, Ohio and Indiana in 1988–90 to examine the impact of Michigan's funding restriction law that took effect in December of 1988. They find that the number of births increased faster in Michigan than the comparison states and conclude that funding restrictions are linked to this increase in births. The results of these two papers are inconsistent, suggesting more data may be required to provide a powerful test.

A recent paper by Kane and Staiger (in press) considers the effects of restricted access to abortion on births directly. They examine the effect of the distance a woman needs to travel to an abortion provider on birth outcomes using county level vital statistics birth data [6]. They find that among women whose access to abortion has been restricted because a local abortion provider shut-down and the next closest provider is some distance away, a reduction in birth rates is observed. This finding is pertinent to the examination conducted here because it strictly represents a relatively modest monetary constraint imposed upon woman seeking an abortion.

5. Data

The analysis to follow will merge information on abortion restrictions in each state and year onto aggregate, state level data on abortions and births. These data have been compiled from several different sources and this section of the paper will briefly document their construction.

Data on the abortion laws in effect in each state over the period 1977 to 1990 were initially obtained from the Alan Guttmacher Institute (AGI) and from the NOW Legal Defense and Education Fund [7]. Subsequently, we obtained a similar coding of these laws used by Blank et al., 1994 and the two sets were then

[5] This law change represents a movement between two years in which Medicaid funding was triggered off and then on during the 1977–80 period in which the legal status of such laws was ambiguous, as described above.

[6] Using these data, these authors have also largely replicated the results presented below for the aggregate, state-level data set we employ.

[7] Through 1988, these restrictions are available in an annual publication of AGI, the *Legislative Record*. Additional data have been published sporadically in *Family Planning Perspectives*. See the data appendix for a complete list of sources.

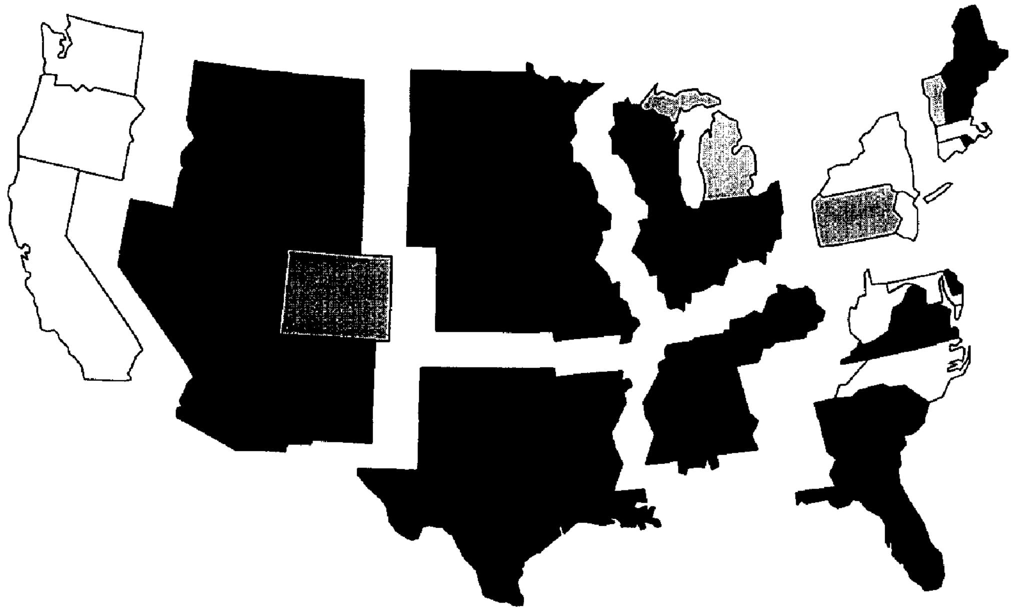

Fig. 1. Medicaid funding restrictions by state and census region, 1977–1990.

reconciled [8]. With one main exception, the coding used here is now the same as that in Blank et al., 1994. We have coded Medicaid funding restrictions differently for 1977–80 in those states whose policies were in flux as a result of the legal challenges described above. With the constitutionality of funding restrictions in doubt during this period, Blank et al. code the fraction of the year that states followed court orders to fund Medicaid abortions. We have chosen to use a discrete measure that equals unity in those states that had definitive funding restrictions and zero for those states without restrictions even for parts of the period [9].

The laws in effect in each state over the 1977–90 time period are summarized in Fig. 1 and reported in detail in the data appendix. In Fig. 1, states filled in with a darker color indicate that restrictions were in effect for more years over this period. An examination of this figure shows that state restrictions are highly correlated with regions of the country. With the exception of the west coast states and some middle Atlantic and New England states, Medicaid funding restrictions have been imposed in virtually every other state in the country at some point over the period.

[8] We also examined parental consent and notification laws, as do Blank et al., 1994 Since these laws only apply to minors, however, the aggregate data and the NLSY data provide very weak tests of the effects of these types of restrictions on women's behavior. As a result, we do not report the results of this analysis.

[9] This difference in coding makes sense given the difference in years examined. Blank et al. consider 1974–88, which covers three distinct periods of funding restrictions (none, ambiguous and definitive) as described above. This paper examines post 1977 data and covers only the latter two periods, making a discrete coding more appropriate. Using Blank et al.'s coding, we obtain qualitatively similar results to those reported below.

Aggregate data used in the empirical work represent state-level data for the years 1977–88. It was constructed from several sources, which are documented in the data appendix. The three dependent variables we consider are the birth rate, the abortion rate and a constructed measure, called the "pregnancy rate." The birth rate is a modified version of that reported in vital statistics and represents the number of births per 1 000 women aged 15–44 [10]. The abortion rate represents the number of abortions performed in a state per 1 000 women aged 15–44 who reside in that state [11]. Data on the abortion rate is obtained from the Alan Guttmacher Institute (AGI) who conducted a survey of abortion providers (hospitals, clinics and doctors) each year between 1977 and 1988 except 1983 and 1986 [12].

The "pregnancy rate" is a measure defined here to be the sum of the abortion rate and birth rate and is designed to approximate the number of pregnancies per 1,000 women aged 15–44. As an identity, $P = B + A + L$, where P, B, A and L represent the number of pregnancies, births, abortions and pregnancy losses, respectively. Therefore, the main shortcoming of the pregnancy rate created here is that it ignores losses [13]. The purpose of this study, however, is to examine the effect of changes in abortion laws on outcomes. If we make the assumption that abortion restrictions have no effect on the probability of experiencing a pregnancy loss, then the impact on pregnancies from a restriction is simply the effect on births plus the effect on abortions [14]. We test this assumption directly for the case

[10] Published birth rates measure the number of births per 1,000 total population. We prefer examining the number of births to women aged 15–44 for the purpose of comparison with the abortion rate data. Estimates of the female population aged 15–44 by state and year are the same as those used in reported abortion rates. We have also experimented with intercensal population estimates provided directly by the Census Bureau and found that results are somewhat sensitive to the data source. Because population data is estimated, reported standard errors in the regression analysis to follow are likely to be somewhat understated.

[11] One potential problem with these data is that abortions are reported by place of occurrence rather than place of residence. This is problematic if people respond to changes in state laws by traveling to a different state to have an abortion performed. Henshaw, 1991 states that 6% of women obtaining an abortion had the procedure performed in a different state than their state of residence. An earlier version of this paper (Levine et al., 1995) examines the bias imposed by this problem and found that it is small.

[12] We experimented with similar data available from the Centers for Disease Control (CDC) to fill in the missing years. Unfortunately, we found these data to be inadequate because the CDC survey is less complete than that conducted by the AGI. In the models reported below, including CDC data yielded virtually identical coefficient estimates with considerably higher standard errors. For this reason, we have chosen to rely on AGI data exclusively.

[13] Another shortcoming of the pregnancy rate measure arises because the abortion data are provided by place of occurrence rather than residence. To the extent that mobility occurs across states in obtaining abortions, this pregnancy measure is somewhat difficult to interpret.

[14] An effect of law changes on losses may be expected if, for example, women who would otherwise have an abortion do not abort, but do not provide good prenatal care. Currie et al., 1993 show that instituting Medicaid funding restrictions does not affect birth weight. Since the potential effect on birth weight arises because of the same type of selection issue, their finding provides evidence supporting this assumption.

of Medicaid funding restrictions using the NLSY data in the last section of this paper.

To these data, we have merged characteristics of both women of childbearing age and the general population in each state and year. Data pertaining to the female population aged 15–44 in each state were obtained from annual outgoing rotation group files from the Current Population Survey. Data on state characteristics, such as the physician, crime and poverty rates, were obtained from the Bureau of the Census and other sources, as documented in the data appendix.

In total, we have data for 10 years and 50 states between 1977 and 1988 (excluding 1983 and 1986). The only sample restriction is that we have eliminated the District of Columbia (DC) from the analysis. The reasons for this decision are both pragmatic and statistical. First, complete data for all of the explanatory variables in all of the years are not available. Second, DC is a huge outlier in the abortion rate. For instance, the abortion rate in DC in 1980 is about 170, almost four times that in California and New York, the next highest states [15]. As a result of this restriction, our sample contains 500 observations.

Means for all variables are presented in Table 1. Column 1 of this table presents means for the aggregate data weighted by the number of women aged 15–44 in each state/year. About 67 women give birth and 28 have an abortion per 1,000 women aged 15–44 every year, on average. This indicates the "pregnancy rate," which we have defined here to be the sum of the abortion and birth rates, is about 95. Over one-third of all women of child-bearing age lived in states with Medicaid funding restrictions over this sample period. Columns 2 and 3 present sample means separately for those who face Medicaid funding restrictions and those who do not. The most striking finding here is that abortions are at least 50% more likely in states that do not have Medicaid funding restrictions, even though birth rates are similar. These cross-tabulations, of course, ignore the role that other variables may play in explaining these results. For instance, marriage rates, which are highly correlated with fertility behavior, are higher in states/years in which Medicaid funding restrictions were in effect.

6. Analysis of natural experiments

If changes in state abortion laws were made by random assignment, then an experimental analysis of the impact of abortion restrictions on abortions, births and pregnancies could be conducted. If the only difference between the "treatment group" (those states instituting restrictions) and "control group" (those states

[15] If DC's abortion rate is high because of migration from Maryland and Virginia, then estimated abortion rates for these states would be understated. However, the population in these two states is considerably larger than that in DC. Thus, a substantially higher abortion rate in DC may be caused by a relatively small amount of migration from Maryland and Virginia.

Table 1
Descriptive statistics, full sample and by restriction status[1]

Variable:	Full Sample (1)	Restricted (2)	Unrestricted (3)
Abortion rate [b]	27.7	21.6	31.4
Birth rate [b]	67.1	68.1	66.5
Pregnancy rate [b]	94.8	89.7	97.9
% In state/year with Medicaid restrictions	37.4	–	–
PERSONAL CHARACTERISTICS			
Years of education	12.4	12.5	12.4
Age	28.9	29.2	28.8
% Nonwhite	15.4	15.0	15.7
% Married	67.6	69.3	66.5
CHARACTERISTICS OF COUNTY (NLSY) OR STATE (Aggregate Data)			
# of physicians per 100000 residents	185.9	166.3	197.6
# of hospital beds per 100000 residents	572.9	558.8	581.4
# of crimes committed per 100000 residents	5,451	5,154	5,628
State per capita income (1990 $s)	16,221	15,492	16,656
Sample size	500	233	267

[a] Means estimated from the aggregate data are weighted by the number of women aged 15–44 in each state/year. Statistics reported for personal characteristics refer to the mean for women aged 15–44 in each state and year. These statistics were estimated from Current Population Survey outgoing rotation group data from 1979–88. Statistics for 1977–78 were obtained by linear interpolation.
[b] Rates are per 1000 women aged 15–44. The pregnancy rate is defined to be equal to the sum of the birth rate and abortion rate and does not include lost pregnancies.

with no change in their laws) was the law change then we could compare the abortion, birth and pregnancy rates for the two groups and generate a measure of the impact of restrictions on these variables. If however, the treatment and control groups differ in the level of abortions or if abortion rates are changing over time with a common linear trend then the treatment effect can be generated by contrasting the difference between the treatment group and the control group before and after the treatment. This "difference-in-difference estimator" would provide a simple estimate of the treatment effect. To the extent that the treatment and control group have different trends the difference-in-difference estimator would provide biased results.

The Supreme Court decision allowing states to restrict Medicaid funding in 1981 provides a natural experiment that approximates the experimental approach previously described. Following this decision, 27 states immediately instituted unambiguous and fully enforceable funding restrictions. Most of these states had been trying to implement these restrictions since 1977 but the status of these laws had fluctuated depending on lower court rulings. Therefore, the specific timing of enactment in 1981 should be unrelated to events occurring in these states at that exact time. To examine the impact of these laws, we compare the fertility behavior of women in "treatment group" states that instituted these laws following the Court's decision and a "control group" of states that have never had such laws. States that changed their laws at other times are not considered in this analysis.

Table 2
Change in birth, abortion and pregnancy rates by changes in state Medicaid abortion funding restrictions: aggregate data [a]

	(1) States instituting restriction in 1981 (1981–88)-(1977–80)	(2) States with no restriction (1981–88)-(1977–80)	(3) Column (1) – column (2)
Change in abortion rate	−0.70 (0.84)	0.93 (1.84)	−1.63 (2.02)
Change in Birth Rate	−3.45 (1.06)	2.62 (1.36)	−6.07 (1.72)
Change in pregnancy rate	−4.14 (1.20)	3.55 (2.60)	−7.69 (2.87)
Number of states	27	12	–

[a] Standard errors in parentheses.

In the first part of this analysis, we compute the difference-in-difference estimator for this experiment. Results of this analysis are reported in Table 2. Medicaid funding restrictions are shown to reduce the aggregate abortion rate by 1.6, although this reduction is not statistically significantly different from zero. On the other hand, this restriction is also shown to significantly *reduce* birth rates by six births per thousand women aged 15–44. Since pregnancy rates are defined as the sum of the abortion rate and birth rate, the large reduction in birth rates is also reflected in a reduction in pregnancy rates.

An alternative technique to examine these experiments is simply to plot the time series fertility behavior of the control and treatment groups, shown in Figs. 2–4. These figures show the differences between the control and treatment groups for each year in the study, rather than summarizing the differences before and after the implementation of the restrictions. In Fig. 2, abortion rates for the treatment group rise from 1977, peak in 1980, fall through 1984 and are then flat at a lower

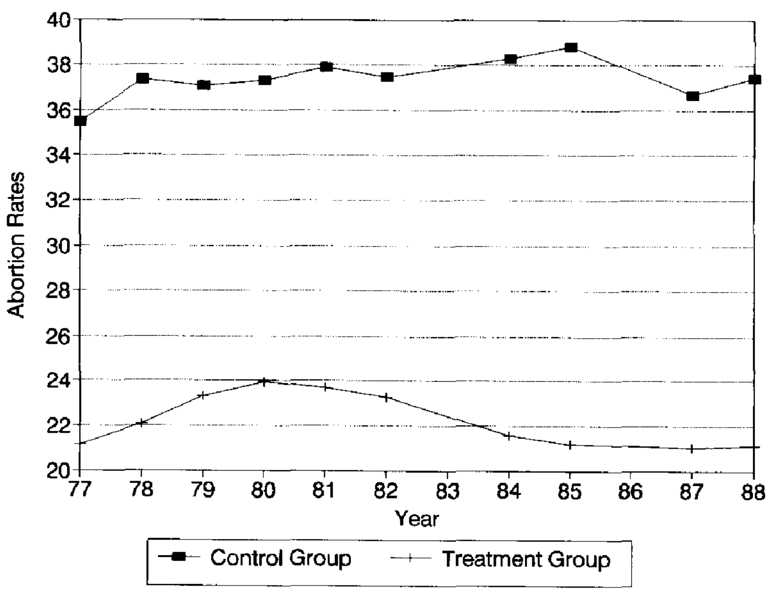

Fig. 2. Abortion rates by year and Medicaid funding restriction status, aggregate data.

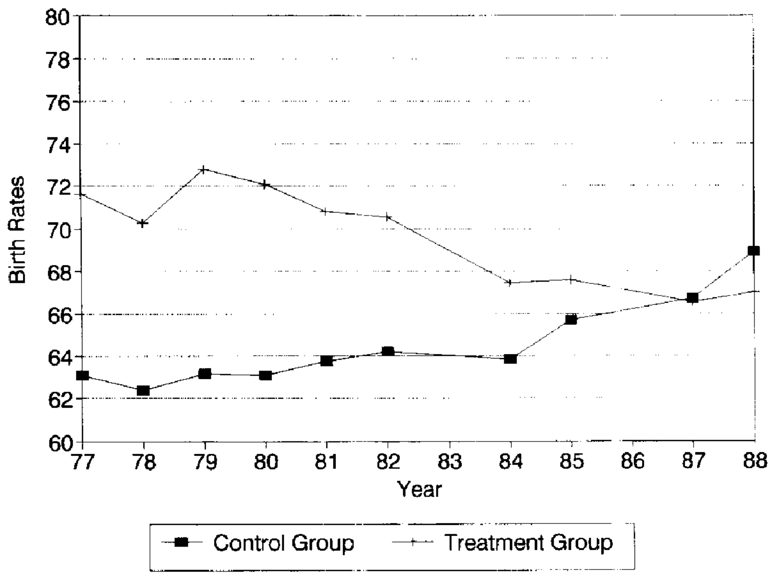

Fig. 3. Birth rates by year and Medicaid funding restriction states, aggregate data.

level [16]. This compares to a control group, with much higher abortion rates that are relatively flat or, perhaps, increasing throughout the period. The difference in levels makes it clear that state-specific factors must be controlled for in estimating treatment effects. Abstracting from the difference in levels, the patterns shown in Fig. 2 strongly indicate a treatment effect. Fig. 3 shows the effect of Medicaid funding on births. Birth rates increase slowly throughout the period for the control group and generally fall for the treatment group over time. Since the peak birth rate for the treatment group is 1979, before the law change, it is difficult to determine whether abortion funding laws lowered birth rates or whether this figure is merely representing mean reversion between the two groups. Any form of differences in trends between the two groups could falsely provide the prediction of a large treatment effect in a difference-in-difference exercise like that presented above. In the multivariate analysis below, we will further explore the effects of different trends across states. Fig. 4 represents pregnancy rates, which are again just the sum of birth and abortion rates in the aggregate data. Since we observe a strong treatment effect for abortions and a potentially large treatment effect in births, pregnancies also show a strong treatment effect. Pregnancy rates move somewhat together until 1980 and then diverge thereafter.

In summary, while caution has to be exercised in drawing inferences from results developed using the simple experimental methodology, the body of evidence presented so far indicate that abortion rates go down when Medicaid

[16] The gradual change in abortion rates in the treatment group states between 1982 and 1984 is somewhat misleading. Since data for 1983 is unavailable, we do not know the precise pattern taking place between these years. The linear trend drawn between 1982 and 1984 is done for convenience.

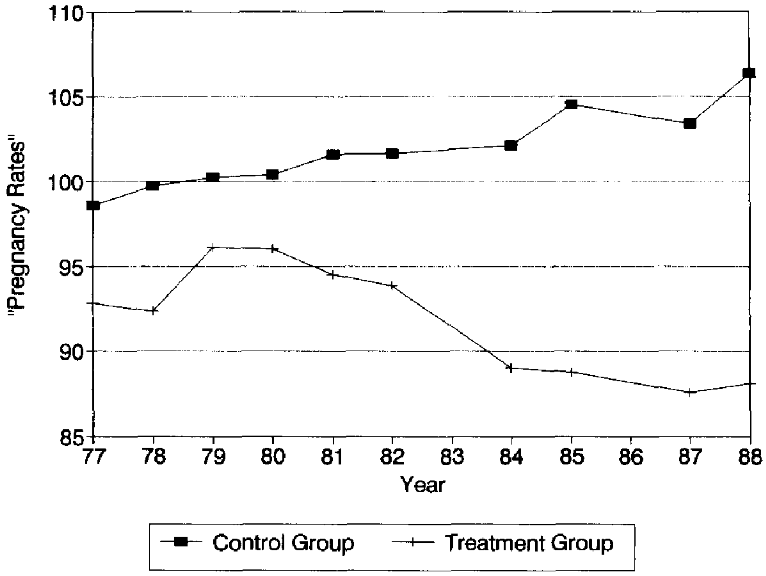

Fig. 4. ''Pregnancy rates'' by year and Medicaid funding restrictio status, aggreate date.

funding restrictions are instituted. Birth rates may also be reduced by these restrictions, but trends in the data make it difficult to draw this conclusion. Importantly, these results suggest the need to control for differences in levels and trends in fertility behavior when estimating treatment effects.

7. Multivariate analysis

Although the experimental approach provides a useful preliminary analysis, we also undertake a more robust estimation strategy that controls for differences across individuals and their places of residence. This alternative approach will be described in this section of the paper.

7.1. Econometric specification

To examine more fully the effects of Medicaid abortion funding restrictions on fertility behavior, we estimate multivariate models of abortion, live-births and our created measure of pregnancies. Specifically, we estimate OLS regression models of the form [17]:

$$Y_{st} = R_{st}\beta_1 + Z_{st}\beta_2 + S_{st}\beta_3 + \gamma_s + \gamma_t + \epsilon_{st} \quad (1)$$

$$Y_{st} = R_{st}\beta_1 + Z_{st}\beta_2 + S_{st}\beta_3 + \gamma_s + \gamma_t + \text{trend}^*\gamma_s + \epsilon_{st} \quad (2)$$

where Y represents the abortion, birth or pregnancy rate, R is an indicator variable for Medicaid funding restrictions, Z is a vector of mean characteristics of the women of childbearing age (15–44), S is a vector of other state characteristics, s

[17] These regressions are weighted by the number of women aged 15–44 in each state/year.

indexes states, t indexes time, γ_s and γ_t represent state and year fixed effects, ϵ represents a residual. Year fixed effects capture time varying factors, such as the business cycle, that impact all states in a given year. Similarly, state fixed effects control for time invariant differences across states, such as differences in attitudes towards abortion. It is also possible that, say, preferences change within states over time in ways that are different from other states. In fact, the figures presented above show that time-series patterns in the control and treatment group states in some cases appear quite different in ways that are unrelated to the timing of the law change. To fully control for these differences would require including the interaction of state and year fixed effects. Unfortunately, a model including these interactions is under-identified. As an alternative, we include a vector of interactions between a time trend and state fixed effects which will account for cross-state differences that are changing linearly.

One control variable that has been considered in previous work is the number of abortion providers in a state/year, which can control for differences in a woman's access to abortion. Blank et al., 1994 point out that this supply side variable may be endogenous, increasing in response to greater abortion demand, for example. Therefore, they instrument the number of abortion providers with the number of physicians and hospital beds relative to the size of the population. In this research, we include both the physician rate and hospital bed rate directly, providing reduced form estimates of the model indicated by Blank et al.

7.2. Results

The results of these analyses are reported in Table 3. Columns 1 and 2 report results from models of abortion rates that control for state and year fixed effects and then state-specific trends, respectively. Results show that Medicaid funding restrictions reduce the number of abortions performed. Estimates in column 1 indicate that imposition of funding restrictions lowers the abortion rate by 1.5 abortions per 1000 women aged 15–44. This effect is statistically significant at the 1% level. Since the mean abortion rate over this period is about 28, this amounts to about a 5.5% reduction. Adding state-specific trend variables signifi-

Table 3
OLS regression estimates of effect of state Medicaid abortion funding restrictions on abortion, birth and "pregnancy rates," aggregate data [a]

Variable name	Abortion rates		Birth rates		Pregnancy rates	
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid Restrictions	−1.442	−0.835	−1.936	−0.582	−3.377	−1.418
	(0.378)	(0.446)	(0.461)	(0.400)	(0.604)	(0.640)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects * trend	No	Yes	No	Yes	No	Yes

[a] Regressions are weighted by the number of women aged 15–44 in each state/year and include all of the independent variables listed in Table 1. Standard errors in parentheses.

cantly lowers the estimated effect of funding restrictions. Estimates in column 2 show that this restriction lowers the number of abortions by less than one per 1,000 women aged 15–44 (about a 3% reduction) and this estimate is now statistically significant at only the 10% level.

The remainder of Table 3 examines the impact of abortion restrictions on birth and pregnancy rates. Columns 3 and 4 present results for birth rates. When state and year fixed effects are included (column 3), instituting Medicaid funding restrictions is shown to statistically significantly reduce the birth rate by roughly two births per 1,000 women aged 15–44. Column 4 includes state-specific trends and parameter estimates reported here indicate that funding restrictions are not significantly related to births, although the point estimate is negative. Since the pregnancy rate is defined as the sum of the birth and abortion rates, pregnancies are also shown to significantly fall by 1.5–3.5 per 1 000 women aged 15–44, depending upon specification (see columns 5 and 6).

To interpret the magnitude of these effects estimated from aggregate data, note that during this sample period about 1.5 million abortions and 3.6 million births occurred annually. Estimates from columns 1 and 2 indicate that roughly 40 000 to 80 000 abortions would be eliminated if the Medicaid system stopped funding abortions in the entire country. These reductions represent a decrease of between 20% and 40% of the roughly 200 000 abortions paid for by Medicaid in 1980, when a court injunction required abortion funding for most of the year Gold, 1982. Point estimates from columns 3 and 4 indicate the number of births would decrease by about 30 000 to 100 000. Therefore, the number of pregnancies are estimated to decrease by roughly 70 000 to 180 000.

Table 4 reports the impact of Medicaid abortion funding policies on the fertility behavior of different age groups. Although it would be beneficial to examine the effect on abortions, AGI data does not desegregate by age, so our analysis is restricted to the effects on birth rates [18]. To calculate age-specific birth rates, the number of age-specific births are obtained from annual editions of Vital Statistics and the number of women of that age in each state/year is obtained from intercensal population estimates provided directly by the Census Bureau. The results reported in Table 4 indicate that, regardless of specification, women aged 20–24 experience the largest reduction in births in response to a Medicaid funding restriction. Parameter estimates for teens are also negative and significant and moderately robust to specification (effect is significant at 10% level only with state-specific trends). Births to women 25–34 and 35–44 are estimated to fall when only state and year fixed effects are included, but these estimated effects

[18] CDC data does provide separate abortion rates for teens and non-teens which have been utilized by Blank et al., 1994. Shortcomings of these data, however, which have been described earlier, make the usefulness of these data questionable. Blank et al. finds no significant effect on the abortion rates of either age group even though they find large effects when using AGI data.

Table 4
Estimated effects of Medicaid funding restrictions on birth rates by age group, aggregate data [a]

Group	Mean birth rate	State and year fixed effects	State and year fixed effects and state specific trends
Age 15–19	51.85	−1.077 (0.593)	−1.399 (0.842)
Age 20–24	110.89	−1.912 (0.703)	−1.820 (0.706)
Age 25–34	89.19	−1.868 (0.472)	0.573 (0.362)
Age 35–44	14.04	−1.282 (0.172)	0.010 (0.094)

[a] Regressions are weighted by the number of women in each age category in each state/year and include all of the independent variables listed in Table 1. Standard errors in parentheses

become positive and insignificant when state-specific trends are added to the model.

Table 5 examines whether the impact of Medicaid funding restrictions on all women is instantaneous or whether the size of the effects changes over time. We report models including funding status lagged one year and a "years since enactment" trend variable, with and without state-specific trends. Columns 1–4 report results for abortion rates. In these models, lagged funding restrictions have negative and significant effects on abortion rates, but years since enactment has no significant effect. These findings indicate that the impact of funding restrictions is larger the year after imposition of the law, but the size of the effect does not significantly increase beyond that.

Parameter estimates reported in columns 5–8 indicate that the effect of Medicaid restrictions on births is dependent upon statistical specification. Recall from Table 3, columns 3 and 4 that a significant reduction in births is estimated in models including state and year fixed effects only, but the estimated effect is not statistically significant at conventional significance levels once state specific trends are added to the model. In Table 5, models including only state and year fixed effects (columns 5 and 6) indicate that funding restrictions significantly reduce births the year following enactment and the effect appears to grow over time. In models that include state-specific trends, however, alternative patterns emerge. In particular, column 8 indicates that birth rates are estimated to significantly fall in response to enactment of a funding restriction and this effect grows significantly over time. Although these results indicate that effects on births are highly sensitive to statistical specification, the range of estimates indicate that births are either estimated to remain unchanged or fall in response to Medicaid funding restrictions. Birth rates are not estimated to rise in any specification. Therefore, pregnancies are unambiguously estimated to fall, as reported in columns 9–12.

8. Analysis of NLSY data

Although the state level data employed in the preceding analysis have the significant advantage of providing reliable data on abortions and births, they have

Table 5
Time series properties of effect of state Medicaid abortion funding restriction status on abortion, birth and pregnancy rates, aggregate data [a]

Variable name	Abortion rates				Birth rates				Pregnancy rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Medicaid restrictions	−0.755 (0.521)	−1.442 (0.427)	−0.527 (0.477)	−1.088 (0.494)	−0.915 (0.666)	−0.912 (0.503)	−0.750 (0.455)	−1.343 (0.435)	−1.670 (0.845)	−2.354 (0.665)	−1.277 (0.706)	−2.430 (0.700)
Lagged Medicaid restrictions	−1.044 (0.525)		−1.042 (0.477)		−1.491 (0.670)		−0.055 (0.464)		−2.534 (0.850)		−1.096 (0.718)	
Years since enactment		−0.019 (0.094)		−0.264 (0.201)		−0.545 (0.110)		−0.674 (0.176)		−0.563 (0.146)		−0.938 (0.284)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects * trend		No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes

[a] Regressions are weighted by the number of women aged 15–44 in each state/year and include all of the independent variables listed in Table 1. Standard errors in parentheses.

a few shortcomings. First, the nature of these data obviously restricts us from controlling for personal characteristics that may affect a woman's fertility behavior. Second, we cannot examine the behavior of population subgroups that may react to abortion restrictions differently, such as poor women responding to restrictions on Medicaid funding of abortion. Finally, the lack of pregnancy loss data led us to assume that there is no effect of funding restrictions on losses, weakening our conclusions regarding the effect of this policy on pregnancies. Therefore, we explore an alternative source of data, the NLSY, which can address each of these shortcomings.

The NLSY first interviewed 6283 women aged 14 to 21 in 1979 and has reinterviewed them annually through 1990, the most recently available survey at the time this project began. These data are *not* nationally representative, oversampling Blacks, Hispanics and lower income families at twice the rate proportional to the population. The survey provides detailed information on personal and family background characteristics, state of residence, economic and demographic characteristics of the respondent's county of residence, and, importantly, complete fertility histories. From these data, we have coded variables indicating whether a woman was pregnant, gave birth, experienced a pregnancy loss, or aborted a pregnancy for each year between 1979 and 1989 [19]. Then we merged the abortion restriction data to this data set to determine which restrictions existed in each respondent's state of residence for each survey year.

Although the NLSY overcomes many of the shortcomings of the aggregate data, it contains perhaps even more serious deficiencies. Most important, abortions are significantly under-reported, at perhaps less than half the rate suggested by external sources Jones and Forrest, 1992. If abortion underreporting in the NLSY is positively correlated with Medicaid funding restrictions, ceteris paribus, then estimation results may erroneously indicate that restrictions lead to fewer abortions when, in reality, they may simply lead to fewer *reported* abortions. This potential for reporting biases should be kept in mind in interpreting the results from these data. Another problem with the NLSY data is that they provide a relatively small number of reported abortions (roughly 600 over the entire 12 year period). We are, therefore, restricted from estimating models that include state-specific fixed effects. In these specifications, the fact that only a few abortions are reported in many states/years yields considerable imprecision in parameter estimates. In spite of these shortcomings, we employ these data because they provide us with the only way to examine the impact of funding restrictions on those directly affected.

Average fertility outcomes from the NLSY, reported in Table 6, are estimated for all women (column 1) and for women differentiated by their exposure to

[19] A unit of observation in the data set employed here is a woman in a year. The final sample size of 51812 is less than 6283 multiplied by 11 (the number of women and the number of years of fertility data, respectively) because there is some sample attrition over time and because some observations had be dropped due to incomplete information.

Table 6
Means of variables, NLSY and aggregate data

Variable	Full NLSY sample			NLSY poverty subsample		
	All women (1)	Restricted (2)	Unrestricted (3)	All Women (4)	Restricted (5)	Unrestricted (6)
Abortion rate [a]	12.9	8.6	16.9	16.4	10.6	21.9
Birth Rate [a]	102.1	107.0	97.5	114.7	111.6	117.7
Pregnancy Rate [a]	129.0	128.2	129.7	146.6	133.0	159.6
Pregnancy Loss Rate [a]	16.7	15.6	17.7	19.1	16.3	21.8
Sample size	51812	25020	26792	11100	5450	5650

[a] Rates are per 1000 women in the sample in the NLSY data.

Medicaid abortion funding restrictions (columns 2 and 3). For every 1 000 women in the full NLSY sample, 129 reported a pregnancy, 102 gave birth, 13 reported having an abortion and 17 reported a pregnancy loss [20]. In comparing these rates with the aggregate data, we see that the abortion rates are considerably lower in the NLSY, which is attributable to problems with underreporting, discussed previously. The birth rate in the NLSY data is higher than in the aggregate data. This finding is consistent with the relatively young age of NLSY respondents. Comparisons with Vital Statistics birth data for women of roughly the same age indicate that the NLSY birth data are quite reliable. In addition, pregnancy loss rates are quite comparable to those estimated from external sources Ventura et al., 1992.

We estimate Probit models using the NLSY data that are analogous to those estimated using aggregate data and that take the form:

$$\text{Prob}(Y_{\text{ist}} = 1) = F(R_{\text{st}}\beta_1 + X_{\text{ist}}\beta_2 + F_{\text{is}}\beta_3 + C_{\text{ist}}\beta_4 + \gamma_{\text{r}} + \gamma_{\text{t}}) \quad (3)$$

$$\text{Prob}(Y_{\text{ist}} = 1) = F(R_{\text{st}}\beta_1 + X_{\text{ist}}\beta_2 + F_{\text{is}}\beta_3 + C_{\text{ist}}\beta_4 + \gamma_{\text{r}} + \gamma_{\text{t}} + \text{trend}^*\gamma_{\text{r}}) \quad (4)$$

$$\text{i} = 1, \ldots, N$$

where Y is now an indicator variable for abortion, live-birth, pregnancy or pregnancy loss, X represents personal characteristics of the respondent, F represents her family background characteristics, C represents characteristics of her county and i indexes the N women in the sample. As indicated above, we cannot employ state fixed effects because the number of women in each state/year with certain outcomes is quite small. As an alternative we include fixed effects (γ_r in Eq. (3)) representing the nine census regions along with a fixed effect indicating

[20] In these data, the number of pregnancies is slightly less than the sum of births, abortions and pregnancy losses, mainly because some women experience more than one pregnancy in a year. For instance if a woman loses a pregnancy and then gets pregnant again leading to a birth, each outcome would be recorded during the year, but only one pregnancy.

states that instituted funding restrictions at some time over the sample period. In Eq. (4), these fixed effects are interacted with linear time trends.

Similar models are also estimated including an interaction term between funding status and a dummy variable indicating whether or not an individual falls below the poverty line [21]. This model uses those women above poverty in restricted states as another form of control group. To the extent that state characteristics are changing in ways that are not captured by the fixed effect specifications, this within state control group can reduce the bias that such changes may impose on the estimated effect of funding restrictions.

Table 7 presents the results obtained from the NLSY data [22]. Reported results represent derivatives multiplied by 1,000, which can be interpreted as the effect on the appropriate ''rate'' (i.e. per 1 000 women in the sample). Columns 1 and 2 present results regarding the probability of abortion. In the full sample, Medicaid funding restrictions are associated with a reduction in abortions by about 2 to 3 women per 1 000. These effects are somewhat higher than those obtained from the aggregate data and are consistent with both the younger age of the sample and the oversample of women in poverty. In models that interact restriction status with a poverty indicator variable, point estimates indicate larger effects on the poverty population. Imprecision in the estimated parameters, however, make drawing firm conclusions difficult.

The remainder of Table 7 considers the effects of funding restrictions on births, pregnancy losses and pregnancies. Results indicate that these restrictions have no significant impact on births for the overall population (columns 3 and 4). When women are differentiated by poverty status, however, it appears that births to women in poverty fell relative to women whose family income fell above the poverty line. This difference is statistically significant in models that include region-specific linear trends. An important finding in these data concerns the impact of funding restrictions on pregnancy losses, reported in Columns 5 and 6. Estimates indicate that pregnancy losses appear to be unrelated to Medicaid funding policy regardless of income level. These findings support the assumption made in the aggregate data that allowed us to create a pregnancy rate by adding births and abortions. The effect of restrictions on pregnancies, reported in columns 7 and 8, show that restrictions have no significant impact on pregnancies for all non-poor women. However, women in poverty are shown to be significantly less likely to get pregnant when faced with Medicaid abortion funding restrictions.

[21] An earlier version of this paper (Levine et al., 1995) report results of models estimated separately by poverty status. The results are very similar.

[22] Since time series patterns are intertwined with life-cycle effects in the NLSY data, we do not estimate models examining the time series properties of estimated effects as we have reported in Table 7 using the aggregate, state-level data.

Table 7
Effect of state Medicaid abortion funding restriction status on fertility outcomes for all women and for women differentiated by poverty status, NLSY data [a]

Variable name	Abortions		Births		Pregnancy losses		Pregnancies	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Aggregate Effects							
Medicaid restriction	−2.281	−3.203	−5.694	−1.606	0.011	0.573	−7.668	−7.249
	(1.409)	(1.592)	(4.703)	(5.244)	(2.059)	(1.865)	(5.278)	(5.898)
	Effects by population subgroup							
Medicaid restriction	−1.554	−2.523	−3.460	1.034	−0.033	0.884	−3.072	−2.350
	(1.565)	(1.742)	(5.076)	(5.662)	(2.214)	(2.015)	(5.721)	(6.387)
Poverty status	3.454	3.513	44.90	44.88	8.881	9.012	58.67	58.39
	(1.360)	(1.359)	(4.919)	(4.93)	(1.998)	(2.006)	(5.48)	(5.483)
Medicaid restriction * poverty status	−2.407	−2.424	−8.994	−9.447	−1.036	−1.151	−17.84	−18.06
	(1.814)	(1.804)	(5.892)	(5.892)	(2.408)	(2.412)	(6.64)	(6.645)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes							
Restriction state fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects * trend	No	Yes	No	Yes	Yes	No	No	Yes
Restriction state fixed effect * trend	No	Yes	No	Yes	Yes	No	No	Yes

[a] Estimates reflect derivatives multiplied by 1000, standard errors in parentheses. All specifications include controls for a woman's level of education, age, race, marital status, number of children, urban residence, mother's and father's level of education, indicator variables representing whether or not respondent grew up in mother only household and whether or not respondent's mother worked when respondent was age 14, and the same state characteristics reported in Table 1 used in the analysis of the aggregate data.

9. Conclusion

This paper has empirically examined the effect of state Medicaid abortion funding restrictions on fertility outcomes. We initially employed aggregate, state level data from the period 1977–1988 and found that funding restrictions reduce the number of abortions performed. These restrictions are also shown to have no effect or a negative effect on births. For abortions to go down and births to be either unaffected or reduced, it is likely to be the case that pregnancies are reduced as well. These findings are generally supported in the NLSY data and additional results indicate that the behavioral effects of funding restrictions are concentrated among women below the poverty line. Although both data sets have some shortcomings for the purposes of the analysis conducted here, their weaknesses are completely unrelated to each other. Relatively consistent findings obtained from them may therefore strengthen the conclusions drawn.

We have attempted several fixed effect specifications to control for unobservable characteristics across states/regions and years. Nevertheless, the nature of the abortion restriction data prevents us from including a full interaction of state and year fixed effects. The results of this analysis may still be biased to the extent that the implementation of abortion restrictions are correlated with unobservable changes within states over time. In other words, it is still possible that the effects of restrictions on fertility behavior reported here are not causal. Alternative methodologies will be required to address this concern.

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Appendix A. Sources of data on abortion laws and aggregate, state-level data

A.1. Abortion laws

The Alan Guttmacher Institute. Legislative Record. 1979–1989.

Bush, Diane. Fertility-related state laws enacted in 1982. Family Planning Perspectives. Volume 15(3), May/June 1983, pp. 111–16.

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George, Christine C. State Laws Restricting Minors' Abortions, by State and Year, Addendum 2 to Blank, et al., State abortion rates: The impact of policies, providers, politics, demographics and economic environment, mimeo, Northwestern University, July 1994.

Gold, Rachel and Sandra Guardado. Public funding of family planning, sterilization and abortion services, 1987. Family Planning Perspectives. Volume 20(5), September/October 1988, pp. 228–33.

Gold, Rachel Benson and Jennifer Macias. Public funding of contraceptive, sterilization and abortion services, 1985. Family Planning Perspectives. Volume 18(6), November/December 1986, pp. 259–64.

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NOW Legal Defense and Education Fund and Dr. Renee Cherow-O'Leary. The state-by-state guide to women's legal rights. New York: McGraw–Hill Book Company, 1987.

Sollom, Terry. State legislation and reproductive health in 1990: What was proposed and enacted. Family Planning Perspectives. Volume 23(2), March/April 1991, pp. 82–94.

Sollom, Terry and Patricia Donovan. State laws and provision of family planning and abortion services in 1985. Family Planning Perspectives. Volume 17(6), November/December 1985, pp. 262–66.

A.2. Aggregate data (variables obtained from each source are listed below the source)

American Hospital Association. Hospital Statistics. Chicago, 1983 and 1987 editions.
Hospital bed rate 1982, 1986

American Medical Association. Physician characteristics and distribution in the United States. Chicago: Survey and Data Resources, 2985 and 1988 editions.
Physician rate 1983, 1986

Bureau of the Census. County and City Data Book. U.S. Department of Commerce. Washington, DC: U.S. Government Printing Office, 1983 and 1986 editions.

Hospital bed rate 1980, 1983, 1988
Physician rate 1980, 1982, 1988

Bureau of the Census. State and Metropolitan Area Data Book 1986. U.S. Department of Commerce. Washington, DC: U.S. Government Printing Office.
Hospital bed rate 1985
Physician rate 1985

Bureau of the Census. Statistical Abstract of the United States. Washington, DC: U.S. Government Printing Office, various editions.
Crime rate 1977–1990
Physician rates 1977, 1978
Hospital Bed rates 1977, 1978
Per capita income 1977–88
Abortion rates 1977–82, 1984, 1985, 1987, 1988
Birth rates 1977–88

U.S. Department of Health and Human Services, Administration for Children and Families, Office of Family Assistance. Characteristics of State Plans for AFDC. Washington, DC: U.S. Government Printing Office, annual editions.
State AFDC benefits, 1977–88.

Table A1: Years abortion restrictions in effect between 1977 and 1990, by state

Medicaid funding restrictions

Alabama *	(1981–90)	Montana *	(1981–90)
Arizona	(1977–90)	Nebraska *	(1981–90)
Arkansas *	(1981–90)	Nevada *	(1981–90)
Colorado	(1985–90)	New Hampshire *	(1981–90)
Delaware *	(1981–90)	New Mexico *	(1981–90)
Florida *	(1981–90)	North Dakota	(1978–90)
Georgia *	(1981–90)	Ohio *	(1981–90)
Idaho *	(1981–90)	Oklahoma *	(1981–90)
Illinois *	(1981–90)	Pennsylvania	(1985–90)
Indiana *	(1981–90)	Rhode Island	(1978–90)
Iowa *	(1981–90)	South Carolina *	(1981–90)
Kansas *	(1981–90)	South Dakota	(1978–90)
Kentucky	(1978–90)	Tennessee *	(1981–90)
Louisiana *	(1981–90)	Texas *	(1981–90)
Maine	(1981–90)	Utah *	(1981–90)
Michigan	(1989–90)	Vermont	(1981–83)
Minnesota *	(1981–90)	Virginia *	(1981–90)
Mississippi *	(1981–90)	Wisconsin *	(1981–90)
Missouri *	(1981–90)	Wyoming	(1978–90)

* Represents states that are counted as "treatment group" states in Table 3. Other states with law changes were excluded from that analysis (i.e. not included in the control group).

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Ventura, S.J., S.M. Taffel, W.D. Mosher and S. Henshaw, 1992, Trends in pregnancies and pregnancy rates, United States, 1980–1988. Monthly Vital Statistics Report 41, 1–11.