TEEN MOTHERHOOD AND ABORTION ACCESS*

THOMAS J. KANE AND DOUGLAS STAIGER

We investigate the effect of abortion access on teen birthrates using county-level panel data. Past research suggested that prohibiting abortion led to higher teen birthrates. Perhaps surprisingly, we find that more recent restrictions in abortion access, including the closing of abortion clinics and restrictions on Medicaid funding, had the opposite effect. Small declines in access were related to small declines among in-wedlock births; out-of-wedlock births were relatively unaffected. Both results are consistent with a simple model in which pregnancy is endogenous and women gain new information about the attractiveness of parenthood only after becoming pregnant.

I. INTRODUCTION

Over the last decade women in the United States have seen an increasing number of restrictions placed on their access to abortion services, ranging from limitations on state and federal funding of abortion to an increase in the distance women must travel to find an abortion provider [Henshaw 1991; Henshaw and Van Vort 1992]. At the same time, recent trends (Figure I) show an explosion in teen birthrates (largely due to out-of-wedlock births) to levels not seen since abortion became legal throughout the United States in 1973. Despite often heated public debate over both abortion and teen motherhood, surprisingly little is known about the impact of recent restrictions of abortion access on fertility.

This paper investigates the relationship between abortion access and teen birthrates. If teen *pregnancy* rates are largely unaffected by abortion access, then restrictions on abortion access necessarily lead to more unwanted, mostly out-of-wedlock teen births (*cf.* Hayes [1987], Ch. 7). In contrast, we find that recent restrictions on abortion access were associated with a *reduction* in teen birthrates, largely among in-wedlock births. While counterintuitive, these findings are consistent with a

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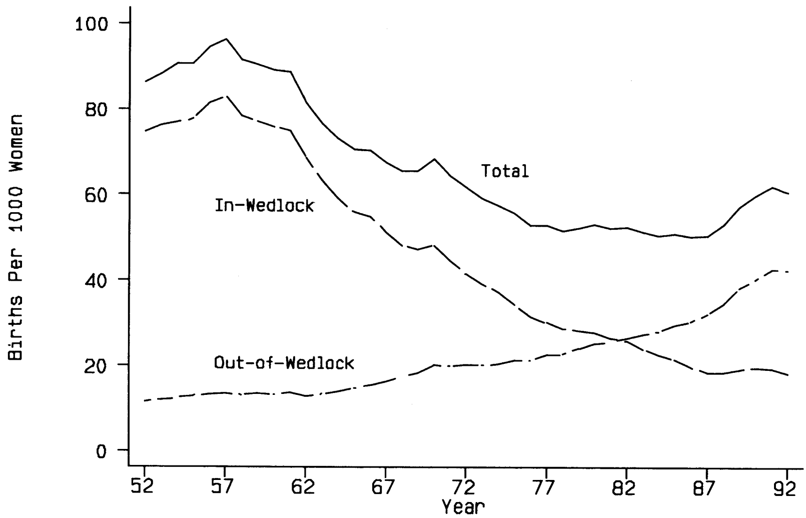

FIGURE I
Trends in Birthrates, Women age 15–19

Source. *Vital Statistics of the United States,* U. S. Bureau of Census.
Note. The denominator for all three rates is the total number of women aged 15–19.

simple model of rational decision-making under uncertainty in which pregnancy is an endogenous decision.

Our simple model assumes that women get information during the early months of pregnancy, and abort the pregnancy if the birth turns out to be unwanted based on this new information. Given that the majority of teen pregnancies are conceived out of wedlock, the father's willingness to marry is an obvious example of such information.[1] Contraception and abstinence decisions are made only on the basis of information available before the pregnancy occurs. In contrast, the abortion decision is made with the benefit of the new information. Abortion (unlike contraception or abstinence) works as an insurance policy to limit the downside risk when that information is negative. Increasing the cost of abortion increases the cost of this insurance policy and discourages women from becoming pregnant. Some of these pregnancies

1. In the late 1970s roughly two-thirds of first-born babies to women aged 15–19 were conceived out of wedlock. Of those women who conceived out of wedlock, roughly one-third were married before the birth of the baby, and nearly half were married within one year of the birth. See O'Connell and Rogers [1984], tables 1–3.

would have resulted in births, so the model implies that an increase in the cost of abortion results in a decline in wanted births. Of course, there is a second more conventional effect as the increased cost of abortion discourages some women from aborting unwanted pregnancies. Thus, the net effect of any restriction of abortion access on birthrates is ambiguous and, in the end, an empirical question.

The bulk of the paper is devoted to an empirical analysis of the relationship between teen birthrates and recent changes in abortion access. We use county-level data over fourteen years to study the effect of three distinct sources of variation in abortion access: the geographic siting of abortion providers, state Medicaid restrictions on abortion financing, and teen parental consent laws. We particularly exploit the county-level variation in our data, e.g., by investigating the differential effect of Medicaid funding restrictions on birthrates in poorer counties, and the impact of the opening of a new abortion clinic in an area that was previously unserved.

One obvious concern is that these measures of abortion access may be endogenous. Changes in these measures may reflect underlying changes in state or local attitudes toward abortion and childbearing that independently influence birthrates. Panel data at the county level allow us to control for such omitted variables to a far greater extent than previous studies of this question. For example, we are able to control for a full set of county and state-by-year fixed effects in addition to county-level demographic and economic characteristics. We further examine birthrates prior to discrete changes in abortion access for evidence of preexisting trends that may be correlated with changes in abortion access. Finally, we compare our results across different samples (e.g., teens and older women, in-wedlock and out-of-wedlock births) for which our theory implies a pattern that would not necessarily be expected if the results were being driven by endogeneity.

We find that restricting access to abortion is consistently associated with a small but significant decline in the teen birthrate, with most of the decline occurring among in-wedlock births. For example, we estimate that for a 25-mile increase in the distance to the nearest abortion provider, birthrates decrease by 1 percent among white 15–19 year-old women with the entire decline occurring among in-wedlock births. Medicaid restrictions and parental consent laws have similarly negative effects, although we argue that it is difficult to distinguish the effects of these laws from general trends in teen birthrates occurring in the states that adopt such laws. Overall, the evidence is quite consistent with a model in which pregnancy is endogenous. Further, even a conservative reading of the evidence would be that there is no empirical support for the claim that recent restrictions on access to abortion have led to higher teen birthrates.

II. Theoretical Discussion

The conventional wisdom holds that an increase in access to abortion services should lead to a decline in teen motherhood. Often implicit in this view is the belief that teen pregnancies are exogenously determined—driven by ignorance of contraceptive technology, accidents in the use of that technology, and unexplained changes in social mores regarding sexual activity. If teen pregnancy is purely exogenous, an increase in access to abortion services would lead to an unambiguous decline in teen motherhood, as those who find themselves pregnant find it easier to terminate their pregnancies.

But pregnancy is not purely an exogenous event. It is clearly influenced by decisions regarding contraceptive use and frequency of intercourse. Improved access to abortion reduces the cost of a pregnancy and, therefore, may encourage young women to adopt behaviors that increase the chance of pregnancy. The important question is whether any of these marginal pregnancies are brought to full term. If so, then greater access to abortion could lead to higher birthrates on net.

A very stylized model demonstrates why the answer to this question is ambiguous. Suppose that at the time of becoming pregnant, women are uncertain about the consequences of giving birth. For example, the father's support may not be easily discerned until after one is pregnant. Abortion (unlike contraception) provides women an option, after this uncertainty has been resolved, of not giving birth. Thus, in this simple model, abortion limits the downside of a pregnancy. Some of those for whom pregnancy becomes a worthwhile risk may find that they want to keep the child ex post. As a result, more women will become pregnant, and some of these pregnancies will go to term. In other words, birthrates can increase as the result of improved abortion access, at least for those women for whom a birth was likely to be a desired outcome.

Slightly more formally, consider a model in which a woman first chooses whether to become pregnant. If the woman becomes pregnant, some information regarding the consequences of giving birth is revealed. For concreteness, suppose that with probability P a woman discovers that the birth will be in wedlock. The probability of an in-wedlock birth, P, is known to the woman ex ante. Of course, P will vary across women. All decisions are made based on expected utility. The utility for not becoming pregnant is normalized to 0 and for giving birth in-wedlock normalized to 1. Let the utility associated with an abortion be $-A$, and for giving birth out of wedlock be $-B$. Finally, we assume that both A and B are positive, so that not becoming pregnant is preferred both to abortion and to giving birth out of wedlock.

The implications of this simple model for fertility are summarized in Figure II. There are three possible outcomes corresponding to the three regions shown in Figure II. Women who are sufficiently pessimistic (low P) and who face a relatively high cost of abortion (A) will not risk pregnancy and, therefore, will not

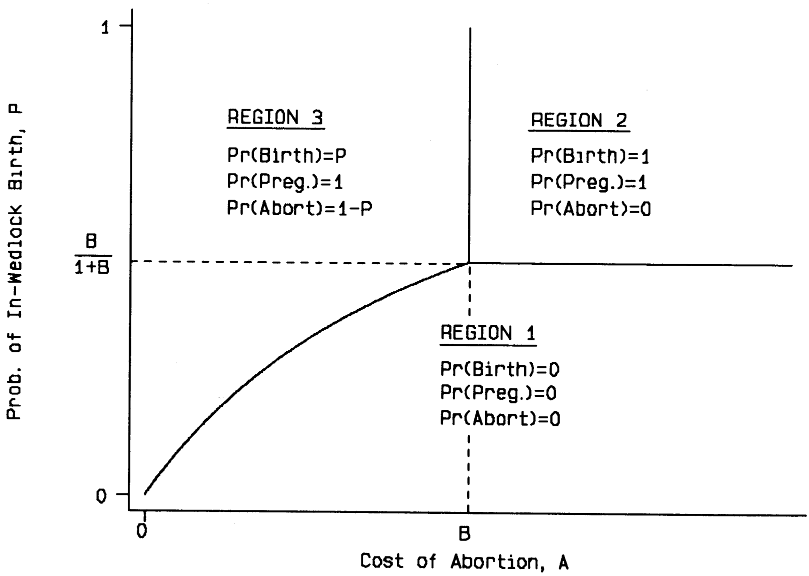

FIGURE II
Implications of the Model

become pregnant (Region 1). These are women for whom $P/(1 - P) < \min(A,B)$, implying that the expected utility of becoming pregnant is less than the utility of not becoming pregnant. On the other hand, women who are optimistic about giving birth in wedlock or who face low costs of abortion may choose to become pregnant. If the cost of abortion is prohibitive ($A > B$), then all of these pregnancies will lead to a birth (Region 2). Otherwise, women will bring these pregnancies to term with probability P (Region 3).

Figure II is useful in thinking about the effects of a change in the cost of abortion (A).[2] As the cost of abortion increases, some women (low P) will no longer become pregnant and thereby reduce both the probability of birth and abortion. Note that these women are made worse off by an increase in the cost of abortion, and the decline in births is entirely among in-wedlock births. In contrast, other women (high P) will not alter their pregnancy decision in response to an increase in the cost of abortion. Eventually the cost of abortion becomes prohibitive, and these women stop having abortions and thereby increase their probability of birth. These women are made worse off by an increase in the cost of abortion, and their additional births are out of wedlock. Thus, an increase in the cost of abortion unambiguously reduces pregnancy, abortion, and women's utility, but has ambiguous effects on births.

A general insight from the model is that the response of the birthrate to a change in abortion access may depend upon the extent of the change in access. Small increases in the cost of abortion, which do not make the cost prohibitive, push women from Region 3 to Region 1 in Figure II. In other words, a small *increase* in the cost of abortion works mostly on the pregnancy margin and leads to a *decline* in in-wedlock births, as those young women with poor prospects for ever deciding to carry a pregnancy to term decide not to become pregnant. In contrast, a more dramatic increase in the cost of abortion (such as making abortion illegal) works on the abortion margin, and pushes some women from Region 3 to Region 2, thereby leading to an increase in out-of-wedlock births. Thus, the model suggests a nonmonotonic relationship between abortion access and the birthrate; both the di-

2. Figure II can be used for other comparative static exercises as well. For example, a cut in AFDC benefits will increase B (the cost of an out-of-wedlock birth) and shift people from Region 2 to regions 1 and 3, unambiguously reducing births and increasing abortions.

rection and the type of birth (in-wedlock versus out-of-wedlock) being affected will differ for small and large changes in abortion access.

Is this a reasonable model? Despite its simplicity, the basic elements of the model are reasonably consistent with what is known about the process leading to teen motherhood. Most teen pregnancies occur out of wedlock, but of those that result in a birth, approximately one-third are legitimized through marriage prior to the birth [O'Connell and Rogers 1984]. Furthermore, marital status is one of the most important determinants of whether a pregnancy will be aborted [Henshaw and Silverman 1988], and problems in the relationship with the father are one of the most common reasons reported by patients for their decision to have an abortion [Torres and Forrest 1988]. Finally, contraceptive use among unwed teens has been shown to be associated with the perceived costs and benefits of pregnancy [Hofferth 1987]. In particular, Philliber et al. [1983] find that a woman is significantly more likely to use contraception if she indicates that she would be unlikely to have an abortion if ever becoming pregnant. Overall, these facts are consistent with a model of behavior in which teens make rational decisions about sexual activity and in which marriage prospects and abortion costs are important determinants of their decision.

Of course, our stylized model cannot hope to capture the full richness and diversity of the process that leads to teen motherhood. Women do not have perfect control over pregnancy, and many women may have unrealistic expectations regarding the consequences of teen motherhood. Furthermore, the in-/out-of-wedlock distinction is, at best, a crude proxy for the desirability of a birth. Nonetheless, the basic insight of the model is quite general: if abortion provides insurance against unwanted pregnancies, then a restriction in abortion access may lead to both a decline in wanted births and an increase in unwanted births for some women.

Akerlof, Yellen, and Katz [1996] have recently proposed two models of out-of-wedlock childbearing that are closely related to our model yet have quite different empirical implications. Two key features distinguish these alternative models from our model. First, the Akerlof-Yellen-Katz models involve negative externalities of abortion access on women. Thus, an increase in access to abortion does not necessarily improve the welfare of women in these models. The second distinguishing feature of these alternative models is that no information is revealed between the time of becoming pregnant and the time of choosing whether to have an abortion. Abortion in these models is only valued as a backup form of birth control if initial contraception should fail. In contrast, abortion has an option value in our model.

In their first model, Akerlof, Yellen, and Katz [1996] argue that the availability of abortion services increases sexual activity among young women who know themselves to be willing to make use of abortion services. This puts pressure upon those young women who know themselves to be opposed to abortion to engage in sex in order to maintain their relationships. As a result, these women have more unintended pregnancies. Like our model, this model implies that an increase in access to abortion may increase teen motherhood rates for some women. Unlike our model, this model implies that an increase in access to abortion results in more unwanted, presumably out-of-wedlock births.

In their second model, Akerlof, Yellen, and Katz [1996] argue that the availability of abortion services makes young men less willing to agree to a shotgun marriage in the event of an unintended premarital pregnancy. Since an unintended pregnancy is less likely to be legitimated, young women are also less likely to engage in premarital sex. In contrast to our model, this model implies that an increase in access to abortion results in fewer in-wedlock births and lower birthrates overall.

Therefore, if decisions about pregnancy are not exogenous, but are affected by the availability of abortion services, the direction of the relationship between abortion access and teen motherhood rates is theoretically unclear and differs across a number of plausible models. Furthermore, lessons learned from the literature on legalization of abortion are not necessarily informative even about the sign of this relationship for more modest changes in abortion access. The magnitude and direction of the impact of recent changes in abortion access is, therefore, an empirical question to which we now turn.

III. Previous Empirical Literature

There is a large body of evidence suggesting that legalization of abortion, in both Europe and the United States, has been associated with declines in birthrates, with the impacts being largest among women whose alternative birth control options are most limited.[3] The most convincing of these studies have examined birthrates before and after legalization of abortion, and have used states or countries that did not legalize as control groups. In several Eastern European countries (with very limited access to alternative forms of birth control) legalization of abortion was associated with a 25 percent decline in birthrates [Frejka 1983]. A smaller impact of abortion legalization on birthrates was observed in the United States, in the 5–10 percent range, perhaps reflecting better availability of contraception in the United States [Sklar and Berkov 1974; Bauman et al. 1977; Levine et al. 1995]. Levine et al. [1995] find that, within the United States, the largest impact of abortion legalization occurred in a handful of states that were the first to legalize abortion. States that later legalized as a result of *Roe v. Wade* experienced smaller declines in birthrates, in part because of the prior availability of abortion in neighboring states.

The evidence on less severe restrictions of abortion access is more mixed. A number of studies have investigated variation across states and counties in abortion access, using state laws and the availability of abortion providers as proxies for access.[4] These studies have generally found that better abortion access is associated with lower birthrates and higher abortion rates, but the findings are sensitive to the choice of control variables (see Garbacz [1990]). The obvious weakness in such cross-section studies is that the variation in abortion access may simply proxy for factors such as public sentiment toward abortion or levels of premarital sexual activity in the area, making causal interpretation of the results difficult.

A few papers exploit changes in state laws restricting abortion access using state-level panel data. Restrictions on state funding of abortions for Medicaid recipients is the most commonly used source of identification. After the *Roe v. Wade* decision in 1973, Medicaid generally covered abortions for AFDC-eligible

3. For Europe see Potts, Diggery, and Peel [1977], Coelen and McIntyre [1978], and Frejka [1983]. For the United States see Tietze [1973], Sklar and Berkov [1974], Bauman et al. [1977], Potts, Diggery, and Peel [1977], Quick [1978], Atrash et al. [1982], Joyce and Mocan [1990], and Levine et al. [1995].

4. Moore and Caldwell [1977], Singh [1986], Medoff [1988], Garbacz [1990], Lundberg and Plotnick [1990], Currie et al. [1994], Meier and McFarlane [1994], and Ohsfeldt and Gohman [1994].

low-income women. The Hyde Amendment prohibited federal Medicaid funding of abortion in 1977.[5] Some states picked up the coverage, and others did not. Therefore, public funding of abortion both increased and decreased in many states during the 1970s and early 1980s, thereby providing a potential natural experiment. A number of papers investigate this natural experiment with state-level panel data and allow for state effects.[6] However, they report conflicting impacts on birthrates, and the results are sensitive to how one controls for state-level trends. For instance, Jackson and Klerman [1994] find that Medicaid funding for abortion was associated with no differences in fertility among white women (except, possibly, 20–21 year-olds) and lower fertility among black women.[7] In contrast, Trussell *et al.* [1980], Levine, Trainor, and Zimmerman, [1995], and Matthews, Ribar, and Wilhelm [1995] find that birthrates were higher—not lower—when states provided public funding of abortions.

Many of the same authors have also studied the impact of state regulations requiring parental consent for teenagers seeking abortions.[8] Matthews, Ribar, and Wilhelm [1995] and Jackson and Klerman [1994] both found that teen birthrates *fell,* rather than rose, following the implementation of parental consent laws.[9] However, birthrates fell for older women at the same time, so that on balance there is no strong evidence that consent laws affected teen birthrates. The literature also suggests two reasons why parental consent laws may have had such small impact. First, Blum, Resnick, and Stark [1987] reported that two-fifths of teen abortion recipients in Minnesota had not, in fact, notified

5. The law provides exceptions in cases where the life of a woman was in danger. In February 1978 the federal funding restrictions were loosened slightly, providing federal funds in cases where the long-term health of the women was to be affected and where the pregnancy was the result of rape or incest. Federal funding was temporarily reestablished between February and September of 1980, as a result of a court's injunction.

6. Blank, George, and London [1994] and Haas-Wilson [1994] consider impacts on abortions only. Trussell et. al. [1980], Jackson and Klerman [1994], Levine, Trainor, and Zimmerman [1995] and Matthews, Ribar, and Wilhelm [1995] also look at birthrates.

7. Jackson and Klerman [1994] also show that without state fixed-effects, they observe results similar to those reported above.

8. Haas-Wilson [1994] found that the parental consent laws were associated with lower abortion rates for teens using within-state differences, while Blank, George, and London [1994] report no effect on abortion rates.

9. Jackson and Klerman [1994] emphasize that birthrates of white teens rose *relative to* the fertility rates of older women, but this occurred only because the birthrates of teens in states enacting these laws fell by less relative to other states than the birthrates of older women.

their parents of their abortions, using instead a court "bypass" provision. Second, in an analysis of a Massachusetts parental consent law enacted in 1981, Cartoof and Klerman [1986] reported a sharp *decrease* in abortions received by minors in Massachusetts clinics, but an equally large *increase* in the number of abortions received by Massachusetts' youth in neighboring states.

Geographic variation in abortion clinic siting has not been used to evaluate the effect of abortion access on teen motherhood rates. This is somewhat surprising, given the important role that travel to a distant provider plays in mitigating the impact of parental consent laws. Shelton, Brann, and Schulz [1976] studied differences in abortion rates (but not birthrates) for Georgia counties by their distance from Atlanta, where the largest abortion providers were located. Indeed, the ratio of abortions to live births was higher in counties close to Atlanta. Further, when abortion clinics opened in two small cities outside Atlanta, the abortion ratio increased disproportionately for the residents of surrounding counties.

Implicit in much of the literature and the public debate is the presumption that each of these policies—from legalization through the requirement of parental consent for teens—will have similar effects on fertility rates, albeit of different magnitudes. However, this is not necessarily true, since each source of variation cuts on a different margin. While not trivial, the more recent access barriers are modest relative to the barriers faced by women prior to the legalization of abortion. For example, based on calculations with our own data, closing of a provider or enforcement of a parental consent law (so that teens must go out of state to avoid the law) seldom increases travel distance to the nearest abortion clinic by more than 100 miles. Similarly, without the Medicaid subsidy, the charge for a first-trimester abortion averaged about $300 in 1989 [Henshaw 1991]. The theory developed in Section II suggests that such modest restrictions may have ambiguous effects on birthrates.

Overall, the existing research suggests that we focus on three important empirical questions. Are the puzzling findings from the recent state-level analyses the result of omitted factors such as changing conservatism in a state? Do other proxies for abortion access, such as distance to the nearest abortion provider, yield similar results? Finally, are additional implications of the behavioral model from Section II, such as the in-wedlock versus out-of-wedlock distinction, supported by the evidence?

IV. Data

We investigate the response of teen birthrates to changes in abortion access. Abortion access is proxied by distance to the nearest abortion provider and state laws that eliminate Medicaid funding of abortion or require parental consent for minors to obtain an abortion. The basic regression used to estimate the relationship between the birthrate and abortion access is

$$\begin{aligned}\text{Birthrate} = f(&\text{Distance, State Laws,}\\ &\text{County Economic Variables)}\\ &+ \text{Year and County Fixed-Effects.}\end{aligned}$$

Summary statistics for all variables used in the analysis may be found in Table I. The unit of observation is the county-year, with the data set covering 3037 counties and the years 1973–1988, excluding 1983 and 1986 due to data limitations. A few counties were excluded from the final analysis because of missing data or obvious inconsistencies in data reporting. For the analysis of nonwhites, any county with a single year in which there were no nonwhite teen residents is excluded, leaving 2524 counties.

We define the teen birthrate as the number of children born to teen mothers in a calendar year divided by the number of teen women. Birthrates for teens (and other age groups) are calculated by year, mother's county of residence, and race of the child (white or nonwhite) with annual data from the National Natality Local Area Summary files that are derived from birth certificate records [National Center for Health Statistics, various years]. Reporting of births by race of the child, rather than race of the mother, implies that births to white mothers and nonwhite fathers are misclassified (for our purposes) as nonwhite births. This leads to an upward bias in nonwhite births, particularly for counties with relatively few nonwhites.

Annual estimates of the number of women residing in a county are available by race and five-year age bracket from the National Cancer Institute [1993]. These are the best available population estimates at the county level for intercensal years, but may be quite inaccurate for small populations and in particular for nonwhites. As a result of the inaccuracy in both the numerator and denominator, nonwhite birthrates are quite noisy at the county level. For example, the nonwhite teen birthrate is greater than 100 percent for over 7 percent of the county-years in our sample.

TABLE I
SUMMARY STATISTICS (N = 42518)

Variable description	Mean (S.D.)
Birth rates:	
Age 15–19, white	0.0450 (0.0201)
Age 15–19, nonwhite	0.1011 (0.0336)
Age 15–17, white	0.0257 (0.0138)
Age 18–19, white	0.0739 (0.0317)
Age 20–29, white	0.1099 (0.0250)
Miles (100s) to the nearest Abortion provider	0.1437 (0.2588)
Additional miles to reach Abortion provider in state with no parental consent > 50?	0.0329 (0.1784)
Medicaid abortion funding restricted?	0.3862 (0.4643)
Parental consent required for abortion?	0.0732 (0.2604)
log (per capita income)	9.085 (0.421)
log (total employment)	11.58 (1.73)
Percent of employment in construction industry	0.0516 (0.0201)
Percent of employment in manufacturing industry	0.1861 (0.0948)
Percent of income derived from unemployment insurance	0.0072 (0.0056)
Percent of women age 15–29 who are nonwhite	0.1203 (0.1200)
Percent of population in poverty	0.0944 (0.0433)

Birthrate statistics are weighted by the number of women in the given age/race category residing in the county. All other statistics are weighted by the number of white women age 15–19 in the county.

Information on the presence and size of abortion providers in each county comes from the Alan Guttmacher Institute's County File of Abortion Data [1993], based on their annual census of providers. This census has been conducted every year since 1973 except for 1983, 1986, 1989, and 1990. Our data span the fourteen years of available data between 1973 and 1988. A provider is defined as any individual or organization that legally provided at least one abortion during the year. Distance to the nearest abortion provider is calculated for each county in each year based on the location of the population centroid for each county.[10]

There are two important sources of measurement error in the distance variable. First, small counties may have abortion providers that occasionally report doing no abortions in a given year. In the AGI data it will appear that the county temporarily lost its provider, although in reality access may have been unchanged. As a result of this type of error, distance to the nearest provider will bounce up and down in surrounding counties—a pattern that is apparent in our data for smaller counties. A second source of measurement error comes from using the county as the unit of analysis. Depending on where in the county a provider is located, distance between population centroids may over- or underestimate the true distance to a provider, particularly for large counties. In counties with a provider we systematically underestimate distance to the nearest provider, since our method sets the distance variable to zero in these counties. Unfortunately, the county level is the most disaggregate level for which provider location information is available.

Data on state laws affecting abortion come from Blank, George, and London [1994]. We consider two relevant laws: state laws restricting Medicaid funding of abortion and state laws requiring parental consent or notification for minors to get an abortion (hereinafter referred to as parental consent laws). These variables range between zero and one, and reflect the fraction of the year in which the law was enforced. Thus, states with court-ordered funding of abortion or parental consent laws that have been enjoined by the courts are coded as zeros.

The impact of these state laws on birthrates might be expected to vary within the state as well. Therefore, to further exploit the county level variation in our data, we interact the state laws with county characteristics that should be positively associated with the impact of the law. Medicaid restrictions should have larger effects on birthrates in counties with a large fraction of the population in poverty. Therefore, we interact the Medicaid restriction dummy with the fraction of the county in poverty as of the 1980 census [United States Bureau of the Census 1983].

10. The PICADAD file, constructed by the United States Bureau of the Census [1978], provides the location (latitude and longitude) of the population centroid of each county in the United States, based on 1970 census populations.

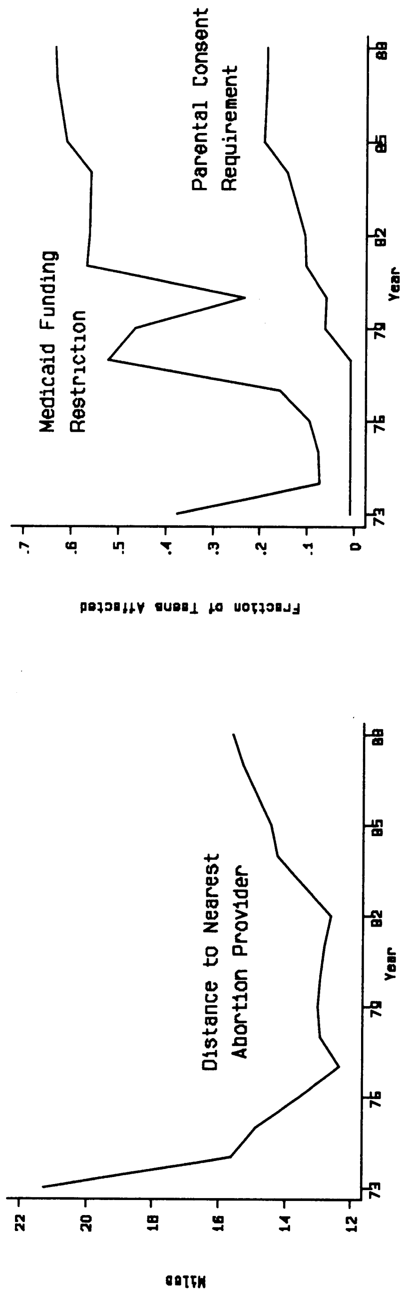

FIGURE III
Trends in Abortion Access

TABLE II
SUMMARY OF VARIATION IN DISTANCE TO NEAREST ABORTION PROVIDER

	County weighted	Population weighted	
	Full sample	Full sample	No provider in at least one year
Provider in county?	17.59%	64.68%	24.68%
Provider in all years?	9.12%	53.11%	0%
Provider in no years?	73.86%	27.23%	58.09%
Distance to provider:			
1–50 miles	49.00%	25.91%	55.26%
50–100 miles?	25.25%	7.93%	16.91%
More than 100 miles?	8.16%	1.48%	3.15%
Average distance	43.73 miles	14.37 miles	30.65 miles
Standard deviation of distance	39.26 miles	25.88 miles	30.48 miles
Within-county standard deviation of distance	16.71 miles	10.93 miles	15.96 miles
Average within-county range of distance	32.51 miles	14.92 miles	31.82 miles
Number of observations	42,518	42,518	38,640

County-weighted statistics weight each county equally.
Population-weighted statistics are weighted by the number of white women age 15–19 in the county.

The poverty rate is race-specific, so that white (nonwhite) poverty is used in the white (nonwhite) birthrate regressions. Similarly, we expect to see border effects of parental consent laws, i.e., smaller effects in counties that are near the border of a state that has no parental consent requirement. Therefore, we interact the parental consent dummy with a dummy variable indicating that women in the county would have to travel more than 50 additional miles to reach a provider in a state with no parental consent law.

Finally, annual data on county-level economic conditions are drawn from the Regional Economic Information System (REIS) data set provided by the United States Bureau of Economic Analysis [1994]. From these data we construct county-level measures of per capita income, total employment in the county, fraction of employment in manufacturing and construction, and fraction of income derived from unemployment insurance.[11] We

11. We use total employment, rather than the employment-to-population ratio, because it avoids having population estimates in the denominators of both the dependent and independent variables. Because of the noise in the population

also use the National Cancer Institute population estimates to construct the percent of women age 15–29 who are nonwhite.

Figure III displays national trends in our proxies of abortion access for whites (trends for nonwhites are quite similar). Nationally, distance to an abortion provider fell nearly by half between 1973 and 1977, was fairly flat until 1982, and then increased steadily through 1988. The fraction of teen women living in states with Medicaid restrictions and parental consent requirements rose dramatically over this period of time, although parental consent laws remained relatively uncommon. Teen motherhood rates follow a shallow U-shaped pattern over this period (see Figure I) so that aggregate trends give the impression that recent restrictions of access to abortion were, perhaps, associated with increased teen motherhood. Of course, other factors such as changing social norms and the increasing participation of women in the labor force could account for the general pattern in fertility, and at least some of the changes in both access and fertility reflect composition changes (such as increased urbanization) among the population of women.

Since the distance measure has not been used previously, Table II provides additional information on the variation in this measure. On average, 18 percent of counties had an abortion provider over our sample period. But 64 percent of teen women lived in these counties. In fact, 53 percent of teen women lived in counties that had a provider throughout the sample period. In other words, the data provide essentially no information on how distance to a provider affects teen women who live in densely populated urban counties, since these counties are always observed to have providers. However, for women living outside of counties with a provider, there is a significant amount of between- and within-county variation in distance to a provider. For example, nearly 10 percent (34 percent) of women (counties) are more than 50 miles from the nearest provider. Among those counties that did not always have a provider, the within-county standard deviation in distance was 16 miles, while the average within-county range in distance over the sample period was 32 miles. Thus, the bulk of the observable variation in distance to the nearest abortion provider is over the 0–100 mile range among women living in nonurban counties.

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estimates, this would be likely to generate a spurious correlation. In any event, the results reported in Tables III through VII are not sensitive to the specification of this variable.

V. Results

A. Basic Specifications

Tables III and IV present the basic specification estimated separately for whites and nonwhites. All regressions in these and subsequent tables are estimated by weighted least squares, with weights equal to the number of teen women living in the county. All specifications control for county-level demographics and economic conditions, as well as year effects. Moving to the right in the table, the specifications add county dummies, and finally county dummies with separate year dummies for each state. Coefficients and standard errors are reported only for the variables directly related to abortion access. Note that the coefficients correspond to the absolute effect of a variable on the birthrate. The numbers in square brackets divide the coefficient estimates by the mean birthrate and correspond to the proportional effect of a variable on the birthrate.

The cross-section results for whites appear in the first two columns of Table III. Living far away from an abortion provider is associated with significantly higher teen birthrates. An increase of 25 miles in the distance to a provider is associated with an additional 1.4 births per 1000 teen women for whites. Relative to the average teen birthrate of 45 per thousand for whites, this is a 3 percent increase. Medicaid funding restrictions have a positive and significant effect on teen birthrates. In contrast, parental consent laws appear to be negatively associated with teen birthrates.

Of course, state laws and distance to a provider may proxy for omitted state and county factors that independently influence teen birthrates. Three facts suggest that these cross-section estimates suffer from considerable omitted variable bias and, therefore, should not be interpreted as causal. First, many of the estimated effects are implausibly large when compared with estimates that legalization of abortion led to a 5–10 percent reduction in births. In addition, the cross-section estimates are quite sensitive to small changes in the list of control variables. For example, dropping percent of the county in poverty from the regression changes each of the reported coefficients by at least 25 percent. Finally, counter to expectations, the interaction terms added in column 2 imply that Medicaid restrictions have smaller effects (less positive or even negative) in counties with a large fraction of the population in poverty, while parental consent laws

TABLE III
THE EFFECT OF ABORTION ACCESS ON TEEN MOTHERHOOD RATES
VARIOUS SPECIFICATIONS, 1973–1988
WHITE BIRTHS
Dependent Variable = (#Births to Mothers Age 15–19)/(#Women Age 15–19)

	Cross-section		County effects		County effects and state × year effects	
Miles to abortion provider (100s)	0.0056 (0.0004) [0.124]	0.0059 (0.0004) [0.131]	−0.0019 (0.0003) [−0.042]	−0.0019 (0.0003) [−0.042]	−0.0017 (0.0003) [−0.038]	−0.0017 (0.0003) [−0.038]
Medicaid abortion funding restricted	0.0021 (0.0002) [0.047]	0.0103 (0.0004) [0.229]	−0.0012 (0.0001) [−0.027]	−0.0005 (0.0002) [−0.011]	—	—
(Medicaid restriction) × (fraction in poverty)	—	−0.0856 (0.0040) [−1.90]	—	−0.0072 (0.0020) [−0.160]	—	−0.0117 (0.0020) [−0.260]
Parental consent required	−0.0045 (0.0003) [−0.100]	−0.0063 (0.0004) [−0.140]	−0.0014 (0.0002) [−0.31]	−0.0012 (0.0002) [−0.027]	—	—
Additional miles to provider with no parental consent > 50?	—	0.0033 (0.0006) [0.073]	—	−0.0005 (0.0003) [−0.11]	—	0.0006 (0.0003) [0.013]
Sample size	42518	42518	42518	42518	42504	42504
Mean of dependent variable	0.0450	0.0450	0.0450	0.0450	0.0450	0.0450

Standard errors are given in parentheses.
Coefficients divided by mean birthrate are given in brackets.
All regressions weight by the number of women age 15–19 in the county. All specifications include year dummies and county-level controls for the fraction of women 15–29 who are nonwhite, the log of per capita income, the log of employment, the percent of employment in construction, the percent of employment in manufacturing, and the percent of income derived from unemployment insurance. Specifications without county dummies also control for percent of county population in poverty as of the 1980 census (race-specific).

TABLE IV
THE EFFECT OF ABORTION ACCESS ON TEEN MOTHERHOOD RATES
VARIOUS SPECIFICATIONS, 1973–1988
NONWHITE BIRTHS
Dependent Variable = (#Births to Mothers Age 15–19)/(#Women Age 15–19)

	Cross-section		County effects		County effects and state × year effects	
Miles to abortion provider (100s)	−0.0004 (0.0008) [−0.004]	0.0011 (0.0008) [0.011]	−0.0022 (0.0010) [−0.022]	−0.0021 (0.0010) [−0.021]	0.0005 (0.0011) [0.005]	0.0008 (0.0011) [0.008]
Medicaid abortion funding restricted	0.0107 (0.0004) [0.106]	0.0342 (0.0011) [0.338]	−0.0000 (0.0004) [0]	0.0021 (0.0011) [0.021]	—	—
(Medicaid restriction) × (fraction in poverty)	—	−0.0828 (0.0036) [−0.820]	—	−0.0066 (0.0034) [−0.065]	—	0.0181 (0.0040) [0.179]
Parental consent required	−0.0027 (0.0007) [−0.027]	−0.0034 (0.0009) [−0.034]	0.0009 (0.0006) [0.009]	0.0003 (0.0009) [0.003]	—	—
Additional miles to provider with no parental consent > 50?	—	0.0010 (0.0012) [0.001]	—	0.0012 (0.0011) [0.012]	—	0.0013 (0.0011) [0.013]
Sample size	35336	35336	35336	35336	35322	35322
Mean of dependent variable	0.1011	0.1011	0.1011	0.1011	0.1014	0.1014

Standard errors are given in parentheses.
Coefficients divided by mean birthrate are given in brackets.
All regressions weight by the number of women age 15–19 in the county. All specifications include year dummies and county-level controls for the fraction of women 15–29 who are nonwhite, the log of per capital income, the log of employment, the percent of employment in construction, the percent of employment in manufacturing, and the percent of income derived from unemployment insurance. Specifications without county dummies also control for percent of county population in poverty as of the 1980 census (race-specific).

have smaller effects (less negative) in counties that are relatively far from a provider in an unrestricted state.

To the extent that omitted state and county factors are stable over time, the omitted variable bias can be eliminated by adding county fixed-effects. As the estimates in columns 3 and 4 demonstrate, removing the cross-section variation with county fixed-effects dramatically changes the estimates. The estimated effect of miles to a provider becomes negative and significant: a 25-mile increase in distance to a provider is associated with a reduction in teen births of 0.5 per thousand, or about 1 percent relative to the mean birthrate. The estimated effect of Medicaid restrictions becomes negative and significant, while the estimated effect of parental consent laws becomes less negative but remains significant. These results are consistent in sign and magnitude with those of other studies that have relied on within-state changes in Medicaid funding or parental consent rather than differences across states [Trussel et al. 1980; Levine, Trainor, and Zimmerman 1995; Matthews, Ribar, and Wilhelm 1995].

With county fixed-effects in the regression, all three proxies for abortion access suggest that reduced access to abortion is associated with a lower teen birthrate. Furthermore, in contrast to the cross-section results, the fixed-effect estimates show no obvious signs of being driven by omitted variable bias: the magnitudes of the estimated effects are plausibly small; the coefficient estimates are not sensitive to selection of control variables;[12] and the interaction terms added in column 4 imply that, as expected, Medicaid restrictions and parental consent laws have larger effects (more negative) in poor and more isolated counties, respectively.

The estimated coefficients for distance to the nearest provider may be biased toward zero due to measurement error in the distance variable. We have considered two methods of correcting for this bias. The first method assumes that any county observed to have a single-year spell without a provider actually had a provider in that year. The second method uses distance to the nearest county with a provider doing at least 25 abortions per year as an instrument for our distance measure. Both methods increase the absolute value of the distance coefficient by 25 percent while

12. Despite this fact, the control variables themselves are highly significant. The coefficients on the economic control variables imply that fertility is procyclical.

not affecting the other coefficients (results available from the authors).

Changes in abortion access may proxy for more fundamental changes in attitudes that are not captured by stable county fixed-effects or the list of economic and demographic control variables. One way of controlling for such factors is to include state-year effects, i.e., a separate set of time dummies for each state. This is done in columns 5–6 of Table III. In this specification the direct effects of state laws cannot be estimated since there is no way of untangling the effect of the state law from the underlying statewide change in attitudes. However, distance to a provider and interactions of the state laws with county characteristics remain identified. The estimated effect of distance is essentially unchanged by the inclusion of state-year effects. The Medicaid interaction term becomes more negative, while the parental consent interaction term goes from being negative to positive but in both cases is only marginally significant. Thus, the estimated effects of Medicaid restrictions and distance are robust to the inclusion of state effects, but the estimated effects of parental consent laws should be treated more cautiously.

The results for nonwhites differ from the results for whites in two important respects (see Table IV). First, the estimates are generally less precise. For example, none of the coefficients is more than marginally significant in the fixed-effect specifications (columns 3–4) despite point estimates similar to those of whites. A second important difference in the nonwhite results is that the point estimates are far less robust to changes in the specification. This is apparent from the dramatic impact state-year effects have on the estimates (columns 5–6). Similarly, large changes in the coefficients occur with slight changes in the list of control variables used in the regression. This lack of precision is perhaps unsurprising given the amount of systematic error involved in estimating both the numerator and denominator used to construct the nonwhite birthrates, as discussed in Section IV. Because of these problems, we focus on the results for whites in the remainder of the paper.[13]

13. Nonwhite results for the specifications in Tables V through VII are available from the authors. Many of the patterns observed for nonwhites in Table IV carry over to these other specifications. In particular, for nonwhites (1) distance effects are similar to those reported for whites, (2) state laws tend to have no consistent or large effects, and (3) most of the results are not robust to the inclusion of state-year effects.

TABLE V
THE EFFECT OF ABORTION ACCESS ON TEEN MOTHERHOOD RATES
IN-WEDLOCK AND OUT-OF-WEDLOCK, 1973–1988
REPORTING STATES ONLY
WHITE BIRTHS

	Teen birthrate		Out-of-wedlock teen birthrate		In-wedlock teen birthrate	
Miles to abortion provider (100s)	−0.0022 (0.0003) [−0.048]	−0.0018 (0.0004) [−0.040]	0.0007 (0.0002) [0.050]	0.0005 (0.0002) [0.036]	−0.0029 (0.0003) [−0.092]	−0.0023 (0.0003) [−0.073]
Medicaid abortion funding restricted	0.0009 (0.0003) [0.020]	—	−0.0004 (0.0001) [−0.029]	—	0.0014 (0.0002) [0.044]	—
(Medicaid restriction) × (fraction in poverty)	−0.0176 (0.0025) [−0.387]	−0.0085 (0.0026) [−0.187]	−0.0038 (0.0012) [−0.274]	−0.0049 (0.0012) [−0.353]	−0.0138 (0.0022) [−0.437]	−0.0036 (0.0023) [−0.114]
Parental consent required	−0.0012 (0.0002) [−0.026]	—	−0.0000 (0.0001) [0]	—	−0.0012 (0.0002) [−0.038]	—
Additional miles to provider with no parental consent > 50?	−0.0006 (0.0004) [−0.013]	0.0002 (0.0003) [0.004]	−0.0008 (0.0001) [−0.058]	−0.0002 (0.0002) [−0.014]	0.0002 (0.0003) [0.006]	0.0004 (0.0003) [0.013]
State × year effects?	No	Yes	No	Yes	No	Yes
Sample size	33291	33277	33291	33277	33291	33277
Mean of dependent variable	0.0455	0.0455	0.0139	0.0139	0.0316	0.0316

Standard errors are given in parentheses.
Coefficients divided by mean birthrate are given in brackets.
All regressions weight by the number of women in the county. All specifications include year and county dummies and county-level controls for the fraction of women 15–29 who are nonwhite, the log of per capita income, the log of employment, the percent of employment in construction, the percent of employment in manufacturing, and the percent of income derived from unemployment insurance.

B. Births by Marital Status

Table V differentiates between in-wedlock and out-of-wedlock teen births. Data for six states (CA, CT, MD, NV, NY, OH) are dropped from this analysis because these states did not directly report information on mother's marital status throughout the sample period. Selected years of data were dropped for another eight states (GA, ID, MA, MI, MT, NM, TX, VT) that did not report marital status information in at least one year during our sample period. All specifications in Table V are for white birthrates and include county fixed-effects. The denominator for both in-wedlock and out-of-wedlock birth rates is the total number of white women age 15–19.

The first two columns of Table V replicate earlier results for the overall teen birthrate using the subsample of data from states reporting marital status. The estimates are quite similar to those using the entire sample (Table III, columns 3 and 5). Corresponding estimates for out-of-wedlock and in-wedlock birthrates are noticeably different from each other in a way that is roughly consistent with the model discussed in Section II. Recall that the model predicted that restricted abortion access would reduce in-wedlock births, and perhaps increase out-of-wedlock births. This is in fact what is observed in Table V for distance to the nearest abortion provider. An increase of 25 miles to the nearest provider is associated with roughly a 2 percent decline in in-wedlock births and a 1 percent increase in out-of-wedlock teen births (relative to their respective means). Both estimates are robust to the inclusion of state-year effects. By implication, the fraction of births that are out-of-wedlock increases with distance, but the primary reason is a decline in the in-wedlock birthrate.

Not only are the distance coefficients from Table V consistent with theory, there is also no strong reason to think that endogeneity of distance should generate this pattern of results. If anything, one might expect the endogeneity bias to go the other way. For example, if conservative attitudes were responsible for higher distances to the nearest abortion provider, then one might expect these same attitudes to be associated with low out-of-wedlock and high in-wedlock birthrates. This type of omitted variable would bias the results against what is found in Table V.

The impacts of parental consent laws are only partially consistent with the theory. Theory predicts that these laws will be associated with lower in-wedlock birthrates and, perhaps, higher out-of-wedlock birthrates. As expected, the estimates in Table V imply that parental consent laws are associated with declines in in-wedlock births. However, these declines are not concentrated in more isolated counties, suggesting that these effects may not be due to the law per se. The effects of consent laws on out-of-wedlock births appears to be most negative in the more isolated counties, but the magnitude of the effect is sensitive to the inclusion of state-by-year effects. Again, this suggests that the consent laws may be proxying for omitted state-level variables such as changing sexual attitudes.

Similarly, the estimated impacts of Medicaid funding restrictions are only somewhat consistent with theory. Note that restricting Medicaid funding of abortion reduces the in-wedlock birth rate in our model because Medicaid provides poor women with insurance in case the father does not agree to legitimize an out-of-wedlock pregnancy. The estimates with no state-by-year effects in Table V imply that, for an average teen living in a county with 9.44 percent of the population in poverty, Medicaid restrictions are associated with a lower out-of-wedlock birthrate and no change in the in-wedlock birthrate. This is not what would be predicted by the simple model of Section II. Only for counties with higher rates of poverty are the estimated impacts on in-wedlock birth negative, and only for poverty rates above 18 percent are the estimated impacts larger for in-wedlock than for out-of-wedlock births.

C. Fertility at Other Ages

As a check on the results for teen births, we estimate identical models (with county fixed-effects) of white birthrates for three alternative age categories: Women age 15–17, 18–19, 20–29. The results are given in Table VI.[14]

A priori, one would expect smaller effects of abortion access on older women, since a much larger fraction of their pregnancies are conceived in wedlock [O'Connell and Rogers 1984]. Presumably, there is less uncertainty about the wantedness of an in-wedlock pregnancy so that abortion access has less influence on the pregnancy and birth decision. In the context of the model developed in Section II, an in-wedlock pregnancy would be very

14. Separate estimates are not available for the number of women age 15–17 and age 18–19 in a county, so these birthrates are calculated using the appropriate fraction (0.6 and 0.4, respectively) of women age 15–19 in the denominator.

TABLE VI
THE EFFECT OF ABORTION ACCESS ON MOTHERHOOD RATES,
VARIOUS AGE GROUPS, 1973–1988
WHITE BIRTHS
Dependent Variable = (#Births to Mothers of Given Age)/(#Women Given Age)

	Age 15–17		Age 18–19		Age 20–29	
Miles to abortion provider (100s)	−0.0023 (0.0003) [−0.089]	−0.0019 (0.0003) [−0.074]	−0.0013 (0.0006) [−0.018]	−0.0014 (0.0006) [−0.019]	−0.0024 (0.0004) [−0.022]	−0.0014 (0.0004) [−0.013]
Medicaid abortion funding restricted	−0.0010 (0.0002) [−0.039]	—	0.0002 (0.0004) [0.003]	—	0.0009 (0.0003) [0.008]	—
(Medicaid restriction) × (fraction in poverty)	−0.0065 (0.0017) [−0.253]	−0.0054 (0.0018) [−0.210]	−0.0083 (0.0037) [−0.112]	−0.0211 (0.0038) [−0.286]	−0.0050 (0.0028) [−0.045]	−0.0207 (0.0027) [−0.188]
Parental consent required	−0.0008 (0.0002) [−0.031]	—	−0.0018 (0.0004) [−0.024]	—	−0.0014 (0.0003) [−0.013]	—
Additional miles to provider with no parental consent > 50?	−0.0010 (0.0003) [−0.039]	0.0002 (0.0003) [0.008]	0.0003 (0.0006) [0.004]	0.0012 (0.0006) [0.016]	−0.0012 (0.0004) [−0.011]	−0.0009 (0.0004) [−0.008]
State × year effects?	No	Yes	No	Yes	No	Yes
Sample size	42518	42504	42518	42504	42518	42504
Mean of dependent variable	0.0257	0.0257	0.0739	0.0739	0.1099	0.1099

Standard errors are given in parentheses.

Coefficients divided by mean birthrate are given in brackets.

All regressions weight by the number of women in the county. All specifications include year and county dummies and county-level controls for the fraction of women 15–29 who are nonwhite, the log of per capita income, the log of employment, the percent of employment in construction, the percent of employment in manufacturing, and the percent of income derived from unemployment insurance.

likely to result in an in-wedlock birth (P near 1), and as a result the woman would become pregnant irrespective of abortion availability.

As expected, the largest effects of distance (relative to the mean birthrate) are estimated among the youngest women with the effects of distance being half the size for older women. An increase of 25 miles to the nearest abortion provider is associated with a 2.2 percent decline in births among women age 15–17, but only about a half percent decline in births among women 18–19 and 25–29. These estimates are little changed by the inclusion of state-year effects.

Note that the absolute size of the distance coefficient is largest for women 20–29, but these women also have much higher birthrates. As a result, the *relative* effect is smaller. Indeed, the relative effect provides a more appropriate comparison for testing the implications of the model. The fact that teen pregnancy rates are roughly half as high as pregnancy rates for older women [Ventura et al. 1995] will necessarily mean that absolute effects on birthrates will tend to be smaller. However, for a given rate of pregnancy, we expect larger effects for teens since a larger fraction of those pregnancies are conceived out of wedlock.

Once again, the pattern of estimates across different age groups is not what would be expected if distance to the nearest provider was endogenous. For example, one might expect the supply of abortion providers to depend positively on pregnancy rates, thus generating the observed positive relationship between birthrates and the availability of a provider. However, over half of the demand for abortions comes from women age 20–29 [Henshaw, Koonin, and Smith 1991], and abortion rates per 1000 women are about the same for women 15–19 and women 20–29 [Ventura et al. 1995]. Therefore, this type of endogeneity bias should be as much (if not more) of a problem in the regressions for older women. The fact that we find weaker effects among the older women suggests that the findings are not driven solely by the endogeneity of provider supply.

The estimated effects of a Medicaid restriction on women of different ages are only partially consistent with the predictions of the model. For the specifications without state-year effects, Medicaid restrictions are estimated to reduce birthrates for an average county with 9.44 percent in poverty by 7 percent among women age 15–17, 1 percent among women age 18–19, and have no effect on birthrates for older women. However, the negative

TABLE VII
THE EFFECT OF ABORTION ACCESS ON TEEN MOTHERHOOD RATES,
ALTERNATIVE SAMPLES AND SPECIFICATIONS,
WHITE BIRTHS
Dependent Variable = (#Births to Mothers Age 15–19)/(#Women Age 15–19)

	Log-odds	No abortion provider 1973–1988	Splines	Presence in county
Miles to abortion provider (100s)	−0.0256	−0.0047	0.0009	—
	(0.0065)	(0.0005)	(0.0006)	
	[−0.024]	[−0.086]	[0.020]	
Max (0, miles-50) (100s)	—	—	−0.0067	—
			(0.0013)	
			[−0.149]	
Max (0, miles-100) (100s)	—	—	0.0032	—
			(0.0016)	
			[0.071]	
Provider in county?	—	—	—	−0.0003
				(0.0002)
				[−0.007]

Medicaid abortion funding restricted	−0.0197	0.0003	−0.0005	−0.0005
	(0.0046)	(0.0004)	(0.0002)	(0.0002)
	[−0.019]	[0.005]	[−0.011]	[−0.011]
(Medicaid restriction) × (fraction in poverty)	0.0105	0.0011	−0.0076	−0.0071
	(0.0413)	(0.0026)	(0.0020)	(0.0020)
	[0.010]	[0.020]	[−0.168]	[−0.158]
Parental consent required	−0.0216	−0.0011	−0.0012	−0.0012
	(0.0046)	(0.0003)	(0.0002)	(0.0002)
	[−0.021]	[−0.020]	[−0.027]	[−0.027]
Additional miles to provider with no parental consent > 50?	−0.0222	0.0008	−0.0005	−0.0003
	(0.0064)	(0.0004)	(0.0003)	(0.0003)
	[−0.021]	[0.015]	[−0.011]	[−0.007]
Sample size	42518	31402	42518	42518
Mean of teen birthrate	0.0450	0.0547	0.0450	0.0450

Standard errors are given in parentheses.
Coefficients divided by mean birthrate are given in brackets.
All regressions weight by the number of women in the county. All specifications include year and county dummies and county-level controls for the fraction of women 15–29 who are nonwhite, the log of per capita income, the log of employment, the percent of employment in construction, the percent of employment in manufacturing, and the percent of income derived from unemployment insurance. The log-odds regression uses a Cox correction for birthrates of 0 or 1.

interaction of the Medicaid restriction with the percent in poverty is of roughly equal magnitude for all age groups particularly when state-year effects are included.

Parental consent laws should most affect women age 15–17, and may have some effect on 18 year olds who became pregnant when age 17. Both the parental consent dummy and the parental consent interaction term have coefficients that are significant and negative for older women, casting doubt on the validity of these estimates. The estimated effects are largest (most negative) relative to mean birthrates for women age 15–17, suggesting that these laws if anything are reducing birthrates of minors relative to older women.[15] However, as in Table III, results for teens are not robust to the inclusion of state-year effects. Overall, these results provide no strong evidence that parental consent laws influenced teen birthrates.

D. Alternative Specifications

Table VII presents estimates for alternative specifications and samples as a further check on the robustness of these results. County fixed-effects are included in all specifications. The first column of the table uses the log of the odds-ratio ($\ln(P/(1-P))$ of the birthrate as the dependent variable. The only notable change from the linear specification is that while Medicaid restrictions continue to have a negative impact on birthrates, the interaction between a Medicaid restriction and the percent of the county in poverty is no longer negative or significant. In the log-odds specification, the effect of each variable is approximately proportional to the mean birthrate in each county. Therefore, even without an interaction effect the implied impact of Medicaid funding restrictions are larger (in absolute terms) in high poverty counties because these counties have higher mean birthrates. In other words, it appears that Medicaid restrictions have larger absolute effects on teen birthrates in high poverty counties but not larger proportional effects on teen births.

The remaining columns of Table VII look more carefully at the relationship between teen birthrates and distance to the nearest provider. The second column restricts the sample to only those counties that never had an abortion provider over the sample period (1973–1988). Changes in distance for this sample

15. Although in absolute terms (as was the case with the distance coefficients) these coefficients are slightly larger for older women.

of counties is the result of provider location decisions in other, often far away, counties. Distant location decisions are less likely to depend on the fertility of these counties, and therefore, the distance to the closest provider is arguably more likely to be exogenous. Also, by eliminating those counties with providers, we eliminate most of the large urban counties, and the remaining counties may provide a more reasonable comparison group for the counties that experienced changes in distance to a provider. The impact of distance based on this subsample is even more negative and significant. In contrast, coefficients on the Medicaid and parental consent variables are quite different from the full sample estimates. It is possible that Medicaid and parental consent restrictions are less relevant in these counties with no local provider.

Columns 3–4 of Table VII try alternative specifications for distance to a provider. The first specification adds a linear spline in distance, with kinks at 50 and 100 miles. There is no significant effect of distance between 0 and 50 miles, but a large negative and significant effect beyond 50 miles that is somewhat less negative beyond 100 miles. (Note that the coefficients are cumulative, so that the impact of an additional mile under 50 miles is .0009, between 50 and 100 miles is .0009 − .0067 = −.0058, and beyond 100 miles is .0009 − .0067 + .0032 = −.0026.) It seems reasonable that only longer distances would pose a significant barrier to abortion access. For example, based on a survey of large providers, Henshaw [1991] reports that 33 percent of abortions are performed outside of the woman's county of residence, but only 9 percent of women receiving abortions travel more than 100 miles from their homes to reach the provider.

An alternative reason for estimating such small effects over the first 50 miles may be the fact that distance is set to zero for counties with a provider. Obviously, the average distance women must travel to reach a provider in their own county is greater than zero. This will tend to bias the effect of distance toward zero, since the variation in distance across counties with and without providers is overstated. When a dummy variable for counties with a provider is added to the regression, the effect of distance becomes more negative. The dummy for having a provider also has a negative coefficient, as would be expected if distance was underestimated for counties with providers.

Column 4 includes *only* a dummy variable for the presence of an abortion provider in the county. Most studies measure abortion access in this way, relying primarily on the availability in a woman's own county of residence (Shelton, Brann, and Schulz [1976] is the notable exception). In this specification we find that improved access (i.e., having a provider in the county) reduces birthrates, but the effect is both small in magnitude and insignificant. This is not surprising in light of the spline estimates. Counties that lose or gain a provider most often have access to another provider within 50 miles, and in this range we estimate no apparent effect of distance. Conversely, counties that never have a provider are more likely to experience meaningful changes (i.e., over 50 miles) in distance to a provider. Therefore, the insignificant effect for this specification appears to be mostly due to inadequately specifying the availability of abortion in nearby counties.

E. Timing of Effects on Birthrates

Perhaps the most likely source of bias in this analysis arises if changes in abortion access proxy for more fundamental unob-

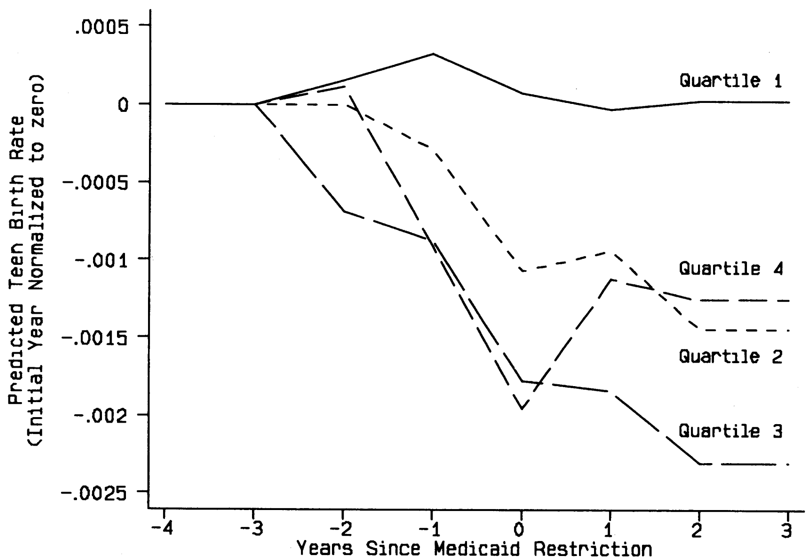

FIGURE IV

Lead and lag effects of Medicaid restrictions on teen birthrates.
By poverty quartile (highest poverty counties are in quartile 4).
Based on model from Table III, column 3, with two leads and two lags of Medicaid restriction dummy interacted with poverty quartile.

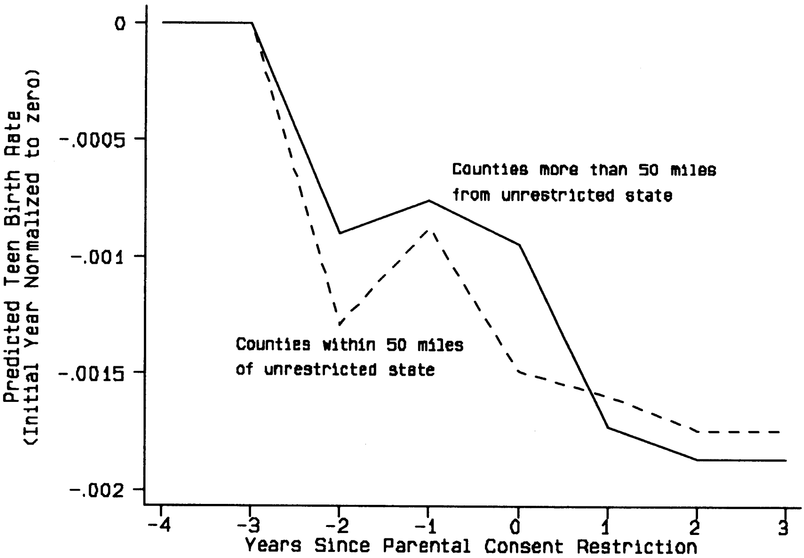

FIGURE V

Lead and lag effects of parental consent laws on teen birthrates.
By distance (> 50 miles or not) to nearest state without parental consent.
Based on model from Table III, column 3, with two leads and two lags of parental consent dummy interacted with dummy for being more than 50 miles from state with no consent law.

served changes in attitudes of the population that independently influence teen birthrates. The robustness of many of the results to state-year effects suggests that this may not be an important source of bias. As a further check, we examine the timing of the estimated effects of sudden changes in access: are large sudden changes in access to abortion closely associated in time with changes in teen birthrates? If the change in access is proxying for general trends, then one would expect to see a trend in birthrates in the years before and after the change in access with no apparent contemporaneous effect.

To examine the timing of the effect of state laws, we estimate models from our base specification (Table III, column 3) with two modifications. First, we replace the continuous Medicaid-poverty interaction with three dummy variables representing interactions between Medicaid restrictions and the top three poverty quartiles. These changes are made to facilitate the graphical display of the results, and do not materially affect the basic results. The second modification to the specification is to include two leads and two lags of all the state law variables. Thus, significant lead effects would suggest that the laws were not instrumental in changing the birthrate, but that the changes in laws were made in response to preexisting trends.

Since the regression coefficients are somewhat difficult to interpret, Figures IV and V graph the results. These figures plot the predicted teen birthrates from these regressions (normalized to be zero in the first year) against the number of years since the change in the law occurred (so that year = 0 represents the first year of the law). In Figure IV the Medicaid effects are plotted separately by poverty quartile, so that the line for quartile 1 represents predicted teen birthrates for counties with the lowest poverty rates. The most striking feature of Figure IV is that the lines for all quartiles appear to be trending down, with perhaps a slightly more rapid fall in teen birthrates in the year in which Medicaid restrictions began. The reason for the steeper decline in birthrates for poorer counties is apparently explained by the higher overall birthrates in these counties. (Recall that logit specification estimates suggested that the decline in birthrates associated with Medicaid restrictions was approximately proportional to the mean birthrate in the county.) Thus, Figure IV suggests that Medicaid restrictions may proxy for more fundamental trends in teen birthrates.

Figure V tells much the same story for parental consent laws. Birthrates are trending down in states that adopt parental consent laws (relative to other states) for both counties within 50 miles and counties farther than 50 miles from an unrestricted state. If anything, there is a slight negative effect in the two years after the law change, as evidenced by the steeper decline in the more isolated counties.

Doing a similar analysis for sudden changes in distance is complicated by the measurement error in distance, which tends to blur many changes, and also by the fact that most of the changes in distance would be expected to have quite small effects because the changes themselves are small or occur for distances below 50 miles. Therefore, we focus attention on counties with clear and dramatic changes in distance. More specifically, we consider 33 counties with one change in distance to the nearest provider of more than 50 miles and no other change during the sample period of more than 10 miles. Furthermore, the change in distance had to occur in the years 1975–1985, so that two years of data are available before and after the change. Of these counties, thirteen had increases in distance, and twenty had decreases.

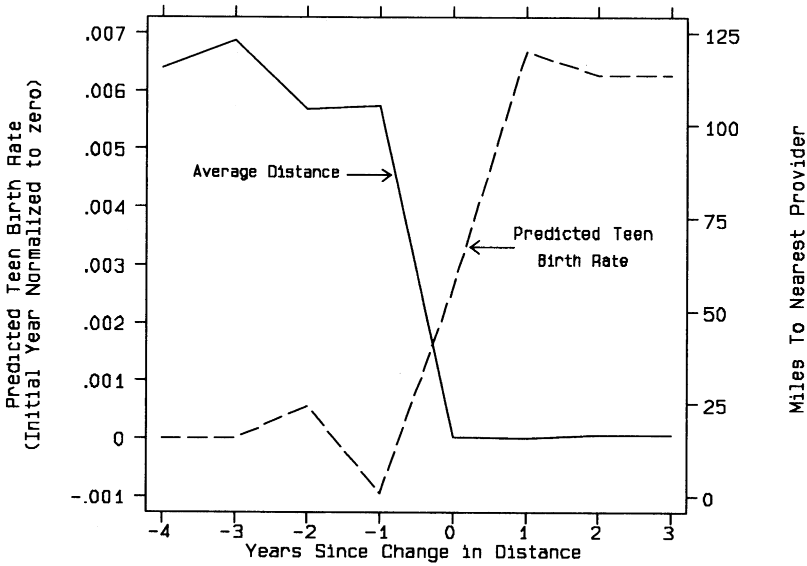

FIGURE VI

Lead and lag effects of decrease in distance to provider on teen birthrates.
Based on sample of twenty counties with single decrease of more than 50 miles.
Model includes year and county dummies, a dummy for years after the decrease, plus two leads and lags of the dummy for years after the decrease.

These changes occur in counties that are disproportionately small and rural, but otherwise are fairly well spread geographically and through time.

For this small group of counties that experience large one-time changes in abortion access, we estimate the basic teen birthrate model with a series of dummy variables that capture the time pattern of teen birthrates in a five-year window (two leads and two lags) around the change in distance. Figures VI and VII graph the results. These figures plot the average distance to a provider and the predicted teen birthrates against the number of years since the change in distance occurred (so that year = 0 represents the first year after the change). The graph for those counties experiencing a decrease in distance (Figure VI) is striking. There is no clear trend in teen birthrates prior to the decrease in distance, but teen birthrates increase dramatically in the year in which distance decreases and again in the following year. In other words, the increase in teen birthrates in these counties occurs exclusively in the two years immediately follow-

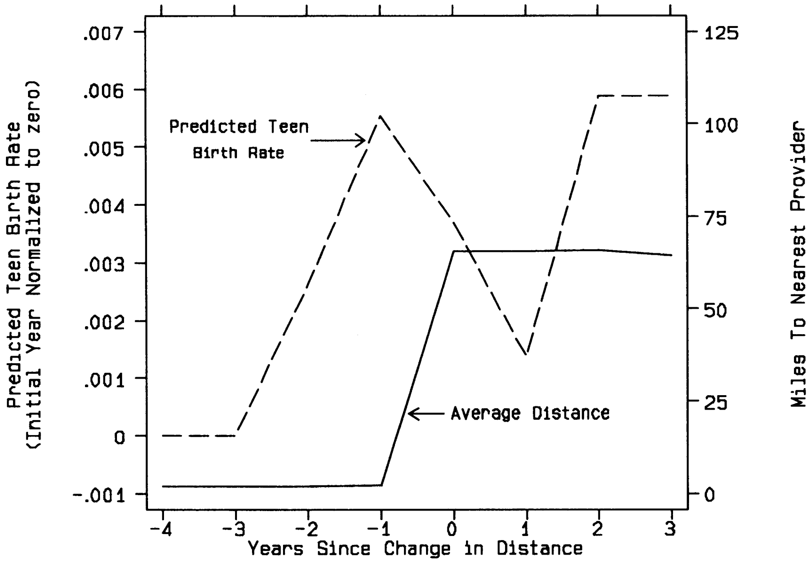

FIGURE VII

Lead and lag effects of increases in distance to provider on teen birthrates.
Based on sample of thirteen counties with single increase of more than 50 miles.
Model includes year and county dummies, a dummy for years after the increase plus two leads and lags of the dummy for years after the increase.

ing a change in distance to a provider. In spite of the small sample, these changes are statistically significant.[16] The results for the counties with an increase in distance are somewhat complicated by what appears to be a general upward trend in teen birthrates in these counties (see Figure VII). Nonetheless, there is a notable decline in teen birthrates that occurs in the two years immediately following the increase in distance to a provider. This result is not as significant as the corresponding result for decreased distance, but at least the two-year change in trend is marginally significant.[17]

16. The joint test that the two coefficients (in the year of change and the year following) are zero is rejected at the .04 level. Alternatively, the test that the two coefficients sum to zero is rejected at the .01 level. Another sensible test of whether the average estimated change in these two years is different from the average estimated change in the remaining three years rejects at the .07 level.

17. E.g., one can reject at the .14 level the hypothesis that the average change in teen birthrates in the two years immediately following the change in distance is equal to the average change in teen birthrates in the remaining three years (two leads and a two-year lag).

In combination, these two figures give the impression that the negative relationship between distance to the nearest abortion provider and teen birthrates, as estimated in Tables III through VII, is not spuriously generated by trends in teen birthrates at the county level. The magnitude of the effects depicted in Figures VI and VII, although imprecisely estimated, is of roughly twice the magnitude of the regression estimates.

VI. Conclusion

We have investigated how modest changes in abortion access have affected the fertility behavior of teen women. Despite a strong positive correlation between distance to an abortion provider and the teen birthrate in the cross-section, we find that increasing distance is associated with fewer teen births within a county. This effect appears for both whites and nonwhites, is largest for young teens, and is primarily occurring when distance to the nearest provider is beyond 50 miles. Furthermore, at least for whites, this effect is independent of state trends, and appears to be concentrated in the two years immediately following a change in distance. Finally, the effect is primarily concentrated on in-wedlock births. Out-of-wedlock births—the primary concern of policy makers—are relatively unaffected by changes in distance to a provider. Thus, the proportion of teen births that are out of wedlock increase with restrictions on abortion access.

Our second finding is that state laws restricting Medicaid funding of abortion and requiring parental consent for abortion have had no clear effect on teen birthrates. If anything, the evidence points to a negative effect of these laws on birthrates. However, it is difficult to distinguish the impact of these laws from general trends in teen birthrates already occurring in states that adopt such laws. These findings are consistent with recent findings by Levine, Trainor, and Zimmerman [1995b] and Matthews, Ribar, and Wilhelm [1995].

This evidence should not be taken to controvert earlier findings that the legal prohibition of abortion was associated with higher birthrates. We show that a simple model of fertility behavior can generate just this type of nonmonotonic effect: small reductions in abortion access could generate fewer births even if outright bans raised the birthrate. Some women may be willing to get pregnant but only bear the child if their partner agrees to marry. An increase in the cost of abortion would discourage some of the women in this group from getting pregnant, and as a result, in-wedlock births would decline. On the other hand, the same increase in cost may force some of the remaining women to bear unwanted children. To the extent that women give birth to unwanted children only when abortion is prohibited, this second effect can explain why outright bans on abortion lead to an increase in out-of-wedlock births. In short, small restrictions may lead to fewer wanted births while prohibition of abortion may lead to more unwanted births.

Our results cast considerable doubt on the concerns that recent restrictions in access to abortion are responsible for an increase in teen births. Our estimates suggest that, if anything, these restrictions have resulted in fewer teen births. Moreover, the magnitude of our estimates are small in relation to recent increases in the teen birthrate. For example, our estimates imply that the closing of abortion providers between 1977 and 1988 had an impact on teen birthrates of less than one-tenth of 1 percent. This is obviously small compared with the approximately 20 percent increase seen recently in teen births. Thus, the cause of the recent rise in the teen birthrate remains in question.

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