THE IMPACT OF STATE-LEVEL RESTRICTIONS ON ABORTION*

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This research examines 23 different laws passed by state governments in an effort to restrict the number of abortions. It assesses both laws passed and laws actually enforced after the Supreme Court permitted states to restrict access to abortion in 1989. None of the policy actions by state governments has had a significant impact on the incidence of abortion from 1982 to 1992. Abortion rates continue to reflect past abortion rates, the number of abortion providers, whether the state funds abortions for Medicaid-eligible women, urbanism, and racial composition of the population. Recent restrictive policies have not affected these trends.

In *Roe v. Wade* (1973), the U.S. Supreme Court held that state governments could not regulate abortions performed in the first trimester of pregnancy and could regulate but not prohibit abortions in the second trimester. Eventually states gained control over whether to fund abortions for Medicaid-eligible women (*Harris v. McRae* 1980; *Maher v. Roe* 1977; *Williams v. Zbaraz* 1980) and over certain aspects of parental involvement for minors' abortions (*Akron v. Akron Center for Reproductive Health, Inc.* 1983). Other restrictions, however, were struck down uniformly, until 1989, when the Supreme Court issued the *Webster* decision (McFarlane 1993).

Webster v. Reproductive Health Services (1989) is regarded as a pivotal case because the Supreme Court reversed its previous trend and upheld several state abortion restrictions. In his dissent, Justice Blackmun wrote that this decision "invites every state legislature to enact more and more restrictive abortion regulations" (Greenhouse 1989:11). In fact, states responded with a wide array of restrictions such as waiting periods, postviability requirements, and abortion-specific "informed consent" (Halva-Neubauer 1990). In addition to the post-*Webster* actions, a number of state restrictions already on the books could now be enforced.

In this study, we examine whether state restrictions both before and after the *Webster* decision actually reduced the incidence of induced abortion. Abortion restrictions, like other public policies, attempt to limit citizens' access to a good or a service (Blank, George and London 1994; Medoff 1988; Meier and McFarlane 1993). Two general approaches are commonly used. First, a policy can seek to alter a person's decision calculus by either increasing the costs of a good or service or decreasing the benefits and, in the process, reducing demand. Second, a policy can attempt to restrict the supply of the good or service that is available. Most of the state abortion policy restrictions follow one or both of these general approaches.

Public policy efforts to restrict abortions merit study for several reasons. In spite of the intense public conflict over the legitimacy of abortion, about one of every four pregnancies is ended by abortion, resulting in about 1.5 million abortions each year. This high level represents a major factor in the fertility of American women, especially unmarried women. Policy debates also swirl around unmarried child bearing; nonmarital births undoubtedly would be even higher if more unmarried pregnancies were not ended by abortion. Further, access to abortion is associated with better birth outcomes, declines in maternal mortality, and lower rates of teen pregnancy (Corman and Grossman 1985; Grossman and Jacobowitz 1981; Meier and McFarlane 1994). Abortion and fertility control also play a part in altering traditional sex roles in the United States (Luker 1984). Consequently, determining the effects of these restrictions is an important policy issue.

METHODS

The literature contains numerous models predicting state levels of abortion (Blank et al. 1994; Gohmann and Ohsfeldt 1993; Hansen 1980, 1993; Medoff 1988). Because virtually all of these models are cross-sectional, they must control for all other factors that affect the incidence of abortion to provide a precise estimate of the impact of legal restrictions.

These models risk the possibility of showing that states with low abortion rates are the states that adopt subsequent restrictions. As a result, spurious findings are possible because omitted variables could be correlated with the adoption of restrictions. Because individual states' abortion rates are fairly stable (Blank et al. 1994; Hansen 1993), an obvious solution is simply to incorporate the abortion rate for the previous year into the models. This strategy indirectly controls for all factors that affect past abortion rates and forces additional independent variables to explain changes from past rates rather than the overall levels of state abortion rates. Because the passage of a law is a new event, it should predict future changes in the level of abortions if it has any impact on abortions.[1]

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1. Wetstein (1995) employs a similar logic in his ARIMA model of national abortion rates. In that case he controls for past abortion rates by differencing the dependent variable.

Design

We use a pooled time series analysis of the abortion rates in the 50 U.S. states from 1982 to 1992 to determine the impact of state policies. First, we introduce a base model of abortion determinants in the 50 states to represent the factors influencing the level of abortions before the *Webster* decision. Second, we add to this model a series of measures that represent the states' efforts to restrict abortions. Third, because some restrictions were enjoined by courts and therefore were not implemented, we replicate the analysis using only those actions which in fact were enforced by the states.

State Abortion Rates

Our dependent variable is the number of abortions performed per 1,000 women between ages 15 and 44. We use the Alan Guttmacher Institute (AGI) data for all years except 1983, 1986, 1989, and 1990, when AGI did not provide state estimates (Henshaw and Van Vort 1988, 1990, 1994). For these years we used the Meier and McFarlane (1994) method of estimating abortion rates, which relies on comparing year-to-year data from the Centers for Disease Control and Prevention (CDC) with data from AGI. (This method is described in the appendix.) The estimation of abortion rates for these years has two benefits. First, it permits the use of sophisticated pooled time-series techniques; second, it allows a precise model to estimate the exact year when laws should have an impact. We use abortion rates by state of occurrence because they should be more sensitive to changes in law than rates by state of residence, simply because women can travel across state lines to obtain an abortion.

A Model of Abortion Determinants

Our initial model-building strategy was to identify variables that would affect either the demand for abortion or the supply of abortions provided. We then tested and evaluated each of these factors while controlling for the previous year's abortion rate, and found four significant determinants.

Two of these are essentially indicators of the demand for abortion: black population and the rate of abortions funded for Medicaid-eligible women. Other factors we considered, which did not significantly affect abortion rates, were unemployment, religion (percentage each of Catholics and Protestant fundamentalists), education levels, proportion of population between ages 18 and 45, income, female labor force participation, percent Hispanic, live birth rates, access to contraceptive technology, and marriage and divorce rates (Powell-Griner and Trent 1987). Blacks have a substantially higher rate of abortions than do nonblacks, so changes in the black population are likely to affect abortion rates (Henshaw, Koonin, and Smith 1991). Funding abortions for Medicaid-eligible women quite logically increases the number of abortions performed because it removes one barrier to demand, namely cost (Blank et al. 1994; Hansen 1993; Meier and McFarlane 1994). The measures are the percentage of black population in the state (taken from the U.S. Census) and the number of abortions funded per 1,000 women age 15–44 (see appendix; Gold and Daley 1991; Gold and Guardado 1988; Gold and Macias 1986; Gold and Nestor 1985).[2]

The other two factors are related to the supply of abortions: urbanism and the number of providers. Abortion rates are higher in urban areas simply because most providers are located in cities; 98% of abortions are performed in metropolitan areas (Henshaw and Van Vort 1990). The shorter the distance a pregnant woman has to travel to obtain an abortion, the more likely she is to have one (Shelton, Brann, and Shultz 1976). Similarly, abortion providers are not distributed uniformly in the United States. Where providers are lacking, women must either travel to another locality or carry the pregnancy to term. Although providers and urbanism are somewhat collinear, they are distinct enough to be used in the same model. The measures are the percentage of population living in urban areas (from the U.S. Census) and the number of abortion providers per 1 million population (Henshaw and Van Vort 1988, 1990, 1994).[3]

We also considered a wide variety of political factors that might create a political climate either more or less favorable to abortion. These factors were the percentage of female legislators, the percentage of Democratic legislators, measures of elected officials' positions on abortion, the presence of required sex education in schools, state spending on Aid to Families with Dependent Children, and public spending for family planning. None of these factors added any additional predictive power to the base model.

The base model is shown as Model 1 in Table 1. Several aspects of the model are noteworthy. First, the fit of the model (97.5% of total variation) is good. Second, the dominant factor in the model is the past abortion rate. The other factors are related to abortion rates in the direction predicted by past research; that is, abortion rates are related positively to black population, funded abortion rates, urban population, and the availability of providers. Third, pooled models are often susceptible to serious problems of autocorrelation and heteroskedasticity that can distort the results (Greene 1993; Maddala 1992; Stimson 1985). We tested for autocorrelation by estimating the amount of first-order and second-order serial correlation. We used the residuals for each state to estimate autocorrelation one state at a time, and then pooled these results. We assessed heteroskedasticity with the White test, using the entire pool. As shown by the diagnostics in the table, neither problem affects Model 1. The lack of autocorrelation and heteroskedasticity is largely a function of lagging the dependent variable. Thus, by building in a state's past abor-

2. The rate of abortions funded is correlated with public policy in regard to funding at .85. We used the funded rate in preference to announced policy because the former captures the nuances of policy, such as the limited amount of funds allocated in states such as North Carolina.

3. Abortion funding rates and abortion providers may not be exogenous. That is, the abortion restrictions also might affect these variables directly or indirectly. To determine whether the inclusion of these variables in the model influenced our results, we replicated Table 1 but omitted abortion funding and abortion providers. The results were identical to those presented here. These analyses are available from the authors on request.

TABLE 1. DETERMINANTS OF ABORTION, 1982–1992

	Model 1		Model 2		Model 3	
Independent Variable	Slope	*t*-Score	Slope	*t*-Score	Slope	*t*-Score
Lagged Abortion Rate	.940	74.06	.935	72.74	.940	73.95
Black Population	.018	2.23	.020	2.48	.018	2.23
Urban Population	.022	3.35	.024	3.66	.020	2.99
Abortion Providers	.037	3.42	.039	3.55	.039	3.50
Funded Abortion Rate	.230	1.94	.234	1.97	.249	2.07
Enforcement Statement	—	—	−1.102	1.79	—	—
Enforced Laws	—	—	—	—	.010	.28
Nonenforced Laws	—	—	—	—	.094	1.12
R^2	.975		.975		.975	
F	4202.96		3517.15		2999.31	
Autocorrelation						
First order	−.01		−.01		−.01	
Second order	−.04		−.04		−.05	
White Test						
(5, 6, and 7 df)	1.54		1.58		1.81	
Probability	.91		.95		.97	
N of Cases	550		550		550	

Note: Dependent variable: Abortions per 1,000 women age 15–44

tion rate, we control not only for unmeasured determinants of past rates but also for two serious statistical problems.

What the States Did

We identified 23 different policy actions either passed or enforced that might have influenced the number of abortions. Except for laws designed to prevent violent protests at clinics, all policies were intended to reduce the incidence of abortion or to restrict the activities of physicians performing abortions. By examining the Alan Guttmacher Institute's *Legislative Record*, we were able to determine the year when each law went into effect and whether it remained in effect. We counted only laws adopted after 1982. With a few exceptions (Medicaid funding, parental involvement), a law adopted after *Roe* but before *Webster* would not have been enforceable under existing law; if it had any other impact, that impact would be incorporated into our model through prior abortion rates.

To make sure this assumption did not affect our results, we replicated this analysis with all laws passed after *Roe* (Halva-Neubauer 1990) and obtained identical results. Except as noted below, each variable is coded 1 if the state had the law and 0 if it did not in a given year. (Changing the coding of all laws to dichotomous variables did not affect the results.) These laws are listed below. Numbers in parentheses refer to the number of states adopting the law after 1982 and the total number of states adopting the law, respectively.

1. *A conscience clause* allows physicians not to provide abortions if they are opposed to abortion (4, 36).
2. *Fetal experimentation* prohibits experiments using material from aborted fetuses and bans abortions for experimentation (6, 30).
3. *Postviability requirements* establish when viability occurs and regulate abortions after viability (11, 30).
4. *Postviability care* requires postabortion care for a viable fetus (2, 30).
5. *Anti-abortion memorials* request that the U.S. Congress ban abortion (2, 25).
6. *Feticide laws* outlaw the killing of a fetus; these are attempts to outlaw abortion through the criminal code (7, 10).
7. *Fetal disposal laws* regulate the disposal of aborted fetuses (4, 11).
8. *Abortion-specific "informed consent"* laws prohibit abortion without the pregnant woman's consent. Here "informed consent" requires that the pregnant woman be given an anti-abortion lecture and/or materials prepared or approved by the state, intended to discourage her from having an abortion. "Informed consent" has a different meaning for other medical procedures (11, 20).
9. *Parental consent* laws require that minors obtain parental consent or that they petition the courts before an abortion (coded 0 for no law, 1 for one parent, and 2 for two parents; 16, 22).
10. *Parental notification* laws require a minor's parents to be notified before an abortion is performed (coded as the number of hours before an abortion that notification must be given and 1 if no specific time period is designated; 13, 21).

11. *Spousal consent* laws require the husband's consent for an abortion (3, 8).
12. In a *no wrongful life* policy, the state has a statute prohibiting lawsuits for wrongful life should an abortion result in a live birth (8, 8).
13. *Private insurance* restrictions permit private health insurance companies to restrict access to abortions or not to cover them at all (8, 11).
14. *Public facilities* restrictions ban the use of public facilities in performing abortions (1, 4).
15. *Fetal pain* laws require the physician to inform the pregnant woman that the fetus feels pain and that anesthesia is available (1, 1).
16. *Gender selection* laws ban abortion for gender selection (2, 2).
17. *Physician-only* laws stipulate that only licensed physicians can perform abortions (6, 6).
18. *Reporting requirements* simply stipulate that all abortions must be reported to a state agency (20, 20).
19. *Clinic violence* laws prohibit violence or harassment of clinics and workers (9, 9).
20. *State insurance* laws ban state health insurance coverage of abortion for state employees (1, 1).
21. *Informed consent of minors* laws apply the informed consent law to minors rather than requiring parental consent (2, 2).
22. *Waiting periods* specify a period that must pass between the request for an abortion and the time when it can be performed (coded in number of hours; 6, 6).
23. *Right to life* laws make it illegal to kill a fetus in the womb unless certain conditions are met, and/or require that any fetus born alive during an abortion must be given a chance to live by the physician (12, 12).

Although not all of these laws place a burden on either the physician or the patient (e.g., 17, 18, 19), we opted to be inclusive in our analysis rather than assuming that some laws would have no impact. Because we tested each law separately, each law received its own opportunity to influence abortion results. As a result, the impact of a law designed to reduce abortions is not diluted statistically by the presence of other laws.

The Impact of These Laws

Our strategy of analysis was to enter each law into the base equation separately and to determine whether it was related significantly to state abortion rates. With 23 laws (variables) and an .05 level of significance, one would expect to find at least one significant relationship by chance alone; none were significant, however. We then entered all the variables into the base model simultaneously to determine whether they might have an impact jointly rather than as individual items. The joint F-test for significance was .85, with a probability of .67 (df = 23, 520). In short, none of the 23 policy actions either separately or together had any effect on the rate of abortions in the states that adopted these laws.

Not all of the laws passed by the states remained on the books; many were passed in an attempt to determine how far the Supreme Court would let states go in regulating abortion. Some laws were challenged immediately in court, and enforcement of the law was enjoined, pending the result of the court case. Laws that are passed but not enforced can still have an effect if an individual does not know about the injunction or the lack of enforcement. Even so, perhaps a more appropriate test would be to assess only those laws and policies which were not enjoined from enforcement. This does not mean that the laws were actually enforced (e.g., that consent from both biological parents was obtained before a minor underwent an abortion procedure), but only that there were no legal barriers to enforcement.

The results of assessing only the laws that were not enjoined were the same as those for all laws. Not a single policy action had a significant negative impact on the abortion rate. One new enforcement-related item came close, however. If the state passed a law saying that it would enforce its abortion laws (that is, any of the other 23 laws which it might have passed), that action had negative impact with a probability of .07, barely missing our criterion for statistical significance (see Model 2). The model suggests that the adoption of an enforcement law results in a decrease of 1.1 abortions per 1,000 women, all other things being equal. Given the large number of variables tested, the minuscule improvement in prediction, and the failure to meet the .05 level of significance in this case, we are skeptical about this finding. Such a pattern could occur easily by chance at least once in examining almost 50 possible relationships; in fact, it would occur twice by chance alone. We also tested all of the nonenjoined laws jointly and found that as a group they also did not affect abortion rates (F-test = .65, probability = .90).

One last possibility exists: that the impact of these laws occurs at the margins and that each of these impacts is rather small. A series of small impacts, however, could have a significant cumulative effect. In other words, what is important is not that a state imposes a specific restriction on abortion but the total number of restrictions that it imposes. Any one restriction might not dissuade a woman from obtaining an abortion, but several at once might do so. To test this possibility, we summed the number of laws passed and not enjoined by each state. The total ranged from 0 to 13. As another, symbolic test, we also summed the laws passed but enjoined. The results are presented as Model 3. Not only is the impact of the restrictions statistically insignificant, but also in both cases the relationship is positive rather than negative. We find no evidence that state restrictions on abortions reduced the overall incidence of abortion.

Parental Involvement

Our findings stand in contrast to other reports that find an impact for some laws (notably parental involvement; see Cartoof and Klerman 1986; Ohsfeldt and Gohmann 1994; Rogers et al. 1991), but are similar to Wetstein's (1995) national-level results. These studies examined abortion rates for specific age groups (only for single states in Cartoof and Klerman and in Rogers et al.). Only Cartoof and Klerman were able to control for past abortion rates (but not for other factors such as providers, etc.). Because our study used an elaborate pooled design for all 50 states and controlled for the previous year's abortion rates, we believe that our test is more rigorous and our findings are more representative of the impact of abortion restrictions on the incidence of abortion.

To investigate further the question of parental involvement, we conducted several additional analyses. In our data set, parental notification is correlated negatively with abortion rates (parental consent is uncorrelated with abortion rates). This negative correlation disappears when previous abortion rates are entered into the equation. We were able to find one ordinary least squares model that showed an impact for parental notification when the only other variables in the model were providers, percent urban, percent black, and the funded abortion rate. This model, however, displayed severe autocorrelation problems (Rho = .82) which can cause nonsignificant relationships to appear significant. When we corrected the autocorrelation problem with an error components analysis (see Greene 1993:chap. 16), parental notification declined to statistical insignificance. Similarly when we conducted a least squares dummy variables model that specified a fixed intercept for each state, parental notification was statistically insignificant. Finally, a simple first-order autoregressive model with a correction for heteroskedasticity also resulted in an insignificant effect for parental notification. We conclude that even in a favorable model (that is, a model with few controls and without a lagged dependent variable), when appropriate procedures are conducted to correct for problems inherent in pooled time series analysis, parental notification had no impact on overall state abortion rates.[4]

CONCLUSION

By allowing certain abortion restrictions to stand, the *Webster* decision encouraged states to enforce and enact more such restrictions. This 11-year analysis of 23 separate policy actions found no evidence that these policies, either individually or aggregated, had an appreciable impact on states' abortion rates.

Although our findings are important, they do not refute the possibility that state policies can affect the rate of induced abortions. Indeed, our base model includes a state restriction that reduces a state's abortion rate: whether a state funded abortions for poor women. The measure used in our model is the funded abortion *rate* (the number of funded abortions per women age 15–44), a direct *outcome* of a state's funding policy. Each abortion funded by the state resulted in an increase of .23 abortion. (The relationship is not 1.0 because many abortions are paid for with private funds if public funds are not available.) As noted earlier, state funding restrictions were found to be constitutional a decade before the *Webster* decision.

Also important is the relationship between the number of providers and the abortion rate. Our model underscores Henshaw and Van Vort's (1994) suggestion that the national decline in abortion rates from 1989 to 1992 may be due to a decline in the number of abortion providers, particularly hospitals.

Other possible effects from post-*Webster* restrictions may not be captured by our outcome measure, state abortion rates. Even though state restrictions may not affect the annual rates, these restrictions may delay abortions for some women (see Rogers et al. 1991). Although legal abortion is a safe procedure, the earlier an abortion is performed, the safer it is (Gold 1990). Women who delay their procedures face higher risks of morbidity and mortality.

Another possible effect of state abortion restrictions is that women living in states with restrictions would travel to states with more permissive abortion laws (Gold 1990). Our dependent variable (state abortion rates by state of occurrence), however, should be more sensitive to this response to changes in state laws than should abortion rates by state of residence. Yet because our analysis shows no effect of the restrictions with this more sensitive measure, we doubt whether these restrictions altered existing patterns of travel.

In sum, the post-*Webster* restrictions did not have the immediate effects that pro-choice advocates feared and pro-life supporters anticipated. As Justice Blackmun predicted, *Webster* encouraged states that were so inclined to enact restrictions that would serve as test cases. Since 1989 the Supreme Court has upheld further state restrictions: *Hodgson v. Minnesota* (1991), *Ohio v. Akron Center for Reproductive Health* (1991), and *Planned Parenthood of Southeastern Pennsylvania v. Casey* (1992). The last of these cases upheld four additional state restrictions, including a 24-hour waiting period. This restriction has been enforced and enacted in many states, and early anecdotal or case analysis suggests that it has an effect (Althaus and Henshaw 1994). The anecdotal evidence of the effect of waiting periods should be explored more systematically when state abortion data for 1993 and 1994 become available.

APPENDIX. ESTIMATING STATE ABORTION RATES

We measured state abortion levels by the abortion rates (the number of abortions per 1,000 women age 15–44). Except for 1983, 1986, 1989, and 1990, all data were obtained from the Alan Guttmacher Institute (AGI). For years when AGI did not report the number of abortions by state, we estimated these data using a procedure developed by Meier and McFarlane (1994).

First we calculated the ratio of abortions reported to CDC in 1983 to those reported in 1982. In this step we recognized that CDC data were biased because of underreporting (Jones and Forrest 1992), but we assumed that the bias was consistent from year to year. We interpreted this ratio as the annual rate of growth in abortions in each state. Next we multiplied the 1982 AGI abortion data by this growth ratio in order to estimate the number of abortions in each state for 1983. These abortion estimates were converted into rates. We then verified these rates by comparing them with future AGI data, making any corrections necessary based on a set of specific decision rules. Using 1985 AGI abortion data, we employed the same procedure to calculate rates for 1986. Similarly, we

4. The decreases found by other researchers in single state studies may well be balanced out by increases in other age groups. The number of abortions performed on minors is a relatively small proportion of the total.

used AGI data for 1988 as well as 1991 and 1992 to estimate 1989 and 1990 rates. Documentation and specific estimates are available from the authors.

Neither the funded abortion rate nor the number of abortion providers is available for every year. Values for the missing years were extrapolated from existing data. All data used in this analysis can be obtained from the senior author or from the Florida State University Public Policy Data Archive (e-mail: cbarrile@garnet.acns.fsu.edu).

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