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The effect of abortion restrictions on the timing of abortions

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Abstract

This paper uses data on the distribution of abortions by weeks of gestation to examine the relationship between abortion restrictions and the timing of abortions. State-level data from 1974 to 1997 indicate that adoption of parental involvement laws for minors or enforcement of mandatory waiting periods is positively associated with the post-first trimester percentage of abortions. However, autocorrelation-corrected specifications indicate that enforced parental involvement laws increase the share of later-term abortions by lowering the first trimester abortion rate rather than by delaying abortions. Medicaid funding restrictions generally do not have a significant effect on the timing of abortions in our results. © 2001 Elsevier Science B.V. All rights reserved.

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1. Introduction

Since the 1973 Supreme Court decision in *Roe v. Wade* made abortion legal across the US, a number of federal and state laws have restricted women's access to abortion, particularly for low-income women and minors. The federal government and many states have stopped funding most abortions for Medicaid recipients, and some states have begun requiring minors to notify their parents or to obtain parental consent before having an abortion. Several states have also imposed mandatory waiting periods before women may obtain abortions. Such laws may lower the number of abortions in a state by making it more difficult for women to obtain abortions, and they may also have more subtle effects, such as delaying the timing of abortions until later in the pregnancy.

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This study examines whether Medicaid funding restrictions, parental notification or consent laws, and mandatory waiting periods affect the timing of abortions by weeks of gestation. Medicaid restrictions may delay abortions among low-income women who need additional time to gather funds together in the absence of Medicaid coverage, and parental involvement laws may delay or prevent abortions among teens reluctant to inform their parents. Although these laws directly affect only Medicaid recipients and minors, they may impact the timing of abortions among other women if the restrictions result in fewer abortion providers in an area. Mandatory waiting periods may delay abortions by requiring women to make more than one visit to a provider. Adoption of a restriction may also lead to confusion or other information problems that lead to delay. Some women may not be aware of the restrictions until they attempt to get an abortion and are then delayed; these women might have adapted their behavior and sought an abortion earlier if they knew about the restrictions.¹

Later abortions are of concern to women, health practitioners, and policymakers for several reasons. The risk of death or major complications is at least twice as high for a post-first trimester abortion as for a first trimester abortion (Atrash et al., 1990). In addition, the likelihood of major complications is at least three times higher for second trimester abortions than for abortions performed at 8 weeks of gestation or earlier (Tietze and Henshaw, 1986). Fewer than one-half of abortion facilities surveyed by the Alan Guttmacher Institute in 1993 offered services at 13 weeks, with the proportion declining rapidly at higher weeks of gestation (Henshaw, 1995a). The average clinic charge for a first trimester abortion is about one-half the average charge for an abortion at 16 weeks (Henshaw, 1982, 1995a). Women are also more likely to experience a negative emotional reaction to a second trimester abortion than to a first trimester abortion (Council on Scientific Affairs, 1992).

We use annual state-level data to examine the association between the timing of abortions, the abortion rate, and the presence of abortion restrictions in a state during 1974–1997. Because we find evidence of autocorrelation within states, we present both ordinary least squares (OLS) and autocorrelation-corrected results. Both specifications indicate that enforced and enjoined parental involvement laws are positively associated with the percentage of abortions occurring after the first trimester. In general, specifications correcting for autocorrelation show smaller direct effects of abortion restrictions than do the OLS specifications. Specifications correcting for autocorrelation within states indicate that enforced parental involvement laws lower the overall abortion rate, suggesting that the laws discourage some women from having abortions but do not delay abortions, whereas OLS results suggest that the laws affect the timing of abortions. Enforced waiting periods, in contrast, do not affect the total abortion rate in either set of results. Medicaid funding restrictions have little effect on the number or timing of abortions when correcting for autocorrelation, although some of the OLS specifications suggest that Medicaid funding restrictions decrease the percentage of post-first trimester abortions.

We briefly discuss the history of abortion restrictions and the previous literature on the effect of these restrictions on abortion rates in Section 2. Section 3 discusses our estimation

¹ Utility maximizing models with non-exponential discounting also imply that some women might wait longer if new restrictions are viewed as raising the cost of getting an early abortion relative to getting a later one. In some models, people avoid making a decision longer when the costs of an early decision are higher (Laibson, 1998).

strategy. Section 4 discusses the data on abortions, abortion restrictions and other controls. The effect of abortion restrictions on the timing of abortions and the abortion rate is discussed in Section 5. The robustness of our results is examined in Section 6, and Section 7 concludes the study.

2. Background

Rather than settling the issue of abortion availability, *Roe v. Wade* spawned numerous federal and state laws regulating abortion access and court injunctions concerning those laws. Some of the most contentious issues involve insurance coverage of abortions for Medicaid recipients, parental notification or consent requirements for minors and mandatory waiting periods for women seeking abortions.

The federal government and many states restrict public funding of most abortions under Medicaid, the public health insurance program for low-income families. Congressional legislation in 1976 cut off federal Medicaid funds for almost all abortions, but a court injunction delayed implementation of the law until August 1977. The federal law was again enjoined for 7 months in 1980 but has been continuously in effect since September 1980.² States can use their own funds to pay for abortions under the Medicaid program, but few have opted to do so. Only 17 states and the District of Columbia funded most abortions under the Medicaid program in 1997, for example, with some required to do so by court order.

Parental notification or consent laws are another restriction commonly adopted by states. Such laws require that a woman under the age of 18 years notify her parents or obtain their consent before an abortion can be performed. The case law on parental involvement laws is complex, but courts have generally upheld laws that incorporate a judicial bypass mechanism, which allows a minor to petition a court for permission to have an abortion instead of involving her parents.³ In 1997, 27 states enforced parental involvement laws for at least part of the year. In addition, courts have enjoined a substantial number of state parental involvement laws.

Some states have also adopted mandatory waiting periods. Such laws typically require women to receive information about abortion procedures and alternatives to abortion and then wait a certain number of hours before the procedure can be performed. In 1992, Mississippi became the first state to enforce such a policy; mandatory delay laws were in effect in 10 states in 1997 and were enjoined in five states for at least part of the year.

Several studies have examined the effect of abortion restrictions on abortion rates. Blank et al. (1996), Haas-Wilson (1993), and Levine et al. (1996) find that the abortion rate in a state, defined as the number of abortions per 1000 women aged between 15 and 44 years, is negatively associated with the presence of a Medicaid funding restriction. Cook et al. (1999) conclude that abortion rates fell when North Carolina did not provide public

² Federal funding has continuously covered abortions necessary to save the life of the woman. Federal funding has also covered abortions for pregnancies resulting from rape or incest since October 1993. However, some states have not extended Medicaid coverage to abortions for pregnancies resulting from rape and incest.

³ Merz et al. (1995) provide a detailed history of court decisions on state parental involvement laws and Medicaid funding restrictions.

funding for all abortions eligible for Medicaid funding. Matthews et al. (1997) report a negative association between the abortion rate among state residents and Medicaid funding restrictions and parental involvement laws in some specifications; however, they find that the estimated relationships are not significant when state-specific time trends are included. Blank et al. (1996) report that parental involvement laws do not appear to reduce abortion rates among women aged between 15 and 44 years, whereas Haas-Wilson (1996) finds that the abortion rate among minors is lower in states that enforce parental involvement laws.

A few studies have examined the effect of restrictions on the timing of abortions. Enforcement of parental involvement laws in Mississippi and Minnesota led to an increase in the fraction of abortions to minors occurring after the first trimester relative to the fraction among older women (Henshaw, 1995b; Rogers et al., 1991).⁴ Adoption of a mandatory delay law in Mississippi in 1992 also appears to have increased the fraction of abortions occurring after 12 weeks of gestation (Althaus and Henshaw, 1994; Joyce et al., 1997; Joyce and Kaestner, 2000, 2001). Average gestation at abortion is higher among Medicaid-eligible women than among non-eligible women in states with Medicaid restrictions, whereas gestations are similar for the two groups in states without restrictions (Henshaw and Wallisch, 1984; Trussell et al., 1980). Henshaw and Wallisch (1984) estimate that 22% of Medicaid-eligible women who had second trimester abortions would have had first trimester abortions if the lack of public funds had not resulted in delay as women tried to raise funds.

Our paper adds to this literature a comprehensive examination of the effect on the timing of abortions of the three main types of abortion restrictions. Previous studies have examined the effect on abortion timing when one or a few states adopted an abortion restriction. Our findings, based on panel data over a period of up to 24 years from 48 states and the District of Columbia, indicate whether the findings of these earlier studies hold across most of the country. We also examine the effect of both enforced and enjoined Medicaid funding restrictions, parental involvement laws and mandatory delay laws, whereas most previous studies focused on a single enforced restriction. As discussed below, many states have adopted more than one type of restriction and have had restrictions enjoined at some point.⁵ In addition, we examine the relationship between abortion restrictions and the abortion rate in order to conclude whether restrictions change the timing of abortions, the number, or both. This study also investigates the robustness of the results to assumptions about identification.

⁴ However, Rogers et al. (1991) emphasize that the late abortion rate among women aged 15–17 years fell after Minnesota's parental notification law went into effect; the decline was smaller than the decline in the first trimester abortion rate among minors, causing the fraction of abortions after the first trimester to increase.

⁵ For example, the Mississippi mandatory delay law was enjoined for a year before going into effect in August 1992. Althaus and Henshaw (1994), Joyce et al. (1997), and Joyce and Kaestner (2000, 2001) include the period when the law was enjoined in the "before" period when comparing abortion rates before and after the law went into effect, but the injunction may have affected the behavior of women and/or providers. In addition, a parental consent law that had been enjoined went into effect in Mississippi in 1993. Joyce and Kaestner (2001) conclude that enforcement of Mississippi's parental consent law led to later abortions among minors but do not examine whether the law had an effect while it was enjoined. Althaus and Henshaw (1994), and Joyce et al. (1997) include minors in their samples without addressing the possible effects of the parental consent law.

3. Estimation methodology

In a simple rational choice model, women's decision whether and when to have an abortion involves several steps. First, women decide whether to have sex. Conditional on having chosen to have sex, women choose contraceptive behavior, and if they become pregnant, women choose whether to abort the pregnancy or carry it to term. Changes in laws can affect women differently at various stages in the fertility decision process. Women who are already pregnant when a restriction is adopted can only adjust their abortion/birth decision. The passage of a law might cause these women to delay the procedure (because the cost of an abortion is now higher) or to instead carry the pregnancy to term. Similarly, if a restriction is enjoined, some pregnant women might have an abortion instead of giving birth, or women who have already decided to abort might do so earlier in the pregnancy. Women who are not yet pregnant when an abortion restriction is adopted have more dimensions on which to adjust their behavior than women who are already pregnant. They can change their level of sexual activity or contraceptive behavior as well as their abortion/birth decision (Kane and Staiger, 1996).

We rely on a simple underlying assumption common to this literature, namely that laws restricting access to abortion increase the cost of obtaining an abortion, leading to fewer abortions and more births or to changes in the timing of abortions or both.⁶ Some of these laws should only increase the cost for certain sub-populations. For example, laws restricting minors' access to abortions should only affect minors. However, parental involvement laws may affect fertility among older women as well if the laws lead to reduced access to abortion providers for all women. We regress various measures of the number of abortions and when they occur on measures of abortion restrictions to estimate the effect of restrictions among all women aged between 15 and 44 years.

We estimate the relationship between measures of abortions and abortion restrictions using regressions of the form:

$$A_{st} = P_{st}\beta_1 + X_{st}\beta_2 + \delta_s + \omega_t + \varepsilon_{st} \quad (1)$$

or

$$A_{st} = P_{st}\beta_1 + X_{st}\beta_2 + \delta_s + \gamma_s^* \text{trend} + \omega_t + \varepsilon_{st} \quad (2)$$

where 's' indexes states and 't' indexes years. The dependent variable A is one of three measures of abortions: the percentage of abortions after the first trimester, the post-first trimester abortion rate, or the overall abortion rate. The percentage of abortions after the first trimester variable is in levels, and the abortion rate variables are in logs.⁷ The vector P includes the measures of state abortion restrictions. The vector X includes other variables that may be determinants of abortion rates or the timing of abortions, such as demographic characteristics of women in the state, economic conditions in the state, and political climate of the state. Observations are weighted using the population of women aged between 15 and 44 years in each state and year. Some specifications correct for autocorrelation using

⁶ See Klerman (1998) for a longer theoretical discussion of the effect of abortion restrictions on fertility.

⁷ In the regression analysis, states that report zero post-first trimester abortions are assigned rates of 0.1 per thousand women. Results available from the authors show that the results are not sensitive to the treatment of zero.

the Prais–Winsten method as outlined in Bhargava et al. (1982) because tests overwhelming rejected the hypothesis of no autocorrelation, as discussed below, while other specifications report OLS estimates not corrected for autocorrelation.⁸

Using state-level panel data and a fixed effects methodology has several advantages. The state fixed effects δ capture unobservable differences that are constant over time across states. Regressions with state fixed effects measure the relationship between the dependent variable and the covariates within a state rather than across states. The year fixed effects ω capture time-varying factors common to all states in a given year, such as the national business cycle. In effect, the regressions measure the relationship between the abortion variable and the presence of an abortion restriction in that state and year. Some specifications, as represented by Eq. (2), also include state-specific linear time trends.

However, the fixed effects approach also has disadvantages. The use of fixed effects can increase the bias associated with measurement error in the right-hand-side variables in a panel data setting (Hsiao, 1986). In addition, as pointed out by Matthews et al. (1997), this identification strategy makes it difficult to identify the effects of slowly changing variables. This problem is exacerbated in regressions that include state-specific time trends.

4. Data

The data used in this paper are described in detail below because some of the variables we use differ slightly from previous research. We first summarize the variables used to measure the timing and number of abortions and the presence of abortion restrictions. We then briefly discuss the other controls included on the right-hand-side of Eqs. (1) and (2). Complete data are available for all of the variables used in our analysis for 855 observations from 1974 to 1997.⁹

4.1. Measures of abortions and abortion restrictions

The number of legal abortions in the US rose steadily during the 1970s before leveling off at slightly more than 1 million per year in the 1980s and 1990s. The Centers for Disease Control (CDC) publishes annual data on the number of abortions and the distribution by weeks of gestation. The data are based on reports from state public health agencies, but not all states report weeks of gestation data every year. In addition, the data are incomplete in states

⁸ We correct for autocorrelation within states by assuming that the autocorrelation is an AR (1) process because this is the simplest form of correction and the one most commonly used when there is no strong prior about the data-generating process. The Prais–Winsten method involves first estimating a weighted least squares regression, where the weights are the female population aged between 15 and 44 years in a state, estimating the autocorrelation parameter for an AR (1) process, transforming the data using the estimated autocorrelation parameter, and then re-estimating the regression using the transformed data. The standard errors are White-corrected for heteroscedasticity, which is also present in the data. Donohue and Levitt (2001) use a similar method. The standard errors in the OLS regressions are White-corrected.

⁹ Like most other researchers, we omit 1973 because of concerns about the quality of abortion data reported for that year and the reliability of information about states' implementation of *Roe v. Wade*. A list of the state and year pairs included in the data is available from the authors.

in which not all providers report to the public health agency. Blank et al. (1996) find that the CDC data consistently include fewer abortions than data from the Alan Guttmacher Institute (AGI), the other primary source of state-level abortion data. Despite these drawbacks, the CDC is the only source of information on the distribution of abortions by weeks of gestation for a large sample of states.¹⁰ Furthermore, the correlation between the two measures of the number of abortions is above 0.98. We focus on abortions occurring after 12 weeks of gestation (abortions occurring after the first trimester).

We use several measures to examine the impact of abortion restrictions on the timing and number of abortions. First, we investigate the relationship between restrictions and the percentage of abortions occurring after the first trimester. Even if abortion restrictions have no effect on whether women have abortions, they could still affect the timing of the abortions by making it more costly for women to receive abortions. This would imply that abortion restrictions lead to an increase in the percentage of later abortions.

If restrictions also affect the pool of women obtaining abortions, then the expected effect of restrictions on timing is more ambiguous. If some women who would have had a relatively late abortion in the absence of abortion restrictions instead give birth or do not become pregnant in the presence of abortion restrictions whereas the behavior of women who have relatively early abortions is unaffected by restrictions, the percentage of abortions occurring after the first trimester may fall when a restriction is imposed.¹¹ On the other hand, the percentage of post-first trimester abortions could rise even if the rate of such later abortions is unchanged. If the imposition of these restrictions only reduces abortions among women who would otherwise get first trimester abortions, the percentage of post-first trimester abortions could be positively associated with abortion restrictions without any increase in the post-first trimester abortion rate. We therefore also examine the relationship between restrictions and the number of abortions after the first trimester per 1000 women of childbearing age (between 15 and 44 years) in a state. We also report the association between abortion restrictions and the overall abortion rate.

A finding that abortion restrictions reduce the overall abortion rate could imply that these restrictions cause some women to give birth instead of having abortions, conditional on their having become pregnant. Alternatively, it would be consistent with the possibility that fewer women become pregnant because of the restrictions. Either could cause the percentage of post-first trimester abortions to be positively related to the imposition of restrictions without the rate of post-first trimester abortions increasing. If, however, there is no relationship between abortion restrictions and the overall abortion rate, then a finding that the percentage of post-first trimester abortions increases when restrictions are imposed implies that restrictions are associated with an increase in the rate of post-first trimester abortions, which has implications for women's health and finances.

¹⁰ The number of states reporting weeks of gestation data to the CDC ranges from a low of 29 in 1974 to a high of 40 in 1993. Delaware and Florida do not have CDC data on gestation. The AGI data on the number of abortions are not available for 7 of the 24 years for all of the states.

¹¹ Studies of the relationship between abortion restrictions and birth rates report mixed effects (Levine et al., 1996; Matthews et al., 1997; Klerman, 1998).

Data on abortion restrictions were obtained from Merz et al. (1995) and the addenda to Blank et al. (1996).¹² As in Levine et al. (1996), our restrictions variables measure the fraction of a given year that a given abortion restriction was in effect. We use this coding for several reasons. If a restriction is lifted or enjoined, women who are already pregnant may be able to obtain an abortion, but the abortion may be later than in the absence of the law. Passage of a law may also affect abortion providers and patients even before implementation. Using the fraction of the year that a restriction was in effect instead of whether a restriction was enforced at any point during a year has little effect on our results, as discussed below. We also include variables that measure the fraction of a year that each type of restriction was enjoined.¹³ Population-weighted averages of enforced abortion restrictions in neighboring states are also included in the regressions.¹⁴

Table 1 displays summary statistics for the abortion and policy variables used in our analysis. The first column shows means and standard errors for all state and year combinations in the sample. The second column shows summary statistics for states without a Medicaid restriction, a parental involvement law, or a waiting period in effect at any point during a given year. Summary statistics are also shown separately for states with a Medicaid restriction, a parental involvement law, or a waiting period in effect at some point during a year. The last column in Table 1 reports summary statistics on restrictions for state and year combinations not included in our sample because of missing CDC data.¹⁵

In our sample, about 11% of abortions occur after 12 weeks of gestation. Slightly more than one-half of these post-first trimester abortions occur between 13 and 15 weeks of gestation, and about one-third occur between 16 and 20 weeks of gestation. Very few abortions occur after 20 weeks of gestation, when the health risks are highest and provider access is the most limited. In our sample, the incidence of post-first trimester abortions is slightly more than 2 per 1000 women aged between 15 and 44 years. The total abortion rate is about 21 per 1000 women aged between 15 and 44 years.

Medicaid restrictions or parental involvement laws were enforced or enjoined for a substantial fraction of our sample. A Medicaid funding restriction was in effect for 52% of our sample, and a parental involvement law was in effect for 21% of the sample. About 15% of states in the sample provided Medicaid funding because of a court order or injunction, and 15% of states were enjoined from enforcing a parental involvement law. Few states in the sample had a waiting period law in effect, reflecting the relatively recent adoption of such laws.

¹² Information on abortion restrictions is also available in the National Abortion Rights Action League annual publication *Who Decides?* after 1988 and sporadically in *Family Planning Perspectives*. We used these sources to obtain information on mandatory waiting periods and to reconcile differences in the chronologies of Blank et al. (1996) and Merz et al. (1995). For North Carolina, we relied on the chronology in Cook et al. (1999).

¹³ An alternative is to use a variable that measures only whether a law restricting abortions was adopted. This would impose the restriction that the coefficients on enjoined laws are the same as those on enforced laws.

¹⁴ The weights are the population of women aged between 15 and 44 years. If all bordering states enforced a Medicaid funding restriction all year, for example, the border states Medicaid variable would equal one. Results in regressions using a distance-weighted average or a simple average are qualitatively the same.

¹⁵ The states per years included and not included in our data are similar except that those excluded are less likely than the included state and year combinations to have enforced a parental involvement law and more likely to have had a Medicaid funding restriction and an enjoined parental involvement law.

Table 1
Summary statistics for entire sample, by abortion policy, and observations not included in sample^a

	Full sample	No restrictions	Medicaid restriction	Parental involvement	Waiting period	Not in sample
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of abortions after 12 weeks of gestation	10.5 (0.16)	12.0 (0.27)	9.6 (0.20)	9.9 (0.31)	13.1 (0.73)	–
Abortions after first trimester per 1000 women	2.3 (0.05)	3.3 (0.11)	1.7 (0.05)	1.5 (0.06)	1.8 (0.16)	–
Abortions per 1000 women aged between 15 and 44 years	21.2 (0.38)	27.8 (0.83)	16.9 (0.27)	14.4 (0.31)	13.2 (0.67)	24.6 (0.66)
Percent of year Medicaid funding restriction in effect	52.2 (1.7)	0	91.0 (1.0)	86.3 (2.1)	97.3 (2.7)	47.5 (2.5)
Percent of year parental involvement law in effect	21.1 (1.3)	0	33.0 (1.9)	86.2 (1.7)	89.3 (4.3)	10.6 (1.6)
Percent of year waiting period in effect	3.1 (0.58)	0	5.3 (0.92)	12.2 (2.0)	90.8 (3.1)	1.4 (0.61)
Percent of year Medicaid funding restriction enjoined	15.2 (1.2)	28.6 (2.8)	4.6 (0.69)	6.4 (1.5)	0.0 (0.0)	28.9 (2.3)
Percent of year parental involvement law enjoined	15.3 (1.2)	6.0 (1.4)	21.9 (1.7)	7.0 (1.3)	7.0 (3.5)	37.2 (2.4)
Percent of year waiting period enjoined	3.6 (0.61)	0.1 (0.21)	6.3 (0.97)	9.3 (1.8)	5.7 (2.2)	0.4 (0.28)
Percent of border states enforce Medicaid restrictions	55.9 (1.3)	33.7 (1.8)	72.2 (1.4)	76.6 (2.2)	67.9 (5.8)	61.7 (1.8)
Percent of border states enforce parental involvement laws	20.7 (0.85)	13.0 (1.1)	26.1 (1.2)	35.1 (2.0)	47.7 (4.9)	20.5 (1.5)
Percent of border states enforce waiting periods	4.0 (0.38)	3.7 (0.72)	4.2 (0.45)	7.5 (0.85)	18.5 (1.7)	0.6 (0.21)
Sample size	855	254	564	244	36	369

^a Each column contains weighted sample means (standard errors) for the following samples: the entire sample; states that do not enforce any restrictions all year; states that enforce a Medicaid funding restriction; states that enforce a parental involvement law; states that enforce a mandatory delay law; and state and year combinations that are not included in the sample because of missing data. The weights are the population of women aged between 15 and 44 years in the state each year.

Many states border on states that enforced a Medicaid funding restriction. About 56% of women aged between 15 and 44 years lived next to states that restricted Medicaid funding for abortions. Most states that enforced a parental involvement law also restricted Medicaid funding for abortions, and almost all states that had a mandatory waiting period in effect also restricted Medicaid funding.

The descriptive statistics generally do not suggest that states with abortion restrictions have more post-first trimester abortions. The percentage of abortions occurring after 12 weeks of gestation and the post-first trimester abortion rate are higher in states without any restrictions than in states that enforced a Medicaid funding restriction or a parental involvement. However, this difference in abortion timing may be due to other differences between states with restrictions and states without restrictions. The total number of abortions per 1000 women aged between 15 and 44 years is lower in states that enforced restrictions than in states that enforced none of the three restrictions.

4.2. Other controls

The regressions include medical, demographic, economic, and political variables to control for other factors that affect the timing and number of abortions, as well as state and year fixed effects in all specifications and state specific time trends in some specifications. Data sources are listed in Appendix A, and sample means are available from the authors.

The percentage of post-first trimester abortions and the abortion rate are likely to be affected by the number of abortion providers in a state. There are drawbacks to directly controlling for the number of abortion providers. Because the AGI is the only source of data on the number of abortion providers, data are not available for several years in our sample. In addition, the number of providers in a state may be endogenous because the supply of providers is partially determined by the demand for abortion (Blank et al., 1996). Following Levine et al. (1996), we control for the availability of medical services by including the number of physicians in a state who are not obstetricians or gynecologists per 1000 state residents and the number of hospital beds per million people.¹⁶

Demographics may affect both the percentage of abortions in a state occurring after the first trimester and the abortion rate for that state. We control for the percentage of a state's female population aged between 15 and 44 years that is under age 20, over age 34, and black.¹⁷ We also control for the number of marriages per 1000 women aged between 15 and 44 years in a state and the percentage of the population living in non-metropolitan areas.

Economic conditions and the political climate within a state may affect the timing of abortions. Our regressions include the annual average unemployment rate, real per capita income, and female labor force participation rate in a state. We also control for the real value of the maximum Aid to Families with Dependent Children (AFDC) benefits available to a four-person family with one adult in a state because welfare generosity may affect

¹⁶ Our approach is the reduced form of the two-stage model used by Blank et al. (1996).

¹⁷ For 1974–1979, we use one-half of the total population aged between 15 and 44 years for the population of women in that age group because population breakdowns by both age and sex are not available. For 1974–1979, we also use the percentage of the total population aged between 15 and 44 years that is under 20 or over 34 years and the percentage of a state's population that is black.

a woman's ability to pay for an abortion and its timing.¹⁸ We also include two dummy variables measuring whether the governor, House majority, and Senate majority in a state are all Republican or all Democratic.¹⁹

The relationship between abortion restrictions and the timing and number of abortions is next investigated in a multivariate setting.

5. Results

The policy implication of our findings depends on the effect of restrictions on both the timing and rate of abortions. There are two ways that imposing abortion restrictions could lead to an increase in the post-first trimester percentage of abortions. The first way is if restrictions do not affect the total number of abortions but result in women delaying abortions until after the first trimester. The second way is if restrictions disproportionately reduce abortions among women who would have had first trimester abortions relative to women who would have had later abortions. We first examine the relationship between abortion restrictions and the percentage of abortions after the first trimester. We then discuss the relationship between abortion rates and abortion restrictions.

5.1. Percentage of abortions after first trimester

Table 2 contains the coefficient estimates and standard errors on the abortion policy variables from autocorrelation-corrected regressions (odd-numbered columns) and OLS regressions (even-numbered columns) that include state and year fixed effects. We present both specifications because some results are sensitive to the identification assumptions.²⁰ Table 3 shows the results from specifications that also include state-specific time trends. This section focuses on the first two columns of Tables 2 and 3.²¹

The results indicate that the percentage of abortions after the first trimester is higher in states with parental involvement laws. The first column of Table 2 indicates that the percentage of abortions occurring after 12 weeks rises by about 0.9 percentage points when a state enforces a parental involvement law all year; not correcting for autocorrelation (column 2), the estimate is almost twice as large. When state time trends are included (Table 3), the estimated coefficient on parental involvement laws remains significant in both specifications and indicates a 1.2–1.4 percentage point increase in the percentage of

¹⁸ AFDC is now recast as Temporary Assistance for Needy Families (TANF).

¹⁹ The political dummy variables are coded as 0.5 for Nebraska, which has a unicameral, nonpartisan legislature. Dropping Nebraska from the sample does not affect the results. The majority party of the Council of the District of Columbia is considered the majority party for the District's Senate and House; the party of the mayor is considered the party of the governor.

²⁰ In all specifications, both the generalized Durbin–Watson test in Bhargava et al. (1982) and a form of the Breusch–Godfrey test (Greene, 1993) in which we regressed the error terms on the covariates and up to six lags of the errors overwhelmingly rejected the hypothesis of no autocorrelation that underlies the OLS estimates. Test statistics are available from the authors.

²¹ All specifications in Tables 2–4 also include the demographic, health care, economic, and political variables discussed above. Tables with the estimated coefficients for these variables are available from the authors.

Table 2
Regression estimates of effect of abortion restrictions on abortion measures^a

	Abortions after first trimester (%)		Abortion rate, post-first trimester		Abortion rate, all abortions	
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid funding restriction	−0.136 (0.633)	−1.229* (0.566)	0.019 (0.101)	−0.196* (0.092)	0.028 (0.035)	−0.008 (0.033)
Parental involvement law	0.917* (0.466)	1.714** (0.456)	0.096 (0.119)	0.308** (0.080)	−0.055* (0.028)	0.004 (0.025)
Waiting period	2.314* (1.108)	2.635** (0.840)	0.407* (0.179)	0.330* (0.131)	0.059 (0.061)	0.082 (0.056)
Medicaid funding restriction enjoined	0.611 (0.818)	0.711 (0.763)	0.243 (0.129)	0.116 (0.134)	0.058 (0.046)	0.071 (0.047)
Parental involvement law enjoined	1.605** (0.314)	2.035** (0.492)	0.299** (0.081)	0.323** (0.090)	0.024 (0.036)	0.023 (0.031)
Waiting period enjoined	0.623 (0.593)	0.179 (0.593)	0.087 (0.099)	−0.011 (0.100)	0.015 (0.033)	0.057 (0.037)
Border states enforce Medicaid restrictions	2.040** (0.627)	1.614 (0.849)	0.378** (0.127)	0.436** (0.166)	0.019 (0.039)	0.130** (0.046)
Border states enforce parental involvement laws	−0.832 (0.709)	−2.880** (0.923)	−0.240* (0.122)	−0.323 (0.174)	−0.128 (0.082)	−0.024 (0.059)
Border states enforce waiting periods	1.212 (1.305)	0.551 (1.214)	0.294 (0.183)	0.483* (0.237)	0.081 (0.071)	0.050 (0.083)
Joint significance of abortion restrictions (<i>P</i> -value)	4.63 (0.000)	7.76 (0.000)	3.45 (0.000)	7.72 (0.000)	1.78 (0.068)	1.73 (0.079)
Corrected for AR (1)	Yes	No	Yes	No	Yes	No
Number of observations	855	855	855	855	855	855

^a Standard errors in parentheses. Dependent variable is identified in the column heading. Abortion rates are the log of abortions per 1000 women aged between 15 and 44 years in the state. All regressions include state and year fixed effects and other variables as described in the text. Observations are weighted by the population of women aged between 15 and 44 years in the state and year. Regressions are estimated using either the Prais–Winsten method of correcting for AR (1) in panel data as in Bhargava et al. (1982) or, where indicated, OLS. Sample is all state and year combinations from 1974 through 1997 where CDC data on the timing of abortions are available.

* Significant at 5% level.

** Significant at 1% level.

Table 3
Regression estimates of effect of abortion restrictions on abortion measures, with state time trends^a

	Abortions after first trimester (%)		Abortion rate, post-first trimester		Abortion rate, all abortions	
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid funding restriction	0.609 (0.595)	0.759 (0.546)	0.163 (0.103)	0.151 (0.092)	0.037 (0.034)	0.088** (0.034)
Parental involvement law	1.244** (0.439)	1.360** (0.378)	0.152 (0.100)	0.264** (0.085)	−0.055 (0.030)	0.023 (0.035)
Waiting period	1.772 (1.076)	2.513** (0.797)	0.131 (0.153)	0.175 (0.157)	−0.088 (0.052)	−0.072 (0.068)
Medicaid funding restriction enjoined	1.049 (0.778)	1.538* (0.655)	0.342** (0.122)	0.320* (0.125)	0.056 (0.039)	0.106* (0.045)
Parental involvement law enjoined	1.855** (0.343)	2.578** (0.340)	0.321** (0.086)	0.452** (0.081)	0.013 (0.035)	0.045 (0.029)
Waiting period enjoined	0.750 (0.553)	0.830 (0.440)	0.028 (0.079)	0.018 (0.086)	−0.046 (0.033)	−0.015 (0.039)
Border states enforce Medicaid restrictions	2.510** (0.599)	2.515** (0.603)	0.435** (0.133)	0.672** (0.146)	0.004 (0.038)	0.162** (0.049)
Border states enforce parental involvement laws	−0.543 (0.689)	0.091 (0.656)	−0.117 (0.127)	0.164 (0.149)	−0.160 (0.090)	−0.038 (0.064)
Border states enforce waiting periods	1.297 (1.191)	1.914* (0.939)	0.390* (0.155)	0.676** (0.191)	0.171* (0.081)	0.213* (0.093)
Joint significance of abortion restrictions (<i>P</i> -value)	5.88 (0.000)	11.84 (0.000)	3.88 (0.000)	6.77 (0.000)	2.37 (0.012)	2.95 (0.002)
Corrected for AR (1)	Yes	No	Yes	No	Yes	No
Number of observations	855	855	855	855	855	855

^a Standard errors in parentheses. Dependent variable is identified in the column heading. Abortion rates are the log of abortions per 1000 women aged between 15 and 44 years in the state. All regressions include state and year fixed effects, state-specific time trends and other variables as described in the text. Observations are weighted by the population of women aged between 15 and 44 years in the state and year. Regressions are estimated using either the Prais–Winsten method of correcting for AR (1) in panel data as in Bhargava et al. (1982) or, where indicated, OLS. Sample is all state and year combinations from 1974 through 1997 where CDC data on the timing of abortions are available.

* Significant at 5% level.

** Significant at 1% level.

post-first trimester abortions. Because the sample mean of the dependent variable is about 10%, the estimated effect of parental involvement laws is qualitatively important.

Enjoined parental involvement laws are also associated with a higher fraction of later-term abortions in all specifications. The similarity of the results for enjoined and enforced laws is surprising but consistent with results reported by Blank et al. (1996) for Medicaid restrictions. Enjoined laws may affect women's behavior in several ways. Women may not be aware that a restriction is enjoined instead of enforced, particularly soon after a previously enforced law is enjoined. A newly issued injunction may change the behavior of some women who are already pregnant. Enjoined restrictions may also affect abortion availability if providers expected injunctions to be lifted at some point or if facilities stopped providing abortions when a law was in effect and did not resume providing abortions when an injunction was issued. In addition, the significance of the enjoined parental involvement law variable may indicate that abortion restrictions are endogenous with respect to abortion measures, an issue addressed further below.

The finding that enforced and enjoined parental involvement laws have sizable effects is striking because the data include abortions to all women, not just minors. Previous research has had difficulty finding effects of parental involvement laws on abortion rates among women aged between 15 and 44 years (e.g. Blank et al., 1996; Matthews et al., 1997). One interpretation of our results is that parental involvement laws have much larger effects on the timing of abortions among minors than suggested by our estimates.²² Another interpretation is that adoption of a parental involvement law may reduce all women's access to abortions.

Our autocorrelation-corrected results suggest that a parental involvement law raises the percentage of post-first trimester abortions by about 0.9–1.2 percentage points. On average, 11% of abortion patients in our sample are under age (18 years), suggesting an 8–11 percentage point rise in the fraction of abortions after the first trimester among minors if a parental involvement law only affects minors, a large effect.²³ In comparison, Rogers et al. (1991) find that Minnesota's parental notification law led to a 3.6 percentage point increase in the fraction of post-first trimester abortions to minors. However, the fraction of abortions after the first trimester also rose among older women in Minnesota, resulting in a 1 percentage point increase across women aged between 15 and 44 years, similar to our results.

The results also suggest that mandatory delay laws affect the timing of abortions. In the fixed effects regressions (Table 2), having a mandatory delay law in effect all year raises the percentage of post-first trimester abortions by 2.3 points or 2.6 percentage points. The result from the autocorrelation-corrected specification falls in magnitude and is significant only at the 10% level when state time trends are included (Table 3), whereas the OLS result is robust to including the time trends. The magnitude of these estimates is similar to previous findings, which found that Mississippi's mandatory delay law led to an increase of 1.8–4.3 percentage points in the percentage of post-first trimester abortions (Althaus and Henshaw,

²² In results available from the authors, we find that the share of abortions to women under 20 years of age is significantly negatively associated with the presence of a parental involvement law.

²³ Data from the CDC indicate that teenagers accounted for 29% of post-first trimester abortions in 1996, for example, and 17% of abortions among teenagers occurred after the first trimester. According to Rosenfield (1994), the highest rates of post-first trimester abortions are among the youngest teenage groups.

1994; Joyce et al., 1997; Joyce and Kaestner, 2000, 2001). Our finding that mandatory delay laws have larger effects on timing than parental involvement laws or Medicaid restrictions is not surprising since all women are affected by such laws.

The effect of Medicaid restrictions depends on the identification assumption. As column 1 of Table 2 indicates, enforced Medicaid restrictions have a small but imprecisely estimated negative effect on the share of abortions occurring after the first trimester when correcting for autocorrelation. The OLS regression, in contrast, indicates a sizable negative effect of Medicaid restrictions on the percentage of abortions occurring after the first trimester. When state time trends are included, the estimates are positive but insignificant in both specifications.

The estimated coefficients on border state laws are not consistent with the expected effects on women's behavior. Enforcement of Medicaid restrictions in border states is positively associated with the percentage of abortions after the first trimester even though Medicaid recipients are ineligible for Medicaid-funded abortions in other states. One possibility consistent with this result is that the number of providers falls in states that restrict Medicaid funding, leading to later abortions in nearby states. The presence of parental involvement laws in neighboring states, in contrast, does not appear to raise the fraction of later-term abortions even though such laws would affect minors.

One concern about giving a causal interpretation to the estimates of the separate coefficients is that the laws are highly correlated. If all states adopted all of the laws at the same time, it would be impossible to separately parse out the effects of the different laws. We therefore report the *F*-statistics and *P*-values for the joint significance of all the restrictions. In all of these specifications, the restrictions are jointly significantly different from zero.

5.2. *Post-first trimester abortion rate*

We now focus on the effect of abortion restrictions on the number of abortions after the first trimester, rather than the percentage. These results are shown in columns 3 and 4 of Tables 2 and 3.

Enforced mandatory delay laws appear to have a significant positive effect on the post-first trimester abortion rate. Table 2 indicates that enforcing a waiting period raises the post-first trimester abortion rate by 3.3–4.1%. In the OLS specification in Table 2, parental involvement laws raise the post-first trimester abortion rate by about 3%, whereas the autocorrelation-corrected specification does not indicate a significant positive effect. The OLS regression also indicates that enforced Medicaid funding restrictions lower the post-first trimester abortion rate, but the autocorrelation-corrected regression yields an insignificant coefficient. Enjoined Medicaid funding restrictions and parental involvement laws as well as enforced Medicaid funding restrictions and mandatory delay laws in neighboring states are significantly positively related to the post-first trimester abortion rate in either the fixed effects or the state time trends specifications or in both.

5.3. *Overall abortion rate*

We also estimate the effect of abortion restrictions on the total number of abortions per 1000 women aged between 15 and 44 years. Interpretation of our results depends critically

on the above findings for post-first trimester abortions and the findings discussed next for all abortions. If there was no change in the overall abortion rate in response to a restriction, then a finding that the share of abortions after the first trimester increased after the restriction implies that the restriction delayed when women obtained abortions. If, however, the overall abortion rate fell, an increase in the percentage of late abortions could have been entirely due to a decrease in the rate of first trimester abortions. As discussed below, some of our results are consistent with each of these scenarios.

Enforced parental involvement laws lower the overall abortion rate by about 5.5% when correcting for autocorrelation, as shown in column 5 of Table 2. The estimated effect is similar in magnitude and remains significant at the 10% level when state time trends are included (Table 3). The OLS specifications, however, indicate a small and positive (although imprecisely estimated) effect of enforced parental involvement laws. Parental involvement laws that are enjoined in the own state or enforced in neighboring states do not have a significant effect on the total abortion rate.

The interpretation of the results for enforced parental involvement laws depends on whether the autocorrelation-corrected or the OLS results are relied upon. The autocorrelation-corrected results suggest that the rise in the fraction of post-first trimester abortions is due to a decline in the incidence of first trimester abortions and not to an increase in the incidence of post-first trimester abortions. In results not shown here, an enforced parental involvement law is associated with a 6.4% decline in the first trimester abortion rate, and the estimate is significant at the 5% level. Extrapolating from the autocorrelation-corrected results, the decline in the first trimester abortion rate resulting from an enforced parental involvement law would lead to a 0.62 percentage point increase in the percentage of abortions after the first trimester; our results in column 1 of Table 2 indicate a total increase of 0.92 percentage points. Thus, the reduction in first trimester abortions can account for most of the change in the distribution of the timing of abortions.²⁴

The OLS results, in contrast, suggest that parental involvement laws change the timing of some abortions without reducing the total number of abortions, or that these laws lead to delays. In results not shown here, an OLS regression examining the first trimester abortion rate yields an estimated coefficient on the enforced parental involvement law variable of -0.016 that is not significant. The OLS regressions suggest that the increase in the post-first trimester abortion rate accounts for all of the estimated 1.7 percentage point increase (column 2 of Table 2) in the percentage of abortions occurring after the first trimester.

Both autocorrelation-corrected and OLS specifications indicate that enjoined parental involvement laws raise the fraction of post-first trimester abortions by raising the incidence of post-first trimester abortions without lowering the total number of abortions. None of the results give clear implications for the effects of parental involvement laws in bordering states.

Taken as a whole, the results for mandatory delay laws indicate that enforced restrictions boost the fraction of abortions occurring after the first trimester by raising the incidence of

²⁴ The share of the change in percentage of abortions after the first trimester that is driven by effects of restrictions on the rate of first trimester abortions and the share driven by effects on the rate of post-first trimester abortions can be backed out from the sample means for first and post-first trimester abortions and the coefficients on the restriction variables in regressions relating the rate of first and post-first trimester abortions to the restrictions.

post-first trimester abortions, not by lowering the total number of abortions. In other results, mandatory delay laws are not significantly associated with the first trimester abortion rate in either OLS or autocorrelation-corrected regressions. Enjoined mandatory delay laws do not have significant effects on the timing or number of abortions regardless of which specification is used.

The results for Medicaid funding restrictions are sensitive to the identification assumptions. The OLS regression with state time trends (Table 3, column 6) indicates that enforced and enjoined Medicaid funding restrictions are positively associated with the overall abortion rate. The autocorrelation-corrected regressions, with and without time trends, do not indicate significant effects; however, the 95% confidence intervals for the enforced and enjoined Medicaid funding restriction variables in column 5 of Table 3 include the OLS estimates reported in column 6. The OLS estimates without time trends (Table 2, column 6) also do not indicate a significant effect of Medicaid restrictions on the overall abortion rate. The OLS estimates indicate a positive effect of enforced Medicaid restrictions in neighboring states, whereas the autocorrelation-corrected estimates do not. Combined with earlier results, this suggests that restrictions on Medicaid funding in neighboring states raise the percentage of abortions occurring later in pregnancy without significantly reducing the total number of abortions.

The results for the fraction of post-first trimester abortions do not suggest that Medicaid restrictions in a given state lead to changes in the timing of abortions in that state. We therefore do not disentangle whether a change in the distribution of the timing of abortions is due to a reduction in first trimester abortions or to an increase in post-first trimester abortions.²⁵ Nonetheless, our results for Medicaid restrictions may be surprising since several previous studies found that Medicaid funding restrictions lower the overall abortion rate. Possible reasons for this difference include differences in the source of the abortion data, in time periods, and in the coding of restrictions. The next section investigates these issues.

6. Robustness

6.1. Comparison to other studies

We rely on data from the Centers for Disease Control because it is the only source of data on the distribution of abortions by weeks of gestation for a large number of states. Because the CDC data on total abortion numbers are less complete than data from the Alan Guttmacher Institute (Blank et al., 1996; Levine et al., 1996) and because our findings for the effects of abortion restrictions on the overall abortion rate differ from previous results, we examine the robustness of our findings by re-estimating the overall abortion rate regressions using both CDC and AGI data.²⁶

²⁵ In results not shown here, the enforced and enjoined Medicaid variables are not significantly associated with the first trimester abortion rate in OLS or autocorrelation-corrected specifications.

²⁶ The AGI data are from Henshaw and Van Vort (1992), Henshaw and Van Vort (1994), and Henshaw (1998). The AGI data used here are abortions by state of occurrence, not state of residence.

Table 4

Regression estimates of effect of abortion restrictions on log of abortion rate, comparison of results using CDC and AGI data^a

	CDC data, all available observations		AGI data, all available observations		CDC data, observations available from AGI	
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid funding restriction	0.026 (0.044)	−0.018 (0.056)	−0.017 (0.032)	−0.069 (0.059)	0.015 (0.048)	−0.009 (0.066)
Parental involvement law	0.001 (0.043)	0.043 (0.035)	0.006 (0.037)	0.034 (0.036)	−0.020 (0.048)	0.047 (0.042)
Waiting period	−0.070 (0.073)	0.042 (0.047)	−0.104 (0.056)	−0.110* (0.053)	0.015 (0.087)	0.033 (0.061)
Medicaid funding restriction enjoined	0.115** (0.043)	0.123* (0.052)	0.030 (0.033)	0.032 (0.044)	0.110* (0.047)	0.128* (0.058)
Parental involvement law enjoined	0.012 (0.026)	0.009 (0.029)	0.033 (0.020)	−0.023 (0.027)	−0.001 (0.032)	−0.003 (0.037)
Waiting period enjoined	−0.045 (0.050)	0.001 (0.033)	−0.017 (0.025)	−0.015 (0.043)	−0.041 (0.049)	0.014 (0.055)
Border states enforce Medicaid restrictions	0.046 (0.045)	0.081 (0.045)	0.046 (0.032)	0.101** (0.038)	0.032 (0.047)	−0.097* (0.049)
Border states enforce parental involvement laws	−0.026 (0.076)	0.009 (0.051)	0.029 (0.042)	0.069 (0.047)	0.053 (0.110)	0.014 (0.065)
Border states enforce waiting periods	0.031 (0.091)	−0.006 (0.102)	0.158* (0.080)	0.218* (0.109)	0.099 (0.127)	0.023 (0.150)
Joint significance of abortion restrictions (<i>P</i> -value)	1.75 (0.074)	1.98 (0.039)	2.20 (0.020)	3.24 (0.001)	1.34 (0.214)	1.72 (0.080)
Corrected for AR (1)	Yes	No	Yes	No	Yes	No
Number of observations	1224	1224	867	867	867	867

^a Standard errors in parentheses. Dependent variable is the log of all abortions by state of occurrence per 1000 women aged between 15 and 44 years in the state. All regressions include state and year fixed effects and other variables as described in the text. Observations are weighted by the population of women aged between 15 and 44 years in the state and year. Regressions are estimated using either the Prais–Winsten method of correcting for AR (1) in panel data as in Bhargava et al. (1982) or, where indicated, OLS. Sample is all state and year combinations from 1974 through 1997 where the indicated data on the number of abortions are available.

* Significant at 5% level.

** Significant at 1% level.

We first estimated the overall abortion rate fixed effects regression using all available CDC observations of the total number of abortions, instead of the subsample with data on the distribution of abortions by weeks of gestation. As columns 1 and 2 of Table 4 show, abortion restrictions do not have significant negative effects on the abortion rate when all observations from the CDC over 1974–1997 are included, regardless of whether the specification corrects for autocorrelation. The results are generally similar when the 17 years of available data from the AGI are used, as columns 3 and 4 show. However, the AGI data suggest a more negative, although imprecisely estimated, effect of enforced Medicaid funding restrictions than do the CDC data. In addition, enjoined Medicaid funding restrictions are positively associated with the total abortion rate in the CDC data, but the relationship is insignificant when using the AGI data.

The pattern of the coefficients is also similar when the CDC data are restricted to the subsample of years for which the AGI data is available, shown in the last two columns. The CDC data again tend to give more positive estimates of the effect of enforced Medicaid restrictions than do the AGI data, although the differences are not statistically significant. These results suggest that part of our failure to replicate previous findings negative effects of Medicaid restrictions on the number of abortions may be related to the choice of data set, although none of the results in either data set indicate a significant negative effect. Confidence intervals for all of the estimates in Table 4 include the -5.5% effect of enforced Medicaid laws reported by Levine et al. (1996) for the period 1977–1990 using AGI data.

We also investigated whether our failure to find negative effects of enforced Medicaid restrictions is due to differences in the time period investigated. In results not shown here, using CDC data from 1974 to 1988 — the period examined by Blank et al. (1996) — gives results similar to those in Tables 2 and 3. In particular, the results using the CDC data continue to not indicate a significant negative effect of enforced Medicaid funding restrictions on abortion rates. Using AGI data for the period 1974–1988 does yield a negative (significant at the 10% level) coefficient on the enforced Medicaid variable in the OLS specification, but the hypothesis of no autocorrelation is overwhelmingly rejected and the autocorrelation-corrected regression gives a positive and insignificant coefficient.

Another potential source of differences is how abortion restrictions are coded. The above analysis uses the fraction of the year that a given restriction was in place; some previous studies instead used a dummy variable to indicate that a restriction was enforced at any time in a given year. In results not shown here, using dummy variables during the period 1974–1997 yields results qualitatively similar to those in Tables 2–4, regardless of whether an OLS or AR (1) specification is used. However, we did find a negative and significant relationship between enforced Medicaid funding restrictions and the overall abortion rate in the AGI data during the period 1974–1988 when coding the abortion restrictions as dummy variables in an OLS specification — the data, specification, and time period used by Blank et al. (1996). Again, however, the hypothesis of no autocorrelation is overwhelmingly rejected and the autocorrelation-corrected regression gives a positive and insignificant coefficient.

Based on these robustness checks, we conclude that some of the difference between our results for the effect of enforced Medicaid restrictions and previous research is due to our autocorrelation correction. The OLS specifications tend to give more negative estimates than the autocorrelation-corrected specifications. In addition, the CDC data appear to give more

positive estimates of the effect of enforced Medicaid restrictions on the overall abortion rate than do the AGI data. Data on the timing of abortions are not available from the AGI, however.

6.2. Policy endogeneity

The analysis thus far has assumed that abortion restrictions provide an exogenous source of variation in the availability or ease of getting an abortion, conditional on the other controls. As with any analysis using state and federal law changes as a source of identifying variation, this approach is subject to the critique of legislative endogeneity (Besley and Case, 2000). We addressed endogeneity concerns by including leads of the abortion policy variables in the regressions. If state lawmakers adopt new laws because of changes in abortion numbers or in public sentiment, future abortion restrictions would be correlated with current abortion numbers. When 1- and 2-year leads of enforced and enjoined abortion restrictions were included in the regressions, the estimated coefficients on the leading variables were not jointly statistically significant. In addition, the coefficients on the contemporaneous policy variables remained similar to those reported in Tables 2 and 3. However, a few of the leads of the enjoined restriction variables were significantly associated with the abortion measures.

7. Conclusion

This analysis examined the effect of Medicaid funding restrictions, parental involvement laws, and mandatory waiting periods on the timing of abortions. We found that enforced and enjoined parental involvement restrictions as well as enforced waiting periods appear to cause a rise in the share of later-term abortions. Enforced parental involvement laws appear to lower the total abortion rate when correcting for autocorrelation, suggesting that the increase in the share of later-term abortions is due to a reduction in the number of early abortions and not to a shift in the timing of abortions. However, OLS specifications suggest that the timing of abortions, not the number, changes when states enforce parental involvement laws. Enjoined parental involvement laws and enforced mandatory delay laws are positively associated with the post-first trimester abortion rate without an accompanying change in the first trimester or overall abortion rate, suggesting that these restrictions cause delays in abortions. We generally failed to find a significant relationship between enforced Medicaid funding restrictions and the timing of abortions in our data, although this depends somewhat on whether we correct for autocorrelation.

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Appendix A. Data sources

- Distribution of abortions by weeks of gestation: Data for 1973–1981 are from various issues of the CDC publication *Abortion Surveillance*, and data for 1982–1997 are from various issues of the CDC publication *Morbidity and Mortality Weekly Report*.
- Abortion restrictions: Merz et al. (1995), the addenda to Blank et al. (1996), and Cook et al. (1999), various issues of the NARAL publication *Who Decides?*, and the sources in *Family Planning Perspectives* listed in the data appendix to Levine et al. (1996).
- Hospital beds: Data are from American Hospital Association, *Hospital Statistics*, various years.
- Non-OB/GYN physicians: Data are from the American Medical Association, *Physician Characteristics and Distribution in the United States*, various years.
- Population of women aged between 15 and 44 years, and breakdowns by age and race: Bureau of the Census, *Current Population Reports*, P25 series.
- Percentage of population living in metropolitan areas: Bureau of the Census, *Statistical Abstract*, various years. Data for 1989, 1991, and 1995 were linearly interpolated.
- Average annual unemployment rate: Bureau of Labor Statistics, *Employment and Earnings*, various years.
- Women's labor force participation rate: Bureau of Labor Statistics, *Geographic Profile of Employment and Unemployment*, various years, and data provided by Rebecca Blank.
- Per capita personal income: Bureau of Economic Analysis, *Survey of Current Business*, various editions. Deflated using the personal consumption expenditures deflator (1992 = 100).
- AFDC benefits for a four-person family with one adult: Ways and Means Committee, US House of Representatives, *Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means*, various years, and data provided by Rebecca Blank. Deflated using the personal consumption expenditures deflator.
- Party affiliation of governor and majority of state Senate and House: Council of State Governments, *The Book of the States*, various years.
- Number of marriages: National Center for Health Statistics, *Vital Statistics of the United States* and *Monthly Vital Statistics Report*, various years.

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