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VARIATIONS IN INFANT MORTALITY RATES AMONG COUNTIES OF THE UNITED STATES: THE ROLES OF PUBLIC POLICIES AND PROGRAMS

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Abstract—The purpose of this paper is to shed light on the causes of the rapid decline in the infant mortality rate in the United States in the period after 1963. The roles of four public policies are considered: Medicaid, subsidized family planning services for low-income women, maternal and infant care projects, and the legalization of abortion. The most striking finding is that the increase in the legal abortion rate is the single most important factor in reductions in both white and nonwhite neonatal mortality rates. Not only does the growth in abortion dominate the other public policies, but it also dominates schooling and poverty.

From 1964 to 1977, the infant mortality rate in the United States declined at an annually compounded rate of 4.4 percent per year. This was an extremely rapid rate of decline compared to the figure of 0.6 percent per year from 1955 to 1964. The reduction in mortality proceeded at an even faster pace in the 1970s than in the late 1960s (5.2 percent per year from 1971 to 1977 versus 3.8 percent per year from 1964 to 1971).[1]

The period from 1964 to 1977 witnessed the introduction of Medicaid, maternal and infant care projects, Federally subsidized family planning services for low-income women, the legalization of abortion, and the widespread adoption of oral and intrauterine contraceptive techniques. These developments have been pointed to in discussions of the cause of the acceleration in the downward trend in infant mortality (for example, Eisner et al., 1978; Lee et al., 1980), but the question has not been studied in a multivariate context. Moreover, the relative contribution of each factor has not been quantified. The purpose of this paper is to estimate the impacts of public policies and programs on infant mortality.

ANALYTICAL FRAMEWORK

Economic models of the family and household production developed by Becker and Lewis (1973) and Willis (1973) provide a fruitful theoretical framework to generate multivariate health outcome functions and to assess the roles of social programs and policies in these functions. Ben-Porath (1973), Ben-Porath and Welch (1976), Williams (1976), and Lewit (1977) have utilized the economic model of the family to study theoretically and empirically the determinants of birth outcomes. Following these authors, we assume that the parents' utility function depends on their own consumption, the number of births, and the survival probability. Both the number of births and the survival probability are endogenous variables. In particular, the survival probability production function depends upon endogenous inputs of medical care, nutrition, and the own time of the mother. In addition, the production function is affected by the reproductive efficiency of the mother and by other aspects of her efficiency in household production. Given the considerable body of evidence that education raises market and nonmarket productivity, one would expect more educated mothers to be more efficient producers of surviving infants.

The above model calls attention to the important determinants of the survival probability and its complement, the infant mortality rate. In general this set of determinants is similar to that used in multivariate studies of infant mortality with different and fewer theoretical points of departure (for example, Fuchs, 1974; Williams, 1974; Brooks, 1978; Gortmaker, 1979). Moreover, the model provides a ready structure within which to interpret the effects of public programs and policies on infant mortality.[2] Thus, Medicaid and maternal and infant care projects lower the direct and indirect costs[3] of obtaining prenatal and obstetrical care, which should increase the likelihood of a favorable birth outcome and lower infant mortality. Federal subsidization of family planning services, abortion reform, and the diffusion of oral and intrauterine contraceptive techniques (the pill and the IUD) reduce the costs of birth control and increase its availability. Within the context of an economic model of the family, these developments raise the "optimal" survival probability and lower the "optimal" number of births. In addition, they will lower the observed infant mortality rate if less healthy fetuses are less likely to be conceived or more likely to be aborted.[4]

To measure the relative importance of the above factors in the recent U.S. infant mortality experience, a cross-sectional regression analysis of variations in infant mortality rates is performed among counties of the United States in 1971. This procedure capitalizes on variations in the programs at issue among counties at a moment in time. Thus it provides a set of impact coefficients to identify the contribution of each program net of basic determinants of infant mortality such as poverty, schooling levels, and the availability of physicians. After estimating the regression, its coefficients are applied to national trends in the exogenous variables between 1964 and 1977 to "explain" the trend in infant mortality.

This methodology has a number of desirable properties. It mitigates the multicollinearity problems that almost certainly would arise in a time-series regression analysis for the United States as a whole. Moreover, the state-of-the-art in neonatology, which has changed over time and is difficult to quantify, is constant in the cross section. Finally, with the exception of abortion reform, the programs that we study are aimed at poor persons. Therefore, the appropriate way to measure their impacts is to interact the policy variables with the fraction of births to poor women. This insight is incorporated into the basic regression specification.

The last point is worth spelling out in more detail. Let d_{pj} be the infant mortality rate of babies born to poor mothers (infant deaths divided by live births) in the j^{th} county, and let d_{nj} be the infant mortality rate of babies born to nonpoor mothers. As an identity,

$$d_j = k_j d_{pj} + (1 - k_j)\, d_{nj}, \tag{1}$$

where d_j is the observed infant mortality rate and k_j is the fraction of births to poor mothers. Specify behavioral equations for d_{pj} and d_{nj} as follows:

$$d_{pj} = \alpha_0 + \alpha_1 x_{pj} + \alpha_2 y_{pj} + \alpha_3 w_{pj} + \alpha_4 z_j \tag{2}$$

$$d_{nj} = \beta_0 + \beta_2 y_{nj} + \beta_3 w_{nj} + \beta_4 z_j. \tag{3}$$

In these equations, x_{pj} is a vector of policy variables that affects the mortality rate of poor babies alone such as Medicaid; $w_{ij}(i = p, n)$ is a vector of policy variables that affects both groups such as the group-specific abortion rate (legal abortions per thousand live births); y_{ij} refers to a group-specific vector of basic determinants of infant mortality such as mother's schooling; and z_j is a vector of variables that has the same value for each group such as physicians per capita. Since there are no data on income-specific mortality rates at the county level, substitute equations (2) and (3) into equation (1) to obtain

$$dj = \beta_0 + (\alpha_0 - \beta_0)k_j + \alpha_1 k_j x_{pj} + \alpha_2 k_j y_{pj} + \beta_2(1 - k_j)y_{nj} + \alpha_3 k_j w_{pj} + \beta_3(1 - k_j)w_{nj} + \alpha_4 k_j z_j + \beta_4(1 - k_j)z_j. \quad (4)$$

Equation (4) gives a multiple regression of d_j on eight variables (vectors): k_j, $k_j x_{pj}$, $k_j y_{pj}$, $(1 - k_j)y_{nj}$, $k_j w_{pj}$, $(1 - k_j)w_{nj}$, $k_j z_j$, and $(1 - k_j)z_j$. Attempts to estimate this equation would be plagued by severe problems of multicollinearity and by the absence of income-specific measures of certain variables such as the legal abortion rate. Therefore, we assume that the income-specific abortion rate (w_{ij}) is proportional to its weighted average ($w_{ij} = r_i w_j$). In addition, we assume that schooling of poor mothers in a given county is proportional to schooling of nonpoor mothers ($y_{pj} = s y_{nj}$). The actual equation that we fit is

$$d_j = \beta_0 + (\alpha_0 - \beta_0)k_j + \alpha_1 k_j x_{pj} + \delta_2 y_{nj} + \delta_3 w_j + \delta_4 z_j, \quad (5)$$

where δ_2 estimates $\alpha_2 k_j s + \beta_2(1 - k_j)$, δ_3 estimates $\alpha_3 k_j r_p + \beta_3(1 - k_j)r_n$, and δ_4 estimates $\alpha_4 k_j + \beta_4(1 - k_j)$. The important point to note is that we employ k_j and the product of k_j and x_{pj} as independent variables in the regression. Thus, we employ a specification that explicitly recognizes that the impact on the observed infant mortality rate of policies aimed at the poor is larger the larger is the fraction of births to poor mothers ($\partial d_j/\partial x_{pj} = k_j\alpha_1$). Moreover, our specification yields a direct estimate of the impact parameter (α_1).

A more general formulation of the above model can be developed by decomposing the observed infant mortality rate in the j^{th} county into rates associated with a variety of birth characteristics such as mother's age, mother's income, parity, birth weight, and legitimacy status of the birth:

$$d_j = \sum_{i=1}^{m} k_{ij}\, d_{ij}. \quad (6)$$

In this equation k_{ij} is the fraction of births in the i^{th} category and d_{ij} is the infant mortality rate associated with that category. An example of one such category is an illegitimate, low-birth weight birth to a low-income, teenage mother with no previous live births. The policies studied here might lower the observed infant mortality rate by lowering the fraction of births in high-risk categories (categories where d_{ij} is higher than on average) and by lowering the mortality rate in a given risk category (d_{ij}). These regression estimates incorporate both effects because there is no control here for characteristics such as the percentage of births to teenage mothers, the percentage of illegitimate births, the percentage of fourth and higher-order births, and the percentage of low-birth weight births. The percentage of births to low-income mothers is included, but as indicated below a measure is employed that varies among counties only because the percentage of the population in poverty varies among counties.

Note that some discussions of the probable impacts of abortion reform on infant mortality assume that this public policy operates solely by reducing the percentage of high-risk births, especially the percentage of low-birth weight births (for example, Lee et al., 1980). Yet abortion reform also might lower infant mortality by lowering risk-specific death rates. In particular, more prenatal and perinatal care may be allocated to pregnancies that are not aborted. Indeed, in the context of the economic model of the family outlined above (Becker and Lewis, 1973; Willis, 1973), it is likely that a reduction in the cost of birth control will have a larger impact on the amount of medical care demanded and therefore on the survival probability than a reduction in the price of care. The reason is that a reduction in the cost of fertility control raises the cost (price) of a birth, while a reduction in the price of medical care lowers the cost of a birth. Although both developments almost certainly will raise the optimal survival probability, a reduction in the cost of fertility control will lower the optimal birth rate, while a reduction in the price of care may increase it. This point should be kept in mind when the effects of abortion reform on infant mortality are compared to the effects of Medicaid coverage of prenatal and perinatal care services.[5]

EMPIRICAL SPECIFICATION

Data and Measurement of Infant Mortality

The basic data set used here is the Urban Institute's expanded version of the Area Resource File (ARF). The ARF is a county-based data service, prepared by Applied Management Sciences, Inc., for the Bureau of Health Professions, Health Resources Administration, U.S. Department of Health and Human Services. It incorporates information from a variety of sources for 3,078 counties in the United States. These counties can also be aggregated into larger geographic areas such as county groups, Standard Metropolitan Statistical Areas, and states. Demographic and socioeconomic characteristics are taken from the 1970 Census of Population. Socioeconomic characteristics of women ages 15 to 49 come from the 1970 Census of Population, Women of Childbearing Age Tape. Deaths by age, race, and sex for the years 1969 through 1976 are obtained from the National Center for Health Statistics (NCHS) Mortality Tape. Births by race for those years are obtained from the NCHS Natality Tape. Health manpower and facilities come from the American Medical Association, the American Hospital Association, and other sources. We have added measures pertaining to the policies and programs discussed previously to the ARF from sources indicated in the next section.

There are two components of infant mortality: neonatal mortality and postneonatal mortality. Neonatal mortality refers to deaths of infants within the first 27 days of life. Postneonatal mortality refers to deaths of infants between the ages of 28 and 364 days. Neonatal deaths are usually caused by congenital abnormalies, prematurity, and complications of delivery; while postneonatal deaths are usually caused by infectious diseases and accidents.

This empirical analysis is limited to the neonatal mortality rate, defined as neonatal deaths per thousand live births. Since the causes of the two types of infant deaths are dissimilar, socioeconomic variables and public programs are likely to have different effects on each. Specifically, these policy variables are more relevant to neonatal mortality than to postneonatal mortality. For instance, the former is considerably more sensitive to appropriate prenatal and obstetrical care than the latter (Lewit, 1977). Another reason for this focus is that the neonatal mortality rate is much larger than postneonatal mortality rate; it was three times as large in 1971. Consequently, trends in the infant mortality rate are dominated by trends in the neonatal mortality rate. Obviously, one cannot hope to explain trends in the infant mortality rate without being able to explain trends in the neonatal mortality rate.

Separate regressions are fitted for white neonatal mortality and for black neonatal mortality. Black neonatal mortality rates are much higher than white rates. In a non-race-specific regression, one would enter the percentage of black births to control for race differences. But this variable would be highly correlated with the percentage of births to low-income women, schooling, and other independent variables. By fitting race-specific regressions, multicollinearity is reduced and the coefficients of the independent variables are allowed to vary between races. Linear regressions are estimated because a linear specification facilitates the aggregation of the two income-specific mortality rate functions given in the first section of this paper into a single equation for the entire population.

Counties are used rather than states or Standard Metropolitan Statistical Areas (SMSAs) as the units of observation. SMSAs and states are very large and sometimes heterogenous. Income, schooling levels, medical resources and other variables may vary greatly within an SMSA or a state. Since counties are much more homogeneous, these problems are reduced in our research. A weakness with the use of counties is that the small size of some of these areas may mean that people may receive medical care outside the county. Moreover, the small number of births in certain counties may increase the importance of random movements or "noise" in the determination of regression coefficients.

These problems with county data are reduced by including in the regressions only counties with a population of at least 50,000 persons in 1970. A county must also have at least 5,000 blacks for inclusion in the black regressions. There are 679 counties in the white regressions and 359 counties in the black regressions. In addition to selecting large counties, we attenuate random elements by employing a three-year average of the race-specific neonatal mortality rate for the period 1970–72 as the dependent variable and by estimating weighted regressions, where the set of weights is the square root of the race-specific number of births in 1971.

Neonatal mortality for the period 1970–72 is studied because measures of all independent variables are available for a year in that period or for 1969. In addition, it provides an ideal time frame to estimate the impact of abortion reform because of substantial cross-sectional variations in the legal abortion rate in that period. Abortion reform proceeded at a rapid pace between 1967 and the middle of 1970. Prior to 1967 all states of the United States had laws which permitted abortion only when it was necessary to preseve a pregnant woman's life. Beginning in 1967 some states started to reform these laws to increase the number of circumstances under which abortions could be performed. The reformed statutes legalized abortions if there was a substantial risk that continuance of the pregnancy would seriously impair the physical or mental health of the woman, or that the child resulting from the pregnancy would be born with a serious physical or mental defect, or in cases of pregnancy resulting from rape or incest. By 1970, twelve states had enacted such statutes. Moreover, in 1970 four additional states enacted extremely liberal abortion laws which placed no legal restriction on the reasons for which an abortion may be obtained prior to the viability of the fetus (Center for Disease Control, 1971). After the middle of 1970, there was no significant changes in abortion law until 1973 when the Supreme Court ruled most restrictive state abortion laws unconstitutional. Concurrent with these reforms, the U.S. ratio of legal abortions per thousand live births rose from 4 in 1969 to 180 in 1972 and to 361 in 1977 (Center for Disease Control, 1971; 1972; 1974; U.S. Bureau of the Census, 1980).

Measurement of Independent Variables

Wherever possible, race-specific variables are employed in the regressions. Such variables are denoted with an asterisk. Except for the Medicaid and abortion measures, all variables are county-specific. Table 1 contains definitions, means, and standard deviations of the dependent and independent variables in the regressions.

The number of active non-federal physicians per thousand population serves as a general proxy for the price and availability of medical care.[6] The roles of the percentage of births to poverty mothers and the percentage of women of childbearing ages who had at least a high school education were discussed above.

Table 1.—Definitions. Means, and Standard Deviations of Variables[a]

Variable Name	Definition
Neonatal Mortality 1970-72 *	Three-year average neonatal mortality rate for the period 1970-72; deaths of infants less than 28 days old per 1,000 live births (μ_w = 12.729; σ_w = 2.076; μ_b = 21.477; σ_b = 3.988)
PB*	Estimated percentage of births to mothers with family incomes less than the poverty level for the period 1969-71 (μ_w = 21.324; σ_w = 8.388; μ_b = 35.188; σ_b = 11.235)
%≥ HS[b] *	Percentage of women aged 15 to 49 who had at least a high school education in 1970 (μ_w = 62.927; σ_w = 7.238; μ_b = 44.096; σ_b = 8.527)
Physicians	Active non-federal physicians per 1,000 population in 1971 (μ_w = 1.505; σ_w = 0.987; μ_b = 1.954; σ_b = 1.220)
MAXPB*	Dichotomous variable that equals one if county is in a state that covers all first-time pregnancies to financially eligible women under Medicaid (MA) multiplied by PB* (μ_w = 7.892; σ_w = 10.850; μ_b = 7.104; σ_b = 12.657)
MUXPB*	Dichotomous variable that equals one if county is in a state that covers first-time pregnancies under Medicaid only if no husband present or if husband present but unemployed and not receiving unemployment compensation (MU) multiplied by PB* (μ_w = 2.810; σ_w = 7.521; μ_b = 3.857; σ_b = 10.219)
MNXPB*	Dichotomous variable that equals one if county is in a state that covers first-time pregnancies under Medicaid only if no husband present (MN) multiplied by PB* (μ_w = 2.284; σ_w = 7.851; μ_b = 7.536; σ_b = 18.185)
MIXPB*	Dichotomous variable that equals one if the county had an M and I project in 1971 (MI) multiplied by PB* (μ_w = 5.339; σ_w = 9.390; μ_b = 16.152; σ_b = 16.577)
PMIBXPB*	Births in M and I projects in 1971 as a percentage of births to women with low income (PMIB) multiplied by PB* (μ_w = 2.174; σ_w = 5.086; μ_b = 8.470; σ_b = 12.670)

Table 1.—(Continued)

Variable Name	Definition
UPXPB*	Percentage of women aged 15 to 44 with family income equal to or less than 150 percent of the poverty level who were served by organized family planning clinics in fiscal 1971 (UP) multiplied by PB* (μ_w = 639.506; σ_w = 521.843; μ_b = 1,435.559; σ_b = 741.955)
Abor. Rate	Three-year average abortion rate for the period 1970-72 of state in which county is located; legal abortions performed on state residents per 1,000 live births to state residents (μ_w = 96.607; σ_w = 80.497; μ_b = 87.156; σ_b = 77.518)
Abor. Reform	Dichotomous variable that equals one if county is in a state that reformed its abortion law by 1970 (μ_w = 0.369; σ_w = 0.483; μ_b = 0.358; σ_b = 0.480)
IMR 66-68	Three-year average infant mortality rate for the period 1966-68, not race or age specific (μ_w = 21.517; σ_w = 3.553; μ_b = 24.380; σ_b = 3.867)

a--Variable names ending in an asterisk(*) indicate variables that are race specific. The symbols μ_w, σ_w, μ_b, and σ_b denote the white mean, the white standard deviation, the black mean, and the black standard deviation, respectively. The white data pertain to 679 counties, while the black data pertain to 359 counties. Means and standard deviations are weighted by the race-specific number of births in 1971.

b--Variable is available only for whites and nonwhites as opposed to whites and blacks.

Note that there are no direct measures of births to poor women, either at the county or at the national level. Therefore, the estimate of the race-specific percentage of births to such women assumes that the race-specific birth rate of poor women does not vary among counties and that the race-specific birth rate of nonpoor women does not vary among counties. Under these conditions, one can compute race-specific birth rates of poor and nonpoor women by regressing the race-specific birth rate (b_j*, the ratio of births to women ages 15 to 44) on the race-specific fraction of women in poverty (π_j*):

$$b_j^* = \gamma_0^* + \gamma_1^* \pi_j^*. \qquad (7)$$

The regression intercept ($\gamma_0{}^*$) gives the birth rate of nonpoor women, and the sum of $\gamma_0{}^*$ and $\gamma_1{}^*$ gives the birth rate of poor women.[7]

After fitting the regressions for whites and blacks, the race-specific percentage of births to poverty women is estimated as

$$PB^* = 100[(\gamma_0{}^* + \gamma_1{}^*)\pi_j{}^*/(\gamma_0{}^* + \gamma_1\pi_j{}^*)]. \tag{8}$$

It is clear that PB^* is a monotonically increasing, although nonlinear, function of the fraction of the population in poverty. Therefore, the regression coefficient of PB^* summarizes the impact of poverty on infant mortality. Since poverty and family income are highly correlated, the latter is omitted from the regression.[8]

One may question the assumption that the birth rate of poor women is the same in every county, especially since subsidized family planning services and abortion reform are likely to have substantial impacts on birth rates of poor women. The aim of this paper, however, is to estimate reduced form, as opposed to structural, effects of public policies on infant mortality (see note 5). That is, these policies can lower the observed infant mortality rate by lowering the fraction of births in high-risk categories and by lowering the mortality rate associated with a given risk category. Since the aim is to measure both mechanisms, the estimated percentage of births to low-income women, which varies among counties only because the percentage of the population in poverty varies among counties, is a superior variable to the actual percentage of such births, even if the latter were available.[9]

The policy and program measures contain variables pertaining to Medicaid coverage of prenatal and perinatal care services, maternal and infant care projects, the use of organized family planning clinics by low-income women in childbearing ages, and abortion reform. In the case of prenatal and obstetrical care services, variations among states in the treatment of first-time pregnancies under Medicaid contribute to substantial variations in the percentage of pregnant low-income women whose medical care is financed by Medicaid. In particular, nineteen states cover *no* first-time pregnancies because their aid to families with dependent children (AFDC) programs do not cover "unborn children."[10] The treatment of first-time pregnancies of low-income women under Medicaid by the state in which the county is located is described by three dichotomous variables (MN, MU, MA). MN equals one for counties in states that cover first-time pregnancies only if no husband is present. MU equals one for counties in states that provide coverage if no husband is present or if the husband is present but unemployed and not receiving unemployment insurance. MA equals one for counties in states that provide coverage to all financially eligible women, regardless of the presence or employment status of the husband. The omitted category pertains to counties in states that cover *no* first-time pregnancies because their AFDC programs do not cover unborn children.[11]

The measurement of Medicaid is imperfect because its impact on neonatal mortality depends on the percentage of second- and higher-order births covered and on the quantity and quality of services provided per birth. There are no data on these variables. In preliminary regressions the average Medicaid payment per adult recipient in AFDC families in the state in which the county is located was included as a proxy for the quantity and quality of services. This variable had a positive and statistically insignificant effect on neonatal mortality. Its inclusion had only minor impacts on the coefficients of the other variables.

The presence of a maternal and infant care project in a county in 1971 is denoted by the dichotomous variable MI. A second measure of the impact of these projects is given by the number of births in a maternal and infant care project in 1971 as a percentage of the estimated births to low-income women in 1971 (PMIB). Both variables are employed because this program is relatively small; there were only 53 projects in 1971. The presence of a project and the number of births in it were taken from Bureau of Community Health Services (n.d.).

The impact of variations in Federal, state and local subsidization of family planning services is given by the percentage of women ages 15-44 with family incomes equal to or less than 150 percent of the poverty level who were served by organized family planning clinics in fiscal 1971 (UP). These clinics are organized by hospitals, state and local health departments, Planned Parenthood, and other agencies such as neighborhood health centers. This variable was taken from a survey conducted by the National Center for Health Statistics and by the technical assistance division of Planned Parenthood, then known as the Center for Family Planning Program Development and now known as the Alan Guttmacher Institute (Center for Family Planning Program Development, 1974). It excludes family planning services delivered to low-income women by private physicians.

Dryfoos (1976) reports that almost all clients of family planning clinics use the pill or the IUD. Therefore, the percentage of low-income women who are served by these clinics is positively related to the percentage of low-income women who select the pill or the IUD as contraceptive techniques. There is no information on the use of these techniques by other women at the county or state level, but it is known that women with at least a high school education are more likely to use them. Therefore, part of the observed effect of schooling in the regressions reflects the impact of the diffusion of the pill and the IUD on neonatal mortality.

The Medicaid, maternal and infant care projects, and family planning variables are interacted with the race-specific percentage of births to women in poverty. Since PB^* is a percentage rather than a fraction, the regression coefficients must be multiplied by 100 to obtain the vector of impact parameters (α_1) associated with policies aimed at low-income women [see equations (2), (4), or (5)].

The role of abortion reform is measured by a three-year average of the legal abortion rate for the period 1970–72 in the state in which the county is located. The measure is an average of legal abortions performed on state residents per 1,000 live births to state residents and is derived from information reported by the Center for Disease Control (1971, 1972, 1974). It is assumed that abortions performed in the first half of a given year affect the neonatal mortality rate in the second half of that year. The computation also takes account of the extremely low legal abortion rates before the second half of 1970 in states that reformed their abortion laws in 1970. The assumptions required to estimate the abortion rate are somewhat arbitary.[12] Therefore, in some regressions the rate is replaced by a dichotomous variable that identifies counties in states that reformed their abortion laws by the middle of 1970.

The final variable in the regressions is a three-year average of the infant mortality rate for the years 1966–68 (IMR66-68). Theoretically, this is an important variable to include in the analysis because programs such as maternal and infant care projects and subsidized family planning clinics for low-income women were designed to service target populations with poor health indicators. Consequently, estimates of their impacts are biased toward zero if the initial level of the mortality rate is omitted from the regression. In the case of abortion reform and liberal treatment of first-time pregnancies under Medicaid, the exclusion of the lagged mortality rate might overstate their contributions to reductions in neonatal mortality. This is because most of the states that reformed their abortion laws by 1970 and enacted generous Medicaid programs were liberal states with relatively large welfare programs and probably lower than average infant mortality rates. In general, the use of the lagged rate as an independent variable controls for unmeasured determinants of infant mortality that are correlated with the included variables.

Given lags between the enactment of the programs at issue and their implementation and given lags between implementation and impacts on neonatal mortality, IMR66-68 provides an ideal control for the initial level of the mortality rate. Note also that IMR66-68 is superior to the corresponding race-specific neonatal mortality rate because the overall infant mortality rate was used to identify target populations and identifies the size of welfare programs at least as well as a race- and age-specific rate.[13] Note finally, that, to the extent that the programs at issue had an impact on mortality between 1966 and 1968, their effects are understated. Preliminary regressions (not shown) suggest that this bias is minor. When the lagged mortality rate is excluded from the regressions, the impacts of abortion reform and liberal Medicaid coverage rise in absolute value, while the impacts of family planning and the maternal and infant care program decline in absolute value. This is precisely what one would expect if the regressions with IMR66-68 provide an adequate control for the mortality rate in the period prior to the initial impact date of the programs.

EMPIRICAL RESULTS

Ordinary least squares regressions of white neonatal mortality rates are contained in Panel A of Table 2, and ordinary least squares regressions of black neonatal mortality rate are contained in Panel B of Table 2. For whites, the percentage of births to poor mothers has a positive and statistically significant effect on neonatal mortality, while mother's schooling has an insignificant negative effect. For blacks, the negative schooling effect is significant, but somewhat surprisingly, there is an *inverse* relationship between the percentage of births to poor black mothers and the neonatal mortality rate. For both races, the coefficient of physicians per capita is positive and not significant. Moreover, the infant mortality rate for the period 1966 to 1968 performs well as a control for the neonatal mortality rate prior to the initiation of the programs at issue and for unmeasured determinants of mortality (see regressions A1, A3, B1, and B3).

Because the poverty variable has the "wrong" sign for blacks, it is excluded in regressions A2, A4, B2, and B4. The main impact of this alternative specification is to increase the absolute value of the schooling effect for whites and to reduce it for blacks. Since the coefficients of the policy variables do not change much when PB* is omitted and since the estimation of separate poverty and schooling effects "taxes" the black data, we stress the results contained in regressions B2 and B4 in the rest of this paper. For whites, both estimates with and without PB* are used. In part more specifications are used for whites because trends in white neonatal mortality dominate trends in total neonatal mortality. In particular, white births account for approximately 80 percent of all births at the national level.[14]

Table 2 sheds considerable light on the roles of the policy variables in neonatal mortality outcomes. Nineteen of the twenty-eight policy coefficients have the anticipated negative signs in the four white regressions. All fourteen coefficients have the anticipated negative signs in the two relevant black regressions (B2 and B4). The exceptions in the white regressions pertain to the coefficients of the variables that identify liberal coverage of first-time pregnancies under Medicaid (MAXPB*, MUXPB*, MNXPB*). Given the high degree of intercorrelation among the variables in the regression and the imprecise mea-

Table 2.—Ordinary Least Square Regressions of Neonatal Mortality Rates[a]

Independent Variable	Panel A: White Regressions				Panel B: Black Regressions			
	(A1)	(A2)	(A3)	(A4)	(B1)	(B2)	(B3)	(B4)
PB*	.037 (3.00)		.042 (3.45)		-.147 (-4.14)		-.133 (-3.83)	
Physicians	.144 (1.60)	.122 (1.35)	.124 (1.37)	.097 (1.07)	.227 (1.03)	.450 (2.05)	.172 (0.79)	.393 (1.84)
%≥ HS*	-.015 (-1.14)	-.036 (-3.13)	-.013 (-0.96)	-.037 (3.22)	-.124 (-2.93)	-.017 (-0.49)	-.137 (-3.31)	-.035 (-1.08)
MAXPB*	.004 (0.39)	.016 (1.83)	-.003 (-0.39)	.008 (1.00)	.0004 (0.00)	-.014 (-0.53)	-.007 (-0.31)	-.010 (-0.46)
MUXPB*	.003 (0.29)	.010 (1.03)	.004 (0.44)	.012 (1.24)	-.038 (-1.78)	-.033 (-1.51)	-.041 (-1.97)	-.034 (-1.61)
MNXPB*	-.006 (-0.67)	.001 (0.13)	-.002 (-0.21)	.007 (0.77)	-.010 (-0.75)	-.032 (-2.47)	-.010 (-0.73)	(-.030) (-2.32)
MIXPB*	-.005 (-0.36)	-.011 (-0.87)	-.008 (-0.67)	-.017 (-1.37)	-.007 (-0.30)	-.003 (-0.15)	-.007 (-0.34)	-.005 (-0.20)
PMIBXPB*	-.022 (-1.06)	-.020 (-0.98)	-.015 (-0.76)	-.011 (-0.56)	-.033 (-1.19)	-.032 (-1.13)	-.037 (-1.35)	-.036 (-1.31)
UPXPB*	-.001 (-2.99)	-.0003 (-1.94)	-.001 (-2.80)	-.0003 (-1.58)	-.0003 (-0.86)	-.001 (-2.37)	-.0001 (-0.34)	-.001 (-1.76)
Abor. Rate	-.004 (-3.25)	-.005 (-3.91)			-.009 (-2.25)	-.007 (-1.58)		
Abor. Reform			-.549 (-3.43)	-.592 (-3.69)			-1.751 (-3.89)	-1.773 (-3.86)
IMR 66-68	.274 (12.34)	.280 (12.53)	.281 (12.73)	.288 (13.04)	.260 (3.98)	.240 (3.61)	.235 (3.65)	.217 (3.31)
CONSTANT	7.554	9.400	7.045	9.094	27.184	17.998	27.618	19.238
$\bar{R}^2$	.315	.307	.317	.305	.125	.084	.149	.116
F	29.38	31.05	29.54	30.80	5.64	4.30	6.70	5.68

t-ratios in parentheses. The critical t-ratio at the 5 percent level of significance is 1.64 for a one-tailed test. The eight F-ratios are significant at the 1 percent level.

sures used, the preponderance of negative effects is an important and impressive finding.

In terms of statistical significance, the hypothesis that no member of the set of policy variables has a non-zero effect on neonatal mortality always is rejected at the 1 percent level. With respect to the four specific policies, in general abortion and the use of subsidized family planning services by low-income women have significant impacts, while Medicaid and maternal and infant care projects do not.[15] Specifically, for whites the abortion rate achieves significance at all conventional levels in regressions A1 and A2. A similar comment applies to the dichotomous variable that denotes abortion reform by the middle of 1970 in regressions A3 and A4. For blacks, abortion reform is significant at all levels in regression B4, while the abortion rate is significant at the 6 percent level, but not at the 5 percent level, in regression B2. For whites, the interaction between the percentage of low-income women who use organized family planning clinics and the percentage of births to low-income women (UPXPB*) is significant at the 5 percent level in the first three regressions and at the 6 percent level in the fourth. For blacks, UPXPB* is significant at the 5 percent level in both regressions.

The significance of the abortion rate is notable because this variable is neither race- or county-specific and must be computed subject to a number of somewhat arbitrary assumptions (see note 12). Therefore, it is probably subject to considerable measurement error, which biases its coefficient toward zero. The sizable and significant impacts of the dichotomous abortion reform variable strengthens confidence in the estimated coefficients of the abortion rate and confirm that the effect for blacks is larger in absolute value than that for whites.

To examine the relative contributions of schooling, poverty, and the public programs to the recent U.S. neonatal mortality experience, we apply the coefficients of regressions A1, A2, and B2 to trends in the exogenous variables between 1964 and 1977. The results of estimating the implied changes in neonatal mortality rates due to selected factors for the period 1964–77 and for the subperiods 1964–71 and 1971–77 and given in Table 3.[16] Results for whites and nonwhites are shown because separate time series for blacks are not available.

Since there is little trend in the percentage of families in poverty after 1971 and since the definition of poverty was altered beginning in 1975, the estimates in Table 3 assume no change in poverty or in PB* between 1971 and 1977. In these computations the national levels of the two maternal and infant care project measures are zero in 1964 and do not change from 1971 to 1977. The three Medicaid measures are treated in the same manner. This treatment is justified because there were few of these projects in operation prior to 1967 and almost no trend in the number of projects or the total number of births in projects after 1971 (Bureau of Community Health Services, n.d.). The Medicaid program was not enacted until July 1965, and the rules governing coverage of first-time pregnancies under Medicaid did not vary between 1971 and 1977.

Our treatment of Medicaid is somewhat controversial because the percentage of Medicaid-financed births to poor women and the real quantity of medical services per birth may have risen between 1971 and 1977. Although definitive evidence on these matters is lacking, a number of observations can be made. Much of the observed decline over time in the relationship between income and physician visits, which Davis and Reynolds (1976) show was caused by Medicaid, occurred by 1971. The percentage of the poverty population that received Medicaid benefits rose by only 6 percentage points between 1970 and 1974 (Davis and Reynolds, 1976; Davis and Schoen, 1978). Real Medicaid benefits per recipient show no trend between 1971 and 1977 (Davis and Schoen, 1978). The percentage of black mothers who started their prenatal care in the first trimester of pregnancy rose betwen 1969 and 1975 (Taffel, 1978). Except for the last observation, this evidence justified our treatment of Medicaid. We do, however, examine the sensitivity of the results to an alternative assumption described below.

As shown in Table 3, the actual decline in the white neonatal mortality rate

Table 3.—Contribution of Selected Factors to Reductions in Neonatal Mortality Rates, 1964–1977

	Panel A: Whites						Panel B: Nonwhites		
	1964 - 1977		1964 - 1971		1971 - 1977		1964-1977	1964-1971	1971-1977
Observed reduction in neonatal mortality rate (deaths per thousand live births)	7.5		3.2		4.3		11.8	6.9	4.9
Annually compounded percentage rate of decline in neonatal mortality rate	4.9		3.2		6.9		4.6	4.4	4.9
Contribution of selected factors to observed reduction in neonatal mortality rate	Reg.A1	Reg.A2	Reg.A1	Reg.A2	Reg.A1	Reg.A2	Reg.B2	Reg.B2	Reg.B2
Physicians	a	a	a	a	a	a	-0.2	-0.1	-0.1
Est. poverty births (PB*)	0.4	b	0.4	b	a	c	b	b	b
%≥High School	0.2	0.6	0.1	0.3	0.1	0.3	0.3	0.1	0.2
Abortion rate	1.5	1.7	0.4	0.4	1.1	1.3	2.5	0.6	1.9
UPXPB*	0.6	0.2	0.3	0.1	0.3	0.1	1.4	0.8	0.6
M and I Projects[d]	0.1	0.1	0.1	0.1	a	c	0.3	0.3	c
Medicaid[e]	a	-0.2	a	-0.2	a	c	0.5	0.5	c
Total explained reduction	2.8	2.4	1.3	0.7	1.5	1.7	4.8	2.2	2.6
Percentage explained	37.3	32.0	40.6	24.7	34.9	39.5	40.7	31.9	53.1

a--Less than .1 in absolute value.

b--Variable omitted from regression.

c--No change in variable.

d--Combined contribution of MIXPB* and PMIBXPB*.

e--Combined contribution of MAXPB*, MUXPB*, and MNXPB*.

between 1964 and 1977 was 7.5 deaths per thousand live births. Regression A1, which incorporates separate poverty and schooling effects, "explains" 2.8 of these deaths or 37 percent of the total reduction. Regression A2, which treats the schooling effects as the joint impact of schooling and poverty, acounts for 2.4 deaths or 32 percent of the total reduction. For nonwhites, the neonatal mortality rate fell by 11.8 deaths per thousand live births between 1964 and 1977. Regression B2 predicts a decline of 4.8 deaths or 41 percent of the observed reduction.

A striking message in Table 3 is that the increase in the legal abortion rate is the single most important factor in reductions in both white and nonwhite neonatal mortality rates. Not only does the growth in abortion dominate the other policies, but it also dominates schooling and poverty.[17] For the entire period, the reduction in the white neonatal mortality rate due to abortion ranges from 1.5 to 1.7 deaths per thousand births. The comparable figure for nonwhites is a whopping 2.5 deaths per thousand births. When the two subperiods are examined separately, abortion makes the largest contribution except for nonwhites in the 1964–71 period. Here it ranks second to the impact of the rise in the use of organized family planning services by low-income women. The extremely large expansion in the abortion rate in the latter period (1971–77) provides a cogent explanation of the acceleration in the percentage rates of decline in both race-specific mortality rates and the acceleration in the absolute rate of change for whites.

The increase in the use of organized family planning services by low-income women is the second-most important factor in reductions in nonwhite neonatal mortality for the entire period (1.4 deaths per thousand live births) and the most important factor in 1964–71 (0.8 deaths per thousand live births). For whites, the estimate of the contribution of family planning is sensitive to the inclusion in or exclusion from the regression of the percentage of births to poor women. When PB* is included, it dominates all the other factors except for abortion in the entire period and in the two subperiods. Its effect is weaker when PB* is omitted and is no larger than the impact of maternal and infant care projects in the earlier subperiod.

There is reason to believe that we understate the impact of the use of all family planning services as opposed to organized services by low-income women. This is because our measure excludes services delivered by private physicians. National trends in the percentage of low-income women serviced by private physicians contained in Family Planning Program Development (1974), Dryfoos (1976), and Cutright and Jaffe (1977) suggest that the estimates in Table 3 should be multiplied by a factor of 1.6. This adjustment makes family planning a more important contributor to neonatal death rate reductions than maternal and infant care projects in the computations based on regression A2. It suggests that the predicted reductions of 1.5 nonwhites deaths per thousand births and between 0.2 and 0.6 white deaths per thousand births due to family planning are conservative lower-bound estimates of the true impact.

Maternal and infant care projects have small impacts on white neonatal mortality regardless of the regression specification employed. For nonwhites the effect is somewhat more substantial; it amounts to a decline of 0.3 deaths per thousand births for the years during which the projects were expanding. Of course the impact of these projects over the entire period is dominated by the impacts of abortion reform and family planning in part because there was no change in the size of these projects between 1971 and 1977. But suppose that the absolute increase in the size of these projects had been the same in the second subperiod as it was in the first. Then their predicted impact on the nonwhite neonatal death rate would amount to 0.6

deaths per thousand births, which still is substantially smaller than the abortion and family planning effects.

Medicaid can be dismissed as a cause of the decline in white neonatal mortality; it predicts either no change or an increase in the white death rate. In the case of nonwhites, Medicaid accounts for a reduction of 0.5 deaths per thousand live births. If the somewhat controversial assumption of no change in the program between 1971 and 1977 is relaxed in the same manner as for maternal and infant care projects, we obtain a reduction of 1.0 deaths per thousand births. This is greater than the reduction associated with maternal and infant care projects but smaller than the reductions associated with abortion reform and family planning.

To summarize, these results, when combined with information on the use of the pill and the IUD by women of all income classes, provide a coherent explanation of the U.S. neonatal experience from 1964 to 1977. After a period of relative stability, the neonatal mortality rate began to decline following 1964 as a lagged response to the extremely rapid increase in the percentage of women who used the pill and the IUD between 1961 and 1964.[18] The decline was further fueled by the increase in the percentage of low-income women who used subsidized family planning services between 1965 and 1971 and by the dramatic rise in the legal abortion rate between 1969 and 1971. The acceleration in the rate of decline in the mortality rate between 1971 and 1977 was due primarily to the literal explosion of the abortion rate in that period. These conclusions are subject to the qualification that we have no estimates of the impact of the pill and the IUD other than those inferred through the use of family planning services by low-income women. They also are subject to the qualification that we cannot estimate the contribution of advances in neonatology.

The above findings do not necessarily imply that increases in the quantity of medical care played an unimportant role in the downward trend in neonatal mortality. To be sure, the impacts of Medicaid and maternal and infant care projects are smaller than the impacts of abortion reform and family planning. But as indicated previously, this simply may mean that the quantity of medical care per birth is more responsive to a reduction in the cost of fertility control than to a reduction in the price of care.

These results with respect to the importance of the legalization of abortion in trends in infant mortality differ from those of Bauman and Anderson (1980). Using states of the United States as the units of observation, they find no relationship between changes in the legal abortion rate and changes in the fetal or infant mortality rate. Bauman and Anderson's findings differ from ours for a number of reasons. First, they do not control for other determinants of infant mortality. Second, they do not use race-specific mortality data. Third, they do not examine the impacts of abortion reform on neonatal mortality.

These results are relevant to current U.S. policy debates with respect to the financing of abortions under Medicaid and with respect to attempts by the Right to Life movement to enact a constitutional amendment that would outlaw abortion except when it is necessary to preserve a pregnant women's life. Under the Hyde Amendment, which was in effect from June 1977 until February 1980, Federal funding of abortions under Medicaid was banned except in cases where the woman's life was in danger. During that period, 28 states refused to pay for "medically necessary" abortions. The other 22 states continued to finance most abortions for Medicaid-eligible women by paying the Federal share as well as the state share. As a result, the number of Federally financed abortions declined from approximately 250,000 per year before 1976 to less than 3,000 in 1978 (Trussell et al., 1980). Federal funding of abortions resumed temporarily in February 1980, pending a review by the

U.S. Supreme Court of a ruling by Federal District Judge John F. Dooling Jr. that declared the Hyde Amendment unconstitutional. In June 1980 the Supreme Court reversed Judge Dooling's decision and upheld the constitutionality of the Hyde Amendment.

In spite of the Hyde Amendment, the abortion rate continued to rise between 1977 and 1978. In part, this trend reflects the continued diffusion of a relatively new method of birth control. In part, it reflects a substitution of private for Federal funds by roughly 80 percent of women who would have been eligible for Federal financing in the absence of the amendment (Trussell et al., 1980). One can speculate, however, that the abortion rate would have risen at a more rapid rate between 1977 and 1978 in the absence of the Hyde Amendment. Given the recent Supreme Court ruling, the abortion rate for poor women probably will grow slower than otherwise and might even fall. According to these findings, this will retard the rate of decline in the neonatal mortality rate of the poor.

Taken at face value, the most striking implication of this study pertains to a constitutional ban on abortions. The current U.S. abortion rate is 400 abortions per thousand live births, while the rate in 1969 was 4 abortions per thousand live births. If a ban reduced the rate to its 1969 level, these regressions predict that the nonwhite neonatal mortality rate would rise by approximately 2.8 deaths per thousand live births or by 19 percent above its 1977 level. The white neonatal mortality rate would rise by approximately 1.8 deaths per thousand live births or by 21 percent above its 1977 level. Yet these estimates must be regarded with caution because they assume that all other factors would remain the same if a ban were enacted. In particular, to the extent that abortion is a substitute for more conventional methods of birth control, the use of these methods would not remain the same.

NOTES

[1] The above computations are based on data contained in U.S. Bureau of the Census (1980).

[2] Descriptive and historical information concerning the programs at issue is available in the expanded version of this paper (available on request), and details on abortion reform are provided below. Briefly, Medicaid, enacted in 1965 as Title XIX of the Social Security Act of 1935, is the joint Federal-state program to finance the medical care services of low-income families who are covered by the aid to families with dependent children (AFDC) program. Maternal and infant care projects originated in the 1963 amendment to Title V of the Social Security Act. The amendment provides special grants for projects designed to provide adequate prenatal and obstetrical care to reduce the incidence of mental retardation and other conditions caused by childbearing complications as well as to lower infant and maternal mortality. Federal subsidization of family planning services for low-income women originated in the 1967 amendments to the Social Security Act. Federal efforts in this area were expanded by the Family Planning Services and Population Research Act of 1970 and by the 1972 amendments to the Social Security Act. These subsidies go to family planning clinics organized by hospitals, state and local health departments, Planned Parenthood, and other agencies such as maternal and infant care projects and neighborhood health centers. The diffusion of the pill and the IUD did not result from actions by the Federal government or by states. This development is important for this research, however, because it meant that an extremely effective method of birth control could be offered to low-income women by Federally subsidized family planning clinics.

[3] The indirect costs of obtaining a good are generated by the time spent traveling, waiting, and obtaining information about the good. The terms indirect costs and availability are used here as synonyms.

[4] Eugene Lewit has emphasized to us that theoretically the direction of the effects of abortion on fertility and infant mortality may be indeterminant. For instance, abortion may substitute for other methods of birth control. Moreover, abortion reform may cause the birth rate to rise by increasing the level of sexual activity in general. In spite of these factors, we feel that the hypothesis that abortion reform lowers the infant mortality rates is very plausible. In part this is because we control for the use of family planning services in the regression analysis.

[5] If abortion reform lowers infant mortality solely by reducing the fraction of high-risk births, a measure of reform such as the legal abortion rate should have no impact on infant mortality in a multiple regression that controls for the percentage of low-birth weight births. This is not the case if the

medical care mechanism outlined above also is relevant. Since there is more than one mechanism via which abortion reform and the other policies can affect infant mortality and since the aim of this paper is to estimate reduced form, as opposed to structural, effects, we omit regressors such as the percentage of low-birth weight births. Another reason for adopting this strategy is that some policy variables may have differential and possibly larger impacts on death rates in high-risk categories. Therefore, a study of the mechanisms via which government policies affect infant mortality should pay careful attention to complicated interactions between the policies and the fraction of high-risk births. Such a study is important, but it is beyond the scope of this paper.

[6] In preliminary regressions, the coefficient of the number of hospital beds per capita was insignificant, and its inclusion had only minor impacts on the coefficients of the other independent variables.

[7]The regression equation for whites is

$$b_j^* = .064 + .169\pi_j^*,\ \bar{R}^2 = .269,\ n = 679.$$
$$(t = 15.90)$$

The regression equation for blacks is

$$b_j^* = .095 + .059\pi_j^*,\ \bar{R}^2 = .118,\ n = 359.$$
$$(t = 6.98)$$

In each regression, the dependent variable is a three-year average of the birth rate for the period 1969–71. The regressions are weighted by the square root of the race-specific number of women ages 15–44 in 1970. The poverty variable pertains to the fraction of families below the poverty level, rather than to the fraction of women ages 15–44. The latter variable is not available on a race-specific basis. Another reason for the use of the fraction of families in poverty is that it facilitates the trend analysis in the third section of this paper. The ratios of births per thousand women ages 15–44 implied by the regressions are 233 for poor whites, 64 for nonpoor whites, 154 for poor blacks, and 95 for nonpoor blacks.

[8]In regressions not shown in the third section, median family income was included as an independent variable. Its coefficient was not significant.

[9]From equation (5), the reduced form effect of x_{pj} on d_j is

$$\frac{\partial d_j}{\partial x_{pj}} = (\alpha_0 - \beta_0)\frac{\partial k_j}{\partial x_{pj}} + \alpha_1 k_j + \alpha_1 x_{pj}\frac{\partial k_j}{\partial x_{pj}}.$$

Note that

$$k_{jp}^* = \pi_j^* b_{jp}^* / b_j^*,$$

where b_{jp}^* is the race-specific birth rate of poor women in the j^{th} county. Clearly, this variable is not held constant in our regressions. Note that reduced form effects also could be estimated by expressing k_j as a function of a set of variables, including the policy measures, in equation (5). This results in an extremely complicated functional form. Specifically, it includes the level of each policy measure, the square of that measure, and its product with each of the other measures. Such an equation is not tractable from the standpoint of estimation.

[10]This list of states includes Arizona which has no Medicaid program.

[11]Our information on the treatment of first-time pregnancies under Medicaid by specific state was obtained from Letty Wunglueck of the Health Care Financing Administration. Note that first-time pregnancies of young mothers who are themselves dependents in AFDC families would be covered under Medicaid in spite of the above provisions. States in one of our three categories, however, cover a larger percentage of first-time pregnancies than other states.

[12]Suppose that the neonatal mortality rate (nm_{jt}) and the legal abortion rate (a_{jt}) are measured in half-year intervals. Let the relationship between the two be

$$nm_{jt} = \beta + \delta a_{jt-1}.$$

Aggregate and average this equation over three years (six half years) to obtain

$$\overline{nm}_j = \beta + \delta\bar{a}_j,$$

where

$$\bar{a}_j = \left(\sum_{t=0}^{5} a_{jt-1}\right)/6.$$

The neonatal mortality rates pertain to the period from the first half of 1970 (70-1) to the last half of 1972 (72-2). Therefore ignore the county subscript, and write $\bar{a}$ as

$$\bar{a} = (a69\text{-}2 + a70\text{-}1 + a70\text{-}2 + a71\text{-}1 + a71\text{-}2 + a72\text{-}1)/6.$$

We have data for $a70$-2, $a71$ (the abortion rate during the entire year of 1971), and $a72$. For states that reformed their abortion laws before 1970, we assume that $a69\text{-}2 + a70\text{-}1 = a70\text{-}2$ due to the rapid upward trend in the abortion rate during this period. We also assume that the birth rate in the first half of 1971 equaled the birth rate in the second half of 1971, so that $a71\text{-}1 + a71\text{-}2 = 2a71$. Finally, we assume $a72\text{-}1 = a72$. Hence for these states

$$\bar{a} = (1/3)\,(a70\text{-}2) + (1/3)\,(a71) + (1/6)\,(a72).$$

For states that reformed their laws in the middle of

1970, we assume a69-2 = a70-1 = 0. Hence, given the other two conditions used above,

$$\bar{a} = (1/6)\,(a70\text{-}2) + (1/3)\,(a71) + (1/6)\,(a72).$$

Since the law for New York State had no residency requirements, states near New York are treated in the same manner as New York in the computation of $\bar{a}$.

[13]Age- and race-specific infant mortality rates for years prior to 1969 are not available on the Area Resource File.

[14]Space limitations prevent the discussion of the effects of poverty, schooling, and physicians in detail and the presentation of additional specifications of the basic regressions. Note the following:

(a) The variables PB* and HSP* are highly correlated for whites (r = −.6) and for blacks (r = −.8). The insignificant regression coefficients of HSP* in regressions A1 and A3 are due in part to multicollinearity. This phenomenon may also contribute to the black results, although the explanation is somewhat more complicated because the simple correlation between the death rate and PB* is negative.

(b) There are few studies of the race-specific impact of poverty on infant mortality. Using a special sample of births and subsequent infant deaths taken by the National Center for Health Statistics, Gortmaker (1979) reports results similar to ours. White babies are more likely to die in poverty families than in nonpoverty families, but this relationship does not hold for black babies.

(c) The unimportance of physicians per capita in our regressions mirrors findings reported by Brooks (1978) in a study of variations in infant mortality rates among SMSAs. The coefficients of other variables are not sensitive to the exclusion of MD. The MD variable is retained because there is almost no trend in it between 1964 and 1977. Hence its retention does not cloud the forecasts and backcasts that follow.

[15]For Medicaid, we always accept the hypothesis that no member of the set given by MAXPB*, MUXPB*, and MNXPB* has a non-zero coefficient at the 5 percent level. For maternal and infant care projects, we accept the hypothesis that no member of the set given by MIXPB* and PMIBXPB* has a non-zero coefficient in five of six cases. The exception pertains to regression A4.

[16]Note that changes in the lagged mortality rate are not relevant in the forecasts and backcasts in Table 3 because the underlying model is not a dynamic one. Rather, the lagged rate serves as a proxy for the initial level, which does not change by definition. In econometric terminology the model is one with "fixed effects" rather than one with "state dependence."

[17]One might argue that we understate the impacts of schooling and poverty by holding constant an average infant mortality rate centered on the year 1967. Although it is reasonable to suppose that changes in the public policies had no impacts until after 1967, this assumption may not be reasonable in the cases of schooling and poverty. This is because the trends in these variables were continuous from 1960 to 1970. To examine the robustness of our conclusion that abortion dominates schooling and poverty, we reestimated the contributions of these variables from regressions that exclude the lagged mortality rate. Although the contribution of schooling rises relative to the contribution in Table 3, it is still smaller than that of abortion. Note that if county-level fixed effects that lower mortality are positively correlated with schooling, we overstate the schooling coefficient by excluding the lagged mortality rate.

[18]Ryder (1972) reports that in 1961 the percentage of married women under age 35 who used the pill stood at approximately 3 percent. By 1964, it had increased to approximately 16 percent.

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