

ECONOMETRIC ANALYSES OF U.S. ABORTION POLICY: A CRITICAL REVIEW

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INTRODUCTION

Few social issues in the U.S. are as contentious as the legal status of induced abortion. Thirty years after the Supreme Court declared state laws restricting abortion unconstitutional in *Roe v. Wade*, 93 S.Ct. 705, poll results suggest that the U.S. population is almost evenly split over whether the next nominee to the Supreme Court should support or oppose legal abortion in most or all contexts.¹ The salience of the issue is arguably among the highest of all national issues.²

The motivation behind many individuals' positions regarding abortion policy hinges on normative judgments. That is, many people arrive at their abortion position based on some moral decision about the relative rights of the mother and the fetus.³ However, there is presumably a consequentialist component to the abortion question that is largely ignored in discussions of public opinion about abortion.⁴ This consequentialist component, however, has received the bulk of attention in the social science literature regarding abortion policy.

The social science literature on the effects of abortion policy has grown tremendously during the last decade. While public health and demographic scholars had consistently examined the effects of changes in abortion policy even before *Roe*,⁵ there has been an explosion of research on the subject in

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¹ An ABC News poll conducted June 11-15, 2003 found that 50 percent of 1,029 adults favored a nominee supporting abortion rights in all or most circumstances, with 43 percent favoring a nominee who opposes abortion in all or most cases (http://abcnews.go.com/sections/politics/2020/scotusabortion030619_poll.html . Viewed December 29, 2003 and printout on file with law review). Nearly three quarters of all respondents indicated that they wanted nominees to disclose their position on abortion rights during the confirmation process.

² Table 29 in Everett Carl Ladd and Karlyn H. Bowman, *Public Opinion About Abortion*, 2d ed. (Washington, D.C.: AEI Press, 1999) [hereafter Ladd and Bowman] suggests that abortion was the decisive issue for between 13 and 18 percent of voters in 1992 and 1996.

³ Ladd and Bowman provide indirect evidence of this by presenting polling results addressing the question of whether abortion constitutes murder (Tables 1 and 2) and questions regarding a woman's right to make decisions about whether to obtain an abortion (Tables 5-8).

⁴ Ladd and Bowman's Table 4 touches on this issue in that it presents poll results from a question concerning whether or not the good effects of abortion outweigh the bad effects of abortion. However, this general question does not explicitly exclude the rights component from the welfare analysis of abortion policy. That is, it is unclear whether individuals are balancing the values of protecting the value of choice for a woman and the value of the fetus's rights against more specific and tangible costs and benefits of abortion policy such as potential reductions in welfare payments, crime, etc.

⁵ See, for example, Bonnie Dauber, Marianne Zalar, and Phillip J. Goldstein (1972), "Abortion Counseling

economics journals in recent years. While a few of these articles are primarily theory based,⁶ the lion's share of the increase in attention devoted to this subject has come in the form of econometric or statistical analyses of changes in abortion policy.

In many ways, changes in abortion policy (especially legalization) present empirical researchers with an ideal mechanism through which to identify causal relationships between changes in incentives relating to sexual behavior and a host of demographic and public health outcomes. The costs and benefits of sexual behavior are subjective and are likely to be correlated with many observable and unobservable individual characteristics. This makes drawing causal inferences about the sensitivity of sexual behavior to incentives difficult. Even if data are available on the costs and benefits of sexual behavior accruing to an individual, a fairly heroic assumption in most contexts, any observed correlations between behavior (or outcomes) and changes in those costs and benefits might very well be artifacts of omitted variables biases.⁷ However, changes in abortion policy generally affect costs and benefits of sexual behavior in known and unambiguous ways. Further, since the changes are generally the result of legislative action or judicial fiat, they are likely to be exogenous. That is, the changes will be orthogonal or unrelated to an individual's characteristics, obviating the concern that the changes in costs and benefits will be correlated with important variables that are omitted in the econometric analysis.⁸

and Behavioral Change," *Family Planning Perspectives*, 4(2): 23-27; Robert G. Potter (1972), "Additional Births Averted when Abortion Is Added to Contraception," *Studies in Family Planning*, 3(4): 53-59; Jean Pakter and Frieda Nelson (1971), "Abortion in New York City: The First Nine Months," *Family Planning Perspectives*, 3(3): 5-12.

⁶ For example, George A. Akerlof, Janet L. Yellen, and Michael L. Katz (1996), "An Analysis of Out-of-Wedlock Childbearing in the United States," *Quarterly Journal of Economics*, 111(2): 277-317 provide a model in which the availability of abortion and birth control lowers the incidence of "shotgun" marriages, systematically disadvantaging women who choose not to use birth control or abortion services. This effect is generated by the fact that such women will be placed at a competitive disadvantage in the mating market relative to women who will avail themselves of birth control and abortion. This competitive disadvantage leads to increases in female poverty as women who reject birth control and abortion must make themselves more available sexually to compete in the mating market without any promise of marriage in the event of an unplanned pregnancy. While the authors do provide some historical evidence to support their model, there is no rigorous econometric testing involved.

⁷ Formally, an omitted variables bias occurs when the true statistical model that describes variable y takes the form $y = \beta X$ where X is a vector of variables that influence the value of y , however the estimated relationship omits one or more of the variables that comprise X and those omitted variables are correlated with one or more of the variables included in the estimation. Effectively, the estimated coefficients for the included explanatory variables will be biased if the variables are correlated with the omitted variables because the estimated coefficient will include some of the effect of the omitted variable on the y variable. On this point, see William H. Greene, *Econometric Analysis*, 4th ed. (Upper Saddle River, NJ: Prentice Hall, Inc., 2000), pp. 334-337.

⁸ The reasons for omitting important variables in a statistical analysis can take a number of forms. One possibility is that it is not obvious *a priori* that a particular variable should be important in analyzing the determinants of the y variable because theory does not provide sufficient guidance or previous empirical analyses have not identified the importance of the omitted variable. Another possibility is that data for the omitted variable have not been collected in sufficient detail. A third (and perhaps the most important) possibility is that the omitted variable is inherently unquantifiable or is unobservable even though it is an important determinant of the y variable under study.

Econometric researchers have exploited this attractive aspect of changes in abortion policy to examine numerous important social and demographic relationships, including the effect of incentives on sexual activity, the effect of limiting unwanted births on welfare payments, crime rates, and women's educational attainment, as well as the direct effect of changes in abortion policy on abortion and fertility decisions.

Setting aside the rights-based arguments for and against abortion availability, these positive analyses have large implications for the evaluation of abortion policy. A sophisticated understanding of what econometric research tells us about the effects of abortion policy could greatly improve social welfare by informing policymakers about the consequences of their actions. Unfortunately, the econometric literature on this topic is technical, placing it beyond the ken of most policymakers. Further, the popularizations of the research in this field that have been provided by the media in some situations have been influenced by the various interest groups who have a vested interest in seeing abortion policy swayed in one direction or another. Both of these concerns create a troubling disconnect between scholarly research and policy in this area.

In this article, I will attempt to describe the econometric research on abortion policy in a manner that is accessible to someone without formal training in econometrics, highlighting the policy implications of the research as well as any serious methodological shortcomings, which might limit the value of any particular piece of scholarship. In many ways, out of necessity, this review will not be exhaustive. I will focus primarily on research using U.S. policy changes and data. This is not meant to suggest that there is no quality research examining the experiences of other countries. There is, in fact, a large literature looking at foreign experiences.⁹ However, if the primary goal of this article is to inform U.S. policy, it will generally be most useful to examine U.S. sources. I have also largely ignored the interesting empirical literature examining the determinants of abortion positions held by voters and politicians.¹⁰ In the interest of relative brevity, I decided that these studies are not directly relevant to evaluating U.S. abortion policy.¹¹

⁹ In fact, some of the earliest literature focuses on other countries due to greater abortion availability. See, for example, Thos. Vibert Pearce (1930), "An Unusual Frequency Distribution – The Term of Abortion," *Biometrika*, 22(1/2): 250-252; Christopher Tietze (1964), "The Demographic Significance of Legal Abortion in Eastern Europe," *Demography*, 1(1): 1191-25; Michael S. Teitelbaum (1972), "Fertility Effects of the Abolition of Legal Abortion in Romania," *Population Studies*, 26(3): 405-417.

¹⁰ For example, see Stephen F. Gohmann and Robert L. Ohsfeldt (1994), "Voting in the U.S. House on Abortion Funding Issues: The Role of Constituents' and Legislators' Ideology, Before and After the Webster Decision," *American Journal of Economics and Sociology*, 53(4): 455-474; George A. Chressanthis, Kathie S. Gilbert, and Paul W. Grimes (1991), "Ideology, Constituent Interests, and Senatorial Voting: The Case of Abortion," *Social Science Quarterly*, 72(3): 588-600.

¹¹ However, it should be noted that these studies might contain useful information to be exploited in situations where the orthogonality of abortion policy is less clear. That is, if state abortion policies are systematically related to variables that also influence the particular y variable being studied, omitted variable bias

In terms of organization, I have divided this review into categories based upon the dependent variables examined in each of the econometric studies. Broadly speaking, the majority of studies can be categorized as looking at outcomes in the following areas: sexual behavior, crime, opportunities for women, and public finance effects. I have chosen to exclude the very large literature examining the effects of abortion policy on demographic patterns. This omission does not reflect any lack of interest in this literature. Covering the research in this area even superficially would more than double the length of this article. Further, a book covering this material in great detail has been written by one of the foremost researchers in this area, Phillip Levine, and will be published later in 2004.¹² Given that Levine discusses the existing research in the non-technical tone to which I aspire in this article, anything I would add by way of comment on this research would be largely superfluous.

Another organizational device that I chose not to employ is differentiating between studies focusing on abortion legalization and studies focusing on subsequent policy changes. While there is a good deal of heterogeneity in the policy variation that econometric studies exploit, as a theoretical matter, all of the changes boil down to changes in access to abortion. Put a different way, policy changes can be seen as changes in the effective cost or price of abortion.

Legalization simply lowers the cost of obtaining an abortion, since it removes any penalties that the state can impose on those seeking or providing abortions; it also reduces the search costs entailed in finding an abortion provider¹³ and, more than likely, it makes abortion procedures safer.¹⁴ Similarly, increased public funding for abortions through, for example, Medicaid lowers the cost of abortion. Parental notice requirements, mandatory waiting periods, requirements that information about abortion alternatives be provided to individuals seeking an abortion, and the like all serve to increase the effective cost of obtaining an abortion, while restrictions on the activities of anti-abortion protestors lower the cost of abortion.¹⁵

could limit the value of a study's results. If, however, we better understand the determinants of abortion policy, we can better evaluate the orthogonality assumption, and, in cases where it is not valid, we might be able to identify exogenous instruments (i.e., variables that affect abortion policy but do not directly affect our y variable) that can be used to induce orthogonality.

¹² Phillip B. Levine, *Sex and Consequences: Abortion, Policy, and the Economics of Fertility* (Princeton, NJ: Princeton University Press, 2004).

¹³ Richard A. Posner, *Sex and Reason* (1997) at 277.

¹⁴ Christopher Tietze, *Induced Abortion: 1979* (3d ed. 1979) at 86 estimates that between 1963 and 1968, when most abortions were performed illegally, the fatality rate was 72 out of every 100,000 abortions performed. In 1976, after national legalization, the rate had dropped to 0.8 per 100,000.

¹⁵ No doubt, many of these costs are psychic as opposed to financial, but that distinction makes little difference at the analytical level.

I. THE EFFECT OF ABORTION ACCESS ON SEXUAL BEHAVIOR

The link between abortion access and sexual behavior is fairly straightforward. The risk of an unwanted pregnancy represents a cost of risky sexual activity. If individuals make their choices about sex rationally, as the marginal cost of engaging in sex decreases, we would expect individuals to have more sex. As access to abortion becomes cheaper, the expected cost of an unwanted pregnancy decreases, since abortion can be viewed as *ex post* birth control or insurance against an unwanted pregnancy, implying that individuals will engage in more risky sex.¹⁶

The rational basis of decisions about sex goes largely unquestioned in the economics literature, though that is not true in other social sciences.¹⁷ Richard Posner provides a good overview of the costs and benefits considered by individuals when making decisions about sexual activities.¹⁸ Levine reviews some of the evidence for the rational choice model of (teenage) sexual behavior, contrasting it with the more spontaneous or irrational conceptions of sex that are prominent in other fields.¹⁹ Using state level data, he shows that variations in cost significantly affect the decision to engage in sexual activities, as well as the decision to use preventive birth control measures.

Formally speaking, if an individual's utility is a positive function of the quantity of sex consumed and the cost of sex is increasing in the likelihood that it will result in an unwanted pregnancy, individuals will consume sex up to the point where the marginal benefit of sex equals the marginal cost of sex. As long as there is decreasing marginal utility of sex, a decrease in the likelihood of an unwanted birth will induce individuals to have more sex.²⁰

Testing the empirical validity of this hypothesis, however, is not easy. Data on sexual activity suffer from many problems. The earliest large-scale dataset on sexual activity, at least for the U.S., was collected by Alfred Kinsey through his Institute for Sex Research, which was founded in 1947. Though the publication of Kinsey's *Sexual Behavior in the Human Male*²¹

¹⁶ On this point, see Phillip B. Levine and Douglas Staiger (2002) "Abortion as Insurance," NBER Working Paper w8813.

¹⁷ For example, Janet B. Hardy and Laurie S. Zabin, *Adolescent Pregnancy in an Urban Environment: Issues, Programs, and Evaluation* (Washington, DC: Urban Institute Press, 1991) suggest that the sex decisions of teenagers (and ultimately pregnancy outcomes) are primarily a function of biological make-up and development, as well as family and community characteristics. While economists recognize that these issues are important, conditional on these factors, individuals will still perform a marginal analysis where they compare the incremental benefit of the sexual act with the incremental cost in expectation.

¹⁸ Richard A. Posner, *Sex and Reason* (1997), especially chapter 5.

¹⁹ Phillip Levine "The Sexual Activity and Birth Control Use of American Teenagers," in Jonathan Gruber, ed, *An Economic Analysis of Risky Behavior Among Youths* (Chicago: University of Chicago Press, forthcoming). Also available as NBER working paper w7601.

²⁰ Jonathan Klick and Thomas Stratmann (2003), "The Effect of Abortion Legalization on Sexual Behavior: Evidence from Sexually Transmitted Diseases," *Journal of Legal Studies*, 32(1): 407-433 at 411.

²¹ Alfred C. Kinsey, Wardell B. Pomeroy, and Clyde E. Martin, *Sexual Behavior in the Human Male* (Bloomington, IN: Indiana University Press, 1998).

in 1948 and his *Sexual Behavior in the Human Female*²² in 1953 can rightly be called the beginning of systematic empirical research in the field of sexuality, the data are far from inclusive in terms of years and areas covered,²³ limiting their usefulness for researchers intending to use them to study reactions to state level abortion policy and access.²⁴ Further, the sampling methods used by Kinsey were less than ideal. Presenting the results of a commission organized by the American Statistical Association, William Cochran, Frederick Mosteller, and John Tukey noted that although “[t]he statistical and methodological aspects of [Kinsey, *et.al.*]’s work are outstanding in comparison with other leading sex studies . . . Many of [Kinsey’s] findings are subject to question because of a possible bias in the constitution of the sample.”²⁵ That is, because Kinsey and his researchers had little guidance from statisticians trained in proper sampling methods (as the American Statistical Association report points out), it is unlikely that unbiased inferences about the population can be drawn from the Kinsey data.²⁶

Even had these problems been remedied (as they are in sophisticated modern survey data collected about sexual behavior), as the American Statistical Society report points out, survey results in this area are also likely to be adversely affected by incorrect responses owing to individuals’ reluctance to be forthcoming about such intimate details or the possibility that individuals are unable to recall their experiences with a great deal of accuracy.²⁷

Given that abortion counts for the pre-legalization period are likely to include significant measurement error, if they are available at all for a given year or state, and there is little variation in pre-legalization state-level abortion policy, the earliest feasible analysis of the relationship between abortion access and sexual behavior would likely focus on the legalization period. This period is generally thought to extend from the legalization of

²² Alfred C. Kinsey, Wardell B. Pomeroy, Clyde E. Martin, and Paul H. Gebhard, *Sexual Behavior in the Human Female* (Bloomington, IN: Indiana University Press, 1998).

²³ The data were collected through interviews conducted by the Institute for Sex Research (or Kinsey and his colleagues in the years before the Institute’s founding) during the period 1938-1963. See Paul H. Gebhard and Alan B. Johnson, *Marginal Tabulations of 1938-1963 Interviews Conducted by the Institute for Sex Research* (Bloomington, IN: Indiana University Press, 1998).

²⁴ Also, the limited variation in official policies related to abortion during this time period would hinder an analyst’s ability to isolate precise correlations between abortion access and sexual behavior even if perfect data were available. Data on the number of illegal abortions performed prior to the legalization period is of varying quality and is unavailable for certain states and certain time periods.

²⁵ William G. Cochran, Frederick Mosteller, and John W. Tukey (1953), “Statistical Problems of the Kinsey Report,” *Journal of the American Statistical Association*, 48(264): 673-716 at 674-5.

²⁶ One example of a source of sampling bias has to do with refusal rates. That is, if the individuals who refuse to answer a sex survey are a non-random subset of the population, it is difficult to draw unbiased inferences about the population. For example, if sexually conservative individuals are less willing to answer a sex survey than are more active individuals, the data will yield an upwardly biased estimate of sexual behavior for the population as a whole.

²⁷ *Ibid.*, 675.

abortion on demand by the California Supreme Court in 1969 through the national legalization that came with *Roe* in 1973. In the interim, Alaska, Hawaii, New York, and Washington State all legalized through legislation in 1970. Additionally, during this period, a number of states liberalized the conditions under which abortions could be deemed medically necessary.²⁸

In some cases, it is difficult to differentiate between a complete legalization and a mere liberalization. For example, in his analysis, Ted Joyce includes the District of Columbia as an early legalizing state as of 1969. Despite the fact that DC is generally not considered a legalizing state, Joyce cites a source suggesting that DC's abortion facilities were "ranked among the busiest in the country, with 20,000 patients in 1971."²⁹ Joyce provides independent evidence suggesting that DC's abortion rate was more than two times higher than that of either New York or California in 1971.³⁰ Because of this, Joyce includes DC as a *de facto* early legalizer.

Interestingly, most analysts have not treated DC this way. Klick and Stratmann discuss the ambiguity involved with respect to DC. "[T]he District of Columbia statute that limited abortion to cases in which the mother's life was in danger was declared to be unconstitutionally vague in the 1969 case of *U.S. v. Vuitch* (305 F. Supp. 1032 [November 10, 1969]). However, the Supreme Court reversed this ruling in 1971 (*U.S. v. Vuitch*, 91 S. Ct. 1294 [April 21, 1971]), declaring that the statute was constitutional. The Court's interpretation of the statute, however, put the burden of proof on the prosecution to show that the mother's life was not in danger. This creates some ambiguity in determining what the effective status of the District of Columbia law was prior to *Roe v. Wade*."³¹

Placing these classification issues aside, and ignoring the sampling issues involved with the Kinsey data on sexual behavior, analyzing this legalization period is still problematic since the Kinsey data stop in the early 1960s, and there are no other nationally inclusive data on sexual activity during the legalization period.

However, Klick and Stratmann propose a novel way around this problem. Instead of examining sexual activity directly, they analyze rates of sexually transmitted diseases (STDs). The Centers for Disease Control (CDC) maintains comprehensive data on STD rates by state, and it has data on gonorrhea and syphilis rates for the legalization period.³² They conjecture that, since STDs are strongly correlated with engaging in risky sexual activity, STD rates should provide a good proxy for the underlying

²⁸ Klick and Stratmann at 414.

²⁹ Lawrence Lader, *Abortion II: Making the Revolution* (New York: Beacon, 1974) at 115.

³⁰ Ted Joyce (2004), "Did Legalized Abortion Lower Crime?" *Journal of Human Resources*, forthcoming at fn 2.

³¹ Klick and Stratmann at fn 13.

³² *Ibid* at 412.

sexual behavior.

In their study, they hypothesize that, if abortion legalization increased sexual activity, we should observe an increase in STD rates for the early legalizing states in 1970 (1969 for California), and a subsequent increase for all other states in 1973, when the Supreme Court legalized abortion nationally. Further, it should be the case that any gap between the difference from pre-legalization rates between early legalizers and late legalizers should disappear in 1973. That is, effectively, we should observe that late legalizing states “catch up” with early legalizers once abortion law is normalized throughout the country as a result of *Roe*.

To test this hypothesis, Klick and Stratmann use state and year fixed effects (as well as state-specific trends in some specifications) in their regressions. Effectively, what this specification does is it compares the change from baseline (i.e., pre-legalization) STD rates in early legalizing states at the time of legalization with the rates in non-legalizing states, controlling for other variables suggested to be important determinants of STD rates in the medical literature. The experiment is repeated in 1973 with the early legalizing states now serving as the control group against which the change from baseline is measured. They find that abortion legalization led to an increase in gonorrhea and syphilis rates on the order of 25 percent.³³

As for the divergence/convergence hypothesis (i.e., the hypothesis that we should observe an increasing gap between early and late legalizing states in 1970, and that gap should subsequently disappear in 1973), Klick and Stratmann get mixed results. Although the divergence/convergence pattern emerges unambiguously for gonorrhea,³⁴ syphilis rates exhibit only a divergence but no subsequent convergence.³⁵ The authors speculate that this unexpected result flows from two idiosyncratic characteristics of syphilis. First, aggregate syphilis rates contain a relatively high proportion of homosexual infection rates, on which abortion legalization will have no effect, and, second, syphilis is still contagious after treatment suggesting that early legalizing states amassed a relatively large pool of infectors between 1970 and 1973, generating a differential infection rate which the late legalizing states could not match.³⁶

Although the Klick and Stratmann study provides relatively strong evidence that increased abortion access induces increased sexual activity and STD rates,³⁷ their analysis is not without its shortcomings. By

³³ Ibid, Tables 3 and 4.

³⁴ Ibid, Figure 1.

³⁵ Ibid, Figure 2.

³⁶ Ibid, 430.

³⁷ Interestingly, although Klick and Stratmann identify an abortion effect on STD rates that is highly significant, both statistically and practically, the medical journals and the CDC do not mention this effect in any of the hundreds of articles discussing the increases in gonorrhea rates observed during this time. Instead, many of

analyzing aggregate STD rates, they necessarily miss potential racial heterogeneity. That is, it may be the case that different sub-populations reacted to increasing abortion access differently.³⁸

Perhaps more important, there is no way for Klick and Stratmann to separate the effects of increased sexual activity from the effects of substituting away from alternate forms of birth control. That is, it is also the case that abortion legalization makes the relative value of using condoms decline. If individuals were less likely to use condoms when abortion was legalized, we would observe increasing STD rates even if individuals had no more sex as a result of the legalization. In essence, Klick and Stratmann's result indicates an increase in risky sex, but that increase is a pooled effect of more sex in the aggregate and a replacement of safe sex for unsafe sex. There is no way to tell from their analysis what the relative proportions are of the two causal mechanisms in their net findings.

Also, though not a problem from a positive standpoint, the normative welfare implications of Klick and Stratmann's findings are unclear. While increased STD rates are presumably an unambiguous loss to society, costing an additional \$300 million in treatment expenses per year,³⁹ this might be a small cost relative to the value derived by individuals from the increase in risky sex. Thus, it is unclear whether Klick and Stratmann's results have any implications for abortion policy, but they certainly do have a bearing on future epidemiological research on STDs.

Reaching a similar, though weaker, conclusion is Sen, who examines the effect of restrictions on Medicaid funding of abortions on female gonorrhea rates. In principle, reductions in public funding of abortions will increase the cost of obtaining an abortion, at least for some subset of a state's population. This decrease in abortion access should lead to a reduction in risky sexual behavior, which will be manifested in lower STD rates.⁴⁰

Sen examines female gonorrhea rates for the period 1975-1995. The time frame exploits the fact that, in 1976, federal Medicaid funding for abortion procedures was cut by the Hyde Amendment, leaving funding decisions up to the states' discretion. By the end of the sample, just 17 states funded abortions for poor women.⁴¹ This significant variation in funding prohibitions allows Sen to separate out the effects of abortion

the CDC publications simply assert that changing youth behavior and social morals (*the sexual revolution*) generated the increase without probing the underlying incentives at work.

³⁸ Although the data do not allow sorting by race, Klick and Stratmann are able to differentiate effects by sex. They find no large differences between males and females regarding the correlation between STD rates and abortion legalization (Tables 5 and 6).

³⁹ Ibid, 431. The authors estimate that if the effects can be extrapolated to other STDs, the cost is closer to \$4 billion annually.

⁴⁰ Bisakha Sen (2002), "A Preliminary Investigation of the Effects of Restrictions on Medicaid Funding for Abortions on Female STD Rates," *Health Economics*, 12: 453-464 at 454.

⁴¹ Ibid at 456.

access from any idiosyncratic state-level STD patterns.

Similar to Klick and Stratmann's results, Sen finds that reducing abortion access leads to a reduction in STD rates. However, Sen's estimates are not statistically significant. This lack of significance does not appear to be the result of a near-zero effect on STD rates. Indeed, Sen finds reductions of as much as 4 percent resulting from Medicaid funding restrictions, which would seem to be fairly large. Sen's estimates however are not very precise, leading to the lack of statistical significance. One way in which an increase in precision could be achieved would be to exploit the fact that prohibitions were introduced at different times throughout any given year. That is, Sen codes a state as having a prohibition in effect for a given year as a zero-one dummy variable, taking the value of one if funding was prohibited at any point during the year. If one state passes a prohibition in January, while another passes a prohibition in September, Sen's analysis treats the prohibitions as equivalent. Perhaps a fractional prohibition variable would be more appropriate to account for this differential timing effect. Further, Sen does not account for the fact that Medicaid funding for abortions in non-prohibition states varies significantly. If a state providing generous funding for abortions enacts a prohibition, we would expect a much larger effect on STD rates than we would from a parsimonious state's prohibition. Each of these specification issues could significantly affect the precision of Sen's estimates, perhaps generating statistically significant coefficients.

A different approach to determining the effect of abortion policy on sexual behavior involves examining pregnancy rates, as opposed to STD rates. The major problem with focusing on pregnancy rates is that the data are of a generally poor quality. Specifically, pregnancy rate data are derived by adding the observed birth rate to the abortion rate. Such a measure ignores pregnancies that end in spontaneous abortion (i.e., miscarriage) and unreported abortions. According to the National Longitudinal Survey of Youth, which started interviewing individuals between the ages 14 and 21 in 1979 and continues to interview them annually, nearly 7 percent of the respondents' pregnancies ended in miscarriage.⁴² If this measurement error (relative to the true pregnancy rate) is systematically related to changes in abortion policy, any inferences drawn about the causal effects of policy will be biased.⁴³

⁴² V. Joseph Hotz, Susan Williams McElroy, and Seth Sanders, "Teenage Childbearing and Its Life Cycle Consequences: Exploiting a Natural Experiment," NBER Working Paper 7397 at 8.

⁴³ Also, as pointed out in Phillip B. Levine, Amy B. Trainor, and David J. Zimmerman (1996), "The Effect of Medicaid Abortion Funding Restrictions on Abortions, Pregnancies, and Births," *Journal of Health Economics*, 15: 555-578 at 561, abortions are recorded at the place of occurrence rather than the state of residence. If we assume that abortions are more difficult to get (e.g., fewer providers, more protestors, etc.) in states that also happen to pass Medicaid restrictions, inducing individuals to travel to other states to get their abortions, the measurement error in the pregnancy rate would be systematically related to the Medicaid restriction variable. This

Despite this problem, a number of studies have examined the effect of abortion policies on pregnancy rates, examining the relationship between abortion and sexual behavior either directly or indirectly. Again using Medicaid restrictions as the policy mechanism of interest, Levine, Trainor, and Zimmerman find that states restricting Medicaid funding for abortion during the years 1977-1988 witnessed a decrease in their pregnancy rates on the order of 7.7 percent.⁴⁴ Disregarding the problems with pregnancy rate data discussed above, this too implies that increasing abortion access increases the incidence of unprotected sex. Interestingly, Levine, Trainor, and Zimmerman find that this effect seems to be driven mostly by behavior among those in the 15-24 age range,⁴⁵ implying that the behavior of young people is the most sensitive to changes in abortion access.

One last metric that has been used to study the effect of abortion access on sexual behavior has been survey data about sexual experiences. Despite the misgivings about survey data in this area presented above, these data do have the benefit of allowing researchers to disaggregate the volume of sex question from the method of birth control question. That is, studies relying on STD or pregnancy rates can only isolate changes in the total amount of risky sex taking place. They cannot discern whether estimates are driven by more total sex or by a comparable level of sex where individuals choose not to use STD or pregnancy prevention measures (e.g., condoms). Phillip Levine investigates this issue using data from the 1988 and 1995 National Surveys of Family Growth, relating the responses of teens to whether or not the state in which they live has enacted a parental involvement law. In general, these laws require minors to inform their parents or to obtain their parents' consent before receiving an abortion.⁴⁶ Levine finds that the introduction of a parental involvement law lowers the incidence of unprotected sex among women ages 15-18 (during the three months prior to the survey) by 6 percent, though the result is only significant at the 8 percent confidence level. Most of this increase is the result of a substitution toward using contraception, rather than an increase in the underlying level of sexual activity.⁴⁷ These results imply that, among young women, abortion is seen as a substitute for *ex ante* birth control, though, as we might expect given reasonable suspicions about survey data in this area, Levine's estimates are not terribly precise.

The most clear-cut finding of these econometric studies of the

would create a downward bias in the estimated effect of Medicaid restrictions on pregnancy rates because abortion rates would be underestimated in states with restrictions and over estimated in states without restrictions, leading to a larger net pregnancy differential than actually exists.

⁴⁴ Ibid at 564.

⁴⁵ Ibid at 569.

⁴⁶ Phillip B. Levine (2003), "Parental Involvement Laws and Fertility Behavior," *Journal of Health Economics*, 22: 861-878 at 862.

⁴⁷ Ibid at 874.

relationship between abortion policy and sexual behavior is that individuals, even young individuals whose sexual behavior is often considered to be driven more by emotion than by calculation, are sensitive to the costs of their sexual activity. When those costs increase, as predicted by the law of demand, individuals engage in less risky sex. Improving abortion access, on the other hand, will lead to more risky sex and its attendant consequences. From a policy standpoint, this implies that lawmakers and their analysts should treat behavior as endogenous. That is, in predicting the net effects of future policy, it is not reasonable to assume that behavior will remain constant when incentives change.⁴⁸

II. ABORTION AND CRIME

Though the causal connection between crime and abortion is less directly obvious than that between abortion and sexual behavior, the literature linking abortion and crime has attracted attention well beyond the academic journals. John Donohue and Steven Levitt first investigated the link in a 2001 article in *The Quarterly Journal of Economics*.⁴⁹ They hypothesized that legalizing abortion would affect subsequent crime rates through a number of different channels.

First, if legalizing abortions reduces the number of children born, it would also necessarily eventually reduce the number of individuals in the age cohort (late adolescence) that commits most of the crimes. Citing work by Levine, *et.al.*,⁵⁰ Donohue and Levitt expected this effect to yield about a 5 percent decrease in crime.⁵¹ Further, they hypothesized that since the decrease in births will disproportionately come from reductions by those women least willing and able to care for their children and children born to these mothers are more likely to resort to criminal activities in their late adolescent years than the rest of the population, the reduction in births will disproportionately affect the subset of the population that commits most of the crimes. This implies an expected abortion-induced decrease in the crime rate on the order of 20 percent for those cohorts whose mothers had

⁴⁸ On this point, see Klick and Stratmann; also Michael Kramer (1996), "Integrating Behavioral Choice Into Epidemiological Models of AIDS," *Quarterly Journal of Economics*, 111: 549-573 and Tomas J. Philipson and Richard A. Posner, *Private Choices and Public Health: The AIDS Epidemic in an Economic Perspective* (Cambridge, MA: Harvard University Press, 1993).

⁴⁹ As noted in John J. Donohue III and Steven D. Levitt (2001), "The Impact of Legalized Abortion on Crime," *Quarterly Journal of Economics*, 116(2): 379-420 at 380, it turns out that some non-academics had previously speculated about this causal link between abortion and crime. Specifically, former Minneapolis Police Chief Anthony Bouza wrote in Anthony V. Bouza, *The Police Mystique: An Insider's Look at Cops, Crime, and the Criminal Justice System* (New York: Plenum Press, 1990), "arguably the only effective crime-prevention device adopted in this nation since the late 1960s [is abortion]."

⁵⁰ Phillip B. Levine, Douglas Staiger, Thomas J. Kane, and David J. Zimmerman (1999), "Roe v Wade and American Fertility," *American Journal of Public Health*, 89(2): 199-203.

⁵¹ Donohue and Levitt (2001) at 386.

access to legal abortions.⁵² This would represent about half of the actual crime decrease witnessed during the 1990s, when the first legalized abortion cohort reached its peak crime years.⁵³

To test these hypotheses and back of the envelope calculations, Donohue and Levitt reasoned that crime should have first decreased in the early legalizing states (Alaska, California, Hawaii, New York, and Washington), with the rest of the states achieving their reductions three years later. Because the abortion rates in these states continued to be higher than other states even after *Roe*, the authors decided to use abortion rates rather than a legalization dummy as their abortion policy variable. They found that, for the period 1985-1997, an additional 100 abortions per 1,000 live births led to a decrease in violent crimes per capita of 13 percent, 9 percent for property crimes, and 12 percent for the murder rate.⁵⁴ These results were roughly consistent with their speculation that abortion legalization might account for as much as half of the crime decrease witnessed during the 1990s, implying that legalization saved the economy \$30 billion annually in terms of crime reduction.⁵⁵

Despite the large magnitude of their results, Donohue and Levitt urged caution in drawing policy implications from their research, writing “While falling crime rates are no doubt a positive development, our drawing a link between falling crime and legalized abortion should not be misinterpreted as either an endorsement of abortion or a call for intervention by the state in the fertility decisions of women.” They go on to suggest the possibility that equivalent improvements in crime could be achieved through other means such as improved *ex ante* birth control availability or by improving the environments in which children at risk for developing criminal tendencies are raised.⁵⁶

These concluding remarks suggest that Donohue and Levitt were expecting the torrent of criticisms their research would eventually create outside of academic law and economics. Prominent pro-life writers linked the research with the field of eugenics. Writing in the August 1999 (while the Donohue and Levitt article was only in working paper form) edition of *Life Insight*, published by the National Conference of Catholic Bishops’ Secretariat for Pro-Life Activities, Susan Wills entitled her criticism of Donohue and Levitt “Legalized Abortion and Crime: Eugenics with a Happy Face.”⁵⁷ Writing for National Review Online, Kathryn Lopez indicated that the research has “eugenics undertones.”⁵⁸ Even when George

⁵² Ibid at 390.

⁵³ Ibid at 391.

⁵⁴ Ibid at 404.

⁵⁵ Ibid at 414.

⁵⁶ Ibid at 415.

⁵⁷ <http://www.nrlc.org/news/1999/NRL999/crime.html>

⁵⁸ Kathryn Jean Lopez, “The Other Death Penalty: Do We Have Less Crime Because of Abortion?” National

Will defended the research by pointing out its essentially positive findings (as opposed to normative prescriptions), he conceded that Donohue and Levitt's research would draw (unwarranted) charges that the two are "racists urging eugenics."⁵⁹

While not nearly as bombastic in tone as the criticism levied in the popular press, Donohue and Levitt's article also drew forceful criticisms within the academic literature. In an unpublished paper, John Lott and John Whitley suggest that abortion legalization might paradoxically lead to more single parent families, limiting the amount of investment made in the children in those families.⁶⁰ This relative deprivation would be expected to increase the likelihood that those children will engage in criminal activities once they reach late adolescence. This effect will counteract some of the reduction in crime generated by fewer unwanted children that is hypothesized by Donohue and Levitt. Thus, the net effect of legalizing abortion on crime rates would be ambiguous.⁶¹

Lott and Whitley draw their hypothesis from earlier work co-authored by Nobel Laureate George Akerlof. In a 1996 article, Akerlof, along with Janet Yellen, and Michael Katz, argue that the legalization of abortion (as well as the introduction of effective *ex ante* birth control) has significant effects on the market for mates. If some women are willing to have abortions in the event of an unwanted pregnancy, they have a competitive advantage in the dating market, since they can offer sexual services at a lower expected price (i.e., there is no expectation that the man will be held responsible in the event of a pregnancy). Women who are unwilling to have abortions, must either drop out of the market or offer sexual services with a lower expectation that the man will help support the woman and child in the event of an unplanned pregnancy. In a world where abortion is unavailable, all women are competitive equals along this dimension, allowing each to leverage sex for the promise of marriage and/or support in the future. Once abortion is introduced, those unwilling to have an abortion are less able to secure such a promise and are more likely, on the margin, to be left as single mothers.⁶²

As an empirical matter, Lott and Whitley's major criticisms of Donohue and Levitt's work focus on the latter's choice to assume that the abortion rate prior to legalization in a given state is zero and their failure to

Review Online: June 19, 2001. http://www.nationalreview.com/nr_comment/nr_comment061901a.shtml

⁵⁹ George F. Will, "More Abortions, Fewer Crimes? Does Correlation Mean Causation? What About the 27 Million Abortions in 18 Years?" *Newsweek*: April 30, 2001, 84.

⁶⁰ A large body of research suggests that children raised in single parent families and children raised by a parent and a step parent have systematically lower educational outcomes relative to children raised in a traditional nuclear family. For example, see Donna K. Ginther and Robert A. Pollak (2003), "Does Family Structure Affect Children's Educational Outcomes?" NBER Working Paper w9628.

⁶¹ John R. Lott, Jr. and John Whitley, "Abortion and Crime: Unwanted Children and Out-of-Wedlock Births," Yale Law School Program for Law, Economics and Public Policy Working Paper #254, at 4.

⁶² Akerlof, Yellen, and Katz (1996).

disaggregate crime rates by the age of the offender. Lott and Whitley indicate that the data suggest that abortion rates were relatively high prior to legalization in many states, with some non-legalizing states exhibiting higher abortion rates than the early legalizers.⁶³ In replicating Donohue and Levitt's results using available data on pre-legalization abortion rates, Lott and Whitley find that the estimated size of the abortion effect on crime drops significantly when the zero illegal abortion assumption is dropped.⁶⁴

Perhaps more central to Lott and Whitley's criticism is the age aggregation. They suggest that if abortion legalization drives the reduction in crime during the 1990s, decreases should first be observed in crimes committed by younger individuals, and the decrease should only spread to crimes committed by older individuals as time passes. To examine this, they use data from the FBI's Supplemental Homicide Report to break up the state homicide rates according to the perpetrator's characteristics, specifically his or her age. Once homicides are linked to the perpetrator's age, Lott and Whitley do not find support for the hypothesis that legalizing abortion lowered crime in the 1990s. They do not find the reductions occurring first in the youngest age groups, but rather they observe reductions in the older groups first, and in some specifications, they actually find homicide rates among the youngest perpetrators rising just when Donohue and Levitt's argument suggests they should be falling.⁶⁵

Along the same lines as Lott and Whitley's criticisms, Ted Joyce also focuses on comparisons of arrest rates and homicide rates between cohorts that were exposed to legalized abortion and those that were not. Finding no consistent evidence that abortion legalization is causally linked to crime, Joyce concludes that Donohue and Levitt's analysis suffers from omitted variables bias, specifically a failure to account for changes in crack use. He also criticizes Donohue and Levitt's assumption that pre-legalization abortion rates were zero.

Joyce suggests that the evolution of crack markets present an important confounding factor in explaining the variation in crime rates from the late 1980s through the 1990s. With respect to abortion, the fact that crack emerged in New York and Los Angeles (representing cities in two of the five states treated as early legalizers) is particularly troublesome.⁶⁶ Controlling for this crack effect at the state level is difficult since it appears as though there was significant within state variation in the development of crack markets in various cities, thus neither year fixed effects nor state fixed effects (nor state specific trends for that matter) sufficiently control for the crack effect. Joyce proposes a novel control strategy of comparing

⁶³ Lott and Whitley (2001) at 6.

⁶⁴ Ibid at 10.

⁶⁵ Ibid at 15.

⁶⁶ Ted Joyce (2004), "Did Legalized Abortion Lower Crime?" *Journal of Human Resources*, forthcoming.

homicide rates and arrest rates for pre and post legalization cohorts within a given state. This strategy obviates the concern of omitted variables bias and measurement error in pre-legalization abortions. Interestingly, Joyce did not find consistent decreases in homicide or arrest rates for the cohorts exposed to legalized abortion relative to the unexposed cohorts. In fact, in some comparisons, he actually finds relative increases.

Unlike the Lott and Whitley paper,⁶⁷ Joyce's research induced a formal reply from Donohue and Levitt, who responded to what they saw as Joyce's five major criticisms of their work.⁶⁸ Regarding the objection to assuming a zero illegal abortion rate, Donohue and Levitt argue that this actually biases their results against their hypothesis. Assuming that states with high rates of legal abortions also (pre-legalization) have high rates of illegal abortions, Donohue and Levitt suggest that their assumption of zero pre-legalization abortions systematically overstates the increase in abortions after legalizations. According to them, this unambiguously biases their abortion coefficient toward zero, implying that the true effect of abortion on crime is even larger in magnitude (i.e., a larger decrease in crime associated with abortion) than their estimate. Joyce argues that the direction of the bias is ambiguous, as it depends upon the relative magnitudes of two components: 1) the (necessarily positive) sum of the variance of pre-legalization abortion rates and the variance of Donohue and Levitt's estimate of pre-legalization abortion rates (0) minus the true pre-legalization abortion rate and 2) the (necessarily negative) covariance of the true pre-legalization abortion rate and the difference between Donohue and Levitt's estimate (0) and the true abortion rate.⁶⁹ Without more information, it is not possible to determine the sign generated by adding a positive and a negative number, implying that we cannot rule out the possibility that Donohue and Levitt's estimates are biased upward because of their assumption of zero pre-legalization abortions.

The second point made by Joyce to which Donohue and Levitt respond regards Joyce's finding that the abortion effect does not show up in crime rates covering the period 1985-1990; it is only apparent beginning in 1991. Joyce conjectures that if the abortion correlation were causal, it should also be apparent in the earlier period. To this claim, Donohue and Levitt suggest that crack-related crime during this period was concentrated in Los Angeles and New York and this confounding effect, for which little in the way of solid data exists allowing researchers to control for it, swamps any existing abortion effect. To support this claim, they present data on drug-related homicides, showing that the gap between early legalizing states and other

⁶⁷ Interestingly, according to Lopez (2001), Levitt referred to Lott and Whitley's paper as "garbage."

⁶⁸ John J. Donohue III and Steven D. Levitt (2004), "Further Evidence that Legalized Abortion Lowered Crime: A Reply to Joyce," *Journal of Human Resources*, forthcoming.

⁶⁹ Joyce (2004) at fn 3.

states peaks in 1990.⁷⁰ Further, they point to Joyce's own analysis showing that an abortion effect is apparent in the 1985-1990 period for property crimes. They argue that since crack is generally recognized to have led mostly to violent crime, as opposed to property crime, this provides indirect evidence that the crack confound is to blame for the abortion effect not showing up generally in the 1985-1990 crime data.

Donohue and Levitt also claim that the crack phenomena is to blame for Joyce's failure to find significantly lower crime rates, during the 1985-1990 period, for individuals from early legalizing states who were exposed to legal abortion relative to individuals from those states who were born just before legalization. Using homicide data, as opposed to the generalized arrest data used by Joyce, they show that if a longer window is examined for these cohorts, a significant reduction in crime is observed for the exposed group relative to the unexposed group. Joyce is limited to a shorter window because the arrest data are not broken down by specific age of the perpetrator beyond the age of 24. Thus, it is not possible to separate arrests for the exposed vs. the unexposed past 1990, while the homicide data, used by Donohue and Levitt, do allow for such separation over a longer time horizon. This data limitation makes it impossible to evaluate Joyce's criticism fully in light of Donohue and Levitt's crack explanation, but their argument seems to be supported, at least for homicides.

The fourth criticism Donohue and Levitt reply to involves Joyce's finding that, in national time series data (from which early legalizing states were excluded by Joyce), there is no significant reduction in crimes committed by individuals born after national abortion legalization in 1973 relative to the cohort born before legalization. Donohue and Levitt's main objection to this criticism is that it does not differentiate on the basis of relative accessibility of abortions, which, they claim, varied widely by state. For example, they point out that while Kansas had 414 abortions per 1,000 live births in 1973, no abortions were reported in Louisiana or North Dakota during that year. A more appropriate analysis, they argue, would look at within state differences between exposed and unexposed cohorts, and they present some evidence for an abortion effect using such a specification.

The last criticism addressed by Donohue and Levitt is Joyce's claim that because early legalizing states continued to exhibit greater reductions in crime relative to later legalizing states even after the abortion effect should have shown up in those states as well, any observed abortion effect cannot be causal. That is, if legalization *per se* decreases the incidence of unwanted births (and leads to the eventual decrease in crime hypothesized by Donohue and Levitt) we should expect the later legalizing states to catch up. However, Donohue and Levitt argue that legalization *per se* is not all

⁷⁰ Donohue and Levitt (2004) Figure 3.

that matters, since access still varies under a fully legalized regime. Thus, they use an abortion rate measure to identify the abortion effect on crime and argue that Joyce's focus on a binary legalization measure misspecifies the true relationship between abortion access and crime.

It is interesting to note that, implicitly, Joyce is seeking a divergence/convergence relationship in crime between early legalizers and late legalizers like that identified in the article on abortion legalization and sexual behavior by Klick and Stratmann that is discussed above. It might be instructive to consider why such a relationship exists in Klick and Stratmann's analysis of STD rates, but why no such relationship exists for crime. One possibility is that, for Klick and Stratmann's moral hazard argument to work, it is not strictly necessary that individuals have easy access to abortion, just that they believe they have easy access to abortion. That is, in evaluating the costs of sexual activity, knowing that abortion is legal might imply enough of a decrease in subjective expected cost to induce an individual to decide to have risky sex while he might have made a different decision if abortion were illegal. However, for Donohue and Levitt's argument, the individual must actually be able to obtain an abortion to avoid an unwanted birth if we are to expect any effective change in future crime rates. Thus, in relative terms, actual access is more important in the crime context, whereas expected access drives the sexual behavior relationship.⁷¹

Unfortunately, Donohue and Levitt and Joyce are correct in their misgivings about the others' use of a given abortion measure. It is undeniable that access to abortion is not homogenous across the states after legalization, implying that regressions using a binary legalization variable to capture access will necessarily generate biased results, and it is unlikely we can know the direction of that bias much less its magnitude. On the other hand, using a measure of the number of abortions performed, even if we had perfect data on illegal abortions, will lead to a simultaneity problem. That is, how many abortions performed will be a function of a host of variables that could also be important in determining the crime rate. For example, if income or education are important determinants of the abortion rate and they also are important determinants of the likelihood that children will engage in criminal activities, any regression focusing on abortion rates will conflate the effects of parental income and education, limiting our confidence in the causal interpretation of the abortion effect (again, both in terms of direction and magnitude).

Recognizing this tension, Joyce does investigate an intermediate abortion index. He indicates that he divides the states into two groups,

⁷¹ Of course, we would expect there to be a significant, though not perfect, relationship between expected access and actual access.

those with abortion rates above the national 1973 median and those below, comparing the eventual crime differential. Joyce argues that there is a causal relationship between abortion access and future crime, he should observe that crime decreases more for the states in the high abortion group. He finds no evidence of this.⁷² In effect, what this analysis does is it allows for more variation in the policy variable than a simple legalization variable does, while still mitigating the simultaneity problem inherent in using abortion rates to identify the effect on crime.

As for the other major argument here, specifically whether it is possible to use existing data to separate an abortion effect from other factors, it is unlikely that we will get a conclusive direct answer with respect to crime. However, indirect evidence might be very useful. If Donohue and Levitt's argument that the reduction of "unwantedness" will lead to fewer individuals with relatively low human capital investments made in them by their parents and this reduction will lower the incidence of future behavior that is costly to society, we should see this effect with respect to non-criminal behavior as well. For example, because there appears to be a strong relationship between a teenage girl's sexual and fertility experiences and family characteristics that relate to "wantedness," including educational investment, communication, and the mother's own teenage fertility history,⁷³ it should be possible to examine the Donohue and Levitt hypothesis by looking at teenage pregnancy differentials between individuals in the cohorts that were exposed to legalized abortion and unexposed cohorts. Along those same lines, STD rates among teenagers might be a useful dependent variable to examine. While it still would not be possible to overcome the problems identified with both abortion access measures, these measures of socially costly behavior would not be affected by the crack period in the way that crime is. If these behaviors confirmed the basic thrust of Donohue and Levitt's hypothesis, Joyce's criticisms would have less bite.⁷⁴

Though not in the same direct line of inquiry as that laid out by

⁷² Joyce does not include the results in his paper, but will provide them on request. He is currently working on a paper examining the low/high abortion differential, extending the analysis into quartiles in addition to halves.

⁷³ See, for example, Cheryl D. Hayes, ed., *Risking the Future: Adolescent Sexuality, Pregnancy, and Childbearing* (Washington, DC: National Academies Press, 1987) at Ch. 4.

⁷⁴ Kerwin Kofi Charles and Melvin Stephens, Jr. (2002), "Abortion Legalization and Adolescent Substance Use," NBER Working Paper 9193 does present some indirect evidence in favor of the Donohue and Levitt hypothesis. They show that individuals in the cohorts that were exposed *in utero* to legalized abortion were much less likely to use controlled substances relative to individuals conceived and born before abortion was legalized. Specifically, Charles and Stephens focus on survey data on whether an individual has ever used marijuana, cocaine, heroin, or amphetamines and whether the individual has used any of these substances during the past 30 days. They also examine the data excluding marijuana. They find a large, statistically significant effect of abortion exposure in both the "ever used" data and the "used in last 30 days" data (on the order of a 15 percent reduction in likelihood). The results appear to be very robust to many specifications, including using birth rate variation to identify the effect of a change in abortion access (rather than *de jure* legalization). This evidence is consistent with the wantedness hypothesis advanced by Donohue and Levitt.

Donohue and Levitt, Marianne Bitler and Madeline Zavodny examine the effect of abortion access changes on a specific category of crime – child abuse. Much like Donohue and Levitt, Bitler and Zavodny speculate that aborted babies are not a random draw from the population of conceived children. Instead, abortion is likely to be systematically related to wantedness. Citing evidence that unwanted or unplanned children are more likely to be the victims of abuse,⁷⁵ Bitler and Zavodny hypothesize that increasing abortion access should lead to less child abuse.⁷⁶

To test this hypothesis, they examine annual state-level data from the American Humane Association and the National Committee to Prevent Child Abuse for the period 1976-1996, focusing on abortion legalization, Medicaid funding restrictions, parental involvement laws, and mandatory waiting periods as their measures of abortion access.⁷⁷ Though these data suffer from significant limitations, such as measurement error owing to fact that abuse is likely not reported in all cases in which it occurs and there are likely to be some spurious reports, they represent the best available information on state-level abuse patterns. Bitler and Zavodny find that legalization significantly lowered the incidence of child abuse for the cohort of children who were conceived after legalization occurred.⁷⁸ Surprisingly, their results suggest that parental consent or notification laws were associated with lower rates of abuse.⁷⁹ While this does not accord with the “wantedness” hypothesis, it does make sense in the light of other research suggesting that parental involvement laws do lead to lower teen birth rates.⁸⁰ In essence, requiring parental involvement mitigates or counteracts the moral hazard associated with increased abortion access, inducing teens to either engage in less sex or to be more likely to use *ex ante* birth control, reducing the number of teen pregnancies. Presumably, this result is driving the parental involvement law effect in Bitler and Zavodny’s analysis, since these laws would not bind for older mothers.

These opposite effects of legalization and parental involvement laws on child abuse rates further underscore the ambiguities that exist in the Joyce and Donohue and Levitt debate. While the legalization result supports Donohue and Levitt’s wantedness argument, the negative effect of involvement laws suggests that the endogeneity of sex argument is

⁷⁵ Susan J. Zuravin (1987), “Unplanned Pregnancies, Family Planning Problems, and Child Maltreatment,” *Family Relations*, 36(2): 135-139.

⁷⁶ Marianne Bitler and Madeline Zavodny (2002), “Child Abuse and Abortion Availability,” *American Economic Review*, 92(2): 363-367 at 363.

⁷⁷ The researchers use a fractional variable measuring the proportion of the year a given policy was in effect. This mitigates some of the temporal aggregation problems that arise when a mere 0-1 policy dummy is used to analyze abortion policies.

⁷⁸ *Ibid* at 365.

⁷⁹ *Ibid* at 366.

⁸⁰ Thomas J. Kane and Douglas Staiger (1996), “Teen Motherhood and Abortion Access,” *Quarterly Journal of Economics*, 111(2): 467-506.

empirically important. Unfortunately, the dearth of data on child abuse keeps us from providing strong indirect evidence, one way or the other, for the larger issue of the relationship between abortion access and crime more generally.

III. ABORTION ACCESS AND OPPORTUNITIES FOR WOMEN

A third aspect of abortion legalization that economists have studied is its effect on opportunities for women, specifically educational opportunities and labor market effects. The intuition behind why we would expect abortion access to affect opportunities for women is clear. Unplanned pregnancy, especially if it occurs during the teenage or young adult years, is likely to disrupt a woman's education, potentially leading to adverse consequences later on as the woman attempts to enter the labor market.

Although the negative relationship between teenage motherhood and educational attainment and income levels has been well documented for quite some time,⁸¹ it is not completely clear that the relationship is causal. For example, there is some evidence that teenage childbearing is related to family characteristics that are also important determinants of educational attainment and economic success.⁸² Also, even beyond potentially quantifiable controls, there are presumably a host of unobservable (and inherently unquantifiable) personal attributes that affect both the likelihood of getting pregnant as a teen and economic success. These attributes could include judgments about risk, subjective discount rates, moral and ethical beliefs, and the like.

The legalization of abortion represents a natural experiment through which researchers can potentially separate out the effect of having an unplanned child from the statistically uncontrollable factors discussed above. If women can now more easily choose to have a baby or not, in essence, every birth is "planned" at least *ex post*. Joshua Angrist and William Evans use this abortion-induced shock⁸³ to teen fertility to isolate the causal effect of teenage childbearing on educational and labor market

⁸¹ See for example, Sandra L. Hofferth and Kristin A. Moore (1979), "Early Childbearing and Later Economic Well-Being," *American Sociological Review*, 44(5): 784-815.

⁸² See, for example, Arline T. Geronimus and Sanders Korenman (1992), "The Socioeconomic Consequences of Teen Childbearing Reconsidered," *Quarterly Journal of Economics*, 107(4): 1187-1214. In that study, the authors examined pairs of sisters who had different timing of their first birth (i.e., one had a child while a teenager, while the other one waited until later to have a child) and found that family background (which is controlled for in the sister pairs) has a significant effect on economic outcomes, and the inability to control sufficiently for background biases most estimates of the effect of childbearing on economic outcomes.

⁸³ Technically speaking, the authors use reduced form OLS models for most of their analyses and an instrumental variables model to examine the robustness of the OLS results for the effect of teenage childbearing on black educational and labor market outcomes wherein they use an interaction between year and state of birth (i.e., effectively an indicator of exposure to legalized abortion) to instrument teenage fertility for the individuals in the sample. They then use the instrumented fertility measure in regressions measuring the effect of childbearing on education and economic status variables. They find that the OLS estimates are biased downward slightly.

outcomes.⁸⁴ Angrist and Evans find that abortion legalization led to large reductions in teenage fertility for black women and a more modest reduction for white women, though they do find that abortion legalization did significantly lower the marriage rate for the white teens in the sample. While it turns out that abortion legalization did not lead to significant improvements in educational or labor market outcomes for white women, black women exhibited significant gains in both categories as a result of legalization. Specifically, they find that the likelihood of a black woman graduating high school decreases by about 25 percent per out-of-wedlock child born. They find an effect of similar magnitude if they examine college entrance likelihood. They also find a negative effect between having out-of-wedlock children and employment rates and income levels.⁸⁵

A recent working paper by Sonia Oreffice suggests an alternate channel through which abortion legalization improves women's opportunities. Using a family bargaining model, she argues that, by giving women control over their fertility, their relative bargaining position in their families. Using data from the Panel Study of Income Dynamics for the period 1970-1979, she tests this hypothesis by examining the effect of legalization on married women's labor supply relative to the labor supply of married men, married women who were past their fertile years, and unmarried men and women. She finds that fertile married women reduced their labor supply significantly as a result of abortion legalization, while married men increased theirs. There was no significant change in any of the other comparison groups, implying that the bargaining position of fertile married women did improve as a result of legalization.⁸⁶

This suggests an interesting dichotomy when compared to Akerlof, Yellen, and Katz's model. Specifically, the availability of abortion lowers the bargaining power of unmarried women, making them less able to demand support and marriage promises in return for sexual services. However, Oreffice's model implies that abortion availability improves the bargaining power of married women who can use their control over fertility decisions to extract concessions from their husbands. While each of these hypotheses is extremely provocative, both need more empirical support to be entirely convincing.⁸⁷ However, they do suggest that there might be

⁸⁴ Joshua D. Angrist and William N. Evans (1996), "Schooling and Labor Market Consequences of the 1970 abortion reforms," NBER Working Paper: 5406.

⁸⁵ Angrist and Evans (1996) at Table 7.

⁸⁶ Sonia Oreffice (2003), "The Legalization of Abortion and Women's Bargaining Power in the Household: Evidence from Labor Supply," manuscript available at <http://home.uchicago.edu/~soreffice/abortion.pdf>.

⁸⁷ For example, in Oreffice's analysis, it is not possible to determine whether a woman is fertile or not (or, on a related note, whether the couple had any intentions of having children, or variations in the use of birth control which also tends to put the woman in control of the fertility decision), so she must rely on age proxies. It then becomes difficult to separate bargaining power effects from cohort effects. For Akerlof, Yellen, and Katz's empirical work, the effect is only demonstrated in aggregate time series, which also limits the ability to separate policy effects from other correlated effects.

heterogeneity in the welfare effects of increased access to abortion for women, depending upon the characteristics of the individual.

IV. PUBLIC FINANCE EFFECTS OF ABORTION LEGALIZATION

Although this article has already viewed a number of topics that have significant implications for public expenditures, such as the health costs associated with treatment for STDs (which are borne disproportionately by public clinics) and the direct and indirect costs of crime, there are a number of other studies that have looked at the relationship between government spending and variation in abortion access.

One of the most interesting and powerful examples of these studies is a paper by Jonathan Gruber, Phillip Levine and Douglas Staiger that examines the question of how much public money is saved because certain children are not born. Drawing on the same intuition as the Donohue and Levitt work on crime, Gruber *et.al.* conjecture that aborted babies are not a random draw from all conceptions.⁸⁸

Specifically, they examine who is the “marginal child.” That is, who is the child who goes unborn as a result of abortion legalization? The researchers hypothesize that if there is positive selection, women will choose (directly or indirectly) abortion to avoid bringing a child into an unfavorable environment, improving the average living standards of the children who are born.⁸⁹ On the other hand, negative selection would imply that relatively well-off mothers will choose abortion, while lower income mothers will be limited in their access. This fiscal constraint effect would lower average living standards as the (presumably) worse off children will be weighted more heavily in determining average living circumstances when the children who would have been born to better off women are excluded from the calculation because they are aborted.

The resolution of the question of which type of selection is taking place is important in the Joyce/Donohue and Levitt debate discussed above. In his conclusion, Joyce suggests that any actual change in selection induced by legalization is likely to have been negative, since previous research suggests that better educated and higher income teens and older women are more likely to seek an abortion in the event of an unwanted pregnancy.⁹⁰

⁸⁸ Jonathan Gruber, Phillip Levine, and Douglas Staiger (1999), “Abortion Legalization and Child Living Circumstances: Who Is the Marginal Child?” *Quarterly Journal of Economics*, 114(1): 263-291.

⁸⁹ The improvement in the average does not arise because the aborted children, in any sense, would have taken resources away from other children (though Gruber’s results do suggest that born children will have more resources over the course of their lives because the others are aborted since the aborted children would appear to have been net drains on society’s resources had they lived). Rather, the improvement in the average occurs simply because the (aborted) children, who would have been born into the worst living standards, are not included in calculating the average.

⁹⁰ On this point, Joyce cites Arleen Leibowitz, Marvin Eisen, and Winston Chow (1986), “An Economic Model of Teenage Pregnancy Decision-Making,” *Demography*, 23(1): 67-86 and Hotz, McElroy, and Sanders

Donohue and Levitt counter that Joyce focuses only on the probability of abortion conditional on being pregnant. Since lower income, less educated women are more likely to develop an unwanted pregnancy, their absolute probability of receiving an abortion is higher than their better-off counterparts, implying that any selection effect will be positive.⁹¹ In some ways then, Gruber's results have an indirect bearing on the abortion and crime debate.

The Gruber results unambiguously support the positive selection premise. According to their analysis, the marginal child, had he not been aborted, would have been 60 percent more likely to have been raised in a single parent household, 50 percent more likely to live in poverty, 45 percent more likely to live in a household that collects welfare, and 40 percent more likely to die during the first year of life.^{92,93} In the aggregate, they estimate that this positive selection effect reduced welfare payments in 1980 (the year of the data used in their analysis) by \$480 million. Extrapolating their results further, they estimate that had all children living in 1980 been exposed to legalized abortion, welfare payments would have been \$1.1 billion lower in 1980.^{94,95}

As discussed above, the research by Klick and Stratmann on STDs and Donohue and Levitt's work on crime also suggest significant public finance effects. The additional treatment expenses (borne primarily by public clinics) arising from the increase in STD rates occasioned by abortion legalization account for between \$300 million and \$4 billion annually, depending on how broadly their results can be extrapolated.⁹⁶ Donohue and Levitt's research suggests an abortion related savings in decreased crime on the order of \$30 billion per year.⁹⁷

(1999). For studies examining the decision for women generally, see Eve Powell-Griner and Katherine Trent (1987), "Socioeconomic Determinants of Abortion in the United States," *Demography*, 24(4): 553-561 and Janet Currie, Lucia Nixon, and Nancy Cole (1995), "Restrictions on Medicaid Funding of Abortion," *Journal of Human Resources*, 31(1): 159-188.

⁹¹ Implicit in this answer to Joyce's argument is the assumption that legalization does have a significant effect on access to abortion. If, as Joyce claims, legal abortions largely replaced illegal abortions, legalization *per se* will not have an appreciable selection effect. One possibility that goes unexplored by either Joyce or Donohue and Levitt is that, while legalization might not have had an appreciable effect on aggregate abortion rates, it might have changed the mix of abortions. That is, it could be the case that individuals differ in their propensity to seek an abortion based on its legality, with the propensity being correlated with socioeconomic status. Further, if legalized abortion drove illegal abortion providers out of business, this market shift could also have important effects on propensity that differ according to socio-economic status.

⁹² Gruber, Levine, and Staiger (1999) at 265.

⁹³ Assuming this last result is robust, we would expect this to cut against Donohue and Levitt's crime finding, given that a large fraction of the "unwants" apparently die during their first year and thus would not have grown up to be criminals.

⁹⁴ Gruber, Levine, and Staiger (1999) at 289.

⁹⁵ It is interesting to note that these results indirectly refute the thrust of the point made in Akerlof, Yellen, and Katz (1996) or, at least, suggest that it is not consequential empirically because the group of women holding out against abortion is relatively small, implying that the gains to those who would consider abortion more than wipe out the losses incurred by the hold-outs.

⁹⁶ Klick and Stratmann (2003) at 431.

⁹⁷ Donohue and Levitt at 414.

The large shortcoming of each of these estimates, however, is the failure to provide a corresponding estimate on the other side of the balance sheet, so to speak. As mentioned earlier, this is probably not possible empirically in the case of STDs since we are limited in our ability to evaluate the value of the increase in risky sex to the individuals whose behavior changes as a result of the abortion-induced moral hazard.⁹⁸ However, for the Gruber study, it should be possible to determine what the foregone net tax receipts would have been for the aborted cohort. It might be the case that these receipts would have been negligible, but it is an empirical question. Although a similar analysis for the Donohue and Levitt study is less obvious, there is a potentially important caveat to their crime savings estimate. Because many crimes are perpetrated on individuals in the same class as the criminals, some of the crime reduction could, in theory, be the result of fewer victims existing because of abortion. While there are good reasons to assume that victims are highly substitutable, implying that crime levels are not determined by the number of targets but rather by the number of criminals, it is an open question since supply and demand are jointly determined in this market.

CONCLUSION

The legalization of abortion and subsequent changes in abortion availability provide numerous “natural experiments” for researchers to examine important issues in behavioral science. Applied econometricians have exploited these experiments to generate a veritable cottage industry of abortion studies, examining everything from sex to crime. These studies are the source of heated debates both within and outside of the academic literature, and it is likely that research in this area will continue to generate provocative results. Because this literature is fairly technical, however, it will be difficult for policymakers to draw informed inferences about abortion law. It is more likely to be the case that lawmakers will gravitate toward the research that supports their pre-existing priors on the subject. Such a situation generates a special responsibility for researchers in this area to be especially circumspect of their results and to make pains not to overstate the evidence for their hypotheses, otherwise we will rightly be shut out of the abortion policy debates.

⁹⁸ We might be able to get a ballpark estimate using contingent valuation methods, or market prices for prostitution. The problems associated with the former valuation method have been explored in great detail elsewhere. See, for example, Peter A. Diamond and Jerry A. Hausman (1994), “Contingent Valuation: Is Some Number Better Than No Number?” *Journal of Economic Perspectives*, 8(4): 45-64. The latter method would likely suffer from large data availability problems. Even the most comprehensive summary of the data available on prices in the prostitution market indicates substantial gaps, Lena Edlund and Evelyn Korn (2002), “A Theory of Prostitution,” *Journal of Political Economy*, 110(1): 181-214 at 190-191.

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