"Momma's Got the Pill":
How Anthony Comstock and *Griswold v. Connecticut* Shaped U.S. Childbearing

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Abstract
The 1960s ushered in a new era in U.S. demographic history characterized by significantly lower fertility rates and smaller family sizes. What catalyzed these changes remains a matter of considerable debate. This paper exploits idiosyncratic variation in the language of "Comstock" statutes, enacted in the late 1800s, to quantify the role of the birth control pill in this transition. Almost fifty years after the contraceptive pill appeared on the U.S. market, this analysis provides new evidence that this technology accelerated the post-1960 decline in marital fertility.

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During the 1960s, significant changes in women's decisions regarding marriage, childbearing, and work ushered in a new era of U.S. demographic history. Over the next twenty years, U.S. fertility rates fell by 50 percent (figure I), and the proportion of women having fewer than three children increased from 42 to 66 percent (figure II).[1] Almost half a century later, the American family and labor force remain fundamentally altered.

Although hundreds of articles in academic and popular journals document these trends and speculate about their origins, the underlying causes of this transition remain a subject of scholarly debate. The demographic literature has emphasized the role of technology in regulating the supply of births and, in particular, oral contraception, better known as "the Pill." Armed with national survey evidence, many population scientists heralded the 1960s as a period of "contraceptive revolution" characterized by substantial declines in unplanned marital childbearing (Norman B. Ryder and Charles F. Westoff 1971, Westoff 1975) and a significant rise in the frequency of marital intercourse (Westoff 1974). In his Presidential Address to the Population Association in 1975, Westoff asserted that "the entire decline in births within marriage across the decade of the 'sixties' can be attributed to the improvement in the control of fertility" (579).[2]

Economists have been critical of this view and have, instead, emphasized changes in the demand for children.[3] Because sharp reductions in the U.S. fertility rate in the 1960s followed the U.S. baby boom, some would argue that this period represents a reversion to the longer-term U.S. trend and not a revolution at all. One need only point to sharp declines in the U.S. fertility during the 1920s to highlight how rapidly behavior might have changed in the absence of the Pill. For these reasons, Gary Becker

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[1] This comparison is made for cohorts entering their childbearing years in different decades. Women born in 1930 (1950) entered their childbearing years during the 1950s (1970s).

[2] Not all demographers were in agreement about the relative importance of unwanted fertility. See, for example, Judith Blake's 1969 article challenging this view.

[3] The longer-term decline in fertility is often attributed to the gradual evolution in the demand for children. This explanation highlights the rising opportunity cost of childbearing associated with increases in women's education, the growth of the clerical sector, and falling discrimination in the workplace. The baby boom interrupted this longer-term decline from around 1940 to 1957.

concludes in his *Treatise on the Family* (1991: 143) that "the 'contraceptive revolution' … ushered in by the Pill has probably not been a major cause of the sharp drop in fertility in recent decades."

The relevance of this debate bears upon theoretical formulations of economic and population growth, many of which implicitly ignore significant changes in the costs of regulating births. It also has substantial implications for economic models of the age distribution, family size and structure, and the composition of the labor force. Finally, it shapes our evaluations of domestic and international family planning policies, which have been the subject of heated scholarly discourse (Lant H. Pritchett 1994a, 1994b; John Bongaarts 1994, James Knowles, John S. Akin, and David K. Guilkey 1994).

Yet fifty years after the advent of the Pill, it is unclear whether the 1960s were a "contraceptive revolution" or a revolution of the times. This uncertainty is due largely to the difficulty of disentangling the contribution of modern contraception from a variety of concurrent demographic, social and economic changes. This is especially true in the United States where the 1957 release of *Enovid*, later approved as the first oral contraceptive, coincided with the peak of the baby boom and rapidly changing awareness, attitudes, and norms about women's rights and roles.[4] Recent empirical work on the Pill provides limited answers, because the legal variation used for identification is concentrated in the early 1970s and only affected access to the Pill among unmarried, childless women under age 21 (Claudia Goldin and Lawrence Katz 2002, Martha J. Bailey 2006, Melanie Guldi 2008). It is also likely that women who were married, had made their human capital investments, and had begun their families responded differently to the introduction of oral contraception. The "power of the Pill" for the population of married women—a group accounting for over 80 percent of the decline in the total fertility rate over the 1960s (Campbell Gibson 1976)—remains an open question.

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[4] In 1963, President John F. Kennedy's Commission on the Status of Women released a report documenting pervasive discrimination against women. Betty Friedan published the best-seller, *The Feminine Mystique,* in 1963, which documented the emotional and intellectual oppression of the middle-class housewife. In 1964, Title VII of the Civil Rights Act codified a prohibition on employer discrimination against women. In 1966, the National Organization for Women was organized by the NAACP. By the end of the decade, the women's movement had become instrumental in generating awareness of gender inequities and creating opportunities for many women. This simultaneity of events renders standard inter-temporal comparisons difficult to interpret.

This paper quantifies the importance of the birth control pill on marital fertility using a newly compiled history of U.S. anti-obscenity statutes better known as "Comstock laws." Although 47 of the 48 coterminous states enacted anti-obscenity laws (most before 1900), idiosyncratic differences in language had an important impact on their relevance for contraceptive access by the 1960s. For instance, only 31 states explicitly enumerated "contraception" among the regulated obscenities, and language in only 24 states additionally banned the "sales" of contraceptive supplies. These sales bans remained on the books and induced substantial cross-state variation in the price of obtaining and using the birth control pill in the early 1960s. The U.S. Supreme Court's 1965 *Griswold v. Connecticut* decision, which struck down Connecticut's ban on the use of contraceptives, altered enforcement and compliance across the U.S. In the aftermath of this ruling, state legislatures actively revised their laws to permit sales of contraceptives to married women.

This paper's empirical strategy exploits these legal differences, the timing of the introduction of the birth control pill, and the timing of the *Griswold* decision to explore the importance of oral contraception for married women during the 1960s. The first component of the empirical analysis establishes both the relevance and the validity of using statutory bans on contraceptive sales to identify the impact of the Pill. Using data from the 1955 *Growth of American Families Study* (*GAF*) and 1965 and 1970 *National Fertility Studies* (*NFS*), differences-in-differences, regression-adjusted estimates provide the first quantitative evidence that Comstock sales bans slowed the diffusion of oral contraception before the *Griswold* decision in 1965.[5] Women in states with sales bans were 25 to 30 percent less likely to have ever used oral contraception before the *Griswold* decision relative to women in the same census regions without these laws—even after adjusting for a host of observable characteristics. The absence of similar

[5] Previous research has been limited by the availability of data with which to test the effectiveness of laws restricting access to the birth control pill (Goldin and Katz 2002, Bailey 2006, Guldi 2008). This is because very little information exists on the attitudes or contraceptive choices of unmarried women during the 1960s and early 1970s. Goldin and Katz (2002) use one cross-section that contains both information on contraceptive choices and state identifiers to verify the relevance of early consent laws for unmarried women's contraceptive decisions.

patterns for other types of contraceptives from 1955 to 1965 and strong, post-1965 convergence in the use of oral contraception in states with sales bans is consistent with statutory bans driving these differences.

The paper's main analysis focuses on quantifying the importance of the birth control pill for marital childbearing in the 1960s. The central results suggest that oral contraception accelerated the reduction in period birth rates and facilitated the eventual transition to the two-child family. The implications of these results reach beyond the 1960s, as lower fertility rates have had a lasting impact on the American family and economy.

I. The Theoretical Relationship of the Cost of Contraception and Number of Children

The pioneering work of Robert T. Michael and Robert J. Willis (1976) laid the conceptual foundation for understanding the role of both the demand and the supply of children in a unified theoretical framework. This framework supplements the now standard, neoclassical approach to modeling the demand for children (Becker 1960, 1965; Willis 1973; Becker and H. Gregg Lewis 1973) with the "supply-side" that is stressed in the demographic literature (Mindel C. Sheps 1964, Sheps and Edward B. Perrin 1966). This approach relaxes two assumptions that limited the potential role of improvements in contraceptive technology or changes in its price.[6] First, Michael and Willis allow pregnancy to occur probabilistically rather than deterministically. The number of children is treated as a random variable, and couples choose a contraceptive strategy to reduce the monthly probability of conception. This is equivalent, under the assumptions in their model, to choosing an *ex ante* distribution of the number of children born that is summarized by its first moment, μ, (the focus of this paper's discussion) and the second moment (omitted from this discussion for simplicity). Second, they relax the assumption that fertility regulation is costless. In their framework, a contraceptive strategy *j*—the

[6] In the Willis (1973) and Becker and Lewis (1973) frameworks, better and lower-cost contraception may only affect the demand for children by reducing the relative price of child quality and, thus, may have induced substitution toward fewer children. Becker and Lewis (1973), for instance, assume that household utility, U(C,Z), is increasing in "children", C=NQ, and a composite commodity, Z. C is increasing in both the quantity of children, N, and child "quality", Q. Consumption is limited by lifetime income, I, the "price of child services," π_c, and the per-unit cost of the composite commodity, π_z. Households choose the optimal number of children deterministically, $N^*=h(\pi_c, \pi_z, I)$, such that, $V(N^*) \geq V(N') \; \forall \; N' \neq N^*$, where $V(N)=\max_{Q,Z} \{U(N,Q,Z) \text{ s.t. } \pi_c NQ \; \pi_z Z \leq I\}$. As noted in Becker and Lewis (1973: S283), the price of birth control may impact childbearing outcomes in this framework by affecting the relative price of child quality.

adoption of a behavior or use of contraceptive supplies over a period of time—is associated with a price, π_j, and couples maximize utility by weighing the marginal costs of averting each birth against the marginal benefit of attaining each *ex ante* distribution of childbearing outcomes. Therefore, couples optimize by choosing a distribution of possible childbearing outcomes, μ^*, to maximize expected utility net of the costs of fertility regulation.

Reference to the marginal costs of adopting a contraceptive strategy is particularly apt in describing changes in contraceptive technology during the 1960s. Consider, as Michael and Willis do, a simple division of costs of attaining a fertility distribution, μ, using contraceptive strategy j, where strategy j entails a fixed, α_j, and a marginal cost, β_j. The total cost of using strategy j to attain an *ex ante* birth distribution, μ, is given by $\pi_j \equiv \pi_j(\mu) = \alpha_j + \beta_j(\mu_N - \mu)$, where μ_N is the mean of the expected distribution of children born in the absence of any contraceptive method. $\mu_N - \mu$ is, therefore, the expected number of births averted. In this context, the marginal cost of averting a birth might be characterized in terms of a behavioral cost (as with abstinence or withdrawal), the inconvenience or discomfort of birth control use at the time of intercourse (as with barrier methods), or the necessity of purchasing refills of supplies (as with condoms or the birth control pill). Fixed costs might include costs such as the price of searching for a supplier and learning about a particular method. For the sake of illustration, panel A of figure III plots an example of total costs by contraceptive strategy and births averted. It is easy to see that different contraceptive strategies may be optimal depending upon the number of births a couple wishes to avoid. For instance, if a couple wishes to avoid one birth, then a strategy that entails a negligible fixed cost but higher marginal cost (like withdrawal, represented by line π_1) may be optimal. A couple wishing to avoid two births may choose the strategy associated with π_2; the high fixed but low marginal cost of strategy π_3 would make it the lowest cost option for preventing five or more conceptions. In this sense, the total cost function for achieving any birth distribution, μ, is given by the bolded, lower envelope, or $C(\mu) = \min_j\{\alpha_j + \beta_j(\mu_N - \mu)\}$.

The introduction of the birth control pill may have altered the total cost function in a manner suggested by strategy π_4 illustrated in panel A of figure III, because the fixed cost of using the Pill would have been at least as high as any other method available in the 1960s. However, the marginal cost of preventing births with the Pill was much lower, as it required no interruption, effort or discomfort at the time of intimacy. As a result, the availability of oral contraception may flatten a portion of the total cost curve (illustrated in the line segment above 2 births averted associated with strategy π_4).

In summary, the Michael and Willis framework yields several important predictions about the diffusion and adoption of the birth control pill. First, the Pill reduced the marginal costs of preventing births above some threshold. Holding the demand for births constant, reducing the marginal costs of preventing births for some women should lead to a reduction in the number of children born per woman. Second, the birth control pill reduced the uncertainty surrounding childbearing outcomes, because it was much more effective than other methods for a given amount of effort. The impact of a reduction in uncertainty, however, is more complicated to assess. On the one hand, greater reliability minimizes the chance of having more children than intended. However, a more reliable contraceptive technology may increase the number of children borne by eliminating the risk of overshooting. For these reasons, the impact of oral contraception on the number of children born over the lifecycle is theoretically ambiguous.[7] By

Another implication of the Michael and Willis framework is that the adoption of the birth control pill is strongly related to the desired number of births to be averted. This has an important relationship to expected duration of use and, thus, to age. Because younger women may desire to avert more births over their remaining lifetimes than older women *after* the Pill is introduced, younger women would be more likely to adopt the birth control pill. In addition, because the desired number of children reflects the demand for children (market prices, household income, and preferences) *and* also determines a woman's

[7] See Michael and Willis (1976) for a succinct treatment.

likelihood of adopting the Pill, comparisons of childbearing outcomes among women using and not using the birth control pill reveal little about the importance of demand or supply factors.[8]

To identify the impact of changes in birth control technology, an ideal experiment would compare the childbearing outcomes of women with identical preferences and household incomes, who face identical market prices except for the cost of regulating births. This paper argues that variation in the restrictiveness of Comstock legislation provides natural randomization in the cost of adopting the birth control pill. The next sections provide historical and quantitative support for these assumptions.

II. A Brief History of Comstock Laws and *Griswold v. Connecticut*

In 1873, the U.S. Congress codified the "Comstock Act" outlawing the interstate mailing, shipping or importation of articles, drugs, medicines, and printed materials of "obscenities," which applied to anything used "for the prevention of conception" (18 U.S.C. §1461-1462).[9] The Act also succeeded in its secondary purpose, which was to "incite every State Legislature to enact similar laws" (C. Thomas Dienes 1972: 43, quoting Representative Merrimam, *New York Times*, Mar. 15, 1873, p. 3, col. 3). By 1900, at least 42 states had enacted or amended anti-obscenity statutes, which directly regulated trade in "obscene" or "immoral" information within states. By 1920, at least 45 states had these laws.[10]

State statutes varied considerably in their language and, consequently, their ultimate implications for access to contraception in the 1960s. Table I documents the date of enactment and groups these laws into four broad categories (see online legal appendix for more information on the exact language and history in each state and the following sources: Dennett 1926, Dienes 1972, Department of Health, Education and Welfare 1974, and Smith 1964).

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[8] This bears on the relevance of Pritchett's assertion that "the challenge of reducing people's fertility is the challenge of reducing people's fertility desires" (1994:3). In the Michael and Willis model, desires for children determine the choice of contraception in the same way the marginal costs of averting births determine the desired number of children. See also Richard Easterlin, Robert A. Pollak and Michael L. Wachter (1980).

[9] This Act was comprehensive. It banned any "book, pamphlet, paper, writing, advertisement, circular, print, picture, drawing or other representation, figure, or image on or of paper or other material, or any cast, instrument, or other article of an immoral nature, or any drug or medicine, or any article whatever for the prevention of conception" (Andrea Tone 1996: 488). This Act is often referred to as the "Comstock Act" after its zealous Congressional advocate, Anthony Comstock of New York.

[10] The remaining states may have passed their legislation earlier. The dates reported in Table I are based only on what could be verified in statute books. Scans of these original statutes are posted at the author's website.

1. General obscenity statutes banned the dissemination of obscene information, the sale of obscene information, and the sale of indecent or immoral "articles or instruments." They did not explicitly categorize the prevention of conception as obscene and so are grouped with control states in the analysis.

2. Advertising or information bans in 30 states explicitly outlawed the distribution of information or advertising about articles, instruments, and medicines for the prevention of conception. Although these laws often banned the sale of information about the prevention of conception, they did not ban the sale of contraceptives themselves. If a state only had this type of law, physicians or pharmacists could fill prescriptions initiated by the requests of their patients without violating state law.

3. Sales bans in 24 states explicitly prohibited the sale of any article, instrument, medicine or secret nostrum for the prevention of conception. These laws banned both physicians and pharmacists from fitting diaphragms, selling condoms, and prescribing or filling prescriptions for the birth control pill. (Each of these states also had an advertising or information ban).

4. Physician or pharmacist exceptions in 7 states provided blanket exceptions for physicians and sometimes pharmacists from advertising and sales bans, and most of these statutes contained this language from the date of enactment. Several states have more ambiguous exceptions for "legitimate business." Because it is unclear whether prescribing or selling contraceptives falls under these exceptions, the analysis treats these states as having restrictive sales bans. The online appendix provides results for coding that treats "legitimate business" exceptions as physician exceptions in the analysis.

Figure IV presents the geographic distribution of these legal restrictions (type 2 to 4) as of 1960. All shaded states had some sort of statute that specifically cited the "prevention of conception" as part of an anti-obscenity statute. The light gray states had advertising and informational bans only (table 1, column 2). The dark gray states banned advertising and information as well as sales; these states, however, made exceptions for physicians (table 1, column 4). The black states had advertising and informational bans as well as sales bans and provided *no* exceptions for physicians (table 1, column 3 without "X"s in column 4). The distribution of these statutes, and specifically sales bans, do not appear to reflect particular political delineations of the 1960s. Laws of different types are found in each census region of the country, and their rosters contain states that were progressive in their family planning and social policies. For instance, California and Washington, two of the three coterminous states to repeal abortion bans prior to *Roe v. Wade*, actually *restricted* sales and the dissemination of information about contraception, respectively, in 1960 (Phillip Levine, Douglas Staiger, Thomas J. Kane, and David J. Zimmerman

1996).[11] Surprisingly, New York, the home state of Anthony Comstock and one of the states to repeal abortion bans before *Roe*, had exempted physicians from its sales ban since 1873. This seemingly idiosyncratic distribution of sales bans in 1960 provides *prima facie* evidence that sales bans were not markers of conservative or more slowly changing attitudes about family planning—a claim tested later in the analysis.

The implications of these laws in the 1960s for oral contraception varied considerably. Statutes limiting advertising or the distribution of information, for instance, were probably not very effective. These laws did limit direct advertising and informational brochures in physicians' offices, but physicians could prescribe contraceptives upon the request of a patient. Also limiting the effectiveness of advertising bans was the murkiness of distinctions between scientific and obscene information. G.D. Searle, the patent holder for *Enovid*, actively pursued "scientific" coverage about the clinical trials of the drug for use as a contraceptive, including features in highly visible, national periodicals like *Time, Business Week, Consumer Reports*, *Fortune*, *Reader's Digest* and the *New York Times* (Elizabeth Siegel Watkins 1998). Similarly, the company cultivated awareness of *Enovid*'s contraceptive properties through mass mailings "warning" physicians about *Enovid*'s anovulant "side-effects." Although *Enovid* was only approved for the regulation of menses in 1957, the public and physicians were well aware of *Enovid*'s contraceptive properties before it was approved for this purpose by the FDA in 1960.

In contrast, statutes banning sales *without* physician exceptions (henceforth referred to as "sales bans") may have had a direct impact on the availability of contraceptives, in general, and the birth control pill in the early 1960s more so than other methods. First, the threat of penalties, fines and jail time could reduce the number of providers for all types of contraceptives. The stakes for professional physicians and pharmacists—exclusive distributors of the birth control pill—would have been especially high, because

[11] Another pattern is the absence of explicit contraceptive bans in the South. My reading of obscenity statute texts suggests that the primary explanation for this is that Southern states had older and broader statutes prohibiting the "corruption of morals." Because these statutes predated the 1870s Comstock amendments and revisions, they did not have language outlawing the "prevention of conception." The implication is that statutes in most of the South could accommodate the evolution of social norms about obscenity and contraception, whereas codified prohibitions on contraception provided much less flexibility.

illegal prescriptions and sales risked their licenses and livelihoods. Diaphragms and condoms, on the other hand, could be purchased through the mail, and condoms could be obtained from gas station clerks or vending machines. The distributors of these products stood to lose much less by selling contraceptive contraband, and these informal supply channels may have functioned quite well (Tone 2000, 2002).[12] Second, unlike other contraceptives, production of the birth control pill required the synthesis of a chemical compound, which probably limited black market production in the shorter run. One final reason that sales bans may have had more bite for the birth control pill is that women could neither verify their safety (ingesting chemicals of unknown quality poses health risks) nor their effectiveness. In contrast, the quality of other illegally obtained methods (such as diaphragms or condoms) had an established reputation and was easier to verify by visual inspection. Coupled with the more frequent need for refills and physician supervision, illegally produced or procured birth control pills may have entailed higher fixed and marginal prices relative to other methods.

These arguments imply that—even without perfect enforcement or compliance—sales bans should make the total cost curve steeper and increase the threshold (in terms of desired births averted), above which women would adopt the Pill. These arguments can be integrated into the total cost framework presented in the previous section in the form of mark-ups, t, on both the fixed cost and marginal costs, $\pi_j^S=\alpha_j(1+t_{\alpha j})+\beta_j(1+t_{\beta j})\ (\mu_N-\mu)$, where the superscript, S, references the total cost function in states with sales bans, $t_\alpha \geq 0$ is the increase in the fixed cost, and $t_\beta \geq 0$ is the mark-up in the marginal cost. Note that a sales ban should have no effect on the price of methods requiring no supplies, such as abstinence, rhythm or withdrawal ($t_\alpha=0$, $t_\beta=0$). For methods requiring fairly low fixed costs but frequent purchase of supplies (sponges, spermicidal jellies, or condoms), sales bans would tend to raise total costs by increasing t_β. For methods like the diaphragm which require a fitting by physician but rare return visits, sales bans would tend to raise total costs by increasing t_α. In both cases, a codified exception for physicians would tend to

[12] There is ample evidence in published newspapers that customers obtained contraceptives through the mail under the auspices of "feminine hygiene" or "womb support." Similarly, male "sheaths" or "shields" and "uterine elevators" were also available for the "prevention of disease."

mitigate the burden of the sales bans by reducing t_α and t_β. As illustrated in panel B of figure III, the arguments above suggest that t_α and t_β would have been larger for the birth control pill than for other methods.

These state laws survived virtually unaltered until the 1960s, when legal challenges in several states and the U.S. Supreme Court's 1965 decision in *Griswold v. Connecticut* in others prompted legislative revisions (table I, column 5). Although the *Griswold* decision enjoined a provision in Connecticut's 1879 law, which uniquely prohibited the "use" of contraception, state legislatures across the U.S. responded by repealing or amending their obscenity statutes in the aftermath of this ruling.[13] By 1971, almost every state had eliminated its bans on contraception sales to married individuals (Harriet Pilpel and Nancy F. Wechsler 1969; Department of Health, Education and Welfare 1974).[14]

III. Using Comstock Sales Bans to Assess the Impact of Oral Contraception on Birth Rates

Three historical fertility studies facilitate direct tests of the identifying assumptions of the analysis. The 1955 *Growth of the American Families* Study (*GAF*) and the 1965 and 1970 *National Fertility Studies* (*NFS*) provide snapshots of contraceptive use among married women before the birth control pill was introduced (1955), in the year of the *Griswold* decision (1965), and five years after *Griswold* (1970). They also contain a rich set of highly comparable (and sometimes identical) questions about respondents' and their husbands' characteristics. State identifiers also allow linking with state-level Comstock laws in table I.[15] Only currently-married, white women ages 18 to 39 were sampled in all three of the surveys. The restriction of the sampling frame to white women is unfortunate, but the limitation to married women poses little problem for this paper's focus on marital women, the main group benefitting from *Griswold*.[16]

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[13] William O. Douglas, writing for the majority, explicitly contrasted Connecticut's law, which forbade the "use of contraceptives", with other states' laws, which merely regulated "their manufacture or sale."

[14] It is unclear whether compliance or enforcement responded to the *Griswold* decision or to the letter of the state law. There is some evidence, for instance, that state programs encouraging and subsidizing family planning were enacted before state sales bans were repealed. For this reason, the analysis will not make use of the date of repeal or amendment of these statutes.

[15] The 1960 *Growth of American Families* study also contains important information on the use of contraceptives, but the publicly-available survey does not contain state identifiers. The data appendix contains more information on the surveys, sampling frames and summary statistics for each of the variables used in the analysis.

[16] *Griswold* extended the rights of married couples only.

This section pools these surveys and provides timeseries and regression-adjusted, difference-in-differences estimates to demonstrate both the relevance and validity of using Comstock sales bans to identify the Pill's impact on marital fertility. The evidence shows that the diffusion of oral contraception was slower in states with sales bans but that this pattern reversed after the *Griswold* decision. These surveys also reveal that differential use of contraception in states with sales bans was limited to the birth control pill, which helps exclude alternative explanations such as differential shifts in the demand for children in states with sales bans and misreporting in states where the birth control pill was illegal.

Did Sales Bans Slow the Diffusion of Oral Contraception?

The historical and legal literatures provide mixed evidence on the importance of Comstock laws, so a central issue in this analysis is whether sales bans reduced the use of oral contraception at all.[17] Justice Felix Frankfurter's 1961 opinion for the majority in an earlier case challenging Connecticut's anti-obscenity statute, *Poe v. Ullman*, noted that, "neither counsel nor our own research have discovered any other attempt to enforce the prohibition of distribution or use of contraceptive devices by criminal process" (367 U.S. 497, 1961). A legal literature has grown from this opinion that treats *Griswold* as a test case about a relatively unimportant and unenforced statute.

There is, however, considerable anecdotal evidence that Connecticut's statute did matter. David J. Garrow's account of this period notes that on the very day the U.S. Supreme Court was voting to dismiss *Poe*, the *Wallingford Post* reported that Thomas Coccomo was arrested for possessing approximately $100 worth of contraceptives (1994: 188). Most notably, the family planning clinic opened by Estelle Griswold and C. Lee Buxton was raided by the police after operating for nine days in 1961. This public event was evidence that Connecticut's law was being enforced and, as a result, provided legal grounds to challenge Connecticut statute—the case that resulted in the 1965 *Griswold* decision. In other less-

[17] For instance, an article in *Medical World News* in 1960 noted that Connecticut physicians were aware of the state's ban on sales and use of contraceptives, but regularly ignored it. One physician said, "Most of us doctors are usually very strict in following the laws relating to medical practice, even when we don't fully agree with them. But this is one law we don't obey." Citing this article, the *New York Herald Tribune* reported that "birth control devices are just as widely used in Connecticut as in any other state" (September 24, 1960: 12).

researched states, there is almost no evidence about the importance of sales bans or other anti-obscenity statutes. Without further evidence, it is impossible to know the extent to which Connecticut's and other states' laws affected most couples' decisions about contraception. The purpose of this section is to provide the first quantitative evaluation of these laws' importance.

As a point of departure, it is useful to consider the popularity of oral contraception among U.S. women in the early 1960s. Among those sampled in the 1970 *NFS*, roughly 15 percent of nonwhites and 25 percent of whites reported having used the Pill by 1965 and these figures rose to 50 and 60 percent, respectively, by 1970. Before 1965, figure V shows that the adoption of oral contraception was slower in states with sales bans. Using the 1965 *NFS* only, panel A plots the differences in the proportion of women ever using the birth control pill (as a fraction of all women residing in those states at the time of the survey) between states with sales bans and other states in the same census region. The quarterly fluctuations in the series are due to small sample sizes and problems of recall (observations are heaped in certain months like January or July). The overall trend in each census region, however, is that use of oral contraception was 3 to 8 percentage points, or 20 to 25 percent, lower in states with sales bans in 1965.

Panel B repeats this exercise for a pooled sample of the 1965 and 1970 *NFS*, which allows comparisons in the cumulative proportion of women using the birth control pill over the entire decade of the 1960s.[18] Notwithstanding potential recall bias or cross-state mobility of respondents after 1965 in the 1970 *NFS*, the results in pooled sample are similar to the 1965 *NFS* in the Midwestern and Western census regions, where women in states with sales bans are slower to begin using the birth control pill before 1965. Following *Griswold*, the proportion of women ever using the Pill in states with sales bans in these regions rapidly converged on rates among women in other states. By 1967, the differences in use appear to have disappeared completely.

The levels in the Northeast are different in the pooled sample and the 1965 *NFS*. Although use of birth control pill was lower in the Northeast in states with sales bans in panel A, there appears to be very

[18] Because there are so few observations in the Southern states with sales bans in the 1970 *NFS*, the South is omitted.

little difference in use before 1965 in the 1970 data.[19] Even so, the change in trends in the aftermath of *Griswold* is striking. After five years of proportional increases in both groups of states, the increase in use of the Pill *among new users* in states with sales bans outstripped increases in states without these bans by 4 percentage points by 1967 and another 2 percentage points by 1970. The post-*Griswold* increase is consistent with the idea that women in Connecticut, Massachusetts and New Jersey (states banning the sales of contraceptives in the Northeast) would have been using the birth control pill in greater numbers had laws not been so restrictive.

Panel C of figure V adjusts the comparisons for the number of women across states who were not potential birth control pill users. Instead of counting all women observed in the state in the denominator, panel C only includes women who had used the birth control pill at some point by the time of the 1970 *NFS*. The idea is to compare the *timing of adoption* among all women who eventually opted to use the birth control pill. Although each of the series should approach zero as dates approach the 1970 survey date, it is striking that the largest difference in the proportion of women ever using the Pill—approximately 6 to 10 percentage points—is observed on the eve of the *Griswold* decision. Just as striking is the rapid disappearance of this difference only two years after *Griswold* was decided.

The robustness of these trends to finer designations of legal regime and adjustments for changes in observable characteristics is explored within the following differences-in-differences framework,

$$\text{(I)} \qquad Pr(C_{ist} = 1) = F(\, Sales_s \boldsymbol{f}_t' \boldsymbol{\tau}_1 + Exception_s \boldsymbol{f}_t' \boldsymbol{\tau}_2 + Advertising_s \boldsymbol{f}_t' \boldsymbol{\tau}_3 + g_{rt} + \boldsymbol{X}_{ist}' \boldsymbol{\delta})$$

where *C* is a binary variable equal to 1 if respondent *i* in state *s* observed in year *t* ever used the Pill; *Sales* is a binary variable equal to one if state *s* had a sales ban (*Sales*=1 for all states checked in column 3 of table I); *Exception* is a binary variable equal to one if a state had a codified physician exception (*Exception*=1 for all states checked in column 4 of table I); and $\boldsymbol{f}_t' = [1, 1(t = 1970)]$ is vector where the first element is a constant and the second element is an indicator variable equal to one if the individual is

[19] Slight differences in the sampling frame may have induced these differences: 1965 survey included Rhode Island whereas the 1970 survey included New Hampshire. On the other hand, measurement error induced by mobility between states may have biased the estimates from the 1970 *NFS*.

observed in 1970. The coefficients of interest are the elements of $\boldsymbol{\tau}_1' = [\tau_{1,1},\ \tau_{1,1970}]$: $\tau_{1,1}$ is the point estimate on the interaction of *Sales* and the constant, and $\tau_{1,1970}$ is point estimate on the interaction of *Sales* and the indicator for observation in 1970. The implied marginal effect associated with the former captures the average difference in Pill use in states with sales bans versus the comparison group (subsequently discussed in detail) in 1965 and the latter how this difference changed from 1965 to 1970.

The analysis examines the sensitivity of the estimates to various comparison groups. In practice, these comparison groups are created by adding the remaining variables in equation I sequentially. The baseline specification includes only *Sales*, *Exception,* and the interaction of census region dummy variables (Northeast omitted) with the elements of $\boldsymbol{f_t}$ represented by g_{rt}. These comparisons are similar to those in figure V, and the marginal effects of interest capture differences between states with restrictive sales bans (and no physician exception) and those without bans in same census region in 1965 and changes in this difference from 1965 to 1970.[20] Next I refine the baseline comparison by including the interaction of *Advertising* (*Advertising*=1 for all states checked in column 2 of table I) and the elements of $\boldsymbol{f_t}$. In this specification, the implied marginal effects of interest capture the 1965, within-census region difference in Pill use between states with sales bans and states with advertising bans only and changes in this difference by 1970. Because advertising bans were quite similar in language but should *not* have restricted physician behavior, this specification tests whether having a sales ban *per se* drives the results. Finally, I refine the comparison group by adjusting the estimates for differences in observable characteristics, $\boldsymbol{X}$, potentially correlated with the demand for children under the Michael and Willis model (age, Catholicism, woman's education, husband's income, and ideals about children).[21]

Table II presents average marginal effects obtained from probit specifications, and robust standard errors corrected for correlation within state are presented in brackets beneath each estimate (Manuel Arellano 1987; Marianne Bertrand, Esther Duflo, and Sendhil Mullainathan 2004). Columns 1 to 4 alter

[20] Treating states with sales bans and physician exceptions as a separate group avoids implicitly assuming they are most comparable to states with sales bans or the comparison group.

[21] Ideals about children are taken from a survey question about the "ideal number of children in the average American family." These variables are captured in $\boldsymbol{X}$ in equation I and are described in details in the notes of table II.

the comparison groups as described above (included variables are indicated in the last rows of the table and in the table notes). Across specifications, women in states with sales bans were significantly less likely to have used oral contraception before the 1965 *Griswold* decision. In columns 3 and 4, this estimate is negligibly affected by adjustments for observable characteristics. Even as more of the variation in the dependent variable is explained by other variables in the model (the pseudo R^2 increases from 0.13 to 0.21 from column 1 to column 4), the estimated difference in 1965 use increases from approximately 6 to 8 percentage points. Despite the small sample sizes, the 1965 differences are statistically significant in each specification.

One final specification (not reported in table II) additionally differentiates between early repeal states (indicated in column 5 of table I) and those that repealed after *Griswold*. I allow use of the birth control pill to evolve separately in these states by adding a dummy variable for early legalization and its interaction with the elements of $\boldsymbol{f}_t$. Because the resulting marginal effects are quite similar to those in columns 1 to 4 (-0.073 for $\tau_{1,1}$, s.e. 0.021; 0.054 for $\tau_{1,1970}$, s.e. 0.025), this specification provides no evidence that early repeal states are driving the results. The estimates across specifications imply that the number of women ever using the birth control pill was approximately 25 to 30 percent lower in states with sales bans by 1965.[22] The age-specific marginal effects also reveal that the effects are strongest among women in their twenties and early thirties.[23]

One interpretation of these results is that demand for the Pill among women in states with sales bans was simply lower for reasons unrelated to statutory differences. If this were the case, then differences in use of the birth control might fail to disappear even as states revised their statutes in the aftermath of *Griswold*. Consistent with timeseries evidence (panel B of figure V), positive marginal effects associated

[22] Specifications that omit census regions one at a time or include all women sampled in 1965 and 1970 yield similar results. These are reported on the online appendix.

[23] I run a regression for the 1965 data only, which interacts each of the variables in equation I with a set of dummy variables for five-year age groups (20-24, 25-29,.., 40-44; 15-19 omitted) in lieu of the interaction with the elements of $\boldsymbol{f}_t$. The average marginal effect for women in the omitted category in states with sales bans is -0.168 (s.e. 0.073) (these women had to be "currently married" to be sampled in 1965); the difference for women ages 20 to 24 (relative to the women in states with sales bans in the omitted category) is smaller than for the omitted group with a relative marginal effect of 0.100 (s.e. 0.116); for women ages 25 to 29 it is 0.167 (s.e. 0.110); for women ages 30 to 34 it is 0.102 (s.e. 0.108); for women ages 35 to 39 the average marginal effect is 0.163 (s.e. 0.104); and for women ages 40 to 44 the average marginal effect is 0.196 (s.e. 0.102).

with $\tau_{1,1970}$ indicate that lower use of the Pill in states with sales bans had all but disappeared by 1970. Across specifications, hypothesis tests fail to reject that the sum of the marginal effects implied by $\tau_{1,1}$ and $\tau_{1,1970}$ is equal to zero at any conventional level of significance. That is, omitted variables threatening the interpretation of these estimates as the impact of statutory differences would need to reduce Pill use prior to 1965 but not from 1965 to 1970. The next section explores the potential influence of time-varying unobservables on these findings.

Were Comstock Sales Bans Related to Changes in the Demand for Children from 1955 to 1965?

Two additional explanations are consistent with the results thus far. One is that differential changes in the demand for children (not captured in $\boldsymbol{X}$) in states with sales bans are responsible for the lower use of oral contraception. The second is that respondents may have underreported use of the birth control pill in the pre-1965 period, because they knew it was illegal. If either of these alternative explanations accounts for the differences in Pill use, then the same patterns of use should appear for other types of contraceptives as well.

Table III examines this by modifying the dependent variable in equation I to be the use of contraceptives other than the birth control pill. Because differences in the use of other types of contraceptives were not zero in 1955, I expand the sample to include the 1955 *GAF* and modify equation (I) by redefining $\boldsymbol{f}_t' = [1, 1(t = 1965), 1(t = 1970)]$ and the associated vectors of point estimates. In this revised specification, the coefficients of interest are $\tau_{1,1}$, the point estimate on the interaction of *Sales* and the constant, as well as $\tau_{1,1965}$ and $\tau_{1,1970}$, the interaction of *Sales* and the indicator variables for observation in the 1965 and 1970 surveys, respectively. The implied marginal effect associated with $\tau_{1,1}$ represents the average difference in Pill use in states with sales bans versus the comparison group in 1955; $\tau_{1,1965}$ and $\tau_{1,1970}$ capture how this difference changed from 1955 to 1965 and 1955 to 1970.

Panel A of table III presents the average marginal effects from probit specifications (corresponding to the columns in table II) for a new dependent variable equal to one if the respondent reported ever using *any* method of contraception. Unlike the results in table II, women in states with sales bans appear no

different in their propensity to have used contraception in 1955. Moreover, this difference does not change in an economically or statistically significant manner in states with sales bans from 1955 to 1965 or from 1955 to 1970. As more covariates are added in columns 2 to 4, the estimates grow even smaller in magnitude.

Because the evolution of "contraceptive use" may mask important changes in the type of contraceptives used, panel B presents estimates using another dependent variable equal to one if a respondent reported ever using either a diaphragm or a condom—two of the most popular and effective methods before the introduction of the birth control pill. Across specifications, women in states with sales bans appear slightly *more* likely (although not significantly so) to have used barrier methods in 1955. In contrast to the absence of changes in panel A, the propensity to use barrier methods in states with sales bans *grew* slightly from 1955 to 1965. This finding is consistent with women in states *without* sales bans opting for the birth control pill in greater numbers, while women in states with sales bans started or continued with use of barrier methods. Although these estimates are never statistically significant, use of barrier methods reverted toward 1955 levels by 1970, which is consistent with women in states with sales bans switching from barrier methods to the Pill in the aftermath of *Griswold*.

In summary, this evidence corroborates the legal and historical literature that argues that sales bans mattered very little for methods pre-dating the birth control pill (Tone 2000, 2002; *Poe v. Ullman* 1961). However, this empirical evidence challenges the claim that sales bans were irrelevant in the 1960s. Sales bans are associated with significantly lower use of the Pill prior to *Griswold*—an association that is robust across specifications and comparison groups. Neither the evidence in panel A or panel B of table III indicate that this association is due to differentially lower demand for children in 1965, more slowly falling demand for children from 1955 to 1965, or differential underreporting of contraceptive use in states with sales bans before 1965. This narrows the potential scope for omitted variables considerably: confounding, unobservables would need to reduce Pill use prior to 1965 but not from 1965 to 1970; moreover, they would need to have no impact on the propensity to use of other contraceptive methods.

Taken together, these findings support the validity of an empirical strategy that uses sales bans to isolate the impact of the Pill on marital fertility in the early 1960s.

IV. The Impact of Oral Contraception on Marital Childbearing

Under the Michael and Willis model, the birth control pill should affect aggregate birth outcomes through two channels: by lowering the marginal cost of averting births (thus increasing the number of women opting to do so) and reducing the monthly probability of conception. Because I have no way of identifying the magnitude of the former, this section begins by generating back-of-the-envelope calculations of the expected magnitude of the change in birth rates through the second channel only. These serve as a benchmark for the subsequent estimates of the importance of sales bans for the U.S. general fertility rate from 1951 to 1980.

The Expected Relationship between Sales Bans and Period Fertility Rates

Denote the number of births to married women of childbearing ages (15 to 44) in year t as B_t. These births consist of planned births, P_t, and contraceptive failures, F_t. Let Q denote the proportion of married women who are fecund and trying to get pregnant, S the proportion of married women who are not fecund, s the time-invariant average rate of success among women trying to get pregnant, and f the average failure rate among married, fecund women using any contraceptive method. Using this notation, the fertility rate in year t can be written as the sum of planned and unplanned births and reflects decisions in the previous period about whether or not to get pregnant as well as the success and failure rates, or

(II) $$B_t = F_t + P_t = Q_{t-1}\, s + (1-Q_{t-1}-S_{t-1})\, f.$$

All but one quantity is straightforward to estimate. Q and S are observed in the 1955 *GAF* and 1965 *NFS*. Census information provides information on the population of women ages 15 to 44, and counts of births are published each year by the Division of Vital Statistics. The average annual success rates of women trying to get pregnant, s, is typically approximated at 0.85 (James Trussell 2004). Estimating the failure rate, f, is difficult, because contraceptive methods cannot be randomly assigned. My approach is to choose the value of f that would satisfy equation II based upon observations of Q and S in the 1955 *GAF* (0.056

and 0.294 respectively) and the U.S. marital fertility rate, B, in 1956 (163 per 1000 married women ages 15 to 44).[24] Therefore, $f = (0.163 - 0.056 \times 0.85)/ (1 - 0.056 - 0.294) = 0.177$. That is, approximately 17.7 percent of married U.S. women, who were *not* trying to get pregnant in 1955, conceived in order for the U.S. marital fertility rate to have reached 163 per 1000 women in 1956 (see online appendix for more information on the computation of these numbers).[25]

How much should birth rates in the population (not only among married women) have changed in states with sales bans after 1957 relative to birth rates in other states? To answer this, redefine f as a weighted average of the failure rates of oral contraception and other methods. Let q_{t-1} denote the proportion of the fecund, married population using oral contraception, f^o be the time-invariant failure rate associated with the birth control pill, and f be the time-invariant failure rate associated with all other methods (calculated above at 0.177). Using this notation, the marital fertility rate at time t can be written as $B_t = Q_{t-1}s + (1-Q_{t-1} - S_{t-1}) [q_{t-1} f^o + (1- q_{t-1}) f]$. The partial impact of changes in failure rates on difference in the marital fertility rate, D_t, between states with sales bans, B_t^S, versus those without these laws, B_t^0, in 1965 *ceteris paribus* can be written as the sum of differences in planned births and failures due to pre-Pill methods and due to the Pill, or

$$\text{(III)} \quad \Delta D \equiv D_{66} - D_{56} = (B_{65}^S - B_{65}^0) - (B_{55}^S - B_{55}^0)$$
$$= [(Q_{65}^S - Q_{65}^0) - (Q_{55}^S - Q_{55}^0)] s$$
$$+ (1 - Q_{65} - S_{65})^S [q_{65}^S f^o + (1 - q_{65}^S) f] - (1 - Q_{65} - S_{65})^0 [q_{65}^0 f^o + (1 - q_{65}^0) f]$$
$$- [(1-Q_{55} - S_{55})^S - (1 - Q_{55} - S_{55})^0] f .$$

Assuming that the gap in Q and S remained constant from 1955 to 1965 and that there were no behavioral differences between states with sales bans and those without, $(1-Q_{65} - S_{65})^S = (1-Q_{65} - S_{65})^0$, ΔD depends only on $(1-Q_{65} - S_{65})(q_{65}^S - q_{65}^0)(f^o - f)$. Using the fact that $(1-Q_{65} - S_{65}) = 0.657$ and $(q_{65}^S - q_{65}^0) \approx 0.07$

[24] Illegitimate births comprised 3.6 percent of all births in 1956, but the 1955 *GAF* provides no information on contraceptive behavior among unmarried women.

[25] Note this estimate of f averages failures among women abstaining from sex (≈ 0) with those not using any methods (considerably higher). Using nonexperimental comparisons, Trussell (2004) calculates that women using condoms and diaphragms today experience failure rates of 16 and 32 percent respectively. My figure may be smaller than the average implied by these numbers due to adjustments in the frequency of coitus and nonrandom selection into methods.

(table II), and $f^{o} \approx 0$, $\Delta D \approx 0.008$. This estimate provides the expected change in the gap in birth rates between states with sales bans and those without from 1966 to 1956 among married women only. Assuming that there was no change in failure rates among unmarried women, the change in this gap among all women ages 15 to 44 in the U.S. is $\Delta D^{US} \approx 0.70 \times \Delta D^{m} \approx 0.0057$. This back-of-the-envelope calculation suggests that the aggregate gap in the U.S should grow by almost six births per 1000 women of childbearing age due to changes in failure rates alone.

The Relationship between Sales Bans and Period Fertility

To estimate the actual changes in aggregate birth rates in states with sales bans, I encoded information from the published Vital Statistics Natality volumes by state from 1950 to 1967 and supplement it with similarly aggregated information from the publicly-available Natality Detail files for 1968 to 1980. Each year-by-state birth count is divided by an estimate of 1/1000 of the relevant population using the Integrated Public Use Microdata Series (IPUMS) in the years of the decennial census and using linear interpolation in the intercensal years (Steve Ruggles et al. 2008). The dependent variable is the general fertility rate, GFR_{st}, for each state, s, and year, t (see the data appendix for more detail). For fertility outcomes, I estimate the following linear, panel analog of equation I,

$$\text{(IV)} \qquad GFR_{st} = Sales_s \boldsymbol{f}_t' \boldsymbol{\tau}_1 + Exception_s \boldsymbol{f}_t' \boldsymbol{\tau}_2 + Advertising_s \boldsymbol{f}_t' \boldsymbol{\tau}_3 + g_{rt} + \boldsymbol{X}_{st}' \boldsymbol{\delta} + \varepsilon_{st}$$

where t = 1950, 1951, …, 1980, $\boldsymbol{f}_t'$ = [1, 1(t=1951), 1(t=1952), …, 1(t=1980)], and $\boldsymbol{X}$ includes a set of time-varying covariates obtained by linearly interpolating variables between census years (proportion of the state population of 15 to 44 year olds residing on a farm, currently married, nonwhite, foreign born, and in poverty and mean total income and mean educational attainment).[26] In some specifications, $\boldsymbol{X}$ also includes a set of state fixed effects. The legal variables remain as previously defined. Because the evolution of births in the U.S. from 1950 to 1980 is highly nonlinear (see figure I), state-specific trends are not included in the analysis. However, all of the specifications include census-region-by-year fixed

[26] This is because Vital Statistics volumes do not contain information on state or individual-level characteristics and because there is no data source containing annual state characteristics for the 1950s or early 1960s. These estimates of state characteristics are imperfect, so specifications with these variables should be viewed as a robustness check. In general, these controls sharpen the results.

effects, g_{rt}, to parallel the analysis on use of the contraceptives presented in section III and to capture the nonlinear evolution of birth rates common to all states within census regions. As before, the point estimates of interest, denoted $\tilde{\boldsymbol{\tau}}_{\mathbf{1}}' = [\tau_{1,1951}, \tau_{1,1952} \ldots \tau_{1,1980}]$, capture changes in the gap in birth rates in states with sales bans relative to those in the comparison groups for 1951 to 1980.

Figure VI plots estimates of $\tilde{\boldsymbol{\tau}}_{\mathbf{1}}$ along with 95 percent confidence intervals using heteroskedasticity-robust standard errors that have been adjusted for serial correlation within states (Arellano 1987, Bertrand et al. 2004). Recall, back-of-the-envelope calculations anticipate a change in the difference in birth rates of at least six births per 1000 women from 1955 to 1965. By the same logic, one would expect the gap in birth rates to narrow between 1965 and 1970 as use of oral contraception converged in the aftermath of *Griswold.* Plots of the estimates of $\tilde{\boldsymbol{\tau}}_{\mathbf{1}}$ in all panels of figure VI correspond closely to these expectations. From 1951 to 1957, the gap in birth rates changed very little. After the introduction of *Enovid* in 1957 and before the 1965 *Griswold* decision, the average within-region difference between states with sales bans and the comparison group grew by approximately seven to nine births per 1000 women, and the estimates are individually and statistically distinguishable from zero from 1961 to 1965. Another striking feature of these plots is that differences in the birth rates fell sharply by around 4 births per 1000 women after 1965. After 1966, the point estimates are no longer (individually) statistically distinguishable from zero, and a Wald test rejects the equivalence of the point estimates in 1967 and 1965 at the 5 percent level in each specification in figure VI. The fact that laws banning contraceptive sales are associated with growing differences in birth rates from 1958 to 1965 and falling differences from 1965 to 1970 is consistent with the waxing of their relevance with the introduction of a refillable, prescription contraceptive and the waning of their relevance in the aftermath of *Griswold*. One final feature of the plots is that differences in the general fertility rate in states with sales bans evolve smoothly after 1968. This implies that pre-existing sales bans were poor predictors of abortion legalization and other determinants of the post-1965 fluctuations in the birth rate.

As in the previous section, figure VI examines the robustness of estimates to the use of different comparison groups. Panel A presents three specifications that are analogous to comparisons in tables II and III for equation I (these specifications omit state fixed effects). The baseline estimates of $\tilde{\boldsymbol{\tau}}_{\mathbf{1}}$ are obtained from a specification that includes *Sales*, *Exception* and census region-by-year dummy variables, so they capture changes in the gap in the birth rate between states with sales bans and other states in the same census region. A second specification refines this comparison by including a dummy variable for advertising bans interacted with the elements of $\boldsymbol{f}_{\boldsymbol{t}}$, so $\tilde{\boldsymbol{\tau}}_{\mathbf{1}}$ captures the differential evolution of birth rates in states with sales bans relative to states with advertising bans only in the same census region. A third specification additionally adjusts for compositional changes by including annual, state-level covariates. The results indicate that sales bans affected the birth rate by affecting the availability of supplies rather than the flow of information (second specification) and that the evolution of observable (and perhaps unobservable) characteristics tended to offset the effects of sales bans (third specification).

Panel B adds state fixed effects and modifies the estimating equation to allow birth rates to evolve differently in states that repealed their sales bans before *Griswold*.[27] The results change negligibly: Least squares estimates of the increase in the gap in birth rates are comparable to those in panel A.[28] The post-1965 decline in the gap, however, is slightly larger indicating that the difference in birth rates fell by approximately 7 births per 1000 women within the five years after *Griswold*. These results are consistent with the results on use of the birth control pill, and their robustness to excluding states that choose to repeal their laws suggests that unobserved political, legal or social changes are not driving the results.

The similarity of these patterns across specifications in panel A and B supports the credibility of this paper's empirical strategy. As in the analysis of contraceptive use in section III, the evolution of birth

[27] In practice, equation I is modified to include a dummy variable equal to one for this group of states as well as dummy variables for the interaction of this variable with each year from 1951 to 1980 as well as state fixed effects. The results are qualitatively similar when I allow the legal variables to "turn off" following a pre-*Griswold* revision (see online appendix for these estimates).
[28] It is worth noting that the robustness to the inclusion of state fixed effects is not terribly surprising. After all, the most concerning threats to identification are *not* time-invariant, state-level factors, but time-varying, unobservables that (1) are concentrated in states with sales bans, (2) correspond in timing to the introduction and diffusion of the Pill between 1957 and 1965, and (3) wane in their importance after the *Griswold* decision.

rates in states with sales bans from 1957 to 1965 is virtually identical when using different comparison groups and strengthened with adjustments for observable characteristics.

Panel C demonstrates that sales bans tended to have similar effects across regions. Each line presents estimates from separate regressions that correspond to the specification in panel B with advertising bans and state-level covariates. If one census region were driving the results, omitting that region from the analysis would alter the pattern of estimates. This does not appear to be the case, as the 1957 to 1965 divergence in birth rates and the post-1965 convergence is similar in magnitude as each census region is omitted.

Finally, panel D explores heterogeneity in the effect of the Pill by age group. I redefine the dependent variable as the state birth rate in year *t* for each of six five-year age groups (15 to 19, 20 to 24, …, 40 to 44) and estimate separate regressions that correspond to the specification in panel B with advertising bans and state-level covariates.[29] The divergence in birth rates is most pronounced among 20 to 24 year olds, where the difference increases by approximately 18 births per 1000 women from 1958 to 1965 and drops precipitously after 1965. The pattern is less pronounced but also present among 15 to 19, 25 to 29, and 30 to 34 year olds. Consistent with the paucity of women ages 35 to 44 who adopted oral contraception, the pattern for this age group deviates little from zero.

V. The Demographic Legacy of the Pill

Fifty years after *Enovid* appeared on the U.S. market, this analysis provides new evidence that the contraceptive pill accelerated the post-1960 decline in marital fertility. The paper's results rely upon a novel empirical strategy, which exploits the idiosyncratic language of Comstock statutes, the timing of the introduction of the birth control pill, and the timing of the 1965 *Griswold* decision. Despite imperfect enforcement and compliance of Comstock laws, women in states with sales bans were less likely to have used the birth control pill before *Griswold*. These legally-induced differences in Pill use had an appreciable impact on marital childbearing. Period fertility rates fell more slowly in states with sales bans

[29] State covariates are redefined to be specific to the particular age group under consideration.

in the period following the introduction of *Enovid* and before *Griswold*. By 1965 the difference in the general fertility rate had grown by approximately 8 births per 1000 women of childbearing age. This figure suggests that approximately 124,600 U.S. births in 1965 occurred in excess of what would have been predicted based upon the pre-existing, state-level differences.[30] Without bans on the sales of contraception, the marital fertility rate could have been 8 percent lower in states with sales bans and 4 percent lower in the U.S. as a whole.

Had the Pill been unavailable during the 1960s, it is likely that the U.S. fertility rate would have fallen more slowly. But how slowly? How much of the *total* decline in the marital fertility rate from 163 births per 1000 married women in 1956 to 131 in 1966 can be attributed to the Pill? To speculate on the answer to this question, I generate a simple counterfactual using equation II and information on behavior and fecundity from the 1965 *NFS*. Assuming that the failure rate remained unchanged at 0.177 after 1955, I simulate the 1966 marital fertility rate based upon the observed number of women trying to get pregnant, Q, and who were fecund, S, to be $\hat{B}_{66} = Q_{65}s + \left(1 - Q_{65} - S_{65}\right)f$ =(.029)(.85)+(.665) (.177) = 0.144. Because this simulation holds constant failure rates at the level imputed from the 1955 *GAF*, it attributes approximately 60 percent of the total change in the martial fertility rate from 1956 to 1966 ($B_{56} - \hat{B}_{66}$=163-144=19) to changes in marital behavior. The remaining component, $\hat{B}_{66} - B_{66}$=144-131=13, can be attributed to changes in the failure rate, f.[31] If all of the changes in the failure rate were due to the introduction of oral contraception, approximately 40 percent (13/32) of the total change in the U.S. marital fertility rate from 1955 to 1965 could be attributed to the Pill.

This is a large number, but it might be even larger. Economic models maintain that changes in marital behavior—couples' decisions to prevent or delay births, for instance—may also respond to changes in contraceptive technology. If the Pill also altered the number of women trying to get pregnant, the proportion of the total decline in U.S. fertility attributed to the Pill could well exceed 40 percent.

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[30] This estimate is obtained by multiplying the growth in the difference between 1957 and 1965 in the number of births per 1000 women ages 15 to 44 in states with sales bans (0.008) by the population of women in that age range in 1965 (15,574,871).

[31] The Pill may have affected women's probability of getting pregnant even after ceasing use. Changes in the failure rate over this period may also have reflected changes in selection into marriage.

It is important to emphasize that these results *do not* discount the role of other forces in the 1960s. On the contrary, increases in the demand for women's work, falling discrimination, and shifting norms and preferences *independent of changes in contraceptive technology* provided powerful incentives for U.S. women to use the Pill once it became available. Therefore, caution should be exercised in extrapolating these results to other places and periods.

In summary, this paper provides new evidence that the contraceptive revolution was an important force behind the sharp drop in U.S. fertility during the 1960s. The evidence suggests a role for contraceptive technology that far exceeds its treatment, implicitly or explicitly, in the economics literature. Simple extensions of economic models to incorporate changes in contraceptive technology may help resolve larger demographic puzzles and illuminate this technology's implications for household labor supply, potential earnings, and the longer-term well-being of parents and children.

VI. Data Appendix

Historical Fertility Studies

1955 Growth of American Families Study: This study sampled 18 to 39 year-old white women, who were currently married. For information on whether respondent ever used contraception in the 1955 *GAF*, information was taken from two questions. The first asked "have you or your husband ever done anything to limit the number of your children or to keep from having them at certain times?" –the respondent could reply "yes" or "no"). For those respondents who indicated "yes", the survey then asked, "Doctors and public health workers are interested in knowing what methods people are using over the country as a whole. What methods have you used? (You can tell me by the numbers on the card, if you wish.)" The options on the card included "Safe period – rhythm", "douche", "withdrawal", "abstinence", "rubber condom", "diaphragm", "jelly", "vaginal suppository", "foam tablets", "tampon, vaginal cap, or stem pessary", and "lactation." From this information, I create two different measures of contraceptive use: a binary variable equal to one if the respondent or her husband had ever used any method of contraception and a binary variable equal to one if the couple had ever used a condom or the diaphragm (barrier methods).

1965 National Fertility Study: This study sampled currently married women ages 18 to 54 in the autumn of 1965. For information on whether respondent ever used contraception in 1965 *NFS*, information was taken from the question that asked the following: "Here is a card with the names of methods couples use to delay or prevent having a baby. (SHOW CARD 1). During this time, which method or methods, if any, did you or your husband use? You may just tell me by number if you like." This question is asked for each pregnancy interval, where these intervals are defined as the period after marriage and before the first pregnancy, the periods between pregnancies, or between the time of marriage/last pregnancy and the date of interview. The options on the card included "rhythm", "douche", "withdrawal", "abstinence", "condom", "diaphragm", "jelly", "suppository", "foam tablets", "sponge", "jelly", "contraceptive pill", and "IUD."

In the 1965 data, the construction of the four measures of contraceptive use is more difficult. For instance, 3072 women are coded as having used some form of contraception in the first pregnancy interval (2045 as using none), but the choices of 469 of the women who are coded as first interval users did not indicate having used any of the methods listed. The documentation does not provide any clues about why this is the case. As a result, these women are coded as missing and not included in the analysis of the outcomes "ever users of contraception" or "ever used barrier methods." The survey separately asked women whether they had ever used the birth control pill and asked them to circle the months it was used on the survey. These questions are used to create the measure of "ever used oral contraception" before the *Griswold* decision.

1970 National Fertility Study: This study sampled ever-married women ages 18 to 44. For information on whether respondent ever used contraception in 1970 *NFS*, information was taken from an almost identical question to the 1965 survey question about contraceptive use in each pregnancy "interval" (where "interval" is defined as in the 1965 survey). The survey codes only allow me to group answers into the following categories, "rhythm", "abstinence (no intercourse) for at least one month", "condom, rubber, safe", "withdrawal, pulling out", "diaphragm, pessary (with or without cream or jelly)", "birth control pill, oral contraceptive pill", "jelly or cream (alone)", "suppositories", "foam tablets, foam spray", "sponge, tampon", "IUD, coil, loop, ring, bow", and "douche." Again, the survey explicitly asked women whether they had ever used the birth control pill and asked them to circle the months it was used on the survey from January of 1960 to1970. These questions are used to create the measure of "ever used oral contraception" across the 1970s.

Pooled Data Sample: I limit the samples to currently-married white women ages 18 to 44 for comparability across studies. I develop three variables of contraceptive use for the analysis:

(1) Ever used any contraceptive method: This is a straightforward adaptation of the survey question in 1955. I generate a comparable variable from the 1965 and 1970 *NFS* that codes a "yes" if a respondent reported using any contraception in any pregnancy interval.

(2) Ever used barrier method: I define "barrier method" as a condom or diaphragm (the closest potential substitutes for the birth control pill). This is straightforward using the survey question

in 1955. I generate a comparable variable from the 1965 and 1970 *NFS* that codes a "yes" if a respondent reported using condoms or diaphragms in any pregnancy interval.

(3) Ever used oral contraception: There is no information about use of oral contraception before 1965. Because *Enovid* was first approved by the U.S. Food and Drug Administration for the regulation of menses in 1957, I code this variable as zero in 1955. In the 1965 and 1970 *NFS*, I use similar questions about whether the respondent had ever used the birth control pill.

Summary statistics for these variables and other covariates are summarized in the data appendix table 1.

Natality Data for 1950 to 1967: The data for 1950 to 1967 was taken from published volumes of the Vital Statistics Division of National Center for Health Statistics (NCHS, formerly the National Office of Vital Statistics). The tables were entered that contained information on the number of live births by birth order, state, age of mother, and race. These volumes were compiled using microfilm copies, transcripts or state data files of all original birth certificates sent from states to the NCHS. All births are based on place of residence. However, births to Americans outside of the United States are not recorded in these data, although births to foreigners in the United States are recorded and assigned to state of occurrence.

The 1950 and 1955 data correspond to a direct count of live births in the United States. Data for the years 1951 to1954 and 1956 to 1966 are based on a 50 percent sample consisting of only the even-numbered live birth records. The data for 1967 are based on a 20 to 50 percent sample (depending upon the state) and similar calculations were used to generate aggregate statistics. The NCHS generated the live birth counts by multiplying the samples by two in the case of a 50 percent sample and 5 in the case of a 20 percent sample. Although the data provide a representative sample of all registered births, birth registration was not 100 percent in the United States from 1950 to 1967. In 1950, the NCHS estimated that 98.1 percent of all births in the nation were registered: 98.8 percent for whites and 94.1 percent for the nonwhite (p. XXI, 1950).

There are several more data limitations that should be noted. For Massachusetts, the 1964 figures exclude approximately 1800 live births, and prior to 1964 Massachusetts' live-birth records were overstated by approximately 4 percent due to duplicate documentation. Also, due to a coding error in 1967, approximately 20,000 to 24,000 births were miscoded. It is believed that most of the misclassified

births were first births. Although this error potentially affected many states, the NCHS documentation notes that it is particularly severe for Massachusetts, Wisconsin, New Jersey, and Utah. The robustness of the results does not appear to be related to decisions about whether to exclude or include these states. Finally, in entering the published data into spreadsheets, we noted several inconsistencies. Specifically, the sum of births of a given parity across age categories within race did not equal the published total. In this instance, the age category numbers were assumed to be correct and the total for the state was corrected accordingly.

Natality Data from 1968 to 1970: The data for 1968 to 1970 are taken from natality micro-data detail files available from ICPSR. The microdata were aggregated by state of residence to correspond to the age of mother and birth order categories available in the published volumes for earlier years. Birth counts are adjusted for 1968 to 1971 to reflect the 50 percent sampling scheme for all states. From 1972 to 1978, birth counts are adjusted to reflect the 50 percent sampling in a subset of states.

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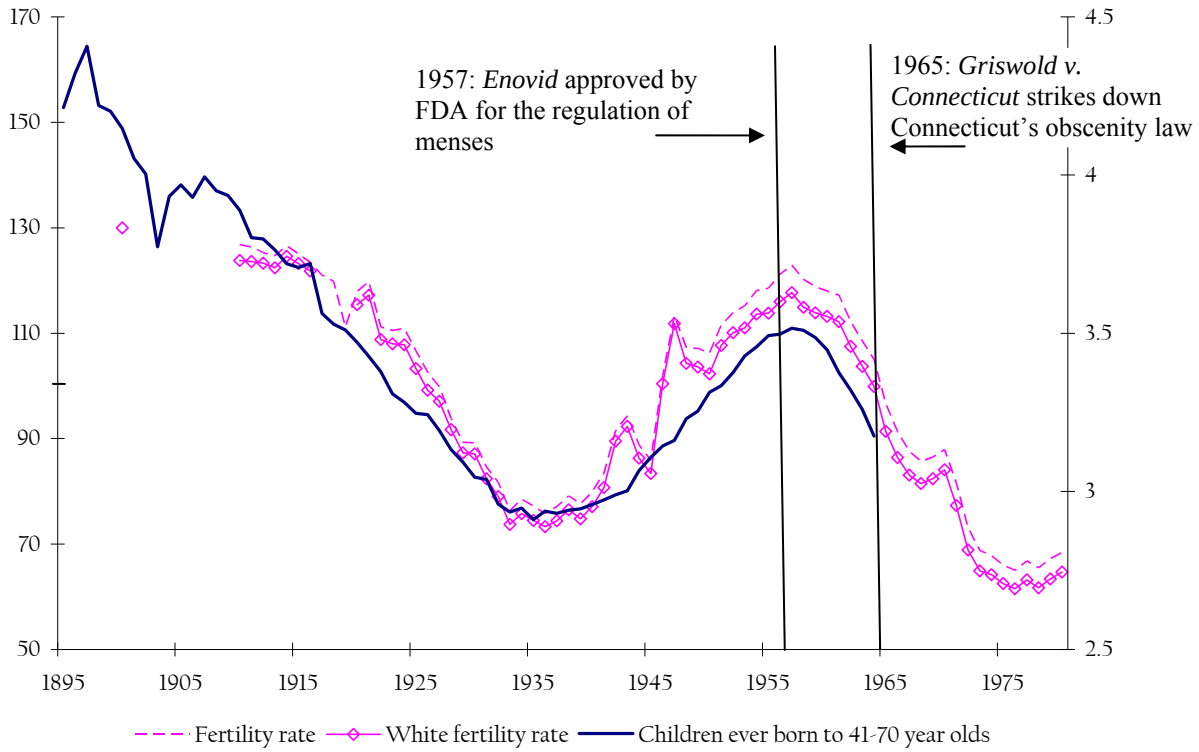
Appendix Table 1.
Summary Statistics for Growth of American Families and National Fertility Studies

	1955	1965	1970
Ever Used Contraception			
Any method*	0.814	0.836	0.862
Barrier method	0.467	0.479	0.314
Birth control pill	0.000	0.278	0.682
Sterilization (respondent or husband)	0.086	0.127	0.134
Respondent characteristics			
White	1.000	1.000	1.000
Currently married	1.000	1.000	1.000
Catholic	0.293	0.291	0.256
Year of birth	1924.7	1935.4	1941.1
Residence in			
Northeast	0.249	0.229	0.199
Midwest	0.316	0.305	0.317
South	0.270	0.294	0.293
West	0.165	0.171	0.191
Education			
Less than 9 years	0.136	0.093	0.052
9 to 11 years	0.250	0.221	0.163
12 years	0.461	0.486	0.505
13 to 15 years	0.107	0.126	0.161
16 years	0.046	0.075	0.119
Ideal number of children**			
Fewer than two	0.003	0.005	0.015
Two	0.207	0.258	0.516
Three	0.344	0.354	0.236
Four	0.379	0.328	0.194
Five or more	0.067	0.056	0.038
Missing	*28*	*27*	*22*
Husband's income			
0 to 4500	0.224	0.128	0.113
4501 to 6500	0.464	0.108	0.182
6501 to 7699	0.152	0.320	0.100
7700 to 11999	0.105	0.257	0.201
1200 and up	0.055	0.187	0.404
Missing	*105*	*82*	*118*
Total respondents	**2607**	**2864**	**4086**

*Any contraceptive method includes "douche". **This variable is constructed from the answer to the question regarding "ideal number of children for the average American family". Statistics for the 1965 and 1970 surveys use the weights recommended in the study documentation. Sample: White, currently married women ages 18 to 39. Sources: 1955 GAF, 1965 and 1970 NFS.

Figure I

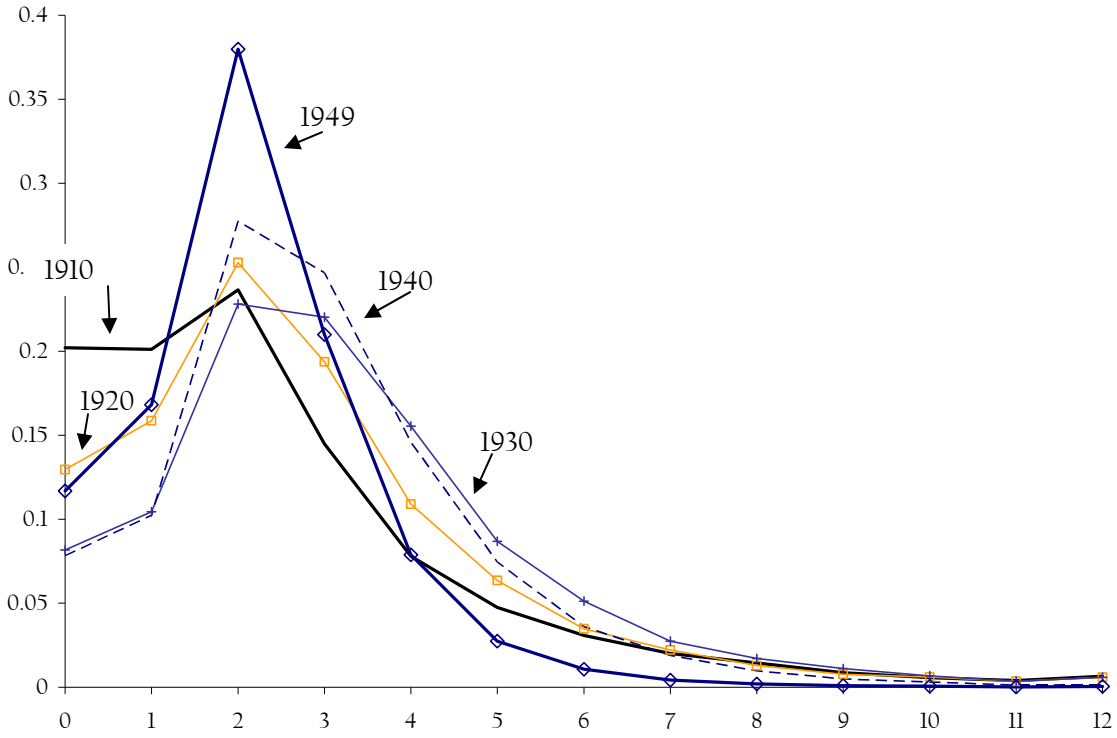
U.S. Fertility and Children Ever Born from 1895 to 1985



The outcome variables are the period fertility rate (and separately for white women) and the mean self-reported number of children by birth cohort. Mean children ever born excludes women who had no children. Birth cohorts are indexed to year of birth and increased by 25 years. (For instance, the birth cohort of 1870 corresponds to the year 1895.) Computations using the IPUMS census weights.
Source: Annual fertility rates are calculated using Historical Statistics, http://www.cdc.gov/nchs/data/statab/t001x01.pdf. The mean number of children ever born per woman is calculated using a sample of ever-married women ages 41 to 70 in the 1950, 1960, 1970, and 1980 IPUMS (Ruggles et al. 2007).

Figure II

Distribution of Children Ever Born by Year-of-Birth Cohort



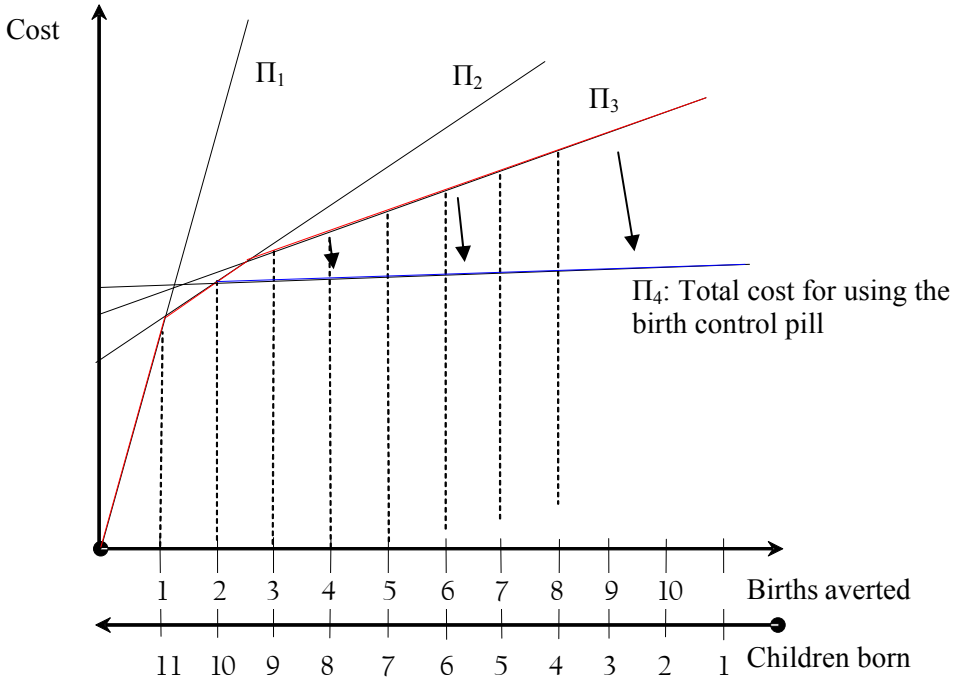
The outcome variable is the proportion of women born in a specific year who report a given number of children. This variable is created using the question of self-reported children ever-born excluding stillbirths, adopted, and step children. In 1960 this question was only asked of ever-married women, so I exclude never-married women in later census years for consistency. I also top-code children ever born at 12 in 1960 for consistency with the later census years. Year of birth is obtained by subtracting age at the time of the census from the year of observation.

Sample: Ever-married women ages 41 to 50 residing the coterminous United States.
Source: 1960-1990 IPUMS (Ruggles et al. 2007).

Figure III

Total and Marginal Cost of Averting Births

A. How the Birth Control Pill Affected the Total Cost of Averting Births



B. How Sales Bans Affected the Total Cost of Averting Births

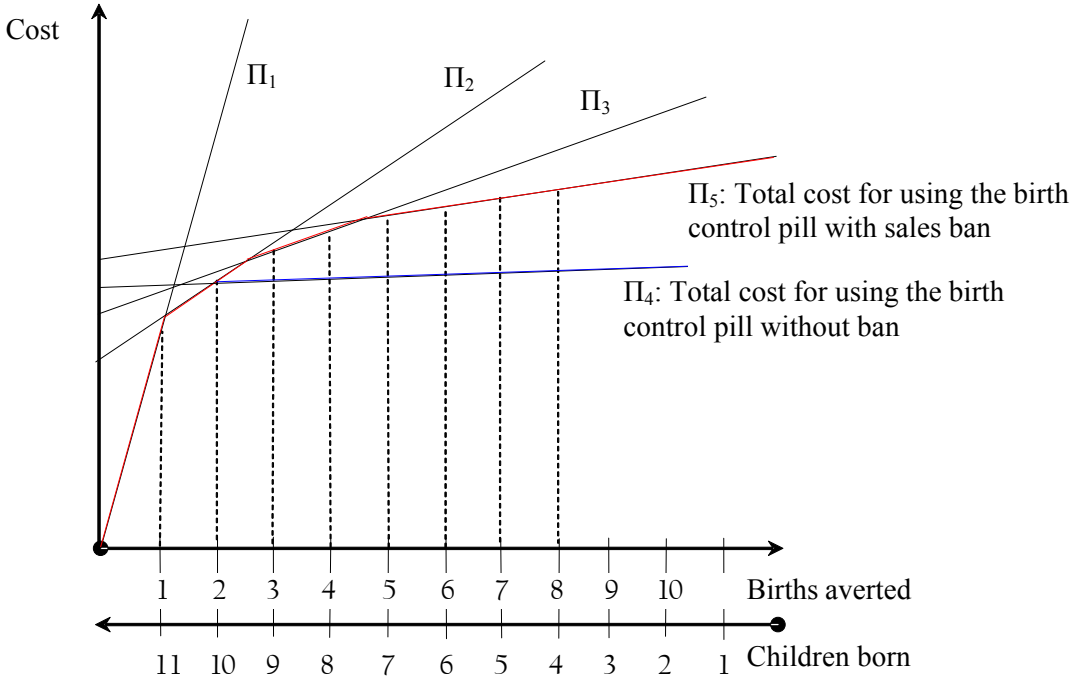
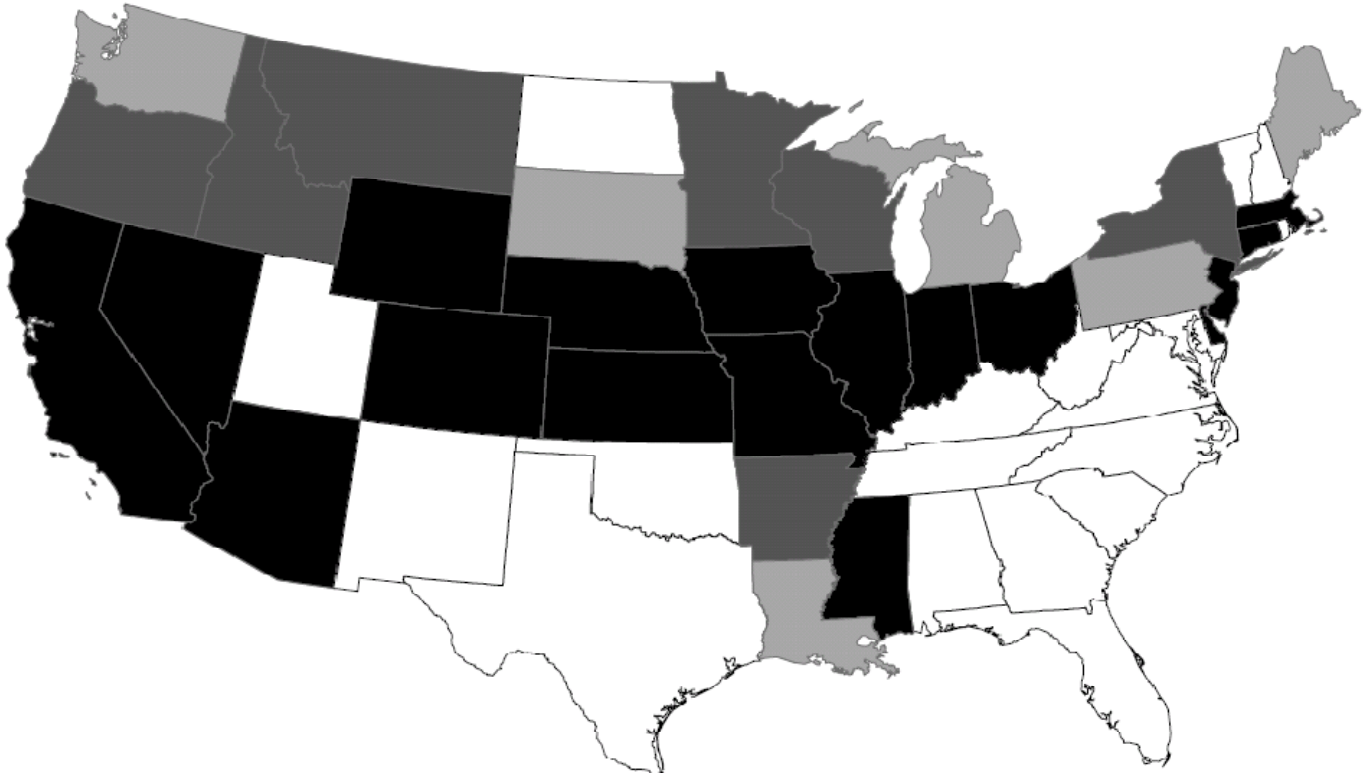


Figure IV

Geographic Distribution of Comstock Sales Laws by Type circa 1960

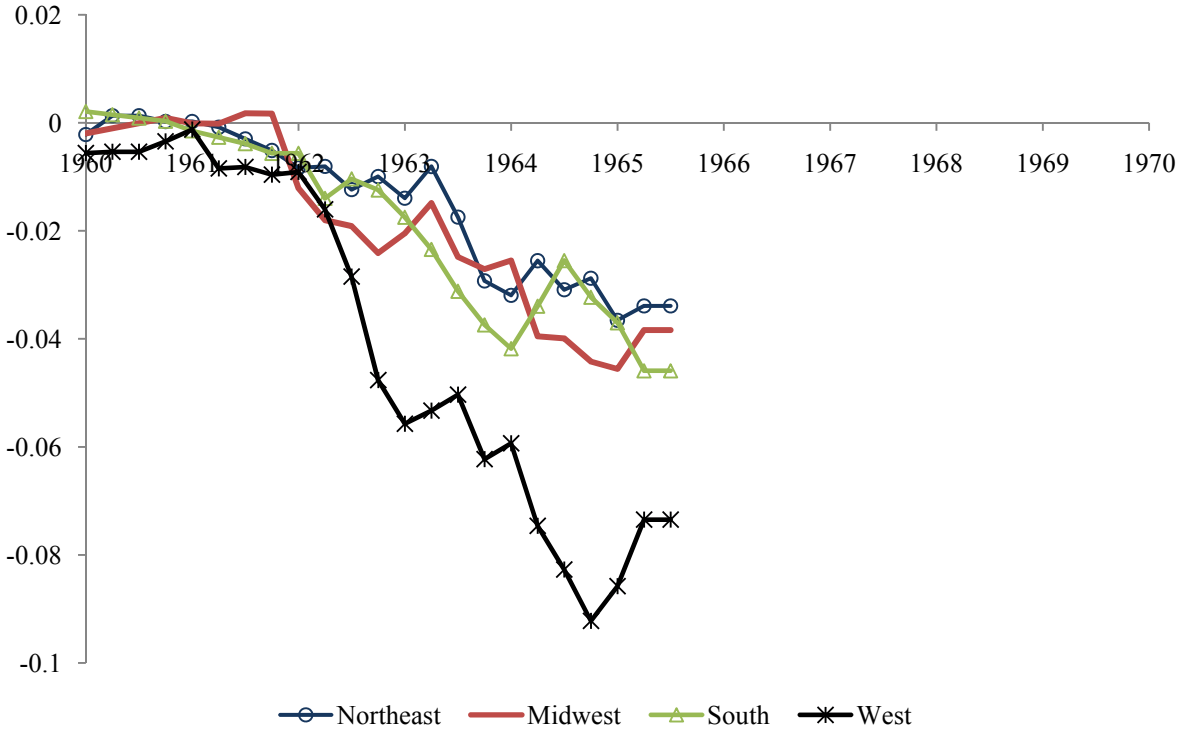


No shading: States with no laws mentioning the "prevention of conception"
Light gray: States banning only advertising or the distribution of information (Table I, columns 2 and 3)
Dark gray: States banning advertising and the sale of contraceptives but with physician exceptions (Table I, columns 3 and 4)
Black: States with sales bans and advertising bans with no exceptions for physicians (Table I, columns 3 and 4)

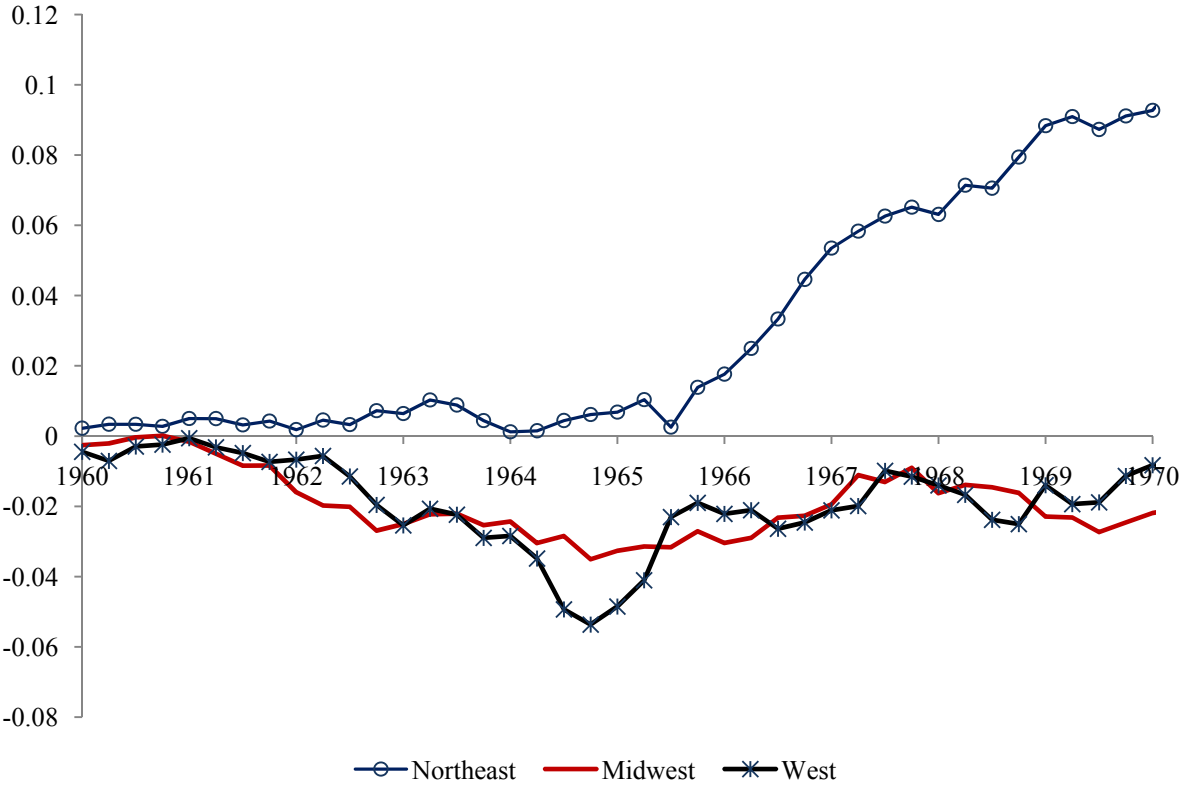
Figure V

Differences in the Cumulative Proportion of Women Ever Using the Pill

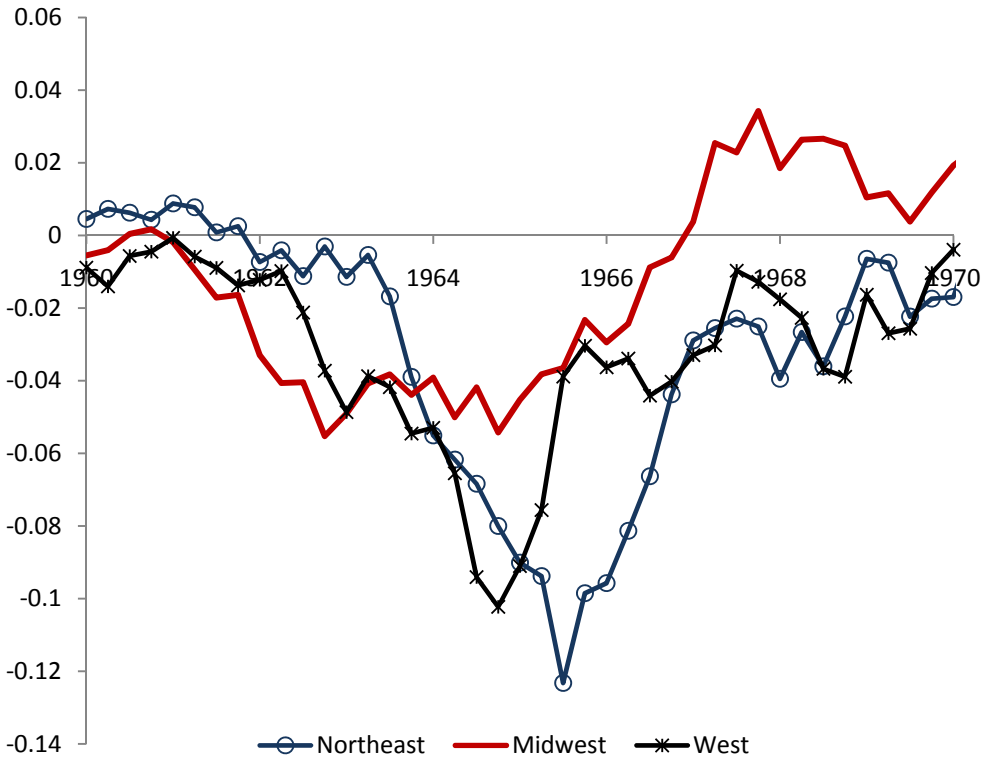
A. Differences in Ever Used the Pill (States with Sales Bans-States without Bans) (1965 NFS)



B. Differences in Ever Used the Pill (Sales with Bans –States without Bans) (1965 and 1970 NFS)



C. Differences in Ever used the Pill Conditional upon using Pill by 1970(States with Bans –States without Bans) (1965 and 1970 NFS)

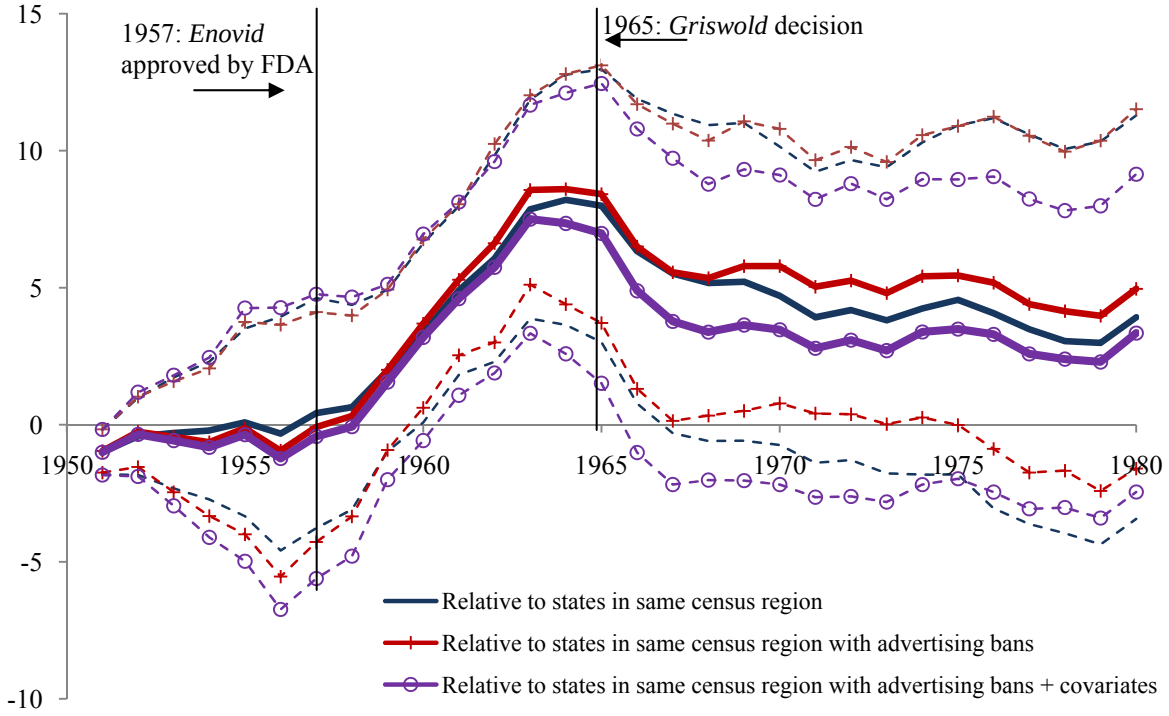


Each point plots the differences in cumulative number of women who report having used oral contraception in states with restrictive sales bans (i.e. no physician exceptions) and women in states without these laws by quarter of first use and region of residence at the time of the survey. Panels A and B take the number of women residing in the region at the time of the survey as the denominator. Panel C takes the number of women who used the birth control pill by 1970 as the denominator. The estimates are noisy due to small sample sizes and the focus should be on trends rather than the quarterly fluctuations. The South is omitted from the post-1965 comparisons, because there are only 22 observations in the sales ban states in the 1970 NFS. Underlying counts are presented in the online appendix. Source: 1965 and 1970 *NFS*.

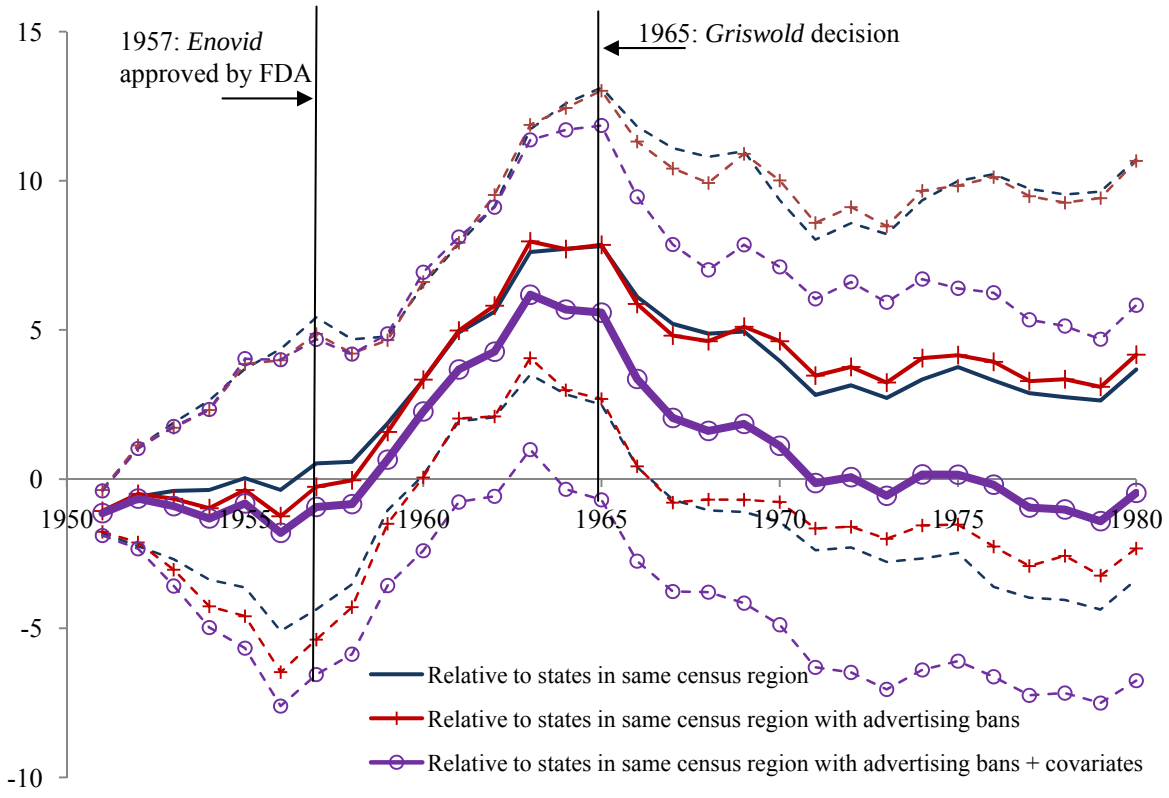
Figure VI

Differential Evolution of Birth Rates in States with Sales Bans, 1951 to 1980

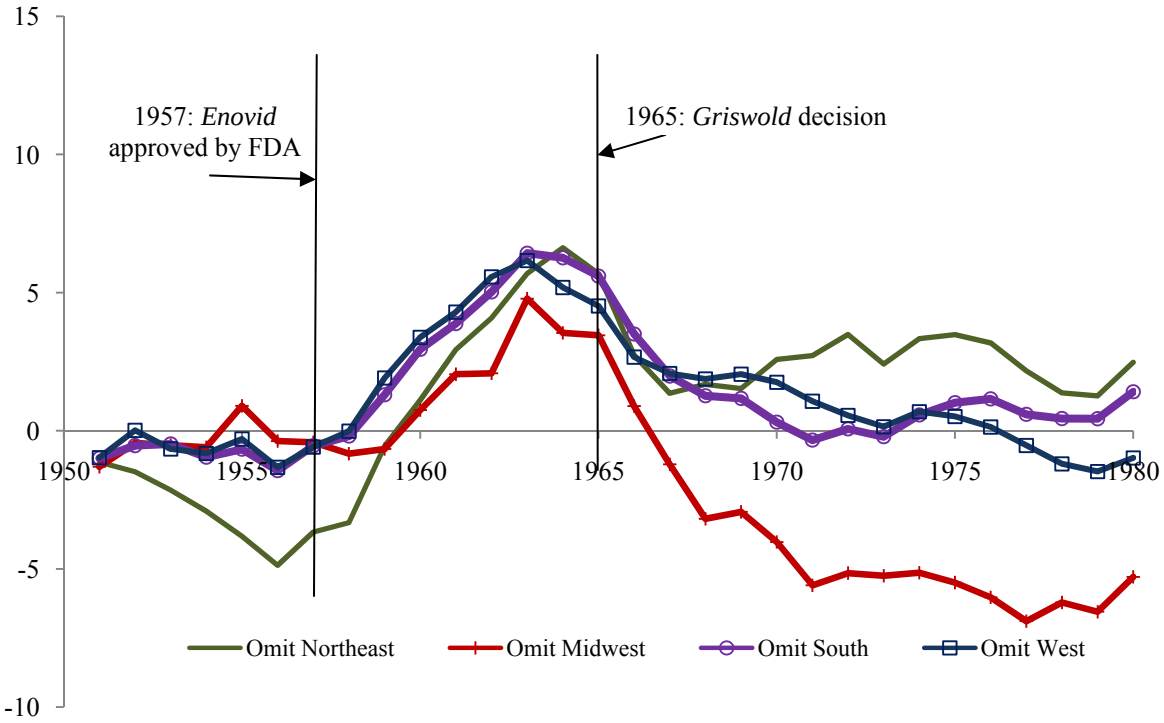
A. Changes in the Difference between States with Sales Bans and the Comparison Group without State Fixed Effects (95 Percent Confidence Intervals in Dashed Lines)



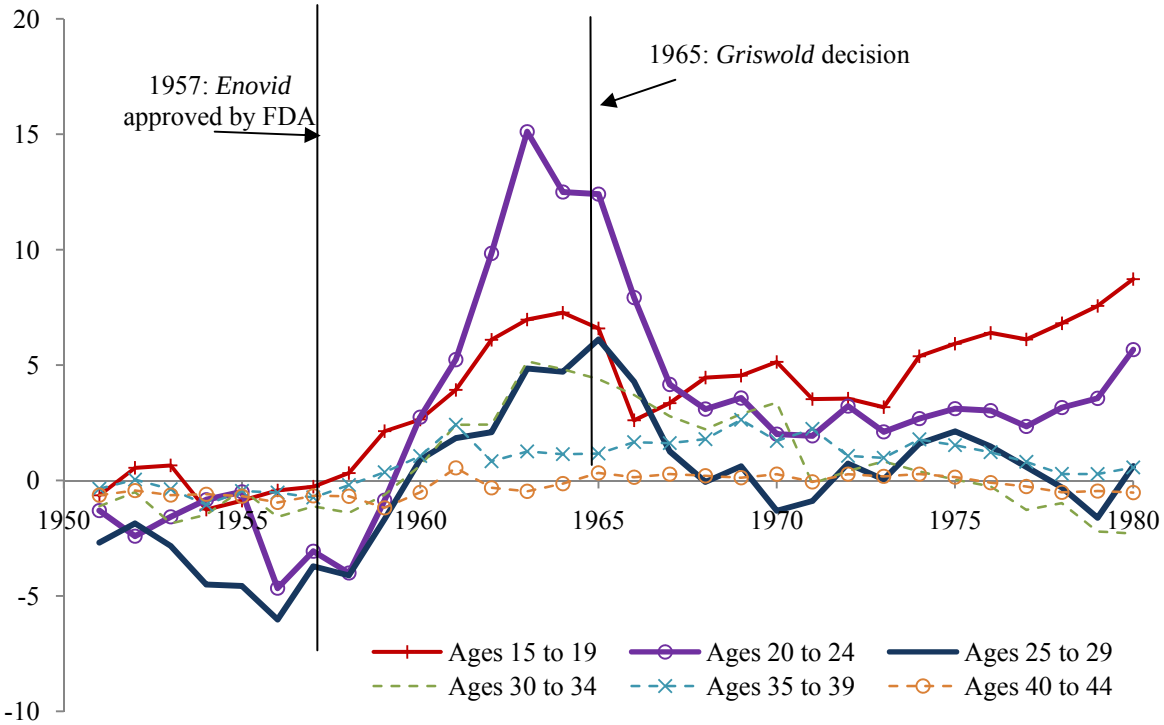
B. Changes in the Difference between States with Sales Bans Until 1965 and the Comparison Group with State Fixed Effects (95 Percent Confidence Intervals in Dashed Lines)



C. Changes in the Difference between States with Sales Bans Until 1965 and the Comparison Group with State Fixed Effects Omitting Each Census Region



D. Changes in the Difference between States with Sales Bans Until 1965 and the Comparison Group with State Fixed Effects by Age Group



Panels A through D present point estimates for τ_1, the coefficients on *Sales* interacted with year dummies, from three alternative specifications of equation (2). See text for details on the alternative specifications. Panel B performs the same analysis but adds a separate term for states that repealed their laws before *Griswold* and its interaction with year dummies and state fixed effects. Panel C omits one census region at a time and reruns the specification from panel B labeled "relative to states in the same census region with advertising bans + covariates". Panel D disaggregates the dependent variable into five-year age groups and for each five-year age groups separately. Each line in panels C and D plots the estimates from a separate regression model. Panels C and D omit confidence intervals, but these estimates are included in an online appendix. Source: 1950 to 1967 Vital Statistics Volumes (see Appendix B) and 1968 to 1980 Natality Files (ICPSR 2002).

Table I
Comstock Laws Related to Contraception in the Continental United States circa 1960

	(1) Earliest Date Obscenity Law Enacted	(2) Advertising Bans	(3) Sales Bans	(4) Physician Exceptions	(5) Date Sales Ban Repealed or Struck Down[1]
Alabama	1884				
Arizona	1870	X	X		1962[3]
Arkansas	1943[2]	X	X	X (1943)	
California	1873	X	X		
Colorado	1885	X	X		1961
Connecticut	1879	X	X		1965
Delaware	1935[2]	X	X		
Florida	1868				
Georgia	1878				
Idaho	1887	X	X	X (1937)	
Illinois	1845	X	X		1961
Indiana	1896	X	X		1963
Iowa	1897	X	X		
Kansas	1920[2]	X	X		1963
Kentucky	1894				
Louisiana	1884	X			
Maine	1857	X			
Maryland	1888				
Massachusetts	1847	X	X		
Michigan	1897	X			
Minnesota	1894	X	X	X	
Mississippi	1892	X	X		
Missouri	1879	X	X		
Montana	1935[2]	X	X	X(1935)	
Nebraska	1885	X	X		
Nevada	1877	X	X		1963
New Hampshire	1891				
New Jersey	1898	X	X		1963
New Mexico	1884				
New York	1868	X	X	X(1873)	
North Carolina	1885				
North Dakota	1895				
Ohio	1885	X	X		
Oklahoma	1891				
Oregon	1864	X(1935)	X(1935)	X(1935)	
Pennsylvania	1870	X			
Rhode Island	1896				
South Carolina	1894				
South Dakota	<1910[2]	X			
Tennessee	1884				
Texas	1897				
Utah	1898				

Vermont	<1880				
Virginia	1874				
Washington	1909[2]	X			
West Virginia	<1899				
Wisconsin	1849	X(1933)	X(1933)	X(1933)	
Wyoming	1890	X	X		
TOTAL	48	31	24	7	7 before 1965

Source: State statute books, Dennett (1925), Smith (1964), Dienes (1972, Appendix B, pp. 317-319), and DHEW (1974). In the cases of discrepancies between sources, we refer to original statute text and trace the evolution of wording until 1965. Dates in column (1) indicate the earliest date that we could verify an anti-obscenity statute in statute books. The less-than sign, "<", indicates that the law was enacted by the date indicated when we could not identify the date the statute was first enacted. Dates in parentheses indicate the date the original law was revised to include the particular provision if it was not part of the original statute.. Ad bans refer to statutes banning advertising or the distribution of information about contraception. Sales bans refer to statutes banning the sales of drugs, instruments or articles relating to contraception. Physician exceptions refer to sales bans exempting physicians or pharmacists from their provisions. [1] These dates are only for statutes repealed or overturned before *Griswold* as used in the sensitivity analysis Because it is unclear whether repeal dates after *Griswold* reflect actual changes in practice or a codification of current practice, the analysis does not make use of actual changes in statutes if they occurred after *Griswold.* See online legal appendix for more information on the repeal dates. [2] Earlier statutes may have been enacted. Dates are the earliest date we could verify an anti-obscenity statute using primary sources. Copies of these statutes are posted at the author's webpage. [3] Judicial decision overturned the statute. Detailed notes on coding decisions can be found in an online legal appendix.

Table II
Changes in Birth Control Pill Use in States with Sales Bans, 1965 to 1970

	(1)	(2)	(3)	(4)
	DV: 1=Ever used the birth control pill			
Sales ban	-0.057	-0.072	-0.073	-0.077
	[0.020]	[0.022]	[0.023]	[0.022]
Sales ban x 1(1970)	0.043	0.040	0.058	0.0061
	[0.029]	[0.030]	[0.024]	[0.023]
Observations	6950	6950	6754	6712
Log likelihood	-4203.19	-4201.65	-3700.85	-3676.13
Additional covariates[a]	R	R	RACEI	RACEIK
Legal variables[b]	PX	PX,AD	PX,AD	PX,AD

Each column presents the average marginal effects from probit specifications corresponding to equation I. Robust standard errors clustered at the state level are presented in brackets. [a] Covariates include fixed effects for census region denoted R in each column. Columns 3 and 4 include dummy variables for age categories, A (18 to 19, 20-24, 25-29, 30-34,35-39); Catholic, C (equal to one if the respondent indicated her religious preference was Roman Catholic); educational categories, E (≤8, 9-11, 12, 13-15, 16 or more); and husband's annual income, I (0-4500,4501-6500, 6501-8500,10500-21000 in 1969 dollars). The omitted category includes 18- to 19-year-old, non-Catholic white women in the Northeast with less than 9 years of education whose husbands report no earnings. Column 4 adds a set of dummy variables for the "ideal number of children in the average American household", K, to proxy for differences in norms about childbearing. The omitted category changes to be all of the categories listed above and women reporting no children as ideal. [b] Legal variables include interactions of states with sales bans and physician exceptions (PX), and states with advertising bans only (AD) with a set of dummies for the years 1965 and 1970. Changes in the number of observations reflects missing values or, in column 4, the lack of variation in the dependent variable for certain categories of the K variable. Sample: Married women between the ages of 18 to 39 at the time of the survey. Sources: 1965 and 1970 *NFS*.

Table III
Changes in Contraceptive Use in States with Sales Bans, 1955 to 1970

	(1)	(2)	(3)	(4)
Panel A	*DV: 1=Ever used any method of contraception*			
Sales ban	0.016	0.012	0.011	0.007
	[0.016]	[0.017]	[0.016]	[0.015]
Sales ban x 1(1965)	-0.028	-0.003	-0.007	-0.005
	[0.040]	[0.052]	[0.043]	[0.043]
Sales ban x 1(1970)	-0.020	-0.015	-0.011	-0.009
	[0.031]	[0.030]	[0.030]	[0.029]
Observations	9557	9557	9256	9188
Log likelihood	-4120.13	-4117.36	-3660.22	-3602.01
Panel B	*DV: 1=Ever used a barrier method (diaphragm or condom)*			
Sales ban	0.035	0.023	0.011	0.007
	[0.033]	[0.032]	[0.030]	[0.029]
Sales ban x 1(1965)	-0.040	0.016	0.036	0.040
	[0.040]	[0.056]	[0.044]	[0.045]
Sales ban x 1(1970)	-0.038	0.010	0.013	0.019
	[0.045]	[0.045]	[0.043]	[0.039]
Observations	9557	9557	9256	9188
Log likelihood	-6252.97	-6242.76	-5341.91	-5272.19
Additional covariates[a]	R	R	RACEI	RACEIK
Legal variables[b]	PX	PX,AD	PX,AD	PX,AD

Notes: Panels A and B present the average marginal effects from probit specifications corresponding to equation I for three years of cross-sectional data (1955, 1965, and 1970). Robust standard errors clustered at the state level are presented in brackets. [a] Covariates include fixed effects for census region denoted R. Columns 3 and 4 include dummy variables for age categories, A (18-19, 20-24, 25-29, 30-34,35-39); Catholic, C (equal to one if the respondent indicated her religious preference was Roman Catholic); educational categories, E (≤8, 9-11, 12, 13-15, 16 or more); and husband's annual income, I (0-4500,4501-6500, 6501-8500,10500-21000 in 1969 dollars). The omitted category includes 18- to 19-year-old, non-Catholic white women in the Northeast with less than 9 years of education whose husbands report no earnings. Column 4 adds a set of dummy variables for the "ideal number of children in the average American household", K, to proxy for differences in norms about childbearing. The omitted category changes to be all of the categories listed above and women desiring no children. [b] Legal covariates include interactions of states with sales bans and physician exceptions (PX), and states with advertising bans only (AD) with a set of dummies for the years 1955, 1965 and 1970. Sample: Married women between the ages of 18 to 39 at the time of the survey. Sources: 1955 *GAF*, 1965 and 1970 *NFS*.