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

Martha J. Bailey

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Abstract

The 1960s ushered in a new era in U.S. demographic history characterized by sharp reductions in family size. To estimate the importance of oral contraception in this transition, this paper develops a new empirical strategy based upon idiosyncratic variation in the language of "Comstock" laws. The central results of this analysis suggest that the birth control pill accelerated the reduction in the fertility rate in the early 1960s and facilitated the transition to the two-child household. The implications of these results reach beyond the 1960s, as reduced fertility has had a lasting impact on the American family and economy.

Contact information

The author is an Assistant Professor in the Department of Economics and a Research Affiliate at the Population Studies Center at the University of Michigan, and a Faculty Research Fellow at the National Bureau of Economic Research. Mailing address: Department of Economics, University of Michigan, 611 Tappan Street, Ann Arbor, Michigan, 48108-1220; email: baileymj@umich.edu.

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Lyrics to Loretta Lynn's 1972 song, “Pill”

(Lorene Allen - Don McHan - T.D. Bayles)

You wined me and dined me when I was your girl
Told me if I’d be your wife you'd show me the world
But all I’ve seen of this old world is a bed and a doctor bill
I'm tearin’ down your brooder house cause now I've got the pill

All these years I’ve stayed at home while you had all your fun
And every year that’s gone by another baby’s come
There’s gonna be some changes made right here on nursery hill
You’ve set this chicken your last time cause now I’ve got the pill

This old maternity dress I’ve got is going in the garbage
The clothes I’m wearin’ from now on won't take up so much yardage
Miniskirts hot pants and a few little fancy frills
Yeah I’m making up for all those years since I’ve got the pill

I’m tired of all your crowing how you and your hens play
While holdin’ a couple in my arms another’s on the way
This chicken’s done tore up her nest and ready to make a deal
And you can’t afford to turn it down cause you know I’ve got the pill

This incubator is overused because you’ve kept it filled
The feeling good comes easy now since I’ve got the pill
It's getting’ dark it’s roostin’ time tonight it’s too good to be real
Oh daddy don’t you worry now cause momma’s got the pill
Oh daddy don’t you worry now cause momma’s got the pill

I. Introduction

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 (Lundberg and Pollak 2007).

Although hundreds of articles in academic and popular journals document these trends and speculate about their origins, the underlying causes of this "second demographic 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, the birth control pill. Armed with national survey evidence, population scientists heralded the 1960s as a period of "contraceptive revolution" characterized by substantial declines in unplanned marital childbearing (Westoff 1975) and a significant rise in the frequency of marital intercourse (Westoff 1974). In his Presidential Address to the Population Association in 1975, Charles 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).

Economists have been critical of this view and have instead emphasized changes in the demand for children.[2] Because sharp reduction 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 rate during the 1920s, for instance, to highlight how rapidly behavior might have changed in the absence of modern contraceptive technology. For these reasons, Gary Becker concludes in his *Treatise on the Family* (1991: 143) that "the

[1] This comparison is made for cohorts entering their childbearing years in difference decades. Women born in 1930 (1949) entered their childbearing years during the 1950s (1970s).

[2] 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.

'contraceptive revolution' … ushered in by the pill has probably not been a major cause of the sharp drop in fertility in recent decades."

The importance of this scholarly debate is difficult to overstate. The relevance of birth control technology to human behavior bears upon theoretical formulations of economic and population growth, many of which implicitly ignore significant changes in the technology and costs of regulating births. It also has substantial implications for economic models of the age-structure, 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 (Pritchett 1994a, 1994b; Bongaarts 1994, Knowles et al. 1994)

Yet the difficulty of disentangling the contribution of modern contraception has limited the conclusions of research on this question. This is especially true in the context of the United States, where the release of the first birth control pill in 1957 coincided with the peak of the baby boom and rapidly changing awareness, attitudes, and norms about women's rights and roles.[3] This simultaneity renders standard, inter-temporal (before-and-after or cross-cohort) comparisons difficult to interpret. Fifty years after the advent of oral contraception, it is unclear whether the 1960s were a "contraceptive revolution" or a revolution of the times.

Recent empirical research provides little insight into this question, 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 (Goldin and Katz 2002, Bailey 2006, Guldi 2007).[4] Although the findings of these studies are suggestive, it is doubtful that they generalize to the 90 percent of childbearing

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[3] In 1963, President 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, a book documenting the emotional and intellectual oppression of the middle-class housewife. In 1964, Title VII of the Civil Rights Act codified a prohibition on employment discrimination against women. (Many believe that "sex" was included as a last-ditch effort to kill the bill. Nevertheless, this piece of legislation formed a legal basis for prosecuting discrimination against women in years to come.) In 1966, the National Organization for Women was organized by the NAACP. By the end of the decade, the women's movement was pervasive and instrumental in generating awareness of gender inequities, if not opportunities, for many women.

[4] Goldin and Katz (2002) and Bailey (2006) use variation in the age of legal consent. This variation existed primarily for unmarried women ages 18 to 20.

decisions that occurred after marriage in the 1960s. The "power of the pill" for the population of married, adult women remains an open question.

This paper seeks to quantify the importance of the birth control pill in shaping married U.S. women's childbearing decisions using a novel empirical strategy. This strategy makes use of a newly compiled legal history of U.S. anti-obscenity, or "Comstock," laws and exploits idiosyncratic variation in statutory language. In particular, some states enacted explicit bans on the sale of contraceptive supplies whereas others enacted only bans on the sale of information and/or provided for physician exceptions. I use the 1955 Growth of American Families Study (GAF) and 1965 and 1970 National Fertility Studies (NFS) to document how Comstock sales bans effectively curtailed use of the birth control pill until 1965, although they appear to have been ineffective in limiting use of other methods. This is an important contribution of this paper, as the relationship between legal restrictions and contraceptive use has been difficult to test in earlier research in this area.[5]

In the interest of a clean and compelling empirical argument, the primary focus of this analysis is on establishing a causal link between the birth control pill and childbearing outcomes in the 1960s. Sections II, III, and IV relate this paper's empirical exercise to neoclassical models of fertility and provide historical and quantitative evidence for its identifying assumptions. Section V documents the strong and robust relationship between Comstock sales bans and reductions in the U.S. fertility rate. Section VI shows how changes in period fertility mapped into smaller families by 1970. The central results of this analysis suggest that oral contraception accelerated the reduction in period birth rates and facilitated the eventual transition to the two-child household. The implications of these results reach beyond the context of the 1960s, as low fertility has had a lasting impact on the American family and economy.

[5] For the 1960s and early 1970s, very little publicly available information exists on the sexual activity or contraceptive use of never-married or adolescent women. For this reason, it has been difficult to verify the relationship between legal barriers and use of the birth control pill in this population.

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

The pioneering work of Michael and Willis (1976) laid the conceptual foundation for understanding both the role of the demand for and supply of children in a single neoclassical framework. Their framework supplements the now standard, neoclassical approach to deriving the demand for children, N^* (Becker 1960, 1965; Willis 1973; Becker and Lewis 1973) with the "supply-side" stressed in the demographic literature (Sheps 1964, Sheps and Perrin 1966).[6] Their extension relaxes two assumptions in the standard model, which limited the potential role of improvements in contraceptive technology or changes in its price.[7] 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 (later referred to as a "failure rate"). This is equivalent, under the assumptions in their model, to choosing an *ex ante* distribution of the number of children born, which is summarized by its first, μ, and second moments. Second, they relax the assumption that fertility regulation is costless. In their framework, contraceptive strategies are associated with prices, π_e, and couples consider the marginal cost of using a variety of contraception strategies relative to 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 costs.

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 *i*, where strategy *i*

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[6] These models typically assume that household utility, U(C,Z), is increasing in the quantity, N, and quality, Q, of children and is defined over the product of these terms, C=NQ, and a composite commodity, Z. Consumption is limited by lifetime income, I, the "price of child services," πc, and the per-unit cost of the composite commodity, πz. 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.

[7] 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.

entails a fixed, α_i, and a marginal component, β_i. The total cost of this decision is $\pi_{ei} \equiv \pi_{ei}(\mu; i) = \alpha_i + \beta_i(\mu_N - \mu)$, where μ_N is the expected distribution of children born in the absence of any contraceptive method. 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 obtaining refills (as with condoms or the birth control pill). Fixed costs might include costs such as the price of searching for or obtaining supplies or learning about a particular method. For the sake of illustration, panel A of figure III plots total costs by birth 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 costs π_{e1}) may be optimal. A couple wishing to avoid two births may choose the strategy associated with π_{e2} but another one associated with π_{e3} to prevent four births. The high fixed but low marginal costs of strategy π_{e4} would be optimal for preventing five or more conceptions. In this sense, the total cost function for achieving any birth distribution, μ, is given by the lower highlighted envelope, or $C(\mu) = \min_i \{\alpha_i + \beta_i (\mu_N - \mu)\}$.

The introduction of the birth control pill may have altered the total cost function in a manner suggested by strategy π_{e5}, which is illustrated in panel B of Figure III. Because the birth control pill required regular visits to a physician, the fixed cost of use would have been at least as high as any other method available in the 1960s. However, the marginal cost of preventing births was much lower, as it required neither effort nor discomfort at the time of intimacy. As illustrated in blue, the availability of oral contraception may shift downward and flatten a portion of the total cost curve. Therefore, the introduction of the birth control pill may be viewed as reducing the marginal cost of averting births above some threshold.

In summary, the Michael and Willis framework yields several important predictions about the diffusion and adoption of the birth control pill. First, the birth control pill reduced the marginal costs of

preventing births. Holding the demand for births constant, reducing the marginal costs of preventing births 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.[8] The impact of a reduction in uncertainty, however, is more complicated to assess.[9] On the one hand, greater reliability minimizes the chance of having more children than intended. However, a more reliable stopping technology may increase the number of children borne by eliminating the risk of overshooting. Therefore, the impact of oral contraception on the number of children born over the lifecycle is theoretically ambiguous.

One final contribution of the Michael and Willis framework is a model of how couples choose a contraceptive strategy in response to a host of parameters including market prices, household income, and preferences. In their framework, factors inducing the adoption of the birth control pill are strongly related to the demand for children. It is for this reason that comparisons of childbearing outcomes among women taking and not taking oral contraception may be misleading.

In order to identify the impact of changes in birth control technology, an empirical strategy 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 central assumptions underlying this analysis are that state-level restrictions on the sales of contraceptives made oral contraception disproportionately more expensive to find, purchase, and refill, but that these laws were otherwise unrelated to cross-state differences in the demand for children. The next section provides historical and quantitative support for these assumptions.

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[8] From the beginning, proponents of the birth control pill promoted the pill as 99 percent effective. Although numbers on the effectiveness of contraception are dubious at best, Planned Parenthood estimates the failure rates associated with *typical use* of the condoms available today at around 15 percent and the failure rates of today's modern diaphragms at around 16 percent and almost twice that for women having borne a child. It is unclear how much of this figure is attributable to inappropriate use. Less effective spermicides and other materials imply that failure rates of these methods would have been much higher in 1960.

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

III. How Comstock Laws and *Griswold v. Connecticut* Shaped the Diffusion of Oral Contraception

In 1873, the U.S. Congress codified a prohibition that outlawed 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).[10] Because the federal government may only regulate interstate commerce or importation, the federal Comstock Act could not ban trade in obscenities within states. Nevertheless, the Act succeeded in its secondary purpose to "incite every State Legislature to enact similar laws" (Dienes 1972: 43, quoting Representative Merrimam, *New York Times*, Mar. 15, 1873, p. 3, col. 3), so that, by 1900, 41 states had enacted or amended anti-obscenity statutes, which directly regulated trade in "obscene" or "immoral" information within states.[11]

As summarized in Table I, state statutes varied considerably in their original language and, as a result, in their ultimate implications for the diffusion of oral contraception approximately three-quarters of a century later. Slight variation in the language of these statutes allows them to be grouped into four categories (more detail is provided on the exact wording of statutes in each state and coding decisions in Appendix A).

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 not used in the analysis.

2. Advertising or information bans in 31 states explicitly outlawed the distribution of information or advertising for articles, instruments, medicines or "secret nostrums" (sometimes items for the exclusive use of females) 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. Presumably, customers or patients could request these and physicians or pharmacists could fill these requests without violating laws in this category.

[10] This Act was quite 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" (Tone 1996: 488). This Act is often referred to as the "Comstock Act" after its zealous Congressional advocate, Anthony Comstock of New York. The federal Comstock Act was amended in 1971 to strike out the language prohibiting contraceptives from being mailed.

[11] Five states (Arkansas, Delaware, Montana, South Dakota and Washington) passed similar legislation in the early 1900s. New Mexico was the only state for which no anti-obscenity statute was identified.

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.

4. Physician or pharmacist exceptions in 8 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. The interpretation of "legitimate business" exceptions are subject to considerable disagreement among legal scholars and, as such, are not treated as exceptions in this analysis.

The most restrictive type of statute for the use of contraceptives was a sales ban *without* a physician exception. These laws, in effect, taxed methods that required the purchase of supplies in three ways: (1) by reducing the number of producers and providers, (2) by forcing black market providers to cover their expected losses by raising prices, and (3) by increasing search costs for potential customers. Consequently, the total cost of each contraceptive strategy requiring supplies would rise due to increases in both the fixed and marginal costs, or $\pi_{ei}^{S} = \alpha_i (1+t_\alpha)+\beta_i (1+t_\beta) (\mu_N - \mu)$, where ^{S} references the cost function in states with sales bans, $t_\alpha \geq 0$ is the fixed mark-up, and $t_\beta \geq 0$ is the marginal mark-up. Note that a sales ban should have no effect on the price of methods requiring no supplies, such as abstinence, rhythm or withdrawal. 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. As a medical technology that also requires frequent refills, the price of the birth control pill would increase via both mechanisms: the prescription requirement would increase the price via t_α and fairly frequent refills would raise t_β. Therefore, laws banning the sales of contraception had a disproportionate impact on the price of the birth control pill.[12] A codified exception for physicians would effectively remove the taxes imposed by a sales ban.

[12] Letters by Anthony Comstock himself suggest that sales bans were exceedingly difficult to enforce. Andrea Tone (2000, 2001) takes this argument further by saying that sales bans were largely irrelevant before the age of the birth control pill.

Qualitative histories provide another compelling reason as to why Comstock statutes may have disproportionately taxed oral contraception. Because diaphragms and condoms were relatively simple to produce, ship, and store, black market availability substantially reduced the burden of the law (2000 2001). Customers could easily obtain barrier contraceptives through the mail by ordering products under the auspices of "feminine hygiene" or "womb support." Male "sheaths" or "shields" and "uterine elevators" were also available for the "prevention of disease." The black market distribution of oral contraception, however, required the production of a pharmaceutical compound, which was, at least in the shorter-term, more difficult for black market entrepreneurs to replicate. Even assuming this technology might be replicated, it is less likely that many women would have opted to ingest illicit chemical pills. Assuming no change in physician compliance with the introduction of the birth control pill, these characteristics of the birth control pill imply a much higher tax on use relative to other contraceptives.

In summary, 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 adopted the birth control pill. With similar marginal benefit curves, differences in price imply that women in restrictive states may have opted (at least in the shorter-term) to avert fewer births. Fewer women residing in states with sales bans would have used oral contraception and would have been more likely to experience a pregnancy in the shorter-term. For both reasons, more children may have been born in restrictive states in the shorter-term.

Statutes limiting advertising or the distribution of information were probably much less important. Although these laws did limit direct advertising in the form of store displays and informational brochures in physicians' offices, customers or patients needed only to ask for supplies. Indeed, G.D. Searle was able to cultivate awareness and demand for *Enovid*, which became the first birth control pill, without direct advertising at all. Even though the U.S. Food and Drug Administration (FDA) had only approved the drug for the regulation of menses in 1957, the company ran an informational campaign to "warn"

physicians about the *Enovid*'s anovulant "side-effects."[13] In addition, the company 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* (Watkins 1998). Consequently, the public and physicians were probably well aware of *Enovid*'s contraceptive properties at least three years before it was approved for this purpose by the FDA in 1960, in spite of federal and state advertising bans.[14]

The end of the Comstock era arrived when the 1965 U.S. Supreme Court enjoined Connecticut's Comstock statute on the grounds that it violated the implied right to marital privacy in the U.S. Constitution. This decision explicitly addressed Connecticut's 1879 provision that uniquely prohibited the "use" of material for the purpose of contraception. *Griswold*, however, left open the issue of constitutionality with respect to state laws banning sales.[15] Nevertheless, most states had responded by revising or amending their statutes by 1971 (Pilpel and Wechsler 1969, DHEW 1974).

IV. Using Comstock Sales Bans to Understand the Impact of Oral Contraception

In order to use Comstock laws and their liberalization in the aftermath of *Griswold* to assess the importance of oral contraceptives, it is necessary assume the laws are unrelated to state differences in the demand for children but did affect the price of oral contraceptives and, therefore, the supply of births. This section provides empirical evidence supporting these assumptions using the 1955 Growth of the American Family Study (GAF) and the 1965 and 1970 National Fertility Studies (NFS). The first subsection shows that the existence of a sales ban appears unrelated to differences in the demand for children as of 1955, which was shortly before the advent of the birth control pill in 1957. The second

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[13] Searle notified physicians of the drug's contraceptive properties in mass mailings noting that "there is adequate evidence to indicate that the drug will inhibit ovulation" (Watkins 1998: 36).

[14] Advertising and informational bans play an important role in this analysis, because they facilitate a test of whether states that specifically characterized obscenities in terms of things related to the "prevention of conception" are fundamentally different in terms of contraceptive use from states that did not.

[15] 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".

subsection shows that sales restrictions appear to have slowed the diffusion of oral contraception in the early 1960s, but not after the Griswold decision in 1965.

Were Sales Bans Related to Differences in the Demand for Children?

A map of states by type of law in Figure IV provides prima facie evidence that sales laws are not simply markers for cross-state differences in the demand for children in 1960. All shaded states had some sort of statute that specifically cited the "prevention of conception" as part of an anti-obscenity statute. The blue states had advertising and informational bans only (Table 1, column 2). The green states additionally banned sales (Table 1, column 3). The yellow states had sales bans but excepted physicians (Table 1, column 4). The orange states liberalized or amended their statutes before 1965 (Table 1, column 5). Simple political or social norms do not appear related to the presence of any Comstock law, or, more specifically, the presence of a sales ban. Laws of different types are found in each census region of the country, and their rosters contain states that proved progressive in their family planning and social policies by the end of the 1970s. For instance, California and Washington, two of the three continental U.S. states to legalize abortion prior to *Roe v. Wade* in 1973, actually *restricted* the sales of or dissemination of information on contraception in 1960.[16] By 1960, this idiosyncratic variation in language may have had important implications for the diffusion of oral contraception.

The 1955 GAF Study provides further evidence on differences between states with and without sales bans before the introduction of the birth control pill. Although this survey queried only white married women ages 18 to 39 who were currently living with their spouses, information on respondents' residence allow legal regime to be linked to respondents' attitudes about family planning policy and use of other contraceptives *before* the introduction of the birth control pill. I examine differences in attitudes about family limitation and contraceptive use using the following linear probability model,

[16] Another pattern is the absence of many laws in the South. I have found no information in the law or the historical literature as to why this is the case. My reading of obscenity statute texts suggests that the primary explanation for this is that Southern states had much broader statutes prohibiting the "corruption of morals." These statutes predated the 1870s Comstock amendments and revisions. The implication is that Southern states tended to have statutes that could accommodate the evolution of social norms about obscenity, whereas codified prohibitions on contraception provided much less flexibility.

(1) $$Y_{irs} = \text{Sales}\ \gamma_1 + \text{Exception}\ \gamma_2 + \text{Advertising}\ \gamma_3 + \Sigma_r g_r \gamma_{4r} + \varepsilon_{irs},$$

where *Sales* is a binary variable equal to one if state *s* had a law that restricted the sale or distribution of contraception in 1960, *Exception* is a binary variable equal to one of a state had a codified physician exception to the sales ban, *Advertising* is a binary variable equal to one if a state banned advertising or publishing of information related to contraception, and g_r includes a constant and three binary variables for census regions (Northeast omitted). Standard errors are corrected for heteroskedasticity and correlation within primary sampling units.

The first outcome variable (estimates presented in table IIA) summarizes the respondent's "approval" of family planning taken from an open-ended question ("Many married couples do something to limit the size of their families and to control when their children come. How do you feel about this?"). Nationally, approximately 76 percent of respondents indicated their approval.[17] Column 1 reports a comparison of mean responses between women in states with sales bans (including those with physician exceptions) to mean responses in states without these bans (*Sales*=1 for all states checked in column 3 of Table I). The specification in column 2 allows the relationship among states with strict sales bans to differ from those with physician exceptions (*Exception*=1 for all states in column 4 of Table I). Column 3 presents estimates comparing states with sales bans to those without in the same census region. It is notable that the negative relationship in columns 1 and 2 is completely accounted for by cross-census region heterogeneity in responses and disappears when census region fixed effects are included in column 3. Finally, column 4 compares states with sales bans to states with virtually identical language (but which banned only advertising) within the same census region (*Advertising*=1 for all states in column 2 of Table I). None of these estimates provide evidence of a strong or systematic relationship between women's attitudes about family planning and residence in states with sales bans.

[17] These responses are encoded in the publicly available dataset into the categories of "Unqualified approval," "Qualified approval," "Pro-Con," "Qualified disapproval," and "Unqualified approval." I code the first two as "Approval."

The apparent lack of a relationship, however, may be due to differences in observable characteristics across states. To explore this possibility, panel B of table IIA adds covariates, which should affect the marginal benefit of averting births under the Michael and Willis model: age, Catholicism, woman's education, husband's income, and the number of children desired before marriage. I also include a set of dummy variables to account for sampling design (dummy variables for the size of the primary sampling unit). The inclusion of these variables increases the variation explained by the model from 0.03 to 0.24, but the magnitude of the point estimates changes negligibly. Panel C additionally explores whether the absence of a difference (especially given the small sample size) is due to one census region being very different from the others. Dropping one census region at a time reveals that approval of family planning does differ substantially across regions, but the presence of a sales ban is a poor predictor of differences in attitudes within regions.

Table IIB examines whether women in states with sales bans were different in their use of more effective barrier methods. Approximately 70 percent of respondents reported having used some type of contraceptive (another 11 percent indicated using douche), and almost 50 percent used the diaphragm or the condom, the most effective methods available in 1955. The panels in table IIB replicate the specifications in table IIA and, again, provide no evidence that women in states with sales bans used barrier methods less frequently than the comparison group. In each specification, the estimates are very small in magnitude (on the order of less than 2 percentage points, implying a 4 percent reduction in use in panel B) and statistically indistinguishable from zero. Even ignoring statistical significance, there appears to be no consistent relationship in panel C as different regions are omitted.

In summary, the 1955 GAF provides no evidence that sales bans reflect underlying differences either in attitudes about family planning or the use of barrier methods. Sales bans do not appear to reflect latent differences in the demand for children.

Did Sales Bans Slow the Diffusion of Oral Contraception?

A central issue in this analysis is whether bans on the sales of contraceptives reduced the use of oral contraception at all. The empirical results in the previous section support the historical record and legal literature on this topic, which claim that Comstock laws were weak or irrelevant.[18] This assertion is also found in Frankfurter's 1961 opinion in *Poe v. Ullman*, where the majority of U.S. Supreme Court voted to uphold the Connecticut law because there was no evidence that it had ever been enforced.[19] In reaction to this, Estelle Griswold and C. Lee Buxton opened a family planning clinic in November 1961, which operated for nine days in New Haven, Connecticut, before being raided by the police. Their very public arrest provided ammunition to those wishing to challenge the Connecticut law in the courts and culminated in the 1965 *Griswold* decision. Nevertheless, in legal circles *Griswold* is largely regarded as a test case about a relatively unimportant statute.

Retrospective information on the *monthly* use of oral contraception among currently married women in the 1965 and 1970 NFS allows a unique test of these claims.[20] Because these surveys also contain rich

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[18] 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).

[19] In *Poe v. Ullman*, Frankfurter writing for the majority says 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…The fact that Connecticut has not chosen to press the enforcement of this statute deprives these controversies of the immediacy which is an indispensable condition of constitutional adjudication." It should be noted that Garrow's account of this period provides several anecdotes to the contrary. He 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).

[20] The 1960 GAF also contains important information on the use of contraceptives, but the publicly available survey does not contain state identifiers. The primary limitation of the 1965 and 1970 NFS is that they are small: the 1965 survey sampled only 5617 currently married women born after 1910 and the 1970 survey sampled only 6752 ever-married women (5981 currently married). Secondly, both surveys contain information on state of residence at the time of survey, but no information on residence during the 1960 to 1965 period. Finally, the surveys were not designed to be representative at the state or local level, which severely limits my ability to break estimates down to the state or local level. Neither the 1965 nor the 1970 survey sampled every state, which prevents me from taking full advantage of the rich cross-state variation in legal regime. The 1965 NFS did not sample women in Arkansas, Nevada, New Hampshire, North Dakota, or Wyoming. The 1970 NFS has a slightly different sampling frame. This survey sampled ever-married women (not just currently married women) born from 1920 to 1955 who resided in the continental U.S. Like the 1965 NFS, the 1970 NFS did not sample from every state. Colorado, Idaho, Rhode Island, and Wyoming are omitted. From the analysis, I exclude the 0.042 in sales ban states and 0.044 in other states of women who were surgically sterilized before 1958.

information on economic variables, education, and religiosity as well as state identifiers, I can link the relevant legal regime to each individual in the sample.[21]

One of the remarkable trends revealed in the NFS is how quickly U.S. women adopted oral contraception. 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 the end of the decade. However, the adoption of oral contraception was slower in states with strict sales bans (that is, sales bans without physician exceptions). Figure V plots the difference in the cumulative proportion of women ever using the birth control pill between states with and without sales bans by region of residence. Fluctuations in these time series reflect heaping of women in certain months like January or July and are especially noticeable in the Northeast and South, where the samples sizes in the states with sales bans are small. However, the negative estimates in each region and the downward trends indicate that use of oral contraception lagged in states with sales bans.

Using the specification in equation 1, table III examines the robustness of this difference to finer designations of legal regime and, as in the previous section, adjusts cross-state differences in pill use for differences in observable characteristics.[22] The dependent variable is equal to one if a respondent reported ever using oral contraception before the *Griswold* decision. (This measure is comparable to the 1955 GAF measure of barrier method use in table IIB.) Panel A reports estimates obtained from linear probability models, where the specifications correspond to those in table II. In contrast to similar patterns of contraceptive use in the 1955 GAF, use of oral contraception is substantially lower in states with sales bans relative to states in the same census region. The fact that physician exceptions completely offset the negative impact of sales restrictions is also consistent with the expected effect of the laws.[23] Moreover,

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[21] Note that 1965 NFS universe, which was limited to currently married women under age 55, poses little problem for the analysis here. This is because *Griswold* extended the realm of "procreative privacy" to married couples only, and these were precisely the women interviewed.

[22] Standard errors in tables III and IV are corrected for heteroskedasticity and correlation within primary sampling units.

[23] Women in states with physician exceptions were not less likely to have used the pill ($\hat{\gamma}_1 + \hat{\gamma}_2$) than women in other states in the same region (column 3) and other states in the same region with advertising bans.

these differences exist in column 4 even when comparing states with sales bans to states with advertising bans only. Because advertising bans were quite similar in their language and should *not* have restricted physician behavior, this final comparison provides perhaps the most compelling test of the way in which Comstock statutes functioned. Whereas sales bans appear to have had no impact on the use of barrier methods (relative to advertising bans), they were associated with lower use of oral contraception. This corroborates the argument that sales bans affected use due to a much more limited black market and the prescribing behavior of physicians.

Panels B and C examine the robustness of this argument to adjustments for observable characteristics. Even as more of the variation in the dependent variable is explained, the correlation between sales bans and pill use changes negligibly. Patterns of lower use are robust to (and actually strengthened by) adjustments for population size, age, race, Catholic religion, mother's education, husband's income, and desired family size and similar across census regions (panel C).[24] On the eve of the *Griswold* decision in June of 1965, use of oral contraception was approximately 25 percent, or 5 percentage points, lower in restrictive states than in other states in the same census region.

An alternative explanation for the observed pattern in pill use is misreporting. As the national press focused on the legality of contraception leading up to *Poe* and *Griswold*, survey respondents may have become less likely to report their illicit use of contraceptives. To test this possibility, I examine whether women in states with sales bans were less likely to report using other types of contraceptives.[25] If misreporting accounts for lower use of oral contraception in sales states, then respondents should also underreport use of other purchased methods. However, the evidence in table IV is inconsistent with this explanation. The first two panels of table IV report estimates from linear probability models for a subset of the specifications in table III. Panel A uses a binary variable equal to one if the respondent ever used

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[24] Panel B also includes a dummy variable to account for the inclusion of nonwhite women in the 1965 NFS.
[25] I obtain information on "ever-used" the birth control pill from pregnancy interval questions. With this information, I create three new dependent variables: ever used *any* type of contraceptive, ever used contraceptives requiring supplies (barrier methods, douche, sponges, pessaries, etc.), and ever used barrier contraceptives (most commonly condoms or diaphragms). In addition, I create an indicator variable for women who were surgically sterilized.

any contraceptive method before June 1965, and panel B uses a binary variable equal to one if the respondent ever used any barrier method (condom or diaphragm) before June 1965. In both cases, rates of use are slightly lower in states with advertising bans but not statistically or economically different in states with sales bans. Ignoring the precision of the estimates suggests that women in states with sales bans were actually slightly *more* likely to have used barrier methods, which is consistent with their being less likely to switch to oral contraception before *Griswold*.

One final explanation for differences in use of oral contraception is differential trends in unobserved characteristics during the 1960s.[26] One way to examine this possibility is to examine changes in the use of the birth control pill in the aftermath of *Griswold*. Because the active revision of state statutes should have equalized the prices across states after 1965, differences in use should wane, if not reverse, by 1970. But this would not have happened if, for instance, women in states with sales bans were particularly resistant to using oral contraception or women in other states were increasingly keen on the technology. Panel C of table IV provides no evidence that this was the case. In contrast to the stark differences in 1965, differences in the use of oral contraceptives do not persist in the 1970 NFS among currently married women.[27]

In summary, women in states with sales restrictions used oral contraception less before the *Griswold* decision. These differences existed within region and after accounting for differences in observable characteristics such as age structure, race, religious differences (Catholic), respondent's educational attainment, her husband's earnings, or ideals about family size (1965 NFS). Differences in the adoption of oral contraception do not appear to reflect pre-existing differences in attitudes about family planning or pre-existing differences in the use of barrier contraceptives (1955 GAF).[28] Nor are these patterns consistent with misreporting or disparate trends in unobserved characteristics over the 1960s. Women did

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[26] The 1955 GAF rejected differences in levels.
[27] Currently married women are used for consistency with the 1965 NFS.
[28] Historians such as Tone (2001) note the thickness of the black market for barrier contraceptives, which is supported by empirical evidence from the 1955 GAF. Taking these estimates at face value implies that illicit supply sources compensated quite well for reduced physician and pharmacist sales of condoms and diaphragms.

not report lower rates of use of contraceptives besides the pill (1965 NFS), and by 1970 women in states with sales bans were no different in their likelihood of having adopted oral contraception than their counterparts (1970 NFS).

It seems fair to conclude that—almost a century after enactment—idiosyncrasies in statutory language exogenously varied the marginal cost of using oral contraception and, therefore, the marginal cost of averting births during the early 1960s. Although sample sizes do not permit an examination of mitigating cross-state travel, even less than perfectly enforced sales bans appear to have induced sizable differences in the use of the birth control pill. The next section exploits this induced variation in the price of oral contraception to examine its impact on childbearing outcomes.

V. The Impact of Oral Contraception on Aggregate Birth Rates

Adoption of the birth control pill should affect aggregate birth outcomes if use is associated with a lower monthly probability of conception (lower period failure rates). The magnitude of the potential effect on the birth rate, therefore, depends importantly upon the reduction in the probability of pregnancy when using the birth control pill relative to the probability of failure using the next best method. Before estimating the relationship of the pill with aggregate birth rates , I generate back-of-the-envelope calculations to predict the magnitude of this relationship using the 1965 NFS and the 1955 GAF.

The Expected Relationship between Sales Bans and Period Fertility

Denote the number of births to married women of childbearing ages (15 to 44) in year t as B_t^m. These births comprise 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 those women, and f the average failure rate in the population of married, fecund women using contraception. 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, or

$$(2) \qquad B_t^m = F_t^m + P_t^m = Q_{t-1}^m s + (1-Q_{t-1}^m - S_{t-1}^m) f.$$

All but one quantity in this tautology 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 (Trussell 2004).

Obtaining informative estimates of f is extremely difficult, because contraceptive methods cannot be randomly assigned. My approach is to calibrate a pre-birth-control-pill value of f based on 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).[29] Rewriting equation (2), $f = (0.163 - 0.056 \times 0.85)/ (1 - 0.056- 0.294) = 0.177$. 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 Appendix B for more information on the computation of these numbers). Note this figure averages failures among women abstaining from sex (≈ 0) with those not using any methods (considerably higher).[30]

How much should birth rates in the population (not among married women) have changed in states with sales bans after 1957 relative to birth rates in other states? This question can be addressed by redefining f as a weighted average of the failure rates for 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 (calibrated at 0.177). Using this notation, the marital fertility rate at time t can be written as $B_t^m = Q_{t-1}^m s + (1-Q_{t-1}^m - S_{t-1}^m)[\, q_{t-1}^m f^o + (1- q_{t-1}^m) f\,]$. The difference in the marital fertility rate, D_t^m, between states with sales bans, B_t^{mS}, versus those without these laws, B_t^{m0}, at a point in time

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

[30] 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. Using an alternative approach of applying computed six-month failure rates to the proportion of women using each method in the 1955 GAF yields implies a fertility rate of well over 300/1000 women of childbearing age.

can be written as the sum of differences in planned births, failures due to methods other than the birth control pill, and failures due to the pill, or

$$(3) \qquad \Delta D^{m} \equiv D_{66}{}^{m} - D_{56}{}^{m} = (B_{65}{}^{mS} - B_{65}{}^{m0}) - (B_{55}{}^{mS} - B_{55}{}^{m0})$$

$$= [(Q_{65}{}^{mS} - Q_{65}{}^{m0}) - (Q_{55}{}^{mS} - Q_{55}{}^{m0})]\, s$$

$$+ (1 - Q_{65} - S_{65})^{mS} [q_{65}{}^{mS} f^{o} + (1 - q_{65}{}^{mS}) f^{m}] - (1 - Q_{65} - S_{65})^{m0} [q_{65}{}^{m0} f^{o} + (1 - q_{65}{}^{m0}) f]$$

$$- [(1 - Q_{55} - S_{55})^{mS} - (1 - Q_{55} - S_{55})^{m0}]\, f .$$

Assuming that the gap in Q and S did not change from 1955 to 1965 and that $(1-Q_{65}-S_{65})^{mS} = (1-Q_{65}-S_{65})^{m0}$, ΔD depends only on $(1-Q_{65}-S_{65})^{m}(q_{65}{}^{mS} - q_{65}{}^{m0})(f^{o} - f)$.[31] Using the fact that $(1-Q_{65}-S_{65})^{m} = 0.657$ and $(q_{65}{}^{mS} - q_{65}{}^{m0}) \approx 0.06$ (panel B of table III), and $f^{o} \approx 0$, $\Delta D^{m} \approx 0.007$. The change in the difference in the population of U.S. women ages 15 to 44 is due, however, to both changes among married women and unmarried women. Assuming that there was no change in contraceptive use among unmarried women, $\Delta D \approx 0.70 \times \Delta D^{m} \approx 0.005$, or five births per 1000 women of childbearing age in the population. If oral contraception reduced the expected failure rate to zero, this exercise predicts that the gap in marital birth rates between states with sales laws and those without would have increased by approximately seven births per 1000 married women and five births per 1000 women of childbearing age in the population by 1965.

The Estimated Relationship between Sales Bans and Period Fertility

To estimate the actual divergence in aggregate birth rates, I encoded information from the published Vital Statistics Natality volumes by state and age group from 1950 to 1967 and supplemented it with similarly aggregated information from the publicly available Natality Detail files for 1968 to 1980. Each year-by-state-by-age group 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

[31] In practice, $[(Q_{65}{}^{S} - Q_{65}{}^{0}) - (Q_{55}{}^{S} - Q_{55}{}^{0})] \approx 0.019$ and $(1-Q_{65}-S_{65})^{S} - (1-Q_{65}-S_{65})^{0} \approx 0.003$ using white married women under 40 only from the 1965 NFS.

linear interpolation in the intercensal years (Ruggles et al. 2006). The dependent is a birth rate, B_{st}, for each state, *s*, and year, *t* (see Appendix B for more detail on the data set).

Using least squares, I estimate the panel analog of equation (1), which interacts each legal designation and each region indicator with a set of year fixed effects,

$$(4) \qquad B_{st} = \Sigma_t \text{ Sales} \times f_t \, \tau_1 + \Sigma_t \text{ Exception} \times f_t \, \tau_2 + \Sigma_t \text{ Advertising} \times f_t \, \tau_3 + \Sigma_{rt} \, g_r \times f_t \, \tau_{4rt} + \nu_{st},$$

where f_t is a set of dummy variables for years from 1951 to 1980 and the remaining notation is comparable to that in equation 1. Census region-by-year fixed effects capture the evolution of unobservable factors which evolve uniformly within regions across years. Standard errors are corrected for heteroskedasticity and autocorrelation within states using the procedure proposed by Arellano (1987).

The elements of $\tau_1 = [\tau_{1,51}, \tau_{1,52} \ldots \tau_{1,80}]$ capture the difference between birth rates in states with sales restrictions and a comparison group of states within the same census region (either all other states in the same census region, the baseline specification, or states in the same census region with advertising bans). The Vital Statistics volumes do not contain information on state-level, time-varying characteristics like proportion Catholic, educational attainment, or husband's income. Although none of these variables can be included here, it reassuring that these covariates had little impact on the magnitude or the precision of prior estimates.

Because inferences in this differences-in-differences framework are robust to time-invariant, pre-pill differences in use of contraceptives, differential *changes* in the demand for children (due to differentially changing attitudes, for instance) in states with sales bans pose the most important threat to the validity of the inferences. This concern cannot be directly addressed using observed births. Indirect evidence from comparisons of differences in the use of barrier methods in 1955 (Table IIB, panel A) and 1965 (Table IV, panel B) provide no evidence that this is the case. In the 1955 GAF, use of barrier methods among women in sales states bans was one percentage point higher (s.e.=0.037) under the specification in column 4. In 1965, the estimate is only slightly higher at 2.7 percentage points (s.e.=0.024). Recall also that the disappearance of differential use by 1970 (Table IV, panel C) would argue against the attribution

of the results to differential trends over the 1960s. For consistency with the results in section VI, the gap in birth rates would need to widen as the birth control pill diffused more slowly in states with sales bans and would narrow as use of oral contraception converged in the aftermath of *Griswold*. Moreover, consistency with the back-of-the-envelope estimates requires the gap in birth rates to widen by approximately five to seven births per 1000 women by 1965.

Plots of the estimates of τ_1 (and the relevant 95 percent confidence intervals) in Figure VI correspond closely to these predictions. From 1951 to 1957, the gap in birth rates changed little from its 1950 level. 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 an amount that corresponds closely to back-of-the-envelope calculations—around six to seven births per 1000 women. Another striking feature of these plots is the smooth evolution of fertility rate differences in the post-period. These laws are poor predictors of abortion legalization and post-1965 fluctuations in the birth rate. The fact that laws banning contraceptive sales are only associated with growing differences in birth rates *from 1958 to 1965* is consistent with the waxing in their effectiveness with the introduction of a refillable, prescription contraceptive and the waning of their relevance in the aftermath of *Griswold*.

These patterns are also apparent across four further specification checks. First, patterns before 1970 are virtually identical when comparing states with sales bans (and no physician exception) to states without in the same census region (plotted in the solid, thick line) or to states with advertising bans only in the same census region (plotted in the line with X-markers). States with advertising bans experienced fertility rates similar to those in states without *any* restrictions. Not only is this finding consistent with the hypothesis that laws banning advertising should not have affected pill use, but it is also consistent with the absence of differences in use of the birth control pill in the 1965 NFS (column 4 of table 3). Finally, it underscores the proposed mechanism of sales bans affecting the birth rate by changing physician prescribing behavior (rather than by restricting the flow of information).

Second, patterns are present and similar among states not liberalizing before *Griswold*. Panel A presents estimates using all states (excluding Alaska and Hawaii), where states which liberalized their

sales bans before the *Griswold* decision are treated as having as having restrictive bans. Panel B presents estimates of τ_1 from specifications which omit these states.[32] The similarity of patterns with (panel A) and without (panel B) across specifications suggests this was not the case. The estimates, therefore, do not appear to be driven by unobserved political, legal or social changes which affected both the birth rate and motivated amendments in obscenity laws.

A third specification check demonstrates that sales bans tended to have similar effects across regions. Similar to the analysis in the lowest panel of Table III, panel C explores the robustness of the results to excluding one census region at a time.[33] For comparison, the thick, solid line represents point estimates from the baseline specification when all the regions are included, and the dashed thick lines represent the 95 percent confidence interval for these averaged estimates. If differences within one census region are driving the aggregate results, omitting that region from the analysis would yield a strikingly different pattern of estimates. This does not appear to be the case, as the 1957 to 1965 divergence in birth rates is similar when each census region is omitted.

Finally, disaggregating the fertility rate into age-group-specific birth rates further illuminates how the availability of the birth control pill affected childbearing outcomes. Using the same specification as in equation (4), I define the dependent variable as the period, state-level birth rate within one of six five-year age groups (15 to 19, 20 to 24, …, 40 to 44). Panel A of figure VII plots the point estimates from the baseline specification. Panel B excludes the subset of states liberalizing their sales bans before 1965. In both specifications, the patterns for 15 to 34 year olds are similar to those in the aggregated regressions. The divergence in birth rates is most pronounced among 20 to 24 year olds, where the difference grows to 17 births per 1000 women from 1958 to 1965 and drops precipitously from 1965 to 1970. The pattern is less pronounced but also present among 15 to 19, 25 to 29, and 30 to 34 year olds. In contrast, the pattern for 40 to 44 year olds barely deviates from zero, which is consistent with the paucity of women who

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[32] The results are qualitatively similar when I allow the legal variables to "turn off" following a pre-Griswold revision.

[33] While only results for the specification omitting *Advertising* are presented, within-region comparisons to states with advertising and information bans are similar.

adopted oral contraception in this age group. Panel B shows that post-1965 is even more striking when the sample excludes states liberalizing before *Griswold*.

Another striking result is that birthrates among women in their thirties in states with sales bans did *not* converge as quickly after 1965. Although birth rates among 30 to 34 and 35 to 39 year olds in states with sales bans fell more slowly from 1957 to 1965, these rates remained slightly elevated after 1965. This may reflect that women in their later thirties were less likely to adopt oral contraception (consistent with Michael and Willis and use rates in the 1965 NFS). Alternatively, this may reflect path-dependence. Women who were less likely to use oral contraception as younger women (in the early 1960s) had more children and pursued different family-career trajectories (cf. Bailey 2006).

What is clear from the results in figures VI and VII is that the birth control pill appears to have shaped aggregate birth rates and increased the tempo of the post-1960 decline in the U.S. fertility rate. These tempo effects, however, do not appear to include prevention of births among older women (sterilization, although irreversible, was available over this period). Instead, the birth control pill was used by younger women to delay or suspend childbearing—perhaps even indefinitely.

VI. The Impact of the Birth Control Pill on Completed Fertility

Up to this point, the analysis has considered changes in the period fertility rate. This final section uses these temporary price differentials to assess the larger impact of the birth control pill on completed fertility and family size, the N* of interest in many economic models.[34] It is worth noting that temporarily higher period fertility rates need not indicate permanently larger family sizes. For instance, women may have used the birth control pill primarily for timing purposes (Bailey 2006). On the other hand, recent research using a period-based empirical strategy suggests that a reduction in the marginal cost of avoiding

[34] Legally induced price differentials were temporary—they lasted at most eight years and became negligible after *Griswold*. Women who bore more children in states banning the sales of contraceptives before 1965 may have borne more or fewer children after 1965. Theory provides little guidance here, because alternative sets of assumptions may lead to different predictions. Michael and Willis (1975) demonstrate how changes in the cost of averting births *may* affect completed fertility, but their model is silent on whether temporary price differentials should have lasting effects.

childbirth may impact completed fertility as well (Ananat, Gruber and Levine 2004). Indeed, the plots in figures V and VI, which never fall below zero, corroborate the notion that completed fertility rates remained slightly higher among women in states with sales bans.

To gauge the permanence of these period differences, I estimate the cohort analog of equation 4 using the 1970 IPUMS census samples, or

$$(5) \qquad N_{sc} = \Sigma_c \text{ Sales} \times h_c \lambda_{1c} + \Sigma_c \text{ Exception} \times h_c \lambda_{2c} + \Sigma_c \text{ Advertising} \times h_c \lambda_{3c} + \Sigma_{rc} g_r \times h_c \lambda_{4rc} + \upsilon_{sc},$$

where N_{sc} is the number of live births to respondents born in state *s* in cohort *c*, h_c contains a constant and a set of dummy variables for five-year birth cohorts (1920-1924, …, 1945-1949; 1915-1919 is omitted), and other notation remains as previously defined. The data are collapsed into state-of-birth-cohort cells, and standard errors are corrected for within-state serial correlation and heteroskedasticity using the procedure proposed by Arellano (1987). In this specification, estimates of λ_{1c} describe relative changes in the within-census-region difference in completed fertility in states with and without sales bans. Note that these changes are relative to the gap in live births to women born from 1915 to 1919.[35]

Table V presents estimates of λ_{1c} from three different specifications using the 1970 IPUMS. These comparisons are not as clean as the analysis of period fertility, because no pre-treatment measure of cohort fertility is available in 1957. As a result, the confidence intervals are larger, and, unlike the analysis of period fertility, the choice of comparison group is important. Using 1970 census data, panel A presents estimates using two comparison groups. Moving from column 1 to column 2 changes the comparison group from states in the same census region without sales bans to states in the same census region with advertising bans only and reduces the magnitudes of the point estimates considerably. Further, omitting states that liberalized their sales bans before Griswold increases the estimates slightly (column 3). I regard comparisons in column 3 as the most credible but present estimates for each of the three comparisons for interested readers.

[35]This comparison is chosen because these women would have been 38 to 42 years in 1957 and, according to figure VII, their period fertility rates were not differentially affected from 1957 to 1965.

The striking pattern in the 1970 data is that there is a sizable and statistically significant relationship between the number of live births among women born between 1920 and 1950 and the presence of a sales ban. For women born from 1920 to 1924 (ages 33 to 37 when *Enovid* is introduced), the estimated magnitudes suggest that approximately 3.3 percent of these mothers had an additional child by 1970. This number is slightly smaller than predicted from the sum of the deviations for women ages 30 to 34 in figure VII (≈ 41 births/1000 women in this age group). The numbers are even larger for the women born from 1940 to 1944, where roughly 7 percent of women born in states with sales bans had an additional child by 1965 (the sum of deviations among women ages 20 to 24 in figure VII is ≈ 93 births/1000 women in this age group).

Because the shift to the two-child family was one of the most prominent changes from cohorts born in the 1920s to those born in the 1940s (Figure II), the right-most three columns of table V describe the relationship between the proportion of women having two or fewer children and birth in a state with a sales ban. For women born from 1920 to 1924, the estimates in column 3 show that restrictive sales bans are associated with a 1.6 percentage point reduction in the number of women having two or fewer children (relative to the difference for the cohorts of 1915 to 1919). For women born at the end of the 1940s, this figure was almost three times as high as 4.7 percentage points, a decrease of approximately 9 percent over the group mean of 54 percent.[36] This suggests that use of the birth control pill among younger women led to ultimately smaller family sizes and a significant increase in the fraction of women having two or fewer children.

One alternative explanation for these results is that completed fertility was trending differently across cohorts in sales states before the birth control pill was introduced (although no changes in the difference in period birth rates are evident before the 1957 in the panel data on births). If this were the case, one might expect these trends to appear in the 1950 census as well. To examine this possibility, I repeat the

[36] It is also important to note that these figures should not capture important changes in abortion policy. Only conceptions in the first half of 1969 (pre-dating the early legalization of abortion in five states) were to be reported due to the April 1, 1970 census reference date.

analysis for women born before 1930 (women old enough to have begun childbearing before 1949) using the 1950 IPUMS. Contrary to this hypothesis, panel B provides no evidence of a pre-existing relationship in 1950 for either dependent variable. In each of the specifications, the relationship is considerably smaller and statistically insignificant.

VII. How Comstock, *Griswold* and the Pill Shaped U.S. Childbearing

The post-1960 era of U.S. history witnessed sharp reductions in period fertility rates and family size. Fifty years after *Enovid* appeared on the U.S. market, this analysis provides new evidence that the contraceptive pill accelerated fertility decline after 1960 and enabled Americans to reduce the size of their families.

To develop these conclusions, I compile a detailed legal history, which traces the evolution of U.S. anti-obscenity statutes from 1873 to 1965, and use this history to develop a novel strategy for quantifying the importance of oral contraception during the 1960s. Specifically, the empirical strategy exploits the interaction of idiosyncratic variation in statutory language (the explicit ban of contraceptive sales in some states) with the pharmaceutical nature of this contraceptive. It appears that black market provision had almost completely compensated for the reduction in availability of condoms and diaphragms, but that this was not the case for oral contraception. It is not clear whether this occurred due to the difficulty of replicating the chemical properties of the birth control pill or due to women's reluctance to ingest illicit chemicals.

Nevertheless, in three historical fertility surveys are consistent with this argument: First, I document that use of barrier methods (diaphragms and condoms) was similar in states with and without sales restrictions before the introduction of the birth control pill (1955 Growth of American Families Study). Second, I show that the birth control pill diffused significantly more slowly in states with laws explicitly banning the sales of contraceptives–even when compared to states with similar anti-obscenity statutes without this language (1965 National Fertility Study). Finally, I demonstrate that the use of oral contraceptives in states with explicit sales bans converged as states amended their statutes in the

aftermath of the *Griswold* decision (1970 National Fertility Study). This evidence not only lends credibility to this paper's empirical strategy, it also bolsters claims that laws affected the use of oral contraceptives among unmarried women in the same period (Goldin and Katz 2002, Bailey 2006).

Despite imperfect enforcement and compliance, small differences in the price of oral contraception had an appreciable impact on the birth rate. Using a newly compiled panel dataset on births, I show that sales bans are associated with growing differences in birth rates *from 1958 to 1965* but not with the evolution of differences before or after this period. The magnitude of these changes is consistent with both back-of-the-envelope calculations and use of contraceptives among married women. Finally, I demonstrate how these short-term differences in the price of oral contraception translated into important differences in family size by 1970.

One remaining question is how much the U.S. fertility rate would have declined during the 1960s in the absence of the birth control pill. Although speculative, I create a simple counterfactual based upon the back-of-the-envelope calculations in section V to address this question. Assuming that nothing changed from 1955 to 1965 except for the number of women trying to get pregnant, Q, and the number sterilized or subfecund, S, and the failure rate, f, the 1965 birth rate can be simulated using equation (2) and the 1965 NFS. The simulated marital fertility rates, $\hat{B}^m_{65} = 0.029 \times 0.85 + 0.665 \times 0.177 = 0.1425$, approximates the expected number of births in the absence of a change in the failure rate from 1955 to 1965. The observed marital fertility rate is $B^m_{65} = 0.140$ ($B^m_{55} = 0.163$). The residual, $B^m_{65} - \hat{B}^m_{65} = 0.0025$ comprises roughly 10 percent of the total change, which should be due to reductions in the failure rate due to the pill.

This is an impressive lower bound and it does not capture potential changes in number of women trying to get pregnant, an important margin of change in the 1960s. Allowing the introduction of the birth control pill to explain one percentage point (of the total 2.6 percentage point drop) in the proportion of women trying to get pregnant increases the fraction explained to approximately 50 percent. It is also possible that the introduction of the birth control pill accounts for the entire 2.6 percentage point decline

in the proportion of married women trying to get pregnant and, therefore, the entire decline in the marital fertility rate during the 1960s as suggested by Westoff (1975). Taken together, the plausibility of a range of estimates suggests a significance of the birth control pill that exceeds that acknowledged, implicitly or explicitly, in the economics literature.

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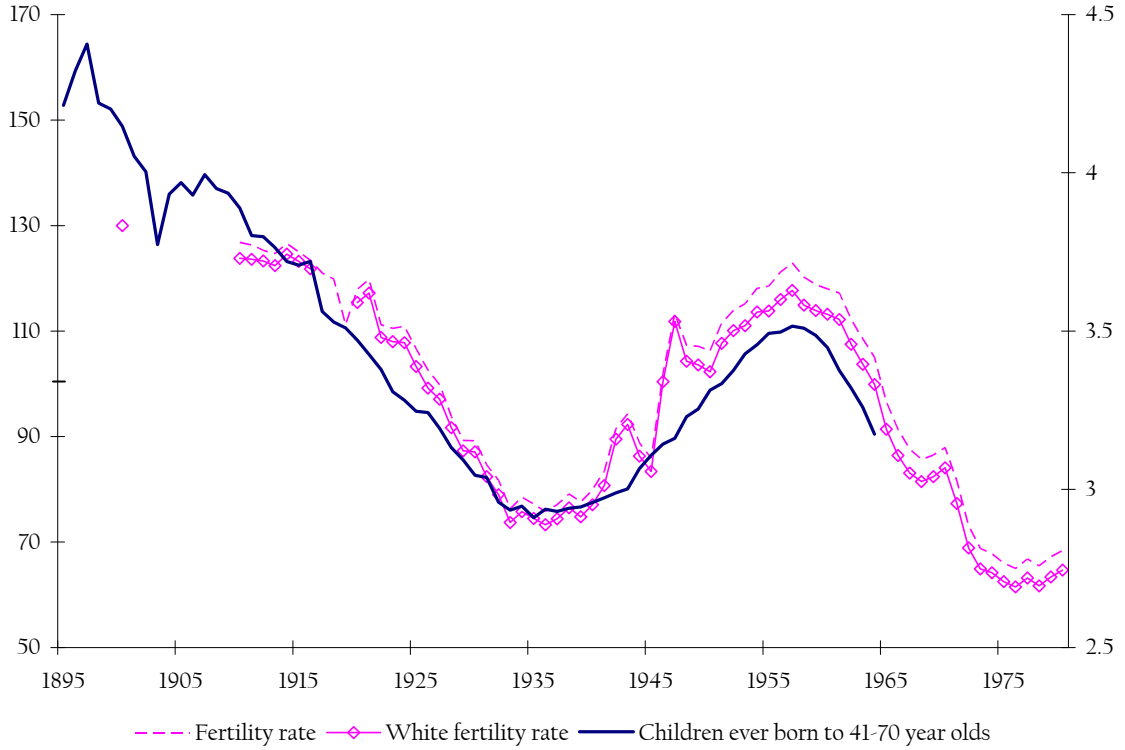
Figure I

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

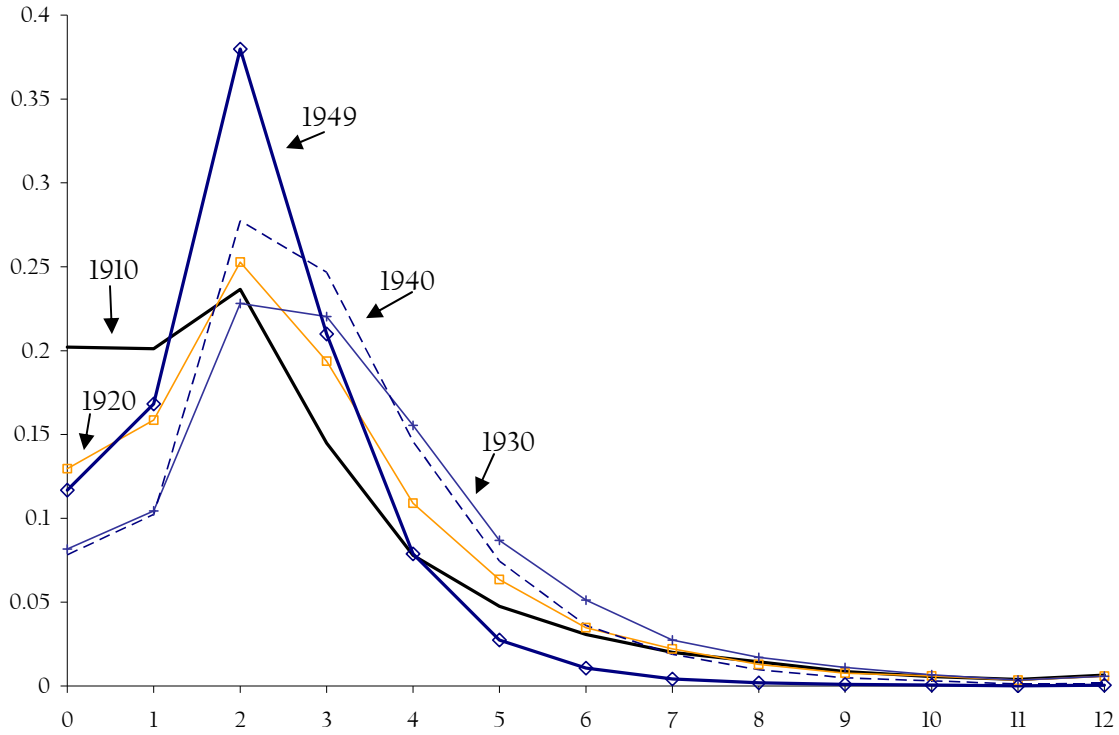


Notes: The outcome variables are the period fertility rate (and separately for white women) and the mean self-reported number of children by birth cohort. Mean children ever born excludes women who had no children. Birth cohorts are indexed to year of birth and increased by 25 years. (For instance, the birth cohort of 1870 corresponds to the year 1895.) Computations using the IPUMS use 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. 2004).

Figure II

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



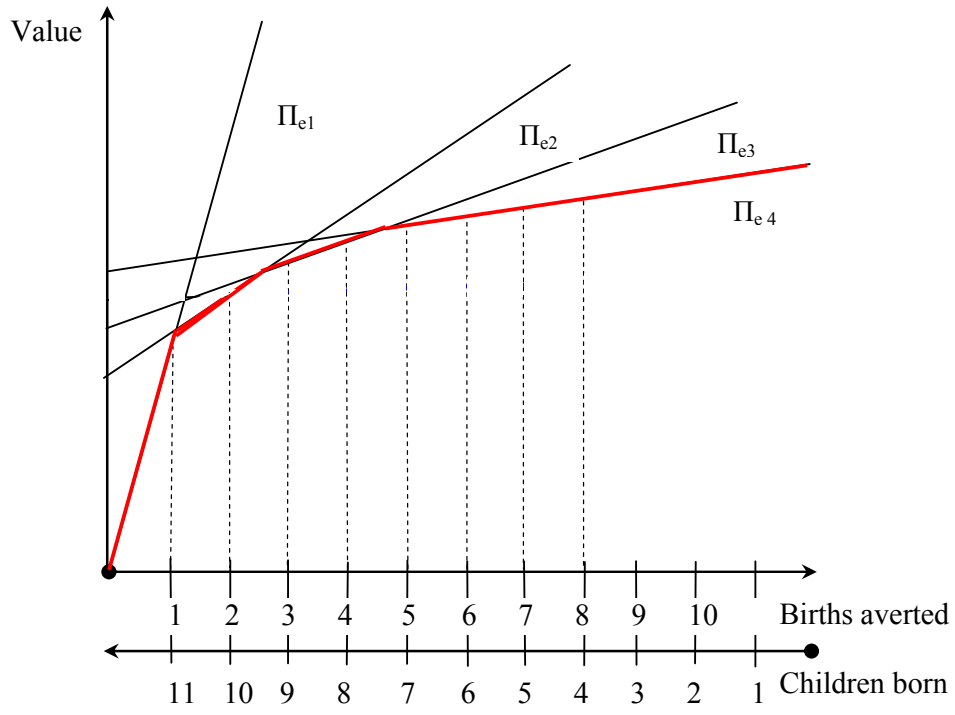
Notes: The outcome variable is the proportion of women born in a specific year who report a given number of children. This variable is created using the question of self-reported children ever-born excluding stillbirths, adopted, and step children. In 1960 this question was only asked of ever-married 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 continental United States.
Source: 1960-1990 IPUMS (Ruggles et al. 2006).

Figure III

Total and Marginal Cost of Averting Births

A. Total cost function by births averted



B. Total cost function by births averted with the birth control pill

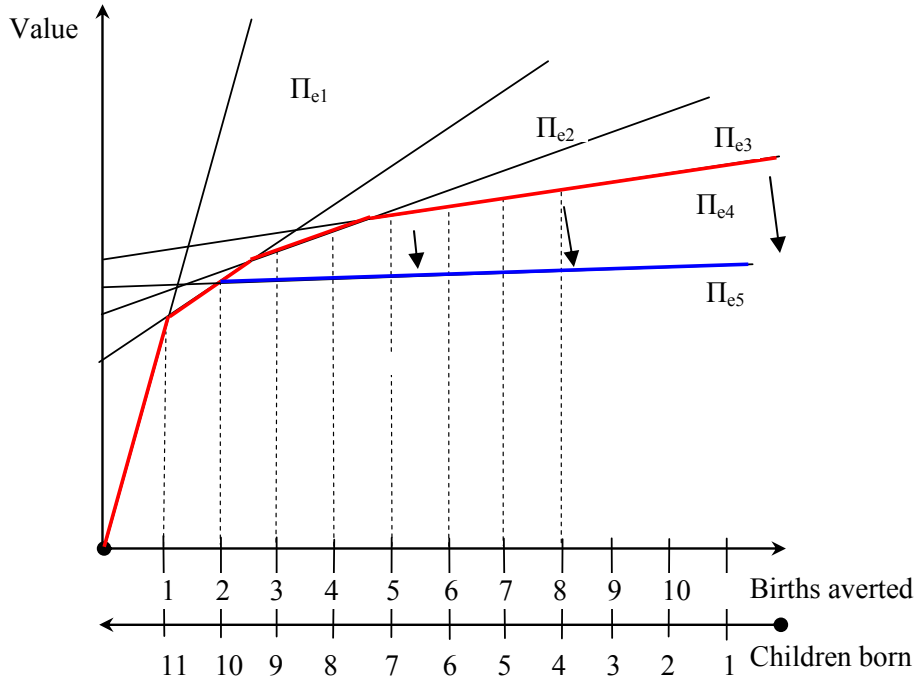
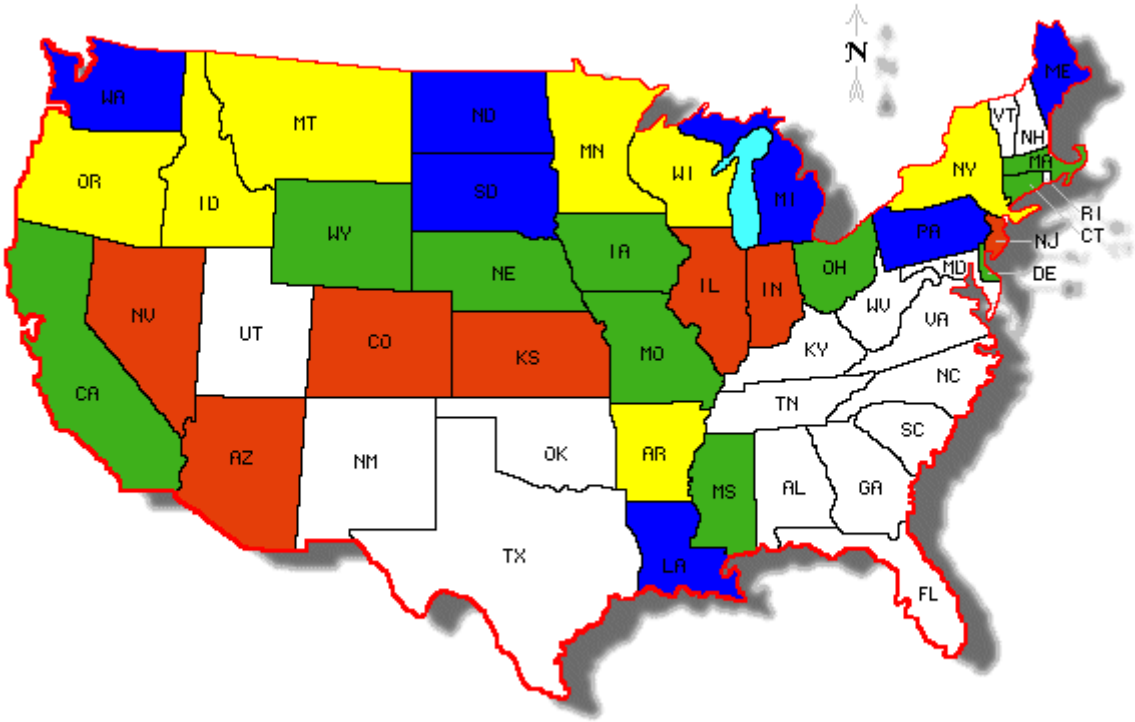


Figure IV

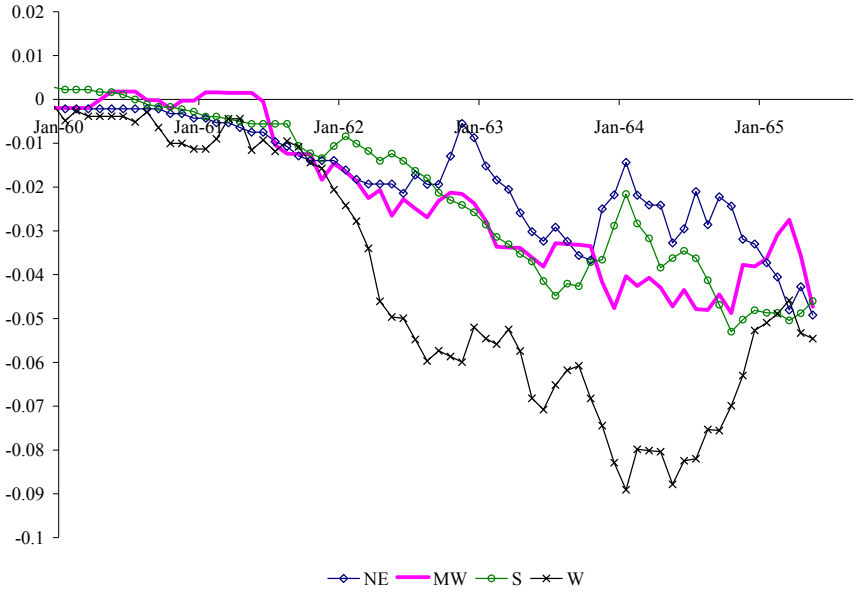
Geographic Distribution of Comstock Sales Laws by Type circa 1960



White: States with no laws or anti-obscenity laws not mentioning the "prevention of conception"
Blue: States banning only advertising or the distribution of information (Table I, column 2)
Green: States banning advertising and the sale of contraceptives (Table I, column 3)
Yellow: States with exceptions for physicians (Table I, column 4)
Orange: States amending their Comstock statutes before 1965 (Table I, column 5)

Figure V

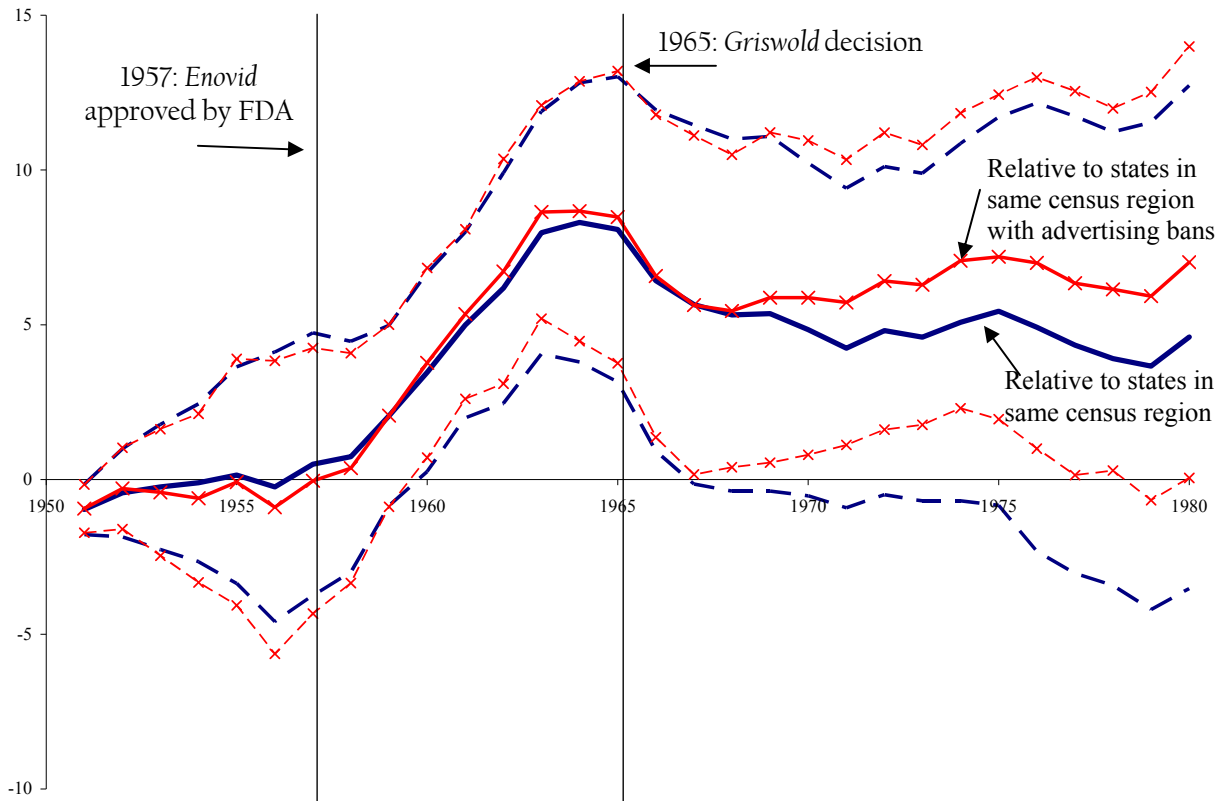
Differences in the Cumulative Proportion of Women Ever Using the Contraceptive Pill by Severity of Sales Restrictions



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 the same statistic among individuals in states without these laws by region and month from January 1960 to June 1965. States that revised their laws before 1965 are omitted. The estimates are fairly noisy (due to small sample sizes) and attention should be given to the trends rather than the monthly fluctuations. Source: 1965 National Fertility Survey

Figure VI
Relative Changes in Birth Rates in States with Sales Bans, 1951 to 1980

A. *All 48 States*



B. Excludes states liberalizing before Griswold

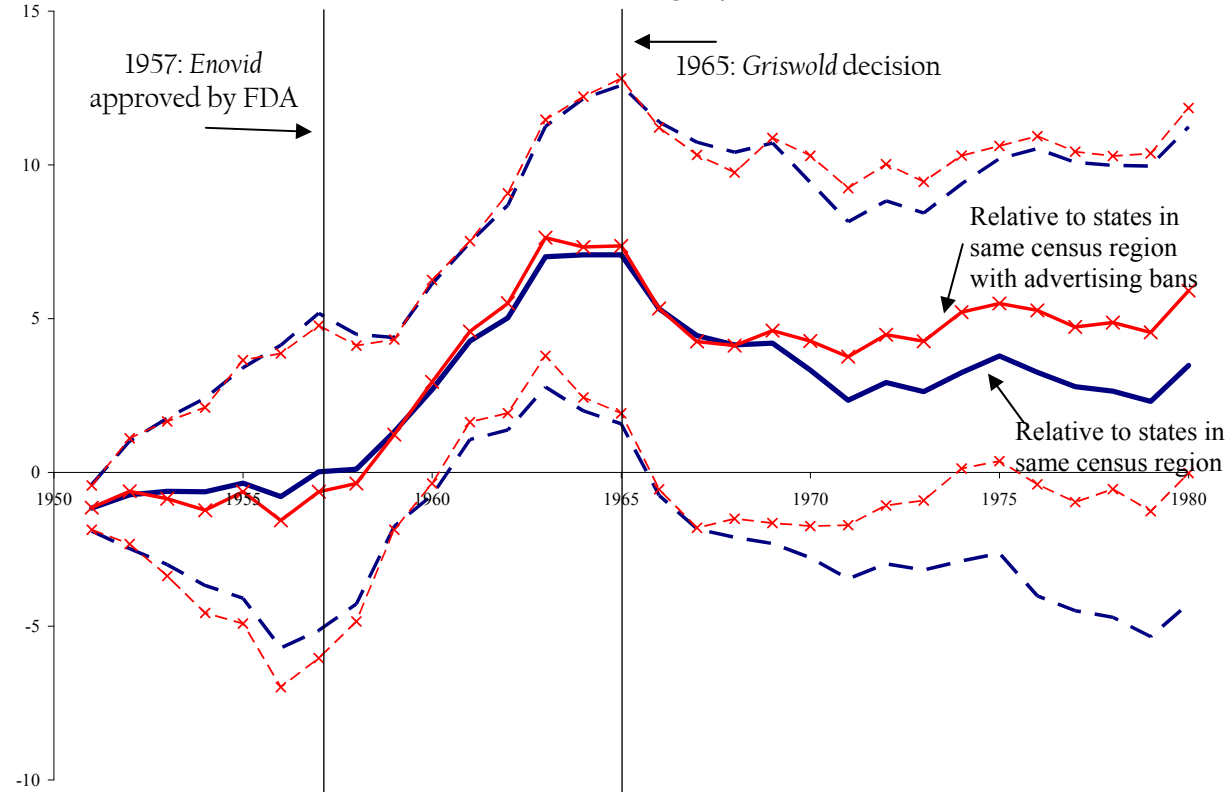
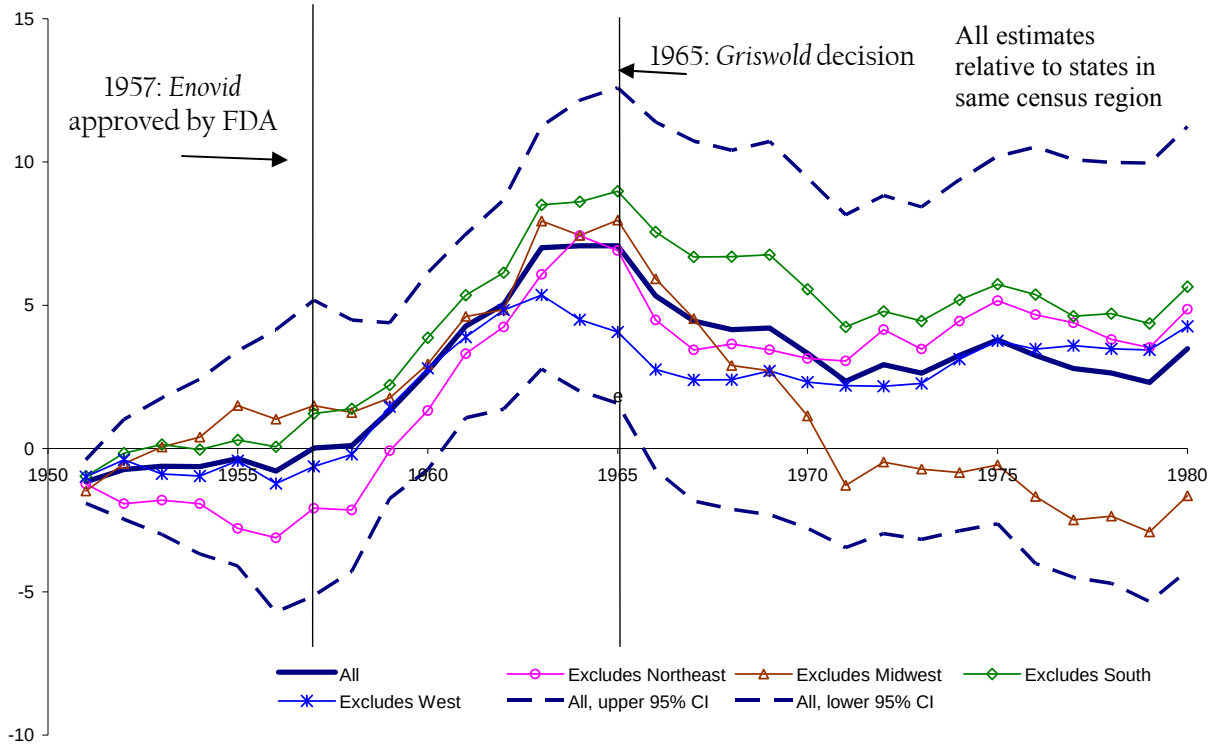


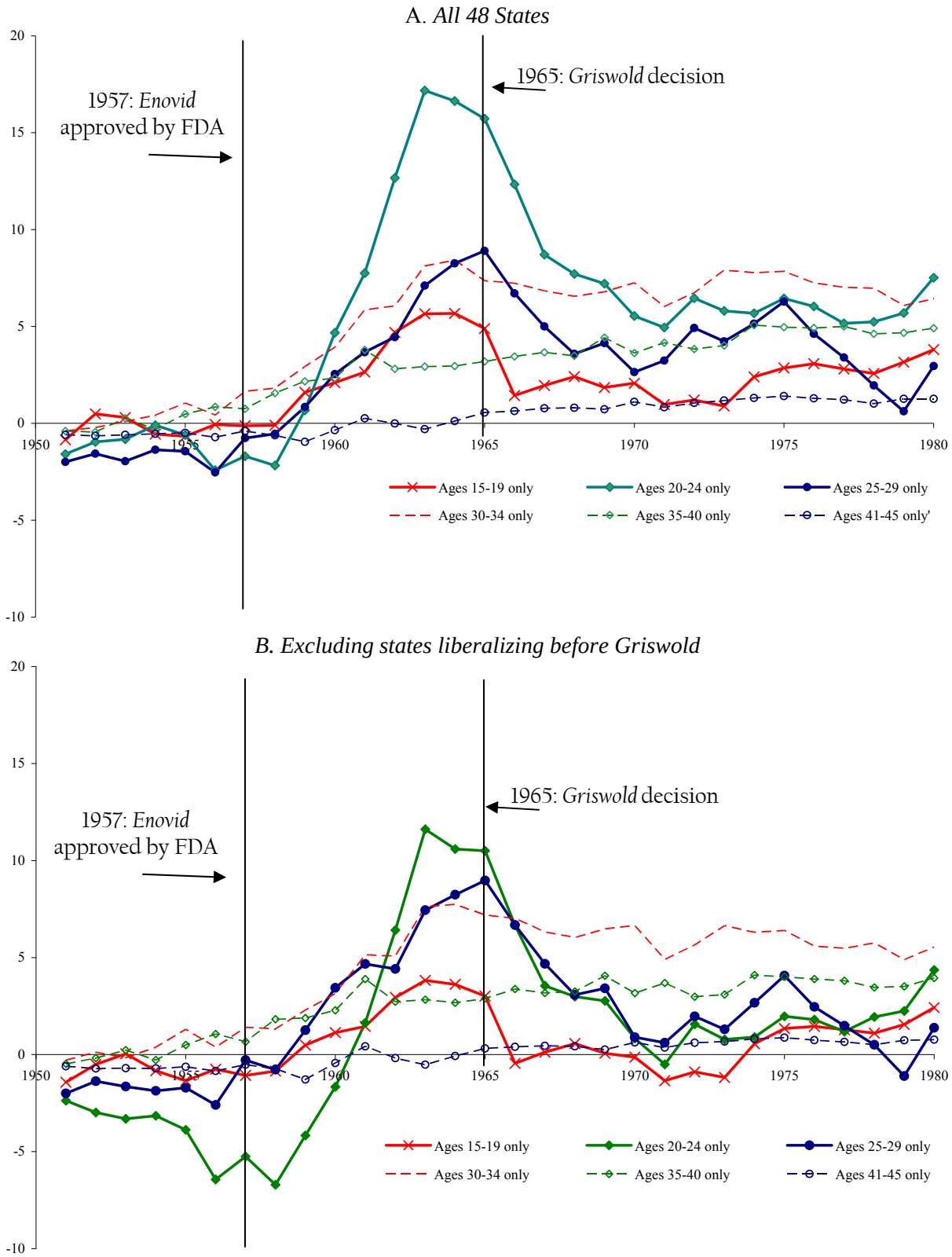
Figure VI (cont'd)
Relative Changes in Birth Rates in States with Sales Bans, 1951-1980

C. Excludes states liberalizing before Griswold



Notes: The plots capture the change in difference in birth rates in states with sales bans and a comparison group of states within the same census region, which differs by specification. The dashed lines represent a 95 percent confidence interval. Panel B excludes states that liberalized their laws prior to 1965. Panel C excludes one census region at a time. All estimates are relative to states in same census region without sales bans. The point estimates averaged across regions and the 95 percent confidence interval for these estimates is presented in bold and bold, dashed lines respectively. Source: 1950 to 1967 Vital Statistics Volumes (see Appendix B) and 1968 to 1980 Natality Files (ICPSR).

Figure VII
Relative Changes in Birth Rates in States with Sales Bans by Age Group, 1951-1980



Notes: The plots capture the change in difference in birth rates in states with sales bans and states in the same census region without sales bans. Each line represents the estimates from a separate regression including only the age group indicated in the table legend. Panel B excludes states that liberalized their laws prior to 1965. Source: 1950 to 1967 Vital Statistics Volumes (see Appendix B) and 1968 to 1980 Natality Files (ICPSR).

Table 1

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

	(1) Date obscenity statute enacted	(2) Publishing or advertising contraceptive information	(3) Distribution or sale of drugs, instruments or articles	(4) Sales allowed by doctors or pharmacists only	(5) Repealed/ amended/ overruled before 1965
Alabama	1884				
Arizona	1887	X	X		1962 (judicial decision)
Arkansas	1943	X	X	X (1943)	
California	1873	X	X		
Colorado	1885	X	X		1961
Connecticut	1879	X	X		
Delaware	1935	X	X		
Florida	1868				
Georgia	1878				
Idaho	1887	X	X	X (1937)	
Illinois	1845	X	X		1961
Indiana	1896	X	X		1961
Iowa	1897	X	X		
Kansas	1886	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(1894)	
Mississippi	1892	X	X		
Missouri	1879	X	X		
Montana	1935	X	X	X (1935)	
Nebraska	1885	X	X		
Nevada	1877	X	X	X(1877)	1963
New Hampshire	1891				
New Jersey	1898	X	X		1963 (judicial decision)
New Mexico	---				
New York	1868	X	X	X (1873)	
North Carolina	1885				
North Dakota	1895	X			
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	<1919	X			
Tennessee	1884				
Texas	1897				
Utah	1898				
Vermont	<1880				
Virginia	1849				
Washington	1909	X			
West Virginia	1882				
Wisconsin	1849	X	X	X(1931)	
Wyoming	1890	X	X		
TOTAL	47	31	24	8	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 a statute was found on the books. Dates in parenthesis indicate date the original law was revised to include the particular provision. Detailed notes on coding decisions can be found in Appendix A.

Table IIA
Differences in Attitudes about Family Planning by Legal Regime, 1955

	(1)	(2)	(3)	(4)
	A. With Region Comparisons			
	DV: 1=Approve of Limiting Family Size			
Sales ban	-0.030	-0.012	0.020	0.002
	[0.042]	[0.044]	[0.033]	[0.032]
Physician exception		-0.073	-0.02	-0.026
		[0.057]	[0.079]	[0.081]
Advertising/publishing				0.130
				[0.061]
Constant	0.753	0.753	0.617	0.503
	[0.028]	[0.028]	[0.033]	[0.060]
Additional covariates*			R	R
Observations	2713	2713	2713	2713
R-squared	0.01	0.01	0.02	0.03
	B. Adjustments for observable characteristics			
	DV: 1=Approve of Limiting Family Size			
Distribution/sales law	-0.011	-0.011	-0.016	0.002
	[0.028]	[0.029]	[0.028]	[0.024]
Physician exception	0.020	0.024	0.027	0.036
	[0.051]	[0.055]	[0.055]	[0.049]
Advertising/publishing	0.071	0.071	0.066	0.014
	[0.042]	[0.043]	[0.043]	[0.051]
Constant	0.815	0.655	0.387	0.542
	[0.051]	[0.058]	[0.441]	[0.472]
Additional covariates*	RAPC	RACPE	RACPEI	RACPEIF
Observations	2666	2542	2542	1848
R-squared	0.19	0.2	0.2	0.24
	C. Exclude each census region			
	DV: 1=Approve of Limiting Family Size			
Distribution/sales law	-0.011	0.005	0.004	-0.001
	[0.030]	[0.030]	[0.033]	[0.038]
Physician exception	-0.172	0.072	-0.022	-0.031
	[0.095]	[0.035]*	[0.087]	[0.091]
Advertising/publishing	0.208	0.082	0.272	0.091
	[0.065]	[0.064]	[0.031]	[0.060]
Constant	0.613	0.513	0.358	0.546
	[0.066]	[0.066]	[0.043]	[0.053]
Additional covariates*	R	R	R	R
Census region excluded*	Northeast	Midwest	South	West
Observations	2044	1860	1972	2263
R-squared	0.02	0.04	0.03	0.02

Table notes appear after Table IIB on the next page.

Table IIB
Differences in Contraceptive Use by Legal Regime, 1955

	(1)	(2)	(3)	(4)
	A. With Region Comparisons			
	DV: 1=Ever used barrier methods			
Sales ban	-0.036	-0.014	0.028	0.010
	[0.055]	[0.060]	[0.039]	[0.037]
Physician exception		-0.091	-0.035	-0.041
		[0.063]	[0.068]	[0.071]
Advertising/publishing				0.126
				[0.100]
Constant	0.513	0.513	0.366	0.255
	[0.036]	[0.036]	[0.035]	[0.098]
Additional covariates*			R	R
Observations	2713	2713	2713	2713
R-squared	0.01	0.01	0.03	0.03
	B. Adjustments for observable characteristics			
	DV: 1=Ever used barrier methods			
Distribution/sales law	-0.010	-0.020	-0.026	-0.028
	[0.030]	[0.030]	[0.029]	[0.037]
Physician exception	0.011	0.023	0.025	0.01
	[0.036]	[0.040]	[0.042]	[0.047]
Advertising/publishing	0.075	0.062	0.056	0.085
	[0.072]	[0.072]	[0.070]	[0.069]
Constant	0.574	0.364	0.427	0.422
	[0.105]	[0.093]	[0.239]	[0.267]
Additional covariates*	RAPC	RACPE	RACPEI	RACPEIF
Observations	2666	2542	2542	1848
R-squared	0.16	0.18	0.19	0.23
	C. Exclude each census region			
	DV: 1=Ever used barrier methods			
Distribution/sales law	-0.014	0.034	0.010	0.003
	[0.038]	[0.022]	[0.037]	[0.042]
Physician exception	-0.193	0.036	-0.033	-0.027
	[0.062]	[0.034]	[0.075]	[0.078]
Advertising/publishing	0.216	0.070	0.435	0.028
	[0.092]	[0.103]	[0.035]	[0.063]
Constant	0.417	0.266	-0.057	0.353
	[0.097]	[0.104]	[0.035]	[0.055]
Additional covariates*	R	R	R	R
Census region excluded*	Northeast	Midwest	South	West
Observations	1848	2044	1860	1972
R-squared	0.23	0.03	0.04	0.04

Notes: Each column presents the results from a linear probability model. *Panel A includes fixed effects for census region denoted R. Panel B runs the specification from Panel A column 4 and adds dummy variables for age, A; population of the primary sampling unit, P; 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); husband's 1965 income, I (0, <2000, 2-2.9k,3-3.9k, …10-11.9k, 12-14.9k, >15k); and the respondent's "ideal number of children for herself," F (0, 1, …,15). The omitted category includes 15 year old white women with less than 9 years of education whose husbands report no earnings and reside in the Northeast. In table IIB panel B, barrier methods include condoms and diaphragms. Panel C omits one census region at a time from specification in column 4 of panel A. Standard errors are clustered at the state-level. Source: 1955 GAF.

Table III

The Relationship of Comstock Laws and Pill Use

	(1)	(2)	(3)	(4)
	A. Within Census Region Comparisons			
	DV: 1=Ever Used the Pill before June 1965			
Sales ban	-0.006	-0.007	-0.050	-0.050
	[0.019]	[0.019]	[0.016]	[0.020]
Physician exception		0.003	0.035	0.035
		[0.040]	[0.035]	[0.036]
Advertising/publishing				0.001
				[0.024]
Constant	0.224	0.224	0.193	0.192
	[0.011]	[0.011]	[0.022]	[0.024]
Additional covariates*			R	R
Observations	4507	4507	4507	4507
R-squared	0.01	0.01	0.01	0.01
	B. Adjustments for observable characteristics			
	DV: 1=Ever Used the Pill before June 1965			
Distribution/sales law	-0.058	-0.060	-0.061	-0.061
	[0.018]	[0.018]	[0.017]	[0.018]
Physician exception	0.057	0.054	0.054	0.056
	[0.033]	[0.032]	[0.032]	[0.032]
Advertising/publishing	0.015	0.013	0.014	0.015
	[0.022]	[0.022]	[0.022]	[0.022]
Constant	0.255	0.186	0.126	0.329
	[0.043]	[0.045]	[0.171]	[0.202]
Additional covariates*	RACPN	RACPNE	RACPNEI	RACPNEIF
Observations	4507	4506	4506	4464
R-squared	0.10	0.10	0.10	0.11
	C. Exclude each census region			
	DV: 1=Ever Used the Pill before June 1965			
Distribution/sales law	-0.075	-0.058	-0.056	-0.062
	[0.017]	[0.028]	[0.018]	[0.020]
Physician exception	0.035	0.089	0.057	0.046
	[0.054]	[0.039]	[0.034]	[0.029]
Advertising/publishing	0.032	0.007	-0.001	0.030
	[0.020]	[0.027]	[0.037]	[0.025]
Constant	0.531	0.389	0.047	0.255
	[0.213]	[0.215]	[0.119]	[0.201]
Additional covariates*	RACPNEIF	RACPNEIF	RACPNEIF	RACPNEIF
Census region excluded*	Northeast	Midwest	South	West
Observations	3478	3228	2850	3836
R-squared	0.12	0.11	0.13	0.11

Notes: Each column presents the results from a linear probability model. *See notes for table II for definitions of the covariates. For the 1965 NFS, covariates additionally include a dummy for nonwhite, N, to account for the inclusion of nonwhite women in the survey. Standard errors are clustered at the state-level. Source: 1965 NFS.

Table IV

The Relationship of Sales Bans to Use of Other Contraceptives and Oral Contraception in 1970

	(1)	(2)	(3)	(4)
	A. DV: 1=Ever Used Any Method before June 1965			
Sales ban	-0.005	0.022	0.035	0.029
	[0.016]	[0.028]	[0.024]	[0.023]
Physician exception	-0.004	-0.008	0.002	-0.004
	[0.017]	[0.018]	[0.018]	[0.016]
Advertising/publishing		-0.079	-0.088	-0.10
		[0.062]	[0.053]	[0.052]
Constant	0.888	0.944	1.037	0.998
	[0.012]	[0.048]	[0.051]	[0.197]
Additional covariates*	R	R	RACNP	RACNPEIF
Observations	4507	4507	4507	4464
R-squared	0.01	0.01	0.05	0.09
	B. DV: 1=Ever Used Barrier Method before June 1965			
Distribution/sales law	-0.021	0.017	0.035	0.027
	[0.034]	[0.041]	[0.027]	[0.024]
Physician exception	0.076	0.071	0.051	0.044
	[0.040]	[0.041]	[0.030]	[0.026]
Advertising/publishing		-0.109	-0.109	-0.129
		[0.089]	[0.049]	[0.045]
Constant	0.464	0.542	0.529	0.288
	[0.029]	[0.081]	[0.066]	[0.144]
Additional covariates*	R	R	RACNP	RACNPEIF
Observations	4507	4507	4507	4464
R-squared	0.01	0.01	0.12	0.15
	C. DV: 1=Ever Used the Pill before 1970			
Distribution/sales law	-0.005	0.015	0.011	0.011
	[0.022]	[0.023]	[0.021]	[0.022]
Physician exception	0.017	0.015	0.001	0.001
	[0.024]	[0.024]	[0.021]	[0.021]
Advertising/publishing		-0.042	-0.042	-0.048
		[0.034]	[0.031]	[0.032]
Constant	1.481	1.507	1.485	1.427
	[0.020]	[0.033]	[0.052]	[0.082]
Additional covariates*	R	R	RACNP	RACNPEIF
Observations	5855	5855	5844	5829
R-squared	0.01	0.01	0.17	0.17

Notes: Each column presents the results from a linear probability model. *See notes for table II for precise definitions of the covariates. For the 1965 and 1970 NFS, covariates additionally include a dummy for nonwhite, N, to account for the inclusion of nonwhite women in the survey. In panel B, barrier methods include condoms and diaphragms. Panel C limits the 1970 NFS to currently married women to be comparable with the 1965 NFS. However, the results are similar when post-married women are included. Standard errors are clustered at the state-level. Source: Panel A and B: 1965 NFS, Panel B: 1970 NFS.

Table V

Sales Bans and Differences in Completed Fertility, 1950 and 1970

	A. 1970 Census					
	DV: Mean children ever born			*DV: Proportion with fewer than three children*		
	(1)	(2)	(3)	(1)	(2)	(3)
Sales ban	-0.200	-0.222	-0.209	0.045	0.050	0.049
	[0.083]	[0.066]	[0.079]	[0.016]	[0.014]	[0.018]
Sales ban x D(1920≤yob≤1924)	0.082	0.075	0.090	-0.01	-0.011	-0.016
	[0.027]	[0.029]	[0.028]	[0.006]	[0.006]	[0.006]
Sales ban x D(1925≤yob≤1929)	0.097	0.051	0.073	-0.018	-0.012	-0.021
	[0.045]	[0.043]	[0.046]	[0.008]	[0.010]	[0.009]
Sales ban x D(1930≤yob≤1934)	0.126	0.075	0.097	-0.020	-0.015	-0.019
	[0.044]	[0.030]	[0.034]	[0.008]	[0.008]	[0.008]
Sales ban x D(1935≤yob≤1939)	0.142	0.100	0.098	-0.023	-0.017	-0.020
	[0.039]	[0.027]	[0.029]	[0.009]	[0.009]	[0.010]
Sales ban x D(1940≤yob≤1944)	0.144	0.123	0.129	-0.023	-0.018	-0.022
	[0.040]	[0.036]	[0.044]	[0.009]	[0.011]	[0.010]
Sales ban x D(1945≤yob≤1949)	0.185	0.179	0.171	-0.048	-0.047	-0.047
	[0.060]	[0.059]	[0.073]	[0.014]	[0.015]	[0.019]
Constant	2.241	2.190	2.188	0.627	0.638	0.64
	[0.045]	[0.116]	[0.115]	[0.009]	[0.020]	[0.020]
Additional covariates	X*	X*	X*	X*	X*	X*
Excludes states liberalizing before Griswold			X			X
Observations	343	343	294	343	343	294
R-squared	0.97	0.97	0.97	0.97	0.98	0.97
	B. 1950 Census					
	DV: Mean children ever born			*DV: Proportion with fewer than three children*		
	(1)	(2)	(3)	(1)	(2)	(3)
Sales ban	-0.01	-0.05	-0.024	0.001	-0.002	-0.005
	[0.068]	[0.046]	[0.054]	[0.007]	[0.008]	[0.009]
Sales ban x D(1920≤yob≤1924)	-0.088	-0.028	-0.055	0.015	0.011	0.009
	[0.064]	[0.059]	[0.061]	[0.008]	[0.009]	[0.009]
Sales ban x D(1925≤yob≤1929)	-0.039	-0.001	-0.041	-0.009	-0.014	-0.012
	[0.055]	[0.051]	[0.051]	[0.009]	[0.008]	[0.010]
Constant	1.478	1.398	1.403	0.213	0.208	0.207
	[0.049]	[0.088]	[0.085]	[0.004]	[0.008]	[0.007]
Additional covariates	X*	X*	X*	X*	X*	X*
Excludes states liberalizing before Griswold			X			X
Observations	147	147	126	147	147	126
R-squared	0.90	0.90	0.90	0.48	0.51	0.49

Notes: Each column presents the estimates obtained using the specification described in equation 5. * Additional covariates include dummy variables for Exception (all columns) and Advertising (column 2 and 3 only), dummy variables for census region and the interaction of these variables with a set of dummy variables for five-year birth cohorts (1920-1924, …, 1945-1949; 1915-1919 is omitted). Standard errors are clustered at the state-level and displayed in brackets. Source: 1950 and 1970 IPUMS (Ruggles et al. 2006).