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Essays in Applied Microeconomics

A dissertation submitted in partial satisfaction

of the requirements for the degree

Doctor of Philosophy in Economics

by

Juan Pantano

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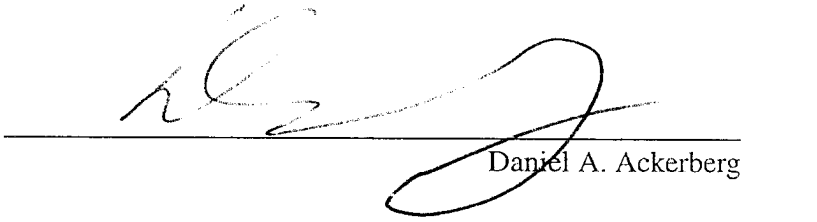
The dissertation of Juan Pantano is approved.



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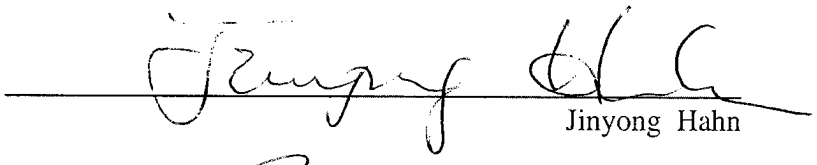

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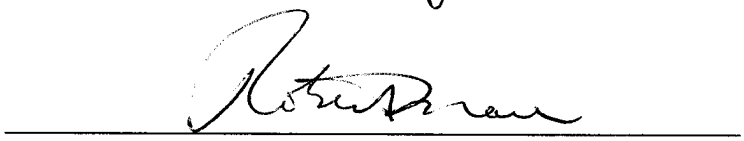

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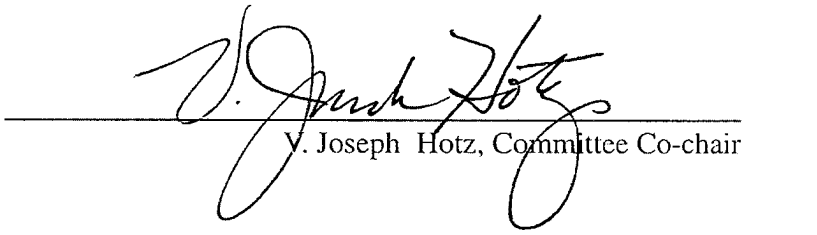

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To my Parents

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ABSTRACT OF THE DISSERTATION

Essays in Applied Microeconomics

by

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Doctor of Philosophy in Economics

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This dissertation contains three essays that apply techniques in applied microeconomics to solve scientific puzzles and questions closely related to practical policy issues. The first essay explores the impact of early access to the birth control pill on the future crime rates of the children who are born to mothers who take advantage of this unprecedented improvement in contraceptive technology. The second essay investigates whether changing parenting strategies associated with parental reputation dynamics generate birth order effects in school performance. The last essay develops and estimates a dynamic model of human capital accumulation and criminal behavior. The estimated model is used to evaluate alternative criminal records policies and to shed light on the causal relationship between education and crime

CHAPTER 1

Unwanted Fertility, Contraceptive Technology and Crime: Exploiting a Natural Experiment in Access to the Pill

Donohue and Levitt (2001) claim to explain a substantial part of the recent decline in U.S. crime rates with the legalization of abortion undertaken in the early '70s. While the validity of these findings remains heavily debated, they point to unwanted fertility as a potentially important determinant of a cohort's criminality. In that spirit, I exploit a natural experiment induced by policy changes during the '60s and '70s. After the introduction of the contraceptive pill in 1960, single women below the age of majority faced restricted access to this new contraceptive method. Mostly as a by-product of unrelated policy changes, these access restrictions were lifted differentially across states during the '60s and '70s. This differential timing of contraceptive liberalization induces exogenous variation that can be used to identify the causal effect of unwanted fertility on crime. Results are consistent with the arguments of Donohue & Levitt. They indicate that greater flexibility to avoid unwanted pregnancies (through better contraceptive technology) reduces crime about two decades later, when undesired children would have reached their criminal prime.

1.1 Introduction

A blossoming literature in the U.S. examines the role of abortion legalization on the criminality of the cohorts born before and after this controversial law change. In the same spirit, I propose to exploit an alternative natural experiment induced by policy changes during the '60s and '70s during the "Contraceptive Revolution". In particular, after the introduction of the contraceptive pill in 1960, different states maintained some form of required parental consent to obtain a doctor's prescription for women below the age of majority. For a particular group of single women in their late teens, these restrictions were lifted differentially across states during the '60s and '70s. This differential timing of contraceptive liberalization induces exogenous variation that can be used to explore the causal link between unwanted fertility and crime. Greater flexibility to avoid unwanted pregnancies is likely to reduce crime two decades down the road, when undesired children born to these women would have reached their maximum criminal potential. In this hypothesis, "wantedness" is conceptualized as an overall indicator of willingness to invest resources in the future child. Rather than joining the already substantial literature in the abortion-crime debate, the contribution here explores the consequences of a set of completely unrelated policy changes which also induce exogenous variation in prevalence of unwantedness for a given birth cohort.

In addition to its scientific value as a potential determinant of a given birth cohort's criminality, understanding the causal link between unwanted fertility and criminality is relevant to policy makers. Potentially higher levels of criminality induced by more unwanted children is a cost that, in principle, should be taken into account when evaluating policies that restrict contraceptive freedom, or more generally, policies that limit women's ability to avoid unwanted children. In 2005-2006 there has been substantial policy debate over the apparent reluctance by the Federal Drug Administration to allow a new contraceptive device, the "day after" pill (Plan B) to be sold over the counter.

While most of the current debate centers on short run fears of increased teen promiscuity and the spread of sexually transmitted diseases, it is important to keep in mind the long run effects of a given contraceptive policy change.

The rest of the chapter is organized as follows. The next section provides some brief background on the institutional and legal history of the pill. Section 1.3 discusses related literature, causal mechanisms and necessary conditions for pill access to have a negative effect on future crime. Section 1.4 describes the data and Section 1.5 presents the basic empirical strategy, results and tests of the maintained hypothesis. A counterfactual policy extrapolation is conducted in Section 1.6. Conclusions follow.

1.2 Institutional Background

Here I provide a brief overview of the institutional and legal history associated with access to the pill.[1] The pill was introduced in the market in 1960 and quickly diffused among American women, becoming one of their preferred methods of contraception. However, underneath this "Contraceptive Revolution", the adoption of the pill as a contraceptive device by younger women faced a number of state-level legal obstacles. In particular, the pill was only available by prescription, and women below the age of majority required parental consent to receive medical services. During the '60s and '70s, different states liberalized their laws governing access to contraception for young women. This process was accomplished by state legislation that reduced the age of majority and granted mature minors capacity to consent to medical care. In some other states this liberalization took the form of judicial mature "minor" rulings or special family planning legislation. As shown in Table 1.1, the timing of this contraceptive liberalization was different for most states, spanning the period from 1960 to 1977.

[1] For more details see Goldin & Katz (2000, 2002), Hock (2005) and Bailey (2006)

Table 1.1: Access to Contraception Among Single Women in Late Adolescence 1960-1977

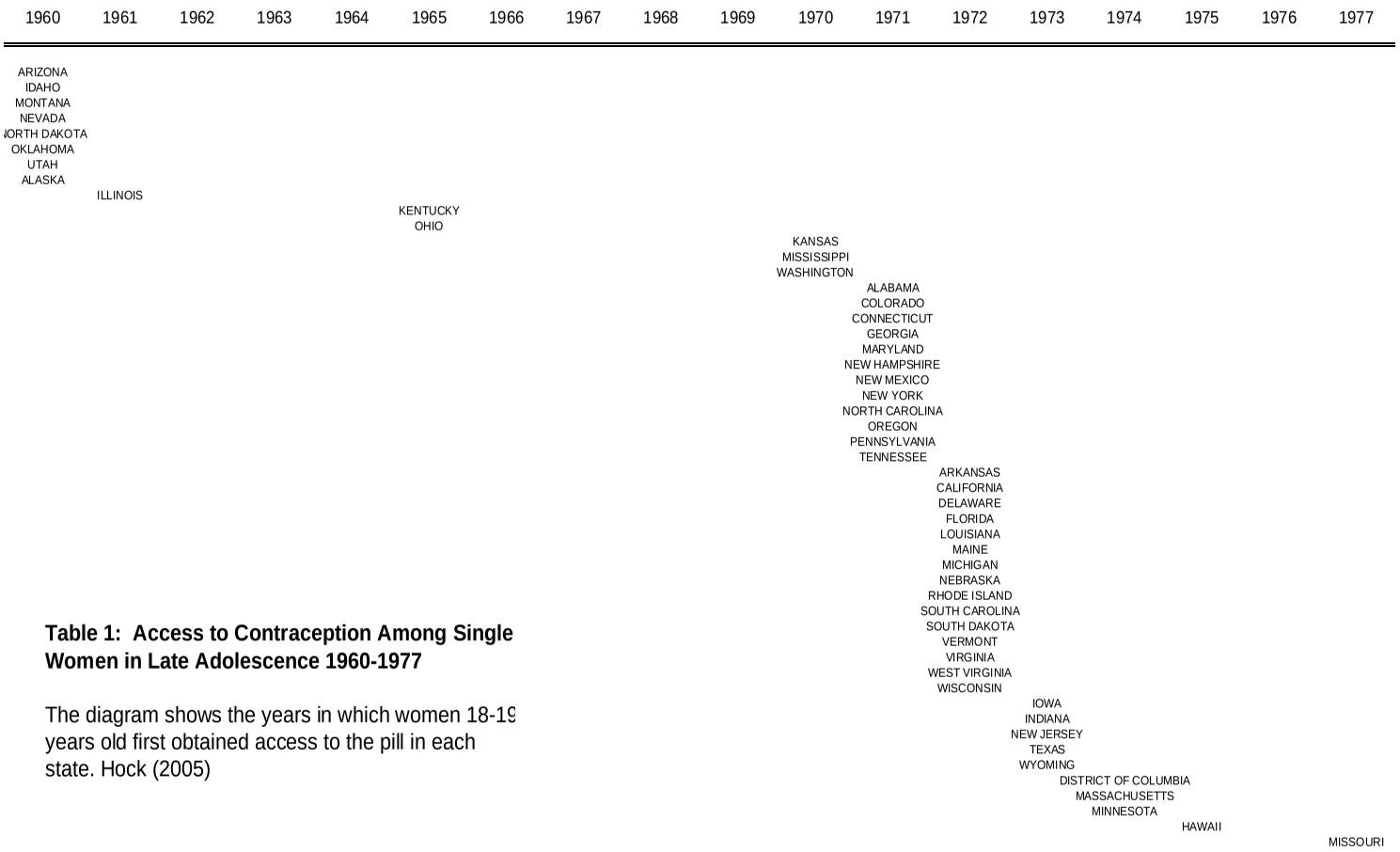

Table 1: Access to Contraception Among Single Women in Late Adolescence 1960-1977

The diagram shows the years in which women 18-19 years old first obtained access to the pill in each state. Hock (2005)

This latter fact induces plausibly exogenous cross-state variation over time that allows me to identify the causal effect of unwanted fertility on crime, in the same spirit of the abortion legalization arguments of Donohue & Levitt (2001). Moreover, note that young women being granted more unrestricted access to this effective contraception technology was by large a by-product of more general legislation drafted to address other unrelated policy concerns. Therefore, the usual threat of policy endogeneity does not appear to be particularly problematic in this context. Bailey (2006) makes a convincing case for the lack of policy endogeneity in the legislative and judicial process that leads, as unintended by-product, to contraceptive liberalization for unmarried teen women. Moreover, federal legislation prohibited individuals from obtaining oral contraceptives by mail shipped from other states. This greatly enhances the reliability of the proposed quasi-experimental design.

1.3 Related Literature

The idea that the levels of criminality of a given cohort can be traced back to how desired or "wanted" were births in that cohort has been around since the seminal contribution by Donohue & Levitt (2001) which exploited abortion legalization as a natural experiment to quantify this effect. In their initial article, Donohue & Levitt claimed that abortion legalization may account for as much as 50 % of the recent decline in crime rates in the U.S.

The pioneering work of Donohue & Levitt was followed by some critiques. In particular, Joyce (2004) casts doubts over the validity of these findings claiming that the authors failed to account for unobserved factors that might vary both across state and over time like the crack cocaine epidemic. A rejoinder by Donohue & Levitt (2004) argued that, if anything, failure to account for the crack epidemic biased the results against and not in favor of their 2001 findings. Other recent challenges to

the findings of Donohue & Levitt (2001) include Foote & Goetze (2005), Sykes et al (2006) and Lott & Whitley (2006). A rejoinder by Donohue & Levitt (2006) and a more comprehensive methodological overview of the subject by Ananat et al (2006) address some of these recent challenges and, to some extent, confirm the provocative magnitudes of the 2001 article, although as in Foote & Goetze (2005), this recent work emphasize the fact that most of the effect is coming simply from declines cohort size as opposed to selection into the cohort.

While much has been written about the so-called "Contraceptive Revolution", the exogenous variation in the number of unwanted children induced by policy changes governing teen access to the pill has not been used to investigate the causal relationship between unwanted fertility and crime. The quasi-experimental variation induced by the differential timing of the contraceptive liberalization in different states has been exploited by some researchers to address other questions. In seminal work, Goldin & Katz (2000, 2002) exploited this variation to analyze the career and marriage decisions of women in the '60s and '70s, a period that witnessed substantial change in those dimensions. More recently, Hock (2005) and Bailey (2006) also exploited the variation available in state laws regarding access to the contraceptive pill. Hock (2005) concluded that by lowering the incidence of early fertility, unconstrained access to the pill increased the enrollment rate of college age women by almost 5 percentage points, and it had a less sizable but still positive and significant impact on college completion rates. Bailey (2006) found significant effects of the pill in women's child bearing timing and life cycle labor supply. In other recent contributions, Guldi (2005) examines the relative impacts of the pill and abortion on the fertility patterns of young women and Ananat & Hungerman (2006) explore how the pill changed the characteristics of the average mother.

Finally, the use of quasi-experimental variation in laws governing access to the pill

for teen women is specially relevant in my context as there exists prolific literature relating teenage and out-of-wedlock fertility to the levels of criminality of the teenage and/or unmarried mother's offspring. For example, Grogger (1997) shows that young men who were born to young teen mothers are 3.5 percentage points more likely to be incarcerated than sons of older mothers. Hunt (2006) uses international victimization data to investigate the effects between teen fertility and crime and concludes that the high rates of teen births in the U.S. have prevented further declines in some types of crimes relative to other countries. Not surprisingly, criminologists have also looked into this question. Nagin, Farrington & Pogarsky (1997) use the Cambridge Study in Delinquent Development to examine alternative mechanisms or "accounts" through which teen fertility of the mother may have a significant effect in the delinquency levels of the children. They consider life course-immaturity, persistent poor parenting and diminished resources as alternative channels, finding some support for the latter two. More recently, Kendall & Tamura (2006) adopt a more historical, long run perspective to look at the effects of unmarried fertility on crime

1.3.1 Causal Mechanisms

Note that unwanted fertility is not likely to have a direct causal effect on crime. Rather, unwanted fertility will manifest itself as a cumulative process of disadvantage, starting right at the instant of conception. Those cumulated disadvantages are the ones that end up increasing criminal tendencies. While this chapter will not be focusing on disentangling these alternative contributing mechanisms, it is worth mentioning some of them. For example, the early harmful effects of being an unwanted child are likely to be channeled through inadequate prenatal care and child abuse and neglect.[2]. The impact

[2]For the impact of child abuse and neglect on future crime see Currie & Tekin (2006). For the relationship between unwanted fertility and inadequate prenatal care see Joyce & Grossman (1990)

of these initial disadvantages as well as the consequences of further underinvestments are likely to be experienced during childhood and early adolescence, therefore increasing the risk of delinquency onset. Note also that unwantedness might cause maternal risky behaviors during pregnancies. These behaviors are likely to lead to negative birth and infant health outcomes. Poor child health and low socio-emotional development are likely disadvantages to affect unwanted children.[3] Moreover, unintended children may, if born, stall maternal human capital accumulation by both, reducing the mother's formal educational attainment[4] and lowering her life-cycle labor force participation[5]. Unwantedness might lead not only to high incidence of child abuse and neglect but also reduce the levels of parental monitoring, control and supervision. This will certainly propel children's potentially deviant behavior. It could also be the case that unwanted children receive lower parental support (both in terms of time and money) for school. This is important because it is likely that lower education might itself lead to higher criminality.[6]

The impact of the pill might operate through channels other than the selection mechanisms discussed above. Indeed, the pill might reduce the criminality of *wanted* siblings through a "family size" effect. There exist evidence that the pill had an impact on completed fertility. Averted children were not compensated for at later stages of women's reproductive cycle. Therefore siblings of the these (unborn) unwanted children might benefit from a more abundant set of parental resources and also reduce their crime rates. Moreover, extending this argument to society at large, general equilibrium effects might operate through the smaller cohort sizes that pill access induces.

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[3] See, for example, Joyce, Kaestner & Korenman (2000)

[4] See for example, Hock (2005) and Ananat & Hungerman (2006)

[5] See Bailey (2006) and Nagin et al. (1997)

[6] See Lochner & Moretti (2004)

In summary, there are many avenues through which higher levels of unwanted fertility can end up leading to higher crime rates. Moreover, many of these avenues or channels have feedback effects between them which will generally reinforce the link to a higher criminal propensity.

1.3.2 Necessary Conditions

Before describing the empirical strategy, it is important to establish whether two necessary conditions for the hypothesis in this chapter to be valid do in fact hold. First, pill access liberalization must lead to increased pill use. If, for whatever reason, access does not translate into actual use, the mechanism advanced in this chapter cannot be set in motion. Second, and most importantly, increased access must lead to a reduction in unwanted fertility. Regarding the first, Goldin & Katz (2002) provide evidence from the National Survey of Young Women showing that early legal access to the pill was indeed associated with greater pill use among young unmarried women. Regarding the second, an even more basic, question like "Does improvement in contraceptive technology succeed in reducing fertility?" remains somewhat debated. Using a moral hazard argument the answer can be: may be not. Indeed, more available insurance provides an incentive to increase the activity level in the risky behavior, say, unprotected sex.[7]. In fact, some recent empirical evidence suggests that legalized abortion led to a significant increase in sexual activity.[8] If this increase in risky behavior is coupled with a failure of the insurance mechanism like, say, improper pill use, the result might be an increase, rather than a decrease in fertility.[9] Despite these appealing theoreti-

[7] See Akerlof, Yellen and Katz (1996)

[8] See Klick & Stratmann (2003)

[9] An alternative theoretical reason involving strong habit persistence induced by sexual debut is explored in a dynamic structural model of teen sex and contraception by Arcidiacono, Khwaja & Ouyang (2006).

cal arguments, the empirical evidence in Hock (2005), Bailey (2006) and Ananat & Hungerman (2007) is more consistent with the standard effect that can be expected a priori: Improved contraceptive technology leads to a decline in fertility.[10]

Finally, it must be noted that the pill made its initial impact mostly on women of advantaged backgrounds, a group that is less likely to generate criminals regardless of the wantedness status of their pregnancies. This would bias the results not in favor of but against finding a pill effect, as we would be mixing in this group for which the pill really does not matter with women of lower socioeconomic status for which the pill is more likely to make a difference.

1.4 Data

1.4.1 The Pill

As mentioned above, this chapter exploits data on the timing of contraceptive liberalization. In particular, I follow the classification adopted by Hock (2005) to identify the years in which single women 18-19 years old first obtained access to the pill. Hock's methodology differs slightly from the one adopted in the works of Goldin & Katz (2000, 2002) and Bailey (2006).[11]

1.4.2 FBI-UCR Data on Arrests

I compute the arrests per-capita for each age category using state level counts of arrests from the Uniform Crime Reports collected by the Federal Bureau of Investigations. In this chapter I work with a version of the UCR-FBI data maintained by the National

[10] See, however, Guldi (2005) for some evidence that access to both, abortion and pill contraception, actually increased the birth rate.

[11] For more details on these differences, see Hock (2005).

Consortium on Violence Research (NCOVR) at Carnegie Mellon. As pointed out by Maltz & Targonski (2002) FBI-UCR data should be used with caution, due to a number of data quality problems, especially at the county level. Note that these very same FBI-UCR data have been used by Donohue & Levitt (2001) in their much debated contribution.

Using these data I am able to observe the behavior of 33 cohorts. The youngest cohort (born in 1988) is 15 years old in the last year of the sample (2003). The oldest cohort (born in 1956) is 24 years old in the first year of the sample (1980). See Table 1.2. The last years of the sample do not provide interesting variation since cohorts who are 15-24 at that time have been mostly born under liberal contraceptive regimes, regardless of state of birth. This is so except for those in their 20s who were born in Missouri.

While most of the analysis is carried out with state level data, a more finely disaggregated version of the UCR-FBI data is later used to provide a test of the hypothesis linking early access to the pill to future crime.

1.5 Empirical Strategy

In principle, I could look at the aggregate state level crime rates. Then, I would estimate the following panel data model for the per capita crime rate

$$\frac{Crime_{st}}{Pop_{st}} = \beta\ D_{s,t-20} + \lambda_s + \lambda_t + \varepsilon_{st} \tag{1.1}$$

where the dependent variable is the per capita number of crimes in state s and time t, λ_s and λ_t denote state and year specific effects and $D_{s,t-20}$ is a dummy variable indicating whether a liberal contraceptive policy was in place, say 20 years before t.

Now, if the pill is responsible for the reduction in crime, we should observe a

decline in the crime rates of those cohorts born under the liberal regime only. The lack of state level crime data by age of the criminal prevents me from testing this hypothesis directly. I therefore turn to FBI-UCR arrest data and estimate the following model for the number of arrests per capita, using age-state-year cells as the unit of observation.

$$\frac{Arrests_{ast}}{Pop_{ast}} = \beta Pill_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \varepsilon_{ast} \tag{1.2}$$

where $a = 15, 16, ..., 24$ indexes single year of age categories, $s = 1, 2, ..., 51$ indexes states and $t = 1980, ..., 2003$ indexes years. λ_t denote year specific effects that capture any national pattern in the time series of percapita arrests which is common across states and age categories. λ_s denote state effects that capture time invariant, unobserved state level characteristics that might affect the arrest rate. Finally, λ_a denote age effects that non-parametrically account for the crime-age profile, one of the most firmly established hard facts in criminology. More importantly, given data constraints (i.e. the fact that FBI arrest data by age is only available from 1980 onwards)

I do not observe the arrest rates for cohorts 5 to 9 before 1980, when their ages range from their mid to their late teens. See Table 1.2.

$Arrests_{ast}$ and Pop_{ast} denote the counts of arrests and population size for individuals of age a in state s in year t. $Pill_{t-a-1,s}$ is a binary indicator which is equal to one if the specific age-state-year combination implies that those individuals were born under a liberal contraceptive regime. In other words, the policy variable $Pill_{t-a-1,s}$ indicates whether a particular cohort that happens to be a years old at calendar year t in state s was born in a state-time combination that allowed single women 18-19 years old to obtain a prescription for contraceptive pills without parental consent.

The coefficient β measures the causal effect of teen access to the pill on the number of arrests per capita. With an estimate of β at hand, back of the envelope calculations can be done to derive an aggregate effect of the pill.

Table 1.2: Cohort Structure of NCOVR Data

	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
1956	1																										
1957	2	1																									
1958	3	2	1																								
1959	4	3	2	1																							
1960	5	4	3	2	1																						
1961	6	5	4	3	2	1																					
1962	7	6	5	4	3	2	1																				
1963	8	7	6	5	4	3	2	1																			
1964	9	8	7	6	5	4	3	2	1																		
1965	10	9	8	7	6	5	4	3	2	1																	
1966	11	10	9	8	7	6	5	4	3	2	1																
1967	12	11	10	9	8	7	6	5	4	3	2	1															
1968	13	12	11	10	9	8	7	6	5	4	3	2	1														
1969	14	13	12	11	10	9	8	7	6	5	4	3	2	1													
1970	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1												
1971	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1											
1972	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1										
1973	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1									
1974	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1								
1975	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1							
1976	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1						
1977	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1					
1978	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1				
1979	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1			
1980	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1		
1981	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3	2		
1982	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4	3		
1983	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5	4		
1984	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6	5		
1985	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7	6		
1986	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8	7		
1987	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9	8		
1988	33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10	9		
1989		33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10		
1990			33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11		
1991				33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12		
1992					33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13		
1993						33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14		
1994							33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15		
1995								33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16		
1996									33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17		
1997										33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18		
1998											33	32	31	30	29	28	27	26	25	24	23	22	21	20	19		
1999												33	32	31	30	29	28	27	26	25	24	23	22	21	20		
2000													33	32	31	30	29	28	27	26	25	24	23	22	21		
2001														33	32	31	30	29	28	27	26	25	24	23	22		
2002															33	32	31	30	29	28	27	26	25	24	23		
2003																33	32	31	30	29	28	27	26	25	24		

As explained above, given data limitations, results in following sections will be all in terms of arrests. It would be interesting to extend these results and look at the impact of the pill in actual crime rates since only a very small fraction of crimes end up in an arrest. While there is no reason to believe that the pill might have had an impact on the arrests-to-crimes ratio, I am ultimately interested in understanding the impact of unwanted fertility on crime, so further research is necessary to confirm that the results on arrests in following sections hold robust when the actual outcome is more directly related to the level of criminal activity.

Finally, note that the quasi-experimental variation is over a relatively small group, namely, single women who are 18 or 19 years old. These women account for a relatively small fraction of births in a given birth cohort. Since I am not able to distinguish who among the arrested individuals was born to a single 18 or 19 years old mother, I cannot look at the impact on the arrest rates for that ideal group, say, $\frac{Arrests_{ast}^{s,18-19}}{Pop_{ast}^{s,18-19}}$ where $Arrests_{ast}^{s,18-19}$ and $Pop_{ast}^{s,18-19}$ would denote the counts of arrests and population size for individuals of age a in state s in year t *who were born to single mothers 18 or 19 years* old at the time of conception. However, under mild assumptions, it can be shown that my estimate of β will recover a lower bound (in absolute magnitude) for the true causal effect of the pill on the arrest rates for this unobserved group.

Indeed, let α_{ast} denote the fraction of births due to single, 18 and 19 years old mothers $(\text{Births}_{s,t-a}^{s,18-19})$ taken relative to the total number of births $(\text{Total Births}_{s,t-a})$

$$\alpha_{ast} = \frac{\text{Births}_{s,t-a}^{s,18-19}}{\text{Total Births}_{s,t-a}}$$

Then, I can always decompose $\frac{Arrests_{ast}}{Pop_{ast}}$ as

$$\frac{Arrests_{ast}}{Pop_{ast}} = \alpha_{ast}\left[\frac{Arrests_{ast}^{s,18-19}}{Pop_{ast}^{s,18-19}}\right] + (1-\alpha_{ast})\left[\frac{Arrests_{ast}^{\tilde{}(s,18-19)}}{Pop_{ast}^{\tilde{}(s,18-19)}}\right]$$

where $Arrests_{ast}^{\tilde{}(s,18-19)}$ and $Pop_{ast}^{\tilde{}(s,18-19)}$ denote the count of arrests and population size for individuals of age a in state s in year t who were born to mothers who *were not* single 18-19 years old at the time of conception.

Then consider the following two population regression functions

$$\frac{Arrests_{ast}^{s,18-19}}{Pop_{ast}^{s,18-19}} = \beta^{*} Pill_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \eta_{ast} \tag{1.3}$$

$$\frac{Arrests_{ast}^{\tilde{}(s,18-19)}}{Pop_{ast}^{\tilde{}(s,18-19)}} = \tilde{\beta} Pill_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \tilde{\eta}_{ast} \tag{1.4}$$

Now, if there are no family size or cohort size effects, all the impact of the pill will be channeled trough a selection mechanism that will only impact the crime rates of those born to single 18-19 year old mothers and therefore we have $\tilde{\beta} = 0$. Then multiplying the first equation by α_{ast} and the second one by $1 - \alpha_{ast}$ and adding the two we get the regression function that I can actually estimate with the available data, namely,

$$\frac{Arrests_{ast}}{Pop_{ast}} = \beta^* \alpha_{ast} Pill_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \varepsilon_{ast} \tag{1.5}$$

If $\alpha_{ast} = \alpha$ then my estimate $\widehat{\beta}$ will be consistently estimating $\alpha\beta^*$, a loose lower bound for the causal parameter β^* given that $\alpha < 1$ by construction and indeed, only about 0.07 overall in the estimating sample. Moreover, since access to the pill will have an impact on α_{ast} we can relax the above assumption and let $\alpha_{s,t-a} = \alpha + \delta Pill_{t-a-1,s} + \nu_{s,t-a}$ with $\delta < 0$. It can be shown that in this case my estimate $\widehat{\beta}$ will be consistently estimating an even less tight lower bound for the causal parameter of interest β^*. Indeed, $\widehat{\beta}$ will be consistent for $\beta^*(\alpha + \delta)$,with $0 < (\alpha + \delta) < 1$ and $\alpha + \delta$ close to zero given $\alpha \approx 0.07$ and $\delta < 0$

1.5.1 Basic Estimates

Table 1.3 shows the baseline results. I estimate equation (1.2) by simple OLS. Column 1 shows that the coefficient for β is negative and significant with a point estimate of -0.004.

Noting that the dependent variable on arrests is in annual per-capita terms, the magnitude of this estimated negative causal effect is not minor. For example, for California, this translates into 450000 $\times$ 0.004 = 1800 fewer arrests on average for each year and each age category. Moreover, if we take into account that arrests are only the tip of the iceberg when it comes to measuring the extent of criminal activity, the impact of the pill cannot be understated.

I explore the robustness of this result to two adjustments that deal with some of the limitations of the data used in this article. First, I am able to observe neither the month of the arrest nor the month of birth of the arrested person. Therefore, while $t-a-1$ is most likely the year in which the arrested individual was conceived, it is possible that conception took place on year $t-a-2$ or, less likely, $t-a$. Assuming that births and arrests are uniformly distributed across the calendar year and that all pregnancies end up in births after the normal 9 months period, I construct an alternative indicator of pill access as

$$Pill_{ast} = \left(\frac{9}{24}\right) Pill_{t-a-2,s} + \left(\frac{12}{24}\right) Pill_{t-a-1,s} + \left(\frac{3}{24}\right) Pill_{t-a,s} \tag{1.6}$$

I then estimate equation (1.2) using $Pill_{ast}$ as defined above instead of $Pill_{t-a-1,s}$.

Another implicit assumption maintained in the previous section is that the state of arrest is the same as the state of birth for all individuals contributing to the aggregate arrest data. But this is not likely to be the case. While it is hard to imagine that the cross-state migration pattern would be systematic in a particular way that might threaten the causal interpretation of the pill effect, internal migration could affect the previous results. Note that so far I am abstracting away from internal migration by assuming that all the good or bad consequences of contraceptive liberalization will be felt within the state that adopts the policy change. In particular, I am assuming that arrested individuals were born in the same state that they are arrested. Problems might arise if states with early liberalization have a systematically different pattern of migration into or out of the state relative to states with late liberalization. Donohue & Levitt (2001) faced similar concerns and showed that their results hold robust when adjusting for cross-state mobility. If measurement error is classical, attenuation bias resulting from state mis-classification would bias results against the hypothesis that access to the pill leads to future declines in the arrest rate, implying that the estimated

magnitude is a lower bound (in absolute value).[12]

In order to address this issue, I use the 1980, 1990 and 2000 decennial censuses' microdata to compute state of birth probabilities, conditional on state of residence at any age (15-24) for each year.[13] With these probabilities at hand, the adjustment is relatively straightforward. I replace the raw policy indicator $Pill_{t-a-1,s}$ with a weighted version of it,

$$Pill^{W}_{t-a-1,s} = \sum_{s'} p_{at}\left(s'|s\right) Pill_{t-a-1,s'} \tag{1.7}$$

where $p_{at}\left(s'|s\right)$ are the conditional probabilities coming from the appropriate age- and year-specific state-of-birth / state-of-residence transition matrix.

Table 1.3: The Effect of Early Access to the Pill on Future Arrests

	Baseline	Alternative Birth Window	Cross State Mobility
Pill Access	-0.004	-0.005	-0.016
	[0.001]***	[0.001]***	[0.002]***
State effects?	YES	YES	YES
Year Effects?	YES	YES	YES
Age Effects?	YES	YES	YES
Observations	10200	10200	10200
R-squared	0.43	0.43	0.43

Robust standard errors in brackets

*significant at 10%; ** significant at 5%; *** significant at 1%

[12]Measurement error might not be classical, though. See Heckman, Farrar and Todd (1996) for an example of the consequences of non-classical measurement error and selective migration for the analysis of state-of-birth/state-of residence transitions.

[13]I use the PUMS microdata to compute these migration transition matrices for 1980,1990 and 2000 and impute the values for intervening years by interpolation.

Table 1.3 shows the results of the two adjustments described above. Column (1) shows the baseline estimate. As can be seen in column (2), the effect of the pill is robust to an alternative definition of pill access that takes into account the likelihood of conception at the two adjacent years. Column (3) shows that the effect of the pill is up to 4 times higher in magnitude when the adjustment for cross-state mobility is implemented by using the weighted pill indicator described in (1.7)

1.5.2 Abortion

Note that when abortion becomes legal the treatment effect provided by access to the pill is not the same. It is less powerful because it implies less of a change in the "possibility frontier" to avoid unwanted children. In the same vein, it would be interesting to check whether the results of Donohue & Levitt (2001) are actually picking up part of the pill effect and verify whether results from the previous section on the impact of the pill stand robust when controlling for abortion legal status. Note that the pattern of abortion legalization might be correlated with the process of contraceptive liberalization, say, for political reasons at the state level.

Five states legalized abortion in 1970. These "early legalizers" provide the variation necessary to identify the impact of abortion on future crime. Abortion becomes legal in the rest of the United States by way of the famous Supreme Court ruling in Roe v. Wade in 1973. I construct an indicator for the availability of legal abortion in the same way I constructed my pill access indicator.

$LegalAbort_{t-a-1,s}$ is a binary indicator which is equal to one if the specific age-state-year combination implies that those individuals were likely to be born under a regime in which abortion was already legal.

To maximize comparability with the results from Donohue & Levitt (2001) I re-

strict the sample to the same period (1985-1997) used by these authors.[14] Then, I augment the model in (1.2) by including the indicator for legal abortion.

$$\frac{Arrests_{ast}}{Pop_{ast}} = \beta\ Pill_{t-a-1,s} + \gamma\ LegalAbort_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \varepsilon_{ast} \qquad (1.8)$$

Table 1.4 reports the results from estimating Equation (1.8).

Table 1.4: The Effect of Early Access to the Pill and Abortion Legalization on Future Arrests

	1	2	3
Pill Access	-0.007		-0.005
	[0.002]***		[0.002]***
Legal Abort?		-0.009	-0.008
		[0.002]***	[0.002]***
State effects?	YES	YES	YES
Year Effects?	YES	YES	YES
Age Effects?	YES	YES	YES
Observations	6630	6630	6630
R-squared	0.49	0.49	0.49

Robust standard errors in brackets

*significant at 10%; ** significant at 5%; *** significant at 1%

In column (1) we corroborate that the results for the pill hold robust to the new sample period. The coefficient is now higher in magnitude (-0.007) and still significantly negative. Column (2) seems to replicate the well known results of Donohue & Levitt: legal abortion is significantly associated with substantial declines in the future

[14]However, as shown below in Table 1.5, these results stand robust when using the full sample and controlling for state-year effects.

rate of arrests per capita.[15] Finally, the model in column (3) includes both policy indicators simultaneously. Both coefficients are slightly smaller in magnitude relative to columns (1) and (2) but remain negative and significant indicating that both, abortion legalization and contraceptive technology, are valid and quantitatively important channels through which reductions in unwanted fertility yield crime declines in the long run. It is surprising however that magnitudes are so similar because the impact of the pill measures a treatment effect on late teen women only, while abortion legalization affects mothers of all ages.[16] In principle, one would expect the magnitude of the latter to be many times larger.

1.5.3 State-Year Effects

In this subsection I address the potential skepticism that may arise, as in the abortion-crime debate, regarding the causal nature of the previous results. In particular, despite the experimental flavor of the research design, it might be the case that by pure chance, there are some other factors operating at the state level that might generate a spurious correlation between pill access and future crime. I therefore turn to a more demanding identification strategy in which I exploit the single year of age dimension of the data to allow for a full set of state-year effects. These state-year effects can account for any state-specific phenomena that is responsible for fewer arrest in specific years during the '80s and '90s and that might be unfortunately correlated with the timing of pill access across states in the '60s and '70s, thus confounding the estimation of the parameter of interest. The following specification is more stringent in the sense that the variation

[15]This replication is not exact, though, because Donohue & Levitt use effective abortion rates rather than a simple dummy variable on whether abortion is legal or not.

[16]It is difficult to measure the impact of the pill on mothers other than 18-19 because in that case the empirical strategy would have to rely only on "before-and-after" designs around 1960. The usual caveats for inference with this type of design would then apply.

left in the data to identify the causal parameter is much smaller. Specifically, I estimate a more saturated model given by:

$$\frac{Arrests_{ast}}{Pop_{ast}} = \beta Pill_{t-a-1,s} + \gamma LegalAbort_{t-a-1,s} + \lambda_{st} + \lambda_{as} + \lambda_{at} + \varepsilon_{ast} \qquad (1.9)$$

where λ_{st} denote state-year effects, λ_{as} denote age-state effects and λ_{at} denote age-year effects. Table 1.5 shows the results of estimating equation (1.9).

Table 1.5: The Effect of Early Access to the Pill on future Arrests Controlling for Abortion Legalization and State-Year Effects

	Basic	Controlling for Abortion and State-Time Effects			
	1	2	3	4	5
Pill Access	-0.004	-0.011	-0.006	-0.007	-0.002
	[0.001]***	[0.001]***	[0.001]***	[0.001]***	[0.001]**
Legal Abort?		-0.004	-0.007	-0.006	-0.008
		[0.001]***	[.0032]**	[0.001]**	[0.001]***
State effects?	YES	YES	YES	YES	YES
Year Effects?	YES	YES	YES	YES	YES
Age Effects?	YES	YES	YES	YES	YES
State-Year Effects?	NO	YES	YES	YES	YES
Age-Year Effects?	NO	NO	YES	NO	YES
State-Age Effects?	NO	NO	NO	YES	YES
Observations	10200	10200	10200	10200	10200
R-squared	0.43	0.78	0.80	0.93	0.95

Robust standard errors in brackets

*significant at 10%; ** significant at 5%; *** significant at 1%

Column (2) shows that the causal effect of the pill is still statistically and economically significant under the more stringent identification strategy that controls for state-year effects. Moreover, as shown in Columns (3)-(5) the effect remains signifi-