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

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Abstract

Donohue and Levitt (2001) explain over 50% 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

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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. Preliminary results are consistent with the controversial arguments of Donohue & Levitt. They indicate that greater flexibility to avoid unwanted pregnancies (through better contraceptive technology) reduces crime two decades later, when undesired children would have reached their criminal prime.

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 another natural experiment induced by policy changes associated with 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 had 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 around short run fears of increased teen promiscuity and the spread of STDs, it is important to keep in mind the long run effects of a given contraceptive policy change.

The rest of the paper is organized as follows. The next section provides some background on the institutional and legal history of the pill. Section 3 discusses related literature. Section 4 describes the data and Section 5 presents the empirical strategy. Preliminary results are discussed in Section 6 along with potential extensions and further sensitivity/robustness analyses. Counterfactual policy extrapolations are conducted in Section 7. Conclusions follow.

2 Institutional Background

Here I provide a brief overview of the institutional and legal history associated with the pill.[1] The pill was introduced in the market in 1960 and quickly diffused among American women, becoming one of their preferred contraception methods. 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 sixties and seventies, 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. Interestingly, the timing of this contraceptive liberalization was different for most states, spanning the period from 1960 to 1977 (See Table 1 in the Appendix). This latter fact induces plausibly exogenous cross-state variation 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.[2]

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

[2] See Bailey (2006) for a compelling argument about the lack of policy endogeneity in the legislative and judicial process that leads, as unintended by-product, to contraceptive

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 for a while. The seminal contribution by Donohue & Levitt (2001) exploited abortion legalization as a natural experiment to quantify this effect. Comparing early legalizers to late legalizers, Donohue & Levitt claim 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 an explosion of critiques. In particular, Joyce (2004a) casts doubts over the validity of these findings. Joyce claims that the authors failed to account for the crack cocaine epidemic. A rejoinder by Donohue & Levitt (2003) argued that, if anything, failure to account for the crack epidemic biased the results against and not in favor of their 2001 findings. A response by Joyce (2004b) insisted that there was little evidence supporting a negative causal effect of abortion on crime. Other recent challenges to the findings of Donohue & Levitt (2001) include Foote & Goetze (2005), Skykes et al (2006) and Lott & Whitley (2006).

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

liberalization.

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 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.

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 fertility and the levels of criminality of the teenage 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.

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 in the instant following conception. Those cummulated disadvantages are a the end the ones who increase criminal tendencies. While the present paper will not be focusing on disentagling these alternative contributing mechanisms, it is worth mentioning that some of the likely early effects will be channeled through inadequate prenatal care and child abuse and neglect.[3]

4 Data

4.1 The Pill

As mentioned above, this paper exploits data on the timing of contraceptive liberalization. In particular, I follow the taxonomy 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).

4.2 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 paper I work with a version of the UCR-FBI data maintained by the National 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,

[3]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) and Lin & Pantano (2007)

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).[4] 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. In fact, in most of the specifications, observations from these years are not used due to perfect multicollinearity.

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

$$\ln\left(\frac{Crime_{st}}{Pop_{st}}\right) = \beta\ D_{s,t-20} + \lambda_s + \lambda_t + \varepsilon_{st} \tag{1}$$

where the dependent variable is the log of 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 prevents me from testing this hypothesis directly. I therefore turn to UCR arrest data and estimate the

[4] See Table 2 in the Appendix

following model for the number of arrests per capita, using age-state-year cells as the unit of observation.

$$\frac{Arrests_{ast}}{Pop_{ast}} = \beta PillAccess_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \varepsilon_{ast} \tag{2}$$

where $a = 15, 16, ..., 24$ indexes 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 effect that capture time invariant, unobserved state level characteristics that might affect the arrest rate. Finally, λ_a denote age effects to 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.[5]

$Arrests_{ast}$ and Pop_{ast} denote the counts of arrests and population size for individuals of age a in state s in year t. $PillAccess_{t-a-1,s}$ 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 $PillAccess_{t-a-1,s}$ indicates whether a particular cohort was born in a state-time combination that allowed single women 18-19 year 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

[5] See Table 2 in Appendix.

the pill. For example, for states liberalizing their contraception access late, I could compute the number of arrests (or crimes) that could have been prevented, had that particular state liberalized teen access to the pill in 1960 when the product became available in the market. For more on this policy extrapolation, see Section 7.

6 Results

6.1 Basic Estimates

Table 3 shows the baseline results. I estimate Equation (2) by simple OLS. Column 1 shows the results of simply pooling OLS. In this case, the coefficient associated with the policy indicator is positive and significant. Models in columns (2) and (3) successively add the full set of state and time effects to the specification. Results from the model accounting for state, time and age effects are shown in column 4. There we can see that the estimated coefficient for β is negative and significant with a point estimate of -0.004.

Table 3 : The effect of early access to the Pill on future Arrests

	1	2	3	4
Pill Access	0.008	0.009	-0.001	-0.004
	[0.001]***	[0.001]***	[0.001]	[0.001]***
State effects?	NO	YES	YES	YES
Year Effects?	NO	NO	YES	YES
Age Effects?	NO	NO	NO	YES
Observations	10200	10200	10200	10200
R-squared	0.01	0.36	0.38	0.43

Robust standard errors in brackets

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

Noting that the dependant variable on arrests is in annual per-capita terms, the magnitude of this estimated negative causal effect is not minor. 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. It is interesting to note that when including time effects in the specification the sign of the coefficient associated with the contraceptive policy indicator changes from positive to negative. Also, only when including age effects it does becomes significant.

6.2 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 technology 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 result 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 Staes by way of the famous Supreme Court ruling in Roe v. Wade in 1973. I contruct an indicator for the availability of legal abortion in the same way I contructed 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 restrict the sample to the same period (1985-1997) used by these authors. Then I augment the model in (2) by including the indicator for legal abortion.

$$\frac{Arrests_{ast}}{Pop_{ast}} = \beta\ PillAccess_{t-a-1,s} + \gamma\ LegalAbort_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \varepsilon_{ast} \tag{3}$$

Table 4 reports the results from estimating Equation (3).

Table 4 : The effect of early access to the Pill & 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 coeficient 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 rate of arrests per capita.[6] 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 both 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.[7] In principle, one would expect the magnitude of the latter to be many times larger.

6.3 Spatial Issues & Extensions

In this section I examine the robustness of the previous results and entertain some potential extensions. In particular I explore the sensitivity of results to two spatial considerations: a) cross-state travel for access to the pill and b) cross-state mobility from time-of-birth to time-of-arrest. Among the extensions, I consider tests to corroborate whether in fact, declines in unwanted fertility due to improved access to contraceptive technology have a negative causal effect on the number of arrests.

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

[7] 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 on "before-and-after" designs around 1960. The usual caveats for inference with this type of design apply.

6.3.1 Spatial Issues

Internal migration could affect the above 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 arrestees 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 in my favor, implying that the estimated magnitude is a lower bound (in absolute value).[8] I use the 1970 and 1980 decennial census microdata to compute state of birth probabilities, conditional on state of residence at any age (15-24). With these probabilities at hand, the adjustment is relatively straightforward. I replace the raw policy indicator $PillAccess_{t-a-1,s}$ with a weighted version of it,

$$PillAccess^{W}_{t-a-1,s} = \sum_{s'} p\left(s'|s\right) PillAccess_{t-a-1,s'}$$

where $p\left(s'|s\right)$ are the conditional probabilities coming from the appropiate state-of-birth/state-of-residence transition matrix.

Another spatial issue arise from geographic spillovers in access to the pill. The most extreme example of these situation is given by teen women living in St. Louis, Missouri, west of the Mississippi. While Illinois liberalized

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

access in 1961, Missouri was the last state to do so in 1977 (See Table 1). Researchers who have investigated the impact of abortion legalization on fertility have addressed similar concerns. In particular, Blank et al (1996) and Levine et al. (1996) emphasize the importance of taking into account cross-state traveling when assesing the effects abortion legalization. Onthe other hand, Goldin & Katz (2002) claimed this should not be a major problem in the case of the pill. To verify this I turn to arrest data from a finer level of geographic disagreggation : metropolitan statistical areas. Crime is, by far, an urban problem. Then, it's not surprising that most of the each state's crime is actually committed in the corresponding metropolitan areas. Having this additional margin of variation within states allows me to explore this issue in more detail. In particular, these data allow me to compute distances to the nearest neighboring state in which the pill is available. This strategy provides an alternative and potentially helpful source of variation when exploring the issue of geographic spillovers in access to the pill.[9]

Given lack of reliable population data for metropolitan areas by year and age, I turn to a slightly different specification. I consider the following model for the logarithm of the raw number of arrests in age category a, in metropolitan area m within state s, at time t.[10]

$$\begin{aligned} \log\left(Arrests_{amst}\right) &= \beta PillAccess_{t-a-1,s} \\ &+\gamma\left[1-PillAccess_{t-a-1,s}\right]Dist_{t-a-1,m} \\ &+\lambda_a+\lambda_m+\lambda_t+\varepsilon_{amst} \end{aligned} \tag{4}$$

[9]Alternatively, one could compare focal states which are surrounded by states with similar policy timing or, more formally use a spatial model.

[10]I exclude metropolitan areas that cross state borders from the analysis.

where

$$Dist_{t-a-1,m} = \min_{c \in D^*_{t-a-1}} d\left(m,c\right)$$

with

$$D^*_{t-a-1} = \{s : D_{s,t-a-1} = 1\}$$

and $d\left(m,c\right)$ denotes the geographic distance between metropolitan area m and a county c. Distance minimization is then conducted between a given metropolitan area and the counties belonging to any of the states in the set of states with liberal contraceptive regimes at time $t-a-1$, (D^*_{t-a-1}).[11]

The first column in Table 5 presents the results without the distance measure (i.e restricting $\gamma = 0$).[12] Note that when looking at data from metropolitan areas the pill still has a negative and significant causal effect on future arrests. Moreover, when estimating the model in expression (4) we observe that the coeficient on pill access reamains negative and significant and γ, the coeffient on distance to the closest county with liberal contraception is positive. The fact that γ is positive and significant is consistent with the general hypothesis relating unwanted fertility and crime. It implies that the contraceptive liberalization in an adjacent state brings down crime in a non-liberalizing state too, specially in metropolitan areas close to the boundary between the two states.

It should be stressed that these findings are consistent with cross-state travel for the pill. It could also be the case that the more "wanted" cohorts born in the adjacent liberalizing states will not be crossing the state line to commit crimes that often two decades later, thus explaining the observed

[11]I am thankful to Leah Boustan and the Minnesota Population Center who kindly provided data and codes to compute these distances.

[12]Note that interpretation of coeficients is not fully comparable with previous sections becasue the specification here is somewhat different.

Table 5 : The effect of early access to the Pill on future Arrests. Meropolitan Areas.
Dependent Variable: Log(Arrests)

	1	2
Pill Access	-0.09	-0.05
	[.008]***	[.008]***
[1-Pill Access]*Dist		1.39E-07
		[8.1E-09]***
MSA effects?	YES	YES
Year Effects?	YES	YES
Age Effects?	YES	YES
Observations	79433	79433
R-squared	0.47	0.48

Robust standard errors in brackets
*significant at 10%; ** significant at 5%; *** significant at 1%

negative impact in crime.

6.3.2 Extensions: Relative Size of population at risk, Placebo Test and Crime

The results so far suggest the existence of a causal link between access to the pill and later crime. However, it would be reassuring to subject these results to further scrutiny. In future versions of this paper I will conduct tests in order to provide more credibility to the findings in previous sections. First, I will use population data from Census to construct a measure of the relative size of the population at risk. Let $F_{t-a-1,s}^{18-19}$ be the proportion of females 18-19 years old in state s at time $t-a-1$. Let this proportion to be taken with respect to the total number of females residents of state s in the age range 18-

45. I will augment the basic model by including this measure of the relative size of the population at risk. Moreover, I will interact this share with the policy indicator, $PillAccess_{t-a-1,s}$. If access to the pill is what really drives down crime two decades later, we should expect a more sizeable negative causal effect in those states with higher fraction of the population at risk of treatment. I proxy this fraction by the share of females in ages 18-19. In other words, the interaction between the fraction of women 18-19 years old and the policy indicator for pill access, should be negative. This would provide a further test that the proposed channel is the one actually driving the results. This is similar in spirit to the diff-in-diff-in-diff strategy adopted by Bailey (2006). The extended specification would be

$$\begin{aligned} \frac{Arrests_{ast}}{Pop_{ast}} &= \beta PillAccess_{t-a-1,s} \\ &+\delta_0 F^{18-19}_{t-a-1,s} + \delta_1 \left(F^{18-19}_{t-a-1,s} \times PillAccess_{t-a-1,s} \right) \\ &+\lambda_a + \lambda_s + \lambda_t + \varepsilon_{ast} \end{aligned} \tag{5}$$

where $F^{18-19}_{t-a-1,s}$ is the proportion of women who were 18-19 years old when the cohort which is at age a in state s and time t was born. If the results of this test are to be supportive of the unwanted fertility story we expect the coefficient δ_1 on the key interaction term in (5) to be negative and statistically significant. This would imply that the effect of the pill was stronger in those states where the relative size of the treatment group was bigger. Similar tests could be conducted with the proportion of single 18-19 females or the fraction of births due to mothers who were 18-19 years old at birth. A caveat on the validity of this latter test might arise if we allow for the possibility that higher levels of teen fertility across states do not really reflect higher levels of unwantedness. In other words, out-of-wedlock teen fertility in Missisippi might be much higher than in California but still the fraction of unwanted births could be lower in the former state than in the latter. Moreover, marital status and fertility are choices that are affected by policy variation. A more crude but cleaner test would then rely only on the relative age structure of the female population, which can be considered predetermined.

In addition to the proposed test above, a battery of alternative placebo tests could be conducted to further corroborate the causal effects of the pill.

Finally, note that results in Sections 6.1 and 6.2 are in terms of arrests. It would be interesting to extend these results and look at the impact of the pill in actual crime rates. As mentioned above, 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 it is necessary to confirm that the results on arrests from the previous section hold robust when the actual outcome is more directly related to the level of criminal activity.

Following Levitt (1998,1999), I can assume constant arrest/crime ratio across age categories (i.e. $\frac{C_{ast}}{A_{ast}} = \frac{C_{st}}{A_{st}}$ for all a) and get a measure of crime by age as

$$\widehat{C}_{ast} = A_{ast} * \frac{C_{st}}{A_{st}} \tag{6}$$

where the crime-to-arrests ratio is allowed to vary by state and year.

7 Counterfactual Policy Extrapolation

Consider the following hypothetical scenario: Supppose unrestricted access to the Pill is granted across the board in 1960. We expect the improved wantedness level to induce lower criminality in cohorts born after 1960. How quantitatively important is this effect? How many arrests would have not taken place?

Integrating over ages, years and states, we can compute the conterfactual change in the number of arrests during the period according to the proposed scenario as:

$$\sum_{s=1}^{51} \sum_{t=1980}^{2003} \sum_{a=15}^{24} Pop_{ast} \left(1 - D_{t-a,s}\right) \widehat{\beta} \tag{7}$$

This simple back of the envelope calculation shows that a counterfactual scenario in which every state grants immediate unrestricted pill access to single teen women in 1960 is consistent with approximately 2 million fewer arrests in the period 1980-2003. To put this number in context, note that over the same period, there are about 97 million arrests reported in the FBI-UCR data. Therefore, the total impact would have been slightly over 2 %. Assuming a crime-to-arrests ratio of 5, about 10 million crimes would have been avoided over the period.

8 Conclusions

Preliminary results show that increased flexibility to avoid unwanted pregnancies reduce crime two decades into the future, when cohorts born in liberal contraceptive regimes reach their criminal prime. These results hold in two different samples and stand robust to adjustment for spatial condiderations.

While further testing and sensitivity analysis is required to place more confidence in these findings, it seems possible to extend the abortion-crime arguments to policies other than abortion legalization, as long as these other policies (i.e. family planning and contraception) also reduce the level of unwanted fertility.

My results suggest that had the Pill been adopted across the board right upon FDA approval in 1960, police forces would have had conducted aproximately 2 million fewer arrests during the period 1980-2003. A decline of about 2%.

Recalling that only a small fraction of crimes leads to an arrest ($\frac{\#Crimes}{\#Arrests}$ is way bigger than one), the number of crimes that could have been prevented would be many times higher.

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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)

1960	ARIZONA, IDAHO, MONTANA, NEVADA, NORTH DAKOTA, OKLAHOMA, UTAH, ALASKA
1961	ILLINOIS
1962	
1963	
1964	
1965	KENTUCKY, OHIO
1966	
1967	
1968	
1969	
1970	KANSAS, MISSISSIPPI, WASHINGTON
1971	ALABAMA, COLORADO, CONNECTICUT, GEORGIA, MARYLAND, NEW HAMPSHIRE, NEW MEXICO, NEW YORK, NORTH CAROLINA, OREGON, PENNSYLVANIA, TENNESSEE
1972	ARKANSAS, CALIFORNIA, DELAWARE, FLORIDA, LOUISIANA, MAINE, MICHIGAN, NEBRASKA, RHODE ISLAND, SOUTH CAROLINA, SOUTH DAKOTA, VERMONT, VIRGINIA, WEST VIRGINIA, WISCONSIN
1973	IOWA, INDIANA, NEW JERSEY, TEXAS, WYOMING
1974	DISTRICT OF COLUMBIA, MASSACHUSETTS, MINNESOTA
1975	HAWAII
1976	
1977	MISSOURI

Table 2 : NCOVR Data on arrests from UCR-FBI (15-24 year olds) and time span of policy change (1960-1977)

	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		