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

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Abstract

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 thank V. Joseph Hotz, Sebastian Galiani, Leah Platt Boustan and Bob Michael for helpful suggestions. Dan Akerberg, Sandy Black, Moshe Buchinsky, Dora Costa, Scott Cunningham, Christian Dustmann, Jinyong Hahn, Kathleen McGarry, Doug McKee, Rob Mare, Maurizio Mazzocco, Philip Morgan, Ernesto Schargrodsky and Duncan Thomas provided valuable comments. So did participants at several conferences and workshops, including the UCLA Proseminar in Applied Microeconomics, the European Society of Criminology (Tubingen, Germany 2006), the Latin American and Caribbean Economics Association (Mexico City, 2006), the Population Association of America (New York City, 2007), the Society of Labor Economists (Chicago, 2007), the European Society of Population Economics (Chicago, 2007) and the Western Economics Association (Seattle, 2007). I have also benefited from conversations with Jenny Hunt, Oscar Mitnik, Mariano Tappata and Emily Wiemers. All errors remain mine.

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. Preliminary 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 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 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 brief background on the institutional and legal history of the pill. Section 3 discusses related literature, causal mechanisms and necessary conditions for pill access to have a negative effect on future crime. Section 4 describes the data and Section 5 presents the basic empirical strategy, results and tests of the maintained hypothesis. A counterfactual policy extrapolation is conducted in Section 6. Conclusions follow.

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 in the Appendix, the timing of this contraceptive liberalization was different for most states, spanning the period from 1960 to 1977.² 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,

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

²See Table 1 in the Appendix

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.

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). The literature however, seems to be unsettled, as a rejoinder by Donohue & Levitt (2006) and a more comprehensive methodological overview of the subject by Ananat et al (2006) address many of these recent challenges and, to some extent, confirm the provocative findings of the 2001 article.

While much has been written about the so-called “Contraceptive Rev-

olution”, 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

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 the present paper 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.³ The impact 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 pregnan-

³For 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)

cies. 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.⁴ Moreover, unintended children may, if born, stall maternal human capital accumulation by both, reducing the mother's formal educational attainment⁵ and lowering her life-cycle labor force participation⁶. 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.⁷

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.

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

⁴See, for example, Joyce, Kaestner & Korenman (2000)

⁵See for example, Hock (2005) and Ananat & Hungerman (2006)

⁶See Bailey (2006) and Nagin et al. (1997)

⁷See Lochner & Moretti (2004)

generally reinforce the link to a higher criminal propensity.

3.2 Necessary Conditions

Before describing the empirical strategy, it is important to establish whether two necessary conditions for the hypothesis in this paper 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 paper 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.⁸ In fact, some recent empirical evidence suggests that legalized abortion led to a significant increase in sexual activity.⁹ 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.¹⁰ Despite these appealing theoretical arguments, the empirical evidence in Hock (2005), Bailey (2006)

⁸See Akerlof et al. (1996)

⁹See Klick & Stratmann (2003)

¹⁰An 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, Kwaja & Ouyang (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.¹¹

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.

4 Data

4.1 The Pill

As mentioned above, this paper 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).¹²

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

¹¹See, however, Guldi (2005) for some evidence that access to both, abortion and pill contraception, actually increased the birth rate.

¹²For more details on these differences, see Hock (2005).

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, 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).¹³ 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.

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

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

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

¹³See Table 2 in the Appendix

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.

$$\frac{Arrests_{ast}}{Pop_{ast}} = \beta Pill_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \varepsilon_{ast} \quad (2)$$

where $a = 15, 16, \dots, 24$ indexes single year of age categories, $s = 1, 2, \dots, 51$ indexes states and $t = 1980, \dots, 2003$ 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.¹⁴

$Arrests_{ast}$ 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

¹⁴See Table 2 in Appendix.

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.

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 $\frac{\text{Arrests}_{ast}}{\text{Pop}_{ast}}$ as

$$\frac{\text{Arrests}_{ast}}{\text{Pop}_{ast}} = \alpha_{ast} \left[\frac{\text{Arrests}_{ast}^{s,18-19}}{\text{Pop}_{ast}^{s,18-19}} \right] + (1 - \alpha_{ast}) \left[\frac{\text{Arrests}_{ast}^{\sim(s,18-19)}}{\text{Pop}_{ast}^{\sim(s,18-19)}} \right]$$

where $\text{Arrests}_{ast}^{\sim(s,18-19)}$ and $\text{Pop}_{ast}^{\sim(s,18-19)}$ 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

$$\frac{\text{Arrests}_{ast}^{s,18-19}}{\text{Pop}_{ast}^{s,18-19}} = \beta^* \text{Pill}_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \eta_{ast} \quad (3)$$

$$\frac{\text{Arrests}_{ast}^{\sim(s,18-19)}}{\text{Pop}_{ast}^{\sim(s,18-19)}} = \tilde{\beta} \text{Pill}_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \tilde{\eta}_{ast} \quad (4)$$

Now, if there are no family size or cohort size effects, all the impact of the pill will be channeled through 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,

$$\frac{\text{Arrests}_{ast}}{\text{Pop}_{ast}} = \beta^* \alpha_{ast} \text{Pill}_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \varepsilon_{ast} \quad (5)$$

If $\alpha_{ast} = \alpha$ then my estimate $\hat{\beta}$ 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 $\hat{\beta}$ will be consistently estimating an even less tight lower bound for the causal parameter of interest β^* . Indeed, $\hat{\beta}$ will be consistent for $\beta^*(\alpha + \delta)$, with $0 < (\alpha + \delta) < 1$ and $\alpha + \delta$ close to zero given $\alpha \approx 0.07$ and $\delta < 0$

5.1 Basic Estimates

Table 3 shows the baseline results. I estimate Equation (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

$$Pill_{ast} = \left(\frac{9}{24}\right) Pill_{t-a-2,s} + \left(\frac{12}{24}\right) Pill_{t-a-1,s} + \left(\frac{3}{24}\right) Pill_{t-a,s} \quad (6)$$

I then estimate equation (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 in my favor, implying that the estimated magnitude is a lower bound (in absolute value).¹⁵

In order to address this issue, I use the 1980, 1990 and 2000 decennial censuses' microdata to compute state of birth probabilities, conditional on

¹⁵Measurement 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.

Table 3 : The Effect of Early Access to the Pill on Future Arrests. Baseline Estimates, Alternative Birth Window and Cross-State Mobility Adjustments

	Baseline	Alternative Birth Window	Cross State Mobility
Pill Access	-0.004 [0.001]***	-0.005 [0.001]***	-0.016 [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%

state of residence at any age (15-24) for each year.¹⁶ 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,

$$Pill_{t-a-1,s}^W = \sum_{s'} p_{at}(s'|s) Pill_{t-a-1,s'} \quad (7)$$

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

Table 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

¹⁶I use the PUMS microdata to compute these migration transition matrices for 1980,1990 and 2000 and impute the values for intervening years by interpolation.

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 (7)

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 restrict the sample to the same period (1985-1997) used by these authors.¹⁷ Then, I augment the model in (2) by including the indicator for legal abortion.

$$\frac{Arrests_{ast}}{Pop_{ast}} = \beta Pill_{t-a-1,s} + \gamma LegalAbort_{t-a-1,s} + \lambda_a + \lambda_s + \lambda_t + \varepsilon_{ast} \quad (8)$$

Table 4 reports the results from estimating Equation (8).

Table 4 : The Effect of Early Access to the Pill & Abortion Legalization on Future Arrests

	1	2	3
Pill Access	-0.007 [0.002]***		-0.005 [0.002]***
Legal Abort?		-0.009 [0.002]***	-0.008 [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

¹⁷However, as shown below in Table 5, these results stand robust when using the full sample and controlling for state-year effects.

substantial declines in the future rate of arrests per capita.¹⁸ 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.¹⁹ In principle, one would expect the magnitude of the latter to be many times larger.

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

¹⁸This 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.

¹⁹It 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 would then apply.

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

$$\frac{Arrests_{ast}}{Pop_{ast}} = \beta Pill_{t-a-1,s} + \gamma LegalAbort_{t-a-1,s} + \lambda_{st} + \lambda_{as} + \lambda_{at} + \varepsilon_{ast} \quad (9)$$

where λ_{st} denote state-year effects, λ_{as} denote age-state effects and λ_{at} denote age-year effects. Table 5 shows the results of estimating equation (9).

Table 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 [0.001]***	-0.011 [0.001]***	-0.006 [0.001]***	-0.007 [0.001]***	-0.002 [0.001]**
Legal Abort?		-0.004 [0.001]***	-0.007 [.0032]**	-0.006 [0.001]**	-0.008 [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 significant when controlling, in addition, for a full set of state-age and year-age effects that allows the crime-age profile to flexibly vary by state and year. The effect of the pill remains significant, but smaller in magnitude, even in the fully saturated model that includes all the possible interactions and puts the most pressure on the data.

5.4 Tests

In this subsection I provide two tests of the proposed causal link between early teen access to the pill and future crime.

5.4.1 Relative size of population at risk of treatment

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 order to provide more credibility to the findings in previous sections. I use data from decennial population censuses to construct a measure of the relative size of the population at risk of treatment. Let $F_{t-a-1,s}^{18-19}$ be the proportion of females who were 18 or 19 years old in state s at time $t - a - 1$. Let this proportion to be taken with respect to the total number of female residents of state s in the reproductive age range, say 15-44.²⁰ I augment the basic model by including this measure of relative size

²⁰To compute $F_{t-a-1,s}^{18-19}$ for years after 1969 I rely on estimates from the Surveillance Epidemiology and End Results (SEER) Program at the National Cancer Institute. I interpolate the years between 1956 and 1968 exploiting the 1950, 1960 and 1970 decennial censuses.

of the population at risk. Moreover, I interact this share with the policy indicator, $Pill_{t-a-1,s}$. 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 a higher fraction of the population at risk of treatment. 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. The extended specification would be

$$\begin{aligned} \frac{Arrests_{ast}}{Pop_{ast}} &= \beta Pill_{t-a-1,s} \\ &+ \delta_0 F_{t-a-1,s}^{18-19} + \delta_1 (F_{t-a-1,s}^{18-19} \times Pill_{t-a-1,s}) \\ &+ \lambda_{st} + \lambda_{as} + \lambda_{at} + \varepsilon_{ast} \end{aligned} \tag{10}$$

where $F_{t-a-1,s}^{18-19}$ 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 conceived. 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 (10) 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 single mothers who were 18-19 years old at the time they got pregnant, say $B_{t-a-1,s}^{18-19}$. 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, unmarried teen fertility in Mississippi might be much higher than in California but still the fraction of unwanted births could be lower

Table 6 : Size of Treatment Group and the Impact of the Pill on Future Arrests

	1	2	3	4
Pill Access	0.022 [0.012]*	-0.006 [0.013]	0.018 [0.007]***	-0.006 [0.007]
F^{18-19} x Pill Access	-0.334 [0.138]**	0.096 [0.152]	-0.290 [0.076]***	0.076 [0.078]
State-Year effects ?	YES	YES	YES	YES
Age-Year effects ?	NO	YES	NO	YES
State-Age effects ?	NO	NO	YES	YES
Observations	10170	10170	10170	10170
R-squared	0.78	0.80	0.93	0.95

Note: Robust standard errors in brackets. All models include state-year effects and control for abortion legal status. Pill Access and F^{18-19} are adjusted for cross-state mobility. * significant at 10%; ** significant at 5%; *** significant at 1%

in the former state than in the latter. Moreover, marital status and fertility are choices that are affected by the policy variation of interest thus inducing potential post-treatment bias in estimation. Therefore I rely on the more crude but cleaner test that relies only on the relative age structure of the female population, using $F_{t-a-1,s}^{18-19}$, which can be considered predetermined.

The impact of the pill is then given by $\beta + \delta_1 F_{t-a-1,s}^{18-19}$. Table 6 presents the results of the test. In columns 2 and 4 both the interaction term and the main effect become not significant. However, specifications in Columns 1 and 3 show that the key interaction term, δ_1 is negative and significant. Noting that the variable $F_{t-a-1,s}^{18-19}$ ranges from 0.05 to 0.11 over the sample period, the total effect is negative for $F_{t-a-1,s}^{18-19} > 0.07$ in model (1) and $F_{t-a-1,s}^{18-19} > 0.065$ in model (3)

5.4.2 Geographic Spillovers in Access to the Pill and Criminal Activity

It is possible that geographic spillovers in access to the pill and criminal activity exist. The most extreme example of the first type of spillover is given by single teen women living in St. Louis, Missouri, west of the Mississippi. While Illinois liberalized access in 1961, Missouri was the last state to do so in 1977 (See Table 1). This creates 16 years of lag in the timing of pill access liberalization within a few miles. 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 assessing the effects abortion legalization. On the other hand, this should be less of a concern in the case of the pill because it would require teens to regularly drive out-of-state for checkups and refillings. This would entail a much greater cross-state travel burden relative to the case of abortion which only involves a single trip. Geographic spillovers in criminal activity are also relevant in my context. They involve state criminals residing close to a state boundary and crossing state lines to commit crimes in a nearby out-of-state city. As explained below, I can exploit the testable implications of these spillovers to provide further causal evidence for the link between pill access and crime.

In this section I turn to arrest data from a finer level of geographic disaggregation: metropolitan statistical areas. Crime is, by far, an urban problem. Then, it's not surprising that most of each state's crime is actually committed in the corresponding metropolitan areas. Having this additional margin of variation within states allows me to explore the issue of geographic spillovers 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 pro-

vides an alternative and potentially helpful source of variation when testing the effects of access to the pill on future crime.²¹

I consider the following model for the number of arrests per capita in age category a , in metropolitan area m within state s , at time t .²²

$$\begin{aligned} \frac{Arrests_{amst}}{Pop_{amst}} &= \beta Pill_{t-a-1,s} & (11) \\ &+ \gamma [1 - Pill_{t-a-1,s}] Dist_{t-a-1,m} \\ &+ \lambda_a + \lambda_m + \lambda_t + \varepsilon_{amst} \end{aligned}$$

where

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

with

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

and $d(m, c)$ 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 $t - a - 1$, namely D_{t-a-1}^* .²³

Table 7 presents the results of estimating the model in equation (11). We observe that the coefficient γ on the key interaction term $[1 - Pill_{t-a-1,s}] Dist_{t-a-1,m}$ is positive across specifications. Note that for metropolitan areas in states that by year $t - a - 1$ still remain in with conservative contraception regimens $[1 - Pill_{t-a-1,s}] Dist_{t-a-1,m}$ captures the distance to the closest county

²¹Alternatively, one could compare focal states which are surrounded by states with similar policy timing or, more formally, use a spatial model.

²²I exclude metropolitan areas that cross state borders from the analysis.

²³I am thankful to Leah Boustan and the Minnesota Population Center who kindly provided data and codes to compute these distances.

with liberal contraception. Since there are no policy reversals, this distance always declines over time as additional states switch from conservative to liberal contraceptive regimes. A by-product of these switches is that they make the distance to liberal contraception closer for those metropolitan that remain in conservative states. Then, it is easier to interpret γ as the impact of declines in this distance. A positive γ implies that declines in the distance to liberal contraception lead to declines in the (future) arrest rate.²⁴ It is hard to imagine an alternative story to rationalize why the number of arrests per capita would be smaller for some MSAs in such a precise spatial pattern if the timing of pill access is not the one to blame. The fact that γ is positive and significant is consistent with the maintained hypothesis relating early access to the pill and future crime. If γ is positive, for a metropolitan area in non-liberal state, declines in the distance to a liberal state are associated with declines in the own number of future arrests. This finding implies that the contraceptive liberalization in an adjacent state will bring down future 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 in the '60s and '70s but it is even more likely that an alternative mechanism is at play: 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 (in the '80s and '90s). However, regardless of the mechanism at play, this evidence is at least suggestive that the pill is really driving future crime down.

²⁴Noting that the distance is measured in miles, the magnitude of the interaction term is small. It is left for future research to investigate whether these magnitudes are consistent with findings in spatial criminology.

Table 7 : The Effect of Early Access to the Pill on Future Arrests. Metropolitan Areas. Dependent Variable: Arrests per capita

	1	2	3
Pill Access	0.004** [0.002]	0.006*** [0.001]	0.004* [0.002]
[1-Pill Access]*Dist	0.012*** [0.003]	0.007*** [0.001]	0.006*** [0.002]
MSA effects?	YES	YES	YES
Year Effects?	YES	YES	YES
Age Effects?	YES	YES	YES
MSA x Year Effects?	NO	YES	YES
MSA x Age Effects?	NO	NO	YES
Observations	34711	34711	34711
R-squared	0.74	0.87	0.88

Robust standard errors in brackets. *significant at 10%; ** significant at 5%; *** significant at 1%. The units used in the distance measure are miles. The interaction coefficient and its standard error have been multiplied by 1000000.

6 Counterfactual Policy Extrapolation

Consider the following hypothetical scenario: Suppose 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 counterfactual change in the number of arrests during the period according to the proposed scenario as:

$$\sum_{s=1}^{51} \sum_{t=1980}^{2003} \sum_{a=15}^{24} Pop_{ast} (1 - D_{t-a,s}) \hat{\beta} \quad (12)$$

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.

7 Conclusions

Preliminary evidence presented in this paper shows that increased flexibility to avoid unwanted pregnancies reduce crime two decades into the future, when cohorts born in more liberal contraceptive regimes reach their criminal

prime. These results hold in different samples and stand robust to several adjustments.

While further testing and sensitivity analyses are warranted 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. However, while results suggest that a selection mechanism is at play, further research is needed to quantify the magnitude of "family size", "cohort size" and "selection" channels.

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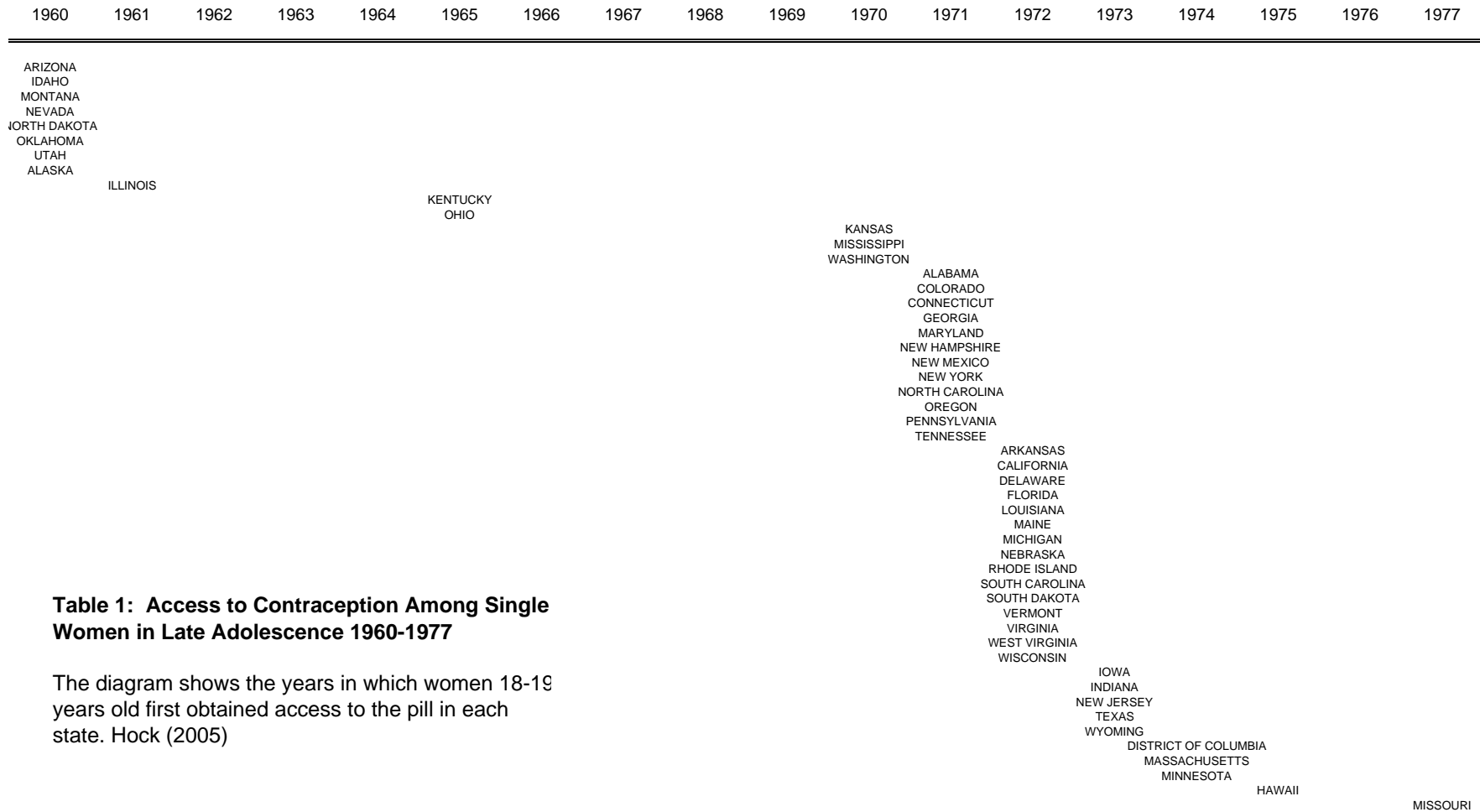


Table 1: Access to Contraception Among Single Women in Late Adolescence 1960-1977

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

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	1		
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	2	1	
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	3	2	1
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	4	3	2
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	5	4	3
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	6	5	4
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	7	6	5
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	8	7	6
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	9	8	7
1990			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	8
1991				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
1992					33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11	10
1993						33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12	11
1994							33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13	12
1995								33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14	13
1996									33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15	14
1997										33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16	15
1998											33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17	16
1999												33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18	17
2000													33	32	31	30	29	28	27	26	25	24	23	22	21	20	19	18
2001														33	32	31	30	29	28	27	26	25	24	23	22	21	20	19
2002															33	32	31	30	29	28	27	26	25	24	23	22	21	20
2003																33	32	31	30	29	28	27	26	25	24	23	22	21