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TWO SIDES OF THE SAME PILL? FERTILITY CONTROL AND MENTAL HEALTH EFFECT OF THE CONTRACEPTIVE PILL

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Two sides of the same pill? Fertility control and mental health effects of the contraceptive pill

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Abstract

I investigate the link between access to the contraceptive pill, mental health, and labor market outcomes. While liberalizing labor market effects of access to the pill are well established, a medical literature suggests a link between hormonal contraception and depression. Exploiting variation in access to the pill, I document substantial mental health effects of the pill. These mental health effects counteract the fertility control effect of the pill on labor market outcomes and are associated with limitations at work and more disability periods. The analysis also shows that the fertility control effect of the pill is larger than previously estimated.

Keywords: Mental Health, Contraceptive Pill, Fertility, Labor Market Outcomes
JEL-Codes: J16, J13, J18, J24, I0

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1 Introduction

When the contraceptive pill was first introduced in the US in the 1960s, it tremendously changed the way women made decisions with respect to childbearing, education, and labor market participation by offering more control over fertility. In 1999, an article in the Economist states that "the pill really did give a woman the right to choose" and that "technology really is liberation".[1] Several empirical studies that are often referred to as the "power of the pill" literature have indeed documented large liberalizing effects of the pill for women, in the form of delayed childbirth and marriage (Bailey, 2006), increased investment into lengthy education (Goldin and Katz, 2002), higher labor market participation, more hours worked, and higher wages (Bailey et al., 2012).

Recently, a medical literature has however uncovered a link between hormonal contraceptive use and mental health side effects such as first diagnosis of depression, anti-depressant use, and suicide attempts. The underlying mechanism suggested by these studies is the effect of the sex hormones progesterone and estrogen which have been discussed to cause depressive symptoms and are contained in many hormonal contraceptives. The results from these medical studies raise the concern that in addition to the liberalizing fertility control effect, the pill may also take a toll on women due to its mental health effect.

While the mental health effect of a widely-used contraceptive is interesting by itself, it can also have further implications for the findings from the "power of the pill" literature given the well-established negative impact of mental health problems on education and labor market outcomes. The existence of a mental health cost of the pill can be expected to counteract the positive fertility control effects of the pill, such that the total effect of the pill on education and labor market outcomes depends on the relative size of these two channels. This also means that previous estimates of the fertility control effect of the pill were potentially downward biased, as mental health costs were contained in its estimate.

In this paper, I re-investigate the effect of the pill on education and labor market outcomes in light of the link between hormonal contraception and mental health. This mental health channel has not been discussed or analyzed in the literature before. I first examine whether access to the pill leads to worse mental health later in life. For this, I use plausibly exogenous variation in access to the pill created by changes in laws governing access to the pill in the US between 1960 and 1977. I combine this information on variation in access with data from the Health and Retirement Study (HRS) for women born between 1934 and 1958 which is a cohort that experienced differential access to the pill during the period of adolescence. I show that access to the pill during this malleable period leads to worse self-reported mental health later in life. Women with access to

[1] "The Liberator", published December 23, 1999, in *The Economist*. Accessed March 2021, `https://www.economist.com/science-and-technology/1999/12/23/the-liberator`

the pill report a 27 percent higher depression score. These results are robust to alternative specifications and I can replicate them using another, independent data set, the Panel Study of Income Dynamics. My results also show important heterogeneity. Mental health effects are particularly large for women who received access early in adolescence and are also larger when mental health is measured at a younger age in the HRS.[2] Using data on polygenic scores, shows that the mental health effect of the pill also increases in the genetic risk to develop a mental illness. I can also provide evidence that these findings are not driven by general changes in life trajectories for women with access to the pill.

In a second step, I investigate the importance of this mental health cost for the liberalizing education and labor market effects established by the "power of the pill" literature. For this, I estimate the effect of the pill on education and labor market outcomes and explicitly take the role of mental health into account. While the effects of the pill on education and labor market participation are mostly small and insignificant, I show that they increase when controlling for mental health. This suggests that previous estimates are composed of two opposing effects: a positive fertility control effect of the pill that increases education and labor market participation and the mental health effect associated with the pill which decreases both outcomes. Along these lines, I also find that access to the pill increases the probability to report limitations at work due to health problems by 7-11 percentage points and increases the number of disability periods by 0.2-0.3. My analysis suggests that part of this stems from the mental health effect of the pill.

With this paper, I contribute to two strands of the literature. First, I add to the abovementioned "power of the pill" literature by considering an important health outcome and its relationship to the labor market. The "power of the pill" literature was initiated by Goldin and Katz (2000) providing evidence that trends such as the delay of marriage and higher rates of female college enrolment in professional programs coincided with the initial diffusion of the pill. The key underlying mechanism is that access to the pill reduces the price and increases the returns to (long-term) investment into education, by lifting both the penalty of abstinence and the uncertainty of pregnancy costs. These direct effects of the pill resulting in delayed fertility are demonstrated by Bailey (2006) and Bailey (2010). Bailey (2010) emphasizes that a large part of the effect on fertility is driven by changes in the timing of childbirth, not by changes in completed fertility. Resulting from the delay in fertility, Hock (2007) shows that access to the pill increased college enrolment rates of women by 5 percentage points and college completion rates by 0.9 percentage points. Steingrimsdottir (2016) demonstrates that women with access to the pill were more likely to enroll in programs leading to more ambitious occupations and higher wages. Larger investments into education were followed by increases in labor force participation, working hours and wages (Goldin and Katz, 2002; Hock, 2007; Madestam and Simeonova, 2012; Bailey et al., 2012; Bailey, 2006).

[2]Given the time lag between pill access during adolescence and the measurement of mental health in the HRS, this paper identifies long-term mental health effects of the pill. These are likely to arise due to the persistence of mental health problems, documented for example by Kessler and Bromet (2013)

Few studies identify negative consequences of access to the pill, such as reduced female bargaining power within marriage (Beauchamp and Pakaluk, 2019) or an increase in out-of-wedlock births due to increased sexual activity (Akerlof et al., 1996). However, the focus of these studies remains on the fertility control channel of the pill. Other effects of the pill such as the effect on mental health have not been taken into account. In terms of methodology, most of the above-mentioned studies use exogenous variation in laws determining age of majority in the US to identify the liberalizing effects of access to the pill. I follow the same approach.

A more recent evolution of this literature takes a more critical point of view toward the very large role of the contraceptive pill claimed by the studies above. Myers (2017) addresses the relative importance of access to abortion versus access to the pill and argues that the effect of the pill is considerably smaller than the one of abortion - if existent at all. She claims that access to the pill did not only lead to improved fertility control but also increased sexual activity which given that the pill is not 100% effective, might have even led to increased fertility. It is, however, less clear whether the more or less zero effect of the pill on fertility masks heterogeneity in pill effects for specific groups of women. Using the same policy coding, Lindo et al. (2020) show positive but mostly insignificant effects of pill access on education. They cannot find an effect on earnings in women's 50s but a positive effect on the probability to work in a Social Security covered job during their 20s and 30s. With this paper I add one potential explanation for the very small effects on education and labor market outcomes: mental health can be thought of as a mediator, preventing the pill from unfolding its true fertility control potential for labor market outcomes.

Second, I also contribute to the literature investigating the relationship between mental health, education, and labor market outcomes. A large amount of studies has identified a substantial negative impact of mental health problems on education and labor market outcomes. Mental health problems have been shown to negatively affect school performance (Ding et al., 2009), and to increase school drop-out (Cornaglia et al., 2015). Effects of mental health on labor market outcomes are substantial: Mental health issues reduce labor supply on both the internal as well as external margin and increase absenteeism (Banerjee et al., 2017; Ojeda et al., 2010) The effect on earnings is also large: Biasi et al. (2019) find earnings penalties of mental illness, ranging from 34% for depression to 74% for schizophrenia. Most of these studies have defined mental health to be pre-determined but have not accounted for the potential role of other influences on mental health that equally affect labor market and education outcomes such as the pill.

Lastly, this paper is more broadly related to the findings from the medical literature that identifies the effect of hormonal contraceptive use on mental health. Skovlund et al. (2016) show that in the entire female population in Denmark, the use of hormonal contraceptives is associated with a 1.2-1.8 higher incidence rate of first anti-depressant usage. Adolescent users have a 1.7 higher rate of first diagnosis of depression. Using the same

study from Denmark, Skovlund et al. (2018) demonstrate that the pill is also associated with a 1.9 higher incidence rate of suicide attempts. Wit et al. (2020) show similar similar patterns for the Netherlands with young women using hormonal contraceptives reporting a 21 percent higher depressive symptom score compared to non-users. The results of these studies can, however, not necessarily be interpreted as causal. The empirical identification is based on correlations between pill usage and mental health coming from cross-sectional variation or within-individual over-time variation. Threats to causality such as selection into the use of hormonal contraceptives are not addressed here. I will mitigate such concerns by using plausibly exogenous variation in access to the pill.

From a policy perspective, this paper addresses two important areas of public health: mental and reproductive health. Given the increasing prevalence of mental health problems, the fight against mental illness has become a priority on political agendas around the globe. In 2019, 20.6% of adult Americans reported suffering from mental illness, as reported by the National Institute of Mental Health.[3] The large prevalence is accentuated by an unequal distribution: Hammarström et al. (2009) show that the odds of experiencing mental illness once in life is around two times higher for women.
Recently, also reproductive health has received a lot of public attention, mostly related to abortion bans and funding cuts for abortion clinics in several US states. Increased barriers to abortion make healthy contraception even more important. Healthy contraception is also relevant in light of the gender imbalance in the bearing of potential mental health costs. While both, men and women benefit from the fertility control function, only women bear the potential costs.

The remainder of the paper is organized as follows: the next section provides background information on the legal environment creating variation in access to the pill. Section 3 describes the data and explains the empirical strategy. Section 4 presents results for the effect of access to the pill on mental health and Section 5 relates this mental health cost to labor market outcomes. Section 6 concludes.

[3] `https://www.nimh.nih.gov/health/statistics/mental-illness`, accessed August 13, 2021.

2 Access to the Pill

In order to identify mental health effects of the pill, I use changes to laws governing access to the pill and their most recent legal coding by Myers (2017). These law changes are derived from general political but also health care access rights. When the first pill in the US, *Enovid*, was approved by the United States Food and Drug Administration (FDA) for contraceptive use in 1960, anti-obscenity statutes, also referred to as Comstock laws, did not grant access to the pill in some states. Struck down in Griswold v. Connecticut, by 1970 every state allowed access at least for married individuals. In 1972, Eisenstadt v. Baird enabled access for unmarried individuals but only above the age of majority or with parental consent. This ruling resulted in the fact that in many states, younger, unmarried women were initially excluded from the benefits of contraceptive technology. Over the course of two decades, changes to laws defining age of majority and medical consent age as well as mature minor doctrines successively lowered the access barriers for young, unmarried women. This is the variation that I will exploit here.

Following the literature, there are two different forms of access for young women. The first is legal access, determining whether a method was legally available but young unmarried women were not able to consent themselves and needed the consent of their parents. The second form of access is legal and consent access. With this type of access, women were able to consent themselves and did not need parental consent. I will refer to legal access for the former and to consent access for the latter for the remainder of the paper. Whether or not young women were able to consent themselves depended on the legal age of majority, medical consent laws for minors, and mature minor doctrines. The "power of the pill" literature considers consent access to be the more relevant type of access. Parental consent to obtain contraceptives is considered an interference with privacy rights, and therefore expected to not reflect full access. The changes governing age thresholds for majority age were plausibly exogenous since they were not related to underlying needs for contraceptives but to the unrelated lowering of legal majority age in light of Vietnam war drafting. The lowering of legal majority age was supposed to diminish the age gap between earliest Vietnam war drafting (age 18) and voting rights (age 21), in order to align voting rights with draft obligations (Bailey, 2006).

Consent access to the pill varied substantially across states. Table 1 provides an overview of the legislation. Legal access can be derived from the existence of laws restricting a minimum access age in Table 1. If no such law existed, access was not legal. The validity of using this variation in access laws heavily depends on whether the lowered access barriers indeed resulted in higher pill usage. The take-up of the pill is thus an implicit first-stage of this identification strategy. Data on pill usage during this time is, however, relatively scarce. Goldin and Katz (2002) rely on a cross-sectional snapshot of the National Study of Young Women and identify an increased pill usage of 4 percentage points for women aged 17 to 19 years. Bailey et al. (2012) use retrospectively reported contraceptive usage data to additionally take into account state and cohort fixed effects. They find that the

probability to use the pill before age 21 increased for women with legal access before age 21 by 16 percentage points, representing a 42 percent rise relative to the national average.

In addition to the mere possibility to obtain the pill, other access barriers might have been important. Insurance for financial coverage did not play a role for access to the pill, since insurance was only mandated with the Affordable Care Act in 2010 to provide birth control. The cost of the pill at the time of the introduction was around 100$ per year (Warsh, 2011), which is equivalent to 760$ in 2010 (Bailey, 2013). Within five years after the introduction, the price already dropped to 25$ per year. In addition to the pill, alternative forms of contraception existed at that time, such as condoms and diaphragms. These barrier methods were, however, also expensive and in contrast to the pill needed to be applied before intercourse and thus represented a higher variable cost of fertility control. They also had a higher failure rate than the pill (Bailey, 2006).

It is important to acknowledge that there is an overlap in timing between the improved access to the contraceptive pill and access to abortion. The advantage of using the coding of Myers (2017) is that she also provides information on legal and consent access to abortion. Table A1 in the appendix shows an overview of access to abortion over time. Abortion access is also important to take into account as it may have a mental health effect by itself. A priori, the effect of access to abortion on mental health is ambiguous and is widely discussed in public debates and in the medical profession. The economic literature on this is relatively scarce. A study by Janys and Siflinger (2021) investigates the mental health effects of having an abortion and finds precisely estimated null effects. Clarke and Mühlrad (2021) investigate the role of abortion legislation in Mexico and also find no effects.

Table 1: Legal and consent access to the pill - law changes coded by Myers (2017)

Age	>21	18-20	<18		>21	18-20	<18
Alabama	1960	1971	1971	Montana	1960	1960	
Alaska	1960	1960	1974	Nebraska	1965	1969	
Arizona	1962	1972	1977	Nevada	1963	1963	1975
Arkansas	1960	1960	1973	New Hampshire	1960	1971	1971
California	1963	1972	1976	New Jersey	1963	1973	
Colorado	1961	1971	1971	New Mexico	1960	1971	1973
Connecticut	1965	1971		New York	1960	1971	1971
Delaware	1965	1971	1972	North Carolina	1960	1971	1977
District of Columbia	1960	1971	1971	North Dakota	1960	1960	
Florida	1960	1972	1972	Ohio	1965	1965	1965
Georgia	1960	1971	1972	Oklahoma	1960	1960	
Hawaii	1960	1960		Oregon	1960	1971	1971
Idaho	1960	1960	1974	Pennsylvania	1960	1970	
Illinois	1961	1961	1969	Rhode Island	1960	1972	
Indiana	1963	1973		South Carolina	1960	1972	1972
Iowa	1960	1972		South Dakota	1960	1972	
Kansas	1963	1970	1970	Tennessee	1960	1971	1971
Kentucky	1960	1965	1972	Texas	1960	1973	
Louisiana	1960	1972		Utah	1960	1960	
Maine	1960	1969	1973	Vermont	1960	1971	
Maryland	1960	1971	1971	Virginia	1960	1971	1971
Massachusetts	1972	1974	1977	Washington	1960	1970	
Michigan	1960	1972		West Virginia	1960	1972	
Minnesota	1960	1973	1976	Wisconsin	1974	1974	
Missouri	1965	1977		Wyoming	1960	1973	
Mississippi	1965	1965	1965				

Note: The table shows years in which states enabled legal and consent access to the pill for a given age group. The coding of these laws is taken from Myers (2017).

3 Data and Empirical Strategy

3.1 Data on pill access

I combine the coding of laws granting access to the pill with data from the Health and Retirement Study (HRS).[4] The HRS is a large panel study of a representative sample of around 20,000 Americans aged 50 and above. The age structure of this data is ideal since it covers women born early enough to be exposed to the early diffusion of the pill, more specifically those cohorts born between 1934 and 1958. The sharp differences in access laws across states and time for this generation of women provides the exogenous variation used to identify the effects of the pill. Women born before 1934 had access to the pill only in their late twenties and those born after 1958 had full access across most states. Myers (2017) argues for restricting the sample to not include women born after 1958 since this is the last birth cohort reaching age of majority before the definite legal status on consent access became unclear in many states given several Supreme Court cases.

I construct information on access to the pill for each individual using the information on respondents' year of birth and the state they lived in at age 10 using restricted HRS geographic data.[5] I focus on access between age 14 and 21 for several reasons. First, this is the age range in which changes in access to the pill occurred as women above age 21 always had access starting in 1972. In this age bracket, individuals also make important decisions regarding human capital investment. In addition, this period is important for mental health development, since mental health is particularly malleable and sensitive to external influence during this time, as demonstrated by Kessler et al. (2005). When documenting the link between contraceptives and mental health, Skovlund et al. (2016) also found larger differences in incidence rates of depression between users and non-users of hormonal contraceptives for adolescent women aged 15-19 compared to all women.

Table 2 shows how access to the pill is distributed in the HRS sample. The first column depicts the number of years an individual had legal but no consent access between ages 14 and 21. The second column does the same for legal and consent access. While the two types of access are mutually exclusive, it is possible for an individual to first have legal access for some years and then to have consent access for the following years. The

[4]More specifically, I use the RAND HRS Longitudinal File. The RAND HRS Longitudinal File is an easy-to-use dataset based on the HRS core data. This file was developed at RAND with funding from the National Institute on Aging and the Social Security Administration.

[5]This is the residence information available closest to the age bracket for pill access that I consider in my analysis. This approach is an improved measurement compared to most previous studies that had to rely on state of birth information which could have led to bias in the measurement of pill access if respondents moved during childhood. I only use the information on the state of birth if there is no information on the state of residence at age 10. Data set: HRS ([Cross-Wave Geographic Information (Detail) [1992-2018] - v8.2, Early]) restricted dataset. Produced and distributed by the University of Michigan with funding from the National Institute on Aging (grant number NIA U01AG009740). Ann Arbor, MI, (1992-2018).

Table 2: Years of pill access during adolescence (age 14-21)

Years	Legal Access	Consent Access
0	35.86%	47.41%
1	5.57%	8.69%
2	7.39%	8.65%
3	5.09%	19.81%
4	18.04%	5.28%
5	6.20%	4.39%
6	5.96%	2.28%
7	15.90%	3.49%
N	7,905	

Note: The table presents the distribution of the number of years that women in the selected sample had legal and consent access to the pill in the HRS. Sample restricted to women born between 1934 and 1958 with mental health information available

distribution of access across these two measures differs strongly, suggesting to include them separately in the estimations later. Around 36% of women had no legal access at all, while 47% percent of women had no consent access at all. Around 28% of women had more than 4 years of legal access, and only 10% percent had more than 4 years of consent access.

In a similar fashion, I construct exposure variables to abortion access, given the timing overlap and the potential importance of abortion availability for mental health. I include them as additional control variables in all estimations.

3.2 Data on Mental Health and Labor Market Outcomes

I use two measures of mental health, both derived from the self-reported Center for Epidemiological Studies Depression (CES-D) scale, developed by Radloff (1977). This scale has been used in the economic literature for the assessment of mental health in several life situations, such as bereavement (Siflinger, 2017), response to family health shocks (Rellstab et al., 2020), improved access or coverage of mental health care (Ma and Nolan, 2017; Ayyagari and Shane, 2015), and experience of major recessions (McInerney et al., 2013). The HRS contains the 8-item version of the CES-D. This version contains six negative and two positive items. Negative items are feeling depressed, sad, restless, alone, feeling that everything is an effort and that one could not get going. Positive items are feeling happy and enjoying life. Each of the items is answered in a binary fashion (yes/no) to an affirmative statement.[6] Positive answers to negative items are added positively, while positive answers to positive items are counted as zero and vice-versa.

[6]For example: "I felt everything I did was an effort."

Therefore, the scale reflects the number of depressive items an individual experiences. The scale ranges from 0 to 8, where a higher value indicates higher levels of depression. In the medical literature, a threshold value of the CES-D scale of three or larger is considered to indicate clinically significant levels of depression (Turvey et al., 1999). I use the CES-D scale itself and an indicator for whether or not an individual reports a scale above the clinically relevant threshold of the CES-D scale. The CES-D scale is collected in waves 2-14 of the HRS. I construct both measures as close as possible to age 60.[7] Table 3 below shows descriptive statistics of variables I will use throughout the analysis. The average CES-D score is 1.64, with a standard deviation of 2.16. That means that women in my sample report on average 1.6 depressive items. Between the ages 55 and 65, 25% of the sample report at least one time a CES-D score above or equal to the critical threshold value of three items.

Given that there is no information available on mental health right before and after pill access, I focus on mental health measured later in life and control for pre-existing mental health problems. These pre-existing conditions are mental health problems reported by the participants in the retrospective childhood questionnaire of the HRS. I use an indicator for whether or not an individual reported depressive symptoms during childhood. Only a small fraction of 4 percent of my sample experience childhood depression. I will also control for the exact age at measurement of the mental health variables and whether respondents are black. Respondents are on average 59.6 years old, and 22% of them are black. In a robustness exercise, I aim at providing a better understanding of the potential channels at work when investigating the effect of access to the pill on mental health. For this, I use additional variables on family formation and stress at work. Descriptives of these variables can be found in Table A7 in the appendix.

In the second part of my analysis, I investigate the effect of the pill on education and labor market outcomes. For this, I use information from the HRS on the years of education, an indicator for whether an individual attended college and an indicator for whether an individual was ever in the labor force. I also use information on the share of interviews in which a respondent reported being limited at work due to health problems. In addition, the HRS provides information on the number of disability periods which I will use as an outcome as well. Respondents have on average 13 years of education and around half of them attended college. 82% are or have been in the labor force. Respondents report on average in 31% of the interviews to have limitations at work due to health problems and report on average 0.37 periods of disability.

Table 4 shows how the two mental health measures differ according to differential access

[7]For some women, mental health information is already available during their 30s and 40s. These women are however only included in the HRS because their partners are older than 50. These are few women and they are also potentially different from the rest of the sample given the large age gap to the husband. Therefore, I mainly focus on mental health measured as close as possible around age 60, where I have data on most individuals. I consider differences according to age at measurement in a robustness analysis.

Table 3: Descriptive statistics of main variables

	Mean	Std. Dev.
Mental health outcomes		
CES-D Score	1.64	(2.16)
CES-D critical threshold	0.25	(0.43)
Pill access		
Fract. years pill legal (14-21)	0.41	(0.38)
Fract. years pill legal & consent (14-21)	0.18	(0.28)
Fract. years abortion legal (14-21)	0.04	(0.10)
Fract. years abortion legal & consent (14-21)	0.12	(0.23)
Control variables		
Childhood depression	0.04	(0.20)
Age at measurement	59.58	(1.07)
Black	0.22	(0.42)
Education and labor market outcomes		
Years of education	13.01	(2.56)
College	0.49	(0.50)
Labor force participation (yes)	0.82	(0.38)
% interviews limitations at work	0.31	(0.35)
Number of disability periods	0.37	(0.85)
N	7,905	

Note: Means and standard deviations (in parentheses). Sample restricted to women born between 1934 and 1958 with mental health information available.

Table 4: Descriptive Statistics of mental health according to pill access

	No access		*Full access*		*Partial access*	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Consent access						
CES-D Score	1.44	(1.98)	1.78	(2.26)	1.83	(2.30)
CES-D critical threshold	0.22	(0.41)	0.25	(0.43)	0.28	(0.45)
Legal access						
CES-D Score	1.45	(1.98)	1.76	(2.23)	1.75	(2.25)
CES-D critical threshold	0.22	(0.41)	0.27	(0.44)	0.26	(0.44)
N	7,905					

Note: Means and standard deviations (in parentheses). No access is defined when the women did not have access (legal/consent) to the pill between age 14 and 21. Partial access is defined as access (legal/consent) for at least one but not all years between age 14 and 21. Full access is defined as access (legal/ consent) where women had access during all years between age 14 and age 21. Sample restricted to women born between 1934 and 1958 with mental health information available.

to the pill. Across both measures and both access types, mental health is worse with full or partial access to the pill, compared to no access. While individuals without access to the pill report a CES-D score of 1.44 and 1.45, those with legal or consent access report scores between 1.75 and 1.83, so 22-26% higher scores. 22% of individuals without access to the pill report a CES-D score equal to or larger than three. Among those individuals with access, 25-28% report a CES-D score equal to or larger than three.

3.3 Empirical Strategy

I define pill access as an exposure measure for the years between age 14 and 21. I use the fraction of years between ages 14 and 21 in which a woman had i) legal but no consent access and ii) legal and consent access to the pill.[8] I estimate the effect of access to the pill in adolescence for woman i living in state s at age 10, born in year t with the following equation:

$$\textit{mental health}_{i,s,t} = \beta_0 + \beta_1 \textit{ legal pill access}_{i,s,t} + \beta_2 \textit{ consent pill access}_{i,s,t} + \boldsymbol{\beta_3 x}_{i,s,t} + \mu_t + \nu_s + \epsilon_{i,s,t} \quad (1)$$

[8]In contrast to Myers (2017), I adapt the fractions for consent access to the pill and to abortion, in case a woman got married younger than the consent age and thus received access to the pill through marriage. I add a dummy controlling for this access through marriage in all specifications. Myers (2017) did not adapt this, since age at marriage is one of her outcomes. In my setting, access to the pill, therefore, does not only vary at the state birth-cohort level but also at the individual level.

I am interested in β_1 and β_2, the effects of exposure to legal and consent access to the pill during adolescence on mental health around age 60. As both variables represent the fraction of years between age 14 and 21 with access, β_1 and β_2 have to be interpreted as the effect of woman i having legal or consent access to the pill during all seven years between age 14 and age 21.[9] Mental health is measured by the CES-D score and by an indicator for crossing the clinically relevant threshold of the CES-D. I estimate this equation using OLS, therefore as a linear probability model for the latter outcome. In every specification, I control for differential access to abortion, by adding the fraction of years between ages 14 and 21 in which woman i had legal and consent access to abortion, captured in $\boldsymbol{x}_{i,s,t}$. I also include age, a dummy for being black, and childhood depression in $\boldsymbol{x}_{i,s,t}$. Additionally, I add birth cohort and state fixed effects in all my regressions, here represented by μ_t and ν_s. I also include linear state-time trends in $\boldsymbol{x}_{i,s,t}$. Later, I test for robustness to different trend specifications.[10]

In the second part of my analysis, I use a similar approach to investigate the effects of access to the pill on education and labor market outcomes, taking the mental health cost of the pill into account. I first estimate the effect of the pill on different education and labor market outcomes, captured by $Y_{i,s,t}$, without taking mental health into account. For this, I use the following equation:

$$Y_{i,s,t} = \alpha_0 + \alpha_1 \textit{ legal pill access}_{i,s,t} + \alpha_2 \textit{ consent pill access}_{i,s,t} + \boldsymbol{\alpha_3 x}_{i,s,t} + \rho_t + \phi_s + \upsilon_{i,s,t} \quad (2)$$

In $x_{i,s,t}$, I here control for access to abortion, age, a dummy for being black, linear state-time trends, state equal pay laws, state acts prohibiting racial discrimination in employment, and laws for no-fault divorce. In the next step, I add the CES-D score measure to the estimations above:

$$Y_{i,s,t} = \gamma_0 + \gamma_1 \textit{ legal pill access}_{i,s,t} + \gamma_2 \textit{ consent pill access}_{i,s,t} + \boldsymbol{\gamma_3 x}_{i,s,t} + \gamma_4 CESD \textit{ score}_i + \theta t + \eta_s + \zeta_{i,s,t} \quad (3)$$

I base my inference on the difference between α_1 and γ_1, as well as between α_2 and γ_2, This comparison shows how taking mental health into account changes the effect of the pill on education and labor market outcomes. Given that the inclusion of mental health as a control might lead to a bad control problem, I estimate equation (3) a second time but instrument the CES-D score with childhood depression. Given that childhood depression

[9] For inference on one additional year of access the coefficients need to be divided by seven.

[10] Ideally, one would add more covariates in this analysis, such as education or fertility outcomes that also shape mental health important ways. In this context, these would however be bad controls, according to the wording of Angrist and Pischke (2008). Bad controls are variables that are also outcomes of the treatment itself. In this case, it has been shown empirically by the "power of the pill" literature that the pill affects education and fertility decisions. I, therefore, do not include these variables. I do, however, investigate the role of fertility and family formation outcomes for mental health in a robustness analysis.

is not affected by access to the pill but strongly correlated with later mental health given the persistence of mental illness, this should mitigate the bad control problem.

4 The Mental Health Effect of Access to the Pill

In this section, I investigate the effect of access to the contraceptive pill during adolescence on later life mental health. Table 5 shows that consent access to the pill is associated with worse self-reported mental health. Having consent access to the pill during all years between age 14 and 21 is associated with a 0.43 higher CES-D score, which means 0.43 additional depressive items. This reflects an increase of 27 percent of the mean. Consent access to the pill also increases the probability to report a CES-D score above the clinically relevant threshold by 7.6 percentage points, which is an increase of a third of the mean, but only marginally statistically significant. The effects for legal access are very small and not statistically significant in both cases, which is in line with the literature suggesting that consent access is the more important access margin. Access to abortion is associated with better self-reported mental health. Legal access to abortion leads to a 0.6 and consent access to a 0.8 lower CES-D score. This positive mental health effect seems plausible if we assume a detrimental mental health effect of unwanted pregnancies. The effects of abortion are not statistically significant for the probability to report a CES-D score above the clinically relevant threshold. Table 5 also shows that mental health problems are very persistent. Having suffered from depression during childhood increases the number of depressive items reported in the CES-D score by almost two, and increases the probability to report a clinically relevant score by 31 percentage points. While there are no differences across age for this closely defined age group, being black is associated with worse mental health, with coefficients of similar size to those of consent access to the pill.

The size of these estimates is large, in particular when considering the long time lag and the fact that this is measuring an intention-to-treat effect. Bailey et al. (2012) report that take-up of the pill before age 21 increased by 42 percent with legal access alone. It is important to keep in mind that the coefficients represent the change in access from 0 to 7 years. Therefore, one additional year of legal access is associated with a 0.010 increase in the CES-D score and one additional year of consent access with an increase of 0.062.

It seems reasonable to relate these findings to results found in the medical literature. Skovlund et al. (2016) report an 80 percent higher incidence rate of first diagnosis of depression for adolescents, which is even larger than my estimates. This is, however, potentially driven by analyzing the pure within-individual correlations over time and not accounting for selection. Nevertheless, one needs to also consider the dosage of hormones in the pill women had access to. The medical studies reporting a mental health effect of hormonal contraceptives measure effects of contraceptive pills produced in the early 2000s, while I am measuring the effect of a drug administered in the 1960s and 1970s that entailed much higher doses of hormones. Liao and Dollin (2012) report that the first available pill contained around 9.5 milligrams (mg) of progestin while pills today contain only 0.1-3 mg.[11] Estrogen levels were also much higher with 150 mg for the first pill,

[11] Progestine is the synthetic hormone that mimics the body's own hormone progesterone. Both

Table 5: Effect of pill access during adolescence (age 14-21) on mental health

	CES-D	> critical threshold
	(1)	(2)
Fract. years legal access pill	0.070	-0.003
	[0.173]	[0.029]
Fract. years consent access pill	0.433**	0.076*
	[0.188]	[0.039]
Fract. years legal access abortion	-0.594*	-0.087
	[0.313]	[0.071]
Fract. years consent access abortion	-0.820**	-0.082
	[0.353]	[0.069]
Childhood depression	1.891***	0.307***
	[0.139]	[0.028]
Age	0.013	0.003
	[0.026]	[0.004]
Black	0.479***	0.082***
	[0.065]	[0.013]
R-squared	0.113	0.057
N	7,905	7,905
Linear time trends	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects.

compared to 20-50 mg for the pill today. This could contribute to explaining the size of the effects.

The effect sizes can also be compared to other studies on the CES-D score in the HRS. One example is the effect of Medicare Part D on the CES-D score as investigated by Ayyagari and Shane (2015): They find a reduction of 0.2 depressive items after the Medicare introduction for eligible individuals and a 4-5 percentage points lower probability to report a CES-D score above the clinically relevant threshold. For drug coverage itself, they identify a reduction of the CES-D score of 1.6 items. Compared to the effect of the 2008 stock market crash on the CES-D score of individuals with stocks below the median before the crash identified by McInerney et al. (2013), my estimates are around 30% larger. In light of these effect sizes, my estimates seem not unrealistic.

Given that the clinically relevant threshold of the CES-D score is a dichotomous vari-

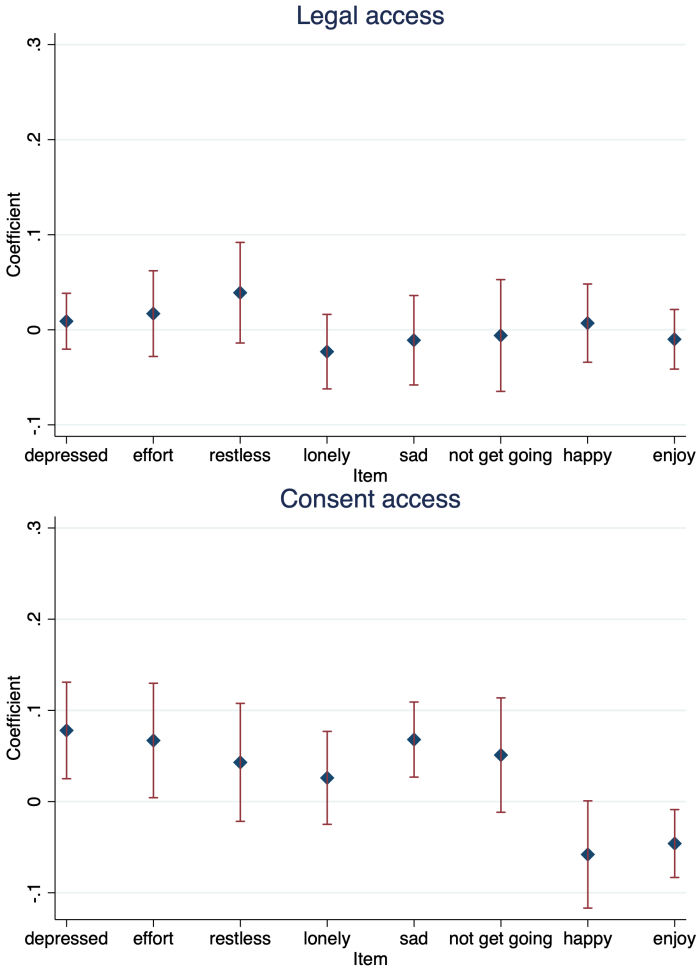
progesterone and estrogen have been discussed as the hormones causing depression.

able, I test whether my results are robust to an alternative specification by using a probit estimation. Table A2 in the appendix shows average marginal effects, that are in line with the results from the OLS in Table 5. The results are also robust to alternative specifications of time trends. Tables A3 and A4 in the appendix show the results for omitting linear time trends and for adding quadratic time trends.

My analysis builds on variation in access caused by variation in the timing and location of law changes, so a staggered treatment on a state-birth cohort level. The use of two-way fixed effect estimators to identify causal effects of treatment at different points in time has recently been challenged by a growing literature, for example by Goodman-Bacon (2021) and de Chaisemartin and D'Haultfœuille (2020). They show that two-way fixed effect methods provide a potentially biased estimate since the effect estimated represents a weighted average of different comparisons. Staggered treatment generates a grouping of units, according to treatment time. The estimated effect from two-way fixed effect methods is a weighted combination of different group effects. Importantly, if the treatment effect varies over time, these weights can become negative. There is a variety of alternative estimators available, addressing these difficulties by re-weighting and the exclusion of some comparisons, such as the one proposed Callaway and Sant'Anna (2020). This type of estimator is, however, not suitable for the "pseudo" DiD approach I use here. The approaches mentioned above need data on never-treated and treated units for all periods for each group. By definition of the treatment variable that exploits a cohort design, this is not available in my setting. The definition of access to the pill after a cutoff, i.e. access for all cohorts born after a specific year, mechanically truncates the data to only cover certain birth cohorts for the groups. In my heterogeneity analysis (Section 4.2), I will however identify effects separately for each treatment time.

Since the CES-D scale consists of eight different items, I also present results for each item separately. Figure 1 plots coefficients with standard error bands. The point estimates for legal access are all close to zero. Point estimates for consent access are in line with what one would expect: consent access is associated with a lower probability to report positive feelings and a higher probability to report negative feelings. The strongest effects come from the items "feeling depressed" and "feeling sad".

Figure 1: Effect of access to the pill on the specific items of the CES-D Score



Note: Coefficients of legal and consent access to the pill from eight estimations, each with one of the CES-D score items as the dependent variable. Includes state and year of birth fixed effects, controls for abortion access, childhood depression, and age. Standard errors are clustered at the state level.

4.1 Robustness

4.1.1 Alternative channels

So far, I have motivated the results above as a direct effect of hormones contained in the contraceptive pill on mental health. There are however two alternative channels that are related to the availability of the pill and potentially affect mental health. The first channel concerns the fact that during the time when access to the pill was granted, other freedoms related to the laws lowering age of majority may have adversely affected mental health which are not necessarily related to the pill. While general time trends are accounted for by the addition of state and year of birth fixed effects, as well as in linear and quadratic state-time trends, one cannot exclude the role of other factors related to the changes in age of majority. The general lowering of the age of majority, for example, allowed voting but also other freedoms that are difficult to control for.[12] A second important channel concerns all other behavioral changes that were triggered by access to the pill that might have affected mental health. By enabling women to take control over their own fertility, the pill has potentially shifted entire life trajectories. The effect that I identify might thus not measure an effect of the hormones contained in the pill but could be a result of the changed life patterns of women. I investigate both these alternative channels below.

I first analyze the role of other factors affecting mental health that are caused by the same reforms also affecting access to the pill. For this, I repeat my main analysis for a sample of men from the same birth cohort that was equally exposed to laws enabling access to the pill and access to other freedoms.[13] If the freedoms associated with the laws lowering the age of majority had a general mental health effect, one should see mental health effects for men. Table 6 below shows that there are no effects of the law reforms on self-reported mental health for men. Neither legal nor consent access to the pill or abortion affect self-reported mental health in a meaningful way.

As a next step, I investigate whether the mental health effect measured operates through the pill changing life trajectories. Changes in trajectories have likely occurred both in family formation but also in professional life. I first investigate the effect of the pill on fertility and marriage outcomes and relate these to mental health. Then, I investigate the role of stress at work, related to the potential change in career paths. Choosing more ambitious careers which is one consequence of access to the pill identified by Goldin and Katz (2002) could come with more stressful jobs. Descriptives for variables measuring family formation as well as stress at work can be found in the appendix in Table A7. Table 7 shows the effect of access to the pill on age at marriage, a dummy for ever being married, age at first child, and a dummy for ever having children. Interestingly, legal access seems to be more important than consent access here. Legal access to the pill

[12] Other trends not directly related to the lowering of age majority such as, for example, the women's rights movement, should not bias the results given the sharp time and regional variation in access.

[13] Table A6 shows descriptive statistics of the main variables for males.

Table 6: Effect of pill access during adolescence (age 14-21) on mental health, males

	CES-D	> critical threshold
	(1)	(2)
Fract. years legal access pill	-0.145*	-0.034
	[0.182]	[0.044]
Fract. years consent access pill	0.007	-0.009
	[0.284]	[0.065]
Fract. years legal access abortion	0.155	0.022
	[0.330]	[0.067]
Fract. years consent access abortion	-0.194	-0.023
	[0.441]	[0.076]
R-squared	0.092	0.064
N	5,788	5,788
Linear time trends	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects, as well as controls for age, being black and childhood depression.

increases age at marriage, and reduces the probability of ever getting married. Consent access does not have an effect on the two measures. These effects are in line with the previous literature. Both types of access are not significantly related to fertility outcomes, measured by age at first child and an indicator for ever having children, except for a marginally significant increase in the probability to have children. These results confirm the lack of a large average fertility shift as documented by Myers (2017).

Building on this, I investigate whether these marriage and fertility outcomes are important for mental health. If this was the case, they might explain part of the effect of the pill on mental health that I identify. Table 8 shows that while the age at marriage and ever having children is not related to mental health, both, ever being married and the age at first child are positively related to mental health. Since the pill increased age at first child, this channel works against the negative mental health channel of the pill and would thus lead to a downward bias of the effect. More problematic is the positive and large effect of ever being married. If the pill reduced the probability of being married and being married has a positive effect on mental health this might contribute to the negative mental health effect that I identify. I, therefore, repeat my main analysis but drop all individuals who have never been married to see whether they drive the main results in Table 5. Table 9, however, shows results very similar to the main results, that are slightly larger and more precisely estimated. Therefore, a negative mental health

Table 7: Effect of pill access during adolescence (age 14-21) on family formation

	Age at marriage (1)	Ever married (2)	Age at first child (3)	Ever children (4)
Fract. years legal access pill	3.198***	-0.059**	1.341	0.076*
	[0.536]	[0.024]	[0.809]	[0.041]
Fract. years consent access pill	-0.241	0.003	-0.047	0.049
	[0.814]	[0.043]	[1.134]	[0.054]
Fract. years legal access abortion	0.586	0.001	0.784	0.028
	[0.983]	[0.058]	[1.574]	[0.038]
Fract. years consent access abortion	1.330	0.060	0.387	0.015
	[1.404]	[0.056]	[1.255]	[0.086]
R-squared	0.128	0.041	0.066	0.037
N	5,495	6,346	5,518	6,348
Linear time trends	*Yes*	*Yes*	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects.

effect through never being married seems not to drive my results.

Lastly, I investigate whether there exist adverse mental health effects through more stressful career paths. This would mean that the negative mental health effects I find are not driven by hormones but by the reforms affecting changes in career paths. For this, I investigate the effect of access to the pill on two measures of stress at the job. First, I use the amount of time pressure that individuals report, and second I use the reported stress level at work. Table 10 shows that access to the pill is not related to a meaningful and significant increase in reported stress at work. If at all, consent access seems to slightly lower the amount of stress at work. This suggests that potential shifts in career paths through the pill seemed not to have resulted in more stressful jobs.

Table 8: Effect of family formation on mental health

	CES-D			
	(1)	(2)	(3)	(4)
Age at marriage	-0.009			
	[0.006]			
Ever married		-0.325***		
		[0.092]		
Age at first child			-0.040***	
			[0.004]	
Ever children				-0.084
				[0.075]
R-squared	0.072	0.076	0.093	0.075
N	5,495	6,346	5,518	6,348
Linear time trends	*Yes*	*Yes*	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects, as well as controls for age, being black and childhood depression.

Table 9: Effect of pill access during adolescence (age 14-21) on mental health, excluding never married individuals

	CES-D	> critical threshold
	(1)	(2)
Fract. years legal access pill	0.062	-0.008
	[0.173]	[0.029]
Fract. years consent access pill	0.500***	0.086**
	[0.179]	[0.035]
Fract. years legal access abortion	-0.682*	-0.115
	[0.342]	[0.074]
Fract. years consent access abortion	-0.907**	-0.099
	[0.396]	[0.071]
R-squared	0.076	0.056
N	7,457	7,457
Linear time trends	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects, as well as controls for age, being black and childhood depression.

Table 10: Effect of pill access during adolescence (age 14-21) on measures of stress at work

	Time pressure at work (1)	Mean stress level job (2)
Fract. years legal access pill	0.121	- 0.083
	[0.134]	[0.089]
Fract. years consent access pill	0.035	-0.200*
	[0.199]	[0.108]
Fract. years legal access abortion	-0.670***	0.047
	[0.235]	[0.151]
Fract. years consent access abortion	-0.379	0.211
	[0.228]	[0.150]
R-squared	0.084	0.038
N	2,411	6,056
Linear time trends	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects.

4.1.2 Replication using PSID

As an additional robustness test, I use data from the Panel Study of Income Dynamics (PSID) to replicate my findings from the HRS. The PSID is a longitudinal survey that started in 1968 and covers a nationally representative sample of around 18,000 Americans. It is less suited than the HRS for the general analysis due to the small sample size for the cohort of interest, essentially limiting any type of heterogeneity analysis. Again, I identify a potential long-term cost of the contraceptive pill by estimating the effect of differential exposure to legal and consent pill access on individuals' later life mental health. In the PSID, mental health is measured with the short version of the Kessler scale. The Kessler scale is a non-specific distress scale developed to predict psychiatric disorders (Kessler et al., 2003). The short version consists of six items: nervousness, hopelessness, restlessness, sadness, worthlessness, and the feeling that everything is an effort. The score ranges from 0 to 24, and a higher score indicates worse mental health. Since its development, it has been widely applied and validated, in psychology research but also in economics.[14] In the PSID, the Kessler scale is collected every other year from 2001 until 2019, except for 2005. Given the lower number of observations in this data set, I construct the mental health measure by using the average of the Kessler scale for each individual from the entries for all years. The average Kessler score for my sample is 3.18. Table 11 below shows that also in the PSID sample, access to the contraceptive pill is associated with worse mental health later in life. Legal access results in 0.51 higher scores on the Kessler scale but the effect is statistically insignificant. This is equivalent to 16 percent of the mean. Consent access is associated with a significant 0.48 higher Kessler scale which is an increase of 15 percent of the mean. Again, childhood experience of mental illness is an important predictor of later life mental health.

[14]The Kessler scale has been shown to be highly correlated to other common measures of mental health problems, Patel et al. (2008). Johnston et al. (2013) use the Kessler scale to study the persistence of mental health issues over different generations. Similar to this paper, Chatterji et al. (2007) use the Kessler scale to link mental health to labor market outcomes. They find that high distress as reflected by the Kessler scale is related to a lower probability of being employed and increased work absences.

Table 11: Effect of pill access during adolescence (age 14-21) on mental health, PSID

	Psychological Distress Scale (Kessler)
	(1)
Fract. years legal access pill	0.512
	[0.268]
Fract. years consent access pill	0.477**
	[0.207]
Childhood depression	3.657***
	[0.600]
Age	-0.066
	[0.041]
Black	0.585***
	[0.210]
N	2,485
R-squared	0.109
Access to abortion controls	Yes

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects. Controls for abortion access are included.

4.2 Heterogeneity Analysis

In this subsection, I take a closer look at heterogeneity in the effect of the pill on mental health. I begin with heterogeneity according to three time dimensions: first, I focus on differences according to age at access, second, on differences according to age at measurement of the mental health variable, and third, on differences according to the relative timing of laws enabling access to the pill. Then, I also investigate the role of genetic predisposition for mental health problems for the effect of the pill on mental health.

Given that the duration of exposure to access to the pill varied substantially (as shown in Table 2), I first investigate whether the identified effect varies according to the specific age at access. For this, I adopt an alternative specification of the pill exposure measure. Instead of using the fraction of years exposed, I use a dummy for whether or not a woman was exposed to the pill between ages 14 and 21 and interact it with age at access.[15] Results in Table 12 show that the exposure variable masks substantial heterogeneity in the effects of consent access to the pill according to age at access. Consent access at age 14 is associated with a 0.39 higher CES-D score, while access at 18 is associated with a 0.01 higher CES-D score. The decrease in the effect sizes with age at access points toward the fact that the effect of the pill is particularly large when young women gain access during the teenage years at which mental health is extremely malleable.

In addition to the differences according to age at access, I also explore whether the effect of the pill varies according to when the CES-D score was measured. A priori, one would expect, that effects should be strongest when mental health is measured more closely to the age where access to the pill was granted. The size of the effect at later ages then depends on the persistence of the mental health shock of the pill. For this part of the analysis, I construct measures of the CES-D at three different points in time: between age 36 and 45, between age 46 and 55, and between age 56 and 65. Given the age restriction of the HRS, earlier measurements are not available. Table 13 shows that the effects are actually strongest for mental health measured between age 36 and 45. These effects are however also estimated less precisely, potentially due to the small sample at that age. The effects then reduce substantially for mental health measured between ages 46 and 55 and reduce slightly for mental health measured between ages 56 and 65.

The last source of time heterogeneity that I consider is heterogeneity related to the timing of lowered barriers to pill usage. As discussed earlier, none of the recently proposed estimators accounting for heterogeneity in treatment timing is suitable given my pseudo-DiD approach. To nevertheless shed some light on the role of treatment effect heterogeneity, I instead estimate the main equation (1) separately for different groups. I define groups by the first birth cohort that received consent access before age 21 in a given state. I thereby only compare states which have the same treatment timing. This grouping is

[15] This is equivalent to using the exposure variable as a factor variable, allowing each exposure fraction to have a different slope.

Table 12: Effect of pill access on mental health, dummies interacted with age at exposure

	CES-D (1)	> critical threshold (2)
Legal access pill	0.061	-0.001
	[0.635]	[0.106]
Legal access pill x age at access	0.001	-0.000
	[0.004]	[0.006]
Consent access pill	1.703**	0.322**
	[0.665]	[0.126]
Consent access pill x age at access	-0.094**	-0.018***
	[0.035]	[0.007]
R-squared	0.078	0.058
N	7,905	7,905
Linear time trends	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects and controls for abortion, childhood depression, age and being black. Also includes interactions between age at access to abortion and abortion access.

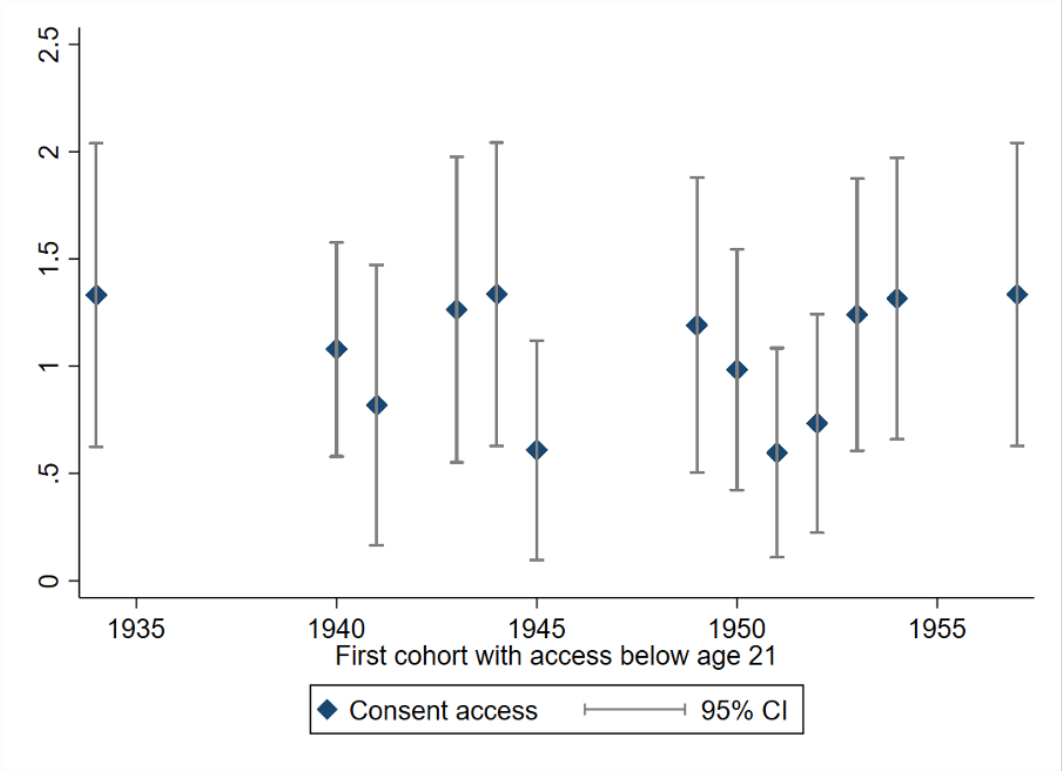
Table 13: Effect of pill access on mental health, for different points of measurement

	CES-D		
	age 36-45 (1)	age 46-55 (2)	age 56-65 (3)
Fract. years legal access pill	4.119	0.380***	0.171
	[2.514]	[0.135]	[0.121]
Fract. years consent access pill	2.373*	0.453**	0.413**
	[1.275]	[0.182]	[0.175]
R-squared	0.477	0.109	0.102
N	286	5,299	7,852
Linear time trends	*Yes*	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects and controls for abortion, childhood depression, age and being black.

equivalent to the one considered in the alternative estimators mentioned above. That also means that here the comparison boils down to a pure across-birth cohort comparison, instead of the comparison before that used variation both across birth cohorts and across countries. Figure 4.2 shows the results from this exercise. It shows that the effect is actually not constant, but ranges from 0.60 to 1.33 more items on the CES-D score. None of the effects are negative, pointing to the fact that my previous estimates only mask heterogeneity in the size of the negative mental health effect of the pill across time.

Figure 2: Effect of consent access to the pill on mental health according to timing of law changes



Note: Plots coefficients from separate estimations of equation (1) for different groups. Groups are defined by the first birth cohort that had access below age 21. Includes state and year of birth fixed effects and controls for abortion, childhood depression, age and being black.

Another important dimension of heterogeneity to consider, especially for the policy relevance of my results, is whether or not the mental health effect of the pill varies for individuals with different predispositions for mental health illness. This can help to understand whether the pill raises mental health problems for everyone equally or whether it is more likely to "push" individuals with a higher predisposition to develop a mental health problem beyond a threshold. One way to measure predisposition toward certain illnesses and general biological characteristics (also referred to as phenotypes) are so-called polygenic scores. Polygenic scores (PGS) are linear indexes summing up genetic

variants. These scores are obtained from genome-wide association studies in which DNA material is scanned and reflect the propensity towards a specific phenotype.
Polygenic scores have mostly been used in the economic literature to investigate the role of genes, different environments, and their interplay, for example, to better understand the effect of education on health (Barcellos et al., 2021), or returns to education (Papageorge and Thom, 2020). Barban et al. (2021) use polygenic scores to investigate the role of genes for a range of fertility outcomes in interaction with variation in access to the pill in the UK.

For a subset of the HRS sample, polygenic scores were collected between 2006 and 2012. There are two polygenic scores related to mental health outcomes: one for general depressive symptoms and a second for major depressive disorder. The higher the score, the higher is the propensity for the respective phenotype. Domingue et al. (2017) use these scores from the HRS to study heterogeneity in mental health response to the death of a spouse. They find that higher polygenic scores were associated with a larger increase in the CES-D score after the death of a spouse.

I re-estimate my main model from equation (1) for the effect of the pill on the CES-D score adding an interaction between access to the pill and the polygenic scores. Column (1) of Table 14 adds the polygenic score for depressive symptoms and column (2) adds the score for major depressive disorder. Both columns show a similar pattern: the mental health effect of access to the pill is stronger the higher the polygenic score, so stronger for individuals with a higher genetic predisposition for mental illness. The increase in the effect is larger for those with a high polygenic score for major depressive disorder compared to those with a higher polygenic score for depressive symptoms. This points toward the potential role of the pill as a trigger for mental illness for those already at genetic risk.

Table 14: Effect of pill access on mental health, interacted with polygenic scores (PGS)

	CES-D	
	(1)	(2)
Fract. years legal access pill	0.162	0.183
	[0.178]	[0.181]
Fract. years consent access pill	0.591**	0.582*
	[0.287]	[0.291]
Fract. years legal access pill x PGS depressive symptoms	0.102*	
	[0.053]	
Fract. years consent access pill x PGS depressive symptoms	0.385**	
	[0.172]	
Fract. years legal access pill x PGS major depressive disorder		0.165***
		[0.061]
Fract. years consent access pill x PGS major depressive disorder		0.188*
		[0.111]
R-squared	0.072	0.072
N	5,491	5,491
Linear time trends	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects and controls for abortion, childhood depression, age and being black.

5 Education and Labor Market Outcomes

A large part of the "power of the pill" literature has documented positive labor market effects of access to the pill. These studies argue that the improved control over fertility due to the pill enabled women to make larger investments in education. Other potential effects of the pill besides fertility control such as mental health have not been taken into account. From a theoretical perspective, mental health can be thought of as a (hidden) cost of fertility control. The effect of the pill on labor market outcomes then operates through two channels: the first one is the above-explained fertility control channel. This includes the aversion but also the timing of childbirth. As has been shown in the literature, this channel has positive effects on education, labor force participation, and wages. The second channel is mental health. As several empirical studies demonstrated, poor mental health has detrimental effects on education and labor market outcomes, ranging from high school completion to labor force participation and earnings. The two channels of the pill are therefore opposed. Mental health thus might counteract the positive pill effect on labor market outcomes through the fertility channel. The total effect of the pill on education and labor market outcomes thus depends on the relative size of both these channels.

I investigate this by estimating the pill effect on measures of education, labor force participation, limitations at work, and disability periods but now taking mental health explicitly into account. In a first step, I estimate the effect of the pill on years of education, college attendance, and labor force participation. This is the set-up of analyses from the "power of the pill" literature and is an estimate of the combined effect of both channels. In a second step, I explicitly add mental health as a control variable. This should net out the mental health channel from the pill coefficient and leave this to represent the fertility control effect. Since the composite effect estimated in the first step had implicitly subtracted the mental health cost from the pill coefficient, this coefficient should now become larger as it only reflects the fertility control effect. The inclusion of the CES-D score, however, also introduces a bad control problem into the estimation. Section 4 has shown that the pill has a negative impact on mental health and thus the CES-D score is an outcome of the treatment. In a third step, I therefore instrument the CES-D score with childhood mental health. Since childhood depression is not affected by later access to the pill, this mitigates the bad control problem.
In addition to these positive labor market outcomes, it seems also reasonable to investigate indicators of reduced labor market productivity. Given that the HRS contains explicit information about reduced labor market productivity, with measures on limitations at work and disability periods, I repeat the same exercise for these two outcomes. This helps to understand whether the mental health cost of the pill is indeed detrimental for labor market productivity. If this is the case, one would expect a positive relationship between access to the pill and reported limitations and disability periods. If the mental health effect of the pill is driving this, one should expect this positive relationship to weaken when explicit controls for mental health are added.

Table 15 shows the effect of the pill on years of education and college attendance, and Table 16 on labor force participation. In column (1) of both tables, I omit the current mental health measure, and in column (2) I add the CES-D score used before. In column (3), I also add the CES-D score but instrument it with childhood depression. I base my inference on how the coefficients of the pill variables change.

Table 15 shows that when not taking mental health into account, legal access to the pill increases the years of education by 0.6 years and consent access by 0.2 years, but the latter effect is statistically insignificant. Legal access to the pill also increases the probability of college attendance by 12 percentage points and consent access by 2 percentage points, but the latter effect is again statistically insignificant. Interestingly, Table 16 shows that the effects on labor market participation are negative, but not significant. The results for education are in accordance with recent results by Lindo et al. (2020) who also use the coding by Myers (2017) for the HRS data.

When adding the CES-D score as a measure for mental health in column (2) of both tables, all coefficients change in a similar way and become (more) positive as hypothesized above. The coefficients almost double for consent access. For being in the labor force, they are very small but no longer negative. I test for the equality of coefficients with a Wald test from a joint estimation of the two specifications using a seemingly unrelated regression. I always compare the coefficients of legal and consent access from an estimation without mental health measures to coefficients from an estimation with the CES-D score added. For legal access, none of the coefficients are statistically different when including the CES-D score. For consent access, all coefficients are significantly different when including mental health measures. When instrumenting the CES-D score with childhood depression in column (3), coefficients for years of education and college are also larger than when ignoring mental health but not as large as when just adding the CES-D score without instrumenting. For labor market participation, coefficients are the largest in the IV specification.

These results suggest that the fertility control effects of consent access to the pill are slightly larger than measured when ignoring the mental health effect. This means that estimates from the literature were a combination of both these effects, fertility control, and mental health effects. While the fertility effect of the pill increases education and labor force participation, the mental health effect reduces these. This is also reflected by the negative and highly significant coefficient of the CES-D score on education measures and labor force participation. The composite effect in the literature thereby slightly underestimates the fertility effect, since it includes the effect of mental health costs. Given that I only find a significant difference when adding mental health for the insignificant effect of consent access, I turn to alternative outcomes related to labor market productivity.

I use the share of interviews in which an individual reports to be limited at work due to health problems and the number of reported disability periods as outcomes. In column (1) of Table 17, I omit the current mental health measure and add it in column (2). In column (3), I instrument for the CES-D score. Column (1) shows that access to the pill is associated with more frequent reporting of limitations at work. Legal access to the pill is associated with 7.4 percentage points more interviews in which limitations at work are reported. Consent access is associated with an 11 percentage points higher share of interviews in which limitations at work are reported. This reflects an increase of around a third of the mean. Legal and consent access also affect the number of reported disability periods. Legal access increases them by 0.22 and consent access by 0.32. This increase is very large compared to the mean number of disability periods of 0.37. When including the measure for mental health in column (2), the effect of the pill on limitations at work becomes slightly smaller. It reduces to 6.5 percentage points for legal access and to 7.4 percentage points for consent access. The effects on reported disability periods also decrease when mental health is added, by 4 percent and 18 percent respectively. The Wald tests show that the coefficients for consent access are significantly different when adding mental health for both outcomes. Column (3) shows an even weaker effect of access to the pill when instrumenting the CES-D score instead of just adding it, for both outcomes. These results suggest that part of the increase in the reporting of limitations and the number of disability periods due to the pill can be attributed to the mental health effects of the pill. It thereby supports the hypothesis that the pill has affected productivity negatively due to its mental health effects.

Table 15: Effect of pill access during adolescence (age 14-21) on education outcomes

	(1) w/o mental health	(2) w. mental health	(3) IV
Panel A: Years of education			
Fract. years legal access pill	0.620**	0.667**	0.647**
	[0.300]	[0.302]	[0.296]
Fract. years consent access pill	0.218	0.384	0.315
	[0.484]	[0.499]	[0.493]
Fract. years legal access abortion	0.885*	0.623	0.731
	[0.485]	[0.462]	[0.478]
Fract. years consent access abortion	1.136**	0.882*	0.987*
	[0.537]	[0.504]	[0.505]
CES-D		-0.223***	-0.130
		[0.018]	[0.095]
Wald statistic legal (p-value)	1.431 (0.232)		
Wald statistic consent (p-value)	6.745 (0.009)		
First-stage coefficient (F-statistic)			1.778 (48.93)
R-squared	0.121	0.152	
N		6,322	
Panel B: College			
Fract. years legal access pill	0.117**	0.125**	0.118**
	[0.057]	[0.058]	[0.056]
Fract. years consent access pill	0.015	0.043	0.017
	[0.083]	[0.083]	[0.083]
Fract. years legal access abortion	0.076	0.037	0.074
	[0.117]	[0.113]	[0.116]
Fract. years consent access abortion	0.213*	0.172	0.210*
	[0.115]	[0.113]	[0.114]
CES-D		-0.036***	-0.002
		[0.003]	[0.016]
Wald statistic legal (p-value)	1.711 (0.191)		
Wald statistic consent (p-value)	7.030 (0.008)		
First-stage coefficient (F-statistic)			1.783 (47.96)
R-squared	0.099	0.121	
N		6,347	
Linear time trends	*Yes*	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects and controls for being black, state equal pay laws, and state acts prohibiting racial discrimination in employment, and laws allowing no-fault divorce.

Table 16: Effect of pill access during adolescence (age 14-21) labor force participation

	(1) w/o mental health	(2) w. mental health	(3) IV
Labor force participation			
Fract. years legal access pill	-0.029	-0.021	0.010
	[0.028]	[0.029]	[0.032]
Fract. years consent access pill	-0.011	0.013	0.051
	[0.045]	[0.046]	[0.052]
Fract. years legal access abortion	0.018	-0.018	-0.073
	[0.066]	[0.062]	[0.058]
Fract. years consent access abortion	-0.005	-0.041	-0.097*
	[0.064]	[0.059]	[0.056]
CES-D		-0.033**	-0.084***
		[0.003]	[0.018]
Wald statistic legal (p-value)	1.607 (0.205)		
Wald statistic consent (p-value)	6.547 (0.011)		
First-stage coefficient (F-statistic)			1.783 (43.53)
R-squared	0.044	0.075	
N		6,346	
Linear time trends	*Yes*	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects and controls for being black, state equal pay laws, and state acts prohibiting racial discrimination in employment, and laws allowing no-fault divorce.

Table 17: Effect of pill access during adolescence (age 14-21) on limitations at work and disability periods

	(1) w/o mental health	(2) w. mental health	(3) IV
Panel A: Health problems limit work			
Fract. years legal access pill	0.074**	0.065**	0.054*
	[0.029]	[0.026]	[0.029]
Fract. years consent access pill	0.110**	0.074**	0.027
	[0.043]	[0.037]	[0.036]
Fract. years legal access abortion	-0.168**	-0.124	-0.067
	[0.063]	[0.055]	[0.054]
Fract. years consent access abortion	-0.134**	-0.076	-0.001
	[0.063]	[0.055]	[0.054]
CES-D		0.064***	0.148***
		[0.002]	[0.012]
Wald statistic legal (p-value)	0.592 (0.442)		
Wald statistic consent (p-value)	7.530 (0.006)		
First-stage coefficient (F-statistic)			1.891 (53.44)
R-squared	0.045	0.194	
N		7,900	
Panel B: Number of disability periods			
Fract. years legal access pill	0.219***	0.202***	0.192***
	[0.073]	[0.065]	[0.063]
Fract. years consent access pill	0.320**	0.252**	0.212*
	[0.131]	[0.117]	[0.116]
Fract. years legal access abortion	0.589***	-0.508***	-0.461***
	[0.179]	[0.164]	[0.157]
Fract. years consent access abortion	-0.253	-0.141	-0.075
	[0.154]	[0.131]	[0.129]
CES-D		0.123***	0.195***
		[0.006]	[0.028]
Wald statistic legal (p-value)	0.581 (0.446)		
Wald statistic consent (p-value)	7.316 (0.007)		
First-stage coefficient (F-statistic)			1.890 (53.64)
R-squared	0.061	0.153	
N		7,905	
Linear time trends	*Yes*	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects and controls for being black, state equal pay laws, and state acts prohibiting racial discrimination in employment, and laws allowing no-fault divorce.

6 Conclusion

In this paper, I investigate the long-term mental health costs of a health technology considered to be one of the most powerful of the 20th century: the contraceptive pill. While previous economic literature has mainly focused on the liberalizing labor market and education effects, I am the first to add mental health as an outcome dimension. This is motivated by evidence from medical studies suggesting a link between hormonal contraceptive use and depression. I document large negative effects of access to the pill on mental health. These results are robust to different specifications and definitions of access. Using males equally exposed to law changes governing access and considering measures of family formation and work stress, I argue against other channels driving my results. Replicating my analysis with another data set, the Panel Study of Income Dynamics, shows similar results.

After providing evidence for a link between access to the pill and mental health, I investigate the importance of this link for the effect of the pill on education and labor market outcomes. Previous literature has established a positive effect of the pill on education and labor market outcomes due to its fertility control function. Adding current mental health measures when estimating the effect of the pill shows a small increase in the effect of the pill on education and labor force participation. This suggests that previous estimates have been a combination of the positive fertility control effect of the pill and the negative mental health effect of the pill. They have thus slightly underestimated the role of fertility control on education and labor market outcomes. Access to the pill is also related to a higher probability to report limitations at work and to a higher number of disability periods. Adding a measure of current mental health here weakens the link between the pill and the probability to report limitations and the number of disability periods suggesting that part of the negative effect of the pill here might be driven by mental health issues.

My analysis suffers from one large limitation. I cannot establish an immediate effect of access to the pill on mental health given that my data only measures mental health at a relatively late point in time. Identifying an immediate mental health effect during women's twenties and thirties would be valuable to further support my reasoning for a strong mental health effect of the pill and could be an important avenue for future research. However, given that data on mental health from the 1960s and 1970s in the US is difficult to obtain, such future research would probably need to rely on alternative identification strategies and settings.

From a policy perspective, my results can inform decision-making processes in the area of reproductive health. It seems important to carefully weigh the potential mental health effects for the prescription of hormonal contraceptives against their fertility control effect and to compare these to non-hormonal contraceptives. This is particularly important in light of the large changes in contraceptive behavior recently, as represented in Figure A1.

Pill usage shares have dropped by about one-third in the last fifteen years. Part of that seems connected to the awareness of side effects that has been increasing over time, as for example reflected by an increase in Google searches for the term pill side effects (see Figure A2). My results thereby support the reasoning for more investment into research on non-hormonal contraceptive methods and research on male contraceptives. The role of contraceptives might potentially become even more relevant in the future given the increasing barriers to abortion in the US. For policies related to mental health and the labor market, my findings show a substantial productivity cost of mental health effects of the pill. This is important to acknowledge, especially in light of gender differences in the prevalence of mental health problems and in their effect on labor market productivity, which may potentially accentuate gender wage gaps.

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Appendix

Table A1: Legal and consent access to abortion - law changes coded by Myers (2017)

Age	>21	18-20	<18		>21	18-20	<18
Alabama	1973	1973	1973	Montana	1973	1973	1973
Alaska	1970	1970	1977	Nebraska	1973	1973	1975
Arizona	1973	1973		Nevada	1973	1973	1976
Arkansas	1973	1973	1976	New Hampshire	1973	1973	1973
California	1969	1971	1971	New Jersey	1973	1973	1973
Colorado	1973	1973	1975	New Mexico	1973	1973	
Connecticut	1973	1973		New York	1970	1970	1970
Delaware	1973	1973	1977	North Carolina	1973	1973	1975
District of Columbia	1971	1973	1973	North Dakota	1974	1974	1979
Florida	1973	1973	1975	Ohio	1973	1973	1973
Georgia	1973	1973		Oklahoma	1973	1973	
Hawaii	1970	1970		Oregon	1973	1973	1973
Idaho	1973	1973		Pennsylvania	1973	1973	1973
Illinois	1973	1973	1973	Rhode Island	1973	1973	
Indiana	1973	1973	1975	SouthCarolina	1973	1974	1974
Iowa	1973	1973	1976	SouthDakota	1973	1973	
Kansas	1973	1973	1973	Tennessee	1973	1973	1979
Kentucky	1973	1973	1974	Texas	1973	1973	
Louisiana	1973	1973	1976	Utah	1973	1973	
Maine	1973	1973	1979	Vermont	1973	1973	
Maryland	1973	1973	1973	Virginia	1973	1973	
Massachusetts	1973	1974	1976	Washington	1970	1970	1975
Michigan	1973	1973	1977	West Virginia	1973		
Minnesota	1973	1973	1973	Wisconsin	1973		
Mississippi	1973	1973	1973	Wyoming	1973		
Missouri	1973	1974	1975				

Note: The table shows years in which states enabled legal and consent access to abortion for a given age group. The coding of these laws is taken from Myers (2017).

Table A2: Effect of pill access during adolescence (age 14-21) on mental health, probit

	> critical threshold Average Marginal Effects
Fract. years legal access pill	-0.003
	[0.029]
Fract. years consent access pill	0.076*
	[0.039]
Fract. years legal access abortion	-0.087
	[0.071]
Fract. years consent access abortion	0.082
	[0.060]
N	7,905
Linear time trends	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects. Dummy for childhood depression and controls for age, abortion access, and being black are included in all columns.

Table A3: Effect of pill access during adolescence (age 14-21) on mental health, without time trends

	CES-D (1)	> critical threshold (2)
Fract. years legal access pill	0.042	-0.004
	[0.149]	[0.026]
Fract. years consent access pill	0.342**	0.049
	[0.152]	[0.032]
Fract. years legal access abortion	-0.213	-0.007
	[0.252]	[0.044]
Fract. years consent access abortion	-0.369	-0.015
	[0.225]	[0.040]
Childhood depression	1.893***	0.306***
	[0.136]	[0.027]
Age	0.016	0.003
	[0.026]	[0.004]
Black	0.488***	0.083***
	[0.066]	[0.013]
R-squared	0.072	0.051
N	7,905	7,905
Linear time trends	*No*	*No*
Quadratic time trends	*No*	*No*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects.

Table A4: Effect of pill access during adolescence (age 14-21) on mental health, with quadratic time trends

	CES-D	> critical threshold
	(1)	(2)
Fract. years legal access pill	-0.108	-0.021
	[0.324]	[0.055]
Fract. years consent access pill	0.498**	0.080*
	[0.211]	[0.043]
Fract. years legal access abortion	-0.696**	-0.089
	[0.337]	[0.071]
Fract. years consent access abortion	-0.687*	-0.070
	[0.397]	[0.075]
Childhood depression	1.873***	0.305**
	[0.136]	[0.028]
Age	0.011	0.002
	[0.027]	[0.002]
Black	0.485***	0.084***
	[0.063]	[0.013]
R-squared	0.083	0.063
N	7,905	7,905
Linear time trends	*Yes*	*Yes*
Quadratic time trends	*Yes*	*Yes*

Note: Standard errors in brackets, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Includes state and year of birth fixed effects.

Table A5: Descriptive statistics of CES-D items

	Mean	Std. Dev.
Much of the time during the past week		
You felt depressed	0.16	(0.37)
You felt that everything you did was an effort	0.25	(0.43)
Your sleep was restless	0.35	(0.48)
You felt lonely	0.17	(0.38)
You felt sad.	0.23	(0.42)
You could not get going	0.21	(0.41)
You were happy	0.84	(0.37)
You enjoyed life	0.90	(0.30)
N	7,905	

Note: Table shows averages of CES-D items. All items are answered as Yes/No. Standard deviations in parentheses.

Table A6: Descriptive statistics of main variables, males

	Mean	Std. Dev.
Mental health outcomes		
CES-D Score	1.32	(1.91)
CES-D critical threshold	0.18	(0.39)
Pseudo Pill access		
Fract. years pill legal (14-21)	0.41	(0.38)
Fract. years pill legal & consent (14-21)	0.19	(0.28)
Fract. years abortion legal (14-21)	0.04	(0.11)
Fract. years abortion legal & consent (14-21)	0.14	(0.24)
Control variables		
Childhood depression	0.02	(0.15)
Age at measurement	59.61	(1.31)
Black	0.19	(0.39)
N		5,788

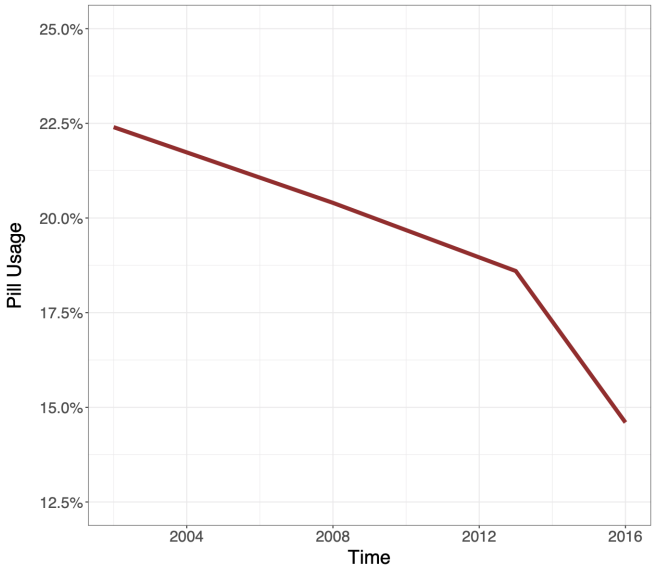
Note: Means and standard deviations (in parentheses). Sample restricted to men born between 1934 and 1958 with mental health information available

Table A7: Descriptive statistics of variables for channel analysis

	Mean	Std. Dev.
Family formation		
Age at marriage	22.96	(5.71)
Ever married	0.93	(0.26)
Age at first child	22.97	(5.46)
Ever children	0.87	(0.34)
Measures of stress at work		
Average time pressure	2.04	(0.90)
Average stress at work	2.74	(0.67)

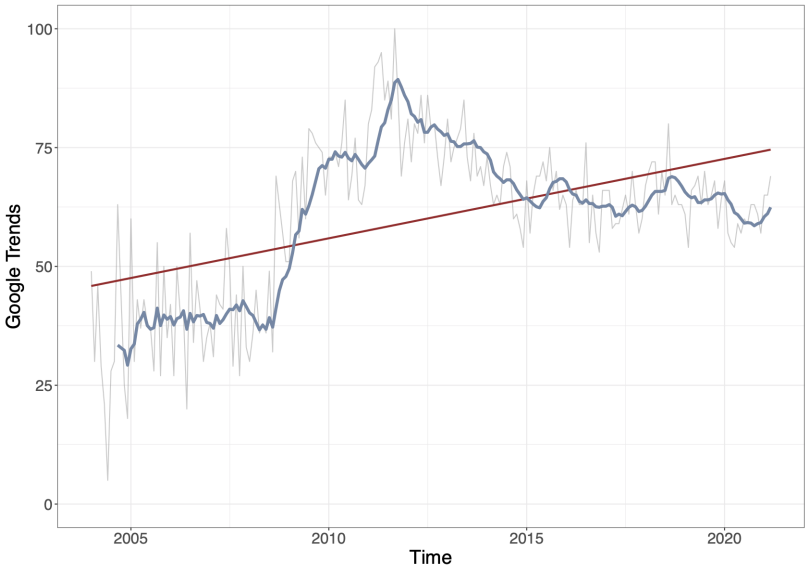
Note: Standard deviations in parentheses. Time pressure is measured on a scale agreeing to the sentence "whether respondent is under constant time pressure due to heavy workload" ranging from 1 (strongly disagree) to 4 (strongly agree). I average answers for each respondent over all waves available. This variable is only measured from waves 9-11. Stress on the job is measured on a scale agreeing to the sentence "current job involves much stress" ranging from 1 (strongly agree) to 4 (strongly disagree). I average answers for each respondent over all waves available and then revert the measure such that a higher value reflects more stress, with a value of 4 indicating a higher degree of agreeing to stress at work.

Figure A1: Share of current pill users among contraceptive users



Source: own configuration with data from Centers for Disease Control and Prevention, *Key Statistics from the National Survey of Family Growth* `https://www.cdc.gov/nchs/nsfg/key_statistics/c.htm#contraception`, accessed March 21, 2021

Figure A2: Google Trends: searches for term “pill side effects"



Source: own configuration from `https://trends.google.com/trends/`. This figure depicts google searches in relation to the highest point for between January 1st 2004 - March 1st 2021. A value of 100 reflects the highest popularity of searches for the term “pill side effects”.