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Moral Barriers to Birth Control Access: How the Pill Changed Dutch Women's Lives – When Religion Did Not Get in the Way

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Moral Barriers to Birth Control Access:
How the Pill Changed Dutch Women's Lives – When Religion Did Not Get in the Way*

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Abstract: We investigate how religious beliefs affected the take up of the birth control pill and impacted women's outcomes using the 1970 liberalization of oral contraceptives in the Netherlands. We first document a massive and immediate drop in fertility among minor women, aged 21 or younger, for whom access restrictions were most drastically lifted. We then evaluate how area level social norms – as proxied by votes for religiously opposed political parties – influenced pill adoption by examining its impact on female fertility control and human capital formation. We find that younger women who grew up in more liberal areas were much less likely to experience a birth or marriage as a minor, invested more in education, and ended up in wealthier households. Finally, we study the potential additional impact of supply side frictions stemming from the moral views of the gatekeepers to the new birth control technology. We show that a larger proportion of religiously opposed health professionals – GPs and pharmacists – around a woman at the time of liberalization cancels out the short- and long-run benefits from pill access.

Keywords: birth control, fertility, marriage, human capital, religion, the Netherlands

JEL codes: I18, J12, J13, Z12

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1. Introduction

Legal access to new contraceptive technology in the form of the birth control pill has had dramatic effects on women's lives. Goldin and Katz (2002) were the first to document the powerful impact of the pill on young college educated women's marital and educational outcomes in the US. Bailey (2006) followed by showing that the pill allowed *all* women to delay motherhood and increase their participation in the labor force. Other works show that the liberalization of the pill led to drops in birth rates (Guldi, 2008); increased the share of children with college-educated and non-divorced mothers in the long-run (Ananat and Hungerman, 2012); can account for part of the convergence of the gender gap in the 1980s and 1990s (Bailey, Hershbein and Miller, 2012); and allowed women to select into higher-paying occupations (Steingrimsdottir, 2016). However, there has always been substantial religious resistance to the liberalization and adoption of new contraceptive methods which might partially account for the non-universal take up of the pill in the United States. Bailey (2006) for example documents that a higher percentage of Catholic parish membership was associated with a significant delay in pill liberalization at the state level. Moreover, it is probable that, even conditional on having legal access to birth control technology, there may be relevant moral frictions that prevent women from accessing it. These could be due to a woman's – or her parents'/husband's – own beliefs, but also by those of the technology providers who are less likely to grant access to contraceptives because of their religious affiliation (Spivack, 1964; Rubin et al., 2006; Lawrence et al., 2010; Stulberg et al., 2011).

This paper studies how women's lives were affected by religious beliefs that influenced the take up of the birth control pill in the Netherlands, a context where legal access to the pill was liberalized after 1970. In doing so, we are the first to investigate the role of religion – from both a demand and a supply side perspective – as an important potential mechanism for differential effects of oral contraceptives on women.[1] We first examine how women's beliefs with regards to the pill – as proxied by area level votes on parties that were in favor of pill liberalization – affect women's outcomes: i.e. the demand side. We then study the role of religious beliefs of the 'gatekeepers' to

[1] Although we are the first to investigate the role of religion in the context of the effects of birth control technology on women's outcomes, other aspects of the relationship between religion and fertility were explored earlier in economics. For example, Beach and Hanlon (2019) show that cultural norms and religion played an important role in historical fertility transition in the UK. Bassi and Rasul (2017) find that papal visits in Brazil led to lower contraceptive use, to more unprotected intercourse and more births. Finally, Farina and Pathania (2020) show that papal visits in Italy led to a lower number of abortions, and in particular when the Pope mentions the topic in his speech.

the new technology, who might be able to annihilate any effect of the pill if they are unwilling to prescribe: i.e. the supply side.[2] Moreover, we are the first to comprehensively study the impact of improved oral contraceptive access outside of the U.S.[3] The Dutch setting is of particular interest because the pill was not only liberalized in 1970, but it was also included in national health insurance the following year and thus available at no cost almost simultaneously. This makes our context ideal for investigating a relatively pure, and potentially powerful, pill access policy effect.[4] Third, our administrative registry data allows us to observe the fertility decisions of all women in the Netherlands, along with information about their marriage, education, labor market, and (household) wealth outcomes up to four decades after access to the pill was liberalized. With these data we can explore both short- and long-run effects for cohorts who are precisely defined in terms of their age at the time of the policy change. Finally, we can produce (tentative) causal estimates of the impact of delaying a birth or marriage on a woman's human capital accumulation using the likelihood of pill take up as an instrument for improved fertility control.

We first show that the birth rate for women of childbearing age (15-50) fell by 25% in the five years following liberalization. More strikingly, the change in the teenage birth rate – the age group for whom oral contraceptive access was previously most restricted and for whom delaying fertility decisions may have had the largest long-term impact – dropped by almost 45% over the same time period (see **Figure 1**). The declining trend in fertility coincided with a large increase in the take up of the pill. The contraceptive pill quickly became the most important birth control technology in the Netherlands after its liberalization in 1970. The percentage of women buying oral contraceptives shot up quickly, and by 1975 more than 40% of women between the ages of 15 and 44 were using it (**Figure 2**). A simple first illustration of the large impact that pill

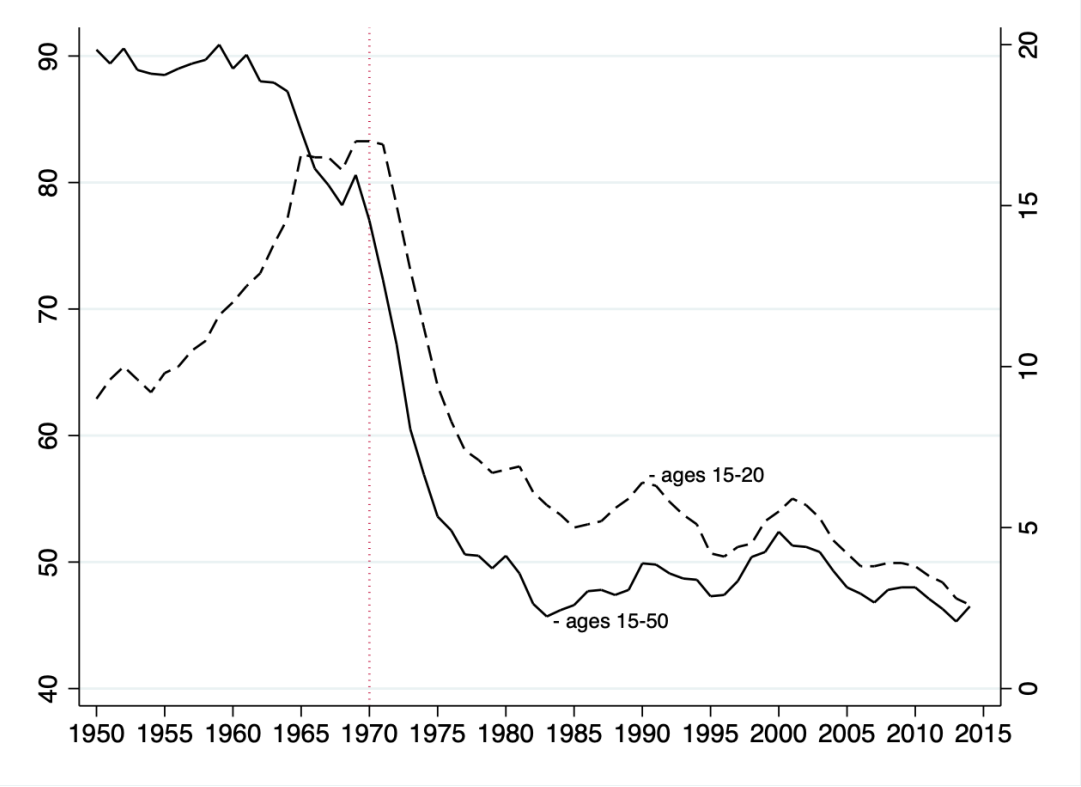
[2] Recent studies have examined the role of other provider beliefs on prescribing behavior and patient outcome including Schnell (2017) who finds that physician altruism influences opioid prescriptions and Alsan et al. (2018) who show that patients opt for more preventive care services when consulting with a doctor of the same race.

[3] Two exceptions are Gronqvist (2012) for Sweden and Rau, Sarzosa, and Urzúa (2017) for Chile who focus especially on how women's outcomes change as a response to changes in the costs of the birth control pill. Both studies find that the take up of the pill is highly sensitive to price changes.

[4] There have been recent discussion in the U.S. about whether the pill or abortion had a greater impact given that access to both was liberalized around the same time period (see Myers, 2017). In the Netherlands, induced abortion was only legalized in 1984. However, despite induced abortion being illegal, it was still allowed in cases to save the mother's life until 1966. In 1967 abortion teams were introduced at 11 clinics, with the power to determine whether a woman was eligible for an abortion. By 1972, women could ask these clinics for an abortion without having to prove or be found eligible on "medical grounds". While this change did not lead to an increase in the number of abortions, which remained low by international standards (see **Figure A1**), and especially, in comparison to the high oral contraceptive take up rates. However, given that the change in abortion access happened at about the same time as pill liberalization, we carefully consider the impact of these clinics and their location for our findings.

liberalization appears to have had on Dutch women's ability to better control their fertility decisions can be seen in an immediate drastic reduction in unplanned pregnancies.[5]

Figure 1. Birth rate and teenage birth rate, Netherlands 1950-2014



Notes: Number of births per 1000 women from 1950 – 2014 in the Netherlands, for women ages 15 to 50 years old (left axis) and women ages 15-20 years old (right axis). The vertical dotted red line marks 1970, the year when pill access was liberalized in the Netherlands. Source: CBS Statline, statline.cbs.nl

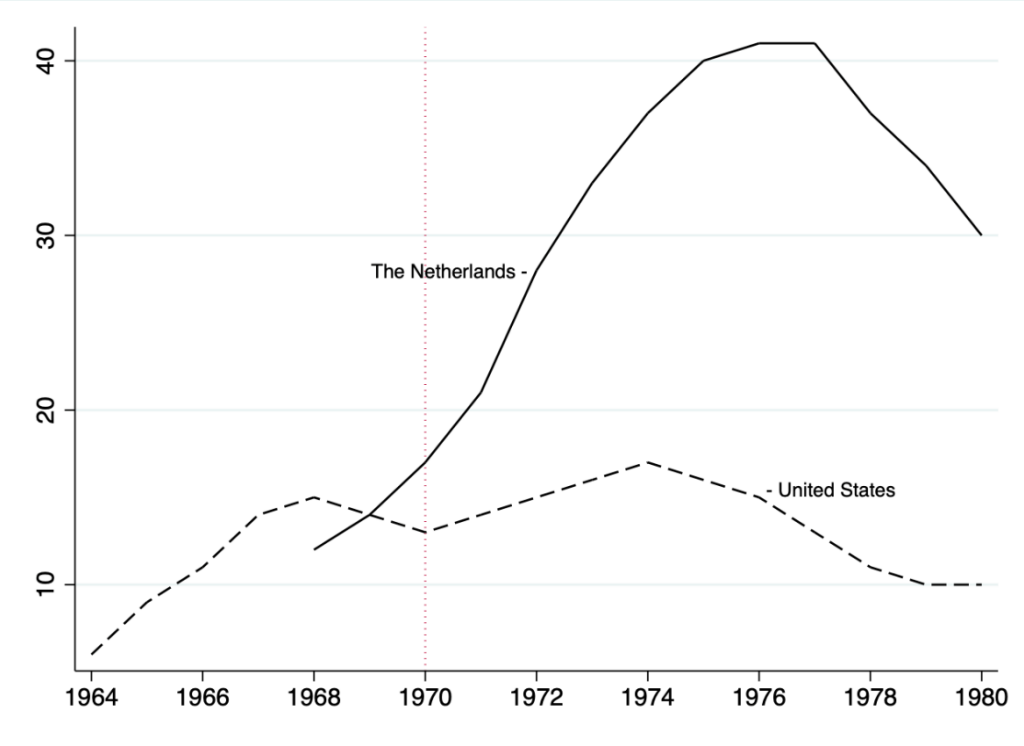
Our exploration of the effects of religion on the take up of birth control technology first focuses on the demand side of the pill. We use the vote share for political parties that were in favor of oral contraceptive liberalization to proxy for area level views with regards to the pill. In the Netherlands, the parties representing Orthodox Protestants were fiercely opposed to contraception, while nondenominational parties and those representing Catholics and Liberal Protestants were in

[5] Vennix (1990) reports on a survey that asks women whether their birth was planned or not. He shows that the difference for women giving birth just before and after the pill liberalization are stark: only 37% of women who gave birth just before the pill became universally available (1966-1970) report that their birth was clearly planned, whereas this is 69.5% for women giving birth from 1971-1975. Similarly, using numbers from a larger more recent family planning survey, we find that for women giving birth between 1966 and 1970, 27.8% reported that their child's birth was unplanned, whereas this was only 8.4% for women giving birth from 1971 to 1975 (**see Table A1**).

favor. Using survey evidence, we document how women voting for these Orthodox Protestant parties were about twice less likely to take up the pill than those voting for any other political party. We then compare outcomes for women from the same areas who gained legal access just before or after turning 21, the age before which pill use was categorically banned until 1970. We find that younger women who lived in an area with a 25 percent higher vote share in favor of liberalizing access to the pill were 28 percent less likely to become mothers as minors and that 14 percent fewer were married before the age of majority. These women were 8.5 percent more likely to complete any higher education degree and the probability that they finished the degrees that take the longest time – Medical Doctor (MD) and Juris Doctor (JD) – increased by over 70 percent. Surprisingly these increases in human capital investment translate into only modest increases in the probability of being economically active by their mid-50s or of being in higher paid jobs. As this may be due to a preference for being out of the labor force (very high among Dutch women in these cohorts), we turn to household wealth as a more objective measure of economic wellbeing later in life. Here, the long-run impact of the pill is clear, as the women who benefited most from improved legal access were significantly more likely to belong to households in the top quartile of the national wealth distribution by their early 60s.

We then investigate the importance of frictions from the supply side of the pill. In the Netherlands, women could only obtain the pill on a doctor's script at a pharmacy, and hence these health professionals were essentially functioning as "gatekeepers" to the pill. We use the 1971 Dutch Census to identify the religious affiliation of general practitioners and pharmacists in every Dutch municipality. We first use survey data to show that both Orthodox Protestants and Catholic gatekeepers were less likely to prescribe the pill in the Netherlands, and in particular to unmarried women. Both the vote share on parties that were in favor of pill liberalization and the fraction of Orthodox Protestant and Catholic doctors and pharmacists varies tremendously at the local level, showing liberal areas with many religiously opposed gatekeepers, and conservative areas with few religiously opposed gatekeepers. We show that in places where women had difficulty finding a gatekeeper who was not religiously opposed to the pill, legal access to the pill had no impact on births to minors, the probability of marrying as a minor, and long-term education and wealth outcomes. Hence, the anti-contraception beliefs of gatekeepers effectively nullified the effects of improved legal access to contraception in these areas.

Figure 2. Percentage of women buying oral contraceptives: Netherlands and US



Notes: Estimated number of oral contraceptives bought in pharmacies each year relative to number of women aged 15-44 in each country. Source: compiled by author using data from figures 2 and 3 in Population Reports (1988).

Overall, we show that the impact of legal access to the pill on improving women's outcomes in our context was strong, but also that this impact was not universally distributed because of religion in two dimensions. First, prevailing religious beliefs in the area where a women grows up can strongly influence her likelihood of adopting a new birth control method and in turn of experiencing its short- and long-run benefits. Second, and more concerning as this does not stem from a woman's own choice, the religious beliefs of 'gatekeepers' are also crucial to pass on the fertility and human capital gains from legal access to contraceptive technologies. Our findings suggest that the existing studies on access to the pill (and abortion) in the US are likely to only represent lower bounds of the potential impact it could have had on women's lives. Finally, they also imply that policies that aim to increase birth control technology availability should seek to minimize interferences through third parties so that *de jure* access changes translate into *de facto* fertility control choice options for women.

In **Section 2** we describe the institutional background surrounding the liberalization of the contraceptive pill in the Netherlands. In **Section 3** we present our continuous difference-in-differences identification strategy that exploits area level variation in views with regards to the pill. In **Section 4** we explain the various data sources we use and their structure. In **Section 5** we present and discuss the effects of liberalized access to the pill on women's long and short-run outcomes. In **Section 6** we focus on the potential impact of 'gatekeepers' in mitigating the full effects of access to birth control. In **Section 7** we put our findings in perspective, present causal estimates of the effect of fertility control on women's long run economic outcomes, and give some policy relevant conclusions.

2. Institutional background

2.1 Dutch morality and politics in the early 1900s

During the last decade of the 19th century the Netherlands was governed by liberal political parties.[6] In 1901 a new cabinet was appointed, led by Prime Minister Kuyper and supported by the Orthodox Protestant 'Anti-Revolutionary Party' and the 'Catholic People's Party'. With this change in political ideology the government became increasingly concerned with and involved in moral wellbeing, leading to the introduction of the Morality Law *(in Dutch: Zedelijkheidswet)* in 1911. The principal objective of the Morality Law was that sexual activity should only take place within a marriage and for the sole purpose of reproduction. It contained provisions about contraceptives, abortion, prostitution, and pornography (Hofstee, 2012).

Contraceptives were targeted because they could protect those in extramarital relationships from the consequences of their immoral behavior. The Morality Law prohibited individuals from openly displaying, offering, or proclaiming to have available any instrument that could prevent or interfere with a pregnancy on penalty of a prison sentence of at most two months, or a fine of 400 guilders (equivalent to about 4,400 euros in 2018).[7,8] Penalties were more severe for displaying or recommending contraceptive methods to minors (the age of majority was 21 in the Netherlands at

[6] The Netherlands was governed by three liberal cabinets between 1891 to 1901.

[7] The Dutch Morality Law is somewhat similar to the Comstock Law in the United States. This law forbade advertising birth control, and in some states, even the sale of contraceptives (Goldin & Katz, 2002).

[8] There was only one exception, contraceptive methods could be obtained from the Dutch Association for Sexual Reform (in Dutch: *Nederlandse Vereniging voor Seksuele Hervorming, NVSH*), but they could only sell to their members. The NVSH reached its peak number of members in 1965, which only accounted for 1.66% of the Dutch population (Hofstee, 2012).

that time): a prison sentence of at most six months or a fine of 1200 guilders (Rensman, 2006). The conservative laws were part of the background for the development and introduction of the oral contraceptive pill in the Netherlands.

2.2 The Dutch pill

The Dutch contraceptive pill was developed parallel to *Enovid*, the contraceptive pill that was developed in the US in the late 1960s. Dutch pharmaceutical company Organon was responsible for the development of the Dutch contraceptive pill. It created an artificial hormone, lynesterol, in the 1950s that by 1957 had been used to create an oral contraceptive for women, *Lyndiol*. The conservative Dutch views towards contraceptives played an important role before the Dutch pill was introduced in the market. Organon was afraid of the conservative backlash and maintained secrecy around the production of the pill. The packaging and distribution of the pill was even outsourced to nunneries when the demand for the pill increased. They feared that the girls in packaging factories in the secular workforce would be tempted to engage in immoral behavior, and nuns were considered to be less 'corruptible' (Rensman, 2006).

The Dutch pill only became available in pharmacies in 1963. Organon recognized that the prevailing moral views could play an important role in the acceptance of Lyndiol and introduced it to the market as a gynecological medicine that regulates the menstrual cycle and has a side-effect of temporary infertility. In reality, the primary purpose of the drug was to prevent pregnancies by suppressing ovulation. Lyndiol was included in the Medication Law, which meant that it was only available on a doctor's script at the pharmacy.[9] Doctors, who were already assisting couples in planning periodic abstinence, acquired an even larger role in family planning (Hofstee, 2012).

Even though the pill was available on the market from 1963, the Morality Laws still forbade promoting and making available any instrument that could interfere with a pregnancy. In the first years after the introduction of the pill it was mainly prescribed to women in very fertile marriages, for whom another pregnancy could have negative health consequences (Bekkering, 1969). Young unmarried women commonly did not have access to the pill in its early years.

[9] From its introduction, the Dutch government categorized the pill as a medication, which implied that it would only be available at a pharmacy on a doctor's prescription. This did not change with the liberalization of the pill in 1970. Which as Hofstee (2012, page 212) argues: "by making ovulation inhibitors only available by doctor's script, the responsibility and ethics of prescribing were all transferred to the doctor".

2.3 The repeal of the Morality Law and access to the pill

The Dutch view regarding contraceptives slowly started to change in the 1960s. The Netherlands was coping with a fast-growing population and was increasingly experiencing the negative consequences of population growth on the availability of resources. Limiting population growth rose high on the political agenda, and this required family planning technologies (Hofstee, 2012). Societal norms with regards to family formation and the role of the woman in the household were also starting to change.[10] This all eventually led to the repeal of the Morality Law in 1969.

The repeal of the Morality Law made it legal to provide information about contraceptives in speech or writing, and contraceptives were no longer age-restricted.[11] The birth control pill became even more accessible with its inclusion in the Dutch National Health Service for low income individuals (in Dutch: *Ziekenfonds*) in 1971 (Ketting and Schnabel, 1980). However, even though the pill became more accessible, women were still relying on 'gatekeepers': the general practitioner who had to recommend and prescribe the pill and the pharmacist who had to dispense it. We discuss the role of the gatekeepers and their religious beliefs for prescribing the pill, and consequently women's outcomes, in **Section 6**.

Figure 1 shows a large drop in the birth rate after the repeal of the Morality Law, and particularly for minors for whom access to the pill was liberalized the most.[12] There are a couple of reasons why we believe that the pill in particular had such a large effect on fertility in the Netherlands after 1970. First, **Figure 2** shows that the percentage of women aged 15-44 using the pill in the Netherlands exploded after 1970 to a striking 40% in 1975. This stands in sharp contrast

[10] The years prior to the repeal of the Morality Law were characterized by changes in social norms that translated into legal changes that gave more freedom to women. Some striking examples include the law that led women to become legally 'incapacitated' as soon as they married – making the husband the head of the household and in charge of all assets and children – which was abolished in 1957 (Pegtel, 2016). A long-standing rule that female public servants (and those employed in many large private firms) would have to give up their job at the time of marriage had been repealed two years earlier in 1955 (Rensman, 2003).

[11] The repeal of the Morality Law also improved access to other types of contraceptives like condoms and diaphragms, but we believe that the pill was the most important contraceptive at the time. First, pill usage exploded after the repeal of the Morality Law as shown in **Figure 2**. A survey by Vennix (1990) administered between 1986 and 1988 shows that the pill is the most used contraceptive by Dutch women at 34.1 percent, compared to condoms at 10.3 percent and the use of diaphragms at 0.2 percent. Second, the pill was the most effective contraceptive, the Dutch pill *Lyndiol* had a zero percent fail rate (Rice-Wray et al., 1966; Moses et al., 1969; Meer, 2007), compared to an effectiveness of 15 percent for condoms and 16 for diaphragms around 2006 (Bailey, 2006). Finally, women could take the pill without their partner's knowledge, whereas this is not the case for condoms, which is why we believe that the pill was particularly important for women around that time.

[12] The birth rate for all women had already started to fall in the early 1960s, which could be the result of secular trends but interestingly also coincides with the contraceptive pill becoming available to married women in the Netherlands who reached their desired level of completed fertility.

with the percentage of women using oral contraceptives in the United States in that same year, which was only about 16%.[13] Second, induced abortion was only legalized in the Netherlands in 1984, and even though induced abortion was tolerated in particular cases starting in the late 1960s, the abortion rate was low, and remained low over the course of the 1970s and mid-1980s (see **Figure A1**). Good access to contraceptives is often proposed as the primary explanation for the low abortion rate in the Netherlands (Ketting and Schnabel, 1980; Ketting and Visser, 1994; Levels et al. 2012). This, again, stands in sharp contrast with the figures for the US, where we see an upsurge in the abortion rate starting in the 1970s.[14] The high usage of the pill and low abortion rates may explain why the liberalization of the Dutch pill had such a large effect on fertility.

2.4 Timing of the repeal and political opposition

This paper exploits the repeal of the Morality Law to identify the effects of contraceptive access on fertility and women's outcomes. The repeal of the Morality Law coincided with societal changes that could also affect fertility behavior and women's outcomes, raising the question of whether the timing of the repeal was exogenous. Fortunately, the delay in the repeal of the Morality Law means that society was ready for the repeal a few years before law was actually abolished.

The Cabinet, ruled by Prime Minister Cals (in office from April 1965), committed to the repeal of the Morality Law and submitted two bills by September 1966 (Hofstee, 2012). However, before the bills could be discussed in parliament, a Cabinet crisis arose due to unrelated financial-economic budget disagreements. The Cabinet had to resign in November 1966, and new elections were held in 1967. It would take until May 1969 before the bills were discussed in parliament, and the bills passed in June 1969 (Rensman, 2006).

A large majority voted in favor of the repeal of the Morality Law. This included the Catholic People's Party (in Dutch: *Katholieke Volkspartij*) which already stated in its 1967 party

[13] One potential explanation for the low take-up of the pill in the US compared to the Netherlands could be the high costs associated with the usage of the pill in the early days. Bailey (2012) reports that an annual supply of the pill was twice as expensive in the 1960s than in 2010, and the costs were equal to more than three weeks of work on 1960s minimum wage. By contrast, the pill became free of charge for most individuals in the Netherlands after its inclusion in National Health insurance (health insurance scheme for low income individuals) in the Netherlands in 1971.

[14] In addition, Myers (2017) mentions the high failure rate of 10 percent of *Enovid* – the US pill – in the first year of use, and the unwanted 'pill pregnancy' this caused, as one of the reasons why the availability of induced abortion is the main driver behind improved fertility control in the US. Bailey (2006) however reports that *Enovid*'s effectiveness was promoted at 99 percent by its advocates at the time. Studies examining the failure rate of the Dutch Pill, *Lyndiol,* have consistently reported a failure rate close to zero if the medication was taken according to instructions (Rice-Wray et al., 1966; Moses et al., 1969; Meer, 2007).

manifestos that the responsibility for determining the size of the family lies with the parents. The only parties who voted against the repeal were those known for strong Christian values that were translated into conservative views regarding moral norms, namely the Orthodox Protestant Party (in Dutch: *Staatkundig Gereformeerde Partij)*, the Anti-Revolutionary Party (ARP), and the farmers' party (Hofstee, 2012). The division in parliament suggests that even though the Morality Law was abolished, there were still big differences in views with regards to the desirability of contraceptive use, which is what we exploit in our identification strategy.

We exploit area level variation in attitudes towards the pill, assuming that the adoption of the pill was slower in more conservative areas. We use the share of votes for the three parties that voted against the repeal of the Morality Act as a proxy for area level social norms surrounding the pill. The vote share data comes from the Dutch Electoral Council (in Dutch: *Kiesraad*), which has collected and published all Dutch election results since 1848. We focus on the votes for the national parliamentary elections in 1967, and we use the distribution of votes in this election at the municipality level. Due to the resignation of Cabinet Cals, this election took place after the bills to abolish the Morality Law were first introduced in 1966, and before they were discussed and voted on in parliament in 1969. Some parties even explicitly mentioned their policy position on the Morality Laws in their election manifestos in 1967 (e.g. the Catholic People's Party), so it is likely that even though people were not voting directly for the repeal of the Morality Law, they were aware of different parties' positions on this issue.[15] The turnout in the 1967 elections was 95% because voting was mandatory (including for women as women's suffrage was introduced in 1919 in the Netherlands). The Orthodox Protestant Party, the Anti-Revolutionary Party, and the Farmers' Party, respectively received 2.0%, 9.6%, and 4.6% of total votes nationally.[16]

Figure 3 shows municipality-level variation in the proportion of votes for the three parties that voted against the repeal of the Morality Law. We omit two provinces in the south of the Netherlands as there is very little variation in voting behavior in these areas: they are principally

[15] To illustrate, the Social-Liberal Party (D'66) mentions adding sex education to the school curriculum and making contraceptives freely available in their 1967 manifesto. The manifesto of the Labor Party (PvdA) mentions that abolishing the limitations surrounding contraceptives should come with sex education in both schools and by health professionals. The party manifesto of the Conservative-Liberal Party (VVD) argues that the decision about the size of the family lies with the parents, contraceptives should be available freely and no longer be subject to the penal code, and sex education is important.

[16] The vote shares for these parties excluding the southern provinces of Limburg and Noord-Brabant, the two majority Catholic provinces we will drop throughout our analysis, are respectively 2.4%, 11.3%, and 4.3%.

Catholic and mainly voted for the Catholic People's Party.[17] **Figure 3** shows that there is quite some variation in the share of votes for the three parties that voted against the repeal of the Morality Law, ranging from 4% to 82%. One can also quite clearly identify the Dutch 'Bible Belt' from the southwest to the northeast.[18]

Figure 3. The Dutch Bible Belt: Share of votes for Orthodox Protestant parties that campaigned against pill liberalization legislation in the 1967 parliamentary elections

Notes: Municipality level proportion of votes for parties voting against the repeal of the Morality Laws (Anti-Revolutionary Party, Orthodox Protestant Party, and the Farmers' Party). The Netherlands, excluding provinces of Noord-Brabant and Limburg. Source: Authors' calculations using data from the 1967 national parliamentary elections from the Dutch Election Council: https://www.verkiezingsuitslagen.nl/verkiezingen/detail/TK19670215

[17] The panel (a) of **Figure A2** shows regional variation in the proportion of individuals who declare that they are Catholic using the 1971 Census, the provinces of Noord-Brabant and Limburg are almost entirely Catholic.
[18] The panel (b) of **Figure A2** shows regional variation in the proportion of individuals who declare that they are Orthodox Protestant in the 1971 Census. The 'Bible Belt' that can be seen in **Figure 3**, also shows up in this picture using the Census data. Hence, the proportion of votes for Orthodox Protestant parties in 1967 and the proportion of individuals who self-report as being Orthodox Protestant in 1971 follow each other closely. **Table A7** shows that the correlation between both measures is 0.808.

A key assumption of our identification strategy is that use of oral contraceptives differs according to political preference. **Table 1** combines two pieces of evidence showing that women voting for Orthodox Protestant Parties are indeed less likely to have used contraceptives and in particular the contraceptive pill. Our first evidence comes from our own calculations based on a survey taken between 1986 and 1989, and shows that women voting for Orthodox Protestant Parties are about two times less likely to have used the contraceptive pill compared to women voting for nondenominational parties, with no party affiliation, and most notably also to women voting for the successor of the Catholic People's Party. The second piece of evidence comes from the Family Planning Survey (1988-2008). The percentage of women not using contraceptives is similar in both surveys, but pill use is higher for all groups in later years. Interestingly, also here pill use is about two times smaller for women voting for Orthodox Protestant Parties. This confirms that Orthodox Protestants were the most resistant to the use of the pill, and this resistance was much milder for Dutch Catholics. This could be explained by differences in "religious rigor", as 54.4% of Dutch Orthodox Protestants go to church at least once per week, whereas this is only about 14.7% for Dutch Catholics (see **Table A2**).

Table 1. Contraceptive use by political party affiliation.

	No contraceptives	Contraceptive pill	N
A: Vennix (1986-1989)			
Orthodox Protestant Parties	42.3%	15.4%	26
Catholic People's Party (and successors)	19.0%	28.3%	226
Nondenominational parties	17.0%	34.9%	665
No party	20.2%	28.7%	248
B: Family Planning Survey (1988-2008)			
Orthodox Protestant Parties	42.1%	27.9%	423
Catholic People's Party (and successors)	22.7%	48.8%	1,645
Nondenominational parties	17.2%	50.7%	4,056
No party	20.4%	54.8%	2,589

Notes: The Catholic People's Party ceased to exist in 1980, and a new party for Christian-Democrats (Christian Democratic Appeal, CDA) was founded from the Catholic People's Party (KVP), the Anti-Revolutionary Party (ARP), and the Christian Historical Union (CHU). Panel A: Authors' calculations based on Table 36 (page 35) from Vennix (1990). The survey was initiated by the Dutch Ministry of Health and executed by NISSO between 1986 and 1989, and has information on 1,165 individuals. Vennix refers to the Orthodox Protestant Parties as "small right". The group of nondenominational parties includes the Labor Party (PvdA), the Conservative-Liberal Party (VVD), the Social-Liberal Party (D'66), and small left-wing parties. Source Panel B: Authors' calculations based on the 1988-2008 waves (8,713 respondents) of the Family Planning Survey (Onderzoek Gezinsvorming, executed by Centraal Bureau voor de Statistiek, and available at DANS). The group of nondenominational parties includes the Labor Party (PvdA), the Conservative-Liberal Party (VVD), the Social-Liberal Party (D'66), and the Green Party (GroenLinks).

3. Identification: Within-Municipality Across-Cohort Variation

So far, we have documented a very large drop in births among minor women (i.e. aged 20 or younger) which coincided with a surge in the uptake of the pill following the repeal of the Morality Law in 1970. The importance of fertility control as an underlying mechanism seems to be confirmed by the sudden falls in unplanned pregnancies around the same period. To causally pin down the impact of pill access on treated women's outcomes, we bring together the two final stylized facts for we have presented evidence in the previous section: (i) that there is a lot of variation in the 1967 vote shares for Orthodox Protestant parties across the Netherlands,[19] and (ii) that pill adoption was much lower among women voting for these parties. We use these two margins to implement a (continuous) difference-in-differences strategy that is identified via within-municipality across-cohort variation in observed outcomes.

3.1. Intuition: comparing younger vs older women in liberal vs conservative areas

A simple way to understand our identification strategy is to consider how the liberalization of the pill might have affected the life decisions of two minor women, depending on how likely they were to take up the newly available birth control technology based on social norms in the area where they lived. We assume that a woman living in a municipality with a much higher vote share for Orthodox Protestant parties would be less likely to use the pill, and as a result would be less likely to show changes in the timing of fertility or marriage in the short-run, and less likely to invest in education and reap the economic benefits in the long-run. Our treatment intensity – the 'pill adoption probability'- is assigned continuously by using the vote share of parties in favor of the pill in each woman's municipality of birth.[20]

[19] A potential concern could be that, even if the vote share from a specific election is a good proxy for area social norms, it is the change in these norms that matters to properly capture factors that could influence women's outcomes differently across areas over time. We believe this is addressed in two ways. First, there is no reason for such norm changes not to be relatively continuous over time and that there should be a specific break for cohorts just too young or old to benefit from pill introduction, the basis of our identification strategy. Second, when comparing the vote share for the parties against pill liberalization in the elections of 1967 and 1971 at the municipality level we find a correlation of 0.98 (see **Table A7**). This is highly suggestive that norms were very sticky at the area level in this time period, at least in terms of voting preference for parties that were very conservative when it came to granting women improved access to birth control methods. Indeed, using either vote share 'for pill' in either election year (we will use 1967 throughout) does not make any difference to any of our results.

[20] We use place of birth as to assign women to municipalities rather than the place of birth of their first child for two reasons. First, because we can observe own place of birth for all women, whereas we cannot observe the place of birth of the first child for women who remained childless. Second, it is possible that treatment intensity may have impacted the probability of moving before making a fertility decision, and hence place of birth of first child may be considered as an outcome of social norms in the area where one is born/grew up.

Comparing two minors in different municipalities does partially account for secular time trends that may have affected their observable outcomes similarly, but that were not due to the liberalization of pill access. Examples of these trends range from changes in sexual behavior and average age at birth or marriage, to increased female participation in both education and the labor market. However, if such changes had less of an impact in more Orthodox Protestant areas – because they are on average more traditional in terms of women's roles within households and/or are economically less developed – then this approach would not yield causally informative estimates of the impact of pill access. To solve this issue, we use variation *across* cohorts and *within* the same municipality, and thereby always compare the outcomes of our two minors to those of slightly older women from the same area. The group of older women is considered untreated as they had reached the age of majority before the pill became accessible to minors in 1970.[21] In practice this means that we categorize women who were aged 16 to 20 in 1970 as 'treated' (i.e. 1950 to 1954 birth cohorts), and compare their outcomes to that of 'control' women from the same municipalities who were between the ages of 21 and 26 in 1970 (i.e. 1944 to 1949 birth cohorts).

Intuitively, this within-municipality across-cohort approach should account for almost all area constant and time-varying factors that may have differentially affected the fertility and subsequent life outcomes of our two cohorts of minors, independently of changes in pill access. We visualize whether this is the case by comparing key characteristics of the households in which these cohorts of women grew up, depending on the vote share in favor of the pill in their municipality. We do this in **Figure A3** for six different key variables that are reliably measured for all household (heads) in the 1971 Census: fertility and divorce (**Figure A3.1**); education and income (**Figure A3.2**); and housing value and access to a phone within the home (**Figure A3.3**). The left-hand side graphs of this figure reveal that the share of votes in favor of the pill is significantly – sometimes strongly – correlated with *all* these household characteristics. It appears that households in more liberal areas are more affluent, with a higher share of high-income household heads, a higher housing value,

[21] Note that women over 21 did actually experience improved access to the birth control pill in our context, but that the change in access to the pill that came with the liberalization was more drastic for younger than for older cohorts. First, the legal punishment for providing or recommending contraceptives to minors were much more severe for minors (women under the ages of 21). Second, women in 'fertile marriages' already could have gained access to the pill from the mid-1960s, and those women are likely older. Finally, and most importantly, these slightly older cohorts never had the opportunity to obtain the pill as a minor, and some of the observed birth and marriage outcomes (i.e. birth/marriage before 21) would already be impacted by the time of repeal of the Morality Law.

and are more likely to own a phone. This is a clear indication that only comparing outcomes of women across these municipalities, given a different hypothesized probability of taking the pill, would not be a good strategy. However, the right-hand side graphs of **Figure A3** show that *none* of these characteristics are significantly different in more or less liberal municipalities when we consider how they have changed between older (control) and younger (treated) households.[22] This is early and reassuring evidence that our chosen identification approach, that compares cohorts within the same area, indeed accounts for most factors that are related to access to the pill and may have affected women's outcomes differently.

One remaining crucial identification concern to address is whether women's outcomes were already on different trajectories across areas before the repeal of the Morality Law. We consider this question below.

3.2. Basic econometric specification: continuous difference-in-differences

We estimate the following continuous difference-in-differences specification, **Equation 1**, for various outcomes Y_{imc}.

$$Y_{imc} = \beta After_{ic} * ShareForPill_{im} + YoB_{ic} + MunB_{im} + \varepsilon_{imc} \quad (1)$$

The coefficient of interest is β which captures the treatment effect: an interaction of the $After_{ic}$ dummy that takes value 1 if woman *i* was still a minor at time of pill liberalization (i.e. from birth cohorts $c \in \{1950,1954\}$) and zero otherwise (i.e. from one of five previous cohorts $c \in \{1944,1949\}$), interacted with the standardized vote share for parties in favor of pill liberalization, $ShareForPill_{im}$, in each municipality m where woman i was born.[23] The specification includes year of birth (YoB_{ic}) and municipality of birth ($MunB_{im}$) fixed effects to

[22] As explained in more detail in the notes to **Figure A3**, we take the typical age of parents of women from our sample (i.e. born 1944 to 1954), and restrict to households declaring to ever have had a child, to define the sample we use for this exercise. We do not only focus on households which contain a woman from the 1944-1954 cohorts for the simple reason that women from older cohorts likely have already formed their own household by marriage at the time they are observed in the 1971 Census. Since we want to know and compare the characteristics of the households in which they grew up, we take typical parental age at that time to classify each household as likely treated (i.e. child aged 16-20 in 1970 with typical parent aged 46 to 55 in 1971) or likely untreated (i.e. child aged 21-26 in 1970 with typical parent aged 52 to 61 in 1970) in terms of early access to the pill.

[23] Since the intensity of treatment is in term of vote share for parties in favor of birth control liberalization (i,e, *ShareForPill = 1* – share vote for 3 Orthodox Protestant parties), the β coefficient reflects the impact on outcomes of an *increase* in the probability that oral contraceptives are adopted by women in a specific municipality.

capture all cohort-specific and area-specific factors that may influence the outcomes we consider. All regressions are weighted by municipality population – to properly reflect the relative impact of each area given its size – and our standard errors ε_{imc} are clustered at the municipality-cohort level to account for potential common shocks to any of the 11 cohorts in each of the 541 municipalities in our sample which might remain unobserved.

3.3. Robustness specifications: dropping extremes, pre-trends, and permutations

Following the presentation of our main results stemming from **Equation (1)**, we consider various alternative specifications that test the robustness of our results and also validate our continuous difference-in-differences approach.

First, we consider the sensitivity of our findings to excluding municipalities at the extremes of the *ShareForPill* distribution. This simple exercise informs us of the importance of the contribution of very pro- or very anti-pill areas for our main difference-in-differences estimates. If these extremely liberal and extremely conservative municipalities are crucial to our results, then this might put into question whether our continuous assignment of treatment approach is appropriate for women in more 'average' towns. The story would then be more about an 'all or nothing' adoption of the pill rather than gradients in the take up probability as proxied by the vote share in favor of pill liberalization. We test this by presenting results where we drop municipalities belonging to the top or bottom 10% and 20% of the vote in favor of the pill distribution. The latter is particularly demanding as it will only use women born in two-thirds of municipalities who have a relatively similar probability of using the pill.

Second, we do a common pre-trends test to validate our difference-in-differences approach. This test tells us whether outcomes for women in different cohorts from relatively more or less liberal municipalities had been diverging *before* pill access was liberalized. If they were, then it would be erroneous to causally interpret any significant β coefficient that **Equation (1)** might have yielded. We estimate **Equation (2),** which should detect the effect of pill access as in our main specification, but where the treatment effect (φ_c) is estimated for all cohorts (c) separately.

$$Y_{imc} = \sum_{c=1944}^{1954} \varphi_c (YoB_{ic} * ShareForPill_{im}) + MunB_{im} + \xi_{imc} \qquad (2)$$

This generates 11 policy estimates $\widehat{\varphi_c}$ which we plot to check the cohort-specific effects of treatment intensity on outcomes. Our common pre-trend assumption holds if the cohort-specific treatment effects are zero for women born in pre-policy cohorts (i.e. those who were a minor before the pill was liberalized, birth cohorts 1944-1949). This exercise can not only validate the common trends hypothesis, but also examine two related temporal elements about the policy impact. The first is that it serves as a 'placebo in time' as it shows whether we detect a policy impact when artificially moving the liberalization of pill access to before 1970. Second, and more importantly, it informs us about the evolution of the policy impact over time. Pill adoption might not have been immediate among young women and its diffusion could have been even stronger for the youngest cohorts. This would be illustrated by increasing sizes for the estimated $\widehat{\varphi_c}s$ among women in the five post treatment cohorts (1950 to 1954).

In our third and final robustness check we randomly assign treatment intensities across municipalities. We take one area's vote share for parties in favor of the pill and arbitrarily assign this value to all women from another area. One could think of it as a 'placebo in place' to test that our results are indeed driven by our treatment intensity – *ShareForPill* – and not by other area-specific factors. This test suits our continuous difference-in-differences approach since we have almost as many different treatment intensities as we have municipalities. We perform this permutation test 500 times, and then check graphically how the resulting coefficients compare to our baseline estimates for different outcome variables.

4. Individual Data, Sample Selection, and Variable Definition

We use administrative data from different registries compiled by Statistics Netherlands.[24] We focus on young women who were born in the Netherlands and aged 16 to 26 in 1970 (birth cohorts 1944 to 1954), for whom the repeal of the Morality Law had the largest effects. For women registered in a Dutch municipality by 1995 we observe their place of birth, marital history, and their fertility far beyond prime childbearing ages. We assign our treatment intensity measure – vote share on parties that were in favor of the pill – based on the woman's municipality of birth. Social norms may affect a wide variety of life choices, including moving behavior, and hence we

[24] See Appendix Section B.1 and B.2 for a very detailed description of the data set-up, sample selection process, and variable definitions

believe that this is the best measure (e.g. as opposed to social norms at the place of first childbirth, which may be a function of the social norms in the place one grows up). When excluding women born in the principally Catholic South, we are left with a sample of 864,370 women.

In the short term we are interested in outcomes related to fertility and family formation. Using the parent-child register we generate a measure indicating that a woman remained childless throughout her life, and a measure for the total number of children per woman (i.e. completed fertility). For women who ever had a child a variable for age at first birth is created, and a minor birth is defined as first birth to an individual below 21 years of age (the age of majority in the Netherlands at that time). The marital state registry, which has information on all past and present marriages, is used to determine whether a woman was ever married during her life, and for women who ever were married a variable is generated for age at first marriage. A minor marriage is defined as a woman who first married when she was younger than 21. A shotgun wedding is defined as a child being born within seven months of the mother's first marriage date. The seven-month time window (instead of eight or nine months) is chosen such that premature births are not 'accidentally' captured as shotgun weddings.

In the longer-term we are interested in outcomes related to education, work and wealth. The registry containing records about educational outcomes started only in 1999, and records for individuals who finished their degree before this time are inferred retrospectively from surveys. This implies that we only observe educational outcomes for about 25% of the sample. We generate a variable that reflects whether an individual finished higher education, which in the Netherlands is equivalent to finishing a university or university of applied sciences degree. In the spirit of Goldin and Katz (2002) we also generate a variable indicating that the woman finished a degree in law or medicine (medical school, dental medicine, or veterinary medicine), as both require a large up front time investment – and are thus more prone to disruption in case of birth or marriage – before one can start practicing.[25]

Information on labor market outcomes is also available from 1999, and we focus on labor force participation and earnings at age 55 – which is the earliest age at which we can observe earnings for women in all birth cohorts – to get a reliable picture before the women in our sample

[25] A law degree takes about 3 to 4 years in the Netherlands, whereas a medical degree typically takes about 6 years. However, given that individuals have to complete occupational training of at least two years before they start practicing as a lawyer, we classify law degrees as long studies.

enter retirement. We focus on earnings from self- and paid employment at age 55, and employment in terms of full-time equivalents for paid work. The latter is important as Dutch women outnumber those in other OECD countries when it comes to working part-time, with 61% of women working part-time in 2011, and only 5% of women reporting that working part-time is involuntary (Boeri & Van Ours, 2021).

Given the preferences of Dutch women for low labor market participation around that time, we also explore the effects of contraceptive access on a woman's household wealth. Household wealth includes all assets owned by the household minus the debts. Assets include the household's bank balance, savings balance, stocks and bonds, the value of their house, and the value of their business. Household wealth therefore depends on both own working behavior and a culmination of life choices (like choice of spouse) and may paint a more accurate picture of overall prosperity. The data on household wealth is available from 2006, and we focus on mean wealth for the women in our sample at the ages 60 to 62.

5. Impact of Pill Access on Women's Outcomes

We now present our analysis of how much pill access affected women's life trajectories in the Netherlands by exploiting area level variation in religiosity that influenced the probability of pill adoption. First, we look at short-run outcomes related to fertility and family formation. We then turn to longer-run outcomes – human capital investment and its returns – which could have been impacted by a woman's ability to delay childbirth and marriage decisions. After presenting the main findings, we test the validity of our methodology as well as the sensitivity of our results as described in **Section 3.3** above.

5.1 Main pill power estimates

Table 2 reports the continuous difference-in-differences estimates – the βs from **Equation (1)** – that indicate the impact of a one standard deviation increase in the vote share in favor of the pill in a woman's municipality of birth (about 10%) for women in treated cohorts (i.e. birth cohorts 1950-1954). These point estimates can be put in perspective relative to the mean of the dependent variable for the untreated cohorts (i.e. birth cohorts 1944-1949), which is also shown in the table.

We then can directly interpret these estimates in terms of a relative percentage effect size, which we report in the second row from the bottom.

Table 2. Short-Run Outcomes: Fertility and Family Formation

	Fertility				Family Formation				
	No Child	# of Children	Age 1st Birth	Mother < 21	Ever Married	Age 1st Marriage	Marriage < 21	Shotgun Wedding	Ever Divorce
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Minor 1970*	.003**	.000	.255***	-.019***	-.005***	.313***	-.017***	-.003*	-.003
Share for Pill	(.001)	(.004)	(.021)	(.002)	(.001)	(.038)	(.003)	(.002)	(.002)
Cohort F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep var	.136	1.91	24.6	.167	.941	23.0	.301	.161	.239
Effect size	+2.2%	-	+1.0%	-11.3%	-0.4%	1.4%	-5.6%	-1.8%	-
N	864,370	864,370	735,204	735,204	864,370	805,870	805,870	727,201	805,870

Notes: Estimated by OLS. ShareForPill is standardized with mean and standard deviation one. One standard deviation in ShareForPill is about 10%. All specifications are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality (541) x cohort (11) level in parentheses. Shotgun wedding is a dummy indicating that a child is born within seven months after a woman married. The sample size is different across the different columns. Columns 1, 2 and 5 use the full sample of women, in columns 3 and 4 we focus on women who ever had a child, in columns 6, 7 and 9 we focus on women who were ever married, and in column 8 we restrict the sample to women who were ever married and ever had a child. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We find that access to the pill as a minor did not have a large effect on a woman's completed fertility. Women are 2% more likely not to have a child for a one standard deviation increase in treatment intensity, but the number of children born per woman remains unchanged. However, pill access clearly led to a significant delay in timing of birth among treated cohorts. This is true in terms of average age at first birth, but in particular for early fertility decisions. If a woman had been born in a municipality with a vote share in favor of the pill that was one quartile (i.e. 2.5 times the standard deviation of 10%) higher, she would have been 28% less likely to experience a minor birth.

Access to birth control did not have a large effect on the probability of marriage– which was almost universal among women from these cohorts – but it did significantly affect the timing of family formation decisions. On average women married later, and again this effect is stronger

at younger ages. Women born in municipalities that were one quartile more liberal were more than 14% less likely to marry as a minor. They were also 5.4% less likely to end up in a 'shotgun wedding' (i.e. with a birth within 7 months of marriage), which indicates that marriage was potentially hurried by fertility circumstances. The resulting marital unions appear to have been neither stronger nor weaker.

Table 3. Long-Run Outcomes: Education, Work, and Wealth

	Education		**Work** (age 55)		**Wealth** (age 62)			
	Higher Educ.	Long Studies	Working (FTE)	Wage (FTE)	Log Wealth	% rank Wealth	Top 25% Wealth	Top 10% Wealth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Minor 1970*	.006**	.002***	.005**	-.005	.016*	.482***	.010***	.003**
Share for Pill	(.003)	(.001)	(.002)	(.004)	(.009)	(.122)	(.002)	(.001)
Cohort F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep var	.174	.007	.218	€29k	€302k	.500	.250	.100
Effect size	+3.4%	+28.6%	+2.3%	-	+1.6%	+0.48%	+4%	+3 %
N	218,119	218,119	758,024	358,381	758,024	758,024	758,024	758,024

Notes: Estimated by OLS. ShareForPill is standardized with mean and standard deviation one. One standard deviation in ShareForPill is about 10%. All specifications are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality (541) x cohort (11) level in parentheses. Higher Educ. is a dummy indicating that a woman finished higher professional or university education. Long studies is a dummy indicating that a woman completed the longest forms of higher education (i.e. Medical Doctor or Juris Doctor Degree). Working and (log) wages are determined at age 55 and are expressed as 'full time equivalent' as part-time work is very common among Dutch women. The sample sizes are different across outcome variables. We observe the educational outcomes for only about a quarter of the women in our sample. We restrict the sample for labor market participation (at age 55) and wealth (at age 62) to individuals with positive wealth and observe this information for 88% of individuals. More details can be found in the Data Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We showed that access to the pill allowed women to delay their fertility and marriage decisions, which could have enabled women to increase investments in their human capital. This is confirmed by the results in **Table 3**, which shows that women in treated cohorts are significantly more likely to finish a higher education degree, as well as finishing degrees that require a larger time investment before one can start working. Treated women who were born in a 25% more pro-pill municipality would have been more than two-thirds more likely to obtain a Medical or Juris

Doctor – MD or JD – degree. Note that the effect size is particularly large given the low baseline, as less than 1 percent of women in untreated cohorts completed such degrees beforehand.

The third and fourth columns of **Table 3** report the impact on labor force participation and wages, and here the picture is less clear cut. We detect a small increase in the probability of working in terms of full time equivalent (FTE) hours, but there is no apparent impact on earnings conditional on working that follows from this employment effect. It is important to note that these outcomes are measured at age 55 – the first age at which we have administrative data for all women in our sample – and a large fraction of women could have decided to exit the labor market by this age. Not working might be an economically optimal decision for women at this age – even after having invested more in human capital earlier in life – if their wealth level permits it.

We investigate the possible wealth effects by using high-quality information on household wealth from the Netherlands, and report results using various measures of household wealth in the last four columns of **Table 3**. Better access to the pill as a minor made women significantly better-off in their early 60s. This is true when considering average increases in terms of wealth elasticity and when considering their overall ranking in the wealth distribution. These effects are strongest at the top of this distribution. A woman who is born in a municipality with a quartile higher share of votes for parties in favor of the pill, is 10% more likely to end up in the top quartile of the wealth distribution. These are the first results that show that the economic impact of improved pill access seems to be especially strong in terms of lifetime income accumulation. These are potentially important results as they would explain why the evidence on labor market outcomes has not been overwhelming so far, despite strong consistent positive education findings.[26] Given that these wealth outcomes are measured at the household level, which is in part determined by partner

[26] Bailey (2006) for example finds that pill liberalization in the US led to somewhat higher labor force participation of women aged 26 to 35 but detects no effects before those ages, which is consistent with human capital investment and delayed birth effects in early 20s. She however also does not find effects of labor market participation for women older than 35 which may be at first somewhat more surprising. This could indeed be, as argued in the paper, caused by measurement error caused by older women being more likely to have moved out of the state in which they grew up leading her state-level instrument to not properly capture pill exposure changes for these older women. Bailey, Hershbein and Miller (2012) do find, using a different identification strategy in the US, negative pill access impacts on women's ages in their early 20s but positive ones in their 30s and 40s (an hourly wage premium of eight percent). This clearly suggests that detecting labor market returns to educational investment for women stemming from improved oral contraceptive availability is very sensitive not only to context and identification approach, but also to the age at which this outcome is observed.

choice, these results reinforce the importance of birth control technology on delaying and improving mating decisions.[27]

5.2 Robustness and validity checks

To check the sensitivity of our results and confirm the validity of our identification approach we present results from the three sets of robustness exercises described earlier.

In the first test, we drop areas at the extremes of the pro-pill vote share distribution to check their importance for our findings. These results are reported in four appendix tables for the short- and long-run outcomes, first when excluding municipalities at the top and bottom 10% (**Tables A3.1** and **A3.2**), and then for excluding municipalities at the top and bottom 20% (**Tables A4.1** and **A4.2**). Our results are stable, and if anything somewhat larger, but not statistically different from the main analysis. This indicates that our pill effect is not just identified from municipalities at the extremes of the vote share for pill distribution, and that a gradient in area level acceptance of the contraceptive pill is important post-liberalization. This is a policy-relevant finding, but one that is also econometrically pertinent given the continuous nature of our treatment measure.

The second test addresses the critical common pre-trends assumption for difference-in-differences designs. We estimate the cohort-specific policy impacts of getting access to the pill – the φ_c of **Equation (2)** – and plot these for four key outcomes in **Figure 4**: minor mother (top left), minor marriage (top right), higher education completion (bottom left), and whether the household is in the top quartile of wealth distribution (bottom right). These show that there is no clear pre-policy pattern for the untreated cohorts – birth cohorts 1944 to 1949, on the left side of the red dashed line – who were older than 21 at the time of the pill liberalization. For the treated cohorts born from 1950 to 1954 for whom pill access liberalized when they were younger than 21, we observe clear deviating trends in most outcomes depending on the share of votes for parties in

[27] We also examine whether the characteristics of a woman's partner change with the liberalization of the Pill (results available on request). We find that, for a quartile higher vote share in favor of the liberalization in the woman's municipality of birth, the first marriage partner is about 4.2% older (effect of 0.424, with a standard deviation of 0.053, on a baseline of 25.32 for a 1 standard deviation increase in the vote share for the pill). We do not find any significant results on both educational and labor market outcomes for the woman's partner at age 55. Given that women on average marry older men (the mean age difference in for untreated women in our sample is 2.3 years), the information on educational outcomes is available for even fewer partners than for the woman in our sample, and similarly these partners may, because of their more advanced age, be even more likely to have exited the labor force by the time we can observe them in our earnings data. Therefore, we cannot say much about the effects of the pill liberalization on 'partner quality', apart from our estimates on household wealth.

favor of the pill in the woman's municipality of birth. This observed difference strongly increases the younger women were at the time of the liberalization.[28]

In addition to confirming the common trend hypothesis, these graphs are informative on two other aspects of the policy impact. The first one simply relates to what we might expect to find if we artificially moved the change in pill access, such that an older cohort is then the first actually treated one, to earlier years. This would not yield any significant results, and serves as a visual 'placebo in time' test. The second more important aspect is that the policy impact is more pronounced as treated cohorts are younger at the time of the liberalization. This could be for two reasons: (i) because pill take up for a given age group (e.g. nineteen-year olds) increases more strongly over time in more liberal municipalities, and/or (ii) because changes in pill access have a larger impact for women at younger ages. Both these explanations are consistent with the pattern displayed in these graphs but cannot be separated. We cannot say with certainty which one of these two mechanisms is at play here.

The third and final robustness/validity exercise is a 'placebo in place'. If our proxy for social norms – vote share in favor of the pill – is not the main driver behind our findings, then we could detect significant coefficients when arbitrarily exchanging treatment intensities across areas. In that case area-specific factors, rather than our treatment intensity measure, would be responsible for our results. Since our treatment is continuous, we are able to do this permutation a large number of times and still assign a new value of the pro-pill vote share distribution (i.e. without replacement) to a municipality. We do this test 500 times and present the resulting 500 estimated coefficients as a density graph next to our main estimate (red solid line).

Figure A4 shows the results of this permutation test for the same four outcomes as reported before. These figures show that the vote share in favor of the pill in a woman's municipality of birth is crucial to identifying our effects. For three of the four outcomes – minor birth, minor marriage and being in the top quartile of the wealth distribution – there is not a single iteration where the random allocation of social norms yields estimates that larger than those in the 'real' allocation of social norms. For higher education completion this is the case in 6 out of 500 permutations, an extremely low occurrence which might partly be explained by the smaller sample

[28] The pattern for higher education completion, even if it points to significant increases for some of the post treatment cohorts, is much noisier. This is probably caused by the much lower number of observations we have for this outcome as we observe education for only 25 percent of women in our sample.

Figure 4. Cohort Specific Policy Effect: Minor Births/Marriage, Higher Education, and Top 25% Wealth

Notes: The graphs plot the point estimates and 90% confidence intervals for the coefficients that are estimated using equation (2), and show 11 policy estimates φ_c, which show the impact of a one standard deviation increased in share for pill (about 10%) for each birth cohort in our sample (akin a common trend assumption in a difference-in-differences setting). The cohorts 1950 and later were exposed to the pill as a minor and hence treated (and hence we should expect to see an effect starting from these birth cohorts), whereas the cohorts 1944 to 1949 did not have access to the pill as a minor and hence are considered untreated (and hence we would expect a zero effect for these birth cohorts).

size this estimation is based on (about one quarter of the sample of women than we have for the other outcomes). We believe this final 'placebo in place' provides very strong evidence that social norms in an area were critical to a woman's likelihood of adopting the newly available birth control technology, and benefiting from it in both the short- and long-run.[32]

Now we consider the possibility that, even if a woman had wanted to exercise her right to use the pill and improve control over her fertility, the 'gatekeepers' who should grant access to the new technology could have prevented this from happening because of their own moral beliefs.

6. The Power of Morally Opposed Gatekeepers

6.1 Physician's beliefs and the likelihood of prescribing the pill

The gatekeepers had a key role in the provision of the contraceptive pill. The pill was only available by doctor's script at a pharmacy, and the general practitioner (GP) was the confidante responsible for guiding and informing patients about family planning practices. Hence, even though the contraceptive pill was supposed to be available for all women after the repeal of the Morality Law, access might still be restricted if the GP did not prescribe the pill and the pharmacist was reluctant to stock the pill. Refusal to supply the pill because of moral constraints like religion has been found in the health care sector: substantial evidence from the US shows that practitioner faith matters for the provision of (emergency) contraceptives and induced abortion.[33]

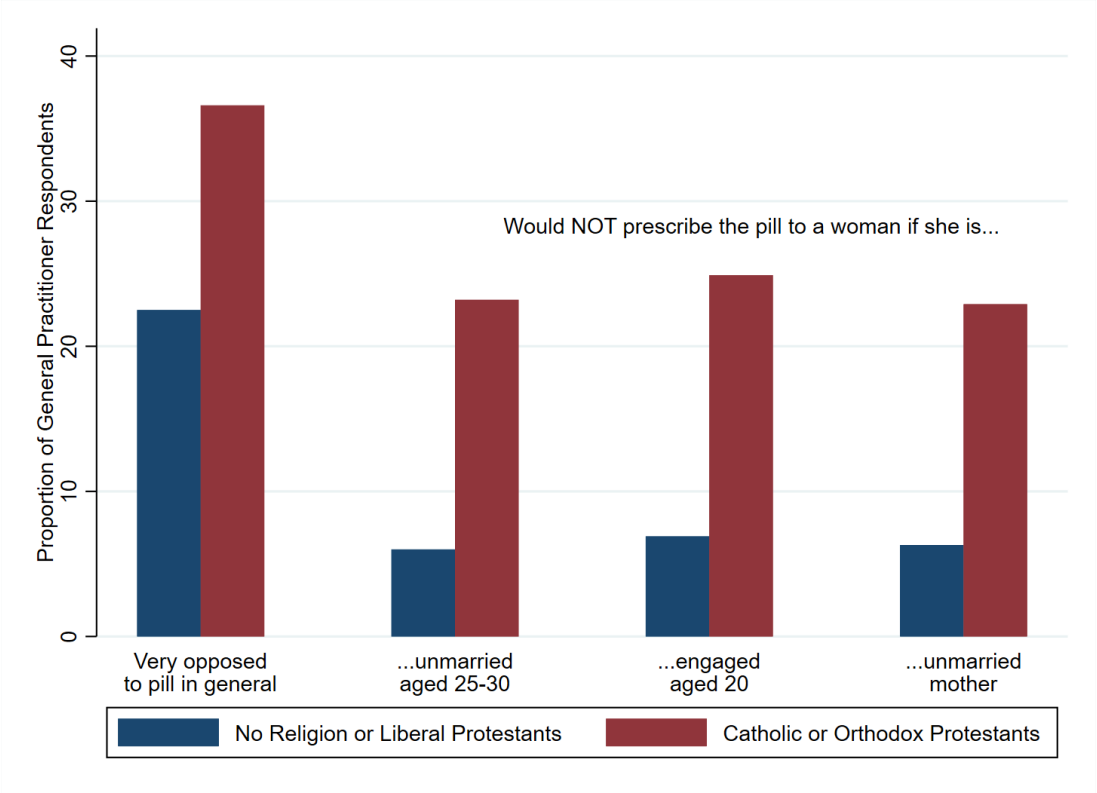
These issues may also have played an important role when it came to accessing the contraceptive pill in the Netherlands. Organon, the company responsible for the development of the Dutch pill, originally marketed the contraceptive pill as a medication for the regulation of the menstrual cycle with the undesired side-effect of temporary infertility, to increase the likelihood

[32] Given that induced abortion rate was and remained very low while pill take-up increased dramatically at the time – as illustrated in **Figure 2** and **Figure A1** – we are not overly worried that access to abortion drives our results. Still to check if the location of any of the ten abortion clinics of the Netherlands which were semi-legally authorized to carry out abortions in specific cases may have any impact on our findings, we carry out the following basic checks. We first see if a municipality's share of 'votes for pill' is correlated to the distance to the closest clinic (median distance 23.5 kilometers). This returns a tiny correlation coefficient of -0.053 which is perhaps not surprising as these are distributed almost evenly across the country. Second, we want to check if this distance did not have a differential impact on fertility behavior across treated and control cohorts in more or less conservative municipalities. We do this by running our basic model from equation (1) with an additional interaction of the continuous difference-in-differences estimator with the distance to the closest abortion clinic. We find that distance to the closest abortion clinic does not have any additional effect on the probability that women exposed to pill liberalization as minors became mothers before age 21. We thus conclude that abortion is very unlikely to have played any important role in the context we study.

[33] See for example Spivack (1964), Rubin, Grumet and Prine (2006), Lawrence, Rasinski, Yoon and Curlin, (2010), and Stulberg, Dude, Dahlquist and Curlin (2012).

that religious doctors would accept and prescribe the pill (Hofstee, 2012). Despite this effort, both Orthodox Protestant *and* Roman Catholic general practitioners (GPs) remained markedly more resistant to prescribing the pill at the time of the liberalization than all other physicians (Bekkering, 1969). This is confirmed in a large survey looking into the attitudes of Dutch GPs towards birth control methods carried out by Bangma (1970).

Figure 5. Physician's opposition to the pill by religious affiliation



Notes: Authors' calculations based on Bangma (1970). Survey administered among 528 general practitioners in 1969, which is about 12% of the total number of GPs in that year (Centraal Bureau voor de Statistiek, 1994, p. 265). The first column shows the percentage of GPs who state that they are very opposed to the use of the pill as a contraceptive method, and the second, third, and fourth shows the percentage of GPs who would never prescribe the pill to the following groups of women: unmarried women aged 25-30, engaged women older than 20, and unmarried mothers.

We use the underlying data to produce **Figure 5** which shows physicians' opposition to the introduction of the pill in general, and to prescribing it to specific groups of women depending on their own religious affiliation. Orthodox Protestant and Catholic doctors were 60% more likely to be 'very opposed' to the use of the pill as a contraceptive, compared to other physicians (36.6 against 22.5 percent). These differences are even larger when looking at the responses of

physicians that they would *never* prescribe the pill to certain women. Orthodox Protestant and Catholic doctors are about 3 times, or 300%, less likely to ever prescribe it to: unmarried women aged 25 to 30; unmarried mothers; or engaged women younger than 21. This huge gap in the probability of prescribing the pill that is driven by religious moral beliefs may have affected women's ability to take advantage of the large short- and long-run effects of the pill that we have documented.

Before turning to our analysis, we first need to look at why Catholic physicians remained so opposed to the pill when, as we have already shown in **Table 1**, Catholic women were adopting it as their chosen birth control method as often as non-religious women? Our main explanation here is that of a generational divide between the (almost exclusively, 87%) male and mostly older (69% over 40) health professionals and the young women demanding the pill. Catholic men above the age of 40 were twice as likely to disapprove of the pill and have intolerant views with regards to premarital sex, compared to Catholic women under the age of 30 (Hutjes, 1974, pp 82 and 168). This can be linked to changes in religious rigor across generations: Catholics over the age of 40 were about twice as likely to feel a strong attachment to their church compared to individuals under the age of 30 (authors' calculations based on Hutjes, 1974, table 8.6). This generational gap in practicing habits is confirmed in **Table A5**, which reports church-going frequency by age for individuals who report belonging to one of the three main religions in the Netherlands. It clearly shows that younger Catholics became much less likely than older generations to attend services, even infrequently, and interestingly this drop is not observed among Liberal or Orthodox Protestants.

6.2 Religious gatekeepers and their location

To gauge how much gatekeepers' religious beliefs matter for the provision of the contraceptive pill and consequently women's fertility and later life outcomes, we use data from the 1971 full count Census to identify the proportion of religious health professionals in each municipality. We define health professionals (HPs) as pharmacists and general practitioners (GPs), as these two professions were the gatekeepers for access to the contraceptive pill: the GP to prescribe the pill and the pharmacist to stock it. We identify a total of 5,261 health professionals: 4,326 GPs and 935 pharmacists in 1971 in the Netherlands, excluding the southern provinces. **Table A6** shows the religious affiliation of the Dutch population compared to the health professionals. In our sample

of health professionals, 38.2% is not religious, 16.8% is Catholic and 9.8% is Orthodox Protestant. Compared to the full population, health professionals are more likely to be non-religious and less likely to be Catholic. We focus on health professionals who are most opposed to the pill, i.e. Orthodox Protestant and Catholic HPs, who represent 26.6% of all health professionals. The women in our sample have on average access to 23.1% religiously opposed HPs in their municipality (with a standard deviation across municipalities of 7.3%).[34]

We calculate the proportion of religious health professionals by dividing the number of religious health professionals by the total number of health professionals in each municipality. This gives a measure of access to doctors who are more or less likely to prescribe the pill in the woman's neighborhood.[35] Hence, it captures the set of 'gatekeepers' women encounter within a municipality. The regional dispersion in this measure is shown in **Figure 6a** and shows considerable variation in the religiosity of the pool of health professionals women can choose from across the Netherlands. **Figure 6b** shows that there is considerable variation in doctor's religiosity and the social norms regarding the pill in each municipality. Hence, we observe liberal areas with predominantly Catholic and Orthodox Protestant HPs and areas for which it is the opposite.[36]

Figure A5 shows the latter plot separately for Catholic and Orthodox Protestant health professionals (HPs), to give a better picture of the level of 'mismatch' with prevalent area-level social norms in the areas where they practice. The figure shows that there are some Orthodox Protestant health professionals in liberal areas, but more strikingly there are many Catholic HPs who are practicing in areas that are not opposed to the pill. The latter finding is probably because general practitioners often locate themselves in areas that are close to the university they attended.

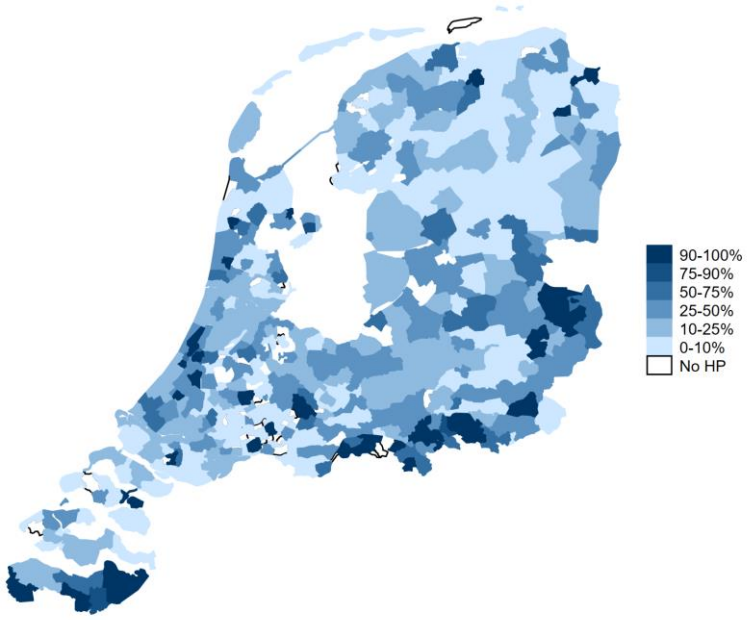
[34] See **Appendix** S**ection B.3** for a detailed description of the set-up of the Census data.

[35] One concern may be that individuals can shop for doctors with the right religious affiliation outside the individual's municipality of residence. Individuals were free to choose their general practitioner (Hofmans, 1970), but in practice the GP wanted to be close to the patient in case of emergency. Today in the Netherlands, GPs can decline patients more than fifteen minutes from the GP practice (see for example the policy guidelines for the Law Access to Healthcare Facilities dd June 3, 2005 (in Dutch: Wet Toelating Zorginstellingen)). Hence, even though individuals were free to choose their own GP it is very likely that their GP was located in the same municipality.

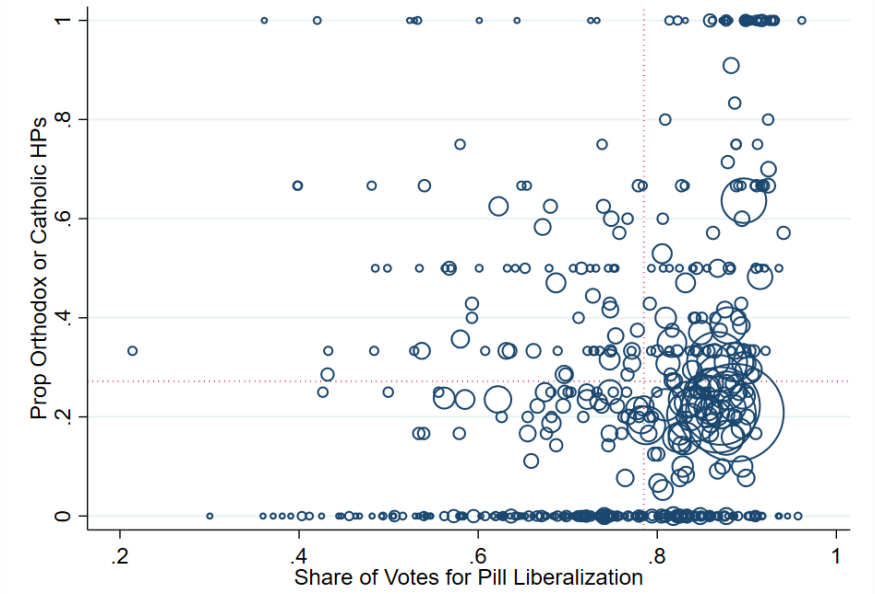
[36] **Table A8** shows the correlation between the proportion of Catholic and Orthodox Protestant health professionals to several other measures at the municipality level.

Figure 6. Variation in the proportion of religious health professionals

(a) Municipality level regional variation



(b) Proportion of religious health professionals and share votes in favor of the pill



Notes: Panel (a) shows regional variation in the proportion of religiously opposed health professionals. Authors' calculations based on the 1971 Census of the proportion of general practitioners and pharmacists who are Orthodox Protestant or Catholic. Panel (b) shows the proportion of Orthodox Protestant and Catholic health professionals by the share of votes on parties in favor of the pill. Weighted by the number of health professionals in each municipality, which is shown by the size of the dot.

Over the period 1957 to 1981, 52-69% of GPs started practicing in the province that their university was located in (Groenewegen, 1985).[37] The fact that this allocation mechanism for Catholics is stronger than for Orthodox Protestants is not surprising given that the Catholic south of the Netherlands did not have a medical school until 1976. Medical students had to move to the north to go to medical school, which eventually affected the location in which they started practicing.[38]

6.3. The impact of religiously opposed gatekeepers

We test the impact of gatekeepers' beliefs on a woman's likelihood of experiencing the short- and long-run benefits of legal access to the pill. We run the specification of **Equation (1)** while adding an interaction between the (continuous) difference-in-differences estimator (i.e. $After_{ic} * ShareForPill_{im}$) with the share of health professionals in each municipality who are religiously opposed to the pill: i.e. the proportion of HPs who are either Orthodox Protestant or Catholic in each municipality, or $PropRelHP_{im}$ in **Equation (3)** below.

$$Y_{imc} = \beta_2 After_{ic} * ShareForPill_{im} + \delta PropRelHP_{im} * After_{ic} * ShareForPill_{im} \\ \gamma After_{ic} * PropRelHP_{im} + YoB_{ic} + MunB_{im} + \varepsilon_{imc} \quad (3)$$

This informs us about the *additional effect* of the increased probability of facing religiously opposed gatekeepers – captured by the triple interaction – *given a level of area social norms* that would have made a woman more or less likely to take up the pill– itself captured by the main difference-in-differences interaction.[39] We first do this using the standardized share (i.e. mean zero and standard deviation one) of religiously pill-opposed HPs and present the resulting

[37] The percentage of GPs practicing in the same province as they went to medical school between 1957-1981 is 61% for the province of North Holland, 59% in South Holland, 52% for Utrecht and 69% for Groningen, Friesland and Drenthe (see Table 6.4 on pages 154-155 of Groenewegen, 1985).

[38] A side effect of this religious mismatch between HPs and the municipality they work in is that they could be more likely to practice for less time in an area where their beliefs are not aligned with the community. This is important in our context as (young) women could feel inclined or not to seek a prescription for the pill from a physician or pharmacist, depending on the time he has been practicing in their municipality. We check this by looking in the Census at the probability that a HPs has been active at least 5 years in an area depending on his religious affiliation. We find a tiny correlation coefficient (-0.037) for the relationship between the proportion of religiously opposed doctors and the proportion of doctors that are active in an area less than 5 years. Hence, the religious affiliation of the doctor is not related to the time spent in their current municipality, and hence not important in our setting.

[39] The estimated γ coefficients that result from the interaction between *After* and *PropRelHP* capture the impact of having a higher proportion of religiously pill opposed HPs *independent of area level social norms*. These are not the relevant measure of the *additional impact of gatekeepers* above and beyond social norms that will influence take up which we are after in our context and thus do not report these coefficients.

estimates, $\hat{\delta}$, along with the associated difference-in-differences coefficients, $\hat{\beta}_2$, for four of our main outcomes of interest in **Table 4**: minor birth and marriage, completing 'long studies', and belonging to a household in the top quartile of the wealth distribution by age 60.

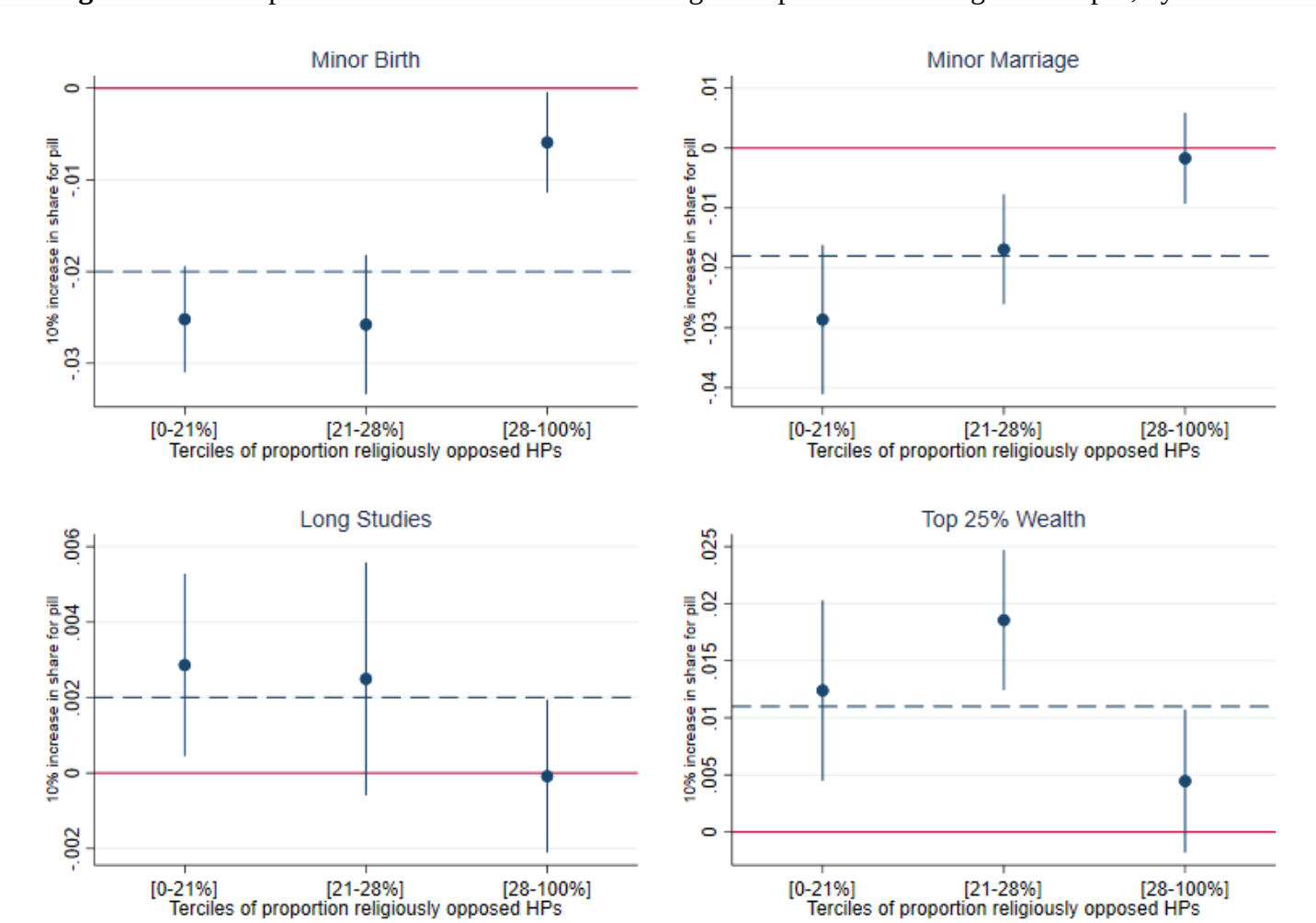
Table 4. Gatekeeper Power: The additional effect of gatekeepers who were against the pill

	(1) Minor Mother (Birth < 21)	(2) Minor Marriage (Wedding < 21)	(3) Long Studies (MD or JD)	(4) Top 25% of Wealth Dist.
Minor 1970*Share for Pill (i.e. DiD_{Treat})	-.020***	-.018***	.002***	.011***
	(.002)	(.003)	(.001)	(.002)
DiD_{Treat}* % Religious Health Professionals	.007***	.010***	-.001**	-.004**
	(.001)	(.003)	(.000)	(.002)
Cohort F.E.	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes
Sample Size	731,184	801,549	217,113	753,849

Notes: Estimated by OLS. ShareForPill is standardized with mean zero and standard deviation one. One standard deviation in ShareForPill is about 10%. The mean proportion of religious HPs women have access to is 23.1%, with a standard deviation of 7.3%. We also standardize this measure with mean zero and standard deviation one. All specifications are estimated for municipalities with at least one HP and are weighted by the number of HPs. Robust standard errors are clustered at the municipality (502) x birth cohort (11) level and are in parentheses.

The estimated effect of having relatively more religiously opposed gatekeepers in your municipality at time of pill liberalization is always strongly significant and almost symmetrically opposite to the impact of the pill due to area level social norms. Concretely, that means that, for marriage, a one standard deviation increase in the proportion of Orthodox Protestant or Catholic health professionals, or +7% more conservative HPs, in your municipality of birth reduces the potential impact of pill access on marrying as a minor (-1.8% for 10% more votes for pill) by more than half (+1%). Although this interpretation is statistically correct, it might not be the best way to understand the impact of religiously opposed gatekeepers on attenuating the effects of the pill. First, it is not straightforward to capture the size of a triple interaction with two continuous variables, and second, because important non-linearities may not be properly captured.

Figure 7. Gatekeeper Power: The additional effect of gatekeepers who were against the pill, by tercile.



Notes: Estimated by OLS. Figures plot the additional effect of religiously opposed HPs (in tercile) on top of the social norms in an area (i.e. a triple interaction with our difference-in-differences estimator:$After_{ic} * ShareForPill_{im}$). *ShareForPill* is standardized with mean zero and standard deviation one. One standard deviation in *ShareForPill* is about 10%. The proportion of religiously opposed HPs (with mean 23.1 and standard deviation of 7.3%) is divided in tercile: 0-21%, 21-28%, and 28-100%. All specifications are restricted to municipalities with at least one HP and are weighted by the number of HPs in every municipality. Robust standard errors are clustered at the municipality (502) and cohort (11) level and are in parentheses.

For this reason, we present results where $PropRelHP_{im}$ in **Equation (3)** is replaced by an indicator of which tercile of the religious gatekeeper distribution a municipality belongs to: the 1st tercile from zero to 21%; the 2nd tercile from 21 to 28%; and the 3rd tercile, 28% or more. The estimated coefficients we obtain are reported, for the same four outcomes as earlier, in **Figure 7** with their respective 95 percent confidence intervals. These figures clearly reveal that, while there is no significant difference in estimated pill impact for the first two terciles of the distribution, when more than a third of HPs are either Catholic or Orthodox Protestant, this effect is entirely wiped out regardless of which outcome is considered.

These findings are unchanged when dropping pharmacists to only consider the impact of religiously opposed general practitioners (**Table A8.1** and **Figure A6.1**) or when allocating the share of religiously opposed gatekeepers from the closest municipality for municipalities without HPs (**Table A8.2** and **Figure A6.2**).[40] We do one last test for whether this effect is partially driven by areas where women do not have much choice locally in the health professionals to consult to get access to oral contraceptives. We produce results for the impact of the proportion of HPs who are religiously opposed to prescribing the pill, restricting our sample to municipalities with at least three active physicians or pharmacists. The results from this robustness check are reported in **Table A8.3** for the continuous triple interaction coefficients and **Figure A6.3** for the graphical tercile decomposition. Both reveal that the morally opposed gatekeeper's capacity to cancel any potential pill effect is the same, even when more options are locally available.

The picture that emerges is of a very large and negative impact of gatekeepers on the ability of women to properly benefit from the life changing advantages of birth control technology. If a woman grew up in an area with fewer doctors that were religiously opposed to the pill, she would have been more likely to select into marriage and motherhood later, have more time to invest in her human capital, and eventually end up in a wealthier household. This further highlights the importance of considering differences between *de jure* and *de facto* legal access to contraceptive methods, especially when the beliefs of third parties are involved. These beliefs may have significant long-term consequences for those who are meant to benefit. Religious opposition to abortion has been studied substantially in the past (e.g. Stulberg et al. (2011) for the US, and Autorino et al. (2020) for Italy). However, these results are, to our knowledge, the first that clearly

[40] Note that the distance to the closest municipality with at least one HP for those women living in municipalities without an HP is very small. The mean distance is 3.5km with a standard deviation of 2.0.

document that religious opposition also plays a big role when it comes to the contraceptive pill: a far less controversial birth control method. Gatekeepers have the power to annihilate the enormous impact that the pill can have on young women's lives.

7. Discussion and Conclusion

Our investigation of the impact of the liberalization of the contraceptive pill in the Netherlands confirms the powerful impact that the availability of birth control can have on a woman's short- and long-run life outcomes. Our results also highlight important heterogeneity on the demand side of the pill as they reveal how the potential benefits of access liberalization were not universally distributed across women. Minors who grew up in areas that were less religiously opposed to the pill were relatively more likely to adopt this technology and this translated into significant delays in both fertility and mating decisions. This enabled them to obtain higher education qualifications, especially in fields with longer qualification periods such as medicine and law. Surprisingly, this did not translate into much higher labor market participation rates by age 55, an age at which labor force participation is low for all women in these cohorts. A better measure of long-term economic success is household wealth, and on that metric, those who benefited most from the pill clearly did much better. It especially lifted these women towards the top of the household income distribution.

These findings are the first to document the life changing effects of pill access outside the US, and an obvious question is how our results compare to previous findings. The heterogeneity across religion on the demand side that we document suggests that existing studies that examine the impact of gaining access to the pill represent only a lower bound of the true effect it could potentially have on women's outcomes. Moreover, differences in the nature of the treatment, the age of affected women, empirical strategy, and outcomes measured make it difficult to compare findings. Still, we can make some rough comparisons on some of the key marriage, fertility, and human capital variables that have been studied in both settings. In the Netherlands, a woman born in a 25 percent more liberal area would have been 14 percent less likely to marry as a minor, and 28 percent fewer women in a more liberal area would have experienced a birth by age 21. The Dutch marriage impact is between those of Goldin and Katz (2002) – 5% fewer marriages by age 22 – and Myers (2017) – 19% fewer marriages by age 19 – for the US, which makes them comparable given the different ages at which the outcome is measured. The picture is also very similar in terms of enabling women to delay fertility, as again our estimated effect is between those

of Bailey (2006) – 14% fewer births by age 22 – and Myers (2017) – 34% fewer births by age 22 – for the US. The most comparable education investment impact estimates are those on 'long studies' by Goldin and Katz (2002), which, as in our context, report an almost doubling of the graduation rate of females for medical and juris doctors' degrees in the US as a result of access to the pill. These might at first seem like huge increases, but in both countries, the baselines are very low: 1.4% in the US and 0.7% in the Dutch context. Finally, as in Bailey (2006), we also do not detect any significant wage effects at later ages albeit for probably different reasons.[41]

The availability of high-quality wealth data means we are the first to use wealth as an alternative measure of economic success. We conclude that the very significant pill access effects on wealth may reconcile the lack of labor market effects found in this literature. This would apply if working was not the optimal choice for women who were 'better off' in our cohorts around that time. Overall, our estimates for the strong impact of oral contraceptives on women's lives in the Netherlands appear not so different from those found for the US. Importantly, this is in a context where abortion was not officially fully liberalized until much later and only very seldomly used by Dutch women – partly because of a very high take up of oral contraceptives – which is why we believe we are measuring a relatively 'pure' pill power effect.

If these life changing impacts of *de jure* pill availability were important, they were not felt equally by all women as *de facto* access remained restricted due to gatekeepers' beliefs. The religious affiliation of the health professionals - the suppliers of the pill - in the municipality where women were born appear to have mattered considerably. We estimate that if over 28% of them were either Orthodox Protestant or Catholic, it is unlikely that a young woman was able to experience any of the benefits from pill access that those in equally liberal areas, with fewer religious gatekeepers, did. This holds true for fertility, marriage, education, and wealth outcomes. These new results on morally opposed gatekeepers offsetting the impact of birth control access policies are important for many reasons. First, it again means that average pill effect estimates are probably lower bounds of the potential true effect of how much it could have altered women's

[41] Bailey (2006) put forward the following reasons for this unexpected result: "The lack of a difference in labor-force participation between women with and without ELA at older ages is more difficult to assess using the CPS. First, women who bore their children during their twenties may be returning to the labor market during their later thirties. Thus, on an aggregate level you may see these women entering the sample again… Second, as women age, they are less likely to reside in the same state as at age 21, so measurement error due to migration may obscure the effect as women age." (page 314)

lives, and not only in our context. Second, while this finding is linked to moral norms of health practitioners half a century ago, the influence of religious beliefs of health professionals on delivering legally available birth control methods – especially abortion – is still hotly debated around the world. Third, our gatekeeper findings have important implications for current and future birth control policies which will likely be more effective if access is independent of third parties who may hinder a woman's right to choose.[42]

We conclude this paper by briefly returning to the main motivating question that has led social scientists to study the impact of pill access policies: do fertility and mating choices have important *causal* effects on economic outcomes or are they just a reflection of endogenous individual choices that also affect human capital investment? In other words, if women who have children or marry young have unobservable traits that make them less likely to invest in their education and are worse off as a result, then naïve correlations would largely overestimate the impact of fertility/marriage decisions on education choices and wealth accumulation. In the odd columns of **Table A9** we confirm with simple OLS regressions that younger mothers or brides do significantly worse when it comes to graduating from higher education and ending up in the top quartile of the wealth distribution. We then propose a very simple exercise that uses the change in pill access as an instrument that exogenously changed fertility and mating decisions of these women, to obtain causal estimates of this relationship. Applying an instrumental variable (IV) approach[43] yields somewhat less precise estimates, but these estimates are two to five times larger than those obtained by OLS (the even columns of **Table A9**). This is strongly suggestive of the fact that fertility and mating decisions *do have* a *very large and significant causal impact* on women's long-run outcomes. Birth control technology gives women more power to control the

[42] Interferences by third parties are actually becoming a political tool used by those morally opposed to abortion in the US to counteract legal access guarantees. Even though Roe v. Wade specifically bans states from making abortion illegal early in a pregnancy, the Texas senate recently found a loophole by passing a law (Senate Bill 8: https://legiscan.com/TX/text/SB8/id/2395961) which allows ordinary citizens to sue anyone who aids a women at any stage from obtaining an abortion procedure after six weeks of gestation.

[43] The first stages are large and significant, which was expected based on the results presented earlier in the paper. Note however that the estimates are different to those presented in **Section 5** as they are based on the sample of women for which we have complete information on both short- and long-run outcomes. The exclusion restriction for this IV approach is that the pill only affected education and wealth outcomes via its effects on fertility/mating. While we believe this to be mostly the case, one could still argue that the pill may have had some other indirect effects that may violate this restriction. The results presented here are only used as a concluding extension to this paper and thus accept this possible methodological caveat that can be applied to these IV findings.

timing of these decisions, and consequently improve their chances to optimally invest in their human capital as they wish and choose to do.

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Appendix A: Additional Tables and Figures

Table A1. Percentage of unplanned pregnancies over time.

A: Vennix (1986-1989)

	1966-1970	1971-1975	1976-1980	1981-1985	1986-1988
Planned pregnancy	37.0%	69.5%	80.3%	81.5%	92.7%
Kind of planned	18.5%	10.4%	7.3%	7.7%	1.6%
Kind of unplanned	16.7%	11.7%	6.4%	7.7%	3.3%
Unplanned pregnancy	27.8%	8.4%	6.0%	3.1%	2.4%
N	54	154	234	286	123

B: Family Planning Survey (1988-2008)

	1969-1971	1972-1974	1975-1977	1978-1980
Unplanned pregnancy	45.4%	24.0%	18.5%	15.2%
N	97	245	406	533

Notes: Panel A comes from Vennix (1990), Table 54, page 71. Based on a survey that was initiated by the Dutch Ministry of Health and executed by NISSO between 1986 and 1989. Panel B is based on authors' calculations using the 1988-2008 waves (8,713 respondents) of the Family Planning Survey (Onderzoek Gezinsvorming, executed by Centraal Bureau voor de Statistiek, and available at DANS).

Table A2. Church attendance by religious denomination.

	Catholics	Liberal Protestants	Orthodox Protestants	Other Religions	All
Every Week or More	14.7%	22.3%	54.4%	51.2%	24.9%
At Least Once a Month	17.5%	16.1%	18.1%	11.3%	16.8
At Least Once a Year	34.4%	20.2%	11.3%	12.9%	26.0%
Almost Never	33.3%	41.4%	16.2%	24.7%	32.3%
Observations	2,769	1,280	728	381	5,158

Notes: authors' calculations based on the Labor Supply Panel 1985-2000 (in Dutch: Arbeidsaanbodpanel, made available by Sociaal en Cultureel Planbureau (2016), and available at DANS).

Table A3.1 Robustness: Fertility and Family Formation, excluding 10% extremes

	(1) Childless	(2) # of children	(3) Age 1st birth	(4) Mother < 21	(5) Ever married	(6) Age 1st marriage	(7) Marriage < 21	(8) Shotgun wedding	(9) Ever Divorce
Minor 1970*	.001	.002	.300***	-.023***	-.005***	.393***	-.021***	-.003	-.005*
Share for Pill	(.002)	(.005)	(.030)	(.002)	(.002)	(.053)	(.004)	(.002)	(.002)
Cohort F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep var	.137	1.90	24.6	.166	.941	23.0	.302	.159	.243
Effect size	-	-	+1.2%	-13.9%	-0.5%	1.7%	-7.0%	-1.9%	-2.1%
N	817,209	817,209	693,487	693,487	817,209	761,286	761,286	685,699	761,286

Notes: Estimated by OLS. We exclude municipalities with ShareForPill in the top and bottom 10% of the distribution (456 municipalities remaining). ShareForPill is standardized with mean zero and standard deviation one. One standard deviation in ShareForPill is about 10%. All specifications are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality (456) x cohort (11) level in parenthesis. Shotgun wedding is a dummy indicating that a child is born within seven months after a woman married. * p < 0.10, ** p < 0.05, *** p <0.01.

Table A3.2 Robustness: Education, Work, and Wealth - Excluding 10% Extreme Municipalities

	(1) Higher Educ.	(2) Long studies	(3) FTEs at age 55	(4) ln(wage) age 55	(5) Log wealth	(6) % rank wealth	(7) Top 25% wealth	(8) Top 10% wealth
Minor 1970*	.008**	.002**	.005*	-.002	.017	.561***	.012***	.003
Share for Pill	(.004)	(.001)	(.003)	(.006)	(.012)	(.176)	(.003)	(.002)
Cohort F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep var	.176	.007	.220	€29k	€301k	.500	.250	.100
Effect size	+4.5%	+28.6%	+2.3%	-	-	+0.56%	+4.6%	-
N	206,753	206,753	715,168	341,433	715,168	715,168	715,168	715,168

Notes: Estimated by OLS. We exclude municipalities with ShareForPill in the top and bottom 10% of the distribution (456 municipalities remaining). ShareForPill is standardized with mean zero and standard deviation one. One standard deviation in ShareForPill is about 10%. All specifications are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality (456) x cohort (11) level in parentheses. Higher Educ. is a dummy indicating that a woman finished higher professional or university education. Long studies is a dummy indicating that a woman completed the Medical Doctor or Juris Doctor Degree. Working and (log) wages are determined at age 55 and are expressed as 'full time equivalent' as part-time work is very common among Dutch women.* p < 0.10, ** p < 0.05, *** p <0.01.

Table A4.1 Robustness: Fertility and Family Formation, excluding 20% extremes

	(1) No Child	(2) N of children	(3) Age 1st birth	(4) Mother < 21	(5) Ever married	(6) Age 1st marriage	(7) Marriage < 21	(8) Shotgun wedding	(8) Ever Divorce
Minor 1970*	.000	.003	.375***	-.031***	-.006**	.603***	-.030***	-.004	-.006
Share for Pill	(.003)	(.008)	(.051)	(.004)	(.003)	(.084)	(.008)	(.004)	(.004)
Cohort F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep var	.140	1.87	24.6	.169	.940	23.0	.305	.160	.251
Effect size	-	-	+1.5%	-18.3%	-0.6%	+2.6%	-9.8%	-	-
N	730,904	730,904	617,667	617,667	730,904	679,909	679,909	610,339	679,909

Notes: Estimated by OLS. We exclude municipalities with ShareForPill in the top and bottom 20% of the distribution (361 municipalities remaining). ShareForPill is standardized with mean zero and standard deviation one. One standard deviation in ShareForPill is about 10%. All specifications are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality (361) x cohort (11) level in parenthesis. Shotgun wedding is a dummy indicating that a child is born within seven months after a woman married. * p < 0.10, ** p < 0.05, *** p <0.01.

Table A4.2 Robustness: Education, Work, and Wealth - Excluding 20% Extreme Municipalities

	(1) Higher Educ.	(2) Long studies	(3) FTEs at age 55	(4) ln(wage) age 55	(5) Log wealth	(6) % rank wealth	(7) Top 25% wealth	(8) Top 10% wealth
Minor 1970*	.017**	.003**	.006	.005	.007	.359	.010**	-.001
Share for Pill	(.007)	(.001)	(.005)	(.009)	(.022)	(.304)	(.005)	(.003)
Cohort F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep var	.180	.007	.224	€28k	€329k	.500	.250	.100
Effect size	+9.4%	+42.9%	-	-	-		+4.0%	-
N	185,650	185,650	637,221	307,510	637,221	637,221	637,221	637,221

Notes: Estimated by OLS. We exclude municipalities with ShareForPill in the top and bottom 20% of the distribution (361 municipalities remaining). ShareForPill is standardized with mean zero and standard deviation one. One standard deviation in ShareForPill is about 10%. All specifications are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality (361) x cohort (11) level in parentheses. Higher Educ. is a dummy indicating that a woman finished higher professional or university education. Long studies is a dummy indicating that a woman completed the Medical Doctor or Juris Doctor Degree. Working and (log) wages are determined at age 55 and are expressed as 'full time equivalent' as part-time work is very common among Dutch women.* p < 0.10, ** p < 0.05, *** p <0.01.

Table A5: Percentage of individuals going to church once a month by religion and age.

	Aged 18-39	Aged 40+
Catholic	22.9	42.1
Liberal Protestants	37.0	39.5
Orthodox Protestants	70.7	74.6

Notes: Authors' calculations based on the Labor Supply Panel 1985-2000 (in Dutch: Arbeidsaanbodpanel, made available by Sociaal en Cultureel Planbureau (2016), and available at DANS).

Table A6: Percentage of individuals by religious affiliation in 1971 Dutch Census, full population and by occupation (i.e. health professionals: general practitioners or pharmacists).

	Dutch Population	Health Professionals	General Practitioners	Pharmacists
No religion	28.6	38.2	36.9	44.1
Orthodox Protestants	11.5	9.8	10.8	5.1
Catholic	27.2	16.8	17.1	15.6
Liberal Protestant	28.7	25.5	26.4	21.4
Other	3.9	9.8	8.9	13.8
Observations	10,233,915	5,261	4,326	935

Notes: Authors' calculations based on the 1971 Census. All columns exclude individuals living in the two southern provinces of the Netherlands (Noord-Brabant and Limburg) because they are principally Catholic. Health professionals are defined as general practitioners and pharmacists. An explanation of the set-up of the religion variable in provided in **Appendix B.3**.

Table A7: Correlation between municipality-level characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
(1) Share against '67	1.000					
(2) Share against '71	0.980	1.000				
(3) Prop. Orthodox	0.808	0.837	1.000			
(4) Number of HPs	-0.366	-0.382	-0.317	1.000		
(5) Prop. Religiously Opposed HPs	-0.108	-0.118	-0.111	-0.153	1.000	
(6.) Total population	-0.360	-0.369	-0.309	0.989	-0.154	1.000

Notes: Authors' calculations based on election data from the 1967 and 1971 national parliamentary elections and from the 1971 Census. Correlations are calculated at the municipality level and exclude municipalities located the southern provinces of Noord-Brabant and Limburg. Share against '67 is the share of votes for parties who were against the liberalization of the pill in the 1967 national parliamentary elections at the municipality level, share against '71 is the share of votes for parties who were against the liberalization of the pill in the 1971 parliamentary elections at the municipality level, the proportion Orthodox is the proportion of individuals in a municipality who declare that they are Orthodox Protestant in the 1971 Census, number of HPs is the number of pharmacists and general practitioners in each municipality, the proportion religiously opposed HPs captures the proportion of HPs that are Orthodox Protestant or Catholic at the municipality level, and total population is the total municipal population as calculated in the 1971 Census.

Table A8.1. Robustness: GP Power: The additional effect of GPs who were against the pill

	(1)	(2)	(3)	(4)
	Mother < 21	Marriage < 21	Long studies	Top 25% wealth
Minor 1970*Share for	-.019***	-.017***	.002***	.011***
Pill (i.e. DiD_{Treat})	(.002)	(.003)	(.001)	(.002)
DiD_{Treat}* % Religious HPs	.007***	.010***	-.001**	-.003**
	(.001)	(.002)	(.000)	(.002)
Cohort F.E.	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes
Sample Size	731,135	801,494	217,109	753,797

Notes: Estimated by OLS. ShareForPill is standardized with mean zero and standard deviation one. One standard deviation in ShareForPill is about 10%. The mean proportion of religiously opposed GPs women have access to is 23.9%, with a standard deviation of 7.5%. We also standardize this measure with mean zero and standard deviation one. All specifications are estimated for municipalities with at least one GP and are weighted by the number of GPs. Robust standard errors are clustered at the municipality (501) x birth cohort (11) level and are in parentheses. * p < 0.10, ** p < 0.05, *** p <0.01.

Table A8.2. Robustness: Gatekeeper Power: The additional effect of gatekeepers who were against the pill - Including municipalities without any HP

	(1)	(2)	(3)	(4)
	Mother < 21	Marriage < 21	Long studies	Top 25% wealth
Minor 1970*Share for	-.019***	-.018***	.002***	.011***
Pill (i.e. DiD_{Treat})	(.002)	(.003)	(.001)	(.002)
DiD_{Treat}* % Religious HPs	.007***	.009***	-.001***	-.004**
	(.001)	(.002)	(.000)	(.002)
Cohort F.E.	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes
Sample Size	735,204	805,870	218,119	758,024

Notes: Estimated by OLS. ShareForPill is standardized with mean zero and standard deviation one. We assign the number of HPs in the closest municipality for individuals living in municipalities with no HP. One standard deviation in ShareForPill is about 10%. The mean proportion of religiously opposed HPs in the municipality closest to you is 23.1% with a standard deviation of 7.5%. We also standardize this measure with mean zero and standard deviation one. All specifications are weighted the number of HPs in the own or closest municipality. Robust standard errors are clustered at the municipality (541) x birth cohort (11) level and are in parentheses. * p < 0.10, ** p < 0.05, *** p <0.01.

Table A8.3. Robustness. Gatekeeper Power: The additional effect of gatekeepers who were against the pill - Restricting sample to municipalities with at least 3 HPs

	(1)	(2)	(3)	(4)
	Mother < 21	Marriage < 21	Long studies	Top 25% wealth
Minor 1970*Share for	-.020***	-.018***	.002***	.012***
Pill (i.e. DiD_{Treat})	(.002)	(.003)	(.001)	(.002)
DiD_{Treat}* % Religious HPs	.007***	.010***	-.001**	-.003*
	(.001)	(.003)	(.000)	(.002)
Cohort F.E.	Yes	Yes	Yes	Yes
Mun. F.E.	Yes	Yes	Yes	Yes
Sample Size	676,793	743,140	202,332	698,009

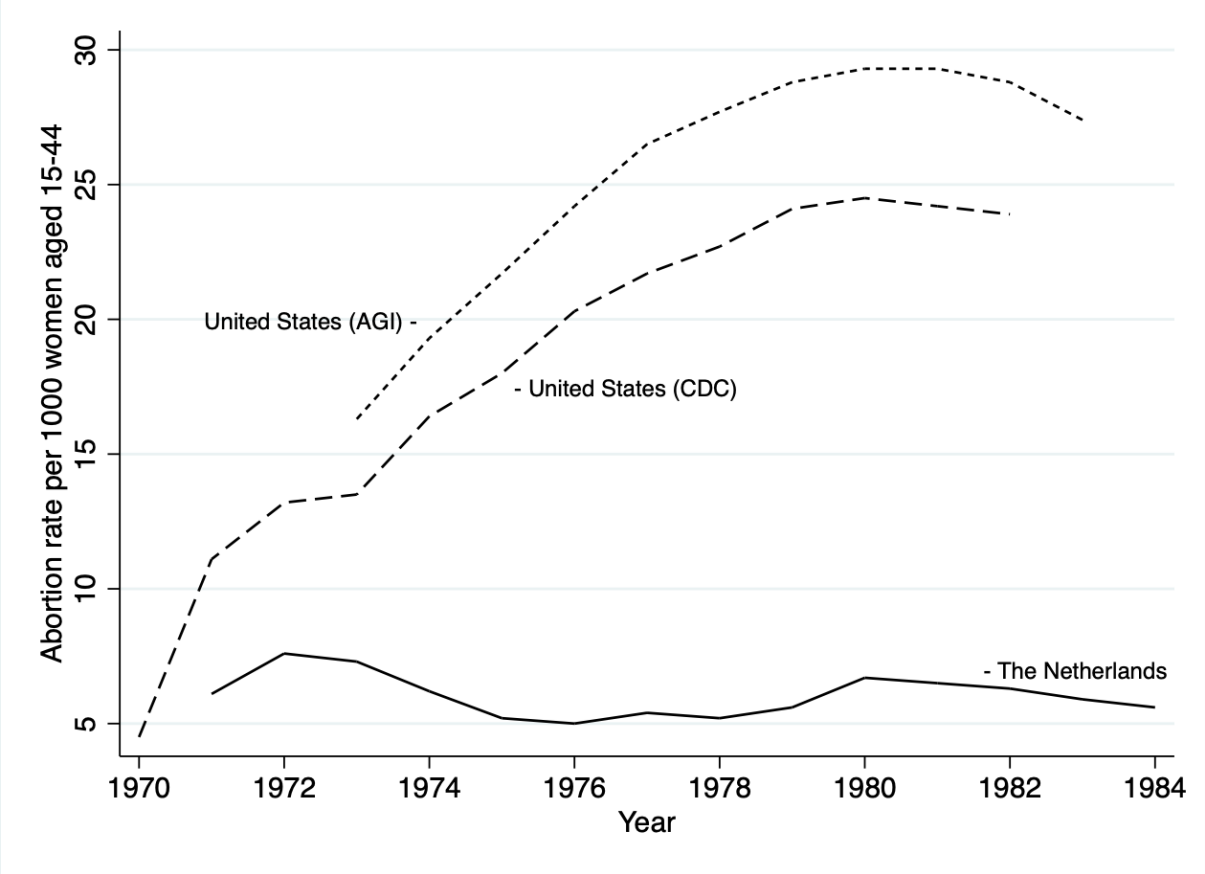
Notes: Estimated by OLS. ShareForPill is standardized with mean zero and standard deviation one. We drop individuals living in the 320 municipalities that have fewer than 3 HPs. One standard deviation in ShareForPill is about 10%. The mean proportion of religiously opposed HPs is 23.1% with a standard deviation of 7.3%. We also standardize this measure with mean zero and standard deviation one. All specifications are weighted the number of HPs in the own or closest municipality. Robust standard errors are clustered at the municipality (320) x birth cohort (11) level and are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: IV Estimates of the Impact of Fertility/Mating Decision on Human Capital Accumulation

	Minor Mother				Minor Marriage			
	Education		Wealth		Education		Wealth	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Minor Mother	-.179***	-.354*	-.128***	-.617***				
	(.006)	(.183)	(.002)	(.137)				
Minor Marriage					-.186***	-.323**	-.114***	-.511***
					(.005)	(.146)	(.002)	(.110)
First stage		-.016***		-.017***		-.018***		-.020***
		(.003)		(.003)		(.004)		(.003)
F-test		42.8		29.9		19.3		38.6
N	218,119	218,119	758,024	758,024	218,119	218,119	758,024	758,024

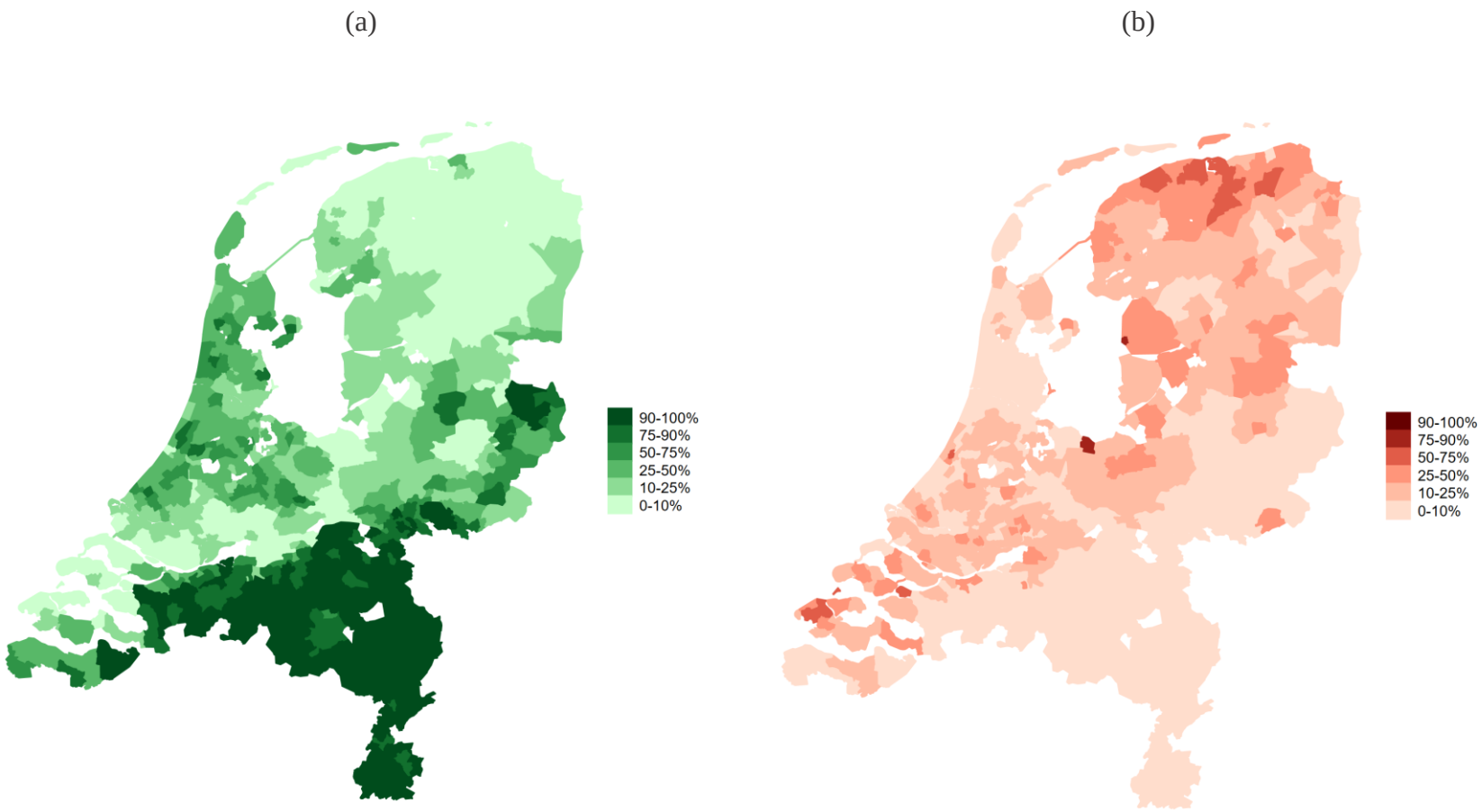
Notes: The odd columns are estimated using OLS and the even columns are estimated using 2SLS. Minor mother is a dummy that is equal to one for women who gave birth before age 21, minor marriage is a dummy that is equal to one for individuals who got married before age 21, education is a dummy that is equal to one if a woman finished a higher professional education or university degree, and wealth is equal to one if a woman is in the top quartile of the wealth distribution by age 60. For the IV estimates we instrument our endogenous variable (minor mother or minor marriage) with the change in pill access for women (ShareForPill interacted with whether the woman was a minor in 1970, hence equal to one for birth cohorts 1950-1954 and zero for 1944-1949) that we have used throughout the paper. The table also reports the first stage coefficients (effects of the instrument on becoming a minor mother or getting married as a minor), and the corresponding first stage F-statistics which are well above 10. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1. Abortion rate per 1000 women aged 15-44, the Netherlands and US, 1970-1984



Notes: US numbers and numbers for the Netherlands 1973-1984 come from Tietze and Henshaw (1986), Table 2, pages 30-42. The numbers for the US are coming from the Alan Guttmacher Institute (AGI) and the Center for Disease Control (CDC). The CDC estimates are lower than the AGI estimates because the CDC obtains its data from state health departments, whereas the AGI uses active outreach. As a number of states does not require the reporting of abortions, some require reporting only from certain types of facilities, and some may be less rigorous in enforcing reporting abortions the numbers for the CDC are about 15-18% lower than those of the AGI. Note that this source incorrectly states abortions per 100 women, but this should be per 1,000 women. The Dutch numbers for 1971 and 1972 are retrieved from Ketting and Schnabel (1980).

Figure A2. Proportion of Catholics and Orthodox Protestants by Municipality in 1971



Notes: Author's calculations based on the 1971 Census. Panel (a) shows the proportion of individuals who declare that they are Catholic at the municipality level. Panel (b) shows the proportion of individuals who declare that they are Orthodox Protestant at the municipality level. An explanation of the set-up of the religion variable in provided in **Appendix B.3**.

Figure A3. Census household characteristics and municipality's share of votes for pill: Mean level (left-hand side graphs) and mean difference across cohorts (right-hand side graphs)

Figure A3.1: Fertility and Marriage

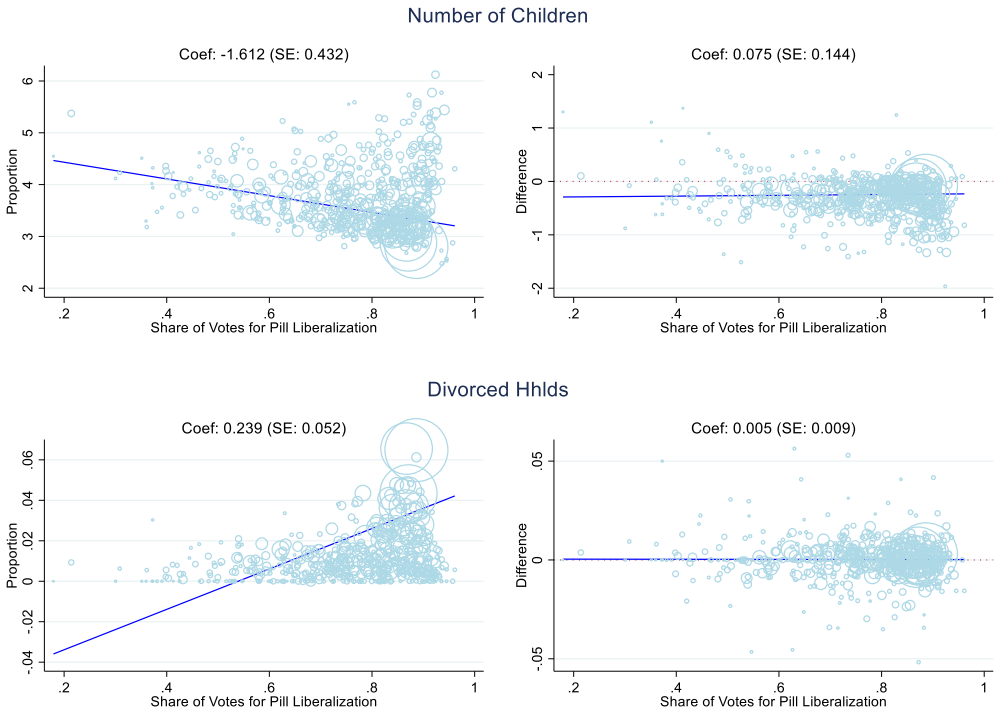


Figure A3.2: Education and Income

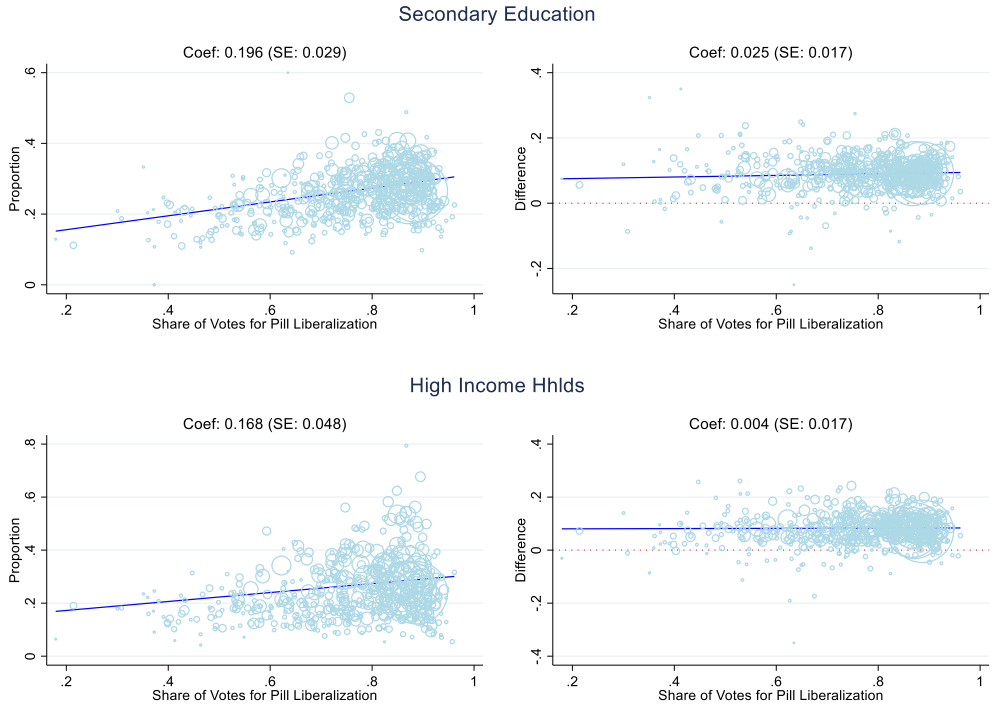
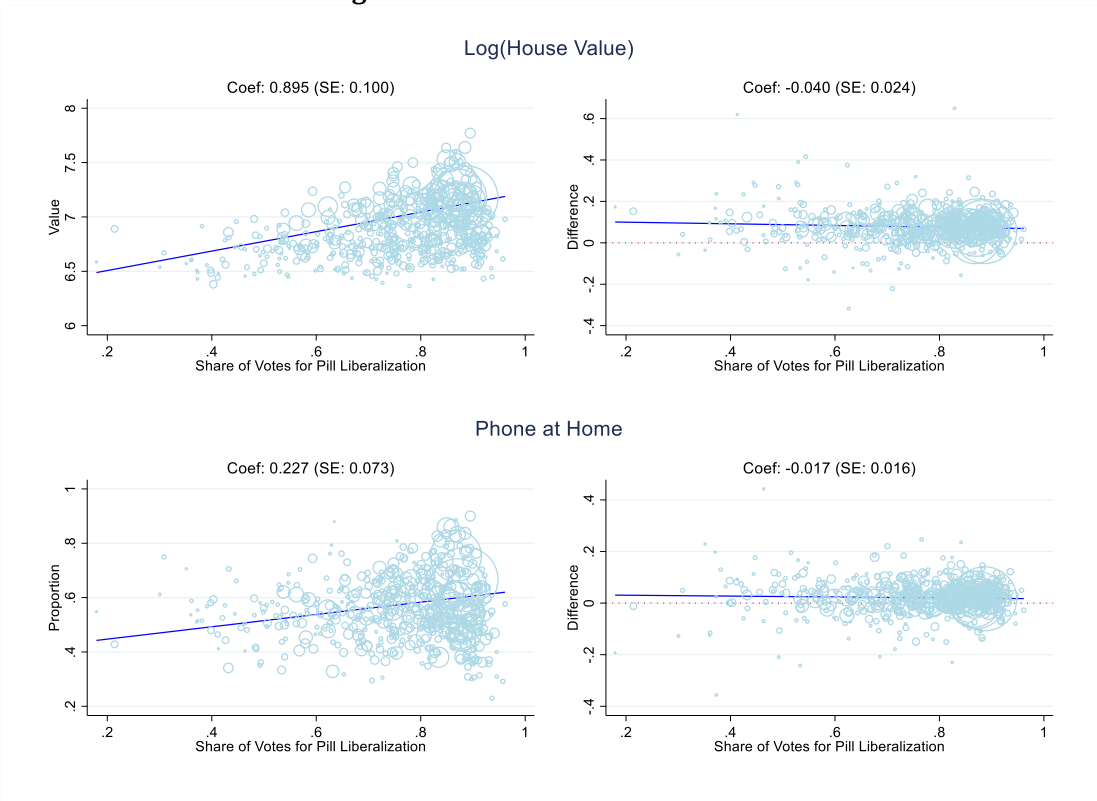


Figure A3. Continued.

Figure A3.3: House Value and Phone



Notes: The graphs above plot the mean value in a municipality of various household characteristics from the 1971 census against the vote share for parties in favor of pill liberalization in the 1967 parliamentary elections in that same municipality. We restrict the census sample to households with a head who reports to ever have had a child and is from a cohort who was statistically most likely to be the parent of a woman born between 1944 and 1954 (i.e. with a head aged between 46 and 61 in 1971). Note here that we do not use actual age of child present in as a household as a significant number of the older women from this sample have already formed their own household by the time of the 1971 census and we thus would not observe the characteristics of the household they actually grew up in. The typical parent of a woman born 1944 to 1949 – control women - is aged 46 to 55 in 1971 and the typical parent for a woman born 1950 to 1954 – treated women - is aged 52 and 61 in 1971. The left-hand side graph shows the mean values of each characteristics for all selected households in a municipality and the right-hand side graphs the mean value of the difference between the treated and control household within a municipality of these characteristics. The circles reflect population size of each municipalities which also serve as weights and the blue lines the fitted value of the correlation with the slope coefficient and standard errors reported above each graphs. Figure A3.1 reports the total number of children born to a head of household and the proportion of household where the head ever divorced. Figure A3.2 reports the proportion of household heads who have completed secondary education and the proportion of household heads which are classified as high income in the census (income higher than 16,000 a year, which are the two highest income groups and applies to 7% of household heads). Figures A3.3 reports the logarithmic value of the house or apartment the household lives in and the proportion of households who declare having a phone at home.

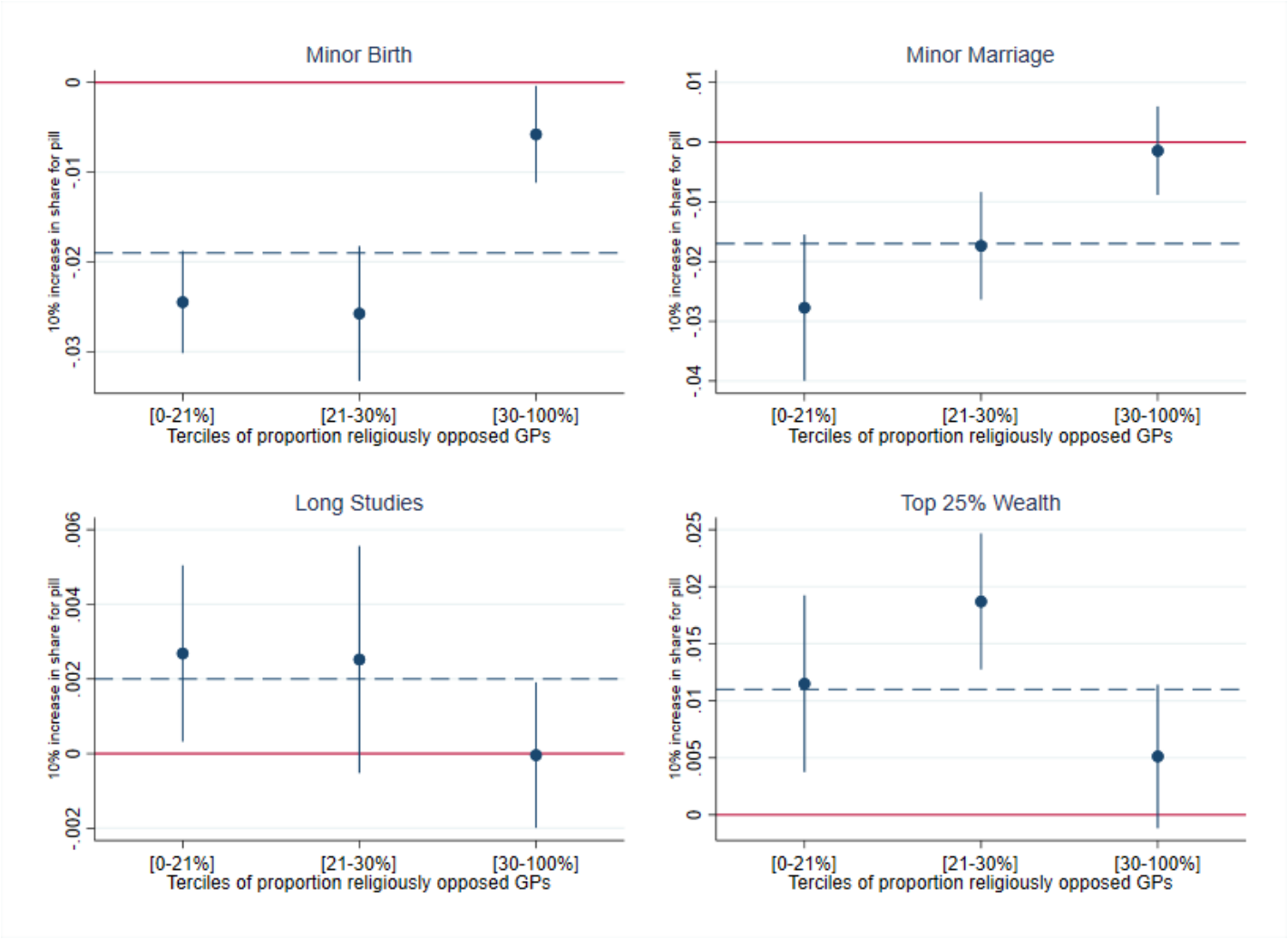
Figure A4. Random Assignment of Municipality Votes for Pill – 500 permutations

Notes: Densities of point estimates that are retrieved by 500 permutations of randomly assigning the instrument (ShareForPill) to other municipalities without replacement. The value of the instrument is randomly assigned at the municipality level, implying that all women in municipality A will now receive a value of the instrument of a different randomly chosen municipality. The figures plot the estimated point estimates for four outcomes: whether the woman became a mother before age 21, whether the woman married before age 21, whether the woman finished higher professional or university degree, and whether the woman ended up in the top 25% of the wealth distribution by age 60. The red line reflects the estimate in our main specification, only the specification for higher education contains some cases in which the estimated coefficients in the permutations are larger than our estimate in our main specification. However, this occurs for only 6 out of 500 times which is a reasonably small occurrence, and is likely caused by the smaller sample size for this outcome variable (about a quarter of the full sample).

Figure A5. Share of votes 'for' parties in favor of pill liberalization and the proportion of Orthodox Protestant (top graph) or Catholic (bottom graph) health professionals (HPs)

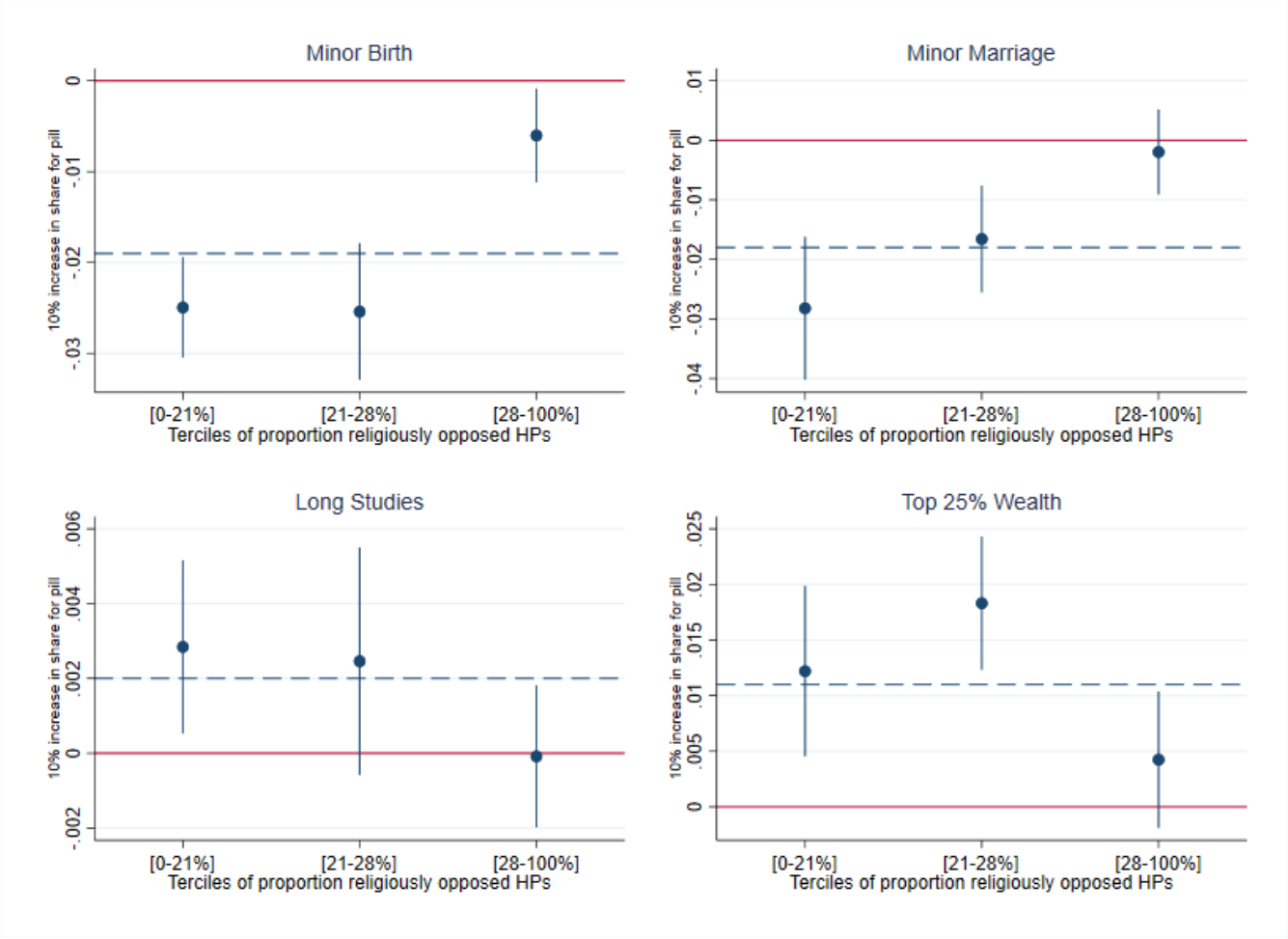
Notes: Variation in the proportion of religious health professionals (HPs) by the share of votes on parties in favor of the pill. The top graph shows the proportion of HPs who are Orthodox Protestant and the bottom graph the proportion of HPs who are Catholic. The circles are weighted by the number of health professionals in each municipality which is indicated by the size of each circle. The vertical dotted line indicate the median vote share for parties in favor of the pill in the 1967 elections (0.785) at the municipality level. The horizontal dotted line the median proportion of HPs who self-declare to be Orthodox Protestant (0.103) and Catholic (0.168) at the municipality level in the 1971 Census.

Figure A6.1 Robustness - GP Power: The additional effect of GPs who were against the pill, by tercile.



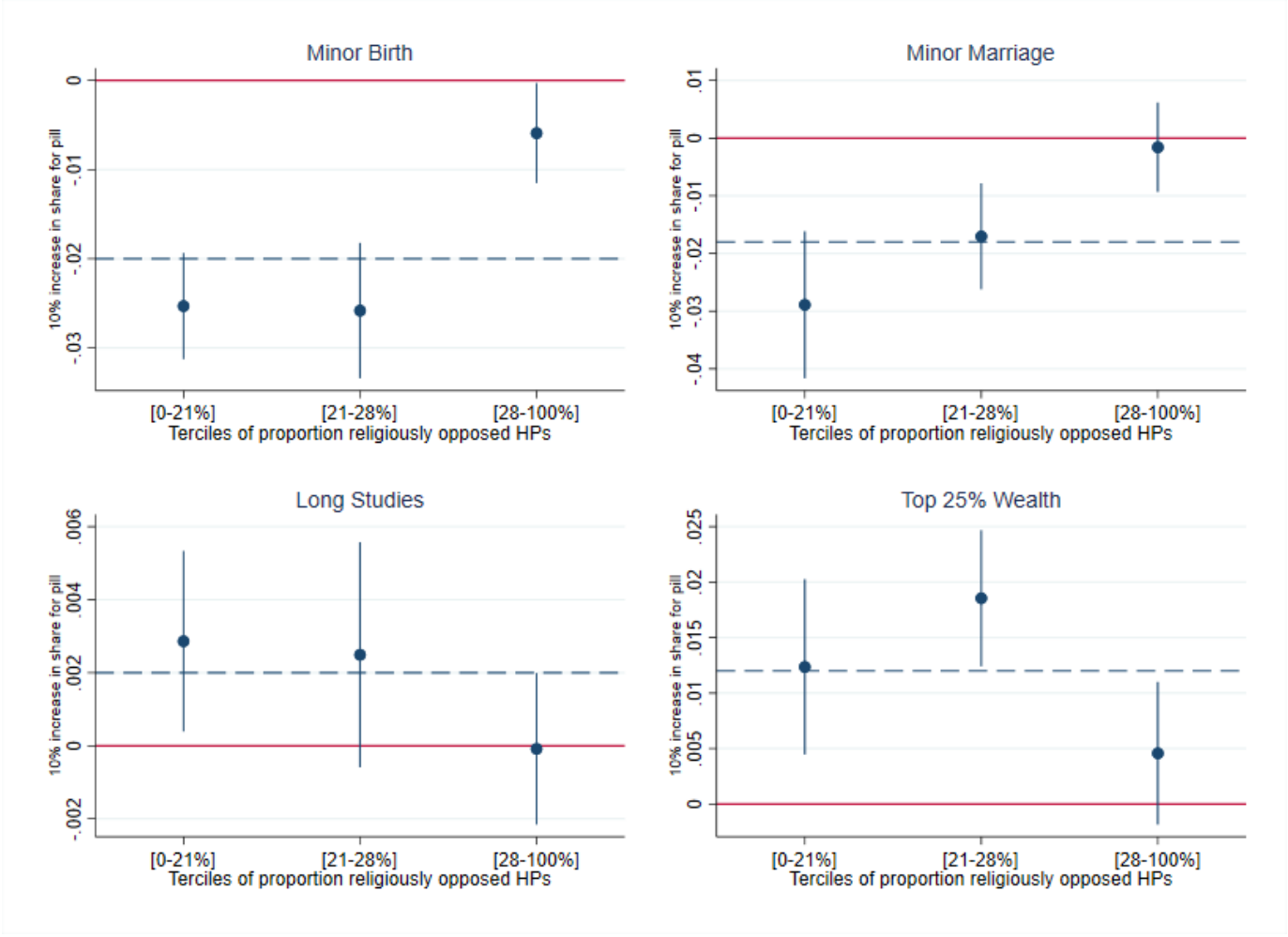
Notes: Estimated by OLS. Figures plot the additional effect of religiously opposed GPs (in terciles) on top of the social norms in an area (i.e. a triple interaction with our difference-in-differences estimator:$After_{ic} * ShareForPill_{im}$). ShareForPill is standardized with mean zero and standard deviation one. One standard deviation in ShareForPill is about 10%. The proportion of religiously opposed GPs (with mean 23.9 and standard deviation of 7.5%) is divided in terciles: 0-21%, 21-30%, and 30-100%. All specifications are restricted to municipalities with at least one GP and are weighted by the number of GPs in every municipality. Robust standard errors are clustered at the municipality (501) and cohort (11) level.

Figure A6.2 Robustness - HP Power: The additional effect of HPs who were against the pill, including municipalities without HP, by tercile.



Notes: Estimated by OLS. Figures plot the additional effect of religiously opposed HPs (in terciles) on top of the social norms in an area (i.e. a triple interaction with our difference-in-differences estimator:$After_{ic} * ShareForPill_{im}$). ShareForPill is standardized with mean zero and standard deviation one. One standard deviation in ShareForPill is about 10%. The proportion of religiously opposed HPs (with mean 23.1 and standard deviation of 7.5%) is divided in terciles: 0-21%, 21-28%, and 28-100%. We assign the proportion of religiously opposed HPs in the closest municipality for municipalities without HP. All specifications are weighted by the number of HPs in the (closest) municipality. Robust standard errors are clustered at the municipality (541) and cohort (11) level.

Figure A6.3 Robustness - HP Power: The additional effect of HPs who were against the pill, restricting to municipalities with at least 3 HPs, by tercile.



Notes: Estimated by OLS. Figures plot the additional effect of religiously opposed HPs (in terciles) on top of the social norms in an area (i.e. a triple interaction with our difference-in-differences estimator:$After_{ic} * ShareForPill_{im}$). ShareForPill is standardized with mean zero and standard deviation one. One standard deviation in ShareForPill is about 10%. The proportion of religiously opposed HPs (with mean 23.1 and standard deviation of 7.3%) is divided in terciles: 0-21%, 21-28%, and 28-100%. We drop 320 municipalities with fewer than 3 HPs. All specifications are weighted by the number of HPs in the municipality. Robust standard errors are clustered at the municipality (320) and cohort (11) level.

Appendix B: Data Appendix

B.1 Construction of sample

We use administrative data from Statistics Netherlands which contains information on all individuals who are registered in a Dutch municipality by 1995.[1] We start with the registry of persons (*GBAPERSOONTAB*) and select all women who were between the ages of 16 and 26 in 1970, and hence are born in the Netherlands between 1944 and 1954, which gives us a sample of 1,138,451 individuals. We then match these women to their municipality of birth using the place of birth file (*VRLGBAGEBOORTEGEMEENTE*).

Note that the Netherlands has constantly been changing municipal boundaries over time, and primarily through merging already existing municipalities. To be able to match our instrument (votes for parties opposing the pill in 1967) to the woman's municipality of birth we need to take the restructuring of municipalities into account.[2] We take these changes into account and assign the new municipal codes to women born in municipalities that have changed. In case municipalities split we aggregate to larger units (e.g. if municipality X splits and half goes to municipality A and half to municipality B we aggregate to one larger unit comprising both municipality A and B). We drop 22,267 women for whom we cannot identify their municipality of birth or cannot determine the vote shares opposing the pill in 1967.

The parent-child registry (*KINDOUDERTAB*) is used link the women in our sample to their children such that we can determine outcomes like age at first birth, as well as completed fertility (the youngest women in our sample are 65 in 2018, implying that we observe them long past their prime childbearing ages). We drop 493 women for whom age at first birth is lower than twelve years of age. We are left with 1,115,691 women who were born in the Netherlands and between the ages of 16 and 26 in 1970. Given the small variation in voting patterns in the southern provinces of the Netherlands (i.e. Noord-Brabant and Limburg) we drop women born in the south, which leaves us with a final sample of 864,370 individuals.

[1] The administrative data from Statistics Netherlands is available at a remote-access facility after signing a confidentiality agreement.

[2] See "*Gebieden: Overzicht vanaf 1830*", available at www.statline.cbs.nl for an overview of changes in municipal boundaries in the Netherlands up until today.

B.2 Construction of outcome variables

Using the parent-child register we generate a measure indicating that a woman remained childless throughout her life, and a measure for the total number of children per woman (i.e. completed fertility). For the 735,204 women who ever had a child we create a variable for age at first birth, and we define a minor birth as a birth to an individual below 21 years of age (the age of majority in the Netherlands at that time).

The marital state register (*GBABURGERLIJKSTESTAATBUS*), contains information on all present and past marriages for individuals registered in a Dutch municipality from 1995. An indicator for whether the woman was ever married in her lifetime (again this implies before 2019) is generated. For the 805,870 women who ever got married we generate a variable for age at first marriage, and we define a minor marriage as a marriage when the individual is below 21 years of age. Finally, for the 727,201 women who ever got married and ever had a child, we define a shotgun marriage if the child is born within seven months of the mother's marriage date. The seven-month time window (instead of eight or nine months) is chosen such that premature births are not 'accidentally' captured. Shotgun wedding is our preferred measure for an unanticipated pregnancy over out-of-wedlock birth a large majority of children were born within wedlock (in 1970 only 2.1% of live births were not born in wedlock)[3].

In the long term we are interested in the effects of birth control technology on the women's human capital formation. We add information from the registry with information on the individual's highest obtained level of education (*HOOGSTEOPLTAB*). This registry has limitations as the collection of educational records only started in 1999, and any degrees that were obtained earlier were retrospectively inferred from surveys. This means that information on educational outcomes is only available for 218,119 women (about 25% of the sample). We examine whether birth control technology allowed women to invest in degrees with longer qualification periods, to this end we create an indicator variable that takes the value one if the woman finished a university of applied sciences degree or university education. We also add a variable indicating that a woman finished a long-duration degree in law or medicine (medical school, dental medicine, and veterinary medicine).

[3] See Statline, Bevolking, huishoudens, en bevolkingsontwikkeling; vanaf 1899 (statline.cbs.nl).

The data on yearly earnings from paid employment (*BAANPRSJAARBEDRAGBUS*) and self-employment (*ZELFSTANDIGENTAB*) is available from 1999, which means that age 55 is the earliest age at which labor market outcomes can be observed for cohorts 1944-1954. The measure of labor force participation at age 55 is continuous and represents the labor force participation in terms of FTEs and only corresponds to working in paid employment (such measure does not exist for self-employment). One FTE represents a full-time job (eight hours a day, and five days a week), but given that Dutch women often work part-time it is important to take hours worked into consideration. For women with non-zero income in both paid and self-employment a variable for earnings at age 55 is created.

Finally, we are interested in how access to the pill at young ages affects the accumulation of household wealth. Information on household wealth (*VEHTAB*) is available only from 2006, when the oldest birth cohort is aged 62. We determine mean household wealth at ages 60-62 for the women in the sample. This implies that for women born in 1944, household wealth is only observed at age 62, but for women born in 1946 household wealth is observed at ages 60 to 62 in which case the mean is taken over these years. The measure of household wealth includes all assets owned by the household minus the debts. Assets include the household's bank balance, savings balance, stocks and bonds, the value of their house, and the value of their business. The wealth outcomes are not observed for individuals who are not living in the Netherlands at the ages of 60-62 or for individuals living in institutional households. For wealth and earnings outcomes we restrict our sample individuals with positive wealth at ages 60-62, which gives a sample of 758,024 individuals, such that our coefficient estimates are comparable.

B.3 Construction of Census data on health professionals

The 1971 full count Census is used to identify the proportion of religious health professionals in each municipality. The 1971 Census contains information on 13,133,333 individuals, we drop 73,216 individuals for whom it is unknown how their outcomes were registered, and 3,588 individuals without a fixed place of residence (in total 0.3% of the sample), which leaves 13,056,529 observations and 10,233,915 when excluding the south.

Health professionals (HPs) are defined as pharmacists and general practitioners (GPs) as those were the key professionals who could provide women access to the birth control pill. In total, we can identify 1,120 pharmacists and 5,265 GPs in the Netherlands in 1971, which is similar to

the numbers reported by Statistics Netherlands for 1971, namely 4,504 GPs and 1,084 pharmacists (Centraal Bureau voor de Statistiek, 1994, p. 265). We exclude health professionals in the southern provinces, which gives us a total of 5,261 health professionals (4,326 GPs and 935 pharmacists).

To determine the proportion of religious health professionals in each municipality we use the religion variable that is available in the Census. Religion was elicited for 95.6% of individuals and imputed for those for whom it was not elicited. Despite this, **table B1** shows that the distribution of religion is very similar in the samples in which religion was and was not elicited, for both the full population and the sample of HPs. Hence, it is unlikely that this will present any bias in the setting up of our religious health professional measure.

We use the religion (*kg*) variable and define Catholics as individuals reporting to be a member of the Roman Catholic, Old Catholic or Free Catholic church (codes 10, 59, and 63), we define Orthodox Protestants as individuals who report to be a member of the Reformed Church, the Free Reformed Church, the Christian Reformed Church, the Reformed Association, or the Old Reformed Association (codes 30, 31, 32, 33 and 34), we define Liberal Protestants as people who report to be a member of the Dutch Reformed Church (code 20). All remaining religions are grouped under "Other", and those who report that they are not religious are defined as such (code 1).

Table B1: Distribution of religion, depending on the elicitation of religion.

	All	All - religion elicited	HPs	HPs – religion elicited
No religion	28.58	28.25	38.17	37.62
Reformatory	11.52	11.73	9.81	10.25
Catholic	27.22	27.34	16.80	16.28
Liberal Protestants	28.74	29.02	25.47	25.96
Other	3.94	3.66	9.75	9.88
N	10,233,915	9,781,219	5,261	4,827

Notes: Authors' calculations based on the 1971 Census. The first column includes all individuals in the 1971 population Census excluding the southern provinces of Noord-Brabant and Limburg. The sample of HPs includes general practitioners and pharmacists. The table compares the distribution of religion for individuals for whom religion was elicited and for those for whom religion was not elicited.

Health professionals are assigned to municipalities based on the municipality in which they live in 1971. The Census also elicited information on the municipality in which individuals are

working, but this information is missing for about 1% of the HPs in our sample. This seems like a small number overall, but it may affect our access measure. At the same time, we know from the Census that about 92% of health professionals do not commute to a different municipality for work. As a robustness check we calculate the proportion of religious health professionals (any religion versus no religion) using municipality of work, and the proportion of religious HPs by restricting to HPs who do not commute. These measures are very highly correlated with the measure on municipality of residence, Pearson correlation coefficients of 0.9977 and 0.9982 respectively.