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## Abstract

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JEL Classification: I18, J12, J13, Z12

Keywords: Birth Control, religion, Fertility, Marriage, Human Capital, The Netherlands



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# Religious Barriers to Birth Control Access*

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## I. Introduction

Legal access to oral contraceptives has had dramatic effects on women's lives. Goldin and Katz (2002) were the first to document the powerful impact of the birth control pill on marital and educational outcomes of young college-educated women in the United States. Bailey (2006) followed by showing that it enabled *all* women to delay motherhood and increase their participation in the labor force. Other works show that pill liberalization in the United States also increased the share of children with college-educated and non-divorced mothers (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). Granting women legal access to any technology that improves fertility control, including oral contraceptives, has always been met by strong religious resistance.[3]

Surprisingly little is known about how much moral opposition to birth control, and in particular the Pill, might affect those who could potentially benefit from its effects. Bailey (2006) does suggest moral opposition might matter when noting that higher Catholic parish membership in American states is associated with delays in pill liberalization. Moreover, legal delays are not the only issue as de jure access may not guarantee de facto availability if moral frictions remain strong enough to prevent women from adopting certain birth control methods. These frictions can be driven by both *demand*, specifically, a woman's own or family religious beliefs, or *supply*, specifically, the moral values of the technology providers. Surveys reveal that religious health professionals are less likely to provide (emergency) contraceptives and induced abortion (Spivack 1964; Rubin et al. 2006; Lawrence et al. 2010; Stulberg et al. 2011), demonstrating that the beliefs of those who grant access to birth control matter. These moral barriers are even sometimes legitimized as, for example, nine U.S. states have laws that allow health providers to refuse

[3] Affiliation with an evangelical Protestant church is today the single most predictive characteristic of an individual's opposition to abortion rights in the United States— more than gender, age, or education—as documented in an analysis of polls following the Supreme Court leak about a possible repeal of *Roe v. Wade.* https://www.economist.com/graphic-detail/2022/05/07/religion-not-gender-is-the-best-predictor-of-views-on-abortion. Historically, in his 1968 encyclical Humanae Vitae (on Human Life), Pope Paul VI dedicated a section to "Unlawful Birth Control Methods," in which he reaffirmed the Catholic church's position not only on abortion but also on the oral contraceptive pill, which were considered to be "intrinsically wrong." He asked public authorities to not tolerate "any legislation which would introduce into the family those practices which are opposed to the natural law of God." https://www.vatican.va/content/paul-vi/en/encyclicals/documents/hf_p-vi_enc_25071968_humanae-vitae.html

contraceptive services, and six U.S. states explicitly allow pharmacists to refuse dispensing contraceptives.[4]

Our study is the first to consider how moral frictions surrounding the liberalization of the birth control pill might impact women's short- and long-run outcomes. The focus is on the Netherlands, where access to the Pill for minors (those aged 21 and younger) was legalized in 1970. We first examine how strong demand side frictions—as proxied by the area-level vote shares for political parties in favor of pill liberalization—affected the extent to which young women benefitted from improved access to oral contraceptives. After finding that the benefits of the Pill are large, we turn our focus to the additional effect of supply-side moral frictions by considering the religious beliefs of the "gatekeepers" women faced when seeking to gain access to birth control. We show that the positive impact on outcomes from birth control liberalization for women inclined to take up the new technology was annihilated when more gatekeepers were morally reluctant to prescribe the Pill.

This paper is the first to comprehensively study the short- and long-run impact of liberalizing access to oral contraceptives outside of the United States[5] and to exploit religious frictions to do so in general. Studying this question in the Dutch setting is of particular interest for several reasons. First, there is substantial local variation in religious beliefs and large (uncorrelated) variation in the religiousness of the technology gatekeepers that allows us to investigate moral frictions on both the demand and supply side. Second, induced abortion was only liberalized 14 years after oral contraceptives,[6] making it possible to estimate a relatively pure, and potentially powerful, pill access policy effect.[7] Third, our administrative registry data allow us to observe the fertility decisions of all women in the Netherlands along with information about their marriage, education,

[4] The Alan Guttmacher Institute documents the state policies in the United States regarding refusing health care services as of June 1st, 2022. https://www.guttmacher.org/state-policy/explore/refusing-provide-health-services

[5] Two papers have studied narrower question of how much uptake of oral contraceptives is sensitive to changes in their prices. They show that both in Sweden (Gronqvist 2012) and Chile (Rau, Sarzosa, and Urzúa 2017), contraceptive consumption is highly elastic and that cost changes have thus important short-run effects. As we explain later, price will not be an issue in our setting as, at the same time as it became legally available to minors in the Netherlands, it was reimbursed by social security.

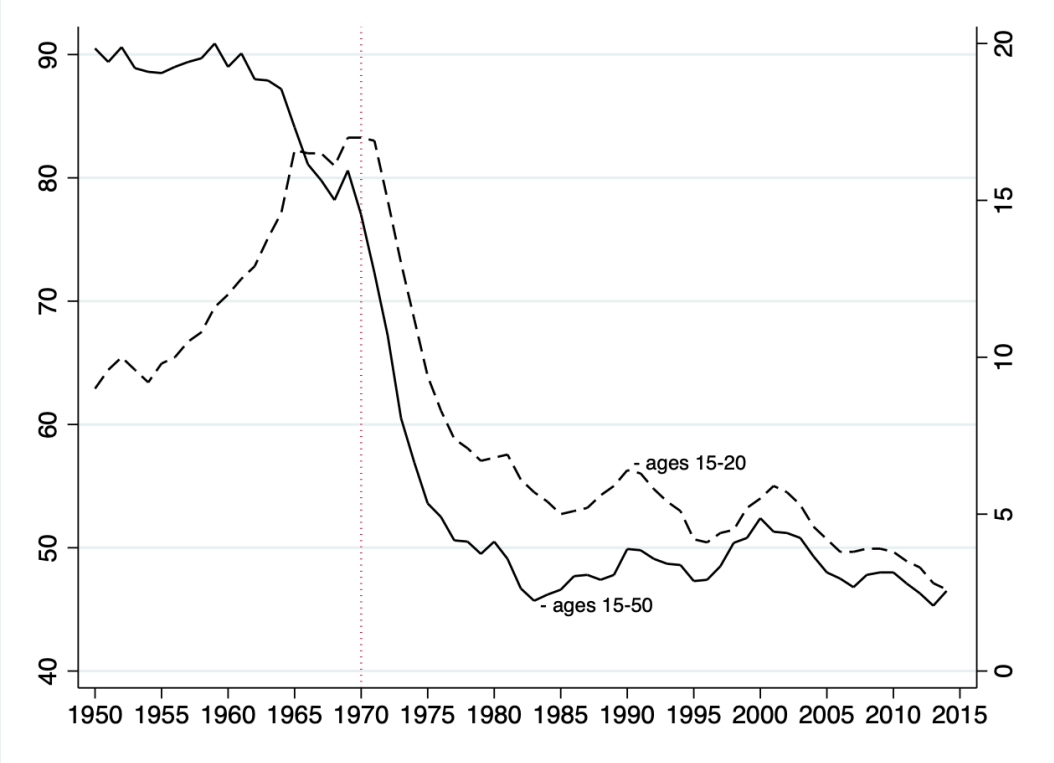
[6] Induced abortion was allowed to save the mother's life until 1966, and by 1967 abortion teams were introduced at 11 clinics, with the power to determine whether a woman was eligible for an abortion. By 1972, women could request an abortion at these clinics without having be found eligible on medical grounds. The abortion rate remained low by international standards despite the 1972 change (see **Figure A1**), and especially in comparison to the high take-up of oral contraceptives (see **Figure 2**). However, given that the latter change in abortion access happened at about the same time as pill liberalization, we carefully show that the availability of these clinics does not impact our results.

[7] There have been recent discussions in the United States about the effect of the Pill and abortion legalization in women's lives given that access to both was liberalized in the same time period (see Myers 2017).

labor market, and (household) wealth outcomes up to four decades after access was gained. We can thus explore both short- and long-run effects for cohorts who are precisely defined in age and location at the time of the policy change. Finally, we have access to self-reported religious affiliation of all individuals in the Netherlands, including health professionals, from the 1971 census. This crucially enables us to measure the supply-side moral frictions women were likely to encounter locally, precisely at the time of pill liberalization.

We first document in **Figure 1** that the birth rate for women of childbearing age (15–50) fell by 25 percent in the five years following pill liberalization. Most strikingly, the change in the birth rate to minors—the age-group for whom oral contraceptive access was previously most restricted—dropped by almost 45 percent in the same period. The declining trend in fertility coincided with a large increase in the take-up of the Pill.

**Figure 1**. Birth Rate and Teenage Birth Rate, Netherlands 1950–2014

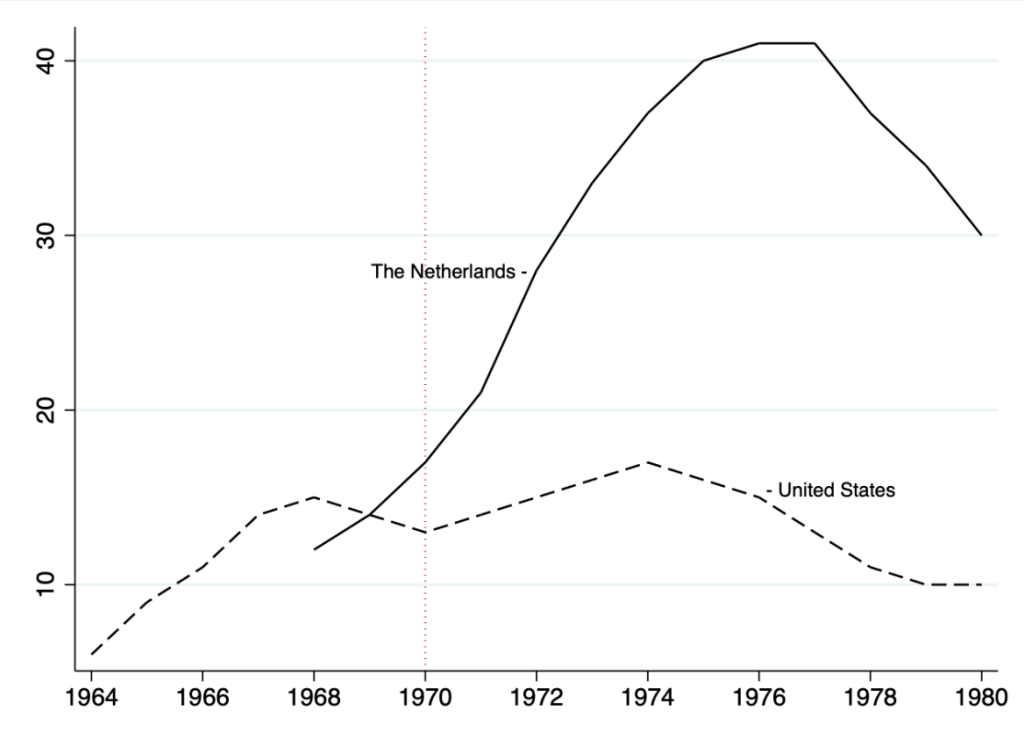



Notes: Number of births per 1000 women for 1950–2014 in the Netherlands, for women ages 15–50 years old (left axis) and women ages 15–20 years old (right axis). The vertical dotted red line marks 1970, the year when pill access was liberalized in the Netherlands. Source: CBS Statline, statline.cbs.nl

**Figure 2** documents how the contraceptive pill quickly became the most important birth control technology in the Netherlands after its liberalization in 1970. By 1975, more than 40

percent of women between the ages of 15 and 44 were using it. We also present evidence in appendix **Table A1** that liberalization coincided with a drastic reduction in unplanned pregnancies.[8] These three stylized facts do suggest that pill liberalization did impact women's ability to control their fertility decisions in the Netherlands. As many other secular trends might be behind this, we turn to investigating moral barriers to access to obtain causally interpretable estimates of its effect.

**Figure 2.** Percentage of Women Buying Oral Contraceptives: the Netherlands and the 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).

We first examine moral frictions on the demand side of the Pill. We use the share of votes for political parties in favor of the liberalization of oral contraceptives to proxy for area-level views about the Pill. In the Netherlands, the parties representing Orthodox Protestants were fiercely

[8] Vennix (1990) states that only 37 percent 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 percent 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 from 1966–1970, 27.8 percent reported that their child's birth was unplanned, whereas this was only 8.4 percent for women giving birth from 1971–1975 (**see Table A1**).

opposed to contraception, while nondenominational parties and those representing Catholics and Liberal Protestants were in favor. In line with this, we document that women voting for Orthodox Protestant parties were about twice less likely to take up the Pill than those voting for any other political party. Our measure thus proxies the "average" moral views about oral contraceptives in an area that will affect the probability of take up of the Pill when it becomes available. This includes the beliefs of the women themselves, but also those beliefs of their partners and parents. We then use a continuous difference-in-difference framework in which we compare the outcomes of women from similar areas, who gained legal access to the Pill just before or after their twenty-first birthday—the age before which pill use was categorically banned until 1970.

Our findings show that women who gained access to the Pill as minors in more liberal areas—that is, a one standard deviation higher vote share, or 10 precent, for pro pill parties—were 12 percent less likely to become mothers as minors and had 6 percent fewer marriages before turning 21. These women were then 28 percent more likely to complete the higher education degrees that take the most time to finish (i.e., Medical Doctor (MD) or Juris Doctor (JD)). These increases in human capital investment translate, for those working, into large increases in the proportion working in high-paying jobs by their mid-50s. As only half of women are in the labor force at this age, we consider household wealth as an alternative measure of economic wellbeing. Here again, the positive long-run impact of the Pill is clear: women who grew up in areas with lower moral resistance to improved legal access were significantly more likely, whether they worked or not, to belong to households in the top quartile of the national wealth distribution.

We then investigate the additional impact that supply-side moral frictions may have had on women's outcomes, given area-level demand-side frictions. In the Netherlands, women could only obtain oral contraceptives with a doctor's script at a pharmacy. This implied that general practitioners (GPs) and pharmacists were essentially functioning as gatekeepers to the Pill. We first document that Orthodox Protestant and Catholic GPs were much less likely to prescribe the Pill, particularly to young or unmarried women. We then use the 1971 Dutch census to identify the religious affiliation of health professionals (HPs, defined as GPs and pharmacists) in every Dutch municipality.

We demonstrate that there is a lot of area-level variation in moral views about the Pill and in the fraction of Orthodox Protestant and Catholic health professionals: there are liberal areas with a large share of gatekeepers opposed to the Pill on religious grounds, and vice versa. Importantly,

this mismatch in moral views of local women and that of their health professionals is not driven by any other access-related observable characteristics. Our analysis reveals that in places where it is harder to find a GP or pharmacist that is not opposed to the Pill on religious grounds, legal access had no impact on any of the main short- and long-run outcomes considered. Hence, supply-side moral barriers effectively nullified the effects of the liberalization of contraceptives in these areas.

In addition to contributing to the literature on the power of birth control technology to shape women's outcomes, especially the literature uncovering new evidence on the importance of contraception's gatekeepers, this paper more broadly relates to studies on religion and fertility in economics. For example, Munshi and Myaux (2006) demonstrate that own contraceptive use responds strongly to changes in contraceptive use behavior in one's own religious group. Beach and Hanlon (2019) show that religion played an important role in historical fertility transition in the United Kingdom. Bassi and Rasul (2017) and Farina and Pathania (2020) find that papal visits, in Brazil and Italy respectively, impacted contraceptive use behavior. Our paper also adds to an evolving branch of literature on the role of health provider beliefs on prescribing behavior and patient outcomes. These include Schnell (2017), who shows that physician altruism influences opioid prescribing, and Alsan et al. (2018), who find that patients opt for more preventive care when consulting with a doctor of the same race.

# 2. Institutional background

## 2.1 The 1911 Morality Law Banning Contraceptives

During the last decade of the nineteenth century, liberal political parties governed the Netherlands. In 1901, a new cabinet was appointed; it was 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 (*Zedelijkheidswet* in Dutch) in 1911. Its principal objective was to legislate sexual activity to take place only within a marriage and for the sole purpose of reproduction. It contained provisions about contraceptive use, 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 (about 5,000 US dollars in 2022).[9,10] Penalties were three times more severe for displaying or recommending contraceptive methods to minors, defined as those below the age 21, meaning a prison sentence of at most six months or a fine of 1,200 guilders (Rensman 2006). These conservative laws stayed unchanged for almost six decades, in the background of the development and (medical) introduction of the contraceptive pill in the Netherlands.

## 2.2 The Development of the Dutch Pill

The Dutch birth control pill, Lyndiol, was developed by pharmaceutical company Organon (around the same time period as Enovid, the Pill developed in the United States). Lyndiol contained an artificial hormone, lynestrenol, that by 1957 could be used as an oral contraceptive for women. Morality laws and conservative views on contraceptives meant that Organon maintained secrecy around its development and production of Lyndiol (the "Pill"). The packaging and distribution of the pill was even outsourced to nunneries when the demand for the Pill later increased: packaging by secular factory workers could have tempted the workers to engage in immoral behavior, but nuns were considered less "corruptible" (Rensman 2006).

The Dutch Pill eventually became first available in pharmacies in 1963, but as a gynecological medicine that regulated the menstrual cycle with a side effect of causing 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.[11] As a result, GPs, 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 beginning in 1963, the Morality Laws still forbade promoting and making the Pill available because it was an instrument that could interfere with a

[9] 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 and Katz 2002).

[10] There was only one exception: contraceptives 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).

[11] 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. Hofstee (2012, 212) argues that "by making ovulation inhibitors only available by doctor's script, the responsibility and ethics of prescribing were all transferred to the doctor."

pregnancy. Thus, in its first years, it was mainly prescribed to women in very fertile marriages who could experience negative health consequences from another pregnancy (Bekkering 1969). Young unmarried women commonly did not have access to the Pill in its early years.

## 2.3 The Repeal of the Morality Law and Access to the Pill

Views regarding contraceptives were evolving in the 1960s. The Netherlands was coping with a fast-growing population and started experiencing the negative consequences this had 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.[12] These factors eventually led to the repeal of the Morality Law in 1969. This repeal made it legal to provide information about contraceptives in speech or writing, and contraceptives were no longer age restricted.[13] 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, as the Pill became more accessible, the role of "gatekeepers"—the general practitioners who had to recommend and prescribe the Pill and the pharmacist who had to dispense it—became enhanced. (**Section 2.6** details their role).

**Figure 1** reveals a large drop in the birth rate after the repeal of the Morality Law, particularly for minors for whom access to the Pill was liberalized the most.[14] There are two reasons why the Pill was most likely behind this large fertility effect in the Netherlands. First,

[12] 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 made women 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) had to give up their job at the time of marriage had been repealed two years earlier, in 1955 (Rensman 2003).

[13] 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 was the most used contraceptive by Dutch women at 34.1 percent, compared to condoms at 10.3 percent and 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, unlike the use of condoms, women could take the Pill without their partner's knowledge, which is why we believe that the Pill was particularly important for women around that time.

[14] 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, it also coincides with the contraceptive pill becoming available to married women in the Netherlands who had reached their desired level of completed fertility.

**Figure 2** shows the fertility effect coincided with a huge increase in take up of the Pill in the Netherlands: almost 40 percent of women aged 16–45 were using it by 1975. This figure is much higher than the proportion using oral contraceptives in the United States in that same year, which was only about 16 percent.[15] Second, induced abortion was only legalized in the Netherlands in 1984 and, even though tolerated in exceptional cases, the practice remained uncommon over the course of the 1970s and mid-1980s (see **Figure A1**). The strong take up of oral contraceptives is often proposed as the primary explanation for the relatively low abortion rate in the Netherlands by international standards (Ketting and Schnabel 1980; Ketting and Visser, 1994; Levels et al. 2012). This, again, stands in sharp contrast with the figures for the United States, where we see a large upsurge in the abortion rate starting in the 1970s.[16]

## 2.4 Timing of the Repeal and Political Opposition

We plan to exploit the repeal of the Morality Law to identify the effects of contraceptive access on women's fertility and economic outcomes. The repeal coincided with other societal changes that could also affect both outcomes, raising the question of its relative exogeneity. Importantly, there were a few political events that created unforeseen delay, implying that society was ready for the repeal a few years before the law was abolished. The coalition government in place beginning in April 1965 had committed to the repeal of the Morality Law and submitted two bills to parliament by September 1966 (Hofstee 2012). Before the bills could be discussed in parliament, a crisis arose due to budget disagreements—unrelated to the Morality Law—which led to a collapse of the coalition government in November 1966, and new elections being held in February 1967. It would take until May 1969 before the bills were discussed in parliament again. The bills eventually passed in June 1969, almost three years after first raised for discussion (Rensman 2006).

[15] One explanation for the low take-up of the Pill in the United States compared to the Netherlands could be the high costs associated with the use 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 at the minimum wage in the 1960s. By contrast, the Pill became free for most individuals in the Netherlands after its inclusion in the National Health Insurance (the health insurance scheme for low-income individuals) in 1971.

[16] 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 pregnancies" this caused, as one of the reasons that the availability of induced abortion was the main driver behind improved fertility control in the United States. 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, 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).

A large majority of members of parliament voted in favor of the repeal of the Morality Law. This included those from the Catholic People's Party (in Dutch: *Katholieke Volkspartij*) which was in line with their 1967 election manifesto that stated that "the responsibility for determining the size of the family lies with the parents." The only parties who voted against the repeal were those linked to the Orthodox Protestant Church with strong Christian values and conservative moral norms: specifically, the Reformed Political Party (in Dutch: *Staatkundig Gereformeerde Partij* or SGP*)*, the Anti-Revolutionary Party (ARP), and the Farmers' Party (Hofstee 2012).[17] The division in parliament suggests that, even though the Morality Law was abolished, there were big differences in views about the desirability of making contraceptive access universal.

## 2.5 Demand-Side Moral Frictions

We investigate the impact of demand-side frictions by exploiting area-level variation in attitudes toward the Pill. Our assumption is that adoption was slower in areas with more-conservative religious views. As a proxy, we use the share of votes for the three parties that voted against the repeal of the Morality Law. Voting 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 use the distribution of votes in this election at the municipality level. Turnout was almost universal because, at the time, voting was mandatory for individuals above the age of majority (including women). The Anti-Revolutionary Party, the Farmers' Party, and the Orthodox Protestant Party, respectively received 2.0 percent, 9.6 percent, and 4.6 percent of votes nationally.

**Figure 3** reports municipality-level variation in the proportion of votes for the three parties that voted against the repeal of the Morality Law.[18] It shows a lot of variation in the share of votes

[17] None of these parties specifically mentioned the potential repeal of the Morality Law in their manifestos, but they all highlighted religion as guiding any policy decision related to societal change. The ARP came the closest by mentioning contraceptive access, stipulating that this should not be at the cost of "good morals, as well as the protection of youths." The SGP simply stated that its first principle was that "Government Policy [must] be in accordance with the Law of God, which implies strong actions against" all forms of "moral degeneration." We refer to all three political parties together as "Orthodox Protestant parties" in the remainder of the paper.

[18] We omit two Catholic-majority provinces in the south of the Netherlands as there is very little variation in religiosity in these areas. In these provinces of North Brabant and Limburg, the Catholic People's Party received 90.7 percent of votes in the 1967 election, and over three-quarters of health professionals self-identified as Catholic in the 1971 census. Panel (a) of **Figure A2** reports the proportion of Catholics in all municipalities of the Netherlands to confirm that these two provinces are almost entirely Catholic, thus providing no real variation in religious norms for our analysis

for the three parties that voted against the repeal of the Morality Law, ranging from 4 percent to 82 percent. It also quite clearly identifies the Dutch "Bible Belt," from the southwest to the northeast.[19]

**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) in 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

A key assumption for using vote share as a proxy for pill use probability is that oral contraceptives adoption is significantly different across political preferences. **Table 1** presents

on either the demand or supply side. Estimates of all results, including these two provinces, all remain significant and are of the same magnitude, if somewhat smaller given the reduction in treatment intensity, than for our main analysis.

[19] Panel (b) of **Figure A2** shows regional variation in the proportion of individuals who declared that they were 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 the Orthodox Protestant parties in 1967 and the proportion of individuals who self-report as being Orthodox Protestant in 1971 follow each other closely. **Table A8** shows that the correlation between both measures is 0.808.

evidence from two sources to show that women voting for the Orthodox Protestant parties are much less likely to have used contraceptives and in particular the Pill. Our first source is a small-scale survey between 1986 and 1989, reported in Vennix (1990), which reveals that women voting for parties that were against the repeal of the Morality Law were about half as likely to have been using the contraceptive pill compared to any other group, including those who voted for the party representing Catholics. Our second piece of evidence comes from the much larger Family Planning Survey (1988–2008). The percentage of women who reported no contraceptive use was very similar in both surveys, but more women were using the Pill in all groups given that it covered later years and overall take-up had increased. However, again, pill use was still about twice as high for women not voting for Orthodox Protestant parties. This evidence confirms that Orthodox Protestants were by far the most resistant to adopting the Pill and that there was a high level of take up among Dutch Catholics.[20]

**Table 1.** Contraceptive Use by Political Party Affiliation

| | No contraceptives | Contraceptive pill | N |
|---|---|---|---|
| ***Panel 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 |
| | | | |
| ***Panel 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 |

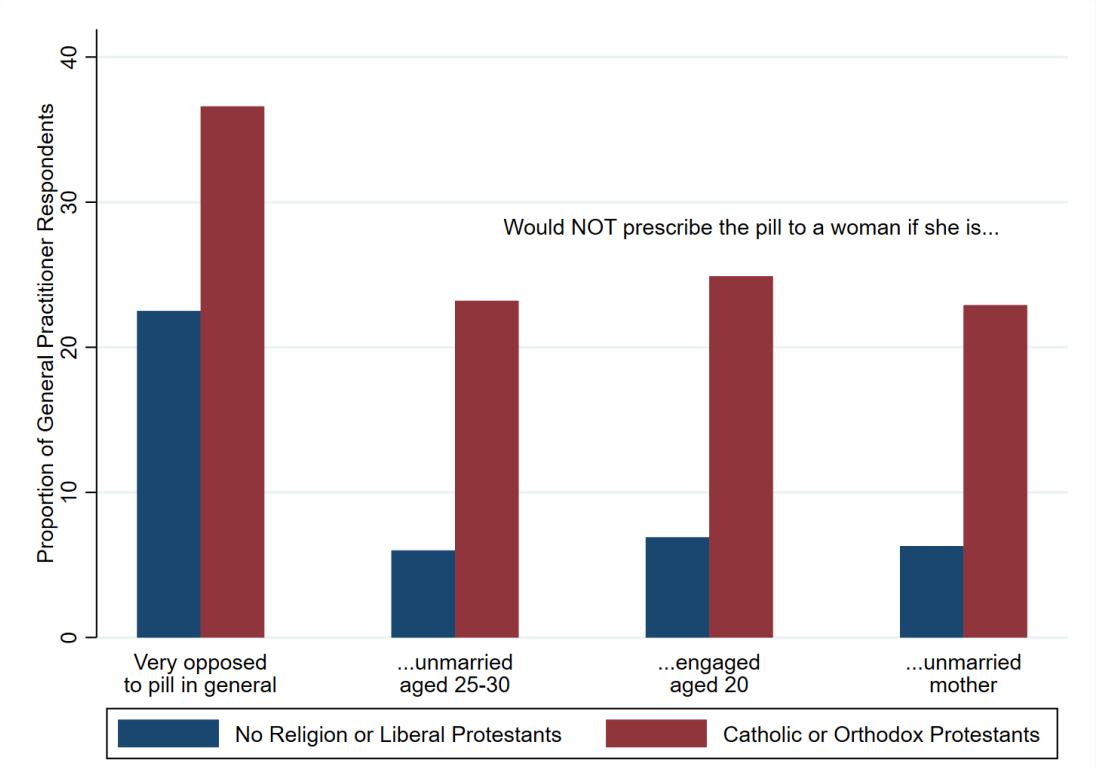
Notes: The Catholic People's Party ceased to exist in 1980, and a new party for Christian Democrats (the Christian Democratic Appeal, or CDA) was founded from the Catholic People's Party (KVP), the Anti-Revolutionary Party (ARP), and the Christian Historical Union (CHU). Panel A: Authors' calculations based on Table 36 (page 35) from Vennix (1990). The survey was initiated by the Dutch Ministry of Health and executed by NISSO (Nederlands Instituut voor Sociaal Seksuologisch Onderzoek) between 1986 and 1989; it 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 (D66), 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 (D66), and the Green Party (GroenLinks).

[20] This could be partially explained by large differences in "religious rigor" between those self-identifying as affiliated with either of these religions in the Netherlands: 54.4 percent of Orthodox Protestants report going to church at least once a week, while this is the case for only 14.7 percent for Catholics (see **Table A2**).

## 2.6 Moral Frictions on the Supply Side

To examine supply-side moral frictions, we exploit variation in the beliefs of gatekeepers at the time of the liberalization. The Pill was only available through a 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 as the pill was legally available for all women after the repeal of the Morality Law, access might still be restricted by GPs who did not want to prescribe it or pharmacists reluctant to dispense it. Differences in the likelihood of supplying otherwise legal contraceptive methods because of one's own religion is not uncommon among health professionals as clearly illustrated by evidence from the United States.[21]

**Figure 4.** Physicians' Opposition to the Pill by Religious Affiliation




Notes: Authors' calculations based on Bangma (1970). Survey administered among 528 general practitioners in 1969, which was about 12 percent of the total number of GPs in that year (Centraal Bureau voor de Statistiek, 1994, 265). The first column shows the percentage of GPs who stated they were very opposed to the use of the Pill as a contraceptive method, and the second, third, and fourth columns show 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.

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

We show here that moral frictions also played an important role when it came to accessing the contraceptive pill in the Netherlands. Its introduction as a medication for the regulation of the menstrual cycle—with the undesired side effect of temporary infertility—was meant to increase the likelihood that religious doctors would accept and prescribe the Pill (Hofstee 2012). Still, both Orthodox Protestant *and* Catholic general practitioners (GPs) remained markedly more resistant to prescribing the Pill at the time of the liberalization than physicians from all other religious persuasions (Bekkering 1969).

**Figure 4** uses a large survey from Bangma (1970) that investigated the attitudes of Dutch GPs towards the pill at the time of the repeal of the Morality Law to illustrate this point. It compares physicians' opposition to the introduction of the Pill in general, and their opposition to prescribing the Pill to specific groups of women, depending on their own religiosity. Orthodox Protestant and Catholic doctors were 60 percent more likely to be "very opposed" to the use of the Pill as a contraceptive compared to other physicians (36.6 versus 22.5 percent). These differences were starker when GPs were asked to state if they would *never* prescribe the Pill to certain groups of women. Orthodox Protestant and Catholic doctors were about 300% less likely to ever prescribe the Pill to unmarried women aged 25–30, unmarried mothers, or engaged women younger than 21.

It is interesting to see that Catholic physicians remained so opposed to the Pill when, as shown in **Table 1**, Catholic women were adopting it as their chosen birth control method as often as nonreligious women.[22] Our main explanation for this finding is a large generational and gender divide among Catholics in views toward both the church and contraceptives. These differences in views must have been especially large between the young women demanding the Pill and the much older (69 percent over 40) and almost exclusively male (87 percent) GPs prescribing it.[23]

[22] The reluctance to prescribe contraceptives by Catholic physicians could also be related to Pope Paul VI's 1968 Humanae Vitae. The document also included a specific (pastoral) directive "To Doctors and Nurses" to "fulfill the demands of their Christian vocation before any merely human interest" so that "when married couples ask for their advice, they may be in a position to give them right counsel and to point them in the proper direction." https://www.vatican.va/content/paul-vi/en/encyclicals/documents/hf_p-vi_enc_25071968_humanae-vitae.html

[23] Catholic men above the age of 40 were twice as likely to disapprove of the pill and have intolerant views regarding premarital sex compared to Catholic women under the age of 30 (Hutjes 1974, 82, 168). These views can also 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 A6**, 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.

To measure the importance of supply-side barriers, we use the 1971 census to calculate the proportion of health professionals who were opposed to the Pill on religious grounds (Orthodox Protestant and Catholic GPs and pharmacists) in each municipality. This will capture the beliefs of the average morally opposed gatekeeper women would face when trying to access the Pill in the period surrounding its liberalization.

Area dispersion in this measure is shown in **Figure 5a** and reveals large differences in the religiosity of the pool of health professionals' women can choose from. Crucially, **Figure 5b** shows that there is considerable variation between doctors' religiosity and our measure of local demand-side resistance to pill adoption. Hence, we observe liberal areas with predominantly conservative health gatekeepers and areas for which the opposite is true.[24]

This "mismatch" in area-level moral values toward the Pill and that of its health professionals is further explored in **Figure A5.** The figure shows there are some Orthodox Protestant health professionals in liberal areas, but more strikingly, that there are many Catholic gatekeepers practicing in municipalities unopposed to pill liberalization. This finding can be explained by general practitioners' high propensity to locate in areas close to the university they attended. Over the period of 1957 to 1981, 52–69 percent of GPs started practicing in the province that their university was in (Groenewegen 1985).[25] The fact that this allocation mechanism creates a high mismatch for Catholic GPs is thus not surprising given that there were no medical schools in the southern Netherlands until 1976. Consequently, any student interested in medicine from one of the two Catholic majority provinces had to move to the north to do so. This eventually strongly affected the location of practicing Catholic health professionals by moving them into areas with more liberal views about contraceptive use.[26]

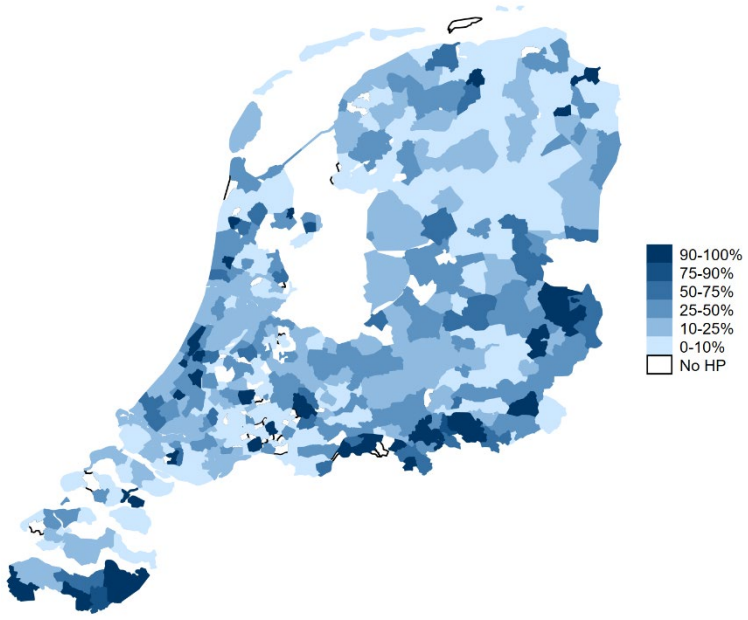
[24] **Table A8** shows the correlation matrix between the proportion of Catholic and Orthodox Protestant health professionals to several other measures at the municipality level.

[25] The percentage of GPs practicing in the same province as the location of their medical school in 1957–1981 was 61 percent for the province of North Holland, 59 percent in South Holland, 52 percent for Utrecht, and 69 percent for Groningen, Friesland, and Drenthe (see Table 6.4 on pages 154–155 of Groenewegen 1985).

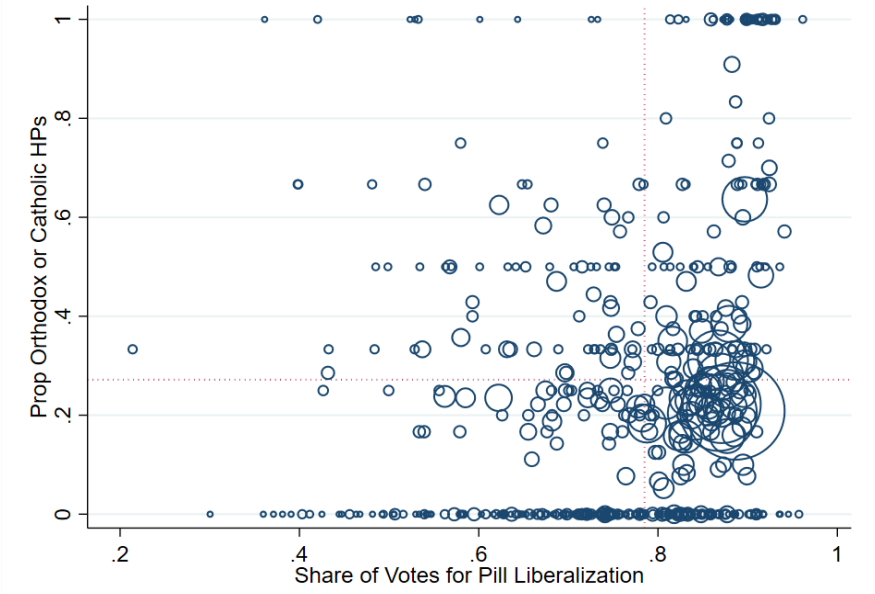
[26] A side effect of this religious mismatch between HPs and the municipality they work in is that they could be more likely to practice for less time in an area where their beliefs are not aligned with the community. This is important in our context as (young) women could feel inclined not to seek 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 an HP has been active at least five years in an area depending on his religious affiliation. We find a tiny correlation coefficient (–0.037) for the relationship between the proportion of doctors opposed on religious grounds and the proportion of doctors that are active in an area less than five years. Hence, the religious affiliation of the doctor is not related to the time spent in their current municipality and therefore not important in our setting.

**Figure 5.** Variation in the Proportion of Religious Health Professionals

(a) Municipality-level regional variation




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




Notes: Panel (a) shows regional variation in the proportion of health professionals opposed to the Pill on religious grounds. Authors' calculations based on the 1971 census of the proportion of general practitioners and pharmacists who were 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.

# 3. Identification: Within-Municipality, Across-Cohort Variation

So far, we have documented a very large drop in births among minor women that 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 demand-side moral frictions for women's outcomes, we bring together two other facts we have already documented: (i) there was a lot of variation in the 1967 vote shares for Orthodox Protestant parties across the Netherlands,[27] and (ii) 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 through within-municipality, across-cohort variation in observed outcomes. Once we have clearly explained this identification strategy and presented the results it yields, we will turn to estimating the added impact of supply-side pill access moral frictions in **Section 6**.

## 3.1. Intuition: Comparing Younger and Older Women in Liberal and 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 exhibit 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. We assign our treatment intensity—or pill adoption probability—continuously by using the vote share of parties in favor of the Pill in each woman's municipality of birth.[28]

[27] A potential concern is 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 there should be a specific break for cohorts just too young or old to benefit from pill introduction, which is 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 A8**). This is highly suggestive that norms were very sticky at the area level in this period, at least in terms of voting preferences 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.

[28] We use place of birth to assign women to municipalities rather than the place of birth of their first child for two reasons. First, we can observe own place of birth for all women, whereas we cannot observe the place of birth of the

Comparing two minors in different municipalities will account for certain secular time trends that may have affected their observable outcomes similarly but that were not due to contraceptive liberalization (e.g., changes in sexual behavior, average age at birth or marriage, 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.[29] In practice, this means that we categorize women who were aged 16–20 in 1970 as "treated" (i.e., 1950–1954 birth cohorts) and compare their outcomes to that of "control" women from the same municipalities who were aged 21–26 in 1970 (i.e., 1944–1949 birth cohorts).

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. To visualize this, we compare characteristics of the households in which these younger and older cohorts of women grew up, depending on municipality pro-pill vote share. 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 reveal that the share of votes in favor of the Pill is significantly—sometimes strongly—correlated with *all* these household characteristics. 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

first child for women who remained childless. Second, it is possible that the treatment intensity may have impacted the probability of moving before making a fertility decision, and hence the place of birth of the first child may be considered as an outcome of social norms in the area where one is born and/or grew up.

[29] Note that women over age 21 did experience improved access to the birth control pill in our context, but the change in access to the Pill that came with the liberalization was more drastic for younger than older cohorts. First, the legal punishment for providing or recommending contraceptives to minors (women under the ages of 21) was much more severe. Second, women in "fertile marriages" already could have gained access to the Pill from the mid-1960s, and those women were 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 age 21) would already be impacted by the time the Morality Law was repealed.

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.[30] This is reassuring evidence that our chosen identification approach does account for most factors related to pill access and may have affected women's outcomes differently.

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

### 3.2. Econometric Specification: Continuous Difference-In-Differences

Our main results stem from estimating 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 $\beta$, which captures the treatment effect: an interaction of the $After_{ic}$ dummy that takes a value of 1 if woman *i* was 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 $c \in \{1944,1949\}$) and is interacted with the standardized vote share for parties in favor of pill liberalization, $ShareForPill_{im}$, in each municipality $m$ where woman $i$ was born.[31] The specification includes year of birth ($YoB_{ic}$) and municipality of birth ($MunB_{im}$) fixed effects to capture all cohort-specific and area-specific factors that may influence the outcomes we consider. All regressions are weighted by female municipality population to properly reflect the relative impact of each area given its size. Standard errors $\varepsilon_{imc}$ are clustered at the municipality level—

[30] 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 1944–1954) 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 that contained a woman from the 1944–1954 cohorts for the simple reason that women from older cohorts likely had already formed their own household by marriage at the time they were observed in the 1971 census. Since we want to know and compare the characteristics of the households in which they grew up, we take the typical parental age at that time to classify each household as likely treated (i.e., child aged 16–20 in 1970 with a typical parent aged 46–55 in 1971) or likely untreated (i.e., child aged 21–26 in 1970 with a typical parent aged 52–61 in 1970) in terms of early access to the Pill.

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

the level of group treatment—to account for potential serial correlation in unobservable factors that impact women's outcomes from the same areas similarly.

### 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 findings and also validate our continuous difference-in-differences approach.

First, we check the sensitivity of the results to excluding municipalities at the extremes of the *ShareForPill* distribution. This informs the importance of the contribution of very pro- or very anti-pill areas. If extremely liberal or extremely conservative municipalities are crucial to our results, this might put into question whether our treatment approach of continuous assignment 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 in which we drop municipalities belonging to the top or bottom 10 percent and 25 percent of the vote in favor of pill distribution. The latter is particularly demanding as it will only use women born in the half of municipalities who have a relatively similar probability of using the Pill.[32]

Second, we do a common pre-trends test to validate our difference-in-differences approach. This will reveal 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 $\beta$ coefficient that **Equation (1)** might have yielded. For this, we estimate **Equation (2),** where the treatment effect ($\varphi_c$) is estimated for all cohorts ($c$) separately.

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

[32] An additional benefit from this exercise is that it will partially alleviate some econometric concerns about the interpretation of continuous difference-in-differences recently raised by Callaway, Goodman-Bacon, and Sant'Anna (2021). Their main concern is that effects might be quite different depending on where in the distribution in terms of treatment the dose was received. Showing results for groups of the population that received very different doses of treatment will inform whether that is an important issue in our setting.

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 born in 1944–1949 who were a minor before pill liberalization[33]). 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.

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 Definitions

Our main data sources are administrative registries compiled by Statistics Netherlands.[34] We focus on young women who were born in the Netherlands and aged 16–26 in 1970. For any woman registered in a Dutch municipality by 1995, we observe her place of birth, marital history, and 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. After

[33] We limit our presentation of pre-trends to the five cohorts preceding the Pill access change to minors for two reasons. First, in all our analyses we use these women as controls as they are the most similar in age to those treated by pill liberalization as minors. Second, administrative data on certain outcomes becomes problematic for women born earlier. Data are more often missing for education and observed only very late in life for labor market outcomes.

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

excluding women born in the Catholic south, we are left with a sample of 864,370 women born in 541 different municipalities.

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 as well as the total number of children per woman (i.e., completed fertility). We create a variable for age at first birth for women who ever had a child, and we define a minor birth as the first birth occurring before age 21. The marital status registry, which has information on all past and present marriages, is used to determine whether a woman was ever married during her life. For those ever married, a variable is generated for age at first marriage and minor marriage is defined as being married before age 21. Finally, a "shotgun wedding" is defined as a child 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 human capital accumulation: education, work, and wealth. The registry containing records about educational outcomes only started in 1999, so the number of individuals who finished their degree before this time is inferred retrospectively from surveys. This implies that we only observe educational outcomes for about 25 percent of our sample. We create a dummy that reflects whether an individual finished higher education, which in the Netherlands means finishing a university degree (general or vocational). In the spirit of Goldin and Katz (2002), we also generate an indicator for women completing "long studies," which includes a degree in law or medicine (medical school, dental medicine, or veterinary medicine). These degrees 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.[35]

Administrative data on labor market and wealth outcomes is also available from 1999 onward. We look at participation and earnings of women at age 55—the earliest age at which we can observe earnings for women in all birth cohorts—to get a picture of labor market participation before most women enter retirement. We create all measures in terms of full-time equivalent

[35] A law degree takes about three to four years in the Netherlands, whereas a medical degree typically takes about six years. However, given that individuals must complete occupational training of at least two years before they start practicing as a lawyer, we classify a law degree as a long study.

(FTE)[36] as women in the Netherlands have the highest propensity to work part time (61 percent) of any OECD country (Boeri and Van Ours 2021). Given the relatively low labor market participation of Dutch women, we also explore the effects of contraceptive access on a woman's household wealth. This 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 its house, and the value of its 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 onward; thus, we focus on mean wealth for women in our sample at ages 60–62.

# 5. Impact of Pill Access on Women's Outcomes

We now present the results from our analysis of how much oral contraceptive access affected women's life trajectories in the Netherlands by exploiting demand-side moral frictions that influenced the probability of pill adoption.

## 5.1 Short-Run Impact: Fertility and Marriage

**Table 2** reports the continuous difference-in-differences estimates—the $\beta$s from **Equation (1)**—of the impact of a one standard deviation increase in the vote share in favor of the Pill (about 10 percent) in a woman's municipality of birth for treated cohorts. These point estimates can be put into perspective relative to the mean of the dependent variable for the untreated cohorts, also shown in the table. We then directly interpret these estimates in terms of a relative percentage effect size, which we report in the second row from the bottom.

We find that access to the Pill as a minor did not have a large effect on women's completed fertility. Women are 2 percent 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 the timing of births among treated cohorts. This is true in terms of average age at first birth, but in particular for early fertility decisions. Women born in a municipality with a 10 percent higher share of votes in favor of the Pill experienced a 12 percent drop in their probability of becoming mothers before the age of 21.

[36] Unfortunately, the information on work hours (full-time equivalents, FTE) is only available from 2001. Therefore, we take earnings and FTE at age 56 for the 1945 birth cohort and at age 57 for the 1947 birth cohort.

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

| | Fertility | | | | | Family Formation | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Chidless | # of children | Age 1st birth | Mother < 21 | Ever married | Age 1st marriage | Marriage < 21 | Shotgun wedding | Ever divorced |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Minor 1970*** | .003* | .000 | .260*** | -.020*** | -.005*** | .320*** | -.018*** | -.003* | -.003** |
| **Share for Pill** | (.002) | (.004) | (.039) | (.003) | (.001) | (.093) | (.004) | (.002) | (.001) |
| 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% | - | +1.1% | -12.0% | -0.5% | +1.4% | -6.0% | -1.9% | -1.3% |
| 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 a mean and standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. All specifications are weighted by the cohort-municipality number of women. Robust standard errors clustered at the municipality level are 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; in column 8 we restrict the sample to women who were ever married and ever had a child. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Access to birth control also did not much change the likelihood 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 in 10 percent more liberal areas were 6 percent less likely to marry as minors. They were also 5.4 percent less likely to end up in "shotgun weddings," an indicator of unions being hurried by fertility circumstances. The resulting marital unions appear to have been neither stronger nor weaker, with divorce rates only decreasing slightly.

## 5.2 Long-Run Impacts: Education, Work, and Wealth

Delays in fertility and marriage decisions from pill access improvements could have enabled women to increase investments in their human capital. Results in **Table 3** confirm this as they show that women in treated cohorts were significantly more likely to complete higher education degrees that require a larger time investment. Those born in 10 percent more liberal municipalities

were 28.6 percent 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 before the liberalization.

**Table 3.** Long-Run Outcomes: Education and Work

| | **Education** | | **Work** (age 55) | | | | |
|---|---|---|---|---|---|---|---|
| | Higher educ. | Long studies | Working (FTE) | Log Wage (FTE) | % Rank wage | Top 25% wages | Top 10% wages |
| | (1) | (2) | (3) | (4) | (5) | (7) | (8) |
| **Minor 1970*** | .006 | .002*** | -.005*** | -.005 | .370* | .023*** | .014*** |
| **Share for Pill** | (.004) | (.000) | (.001) | (.004) | (.208) | (.006) | (.003) |
| Cohort F.E. | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mun. F.E. | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean dep var | .174 | .007 | .268 | €29k | 50.0 | .250 | .100 |
| Effect size | - | +28.6% | -1.9% | - | +3.7% | +9.2% | +14.0% |
| N | 218,119 | 218,119 | 864,370 | 405,066 | 405,513 | 405,513 | 405,513 |

Notes: Estimated by OLS. *ShareForPill* is standardized with a mean and standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. All specifications are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality level are in parentheses. Higher Educ. is a dummy indicating that a woman obtained a university degree. 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. Wage results are also presented in terms of position of each woman in the distribution of all working women (per exact percentile rank and belonging to the top quartile or decile). The sample sizes are different across outcome variables because educational outcomes are only observed for about a quarter of the women in our sample and wages are conditional on working, which is only the case for about half of women in these cohorts at that age. More details can be found in the Data Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The small but significant negative impact on labor force participation reported in column 3 of **Table 3** is somewhat unexpected. However, less than half of women in our cohorts are working by age 55 and only very few do so full time. For those who are in the workforce, average wages earned per hour (FTE) do not at first appear to be very different, but they are much more likely to be in the top of the earnings distribution. Given that the education effects were concentrated in the longest type of degrees, that is perhaps not surprising. Therefore, women with more access to the Pill as minors seem to have been more likely to either chose not to work or to only do so if the rewards were high. Not being economically active might be an optimal decision

for many women at this age—even after having invested more in human capital earlier in life—especially if their household wealth level permits it.

**Table 4.** Long-Run Outcomes: Wealth

| | **Wealth** (age 60-62) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | **All** | | | | **Not working at 55** | | **Working at 55** | |
| | Log wealth | % Rank wealth | Top 25% wealth | Top 10% wealth | Top 25% wealth | Top 10% wealth | Top 25% wealth | Top 10% wealth |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Minor 1970*** | .018 | .145 | .009*** | .002 | .009*** | .001 | .004** | .001 |
| **Share for Pill** | (.012) | (.201) | (.003) | (.002) | (.003) | (.003) | (.002) | (.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 | €302k | 50.0 | .250 | .100 | .250 | .100 | .250 | .100 |
| Effect size | - | - | +3.6% | - | +3.6% | - | +1.6% | - |
| N | 758,024 | 810,525 | 810,525 | 810,525 | 413,196 | 413,196 | 397,329 | 397,329 |

Notes: Estimated by OLS. *ShareForPill* is standardized with a mean and standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. All specifications are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality level are in parentheses. We have information of household wealth (at age 60-62) for 94 percent of women considered in our main analysis sample. More details can be found in the Data Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We investigate possible wealth effects by using high-quality information available for most Dutch households and report results using various measures in **Table 4**. Again, the impact of pill access as a minor in the long run does not seem to be linear as no average increase in wealth is detectable. There is however a significant increase in the probability of (more) treated women of being in households located in the top quartile of the wealth distribution, and this holds for women who are active in the labor market and those who are not. This first evidence on a pill access effect on wealth is potentially important as it would explain why its impact on labor market outcomes has not been overwhelming so far, despite strong consistent positive education findings.[37] Given

[37] For example, Bailey (2006) finds that pill liberalization in the United States led to somewhat higher labor force participation of women aged 26–35 but detects no effects before those ages, which is consistent with human capital investment and delayed birth effects in women's early 20s. However, Bailey does not find effects of labor market participation for women older than 35, which may be somewhat more surprising at first. 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 Bailey's state-level instrument to not properly capture pill exposure changes for

that these wealth outcomes are measured at the household level, which is in part determined by partner choice, these results reinforce the importance of birth control technology on delaying and improving mating decisions.[38]

## 5.3 Robustness and Validity Checks

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

In the first test, we drop areas at the extremes of the pill liberalization vote share distribution. 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 percent, and then for excluding municipalities at the top and bottom 25 percent (**Tables A3, A4, and A5**). All results are stable, if somewhat larger, but not statistically different from the main analysis. This indicates that impact is not just identified from municipalities that are extremely conservative or liberal, and that a gradient in area-level acceptance of oral contraceptives is important post-liberalization. This is a policy-relevant finding, but also an econometrically pertinent one 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 $\varphi_c$ of **Equation (2)**—and plot these for four key outcomes in **Figure 6**: 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).

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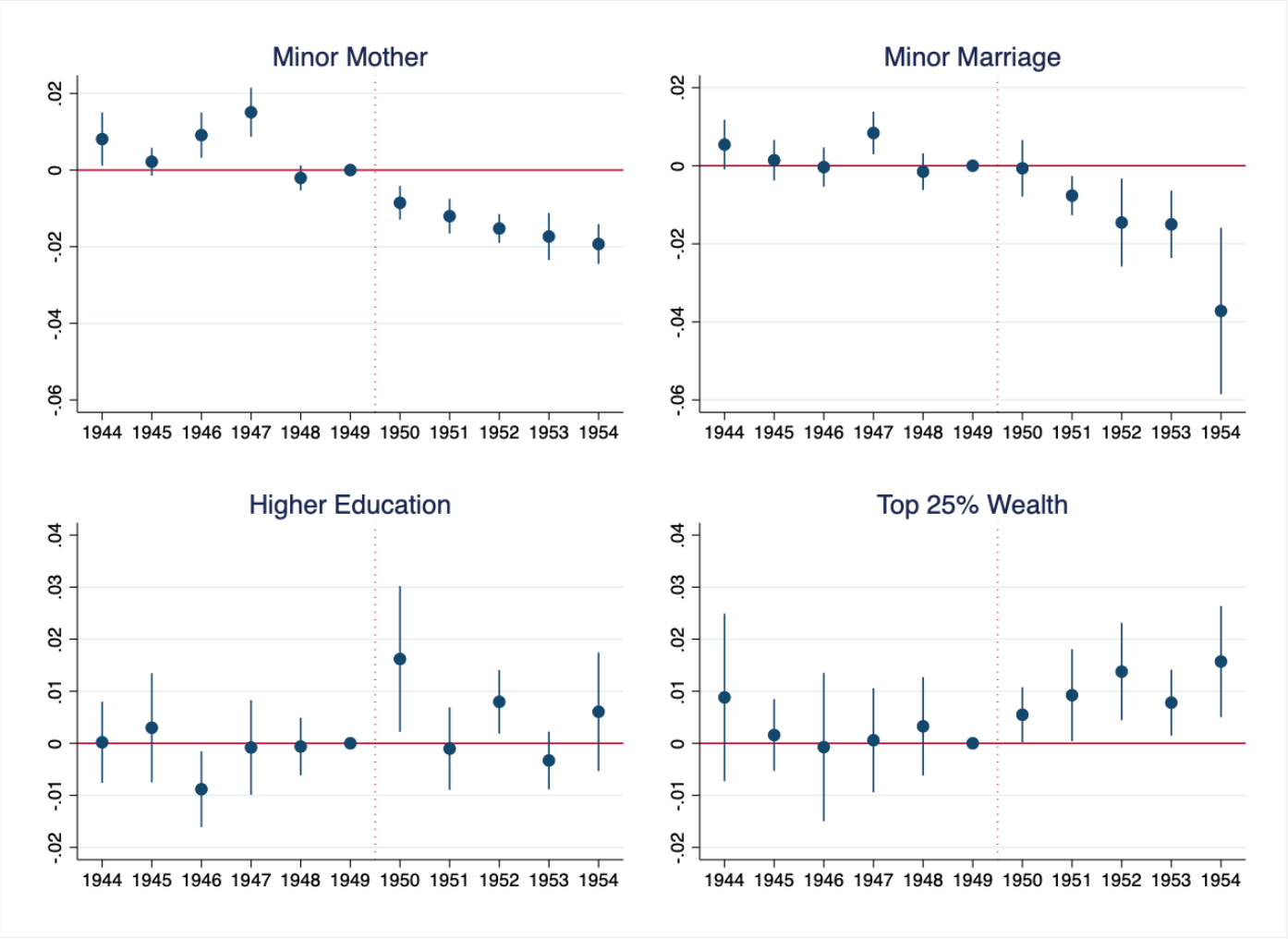
these older women. Using a different identification strategy, Bailey, Hershbein and Miller (2012) find that pill access in the United States negatively impacts women's wages in their early 20s but positively impacts wages in their 30s and 40s (an hourly wage premium of 8 percent). This suggests that detecting labor market returns for women stemming from improved oral contraceptive access is very sensitive to context and identification approach as well as the age at which this outcome is measured.

[38] We also examined 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 percent older (effect of 0.424, with a standard deviation of 0.053, on a baseline of 25.3 for a one 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 women 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.

**Figure 6.** Cohort-Specific Policy Effect: Minor Births/Marriage, Higher Education, and Top 25% of Wealth




Notes: The graphs plot the point estimates and 95 percent confidence intervals for the coefficients that are estimated using **Equation (2)** and show 11 policy estimates $\varphi_c$, which show the impact of a one standard deviation increase in the vote share in favor of the Pill (about 10 percent) for each birth cohort in our sample (akin to a common trend assumption in a difference-in-differences setting). The cohorts from 1950 and later were exposed to the Pill as a minor and hence treated (and therefore we expect to see an effect starting from these birth cohorts), whereas the cohorts of 1944–1949 did not have access to the Pill as a minor and thus are considered untreated (and therefore we expect a zero effect for these birth cohorts).

The graphs show that there is no clear pre-policy pattern for the untreated cohorts, specifically, birth cohorts 1944–1949, on the left side of the red dashed line, who were older than age 21 at the time of pill liberalization. For the treated cohorts of women for whom the Pill was liberalized when they were minors, we observe clear deviating trends in most outcomes depending on the share of votes for parties in favor of the Pill in the woman's municipality of birth. This observed difference strongly increases the younger the women were at the time of the liberalization.[40] In addition to confirming the common trend hypothesis, these graphs are informative on two other aspects of the policy impact. The first relates to what we would find if we artificially moved pill access to years earlier than 1970, such that older cohorts would be considered as treated. This would not yield any significant results and serves as a visual "placebo in time" test. The second 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.

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 can do this permutation many 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 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. It confirms 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 in which the random allocation of social norms yields

[40] The pattern for higher education completion, even if it points to significant increases for some of the post-treatment cohorts, is much noisier, which 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.

estimates that are larger than those in the "real" allocation of social norms. For higher education completion, this is the case in six out of 500 permutations, an extremely low occurrence that might partly be explained by the smaller sample 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 strong evidence that social norms in an area were critical to a woman's likelihood of adopting the Pill and benefiting from its effects in both the short and long run.[41]

# 6. Supply-Side Moral Barriers: Gatekeepers Opposed to the Pill on Religious Grounds

We now turn to the possibility that, even if a woman had wanted to exercise her legal right to use the Pill to improve control over her fertility, the gatekeepers who could grant access to the new technology might have prevented this from happening because of their own moral beliefs.

## 6.1 The Influence of Gatekeepers Opposed to the Pill on Religious Grounds

To gauge how much gatekeepers' own religious beliefs matter for de facto access to the contraceptive pill, we identify the proportion of religious health professionals in each municipality. This measure reflects the average willingness of health professionals in the area to provide women with oral contraceptives. We do not know which provider the woman ends up seeing—a choice that is in any case endogenous—but argue that women are more likely to match with a doctor who is unwilling to prescribe in areas where more health professionals are morally opposed to the use of contraceptives. This area-level "willingness to prescribe" measure is similar than those used in

[41] 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 whether the location of any of the ten abortion clinics in 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. First, we see whether 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 whether 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.

papers where area-level prescribing measures are used to instrument for the patient's likelihood to receive medication as the patient is more likely to match with a high-prescribing provider (Currie and MacLeod 2017, 2020; Cuddy and Currie 2020, Currie and Zwiers 2021). A potential concern is whether women would consult pro-pill doctors outside their municipality of residence. This is probable as in our setting individuals were free to choose their general practitioner. Still, in practice, most patients would register with their closest GPs so they would be nearby in case of emergency. More importantly, traveling to another area to find a prescribing physician or pharmacist who stocks oral contraceptives would come at a cost that would reduce the probability of pill take-up for the marginal woman.[42]

Using data from the 1971 full count census[43] 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 was responsible of proposing and prescribing the Pill and the pharmacist was in charge of stocking it. We identify a total of 5,261 practicing health professionals: 4,326 GPs and 935 pharmacists in the Netherlands in 1971, excluding the southern provinces. **Table A7** shows the religious affiliation of the Dutch population compared to the health professionals. In our sample of health professionals, 38.2 percent were not religious, 16.8 percent were Catholic, and 9.8 percent were Orthodox Protestant. Compared to the full population, health professionals were more likely to be nonreligious and less likely to be Catholic. We focus on health professionals who were most opposed to the Pill, that is, Orthodox Protestant and Catholic HPs.

The proportion of health professionals who are opposed to the Pill on religious grounds is calculated by dividing the number of HPs from either of these two religions by the total number of HPs in each municipality. Women in our sample faced on average 23.1 percent gatekeepers in their birth municipality (with a standard deviation across municipalities of 7.3 percent) who were opposed to the Pill on religious grounds. This measure captures the probability of women encountering a gatekeeper who was morally opposed to the Pill in 1971. As previously illustrated in **Figure 5a,** there was considerable variation in the religiosity of the pool of health professionals women could choose from across areas. **Figure 5b** also showed that there was substantial variation

[42] To test whether distance to a municipality with more prescribing GPs or pharmacists matters for take-up probability, we look at how much this margin matters for women's long- and short-run outcomes. We do find significant (inverse) effects of interacting distance to the proportion of nonreligious HPs in the nearest town for women with no HPs—at all or willing to prescribe—in their own municipality. This suggests that traveling costs are relevant and we will take them into account in a robustness check in what follows.

[43] See **Appendix Section B.3** for a detailed description of the set-up of the census data.

between HPs' moral resistance to contraceptives and pro-pill votes in the areas they serve. Hence, there are many liberal areas with predominantly Catholic or Orthodox Protestant HPs.

A final concern about the validity of this measure to capture demand-side access restrictions due to HPs' beliefs is that it could be correlated with other municipality characteristics that affect access to health services. We check for this possibility using information from the 1971 census and relate our anti-pill HP measure to: (i) number of GPs and pharmacists, (ii) education and income (to check whether certain types of HPs are more present in richer more educated areas), and (iii) distance to hospital and number of nurses living locally (to see whether access to an alternative to local HPs was easier). We show in **Figures A6.1, A6.2, and A6.3** respectively that none of these measures of health access are correlated with the proportion of HPs who self-identify as Catholic or Orthodox Protestant.

## 6.2 Estimating Supply-Side Moral Barriers

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 opposed on to the Pill on religious grounds (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 gatekeepers opposed to the Pill on religious grounds, which is captured by the triple interaction. Note that, we interpret this triple interaction term *conditional on the level of area social norms* that would have made a woman more or less likely to take up the Pill, which is itself captured by the main difference-in-differences interaction.[44]

[44] The estimated γ coefficients that result from the interaction between $After_{ic}$ and $PropRelHP_{im}$capture the impact of having a higher proportion of HPs opposed to the Pill on religious grounds *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 seek in our context, and thus we do not report these coefficients.

We first estimate **Equation (3)** by using the standardized share (i.e., a mean of zero and a standard deviation of one) of pill-opposed HPs and present the resulting estimate, $\hat{\delta}$, along with the associated difference-in-differences coefficients, $\hat{\beta}_2$, for four of our main outcomes of interest in **Table 5**: minor birth, minor marriage, completing "long studies," and belonging to a household in the top quartile of the wealth distribution by age 60.

**Table 5.** The Additional Effects of Gatekeepers Who Were Opposed to the Pill on Religious Grounds

| | (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*** | .010*** |
| | (.003) | (.004) | (.001) | (.003) |
| **$DiD_{Treat}$* % Religious Health Professionals** | .014*** | .020*** | -.002*** | -.006*** |
| | (.003) | (.003) | (.001) | (.002) |
| Cohort F.E. | Yes | Yes | Yes | Yes |
| Mun. F.E. | Yes | Yes | Yes | Yes |
| Sample Size | 731,184 | 801,549 | 217,113 | 806,178 |

Notes: Estimated by OLS. *ShareForPill* is standardized with a mean zero and a standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. The mean proportion of religious HPs that women have access to is 23.1 percent, with a standard deviation of 7.3 percent. We also standardize this measure with a mean of zero and a standard deviation of 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 level and are in parentheses.

The estimated effect of having relatively more gatekeepers opposed to the Pill in a municipality at the time of pill liberalization was always strongly significant and symmetrically opposite to the impact of the Pill due to area-level social norms. Concretely, it means that, for minor birth, a one standard deviation increase in the proportion of Orthodox Protestant or Catholic HPs, or +7 percent more religious HPs, in a woman's municipality of birth reduces the potential impact of pill access (–2.0 percent for 10 percent more votes for pill) by two-thirds (+1.4%). Although statistically accurate, this interpretation might not be the best way to understand the impact of gatekeepers who are opposed to the Pill on religious grounds on attenuating the effects of the Pill. First, it is not

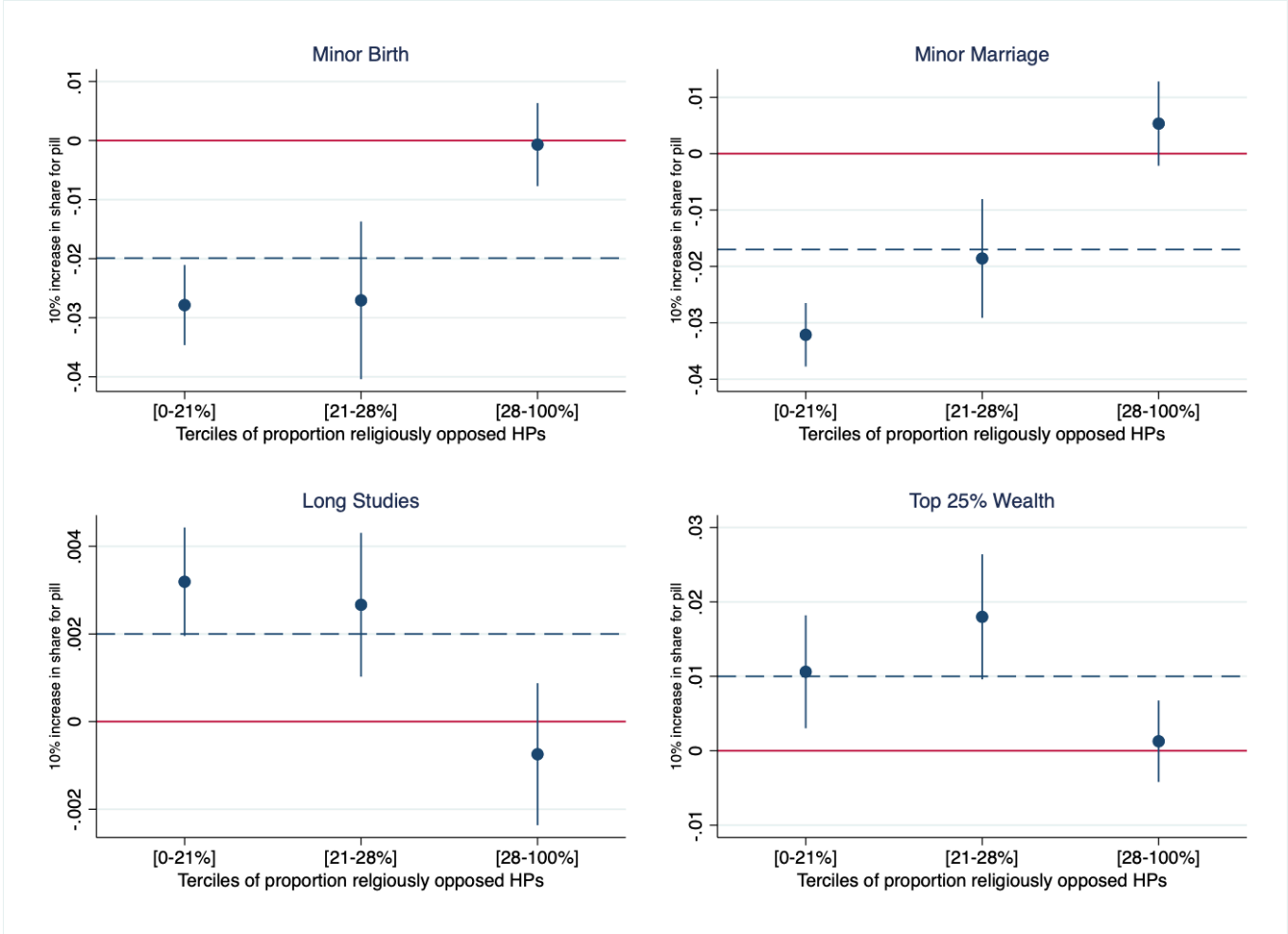
straightforward to capture the size of a triple interaction with two continuous variables, and second, important non-linearities may not be properly captured.

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 first tercile (from zero to 21 percent); the second tercile (from 21 to 28 percent); and the third tercile (from 28 percent or 100 percent). The estimated coefficients we obtain are reported in **Figure 7** for the same four outcomes as earlier, with their respective 95 percent confidence intervals. These figures are very revealing: while there is often no significant difference in estimated pill impact for the first two terciles of the distribution, when more than about a third of HPs are either Catholic or Orthodox Protestant, the effects of pill access are entirely wiped out regardless of which outcome is considered.

These findings are unchanged when dropping pharmacists so that we only consider the impact of general practitioners who were opposed to the Pill on religious grounds (**Table A9, panel A** and **Figure A6.1**) or when allocating the share of gatekeepers opposed on religious grounds from the closest municipality for municipalities without HPs (**Table A9, panel B** and **Figure A6.2**).[45] We do one last test for whether this effect is partially driven by areas where women do not have much local choice 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 opposed to prescribing the Pill on religious grounds, restricting our sample to municipalities with at least three active physicians or pharmacists. The results from this robustness check are reported in **Table A9, panel C** 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.

[45] 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 3.5 km with a standard deviation of 2.0.

**Figure 7.** The Additional Effect of Gatekeepers Who Were Opposed to the Pill on Religious Grounds, by Tercile




Notes: Estimated by OLS. Figures plot the additional effect of HPs opposed to the Pill on religious grounds (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 a mean of zero and a standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. The proportion of HPs opposed to the Pill on religious grounds (with a mean of 23.1 and a standard deviation of 7.3%) is divided into terciles: 0–21 percent, 21–28 percent, and 28–100 percent. 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 level.

The picture that emerges is of a very large and negative impact of gatekeepers' beliefs on the ability of women to properly benefit from the life-changing advantages of birth control technology. 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 United States, and Autorino et al. (2020) for Italy). However, our results are, to our knowledge, are the first that clearly 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 very large positive impact that the Pill can have on women's lives.

# 7. Concluding Remarks

Our investigation of the impact of the liberalization of the 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 heterogeneities because of demand- and supply-side moral barriers to access that reveal how the potential benefits of liberalization were not universally distributed across women. Minors who grew up in areas that were less opposed to the Pill on religious grounds 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. This human capital accumulation produced more high earners among those (fewer) who chose to work, and it lifted (all) women who benefited more from access toward the top of the household income distribution.

These findings are the first to document the life-changing effects of pill access outside the United States and to exploit religious margins that affect take-up to do so. The heterogeneity across demand-side moral barriers that we document suggests that existing studies that use legal access changes are probably only a lower bound of the true effect of the Pill on women's outcomes. However, 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 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 10 percent more liberal became 6 percent less likely to marry as a minor, and 12 percent fewer women experienced a birth by age 21. The Dutch marriage impact is between those of Goldin and Katz (2002)—5 percent fewer marriages by age 22—and Myers (2017)—19 percent fewer marriages by age 19—for the United States, 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 close to that of Bailey (2006)—14 percent fewer births by age 22 for the United States. 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 doctor degrees in the United States as a result of access to the Pill.[46] 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 United States. Importantly, this is in a context in which 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 effect of the power of the Pill.

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 health professionals—the suppliers of the Pill—in the municipality where women were born mattered considerably. We show that if more than a third of them were either Orthodox Protestant or Catholic, it was unlikely that a young woman was able to experience any of the benefits from pill access that those in equally liberal areas but 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 pill use could have altered women's 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

[46] These might at first seem like huge increases, but in both countries, the baselines are very low: 1.4 percent in the United States and 0.7% in the Dutch context.

around the world. Third, our gatekeeper findings have important implications for current and future birth control policies that will likely be more effective if access is independent of third parties who may hinder a woman's right to choose. The importance of moral barriers to birth control access uncovered in this paper may become especially relevant for U.S. women in a post-Roe world.

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# Appendix A: Additional Tables and Figures (For Online Publication)

**Table A1.** Percentage of Unplanned Pregnancies Over Time

***Panel 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 |

***Panel 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 (Nederlands Instituut voor Sociaal Seksuologisch Onderzoek) between 1986 and 1989. Panel B is based on the 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** Fertility and Family Formation, Dropping Municipalities at Top and Bottom of Vote Distribution

| | (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 divorced |
|---|---|---|---|---|---|---|---|---|---|
| ***Panel A:*** *Excluding municipalities with 10% highest and lowest vote share* | | | | | | | | | |
| Minor 1970* | .001 | .002 | .306*** | -.024*** | -.005*** | .402*** | -.021*** | -.003 | -.005*** |
| Share for Pill | (.002) | (.006) | (.053) | (.004) | (.002) | (.129) | (.006) | (.003) | (.002) |
| Mean dep var | .137 | 1.90 | 24.6 | .166 | .941 | 23.0 | .302 | .159 | .243 |
| Effect size | - | - | +1.2% | -14.5% | -0.5% | +1.7% | -7.0% | - | -2.1% |
| N | 817,209 | 817,209 | 693,487 | 693,487 | 817,209 | 761,286 | 761,286 | 685,699 | 761,286 |
| ***Panel B:*** *Excluding municipalities with 25% highest and lowest vote share* | | | | | | | | | |
| Minor 1970* | .003*** | -.001 | .228*** | -.016*** | -.005*** | .261*** | -.015*** | -.002 | -.002 |
| Share for Pill | (.001) | (.003) | (.020) | (.002) | (.000) | (.032) | (.002) | (.001) | (.001) |
| Mean dep var | .130 | 1.96 | 24.6 | .159 | .943 | 23.0 | .290 | .161 | .220 |
| Effect size | +2.3% | - | +0.9% | -10.1% | -0.5% | +1.1% | -5.2% | - | - |
| N | 428,204 | 428,204 | 367,437 | 367,437 | 428,204 | 400,314 | 400,314 | 363,675 | 400,314 |

Notes: Estimated by OLS. We exclude in Panel A the municipalities in the top and bottom 10 percent of the *ShareForPill* distribution (456 municipalities remaining) and in Panel B the municipalities with *ShareForPill* in the top and bottom 25 percent of the *ShareForPill* distribution (377 municipalities remaining). *ShareForPill* is standardized with a mean of zero and a standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. All specifications contain birth cohort and municipality fixed effects and are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality level are in parentheses. Shotgun wedding is a dummy indicating that a child is born within seven months of a woman's marriage. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Table A4** Education and Work, Dropping Municipalities at Top and Bottom of Vote Distribution

| | **Education** | | **Work** (age 55) | | | | |
|---|---|---|---|---|---|---|---|
| | Higher educ. | Long studies | Working (FTE) | Log Wage (FTE) | % Rank wage | Top 25% wages | Top 10% wages |
| | (1) | (2) | (3) | (4) | (5) | (7) | (8) |
| ***Panel A:*** *Excluding municipalities with 10% highest and lowest vote share* | | | | | | | |
| **Minor 1970*** | .008 | .002*** | -.007*** | -.001 | .498* | .028*** | .017*** |
| **Share for Pill** | (.006) | (.001) | (.002) | (.005) | (.284) | (.008) | (.004) |
| Mean dep var | .176 | .007 | .270 | €29k | 50.0 | .250 | .100 |
| Effect size | - | +28.6% | -2.6% | - | +5.0% | +11.2% | +17.0% |
| N | 206,753 | 206,753 | 817,209 | 385,615 | 386,041 | 386,041 | 386,041 |
| ***Panel B:*** *Excluding municipalities with 25% highest and lowest vote share* | | | | | | | |
| **Minor 1970*** | .005*** | .001*** | -.004*** | -.009*** | .154 | .017*** | .011*** |
| **Share for Pill** | (.002) | (.000) | (.001) | (.003) | (.136) | (.003) | (.002) |
| Mean dep var | .165 | .006 | .258 | €28k | 50.0 | .250 | .100 |
| Effect size | +3.0% | +16.7% | -1.6% | -0.9% | - | +6.8% | +11% |
| N | 106,930 | 106,930 | 428,204 | 196,905 | 197,151 | 197,151 | 197,151 |

Notes: Estimated by OLS. We exclude in Panel A the municipalities in the top and bottom 10 percent of the *ShareForPill* distribution (456 municipalities remaining) and in Panel B the municipalities with *ShareForPill* in the top and bottom 25 percent of the *ShareForPill* distribution (377 municipalities remaining). *ShareForPill* is standardized with a mean of zero and a standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. All specifications contain birth cohort and municipality fixed effects and are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality level are 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 a 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** Wealth, Dropping Municipalities at Top and Bottom of Vote Distribution

| | **Wealth** (age 60-62) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | **All** | | | | **Not working at 55** | | **Working at 55** | |
| | Log wealth | % Rank wealth | Top 25% wealth | Top 10% wealth | Top 25% wealth | Top 10% wealth | Top 25% wealth | Top 10% wealth |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| ***Panel A:*** *Excluding municipalities with 10% highest and 10% lowest vote share* | | | | | | | | |
| **Minor 1970*** | .019 | .208 | .011*** | .001 | .011** | -.000 | .007** | .000 |
| **Share for Pill** | (.018) | (.291) | (.004) | (.003) | (.005) | (.004) | (.003) | (.002) |
| Mean dep var | €301k | 50.0 | .250 | .100 | .250 | .100 | .250 | .100 |
| Effect size | - | - | +4.4% | - | +4.4% | - | +2.8% | |
| N | 715,168 | 765,760 | 765,760 | 765,760 | 387,566 | 387,566 | 378,194 | 378,194 |
| ***Panel B:*** *Excluding municipalities with 25% highest and 25% lowest vote share* | | | | | | | | |
| **Minor 1970*** | .013** | .126 | .008*** | .003** | .008*** | .001 | .004** | .002** |
| **Share for Pill** | (.005) | (.097) | (.002) | (.001) | (.002) | (.001) | (.002) | (.001) |
| Mean dep var | €310k | 50.0 | .250 | .100 | .250 | .100 | .250 | .100 |
| Effect size | +1.3% | - | +3.2% | +3.0% | +3.2% | - | +1.6% | +2.0% |
| N | 378,336 | 402,334 | 402,334 | 402,334 | 209,088 | 209,088 | 193,256 | 193,256 |

Notes: Estimated by OLS. We exclude in Panel A the municipalities in the top and bottom 10% of the *ShareForPill* distribution (456 municipalities remaining) and in Panel B the municipalities with *ShareForPill* in the top and bottom 25% of the *ShareForPill* distribution (377 municipalities remaining). *ShareForPill* is standardized with a mean of zero and a standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. All specifications contain birth cohort and municipality fixed effects and are weighted by cohort-municipality number of women. Robust standard errors clustered at the municipality level are in parentheses. We have information of household wealth (at age 60-62) for 94 percent of women considered in our main analysis sample. More details can be found in the Data Appendix. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Table A6:** 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 A7:** 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 A8:** Correlations 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. of HPs opposed on religious grounds | -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 |

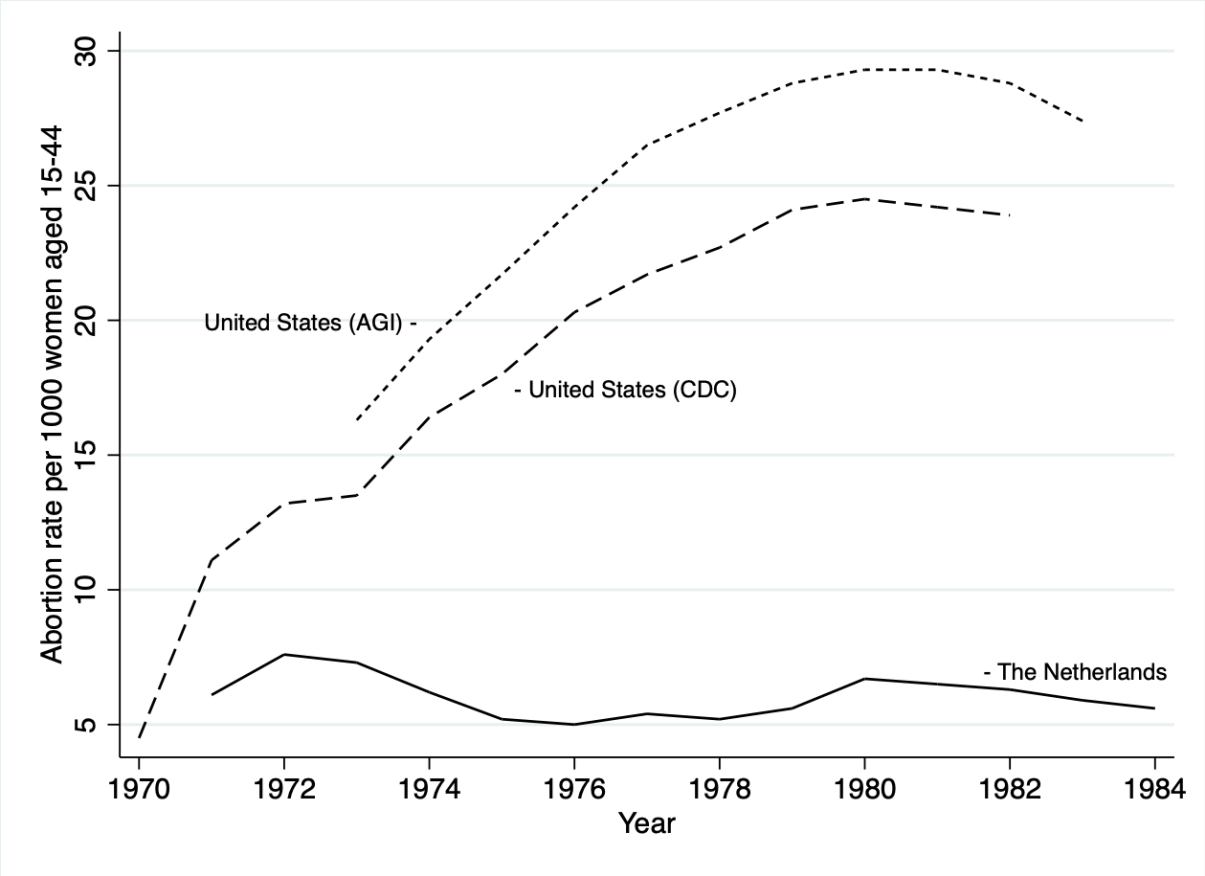
Notes: Authors' calculations based on election data from the 1967 and 1971 national parliamentary elections and from the 1971 census. Correlations are calculated at the municipality level and exclude municipalities located in the southern provinces of Noord-Brabant and Limburg. Share against '67 is the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

**Table A9** Robustness: Additional Effect of Gatekeepers Who Were Opposed to the Pill on Religious Grounds

| | (1) Mother < 21 | (2) Marriage < 21 | (3) Long studies | (4) Top 25% wealth |
|---|---|---|---|---|
| ***Panel A:*** *Only using GPs opposed on religious grounds (i.e., not pharmacists)* | | | | |
| Minor 1970*Share for Pill (i.e. $DiD_{Treat}$) | -.019*** | -.017*** | .002*** | .009*** |
| | (.003) | (.003) | (.001) | (.003) |
| **$DiD_{Treat}$* % Religious HPs** | .013*** | .019*** | -.002** | -.006*** |
| | (.003) | (.003) | (.001) | (.002) |
| N | 731,135 | 801,494 | 217,109 | 806,126 |
| ***Panel B:*** *Including municipalities without HPs (i.e., assign closest town proportion of HPs opposed on religious grounds)* | | | | |
| Minor 1970*Share for Pill (i.e. $DiD_{Treat}$) | -.019*** | -.017*** | .002*** | .009*** |
| | (.003) | (.003) | (.001) | (.003) |
| **$DiD_{Treat}$* % Religious HPs** | .013*** | .018*** | -.002*** | -.006** |
| | (.003) | (.003) | (.001) | (.002) |
| N | 735,204 | 805,870 | 218,119 | 810,525 |
| ***Panel C:*** *Restricting to municipalities with at least three HPs* | | | | |
| Minor 1970*Share for Pill (i.e. $DiD_{Treat}$) | -.020*** | -.018*** | .002*** | .010*** |
| | (.003) | (.004) | (.001) | (.003) |
| **$DiD_{Treat}$* % Religious HPs** | .015*** | .021*** | -.002*** | -.007*** |
| | (.003) | (.003) | (.001) | (.002) |
| N | 676,793 | 743,140 | 202,332 | 747,559 |

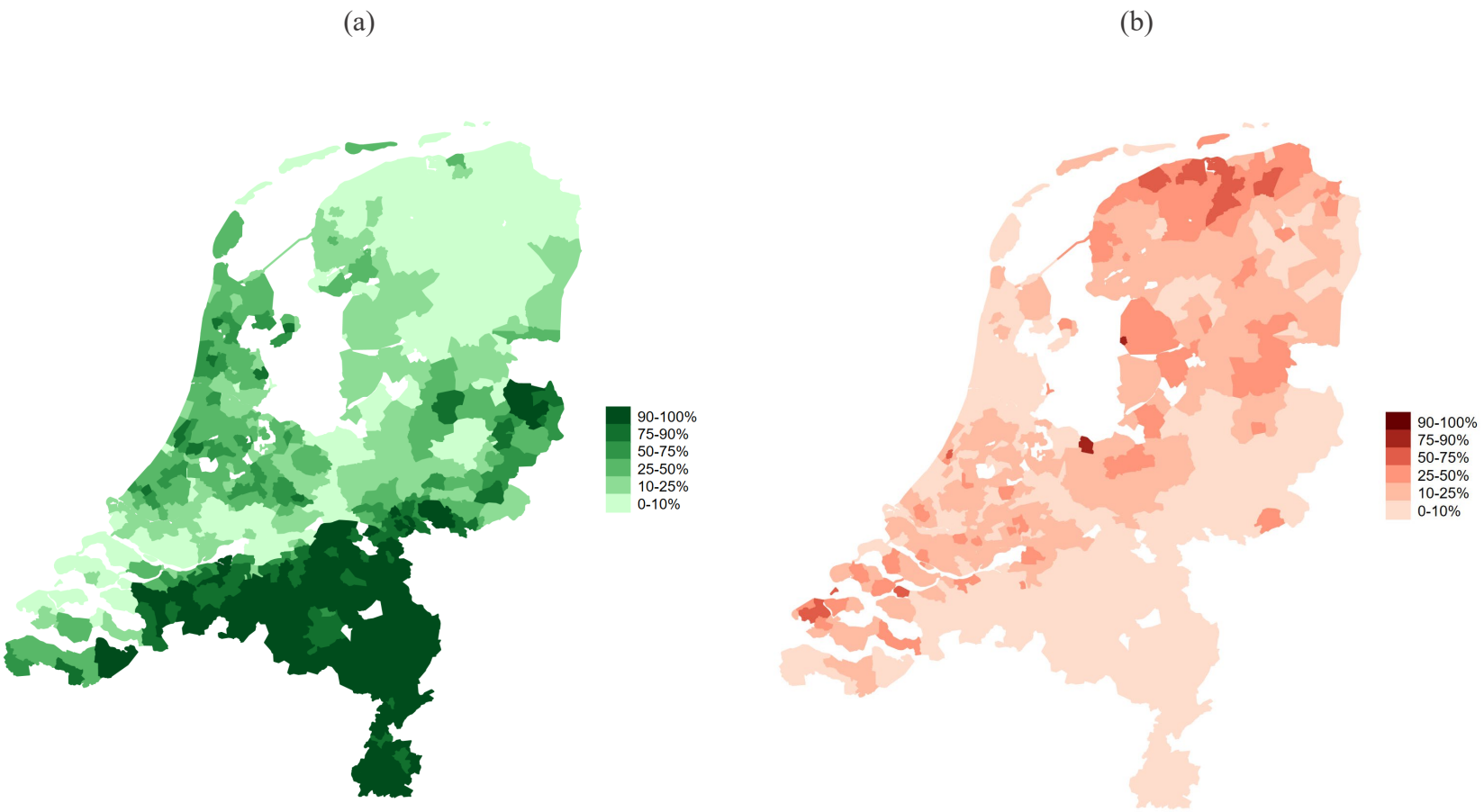
Notes: Estimated by OLS. *ShareForPill* is standardized with a mean of zero and a standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. Panel A: the mean proportion of GPs opposed on religious grounds that women have access to is 23.9 percent, with a standard deviation of 7.5 percent. Panel B: The mean proportion of religiously opposed HPs in the municipality closest to you is 23.1 percent with a standard deviation of 7.5 percent. Panel C: the mean proportion of religiously opposed HPs (when restricting to municipalities with at least three HPs) is 23.1 percent with a standard deviation of 7.2 percent. We also standardize this measure with mean zero and standard deviation one. We also standardize this measure with a mean of zero and a standard deviation of 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 level and are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Figure A1.** Abortion Rate per 1000 Women Aged 15–44, the Netherlands and the US, 1970–1984




Notes: The numbers for the United States and the numbers for the Netherlands 1973–1984 come from Tietze and Henshaw (1986), Table 2, pages 30–42. The numbers for the United States originate 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 several states do 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 percent 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: Level (Left-Hand-Side Graphs) and Mean Difference Across Cohorts (Right-Hand-Side Graphs)

**Figure A3.1**: Fertility and Marriage

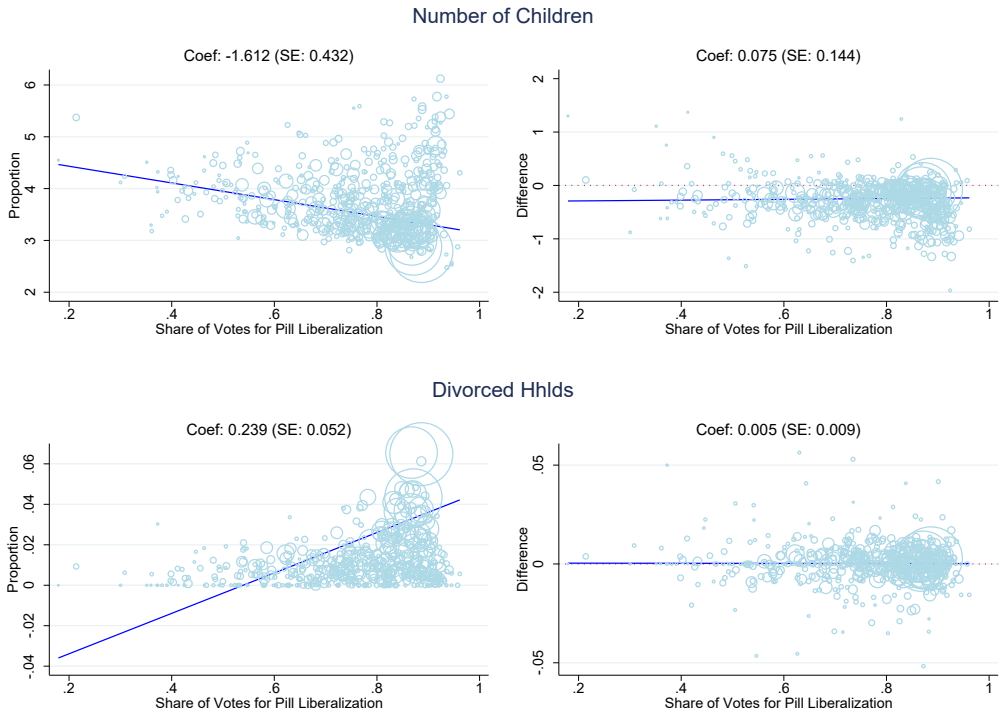



**Figure A3.2**: Education and Income

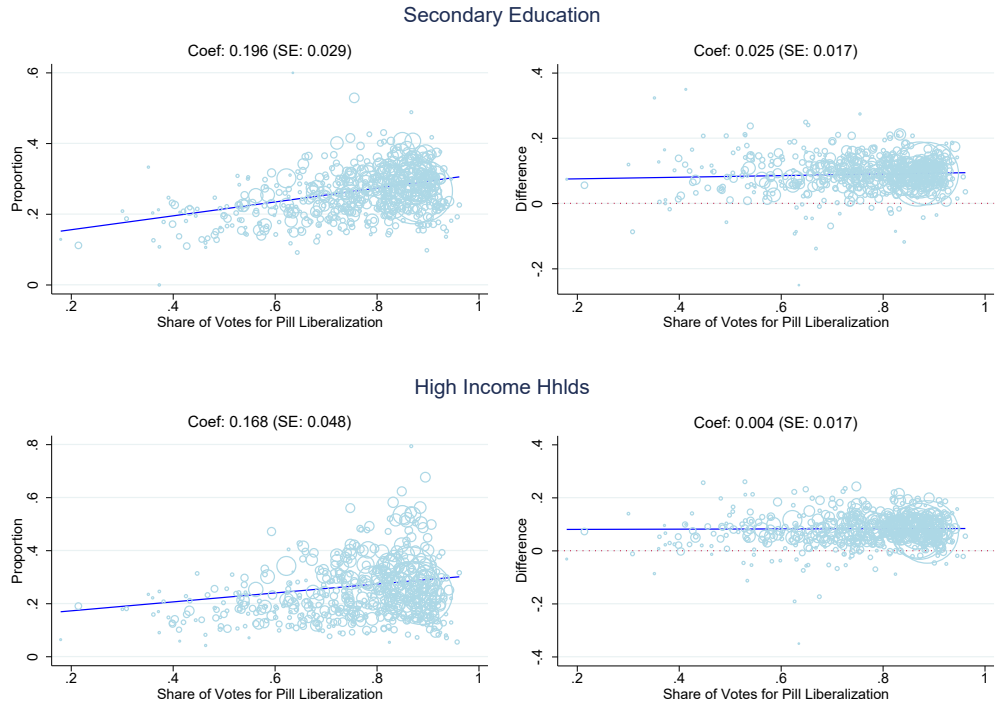
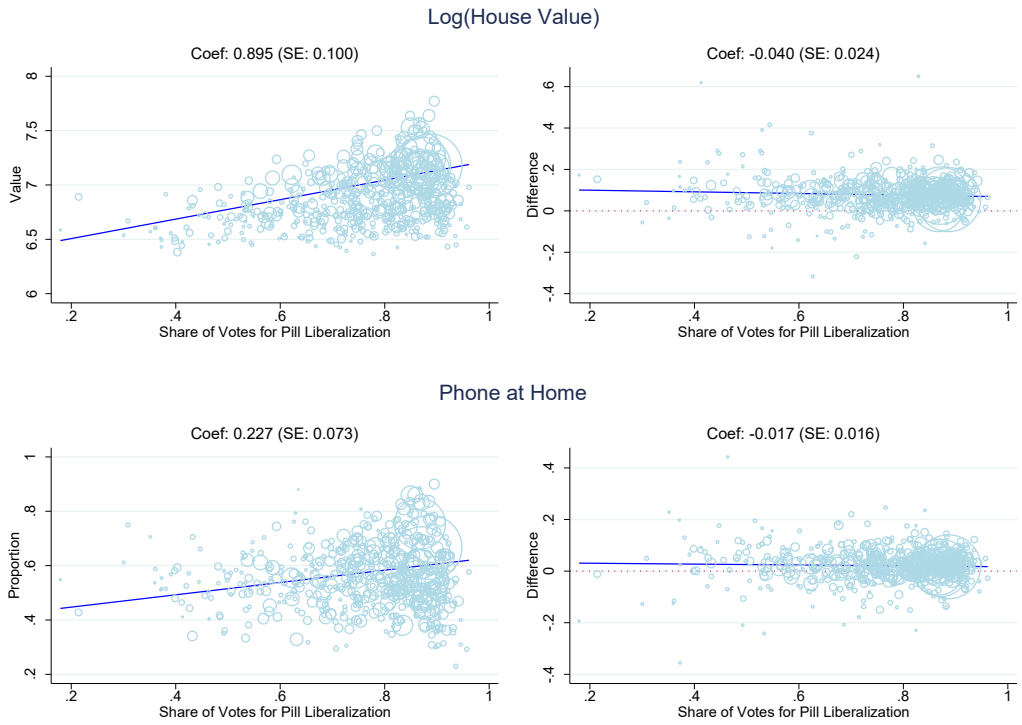


**Figure A3.** Continued

**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 having had a child and is from a cohort that was statistically most likely to be the parent of a woman born between 1944 and 1954 (i.e., with a head aged 46–61 in 1971). Note here that we do not use actual age of a child present in a household because a significant number of the older women from this sample had already formed their own household by the time of the 1971 census and we thus would not observe the characteristics of the household they grew up in. The typical parent of a woman born in 1944–1949 (control women) was aged 46–55 in 1971 and the typical parent for a woman born in 1950–1954 (treated women) was aged 52 and 61 in 1971. The graphs on the left shows the mean values of each characteristic for all selected households in a municipality and the graphs on the right show the mean value of the difference between the treated and control households within a municipality of these characteristics. The circles reflect the population size of each municipality, which also serves as weights, and the blue lines reflect the fitted value of the correlation with the slope coefficient and standard errors reported above each graph. **Figure A3.1** reports the total number of children born to a head of household and the proportion of household in which the head ever divorced. **Figure A3.2** reports the proportion of household heads who completed secondary education and the proportion of household heads classified as high income in the census (income higher than 16,000 a year, which encompasses the two highest income groups and applies to 7 percent of household heads). **Figures A3.3** reports the logarithmic value of the house or apartment the household lives in and the proportion of households that 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 obtained a university degres, 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 only 15 out of 500 times 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 for parties in favor of pill liberalization. The top graph shows the proportion of HPs who were Orthodox Protestant and the bottom graph the proportion of HPs who were 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 indicates 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 represents the median proportion of HPs who self-declared to be Orthodox Protestant (0.103) or Catholic (0.168) at the municipality level in the 1971 census.

**Figure A6.** Differences in Municipality Characteristics by Proportion of Health Professionals Who Were Opposed to the Pill on Religious Grounds

**Figure A6.1**: Number of GPs and Pharmacists per 1,000 Population

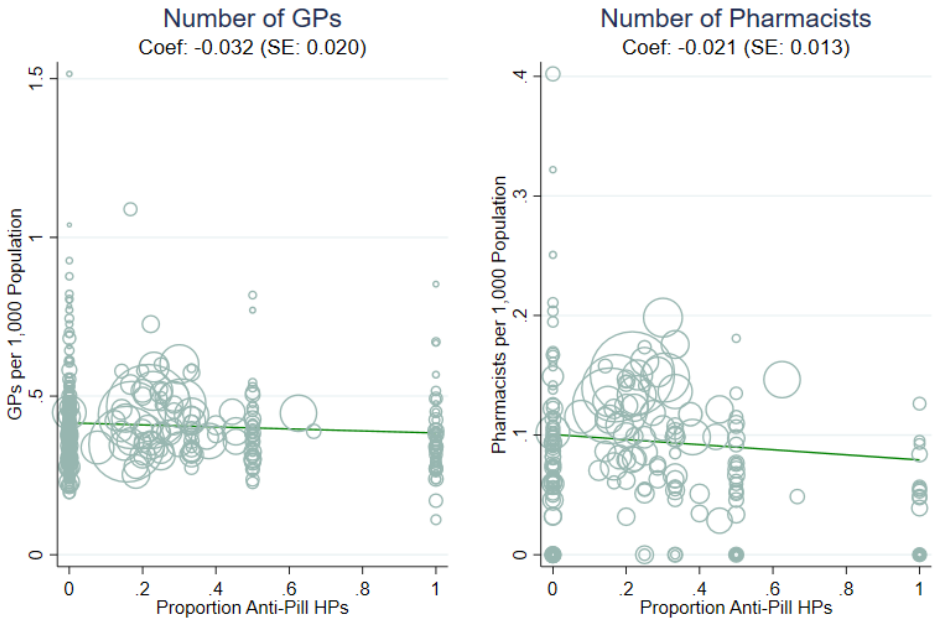



**Figure A6.2**: Proportion of Household Heads with Secondary Education and High Income

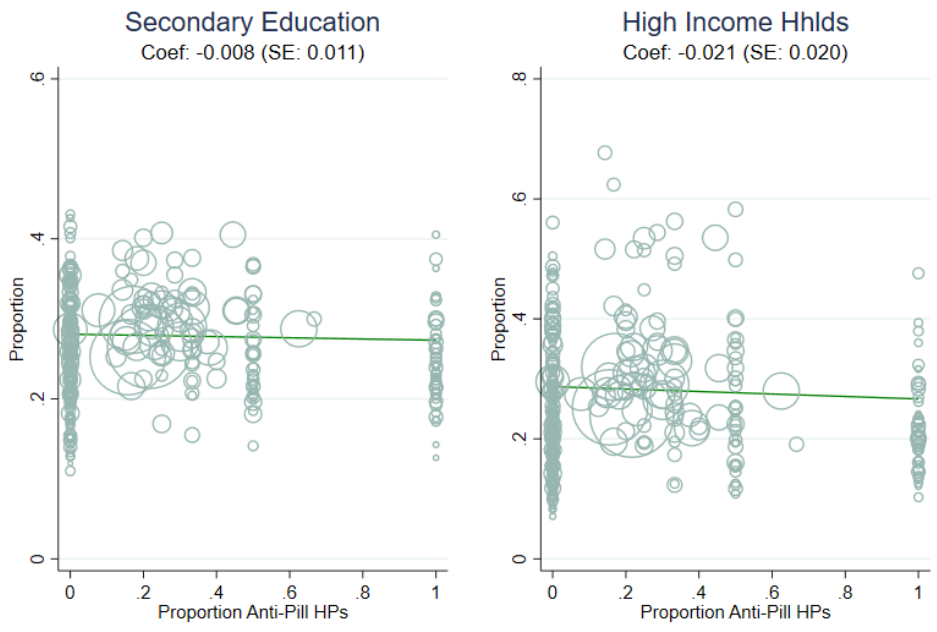
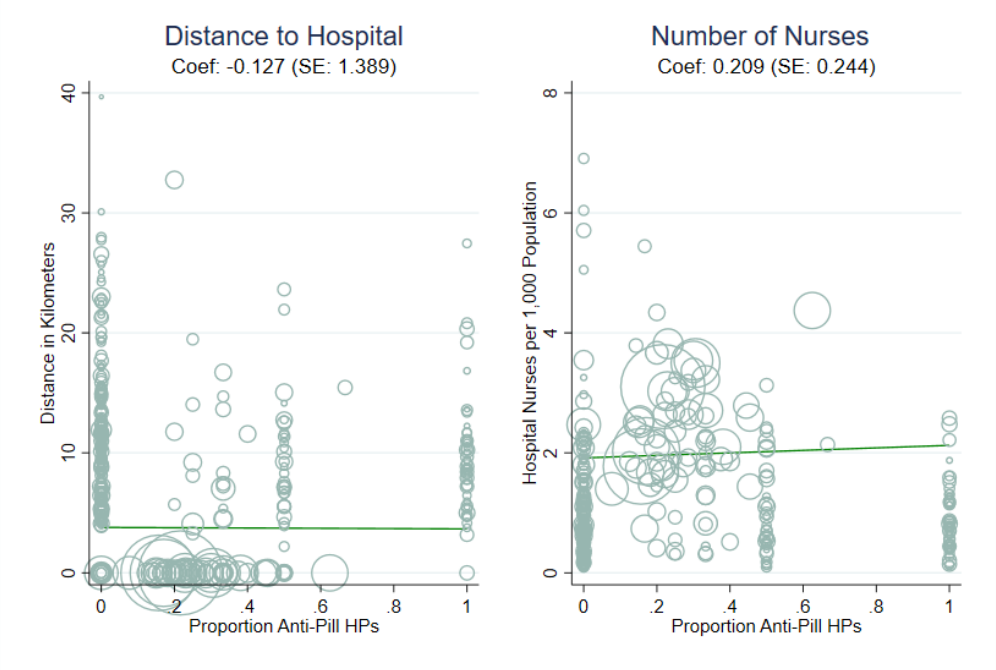


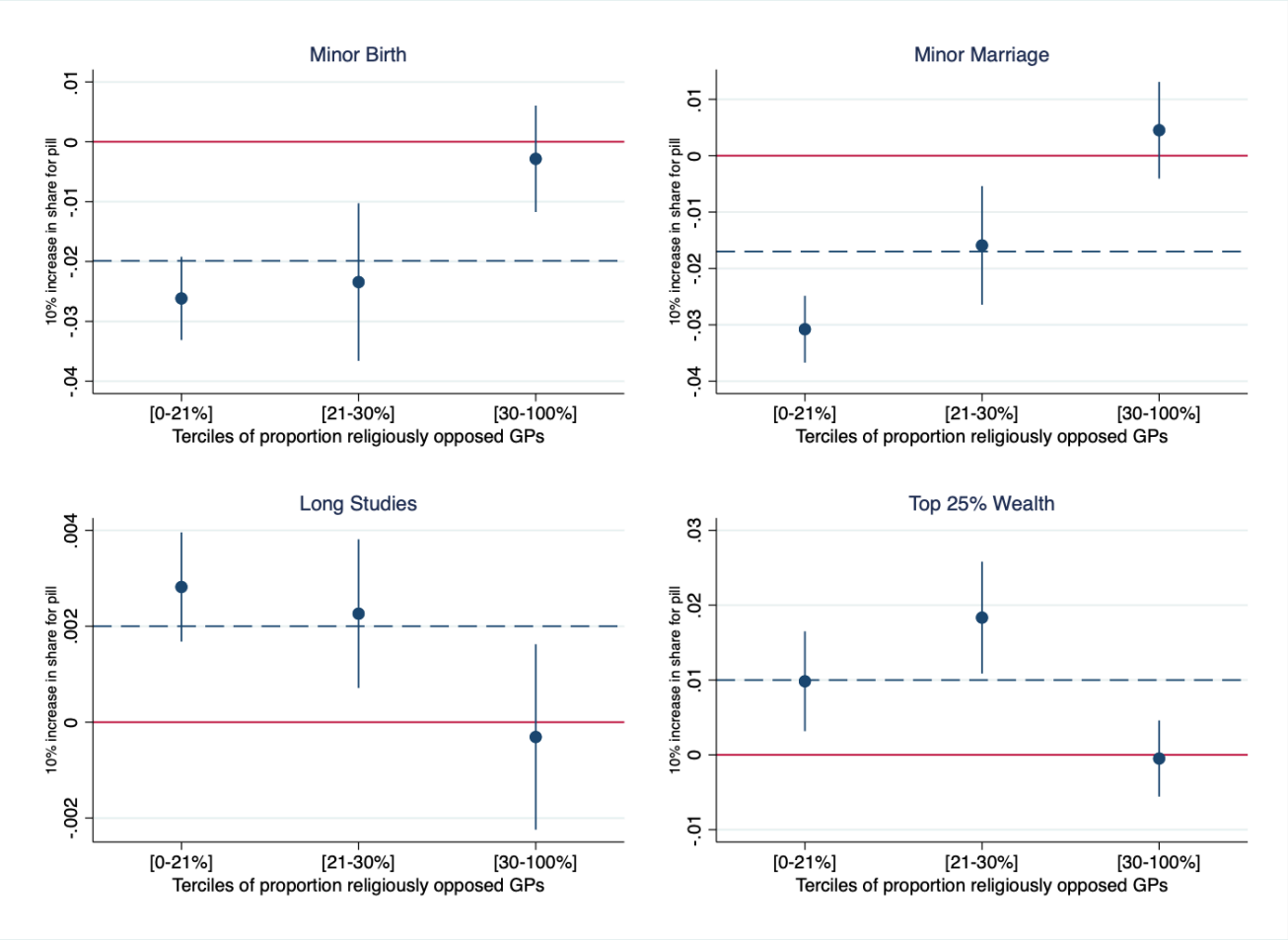
**Figure A6.** Continued

**Figure A6.3**: Distance to Hospital and Number of Nurses



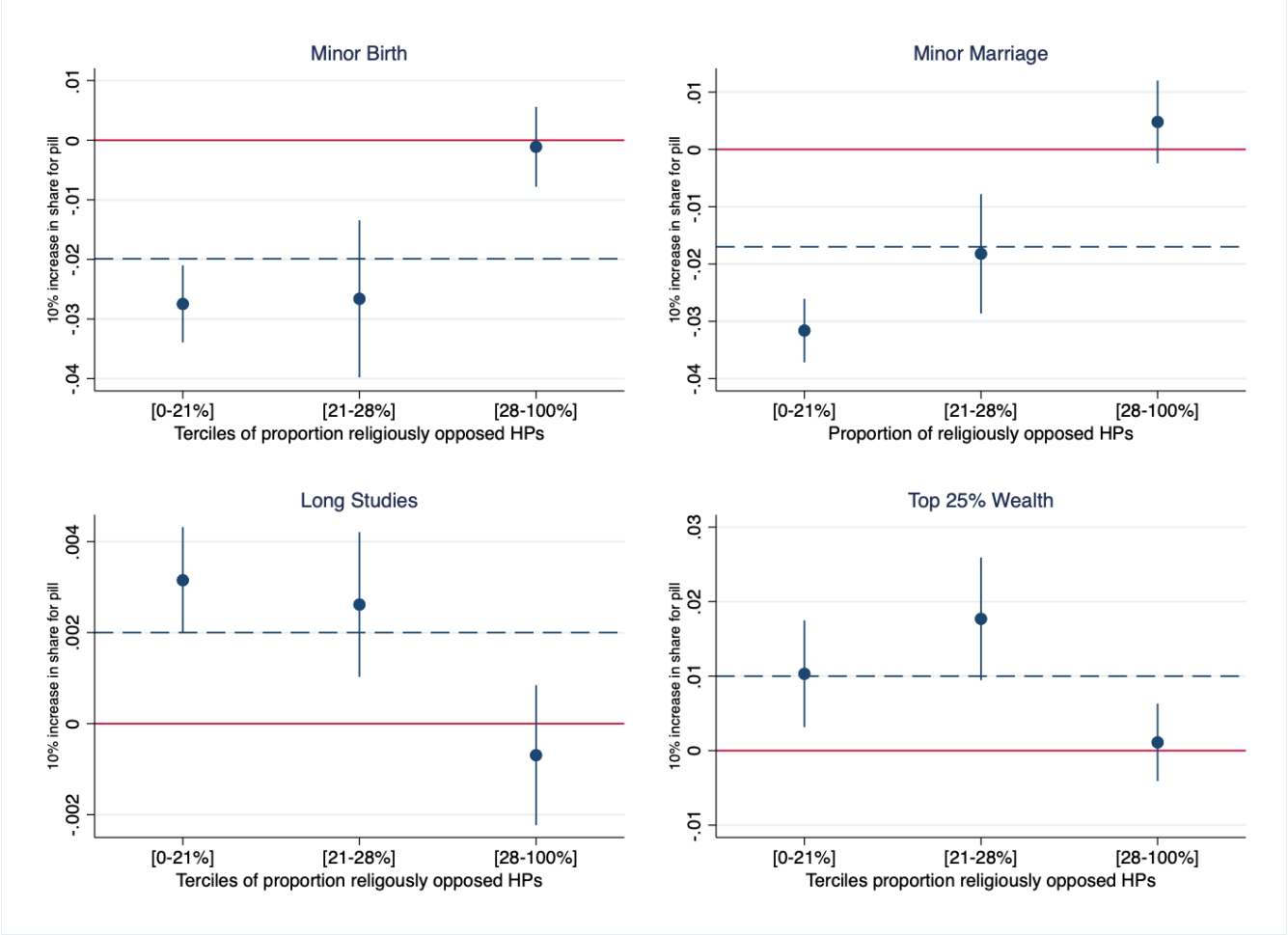

Notes: The graphs above plot the mean value in a municipality of various characteristics extracted from the 1971 Census against the proportion of health professionals (HPs) classified as "anti-pill" (i.e., self-identify as Catholic or Orthodox Protestant in the census) in the same municipality. The hollow circles reflect the total number of HPs in each municipality, which also serve as weights, and the lines represent the fitted value of the correlation with the slope coefficient and standard errors reported above each graph. **Figure A6.1** reports the number of general practitioners (GPs) and pharmacists per 1,000 population in each municipality, in the left and right graphs respectively. **Figure A6.2** reports the proportion of household heads who have completed secondary education and the proportion of household heads that are classified as high income in each municipality, left and right graphs respectively. **Figure A6.3** reports the distance in kilometers to the nearest (mid-size) hospital and the number of nurses per 1,000 population in each municipality, in the left and right graphs respectively.

**Figure A7.1** The Additional Effect of Gatekeepers Who Were Opposed to the Pill on Religious Grounds, by Tercile, Only Using General Practitioners (i.e., Excluding Pharmacists)



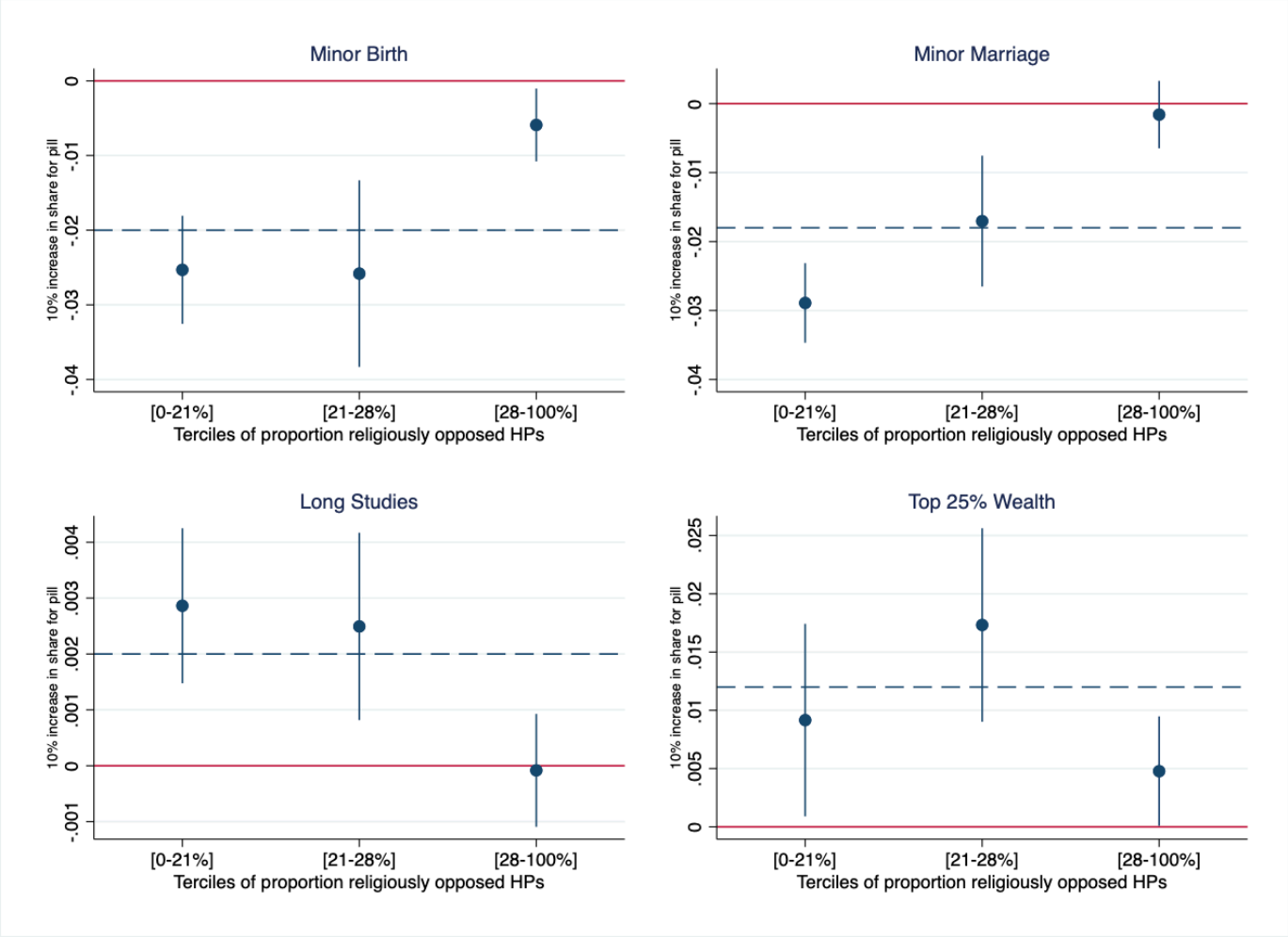

Notes: Estimated by OLS. Figures plot the additional effect of GPs who were opposed to the Pill on religious grounds (in terciles) on top of the social norms in an area (i.e., a triple interaction with our difference-in-differences estimator: $After_{ic} * ShareForPill_{im}$). *ShareForPill* is standardized with a mean of zero and a standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. The proportion of GPs opposed on religious grounds (with a mean of 23.9 and a standard deviation of 7.5 percent) is divided into terciles: 0–21 percent, 21–30 percent, and 30–100 percent. 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 level.

**Figure A7.2** The Additional Effect of HPs Who Were Against the Pill, Including Municipalities Without an HP, by Tercile.




Notes: Estimated by OLS. Figures plot the additional effect of HPs who are opposed to the Pill on religious grounds (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 a mean of zero and a standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. The proportion of HPs who were opposed on religious grounds (with a mean of 23.1 and a standard deviation of 7.5 percent) is divided into terciles: 0–21 percent, 21–28 percent, and 28–100 percent. We assign the proportion of HPs who were opposed on religious grounds in the closest municipality for municipalities without an HP. All specifications are weighted by the number of HPs in the (closest) municipality. Robust standard errors are clustered at the municipality level.

**Figure A7.3** The Additional Effect of HPs Who Opposed the Pill on Religious Grounds, Restricting to Municipalities With At Least Three HPs, by Tercile




Notes: Estimated by OLS. Figures plot the additional effect of HPs opposed to the Pill on religious grounds (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 a mean of zero and a standard deviation of one. One standard deviation in *ShareForPill* is about 10 percent. The proportion of HPs opposed on religious grounds (with a mean of 23.1 and a standard deviation of 7.3 percent) is divided into terciles: 0–21 percent, 21–28 percent, and 28–100 percent. We drop 320 municipalities with fewer than three HPs. All specifications are weighted by the number of HPs in the municipality. Robust standard errors are clustered at the municipality level.

# Appendix B: Data Appendix (For Online Publication)

## B.1 Construction of sample

We use administrative data from Statistics Netherlands, which contains information on all individuals who were registered in a Dutch municipality by 1995.[1] We start with the registry of persons (*GBAPERSOONTAB*) and select all women who were between ages 16 and 26 in 1970, and hence were 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 changed municipal boundaries over time, 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 consider the restructuring of municipalities.[2] We take these changes into account and assign the new municipal codes to women born in municipalities that changed. In cases in which 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 so we can determine outcomes like age at first birth, as well as completed fertility (the youngest women in our sample were age 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 12 years of age. We are left with 1,115,691 women who were born in the Netherlands and were between the ages of 16 and 26 in 1970. Given the small variation in voting patterns in the southern provinces of the Netherlands (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 less than 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 younger than 21 years of age. Finally, for the 727,201 women who ever got married and ever had a child, we define a shotgun marriage as one in which the child was born within seven months of the mother's marriage date. The seven-month time window (instead of eight or nine months) is chosen so premature births are not accidentally captured.

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 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 completed a university degree, whether in general or technical education. We also add a variable indicating that a woman finished a long-duration degree in law or medicine (medical school, dental medicine, or veterinary medicine).

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. Given that information on part-time work is only available from 2001 onwards, we take the labor market outcomes at age 56 for birth cohort 1945, and at age 57 for birth cohort 1946.

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 was 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 60–62 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 savings, stocks and bonds, the value of their house, and the value of their business. The wealth outcomes are not observed for individuals who were 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 to individuals for whom we observe wealth ages 60–62, which gives a sample of 810,525 individuals or 94 percent of the main sample of analysis.

### 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 southern provinces.

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 like the numbers reported by Statistics Netherlands for 1971, namely 4,504 GPs and 1,084 pharmacists (Centraal Bureau voor de Statistiek, 1994, 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 percent 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 were working, but this information is missing for about 1 percent 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 percent 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, with Pearson correlation coefficients of 0.9977 and 0.9982 respectively.