Journal of Health Economics 108 (2026) 103154

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/jhe

Consent-based laws and aggregate fertility☆

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ARTICLE INFO

JEL classification:
codes
I12
J13
K14
Z13

ABSTRACT

This paper examines how expanding the legal definition of sexual assault affects fertility and sexual behavior, using a panel of European countries. I find that switching to tacit consent-based legislation reduces fertility by about 4% relative to the mean. This effect is driven by a decrease in couple formation and an increase in abortion rates. Supporting evidence is consistent with a behavioral channel in which more risk-averse individuals withdraw from dating and partner markets following the reform, altering the composition of those who remain active toward a pool that is less precautionary. Consistent with this compositional shift, contraceptive use rises among younger women but declines among older age groups, while condom use falls among young men. Finally, an analysis of appeals court verdicts in Sweden following the adoption of consent-based legislation shows a decline in unanimous guilty verdicts, indicating challenges in assessing tacit consent. These results are consistent with a simple framework in which heterogeneity in risk perceptions and precautionary behavior in dating and partner markets, including reduced participation by some individuals, helps explain the observed decline in fertility following the reform.

1. Introduction

In recent years, many jurisdictions have shifted from coercion-based to consent-based sexual assault laws. Under such laws, absence of consent is sufficient for conviction, meaning that force is not required. This represents an expansion of the legal definition of sexual assault or rape. As of 2026, around one-third of European countries and nearly half of U.S. states have adopted consent-based legislation.

Proponents argue that changing to consent-based legislation shifts the responsibility from the victim to the perpetrator. Historically, sexual assault was often blamed on the woman, but under consent-based legislation, the responsibility shifts to the man to ensure consent (Bladini and Andersson, 2020). Another advantage is that consent-based laws recognize forms of coercion beyond physical force or threats of violence, such as psychological pressure (Dowds, 2020; Licea, 2022). Additionally, applying the term *rape* for non-consensual acts without coercion, as is done in some jurisdictions, amplifies the severity of the offense and may have a deterrent effect (Gruber, 2023). Finally, consent-based laws may contribute to behavioral changes by normalizing consent in sexual interactions, thereby increasing women's sexual safety by reducing the risk of unwanted encounters, and foster better sexual norms among boys and young men in particular (Hörnle, 2024). Specifically, women who previously perceived dating environments

☆ I am grateful to the Editor, Maria Polyakova, and to two anonymous referees. This work has benefited from discussions with Niclas Berggren, Andreas Bergh, Pascaline Dupas, Malin Gardberg, Peter Jochumzen, Johanna Möllerström, Martin Olsson, Dan-Olof Rooth, Sebastian Siegloch, and numerous seminar participants. Petter Gunnäs provided excellent research assistance. I acknowledge financial support from the Jan Wallander and Tom Hedelius stiftelse, grant number W23–0009.

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https://doi.org/10.1016/j.jhealeco.2026.103154
Received 16 September 2025; Received in revised form 18 May 2026; Accepted 20 May 2026

Available online 1 June 2026
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as unsafe might now feel more secure, thereby potentially increasing their engagement in relationship-building activities. In turn, this could have positive impacts on aggregate fertility.

Critics argue that proving non-consensual sex is problematic due to the scarcity of tangible evidence for non-consent. Thus, shifting to consent-based legislation may not lead to more convictions, which is often a chief reason why jurisdictions introduce such laws (Collins, 2016). Additionally, since most countries with consent-based legislation do not require affirmative consent, assessing what constitutes tacit consent can be difficult, leading to information asymmetry between sexual partners. Thus, consent-based laws that allow tacit consent, rather than requiring affirmative consent, may not be sufficient to ensure clarity in sexual interactions (MacKinnon, 2016). With this in mind, legal experts in some countries have advised against implementing consent-based laws.[1] Related to this, although false accusations are rare in practice, many men still worry about being falsely accused of sexual assault. A 2018 U.S. survey found that approximately one-third of men fear being unfairly accused of sexual assault (Ipsos, 2018). This concern is likely to be exacerbated when absence of consent is sufficient for conviction, and may lead men to become overly cautious around women.[2] Such a channel would, in turn, imply a decline in fertility when consent-based laws are enacted.

This paper examines the impact of changing from coercion- to consent-based sexual assault laws on fertility and sexual behaviors. In the empirical analysis, I use a panel of 31 European countries covering the period 2000–2023 and apply a staggered difference-in-differences framework. To the best of my knowledge, this paper is the first to study the effects of changing to consent-based laws on fertility and family formation. The results suggest that consent-based laws decrease aggregate fertility by an average of 0.07 children for the five-year period after enactment, corresponding to a 4% decline relative to the treatment group's baseline mean one year prior to treatment. The empirical results are robust across several alternative specifications, including the use of recently developed estimators to address concerns about the consistency of staggered difference-in-differences designs.

There are several potential reasons for why fertility declines after the introduction of consent-based laws. First, fertility may decline because fewer stable partnerships form, reducing the number of individuals who have children at all. Second, fertility may fall within existing relationships, either because contraceptive practices change or because unintended pregnancies are more likely to be terminated. To bridge these two channels, I first develop a unified theoretical framework to show that both channels can arise from changes in perceived risk and uncertainty in the marriage market. According to the model, individuals differ in their tolerance for risk, and legal changes may alter the perceived costs of intimate interactions. While consent-based laws are intended to increase safety, particularly for women, they may also increase legal uncertainty about tacit consent. In response, some otherwise law-abiding individuals — particularly men with higher risk aversion — may reduce their participation in dating markets or withdraw altogether. As a result, matching declines and the remaining dating pool becomes selectively composed of relatively more risk-tolerant men, which may increase risky sexual behavior post-law change. This is consistent with previous research showing that less risk-averse individuals are more likely to engage in risky sexual behavior (Westerlund et al., 2010; Buttmann et al., 2011; Caudillo et al., 2020; Lépine and Treibich, 2020). Importantly, this mechanism does not imply that the marginal individual affected by the reform is a potential perpetrator of sexual assault, but rather that behavioral adjustments arise from changes in perceived risk and uncertainty; in essence, an unintended "cooling" of the marriage market.

Consistent with this framework, I first document a decline in couple formation following the introduction of consent-based laws. The share of young adults living with their parents increases, as does the share of adults who have never married. These results suggest that reduced matching in the marriage markets directly accounts for part of the observed decline in fertility. At the same time, abortion rates increase, even after controlling for changes in national abortion laws. While prior work has emphasized the role of abortion stigma and legislation in shaping abortion behavior (Kumar et al., 2009), abortion has been legal for decades in most countries in the sample, making stigma-based explanations less likely. Instead, the combination of reduced matching and higher abortion rates is consistent with a compositional change in the marriage market post-reform: as more risk-averse men withdraw from dating markets, the remaining pool of potential partners becomes relatively less likely to practice safe sex. In turn, this may increase abortion rates and the use of contraceptives among females.

To further assess the mechanisms underlying these effects, I examine a number of other outcome variables that shed light on how individuals adjust to changes in dating and partner markets following consent-based reform. Using cross-country survey data, I first show that condom use among young men declines after the introduction of consent-based laws. I then analyze female contraceptive behavior using administrative prescription data from Denmark, Norway, and Sweden. Contraceptive use among women aged 20–24 increases modestly, driven by higher uptake of oral contraceptive pills, while overall contraceptive use declines among women aged 25–44. These age differences are consistent with heterogeneous adjustment, with younger women relying on flexible fertility-control methods and older women responding primarily by reducing participation rather than substituting toward long-acting contraception. I also document a corresponding increase in the prevalence of bacterial sexually transmitted diseases (STDs) across all age groups. While these outcomes do not map directly to fertility, they are broadly consistent with an increase in precautionary behavior, reinforcing the cooling mechanism emphasized in the paper.

[1] In Sweden, the government's advisory body on legislation, consisting of current and former Supreme Court justices, advised against introducing consent-based laws. Their main criticism was that such legislation "relies on subjective judgment of individual judges regarding which [sexual] acts should not be considered voluntary in each specific case" (Swedish Council on Legislation, 2018). This, they argued, undermines the established legal principle that a person can only be punished for an act if the penalty for it is clearly defined in advance (*nulla poena sine lege*). Still, Parliament unanimously passed the law change in 2018.

[2] In the survey described above, 56% of respondents believe that false accusations of sexual assault against men are very common, including 51% of women. In reality, however, false accusations of sexual assault are highly unusual. Recent studies have estimated the rate of false reports at 5% or less (Ferguson and Malouff, 2016). These differences in perception between the public and academics may partially stem from the tendency of police officers to overestimate the proportion of false reports of sexual assault (Saunders, 2012; McMillan, 2018). See also Rumney (2006) for a general discussion about methodological issues in this context.

This sequence of behavioral adjustments is consistent with evidence from other settings where perceived sexual risk changes. For example, Spencer (2024) shows that the spread of HIV/AIDS in the United States led to simultaneous increases in births and abortions through behavioral responses in partnership formation and contraceptive choice. Taken together, these findings illustrate how shifts in sexual risk and protection can generate counterintuitive fertility outcomes, including the coexistence of declining fertility, rising abortion rates, and changes in contraceptive use.

Finally, if it is the case that consent-based reforms increase legal uncertainty around tacit consent — a concern frequently raised by critics and implied by the cooling hypothesis — I construct a novel dataset covering the majority of appellate court verdicts from Sweden, which switched to a consent-based law in 2018. Sweden's legal system does not require unanimous verdicts in criminal trials, and professional judges decide on guilt and sentencing together with jurors, who are members of the public. I show that after the introduction of the consent-based law, the share of unanimous verdicts with respect to guilt decreased by over 60%. In cases where the defendant was ultimately convicted, dissenting opinions more commonly came from jurors compared to professional judges. This suggests that under a consent-based system, some members of the public may struggle to assess what constitutes tacit consent.

This paper contributes to several strands in the literature. A number of recent papers have examined the role of statutory rape laws in reducing teenage fertility (Frakes and Harding, 2015; Lepage, 2022).[3] Consistent with the conclusions of this paper, these two studies suggest that the expansion of such laws reduces birth rates. A key advantage of the present study is that it examines overall fertility, as opposed to just teenage fertility. Other legal reforms commonly studied by economists include criminalization and decriminalization of sex work, as well as changes in abortion legislation. For laws regulating sex work, most studies have found that criminalization increases cases of sexual assault, and that the reverse holds true for decriminalization (Cunningham and Shah, 2021). Additionally, a substantial body of research has examined the impact of abortion legislation on a wide range of outcomes. In terms of fertility, most studies find that direct measures, such as changes in gestational limits or the regulation of abortion providers, are associated with higher fertility rates when restrictive laws are implemented (Dench et al., 2024; Jones and Pineda-Torres, 2024). Similarly, fertility rates decline following the liberalization of abortion (Levine et al., 1999; Valente, 2014). However, the effects of more indirect legal measures, such as parental involvement laws, are less consistent, with some studies identifying significant impacts on birth rates, while others report no effects (Ellertson, 1997; Myers and Ladd, 2020). A related paper is also the work by Muratori (2025), showing that more restrictive abortion legislation is associated with an increase in the number of reported violent offenses against women.

Secondly, the paper relates to a growing literature documenting unintended effects in two-sided matching environments arising from increased legal or reputational risk. Recent work shows that the #MeToo movement led to reductions in cross-gender professional interactions and collaboration, consistent with precautionary behavior rather than changes in underlying preferences (Amano-Patiño et al., 2025; Gertsberg, 2026). While these studies focus on labor markets effects, they provide evidence consistent with the idea that heightened perceived risk can reduce interaction and matching between men and women even in the absence of explicit prohibitions. The setting in this paper differs in that it studies the marriage and dating market rather than labor-market collaboration, but the underlying mechanism — reduced matching driven by precautionary responses to legal or reputational risk — is closely related. These findings support the interpretation that consent-based reforms may generate cooling effects in two-sided matching markets through similar channels.

A small but growing body of research also examines how legal changes affect risky sexual behavior, providing evidence on intensive-margin responses among individuals who remain sexually active. Studies from developed nations tend to use infection rates of bacterial STDs, such as chlamydia and gonorrhea, as proxies for risky sexual behaviors, with commonly studied legal changes including abortion restrictions, parental involvement laws, and alcohol laws (Carpenter, 2005; Markowitz et al., 2005; Klick et al., 2012; Colman et al., 2013). Similarly, Cameron et al. (2021) show that criminalization of sex work may have unintended consequences in the form of a rise in STDs among sex workers, but also in the general population.

Additionally, since expanding the legal definition of sexual assault and rape makes more acts illegal and, in some jurisdictions, classifies them as rape — the most serious sex offense — this paper contributes to the broader literature on the effects of increasing punishment severity (see Chalfin and McCrary, 2017 for a summary). The finding that expanding the legal definition of sexual assault leads to a larger share of dissents among judges and jurors is consistent with experimental evidence showing significant variation in public views on the appropriate punishment for sex crimes, depending on the circumstances and legal frameworks surrounding the specific case (Ben-David and Schneider, 2005; Schwarz et al., 2022). Thus, changing to consent-based laws adds an extra layer of complexity to each case, as absence of consent is harder to assess than presence of coercion. It also aligns with previous findings suggesting that increasing punishment severity may reduce courts' propensity to convict the defendant (Bindler and Hjalmarsson, 2018).

Finally, this paper contributes to the broader theoretical literature on asymmetric information in the marriage market. The finding on compositional changes aligns with Akerlof (1970). However, the closest related work is Angelucci and Bennett (2021), who specifically examine compositional changes in the marriage market and show that reducing adverse selection in the dating market can increase fertility and marriage rates. In this paper, I study the opposite situation, finding that fertility rates may decrease if consent-based laws lead to differing interpretations of consent and, consequently, greater information asymmetry. More broadly, this paper also relates to previous theoretical research on partner choice and matching (Becker, 1973; Burdett and Coles, 1997; Smith, 2006; Chiappori et al., 2012; Galichon and Salanié, 2022; Antler and Bachi, 2022).

[3] Statutory rape refers to the situation in which a person above the age of consent engages in sexual activities with a person below the age of consent, regardless of whether the younger person has given consent.

The rest of the paper is structured as follows. Section 2 describes the differences between coercion- and consent-based sexual assault laws, and provides a historical background. Section 3 describes the data. Section 4 provides the cross-country results on fertility, while Section 5 discusses potential mechanisms. The paper concludes with Section 6.

2. Coercion- versus consent-based laws

2.1. Historical background

The traditional definition of rape involves coercion of some sort, which may include, for instance, violence, physical force, or threats of violence. Globally, most jurisdictions retain this definition, while some jurisdictions require only the absence of consent for a conviction of rape or sexual assault. Hong Kong was the first jurisdiction to adopt consent-based legislation in 1978. While a sizable number of states in the U.S., along with Australia, Canada, New Zealand, and a few countries in Africa, have implemented similar laws, the majority of countries with consent-based legislation are in Europe. Two European countries, Ireland and Belgium, had changed to consent-based legislation already in the 1980s; however, there was little progress in other nations until the early 2000s.

During this time, two events can be said to have laid the foundations for a broader roll-out of consent-based legislation throughout Europe. The first was the case *M.C. v. Bulgaria* (39272/98) before the European Court of Human Rights, which was decided in 2003. In this case, a 14-year-old girl was alleged to have been sexually assaulted by two men. The Bulgarian police closed the investigation, as it could not be proven that the perpetrators had used threats or violence. In her application before the Court, she argued that Bulgaria had violated basic human rights since only cases where the victim had resisted actively were criminalized. The Court found that Bulgaria had indeed violated the human rights of the applicant, finding, *inter alia*, that "the [police] investigation and its conclusions must be centered on the issue of non-consent" (see *supra*, at p. 181). Following the verdict, there was a debate in several countries if the verdict required member states of the Council of Europe to adopt consent-based legislation (Conaghan, 2005; Asp, 2009).

Secondly, in 2011, the member states of the Council of Europe adopted the Istanbul Convention, which is a legally binding document opposed to violence against women and domestic violence. Article 36 of the Convention states that "parties shall take the necessary legislative or other measures to ensure that [...] engaging in non-consensual vaginal, anal or oral penetration of a sexual nature of the body of another person [...] [is] criminalised". The convention was eventually ratified by 38 of the Council of Europe's 46 member states. In 2011, Luxembourg became the first country in continental Europe to adopt a consent-based law. By the late 2010s, several additional countries introduced consent-based laws, including Denmark, Germany, Sweden, and Spain. Both in Germany and Sweden, the Istanbul Convention was explicitly mentioned in the legislative histories for the laws.[4]

2.2. Affirmative and tacit consent

As explained previously, most jurisdictions where consent-based legislation is applied do not require affirmative consent.[5] Instead, for a criminal conviction, it usually suffices that the victim's absence of consent is known to the perpetrator. Thus, there must be tacit consent, meaning that the parties must express their willingness to participate through words or actions, but it need not be explicit, such as through a verbal "yes".[6] With this in mind, how non-verbal consent should be expressed is subject to significant debate within the juridical literature (Wertheimer, 2003; McGregor, 2005; Gruber, 2016; Dsouza, 2022).

In economic terms, relying on tacit rather than affirmative consent may introduce asymmetric information, which, in sexual situations, is fundamentally different from asymmetric information in other contexts. The chief reason for this is that in sexual situations, the physical proximity between participants allows them to express consent in many more ways than is normal. While it is possible for two individuals engaging in sex to explicitly consent to the other person's actions by saying "yes, continue" for each move, it is likely far more common for consent in sexual situations to be given non-verbally, chiefly through reciprocal actions. Such levels of non-verbal communication are impossible in most other transactions. Instead, in sexual situations, the problem of information asymmetry arises when there is misinterpretation of tacit consent. One party — typically the man — may interpret certain tacit signals from the other party as an approval to proceed with the sexual act. The other party — usually the woman — who believes that she has expressed her lack of consent, may interpret the man's persistence as a sign that he intends to have sex with her against her will. In turn, this can cause her to become frightened and submit against her will (Bryden, 2000).

On the other hand, while in some situations it may not be clear how tacit consent should be interpreted, some have argued that consent-based laws could foster a more positive attitude toward sexual interactions more generally (Buchhandler-Raphael, 2011).

[4] The legislative history is the collection of materials generated throughout the lawmaking process. In many European countries, the most important part of the legislative history are committee reports, and in some jurisdictions, courts are allowed to use the legislative history when deciding cases, as they help to clarify the lawmaker's intent.

[5] There have been discussions in individual countries about requiring affirmative consent. For instance, the Swedish government, in its reasoning for accepting tacit consent in conjunction with the law change, deemed that "it would be detached from reality to impose formal requirements on how the parties should express themselves during sexual activity" (Swedish Government, 2018, p. 32).

[6] For instance, the California Penal Code §261.6 defines consent as "positive cooperation in act or attitude pursuant to an exercise of free will. The person must act freely and voluntarily and have knowledge of the nature of the act or transaction involved". Similarly, in the legislative history for the revision of §177 of the German criminal law, the parliamentary committee responsible stated that "it should be sufficient [for conviction] that the will of the victim was known, and that it was ignored by the perpetrator".

This has also been recognized by policymakers. For instance, in the legislative history of the Swedish law change, one motivation for changing to a consent-based system was to encourage behavioral changes in sexual situations; in particular, to promote consent-seeking and carefulness more broadly (SOU 2016:60, 2016, p. 418). An example of a situation where consent-based laws could help prevent unwanted sex is if the other party is intoxicated—a scenario in which it is typically difficult to obtain informed consent. However, a potential problem arises when individuals fear they might overstep boundaries even when the other party is willing to engage in sex, leading to excessive caution.[7]

3. Data

The paper uses four sets of data: a cross-country panel on fertility, abortion rates, and health outcomes, cross-country survey data, Scandinavian register data on female contraceptive prescriptions, and data from Swedish courts on rape verdicts.

3.1. Cross-country data

The cross-country data consists of a panel of most of the European OECD member states, covering the years 2000–2023. Besides the European OECD member states, I additionally include Bulgaria, Croatia, Cyprus, Malta, Montenegro, Romania, and Ukraine.[8] I exclude the Republic of Ireland and the United Kingdom, as these countries have a common law legal system, which differs substantially from the civil law system found in other European nations. I also exclude Belgium, which adopted a consent-based system as early as 1989. In total, this leaves us with 31 countries. Fig. 1 displays a map of Europe, showing treated countries, never treated countries, as well as countries excluded from the dataset.

Countries adopt consent-based laws at different points in time. For the purposes of the staggered difference-in-differences analysis, we define treatment cohorts by the first calendar year in which a country adopts a consent-based law. Panel A of Table A.1 of Online Appendix A lists all treated countries grouped by their cohort, along with the number of countries in each cohort. The table also reports two measures of cohort size. The first is an unweighted share, defined as the fraction of all treated countries that first adopt in a given year. The second is a population-weighted share, defined as the fraction of the total treated population residing in countries within each cohort, using population measured in the year immediately prior to treatment. Both shares are non-cumulative and sum to one across cohorts. Panel B of Table A.1 of Online Appendix A displays the staggered timing of treatment adoption across countries in a figure.

Fertility is defined as the total fertility rate (TFR), which is the average number of children born to a woman over her lifetime. For the whole sample period, the average TFT is around 1.54, while the replacement fertility rate is usually assumed to be around 2.1. As further outcome variables, I use data on the share of young adults (aged 25–34) who live with their parents, the overall share of unmarried adults, the abortion rate, and the infection rates per 100,000 population of chlamydia and gonorrhea. As controls, I use the unemployment rate and Eurostat's housing price index, which takes the value 100 at year 2015. Table A.2 of Online Appendix A presents the summary statistics. Online Appendix B presents the data sources for all variables used in the paper. Online Appendix C compares the wording of the laws in all countries that have adopted consent-based legislation.

3.2. Survey data

To answer questions about changes in sexual habits, I use data from the WHO's Health Behaviour in School-aged Children (HSBC) study. This is a cross-country survey of adolescent health and well-being conducted every four years. Since the survey is done quadrennially, the missing values are imputed with the most recent non-missing value. The subjects are aged 15, and for the adolescents who report that they have lost their virginity, the survey asks about contraception use at the time of the most recent intercourse. This allows us to check whether contraceptive use among adolescents was affected by the law change. Here, I focus on male condom use, and rely on higher-quality register data to analyze female contraceptive use.

3.3. Scandinavian register data

I complement the cross-country survey data with administrative prescription register data from Denmark, Norway, and Sweden. These registers record all dispensed prescription contraceptives at the individual level and provide comprehensive coverage of the population. I use information on the type and timing of dispensed contraceptives to construct age-specific measures of contraceptive use by method, including oral contraceptive pills and long-acting reversible contraceptives. However, since these prescription registers are only available for a subset of countries, these analyses are somewhat limited in geographic scope and should be interpreted accordingly.

[7] Although we typically associate such situations with temporary sexual relationships (e.g., "one-night stands"), previous research has shown that many long-term relationships begin with such temporary sexual connections (Fisher, 2015). Thus, even a modest decrease in temporary sexual relations could adversely impact couple formation and, in turn, fertility.

[8] For Ukraine, the panel ends in 2021.

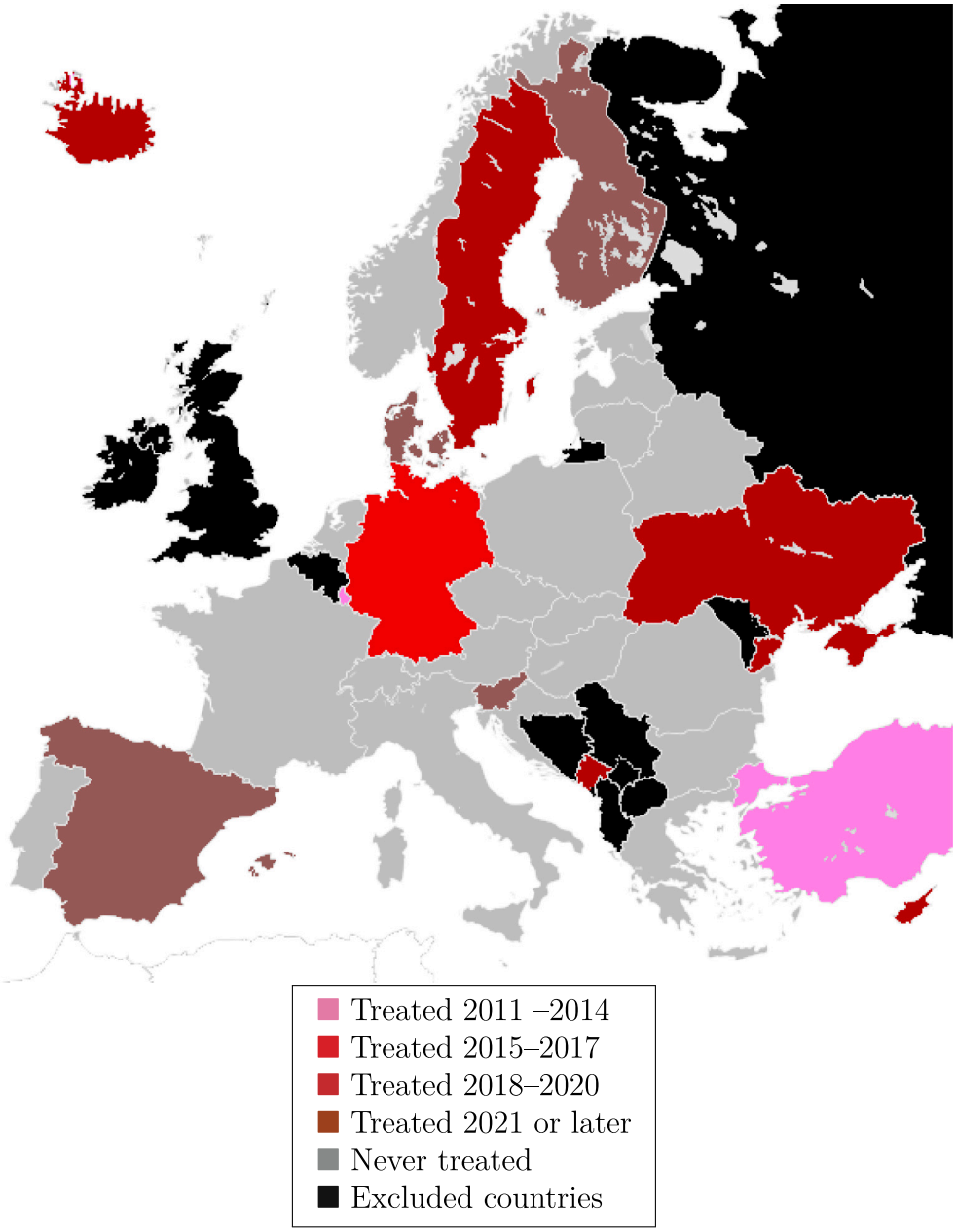


Fig. 1. Map of Europe displaying treated and control countries, with treatment years given as intervals.

3.4. Swedish court data

The dataset on verdicts covers the time period from 2015 to 2022, and consists of verdicts from Sweden's six appellate courts. I exclude cases about other sex crimes than rape, such as sexual harassment. In addition, I exclude cases of attempted rape, cases of aggravated rape, and cases where the victim was below 15 years of age, which is the legal age of consent in Sweden.[9] This leaves us with a total of 870 verdicts, of which the defendant was convicted in 676, or 78%, of these cases. In 150 cases — corresponding to 17% of total verdicts — there was at least one dissenting opinion regarding the guilt of the defendant.[10]

[9] Aggravated rape is a felony of its own, and includes cases where there is significant violence involved, such as when the victim was assaulted. Thus, questions about tacit consent are not relevant for this felony.

[10] Cases in which the dissent was about what penalty should be imposed on the defendant are classified as unanimous.

Table 1
Two-way fixed effects estimates.

Outcome variable: Total fertility rate	(1)	(2)
Treated	−0.141**	−0.045
	(0.065)	(0.065)
Country FE	Yes	Yes
Year FE	Yes	Yes
Controls	No	Yes
Observations	742	475
R^2	0.210	0.269
Mean dep. var.	1.58	1.58

Note. Outcome variable: Total fertility rate. Standard errors clustered by country in brackets. ** denotes significance at the 5% level.

4. Cross-country results

4.1. Two-way fixed effects estimates

To evaluate the effect of consent-based sexual assault laws on aggregate fertility, I begin by estimating a conventional static two-way fixed effects (TWFE) difference-in-differences model:

$$y_{it} = \alpha_i + \gamma_t + \beta \text{Treated}_{it} + \varepsilon_{it}, \tag{1}$$

where y_{it} denotes fertility in country i and year t, α_i are country fixed effects, γ_t are year fixed effects, Treated_{it} is an indicator equal to one if country i has adopted a consent-based law by year t, and ε_{it} is an idiosyncratic error term.

The results are presented in Table 1. Column (1) includes country and year fixed effects only, while column (2) additionally controls for the unemployment rate and house prices, measured using Eurostat's housing price index. When no additional controls are included, the estimates suggest that switching to consent-based legislation is associated with a reduction in aggregate fertility of approximately 0.14 births per woman-year. Adding controls reduces the point estimate to around 0.05 and renders it statistically insignificant. This sensitivity is consistent with the well-known fragility of TWFE designs in settings with staggered treatment adoption and heterogeneous treatment effects, as well as with the fact that time-varying controls can absorb variation correlated with treatment timing. Accordingly, these estimates should be descriptively rather than causally interpreted.

4.2. Event-study estimates

This section presents the main empirical results. I estimate the effect of consent-based legislation on fertility using difference-in-differences methods that account for staggered policy adoption, and assess the robustness of these findings through a range of sensitivity and diagnostic analyses.

1. *Empirical Strategy and Identification Assumptions*

A limitation of the static TWFE specification is that it assumes homogeneous treatment effects across groups and over time. In settings with staggered adoption such as in the one studied in this paper, this assumption is often implausible: countries adopt consent-based legislation at different points in time, and the effects of such reforms may evolve over time. As shown by Goodman-Bacon (2021), the TWFE estimator may be biased if the goal is to estimate average treatment effect on the treated (ATT). To allow treatment effects to vary flexibly with time since adoption and to examine pre-trends, I summarize outcomes using an event-study framework based on relative time indicators:

$$y_{it} = \alpha_i + \gamma_t + \sum_{e=-4}^{6} \beta_e D_{it}^e + \varepsilon_{it}, \tag{2}$$

where D_{it}^e is an indicator equal to one if, for country i in year t, the introduction of the consent-based law occurred e years earlier (or will occur $|e|$ years later). I include four pre-treatment periods and six post-treatment periods for the fertility estimates, while I use five post-periods for the mechanism results in Section 5, due to data limitations for the abortion variable.

Eq. (2) serves as a representation of event-time coefficients rather than as the estimating equation underlying the main results. In the presence of staggered adoption, different estimators map these coefficients into distinct causal parameters under different identifying assumptions. I therefore rely on the estimator proposed by Callaway and Sant'Anna (2021), which directly identifies the ATT under a group-specific parallel-trends assumption. This assumption requires that, absent treatment, outcomes for each treated country would have followed the same trend as outcomes for the appropriate comparison group. The Callaway–Sant'Anna estimator allows treatment effects to vary across adoption cohorts, time periods, and covariates, and does not impose functional-form restrictions on treatment-effect dynamics.

The baseline estimates are population-weighted, so that treatment effects are interpreted as the average impact on an individual residing in a treated country rather than the average impact across treated countries. For fertility outcomes, a population-weighted

Table 2
Callaway–Sant'Anna ATT estimates.

Outcome variable: Total fertility rate	(1)	(2)
Pre-reform ATT	−0.010	−0.005
	(0.010)	(0.012)
Post-reform ATT	−0.067**	−0.073***
	(0.033)	(0.019)
Country FE	Yes	Yes
Time FE	Yes	Yes
Controls	No	Yes
Observations	742	475
Mean dep. var.	1.58	1.58

Note: Post-reform average treatment effects on the treated (ATTs) estimated using the Callaway–Sant'Anna (2021) estimator. Outcome variable: Fertility rate. Controls: Unemployment rate and house prices. Standard errors clustered by country in brackets. ** and *** denote significance at the 5% and 1% level, respectively.

estimand is more appropriate than an unweighted country-level estimand because fertility is defined over individuals rather than countries. Weighting by population yields an average effect at the individual level, whereas an unweighted estimand assigns equal influence to countries with vastly different population sizes.

Before presenting the results, I summarize the key assumptions required to identify average treatment effects on the treated (ATT) in the staggered difference-in-differences design. The first assumption is that countries do not adjust fertility-related outcomes in anticipation of the adoption of consent-based laws, so that pre-treatment outcomes are unaffected by future treatment status. This assumption is supported by the absence of pre-trends in the event-study analyses and by the institutional features of the reforms, which are implemented through legislative processes with uncertain timing. Second, in the absence of treatment, treated and not-yet-treated countries are assumed to follow parallel trends in outcomes, conditional on observed covariates, including country and year fixed effects and additional controls. Under this assumption, post-treatment differences can be attributed to the adoption of consent-based laws. Finally, identification requires sufficient overlap in observable characteristics between treated and comparison countries. Overlap is assessed using a propensity score histogram in Figure A.4 of Online Appendix A.

2. *Main Results*

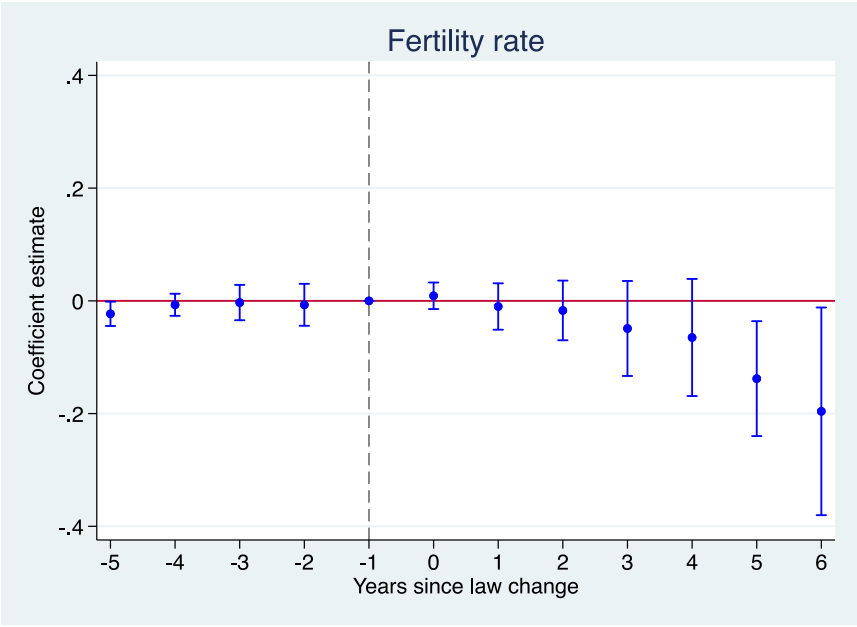
Fig. 2a presents the event-study estimates, providing the point estimates and corresponding 95% confidence bands for the Callaway–Sant'Anna estimator without additional controls, weighted by population. First, the pre-reform results support the assumption of an absence of pre-trends, with the pre-reform point estimates all being close to zero in magnitude. Second, for the post-reform estimates, the results indicate that negative effects on fertility appear three to four years after the law change, with the coefficients being statistically significant for the fifth and sixth post-periods. This is consistent with observed time lags from couple formation to first child.[11] Numerically, the Callaway–Sant'Anna estimates suggest that the effect sizes correspond to a decline in fertility of around 0.15–0.20 children per year after four years. This is approximately 9–13 percent relative to the treatment group's weighted baseline mean at $t-1$, which is equal to 1.58.

We may also include controls for unemployment rates and housing prices. These event-study plots are reported in Fig. 2b. The effects are significant at the 5% level for time periods +3 to +5, and at 10% for time +6, and of similar magnitude as the unweighted coefficients. In addition to the visual guidance provided by the event-study plots, Table 2 reports the numerical estimates for the ATTs using the Callaway–Sant'Anna estimator, again with a window of five pre- and six post-periods. The ATT for fertility is estimated to be around 0.07 children regardless of whether controls are included. This corresponds to a decrease in fertility of approximately 4 percent relative to the treatment group's weighted baseline mean at $t-1$. The post-reform ATT coefficients are significant at the 5% level when excluding controls, and at the 1% level when including controls. Note, however, that the estimated ATTs also take into account the first few years after the law change, when there was a null effect. The pre-reform ATTs are all close to zero in magnitude, indicating no significant issues with pre-trends.

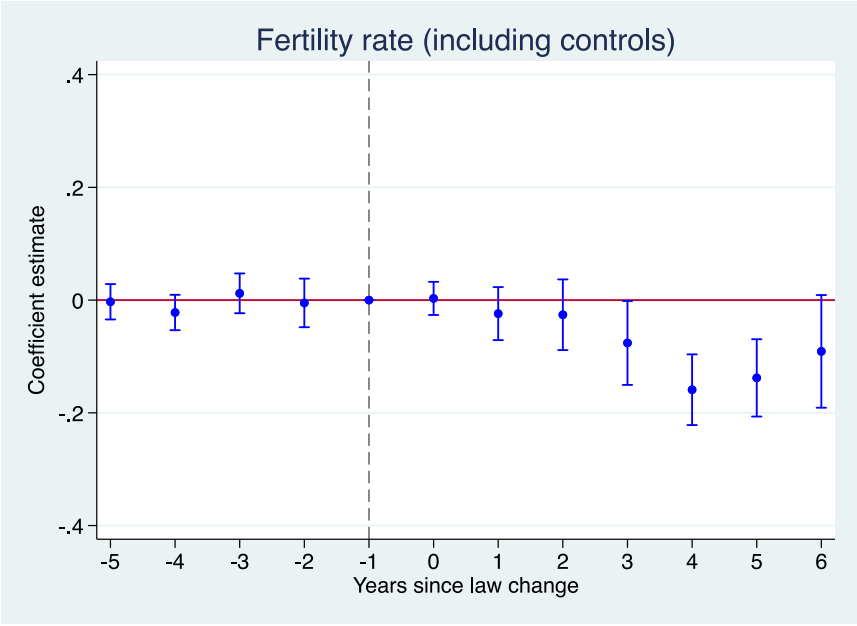
For completeness, Figure A.1 of Online Appendix A reports unweighted country-level estimates without additional controls. This estimand corresponds to a different target parameter — the average impact across treated countries rather than the average impact on an individual residing in a treated country — however, these estimates are similar in magnitude compared to the population-weighted estimates.

Regardless of specification, the results in event time $t=0$ consistently show a near-zero effect on fertility. Given that it takes approximately nine months from conception to childbirth, we would not expect to see any policy-related fertility changes already at $t=0$, and the null finding at this point can be viewed as a placebo-style diagnostic, supporting the interpretation that the observed fertility effects in later periods are not driven by spurious pre-trends or contemporaneous shocks. Moreover, the delayed emergence

[11] In a sample of married adults in the U.S. used in Francis and Mialon (2015), the median time from first date to marriage proposal was around three years. Data from the U.S. Census Bureau indicates that the time passed from marriage to the birth of the first child is a further three years (Cueto, 2015). However, in northern and western Europe in particular, many couples have their first child before marrying, shortening this time frame.



(a) The effect of consent-based laws on fertility (no additional controls).



(b) The effect of consent-based laws on fertility (including country-level controls).

Fig. 2. Event-study figures for the effect of consent-based legislation on fertility, applying the Callaway–Sant'Anna (2021) estimator. Weighted estimates. Top: No controls. Bottom: Controls for unemployment rate and house prices included. Event year –1 is normalized to 0. Country and time fixed effects included. The bars represent 95 percent confidence intervals, with standard errors clustered at the country level.

of the effects is consistent with the core mechanism emphasized in the paper, since adjustments in couple formation and abortion behavior are expected to occur gradually rather than immediately.

As in any staggered-adoption design, event-study coefficients at longer horizons are identified from a smaller set of countries, since later adopters cannot contribute to distant post-treatment periods (Baker et al., 2026). Table A.3 of Online Appendix A reports the number of treated countries contributing to each event-time coefficient. While cohort coverage declines slightly at longer horizons, treatment effects through event times +4 to +6 are still identified from six to seven countries rather than from one or two

countries. The reduction in contributing countries is reflected in larger standard errors at longer horizons, appropriately capturing the reduced information content of these estimates.

3. *Robustness and Alternative Specifications*

For comparison, Figure A.2 of Online Appendix A reports event-study estimates obtained using the Chaisemartin and D'Haultfœuille (2020) and Sun and Abraham (2021) estimator, as well as a standard event-study TWFE specification. However, TWFE event-study coefficients can be contaminated by treatment effects from other cohorts in settings with staggered adoption, which can generate contamination (Sun and Abraham, 2021). Consistent with this, the TWFE specification displays positive pre-period coefficients, whereas the estimators designed to address staggered adoption, in particular the de Chaisemartin–D'Haultfœuille estimator, yield similar results to Callaway–Sant'Anna estimates in the main text. For the de Chaisemartin–D'Haultfœuille estimator, the post-treatment ATT is equal to –0.073 with a standard error of 0.035 ($p = 0.037$), which closely mirrors the estimated Callaway–Sant'Anna ATT.

I also assess the robustness of the event-study estimates to violations of the parallel trends assumption using the sensitivity analysis framework of Rambachan and Roth (2023). Rather than imposing exact parallel trends, this approach allows for deviations from linear pre-treatment trends bounded by a sensitivity parameter M, and constructs robust confidence intervals for post-treatment effects under the worst-case bias consistent with these deviations. I focus on the estimated treatment effects in the fifth and sixth post-treatment periods, for which the Callaway–Sant'Anna estimates were significant at the 5% level. To calibrate the value of M, I examine the curvature of the estimated pre-treatment coefficients. The largest observed second difference in the pre-treatment period is approximately 0.02, indicating only mild departures from linear trends prior to treatment.[12] Consequently, assuming values of M substantially larger than this benchmark corresponds to allowing for violations of parallel trends that are an order of magnitude larger than those observed in the data.

Figure A.3 of Online Appendix A reports the Rambachan–Roth robust 95 percent confidence intervals for the Callaway–Sant'Anna treatment effects at event times +5 and +6 as a function of M, allowing deviations up to $M = 0.5$. For both post-treatment horizons, the robust confidence intervals remain below zero for all values of M. Thus, the estimated negative effects on fertility remain statistically significant even under substantial relaxations of the parallel trends assumption.

Table A.4 of Online Appendix A reports a covariate balance test for treated and untreated countries in 2010, which was one year before the first country, Luxembourg, introduced consent-based laws. Balance is evaluated using standardized differences, defined as the difference in mean covariate values between groups. As a rule of thumb, absolute standardized differences below 0.25 are typically interpreted as indicating good balance (Imbens and Rubin, 2015). The resulting differences are small in magnitude, suggesting that treated and comparison countries are well balanced along these observable dimensions prior to adoption. Finally, Figure A.4 of Online Appendix A plots the distribution of estimated propensity scores for ever-treated and never-treated countries based the two covariates measured in 2010. The distributions indicate substantial overlap when conditioning on unemployment and house prices.

5. Evidence on mechanisms

This section examines the mechanisms underlying the main results. I begin by outlining a simple conceptual framework that organizes the empirical analysis around compositional changes in partner markets. I then study outcomes directly related to fertility, including couple formation and abortion. Finally, I examine behavioral adjustments consistent with a cooling dating market, such as changes in sexual protection and contraceptive behavior, and relate these patterns back to the framework.

5.1. Conceptual framework: Marriage market cooling

Why did fertility decline following the enactment of consent-based laws? This section develops a conceptual framework to organize the empirical evidence. The key idea is that the introduction of consent-based laws can affect fertility through dating and partner markets by altering perceived risk and uncertainty. Fertility may decline because fewer stable partnerships form, or because unintended pregnancies are more likely to be avoided or terminated. Both channels operate through changes in participation and selection in two-sided matching markets.

To rationalize the empirical findings, I begin by developing a simple theoretical model, formally presented in Online Appendix E, with Monte Carlo simulations reported in Online Appendix F. The model assumes that individuals differ in their tolerance for risk and that switching to consent-based legislation increases perceived legal uncertainty, particularly among men. While the reform is intended to increase safety, especially for women, it can also raise the perceived cost of intimate interactions for some otherwise law-abiding individuals. According to the model, sufficiently risk-averse men respond to higher perceived uncertainty by reducing their investment in dating or abstaining from the market altogether. This precautionary exit among some individual reduces couple formation, leading to lower aggregate fertility. At the same time, the model allows for an entry margin: some women who previously perceived dating as unsafe may enter the market following the reform. Simulation results show, however, that the exit of risk-averse men dominates this entry effect, resulting in a net decline in matching and fertility.

This exit from the dating and partner markets is unlikely to be homogeneous across different types of individuals. Drawing on insights from models of adverse selection in markets with asymmetric information (Akerlof, 1970), increased uncertainty

[12] See Online Appendix D for these calculations, as well as additional details about the method.

Table 3
Partnership formation and abortion outcomes.

Outcome variable:	Share living with parents	Share never-married	Abortion rate, corrected	Abortion rate
Post-reform ATT	0.090**	0.111**	0.100***	0.071*
	(0.038)	(0.054)	(0.033)	(0.038)
Time FE	Yes	Yes	Yes	Yes
Observations	262	123	492	550
Mean dep. var.	0.000	0.000	0.000	0.000

Note: Post-reform average treatment effects on the treated (ATTs) estimated using the Callaway–Sant'Anna (2021) estimator. Outcomes capture partnership formation and abortion behavior discussed in Section 5.2. All variables are standardized to have mean zero and unit variance. Standard errors clustered by country are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

disproportionately pushes cautious individuals out of the dating market. This generates compositional changes in the market—since more risk-averse individuals are also less likely to engage in risky sexual behavior, their exit leaves behind a pool of participants that is, on average, more risk-tolerant. Thus, if the model is correct, in addition to a decline in fertility we would expect to observe a reduction in couple formation. We would also expect behavioral adjustments along outcomes related to fertility control, including changes in contraceptive choices and abortion rates, as individuals respond to compositional changes in the dating market, and associated increased uncertainty. Secondary outcomes, such as changes in condom use and the incidence of sexually transmitted diseases, can provide supporting evidence on how the composition of participants in the dating market evolves following the reform, but are not the primary channels through which fertility responds.

Overall, the framework highlights how legal reforms intended to increase safety can generate unintended equilibrium effects in two-sided matching markets by altering participation and selection rather than underlying preferences.

5.2. Couple formation and abortion

1. *Couple Formation*

First, I examine whether the decrease in fertility can be explained by a decrease in couple formation. Fig. 3a displays the corresponding event-study plot for the share of young adults (aged 25–34) who live with their parents, while Fig. 3b shows the event-study plot for the overall share of adults who have never been married. A positive effect on these variables would suggest a decrease in couple formation. However, these effects would likely materialize toward the end of the period due to the time lag between the beginning of the relationship and cohabitation or marriage.

The results suggest a positive effect on the share of young adults who live with their parents and the share of never-married adults, respectively. For both of the variables, the effect becomes statistically significant at the 5% level at the third year.[13] Taken together, these results collectively suggest a decline in couple formation and an increase in the share of singles following the introduction of the consent-based law.

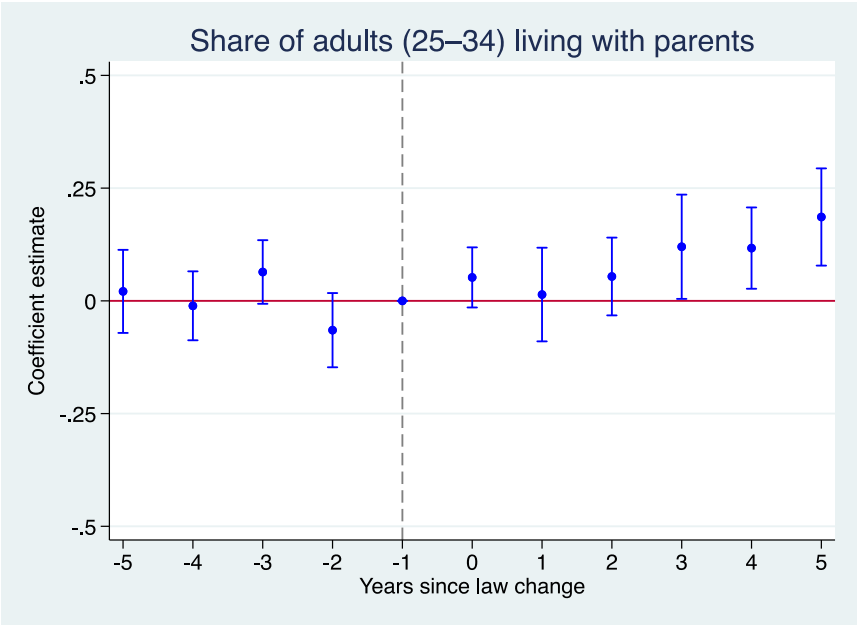
2. *Abortion Rates*

Second, I consider the effect on abortion rates. If the cooling channel described in the Introduction is correct — such that dating markets are relatively riskier following the law change — abortions may increase. In turn, this may explain part of the decrease in fertility. On the other hand, if consent-based laws lead to a diffusion of more positive attitudes toward sex, the number of unwanted pregnancies could decrease as a result of increased contraceptive use. This would then decrease abortion rates.

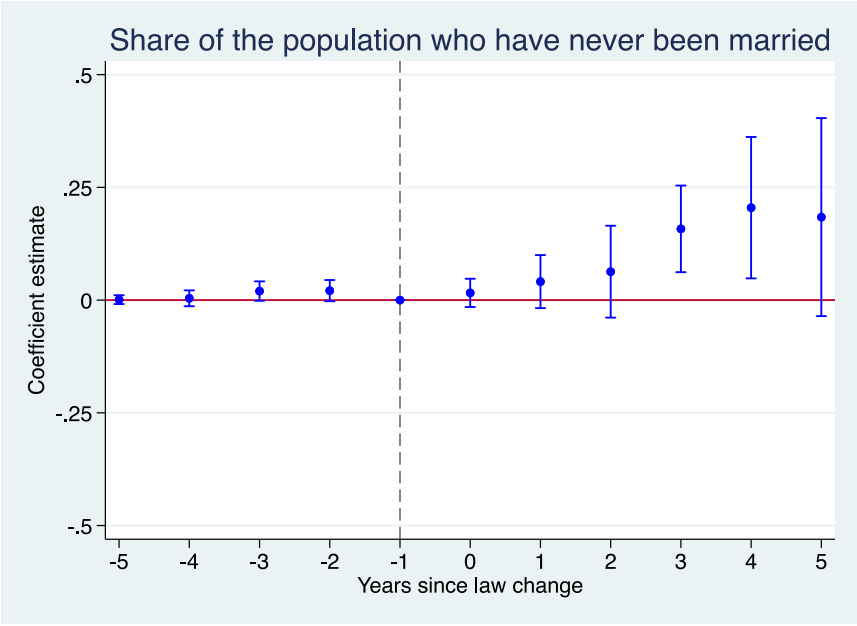
Since abortion rates are significantly influenced by abortion legislation, a potential concern with using cross-country abortion data is that changes in national abortion laws could confound the causal estimates of the effect of consent-based laws on the abortion rate. For instance, the public debate (such as in the wake of the #MeToo movement) could impact both the laws about sexual assault and the laws related to abortion. To address this concern, I collect data on all changes in abortion legislation for the 31 countries in the dataset and exclude, for each country, time periods following any liberalization or tightening of national abortion laws. This approach ensures that abortion legislation remains constant across all time periods within each country.

Fig. 4a presents the adjusted event-study plots for the impact of consent-based laws on the abortion rate. There is a positive effect on abortion rates, which becomes statistically significant after three years. The coefficient estimate is equal to about 0.20σ five years post-reform. Similarly, Fig. 4b shows the non-adjusted results. This only marginally changes the estimated coefficients, with the finding that the abortion rates increase following the enactment of consent-based laws remaining robust. Table 3 displays the estimated post-reform ATTs for the couple formation and abortion variables, with the coefficient corresponding to the corrected abortion rate estimated at 0.100σ and significant at the 1% level.

[13] There are slightly fewer observations for the share of the population who have never been married, which leads to wider confidence bands and greater year-to-year variation. This is because some countries, primarily in Eastern Europe, do not provide data on the number of never-married individuals. However, this variable also becomes significant from time period +3.



(a) The effect of consent-based laws on the share of adults aged 25–34 living with their parents.



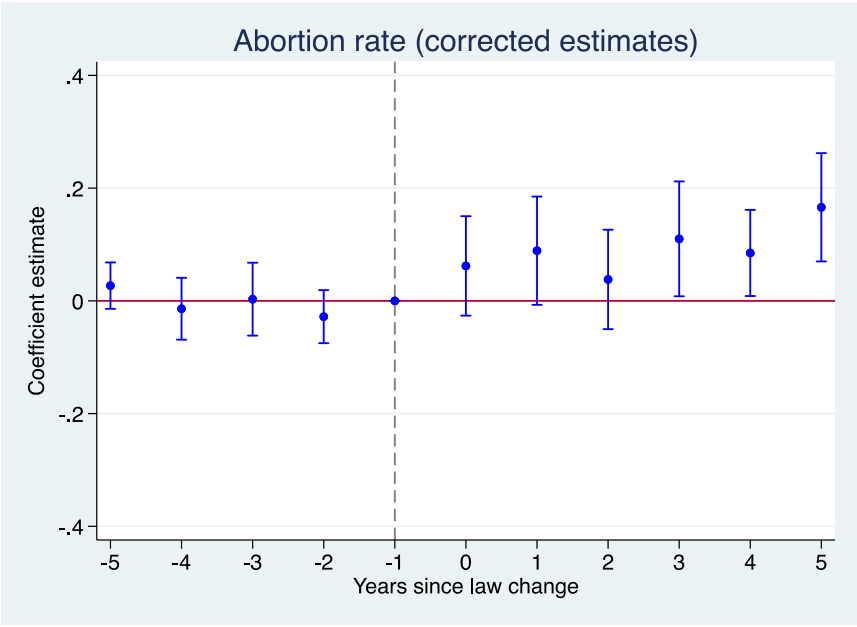
(b) The effect of consent-based laws on the share of the population (regardless of age) who have never been married.

Fig. 3. Event-study figures for the effect of consent-based legislation on couple formation, applying the Callaway–Sant'Anna (2021) estimator. Weighted estimates. Event year –1 is normalized to 0. Country and time fixed effects included. The bars represent 95 percent confidence intervals, with standard errors clustered at the country level.

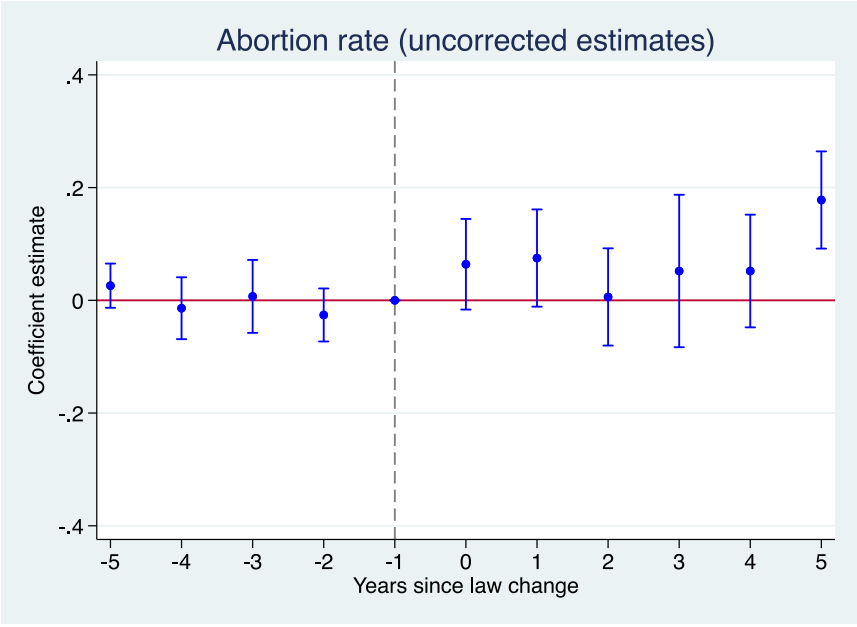
5.3. Behavioral adjustments consistent with cooling

1. *Male Condom Use*

Under the cooling mechanism, changes in fertility may operate through behavioral adjustments conditional on sexual activity, including changes in contraceptive use. If the composition of the dating pool has changed, aggregate male precautionary behavior



(a) Corrected estimates for the effect on abortion rates, excluding countries that have changed their abortion laws.



(b) Uncorrected estimates for the effect on abortion rates.

Fig. 4. Event-study figures for the effect of consent-based legislation on abortion rates, applying the Callaway–Sant'Anna (2021) estimator. Weighted estimates. Event year –1 is normalized to 0. Country and time fixed effects included. The bars represent 95 percent confidence intervals. Standard errors are clustered at the country level.

may decline. In turn, the risk of unintended pregnancy increases, with potential implications for abortion and fertility outcomes. To study whether this is the case, I turn to the WHO survey described in Section 3.2. Here, I examine the share of 15-year-old boys who report having used a condom at their latest sexual intercourse. The event-study plots are presented in Figure A.5 of Online Appendix A. There is a significant, negative impact on reported condom use for boys. The decrease is equal to around 0.20σ after four years. These survey results, while based on adolescents, are consistent with the broader compositional mechanism emphasized in the paper.

Table 4
Effects of consent-based laws on contraceptive use (Denmark, Norway, Sweden).

	Overall		Oral contraceptive pills (OCP)		Intrauterine + intravaginal		Abortion rate	
	Age 20–24	Age 25–44	Age 20–24	Age 25–44	Age 20–24	Age 25–44	Age 20–24	Age 25–44
Post-treatment ATT	0.198*	−0.415**	0.338**	−0.282*	−0.492**	−0.034**	0.032	0.213
	[0.065]	[0.041]	[0.031]	[0.058]	[0.010]	[0.040]	[0.826]	[0.172]
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58	58	58	58	58	58	71	71
Mean dep. var.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Countries	Denmark, Norway, Sweden							

Notes: This table reports post-treatment average treatment effects on the treated (ATTs) estimated using administrative data from Denmark, Norway, and Sweden. Estimates are computed without additional controls. Outcomes are age-specific measures of dispensed contraceptives by method and abortion rates, standardized so that the mean is equal to one and the standard deviation is equal to unity. "Intrauterine + intravaginal" pools intrauterine and intravaginal contraceptive methods. To adjust for the small number of clusters, inference is based on p-values from a t distribution with $G-1$ degrees of freedom in square brackets, where G is the number of clusters (countries); see also Cameron and Miller (2015).

2. *Female Contraceptive Use*

A priori, the effect of consent-based reforms on women's contraceptive behavior is ambiguous. If the reform primarily increases perceived safety and encourages greater participation in dating and sexual markets, one might expect higher use of female-controlled contraception as sexual exposure increases. Alternatively, if the reform operates mainly through compositional changes in the partner market — by inducing more risk-averse men to withdraw — female responses may be heterogeneous, as some women withdraw from the dating market when realizing that the remaining men in the dating pool are less safe. To examine these mechanisms among populations more directly relevant for fertility and abortion outcomes, I supplement the adolescent survey evidence with administrative prescription data on contraceptive use from Denmark, Norway, and Sweden, defined as the number of users per 1000 women. These registers provide high-quality, age-specific information on dispensed contraceptive methods and allow analysis of adult behavior, though the results should be interpreted cautiously given the limited sample size.

Table 4 reports estimates separately for women aged 20–24 and 25–44. Among women aged 20–24 — the group most likely to be engaged in temporary sexual relationships — overall contraceptive use increases modestly following the reform. This is driven primarily by higher use of oral contraceptive pills, while uptake of longer-acting methods declines. This pattern is consistent with increased participation in dating and sexual markets following the reform, which mechanically raises demand for female-controlled contraception. It is also consistent with greater caution in response to uncertainty about partner quality, as oral contraceptives provide a flexible form of fertility control that does not require commitment to long-acting methods. While the data do not allow us to distinguish between these channels, both interpretations align with a cooling dating market characterized by increased entry and reduced relationship stability.

In contrast, among women aged 25–44 — the group accounting for the majority of births and abortions — I find a decline in contraceptive use across all methods, including long-acting reversible contraceptives. This result is consistent with reduced entry into new sexual relationships or delayed partnering following the reform, rather than increased reliance on ex ante fertility control. Importantly, lower average contraceptive use in this group is compatible with increases in abortion, since ex ante contraception and ex post fertility control are not perfect substitutes and may be adopted by different individuals.

Consistent with this interpretation, abortion rates increase slightly among women aged 25–44, although the estimate is statistically insignificant. Taken together, the age gradient in contraceptive and abortion responses supports a cooling mechanism: younger women adjust primarily on the ex ante margin by increasing use of flexible contraceptive methods, while older women respond mainly through reduced dating intensity and, conditional on exposure, greater reliance on abortion as a last-resort fertility control.

Overall, these findings suggest that consent-based reforms do not generate large or uniform increases in effective contraceptive use. Instead, female behavioral responses are heterogeneous, reinforcing the interpretation that the observed declines in couple formation and fertility arise primarily through changes in partner-market composition and relationship stability rather than widespread increases in the use of contraceptives.

3. *Sexually Transmitted Diseases*

A concern about the results on young men's condom use is that these results come from a survey of sexually active 15-year olds, which may not be representative of the population that is driving the main findings on fertility and abortion in the paper. If overall condom use is lower following the law change, for instance due to compositional changes in the partner market, we should observe an increase in the prevalence of STDs. To answer this, I turn to data from the European Centre for Disease Prevention and Control (ECDC) that covers the population-wide incidence of the two most prevalent STDs in Europe, namely chlamydia and gonorrhea. The top panel of Figure A.6 of Online Appendix A shows the event-study estimates using the Callaway–Sant'Anna estimator for the first principal component of these STDs, while the bottom panel shows a similar plot when using gonorrhea only.[14] These

[14] Gonorrhea is more prevalent than chlamydia in the U.S., so many U.S studies use only the former as a proxy for risky sex. However, in Europe, chlamydia is more prevalent.

results suggest that following the implementation of the consent-based law, the prevalence of STDs increased significantly. Table A.5 of Online Appendix A presents post-treatment ATTs for boys' condom use, overall gonorrhea and chlamydia prevalence, and gonorrhea prevalence across all age groups. For the latter outcomes, I find increases of roughly 0.15–0.20σ following the enactment of consent-based laws. However, while there is no upwards sloping pre-trend, it should be noted the infections rates of both these STDs were slightly higher in treatment countries already before the law changes.

5.4. Mechanism validation: Evidence from individual verdicts

For the compositional change mechanism to be valid, uncertainty about what constitutes sexual assault from a legal perspective must have increased. However, the outcome variables studied — such as abortion rates and the share of singles — do not provide clear evidence of this. To assess whether legal uncertainty has, in fact, increased, I turn to the data on individual verdicts from Swedish appellate courts before and after the 2018 law change, when the country switched from a coercion-based system to a consent-based one. If adopting consent-based legislation increases uncertainty about what constitutes sexual assault, we would expect to see a rise in the share of non-unanimous verdicts following the law change. While these results should be interpreted with some caution, as Sweden is only one of several countries that have implemented consent-based legislation, the Swedish legal system closely mirrors those of most European countries. Moreover, the Swedish definition of consent — and the circumstances indicating its absence — is broadly consistent with the definitions adopted by other European countries that have implemented similar laws, making it appropriate for a case study.

1. *Background: The Definition of Rape and the 2018 Reform*

Until July 1, 2018, the definition of rape according to the Swedish criminal code was as follows:

> "Whoever, by assaulting or otherwise using force or threats of criminal action, forces an individual to perform sexual intercourse or another sexual act equivalent to rape, shall be guilty of rape, and sentenced to a prison term of two to six years".

From July 1, 2018, this section reads:

> "Whoever performs a vaginal, anal, or oral intercourse, or another sexual act equivalent to rape, with an individual who does not participate voluntarily, shall be guilty of rape, and sentenced to a prison term of two to six years. When deciding whether the participation was voluntary or not, particular weight is to be placed on whether consent was given in the form of verbal discourse or action, or in some other form".

An amendment entered into force on August 1, 2022, increasing the minimum penalty for rape from two to three years. Descriptively, Figure A.7 of Online Appendix A shows that there was a large increase in convictions for rape, but not for felonies and misdemeanors overall, after the 2018 law change. Between 2017, the last year with the coercion-based law, and 2019, when a majority of defendants were sentenced under the consent-based law, the number of convictions of rape increased from 190 to 321, representing an increase of 69%, while the total number of felonies and misdemeanors increased from 97,058 to 104,355, which corresponds to an increase of 8%.

2. *Some Aspects of the Swedish Legal System*

Before proceeding, this section will briefly discuss some aspects about the Swedish legal system. Most countries in the world follow either the common law legal system, which is based on medieval English law, or the civil law legal system, derived from Roman law. Sweden's system is based on the latter, as are most legal systems in Europe. There are three tiers of courts: the district courts, the appellate courts, and the Supreme Court. However, the Supreme Court grants *certiorari* only in around 2% of cases, so in most cases, the decision of the appellate court is final.

The decision about conviction and sentencing is done by a panel of four individuals in the district courts: one judge and three jurors. In the appellate courts, the decision is made by five people: three judges and two jurors. Hence, judges and jurors decide together on both the verdict and the sentence. Different from some other legal systems, the Swedish system does not require unanimous decisions.[15] As an example of this, a 3–2 decision in favor of conviction in the appellate court would be sufficient. In addition, a "not guilty" verdict may be appealed by the prosecution, so a conviction in the appellate court is possible even if the defendant was acquitted by the district court.[16] On the other hand, the role of the judges during the trials is closer to that of common law systems, with questions asked almost exclusively by the prosecution and defense. This contrasts other civil law systems, such as the French system, in which judges have a more active role during the trial. Different from the United States, judges cannot prevent evidence from being shown at the trial. Instead, both the prosecution and defense are allowed to bring forward whatever evidence they find appropriate.

3. *Empirical Results*

[15] In the U.S., conviction in criminal trials requires unanimous jurors. However, Louisiana and Oregon had previously allowed conviction even with the jury split 10–2. This was declared unconstitutional by the Supreme Court in 2020 (see *Ramos* v. *Louisiana*, 590 U.S. 83). This decision overturned a previous decision, *Apodaca* v. *Oregon*, 406 U. S. 464 (1972), which had allowed non-unanimous verdicts.

[16] However, the higher court may not issue a more severe penalty if only the defendant has appealed (in legal terms, such a situation would be known as *reformatio in peius*).

Table 5
Unanimity of verdicts.

Outcome variable: Probability of unanimous verdict		Outcome variable: Probability of at least one court member dissenting					
		All judges		Professional judges only		Jurors only	
		All verdicts	Only convicted def.	All verdicts	Only convicted def.	All verdicts	Only convicted def.
Post	0.381***	2.625***	2.846**	1.781	2.549	3.806***	3.240**
	(0.122)	(0.842)	(1.251)	(0.761)	(1.506)	(1.546)	(1.823)
Court FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Method	Logit	Logit	Logit	Logit	Logit	Logit	Logit
Observations	870	870	676	870	676	870	676
Pseudo R^2	0.033	0.033	0.053	0.025	0.053	0.041	0.063
Mean dep. var.	0.828	0.171	0.143	0.086	0.056	0.159	0.138

Note: Outcome variable: Probability of unanimous verdict and the probability of at least one court member dissenting, respectively. The table gives the estimated odds ratios. Standard errors clustered by verdict in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

If it is difficult to assess what constitutes tacit consent, we would expect a greater share of dissenting opinions among judges and jurors following the introduction of the consent-based law. Alternatively stated, we would expect the share of unanimous verdicts to decline after the law change. Thus, we may estimate the logit model

$$\ln\left(\frac{\Pr(\text{Unanimous}_{ic}=1)}{1-\Pr(\text{Unanimous}_{ic}=1)}\right)=\alpha_i+\beta\text{Post}_i+\xi_c+\epsilon_{ic} \tag{3}$$

for verdict i from appellate court c, where $Unanimous_{ic}$ is an indicator variable equal to one if the court was unanimous, and zero else, $Post_i$ is equal to unity if the (alleged) crime took place after July 1, 2018, ξ_c are court fixed effects, and ϵ_{ic} is an error term. To check whether professional judges valued evidence differently than jurors, I estimate

$$\ln\left(\frac{\Pr(\text{At least one court member dissenting}_{ic}=1)}{1-\Pr(\text{At least one court member dissenting}_{ic}=1)}\right)=\alpha_i+\beta\text{Post}_i+\xi_c+\epsilon_{ic} \tag{4}$$

The dependent variable in Eq. (4) takes the value one if there was at least one judge or juror dissenting, and zero else. The results are presented as odds ratios in Table 5, with the court fixed effects included. The first two columns in Table 5 are equivalent; the estimate for the odds ratio with the share of unanimous verdicts as the dependent variable is approximately equal to 0.38 (first column), which is equivalent to an odds ratio of around 2.7 (second column) when the dependent variable is expressed as the probability that at least one person in the five-individual panel is dissenting. This finding suggests that there was a large decrease, around 65%, in the share of unanimous verdicts after the law change. Split by professional judges and jurors, the jurors are considerably more likely to dissent. The probability that at least one juror dissented was over 200% higher after the law change, compared to before the law change. The difference was less pronounced for the three-person panel of professional justices, who were around twice as likely to the dissent after the law change. For professional judges, the probability of dissent was lower when the defendant was convicted, while it was reversed for the members of the public serving as jurors.

To evaluate the results year-by-year, and to examine whether there are any pre-trends, an alternative approach is to estimate an event-study specification. Since the law was enacted July 1, 2018, I classify charges corresponding to the period from January 1 to June 30, 2018, as belonging to the year 2017, and events from July 1 to December 31, 2018 as belonging to 2019. The pre-reform year, 2017, which thus, includes events from January 1, 2017 to June 30, 2018, is excluded for comparison. I then estimate

$$\ln\left(\frac{\Pr(\text{Unanimous}_{ic}=1)}{1-\Pr(\text{Unanimous}_{ic}=1)}\right)=\alpha_i+\sum_{t\neq 2017}\beta_t I_t+\xi_c+\epsilon_{ict} \tag{5}$$

where for verdict i from appellate court c in year t, and I_t is an indicator for year t, excluding the baseline year 2017. Table A.6 of Online Appendix A presents the estimates as odds ratios. In the first year after the law change, 2019, the share of unanimous verdicts decreased by more than 60%, with the coefficient estimates largely unchanged for the remaining years. There are no indications of pre-trends, with the pre-reform coefficients for 2015 and 2016 all close to unity in magnitude.

One final note concerns the direction of dissenting opinions among jurors. Table A.7 of Online Appendix A presents a crosstab analysis of the direction of dissents — specifically, whether the juror favored acquittal or conviction — before and after the legal change. Before the law change, an equal number of dissenting jurors favored convicting the defendant, compared to acquitting. However, following the introduction of consent-based legislation, a large majority of dissents favored acquittal. While this difference is not statistically significant (Fisher's exact test p–value = 0.179), considered together with the decline in unanimous verdicts, these findings suggest that the shift to consent-based legislation increased uncertainty regarding which actions constitute sexual assault and how tacit consent should be interpreted.

6. Concluding remarks

This paper has examined the effects of expanding the legal definition of sexual assault on fertility outcomes. Using the staggered adoption of consent-based legislation across European countries, the analysis shows that these reforms are followed by declines

in fertility and couple formation. Consistent with changes in dating and partner markets, I also document behavioral adjustments along margins related to sexual risk, including reduced condom use among young men and corresponding increases in sexually transmitted infections. To interpret these findings, I develop a simple theoretical framework in which consent-based legislation alters the composition of participants in the marriage market, in turn, leading to fewer stable matches and lower fertility, while changes in sexual behavior arise as a by-product of these compositional shifts.

The results in this paper should be interpreted with appropriate caution. Consent-based laws are a relatively new phenomenon, and their long-term effects on sexual behavior may take time to materialize. Despite these caveats, the results in this paper have implications for policymakers. While there is broad agreement that consent-based legislation addresses a significant gap in the legal system — namely, that many cases of sexual assault are not covered by traditional coercion-based laws — such laws are not without challenges. As discussed in this paper and in the legal literature, part of the issue with consent-based legislation as currently implemented in many jurisdictions is that tacit consent is considered acceptable. This creates uncertainty about how non-verbal consent should be interpreted. Additionally, while this is not the first case of legal reforms aimed at increasing sexual safety bringing about unintended negative consequences (see e.g., Beauchamp and Pakaluk, 2019; Willage, 2020; Cunningham and Shah, 2021), more research is needed to assess how different legal interpretations of consent affect courtroom decisions and broader societal attitudes toward sexual violence.

There are several potential policy responses to the issue with tacit consent. One solution is to require affirmative rather than tacit consent, which could help reduce uncertainties and, in turn, increase sexual safety and fertility. A second option is to further increase public awareness, emphasizing that consent should be the norm, rather than codifying affirmative consent directly. The court data discussed in the paper suggest that professional judges are less likely to diverge internally in their views about which sexual acts are acceptable and which violate bodily integrity, suggesting that discrepancies in consent interpretations primarily stem from societal attitudes rather than inconsistencies within the legal system. Therefore, any public awareness programs should be aimed at the general public rather than at law enforcement officials.

CRediT authorship contribution statement

Adrian Mehic: Writing – review & editing, Writing – original draft, Visualization, Validation, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jhealeco.2026.103154.

Data availability

Data will be made available on request.

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