Only yes means yes! The effect of the introduction of the consent law in Sweden

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Abstract:

Many countries are moving towards a consent-based definition of rape. Despite the fact that the pros and the cons of this type of reforms have been widely discussed in legal and psychological fields, there are no quantitative studies evaluating the effect of these reforms on sexual harassment and gender-based violence. Using the European Working Conditions Survey, we assess the impact of the 2018 consent law reform in Sweden on unwanted sexual attention occurrences at work, using a difference-in-differences approach. Taking Finnish and Norwegian workers as a control group, we find that, by 2021, the reform had reduced unwanted behaviours by 54% with respect to 2015. The results are robust to various tests and sensitivity analyses, but the precision of the estimates can be improved, by using a larger sample.

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Introduction

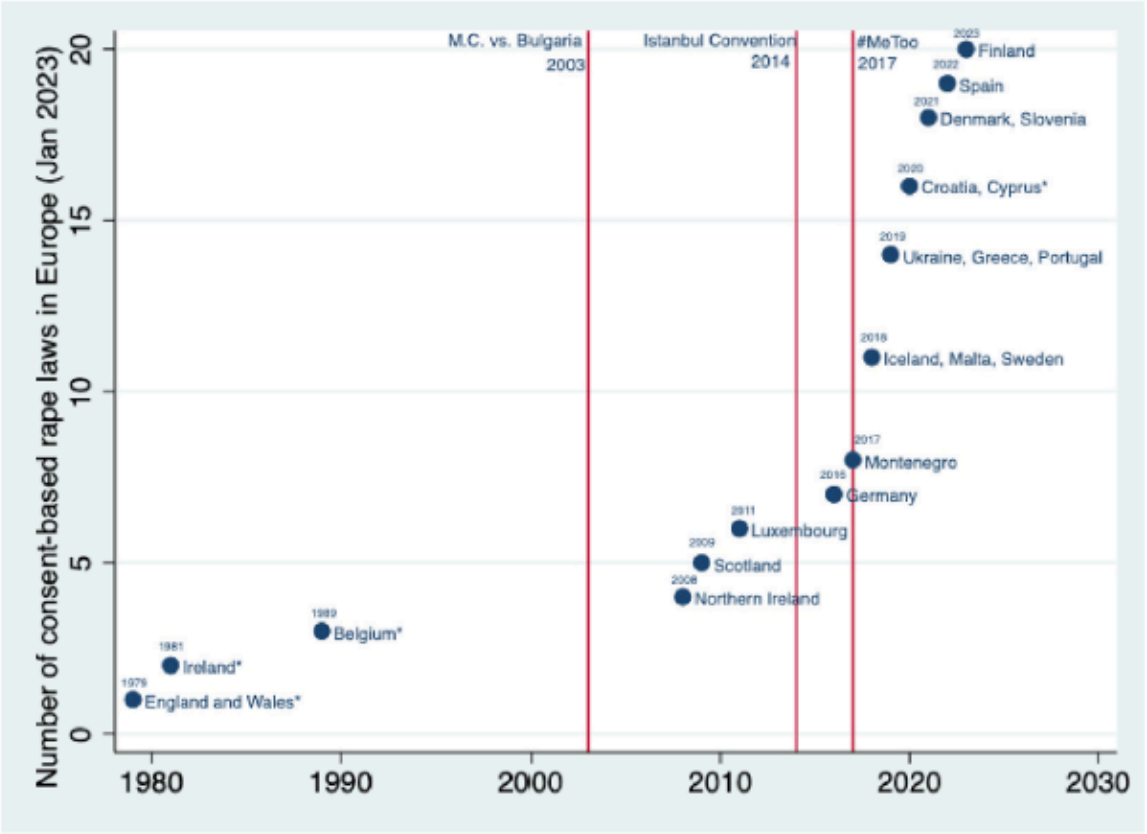
Sexual violence is a widespread phenomenon in the world. About 1 in 3 women worldwide have experienced either physical and/or sexual intimate partner violence or non-partner sexual violence in their lifetime (World Health Organization, 2021).

Criminal legislation on rape is widely debated today worldwide. The discussion concerns mainly the two main models regarding the treatment of sexual violence and the definition of rape in criminal law. Firstly, the coercion-based model, and most common one, requires that the sexual act was done by coercion, violence, physical force or threat of the use of violence or physical force in order for the act to amount to rape. The consent-based model, instead, requires that, for the act to qualify as rape, there must be a sexual act that the other one did not consent to. Historically, Nordic countries alongside the other continental European countries have used the first model, with a coercion-based definition, whereas Anglo-American countries have introduced the concept of consent (or lack thereof) earlier on (Bladini and Svederg Andersson, 2020).

Before the explosion of the MeToo movement, in Oct. 2017, only 6 European countries had switched to the consent-based model, namely Ireland, Belgium, Cyprus, UK, Luxembourg and Germany, despite the fact that most European countries ratified the Istanbul Convention promoted by the Council of Europe, which would require defining rape as sex without explicit consent (Amnesty International, 2018) – see Figure 1. Since then, at least 13 other countries have switched systems. The MeToo movement was particularly strong in Nordic countries (Nordic Council of

Ministers, 2018). As a result, Sweden and Iceland introduced a new consent law in 2018, Denmark did it in 2021, Finland in 2023 and Norway is considering it[1].

Figure 1: Timeline of the development of consent-based rape laws in Europe



Source: Uhnoo, Erixon & Bladini (2024), The wave of consent-based rape laws in Europe. International Journal of Law, Crime and Justice, 7(2), 1-5

Note: The first significant case in the European Court of Human Rights was MC vs. Bulgaria in 2003. The Court ruling asserted the obligation to effectively criminalise and prosecute all non-consensual sexual acts, leaving countries free to define the offence in their own national law.

The new Swedish law criminalised sexual relations where clear verbal or physical consent has not been expressed (Holmström et al, 2020). Prior to the sexual

[1] See Amnesty International (2018); Peltier (2020); Finnish Ministry of Justice (2023); and Norwell (2023).

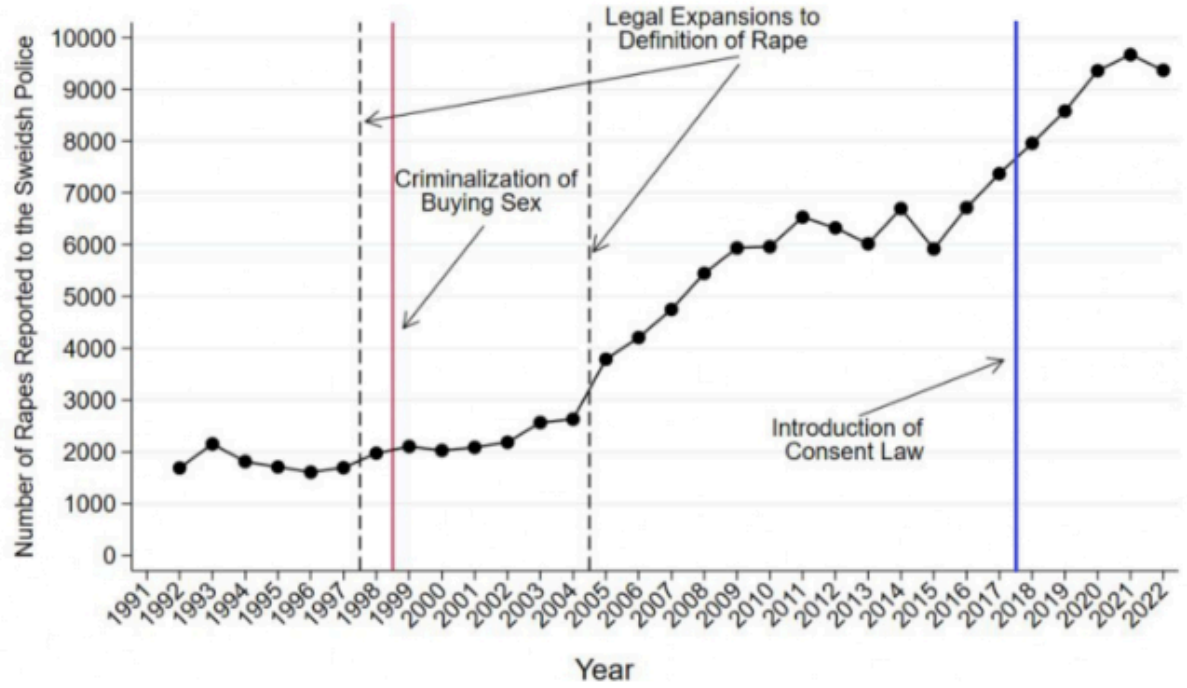
consent law, a sexual act would be considered only if there was evidence that the victim resisted it or was coerced into it. Nowadays, in the countries where sexual consent laws have been implemented, as sexual act is considered rape unless there is evidence that both parties agreed to it. A person is never seen as participating voluntarily if: (1) participation is a result of assault, violence or threat, (2) the defendant improperly exploits that a person is in a 'particularly vulnerable situation' or (3) induces a person to participate by seriously abusing her/his position of dependence on the perpetrator. The law also introduced the concept of *negligent rape*, where a sexual act is considered negligent rape when there has not been a (clear) consent, but in which the perpetrator had not intended to commit rape or assault. For example, if an individual is sleeping next to someone else and starts initiating sex while that person is in fact sleeping, even if the individual believes the person is awake, the act is considered negligent rape as the first individual is not making sure to see and ask if the person is awake and consensual.

However, this change of legislation raises several questions due to its limits. The main one is regarding the establishment of whether a consent-based model for the definition of rape reduces unwanted sexual behaviours with respect to a coercion-based definition. The aim of this thesis is to evaluate the effectiveness of the consent law reform, by determining whether the change in the definition of rape in Sweden increased or decreased unwanted sexual behaviours.

In order to pursue this goal, we need to identify an appropriate measure of the dynamics of these behaviours. In Sweden, there is a clear upper trend in reported rapes after the reform (this appears to have begun before the law entered into force – see Figure 2). However, we need to underline that this is not a good outcome

measure: of course, if the definition of rape is enlarged, in the beginning at least, it is likely that more complaints will be reported, and there is evidence in the data that each time the definition of rape has been enlarged (such as in 1997 and 2004), the number of reported cases has increased. But this is an outcome that is neither good nor bad. For example, before the change in legislation, if a person had many drinks with another and then they had sexual intercourse once the latter has partially their ability to reason, the first person would not have not been considered committing rape because due to a lack of coercion. After the reform, this would have likely be classified as rape. Even if the frequency of this type of events does not change around the time of the reform, statistics would pick up an increase in the number of rapes because only after the reform this type of event would be classified as rape.

Figure 2: Number of reported rapes in Sweden, 1992-2002



Source: Adema and Rikne, Re-analysis of Ciacci, R. (2024), Banning the purchase of sex increases cases of rape: evidence from Sweden. Journal of Population Economics, 37(2), 1-30

Nevertheless, there are other ways to assess the effectiveness of the change in the definition of rape. Sexist and sexual violence are not limited to the intimacy of the home, but they are also widespread in the workplace (Basu, 2003, Folke & Rickney, 2022). People spend a lot of time at their workplace, which thus becomes one of the main places to develop social interactions and it is also a place where asymmetric power relationships are important (e.g. between an employee and their boss). We can thus look at self-reported unwanted sexual attention experiences in the workplace, using data from various waves of the European Working Conditions Survey (EWCS), which is the only data reporting on unwanted sexual attention occurrences before and after the reform in Sweden, beyond data on criminal cases (cf. Figure 2).

We will consider a difference-in-differences setting in which we compare changes in unwanted sexual attention occurrences at work in Sweden and in our control group between 2 dates: one before and one after the reform.

Our control group will be built of 2 other Nordic countries (Norway and Finland). We will exploit the fact that in these 2 countries, consent laws were introduced much later than in Sweden (in 2023 in Finland) or not at all (Norway). These countries both constitute a potential good comparison group with regards to Sweden since they all have similar cultures, in particular similar legal cultures (Ervo et al., 2021)[2]. They all had powerful MeToo movements (Nordic Council of Ministers, 2018), which can lead us to assume that the public perception of what sexual violence has changed similarly in all 3 countries. Most importantly, they are all relatively rich and developed countries with a similar role of women in society. All 3

[2] The Nordic model is in between civil law (they have codified laws into codes) and common law (they use traditions a lot - with huge importance given to precedents and tradition) and their law is not super prescriptive.

countries have a GDP per capita above the European Union's average in 2016 (see Table 1). Their female activity rate and female employment rate in 2016 are well above EU average. Their female tertiary education rates in 2016 are about only 2 percentage points apart and more than 17 percentage points higher than the average of European countries. Their human development indexes range between 0.930 and 0.952, when the EU's average is 0.782 in 2015. The 2022 gender development indexes of Sweden, Norway and Finland are very close together (with a difference ranging between 0.003 and 0.006 points). When looking at the gender equality indexes in 2022, the similarities between the 3 countries become even stronger, since their indexes do not go beyond 0.032, whereas the EU average is 0.224. Finally, they all have pretty similar rates of reported unwanted sexual attention (considering both all workers and only female workers) in 2015[3].

[3] These rates are higher than the EU average, which does not necessarily indicate that there are more occurrences of unwanted sexual attention in Sweden, Norway and Finland. It might just be that in these countries, there is a different perception of what can be considered an unwanted sexual attention occurrence than in the rest of the EU. In fact, as reported in Amnesty International (2018) using Eurobarometer data of 2016, these countries share a similar level of intolerance of inappropriate sexual behaviours.

Table 1: Aggregate socio-economic comparison

	Sweden	Norway	Finland	EU
GDP per capita (PPP 2016)	50,430 $	59,280 $	44,934 $	40,551 $
Female activity rate (%, 2016)	83.4	80.9	77.2	70.7
Female employment rate (%, 2016)	78.4	78.1	70.8	64.1
Female tertiary education rate (%, 2016)	48.0	48.0	50.2	30.9
Human development index** (2015)	0.937	0.952	0.930	0.782
Gender development index ** (2022)	0.983	0.986	0.989	0.963
Gender inequality index** (2022)	0.023	0.012	0.032	0.224
Unwanted sexual attention rate (% of all workers, 2015)	3.7	3.0	2.9	1.8
Unwanted sexual attention rate (% of female workers, 2015)	5.2	4.8	5.3	2.7

*Source: GDP per capita: World Bank; Female activity, employment and tertiary education rate: Eurostat; Human and gender development, gender inequality: UNDP Human Development Reports; Unwanted sexual attention rate: EIGE (based on data from EWCS, 2015)

**Index going from 0 to 1 (from lowest to highest)

Literature review

A large literature has studied levels and determinants of gender-based violence. Several empirical papers have studied sexual related violence and sexual harassment, because, though sexual violence occurrences seem to have decreased over time, they are still a widespread phenomenon affecting both men and women, though the latter have a higher rate of occurrences (Borumandnia et al., 2020). Amongst women in the European Union, and especially young women, sexual violence remains a very common experience, with one out of two women having experienced at least one form of sexual harassment during their life (Latcheva, 2017). Hardt, Stöckl, Wamoyi and Ranganathan (2022) show that conceptions of sexual violence occurrences vary strongly according to contextual and sociocultural factors.

This literature has also studied what the determinants of sexual related violence are. For instance, literature shows that sociocultural characteristics heavily affect interactions between individuals and can motivate sexual violence. Indeed, the normalization and systematization of gender-based discrimination have been shown to lead to increases in sexual violence (Davies & True, 2015). More generally, there is evidence that gender norms and patriarchal views of the society lead to more sexual-related violence (against women) and lower rates of effective repression (resulting in lower disincentives against sex-related crimes, Rosenberg, 2022). Furthermore, media have a strong impact on the perpetuation of sexual violence, by pushing forward discourses of gender inequality, reinforcing male dominance, and sexualizing or romanticizing violence (Russo & Pirlott, 2006). To mitigate this, social media campaigns have emerged to raise awareness on sexual violence, showing

promising results (Lee et al., 2023). Furthermore, economists have conducted policy evaluation studies of changes in laws (or law enforcement) concerning prostitution and their impact on the incidence of sexual violence. It has been shown that decriminalisation of prostitution decreases sexual violence (Bisschop, Kastoryano & van der Klaauw, 2017; Cunningham & Shah, 2017), without negatively impacting the level of any other crime (Ciacci & Sviatschi, 2021). On the contrary, the criminalization of prostitution leads to an increase in sexual violence (Ciacci, 2024), particularly domestic violence (Perrotta Berlin et al., 2019). Other factors have been studied in relation to sexual harassment, such as the amount of time spent at home and its impact on sexual violence. During the Covid-19 pandemic, an increase in sexual violence was observed, and specifically domestic sexual violence, which seems to indicate that the more time one spends at home, the higher is the risk of experiencing intimate partner violence (Perrotta Berlin & Gerell, 2022). The use of alcohol and drugs is also one of the key determinants of sexual violence, playing a crucial part in increasing perpetuations of gender violence (Perrotta Berlin & Gerell, 2022; Shiva, Shukla & Chandra, 2021).

Sexual harassment is extremely difficult to measure since there is not one unique definition, which makes it hard to monitor it (Hensch, 2024). However, sexual harassment is a widespread phenomenon, which is why it has been widely studied, specifically sexual harassment in the workplace. Indeed, one in every two women in the world have experienced sexual harassment in the workplace in their life (Fitzgerald, 1993; Cortina & Areguin, 2021[4]). Higher rates of harassment have been observed in data in male-dominated workplaces (Folke & Rickney, 2022), in particular

[4] The most common manifestation of sexual harassment is however gender harassment and contempt, which aims at putting women workers down and pushing them out, and not necessarily pulling them into sexual activity (Cortina and Areguin, 2021).

in the case of female supervisors (McLaughlin et al., 2012). This leads women to self-select into female-dominated, low-risk establishments and sectors (Batut et al., 2021; Folke & Rickney, 2022), which tends to pay less, thereby resulting in a lower pay and contributing to strengthened gender inequality. This reality has led some scholars to argue that the right to a workplace without sexual harassment should be considered the same as any other labour standard such as the right to a healthy working environment (Basu, 2003).

Nonetheless, there is only a surprisingly slim literature with quantitative evaluations of consent-based reforms, and they don't evaluate the main effect, since they concentrate on voting patterns. Consent law reforms and, more generally, reforms consisting in the liberalization of gender values seem to trigger some kind of backlash on the voting field, sparking more conservative voting behaviours, fueling far-right parties, as seen in Sweden with the 2018 elections (Off, 2023). In 2023, in Spain, this dynamic was seen especially amongst the male population, which moved further to the right on gender related issues (Vall-Prat & Rodon, 2024).

Despite these few quantitative evaluations, there is, by contrast, a large literature that discusses possible effects of consent-law reforms with an ex-ante or qualitative perspective, particularly in law. The legislative change in the definition of rape translated an important cultural shift in the understanding of rape and in the responsibility associated with it (Bladini & Svederg Andersson, 2020), but it also raised some issues in several academic fields.

In fact, this legislation is based on a dichotomous system where one either consents or does not consent to the sexual act, leaving aside some cases that do not fall clearly in one category or the other. For instance, there may be cases where

consent changes during the sexual encounter. Consent can be withdrawn at any time during a sexual encounter. If one party does not recognize or respect the withdrawal, it can result in accusations of rape, leading to complex legal battles about the timing and clarity of the withdrawal of consent (Pineau, 1989). Furthermore, if both individuals are in a state of intoxication, they might have their memory impaired. This creates potential for false positives when intoxicated individuals later interpret their impaired decisions as non-consensual (Horvath and Brown, 2007). Moreover, some concerns were raised regarding the change in legislation with respect to the definition of rape, since it amounted in a switch of the burden of the proof from the plaintiff to the accused side. The accused now has to demonstrate that consent was obtained, which can lead to false positives if the accused cannot provide clear evidence, despite the act being consensual (Hurd, 1996). In the attempt to reduce false negatives, the consent-based system is therefore criticized for increasing false positives (Wegerstad, 2021; Vestergaard, 2020). Furthermore, the unclear boundaries around voluntariness, between voluntary and non-voluntary participation in sexual intercourse, also raises questions. For example, if someone agrees to having sexual intercourse because they feel forced to, they have not consented to it (Vandervort, 2013, Wegerstad, 2021). This is something difficult to prove, but it could also lead to people not having sexual intercourse altogether because they are not perfectly confident that the other person's 'yes' is a genuine one. During the debate concerning the possible introduction of the Swedish law, these concerns were quickly brushed off by the Swedish government. The government's argument was that it was better that one does not engage with another person, rather than doing it without being completely sure that the other person fully agrees to it (Bladini & Svederg

Andersson, 2020). Moreover, personal cultural biases seem to limit condemnation for sexual assault, because often legal analysis cuts short to the profit of popular narratives, leadings all actors involved (judges, complainants, etc.) to express wrong conclusions about consent (Vandervort, 2013). Pineau (1989) had already highlighted this problem, making sexual assault victims vulnerable, and had explored how potential changes in legislation on rape and, specifically on the issue of consent, could solve it.

In sociology, Holmström, Plantin and Elmerstig (2020) show that, though most people agree on what a ‘no’ means, they may have very different reactions and expectations to specific situations, highlighting that sexual consent and sexual negotiations are all subject to interpretations, which can vary a lot. Other scholars have focused on the implications that power imbalance can have on consent. Indeed, if an individual is offered by their boss to have sexual intercourse, they might not feel in a position to say no and end up saying yes, without actually wanting to (Jones, Milnes & Turner-Moore, 2022). Society also has stereotypes of what a relation between victim and aggressor looks like: the victim being weak and not fighting back and the aggressor being violent. Deviating from them may complicate the recognition of consent or lack thereof (Dowds, 2022).

Data description

As previously mentioned, we use the European Working Condition Survey (EWCS), which is a survey of workers in the workplace conducted by Eurofound every 5 years[5].

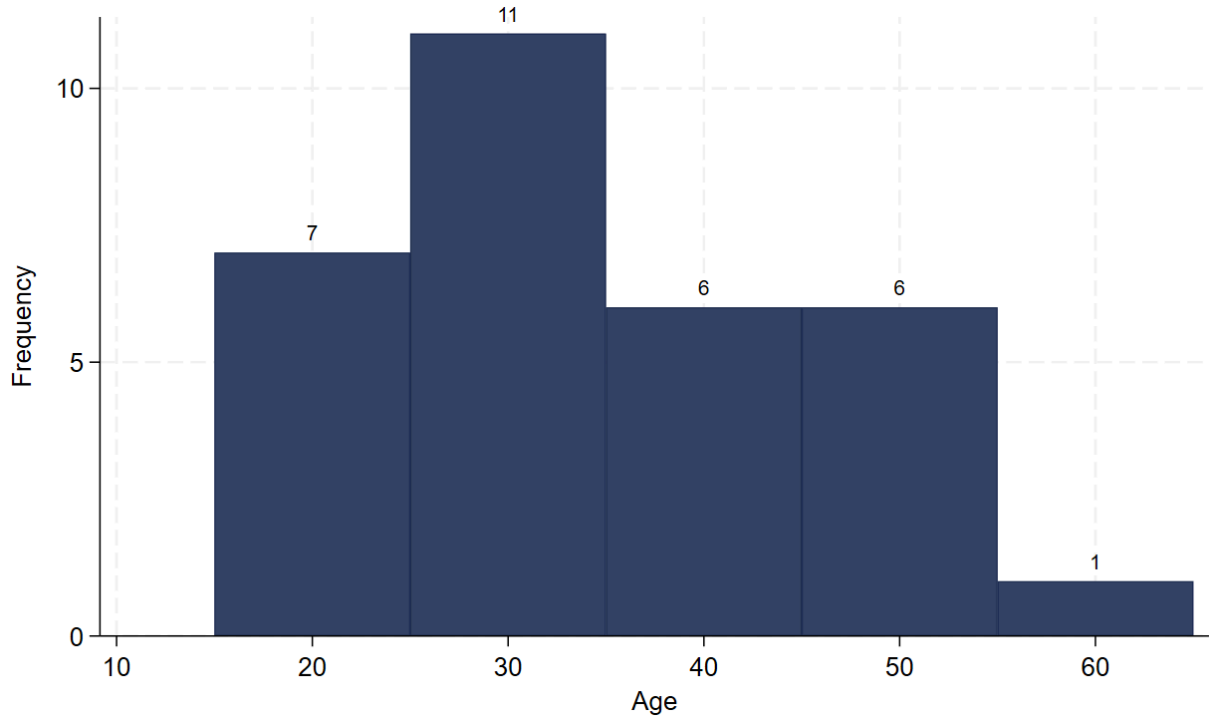
Up until 2015, there are about 800 observations per country and per wave of the survey, and about 2,000 observations in 2021. We keep men in our dataset since sexual violence is something that affects all genders (Borumandnia et al, 2020), even though women are the main victims.

In order to establish whether the consent law reduced or not the amount of occurrences of unwanted sexual attention at work in Sweden, we will use as dependent variable the question of the questionnaire “Over the last month, during the course of your work, have you been subjected to unwanted sexual attention?”. We exclude from our analysis observations with missing reply to this question. We will also exclude the data collected from respondents aged 55 years and above since, in that category of age, the exposure to unwanted sexual attention occurrences almost disappears. In fact, as shown in the graph displayed below (Figure 3), in 2015, we count only one occurrence for the respondents aged 55 and above.

Figure 3: Number of unwanted sexual attention occurrences for each age category reported in the EWCS in Sweden in 2015[6]

[5] The last waves were in 2015 and 2021, instead of 2020 because of Covid-19. The latter survey was also made by telephone due to the pandemic and thus in a reduced format. The survey is available (upon registration) through the Eurofound website, but the microdata is only available (upon registration) through the UK Data Service.

[6] Age in years is grouped by 10-year classes. Numbers above the columns report the observed occurrences in the sample. There were no occurrences for ages above 64. Source: European Working Conditions Survey 2015.



In 2015, 725 people responded to this question in Sweden and 1,493 in the control group, whereas there are 790 responses in Sweden in 2021 and 2,636 in the control group for the same year.

There are many covariates (which may be correlated to occurrences of unwanted sexual attention) that we can use in the survey to increase the validity of our model, such as the level of education, the type of employment, the type of occupation etc. of the respondents. The number of observations slightly decreases for all covariates due to missing values. For instance, for the type of employment for the 2015 wave, we have 721 observations in Sweden and 1,491 observations in the control group.

We distinguish the respondents classifying them into 3 levels of education: lower education, secondary education and tertiary education, using the respondents' answers to the question "What is the highest level of education or training that you have?". We include in the category of lower education respondents whose highest

level of education or training is no education, early childhood education, primary education and lower secondary education. The secondary education level is composed of the respondents having answered “upper secondary education” or “post-secondary non-tertiary education”. Finally, tertiary education is assigned to the respondents having a level of education equal to a short-cycle tertiary education, a bachelor's degree or equivalent, a master’s degree or equivalent, or a doctorate or equivalent.

We also make a distinction between respondents according to their type of occupation, using the International Standard Classification of Occupation (ISCO). The latter is composed of the respondents having been classified in the ISCO group with the same appellation. In the analysis we will use the first digit of the ISCO classification, but in descriptive statistics we will group them into 3 categories: blue collar, white collar and armed forces The category of blue collars is composed the respondents belonging to the following ISCO categories: skilled agricultural, forestry and fishery workers; craft and related trades workers; plant and machine operators, and assemblers; and elementary occupations. Finally, the white collar category is made of the respondents classified as belonging to the ISCO categories managers; professionals; technicians and associate professionals; clerical support workers; and service and sales workers.

The respondents are also divided in 2 categories based on whether they are working part-time or full-time, using their answer to the question “How many hours do you usually work per week in your main job?”. All respondents working 29 hours or less per week in their main job are considered to be working part-time. All

respondents having declared to be usually working at least 30 hours a week in their main job are considered to be working full-time.[7]

The questionnaire allows us to distinguish between 2 more categories of respondents: those who are employees and those who are self-employed, based on their answer to the question: “Are you working as an employee or are you self-employed?”.

Overall, in our sample, the proportions of both the respondents in Sweden and the respondents in our control group are fairly similar in 2015 (see Table 2). For instance, the 2015 sample is made of 47% of women regarding the respondents in Sweden and 50.5% of women regarding the respondents in the control group (Finland and Norway).[8] The average age is about 38 years in both groups. The proportion of respondents in 2015 having experienced unwanted sexual attention is slightly lower in the control group (3.7%) than in Sweden (4.7%). The size of the different groups of respondents by level of education and type of occupation are only slightly different. By contrast, in Sweden in 2015, there were fewer self-employed and part-time workers than in the control countries.

In 2021 (see Table 3), the dataset contained almost the same proportion of men and women for Sweden and for the control group and the distribution of the level of education is different by 1 percentage point between the two groups. The average age for the Swedish group is less than 38 years and a little over 37 for Norway and Finland. The proportions of the different types of occupation, however, differ slightly more, with 18% of blue-collar workers among Swedish respondents and 24% in the

[7] We use here the definition suggested by the OECD for international comparisons – see https://www.oecd.org/en/publications/the-definition-of-part-time-work-for-the-purpose-of-international-comparisons_132721856632.html.

[8] Sampling weights provided by EWCS are used to obtain statistics that are representative at the country level.

control countries. Nevertheless, the amount of self-employed workers among the respondents in the 2 groups differs by less than one percentage point.

Moreover, it is important to note that the occurrences of unwanted sexual attention have decreased by roughly 25% in Sweden between 2015 and 2021, whereas they increased in the control countries over the same time lapse.

Table 2: Descriptive statistics – 2015 (with age<55)

	Obs	Mean	Std. Dev.	Min	Max
Panel A : Sweden					
Occurrence of unwanted sexual attention	725	0.0470	0.2118	0	1
Female	725	0.4702	0.4995	0	1
Male	725	0.5298	0.4995	0	1
Age (years)	725	37.969	9.952	17	54
Lower education	725	0.0557	0.2295	0	1
Secondary education	725	0.5209	0.4999	0	1
Tertiary education	725	0.4234	0.4944	0	1
Blue collars	721	0.2027	0.4023	0	1
White collars	721	0.7910	0.4069	0	1
Armed forces	721	0.0063	0.0791	0	1
Self employed	724	0.0546	0.2273	0	1
Employee	724	0.9454	0.2273	0	1
Part time	720	0.1195	0.3247	0	1
Full time	720	0.8805	0.3247	0	1
Panel B : Control countries					
Occurrence of unwanted sexual attention	1,493	0.0342	0.1817	0	1
Female	1,493	0.5055	0.5001	0	1
Male	1,493	0.4945	0.5001	0	1
Age (years)	1,493	38.064	10.404	15	54
Lower education	1,491	0.0915	0.2884	0	1
Secondary education	1,491	0.4026	0.4906	0	1
Tertiary education	1,491	0.5059	0.5001	0	1
Blue collars	1,490	0.2445	0.4299	0	1
White collars	1,490	0.7512	0.4325	0	1
Armed forces	1,490	0.0044	0.0659	0	1
Self employed	1,488	0.1007	0.3011	0	1
Employee	1,488	0.8993	0.3011	0	1
Part time	1,483	0.1742	0.3794	0	1
Full time	1,483	0.8258	0.3794	0	1

Note: Sampling weights provided in EWCS are used.

Table 3: Descriptive statistics – 2021 (with age<55)

	Obs	Mean	Std Dev.	Min	Max
Panel A : Sweden					
Occurrence of unwanted sexual attention	790	0.0375	0.1902	0	1
Female	790	0.4891	0.5002	0	1
Male	790	0.5109	0.5002	0	1
Age (years)	790	37.630	9.945	16	54
Lower education	787	0.0475	0.2127	0	1
Secondary education	787	0.4070	0.4916	0	1
Tertiary education	787	0.5456	0.4982	0	1
Blue collars	790	0.1789	0.3835	0	1
White collars	790	0.8120	0.3910	0	1
Armed forces	790	0.0091	0.0950	0	1
Self employed	790	0.0809	0.2729	0	1
Employee	790	0.9191	0.2729	0	1
Part time	762	0.0879	0.2833	0	1
Full time	762	0.9121	0.2833	0	1
Panel B : Control countries					
Occurrence of unwanted sexual attention	2,636	0.0497	0.2173	0	1
Female	2,636	0.4756	0.4995	0	1
Male	2,636	0.5229	0.4996	0	1
Age (years)	2,636	37.121	10.372	16	54
Lower education	2,632	0.0487	0.2153	0	1
Secondary education	2,632	0.4138	0.4926	0	1
Tertiary education	2,632	0.5374	0.4987	0	1
Blue collars	2,636	0.2373	0.4255	0	1
White collars	2,636	0.7552	0.4300	0	1
Armed forces	2,636	0.0074	0.0859	0	1
Self employed	2,634	0.0713	0.2574	0	1
Employee	2,634	0.9287	0.2574	0	1
Part time	2,551	0.1413	0.3484	0	1
Full time	2,551	0.8587	0.3484	0	1

Note: Sampling weights provided in EWCS are used.

It's important to control for these covariates because we observe that the frequency of occurrences varies quite a bit across them. For instance, we see that, in Sweden in 2015, there were twice as many women as men who had experienced unwanted sexual attention occurrences at work, in terms of percentage points. At the same time, in control countries, in the same year, over 5.69% of women experienced unwanted occurrences, which is more than 4 percentage points above the percentage of men having experienced these unwanted attentions (see Table 4). Moreover, people with lower education have a higher rate amongst them of people who have experienced unwanted attention in Sweden than people with higher education. However, for control countries, it is the contrary. People with a lower level of education are less likely to be victims of unwanted attention. Strangely, in 2015 both in Sweden and in control countries, the proportion of people having experienced unwanted sexual attention is lower amongst people who work full time than those who work part time. In fact, one would have expected that a worker who is less present in the workplace, will have fewer occasions to experience unwanted sexual attention. However, the results on part-time in Table 4 might be due to the fact that part-time is more widespread among women and low-educated workers.

Table 4: Descriptive statistics – percentage of occurrences across covariates (with age<55)

	Percentage of occurrence
Panel A : Sweden	
Female	6.65
Male	3.00
Lower education	8.65
Secondary education	4.96
Tertiary education	3.89
Blue collars and armed forces	3.33

White collars	5.12
Self employed	6.70
Employee	4.61
Part time	9.89
Full time	4.04
Panel B : Control countries	
Female	5.69
Male	1.10
Lower education	1.81
Secondary education	3.80
Tertiary education	3.42
Blue collars and armed forces	5.90
White collars	4.45
Self employed	0.58
Employee	3.76
Part time	4.16
Full time	3.29

Note: Sampling weights provided in EWCS are used.

Methodology

To carry out our project, we use a difference-in-difference model, controlling for potential confounders, using the 2015 and the 2021 waves, giving us the following equation:

$$Y_{ist} = \gamma_s + \Lambda_{2021} + \delta T_{s2021} + \sum_j \pi_j R^j_{ist} + \varepsilon_{ist},$$

from which we obtain our results[9].

Y_{ist} is our outcome of interest, (reported) unwanted sexual attention occurrences at work of individual i who is included in the sample of country s at time t. R^j_{ist} are covariates which may be correlated with unwanted sexual attention occurrences and may vary across countries and over time, with π_j indicating their parameters. γ_s represents country fixed effects[10], Λ represents time fixed effects, with Λ_{2021} excluded to avoid multicollinearity[11]. T is the indicator of treatment: it takes value 1 in 2021 in Sweden and 0 otherwise. δ is the difference-in-difference effect which we estimate, that is the estimated effect of the reform on unwanted sexual attention occurrences at work. The above equation is estimated using the sampling weights provided in the dataset. We do so since the samples in 2015 and 2021 do not contain the same individuals and we are interested in the country-wide effect of the reform.

In the simplest version, we estimate the model without covariates. In a second version we include individual socio-demographic characteristics (gender, age –

[9] The results will be discussed later.

[10] There may be unobservable characteristics, which vary across countries, without varying over time. Country fixed effects allow us to control for these characteristics. In practice, s is an abbreviated notation standing for the product of a dummy for country *s* and its coefficient to be estimated.

[11] There may be unobservable characteristics, which vary over time, without varying across countries. Time fixed effects allow us to control for these characteristics. In practice, $\Lambda 2021$ is an abbreviated notation standing for the product of a dummy for 2021 and its coefficient to be estimated.

divided into 4 ten-year classes – and education – divided into three classes as explained above). In a third version, we also include job characteristics.

Our key identification assumption is that any differential change in the rate of unwanted sexual attention occurrences at work between Sweden and the control group is only due to the implementation of the consent law (parallel trends assumption). To partially test this assumption, we will look at parallel trends before the reform using also the 2005 and 2010 waves: in practice this means checking that the changes of *Y* between 2010 and 2015 (and those between 2005 and 2010) are not statistically different between the control countries and Sweden, using the following equations:

$$Y_{ist} = \gamma_s + \lambda_{2010} + \delta_{2010} T_{s2010} + \sum_j \pi_j R^j_{ist} + \varepsilon_{it}$$

and

$$Y_{ist} = \gamma_s + \lambda_{2005} + \delta_{2005} T_{s2005} + \sum_j \pi_j R^j_{ist} + \varepsilon_{it},$$

where T takes value 1 in Sweden in 2015 (or 2010 for the second equation) and 0 otherwise. Finding δ_{2010} and δ_{2005} not significantly different from 0 would be consistent with the parallel trends assumption.

As in a standard difference-in-difference design, we make two additional assumptions. The first one is that there are no spillovers, meaning that control countries are not affected by the consent law reform in Sweden. The second assumption we make is that there is no change in composition, meaning that individuals do not switch countries because of the reform. [12] Even though these assumptions are not directly testable with our data, they seem plausible.

[12] For example, individuals prone to commit rape (or sexual violence of other kind) may migrate out of Sweden following the reform whereas individuals trying to escape such behaviours may migrate into Sweden, violating the stable composition assumption.

However, we must point out the limitations of our model. In fact, other events or reforms affecting sex-related behaviours or perceptions of “unwanted” sexual attention may have occurred only in Sweden between 2015 and 2021. For example, the culture of respect in the workplace could have evolved differently between Sweden and the control countries, leading us to mistake an eventual decrease (or increase) in unwanted sexual attention due to an improvement in cultural attitudes for an effect of the new legislation. However, we can test for this alternative explanation by constructing from the data a variable of occurrence of verbal abuse or threat and use it as an alternative dependent variable.

Moreover, it is likely to take time for this type of reform to have an effect on behaviours, which means that even if we had had data for an earlier year just after the reform (2019 or 2020), it would have been too close to the reform to see an effect.

Furthermore, perceptions of “unwanted” sexual attention are self-reported and somewhat subjective. Thus, an increase in perceptions (automatically inducing more reported cases) does not necessarily mean an increase in overall wrongful behaviours: it could just mean that people are more conscious of them. This implies that if we find a positive effect it might be due to the fact that the reform has made people more conscious of sexual attention. However, looking at the descriptive statistics in 2021 we see that unwanted sexual attention occurrences decreased in Sweden with respect to 2015 while it did not in the other two countries, as mentioned before (see Data description). Because Sweden, Norway and Finland have similar cultures and had equally strong MeToo movements, we can assume that if the increase in reported unwanted sexual attention occurrences in Sweden were due to a

change in this type of perceptions unrelated to the reform, we would have also observed an increase in reported occurrences in the control countries. Since we did not observe such a phenomenon, we could rule out this risk.

The sample being relatively small and, in particular, the victims of unwanted sexual attention being few (see Tables 2 and 3), the standard error of our regressions' coefficient could be large, leading us not to find a significant effect of the reform even where there was one. If that were to happen, we would not be able to conclude that the reform has no effect on unwanted sexual attention occurrences since this result might only be due to the size of the sample.

Finally, the Covid shock might affect the outcome differently in different countries and 2021, our post-treatment year, was still in a period of high intensity of the Covid epidemic. According to the OECD, facilitating teleworking arrangements was one of the main labour market policy responses to the Covid-19 epidemic in Finland and Sweden but less so in Norway (OECD, 2023). In a day in which workers work from home, then there is little risk that they are victims of unwanted sexual attention.

To deal with this latter issue, we will do a robustness check including a teleworking variable in our regression. This variable allows us to take into consideration how much respondents work remotely. We built the variable teleworking as a dummy variable taking value 1 if the respondent had answered "often" or "always" to the question "During the last 12 months, how often have you worked in your own home?" and 0 otherwise. We find that teleworking has increased slightly more in Sweden between 2015 and 2021 (+19%) than in the control group over the same period of time (+13%) – see Table 5. However, since the control group

had a higher share of respondents teleworking in 2015, in 2021 the difference between the proportion of people who telework in Sweden and in the control countries remain of less than 4 percentage points[13].

Table 5: Teleworking (with age<55)

	Obs	Mean	Std Dev.
Panel A : Sweden			
Teleworking - 2015	722	0.1555	0.3627
Teleworking - 2021	788	0.3467	0.4762
Panel B : Control countries			
Teleworking - 2015	1,490	0.1762	0.3811
Teleworking - 2021	2,633	0.3097	0.4625

Note: Sampling weights provided in EWCS are used.

[13] We will test more formally whether, in your sample, the incidence of teleworking has varied differentially between Sweden and the other countries.

Results

As previously mentioned, our baseline model includes occurrence of unwanted sexual attention as dependent variable, as well as country fixed effects and time fixed effects. However, in order to increase the preciseness of our estimation of the effect of the rape definition reform in Sweden on sexual violence, we can expand our model including more covariates (see Table 6).

Table 6: Baseline regression table

	(1)	(2)	(3)
Sample	2015-21	2015-21	2015-21
Dependent variable	Occurrence of unwanted sexual attention		
Treated	-0.0255*	-0.0245*	-0.0244*
	(0.0145)	(0.0142)	(0.0143)
Self-employed	-	-	0.0049
			(0.0129)
Part-time	-	-	-0.0373*
			(0.0211)
Observations	5,644	5,635	5,494
R-squared	0.0463	0.0856	0.0948
Country fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Gender	no	yes	Yes
Age	no	yes	Yes
Education	no	yes	yes
Occupation	no	no	yes

Notes: Treated stands for a dummy taking value 1 in Sweden in 2021 and 0 otherwise. Weighted regressions using individual weights. Gender controls: 2 classes. Age controls: 4 classes. Education controls: 3 classes. Occupation controls: 10 classes (ISCO 08, 1-digit). Robust standard errors in parentheses. *: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level.

The estimated coefficient associated with the variable “treated” represents the difference between the changes in outcome between 2015 and 2021 for the treated

group (Sweden) and the changes in outcome between the same time period for the control group (Norway and Finland). For the treated group, following the reform, we observe a decrease in the outcome by 2.55 percentage points with respect to the changes in outcome of the control group, significant at 10% level. This means that in Sweden, with respect to 2015, in 2021, the amount of reported unwanted sexual attention occurrences at work have decreased significantly. Controlling for demographics, we find that following the reform, the treated group experiences a decrease in unwanted sexual attention occurrences of 2.45 percentage points with respect to the changes in the control countries, significant at 10% level. Finally, controlling for both demographics and job characteristics, we observe that the treated group, after the reform, experience a reduction in the outcome of 2.44 percentage points with respect to the change in unwanted sexual attention occurrences of the control group, significant at 10% level and very close to the effect we estimate including only demographics. We also see that working part-time, as expected, reduces unwanted sexual attention occurrences, probably because, when a worker is less present in the workplace than someone who works full-time, there are less occasions to experience unwanted sexual attention.[14] However, we also find that being self-employed slightly increases the probability of experiencing unwanted sexual attention (by 0.49 percentage points), although insignificantly.

As mentioned in the Data description, we need to account for teleworking because whether people work from their own home or at the workplace may strongly influence unwanted sexual attention occurrences. If a person teleworks, it is indeed normal that they have a smaller probability to receive unwanted sexual attention. In

[14] This confirms that the strange results as regards part-time in Table 4 are due to the fact that part-time workers have socio-economic characteristics with greater incidence of occurrences of unwanted sexual attention. Once we control for these characteristics, the effect of part-time is negative as expected.

fact, if teleworking in Sweden had increased significantly more between 2015 and 2021 than in the control countries, we could have the doubt that the decrease in unwanted sexual attention occurrences previously seen would be due to people working from home rather than the reform of the definition of rape. We see that, if we do not control for anything, teleworking has indeed increased more in Sweden between 2015 and 2021 than in the control group (see Table 7). However, if we control for demographics, this increase in teleworking becomes insignificant (the significance level is above the 10% threshold). If we control for both demographics and job characteristics, the estimated coefficient associated with teleworking in Sweden in 2021 becomes small and remains insignificant at the 10% significance level.

Table 7: Baseline regression table – using teleworking as dependent variable

	(1)	(2)	(3)
Method	OLS	OLS	OLS
Dependent variable		Teleworking	
Treated	0.0589**	0.0313	0.0170
	(0.0274)	(0.0265)	(0.0262)
Self-employed	-	-	0.1941***
			(0.0385)
Part-time	-	-	-0.0366
			(0.0282)
Observations	5,633	5,624	5,484
R-squared	0.3298	0.4163	0.4983
Country fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Gender	no	yes	yes
Age	no	yes	yes
Education	no	yes	yes
Occupation	no	no	yes

Notes: Teleworking is a variable taking value 1 if teleworking occurs often or always and 0 otherwise. Treated stands for a dummy taking value 1 in Sweden in 2021 and 0 otherwise. Weighted regressions using individual weights. Gender controls: 2 classes. Age controls: 4 classes. Education controls: 3 classes. Occupation controls: 10 classes (ISCO 08, 1-digit). Robust standard errors in parentheses. *: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level.

We can also control for teleworking, adding the variable as a covariate in our baseline model. Since we know that in Sweden, teleworking has increased between 2015 and 2021 more than in the control countries, though not significantly, we could still fear that part of the decrease in the outcome which is being attributed to the reform would actually be due to the increase in teleworking. Including teleworking in our regression allows us to condition the results to a specific level of teleworking (see Table 8).

Table 8: Baseline regression table – controlling for teleworking

Method	(1) OLS	(2) OLS	(3) OLS
Dependent variable	Occurrence of unwanted sexual attention		
Treated	-0.0237	-0.0239*	-0.0243*
	(0.0147)	(0.0143)	(0.0144)
Teleworking	-0.0336***	-0.0211**	-0.0138
	(0.0089)	(0.0085)	(0.0093)
Self-employed	-	-	0.0074
			(0.0126)
Part-time	-	-	-0.0371*
			(0.0212)
Observations	5,633	5,624	5,484
R-squared	0.0521	0.0877	0.0957
Country fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Gender	no	yes	yes
Age	no	yes	yes
Education	no	yes	yes
Occupation	no	no	yes

Notes: Teleworking is a variable taking value 1 if teleworking occurs often or always and 0 otherwise. Treated stands for a dummy taking value 1 in Sweden in 2021 and 0 otherwise. Weighted regressions using individual weights. Gender controls: 2 classes. Age controls: 4 classes. Education controls: 3 classes. Occupation controls: 10 classes (ISCO 08, 1-digit). Robust standard errors in parentheses. *: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level.

When including teleworking in the model without covariates, we see that, for the treated group, following the reform, we observe a decrease in the outcome with respect to the changes in outcome for the control groups, which is very slightly smaller than when not including teleworking, by 2.37 percentage points. Nevertheless, the inclusion of teleworking makes the effect of the reform insignificant even at the 10% level. When controlling for both teleworking and demographics, however, following the reform, the treated group is estimated to have experienced a decrease in unwanted sexual attention occurrences by 2.39 percentage points with

respect to the changes in outcome for the control countries, significant at the 10% significance level. Finally, if we control for demographics, job characteristics and teleworking, we find that, after the reform, the Swedish respondents experienced a decrease in unwanted sexual attention by 2.43 percentage points with respect to the changes in outcome for the control group, still significant at the 10% level. As in our baseline model, the estimated coefficient associated with self-employment is slightly positive (but not significant) and the estimated coefficient associated with working part-time is significantly negative, reducing unwanted sexual attention occurrences by 3.71 percentage points with respect to working full-time. Regarding the estimated coefficient associated with teleworking, it is significant without any controls, but the more we add covariates, the less significant it becomes. Taken all together, the results of Tables 7 and 8 are consistent and show that individual and job characteristics already account for most of the effect of teleworking on unwanted sexual attention occurrences.

As previously mentioned in the Data description, the culture of respect in the workplace could have evolved differently between Sweden and the control countries, leading us to mistake an eventual decrease (or increase) in unwanted sexual attentions as an effect of the new legislation, when instead it would just be a consequence of an evolution in culture. We test for this alternative explanation by constructing from the data a variable of occurrence of verbal abuse or threats and plugging it in our baseline regression model as dependent variable. We then observe that the estimated coefficient associated with a differential increase occurrence of verbal abuse or threat for Sweden in 2021, with respect to the control countries, is not significant, meaning that the occurrences of verbal abuse or threat in Sweden

have not varied differently between 2015 and 2021 than they did in the control countries (see Table 9). This is consistent with the hypothesis that the evolution of cultural attitudes as regards interpersonal relationships at work have not evolved differently in Sweden and the control countries. It is therefore unlikely that a different cultural trend was the cause of the greater reduction of sex-related behaviours or perceptions of "unwanted" sexual attention that has occurred only in Sweden between 2015 and 2021.

Table 9: Baseline regression table – using verbal threat as dependent variable

	(1)	(2)	(3)
Method	OLS	OLS	OLS
Dependent variable		Verbal threat	
Treated	-0.0023	-0.0046	-0.0011
	(0.0220)	(0.0220)	(0.0222)
Self-employed	-	-	-0.03296
			(0.0228)
Part-time	-	-	-0.0269
			(0.0228)
Observations	5,442	5,433	5,299
R-squared	0.1030	0.1128	0.1238
Country fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Gender	no	yes	yes
Age	no	yes	yes
Education	no	yes	yes
Occupation	no	no	yes

Notes: Verbal threat is a variable taking value 1 if the respondent declares having experienced verbal abuse or threats in the last month and 0 otherwise. Treated stands for a dummy taking value 1 in Sweden in 2021 and 0 otherwise. Weighted regressions using individual weights. Gender controls: 2 classes. Age controls: 4 classes. Education controls: 3 classes. Occupation controls: 10 classes (ISCO 08, 1-digit). Robust standard errors in parentheses. *: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level.

Looking at the pretrends, using the 2010-2015 waves, we see that the results of all regressions, including the ones controlling for demographics and job characteristics, are all not significant (see Tables 10 and 11). The pre-reform change 2010-2015 was more positive in Sweden by 0.7-0.9 percentage points (that is about 2%), with standard error 1.65-1.66 percentage points, meaning that there is strong evidence in favor of the parallel trends assumption.

Table 10: Pretrends regression table: 2010-2015[15]

	(1)	(2)	(3)
Method	2010-15	2010-15	2010-15
Dependent variable	Occurrence of unwanted sexual attention		
Treated15	0.0072	0.0080	0.0087
	(0.0166)	(0.0165)	(0.0165)
Self-employed	-	-	0.0056
			(0.0131)
Part-time	-	-	-0.0140
			(0.0162)
Observations	4,462	4,456	4,355
R-squared	0.0430	0.0676	0.0770
Country fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Gender	no	yes	yes
Age	no	yes	yes
Education	no	yes	yes
Occupation	no	no	yes

Notes: Treated15 stands for a dummy taking value 1 in Sweden in 2015 and 0 otherwise. Weighted regressions using individual weights. Gender controls: 2 classes. Age controls: 4 classes. Education controls: 3 classes. Occupation controls: 10 classes (ISCO 08, 1-digit). Robust standard errors in parentheses. *: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level.

Then, looking at the pretrends using the 2005 and the 2010 waves, we see that the results of all regressions, including the ones controlling for demographics

[15] Not including teleworking in the table, but the results are robust.

and job characteristics, are all not significant. The pre-reform change 2005-2010 was more negative in Sweden by 0.1-0.5 percentage points, with standard error 1.54-1.59 percentage points, supporting the evidence previously found in favor of the parallel trends assumption.

Table 11: Pretrends regression table: 2005-2010[16]

Method	(1) OLS	(2) OLS	(3) OLS
Dependent variable	Occurrence of unwanted sexual attention		
Treated10	-0.0047 (0.0159)	-0.0033 (0.0154)	-0.0006 (0.0154)
Self-employed	-	-	0.0021 (0.0120)
Part-time	-	-	-0.019 (0.0156)
Observations	4,702	4,694	4,523
R-squared	0.0393	0.0656	0.0744
Country fixed effects	yes	yes	yes
Time fixed effects	yes	yes	yes
Gender	no	yes	yes
Age	no	yes	yes
Education	no	yes	yes
Occupation	no	no	yes

* Notes: Treated10 stands for a dummy taking value 1 in Sweden in 2010 and 0 otherwise. Weighted regressions using individual weights. Gender controls: 2 classes. Age controls: 4 classes. Education controls: 3 classes. Occupation controls: 10 classes (ISCO 08, 1-digit). Robust standard errors in parentheses. *: significant at the 10% level, **: significant at the 5% level, ***: significant at the 1% level.

[16] Not including teleworking in the table, but the results are robust.

Conclusions

This thesis investigates the effect of the change in definition of rape in the legislation, from coercion based to consent based, in Sweden, on unwanted sexual attention occurrences at work. To the best of the author's knowledge , this is the first study which examines the quantitative impact of consent law reform on a type of outcome for which the reform was designed. As discussed, the literature has only examined the impact on secondary unexpected effects.

We have seen that, controlling for teleworking, demographics and job characteristics, following the reform, there has been a decrease in unwanted sexual attention occurrences in Sweden of 2.56 percentage points, meaning a decrease of 54% with respect to 2015. This is a large effect, which remains robust when controlling for a large set of demographics and characteristics, including teleworking.

What we study here, the decrease in unwanted sexual attention occurrences at work, is a desired effect, but it is a minor effect. In the end, we want the reform to decrease more serious dangerous behaviours, such as rape, for which we would need data measuring rapes in the same way before and after the reform. It would be important for the countries, which still have to implement these types of reform to respect the Istanbul Convention, to measure rapes in the way they are defined in the reform, before and after the change in legislation, in order for us to be able to account for the effect of the reform on rapes. Furthermore, the effect we mentioned above on the voting behaviour is important because it represents a "cost" of the reform. However, up until now, it has only been measured with descriptive statistics. We could use the European elections of 2014 and 2019, applying the same

framework, but replacing Norway (which did not vote) with Denmark, to test the effect of the reform on voting behaviours.

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