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Dark Side of Desire: Examining the Spillover Effects of the Nordic Model of Prostitution

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Abstract

Prostitution and its regulation is a topic of frequent discussion and is continuously debated. This paper provides estimates on the spillover effects of the “Nordic Model” of prostitution legislation adopted in Norway in 2009, according to which only sex buyers are criminalised. By employing a difference-in-differences framework, and examining the variation between the Norwegian counties, the paper explores potential spillovers in sexual offences and violence against women from the prostitution legislation. The empirical analysis found no significant results to support or reject any of the investigated spillover effects from the prostitution regulation. The findings provide insights for the Nordic Model of prostitution legislation, more concretely that the spillovers are sensitive to timing, national prerequisites and normative framework.

Keywords: Economics of Crime, Laissez-faire, Policy evaluation, Prohibition, Prostitution Regulation, Sexual Offences, Violence against women

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*"Slavery still exists, but now it applies only to women
and its name is prostitution."*

Victor Hugo

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1 Introduction

The prostitution industry has evoked many opinions, feelings and studies on if and how the market should be managed and regulated. On February 26, 2014, the European Parliament in its resolution on *Sexual exploitation and prostitution and its impact on gender equality*, concluded prostitution to be a form of slavery “incompatible with human dignity and fundamental human rights”, resulting in and exacerbating gender inequality (European Parliament, 2021). This view is further supported by the United Nations (UN, 1949), as well as by several human rights organizations, such as CAP (CAP, n.d.) and Talita, concluding that prostitution is not a transaction between equal parties who enter into an agreement on equal terms, but a trade exchange where the two parties have different conditions and are in different situations (Talita, 2023). The word prostitute itself stems from the Latin word pro-stituere, which translates into “to publicly exhibit” (Talita 2023), and more formally prostitution is defined as “*when at least two parties buy or sell sexual services for compensation (usually financial); which constitutes a prerequisite for the sexual service*” (Ministry of Social Affairs, 1995).

Despite the consensus that the prostitution market is a harmful industry for the majority of sex workers involved, there is an ongoing debate on if, and how, to regulate the prostitution market (Perrotta Berlin et al., 2019). The discussion has several resemblances with many arguments in the debate for alcohol prohibition or drug liberalization. Criminalizing an activity is likely to shrink the corresponding market by increasing the cost of participation (Jakobsson and Kotsadam, 2013) and changing behaviors due to the legislative signal about what actions are approved by society (McAdams, 2000). However, imposing a regulation may push the market into dark-

ness, with a risk of increased violence and criminal activities (Miron and Zwiebel, 1995). Furthermore, prohibiting a market likely creates negative effects spilling over into other, unintended, parts of society (Jakobsson and Kotsadam, 2013). All in all, this poses the question if, and in which ways, prostitution should be regulated.

In 1999, Sweden introduced an asymmetric prostitution legislation that criminalizes sex buyers but not sex sellers (Holst and Lindström, 2000). This new approach to prostitution regulation, aimed at shrinking the demand for buying sexual services, was named the “Nordic model” as it was followed by other Nordic countries, Norway and Iceland, in 2009 (Adams, n.d.). The Nordic model has been favoured by politicians and implemented in a range of countries, as well as been promoted by the European Union, urging member states to take the Nordic model approach to regulate prostitution (European Parliament, 2014). However, recent reports, for example by Kingston and Thomas (2019) and Human Rights Watch (2019), have questioned the effect of the Nordic model and expressed concerns about the overall effects of it. Many reports take the Swedish perspective; therefore, this paper will examine the potential spillover effects of the prostitution legislation in Norway in 2009, and if there are any, what mechanism is driving them.

The remainder of this paper is organized as follows. The next section of this thesis will provide a more extensive background to the topic. Starting with a general background of the prostitution market and the Nordic model of prostitution legislation, the topic will be placed in a historical, as well as economic, context followed by a more thorough review of the theoretical literature on the economics of crime and the previous studies on the prostitution legislation. Thereafter, the research questions this paper aims to answer are presented, followed by an exposition of the economet-

ric model and the data. Lastly, the empirical results are presented and discussed, ending with suggestions for future research.

2 Background

2.1 Overview of the prostitution market

The prostitution market has far-reaching historical roots with a varying degree of judgment towards the mostly female workers' profession. In some areas, selling sexual actions was a recognized profession, while other societies punished, stoned and imprisoned prostitutes. During the European Middle Ages, prostitution flourished and was not only tolerated but was licensed, protected and a substantial source of public revenue, despite opposing forces from the contemporary church. The ambiguity of how prostitution should be treated on the moral and policy-making scale is a red thread through history, and is ultimately determined based on the culturally underlying values of the society in question (Jenkins, 2023).

During the 1980's, attitudes toward prostitution altered entirely due to two developments. First, when AIDS spread rapidly, concern increased about the increased health risks the prostitution workers now face. Additionally, the feminist view of prostitution as a symbol, consequence and symptom of gender-based exploitation altogether shifted the societal attitude towards prostitution to a more negative one. (Jenkins, 2023) For example, non-profit organisations working against prostitution claim that "Prostitution is a form of men's violence against women/children" (Talita, 2023) and in prostitution, not only the body, but the entire human value is for sale (Talita, 2023). In accordance with Talita, United Nations Human Rights office also take a clear stance against prostitution; "Whereas prostitution and the accompanying evil of the traffic in persons for the purpose of prostitution are incompatible

with the dignity and worth of the human person and endanger the welfare of the individual, the family and the community“ (UN, 1949).

Today, prostitution is a multibillion-dollar business employing millions worldwide and the workforce is low-skilled, labour-intensive and mostly female (Edlund and Korn, 2002). The customer is, conversely, mostly male (Talita, 2023). However, heightened market rotation increases the uncertainty in the collected statistics about the exact estimated numbers (Rasmussen et al., 2014). Furthermore, there is likely a high number of undetected cases not reflected in the statistics, since buying and selling sexual services can be illegal and is associated with shame (Holmberg et al., 2011).

Many countries regulate prostitution in some way; however, the view of prostitution and how to regulate it differ largely among countries (Holst and Lindström, 2000). There are mainly two different approaches to regulating prostitution - permissive and forbidding. For example, Germany and the Netherlands are countries with a permissive view on prostitution, where prostitution is allowed, while regulated; such as it is only allowed in certain places or that the prostitutes need to undergo regular medical check-ups. On the other hand, Russia, Iran, Philippines, and certain states in Australia and the USA are examples of countries where prostitution is entirely illegal and parts associated with prostitution can be punished (Holst and Lindström, 2000). Some arguments against criminalizing prostitution are that it pushes the prostitution market into the dark, with an increased risk of undetected crimes against the prostitutes, as well as hindering the prostitutes from receiving help, while the compliance with the regulation would remain hard to control (Holst and Lindström, 2000). All in all, this poses the question if, and in which ways, prostitution should be regulated.

2.2 The Nordic model

The so-called Nordic Model of prostitution was introduced through legislation in Sweden in 1999 and has since then been implemented in a series of other countries, including Norway (2009), Iceland (2009), Canada (2014) and France (2016) (Adams, n.d.). The regulation was unique in regulating the purchase of sexual services so that it only criminalizes the buyer of prostitution, not the seller (Holst and Lindström, 2000). Therefore the regulation gained large attention and in 2014 the European Union passed a non-binding resolution urging member states to take the Nordic model as an example, tackling prostitution by punishing the clients, not the prostitutes (European Parliament, 2014). Mary Honeyball, member of the European Parliament, upon passing the resolution, stated “We send a strong signal that the European Parliament is ambitious enough to tackle prostitution head on rather than accepting it as a fact of life” (European Parliament, 2014). In addition, a report by the Swedish National Council for Crime Prevention from 2000 found the regulation to have decreased street prostitution, while at the same time several prostitutes sought help from social services to break away from prostitution (Holst and Lindström, 2000).

Even if the Nordic Model has been greatly appreciated by politicians, recent reports have questioned the effects of the Nordic Model. Human Rights Watch, among others, has criticized the regulation as they found sex workers at high risk of abuse caused by the legal framework. They state that reliance on abusive buyers has increased and “full decriminalization is a more effective approach to protecting sex workers’ rights” (Human Rights Watch, 2019). Also, a report by Kingston and Thomas (2019), examining the effect of the Nordic Model found that in all observed countries, sex workers

experience higher levels of violence and risks as well as a shift in the power in the relationship between the buyer and the seller after the implementation of the Nordic Model.

2.3 Norway and prostitution

Norway adopted the Nordic Model of prostitution and criminalized the purchase of sex from 1st of January 2009 (Rasmussen et al., 2014). More specifically, money, alcohol, drugs and other objects or services are covered as prohibited forms of payment for a sexual service (Skillbrei and Elden, 2022). The intended aim of the Norwegian government was to avoid sex tourism in the country (Rasmussen et al., 2014), change the attitudes towards prostitution in the population and weaken the recruitment of new customers, which will impede the prostitution market from increasing further (Bjørnflaten, 2008). The regulation, in combination with the existing regulations regarding human trafficking and hallicking, was expected to reduce and prevent both the sex market and human trafficking in Norway (Rasmussen et al., 2014).

However, several reports voice concerns regarding whether or not the legislation has had positive overall effects in Norway. For example, in Norway, 59% sex workers said to have experienced increased violence after introducing the sex purchase law (Kingston and Thomas, 2019). Also, the active enforcement of employing the Norwegian prostitution law shut down formal rental opportunities, which pushed sex workers into more informal and exploitative arrangements (Vuolajärvi, 2022). However, the government of Norway produced another narrative and reported positive changes after the 2009 legislation, and did not find any evidence of more violence against prostitutes after the legislation was introduced (Rasmussen et al., 2014).

Instead, they claim violence is reduced since the seller can report “bad” customers, which fosters less violent and more careful behaviour in the remaining customer base. Furthermore, they state that, despite data limitations, there is a clear declining trend in the prostitution market after the law was implemented, as seen in Figure 1 (Rasmussen et al., 2014).

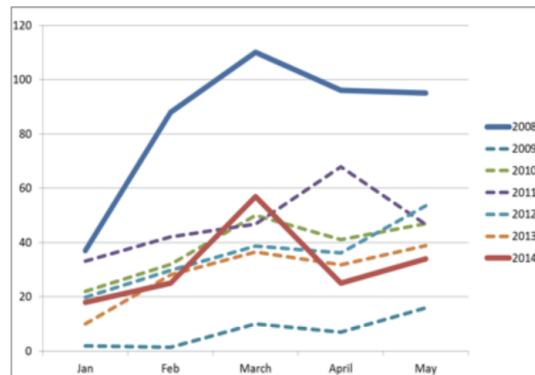


Figure 1: The number of street prostitutes in Oslo 2008-2014. Source: Rasmussen et al., 2014.

3 Previous Literature

3.1 Laissez Faire

Classical economics dominated economic thinking in Europe in the 1870s, where laissez-faire ideas, the free market, free competition and minimum governmental interference in economic affairs were emphasized to achieve economic growth (Britannica, 2023). Economist Adam Smith created strong support for the policy, and according to Smith “the social division of labor would evolve in a satisfactory manner without recourse to outside intervention” (Perelman, 1985). However, one can date the phrase “laissez-faire” back to 1751, and in the 19th century John Stuart Mill introduced it properly (Mill, 1848). Mill (1848) elaborated on laissez-faire, or

non-interference principle, as being the general rule, in his *Principles of Political Economy* and he categorizes governmental intervention into two forms. One is the “authoritative interference of government” that ultimately controls the free agency of individuals, and the second form of intervention adopts the course of giving advice and providing information instead of issuing commands, enforced by penalties. Few things are said to be justifiable as exceptions to interfering with the law of individual freedom, “unless it can also be made to recommend itself to the general conscience; unless persons of ordinary good intentions either believe already, or can be induced to believe, that the thing prohibited is a thing which they ought not to wish to do” (p. 1093). In other words, prohibition of individual behaviour is acceptable if the general conscience considers it coercive (Mill, 1848). Thus, regulating prostitution does interfere with individual freedom, but it may adhere to a *laissez-faire* exemption given its coercive nature and harmful outcomes.

3.2 Economics of crime

The literature regarding the economics of crime outlines a reasoning where prohibiting the market of a commodity raises suppliers’ costs, resulting in a higher price and ultimately lowering the quantity consumed (Jakobsson and Kotsadam, 2013). Becker et al. (2006) take the example of prohibiting selling and using drugs and its effects on supply and demand. They conclude that when drug production is decreased as the good is prohibited, the more inelastic either demand or supply of the drug is, the greater the increase in social costs. In this context, Becker et al. (2006) refer to social costs as corruption of officials, imprisonment of producers and users, and resources spent on enforcement. Moreover, when demand or supply is inelastic, enforcing a prohibition may only be viable if the social value is negative. The magnitude of

the effect on consumption after prohibiting the market depends on the elasticity of demand, where the more inelastic demand, the smaller the effect (Becker et al., 2006).

Miron and Zwiebel (1995) elaborate on the illegal drug market further, and conclude that the majority view is that the problems associated with drug use, like health issues, decreased earnings and moral degradation, must be controlled by governments imposing a prohibition. However, they bring forward a potential reality that the prohibition may cause many problems with illegal drugs. For example, prohibiting drugs is likely to raise marginal benefits and lower marginal costs of resorting to violence without a legal and juridical system in the illegal sphere. This, in turn, leads to an increased ease of establishing a cartel in the prohibited industry. Furthermore, Miron and Zwiebel (1995)'s analysis concludes that profits and violence are related under prohibition. Finally, Jakobsson and Kotsadam (2013) connect the economics of crime to the prostitution and trafficking market. In conjunction with the previously mentioned literature, they explain that imposing a prohibition policy in the prostitution market might have ambiguous welfare effects in addition to the decreased quantity consumed (Jakobsson and Kotsadam, 2013).

Another line of reasoning around prohibiting a market by legislation concerns how prohibition affects attitudes. McAdam's attitudinal theory (2000) implies that an individual's behaviour is steered, in part, by what actions she believes others will approve or disapprove. This is either motivated by the individual valuing the approval on its own, or that she wants the approval to achieve another objective. Furthermore, McAdams acknowledges how, due to imperfect information about what "others" approve of, individuals are more sensitive when new sources of information are presented. A democratically produced law can be said to be positively correlated

with popular attitudes. Therefore the legislative signal influences people to update their prior beliefs about what others approve and disapprove of and act accordingly (McAdams, 2000). This is further elaborated by McAdams and Rasmusen (2007), who discuss two lines of the importance of norms in regard to the economic analysis of law. Norms play an important line in predicting how a regulation change affects behavior and explaining how a law is formed. They stress the importance of carefully considering how norms may dictate the behaviour when a law is absent and how a new regulation can, intentionally or unintentionally, change a norm. A new regulation can shift the underlying norms by changing perceptions of approved behaviour or creating a new basis of shame (McAdams and Rasmusen, 2007).

3.3 Previous studies

Deriving from the debate on whether the Nordic Model achieved its outcomes or not, various strands of literature have developed.

One strand of literature, with authors like Lee and Persson (2022), Cho et al. (2013), Nicola et al. (2005) as well as Jakobsson and Kotsadam (2013) discuss the impact of prostitution legislation on human trafficking. Cho et al. (2013) investigate the impact of legalised prostitution on human trafficking inflows, and find an extension of the prostitution market and increased human trafficking. Similarly Jakobsson and Kotsadam (2013) find that human trafficking is less prevalent in countries where prostitution is illegal, and thus conclude that criminalizing the purchase of sex can decrease trafficking. In line with this, Nicola et al. (2005) examine data from eleven EU countries and concludes that a less sharp legislation against prostitution may increase the inflow of trafficking victims. However, Lee and Persson (2022) find that

criminalizing the buy side of the prostitution markets cannot eliminate trafficking.

Another strand of literature (Della Giusta et al. 2008; Jakobsson and Kotsadam 2011a; Kotsadam and Jakobsson 2011b) discusses the changes in attitudes towards prostitution from imposing prostitution legislation. Della Giusta et al. (2008) examine the effect of stigmatization on the demand and supply of sex work. The authors conclude that both buyers and suppliers of prostitution generate a reputation cost if engaging in prostitution, as both parties care about their social status in the community and engaging in prostitution will reduce their prestige. Jakobsson and Kotsadam (2011a) investigate the attitudes towards prostitution in Sweden and Norway, where the purchase of sex is illegal, and find that, in general, liberals and men are more positive towards prostitution, while conservative men or women, and those supporting gender equality generally are more negative towards buying sex. Jakobsson and Kotsadam (2011b) also assess the short-run effect on attitudes after implementing the Norwegian prostitution legislation. They find that the regulation did not impact the moral attitudes toward prostitution short-term, except in Oslo, the Norwegian capital, where the attitude became more negative. Therefore they conclude that proximity and visibility are important factors for internalizing legal norms.

Another concern in the debate on the Nordic Model of prostitution is the spillover effects of implementing such legislation that may affect areas beyond the initially targeted prostitution market. Cameron et al. (2021), Ciacci (2020), and Perrotta Berlin et al. (2019) discuss these unintended effects of imposing the legislation. Cameron et al. find evidence in Indonesia that the criminalization of sex work increases sexually transmitted diseases and decreases the earnings among sex workers. They also

conclude that even if imposing a prostitution legislation might shrink the market in the short-term, five years post the legislation the market has rebounded and sexually transmitted diseases will have stabilised at a higher level.

Ciacchi (2020) adds to the recent line of literature by examining the effect of the prostitution regulation in Sweden on rape. The paper uses the variation in the availability of sex tourism in different regions to determine the effect of criminalizing prostitution on rape. The evidence shows that the implementation of the prostitution legislation raised the reported rape between 1999 and 2014 by 47%. The report also finds changes in the composition of rapes committed, with an increase in completed and outdoor rapes and a reduced amount of attempted rapes. The increase in rapes is found to be attributed to a shift in the demand for prostitution, however, they find no evidence that the increase is due to an increased supply (Ciacchi, 2020).

Perrotta Berlin et al. (2019) analyse the spillover effects of the 1999 prostitution legislation in Sweden, focusing on various measures of reported violence. The paper concludes that the Nordic Model of prostitution in Sweden results in a smaller prostitution market, however the regulation has spillover effects, mainly in the form of increased domestic violence. Furthermore, the paper shows the increase in reported violence to be an increase in reported domestic violence, rather than violence towards sex workers. This aligns with the findings that restrictions on the prostitution market have substantial negative effects on communities, compared to not regulating the sexual service market (Perrotta Berlin et al. 2019).

3.4 Difference-in-differences framework in recent literature

To be able to give a causal claim to the aforementioned spillover effects, Perrotta Berlin et. al (2019) add to recent literature by employing a difference-in-differences framework, allowing to control for changes that are not related to the new prostitution regulation in Sweden 1999 (Perrotta Berlin et al., 2019). To isolate the new law's effect, they focus on a comparison between the different counties in Sweden, using monthly data. They classify the different counties as treatment groups, where the regulation is more strictly implemented, and compare it to a control group with counties where the regulation is less strictly implemented. This provides for the possibility to compare the different groups after the threshold in 1999 to establish potential spillover effects that are more apparent in the treatment group.

To assign counties the treatment and control group, Perrotta Berlin et al. (2019) draw on previous literature (Iyer et al., 2012) showing that an increase in female representation at a local government level provides for a large and significant increase in reported crimes against women (in India). This increase is primarily driven by higher reporting, rather than higher extent of crimes. The paper concludes that female political representation at a local level increases the ability of women to report crimes committed against them. Similarly, Miller and Segal (2018), who study the impact of women police on violent crimes and domestic violence reporting, show that an increase in female representation in the police force increases the rate at which crimes against women are reported. The police departments with a larger share of women proved to respond more effectively to cases of sexual offences and domestic violence.

Based on this literature Perrotta Berlin et al. (2019) categorizes counties into treatment and control groups based on if they are above or below median, either in terms of women among elected politicians or among the police force before the time of the implementation of the 1999 prostitution legislation.

4 Research Specification

4.1 The aim of the essay

This essay aims to analyse the potential spillover effects of the Norwegian prostitution legislation in 2009. The purpose is to examine if the law has resulted in any spillover effects, that is, any outcomes apart from the intended ones, and establish if there is a causal effect between the spillover effects and the Norwegian prostitution legislation. First, this will be done by proposing and testing three different mechanisms, to further discuss its comparison to recent Swedish studies' results, and lastly address the overall implications of the Nordic Model legislation.

4.2 The hypothesised mechanisms

Imposing a market prohibition may have various and ambiguous effects, both on the intended market and society outside of it. Thus, criminalising buying sexual services may result in several outcomes and spillover effects, generated by three potential mechanisms. The following part will highlight the three mechanisms tested in this paper.

MECHANISM 1: Regulating the prostitution market will restrict access to the service, which may lead to previous customers looking elsewhere to fulfil their demand.

As implied by Becker et al. (2006), the effect on consumption after prohibiting a market depends on the elasticity of demand. The more inelastic demand, the smaller the effect is. Therefore, suggesting that the former client's demand is inelastic, there would be a small impact on its consumption, and that they would instead resort to other ways of fulfilling the need. Furthermore, Gao and Petrova (2022) indicate that prostitution is a substitute for sexual violence and that prohibiting commercial sex, like in the Nordic model, could have the unforeseen consequence of increased sexual violence. Ciacci's study (2020) also shows that the implementation of the prostitution legislation raised the reported rape, and the increase is found to be attributed to a shift in the demand for prostitution. Hence, if this is the underlying mechanism, an increase in sexual offences is expected.

MECHANISM 2: Another alternative outcome of restricting the prostitution market is that violence increases. Since sex workers are more legally protected, prostitutes may report more violent crimes. Acknowledging an increase in violence is supported by Miron and Zweibel (1995), where prohibition relates to increased violence in the absence of a legal and juridical system in the remaining illegal sphere. Both these scenarios would imply that reported violence increases. Furthermore, following Perrotta Berlin et al. (2019), this mechanism assumes that some individuals are "intrinsically violent" implying that violence is constant. As the market shrinks when it is prohibited, there are fewer sex workers in the industry and violence is transferred from inside the prostitution market to outside. Concretely, this can take the form of non-prostitute, domestic violence, as also shown in the Swedish case (Perrotta Berlin et al., 2019). Consequently, if this is the underlying mechanism, an increase in violence against women is expected.

MECHANISM 3: Imposing the prostitution legislation sends a societal signal of what actions are approved and disapproved in sex work, and pushes people to revise their general attitudes towards women. Therefore, suggesting that the former client's demand is elastic, there would be a large impact on consumption, and that they would abandon the service (Becker et al., 2006). A democratically produced law can be positively correlated with popular attitudes, according to McAdams (2000). Therefore the legislative signal influences people to update their prior beliefs about what others approve and disapprove of and act accordingly. In accordance with Della Giusta et al. (2008), this implies a reputation cost if engaging in prostitution, shrinking the demand. The intended aim of the Norwegian government was to influence attitudes in this way, in the hope that the more negative attitude would shrink the market. The shift in general attitudes could then result in a decrease in crimes committed against women outside of the prostitution market, for example through a decrease in both sexual offences and violence against women, as general attitudes are updated. Therefore, this could decrease both sexual offences and violence against women, as general attitudes are updated. However, the spillover effects could also increase as the legislative signal can make more people report crimes. This can counteract the total decrease in crimes against women. Consequently, if this is the mechanism in place, the results would be ambiguous for both sexual offences and violence against women.

4.3 Contribution

Firstly, this essay adds to the recent debate on the Nordic Model and further informs the connected policy decisions by providing improved insights on the effects of the 2009 Norwegian prostitution law. Secondly, this paper contributes to the already mentioned growing set of academic literature addressing spillover effects of different

prostitution regulations. This study uses a quantitative quasi-experimental approach based on observational data, which complements the previously mentioned research in Norway. Perrotta Berlin et al. (2019) and Ciacchi (2020) recently concluded negative spillover effects associated with the Nordic Model in Sweden, with important implications for Swedish policy choices. However, there is a lack of research on the spillover effects in other countries that have implemented the Nordic Model. This paper aims to address this gap in the literature and contribute to Norwegian policy-making decisions by drawing on Perrotta Berlin et al.’s (2019) paper and the method used to examine the Swedish case. Moreover, this also provides an opportunity to contribute to an assessment of the Nordic Model’s approach to asymmetric criminalization.

4.4 Research questions

- Did the Norwegian prostitution law result in any spillover effects related to the proposed mechanisms?
- Based on the results, what can be concluded about the proposed mechanisms?
- How do the results relate to similar Swedish studies, and what does this say about the Nordic model of prostitution legislation in general?

5 Methodology

This section will first explain and motivate the methodology to establish the causal effect between the Norwegian 2009 prostitution legislation and spillover effects in sexual offences and violence against women. Secondly, this section will elaborate on the econometric model underpinning the regressions, followed by descriptive statistics

and information about the associated panel data and variables. Lastly, this segment will end with limitations with the chosen method and data.

5.1 The Difference-in-differences framework

The difference-in-differences framework is a useful quasi-experimental design to estimate causal effects, especially in the context of policy evaluation. By using longitudinal data ¹ divided into a treatment and a control group, the difference-in-differences framework provides the opportunity to estimate the effect of a specific treatment, for example, the passing of a new law. The difference-in-differences estimator can estimate the treatment effect if the treatment is as-if randomly assigned. The framework compares the differential change in outcome between the treatment and the control group before and after the treatment group is exposed to the treatment. That is, none of the groups are treated during the first period, while one of the groups receives the treatment in the second period. A comparison can then be made, not by the absolute outcomes Y , but by the change in the outcomes before and after the treatment (see equation 1).

$$\hat{\beta}_1 = (\bar{y}_{\text{treatment, after}} - \bar{y}_{\text{treatment, before}}) - (\bar{y}_{\text{control, after}} - \bar{y}_{\text{control, before}}) \quad (1)$$

To give a causal claim of the 2009 Norwegian prostitution legislation on spillovers, this paper will draw on Perrotta Berlin et al.'s (2019) paper and the method used to examine the Swedish case described in 3.4. Just as Perrotta Berlin et al. (2019), this essay will focus on comparing geographical regions, both at the county and police district level, using data on reported crimes. Even if the prostitution legislation is applied nationwide, the implementation and the policy priorities can be argued to

¹Longitudinal data, or panel data, refers to data for i different entities observed at t different time periods.

differ significantly between geographical areas.

First, Iyer et al.'s (2012) study show that an increase in female representation at a local government level provides for a large and significant increase in reported crimes against women. Furthermore, a county with a higher share of female politicians might influence the agenda in favour of women-related issues, such as prioritising the implementation of the prostitution legislation (Perrotta Berlin et al., 2019). Therefore, counties will be divided into two groups, one treatment and one control group based on whether their share of women among elected politicians is above or below average. Counties with a high share of women among elected politicians will be assigned to the treatment group and counties with a share of women below average will be part of the control group.

Secondly, a high share of female police officers also increases the rate of reported crimes against women and departments with a higher proportion of female police were shown to more effectively respond to cases of both domestic violence and sexual offences (Miller and Segal, 2018). Therefore, this study will also assign police districts to treatment and control groups based on the share of women in the police force. Police districts with a high share of women in the police force will be assigned to the treatment group and police districts with a share of women below average will be part of the control group. Assigning both counties and police districts into treatment and control groups, based on two different factors, enabling comparison of the results. Hence, this will provide a more robust conclusion of the outcomes of the legislation. As the law is implemented, potential spillover effects will be observed to a greater extent in the treatment group, compared to the control group.

Therefore, this method both provides us with the opportunity of observing the effect of mechanism 1 (see more in part 4.2), through a potential difference in sexual offences between the treatment and control groups, but also the effect of mechanism 2, through a potential change in violence against women. In this way, comparing the differences in the treatment and the control group isolates the potential causal effect the legislation has on reported crimes, ie. the examined spillover effects. Even though the attitude shift, according to mechanism 3, would be more visible in treated counties since the law is more implemented and thus provides a larger societal signal, this method would capture ambiguous results following mechanism 3. All in all, the method used allows one to draw conclusions about mechanisms 1 and 2, and could provide an indication about mechanism 3.

5.2 The econometric model

In establishing the relationship between implementing the prostitution law and the crimes against women the following two econometric models will be used.

The first model uses data from the 19 counties and the share of women among elected politicians as treatment level:

$$\begin{aligned}
Y_{it} = & \beta_0 + \beta_1(postXtreatment)_{it} + \beta_2(treatment)_i + \beta_3post_t + u_{it} \quad (2) \\
& + \beta_4Unemployment \\
& + \beta_5Education \\
& + \beta_6Gini \\
& + \beta_7Compensation \\
& + \beta_8GFCF
\end{aligned}$$

In the model depicted in equation 2, i denotes county, t denotes time, Y denotes either reported sexual offences or reported violence against women, $treatment$ takes on either 1 or 0 depending on if the counties are above or below average in terms of women's representation in the elected county politicians and $post$ takes on the value 0 or 1, given if the time period is before or after 2009. Controls according to line 2, 3, 4, 5, and 6 in equation 2 will be added. All sets of regressions include year and county fixed effects ², as well as robust standard errors to account for heteroscedasticity ³. The dependent variables are converted into the natural logarithm to make the estimates comparable between counties. ⁴

²Both controls and fixed effects are included to isolate the exogenous variation in our dependent variable and to account for unobserved time-invariant heterogeneities between cross-sectional units in the panel data of regions, and control for factors that are constant over time but differ across entities. By using panel data, one can estimate the relationship based on the within-group variation, i.e. within the cross-sectional units (regions) over time.

³Potential heteroscedasticity and the choice of standard errors will be elaborated on in part 7.2.2

⁴By taking the natural logarithm, changes in variables are expressed in percentage which facilitates interpretation of effects in different sized counties.

The second econometric model that will be tested, using data on the 27 police districts and above average share of female police as treatment level, is as follows:

$$Y_{it} = \beta_0 + \beta_1(postXtreatment)_{it} + \beta_2(treatment)_i + \beta_3post_t + u_{it} \quad (3)$$

In the model, i denotes county, t denotes time, Y denotes reported sexual offences, *treatment* takes on either 1 or 0 depending on if the counties are above or below average in terms of women’s representation in the police force and *post* takes on the value 0 or 1, given if the time period is before or after 2009. All sets of regressions include year and county fixed effects, as well as robust standard errors to account for heteroscedasticity. The dependent variable is converted into the natural logarithm to make the estimates comparable between police districts.⁵

6 Data and Limitations

The panel data used in this paper is compiled by Statistics Norway (SSB), Norway’s official central statistical agency, a government institution tasked with collecting, processing and disseminating official statistics. The dependent variables, reported sexual offences and reported violence maltreatment, the control variables and the data used to determine the treatment are sourced from this database and are displayed annually. The years used in this paper are limited to 2004-2014 to retrieve the most accurate and comparable results, which will be further elaborated on below.

⁵Part 6.1.2 will elaborate on the absence of controls in Equation 3

6.1 Descriptive statistics

This section provides descriptive information about the data. A summary of the used variables can be found in table 1 and table 2.

6.1.1 Dependent and control variables

To increase the precision of the estimates and the explanatory power of the model, controls are included in the regressions. These additional control regressors are not the object of interest in this study, but rather are included to hold constant factors that, if not included, could lead to omitted variable bias.⁶ The set of controls, together with time- and entity fixed effects allows us to control for unobserved time-independent factors and thereby potentially reducing omitted variable bias.

Thus, the reasoning above, together with the assumption of no perfect multicollinearity⁷, will guide the choice of controls in the following section.

SEXUAL OFFENCES

The first dependent variable tested in this paper is reported sexual offences, and the data includes all subcategories within sexual offences to capture all forms of sexual violence: rape, including rape against children and aggravated rape, attempted rape, sexual act, sexually abusive behaviour and other or unspecified sexual offences. The

⁶Omitted variable bias occurs when an omitted variable is correlated with the regressor, and is a determinant of the dependent variable. In this case, if omitted variables are correlated with the implementation of the prostitution legislation and determinants of sexual offences and violence and maltreatment. Then, the estimates of the causal effect will be biased and reflect both the effect of the regressor and the effect of the omitted variables. This results in the dependent variable correlating with the error term, but by adding appropriate controls omitted variable bias is reduced.

⁷Perfect multicollinearity makes it impossible to compute the OLS estimator. It happens if one of the regressors is a perfect linear function of the other regressors. A general solution is to modify the choice of regressors to eliminate the problem, and a correlation matrix is attached in the Appendix to display the validation of this assumption.

⁸Note that the variables are reported from 2004-2008 to display a benchmark of the pre-legislation period

Table 1: Summary Statistics Pre-Treatment (2004-2008) ⁸

| Variable | N | Mean | Median | SD | Min | Max |
|-------------------------|----|--------|--------|--------|-------|-------|
| Sexual Offences | 95 | 155.58 | 139 | 81.73 | 34 | 388 |
| Violence & Maltreatment | 95 | 580.91 | 490 | 387.93 | 163 | 1909 |
| Unemployment | 95 | 2.78 | 2.7 | 1.05 | 1.1 | 6 |
| Education | 95 | 18.71 | 18.3 | 2.74 | 15.1 | 28.5 |
| Gini | 95 | 24.03 | 22.8 | 4.55 | 19.2 | 45.8 |
| Compensation | 95 | 17.21 | 16.04 | 5.15 | 11.76 | 38.48 |
| GFCF | 95 | 18.59 | 12.44 | 15.39 | 5.24 | 80.24 |

Author's summary of retrieved data from SSB.

data is disaggregated along the dimension by county and by year, however not by gender. Nevertheless, as seen in figure 2, the greater part of the reported sexual offences is committed against women, and the over representation of women remains constant over time, and therefore the total sexual offences will be used as a proxy for sexual offences on women. This will display mechanism 1 and 3. The data on the variables used is only available from the year 2004, and the classification of the crime (sexual offences) changed after 2014. Therefore the study has limited the timespan from 2004-2014, to assure a homogeneous data input during the examined time.

VIOLENCE MALTREATMENT

The other dependent variable used in this study is reported violence maltreatment, and includes offences of violence (including assault, bodily harm, murder, and attempted murder), threats, robbery and extortion, coercion, deprivation of liberty, offences within family relations and the like, reckless behavior and stalking, human trafficking and other violence and maltreatment. This variable will display the effect in mechanism 2 and 3, and is disaggregated along the dimension by county, by year and filtered to female victims only. Due to limitations in the data, this variable can not be disaggregated more narrowly on types of violence, for example on domestic violence. Furthermore, another limitation in the variable is that it can not be filtered

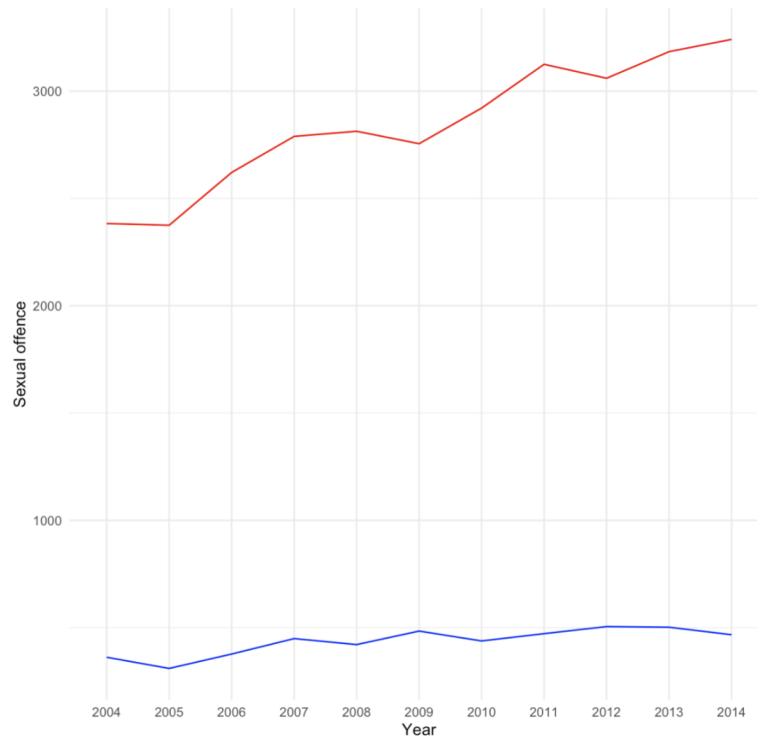


Figure 2: Sexual offences 2004-2014 (Blue = Men, Red = Women) Source: Author's illustration of retrieved data from SSB

on gender at police district level, and is therefore only used in Equation 2.

UNEMPLOYMENT

Unemployment on county level is used as a control variable in the regression to account for differences between the counties. The data is based on NAV's, the Norwegian employment agency, insurance fund, pension authority and social service register of unemployed persons registered at the employment office. The annual average is based on the number of unemployed persons at the end of each month.

EDUCATION

To account for differences in education between the counties, a measure of education is controlled for. Education corresponds to tertiary education, that is post-secondary education, up to 4 years in duration.

GINI

To account for the inequality in income, the Gini score is used. The Gini variable represents the income dispersion within the different counties. A Gini coefficient is a number between 0 and 1, where a score of 0 reflects perfect equality, where all income values are the same, while a Gini coefficient of 1 reflects maximal inequality among the counties.

COMPENSATION

As a proxy for income, compensation of employees (per capita) has also been added as a control variable to control for the differences in income between the counties. It describes the cash rewards or other company benefits in rewards to employees, such as base salary, wages, incentives and commission. The variable is expressed in hundred thousand NOK.

GFCF

To control for differences in business activity across counties, which will in turn affect municipality tax and ultimately the economic muscles of the counties, the gross fixed capital formation (GFCF) has been controlled for. GFCF can also be called “investments”, and consists of the acquisition of produced assets, deducting disposals, as well as the production of such assets by producers for their own use. The variable is expressed in billion NOK.

All the choices of controls are based on their contribution to a measure of counties' socio economic status. High unemployment and gini-coefficient, as well as low education, compensation and GFCF all contribute to a socio-economically weak county, with residents more prone to resorting to criminal behaviour, which is supported by BRÅ (Larsson and Shannon, 2023). The fixed effects, included in all regressions, will capture underlying county specific criminal behaviour that is fixed over time, but these controls are likely to be time-varying and must therefore be included to eliminate omitted variable bias. Therefore, by controlling for counties' socio-economic status, the variation of the interaction term (capturing the effect the implementation of the prostitution legislation has on sexual offences versus violence and maltreatment) can be less influenced by omitted variables' and are therefore more isolated and ultimately produce a more precise estimate.

6.1.2 Treatment variables

TREATMENT VARIABLES

As mentioned, this essay focuses on comparing geographical regions, both at the county and police district level. Geographical regions are assigned to treatment and control groups based on the share of women among elected politicians and police.

Table 2: Share of female politicians, 2008

| Variable | N | Min | Median | Max | Mean | SD |
|-------------|----|------|--------|------|------|------|
| Treatment 1 | 19 | 0.34 | 0.38 | 0.46 | 0.38 | 0.03 |

Author's summary of retrieved data from SSB.

Table 3: Share of female police, 2005

| Variable | N | Min | Median | Max | Mean | SD |
|-------------|----|------|--------|------|------|------|
| Treatment 2 | 27 | 0.08 | 0.17 | 0.24 | 0.16 | 0.04 |

Author's summary of retrieved data from SSB.

Firstly, table 2 describes the county share of female politicians. In 2020 the division of counties in Norway merged from 19 to 11 counties. This paper uses the administrative division of the 19 counties used until 2019, as this was the division during the years examined. Further details of these 19 counties can be seen in table 2, where the share of female politicians is 38% on average. These 19 counties combined with the studied period of 11 years (2004-2014) results in a balanced panel⁹ of 209 observations. Figure 3 describes which of these are ultimately assigned to the treatment (red) and control group (grey) based on their high respectively low share of female politicians. The measure of the share of female politicians is retrieved from the term of office between 2007-2011, as it provides the latest pre-legislation data on politicians. Even if the shares remain more or less the same even the next term of office (between 2011-2015), the data chosen is the term of office before to avoid endogeneity.

Secondly, table 3 describes the police district's share of female police. Police districts were reclassified in 2016 to include 12 regions, but since this study examines the period before 2016 the older classification of 27 police districts is used (SSB, n.d). As seen in Table 3, the mean share of female police officers in all police districts is 16% and no district exceeds 25% of female police officers. The 27 observations of districts combined with the 11 years of data results in a balanced panel of 297 observations. Furthermore, when comparing Equation 2 and Equation 3, the set of controls used on the county level can not be retrieved from SSB on the police district level since SSB only reports crime-related statistics connected to the police districts. The only available data on the share of female police before the legislation in 2009

⁹A balanced panel has all observations for each entity and each time period.

¹⁰In lack of sufficient graphical information about the exact geographical border between the police districts before 2016, no map can be made that corresponds to the one about the treatment and control groups of the police district.

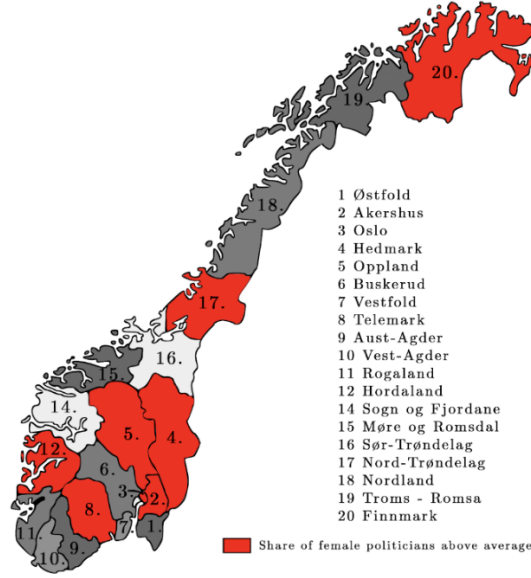


Figure 3: Map of the treated counties. Source: Author's illustration of retrieved data from SSB. ¹⁰

is from 2005. Thus, this year has been chosen to avoid endogeneity.

6.1.3 Parallel trends

As mentioned, the difference-in-differences method compares the differential change in outcome between the treatment and the control group before and after the treatment group is more or less exposed to the treatment. To compare the groups where none of the groups are treated during the first period, while one of the groups receives a stricter treatment in the second period, showing the groups' parallel trends before the treatment is crucial. In other words, in the absence of treatment, i.e., before the law was implemented, the difference between the treatment and control group should remain the same over time. This ensures that after treatment, the treated entities would have followed the same trends if it was not for the treatment. A visual inspection can be made to ensure the parallel trend assumption holds by plotting

the dependent variable of the treatment and control groups. As seen in Figures 4, 5 and 6, there was a parallel trend between treatment and control groups before the law's implementation in 2009, for both the dependent variables and divisions into treatment groups.

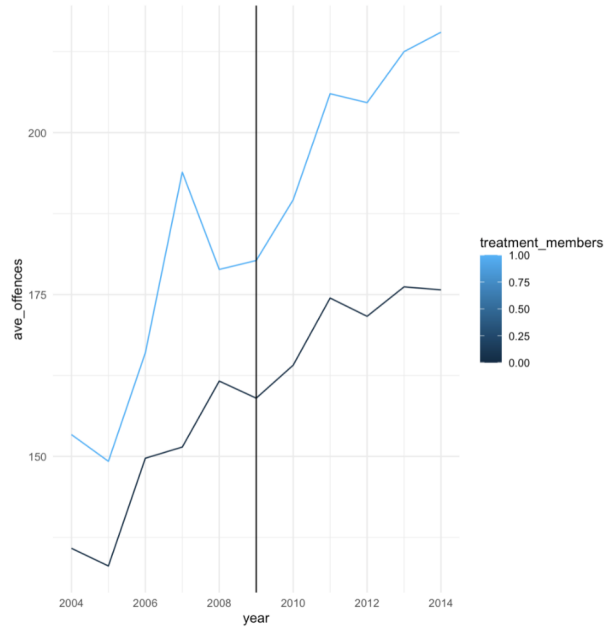


Figure 4: Yearly average of sexual offences, assigned into treatment and control based on share of female politicians. Source: Author's illustration of retrieved data from SSB.

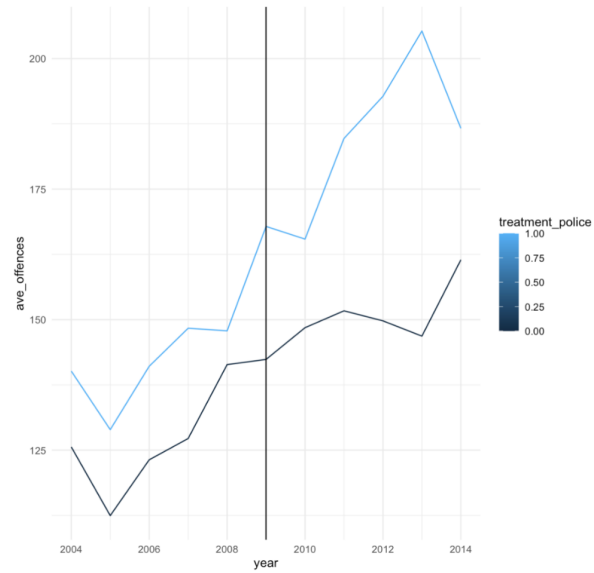


Figure 5: Yearly average of sexual offences, assigned into treatment and control based on share of female police. Source: Author's illustration of retrieved data from SSB.

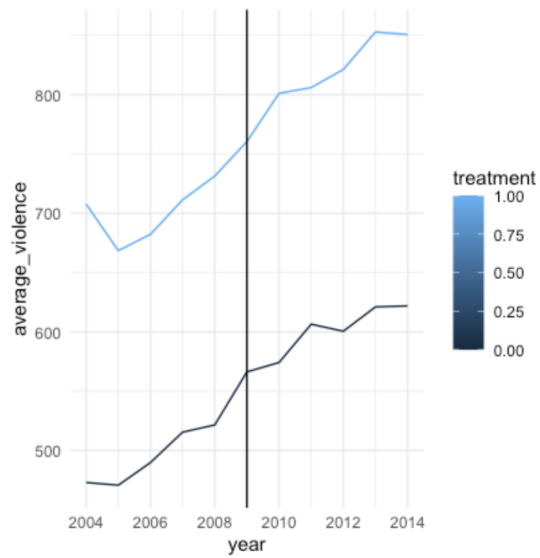


Figure 6: Yearly average of violence maltreatment, assigned into treatment and control based on share of female politicians. Source: Author's illustration of retrieved data from SSB.

6.2 Limitations

6.2.1 Data limitations

The data used is subject to several limitations. Firstly, the existing data on Norwegian crimes at the county level is not available at a detailed level. Firstly, victim data is sensitive due to privacy, especially when numbers are small. Therefore, Statistics Norway (SSB) has decided not to publish data on the smaller categorization of crimes, but instead display the broader definitions like “sexual offences” and “violence maltreatment”. Secondly, SSB does not provide information regarding *modus operandi*. This means details are absent in the crime data, like whether the victim knew the perpetrator or whether the crime was committed outside or inside. Not being able to filter down on the type of perpetrator results in difficulties in drawing conclusions about the spillover effects driven by mechanism 2 (part 4.2), likely resulting in heightened domestic violence. Third, there is no data on a monthly level, rather it is only displayed yearly. This results in a limitation as it impedes the possibility of drawing conclusions on a finer scale, both in terms of short-term time effects of the legislation, and obtaining more data points to arrive at more precise conclusions.

Furthermore, the dependent variables used to detect the societal spillover effects are reported crimes against women, which may not reflect the true total population of crimes committed. The nature of the crime data could disguise the true causal relationship between the prostitution legislation and violence against women, and impose barriers to getting realistic results that correspond to the actual reality. Especially, sexual crime is characterised by many unknowns in the statistics of crimes reported, due to shame and stigma associated with reporting (BRÅ, 2019). Factors like deficient knowledge of the judiciary system, fear of various types of negative

consequences of a report and lack of social and curative support also contribute to the under-reporting of sexual offences (Department of Justice, 2005). Furthermore, it is difficult to assess the extent to which changes in the reported crimes reflect an actual increase in sexual crime, since the increases may be due to an actual increase, an increased tendency to report or a combination of these two (BRÅ, 2019).

As mentioned, reporting is always imperfect in these types of crimes, due to for example stigma and shame, which creates measurement error that could impose a threat towards internal validity. However, it is assumed that the reporting behaviour does not change by the implementation of the legislation and it is not expected that the measurement error in reporting is larger in the treatment group versus control group, nevertheless rather remains evenly distributed in both groups. In other words, measurement error created by underreporting is likely not affected by the share of female politicians and police. Thus, in this study it is expected that the measurement errors are not systematically related to the treatment and do not introduce bias, and the existing measurement error only causes the estimates to be less precise due to larger standard errors. Furthermore, spillover effects across counties may affect the study's internal validity and the preciseness of the results. As counties belonging to either a treatment or control group are not isolated from each other, counties in the control group may experience a spillover from counties who were assigned to a treatment group. This may produce less precise results, and the magnitude of the effect of the treatment might be smaller.

6.2.2 Limitations and validity

The study may be subject to several limitations and potential threats to internal and external validity. Firstly, the study's internal validity depends on the as-if randomization in the treatment level (Stock and Watson, 2019). The way of dividing counties into treatment and control groups based on their share of women in the police force and elected politicians can be considered exogenous. For example, regarding the amount of police, there is likely a higher absolute number of police in a high crime rate area, which could be an argument against exogeneity (Ministry of Justice and Public Security, 2017). However, since this study looks at the share of women and not total police workers, which is not affected by the overall crime rate, this will likely not be a threat against internal validity. It could also be argued that more female police and politicians are assigned to counties that comparatively have more women related crime. For example, more female police and politicians can be allocated to counties where there are specific problems with implementing the legislation which would make the treatment systematically correlated with the outcome. However, since the assignment into treatment and control groups is based on the level of female police and politicians before the law was implemented, the treatment can be considered to remain exogenous and unaffected by events post-implementation. This also validates one of the OLS assumptions, which will be further elaborated on in part 7.2.2.

As discussed, omitted variables might bias the estimate. The used controls and time and entity fixed effects could mitigate potential bias, however, the estimates could still endure omitted variable bias. As discussed in part 6.1.2, when instead basing the treatment on the share of female police officers, only crime statistics is provided

at the police district level. This results in the absence of appropriate controls to mitigate omitted variable bias. Given the limitations in using the share of female police officers as the treatment criteria, the results using the share of female police as treatment will mainly be used as a comparison to the main results based on the share of female politicians.

One could problematize this study’s external validity and the possibility of generalising the results to other countries implementing the Nordic Model. Even though the countries employ the same legislation rationale, there are fundamental differences between the countries which makes comparisons and generalizations questionable. As Östergren points out, “there is the morally and politically charged nature of sex work” (Östergren, 2020) which naturally alters from country to country, depending on the country in question’s political system, what party that has the political power, and the popular moral discourse of the general public. Added to this, these aspects may change over time, resulting in an even less comparable result of the effects of the prostitution legislation.

7 Results

This section will provide the empirical results obtained from the study. First, the results obtained using the dependent variable sexual offences and the dependent variable violence maltreatment on the county level will be presented. Second, results on the police district level will be presented. Lastly, the robustness of the results will be discussed.

7.1 Regressions

¹¹The BG-test refers to testing autocorrelation as will be further elaborated on in part 7.2.2 ; Note that the interaction term ‘treatmentXpost’ is the difference-in-differences estimator capturing the treatment effect

Table 4: ‘Sexual Offences’ and ‘Violence & Maltreatment’ based on share of female county members
11

| | <i>Dependent variable:</i> | |
|---------------------------|----------------------------|-------------------------|
| | Sexual Offences | Violence & Maltreatment |
| | (1) | (2) |
| post | −0.056 (0.045) | 0.079*** (0.025) |
| treatmentXpost | 0.032 (0.035) | −0.015 (0.019) |
| unemployment | 0.001 (0.022) | 0.005 (0.012) |
| education | 0.136*** (0.030) | 0.011 (0.017) |
| gini | −0.017*** (0.005) | −0.002 (0.003) |
| compensation | −0.022** (0.011) | 0.012* (0.006) |
| gfcf | 0.003*** (0.001) | 0.001* (0.0005) |
| Observations | 209 | 209 |
| R ² | 0.450 | 0.666 |
| Adjusted R ² | 0.374 | 0.621 |
| F Statistic (df = 7; 183) | 21.360*** | 52.216*** |
| BG-test (p-value) | 0.009 | 0.004 |

*p<0.1; **p<0.05; ***p<0.01

Data source: SSB.

Table 4 reports the coefficients, standard errors and p-values from regressions of the dependent variables ‘Sexual Offences’ and ‘Violence Maltreatment’.

The first regression (1) in table 4 shows the effect of the implementation of the prostitution legislation, based on the share of female county politicians, on ‘Sexual offences’ through the interaction term *treatmentXpost*. The variable *post* captures a statistically insignificant decrease of 5,6% in terms of reported sexual offences after the legislation was implemented. The difference-in-differences estimator, the positive interaction term capturing the treatment effect, of 3,2% is small with no statistical significance. This means that this regression does not establish an effect that is distinguishable from random variation in the sample, and hence no evidence is found to support mechanism 1 measured in this way.

The dependent variable ‘Violence and Maltreatment’, in the second regression (2) in table 4, reports a statistically significant increase in the *post* variable of 7,9% in terms of reported violence and maltreatment after the legislation was implemented. Simultaneously, it reports a difference-in-differences estimator, the negative interaction term capturing the treatment effect, of 1,5%. However this effect is also small without statistical significance. No evidence is found to support mechanism 2 measured in this way. When adjusting the R-squared for the amount of variables in the regressions, the adjusted R-squared ranges from 0,374 to 0,621 respectively, which implies that 37,4% respectively 62,1% of the sample variance of reported ‘Sexual Offences’ and ‘Violence Maltreatment’ is explained by the regressors. The higher R-squared and adjusted R-squared of the second regression compared to the first one indicates that the variation of the dependent variable ‘Violence Maltreatment’ is better explained by the independent variables in the regression model.

Table 5: Comparison of sexual offences based on share of female police officers and share of female county members

| | <i>Dependent variable: Sexual Offences</i> | |
|-------------------------|--|-----------------------------|
| | Share of female police officers | Share of female politicians |
| | (3) | (4) |
| post | 0.182*** (0.033) | 0.139*** (0.026) |
| treatmentXpost | 0.022 (0.046) | 0.041 (0.040) |
| Observations | 297 | 209 |
| R ² | 0.212 | 0.249 |
| Adjusted R ² | 0.129 | 0.169 |
| F Statistic | 36.005*** (df = 2; 268) | 31.104*** (df = 2; 188) |
| BG-test (p-value) | 0.001 | 0.001 |

*p<0.1; **p<0.05; ***p<0.01

Data source: SSB.

Table 5 presents the results of changing the treatment-level of the treatment and control group to the share of female police, with the dependent variable sexual offences. Hence, regression (3) and (4) show the effect of the implementation of the prostitution legislation on sexual offences with both ways of classifying the treatment groups. However, both regressions exclude controls ¹², which might lower the fraction of the sample variance of the dependent variable that is explained by the regressors, which thus explains the lower adjusted R-squared.

In table 5 regression (4), there is a significant increase of 13,9% in reported crimes of sexual offences after the reform was introduced, captured by *post*. Regression (4) in table 5 corresponds to regression (1) in table 4, including time and entity fixed

¹²see section 6.1.2

effects, but without controls. When comparing the variable *post* in these regressions, the sign switches from positive to negative and the variable is statistically insignificant as controls are added. However, when comparing the estimates of the variable *treatment \times post* across the two regressions, it is evident that the effect of implementation of the prostitution law is still small and insignificant despite how treatment and control groups are divided. Regression (3) and (4) report insignificant, positive, difference-in-differences estimators of 2,2% respectively 4,1%. Given that there is no data on police district level of violence and maltreatment against women, the dependent variable in table 5 is only sexual offences. Therefore, the regressions in table 5 refer to testing mechanism 1, and not 2.

Since the results do not differ significantly when altering the treatment criteria from the share of female politicians to police, as seen in regression (3) and (4), these regressions show that all the obtained results do not depend highly on what criteria the regions are divided into control and treatment groups based on.

7.2 Robustness check

To verify the robustness of the results, tests of the effect of altering the division into treatment groups, discussion about the standard errors and the assumptions of no autocorrelation and no problems with multicollinearity will be discussed.

7.2.1 Changing the threshold of treatment

In this paper, counties are assigned into either control or treatment groups based on if their share of female politicians or police is above or below a threshold. Cur-

rently, this threshold is at mean female representation, but Table 6 shows changing the threshold to the median, or based on removing the mid-25% counties from the sample as their share of female politicians fluctuate closely around the average to potentially get a higher magnitude of the treatment.

Table 6: Division into treatment group based on mean, median and top low 75%.

| | <i>Dependent variable: Sexual Offences</i> | | |
|-------------------------|--|-------------------------|-------------------------|
| | Mean (1) | Median (5) | Top Low 75% (6) |
| post | −0.056 (0.045) | −0.047 (0.047) | −0.108** (0.054) |
| treatmentXpost | 0.032 (0.035) | 0.011 (0.035) | 0.050 (0.041) |
| unemployment | 0.001 (0.022) | 0.0004 (0.022) | 0.032 (0.022) |
| education | 0.136*** (0.030) | 0.137*** (0.030) | 0.116*** (0.020) |
| gini | −0.017*** (0.005) | −0.017*** (0.005) | −0.012** (0.006) |
| compensation | −0.022** (0.011) | −0.023** (0.011) | −0.002 (0.002) |
| gfcf | 0.003*** (0.001) | 0.003*** (0.001) | 0.0004 (0.001) |
| Observations | 209 | 209 | 154 |
| R ² | 0.450 | 0.447 | 0.469 |
| Adjusted R ² | 0.374 | 0.372 | 0.389 |
| F Statistic | 21.360*** (df = 7; 183) | 21.166*** (df = 7; 183) | 16.750*** (df = 7; 133) |

*p<0.1; **p<0.05; ***p<0.01

Data source: SSB.

As seen in table 6 the results do not alter much depending on how one changes the threshold of treatment. This provides an overall indication that the results do not depend highly on the division of the counties into treatment and control groups. Therefore, these regressions serve as an overall robustness check to the results.

7.2.2 Testing the assumptions

The Ordinary least squares (OLS) estimator, is based on several assumptions that ensure estimators remain unbiased. When n is large, and the least squares assumptions for panel data hold, the fixed effects estimator is consistent and is normally distributed. Therefore, the assumptions will be addressed to ensure that the regressions in this paper are correct. Firstly, OLS assumes exogeneity, which in part 6.2.2 is considered to hold. Secondly, OLS assume the absence of a heteroscedastic error term, meaning that the variance of the conditional distribution of ui given Xi , $var(ui | Xi = x)$ is constant for $i = 1 \dots n$, for the OLS estimators to remain efficient and unbiased. Therefore, all regressions in this study employ robust standard errors to account for potential heteroscedasticity of the errors (Stock and Watson, 2019). Also, no perfect multicollinearity is also assumed in fixed effects regressions, which arises when one regressor is a perfect linear combination of the others. As seen in Appendix A, there is no perfect multicollinearity; thus, this assumption holds.

Furthermore, another assumption is that the variables belonging to one entity are distributed identically to, and independently of, the variables belonging to another entity. In the Norwegian case, this would imply that variables are independent, i.e. not correlated, across counties. However, in panel data variables do not have to be independent within an entity. The potential presence of autocorrelation, namely that ui is correlated over time within an entity in a time series does not affect the

consistency or introduce bias of the OLS estimators. But, in general, the standard errors are inconsistent since the variance of the fixed effects estimator is affected (Stock and Watson, 2019). Therefore, using clustered standard errors could be a more appropriate method of computing standard errors with panel data, since it accounts for heteroscedasticity and is autocorrelation-robust, but assumes that the regression errors are uncorrelated across clusters (Stock and Watson, 2019). In this study, the appropriate clustering would be at the county level, but there has to be a large number of clusters to avoid “small sample problems” (Abadie et al., 2023), and one can therefore argue that the 19 counties in this study is too few to use clustered standard errors. For example Moody (2020) finds stabilized values after 40 clusters. Hence, given the low number of clusters at hand, the most conservative errors to use in this study are heteroscedastic ones.

Since clustered standard errors would remedy potential autocorrelation, but can not be applied in this case, further tests of the potential autocorrelation have been made. The Breusch-Godfrey (BG) test for serial correlation in panel models tests the alternative hypothesis of serial correlation in the idiosyncratic errors, and rejects the null if the p-value is below 0,05 (5%). As seen in Table 4 and 5 the BG tests reject the null hypotheses ($p\text{-value} < 0,05$) which implies absence of autocorrelation in the idiosyncratic errors across all regressions.

8 Discussion

For the discussion, let us return to the research questions of this paper:

- Did the Norwegian prostitution law result in any spillover effects related to the proposed mechanisms?

- Based on the results, what can be concluded about the proposed mechanisms?
- How do the results relate to similar Swedish studies, and what does this say about the Nordic model of prostitution legislation in general?

8.1 Interpretation of the results

This study set out to establish the spillover effects of the Norwegian prostitution law by testing the three proposed mechanisms. The first mechanism proposed customers with inelastic demand for sexual services find other ways to fulfil their needs, increasing sexual offences. The second mechanism suggested that violence against women increases, since sex workers are more legally protected and prostitutes may report more violent crimes, and frustrated former customers resort to violence, especially domestically. Finally, the last mechanism proposed that a shift in attitudes would result in ambiguous results, and possibly a decrease, in both sexual offences and violence against women.

The result from the regression using the share of female politicians as the level of treatment, showed a 3,2% positive estimate of sexual offences as a result of the regulation. The small increase in effect in this context and the non-significant result mean that it can not establish enough evidence to support mechanism 1 measured in this way. Furthermore, when altering the criteria from share of female politicians to police, the coefficient still remains small, around 2%, and insignificant. The variable violence and maltreatment also showed a small interaction effect of the impact of the regulation, with a 1,5% negative, insignificant coefficient. Even though the variable is not disaggregated enough to capture only the effect in violence against prostitutes or domestic violence, no evidence is found to support mechanism 2 measured in this way. Given the insignificant results, no indication can be found on whether mech-

anism 3 is the driving force behind the effects from the prostitution legislation or not.

All in all, from this study, one can not establish if there is a causal spillover effect from implementing the prostitution legislation. This leads to the question:

Why is it this way? Either there is an effect, that can not be established, or there is no effect. The following parts of the discussion will elaborate on the “why” of the results.

8.2 There is an actual effect

Although the study can not establish a statistically significant causal spillover effect from implementing the prostitution legislation, there may still be an undiscovered actual effect. There are possibly several reasons why this “true” causal effect is concealed in this study, but this section will elaborate on three potential reasons. Firstly, the study may be subject to limitations that result in vague and imprecise results. The data limitations discussed in section 6.2 might end up hazing the relationship between implementing the legislation and the effects on sexual offences and violence against women, and thereby concealing the true, actual, effect. For example, estimates are less precise since crimes are displayed on a too aggregated level, where filtering on gender, type of perpetrator and type of crime is not possible.

Secondly, the nature of the crime data is complex, especially when assessing a smaller area like counties. An increase in reported crimes during a year may deceive since it can easily be misinterpreted as violence has generally become more widespread in that particular year, when in fact it may be a single perpetrator who has committed many crimes against the same person over multiple years (BRÅ, n.d.) Overall, it could be beneficial to complement traditional crime statistics on county level with

qualitative data to establish the true total effect of the crime.

Thirdly, as previously elaborated in section 6.2, the yearly data might mask short-term effects of the legislation. Even though the study finds no overall significant causal effect of the legislation on violent crimes against women, there could be a short-term effect of the legislation. Imposing a prostitution legislation might have an effect in the short-term which could support any of the three mechanisms, however this would not be captured in this study with yearly data. For example, Perrotta Berlin et al. (2019) finds an effect of the Swedish prostitution legislation lasting three months. Also, Jakobsson and Kotsadam (2011b), find a short-term shift in attitudes after the prostitution legislation in Norway. All in all, there might be a significant short-run increase in sexual offences and violence against women due to the legislation, but shielded in the yearly data.

All in all, there is a possibility that there is an actual effect that can support all three mechanisms, but this study can not establish if and which due to the above reasoning.

8.3 There is no actual effect

Another possible reason this study does not arrive at a causal spillover effect from implementing the prostitution legislation is that there is no significant causal relationship in Norway. There may be correlations between implementing a prostitution legislation and societal spillover effects, but no causality. There may be other societal factors (e.g. culture and gender equality) that are stronger drivers and explanatory variables to an increase or decrease in sexual offences and violence against women. It might be that the prostitution legislation does not affect unintended variables in

violence against women and sexual offences, and mainly affects the intended target group. There possibly being no actual effect, in conjunction with the study's limitations, might therefore result in the study's almost zero, insignificant coefficients. If this is the case, and there is no actual effect, mechanism 2 is not the probable mechanism since violence against women would increase. Furthermore, mechanism 1 is not probable, as it would imply an increase in rape due to the inelastic demand of sex buyers. Rather, the demand might be elastic, which in turn supports mechanism 3. The cost of engaging in prostitution is now larger due to a shift in general attitudes, which may result in a change in behaviour rather than a change in sexual offences and violence against women.

8.4 There are counteractive effects

However, according to the discussion regarding mechanism 3, the legislation might have ambiguous and counteractive effects ultimately resulting in no single, one-way, actual effect. For example, changed attitudes might set new standards for reporting violent crimes against women, which increases reported sexual offences and violence. At the same time, altered attitudes leading to a shift in behaviour of the perpetrators, may result in less true total sexual offences, and in turn less reporting. These two scenarios may cancel each other out in the aggregated reported number of sexual offences, and hence result in, what looks like, no effect of the legislation. Lastly, it might also be that all three mechanisms occur simultaneously and with different magnitudes in different counties. This would likely result in a mixed, insignificant and ambiguous ultimate result and a clear causal spillover effect can not be established measured in this way.

8.5 Implications to the Nordic Model

From the discussion above, important insights and implications for the Nordic Model as a framework in prostitution legislation can be drawn.

Perrotta Berlin et al.'s (2019) study on the Swedish prostitution legislation finds a statistically significant increase in domestic violence due to the Nordic model and Ciacci (2020) finds an increase in reported rape. This study in Norway could not establish the same statistically significant increase in domestic violence or sexual offences. This brings the study to conclude that implementing the same legislation can have different effects and that even if countries employ the same legislation rationale, the fundamental differences between the countries make comparison and generalizations questionable (Östergren, 2020). Comparing the effects of the regulation is hard as the cultures, conditions and reasons for implementing the regulation differ. For example, one important reason behind the Norwegian implementation of the prostitution legislation was the high level of sex-tourism which was estimated to have increased since the implementation of a prostitution legislation in neighbouring Sweden 1999, and thus it is reasonable that the prostitution legislation will have different effects in Norway compared to the one in Sweden, as the rationale behind implementing the law differs. Also, the countries' specific prerequisites regarding data collection and the crime statistic's standard further complicates a straightforward comparison.

Also, given the proximity between the Norwegian and Swedish borders, cultures and history imply that the countries' normative frameworks influence each other. Furthermore, the prostitution market is international, and thus reaching across borders,

and therefore a change in the prostitution legislation in one country has effects on other countries sharing the same prostitution market. Conversely, if Norway and Sweden were two separate islands implementing the law simultaneously, the effects could have been more similar. But, as the countries influence each other and since Sweden implemented the law before Norway, mechanisms 1, 2 and 3 could already be influenced in Norway before the Norwegian government put the legislation in place, so a direct comparison is difficult. Furthermore, as McAdams and Rasmusen (2000) explain, norms play an important role in how regulation affects behaviour. A lot can have happened to general Nordic attitudes towards prostitution between when Sweden implemented the Nordic model in 1999, and Norway implementing it 10 years later. As Kotsadam and Jakobsson (2011a) conclude in their study, Swedish and Norwegian prostitution attitudes are influenced by political views, where liberals are more positive towards prostitution, and vice versa with conservatives. It is not unreasonable to assume that the dynamic nature of popular political beliefs differs between Sweden 1999 and Norway 2009. This further makes the years after implementation in Sweden and Norway even less comparable given their different starting points in attitudes and political views to begin with. This reasoning about how national differences in time aspects, and the normative starting points steer the outcome of the legislation, can be generalized to the overall evaluation of the Nordic model. Is it really reasonable to draw conclusions about the Nordic model when timing, prerequisites and normative framework differ substantially?

9 Conclusion

Prostitution is a broad and worldwide market with a multibillion turnover employing millions each year, however the industry is highly debated due to the sensitive moral aspects of selling sexual services. As the prostitution market often involves violence

and violates human rights, the market is considered dangerous to the sex workers and exacerbates gender inequality. This has resulted in a debate on whether, and in that case, how, the market should be regulated. In 2009, Norway implemented a new regulation, according to the Nordic model approach to prostitution legislation, where sex buyers are criminalized while prostitutes are decriminalized. Previous Swedish research on the Nordic Model of prostitution showed spillover effects from the legislation in terms of increased domestic violence against women as well as an increase in rape. Therefore this paper set out to establish if there are also spillover effects in the Norwegian case, and if any, what mechanism is driving them.

This paper found no significant results to support or reject any spillover effects from the prostitution regulation. This implies that we could not support any of the proposed mechanisms; that the legislation resulted in increase in sexual offences due to previous customers' inelastic demand, that the regulation resulted in an increase in domestic violence due to former customers becoming frustrated and resorting to violence, or that the regulation resulted in an ambiguous or negative change in both sexual offences and violence against women due to a shift in societal attitudes. The study discusses different arguments for the inconclusive results, including the study's limitations that hide an actual effect, the potential of there being no spillover effects, or that the effect is ambiguous due to a complex reality where all the mechanisms interplay and counteract each other. This has implications for the discussion of the Nordic Model of prostitution legislation. It may be that the spillovers are more sensitive to timing, national prerequisites and normative framework than anticipated, and hence directly comparing between countries is not qualified.

Given the many questions about the true societal effects of the prostitution legisla-

tion, and its potential spillover effects, this calls for further research to add to this debate and to establish a causal relationship. Therefore, three recommendations for further research will be presented. First, using the same method and variables to test the spillover effects on another, third, country would provide the possibility to further evaluate the Nordic Model as a framework in prostitution legislation. By choosing a country implementing the regulation in close proximity, both in time, prerequisites and normative framework, such as Iceland who also implemented the Nordic model 2009, the results could potentially be more comparable. Second, another recommendation for further research would be to further investigate the effects of the prostitution legislation in Norway, but from another angle to avoid the data limitations in this study. This could be done by using another dependent variable, such as perhaps Google searches that capture women reaching out to female helplines as a proxy for domestic violence. Lastly, this study highlighted the potential attitude change due to the prostitution legislation. Thus further studies where data on attitudes that complement crime data, would be encouraged. This provides the opportunity to understand whether there is a change in attitudes, and the unobserved levels of crimes committed, which is not reflected in reporting rates.

Altogether, this paper concludes with the hopes of inspiring further research into the prostitution legislation field, and with great anticipation that this important and evidently relevant policy issue, and its spillover effects, will be further explored.

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Appendix

Appendix A

Table 1: Correlation matrix Sexual Offences using politicians as treatment-level

| | post | treatment | treatmentXpost | unemployment | education | gini | compensation | gfcf |
|----------------|-------|-----------|----------------|--------------|-----------|-------|--------------|-------|
| post | 1.00 | -0.01 | -0.02 | -0.13 | 0.03 | -0.01 | 0.01 | 0.03 |
| treatment | -0.01 | 1.00 | 0.29 | -0.16 | -0.13 | -0.14 | -0.03 | 0.03 |
| treatmentXpost | -0.02 | 0.29 | 1.00 | -0.04 | -0.05 | -0.04 | -0.03 | 0.02 |
| unemployment | -0.13 | -0.16 | -0.04 | 1.00 | 0.11 | 0.10 | 0.06 | -0.12 |
| education | 0.03 | -0.13 | -0.05 | 0.11 | 1.00 | 0.11 | 0.02 | 0.08 |
| gini | -0.01 | -0.14 | -0.04 | 0.10 | 0.11 | 1.00 | 0.03 | -0.01 |
| compensation | 0.01 | -0.03 | -0.03 | 0.06 | 0.02 | 0.03 | 1.00 | -0.01 |
| gfcf | 0.03 | 0.03 | 0.02 | -0.12 | 0.08 | -0.01 | -0.01 | 1.00 |

Table 2: Correlation matrix Violence Maltreatment using politicians as treatment-level

| | post | treatment | treatmentXpost | unemployment | education | gini | compensation | gfcf |
|----------------|-------|-----------|----------------|--------------|-----------|-------|--------------|-------|
| post | 1.00 | -0.00 | 0.50 | -0.06 | 0.36 | -0.24 | 0.18 | 0.17 |
| treatment | -0.00 | 1.00 | 0.64 | 0.12 | 0.24 | 0.09 | 0.24 | 0.26 |
| treatmentXpost | 0.50 | 0.64 | 1.00 | 0.04 | 0.33 | -0.06 | 0.26 | 0.27 |
| unemployment | -0.06 | 0.12 | 0.04 | 1.00 | -0.20 | 0.27 | -0.08 | -0.22 |
| education | 0.36 | 0.24 | 0.33 | -0.20 | 1.00 | -0.04 | 0.21 | 0.62 |
| gini | -0.24 | 0.09 | -0.06 | 0.27 | -0.04 | 1.00 | 0.62 | 0.23 |
| compensation | 0.18 | 0.24 | 0.26 | -0.08 | 0.21 | 0.62 | 1.00 | 0.50 |
| gfcf | 0.17 | 0.26 | 0.27 | -0.22 | 0.62 | 0.23 | 0.50 | 1.00 |

Table 3: Correlation matrix Sexual Offences using police as treatment-level

| | post | treatment | treatmentXpost |
|----------------|------|-----------|----------------|
| post | 1.00 | 0.00 | 0.57 |
| treatment | 0.00 | 1.00 | 0.61 |
| treatmentXpost | 0.57 | 0.61 | 1.00 |