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# Do laws affect attitudes?

An assessment of the Norwegian prostitution law using longitudinal data

Niklas Jakobsson and Andreas Kotsadam\*

May, 2010

## Abstract

The question of whether laws affect attitudes has inspired scholars across many disciplines, but empirical knowledge is sparse. Using longitudinal survey data from Norway and Sweden, collected before and after the implementation of a Norwegian law criminalizing the purchase of sexual services, we assess the short-run effects on attitudes using a difference-in-differences approach. In the general population, the law did not affect moral attitudes toward prostitution. However, in the Norwegian capital, where prostitution was more visible before the reform, the law made people more negative toward buying sex. This supports the claim that proximity and visibility are important factors for the internalization of legal norms.

*Keywords:* attitudes, norms, law, prostitution

*JEL classification:* K14, K40

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## 1. Introduction

In January 2009, buying sex became a criminal offense in Norway. One of the main aims of the law was to make people more negative toward buying sex (Holmström and Skilbrei 2008; Norwegian Ministry of Justice 2008; and Skilbrei 2008). In the present paper, we investigate whether it succeeded. That citizens internalize the values signaled by laws is a common argument (e.g., McAdams 2000; McAdams and Rasmusen 2007). There is, however, an explicitly acknowledged lack of studies on the causal relationship between laws and attitudes (e.g., Ellickson 2001; McAdams 2000).<sup>1</sup>

Norms as a means of explaining individual behavior has gained increasing focus in the economics literature (e.g., Akerlof 1980; Binmore and Samuelson 1994; Becker 1996), and the claim that people internalize societal norms and laws is widely accepted (Tyler 1990; McAdams and Rasmusen 2006; Cooter 2008). More recent contributions model the interactive process between attitudes and laws (e.g., Carbonara et al. 2008), while others try to identify the effect of institutions and policies on attitudes empirically (Alesina and Fuchs-Schündeln 2007; Fong et al. 2006; Soss and Schram 2007; and Svallfors 2009).

Alesina and Fuchs-Schündeln (2007) investigate whether individual policy preferences are endogenous to political regimes and use post-war Germany to analyze the effects of communism on people's preferences regarding market capitalism and the role of the state in providing social services. Using the German Socioeconomic Panel, they find a large and statistically significant effect of former East Germans being more positive toward state intervention. Svallfors (2009) also investigates the role of institutions on the formation of values using the German natural experiment and, similarly, finds that mass publics are affected by institutional design. Soss and Schram (2007) investigate whether public opinion shifted as a result of welfare reform in the US in the 1990s. Using cross-sectional survey data, they find few opinion changes. They argue that the reforms did not affect mass opinion since they were distant to most people. Several studies try to assess the effect of smoke-free laws on attitudes (e.g., Heloma and Jakkola 2003; Tang et al. 2003; Gallus et al. 2006), but since most of them use cross-sectional data without control groups, they can not identify

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<sup>1</sup> How laws affect behavior is studied to a larger extent (see, e.g., Donohue and Levitt (2001), Levine and Staiger (2004), Lott (2001), and Mocan (2006)).

causal effects. An important exception is Fong et al. (2006), who study the effects of an Irish smoke-free law on attitudes using longitudinal data with UK residents as control group. They find clear increases in support for total bans among smokers.

In the present study, we explore the effect of the Norwegian criminalization of buying sex on attitudes toward prostitution using longitudinal survey data from Norway and Sweden. These countries are very similar neighboring Scandinavian welfare states with similar languages and institutions (Esping-Andersen 1990; 1999). They are also similar in other respects. For example, the Global Gender Gap Report 2009 (Hausmann et al., 2009) ranks Norway and Sweden as the third and fourth most gender equal countries in the world, respectively. During the investigated period, Norway, but not Sweden, changed its legal framework surrounding prostitution. This allows us to evaluate the effects of the law using a difference-in-differences methodology, comparing changes in attitudes between the two countries. Apart from issues linked directly to prostitution, the data contains information on age, gender, income, cohabitation status, education, region of residence, and attitudes on issues linked to equality between the sexes, immigration, sexual liberalism, religious activities, and political views.

Our study has several advantages compared to previous studies. First of all, we use individual-level longitudinal data collected before and after the passing of a law, while Soss and Schram (2007) do not have longitudinal data and neither Svallfors (2009) nor Alesina and Fuchs-Schündeln (2007) have data on the East German population before reunification. We also have a control group, as opposed to Soss and Schram (2007), allowing us to compare the changes in attitudes among individuals in a country where there has been a change in the law (Norway) to the changes in attitudes among individuals in a similar country without such a change during the period (Sweden). These two factors in principle facilitate identification of causality. Compared to Alesina and Fuchs-Schündeln (2007) and Svallfors (2009), who study the effects of regimes on attitudes, we assess the effect of a specific law on attitudes. The results the present paper thereby have more practical relevance for policymakers interested in norm entrepreneurship. As opposed to Fong et al. (2006), who look at smokers' attitudes before and after the implementation of a smoke-free law, we study the effect of laws on attitudes in the general population and in groups that are more directly

affected by the law. This enables us to investigate the role of the context in which a reform is introduced.

When comparing changes in attitudes between the two countries, we find that criminalizing buying sex in Norway did not have large short-term effects on people's attitudes in general. More exactly, it did not affect moral attitudes toward buying and selling sex and it did not make Norwegians, as compared to Swedes, more likely to want buying sex to be illegal, although it did make them more likely to want selling sex to be illegal. The summary statistics reveal, however, that Norwegians think it should be illegal to sell sex to a lesser extent after the implementation of the law than before. Our results are thus driven by driven by Swedes having changed even more into thinking selling sex should not be illegal.

However, for respondents living in Oslo (the Norwegian capital), where the sex trade was clearly visible before the reform, there were clear effects on attitudes toward prostitution: People in Oslo now think that it should be illegal to buy sex to a larger extent than before the law. This supports the claim of proximity; that attitudes should be affected most for those most affected by a law. We also find that young people generally were more inclined than older people to change their views following a legal change. Finally, we find no support for the hypothesis that those who trust politicians more change their attitudes more in line with lawmakers' intentions when there is a legal change.

In order to generalize the results, a few caveats are necessary, especially since we might underestimate the effects of legal change on attitudes for several reasons. First of all, it is likely that laws affect attitudes more over longer time periods. It is therefore important to keep in mind that the results of this paper concern the short-run effects of laws on attitudes. Also, since we are unable to distinguish between any "direct effect" of the law and the effect attained via the media debate, a related issue is that the media discussion had started before the first wave of the survey was distributed. In addition, it was at this point clear that the law would be implemented. Both these factors are likely to underestimate the effects of the law reported in this paper.

The remainder of the paper is organized as follows. Section 2 presents our hypotheses, Section 3 describes the data and descriptive statistics, and Section 4 describes the empirical framework. Section 5 presents the results and Section 6 concludes the paper.

## 2. Hypotheses

As mentioned in the introduction, there is a large literature in different disciplines of social science stipulating theoretical effects of laws on attitudes. In this section, we will briefly describe the theoretical arguments in favor of a general effect and then move on to more specific hypotheses.

Why would laws affect attitudes? A common argument is that once institutions are in place, they create feedback effects, including normative feedback. Normative feedback effects are likely to arise when public policies provide citizens with a sense of what is desirable (Svallfors 2009). The enactment of laws is a means by which policymakers are able to signal “good” values, and this expressive function of law is argued to be most common in criminal law (McAdams 2000; McAdams and Rasmusen 2007). The values may be internalized by the citizens for a number of reasons. McAdams and Rasmusen (2007) argue that new laws may affect the incentives that underlie norms by changing perceptions of what incurs disapproval or by creating a new basis for shame<sup>2</sup>. According to Cooter (2008), people internalize values signaled by laws in order to increase their cooperation opportunities, especially in long-run projects. Also Posner (1998; 2000) argues that people internalize norms to signal that they are of “good type.” McAdams (2000) argues that laws may change behavior by signaling underlying attitudes in society to individuals concerned with approval. In such cases, a law helps people update their prior beliefs by creating a focal point (Cooter 1998). However, the direction of the possible attitudinal change does not necessarily follow the signals sent out by the legislature. Social response theory highlights how the reaction to a law can either reinforce or undermine its effect (Carbonara et al. 2008). In the present paper, we first test the hypothesis that laws affect attitudes.

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<sup>2</sup> How shame may affect criminal behavior is discussed by Kahn and Posner (1999).

Yet, laws may affect people differently depending on the context in which they are introduced. Soss and Schram (2007) discuss under which conditions laws and policies can be assumed to affect attitudes. A high degree of societal visibility and proximity (i.e., the degree to which individuals notice and become directly affected by the policy) makes attitudinal change more likely. The criminalization of buying sex in Norway was a highly visible reform in the sense that the media coverage was extensive (Jahnsen 2008). Thus, there was a higher likelihood that the reform would affect attitudes than if it had not been as visible. Turning to proximity, most Norwegians are not affected directly by the law. This implies that it should not affect people's attitudes as much as it would have had the law affected them more directly. People living in Oslo, however, were more proximate to prostitution and thereby to the effects of the law. To them, prostitution was a clearly visible phenomenon before the enactment of the law (Skilbrei 2001) but has since then become much less noticeable (Strøm 2009). Thus, we expect the change in attitudes to be larger in Oslo than in the rest of the country.

The effects of laws on attitudes seem to be linked to other factors as well. Trust in politicians is argued to be important for internalization of legal norms (McAdams 2000; Ellickson 2001; McAdams and Rasmusen 2007), which is also a common argument among scholars of legal philosophy (e.g., Cserne 2004) and political science (e.g., Peters 2005). As argued by Ellickson (2001), some people may feel that the government has better and more accurate information and may therefore internalize legal norms. These arguments imply that people who trust politicians should be more inclined than people who do not trust politicians to change their attitudes in accordance with legal changes.

The effects of laws on attitudes may also differ by age and across cohorts. Svallfors (2009) argues that people whose life course transition into adult life has already been fully accomplished should be more resistant to attitudinal change. Similarly, young people are expected to adapt quicker to new rules since they have fewer previous formative experiences that need to be reconsidered (Svallfors 2009). Thus, we expect the change in attitudes to be larger among younger persons. The hypotheses to be tested in this paper are summarized below:

- The criminalization of buying sex affects attitudes toward prostitution.

- The effect of the law is greater in the area where the effects of the reform were most proximate, i.e., in Oslo.
- People who trust politicians are more inclined to change their attitudes in accordance with a legal change.
- Younger persons are more inclined to change their attitudes in accordance with a legal change.

### 3. Data and descriptive statistics

We conducted a longitudinal Internet-based survey sent out by TNS Gallup ([www.tns-gallup.se/summary.aspx](http://www.tns-gallup.se/summary.aspx)) in August 2008 and August 2009 to a random sample of 2,500 Norwegians and 3,000 Swedes aged 15-65. By the end of the second survey period, 1,034 Norwegians (41.4 percent) and 1,317 Swedes (43.9 percent) had responded to both surveys. The response rate in the first wave was 68.6 percent in Norway and 60.5 percent in Sweden. The respondents had three weeks to answer the first wave of the survey, and they received two reminders. Those who accepted also taking part in the second wave of the survey (in August 2009) had three weeks to answer, and received four reminders.<sup>3</sup>

The survey included four main questions on people's attitudes toward prostitution. More exactly, the respondents were asked whether they felt that it is morally acceptable or morally unacceptable to buy sex and sell sex, respectively. They responded on a 0-10 scale, where 0 implied "morally acceptable" and 10 implied "morally unacceptable." The respondents were also asked whether they thought it should be illegal to buy sex and sell sex, respectively; here the possible answers were *yes* and *no*. In addition to these questions, we asked for the respondents' attitudes on issues linked to equality between the sexes, immigration, sexual liberalism, religious activities, political views, their knowledge about the law, and their trust in politicians. We also have information on the respondents' age, gender, income, cohabitation status, education, and region of residence, but only for the first wave. The choice of control variables follows Jakobsson and Kotsadam (2010a), who investigate what determines attitudes toward prostitution.

Descriptive statistics are presented in Table 1. Regarding the dependent variables (Selling wrong, Buying wrong, Illegal selling, and Illegal buying), we see that Swedes are significantly

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<sup>3</sup> For more information on the data, see Jakobsson and Kotsadam (2010a and 2010b).



more negative toward prostitution. They think it is more morally wrong both to buy and to sell sex and they are more inclined than Norwegians to think that both buying and selling sex should be illegal. Looking at the statistically significant trends over time, we see that respondents in both countries showed less moral concern with respect to selling sex in the second than in the first survey, and Swedes felt that selling sex should be illegal to a lesser degree than one year earlier.

Table 1. Descriptive statistics.

Variable	Explanation	Norway		Sweden	
		Wave 1	Wave 2	Wave 1	Wave 2
<b>Selling wrong</b>	Answer to the question “ <i>In your opinion, is it morally acceptable or morally unacceptable to sell sex?</i> ” ranging from 0 for <i>Totally morally acceptable</i> to 10 for <i>Totally morally unacceptable</i> .	6.269 (3.170)	6.117 (3.085)	6.728 (3.158)	6.540 (3.107)
<b>Buying wrong</b>	Answer to the question “ <i>In your opinion, is it morally acceptable or morally unacceptable to buy sex?</i> ” ranging from 0 for <i>Totally morally acceptable</i> to 10 for <i>Totally morally unacceptable</i> .	6.822 (3.132)	6.770 (3.088)	7.403 (2.986)	7.439 (2.903)
<b>Illegal selling</b>	= 1 if respondent thinks it should be illegal to sell sex	0.466 (0.499)	0.456 (0.498)	0.551 (0.498)	0.510 (0.500)
<b>Illegal buying</b>	= 1 if respondent thinks it should be illegal to buy sex	0.518 (0.500)	0.522 (0.500)	0.632 (0.482)	0.618 (0.486)
<b>Male</b>	= 1 if respondent is male	0.457 (0.498)		0.497 (0.500)	
<b>Age</b>	respondent age	37.525 (13.458)		42.403 (13.928)	
<b>Capital</b>	= 1 if respondent lives in the capital city	0.122 (0.327)		0.199 (0.400)	
<b>Cohabit</b>	= 1 if respondent is married or cohabiting	0.655 (0.476)		0.673 (0.4694)	
<b>High education</b>	= 1 if respondent has at least some university education	0.529 (0.499)		0.457 (0.498)	
<b>Low education</b>	= 1 if respondent only has elementary education or less	0.080 (0.272)		0.164 (0.370)	
<b>High income</b>	= 1 if respondent earns >45,000 SEK per month, or >600,000 NOK per year.	0.077 (0.267)		0.032 (0.177)	
<b>Low income</b>	= 1 if respondent earns <20,000 SEK per month, or <200,000 NOK per year.	0.245 (0.430)		0.385 (0.487)	
<b>Religious</b>	= 1 if respondent participates in religious activities at least once a month.	0.098 (0.297)	0.090 (0.286)	0.080 (0.271)	0.068 (0.251)
<b>Trust</b>	Answer to the question “ <i>In general, do you trust politicians?</i> ” ranging from 0 for <i>Not at all</i> to 10 for <i>Very much</i> .	4.322 (2.032)	4.652 (2.039)	4.579 (2.025)	4.972 (2.026)
<b>Anti immigration</b>	Answer to the question “ <i>Do you think that there are too many foreigners in Norway/Sweden?</i> ” ranging from 0 for <i>No, not at all</i> to 10 for <i>Yes, for sure</i> .	3.610 (2.755)	3.277 (2.728)	4.544 (2.852)	4.426 (2.835)
<b>Public sector</b>	Answer to the question “ <i>How large should the public sector be?</i> ” ranging from 0 for <i>Much smaller than today</i> to 10 for <i>Much larger than today</i> .	4.730 (1.775)	4.775 (1.675)	5.244 (1.769)	5.347 (1.746)
<b>Gender equality</b>	Answer to the question “ <i>Do you think that gender equality is an important issue?</i> ” ranging from 0 for <i>No, not at all</i> to 10 for <i>Yes, for sure</i> .	8.368 (2.138)	8.617 (1.983)	8.879 (1.905)	8.926 (1.848)
<b>Co-responsible if abused</b>	Answer to the question “ <i>Do you think women who dress challengingly are co-responsible if they become sexually abused?</i> ” ranging from 0 for <i>No, not at all</i> to 10 for <i>Yes, for sure</i> .	2.050 (2.753)	2.173 (2.843)	1.764 (2.679)	1.757 (2.678)
<b>Sexual liberal</b>	Answer to the question “ <i>Do you think it is okay to have sex with unknown people?</i> ” ranging from 0 for <i>No, not at all</i> to 10 for <i>Yes, for sure</i> .	4.838 (3.445)	5.000 (3.413)	5.975 (3.559)	6.044 (3.492)
<b>Know 1</b>	= 1 if Swedish respondent answers yes “ <i>To your knowledge, is it illegal to buy sex?</i> ”, and no to “ <i>To your knowledge, is it illegal to sell sex?</i> ” in the first wave of the survey. Or if Norwegian respondent answers no to “ <i>To your knowledge, is it illegal to buy sex?</i> ” and no to “ <i>To your knowledge, is it illegal to sell sex?</i> ” in the first wave of the survey	0.428 (0.495)		0.624 (0.485)	
<b>Know 2</b>	= 1 if respondent answers yes to “ <i>To your knowledge, is it illegal to buy sex?</i> ” and no to “ <i>To your knowledge, is it illegal to sell sex?</i> ” in the second wave of the survey.		0.588 (0.492)		0.671 (0.470)

Mean values presented; standard deviation in parentheses.

To assess the representativeness of our sample, we compare the descriptive statistics of the respondents to national statistics. In Sweden, 50.8 percent of the population are men, which corresponds well with our Swedish sample where 49.7 percent are men. However, only 45.7 percent of the Norwegian respondents are men, while the share of all Norwegians is 50.9 percent. The mean ages among 15-65 year olds are 40.1 in Sweden and 39.7 in Norway, while in our samples the mean ages are 43.4 and 38.5 years, respectively (Statistics Sweden 2008a; Statistics Norway 2008). What is more problematic is the representativeness of our sample with respect to education: While the share of Swedes aged 16-65 who have higher education is 31.8 percent, the share in our sample is 45.3 percent (Statistics Sweden 2008b). For Norway, the percentages differ even more: 27.0 percent of all Norwegians aged 16-66 have higher education, while the corresponding figure in our sample is 56.7 percent (Statistics Norway 2008). Furthermore, the bias toward including highly educated people is linked to non-random attrition, especially in Norway. In the first wave, 43.4 percent of the Swedes and 48.8 percent of the Norwegians had university education. We conclude that our sample is fairly representative regarding gender and age while in terms of education it is biased toward including highly educated people, and there are serious concerns regarding non-random attrition. While this should be considered when comparing raw correlations and mean values, the problem is somewhat alleviated in the regression analyses by explicitly controlling for education and other confounding factors. Furthermore, even though initial attitudes in our sample may not be representative for the whole population, the change in attitudes may be representative, and we can in fact test whether education affects attitude change.

#### **4. Empirical framework**

Since we have individual level panel data from both Norway (where the law changed during the period) and Sweden (where there was no legal change), we are able to apply a difference-in-differences method. The average difference over time in the control group is subtracted from the average difference over time in the treatment group. However, since the assignment of subjects to the two groups was not randomized, further assumptions must be made in order to establish causality.

Norway and Sweden are very similar neighboring Scandinavian welfare states with similar languages and institutions (Esping-Andersen 1990; 1999). They are also similar in other respects. For example, the Global Gender Gap Report 2009 (Hausmann et al., 2009) ranks Norway and Sweden as the third and fourth most gender equal country in the world, respectively. Since the countries are very similar, a reasonable assumption is that attitudes in the countries evolve in a similar way. Therefore, we make the identifying assumption that, conditional on the observed individual characteristics, the change in average attitudes of Norwegians (who did experience a legal change during the investigated period) would have been the same without the new law as the change in average attitudes during the same period in Sweden (where no such new law was implemented). Under this identifying assumption, we can evaluate the causal impact of the reform. However, if the change in attitudes would have been different in the two countries in the absence of the Norwegian criminalization, the identifying assumption is problematic. Since we do not have more than one wave of data from before the implementation of the law, we cannot test this assumption, so care should be taken when making inferences. The identifying assumption is further problematized in the concluding discussion.

We estimate the following specification:

$$Y_{it} - Y_{i0} = \beta_0 + \beta_1 N_i + \beta_2 \mathbf{Z}_{i0} + \beta_3 (\mathbf{X}_{it} - \mathbf{X}_{i0}) + \varepsilon_i, \quad (1)$$

where  $Y_{it}$  is the moral attitude toward buying/selling sex (ranging from 0 for “morally acceptable” to 10 for “morally unacceptable”) or attitude toward criminalization (taking the value one if the respondent thinks buying/selling sex should be illegal) for individual  $i$  in period  $t$ . The estimations are carried out using ordinary least squares (OLS).<sup>4</sup>  $N_i$  is our explanatory variable of main interest; it is a Norway indicator that takes the value one if individual  $i$  lives in Norway.  $\mathbf{Z}_{i0}$  is a vector consisting of age, gender, income, cohabitation status, education, and region of residence for individual  $i$  observed in the first period only.  $\mathbf{X}_{it}$  is a vector of observed individual characteristics for individual  $i$  in period  $t$  (religious,

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<sup>4</sup> Ordered logit regressions yield very similar results as the OLS estimates (available upon request).

trust, anti immigration, public sector, gender equality, co-responsible if abused, and sexual liberal, described in Table 1). Since these variables are observed at both time periods, they enter as differences.  $\varepsilon_i$  is the random error term, which is assumed to be uncorrelated with  $N$  conditional on the other variables. Variables entering as differences may also be affected by the law, since they are recorded in the second period as well, and may hence be endogenous, and we therefore present results including only  $\mathbf{Z}_{i0}$  as well. The vector  $\mathbf{Z}_{i0}$ , is only recorded for the first period and included to control for potential time varying effects from these variables. As hypothesized, the change may be larger among younger people or by people living in the capital. This may also be true for gender, income, cohabitation status and education. For example, respondents with higher education may be affected differently than respondents without. We also run specifications including only the first wave of all control variables (that is, controlling for  $\mathbf{Z}_{i0}$  and  $\mathbf{X}_{i0}$ ) and specifications including only those variables for which we have data in both years as differences (that is, only  $\mathbf{X}_{i1}-\mathbf{X}_{i0}$ ). The results (available upon request) do not alter the conclusions.

## 5. Results

In this section, we present results regarding change in moral attitudes in the general populations (5.1) and toward the legal setting (5.2). In Section 5.3, we present the results regarding attitude change in Oslo as well as for different age groups. In Section 5.4, we problematize and discuss the results more broadly.

### 5.1 Moral attitudes toward prostitution

We start by looking at the difference in moral attitudes toward buying sex. The coefficients of OLS regressions are presented in Panel A in Table 2.<sup>5</sup> Our main variable of interest is the coefficient for the Norway dummy, which is our difference-in-differences (dd) estimate as described above. In the first column, we only control for gender, age, education, living in the capital region, and civil status ( $\mathbf{Z}_{i0}$ ). We see that the dd estimate (*Norway*) is insignificant. In Column 2, we also include the other attitude variables as controls. These are also variables for which we have data for both years, so they enter as first differences ( $\mathbf{X}_{i1} - \mathbf{X}_{i0}$ ). Also here we see that the dd estimate is insignificant. Moving to the results on moral attitudes

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<sup>5</sup> The full regression tables are presented in Appendix.

toward selling sex, the results in Panel B (Table 2) show that the dd estimates are not statistically significant for either specification (1 or 2). This indicates that the law did not affect moral attitudes toward selling sex in Norway in the general population.

Table 2. Effect of law on attitudes

	(1) Base	(2) Full	(3) Trust	(4) Know 2	(5) Know 2+Trust
Panel A. Difference in moral attitudes toward buying sex.					
Norway	0.088 (0.119)	0.116 (0.120)	0.264 (0.186)	0.023 (0.143)	0.156 (0.228)
<b>Z<sub>i0</sub></b>	YES	YES	YES	YES	YES
<b>X<sub>i1</sub>-X<sub>i0</sub></b>	NO	YES	YES	YES	YES
Observations	2104	2067	862	1323	598
Panel B. Difference in moral attitudes toward selling sex.					
Norway	0.098 (0.125)	0.136 (0.126)	0.273 (0.193)	0.142 (0.151)	0.097 (0.229)
<b>Z<sub>i0</sub></b>	YES	YES	YES	YES	YES
<b>X<sub>i1</sub>-X<sub>i0</sub></b>	NO	YES	YES	YES	YES
Observations	2098	2062	860	1318	597
Panel C. Difference in attitudes toward criminalization of buying sex.					
Norway	0.014 (0.020)	0.016 (0.021)	0.098*** (0.032)	0.023 (0.025)	0.061 (0.040)
<b>Z<sub>i0</sub></b>	YES	YES	YES	YES	YES
<b>X<sub>i1</sub>-X<sub>i0</sub></b>	NO	YES	YES	YES	YES
Observations	2103	2063	859	1319	596
Panel D. Difference in attitudes toward criminalization of selling sex.					
Norway	0.037* (0.021)	0.037* (0.022)	0.100*** (0.035)	0.063** (0.027)	0.062 (0.042)
<b>Z<sub>i0</sub></b>	YES	YES	YES	YES	YES
<b>X<sub>i1</sub>-X<sub>i0</sub></b>	NO	YES	YES	YES	YES
Observations	2087	2048	852	1310	591

Notes: This table reports the effect of the law on attitudes. Panels A-D present the four different dependent variables. Regressions are conducted using OLS. Controls in all regressions include age, gender, income, cohabitation status, education, and region of residence for individual  $i$  observed in the first period ( $Z_{i0}$ ). Columns 2-5 also include  $\Delta$ Trust,  $\Delta$ Religious,  $\Delta$ Public sector,  $\Delta$ Gender equality,  $\Delta$ Co-responsible,  $\Delta$ Anti immigration and  $\Delta$ Sexual liberal as controls ( $X_{i1}-X_{i0}$ ). In Column 3, the sample is restricted to those who trust politicians. Column 4 includes those who know what the law says. In Column 5, the sample is restricted to those who both trust politicians and know the law. In Columns 3 and 5,  $\Delta$ Trust is not included since the sample is restricted with respect to trust. Standard errors in parentheses. Full tables are presented in Appendix.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

To test the hypothesis that people who trust politicians are more inclined to change their opinions in line with the signals sent out by the law, we restrict the sample to those who

trust politicians i.e., those who answered 6 or above on a 1-10 scale to the question, “In general, do you trust politicians?” in the second survey (Column 3).<sup>6</sup> Since the dd estimate is still insignificant for this group (both in Panels A and B), the hypothesis can not be confirmed. In Column 4, we restrict the sample to those who actually knew about the law (i.e., those who answered the question, “To your knowledge, is it illegal to buy/sell sex?” correctly in the second period<sup>7</sup>), and in the last column, we include those who both knew about the law and claimed to trust politicians. The dd estimate is insignificant for these two specifications as well, and we conclude that we find no evidence that the law changed Norwegians’ moral attitudes toward buying or selling sex.

## 5.2 Attitudes toward the law

We then proceed to investigate the changes in attitudes toward criminalization of buying sex; the results of the OLS regressions are shown in Panel C (Table 2). As in the case of moral attitudes, we see that our dd estimate is insignificant in the full sample. Yet the dd estimate in Column 3 indicates support for the hypothesis that those who claimed to trust politicians were more inclined to change their attitudes. However, once we condition on actually knowing the law, which should be a necessary condition for this mechanism, there is no effect. We therefore conclude that we find no evidence that the law changed Norwegians’ attitudes toward criminalization of buying sex.

The picture changes when looking at the results on changes in attitudes toward criminalization of selling sex, which are presented in Panel D (Table 2). We note that the dd estimate is statistically significant for all specifications, except for the one in Column 5. Living in Norway increases the probability of having changed into wanting selling sex to be illegal and decreases the probability of having changed into wanting it to be legal. The higher marginal effects are found in the subsample with people who trust politicians to a greater extent. While this seems to suggest some support of the hypothesis that trust in politicians is important, one should keep in mind that the direction is the opposite of what was intended (the lawmakers wished for more negative attitudes toward buying sex but explicitly not

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<sup>6</sup> We also conducted the same analysis with the trust question from the first wave of the survey, and the results were very similar.

<sup>7</sup> We only require a correct answer in the second wave since people may have updated their beliefs as an effect of the law (but the results do not change if we require a correct answer also before the criminalization).

toward selling). Furthermore, restricting the sample to those who actually know the law and trust politicians removes the significance of the effect. Thus, there is no support for the claim that trust in politicians affects attitudes in the intended way. Also, when using the responses to the trust question from the first wave, the marginal effects are larger for the subgroup trusting politicians, but the effect becomes insignificant when conditional on knowing the law.

That the legal change seems to have affected attitudes toward criminalization of selling sex but not toward criminalization of buying sex may come as a surprise since the law focuses only on buying sex. As suggested by social response theory, a legal change can lead to attitude changes contrary to the expectations of lawmakers (e.g., Carbonara et al. 2008). Whether our results should be interpreted in such a way is not clear since the attitudes toward buying sex did not change into being more negative. However, as put forth in the Norwegian debate (especially by Pro Sentret,<sup>8</sup> whose position is that the stigmatization of sellers will increase as a result of the recently implemented law), a law that criminalizes buyers is likely to affect attitudes toward selling as well, since it puts focus on the issue and signals that there is a problem. Another interpretation is that the law led to opposition in the sense that people now think that both parties of the transaction should be liable, which is contrary to the lawmakers' view. That is, people prefer symmetry where both buying and selling sex should be treated in the same way by the law.

The summary statistics reveal, however, that the effect described above is driven by Swedes having changed more into thinking selling sex should not be illegal and Norwegians in fact thinking it should be illegal to a lesser extent after the implementation of the law than before. Given our identifying assumption, the effects of the law are, however, that Norwegians became more likely to think it should be illegal to sell sex than they would have been in the absence of legal change (where they would have changed even more). Since we are not able to test this assumption, care should be taken when interpreting this result. If the identifying assumption does not hold, this conclusion is not correct.

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<sup>8</sup> Pro Sentret is a non-governmental organization that represents prostitutes and provides information on prostitution.

### 5.3 Attitudes among different age groups and in Oslo

To test the hypothesis of younger people being more prone to change their attitudes as a consequence of the law, we interact the Norway indicator variable with the vector  $\mathbf{Z}_{i0}$ . The results are presented in Table 3 below.

Table 3. Regressions with interaction terms.

	(1) Buying wrong	(2) Selling wrong	(3) Illegal buying	(4) Illegal selling
Norway	0.440 (0.496)	0.816 (0.521)	0.094 (0.084)	0.048 (0.089)
Age	0.025*** (0.006)	0.012* (0.007)	0.003** (0.001)	0.003** (0.001)
Age*Norway	-0.017* (0.010)	-0.015 (0.010)	-0.004** (0.002)	-0.002 (0.002)
$\mathbf{Z}_{i0}$	YES	YES	YES	YES
$\mathbf{Z}_{i0}$ *Norway	YES	YES	YES	YES
Observations	2104	2098	2103	2087

Notes: This table reports the effect of the law on attitudes. Regressions are conducted using OLS. Controls in all regressions include age, gender, income, cohabitation status, education, and region of residence for individual  $i$  observed in the first period ( $\mathbf{Z}_{i0}$ ), as well as these variables interacted with Norway. Standard errors in parentheses. Full tables are presented in Appendix.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

We see that for all variables, the coefficient of age is positive, hence, the change in opinion in favor of criminalization increases with age. The Norway indicator variable interacted with age is negative and statistically significant for the two specifications regarding buying sex.<sup>9</sup> This means that older Norwegians changed less toward thinking that buying sex is immoral and also changed less toward thinking that buying sex should be illegal. Analysis with cohort dummies (available upon request) further confirms that younger Norwegians changed their attitudes more than older Norwegians as an effect of the law. We thereby confirm the hypothesis that younger people are more prone to adapt their attitudes in response to legal changes and we also note that the direction of change follows the lawmakers' intentions. This supports claims from institutional and socialization theory (e.g., Svallfors 2009) that those with fewer previous formative experiences in need of reconsideration are more prone to internalize legal norms.

<sup>9</sup> As a sensitivity analysis we also included  $\mathbf{X}_{i1}-\mathbf{X}_{i0}$  and interacted it with the Norway indicator variable. The results are very similar although the coefficient for believing that buying sex is wrong moves from being significant at the 10 % level to being significant at the 13 % level.



We also note that education level does not seem to affect the changes in attitudes, which is important considering our biased sample.

Finally, in order to test the hypothesis of proximity suggested by Soss and Schram (2007), according to which there should be a greater effect in Oslo than in the rest of Norway, we restrict the treatment group to include only people living in Oslo. The comparison group is still the Swedish sample. This is again done to establish an effect of the law as opposed to describing a correlation arising from a general trend. Table 4 presents the results. Interestingly, we see that people in Oslo changed their attitudes toward thinking that buying sex is more immoral and also toward wanting buying sex to be illegal. They do not think that selling sex is more immoral or that it should be illegal to a greater extent than they did before. The marginal effect of living in Oslo implies an 8.2 percentage point higher probability of having changed opinion from wanting buying sex to be legal to wanting it to be illegal, and Oslo residents are also 5.3 percentage points less likely to have changed into thinking buying sex should be legal.<sup>10</sup>

Table 4. Difference in attitudes toward prostitution in the Norwegian capital as compared to Sweden.

	(1) Buying wrong	(2) Selling wrong	(3) Illegal buying	(4) Illegal selling
Oslo	0.509* (0.288)	0.289 (0.322)	0.134** (0.054)	0.041 (0.058)
<b>Z<sub>i0</sub></b>	YES	YES	YES	YES
<b>X<sub>i1</sub>-X<sub>i0</sub></b>	YES	YES	YES	YES
Observations	1281	1277	1280	1270

Notes: This table reports the effect of the law on attitudes in the Norwegian capital as compared to Sweden. Regressions are conducted using OLS. Controls in all regressions include age, gender, income, cohabitation status, education, and region of residence for individual  $i$  observed in the first period ( $Z_{i0}$ ), as well as  $\Delta$ Trust,  $\Delta$ Religious,  $\Delta$ Public sector,  $\Delta$ Gender equality,  $\Delta$ Co-responsible,  $\Delta$ Anti immigration, and  $\Delta$ Sexual liberal ( $X_{i1}-X_{i0}$ ). Standard errors in parentheses. Full tables are presented in Appendix.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

It should also be noted that these changes are driven by Oslo residents thinking that buying sex is more immoral and that it should be illegal, e.g., 51.6 percent of the people living in Oslo thought it should be illegal prior to the law while 58.7 thought so in the second survey. When using only the Swedish capital (Stockholm) as control group, the statistical significance

<sup>10</sup> These effects are calculated using ordered probit regressions (results available upon request).

of the effect on moral attitudes toward buying sex disappears. This effect is only significant at the 10 percent level when comparing to the whole of Sweden, and we lose around three-quarters of the sample size by only including Stockholm. Regarding the other dependent variables (Selling wrong, Illegal selling, and Illegal buying), the results are similar to before (all results are available upon request).

Having established that there is indeed an effect in Oslo, we also compare the changes in attitudes in Oslo to the changes in the rest of Norway. This has the advantage of isolating the proximity aspect since Oslo residents were more affected by the legal change. These results (in Table 5) indicate that the changes were larger in Oslo than in the rest of Norway regarding buying sex. That is, Oslo residents changed into wanting buying sex to be criminalized ( $p=0.06$ ) and there is some support for thinking that buying sex is more morally wrong ( $p=0.14$ ). Taken together, the cross-country dd estimates and the within-Norway estimates support the hypothesis that proximity affects attitudinal change.

Table 5. Difference in attitudes toward prostitution in the Norwegian capital as compared to the rest of Norway.

	(1) Buying wrong	(2) Selling wrong	(3) Illegal buying	(4) Illegal selling
Oslo	0.468 (0.315)	0.269 (0.301)	0.088* (0.047)	0.019 (0.049)
$Z_{i0}$	YES	YES	YES	YES
$X_{i1}-X_{i0}$	YES	YES	YES	YES
Observations	888	887	885	879

Notes: This table reports the effect of the law on attitudes in the Norwegian capital as compared to the rest of Norway. Regressions are conducted using OLS. Controls in all regressions include age, gender, income, cohabitation status, education, and region of residence for individual  $i$  observed in the first period ( $Z_{i0}$ ), as well as  $\Delta$ Trust,  $\Delta$ Religious,  $\Delta$ Public sector,  $\Delta$ Gender equality,  $\Delta$ Co-responsible,  $\Delta$ Anti immigration, and  $\Delta$ Sexual liberal ( $X_{i1}-X_{i0}$ ). Standard errors in parentheses. Full tables are presented in Appendix.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

## 5.4 Discussion

In sum, the law did not affect moral attitudes toward prostitution in the general Norwegian population. However, in the Norwegian capital, where prostitution was more visible before the reform, the law actually made people more negative toward buying sex. We also find that younger people changed their attitudes more, and in the direction of the lawmakers' intentions, than older people as a result of the law. The hypothesis that people who trust

politicians change attitudes more in the intended direction when a law is enacted is not supported. One possible reason for this is that they already before the implementation of the law supported the view put forward by the politicians.

In order to generalize the results, a few caveats are necessary, especially since we might underestimate the effects of legal change on attitudes for several reasons. First of all, it is likely that laws affect attitudes more over time periods that are longer than eight months, and there is indicative evidence that the enactment of the same law changed attitudes in Sweden to a considerable degree (Jakobsson and Kotsadam 2010a). As Ellickson (2001) argues, there may be lags in the effects on attitudes due to cognitive biases toward status quo derived from loss aversion or due to a difficulty of displacing already internalized norms. A related mechanism through which laws may have long-run effects is the replacement of cohorts as suggested by Svallfors (2009), and our results of more change among younger people indicate that this is likely. It is therefore important to keep in mind that the results of the present paper concern the short-run effects of laws on attitudes only, and that we cannot say anything about long-run effects.

Since we are unable to distinguish between any “direct effect” of the law and the effect attained via the media debate, a related issue is that the media discussion had started before the first wave of the survey was distributed (see, e.g., Jahnsen 2008). In addition, it was at this point clear that the law would be implemented. Both these factors are likely to underestimate the effects of the law reported in this paper. However, the debate was more widespread during the final months before implementation (and hence after the first survey was sent out), and we can see that the level of knowledge about the law was lower when respondents answered the survey the first time (43 percent of the Norwegian respondents knew the legal framework in the first survey while 59 percent did in the second). It is therefore likely that people updated their knowledge between the two surveys.

These caveats are also important for our identifying assumption that the change in average attitudes among individuals living in Norway would have, without the law, been the same as the change among individuals living in Sweden. Since the media debate started and information about the reform became available before we sent out the first survey, the

possible process of attitudinal change had probably already started. As we show, however, knowledge was updated and media coverage became intense after the respondents had answered the first survey, probably implying a possible underestimation of the magnitude of the causal effect; yet it does not imply that the effects we find are not causal. The problem of lags in response to legal change is also problematic since if there are long lags with considerable effects, Swedes may constitute an inappropriate control group as a similar law was enacted in Sweden ten years earlier. In the worst case scenario (for our assumption) of still persisting effects of the Swedish law on the rate of change in attitudes among Swedes, our results are still important for comparing the difference between short-term and long-term effects. Both of these limitations of the identifying assumption could have been resolved by collecting more waves of data further back in time, which is a path we recommend future researchers to take (although it is difficult to gather detailed information on attitudes toward a relevant law that nobody knows will be implemented). Compared to existing literature, however, this paper amplifies the available knowledge in the area.

## **6. Conclusion**

Using longitudinal data, we investigate the attitudinal effects of the criminalization of buying sex in Norway (1 January 2009), which had as one of its key aims to make people more negative toward buying sex. We conducted surveys in Norway and Sweden where we asked for people's opinions about prostitution during the fall of 2008 and the fall of 2009, i.e., before and after the criminalization of buying sex in Norway, and evaluated the effects in a difference-in-differences estimation with Swedish respondents as control group.

Our main results are that, in the general population, the law did not affect moral attitudes toward buying or selling sex. However, in accordance with our hypothesis, we find that people living in the Norwegian capital (Oslo) became more opposed to prostitution than the general population. This supports the more general hypothesis suggested by Soss and Schram (2007) that laws and policies are more likely to affect attitudes the more visible and proximate they are to people.

Comparing the results of previous studies on the effects of laws, regimes, and policies on attitudes further strengthens this point. The division and re-unification of Germany

(Svallfors 2009; Alesina and Fuchs-Schündeln 2007) was clearly visible and proximate to people and also affected attitudes as expected. In contrast, the US welfare reform studied by Soss and Schram (2007) was distant to most Americans, as was the law studied here to most Norwegians, and consequently there were limited effects on attitudes in both cases. The clear effects found on attitudes toward the Irish smoke-free law (Fong et al. 2006) are also expected since the effects were evaluated only among smokers. For this group, the law was clearly proximate, which can be compared to our Oslo sub-sample for which we also find the expected effects. Comparing the intended effects of the law to the results in the Oslo region, we can see that the politicians' intentions have been fulfilled. People in Oslo now think it is more immoral to buy sex than they used to. Given our identifying assumptions, these changes are not merely trends – they are causal effects of the law.

Our results are important for both policy and research. A large literature in economics, political science, and sociology has explored how laws may affect attitudes, yet the knowledge in this area is still sparse. More broadly, the literature on the importance of institutions often explores the effects of institutions via large-scale and politically infeasible changes (e.g., the division of Germany or Korea, colonialism, natural disasters, and wars). As Bhavnani (2009) argues, such natural experiments provide few possibilities for policy advice compared to investigations of effects of small-scale policy change.

We suggest that further research be undertaken to investigate the longer run effects of laws on attitudes and the effects of different types of laws and in different contexts. The comparison of realized and intended effects in the general population and in Oslo raises interesting questions not only about the contextual prerequisites for effects but also about their direction.

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## Appendix. Full tables

Table A1. Difference in moral attitudes toward buying sex.

	(1)	(2)	(3)	(4)	(5)
	Base	Full	Trust	Know 2	Know 2+Trust
Norway	0.088 (0.119)	0.116 (0.120)	0.264 (0.186)	0.023 (0.143)	0.156 (0.228)
Male	0.156 (0.116)	0.148 (0.117)	0.247 (0.179)	0.075 (0.140)	0.124 (0.219)
Age	0.017*** (0.005)	0.016*** (0.005)	0.023*** (0.007)	0.018*** (0.006)	0.026*** (0.009)
High education	0.038 (0.121)	0.040 (0.122)	0.246 (0.192)	0.069 (0.146)	0.248 (0.238)
Low education	-0.078 (0.194)	-0.039 (0.197)	-0.127 (0.337)	0.018 (0.255)	0.222 (0.430)
High income	-0.220 (0.258)	-0.107 (0.262)	-0.344 (0.380)	0.210 (0.306)	-0.369 (0.467)
Low income	0.290** (0.137)	0.257* (0.139)	0.496** (0.213)	0.205 (0.167)	0.445* (0.265)
Capital	0.259* (0.154)	0.237 (0.155)	0.082 (0.228)	0.183 (0.176)	0.086 (0.271)
Cohabit	0.253** (0.125)	0.254** (0.126)	0.291 (0.191)	0.016 (0.151)	0.023 (0.235)
$\Delta$ Trust		-0.001 (0.037)		0.058 (0.045)	
$\Delta$ Religious		-0.137 (0.361)	0.278 (0.560)	0.067 (0.431)	0.438 (0.679)
$\Delta$ Public sector		0.040 (0.039)	0.062 (0.074)	0.029 (0.051)	0.037 (0.097)
$\Delta$ Gender equali.		0.019 (0.035)	0.044 (0.063)	0.038 (0.044)	0.050 (0.084)
$\Delta$ Co-responsib.		-0.036 (0.026)	-0.026 (0.041)	-0.031 (0.033)	-0.017 (0.053)
$\Delta$ Anti immigrat.		-0.022 (0.026)	-0.025 (0.041)	-0.015 (0.032)	-0.006 (0.052)
$\Delta$ Sexual liberal		-0.077*** (0.021)	-0.074** (0.035)	-0.063** (0.026)	-0.094** (0.041)
Constant	-1.106*** (0.261)	-1.076*** (0.264)	-1.653*** (0.389)	-0.900*** (0.311)	-1.342*** (0.470)
Observations	2104	2067	862	1323	598
R-squared	0.01	0.02	0.03	0.02	0.03

Standard errors in parentheses.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.



Table A2. Difference in moral attitudes toward selling sex.

	(1)	(2)	(3)	(4)	(5)
	Base	Full	Trust	Know 2	Know 2+Trust
Norway	0.098 (0.125)	0.136 (0.126)	0.273 (0.193)	0.142 (0.151)	0.097 (0.229)
Male	0.007 (0.122)	0.002 (0.122)	0.014 (0.186)	-0.054 (0.148)	-0.054 (0.221)
Age	0.006 (0.005)	0.005 (0.005)	0.014* (0.008)	0.004 (0.006)	0.016* (0.009)
High education	0.080 (0.127)	0.052 (0.127)	0.436** (0.200)	0.222 (0.154)	0.337 (0.239)
Low education	0.039 (0.204)	0.056 (0.205)	0.327 (0.350)	0.187 (0.269)	0.681 (0.432)
High income	0.173 (0.271)	0.216 (0.273)	0.298 (0.395)	0.202 (0.322)	-0.242 (0.469)
Low income	0.031 (0.145)	-0.005 (0.145)	0.117 (0.223)	0.024 (0.177)	0.093 (0.267)
Capital	0.159 (0.161)	0.181 (0.161)	0.311 (0.237)	0.189 (0.185)	0.235 (0.272)
Cohabit	0.020 (0.132)	-0.004 (0.132)	-0.032 (0.199)	-0.273* (0.159)	-0.453* (0.236)
$\Delta$ Trust		-0.005 (0.038)		0.041 (0.048)	
$\Delta$ Religious		-0.163 (0.380)	-0.182 (0.582)	0.075 (0.453)	0.026 (0.682)
$\Delta$ Public sector		0.092** (0.041)	0.039 (0.077)	0.067 (0.054)	-0.024 (0.098)
$\Delta$ Gender equali.		0.016 (0.036)	0.089 (0.066)	0.047 (0.047)	0.050 (0.084)
$\Delta$ Co-responsib.		0.001 (0.027)	0.013 (0.042)	0.009 (0.035)	0.076 (0.053)
$\Delta$ Anti immigrat.		-0.026 (0.027)	0.006 (0.043)	-0.028 (0.034)	0.040 (0.052)
$\Delta$ Sexual liberal		-0.105*** (0.022)	-0.116*** (0.036)	-0.105*** (0.027)	-0.121*** (0.042)
Constant	-0.550** (0.274)	-0.467* (0.276)	-1.189*** (0.406)	-0.347 (0.328)	-0.753 (0.473)
Observations	2098	2062	860	1318	597
R-squared	0.00	0.02	0.03	0.02	0.04

Standard errors in parentheses.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table A3. Difference in attitudes toward criminalization of buying sex.

	(1)	(2)	(3)	(4)	(5)
	Base	Full	Trust	Know 2	Know 2+Trust
Norway	0.014 (0.020)	0.016 (0.021)	0.098*** (0.032)	0.023 (0.025)	0.061 (0.040)
Male	0.012 (0.020)	0.016 (0.020)	0.055* (0.031)	0.005 (0.024)	0.060 (0.038)
Age	0.001 (0.001)	0.001 (0.001)	0.003** (0.001)	0.001 (0.001)	0.002 (0.002)
High education	0.014 (0.021)	0.015 (0.021)	0.012 (0.034)	0.000 (0.026)	0.022 (0.041)
Low education	0.001 (0.033)	-0.005 (0.033)	-0.016 (0.059)	-0.021 (0.044)	0.052 (0.074)
High income	0.037 (0.044)	0.044 (0.045)	0.054 (0.066)	0.084 (0.053)	0.054 (0.081)
Low income	0.027 (0.023)	0.024 (0.024)	0.059 (0.037)	-0.012 (0.029)	0.031 (0.046)
Capital	0.035 (0.026)	0.029 (0.026)	0.011 (0.040)	0.020 (0.031)	0.009 (0.047)
Cohabit	0.001 (0.021)	-0.002 (0.021)	-0.009 (0.033)	0.005 (0.026)	0.002 (0.041)
ΔTrust		0.006 (0.006)		0.021*** (0.008)	
ΔReligious		-0.027 (0.061)	0.074 (0.097)	-0.151** (0.075)	-0.036 (0.117)
ΔPublic sector		0.002 (0.007)	-0.010 (0.013)	0.002 (0.009)	-0.016 (0.017)
ΔGender equali.		0.002 (0.006)	-0.005 (0.011)	0.007 (0.008)	-0.004 (0.015)
ΔCo-responsib.		-0.004 (0.004)	-0.008 (0.007)	-0.008 (0.006)	-0.007 (0.009)
ΔAnti immigrat.		-0.002 (0.004)	-0.010 (0.007)	-0.007 (0.006)	-0.008 (0.009)
ΔSexual liberal		-0.011*** (0.004)	-0.016** (0.006)	-0.010** (0.004)	-0.020*** (0.007)
Constant	-0.079* (0.045)	-0.082* (0.045)	-0.212*** (0.068)	-0.060 (0.054)	-0.160** (0.081)
Observations	2103	2063	859	1319	596
R-squared	0.003	0.009	0.035	0.021	0.031

Standard errors in parentheses.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table A4. Difference in attitudes toward criminalization of selling sex.

	(1)	(2)	(3)	(4)	(5)
	Base	Full	Trust	Know 2	Know 2+Trust
Norway	0.037*	0.037*	0.099***	0.063**	0.068
	(0.021)	(0.022)	(0.035)	(0.027)	(0.042)
Male	0.018	0.018	0.045	0.029	0.064
	(0.021)	(0.021)	(0.034)	(0.026)	(0.041)
Age	0.002**	0.02**	0.003**	0.002**	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
High education	0.012	0.010	0.000	-0.012	-0.017
	(0.022)	(0.022)	(0.036)	(0.027)	(0.044)
Low education	0.005	0.003	0.012	-0.008	0.066
	(0.035)	(0.035)	(0.063)	(0.047)	(0.079)
High income	-0.036	-0.041	-0.030	-0.073	-0.106
	(0.047)	(0.047)	(0.072)	(0.057)	(0.086)
Low income	0.002	-0.002	-0.012	-0.041	-0.044
	(0.025)	(0.025)	(0.040)	(0.031)	(0.049)
Capital	0.027	0.024	-0.019	0.043	0.001
	(0.028)	(0.028)	(0.043)	(0.033)	(0.050)
Cohabit	-0.003	-0.009	-0.053	-0.046*	-0.110**
	(0.023)	(0.023)	(0.036)	(0.028)	(0.043)
ΔTrust		0.008		0.017**	
		(0.007)		(0.008)	
ΔReligious		-0.062	0.075	-0.139*	0.043
		(0.066)	(0.108)	(0.082)	(0.129)
ΔPublic sector		0.003	-0.001	-0.002	-0.016
		(0.007)	(0.014)	(0.009)	(0.018)
ΔGender equali.		0.004	-0.0027	0.005	-0.005
		(0.006)	(0.012)	(0.008)	(0.015)
ΔCo-responsib.		0.001	-0.002	0.003	0.004
		(0.005)	(0.008)	(0.006)	(0.010)
ΔAnti immigrat.		-0.002	-0.009	0.000	-0.004
		(0.005)	(0.008)	(0.006)	(0.010)
ΔSexual liberal		-0.014***	-0.016**	-0.010**	-0.018**
		(0.004)	(0.007)	(0.005)	(0.008)
Constant	-0.131***	-0.121**	-0.171**	-0.122**	-0.141
	(0.047)	(0.047)	(0.073)	(0.058)	(0.087)
Observations	2087	2048	852	1310	591
R-squared	0.005	0.012	0.031	0.024	0.045

Standard errors in parentheses.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table A5. Regressions with interaction terms.

	(1)	(2)	(3)	(4)
	Buying wrong	Selling wrong	Illegal buying	Illegal selling
Norway	0.440 (0.496)	0.816 (0.521)	0.094 (0.084)	0.048 (0.089)
Male	0.255* (0.152)	0.046 (0.160)	-0.012 (0.026)	-0.008 (0.027)
Age	0.026*** (0.006)	0.012* (0.007)	0.003** (0.001)	0.003** (0.001)
High education	-0.016 (0.164)	0.063 (0.172)	-0.003 (0.028)	0.015 (0.030)
Low education	-0.209 (0.239)	-0.028 (0.252)	-0.042 (0.041)	0.020 (0.043)
High income	0.048 (0.421)	0.720 (0.442)	0.083 (0.072)	-0.010 (0.077)
Low income	0.237 (0.170)	0.128 (0.179)	0.029 (0.029)	-0.002 (0.031)
Capital	0.113 (0.187)	0.089 (0.197)	-0.002 (0.032)	0.025 (0.034)
Cohabit	0.070 (0.168)	0.026 (0.177)	-0.018 (0.029)	-0.041 (0.030)
Age*Norway	-0.017* (0.010)	-0.015 (0.011)	-0.004** (0.002)	-0.002 (0.002)
Male*Norway	-0.204 (0.237)	-0.036 (0.249)	0.065 (0.040)	0.069 (0.043)
High*Norway	0.090 (0.244)	0.010 (0.257)	0.024 (0.042)	-0.009 (0.044)
Low*Norway	0.202 (0.428)	0.046 (0.450)	0.091 (0.073)	-0.066 (0.077)
Highi*Norway	-0.347 (0.535)	-0.859 (0.563)	-0.081 (0.091)	-0.050 (0.097)
Lowi*Norway	0.154 (0.290)	-0.280 (0.306)	-0.019 (0.050)	0.004 (0.052)
Capital*Norway	0.331 (0.334)	0.132 (0.351)	0.092 (0.057)	-0.007 (0.060)
Cohab*Norway	0.413 (0.252)	-0.035 (0.265)	0.042 (0.043)	0.082* (0.045)
Constant	-1.318*** (0.329)	-0.855** (0.346)	-0.114** (0.056)	-0.124** (0.059)
Observations	2104	2098	2103	2087
R-squared	0.016	0.005	0.009	0.008

Standard errors in parentheses.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table A6. Difference in attitudes toward prostitution in the Norwegian capital with Sweden as comparison group.

VARIABLES	(1) Buying wrong	(2) Selling wrong	(3) Illegal buying	(4) Illegal selling
Oslo	0.509* (0.288)	0.289 (0.322)	0.134** (0.054)	0.041 (0.058)
Male	0.220 (0.135)	0.039 (0.151)	-0.003 (0.025)	0.000 (0.0270)
Age	0.022*** (0.006)	0.009 (0.006)	0.003** (0.001)	0.002 (0.001)
High education	-0.091 (0.146)	0.019 (0.163)	0.012 (0.027)	0.018 (0.029)
Low education	-0.167 (0.217)	0.048 (0.242)	-0.030 (0.041)	0.029 (0.044)
High income	0.112 (0.350)	0.556 (0.391)	0.059 (0.066)	-0.064 (0.071)
Low income	0.131 (0.151)	0.084 (0.170)	0.018 (0.028)	-0.012 (0.030)
Capital	0.103 (0.173)	0.150 (0.193)	-0.012 (0.032)	0.022 (0.035)
Cohabit	0.141 (0.147)	-0.019 (0.164)	-0.010 (0.028)	-0.020 (0.030)
ΔTrust	-0.017 (0.042)	-0.047 (0.047)	-0.002 (0.008)	0.004 (0.008)
ΔReligious	0.101 (0.414)	0.185 (0.469)	-0.043 (0.078)	-0.060 (0.085)
ΔPublic sector	0.069 (0.049)	0.119** (0.055)	-0.002 (0.009)	0.006 (0.010)
ΔGender equali.	0.015 (0.045)	-0.017 (0.051)	0.007 (0.009)	0.014 (0.009)
ΔCo-responsib.	-0.030 (0.032)	0.004 (0.036)	0.001 (0.006)	0.003 (0.006)
ΔAnti immigrat.	-0.032 (0.031)	-0.037 (0.034)	0.003 (0.006)	0.002 (0.006)
ΔSexual liberal	-0.062** (0.025)	-0.068** (0.028)	-0.012** (0.005)	-0.012** (0.005)
Constant	-1.131*** (0.297)	-0.674** (0.332)	-0.121** (0.056)	-0.110* (0.059)
Observations	1281	1277	1280	1270
R-squared	0.027	0.016	0.016	0.013

Standard errors in parentheses.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table A7. Difference in attitudes toward prostitution in the Norwegian capital with Norway as comparison group.

VARIABLES	(1) Buying wrong	(2) Selling wrong	(3) Illegal buying	(4) Illegal selling
Oslo	0.468 (0.315)	0.269 (0.301)	0.088* (0.047)	0.019 (0.049)
Male	-0.011 (0.209)	-0.022 (0.199)	0.058* (0.031)	0.066** (0.032)
Age	0.009 (0.009)	-0.002 (0.008)	-0.001 (0.001)	0.000 (0.001)
High education	0.072 (0.207)	0.050 (0.197)	0.020 (0.031)	-0.000 (0.032)
Low education	0.087 (0.413)	0.041 (0.394)	0.023 (0.061)	-0.062 (0.064)
High income	-0.148 (0.383)	-0.122 (0.365)	0.009 (0.057)	-0.071 (0.060)
Low income	0.319 (0.270)	-0.188 (0.258)	0.011 (0.040)	0.009 (0.042)
Cohabit	0.463** (0.215)	-0.065 (0.205)	0.019 (0.032)	0.030 (0.033)
ΔTrust	-0.006 (0.065)	0.056 (0.063)	0.020** (0.010)	0.015 (0.010)
ΔReligious	-0.409 (0.615)	-0.370 (0.587)	0.0450 (0.092)	0.037 (0.095)
ΔPublic sector	0.040 (0.063)	0.081 (0.060)	0.006 (0.009)	-0.001 (0.010)
ΔGender equali.	0.019 (0.053)	0.032 (0.051)	-0.002 (0.008)	-0.004 (0.008)
ΔCo-responsib.	-0.026 (0.042)	0.018 (0.040)	-0.009 (0.006)	-0.001 (0.006)
ΔAnti immigrat.	-0.043 (0.047)	-0.023 (0.045)	-0.009 (0.007)	-0.005 (0.007)
ΔSexual liberal	-0.092** (0.039)	-0.141*** (0.037)	-0.008 (0.006)	-0.015** (0.006)
Constant	-0.809* (0.426)	0.025 (0.406)	-0.016 (0.063)	-0.068 (0.066)
Observations	888	887	885	879
R-squared	0.019	0.023	0.024	0.019

Standard errors in parentheses.

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.