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Gender, Work, and Attitudes

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□onnerud□Norway□April □□□□

Summary of the thesis

The thesis consists of four self-contained papers.

Paper 1:
The long term effect of own and spousal parental leave on mothers' earnings

We take advantage of the introduction of a Norwegian parental leave reform in [illegible] to identify the causal effect of parental leave on mothers' long-term earnings. The reform raised the total leave period by seven weeks, but reserved four weeks for the father. The reform process was fast, so all mothers were already pregnant at the time of the policy announcement. Applying a regression discontinuity design we find that women who had their last child immediately after the policy change had higher mean yearly earnings from [illegible] to [illegible] and long-run yearly earnings (in our last year of data in [illegible]) compared to women who had their last child immediately before the reform. However, the estimate is sensitive to extreme observations, to restrictions regarding eligibility, and to the exclusion of observations within a window of three days before and after the reform.

Paper 2:
Do laws affect attitudes? An assessment of the Norwegian prostitution law using longitudinal data (Forthcoming in *International Review of Law and Economics*)

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.

Paper 3:
Does informal eldercare impede women's employment? The case of European welfare states (Forthcoming in *Feminist Economics*)

European states vary in eldercare policies and in gendered norms of family care, and this study uses these variations to gain insight into the importance of macro-level factors for the work-care relationship. Using advanced panel data methods on European Community Household Panel (ECHP) data, this study finds women's employment to be negatively associated with informal caregiving to the elderly across the European Union. The effects of informal caregiving seem to be more negative in the Southern European countries, less negative in the Nordic countries, and in between these extremes in the Central European countries included in the study. This study explains that since eldercare is a choice in countries with more formal care and less pronounced gendered care norms, the weaker impact of eldercare on women's employment in these countries has to do with the degree of degree of coercion in the caring decision.

Paper 4:
The employment costs of caregiving in Norway

Informal eldercare is an important pillar of modern welfare states and the ongoing demographic transition increases the demand for it while social trends reduce the supply. Substantial opportunity costs of informal eldercare in terms of forgone labor opportunities have been identified, yet the effects seem to differ substantially across states and there is a controversy on the effects in the Nordic welfare states. In this study, the effects of informal care on the probability of being employed, the number of hours worked, and wages in Norway are analyzed using data from the Life course, Generation, and Gender (LOGG) survey. New and previously suggested instrumental variables are used to control for the potential endogeneity existing between informal care and employment-related outcomes. In total, being an informal caregiver in Norway is found to entail substantially less costs in terms of forgone formal employment opportunities than in non-Nordic welfare states.

Paper □

The long-term effect of parental leave on mothers' earnings

Andreas Kotsadam[a,b], Elisabeth Ugreninov[a] and Henning Finseraas[a]

Abstract

We take advantage of the introduction of a Norwegian parental leave reform in [illegible] to identify the causal effect of parental leave on mothers' long-term earnings. The reform raised the total leave period by seven weeks, but reserved four weeks for the father. The reform process was fast, so all mothers were already pregnant at the time of the policy announcement. Applying a regression discontinuity design we find that women who had their last child immediately after the policy change had higher mean yearly earnings from [illegible] to [illegible] and long-run yearly earnings (in our last year of data in [illegible]) compared to women who had their last child immediately before the reform. However, the estimate is sensitive to extreme observations, to restrictions regarding eligibility, and to the exclusion of observations within a window of three days before and after the reform.

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1□□ntroduction

□hy do mothers have lower earnings than childless women? Three hypotheses have □een particularly prominent in the literature□According to the depreciation hypothesis□career interruptions due to maternity leave reduce wages via less wor□e□perience or depreciation of human capital □Al□recht et al□□□□□□ □incer and □olache□ □□□□□The selection hypothesis argues that the correlation □etween motherhood and earnings □□the child penalty□ or □the family gap□□is spurious and reflects selection into motherhood □□und□erg and □ose □□□□□ and perhaps into family□friendly □ut low□wage sectors □Nielsen et al□□□□□□ □inally□the speciali□ation hypothesis argues that the correlation is due to mothers speciali□ing in domestic wor□□which ma□es them less productive in the la□or mar□et □□ec□er □□□□□or that employers □ehave as if this is the case□

□n the present study we ta□e advantage of the introduction of a Norwegian parental leave reform□which affects parents with children □orn after □April □□□□to identify the composite causal effect of own and spousal parental leave on mothers' earnings in the □□□□□□□□period□ and to investigate the arguments underlying the different mechanisms used to e□plain the child penalty□□arental leave has □een found to reduce earnings for mothers □e□g□□Al□recht et al□□□□□□□□owever□most studies have □een una□le to control for the inherent pro□lems of selection into parental leave and the endogeneity of the decision to □ecome a parent□The reform we investigate raised the total leave period □y seven wee□s□□ut at the same time introduced a daddy □uota of four wee□s□that is□four wee□s were tied to the father□and the parents lost these wee□s of leave if the father did not use them□The remaining increase of three wee□s could □e used □y any parent□□ostly mothers have ended up using this e□tra time□ as with the other transfera□le wee□s□Thus□the nature of this policy reform allows us to e□amine the strength of the different mechanisms proposed to e□plain the child penalty since we identify the net long□run effect of these opposing mechanisms□□f time away from wor□ depreciates mothers' human capital□as the depreciation hypothesis argues□the reform should have a negative effect on mothers' earnings□□f instead speciali□ation is the □ey mechanism□ we should e□pect the reform to increase mothers' earnings since mothers' relative speciali□ation into child□rearing is reduced□

□ur identification strategy uses the fact that the reform cutoff date is sharp□that the mothers were already pregnant when the reform was decided□and that the data has the e□act day of □irth for all parents of Norwegian children □orn around the time of the cutoff□□n particular□

the regression discontinuity design allows us to estimate the long-run effects of the reform by comparing mothers who are generally similar with the exception that some gave birth immediately before the reform and some gave birth immediately after the reform. Since selection into having children is thereby controlled for, we have a very promising research design to detect the causal effect of the reform on mothers' earnings.

Several studies, starting with Mincer and Polachek ([illegible]), examine the effects of career interruptions on women's earnings and find that long-term earnings are negatively affected by time away from work. This finding is usually interpreted as an effect of human capital depreciation. Ruhm ([illegible]) explores how changes in parental leave schemes affected the gender gap in employment outcomes in nine European countries from [illegible] to [illegible] and finds that parental leave increases the employment probability of women but that extended durations (more than nine months) reduce women's wages as compared to men's. Albrecht et al. ([illegible]) use Swedish data and rely on fixed effects estimations to examine the effects of taking parental leave on mothers' and fathers' future wages and find that the effect is lower on mothers' wages than on fathers' wages. Since almost all mothers took parental leave at the time of the study there was no signaling effect for women (i.e., taking parental leave did not signal a low attachment to the job or a low motivation for work), while parental leave for men may have conveyed a strong signal since there was no daddy quota in Sweden at the time of their data collection and very few fathers were on parental leave ([illegible]).

A number of recent studies address the selection problem of the early studies by using parental leave reforms as natural experiments. Ekberg et al. ([illegible]) compare parents with children born just before and just after the introduction of the Swedish daddy quota and find strong effects on fathers' leave-taking, but no effects on subsequent leave taken for sick children. They interpret the latter finding as a no learning-by-doing effect of domestic labor specialization. This interpretation is in contrast to the results of Kotsadam and Finseraas ([illegible]), who, using a similar design, find a long-run ([illegible] years) effect of the first Norwegian daddy quota on the division of household labor. A plausible interpretation of the different findings is that while Kotsadam and Finseraas ([illegible]) rely on various survey items of household division of labor, Ekberg et al.'s ([illegible]) proxy for household work (i.e., leave to take care of sick children) also involves a relationship to employers. As Ekberg et al. ([illegible]) readily admit, taking the signaling theory as a basis for the negative effect found on earnings in previous studies, the daddy month made a lot of fathers take parental leave, thus the signaling effect was low□□owever□since the reform did not affect sic□leave □enefits□ta□ing sic□leave may involve a lot of signaling□These studies have a high internal validity□yet the results regarding the long□run effects of speciali□ation are mi□ed□□urthermore□the studies do not e□amine the wage effects of parental leave□

□luve and Tamm □□□□□□evaluate the effects of parental leave on female employment □y using a □erman reform in □□□□ with strong incentives for fathers to ta□e parental leave□ □nterestingly□they find no □long□run□ □□□year□effects for mothers□□owever□mothers were more li□ely to wor□ □□□ years after the reform if they were su□□ect to the reform□No effects are found for fathers□□nfortunately□their data only includes month of □irth and they do not have a representative sample of the population□as their sample is □iased with regard to age□ num□er of children□and income□□alive and □weim□ller □□□□□□use the increase of parental leave from one to two years in □□□□ and the decrease to □□ months in □□□□ in Austria to investigate the effects on employment□wages□and fertility of mothers who had their first child around the reform dates□They find that longer parental leave increases fertility and reduces employment and wages in the short run□□ut not in the long run □□□years□□□ oreover□ the pro□a□ility of □eing employed does not differ □etween the treatment and control groups from the third year onwards□and the level of earnings is not different from the fourth year onwards□Although interesting□the study does not shed any light on the effects of spousal parental leave for women□

□ohansson □□□□□□e□plores the effects of □oth own and spousal parental leave on earnings using two □wedish parental leave reforms□□he first controls for time□invariant heterogeneity using fi□ed effects models and finds that □oth own and spousal parental leave affect future earnings of parents□□nterestingly□while own leave is negative for earnings□spousal leave raises earnings□□ut only for women□□n fact□the effect of spousal leave is found to □e larger than the effect of own leave for women□□he then uses the reforms to estimate triple difference models□using families who gave □irth to their first child in Decem□er or □anuary around the time of the reforms□which were implemented on □ □anuary□or one year □efore□The families are o□served one year □efore the reform and three years after the reform□□ hile the estimates are imprecise□they point in the same direction as the fi□ed effects estimates□The fi□ed effects estimates are□however□su□□ect to criti□ue since fertility decisions may □e correlated with time variant uno□served heterogeneity□□ohansson □□□□□□herself gives an e□ample where fertility responds to income shoc□s□The more fle□i□le triple differences model is more ro□ust to such criticism□yet the resulting estimates □ecome very imprecise□

□ege and □olli □□□□□use Norwegian registry data to investigate the long□run effects of parental leave on full□time employed fathers' earnings□They restrict the sample to fathers with their youngest children □eing □□□ years old during the years □□□□□□□□□They ta□e advantage of the daddy □uota reform in □□□□and compare earnings in a given year □etween treated and non□treated fathers □□ased on their children's age in years□and find that the reform reduces fathers' earnings □y □□□□ percent□□ince the fathers in the sample have children of different ages□the authors estimate a difference in differences model and compare with the corresponding earnings difference □efore the daddy □uota□ □ince their sample includes children aged □□□□the usual difference in differences assumption of similar time trends of fathers a□sent the reform is unli□ely to □e very relia□le□mainly □ecause other family policies were introduced during the period and some parents had children in school and some did not□ □urthermore□they only have yearly data on time of □irth and treat children □orn in □□□□as the first fully treated cohort□□astly□they do not investigate the impact of the reform on mothers' earnings□

□inally□□ools et al□□□□□□□use Norwegian registry data to investigate the long□run effects of parental leave on several different outcomes□ including mothers' wages□ □omewhat surprisingly□they find a statistically significant negative effect of the daddy □uota on mothers' earnings and speculate that there are complementarities in child rearing□They restrict their sample to parents with earnings a□ove two times the □asic amount in the Norwegian social insurance system and e□clude those who gave □irth two wee□s □efore and after the reform date since they find an indication of strategic □irth planning□Their treatment and control groups are not as clean as ours as they do not restrict the analysis to the last child only□□y allowing parents in the control group to have children in later years□they □ecome treated □y the reform as well□Even more pro□lematic is the fact that the groups differ in the type of treatment they received□e□g□□no parents in the control group e□perienced the reform for their first child□As we discuss □elow□there are arguments for and against the different restrictions□ and we e□amine how the results change accordingly□

The policy change we use creates a natural e□periment that allows us to evaluate the net long□ run effects of □oth own and spousal parental leave on mothers' earnings□the theoretical mechanisms □ehind women’s child penalty can there□y □e investigated in a credi□le way□□n terms of identification□the present paper is the first paper to use a formal regression discontinuity design to investigate the effects of parental leave on earnings□The long□term effects may □e su□stantial if the daddy □uota reduces mother speciali□ation into child rearing □□ege and □olli □□□□□and if it affects the future division of household tas□s or spousal relative human capital endowments □□otsadam and □inseraas □□□□□□ohansson □□□□□□

□ e find that women who had their last child immediately after the policy change have higher mean yearly earnings from □□□□to □□□□and long□run yearly earnings □in our last year of data in □□□□□compared to women who had their last child immediately □efore the reform□ □owever□the estimates are sensitive to e□treme o□servations and to the e□clusion of cases where the parents might not have □een eligi□le for paid parental leave□□erhaps more alarming□the results are sensitive to the e□clusion of o□servations in the days around the cutoff□This finding supports those of □ools et al□□□□□□□and suggests that strategic □irth planning may have ta□en place even though the mothers were already pregnant at the time of policy announcement□

The rest of the paper is structured as follows□The ne□t section presents the reform and outlines our hypotheses□□ections □and □present the empirical strategy and the data□□ection □presents the results□and □ection □entails further ro□ustness tests of those results□The final section concludes the paper□

2□□he □orwegian parental lea□e scheme and the 1□□3 reform

Norway□li□e the other □candinavian countries□has for decades operated what has □een la□eled a women□friendly welfare state □□ernes □□□□□where the topic of e□ual opportunities in employment and domestic wor□has □een central□□n Norway paid parental leave has a long history and three historical shifts can □e identified □N□□ □□□□□□The parental leave system was first □ustified □y mothers’ health□related necessity to □e a□sent from wor□and aimed to compensate for lost income in connection with pregnancy and care for small children□A si□□ wee□ paid maternity leave was introduced as far □ac□ as in □□□□ and a □□wee□ paid maternity leave was introduced in □□□□□although only for women with health insurance□□n □□□□□sic□ness □enefit □ecame compulsory for all employed citi□ens□and thus a □□wee□paid maternity leave □ecame availa□le for all wor□ing women□

The second shift started in the late □□□□s when the pu□lic de□ate for a further increase in the num□er of days turned from protection of women's health and employment to e□ual rights in the la□or mar□et□Not until □□□□ did fathers gain the right to go on parental leave as it was e□panded to □□ wee□s and only the first si□ wee□s after the □irth were reserved for mothers□ During the □□□□s the num□er of wee□s was increased several times□

The third shift in Norwegian family□wor□ policies occurred in the □□□□s as the parental leave policy turned from e□ual rights to e□ual opportunities□□rom □□□□ to □□□□ the right to ta□e paid parental leave was gradually e□tended from □□ to □□ wee□s□□t was a disappointing matter of fact that an overwhelming ma□ority of the parental leave was ta□en □y mothers □N□□ □□□□□To increase the fathers' upta□e rates□Norway was the first country in the world to introduce a □daddy □uota□on □April □□□□□where fathers to children □orn on or after this date got an independent right to parental leave□The reform e□tended the parental leave from □□to □□wee□s with full earnings compensation[□] of which four wee□s were reserved for the father[□] At this time□paid paternity leave was contingent on □oth parents wor□ing at least □□ percent □efore the child was □orn□and the payment to fathers was reduced if the mother did not wor□ full time□□n addition□fathers were not eligi□le for paid parental leave unless they had wor□ed at least si□out of the last ten months□□athers were entitled to use the daddy □uota up until the child turned three years of age although □□percent of those ta□ing leave in □□□□□ □□□□did so during the child's first year □□ege and □olli □□□□□

□nducing fathers to ta□e more responsi□ility for child rearing was seen as an important step on the way to e□ual division of la□or and toward reducing the gender wage gap□The political arguments to earmar□ some of the parental leave for fathers were threesome□firstly□this policy implementation gives a strong signal and possi□ilities to □e more actively involved in child rearing and hence to challenge norms of male □readwinning □□eira □□□□□□econdly□an independent right to parental leave gives fathers an advantage when two parents discuss the distri□ution of their parental leave□Thirdly□the law strengthens fathers' argument for parental leave in discussions with reluctant employers□The reform led to a sharp increase in the upta□e

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[□] □ncome compensation spanned up to a ceiling of si□times the □asic amount of the Norwegian social insurance system□The □asic amount is ad□usted on a yearly □asis and was □□□□□N□□ in □□□□□□ost employers compensate for the amount a□ove the ceiling□

[□] □n fact□parents could choose to either ta□e the □□wee□s with full compensation or □□wee□s with □□□ earnings compensation□Note that the choice □etween ta□ing a shorter period with full coverage or a longer period with less coverage has □een availa□le since □□□□and was not a new feature of this reform□

rate from less than four percent prior to the reform to □□ percent in □□□□ □□running and □lantenga □□□□□□

3□□mpirical strategy

□ince all parents who had their last □latest□child after the reform date were treated □y the reform and no parents who had their last child □efore the reform date were treated□we should □e a□le to compare the two groups of parents in order to identify the causal effects of the reform□□ e also e□ploit the fact that since the policy process was so fast□parents who gave □irth around the time of the reform threshold could not have □nown a□out the reform at the time of conception□The specific design□including □April □□□□as the day of implementation□was proposed on □Decem□er □□□□and decided in parliament on □□ □anuary □□□□□[□]

□ e start □y running □□□ regressions of earnings on treatment for groups who had children □ust □efore and □ust after the reform□The e□uation to □e estimated is thus□

$$\text{Earnings}_i = \alpha + \chi \text{Treatment}_i + \beta \boldsymbol{X}_i + \varepsilon_i \quad □$$

where *Treatment* is an indicator varia□le that e□uals one for those who had children □ust after the reform in □□□□□***X*** is a vector of predetermined varia□les □the age of the parents at the time of □irth□num□er of children □efore □□□□□and lagged values of income□□and ε_i is an error term□The sample windows presented in the main analyses are chosen to □e two wee□s□si□ wee□s□and three months□

The two□wee□sample is our □random□sample□□n theory□using this sample corresponds well with what □osen□weig and □ olpin □□□□□□la□el a □natural□natural e□periment where nature determines which side of the cutoff date people end up on□□irst□it is not possi□le for parents to completely control the date of conception □Eri□sson □□□□□□alive and □weim□ller □□□□□□ □econd□a pregnancy ta□es on average □□wee□s and the duration is normally distri□uted with a standard deviation of two wee□s □E□□erg et al□□□□□□Eri□sson □□□□□□□ ost importantly□however□none of the parents □new that they would □e treated at the time of conception□Thus□

[□] The □overnment first proposed to introduce a daddy □uota of four wee□s in the state □udget for □□□□□which was accepted □y the Norwegian parliament on □Novem□er □□□□ □□uds□ett□innst□□□nr□□ □□□□□□□□□□At this time□however□the e□act date of implementation was not □nown□

it seems reasonable that the reform creates exogenous variation in own and spousal parental leave, and long-run differences in outcomes can plausibly be attributed to the change in legislation (cf. Kluve and Tamm (□□□□), Lalive and Zweimüller (□□□□)). Births can not be postponed and the studied reform is strictly favorable for parents, so triggering of birth by medical means such as by a cesarean section (see Johansson (□□□□)) should in principle not be a problem. A problem may occur, however, if triggering of births is postponed by the reform. We will assess such fine tuning by excluding mothers who gave birth three days before and after □ April.

In the three-month sample there is a statistically significant difference between the groups with respect to the parents' age. In the other samples this is not the case. We choose to present results both with and without parents' age in (□□□) since it is predetermined and plausibly exogenous. Including exogenous variables is likely to increase the precision of the estimates without biasing the treatment coefficient.

We also use the reform in a sharp regression discontinuity (RD) design as the treatment of being offered a daddy quota and a prolonged leave is a deterministic and discontinuous function of the birth date. That is, we center the treatment at day zero for □ April, which yields:

$$Treatment_i = \begin{cases} \square \; if \;\; days_i \geq \square \\ \square \; if \;\; days_i < \square \end{cases} \quad \square$$

The forcing variable, *days*, is expected to be negatively associated with earnings as parents of younger children are younger and since they have a higher workload at home. Importantly, however, the relationship between *days* and earnings is assumed to be smooth so that any discontinuity at the threshold can be attributed to the causal effect of the parental leave reform. In our case, the continuous effect of *days* is controlled for by estimating:

$$Earnings_i = a + \beta days_i + \chi Treatment_i + \lambda days_i \, \square \, Treatment_i + \varepsilon_i \quad \square$$

The smoothness assumption allows us to estimate the difference between two regression functions at day □. χ is still our parameter of interest, and it is identified by separating the continuous function of days from the discontinuity imposed by the treatment. By including the interaction term between *days* and *Treatment*, we allow the slope coefficients to differ on each side of the threshold. This is the same as estimating the two regression functions below and calculating the difference in intercepts (*a1-a2*).

$$Earnings_i = a1 + \beta days_i + \varepsilon_i \; if \; days_i \geq 0$$
$$Earnings_i = a2 + \beta days_i + \varepsilon_i \; if \; days_i < 0$$

A first step in the RD will be to estimate the earnings equation with a linear time trend and samples close to the cutoff. This local linear regression approach is less likely to be valid with larger bandwidths, unless we know that the underlying function for the forcing variable is indeed linear, and the robustness should be checked by varying the time window (Lee and Lemieux, [illegible]).

The function for days does not have to be linear, and we relax the linearity assumption by including polynomial functions of *days* in the regression model. That is, in order to assess the robustness of the treatment effect, we also estimate 2nd, 3rd, and 4th order polynomial functions. Comparing the RD results to the results of a "discontinuity sample" with observations close to the discontinuity such as the two weeks sample, is an important robustness check since the treatment effect in such a sample does not depend on neither the model specification or a constant effects assumption (Angrist and Pischke, [illegible]).

One potential problem for identification of causal effects of the reform is that there is a difference among parents of children born at different times. This difference arises by construction since the data is collected at the end of the year implying that one group always has younger children. We deal with this issue by presenting regression results on falsification samples where those included had children either during the month before or the month after the reform. These "placebo" regressions should yield statistically insignificant results as the groups are faced with the same parental leave regulation. Finally, neither of the approaches discussed thus far account for possible biological or social differences between parents of children born in March or April. To account for such differences we also present regression results on falsification samples where those included had children around the same dates but one year after the reform.

4□□ata□samples□and descripti□e statistics

□ e rely on high □uality register data encompassing all individuals in Norway□The data is gathered from several administrative registers used to calculate ta□es□pension rights□and unemployment □enefits and attrition□ □elf□report pro□lems and □ias due to refusal to participate in the study are non□e□istent□

□ur dependent varia□les are derived from two different measures□yearly income □ased on accumulation of pension □*Personal income*□and yearly la□or income□□oth measures are gross of ta□es and are measured at the end of the year□*Personal income* mainly includes employment income and income from self□employment□□n addition□unemployment □enefit□ sic□ness □enefit□maternity □enefit□and adoption allowance are included and it is also possi□le to ac□uire accumulation of pension on the □asis of non□paid caring wor□□for family mem□ers□ A disadvantage□in addition to measuring not only income stemming from wor□□is that *personal income* is left□and right□censored□□ncomes □elow or e□ual to one □asic amount and a□ove or e□ual to □□ □asic amounts do not □ualify for accumulation of pension and therefore do not enter into the measure of *Personal income*□*Labor income* includes wages and salaries from paid employment as well as net entrepreneurial income□□rom the *Labor income* varia□le we create our two main dependent varia□les□i□e□□*Earnings 2005*□which is simply la□or income in □□□□□and *Mean earnings*□which is the mean yearly la□or income from □□□□ to □□□□□*Labor income* is only availa□le from □□□□ onwards and the use of *personal income* is restricted to estimations including o□servations □efore □□□□□

The data includes information on the e□act day of □irth of all children □orn in Norway□□ e restrict our sample to individuals □orn in Norway for whom we have information a□out □oth the parents and the children□As mentioned in the empirical strategy□we focus on samples with parents of children □orn close to the reform cutoff and only on children □orn in the same year□□ e do this to minimi□e other confounding factors such as different school enrollment years□□urthermore□we focus on parents whose last child was □orn in □□□□ since those who also had children later on were then affected □y the reform□□nvestigating the effects only for these parents is necessary in order to have a clean comparison □etween treated and control individuals□yet it may □e pro□lematic to generali□e the results to the total population if the reform affected the total fertility rate□This is so since our sample then consists of a special type of individuals not affected in their fertility decision □y the reform□□ e investigate this □y comparing *all* mothers who had a child around the reform and find no difference □etween mothers who had a child just before and mothers who had a child just after the reform in the number of children they had after the reform (results are available upon request). This is important since it implies that those affected by the reform are not different in their completed fertility patterns from those in the control group, a crucial feature for the internal validity of the estimation strategy. It is also problematic to focus on the last-born child if the reform affects mothers differently depending on whether or not they have other children. If the treatment effect is larger for those having their first child (e.g., by setting durable patterns for new parents), we are likely to underestimate the effect of the reform, and if the reform affected mothers who already had other children more strongly (e.g., since they had a larger workload with respect to unpaid childcare), we may be overestimating the treatment effect. We investigate this by matching mothers of the same birth parity and comparing those who had their last child just before and just after the reform conditional on birth parity. The results (available upon request) suggest that the coefficient for the treatment indicator generally rises with the number of children born before the reform, suggesting that there are larger effects of the reform on women's earnings if the women already had children from before. This has implications for the external validity of our results as we are more likely to include mothers who already had other children at the time of the reform by focusing on the last-born child (which is necessary for internal validity) than if we would have focused on mothers of any child born around the time of the reform. Hence, our results may be overestimated as compared to the average effect of the reform for all parents.

Table □ presents summary statistics by treatment status for our three main samples. We see that the mothers in the treatment group had higher yearly earnings in □□□□ (our last year of data) and higher mean earnings from □□□□ to □□□□ than mothers in the control group (note, however, that the second difference is not statistically significant at conventional levels in the three-month sample). We also see that there is a difference between the parents in the treatment and control groups in the three-month sample with respect to their age at the time of birth. No such difference is present for the shorter time windows and the samples are also balanced in the number of children they had before the last child was born. Finally, it is reassuring to see that the mothers in the treatment and control cohorts did not have statistically significant different personal income in □□□□, and as seen in Table □ below, neither is there a difference between the groups in personal income for other years before the reform (we do not have data on labor income before □□□□).

Table [illegible]: Summary statistics of treatment and control groups for different time windows

	Control groups			Treatment groups		
			Three-month sample			
Variable	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Earnings 2005	5647	246948*	173914	6138	253227	191021
Mean earnings	5647	177365	111404	6138	180065	114231
Mothers' age 93	5647	31.2***	4.8	6138	30.8	4.8
Fathers' age 93	5647	33.9***	5.8	6138	33.6	5.8
No. of children before	5647	1.2	0.8	6138	1.2	0.8
Personal income 1988	5647	81428	64725	6138	83173	64615
			Six-week sample			
	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Earnings 2005	2812	243883**	161490	2990	253936	208096
Mean earnings	2812	175015**	108173	2990	180900	116673
Mothers' age 93	2812	31.0	4.8	2990	30.9	4.7
Fathers' age 93	2812	33.6	5.8	2990	33.8	5.8
No. of children before	2812	1.2	0.8	2990	1.2	0.8
Personal income 1988	2812	81196	64377	2990	82835	64593
			Two-week sample			
	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Earnings 2005	870	247117*	158938	1018	262989	225270
Mean earnings	870	178058*	109583	1018	188913	130499
Mothers' age 93	870	30.9	4.8	1018	30.9	4.7
Fathers' age 93	870	33.5	5.6	1018	33.9	5.7
No. of children before	870	1.2	0.8	1018	1.2	0.9
Personal income 1988	870	80524	62669	1018	81054	65388

[illegible] p [illegible] p [illegible] p [illegible] p-values in two-sided t-tests of the difference between treatment and control groups.

A crucial assumption of the identification strategy is that the reform is exogenous and hence that the density function of the forcing variable, *days*, is continuous. If agents are able to manipulate the time of birth, the continuity assumption underlying identification may be violated. As already discussed, it is unlikely that parents could precisely manipulate the time of birth since it is not possible for parents to completely control the date of conception and since none of the parents knew at that time that they would be treated. Figure [illegible] shows histograms of the forcing variable with different bin widths ([illegible] days and [illegible] day) and a visual inspection of the densities for *days* suggests that parents did not manipulate the time of birth. We examine this issue more rigorously in Section [illegible].

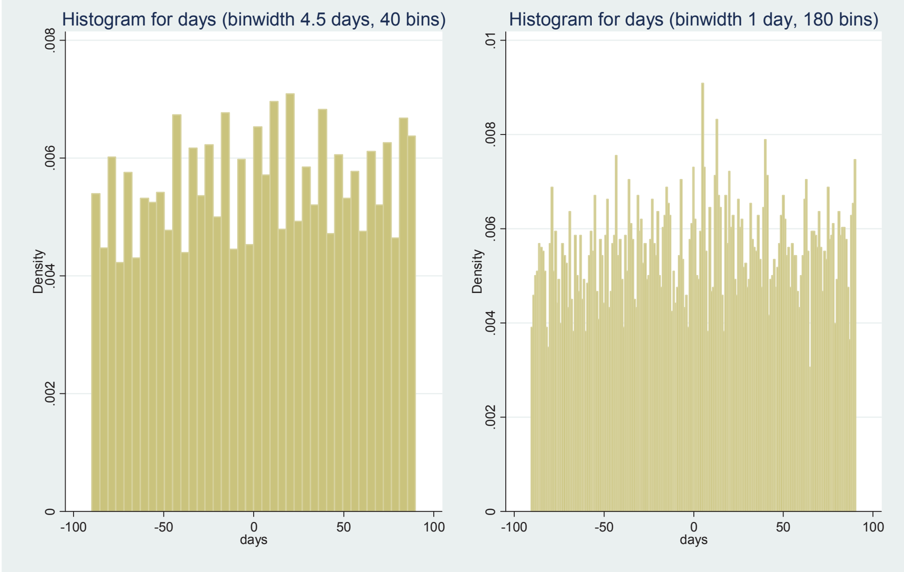


Figure 1: Histograms for days

□□esults

□ e start □y running □□□ regressions of earnings on the treatment varia□le with different time windows□Ta□le □shows the effect of the reform on mothers' mean yearly earnings from □□□□ to □□□□ □anel A□and on mothers' yearly earnings in □□□□ □anel □□□□or transparency□the first columns in each pair do not control for mothers' age even though there is a statistically significant difference □etween the groups in the three□month sample□

The two□wee□ sample is the theoretically □random□ sample□As discussed in the empirical strategy□since a pregnancy lasts □□wee□s on average with a standard deviation of two wee□s□ and since the reform was un□nown at the time of conception□this estimate should □e as good as a random measure of the effect of the reform□The three□month and si□□wee□ samples are included for completeness to show the sensitivity with respect to the time window chosen□

Table [illegible] regressions of earnings on the treatment variable for different time windows.

Panel A: Dependent variable is Mean earnings [illegible]

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
VARIABLES	[illegible] months	[illegible] months	[illegible] weeks	[illegible] weeks	[illegible] weeks	[illegible] weeks
Treatment	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Mothers' age [illegible]		[illegible]		[illegible]		[illegible]
		[illegible]		[illegible]		[illegible]
Constant	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Observations	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
R-squared	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Robust standard errors in parentheses
[illegible] p [illegible] p [illegible] p [illegible]

Panel [illegible]: Dependent variable is Earnings in [illegible]

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
VARIABLES	[illegible] months	[illegible] months	[illegible] weeks	[illegible] weeks	[illegible] weeks	[illegible] weeks
Treatment	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Mothers' age [illegible]		[illegible]		[illegible]		[illegible]
		[illegible]		[illegible]		[illegible]
Constant	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Observations	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
R-squared	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Robust standard errors in parentheses
[illegible] p [illegible] p [illegible] p [illegible]

As seen in Panel A, the coefficient for the treatment indicator is positive (as expected) and mostly statistically significant (i.e., the reform affected mothers' average yearly earnings positively). The coefficient for the mothers' age at the time of birth points in the expected direction, and including it always raises the point estimates for the treatment effect and increases precision slightly. The treatment effect is large, but somewhat imprecisely estimated, and it varies substantially across time windows. Basing our inference on the two-week before and after sample and controlling for age differences, we see that mothers with children born after the reform earned on average [illegible] NOK (approximately [illegible] USD) more per year from [illegible] to [illegible].

In Panel B we show the corresponding long-run effects of the daddy quota by examining the difference between treated and untreated mothers in yearly earnings in [illegible], i.e., more than [illegible] years after the reform. The results are qualitatively similar to those in Table [illegible], and we again note a sizeable treatment effect. In the two weeks before and after sample, mothers who had their last child immediately after the daddy quota was introduced earned [illegible] NOK (approximately [illegible] USD, controlling for age) more in [illegible] than mothers who gave birth to their latest child immediately before the reform.

Table [illegible] shows the labor income and personal income in the two weeks before and after sample for all the years for which we have data. For *Labor income* we see a statistically significant difference between the groups for all but two years after [illegible]. Since we do not have data on labor income before [illegible], it is reassuring to see that *Personal income* is never statistically significantly different between the groups before the reform, but that the difference is statistically significant in [illegible] out of [illegible] years after the reform.

Table [illegible] Treatment effects by year for the two weeks before and after sample obtained from [illegible] regressions. Dependent variables are yearly labor income (for all years after [illegible]) and yearly personal income (for all years).

Year	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Labor income	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Personal income	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Year	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Labor income	[illegible]	[illegible]	[illegible]	[illegible]	missing	missing	missing	missing	missing
	[illegible]	[illegible]	[illegible]	[illegible]					
Personal income	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Robust standard errors in parentheses
[illegible] p [illegible] p [illegible] p [illegible]

To take advantage of the longitudinal feature of the data, we estimate a difference in differences model with the two weeks before and after sample, where the differences in earnings from [illegible] to [illegible] for the treatment group are compared to the same differences in earnings for the control group. The results are presented in Table [illegible]. Since we do not have data on our preferred variable for earnings before [illegible], the first column shows the results when we take the difference between *Labor income* in [illegible] and *Personal income* in [illegible]. The results are similar to the treatment effect obtained above (in Tables [illegible] and [illegible]). When using our inferior measure of earnings in both [illegible] and [illegible], we see that the results point in the expected direction but that the treatment effect is smaller (possibly since the variable is censored) and it is not statistically different from zero.

Table [illegible]: Difference in differences estimates, comparing differences in earnings between treatment and control cohorts from [illegible] to [illegible]

	(1)	(2)
VARIABLES	Labor income [illegible] Personal income [illegible]	Personal income
Treatment	[illegible]	[illegible]
	[illegible]	[illegible]
Constant	[illegible]	[illegible]
	[illegible]	[illegible]
Observations	[illegible]	[illegible]
R-squared	[illegible]	[illegible]

Robust standard errors in parentheses

[illegible] p [illegible] p [illegible] p [illegible]

We have seen that the results hint at a potentially large, although somewhat imprecisely estimated, effect of the reform that is in most cases statistically significant for both outcome measures. There are, however, still some concerns that need further investigation. First of all, we note that the treatment effect is systematically larger in the two-week sample than in the other samples. This is worrisome as we do not want our results to be driven by outliers close to the cutoff or, even worse, by strategic birth planning. To reduce the influence of potential outliers, we estimate the models with logged dependent variables. In fact, we take log[illegible]earnings, in order not to drop individuals with zero earnings (and thereby condition on a possibly endogenous variable). Table [illegible] presents the results, and we note that while the results point in the same direction, they are no longer statistically significant for *Mean Earnings* and not statistically significant in the two weeks before and after sample for *Earnings 2005*. We thereby conclude that the results for *Mean Earnings* are not robust to a log transformation. This will be examined further in Section [illegible].

Table [illegible] regressions of log earnings on the treatment variable for different time windows.

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
VARIABLES	Earnings [illegible] [illegible] months	Earnings [illegible] [illegible] weeks	Earnings [illegible] [illegible] weeks	Mean earnings [illegible] months	Mean earnings [illegible] weeks	Mean earnings [illegible] weeks
Treatment	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Constant	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Observations	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
R-squared	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Robust standard errors in parentheses
[illegible] p [illegible] p [illegible] p [illegible]

[illegible]1 Calendar effects

As discussed in the empirical strategy section, we may worry that the effect is driven by a calendar effect where those treated have younger children at all times of measurement since earnings are measured at the same times for both groups. Thus, we also use the month before and the month after implementation of the reform as a basis for placebo regressions with a two-week window before and after the reform. These results are presented in Table [illegible] below. The coefficients are statistically insignificant and they alternate in sign. As a comparison, the April (real) treatment effect is shown in Columns [illegible] and [illegible].

However, we may also worry that parents of children born in the month after the reform are different than parents with children born before the reform for other reasons than pure calendar effects. For instance, Buckles and Hungerman ([illegible]) find that the timing of births across the year in the US is dependent on mothers' social standing. In particular, children born in the winter are more likely to have unmarried, low-educated, and young mothers. These differences may for example occur if weather affects the riskiness of sexual behavior differently among different groups of women. Buckles and Hungerman ([illegible]) find, however, that the difference is driven by wanted births and no effect is documented among mothers of unplanned children. Hence, it seems to be the case that women of higher socioeconomic status have stronger preferences for non-winter births. As argued above, it is not likely that such differences exist in the two-week sample, where birth can be seen as a random event. Nonetheless, Table [illegible] presents results from [illegible] regressions for a falsification two weeks before and after sample consisting of parents having their last child around the same calendar date in [illegible].[illegible] As seen in the tables, the treatment coefficients actually point in the other direction.

Table [illegible]: results from placebo regressions one month after and one month before the true treatment. Dependent variables are Mean Earnings (Columns [illegible]) or Earnings [illegible] (Columns [illegible])

	Mean earnings			Earnings 2[illegible]		
VARIABLES	(1) (True) treatment	(2) Month after	(3) Month before	(4) (True) treatment	(5) Month after	(6) Month before
Treatment	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Mothers' age [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Constant	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Observations	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
R-squared	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Robust standard errors in parentheses
[illegible] p [illegible] p [illegible] p [illegible]

[illegible] The corresponding figures for [illegible] can not be used in this respect since there was a reform of the parental leave system was implemented on [illegible] April [illegible] as well.

Ta le [illegible] results from place o regressions with mothers having their last child within a window of two wee s efore or after April [illegible] Dependent varia les are ean earnings olumns [illegible] and Earnings [illegible] olumns [illegible]

	[illegible]	[illegible]	[illegible]	[illegible]
A A E	ean earnings for [illegible] mothers	ean earnings for [illegible] mothers	Earnings [illegible] for [illegible] mothers	Earnings [illegible] for [illegible] mothers
Treatment	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]
others' age [illegible]		[illegible]		[illegible]
		[illegible]		[illegible]
onstant	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]
servations	[illegible]	[illegible]	[illegible]	[illegible]
s uared	[illegible]	[illegible]	[illegible]	[illegible]

o ust standard errors in parentheses
[illegible] p [illegible] p [illegible] p [illegible]

2 egression discontinuity results

The results a ove are in most cases consistent with the hypothesis of a positive effect of the reform on mothers' earnings n this section we will further e amine the causal effect y treating the reform as if it were random y inspecting the discontinuity in the earnings regression at the date of reform

igures A and [illegible] show the main D results graphically using inned local averages for mean earnings A and earnings [illegible] The averages are daily unconditional means over the support of *days* and as seen in the superimposed regression lines of different polynomial orders there is a ump in mean earnings and earnings in [illegible] at day [illegible] i e on April [illegible] All umps are statistically significant at least at the [illegible] percent level more on this in the formal analysis and as can e seen in the figures the results seem to e ro ust to different polynomial specifications

Figure 2A: Mean earnings between 1995 and 2005 (1000 NOK)

Binned local averages. Bandwidth of one day (180 bins)

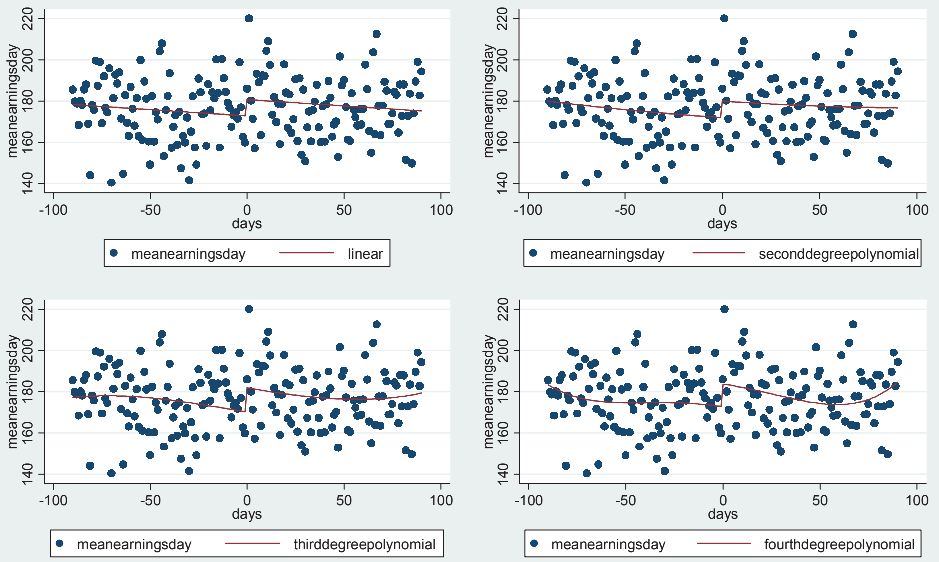
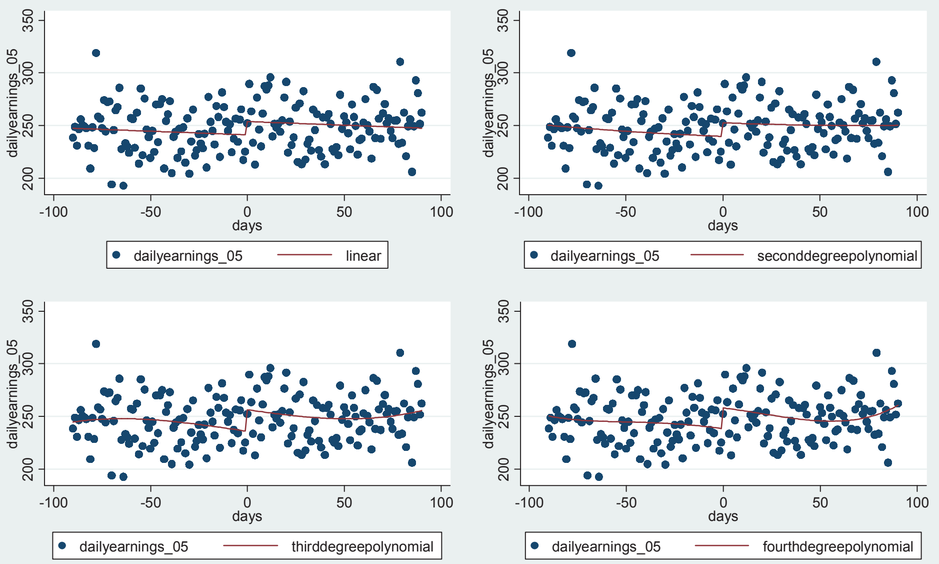


Figure 2B: Graphical representation of 2005 Earnings (1000 NOK)

Binned local averages. Bandwidth of one day (180 bins)



A ma□or advantage of the □D design is that treatment is as good as randomi□ed in cases where individuals are una□le to precisely control the forcing varia□le □□ee and □emieu□ □□□□□□As in a randomi□ed controlled trial□we can try to re□ect the assumption of randomi□ation since imprecise control has the testa□le prediction that the mean of the □aseline varia□les are continuous in *days*□□ hile we cannot directly test whether the uno□serva□le characteristics change discontinuously at the threshold□it is more unli□ely that they do so if there are no discontinuities in the o□serva□le characteristics□

□n □igure □ □elow we conduct linear graphical □D analyses on the □aseline covariates□□ e start □y showing the graph for the lagged personal income of the treatment and control mothers in the top left graph□□ince these earnings were measured in □□□□□five years □efore the mothers had their latest child□we do not e□pect any difference □etween the groups□□n fact□ we a see a small □ump in the figure even at this date although it is very small and highly statistically insignificant□The actual □D estimate using a linear model is □□□with a standard error of □□□□□ □ince personal income □□□□ and earnings after the reform are highly correlated□finding a discontinuity for the latter □ut not for the former increases our faith in the validity of the □D design□□ e also present similar graphs for mothers' age at □irth □the top right graph□and for the num□er of children □efore the reform □the □ottom left graph□□and again the data fails to re□ect the assumption of randomi□ation□The actual □D estimates have p□values of □□□ □age at □irth□and □□□ □num□er of children□□

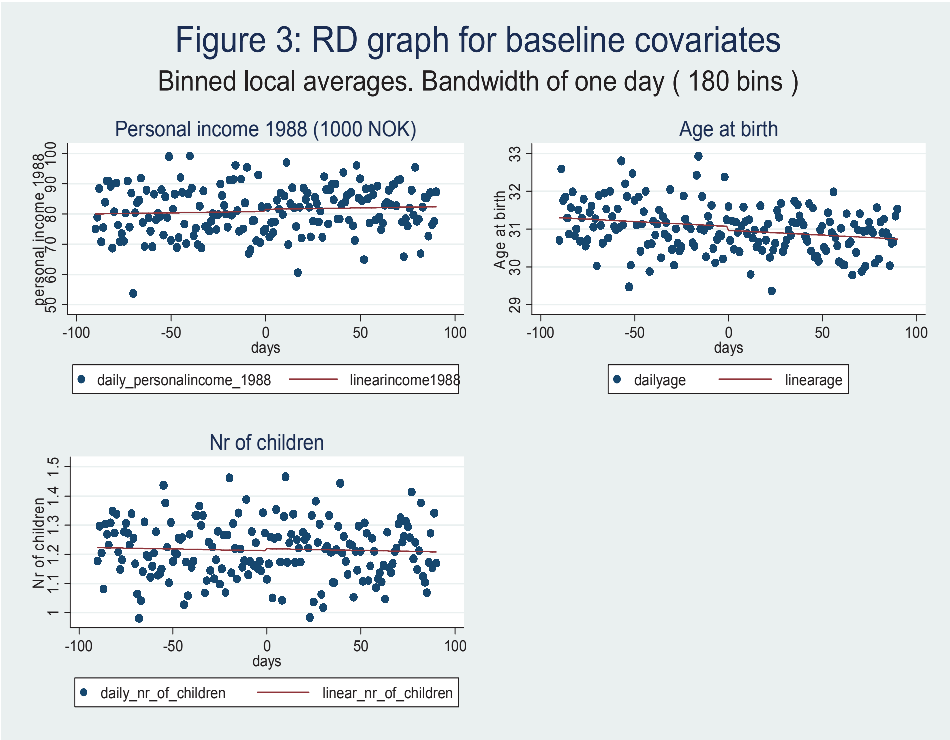


Figure 3: RD graph for baseline covariates

Binned local averages. Bandwidth of one day (180 bins)

Next we move on to the numerical results of the RD design, which are shown in Table □. The table shows the results for the three different time windows (bandwidths) and for different polynomial functions. The first row of results presents □□□ regressions without controlling for days. This amounts to a comparison of raw means between treatment and control groups. We see that the results always point in the expected direction but that the estimate fails to reach conventional levels of statistical significance when the bandwidth is three months before and after the reform for mean earnings (Column □). We then present the local linear regressions (polynomial of order one) and note that the estimates are less robust to varying the bandwidth as the statistical significance fails to reach conventional levels for half of the estimated models. We then proceed to add higher order polynomials to the regression functions in order to assess the robustness of the results. Our preferred specifications are shown in bold and two test results guide us in this choice. The first test is a goodness-of-fit test where the significance of a set of one-day bin dummies are included as additional regressors in the models and p-values of joint tests of statistical significance of these bin dummies are presented in square brackets. The decision rule is to add a higher order term to the polynomial until the □in dummies are no longer □ointly significant at the five percent level □□ee and □emieu□ □□□□□□Alternatively□we can use the A□ai□e information criterion □A□□□of model selection□which rewards goodness of fit □ut also penali□es overfitting□The preferred model according to this test is the one with the lowest A□□ value□and this is presented in the penultimate row of the ta□le □optimal order of the polynomial□□□ hen the two tests do not allow us to reach the same conclusion□as happens in □olumn □□we give priority to the first test□That is□if the function with the optimal order of the polynomial also passes the goodness of fit test□then it is preferred□otherwise we add polynomials until the first test is passed□

The sensitivity of the □D results can also □e assessed □y including □aseline covariates□□ e see in Ta□le □that adding age at the time of □irth and num□er of children □efore □□□□ does not alter the discontinuity results of the preferred specifications□This is interpreted as an additional test of whether the o□serva□le characteristics are distri□uted smoothly around the threshold and the finding raises our confidence in the no manipulation assumption□Ne□t□we add personal income in □□□□as an additional regressor and note that the results are ro□ust to this inclusion as well □e□cept for the three□month sample of earnings in □□□□□which has a p□value slightly a□ove □□□□□□inally□□ee and □ard □□□□□□suggest that when the forcing varia□le is discrete□a parametric approach with clustered standard errors is preferred in order to reflect the imperfect fit of the function away from the threshold□□ur forcing varia□le□*days*□is indeed discrete□and we therefore also run a regression including □aseline control varia□les □including also personal income in □□□□□ and cluster the standard errors at days □cf□Do□□in and □erreira □□□□□who also use daily age as the forcing varia□le in identifying the effects of school entry laws□□□omparing the standard errors to those of the previous model without clustered standard errors□we see that they are similar although the treatment effect in the two□wee□ sample for earnings in □□□□is now only statistically significant at □□percent□

Table [illegible] Regression discontinuity results. Dependent variables are Mean earnings (Columns [illegible]) or Earnings [illegible] (Columns [illegible])

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Polynomial of order:	Mean earnings Three months	Mean earnings Six weeks	Mean earnings Two weeks	Earnings [illegible] Three months	Earnings [illegible] Six weeks	Earnings [illegible] Two weeks
Zero	[illegible]	[illegible]	**1**[illegible]	[illegible]**2**[illegible]	[illegible]	**1**[illegible]**2**[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
One	[illegible]	[illegible]	[illegible]	[illegible]	**1**[illegible]**2**[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Two	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Three	[illegible]	**31**[illegible]**4**[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Four	**2**[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Including baseline controls	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Including personal income in [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Including controls and clustering standard errors at days	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Optimal order of the polynomial	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Observations	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Notes: Robust standard errors in parentheses [illegible] p [illegible] p [illegible] p [illegible] values in square brackets from a goodness-of-fit test where the significance of a set of one-day bin dummies included as additional regressors is jointly tested. The optimal order of the polynomial is obtained by Akaike's information criterion. The preferred specifications using these criteria are presented in bold. The baseline controls include mothers' age at birth and number of children before the reform, and the next row adds to these controls individual personal income in [illegible]. The final row of results presents results where the standard errors are clustered at *days*.

[illegible] Further robustness checks

[illegible]1 Extreme observations and strategic birth planning

The results so far indicate that the treatment effect of the reform is not very robust to various choices of the researcher. We examine this further in this section. That the results for *Mean earnings* are not robust to a log transformation points to problems of outliers. To explore this, we examine how sensitive the estimates are to the exclusion of extreme observations, i.e., more specifically the one percent in our sample with the lowest and highest earnings.

Table [illegible] shows that some estimates are sensitive to exclusion of extreme observations. The results still point in the expected direction, yet it seems as if the previous estimates were biased upwards, especially if we include those with very high earnings. Together with the fact that the treatment effect increases as we narrow the time window, this points toward outliers close to the cutoff. While we have argued that the exact birth date in a narrow interval is close to random since a birth can not be postponed and since triggering of birth is not likely due to the strictly better parental leave conditions after the reform, it is still possible that people have postponed triggering births. For example, a mother planning a caesarian section may want to have it done after the reform instead of before. To assess this type of fine tuning, we exclude individuals within a range of three days before and after the reform. The logic is that while the triggering may be postponed, it is unlikely to be postponed for a longer period (cf. Lalive and Zweimüller [illegible]).

Table [illegible] regressions of earnings on the treatment variable for different time windows excluding extreme observations

Panel A: Dependent variable is Mean earnings [illegible]

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible] months excluding [illegible] low	[illegible] weeks excluding [illegible] low	[illegible] weeks excluding [illegible] low	[illegible] months excluding [illegible] high	[illegible] weeks excluding [illegible] high	[illegible] weeks excluding [illegible] high
Treatment	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Mother's age [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Constant	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Observations	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
R-squared	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Robust standard errors in parentheses

[illegible] p [illegible] p [illegible] p [illegible]

Panel B: Dependent variable is Earnings in [illegible]

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible] months excluding [illegible] low	[illegible] weeks excluding [illegible] low	[illegible] weeks excluding [illegible] low	[illegible] months excluding [illegible] high	[illegible] weeks excluding [illegible] high	[illegible] weeks excluding [illegible] high
Treatment	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Mother's age [illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Constant	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Observations	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
R-squared	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Robust standard errors in parentheses

[illegible] p [illegible] p [illegible] p [illegible]

The results for our dependent variables and different time windows are presented in Table [illegible] As is evident in the table, the results are not robust to the exclusion of mothers giving birth in the days just around the cutoff. This finding supports that of [illegible]ools et al [illegible] and although it may be a coincidence it may also be an effect of strategic birth planning. Although we are somewhat skeptical of the strategic birth argument since the dates of planned caesarian sections is a medical decision, these results nevertheless imply that we cannot rule out that it indeed occurred.

Table [illegible]: [illegible] regressions of earnings on the treatment variable for the different time windows, excluding observations three days before and after the cutoff date. Dependent variables are Earnings [illegible] (Columns [illegible]) and Mean income (Columns [illegible]).

VARIABLES	[illegible] Earnings [illegible] [illegible] months	[illegible] Earnings [illegible] [illegible] weeks	[illegible] Earnings [illegible] [illegible] weeks	[illegible] Mean income [illegible] months	[illegible] Mean income [illegible] weeks	[illegible] Mean income [illegible] weeks
Treatment	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Constant	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Observations	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
R-squared	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Robust standard errors in parentheses
[illegible] p[illegible] p[illegible] p[illegible]

[illegible]2 Approximating eligibility rules

Not all parents in our treatment group were eligible for paid parental leave, as eligibility was contingent on mothers working at least [illegible] percent of full-time in six out of the ten months before the child's birth. In addition, the father had to have had paid work in at least six out of the last ten months to be eligible. Although we unfortunately do not have data on eligibility or parents' working hours, we can use the information on personal income in [illegible] to create a proxy for eligibility. To exclude mothers and fathers who are not eligible for parental leave because of a weak attachment to the labor market, we follow Rege and Solli ([illegible]) and limit our sample to cases where the mother had earnings above an indexed minimum of the basic amount ([illegible] NOK [illegible]) in [illegible] and the father had earnings twice this level.

Table [illegible] regressions of earnings on the treatment variable for the different time windows, excluding parents with a weak attachment to the labor market. Dependent variables are Earnings [illegible] (columns [illegible]) and Mean income (columns [illegible]).

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	Earnings [illegible] [illegible] months	Earnings [illegible] [illegible] weeks	Earnings [illegible] [illegible] weeks	Mean income [illegible] months	Mean income [illegible] weeks	Mean income [illegible] weeks
Treatment	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Constant	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Observations	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
R-squared	[illegible]	[illegible]	[illegible]			

Robust standard errors in parentheses
[illegible] p [illegible] p [illegible] p [illegible]

As seen in Table [illegible], the treatment effect does not seem very robust to this restriction as it is mostly statistically insignificant and varies a great deal with respect to the bandwidth chosen. Together, the results of the further robustness tests suggest that the treatment effects found in Table [illegible] are sensitive, not only to extreme observations and to observations closely around the reform date, but also to the exclusion of parents who are potentially not eligible for paid parental leave.

[illegible] Conclusion

This paper is motivated by the questions of why mothers have lower earnings than childless women and whether parental leave for fathers can reduce women's child penalty. Previous research has argued that the negative correlation between motherhood and earnings is due to the negative influence of career interruption on human capital formation, selection into motherhood, or mothers specializing in domestic work. Accounting for selection into motherhood and distinguishing between the human capital and the specialization hypothesis are empirically challenging tasks, not only because it is difficult to discover exogenous changes in the incentives to take parental leave but also because gathering the empirical data to assess the impact of such a change is difficult.

We take advantage of the introduction of a Norwegian parental leave reform that implied that parents with children born on or after [illegible] April [illegible] had access to seven additional weeks of parental leave, of which four weeks were reserved for the father (a so-called daddy quota). We have access to register data with exact birth dates for all children born around the reform and their mothers' earnings development over a long period, which allows us to establish the composite causal effect of the reform on earnings. According to the human capital depreciation hypothesis, we should expect a negative effect of the reform on mothers' earnings since the leave period was extended, while according to the specialization hypothesis, the effect is likely to be positive as the daddy quota decreases mothers' specialization into child rearing.

We find that the mothers in the treatment group have higher mean earning in the [illegible]-[illegible] period and higher earnings in [illegible] than mothers in the control group. Results from a regression discontinuity analysis suggest that there is a jump in earnings as a consequence of the reform. The size of the treatment coefficient and the level of statistical significance do, however, vary somewhat depending on the time window chosen. Moreover, the estimate of the earnings effect of parental leave is sensitive to extreme observations, in particular to the exclusion of the one percent highest earners in our data, and the estimate is sensitive to the exclusion of parents suspected not to be eligible for paid parental leave. In addition, the estimate is sensitive to the exclusion of observations three days before and after the reform. This last finding implies that we cannot rule out strategic birth planning even though the parents had no way of knowing about the reform at the date of conception. A possible mechanism may be postponement of planned caesarian sections. The sensitivity of the estimates to the exclusion of observations in the days around the threshold is even more puzzling as we could not reject the assumption of randomization by testing the continuity of the baseline covariates. Hence, the sensitivity may simply be a coincidence and we urge future research to investigate the effects of parental leave reforms on fine tuning of the birth date. Generally, we urge researchers to examine very closely whether sorting can potentially pollute identification in regression discontinuity settings, even in cases like ours where sorting seems very unlikely.

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Paper

Do laws affect attitudes?

An assessment of the Norwegian prostitution law using longitudinal data*

Andreas Kotsadam and Niklas Jakobsson*

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).[1]

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.[2] 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

[1] 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)). Vereeck and Vrolix (2007) also show that the social willingness to comply with the law affect the behavior altering effects of laws.

[2] The study by Soss and Schram (2007) was inspired by the claim that changes in policies create new politics (e.g. Schattschneider 1935; and Pierson 1993).

not identify causal effects.[3] 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.

There is also a literature on attitudes toward prostitution among the general public (e.g. Basow and Campanile 1990; Cotton et al. 2002; Kousmanen 2010; Jakobsson and Kotsadam 2011). Jakobsson and Kotsadam (2011), investigate attitudes in Norway and Sweden and argue that the criminalization of buying sex may have changed attitudes in Sweden but they can not make a causal argument since they only have data from one point in time. Kousmanen (2010) also finds, in a nation-wide survey, that some Swedes claim to have changed attitudes as a result of the Swedish legislation. While these studies point in the direction of that the criminalization of buying sex in Sweden affected attitudes, none of them can rule out simple time trends or differences across countries.

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. This approach combines a comparison over time, which by itself may simply reflect a general time trend, with a comparison across countries, which is also insufficient on its own since it may simply reflect other differences between countries that have nothing to do with legal change. The method is further explained in section 4. 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.

[3] How the ban of parental corporal punishment has affected public opinion is another example of a debated issue (e.g. Straus 1994; Roberts 2000; and Durrant 2003) due to the problems of establishing causality.

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, i.e. they facilitate claims about causes and effects. 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 of 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 in addition to 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. According to Cooter (1998), 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, the law expresses what society considers to be acceptable, and may thus induce individuals' to act and think according to the law (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.

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) 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.[4]

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,

[4] For more information on the data, see Jakobsson and Kotsadam (2010 and 2011).

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 (2011), 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 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.

		Norway		Sweden	
Variable	Explanation	**Wave 1**	**Wave 2**	**Wave 1**	**Wave 2**
Selling wrong	Answer to the question "*In your opinion, is it morally acceptable or morally unacceptable to sell sex?*" ranging from 0 for *Totally morally acceptable* to 10 for *Totally morally unacceptable.*	6.269 (3.170)	6.117 (3.085)	6.728 (3.158)	6.540 (3.107)
Buying wrong	Answer to the question "*In your opinion, is it morally acceptable or morally unacceptable to buy sex?*" ranging from 0 for *Totally morally acceptable* to 10 for *Totally morally unacceptable.*	6.822 (3.132)	6.770 (3.088)	7.403 (2.986)	7.439 (2.903)
Illegal selling	= 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)
Illegal buying	= 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)
Male	= 1 if respondent is male	0.457 (0.498)		0.497 (0.500)	
Age	respondent age	37.525 (13.458)		42.403 (13.928)	
Capital	= 1 if respondent lives in the capital city	0.122 (0.327)		0.199 (0.400)	
Cohabit	= 1 if respondent is married or cohabiting	0.655 (0.476)		0.673 (0.4694)	
High education	= 1 if respondent has at least some university education	0.529 (0.499)		0.457 (0.498)	
Low education	= 1 if respondent only has elementary education or less	0.080 (0.272)		0.164 (0.370)	
High income	= 1 if respondent earns >45,000 SEK per month, or >600,000 NOK per year.	0.077 (0.267)		0.032 (0.177)	
Low income	= 1 if respondent earns <20,000 SEK per month, or <200,000 NOK per year.	0.245 (0.430)		0.385 (0.487)	
Religious	= 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)
Trust	Answer to the question "*In general, do you trust politicians*?" ranging from 0 for *Not at all* to 10 for *Very much.*	4.322 (2.032)	4.652 (2.039)	4.579 (2.025)	4.972 (2.026)
Anti immigration	Answer to the question "*Do you think that there are too many foreigners in Norway/Sweden?*" ranging from 0 for *No, not at all* to 10 for *Yes, for sure.*	3.610 (2.755)	3.277 (2.728)	4.544 (2.852)	4.426 (2.835)
Public sector	Answer to the question "*How large should the public sector be?*" ranging from 0 for *Much smaller than today* to 10 for *Much larger than today.*	4.730 (1.775)	4.775 (1.675)	5.244 (1.769)	5.347 (1.746)
Gender equality	Answer to the question "*Do you think that gender equality is an important issue?*" ranging from 0 for *No, not at all* to 10 for *Yes, for sure.*	8.368 (2.138)	8.617 (1.983)	8.879 (1.905)	8.926 (1.848)
Co-	Answer to the question "*Do you think women who dress challengingly are*	2.050	2.173	1.764	1.757

responsible if abused	*co-responsible if they become sexually abused?*" ranging from 0 for *No, not at all* to 10 for *Yes, for sure.*	(2.753)	(2.843)	(2.679)	(2.678)
Sexual liberal	Answer to the question "*Do you think it is okay to have sex with unknown people?*" ranging from 0 for *No, not at all* to 10 for *Yes, for sure.*	4.838 (3.445)	5.000 (3.413)	5.975 (3.559)	6.044 (3.492)
Know 1	= 1 if Swedish respondent answers yes "*To your knowledge, is it illegal to buy sex?*", and no to "*To your knowledge, is it illegal to sell sex?*" in the first wave of the survey. Or if Norwegian respondent answers no to "*To your knowledge, is it illegal to buy sex?*" and no to "*To your knowledge, is it illegal to sell sex?*" in the first wave of the survey	0.428 (0.495)		0.624 (0.485)	
Know 2	= 1 if respondent answers yes to "*To your knowledge, is it illegal to buy sex?*" and no to "*To your knowledge, is it illegal to sell sex?*" 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. This is generally better than a simple comparison over time, which may simply reflect a general time trend, or a simple comparison across countries, which may simply reflect other differences between countries that have nothing to do with the legal change. The approach combines the two methods of analysis in order to make more robust causal claims (see for instance Cameron and Trivedi 2005 or Wooldridge 2008 for an introduction to the difference-in-differences methodology).

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:

$$Y_{i\square} - Y_{i\square} = \beta_{\square} + \beta_{\square} N_i + \beta_2 \mathbf{Z}_{i\square} + \beta_{\square} \square \mathbf{X}_{i\square} - \mathbf{X}_{i\square} \square + \varepsilon_i \,, \tag{1}$$

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).[5] 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, 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. ε_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.[6] Our main variable of interest is the coefficient for the Norway dummy, which is our difference-in-differences (dd) estimate

[5] In theory, ordered logit regressions may be preferable since the dependent variable is an ordered count variable, as it takes on integers values between zero and ten. Ordered logit regressions yield very similar results as the OLS estimates (available upon request) and we prefer the latter for their ease of presentation.
[6] The full regression tables are presented in Appendix.

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 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 we do not find any evidence that the law did affect moral attitudes toward selling sex in Norway in the general population.

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).[7] 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[8]), 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.

[illegible] 2 [illegible]

	(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	0.116	0.264	0.023	0.156
	(0.119)	(0.120)	(0.186)	(0.143)	(0.228)
$\mathbf{Z}_{i0}$	YES	YES	YES	YES	YES
$\mathbf{X}_{i1}$-$\mathbf{X}_{i0}$	NO	YES	YES	YES	YES
Observations	2104	2067	862	1323	598
Panel B. Difference in moral attitudes toward selling sex.					
Norway	0.098	0.136	0.273	0.142	0.097

[7] We also conducted the same analysis with the trust question from the first wave of the survey, and the results were very similar.

[8] 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).

	(0.125)	(0.126)	(0.193)	(0.151)	(0.229)
Z_{i0}	YES	YES	YES	YES	YES
X_{i1}-X_{i0}	NO	YES	YES	YES	YES
Observations	2098	2062	860	1318	597

Panel C. Difference in attitudes toward criminalization of buying sex.

Norway	0.014	0.016	0.098***	0.023	0.061
	(0.020)	(0.021)	(0.032)	(0.025)	(0.040)
Z_{i0}	YES	YES	YES	YES	YES
X_{i1}-X_{i0}	NO	YES	YES	YES	YES
Observations	2103	2063	859	1319	596

Panel D. Difference in attitudes toward criminalization of selling sex.

Norway	0.037*	0.037*	0.100***	0.063**	0.062
	(0.021)	(0.022)	(0.035)	(0.027)	(0.042)
Z_{i0}	YES	YES	YES	YES	YES
X_{i1}-X_{i0}	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 ΔTrust, ΔReligious, ΔPublic sector, ΔGender equality, ΔCo-responsible, ΔAnti immigration and ΔSexual 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, ΔTrust 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%.

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 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,[9] 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 [illegible]

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.

[9] Pro Sentret is an 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.

We see that for all specifications, the coefficient of age is positive, hence, the change in opinion in favor of criminalization increases with age in our control group. The Norway indicator variable interacted with age is negative and statistically significant for the two specifications regarding buying sex.[10] 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. We also note that education level does not seem to affect the changes in attitudes, which is important considering our biased sample.

[illegible]

	(1) Buying wrong	(2) Selling wrong	(3) Illegal buying	(4) Illegal selling
Norway	0.440	0.816	0.094	0.048
	(0.496)	(0.521)	(0.084)	(0.089)
Age	0.025***	0.012*	0.003**	0.003**
	(0.006)	(0.007)	(0.001)	(0.001)
Age*Norway	-0.017*	-0.015	-0.004**	-0.002
	(0.010)	(0.010)	(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 (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%.

[10] 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 age*Norway in the specification on thinking that buying sex is wrong moves from being significant at the 10 % level to being significant at the 13 % level.

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 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.[11]

[illegible]

	(1) Buying wrong	(2) Selling wrong	(3) Illegal buying	(4) Illegal selling
Oslo	0.509*	0.289	0.134**	0.041
	(0.288)	(0.322)	(0.054)	(0.058)
$\mathbf{Z_{i0}}$	YES	YES	YES	YES
$\mathbf{X_{i1}}$-$\mathbf{X_{i0}}$	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 ΔTrust, ΔReligious, ΔPublic sector, ΔGender equality, ΔCo-responsible, ΔAnti immigration, and ΔSexual 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 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).

[11] These effects are calculated using ordered probit regressions (results available upon request).

We also compare the changes in attitudes in Oslo to the changes in the rest of Norway. 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. It could also be the case (as pointed out by an anonymous referee) that time variant unobservable differences drive the results. One such potential is if the media debate differed between Oslo and the rest of Norway, in quantity of reports or in the nature of coverage of prostitution. In that case, our results suggest that proximity and visibility are important factors but we can not discriminate between them.

[illegible]

	(1) Buying wrong	(2) Selling wrong	(3) Illegal buying	(4) Illegal selling
Oslo	0.468	0.269	0.088*	0.019
	(0.315)	(0.301)	(0.047)	(0.049)
$\mathbf{Z_{i0}}$	YES	YES	YES	YES
$\mathbf{X_{i1}}$-$\mathbf{X_{i0}}$	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 ΔTrust, ΔReligious, ΔPublic sector, ΔGender equality, ΔCo-responsible, ΔAnti immigration, and ΔSexual 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, we do not find any evidence that the law did affect moral attitudes toward prostitution in the general Norwegian population. However, in the Norwegian capital, where prostitution was more visible before the reform, it seems as if 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 2011). 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. Furthermore, it is possible that the Norwegian law affected the Swedish media debate as well, which would further underestimate our findings. 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. All 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. More research on the links between attitudes and behavior is also needed, especially regarding this particular legal change as the aim of the lawmakers was to change attitudes *in order to* decrease demand.

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Appendix. Full tables

Table A1. Difference in moral attitudes toward buying sex.

	(1) Base	(2) Full	(3) Trust	(4) Know 2	(5) Know 2+Trust
Norway	0.088	0.116	0.264	0.023	0.156
	(0.119)	(0.120)	(0.186)	(0.143)	(0.228)
Male	0.156	0.148	0.247	0.075	0.124
	(0.116)	(0.117)	(0.179)	(0.140)	(0.219)
Age	0.017***	0.016***	0.023***	0.018***	0.026***
	(0.005)	(0.005)	(0.007)	(0.006)	(0.009)
High education	0.038	0.040	0.246	0.069	0.248
	(0.121)	(0.122)	(0.192)	(0.146)	(0.238)
Low education	-0.078	-0.039	-0.127	0.018	0.222
	(0.194)	(0.197)	(0.337)	(0.255)	(0.430)
High income	-0.220	-0.107	-0.344	0.210	-0.369
	(0.258)	(0.262)	(0.380)	(0.306)	(0.467)
Low income	0.290**	0.257*	0.496**	0.205	0.445*
	(0.137)	(0.139)	(0.213)	(0.167)	(0.265)
Capital	0.259*	0.237	0.082	0.183	0.086
	(0.154)	(0.155)	(0.228)	(0.176)	(0.271)
Cohabit	0.253**	0.254**	0.291	0.016	0.023
	(0.125)	(0.126)	(0.191)	(0.151)	(0.235)
ΔTrust		-0.001		0.058	
		(0.037)		(0.045)	
ΔReligious		-0.137	0.278	0.067	0.438
		(0.361)	(0.560)	(0.431)	(0.679)
ΔPublic sector		0.040	0.062	0.029	0.037
		(0.039)	(0.074)	(0.051)	(0.097)
ΔGender equali.		0.019	0.044	0.038	0.050
		(0.035)	(0.063)	(0.044)	(0.084)
ΔCo-responsib.		-0.036	-0.026	-0.031	-0.017
		(0.026)	(0.041)	(0.033)	(0.053)
ΔAnti immigrat.		-0.022	-0.025	-0.015	-0.006
		(0.026)	(0.041)	(0.032)	(0.052)
ΔSexual liberal		-0.077***	-0.074**	-0.063**	-0.094**
		(0.021)	(0.035)	(0.026)	(0.041)
Constant	-1.106***	-1.076***	-1.653***	-0.900***	-1.342***
	(0.261)	(0.264)	(0.389)	(0.311)	(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) Base	(2) Full	(3) Trust	(4) Know 2	(5) Know 2+Trust
Norway	0.098	0.136	0.273	0.142	0.097
	(0.125)	(0.126)	(0.193)	(0.151)	(0.229)
Male	0.007	0.002	0.014	-0.054	-0.054
	(0.122)	(0.122)	(0.186)	(0.148)	(0.221)
Age	0.006	0.005	0.014*	0.004	0.016*
	(0.005)	(0.005)	(0.008)	(0.006)	(0.009)
High education	0.080	0.052	0.436**	0.222	0.337
	(0.127)	(0.127)	(0.200)	(0.154)	(0.239)
Low education	0.039	0.056	0.327	0.187	0.681
	(0.204)	(0.205)	(0.350)	(0.269)	(0.432)
High income	0.173	0.216	0.298	0.202	-0.242
	(0.271)	(0.273)	(0.395)	(0.322)	(0.469)
Low income	0.031	-0.005	0.117	0.024	0.093
	(0.145)	(0.145)	(0.223)	(0.177)	(0.267)
Capital	0.159	0.181	0.311	0.189	0.235
	(0.161)	(0.161)	(0.237)	(0.185)	(0.272)
Cohabit	0.020	-0.004	-0.032	-0.273*	-0.453*
	(0.132)	(0.132)	(0.199)	(0.159)	(0.236)
ΔTrust		-0.005		0.041	
		(0.038)		(0.048)	
ΔReligious		-0.163	-0.182	0.075	0.026
		(0.380)	(0.582)	(0.453)	(0.682)
ΔPublic sector		0.092**	0.039	0.067	-0.024
		(0.041)	(0.077)	(0.054)	(0.098)
ΔGender equali.		0.016	0.089	0.047	0.050
		(0.036)	(0.066)	(0.047)	(0.084)
ΔCo-responsib.		0.001	0.013	0.009	0.076
		(0.027)	(0.042)	(0.035)	(0.053)
ΔAnti immigrat.		-0.026	0.006	-0.028	0.040
		(0.027)	(0.043)	(0.034)	(0.052)
ΔSexual liberal		-0.105***	-0.116***	-0.105***	-0.121***
		(0.022)	(0.036)	(0.027)	(0.042)
Constant	-0.550**	-0.467*	-1.189***	-0.347	-0.753
	(0.274)	(0.276)	(0.406)	(0.328)	(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) Base	(2) Full	(3) Trust	(4) Know 2	(5) Know 2+Trust
Norway	0.014	0.016	0.098***	0.023	0.061
	(0.020)	(0.021)	(0.032)	(0.025)	(0.040)
Male	0.012	0.016	0.055*	0.005	0.060
	(0.020)	(0.020)	(0.031)	(0.024)	(0.038)
Age	0.001	0.001	0.003**	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
High education	0.014	0.015	0.012	0.000	0.022
	(0.021)	(0.021)	(0.034)	(0.026)	(0.041)
Low education	0.001	-0.005	-0.016	-0.021	0.052
	(0.033)	(0.033)	(0.059)	(0.044)	(0.074)
High income	0.037	0.044	0.054	0.084	0.054
	(0.044)	(0.045)	(0.066)	(0.053)	(0.081)
Low income	0.027	0.024	0.059	-0.012	0.031
	(0.023)	(0.024)	(0.037)	(0.029)	(0.046)
Capital	0.035	0.029	0.011	0.020	0.009
	(0.026)	(0.026)	(0.040)	(0.031)	(0.047)
Cohabit	0.001	-0.002	-0.009	0.005	0.002
	(0.021)	(0.021)	(0.033)	(0.026)	(0.041)
ΔTrust		0.006		0.021***	
		(0.006)		(0.008)	
ΔReligious		-0.027	0.074	-0.151**	-0.036
		(0.061)	(0.097)	(0.075)	(0.117)
ΔPublic sector		0.002	-0.010	0.002	-0.016
		(0.007)	(0.013)	(0.009)	(0.017)
ΔGender equali.		0.002	-0.005	0.007	-0.004
		(0.006)	(0.011)	(0.008)	(0.015)
ΔCo-responsib.		-0.004	-0.008	-0.008	-0.007
		(0.004)	(0.007)	(0.006)	(0.009)
ΔAnti immigrat.		-0.002	-0.010	-0.007	-0.008
		(0.004)	(0.007)	(0.006)	(0.009)
ΔSexual liberal		-0.011***	-0.016**	-0.010**	-0.020***
		(0.004)	(0.006)	(0.004)	(0.007)
Constant	-0.079*	-0.082*	-0.212***	-0.060	-0.160**
	(0.045)	(0.045)	(0.068)	(0.054)	(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) Base	(2) Full	(3) Trust	(4) Know 2	(5) 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) Buying wrong	(2) Selling wrong	(3) Illegal buying	(4) Illegal selling
Norway	0.440	0.816	0.094	0.048
	(0.496)	(0.521)	(0.084)	(0.089)
Male	0.255*	0.046	-0.012	-0.008
	(0.152)	(0.160)	(0.026)	(0.027)
Age	0.026***	0.012*	0.003**	0.003**
	(0.006)	(0.007)	(0.001)	(0.001)
High education	-0.016	0.063	-0.003	0.015
	(0.164)	(0.172)	(0.028)	(0.030)
Low education	-0.209	-0.028	-0.042	0.020
	(0.239)	(0.252)	(0.041)	(0.043)
High income	0.048	0.720	0.083	-0.010
	(0.421)	(0.442)	(0.072)	(0.077)
Low income	0.237	0.128	0.029	-0.002
	(0.170)	(0.179)	(0.029)	(0.031)
Capital	0.113	0.089	-0.002	0.025
	(0.187)	(0.197)	(0.032)	(0.034)
Cohabit	0.070	0.026	-0.018	-0.041
	(0.168)	(0.177)	(0.029)	(0.030)
Age*Norway	-0.017*	-0.015	-0.004**	-0.002
	(0.010)	(0.011)	(0.002)	(0.002)
Male*Norway	-0.204	-0.036	0.065	0.069
	(0.237)	(0.249)	(0.040)	(0.043)
Highe*Norway	0.090	0.010	0.024	-0.009
	(0.244)	(0.257)	(0.042)	(0.044)
Lowe*Norway	0.202	0.046	0.091	-0.066
	(0.428)	(0.450)	(0.073)	(0.077)
Highi*Norway	-0.347	-0.859	-0.081	-0.050
	(0.535)	(0.563)	(0.091)	(0.097)
Lowi*Norway	0.154	-0.280	-0.019	0.004
	(0.290)	(0.306)	(0.050)	(0.052)
Capital*Norway	0.331	0.132	0.092	-0.007
	(0.334)	(0.351)	(0.057)	(0.060)
Cohab*Norway	0.413	-0.035	0.042	0.082*
	(0.252)	(0.265)	(0.043)	(0.045)
Constant	-1.318***	-0.855**	-0.114**	-0.124**
	(0.329)	(0.346)	(0.056)	(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*	0.289	0.134**	0.041
	(0.288)	(0.322)	(0.054)	(0.058)
Male	0.220	0.039	-0.003	0.000
	(0.135)	(0.151)	(0.025)	(0.0270)
Age	0.022***	0.009	0.003**	0.002
	(0.006)	(0.006)	(0.001)	(0.001)
High education	-0.091	0.019	0.012	0.018
	(0.146)	(0.163)	(0.027)	(0.029)
Low education	-0.167	0.048	-0.030	0.029
	(0.217)	(0.242)	(0.041)	(0.044)
High income	0.112	0.556	0.059	-0.064
	(0.350)	(0.391)	(0.066)	(0.071)
Low income	0.131	0.084	0.018	-0.012
	(0.151)	(0.170)	(0.028)	(0.030)
Capital	0.103	0.150	-0.012	0.022
	(0.173)	(0.193)	(0.032)	(0.035)
Cohabit	0.141	-0.019	-0.010	-0.020
	(0.147)	(0.164)	(0.028)	(0.030)
ΔTrust	-0.017	-0.047	-0.002	0.004
	(0.042)	(0.047)	(0.008)	(0.008)
ΔReligious	0.101	0.185	-0.043	-0.060
	(0.414)	(0.469)	(0.078)	(0.085)
ΔPublic sector	0.069	0.119**	-0.002	0.006
	(0.049)	(0.055)	(0.009)	(0.010)
ΔGender equali.	0.015	-0.017	0.007	0.014
	(0.045)	(0.051)	(0.009)	(0.009)
ΔCo-responsib.	-0.030	0.004	0.001	0.003
	(0.032)	(0.036)	(0.006)	(0.006)
ΔAnti immigrat.	-0.032	-0.037	0.003	0.002
	(0.031)	(0.034)	(0.006)	(0.006)
ΔSexual liberal	-0.062**	-0.068**	-0.012**	-0.012**
	(0.025)	(0.028)	(0.005)	(0.005)
Constant	-1.131***	-0.674**	-0.121**	-0.110*
	(0.297)	(0.332)	(0.056)	(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	0.269	0.088*	0.019
	(0.315)	(0.301)	(0.047)	(0.049)
Male	-0.011	-0.022	0.058*	0.066**
	(0.209)	(0.199)	(0.031)	(0.032)
Age	0.009	-0.002	-0.001	0.000
	(0.009)	(0.008)	(0.001)	(0.001)
High education	0.072	0.050	0.020	-0.000
	(0.207)	(0.197)	(0.031)	(0.032)
Low education	0.087	0.041	0.023	-0.062
	(0.413)	(0.394)	(0.061)	(0.064)
High income	-0.148	-0.122	0.009	-0.071
	(0.383)	(0.365)	(0.057)	(0.060)
Low income	0.319	-0.188	0.011	0.009
	(0.270)	(0.258)	(0.040)	(0.042)
Cohabit	0.463**	-0.065	0.019	0.030
	(0.215)	(0.205)	(0.032)	(0.033)
ΔTrust	-0.006	0.056	0.020**	0.015
	(0.065)	(0.063)	(0.010)	(0.010)
ΔReligious	-0.409	-0.370	0.0450	0.037
	(0.615)	(0.587)	(0.092)	(0.095)
ΔPublic sector	0.040	0.081	0.006	-0.001
	(0.063)	(0.060)	(0.009)	(0.010)
ΔGender equali.	0.019	0.032	-0.002	-0.004
	(0.053)	(0.051)	(0.008)	(0.008)
ΔCo-responsib.	-0.026	0.018	-0.009	-0.001
	(0.042)	(0.040)	(0.006)	(0.006)
ΔAnti immigrat.	-0.043	-0.023	-0.009	-0.005
	(0.047)	(0.045)	(0.007)	(0.007)
ΔSexual liberal	-0.092**	-0.141***	-0.008	-0.015**
	(0.039)	(0.037)	(0.006)	(0.006)
Constant	-0.809*	0.025	-0.016	-0.068
	(0.426)	(0.406)	(0.063)	(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%.

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JEL classification: [illegible]2 [illegible]22

Keywords: [illegible]

* [illegible] *Feminist Economics* [illegible]

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Figure 1

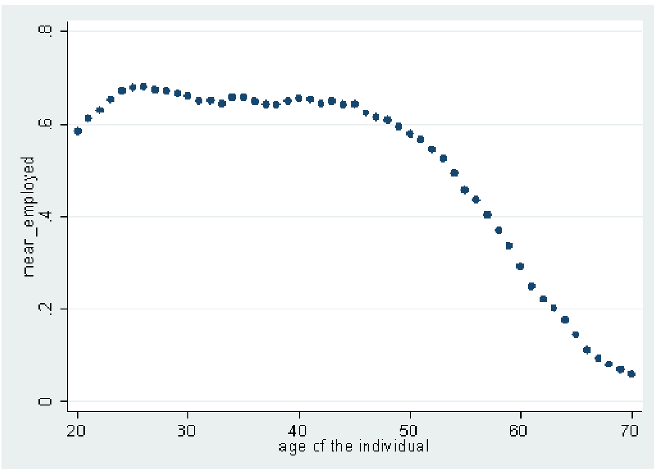
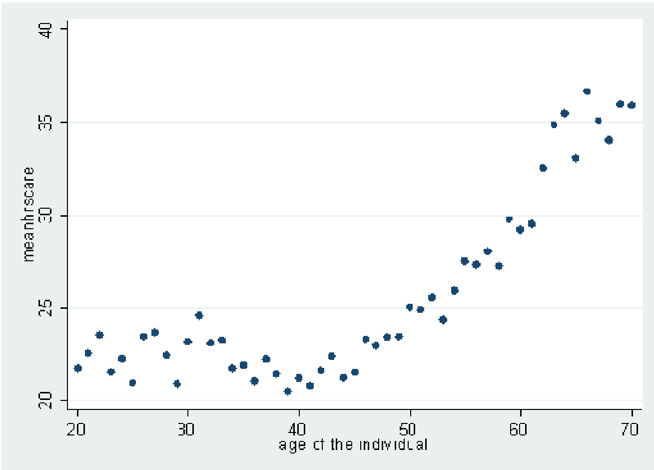


Figure 2



Source

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Table 1

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employed

hrsworked

carehrs

care

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divorced	[illegible]
widow	[illegible]
single	[illegible]
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agesq	[illegible]
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age4	[illegible]
age5	[illegible]
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age7	[illegible]
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age9	[illegible]
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edu2	[illegible]
edu3	[illegible]
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wage	[illegible]
hwage	[illegible]
ch	[illegible]

Table 2 [illegible]

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employed	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
hrsworked	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]					
care	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
carehrs	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]					
married	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
divorced	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
widow	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
single	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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hhsize	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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Table 3 [illegible]

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employed	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
hrsworked	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]					
care	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
carehrs	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]					
married	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
divorced	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
widow	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
single	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
age	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
agesq	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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edu2	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
edu3	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
badh	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
hwage	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
hhsize	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
ch	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Source [illegible]

Table 4 Employed, care [illegible] *carehrs* [illegible]

[illegible]	[illegible]	[illegible]	[illegible]	[illegible] *carehrs* [illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	2[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]2[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	2[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Source [illegible]

[illegible]

[illegible] 2 [illegible]

[illegible] 2 [illegible] 2 [illegible]

Table 5

2

[illegible]

[illegible] 2 [illegible] 2 [illegible]

[illegible] 2 [illegible] 2 [illegible] 2 [illegible] 2 [illegible] 2 [illegible] 2 [illegible]

[illegible]

[illegible]

[illegible]

$$employed = x = \alpha + \beta\, care + \boldsymbol{\beta}$$

$$employed = x = \alpha + \beta\, carehrs + \boldsymbol{\beta}$$

[illegible] *employed* [illegible] *care* [illegible] *carehrs* [illegible] x [illegible][2] [illegible]

[illegible] $=$ [illegible] β [illegible] G [illegible] β [illegible] x [illegible] *carehrs* [illegible] *care* [illegible] $=$ [illegible] $\beta +$ [illegible] c_i [illegible]

[illegible] $\sum_{t=1}^{T} y_{it}$ [illegible] 2 [illegible]

[2] [illegible]

[illegible] *care* [illegible] *carehrs* [illegible] *care* [illegible] *carehrs* [illegible].[8]

Table 6 [illegible] *care* [illegible] *carehrs* [illegible] *employed*

[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
care						
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
carehrs						
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Note [illegible] *age*, *agesq*, *hhsize*, *hwage*, *ch* [illegible]

[8] [illegible]

carehrs

c_i x_{it}

age

agesq,

Table 7 [illegible] *care* [illegible] *carehrs* [illegible] *employed*

care

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

carehrs

	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Notes [illegible] *age* [illegible] *agesq* [illegible] *hhsize* [illegible] *hwage* [illegible] *ch* [illegible]

[illegible] *care,* [illegible] *carehrs* [illegible] *care.* [illegible]

[illegible]

c_i x_{it}

Table 8 *care* *carehrs* *employed*

care

carehrs

Notes *age* *agesq* *hhsize* *hwage* *ch*

care

carehrs

carehrs

care *carehrs*

[illegible] 2 [illegible]

Table 9 [illegible] *care* [illegible]

	[illegible]	2	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
care	[illegible]2[illegible]	[illegible]2[illegible]	[illegible]	[illegible]2	[illegible]2[illegible]	[illegible]2[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	2[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Notes [illegible] *wage* *age* *agesq* *hhsize* *hwage* *ch* [illegible]

Table 10 [illegible] *care* [illegible]

	[illegible]	2	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
care	[illegible]2[illegible]	[illegible]	[illegible]2[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	2[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Notes [illegible] *wage* *age* *agesq* *hhsize* *hwage* *ch* [illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

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[illegible] *Journal of European Social Policy* [illegible] 2 [illegible]

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Health Economics 22 [illegible]

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[illegible] 2 [illegible] *Journal of Health Economics* 2 [illegible]

[illegible] 2 [illegible] 2 [illegible]

[illegible] 2 [illegible] *Journal of Health Economics* 2 [illegible] 2 [illegible]

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[illegible] 2 [illegible] *Nordic Welfare States in the European Context* [illegible]

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[illegible] 2 [illegible] *Journal of Aging Studies* [illegible]

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[illegible] *Journal of Human Resources* 2 [illegible] 2 [illegible]

[illegible] [redacted]

The employment costs of caregiving in Norway

[illegible]*

Abstract

[illegible]

JEL classification: [illegible]2 [illegible]22

Keywords: [illegible]

* Norwegian Social Research, [illegible]22[illegible]2[illegible].and Department of Economics, the University of Gothenburg, Sweden, Box 640, SE-405 30, Gothenburg, Sweden. E-mail andreas.kotsadam@economics.gu.se. The paper has benefited from comments by seminar participants at the Institute for Social Research (ISF). I would also like to thank Dominique Anxo, Henning Finseraas, Lennart Flood, Niklas Jakobsson, Viggo Nordvik, and Måns Söderbom for useful comments. The research reported in this paper was supported by a grant from The Research Council of Norway (project EqualCare 196425/V50), which is gratefully acknowledged.

1. Introduction

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[illegible]
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2. Context

3. Data, sample, and descriptive statistics

[illegible] 2 [illegible] [2]

[illegible] 2 [illegible]

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[illegible] 2 [illegible] 2 [illegible]

[2] [illegible]

[illegible] 2 [illegible] 2 [illegible] 2 [illegible] 2 [illegible]

[illegible]

[illegible] 2 [illegible] 2 [illegible]

[illegible]

[illegible]

Dependent variables

Main independent variables

2

Control variables

2

Possible instruments

[illegible] 2 [illegible]

	Obs	Mean	Std. Dev.	Min	Max
Dependent variables					
employed	12683	0.85	0.36	0	1
hrswork	10630	3.56	0.44	0	4.62
wage	10682	12.25	1.84	0	16.08
Main independent variables					
care	11082	0.09	0.28	0	1
intensive_care	11045	0.02	0.14	0	1
Control variables					
badhealth	12703	0.04	0.21	0	1
woman	12752	0.51	0.50	0	1
age	12752	42.14	13.02	18	65
agesq	12752	19.45	11.04	3.24	42.25
higed	12752	0.37	0.48	0	1
meded	12752	0.44	0.50	0	1
lowed	12752	0.19	0,39	0	1
married	12751	0.49	0.50	0	1
widow	12751	0.01	0.12	0	1
divorced	12751	0.11	0.32	0	1
single	12751	0.38	0.48	0	1
children	12752	0.28	0.45	0	1
partnerincome	11858	8.12	6.03	0	16.18
Possible instruments					
fatherage	12613	74.11	15.81	22	126
motherage	12676	70.72	15.13	36	118
motherbadhealth	12681	0.27	0.44	0	1
fatherbadhealth	12591	0.15	0.36	0	1
siblings	11736	2.34	1.53	0	18
fatherbadmemory	12692	0.03	0.16	0	1
motherbadmemory	12738	0.05	0.21	0	1
fainneed	12575	0.05	0.21	0	1
moinneed	12666	0.11	0.31	0	1
livemother	12752	0.01	0.10	0	1
livefather	12752	0.01	0.10	0	1
Type of care					
within household(hh)	11084	0.03	0.17	0	1
outside hh	11091	0.06	0.23	0	1
within hh intensive	10453	0.01	0.12	0	1
outside hh intensive	10755	0.01	0.09	0	1

[illegible]

	non-caregiver	caregiver	intensive caregiver
Dependent variables			
employed	0.85	0.84	0.78**
hrswork	3.57	3.54**	3.57
wage	12.29	12.29	12.21
Control Variables			
badhealth	0.04	0.05	0.07*
woman	0.51	0.62***	0.63***
age	43.09	45.49***	46.43***
agesq	20.18	22.14***	22.82***
higed	0.36	0.36	0.33
meded	0.45	0.43	0.43
lowed	0.19	0.21	0.24**
married	0.56	0.58	0.60
widow	0.01	0.02	0.02
divorced	0.12	0.15**	0.19**
single	0.30	0.25***	0.19***
children	0.33	0.27***	0.31
partnerincome	9.41	9.37	9.54
Possible instruments			
fatherage	74.99	77.87***	78.91***
motherage	71.56	74.74***	75.77***
motherbadhealth	0.27	0.36***	0.38***
fatherbadhealth	0.16	0.18**	0.23**
siblings	2.37	2.42	2.50
fatherbadmemory	0.03	0.06***	0.09***
motherbadmemory	0.05	0.11***	0.10***
fainneed	0.04	0.11***	0.14***
moinneed	0.10	0.23***	0.24***
livemother	0.01	0.02**	0.05***
livefather	0.01	0.01	0.03**

[illegible]

4. Empirical strategy

[illegible]

$$hrsworked\,(wage) = \alpha + \beta\,care + \mathbf{\beta x} + \varepsilon \quad [illegible]$$

$$[illegible] = [illegible] = [illegible]\,\alpha + \beta\,[illegible] + \mathbf{\beta x} > \varepsilon \quad (2)$$

[illegible] *hrsworked* [illegible] *wage* [illegible] *employed* [illegible] *care* [illegible] $\mathbf{x}$ [illegible] ε [illegible]

[illegible] *wage* [illegible] *hrsworked* [illegible] 2 [illegible] $= [illegible] = [illegible] \mathbf{x\beta}$ [illegible] $G(\mathbf{x\beta})$ [illegible] $\mathbf{x}$ [illegible] *care* [illegible]

[illegible] $Cov(x, \varepsilon) =$ [illegible] *care* [illegible] 2 [illegible]

5. General results

[illegible] 2 [illegible]

[illegible]

[illegible]

[illegible]

[illegible] *care* [illegible] *intensive_care* [illegible]

[illegible]

6. *Treating intensive caregiving as endogenous in the employment equation*

[illegible]

[illegible] *intensive_care* [illegible]

[illegible] *employed* [illegible] *intensive_care* [illegible]

[illegible]

[illegible] 2 [illegible] 2 [illegible] 2 [illegible] 2 [illegible][1]

[illegible] $\chi =$ [illegible] 2 [illegible] *intensive_care* [illegible] *intensive_care* [illegible] $\chi =$ [illegible] 2 [illegible] *intensive_care* [illegible] 2 [illegible]

[1] [illegible] 2 [illegible] 2 [illegible] 2 [illegible] *Intensive_care* [illegible] 2 [illegible] 2 [illegible]

care

intensive_care

7. Effects of caregiving on employment for different types of caregivers

intensive_care χ =

[illegible] *employed* [illegible] *intensive_care* [illegible]

[illegible]

8. Sensitivity analysis:

8.1 Different operationalizations of intensive care

[illegible] *intensive_care* [illegible]

[illegible]

[illegible]

[illegible]	[illegible] *Intensive_care*	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
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	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

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8.2. Heterogeneous responses and local average treatment effects of the IV estimator

9. Discussion

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2 **Ambjörn, Erik** [illegible] 2 [illegible]
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