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1. Introduction

Family policies are often argued to change gender relations in a society, but credible evidence is largely lacking. Cross-national differences in to what degree men participate in housework co-vary with family policy differences across countries (e.g., Fuwa and Cohen, 2007; Hook, 2006, 2010; Ruppander, 2010), but a causal interpretation of this correlation is problematic. Since countries do not implement parental leave arrangements randomly, we should worry that other differences between the countries drive both patterns of household work and leave regulations, and that the patterns in household work we are trying to explain drives the leave regulations we are proposing to be a causal factor. In the present study we instead analyze differences in household work across people within the same country, where the only plausible difference is exposition to different parental leave rules.

Paternal involvement in childrearing is increasing (Hook 2010), and several countries have policies to increase it by means of father quotas, i.e. weeks of parental leave reserved for the father. Yet, only a few studies have investigated the effects of these reforms, and most of them study labor market outcomes of parents (Cools et al. 2011; Johansson 2010; Kotsadam et al. 2011; Rege and Solli 2010). However, Ekberg et al. (2006) and Kotsadam and Finseraas (2011) examine the effects of paternity leave on household division of labor. While Ekberg et al. (2006) find no effects of the first Swedish daddy month in 1994 on subsequent leave taken for sick children, Kotsadam and Finseraas (2011) find an effect of the first Norwegian daddy month in 1993 on the division of household labor 13 years later. More specifically, Kotsadam and Finseraas (2011) find that couples with children born after the reform have fewer conflicts about household work and that they share household tasks more equally.[1]

As among adults, female teenagers do more household work than their male counterparts (Brannen 1995; Gager et al. 1999; Dodson and Dickert 2004). The family is important in shaping these conditions, partly as children inherit social norms and cultural beliefs from their parents (e.g., Farré and Vella 2007; Fernández 2007a; Guiso 2006; McHale et al. 1999). It is frequently argued that family policies have important consequences for family decisions (Dauphin et al. 2011), which are essential for the degree of gender equality (e.g., Sullivan et al. 2009). Consequently researchers

[1] A plausible interpretation of the different findings is that while Kotsadam and Finseraas (2011) rely on various survey items of household division of labor, Ekberg et al.'s (2006) proxy for household work, i.e., leave to take care of sick children, also involves a relationship with employers.

have begun to study the impact of parental leave on children (Baker and Milligan 2010, 2011; Carneiro et al. 2010; Cools et al. 2011; Dustmann and Schonberg 2008). Only one study (Cools et al. 2011) has investigated the effects of *paternity* leave on children.

In the present study, we ask whether parental leave shape the decision to conduct household work and the amount of household work conducted by adolescents. We exploit a Norwegian parental leave reform in 1993, which increased the parental leave time by seven weeks of which four were reserved for the father, to evaluate the long-term effects on the affected children’s household work.

We build on a simple model contrasting within-family socialization with the parent’s need for help to derive testable hypotheses on the mechanisms for how paternity leave can influence the amount of household work conducted by adolescents. In accordance with empirical findings (Kotsadam and Finseraas 2011), we believe that the paternity leave reform equalized the division of household work between the mother and the father. If the more equal division of household work is being transmitted to the next generation as parents transfer their norms and practices to their children, we would expect that the parental leave reform has gendered effects so that it decreases girls’ household work, increases boys’ household work, or both. If, however, the more equal division of parent’s work simply reduced the need for children’s work we would expect a similar decrease in household work for both boys and girls.

The 1993 reform did not only implement a father’s quota, but also extended the total parental leave time. To isolate the effects of paternity leave from a general increase in parental leave, we also investigate the effects of a parental leave reform in 1992 that increased the general parental leave by three weeks, without any reservation for the father. Since this reform increased mothers’ time at home, we expect opposite effects compared to our expectations regarding the paternity leave reform.

We find a robust and substantive effect of the 1993 reform implying that adolescent girls born immediately after the reform are less likely to be *involved* in household work. The results regarding the *amount* of household work point in the same direction but are less precisely estimated. Analyzing the two reforms together in a difference-in-differences estimation makes us more confident that the results are driven by the daddy quota, and our results from only examining the 1992 reform further indicate that the general increase in parental leave may reduce the amount of household work conducted by boys. Our results indicate that the socialization and need mechanisms interact, as the gendered nature of the effects gives support for the socialization mechanism, while the reduction of the total amount of children's household work gives support to the need mechanism.

2. The reform and testable hypotheses

2.1. The Norwegian parental leave scheme and the two reforms

Norway, like the other Scandinavian countries, has for decades operated what has been labeled a women-friendly welfare state (Hernes 1987) where equal opportunities in employment and domestic work have been central topics. In Norway, paid parental leave has a long history, and three historical shifts can be identified (NOU 1996). The parental leave system was first justified based on mothers' health-related necessity to be absent from work, and to compensate for lost income in connection with pregnancy and care for small children. A six-week paid maternity leave was introduced as far back as in 1909 and a 12-week paid maternity leave was introduced in 1946, but only for women with health insurance. In 1956, sickness insurance became compulsory for all employed citizens, and thus a 12-week paid maternity leave became available to all working women.

The second shift started in the late 1960s, when the public debate concerning a further increase in the number of days turned from protection of women's health and employment to equal rights in the labor market. In 1977, fathers gained the right to use parental leave as the parental leave was expanded to 18 weeks where only six weeks after the birth were reserved for mothers. During the 1980s, the number of weeks was increased in several rounds.

The 1990s represent the third shift in Norwegian family-work policies as the parental leave policy turned from equal rights to equal opportunities. From 1990 to 1992, the right to take paid parental leave was gradually extended from 28 to 35 weeks. In particular, on April 1st, 1992 a reform was introduced that increased the paid parental leave from 32 to 35 weeks.

For policy-makers it was disappointing to observe that an overwhelming majority of the parental leave was used by mothers (NOU 1995). To increase fathers' take-up rates, Norway was the first country in the world to introduce a "daddy quota" on April 1st, 1993, where fathers to children born at or after this date got an independent right to parental leave. The reform extended the parental leave from 35 to 42 weeks with full earnings compensation,[2] of which four weeks were reserved for the father.[3] At this time, paid paternity leave was contingent on both parents working at least 50 percent before the child was born, and the payment to fathers was reduced if the mother did not work full time. In addition, fathers were not eligible for paid parental leave unless they had worked at least six out of the last ten months. Fathers were entitled to use the daddy quota up until the child turned three years old, although 95 percent of those taking leave in 1993-1995 did so during the child's first year (Rege and Solli 2010).

Inducing fathers to take more responsibility for childrearing was seen as an important step on the way to equal division of labor and toward reducing the gender wage gap. The political arguments to earmark some of the parental leave for fathers were threesome; firstly, this policy implementation gives a strong signal as well as possibilities to be more actively involved in child rearing and hence to challenge norms of male breadwinning (Leira 1998). Secondly, an independent right to parental leave gives fathers an advantage when parents discuss the distribution of the parental leave between them. Thirdly, the law strengthens fathers' argument for parental leave when dealing with reluctant employers. The reform led to a sharp increase in the take-up rate from less than four percent in March 1993 to 39 percent in April 1993 and is now over 80 % (Cools et al. 2011). The average paternity leave taken by fathers to children born after the reform was five weeks and the vast majority took exactly four weeks. On average, the fathers started their leave when the child was nine months old. There was no obligation for the mother to return to work when the father was home on parental leave, and a survey conducted in 1995 shows that 35% of the mothers were home (for instance by taking vacation from work) during the father's leave period (Brandt and Øverli 1998).

2.2 Parental leave, the need for household work, and the within-family socialization of gender roles

The main aim of the article is to test whether parental leave reforms, and in particular a daddy quota, affect children's household work. In order to understand why and how this may happen we build on a simple model contrasting within-family socialization of gender roles with the need for

[2] Income compensation had a ceiling of six times the so-called base amount of the Norwegian social insurance system. The base amount is adjusted on a yearly basis and was 36,167 NOK (about 6,300 USD) in 1992. Most employers compensate for the amount above the ceiling.

[3] In fact, parents could choose to either take the 42 weeks with full compensation or 52 weeks with 80 % earnings compensation. The choice between taking a shorter period with full coverage and a longer period with less coverage has been available since 1989.

household work to derive testable hypotheses for the mechanisms of how parental leave might influence adolescents' household work. Blair (1992) highlights two reasons for why children do household work, namely socialization and need. Parents may assign household work to children as a socializing experience in order to promote responsibility or important gendered tasks. Alternatively, parents may use children as a labor source and assign household tasks to them since the available time the parents themselves are able or willing to allocate to household tasks is not sufficient to cover the need. These two motivations may differ across households and need not be mutually exclusive even within the same household. Nonetheless, they are the two most likely mechanisms for how changes in household work of children may appear and they both explain why the effects may be durable. As we also show, the two mechanisms yield different predictions that are used to gain insight into the reduced form results we obtain.

The first step in our model regards the impact of parental leave on the adolescents' parents. In essence, the introduction of a daddy quota challenges norms of male breadwinning (Gornick and Meyers 2008; Hook 2006; 2010). Within the family, the daddy quota effectively increases the time fathers' spend with the child during the child's first years and improve the father-child relationship. Beyond the quota's impact on the father-child-relationship, we argue that the quota had an impact on the division of household work between the mother and the father. The quota improves the bonding between father and child, which could affect attitudes toward activities traditionally performed by women, and men who are exposed to non-traditional experiences are more likely to change their views on gender equality (Klein 1987: 35). Even though the paternity leave only covers a short period, it intervenes at a critical time for renegotiating household work (e.g. Hook 2010). Fathers are expected to do more household work during the leave and arguments of returning to traditional gender roles after the leave thus lack credibility. Kotsadam and Finseraas (2011) find, in line with our argument, survey data evidence of more equal sharing of household tasks when comparing the division of household work of parents with access to the daddy quota (i.e. with children born right after the implementation of the reform) and parents without this access (i.e. children born right before the reform).

The second step in our theoretical argument regards the impact on the children. One mechanism for how paternity leave may affect children's household work is if the father's increase in household work substitutes for children's household work. A testable hypothesis derived from this mechanism is whether the total amount of household work reduces for the children as a result of the daddy quota.

A second mechanism is that the paternity leave affects the gender socialization of children. It has been argued that children's participation in household work is particularly important to study as it is one area where gender roles are clearly spelled out and visible across social classes (Raley and Bianchi 2006: 406). The family is probably the most important area for social learning, and we may expect that the more equal division of household work among parents with access to the daddy quota, is being transmitted from one generation to the next (see e.g., Farré and Vella 2007 and McHale et al. 1999 for similar arguments). The socialization are likely to occur if the parents' transfer their own gender practices to their children by making boys conduct more household work or making girls conduct less. A testable hypothesis derived from this mechanism is therefore whether we observe a less gendered pattern of children's household work in families where the parents had access to the daddy quota.

Hence, in order to differentiate between the two main mechanisms for household work of the adolescents we test the following two hypotheses:

1) The daddy quota reduces children's household work similarly for both sexes.
2) The daddy quota affects the gendered pattern of children's household work without affecting the total amount.

As these two mechanisms are not mutually exclusive a third hypothesis is that they work together leading to the following hypothesis:

3) The daddy quota reduces children's household work and this decline follows the gendered dimensions outlined above.

To be even more specific we may expect the two features of the 1993 reform, i.e., the more general parental leave and the daddy quota, to work in opposite directions. When more general time is given to parents, it has been the case that the extended time is used by the mother. When mothers' increase their relative time at home, we expect a reinforcement of the existing gendered patterns of household work. On the other hand, by increasing the time fathers stay home, we expect traditional gender roles to be challenged.

We expect the daddy quota aspect of the reform to be of more importance for gender roles than the general increase in parental leave. The marginal effect of increasing the general parental leave by three weeks from 35 weeks is likely to be smaller than increasing father's time at home from a modal zero to a modal of four weeks (Cools et al. 2011). We also analyze the effects of the 1992 reform, which only increased the general parental leave. A corollary expectation is that this reform has less impact, and in the opposite direction, than the father's quota.

Theoretical models of gender socialization give prominence to the family setting an while no one is arguing that the family is the only arena for socialization, the family is considered central (see Owen Blakemore and Hill 2008 for an overview). Hence, we assume that the family is *one* important area of socialization of household work, which is in fact sufficient for our mechanisms to come into play. Of course children with parents having access to the daddy quota will mix and interact with children socialized in families without daddy quota experience, and their parents will also interact. Attitudes and practices can therefore spread from one group to the other. This implies that we investigate whether the reform creates a change in gender roles over and above the possible effects of the reform on gender roles in the total population, i.e. in addition to peer effects and other societal changes. Hence, the total effects of the reform are likely to be even larger than what we identify here.

There is no denying that the causal chain we propose is long, involving changes among the parents in practices and attitudes, which in turn affect their children. The empirical analysis is however in "reduced form" in the sense that irrespective of the large and complex chain of events, we are able to credibly assess whether there is any effect of the reform. This assessment is a crucial first step. The nature of the empirical analysis implies that we do not establish the exact mechanism linking the reform to children's household work. Nonetheless, by using the hypotheses above we are able to speak to the two most likely reasons that the reform would have an effect and see whether one of these two mechanisms work in isolation, or whether they work together to produce results with a high level of internal validity.

3. Data and descriptive statistics

We rely on data from the 2010 edition of the Young in Norway study, which is a cross-sectional study of students in elementary school, junior high school, and senior high school. In total, 11,659 students aged 12-19 participated, with a response rate of 73 percent. Since we are mainly interested in the 1992 and 1993 year cohorts, we rely mostly on data from the senior high schools, for which the response rate was 84 percent. The questionnaires were completed from January to March 2010.

The most important feature of this dataset is that we have access to exact birthdates of the respondents so that we are able to construct treatment and control groups based on when the child was born. Those born on or after April 1st are the treatment group since the parents of these students were treated by the reform. Those born before April 1st constitute the control group since the parents of these students were not treated by the reform. To our knowledge, this dataset is the only available dataset that allows us to investigate the effects of parental leave on children's participation in household work.

Our main variable of interest is a survey question that asks how many times in the last seven days the respondent did household work. The question included three examples of such tasks, namely washing, cleaning, and shoveling snow (the survey was distributed during winter). We recode this variable into two dependent variables, first, a binary recoding of those who report that they did household work in the last seven days (*household_work*=1) against those who report that they did not (*household_work* =0). Second, we analyze the logged number of times the respondent did household work (*nr_of_times*) in the last seven days.[4] It would have been preferable to have a household work variable that separates the different types of household work, in particular since the specific tasks are gendered. This lack of separation does not affect the identification of causal effects, as it is the same question for all groups, but it would be useful in terms of identifying the mechanisms. Ideally, we would have had a measure of the amount of time spent on housework as a complement to the number of times the respondent did household work. Unfortunately we do not, and this is the only dataset we know of that includes exact birthdates of children born around the reform date. While we readily acknowledge that more specific outcome variables would be preferred, the ability to credibly assess the causal effects makes using this dataset strictly advantageous to using other datasets.

Table 1 presents the mean values of our dependent variables for the group affected by the 1993 policy and for the control group. The numbers for nr_of_times are presented in unlogged form to ease the interpretation of the results. We focus on three different time windows in order to show the robustness of the results. The first time window is three months before and after April 1st

[4] We have experimented with recoding the amount of household work into a categorical variable (none, low amount, high amount). Recoding the variable in this manner produces similar results as for the logged number of times.

(n=620). This is our longest possible time window as we want the respondents to be in the same school year, which has a cut-off on the last day of the year. We also present results from two shorter time windows, namely one month before and after April 1st (n=224) and two weeks before and after April 1st (n=99).

Our first finding is that a lower proportion of respondents in the treatment group report that they conducted household work in the last seven days. This is the case in all windows. We also find that respondents in the treatment group do household work less frequently, although this difference is not statistically significant in the three-month sample.

Table 2 presents descriptive statistics of other variables by treatment/control group (three-month window). These variables are the gender of the respondents and their parents' characteristics (all variables are described in the online Appendix in connection to Table A1 and the control variables are discussed in connection to Appendix Table B1) (REVIEWERS CAN FIND THE ONLINE APPENDIX AT THE END OF THE PAPER). Reassuringly, we find no significant or substantial differences between the groups in the mean values of these variables. Nonetheless, we present results both with and without these variables as control variables to show the robustness of the results.

[TABLE 1 ABOUT HERE]

[TABLE 2 ABOUT HERE]

In order to establish which variables are important predictors of our dependent variables, we conducted a series of regression models including all the control variables for the largest possible sample of students with equal regulation of parental leave (children born 1994-1997). The results are presented in the online appendix (Section B, Table B1).

Two findings are of particular importance. First, the gender difference is robust to the inclusion of the control variables. Second, and most importantly, age is not statistically significantly correlated with our dependent variables—neither in years nor in months.[5] Since we compare people born in

[5] The conclusions remain the same if we also include age squared. We also included flexible specifications for age in days, which did not show any substantial or statistically significant effects, and finally birth month fixed effects were

the second quarter of the year to those born in the first quarter, we also investigate whether there are any differences between those born in these two quarters when they have equal parental leave regulation. We fail to reject that quarter of birth does not have an impact on our dependent variables (columns 3 and 6 in Table B1), thus, we are more confident in that age differences or birth months are not driving the difference between the treatment and the control group.

4. Empirical strategy

We exploit the paternity leave reform as a natural experiment where the treatment consists of having parents affected by the new paternity leave reform, i.e., having children born on or after April 1st, 1993. Since all children born after the reform date were treated by the reform and no children born before the reform date were treated, we are able to compare the two groups of children in an attempt to identify the causal effects of the policy. We also exploit the fact that the policy process was so fast that parents giving birth around the reform threshold could not have known about the reform at the time of conception. The specific design of the reform, including April 1st, 1993 as the day of implementation, was proposed on December 1st, 1992 and decided in parliament on January 22nd, 1993.[6] We do not have information on actual take-up of parental leave. However, the main point of our empirical strategy is to exploit the exogenous variation induced by the reform implying that we do not need to condition on actual take-up. This allows us to estimate the intention-to-treat effect of the reform. If we had access to take-up at the individual level, we could have estimated the treatment effect of actually using the daddy quota with the help of an instrumental variables approach. Yet, for policymakers, the intention-to-treat effect is probably more relevant as it identifies the total effects of the reform. In the online appendix we also present the setup and results of a regression discontinuity analysis which yield the same qualitative conclusions as those presented in the main text of the article.

4.1. Local regressions

We start by running OLS regressions of the dependent variables on *Treatment*, which is an indicator variable that equals 1 for children born just after the reform in 1993, and on a vector of control variables. We rely on an OLS specification even though the dependent variable *household_work* is binary. This specification gives us the linear probability model, which has the

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included and no birth month had a statistically significant effect on the dependent variables (results are available upon request).

[6] The Norwegian Government first proposed introducing a daddy quota of four weeks in the state budget for 1993, which was accepted by the parliament on November 4th, 1992 (Budsjett-innst. S. nr.2 1992-1993). At this time, however, the exact date of implementation was not known.

advantage that the interaction terms are more easily interpreted. Furthermore, as all our covariates are discrete and the model is fully saturated in most specifications, the linear probability model is as appropriate for limited dependent variables as OLS is for continuous dependent variables (Angrist 2001).[7] We also run all regressions with a probit model and all conclusions are the same (results available upon request).

Following Angrist (2001), we choose an OLS specification also for our second dependent variable, *nr_of_times*, which is the log of number of times the respondent did household work plus 1 to retain the zeros. One alternative is to estimate a Tobit model, but we want to avoid assumptions of an underlying continuously distributed latent variable. A second alternative is to estimate the outcome only for individuals doing some amount of household work. Such conditional-on-positive effects are harder to interpret as they are not necessarily causal effects on the number of times housework is conducted for the subset of individuals that would have done household work irrespective of the reform. If the reform affects participation, we do not identify an effect *among children that do household work*, even if we condition on positive effects, as the composition of this group changes. Rather, the identified effect would be a combination of selection effects from those who would not have participated if not for the reform, and increases in the amount of household work for participants.

We always limit the sample so that we compare children born just before and just after the reform and within the same school year. We present results from different sample windows as a robustness check. The sample windows presented in the main analyses are chosen to be three months, one month, and two weeks. The two-week sample has been argued to be a random sample (e.g., Ekberg et al. 2006). In theory, using this sample corresponds well with what Rosenzweig and Wolpin (2000) label a “natural” natural experiment where nature determines the institutional setting the parents end up in, and thus allocation to either treatment or control group. First, it is not possible for parents to completely control the date of conception (Eriksson 2005; Lalive and Zweimüller 2009). Second, a pregnancy takes an average of 40 weeks and the duration is normally distributed with a standard deviation of two weeks (Ekberg et al. 2006; Eriksson 2005). Most importantly, however, none of the parents knew that they would be treated at the time of conception. Thus, it seems reasonable that the reform creates exogenous variation in own and spousal parental leave,

[7] In some specifications, the full vector of controls is included and the model is then not fully saturated (as we do not include all possible interaction terms). However, as identification is not based on the conditional independence assumption, these results are merely included to show the robustness of the results.

and long-run differences in outcomes can plausibly be attributed to the change in legislation (cf. Kluve and Tamm 2009; Lalive and Zweimüller 2009). Births cannot 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 2010) should not be a problem. A problem may occur, however, if triggering of births is postponed by the reform. In fact, Cools at al. (2011) show that about 5.7 percent of the births that were predicted to occur in late March instead occurred in early April. We address this potential problem in the same vein as Cools et al. (2011) by excluding births around the two last weeks of March and the two first weeks of April 1993 as yet another robustness check.

4.2. Difference in differences

Next we present difference-in-differences estimations with children born on the same calendar dates in 1994 and 1995. Taking *number_of_times* and the difference-in-differences estimation with the 1994 cohort as an illustrative example, we estimate the following regression:

$$\mathrm{nr\_of\_times}_{icm} = \alpha + \chi \mathrm{Treatment}_{icm} + \delta \mathrm{Treatment\_months}_{m} + \mathrm{Treatment\_cohort}_{c} + \varepsilon_{icm},$$

where the subscript c accounts for different cohorts (i.e., 1993, 1994, 1995) and the subscript m refers to which months the individuals were born (January-March or April-June). *Treatment_months* equals one for those born in the months after the reform and zero for those born in the months before the reform. *Treatment_cohort* equals one for those born in 1993 and zero for those born in 1994. χ is our parameter of main interest as *Treatment* equals one for those being exposed to the reform in 1993. Note that this variable is the same as an interaction term between *Treatment_months* and *Treatment_cohort*. The advantage of the difference-in-differences estimation is that it eliminates the within-year differences in age between treatment and control individuals since the difference between treated and control children is compared to the difference of children with the same age difference in other years.

Finally, we also analyze the 1992 reform, which increased the general parental leave by three weeks. First we examine the effects of the 1992 reform in a set of local regressions as with the 1993 reform. We then proceed to use the two reforms in a difference-in-differences estimation. Remember, in contrast to the 1993 reform, which increased the parental leave by a total of seven weeks but tied four weeks to the father, the 1992 reform added three weeks of general parental leave but did not entail any daddy quota. We can therefore use the two reforms to cancel out the general increase in parental leave, and the remaining difference pertains to the four weeks of paternity leave (see Cools et al., 2011 for a similar strategy). A crucial assumption in order for this strategy to identify the causal effect of the daddy quota is that there are no other differences between the children born after and before the reform in 1993 that do not appear in 1992 as well.[8]

5. Results

We now proceed to present the results and discuss some estimation issues. The section follows the same disposition as the empirical strategy. For space reasons, the control variables are not presented, and the full set of tables are found in the online Appendix, section B.

5.1 Results from the local regression analysis

Table 3, panel A, presents the OLS results with *household_work* as the dependent variable. The first column includes the treatment status as the only independent variable and shows that individuals in the treatment group are about eight percentage points less likely to report that they conducted any household work in the last seven days. In column 2 we add gender (Boy=1) and its interaction with treatment status, which shows that girls in the treatment group are about 12 percentage points less likely to report doing housework compared to girls in the control group, a difference that is strongly statistically significant. The treatment effect differs between boys and girls by nine percentage points, but this difference is only statistically significant when adding control variables and then only marginally so. In fact, boys in the treatment group are only about three percentage points less likely to report doing housework compared to boys in the control group, a difference that is not statistically significant.[9] In other words, the reform appears to have reduced the probability of doing housework among girls, yet this decrease is not matched by an increase among boys. This finding supports the socialization theory as the results are gendered and it also supports the need theory as the total probability of doing household work is lower in the treatment group. In essence it gives support to our third hypotheses above that the mechanisms work together.

[TABLE 3 ABOUT HERE]

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[8] As with the daddy quota reform, the 1992 reform was also announced very close to its implementation (it became public information in October 1991). Hence, the children born around the reform date were already in utero at the time of the announcement.

[9] Keep in mind that the difference between treated boys and boys in the control group, and the accompanying significance test, is not directly observable in Table 4. We have therefore re-estimated the model with boys in the control group as the reference category to get a direct estimate of this difference. The resulting table is available upon request.

The coefficients do not change much when we add a vector of plausibly exogenous control variables (column 3) or the full set of control variables (column 4),[10] while columns 5 and 7 show that the treatment effect becomes even more negative when we decrease the time window to one month and two weeks, respectively. Finally, column 9 shows that the treatment effect is only statistically significant at the 10 percent level, somewhat smaller compared to what we find in the main window, and substantially smaller compared to the smaller windows, when we exclude the two weeks around the reform date, which are potentially polluted due to strategic birth planning (Cools et al. 2011). The effect by gender in the smaller samples (in columns 6, 8, and 10) show a similar pattern as in the three-month sample.

Panel B of Table 3 shows the OLS regression results for the logged number of times household work was conducted in the last week (*nr_of_times)*. The results point in the same direction as the results on the probability of doing household work, but the coefficients are imprecisely estimated and often not statistically significant.

One potential problem for identification of causal effects of the reform is that children born after the reform are slightly younger than children born before the reform. Thus, it is very important to make sure that our treatment effect is not biased by a spurious age effect. Reassuringly, we find no statistically significant age differences on household work (see previous discussion in section 3). In addition, we conduct placebo analyses where we "pretend" that the reform was introduced at times when there were no changes in parental leave legislation. We pretend that the reform happened one month before, one month after, one year after, and two years after April 1st, 1993. If the results are merely driven by age differences, we expect to find similar results in these placebo regressions since the two groups then face identical parental leave regulations. The placebo regressions in 1994 and 1995 furthermore account for possible biological or social differences between children born in the first versus the second quarter of the year. Due to space constraints we report the full results in the online appendix. Reassuringly, the placebo regressions produce insignificant treatment effects, which strengthen our confidence in the validity of the estimation strategy.

5.2 Results from the difference-in-differences analyses

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[10] The number of observations drops slightly when we include the full set of control variables. Listwise deletion of missing data might cause bias in coefficient estimates, however, but all substantive conclusions remain if we instead impute missing data (results available upon request).

Table 4 presents the difference-in-differences estimations with data on children born around April 1st, 1993 (i.e., the cohort including the treated children) and children born on the same calendar dates in 1994 and 1995. These regressions eliminate the within-year differences in age between treatment and control individuals since the difference between children in the treatment and control group is compared to the corresponding difference in other years. Thus, we further reduce the worry that age differences between the groups drive the results.

Starting with an estimation using those born in 1994 as a counterfactual group, we see a negative treatment effect on *household_work,* but it is not statistically significantly different from zero in the basic regression (column 1). Analyzing the effects by gender of the child in column 2, we again note a negative treatment effect on girls (*Treatment*) and a difference between the effects for girls and boys (*Treatment*Boys*). These coefficients are only statistically significant at the 10 percent level, however. For *nr_of_times* we find a negative, albeit statistically insignificant, treatment effect in column 3 and a corresponding statistically insignificant gender difference in column 4. We see a similar pattern when using the 1995 cohort as a counterfactual group, with the difference that the treatment effects in the *household_work* regressions seem more robust and larger in magnitude. These results increase the confidence in the previous estimations. In particular, they suggest that the differences in the probability of doing household work between treatment and control respondents in the 1993 cohort are not driven by age differences or some spurious calendar effects.

[TABLE 4 ABOUT HERE]

So far we have seen that there is a robust treatment effect, implying that those with parents affected by the 1993 reform are less likely to do household work. When disentangling the effects by gender, it becomes clear that the reform affected girls more than boys. When investigating the amount of reported household work, we find similar, but less robust, results.

Keep in mind that results so far constitute the composite causal effects of the two components of the 1993 reform (extended parental leave and the daddy quota). Thus, to conclude that these results are an effect of the daddy quota, we need to disentangle the causal effect of the daddy quota from the causal effect of extending the general parental leave. We therefore analyze the 1992 reform, which increased the general parental leave by three weeks, but did not implement a daddy quota.

Table 5 presents the results using a sample of respondents born three months before and after April 1st, 1992, which is the date of the introduction of the general reform. The treatment effects on the probability of doing household work (columns 1 and 2) are not statistically significant. Furthermore, we see that there is a difference in the treatment effect for boys and girls in the amount of household work done. The treatment effect is also statistically significant for boys; that is, boys do household work less frequently if they are born after the parental leave reform in 1992. The results thereby suggest that the increase in general parental leave reinforces traditional gender norms and propagates them across generations. As these results are not accompanied by a total decline in children's household work they more strongly suggest that it is the socialization that drives the results.

[TABLE 5 ABOUT HERE]

Finally, and in order to purge away the effects of the general parental leave increase from the effects of the 1993 daddy quota reform, we use the two reforms in a difference-in-differences estimation. Table 6 presents the results, which suggest an effect of the daddy quota on the probability of doing household work. Again we find, in column 2, a negative treatment effect on girls (Treatment). The results suggest that girls in the treatment group are about 15 percentage points less likely to do household work. The results furthermore show that the treatment effect is significantly (although at the 10 percent level in terms of statistical significance) smaller for boys than for girls, as seen by the positive interaction term for Boy*Treatment, and that the total treatment effect for boys is insignificant. The results presented in columns 3 and 4 show that the effect of the daddy quota on the amount of household work points in the same direction, but the effect is not statistically significant.

[TABLE 6 ABOUT HERE]

Conclusion

Gender inequalities in household work are a persistent feature across societies, even as women have dramatically increased their participation in the labor market. At the same time, there are cross-cultural differences, and over time men have become more involved in household work and childrearing (Hook 2006). An important question is whether policies, and family policies in particular, can affect household work and thus partly explain the pattern of change over time.

Furthermore, since gender attitudes are products of childhood socialization (McHale et al. 1999), it is important to investigate the effects of family policy on children.

We exploit two Norwegian parental leave reforms to investigate their effects on adolescents' household work. Thus, we examine the long-run effects of the reforms on an important aspect of social change. In particular, we exploit a parental leave reform that increased the parental leave time by seven weeks, of which four weeks were reserved for the father. This reform has been shown to affect the parents' household work as couples with children born just after the reform share household tasks more equally than couples with children born just before the reform (Kotsadam and Finseraas 2011).

The nature of the reforms and our data allows us to test whether the reforms have a causal effect on household work of the children born immediately before or after the reform. Building on a model contrasting within-family socialization of gender roles with the need for household work we are further able to highlight some of the pathways for how and why these long term effects may be observed. In particular we hypothesize that the more equal division of household work is being transmitted to the next generation as parents either transfer their norms and practices to their children, thus reducing the gender gap in household work among adolescents, or that the need for children's household work is reduced as the father now does more or both. Hence, the degree to which the effects of the reform are gendered allows us to draw conclusions about the mechanisms.

We find a robust and substantial effect of the reform which included the daddy quota (the 1993 reform). Girls born immediately after the reform are less likely to be involved in household work. For boys we find either a much less pronounced effect or no statistically significant effect. We reveal a similar, although not statistically significant, effect on the number of times household work was conducted in the previous seven-day period.

When analyzing the 1992 reform, which increased the parental leave period but did not include a daddy quota, we find no effects on the probability of doing household work, yet we do find that boys do household work less frequently if born just after the reform. In other words, the gendered pattern of household work was strengthened, which is to be expected since the reform reinforced traditional gender roles by increasing the time mothers spend at home.

By using a difference-in-differences estimation strategy where we analyze the effects of the two reforms simultaneously, we are able to isolate the effects of the daddy quota from the increase in the parental leave period. The results strongly suggest that the introduction of the daddy quota lowered the probability of girls doing household work. At the same time, we find no effects of the daddy quota on the frequency of household work. Together the results suggest that the daddy quota equalizes the probability of doing household work between the genders, while the effect on the number of times per week that boys do household work is uncertain. In terms of mechanisms the results therefore indicate that the need for housework theory and the socialization theory combined best explain the observed pattern.

The present article contributes to several different academic literatures. First, our results speak to the large sociological literature on the effects of parental leave on gender equality (e.g., Fuwa and Cohen, 2007; Gornick and Meyers 2008; Hook, 2006, 2010; Ruppander, 2010) by identifying a causal effect of paternity leave on the gender gap in adolescents' household work. Second, the results contribute to a small but growing literature investigating children's household work (e.g., Dauphin et al. 2011; Salman Rizavi and Sofer 2010) by illustrating the importance of public policy. Third, the article identifies a long-run effect of parental leave. While previous studies using parental leave reforms have focused on cognitive effects on the children (Baker and Milligan 2010, 2011; Carneiro et al. 2010; Cools et al. 2011; Dustmann and Schonberg 2008), our study investigates the effects on children's household work, thereby extending the outcome set of interest to also include social variables. Fourth, our results indicate that policy changes can have intergenerational effects. Intergenerational transmission of values and attitudes has been argued to be important in explaining fertility patterns and the increasing labor market participation of women (e.g., Blau et al. 2010, 2011; Fernández 2007a, 2007b; Fernández and Fogli 2006; 2009; and Fernández et al. 2004). Finally, the article shows that policy may affect cultural change and is in fact susceptible to rather small policy changes. Future studies are needed to show under which contexts and policies transformative effects are most likely to occur. Other outcome variables, such as attitudes, should also be investigated in order to grasp the total societal effects of the reforms.

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Table 1: Mean values of the dependent variables for the treatment and control groups.

Treatment group				Control group			
3 Months				3 Months			
Variable	Mean	Standard deviation	N	Variable	Mean	Standard deviation	N
household_work	0.820**	0.385	316	*household_work*	0.898	0.303	304
nr_of_times	2.326	2.151	316	*nr_of_times*	2.536	2.560	304
1 Month				1 Month			
Variable	Mean	Standard deviation	N	Variable	Mean	Standard deviation	N
household_work	0.740***	0.441	104	*household_work*	0.925	0. 264	120
nr_of_times	2.106**	2.310	104	*nr_of_times*	2.833	2.772	120
2 Weeks				2 Weeks			
Variable	Mean	Standard deviation	N	Variable	Mean	Standard deviation	N
household_work	0.768**	0.426	56	*household_work*	0.953	0.213	43
nr_of_times	1.946*	2.203	56	*nr_of_times*	2.791	2.054	43

*** p<0.01, ** p<0.05, * p<0.1 (p-values in two-sided t-tests of the difference between treatment and control groups).

Table 2: Differences in control variables between treated and control respondents.

Treatment group				Control group			
3 Months				3 Months			
Variable	Mean	Standard deviation	N	Variable	Mean	Standard deviation	N
Boy	.50	.50	337	boy	.53	.50	324
mother_foreign	.06	.24	338	mother_foreign	.06	.24	326
father_foreign	.06	.25	339	father_foreign	.07	.26	326
father_fulltime	.81	.39	339	father_fulltime	.78	.42	327
mother_fulltime	.68	.47	339	mother_fulltime	.68	.47	327
father_parttime	.08	.27	339	father_parttime	.09	.28	327
mother_parttime	.18	.38	339	mother_parttime	.19	.39	327
father_unemployed	.02	.15	339	father_unemployed	.01	.11	327
mother_unemployed	.03	.16	339	mother_unemployed	.02	.14	327
father_home	.03	.16	339	father_home	.03	.18	327
mother_home	.05	.21	339	mother_home	.07	.26	327
father_dead	.01	.08	339	father_dead	.003	.06	327
mother_dead	.01	.11	339	mother_dead	.003	.06	327
father_university	.41	.49	315	father_university	.45	.50	306
mother_university	.46	.50	313	mother_university	.48	.50	308
good_income	.71	.46	328	good_income	.76	.43	317

Table 3:

Panel A. Dependent variable *household_work*; OLS regressions.

VARIABLES	(1) basic	(2) by gender	(3) exogenous	(4) all controls	(5) one month	(6) by gender	(7) two weeks	(8) by gender	(9) reduced	(10) by gender
Treatment	-0.078***	-0.123***	-0.128***	-0.120***	-0.185***	-0.192***	-0.186***	-0.240***	-0.058*	-0.102**
	(0.028)	(0.038)	(0.038)	(0.040)	(0.049)	(0.069)	(0.066)	(0.087)	(0.030)	(0.042)
Boy		-0.056	-0.055	-0.077**		-0.044		-0.095		-0.048
		(0.035)	(0.035)	(0.037)		(0.048)		(0.065)		(0.039)
Treatment*Boy		0.089	0.100*	0.113**		0.016		0.109		0.089
		(0.056)	(0.056)	(0.058)		(0.099)		(0.133)		(0.061)
Constant	0.898***	0.926***	0.936***	0.854***	0.925***	0.947***	0.953***	1.000	0.889***	0.913***
	(0.017)	(0.022)	(0.023)	(0.076)	(0.024)	(0.030)	(0.032)	(.)	(0.019)	(0.025)
Observations	620	616	615	579	224	223	99	99	521	517
R-squared	0.013	0.017	0.028	0.043	0.063	0.065	0.066	0.074	0.007	0.011

Panel B. Dependent variable, *nr_of_times*; OLS regressions.

VARIABLES	(1) basic	(2) by gender	(3) exogenous	(4) all controls	(5) one month	(6) by gender	(7) two weeks	(8) by gender	(9) reduced	(10) by gender
Treatment	-0.066	-0.063	-0.070	-0.077	-0.250***	-0.163	-0.326***	-0.310*	-0.016	-0.019
	(0.049)	(0.067)	(0.067)	(0.069)	(0.086)	(0.121)	(0.116)	(0.161)	(0.054)	(0.073)
Boy		-0.023	-0.020	-0.063		-0.002		-0.109		-0.006
		(0.068)	(0.069)	(0.073)		(0.110)		(0.161)		(0.075)
Treatment*Boy		-0.012	0.002	0.040		-0.166		-0.017		0.002
		(0.099)	(0.099)	(0.104)		(0.172)		(0.237)		(0.108)
Constant	1.076***	1.089***	1.099***	1.063***	1.155***	1.157***	1.201***	1.254***	1.056***	1.060***
	(0.034)	(0.043)	(0.044)	(0.131)	(0.055)	(0.068)	(0.080)	(0.084)	(0.037)	(0.048)
Observations	620	616	615	579	224	223	99	99	521	517
R-squared	0.003	0.004	0.012	0.018	0.037	0.045	0.072	0.082	0.000	0.000

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Column 1 presents the basic regression with a time window of 3 months before and after the reform. In column 2 we let the treatment effect vary by gender by interacting the treatment variable with an indicator variable for being a boy In column 3 we include controls for mother_foreign, father_foreign, father_dead, and mother_dead. In Column 4 we add the additional control variables. In column 5 the time window is +/- one month around April 1st. In column 7 the time window is +/- two weeks around April 1st. In column 9, we repeat the basic regression in the three month sample, but exclude individuals born in the last two weeks of March or the first two weeks of April. Columns 6, 8, and 10 allow for gender differences by interacting Boy with Treatment.

Table 4: Difference-in-differences regressions comparing the difference in 1993 to the difference in 1994 (columns 1 to 4) and 1995 (columns 5 to 8); dependent variables *household_work* (OLS) and *nr_of_times* (OLS).

	Difference-in-differences with the 1994 cohort				Difference-in-differences with the 1995 cohort			
	household_work		nr_of_times		household_work		nr_of_times	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Basic	Gender	Basic	Gender	Basic	Gender	Basic	Gender
Treatment	-0.026	-0.102*	-0.002	-0.030	-0.111***	-0.197***	-0.092	-0.133
	(0.046)	(0.058)	(0.084)	(0.103)	(0.040)	(0.048)	(0.077)	(0.093)
Boy* Treatment		0.154*		0.051		0.188***		0.094
		(0.079)		(0.144)		(0.068)		(0.129)
Treatment_months	-0.052	-0.022	-0.064	-0.032	0.033	0.073**	0.026	0.071
	(0.037)	(0.044)	(0.068)	(0.078)	(0.029)	(0.029)	(0.060)	(0.065)
Treatment_cohort	0.041	0.069**	-0.051	-0.033	0.022	0.045	-0.048	-0.040
	(0.029)	(0.032)	(0.056)	(0.062)	(0.028)	(0.031)	(0.056)	(0.062)
Boy* Treatment_months		-0.065		-0.062		-0.099**		-0.105
		(0.056)		(0.104)		(0.039)		(0.082)
Boy* Treatment_cohort		-0.056		-0.023		-0.056		-0.023
		(0.035)		(0.068)		(0.035)		(0.068)
Constant	0.857***	0.856***	1.127***	1.123***	0.876***	0.880***	1.124***	1.129***
	(0.024)	(0.024)	(0.044)	(0.045)	(0.022)	(0.022)	(0.044)	(0.044)
Observations	1,042	1,035	1,042	1,035	1,079	1,072	1,079	1,072
R-squared	0.010	0.015	0.004	0.005	0.012	0.020	0.007	0.010

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. All results are based on the +/- three months sample. The 1993 cohort is the reform cohort and is contrasted to the 1994 cohort (columns 1 to 4) or the 1995 cohort (columns 5 to 8).

Table 5: Effects of the 1992 reform : dependent variables *household_work* (OLS, columns 1 and 2) and *nr_of_times* (OLS, columns 3 and 4).

VARIABLES	(1) *household_work* basic	(2) *household_work* by gender	(3) *nr_of_times* basic	(4) *nr_of_times* by gender
Treatment	0.003	0.032	-0.023	0.131
	(0.035)	(0.050)	(0.060)	(0.085)
Boy		0.013	-0.028	0.122
		(0.050)	(0.060)	(0.084)
Treatment*Boy		-0.065		-0.300**
		(0.071)		(0.120)
Constant	0.816***	0.812***	1.036***	0.960***
	(0.025)	(0.036)	(0.050)	(0.056)
Observations	482	477	477	477
R-squared	0.000	0.002	0.001	0.014

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. All results are based on a +/- three months sample.

Table 6: Difference-in-differences regressions comparing the difference in 1993 to the difference in 1992: dependent variables *household_work* (columns 1 and 2, OLS) and *nr_of_times* (columns 3 and 4, OLS).

VARIABLES	(1) *household_work*	(2) *household_work* by gender	(3) *nr_of_times*	(4) *nr_of_times* by gender
Treatment	-0.081*	-0.148***	-0.048	-0.132
	(0.045)	(0.057)	(0.077)	(0.102)
Boy* Treatment		0.140*		0.166
		(0.075)		(0.131)
Treatment_months	0.003	0.025	-0.018	0.069
	(0.035)	(0.042)	(0.060)	(0.077)
Treatment_cohort	0.082***	0.107***	0.059	0.068
	(0.031)	(0.033)	(0.054)	(0.060)
Boy* Treatment_months		-0.051		-0.177**
		(0.050)		(0.086)
Boy* Treatment_cohort		-0.056		-0.023
		(0.035)		(0.068)
Constant	0.816***	0.819***	1.018***	1.022***
	(0.025)	(0.025)	(0.042)	(0.042)
Observations	1,102	1,093	1,102	1,093
R-squared	0.009	0.013	0.002	0.007

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. All results are based on the +/- three months sample. The 1993 cohort is the reform cohort and is contrasted to the 1992 cohort. The first two columns present results after OLS regressions for the dependent variable *household_work*.