Peer Effects in Program Participation

Gordon B. Dahl[*] Katrine V. Løken[†] Magne Mogstad[‡]

September 12, 2012

Abstract: The influence of peers could play an important role in the take up of social programs. However, estimating peer effects has proven challenging given the problems of reflection, correlated unobservables, and endogenous group membership. We overcome these identification issues in the context of paid paternity leave in Norway using a regression discontinuity design. In an attempt to promote gender equality, a reform made fathers of children born after April 1, 1993 in Norway eligible for one month of governmental paid paternity leave. Fathers of children born before this cutoff were not eligible. There is a sharp increase in fathers taking paternity leave immediately after the reform, with take up rising from 3% to 35%. While this quasi-random variation changed the cost of paternity leave for some fathers and not others, it did not directly affect the cost for the father's coworkers or brothers. Therefore, any effect on the coworker or brother can be attributed to the influence of the peer father in their network. Our key findings on peer effects are four-fold. First, we find strong evidence for substantial peer effects of program participation in both workplace and family networks. Coworkers and brothers are 11 and 15 percentage points, respectively, more likely to take paternity leave if their peer father was induced to take up leave by the reform. Second, the most likely mechanism is information transmission about costs and benefits, including increased knowledge of how an employer will react. Third, there is essential heterogeneity in the size of the peer effect depending on the strength of ties between peers, highlighting the importance of duration, intensity, and frequency of social interactions. Fourth, the estimated peer effect gets amplified over time, with each subsequent birth exhibiting a snowball effect as the original peer father's influence cascades through a firm. Our findings demonstrate that peer effects can lead to long-run equilibrium participation rates which are substantially higher than would otherwise be expected.

Keywords: Program Participation, Social Interactions
JEL codes: H53, J13, I38

[*]Department of Economics, UC San Diego; e-mail: gdahl@ucsd.edu
[†]Department of Economics, University of Bergen; e-mail: katrine.loken@econ.uib.no
[‡]Department of Economics, University College London; Research Department, Statistics Norway; e-mail: magne.mogstad@gmail.com

1 Introduction

Economists and policymakers are keenly interested in understanding the effects of social interactions on individual behavior. One question of particular interest is how peer groups influence the take-up of government social programs. Peer groups could serve as important information transmission networks or be influential in changing social norms, particularly in settings where information is scarce and perceptions are in their formative stage. Social interactions could reinforce or offset the direct effects on take-up due to a program's parameters, leading to a long-run equilibrium take-up rate which is substantially lower or higher than otherwise expected.

Estimating the causal effect of social interactions has proven difficult given the well-known problems of reflection, correlated unobservables, and endogenous group membership. On top of these identification issues, it is often challenging to define the appropriate peer group and access data which links members of a peer group together. Early and ongoing research attempts to control for as many group characteristics as possible or use instrumental variables.[1] Recognizing that estimates could still be biased, another set of papers attempts to measure peer effects by exploiting exogenous assignment to peer groups.[2]

In contrast, we focus on peer influence in naturally occurring peer groups, but exploit variation in the "price" of a social program for a random subset of individuals in the spirit of Moffitt's (2001) "partial-population" identification approach. This approach takes advantage of the fact that treatment is randomly assigned and therefore unrelated to any other factors which might influence take-up.[3] As we discuss later, with random variation in treatment (and with group membership determined prior to treatment), the triple threats of reflection, correlated unobservables, and endogenous group membership no longer bias the estimates of peer effects.

[1]For examples, see Bandiera and Rasul (2006), Bayer, Ross, and Topa (2008), Bertrand, Luttmer, and Mullainathan (2000), Case and Katz (1991), Carrell et al. (2008), Gaviria and Raphael (2001), Glaeser, Sacerdote, and Scheinkman (1996), Hensvik and Nilsson (2010), Markussen and Roed (2012), Maurin and Moschion (2009), Munshi (2003), and Rege, Telle, and Votruba (2009).

[2]See, for example, Babcock, Bedard, Charness, Hartman, and Royer (2011), Bandiera, Barankay, and Rasul (2009, 2010), Carrell, Fullerton, and West (2009), Carrell and Hoekstra (2010), Carrell, Hoekstra, and West (2011), Cullen, Jacob, and Levitt (2006), Duncan et al. (2005), Hanushek et al. (2003), Hoxby (2000), Imberman, Kugler, and Sacerdote (forthcoming), Jacob (2004), Katz, Kling, and Liebman (2001), Kling, Liebman, and Katz (2007), Kremer and Levy (2008), Lefgren (2004), Ludwig, Duncan, and Hirschfield (2001), Mas and Moretti (2009), Sacerdote (2001), Stinebrickner and Stinebrickner (2006), and Zimmerman (2003).

[3]A small but growing literature uses a partial population approach to estimate peer effects in naturally occuring, self-chosen social networks. See Angelucci et al. (2010), Baird et al. (2012), Bobinis and Finan (2009), Bursztyn et al. (2012), Duflo and Saez (2003), Kremer and Miguel (2007), Kuhn et al. (forthcoming), and Lalive and Cattaneo (2009). None of these studies look at peer effects in participation in social programs.

We estimate peer effects in the context of a social program in Norway designed to promote gender equality. To induce fathers to become more involved in early childrearing, a reform was passed which made fathers of children born after April 1, 1993 in Norway eligible for one month of governmental paid paternity leave, while fathers of children born before this cutoff were not.[4] Before the introduction of this paternity leave program, parents had a shared leave quota which could be split between the mother and father. In practice, however, most mothers took the entire amount of leave, with very few fathers taking any leave at all. To encourage more fathers to take leave, the 1993 reform stipulated this extra month of paid leave could only be taken by fathers.

We study whether social interactions matter for paternity leave take-up along two dimensions: workplace networks (coworkers) and family networks (brothers). Taking advantage of the timing of the reform, we estimate peer effects using a regression discontinuity (RD) design. There is a sharp increase in fathers taking paternity leave immediately after the reform, from a pre-reform take up of 3% to a post-reform take up of 35%. This quasi-random variation changed the cost (or price) of paternity leave for some fathers and not others. However, it did not directly affect the cost of taking leave for the father's coworkers or brothers, since they were all eligible for paid paternity leave when they had children in the post-reform period. Therefore, any effect on the coworker or on the brother can be attributed to the influence of the reform-window father in their network (the peer father), and not a change in the fundamental parameters of the leave program.

Our key findings on peer effects are four-fold. First, we find strong evidence for substantial peer effects in program participation in both workplace and family networks. Coworkers are 3.5 percentage points more likely to take paternity leave if their colleague was eligible versus not eligible for paternity leave around the reform cutoff. Since the first-stage estimate on take up is 32%, this implies a peer effect estimate of 11 percentage points. For the family network, we find that brothers of reform-window fathers who were eligible for leave are 4.7 percentage points more likely to take paid leave after the birth of their first child. This implies a peer effect estimate of roughly 15 percentage points. The results for both the family and workplace networks are statistically significant and robust to a variety of alternative RD specifications and control variables.

Second, the most likely mechanism is that peers transmit information about the costs and benefits of participation, including how employers will react and

[4]Many other European countries have recently reserved a share of the parental leave for fathers. For instance, in 2007 Germany introduced a two month paternity quota.

whether there is a social stigma. Our results are consistent with a model where the information provided by peers reduces uncertainty, which in turn increases take up among risk averse individuals with unbiased expectations. Because the parental leave system is universal, simple, and already well-known, we do not think a key mechanism is information about either the existence of the program or how to sign up. There is also limited scope for leisure complementarities or direct consumption externalities since coworkers and brothers do not take leave at the same time as the original peer father. Interestingly, we find suggestive evidence that the workplace and family networks transmit different types of information about the costs and benefits of participation. This makes sense, as a coworker can reveal important information about how a particular firm will react, while a brother is more likely to pass on information related to the family setting.

Third, we find essential heterogeneity based on the strength of interpersonal ties between peers. We operationally define the strength of ties by the nature of the relationship and the type of interactions. Strong peer effects are found for long-term familial relationships such as brothers and for male coworkers who have frequent interactions in a firm. Looking at weaker ties in extended family and extended workplace networks, we find no significant evidence of peer effects. In particular, peer fathers do not appear to influence their brother-in-laws or their female coworker's husbands. We next look at neighborhood peers, defining neighbors precisely (i.e., the two closest households). In our setting of paternity leave, neighbors defined by close geography exert no discernable influence on each other.[5] Our findings highlight the importance of duration, intensity, and frequency of social interactions in understanding how peer groups influence the take up of social programs.

Fourth, we find the estimated peer effect gets amplified over time within a firm, with each subsequent birth exhibiting a snowball effect in response to the original reform. The peer effect cascades through the firm network as the first peer interacts with a second peer, the second peer interacts with a third peer, and so on. The total peer effect can be decomposed into the direct influence of the peer father and the indirect snowball effects operating through the increase in take up of other coworkers. The snowball portion is large, accounting for over 50% of the total peer effect for the third and higher-order coworkers in a firm who have a child after the original peer father. We further decompose these direct and indirect effects over time. In the early years after the reform, most of the estimated peer effect can be attributed to the direct effect, as there is little opportunity for intervening births to

[5]Many studies find that neighborhoods are important. However, it is important to draw a distinction between neighborhoods and neighbors; neighborhoods include an entire vector of attributes, of which neighbors are just one element.

create a snowball effect. However, over time, the direct influence of the original peer father decays so that later in the sample period it is virtually zero. In contrast, the snowball effect gets larger over time as more coworkers have a child within a given firm. Even though the snowball effects also decay, the accumulation of effects from intervening coworkers more than offsets this decay.

Taken together, our results have important implications for the peer effects literature and for the evaluation of social programs. Our study points out that both the workplace and family can serve as important networks in settings where information about the benefits and costs of program participation is scarce and perceptions are in their formative stage. This finding is of particular importance for the ongoing debate about policies aimed at promoting gender equality, ranging from family policy to affirmative action programs. Advocates of such public interventions often argue that traditional gender roles in both the family and the labor market can be changed or modified through peer influence. Our study also highlights that peer effects can have long-lasting effects on program participation, even in the presence of decay, since any original peer effect cascades through a network over time. This is especially important when considering the design and implementation of new social programs, since the initial group of participants can play a large and lasting role in the evolution of take-up patterns. Social interactions can reinforce the direct effects on take up due to a program's parameters, leading to a long-run equilibrium take-up rate which can be substantially higher than in the absence of peer effects.

The remainder of the paper proceeds as follows. Section 2 discusses the challenges in estimating social interaction effects, the previous literature, and our identification strategy. In Sections 3 and 4, we discuss the 1993 leave reform, our data, the RD design, and validity tests. Section 5 presents our main findings on peer effects in the workplace and family networks. Section 6 explores possible mechanisms, Section 7 examines the importance of strong versus weak ties, and Section 8 estimates how peer effects cascade through the workplace network. The final section offers some concluding remarks.

2 Identifying Social Interactions

2.1 Threats to Identification

A social interaction or peer effect occurs when the action of one individual affects the actions of other individuals in the same social group. As Manski (1993) and others have pointed out, estimation of these effects is difficult given the problems of simultaneous causality (reflection), correlated unobservables (contextual effects), and endogenous group membership. To illustrate these identification issues, consider a model which is linear in the social interaction effect. For simplicity, we assume there are only 2 individuals in each group, although this could easily be generalized. Letting y_{ig} denote the outcome for individual i in group g, the system of simultaneous equations for peer effects is:[6]

$$y_{1g} = \alpha_1 + \beta_1 y_{2g} + \gamma_1 x_{1g} + \tau_1 x_{2g} + \theta_1 w_g + e_{1g} \tag{1}$$

$$y_{2g} = \alpha_2 + \beta_2 y_{1g} + \gamma_2 x_{2g} + \tau_2 x_{1g} + \theta_2 w_g + e_{2g} \tag{2}$$

where x_{ig} are observable characteristics of individual i in group g, w_g are characteristics which vary only at the group level, and e_{ig} is an error term. This model captures the idea that individual 2's choice is influenced by the choice individual 1 makes, and visa versa. It also allows individual 2's choice to depend on his own characteristics, the characteristics of individual 2, and common group-specific variables.

The equations above are an example of simultaneous causality, since individual 1's choice affects individual 2's choice, and there is no exclusion restriction. Manski (1993) points out that the coefficients are not identified and labels this the reflection problem. The problem of correlated unobservables arises when not all relevant group-level (w_g) or individual variables (x_{1g}, x_{2g}) are observed, leading to an omitted variable bias in the estimated peer effect due to what Manski calls contextual effects. Finally, the problem of endogenous group membership arises when individuals chose which group to belong to as a function of the characteristics and choices of the group.

All three of these problems arise when trying to estimate the take-up of social programs. In our setting which looks at paternity leave in workplace and family networks, coworkers and brothers are likely to influence each other. There are also a variety of workplace and family characteristics, such as a family-friendly work environment or supportive grandparents, which are likely to be both unobserved and correlated within groups. While endogenous group membership is less of an issue for brothers, it is an obvious problem for coworkers, as fathers who are inclined to take paternity leave might naturally be attracted to seek employment with coworkers who feel the same way.

[6]Manski's formulation of the problem replaces x_{ig} and y_{ig} with their expected group values; we use notation similar to Moffitt (2001), since this seems more natural in our setting (2 members in a peer group) and is more general in that it allows for e_{2g} to affect y_{1g} (and e_{1g} to affect y_{2g}). A linear model guarantees a unique equilibrium, rather than multiple equilibria, which may partly explain why it is the most widely used model to study social interactions. Allowing for more than two members in a peer group does not change the key insights; in practice, when there are multiple members of a peer group, most researchers assume a linear-in-means model, where an individual's choice depends on the leave-out mean for the other members in the group.

2.2 *Previous Research*

The existing literature has tried several approaches to overcome the challenges inherent in estimating peer group effects. A large set of papers document correlations in behavior and choices within peer groups for a variety of outcomes. The most common research design controls for a large number of group and individual level characteristics in an attempt to minimize the bias caused by simultaneous causality, correlated unobservables, and endogenous group membership. Several studies have also taken care to define networks precisely, narrowing in on the most likely peer group while controlling for more aggregate group effects. A leading example of this approach is the study of welfare take up by Bertrand, Luttmer, and Mullainathan (2000). They use the interaction of language spoken at home and geographical neighborhoods to define peer groups, which allows them to include local area and language group fixed effects. Other examples using this strategy were listed in footnote 1.

The hope in these non-experimental studies is that any remaining bias after carefully controlling for covariates is small. However, some researchers have pointed out inherent difficulties (Evans, Oates, and Schwab, 1992; Ross, 2009; Currie and Aizer, 2004). To address such concerns, some papers provide tests of the identifying assumptions made in observational studies (Bayer, Ross, and Topa, 2008; Hensvik and Nilsson, 2010) or use instrumental variables (Maurin and Moschion, 2009; Monstand, Propper, and Salvanes, 2011; Rege, Telle, and Votruba, 2009).

To avoid the problems associated with observational studies, other researchers have taken advantage of random assignment to peer groups (see footnote 2). These studies are both convincing and important. They answer the question of what happens when individuals are placed into social networks or environments which are different from what they are used to. But they cannot answer questions about social interactions in naturally occuring, self-chosen peer groups. This distinction is particularly important for designing and evaluating social programs, where a key question is whether endogenously-formed social networks transmit information or otherwise influence participation. As Carrell, Sacerdote, and West (2012) show, endogeneous sorting into peer groups is a powerful force. Even when individuals are randomly assigned to modestly-sized groups, they self-select into more homogeneous sub-groups, subverting the intended peer group assignment.

2.3 *Using Experimental Variation within Naturally Occurring Peer Groups*

In contrast to the previous literature discussed above, we study naturally occurring peer groups, but exploit variation in the "price" (or cost) of a social program for a random subset of individuals within groups. Instead of randomly assigning individuals to groups and seeing how participation is affected, we randomly vary the net benefit of participation for some individuals in a group and see how other members in the group change their behavior. Moffitt (2001) calls this the "partial-population" approach.

To fix ideas, consider an experiment where (i) the price, p_{1g}, of program participation for individuals with the label 1 is varied randomly across groups and (ii) there is no change for any individuals with the label 2. Equations (1) and (2) become:

$$y_{1g} = \alpha_1 + \beta_1 y_{2g} + \gamma_1 x_{1g} + \tau_1 x_{2g} + \theta_1 w_g + \lambda p_{1g} + e_{1g} \tag{3}$$

$$y_{2g} = \alpha_2 + \beta y_{1g} + \gamma_2 x_{2g} + \tau_2 x_{1g} + \theta_2 w_g + e_{2g} \tag{4}$$

Since p_{1g} is assigned randomly to individuals with the label 1 in group g, it will be uncorrelated with x_{1g}, x_{2g}, w_g, e_{1g}, and e_{2g}. This immediately implies that λ can be identified from a regression of y_{1g} on p_{1g}. More importantly, it also means that a consistent estimate of the peer effect β can be obtained by regressing y_{2g} on p_{1g} and scaling by $\hat{\lambda}$.[7]

The presence of an excluded variable which appears in individual 1's outcome equation but not individual 2's solves the reflection problem of simultaneity. Moreover, since p_{1g} is orthogonal to all observed and unobserved covariates, correlated unobservables can no longer bias the estimates. And finally, if peer groups are measured before the price shock p_{1g}, endogenous group membership does not create a bias either; any changes in group membership which happen after the price shock are either a causal result of changes in p_{1g} or orthogonal to changes in p_{1g}.

A handful of other researchers have used a partial population approach to study social interactions (see footnote 3). Our study complements this strand of the peer effects literature in several ways. None of these studies look at peer effects in participation in social programs. Our setting is also fundamentally different, in that there is little role for consumption or outcome complementarities.[8] The

[7]Since p_{1g} is orthogonal to all other covariates, for consistency it does not matter whether other covariates are included in either regression. With or without covariates, the experimental estimate of β can be interpreted as an IV estimate.

[8]In the examples cited in footnote 3, a large part of the estimated effects are due to consumption complementarities: when an individual is treated for worms, the benefit of the technology to their peers declines (Kremer and Miguel, 2007); when a child goes to school, the benefit to their friends of attending also goes up (Lalive and Cattaneo, 2009; Angelucci et al., 2010; Bobinis and Finan, 2009); and when an employee attends an information session for a cash incentive, untreated colleagues in the same peer group now have someone to attend the seminar with (Duflo and Saez, 2003). Angelucci and Di Georgi (2009) argue that experimental designs should use group-level randomization in such settings, rather than selecting treatment and control subjects randomly within groups, and Baird et al. (2012) discuss how to design experiments to measure these complementarities.

reason is that fathers take leave for a limited amount of time and the period when reform-window fathers can take leave is far removed from when their brothers and colleagues are eligible for leave. Due to the richness of Norwegian registry data, we are also able to demonstrate the importance of duration, intensity, and frequency of social interactions across different social networks. Lastly, we show how the total peer effect estimate can be decomposed into a direct peer influence and an indirect snowball effect that accumulates within a network over time.

2.4 Estimating Peer Effects in an RD design

In our setting of paternity leave, the price of leave changes discontinuously based on when a child is born. Using an RD design, we get quasi-random variation in the cost of taking leave for fathers (individual 1 in group g) whose children are born in a window surrounding the reform. We can estimate λ in equation (3) as the jump in take-up at the reform date cutoff in a first stage RD regression. We can then examine whether this quasi-random variation in cost for father 1 changes the leave taking behavior of father 2. This reduced form RD estimate can be scaled by $\hat{\lambda}$ to get an estimate of β. The details of the reform and the RD procedure are outlined in the next section.

To our knowledge, RD has not previously been used to estimate peer effects within naturally occuring peer groups.[9] Using an RD approach for this purpose involves a particular set of challenges because of what might be called the "many to one" feature which is inherent in peer groups. By many to one, we mean that multiple peers in a network can affect the same individual. For example, in our firm setting, a coworker can potentially be affected by multiple peer fathers.

In an RD design, a window surrounding the cutoff (i.e., the reform date) needs to be specified. Several issues arise when multiple peer fathers appear in the chosen RD window. First is the issue of how to define the running variable when there is more than one peer father in the window. This is particularly problematic when there are some peer fathers before the cutoff and some peer fathers after the cutoff. A second issue relates to functional form. Is a coworker affected by (i) the average number of peer fathers with children born before versus after the cutoff, (ii) the number of peer fathers with children born after the cutoff, or (iii) simply whether any peer father had a child after the cutoff? For instance, if the average number is used, the implicit assumption is that the number of peers doesn't matter. Even if the running variable can be defined and a functional form decided on, a final issue

[9] There are studies using an RD design to exploit quasi-random assignment to peer groups (e.g., Ding and Lehrer 2007).

is that for large networks, an RD approach will have little power. The reason is that as the number of peer fathers appearing in the reform window increases, the variation in peer exposure to the reform decreases, since roughly an equal number of peer fathers will give birth before versus after the cutoff.

We sidestep these issues by looking at networks where there is a single peer father in the reform window. This strategy makes it easy to define the running variable, requires no additional functional form assumptions, and provides ample variation in peer exposure to the reform.

3 Background, Data, and Empirical Strategy

3.1 Paternity Leave

Governmental paid parental leave has a long history in Norway.[10] In 1977, parents were granted 18 weeks of paid leave. During the 1980s and 1990s, the leave period gradually expanded, and by 2011 there was a maximum of 47 weeks of paid leave. The parental leave mandates offer employment protection and income replacement. The parental leave policy is part of the broader Social Security System, and is financed through employer and employee taxes. Apart from a few weeks reserved for the mother, parents could share the parental leave between them as they desired before 1993. Until recently, however, fathers were taking little, if any, leave.

In an effort to promote gender equality, the labor party government introduced a paternal-leave taking quota in their proposed national budget of 1993. The reform was passed in parliament in December 1992 and implemented on April 1, 1993. The key feature of the paternal quota was that four out of 42 weeks of paid parental leave were reserved exclusively for the father.[11] Apart from exclusive quotas of four weeks for fathers and the pre-existing nine weeks for mothers, parents could share the parental leave between them as they desired.

While paid maternity leave was only contingent on the mother working at least 6 of the last 10 months before birth, paid paternity leave was contingent on both parents (whether married or cohabiting) working at least 6 of the last 10 months. Income payments were based on the earnings of the person on leave, but a father's payment was reduced proportionally if the mother did not work full-time prior to

[10] Our description of the parental leave system and the paternity leave reform builds on Rege and Solli (2011) and Fiva et al. (2011).

[11] At the same time as the four-week paternity quota was implemented, the leave amount that could be shared between parents was extended by three weeks. This means that we cannot tell for sure whether the estimated peer effects reflect the introduction of the paternity quota or the extension of shared parental leave. We expect, however, that the paternity quota is the driving force behind the increased fraction of fathers taking leave. The reason is that none of the previous extensions of shared parental leave increased the take-up of leave among fathers.

birth. In families with full-time working mothers prior to childbirth, the parental leave scheme offers 100% income compensation, subject to a capped amount, for both men and women. The income cap is non-binding for most parents, and when it is exceeded, most public and private employers top up benefits so that income is fully compensated.[12] The firm is not allowed to dismiss the worker for taking leave, and the parent has the right to return to a comparable job.

The parental leave system is universal, simple, and well-known (including details about eligibility, benefit amounts, and the application process). To apply for parental leave benefits, parents must inform their employers and submit a *joint* application to a Social Security Administration field office at least six weeks before the pregnancy due date. For each spouse, the family must specify days of leave and when the leave period will start and end.[13] Because almost all eligible women take leave and the family must specify maternity and paternity leave on the same form, the introduction of the paternal-leave taking quota had few, if any, practical implications for the application process. The key change was that more families filled in non-zero days of paternity leave in the application form, instead of leaving it blank.

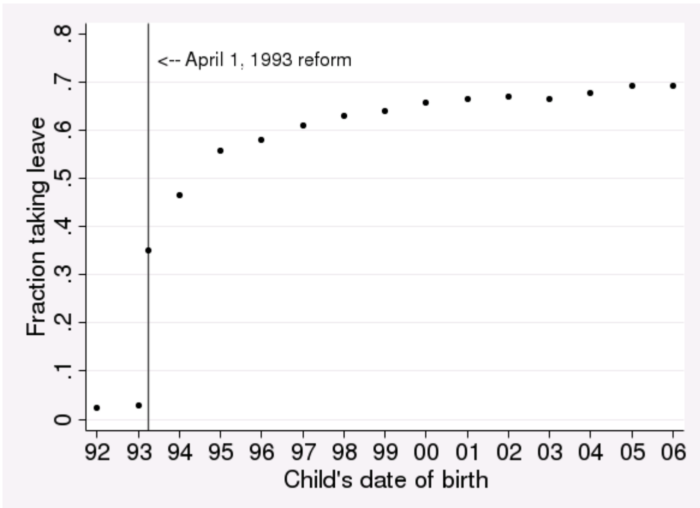


Figure 1. Paternity leave take up for all eligible fathers, 1992-2006.

The introduction of the paternity quota led to a sharp increase in take-up rates of parental leave by fathers. Figure 1 shows the fraction of fathers taking paternity leave by the birth year of their child. There is a stark increase in the share of fathers

[12]In 2010, benefits were capped at NOK 437,400 (approximately $75,000). Thirty-four percent of fathers and only 7% of mothers earned more than the benefits cap. The replacement rate for self-employed individuals is 65% of income. Parents could also choose 80% income replacement and receive an additional 6 weeks leave.

[13]Most fathers taking leave have only one spell of paternity leave. Their leave period typically comes after the maternity leave period, when the child is at least nine months old.

taking paternity leave immediately after the reform: While only 3% of fathers took leave prior to the reform, the take-up rate jumped to approximately 35% in 1993 after the reform was implemented. The take-up rate continued to rise over the next decade, climbing to 70% of eligible fathers by 2006.

3.2 Data and Sample Restrictions

Our analysis employs several data sources that we can link through unique identifiers for each individual. The data on parental leave comes from social security registers that contain complete records for all individuals for the period 1992-2006.[14] We link this data with administrative registers provided by Statistics Norway, using a rich longitudinal database that covers every resident from 1967 to 2006. For each year, it contains individual demographic information (including gender, date of birth, and marital status), socio-economic data (including years of education and earnings), and exact geographical identifiers (including street address). The data contains unique identifiers that allow us to match spouses and parents to their children. Lastly, we merge these data sets with linked employer-employee data that contains complete records of all firms and workers for the period 1992 to 2006. A number of firm specific variables are available, such as firm size and industry. The coverage and reliability of Norwegian registry data are considered to be exceptional (Atkinson et al. 1995).

For both workplace networks (coworkers) and family networks (brothers), we restrict the sample to fathers predicted to be eligible in order to gain precision in the RD estimation. Actual eligibility is based on (i) both the father and the mother working at least 6 out of 10 months immediately preceeding the birth, and (ii) both the father's and mother's earnings in the prior 10 months exceeding a "substantial gainful activity" threshold (approximately NOK 72,900 in the year 2010 or $12,500).

Since we do not observe months of work, we predict eligibility based on earnings in the year prior to childbirth; we count a father as eligible if both the father's and mother's annual earnings exceed the "substantial gainful activity" level. There is a tradeoff between using too strict of an earnings requirement and excluding parents from our sample who were, in fact, eligible, and using a less strict earnings requirement and including parents who actually were ineligible. While including ineligible fathers may increase the residual variation and thus the standard errors in the RD estimation, excluding eligible fathers may affect the external validity of our

[14]Note that since the data on parental leave is not available prior to 1992, a study similar to ours cannot be done for earlier parental leave reforms.

results.[15] By using a fairly weak earnings requirement in the prediction of eligibility, we assign more weight to the generalizability of our results. This conservative approach yields an average take-up for predicted eligible fathers of 60% over the entire post-reform period, while it is only 4 % for predicted non-eligible fathers.

For each peer group, we further refine the sample to be appropriate for the relevant social network. For the family network, we include fathers with a child born of any parity within one year of the reform, who have brothers whose first child is born after the peer father's child and after the reform. For the employment network, we restrict the sample to firms which have only one birth of any parity to male employees in the one-year interval straddling the reform (six months on each side) and coworkers whose first child is born after the peer father's child and the reform. The tighter sample window for firms reflects that fact that we have a larger sample of coworkers in our data. As discussed in section 2.4, these restrictions allow us to cleanly identify a single peer father and use a straightforward RD design.

One implication of our approach is the estimation sample will be comprised of small- and medium-sized Norwegian firms (measured at the plant level). The median firm size for workers in our restricted sample is 27 employees, while the median firm size for all workers in Norway is 58 employees.[16] These small and medium firms are suitable for a study of peer effects, because it is likely that employees in these types of firms interact with each other directly. For the family network, we also restrict the sample of peer fathers to families which have only one brother with a child being born in a window straddling the reform. This restriction is generally not binding, as few families have multiple brothers giving birth in the reform window.

In Appendix Table A1, we document the average characteristics of fathers in each of our networks. Our coworker sample contains approximately 20% of the entire population of eligible fathers in the relevant one-year window. Because of our sample restrictions, the reform window fathers in our employment network sample are on average less educated and slightly less likely to be married, but have otherwise similar characteristics for age, child gender, and number of children. Our brother sample contains approximately 13% of eligible fathers in the corresponding two-year window. The reform window fathers in our brother sample are younger on average and as a consequence, also less likely to be married, but are similar in other respects.

[15]Provided that eligibility cannot be manipulated, the internal validity of the RD estimates are unaffected by the exclusion of ineligibles. Because of the timing of the reform announcement, there is little chance for eligibility manipulation, a claim we verify empirically in the next section.

[16]We do not have many large firms in our sample, since these firms are likely to have more than one birth in a one year window of the reform. Likewise, we have few of the smallest firms, since these firms are less likely to have any births in a one-year window of the reform.

3.3 Empirical strategy

We use a fuzzy RD design to estimate the peer effects of parental leave take up. The discontinuity we exploit arises from the introduction of the paternity quota: fathers of children born after April 1, 1993 were eligible for paid paternity leave, while fathers of children born before this cutoff were not.

The RD design can be implemented by the following two-equation system:

$$y_{2g} = \alpha_2 + \beta y_{1g} + 1[t \geq c] f_l(t-c) + 1[t < c] f_r(c-t) + e_{2g} \tag{5}$$

$$y_{1g} = \alpha_1 + 1[t \geq c](g_l(t-c) + \lambda) + 1[t < c] g_r(c-t) + e_{1g} \tag{6}$$

where c is the cut-off date, and f_l,f_r,g_l, and g_r are unknown functional forms. We will estimate the system of equations using both polynomial and local linear regressions. The 2SLS estimate of β gives the peer effect. The identifying assumption of our fuzzy RD design is that individuals are unable to precisely control the assignment variable, date of birth, near the cutoff, in which case the variation in treatment at c is random.

We can estimate λ as the jump in take-up at the reform date cutoff in a first stage RD regression, given by equation (6). By estimating the following reduced form model, we can examine whether this quasi-random variation in cost of paternal leave for the peer father (assigned the label 1) changes the leave taking behavior of the peer father's coworker or brother (assigned the label 2):

$$y_{2g} = \gamma_2 + 1[t \geq c](f_l(t-c) + \pi) + 1[t < c] f_r(c-t) + u_{2g} \tag{7}$$

where π can be interpreted as an "intention-to-treat" (ITT) effect of the paternity quota on the leave taking behavior of the peer father's coworker or brother. Since the two-equation system is exactly identified, the 2SLS estimate of β is numerically equivalent to the ratio of the reduced form coefficient π and the first stage coefficient λ, provided that the same bandwidth is used in equations (6) and (7) in the local linear case, and the same order of polynomial is used for f and g in the polynomial regression case.

4 Potential Manipulation

4.1 Strategic Timing of Births

The validity of our RD design requires that individuals cannot manipulate the assignment variable, which is the birthdate of the peer father's child. If date of birth cannot be timed in response to the paternity leave reform, the aggregate distribution of the assignment variable should be continuous around the cutoff date.

There is little opportunity to strategically time conception, as the implementation date for the reform was announced far less than nine months in advance. The national budget which proposed the paternal quota was publicly introduced on October 7, 1992 and passed by parliment in December of the same year. Therefore, mothers giving birth close to April 1, 1993 were already pregnant before the announcement of the reform. Searches in newspaper archives indicate the date of implemention was not discussed publicly before the national budget was passed. Furthermore, the month of implementation varied for previous parental leave reforms.

While strategic timing of conception is unlikely, it is still possible that mothers with due dates close to the cutoff date could postpone induced births and planned cesarean sections.[17] We test for strategic timing in Appendix Table A2 by regressing the birthdate of the child on dummies for one week intervals before and after the reform date of April 1, 1993. To increase precision, for this regression we use the entire sample of all births to fathers eligible for paternity leave in Norway, and not just those in the family or workplace networks. We find some evidence that a small number of births are delayed. In the week immediately before the reform, there are an estimated 10 fewer births in all of Norway (relative to the average of 840 births per week); in the week immediately after the reform, there are an estimated 11 more births. Both of these differences are significantly different from zero. However, we do not find evidence of delay further away from the reform window; this is as expected, since it is medically difficult to delay childbirth for very long and most inductions in Norway were for late-term pregnancies.

To avoid the possibility that some births in our sample are strategically delayed, our baseline RD results exclude the week immediately before and the week immediately following the reform date of April 1, 1993. As we will show, using a wider donut of 2 weeks (2 weeks on each side of the reform), or no donut at all, does not materially affect our findings. Appendix Figure A1 graphically illustrates there is no measurable effect on the fertility of coworkers or brothers of peer fathers with a one-week donut. While there are seasonal patterns in the number of births (with more births in the spring), there is no jump in fertility around the discontinuity.[18]

[17] In contrast to current birth practices in the U.S., the vast majority of births in Norway around the time of the reform were spontaneous vaginal deliveries. In 1993, the c-section rate was 12% and only 12% of vaginal deliveries were induced (Folkehelseinstituttet, http://mfr-nesstar.uib.no/mfr).

[18] A formal statistical test, mirroring the RD regression specification of 1, confirms that the fertility of brothers and coworkers was not significantly affected by the reform. The RD peer effect estimate for fertility is -.004 (s.e.=0.10) for the workplace network and -.001 (s.e.=.014) for the family network.

4.2 *Eligibility*

Another threat to our identification strategy is that the announcement of the reform could cause a change in eligibility among peer fathers around the cutoff date. If it did, then restricting the sample to eligible fathers could bias the estimated peer effects.

As explained above, we predict eligibility based on annual earnings in the year prior to childbirth; we count a peer father as eligible if both the father's and mother's annual earnings exceed the substantial gainful activity level. As the vast majority of fathers (91%) already work and earn more than the substantial gainful activity level, there is little opportunity for reform-induced changes in eligibility for peer fathers. It is still possible that mothers' earnings could respond to the announcement of the reform. Recall, however, that predicted eligibility of fathers who have a child (in the window surrounding the reform) is based on annual earnings in 1992. As the reform was announced in December 1992, it leaves the mother with only one month in which she can increase her earnings enough to make the father eligible by our definition. Given this short time frame, there is limited scope for mothers to manipulate the predicted eligibility status of the father.

Appendix Figure A2 graphically illustrates there is no measurable change in predicted eligibility of peer fathers around the cut-off date. While there is some seasonal variation in earnings and thus in predicted eligibility, there is no jump in the fraction of predicted eligible fathers around the discontinuity, a finding which is confirmed with an RD regression in Appendix Table A3.

4.3 *Covariate Balance*

If families time date of birth or change eligibility status in response to the reform, then we would expect to see changes in the distribution of pre-determined characteristics of the parents around the reform date of April 1, 1993. In Appendix Table A3, we test whether these covariates are directly affected by the 1993 reform. We run the RD regression given by equation (6) with individual, family, and child characteristics as the dependent variable. It is reassuring to find that the RD estimates are close to zero and always insignificant.

5 Results for the Workplace and Family Networks

5.1 *Graphical Results*

A virtue of the RD design is that it provides a transparent way of showing how the peer effects are identified. To this end, we begin with a graphical depiction of how leave take up varies around the cutoff date before turning to a more detailed regression-based analysis.

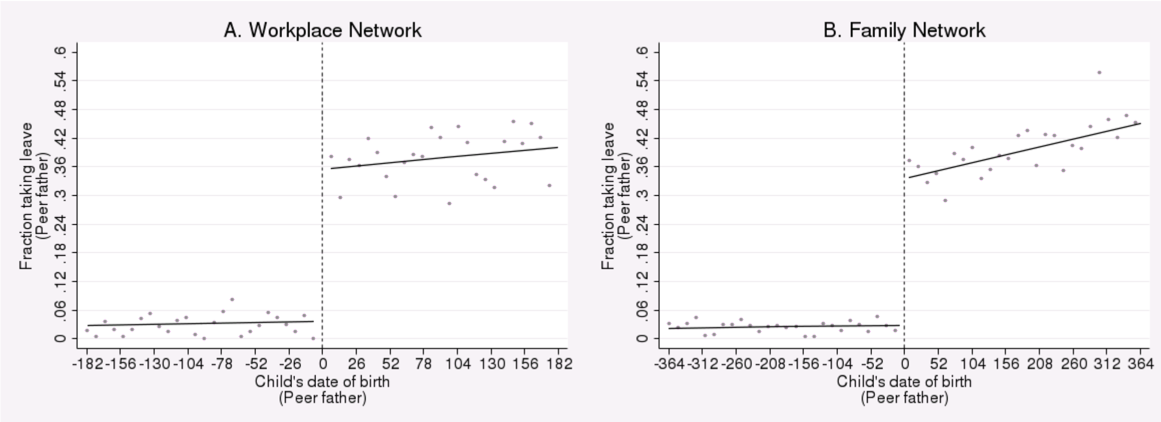


Figure 2. Fraction of peer fathers taking leave.
Notes: Each observation is the average number of peer fathers taking paternity leave in one-week bins (left panel) or two-week bins (right panel), based on the birthdate of their child. Dashed vertical lines denote the reform cutoff of April 1, 1993, which has been normalized to zero.

Figure 2 displays the fraction of peer fathers taking any amount of paid leave in a window surrounding the reform. The left graph plots this first stage for the workplace network and the right graph for the family network. In both graphs, the running variable, child's date of birth, has been normalized so that April 1, 1993 is time zero. For the workplace network, each observation is the average number of peer fathers taking paternity leave in one-week bins, based on the birthdate of their child. For the family network, we plot unrestricted means for two-week bins since we have fewer observations.[19] For both networks, there is a sharp jump in the take-up rate of peer fathers at the cutoff, with program participation rising from around 3% to approximately 35%. These graphs provide strong evidence that the reform had large direct effects on the leave behavior of peer fathers. As we document in Section 6.1, the reform had no other direct effects on peer fathers for outcomes we observe, including no direct efffects on labor market outcomes, child achievement, marital stability, or fertility.

Figure 3 captures our main results on peer effects. In each graph, we plot unrestricted averages in one- or two-week bins and include estimated regression lines using separate linear trends on each side of the cutoff date. Whereas the regression

[19]There are 242 brothers (with 233 peer fathers) and 550 coworkers (with 153 peer fathers) on average in a one-week interval. While few fathers have multiple brothers, each peer father has an average of 3.6 coworkers. We use larger bins for the family network, since our samples contain relatively fewer brothers compared to coworkers.

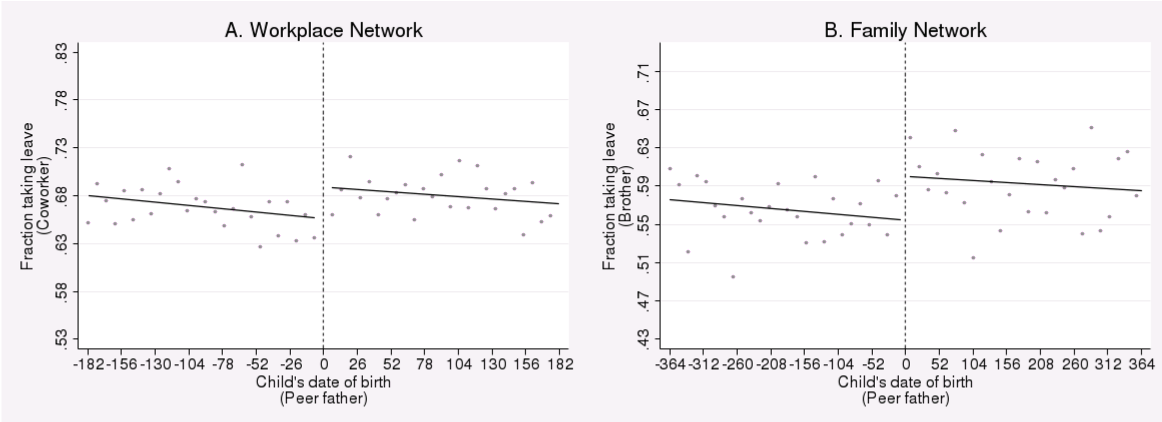


Figure 3. Coworker's and brother's leave take up.
Notes: Each observation is the average number of coworkers taking paternity leave in one-week bins (left panel) or brothers taking paternity leave in two-week bins (right panel), based on the birthdate of the peer father's child. Dashed vertical lines denote the reform cutoff of April 1, 1993, which has been normalized to zero.

lines better illustrate the trends in the data and the size of the jumps at the cutoff dates, the unrestricted means indicate the underlying noise in the data. Each graph sets the scale of the y-axis to ±.3 standard deviations of the respective variable.

The left panel of Figure 3 plots coworker's leave take up as a function of the birthdate of their peer father's child around the reform window in one-week bins. The jump at the cutoff is the ITT estimate. As a reminder, these coworkers are all eligible for the extra 4 weeks of exclusive paternity leave since they have their first child after the reform has been implemented. The difference is that some coworkers had peer fathers who were not eligible for 4 extra weeks (those observations to the left of the reform, labeled 0 in the graphs) while other coworkers had peer fathers who were eligible (those observations to the right of 0). The right panel of Figure 3 presents a similar graph for the family network, with data aggregated into two-week bins (since there are fewer observations). The panels reveal a sharp jump in leave take up of a coworker or a brother if their peer father had his child immediately after, versus immediately before, the reform date of April 1, 1993.[20]

In our appendix, we provide futher visual evidence for a sizeable reduced form peer effect in both the workplace and family networks. Appendix Figure A3 presents graphs similar to Figure 3, but aggregating the raw data into bigger bins. In

[20]There is a negative slope as a function of the running variable, both before and after the cutoff. This negative slope is a function of the sample restriction that coworkers and brothers have their children after their peer father and after the reform cutoff, which affects when coworkers and brothers have children during our sample period. It does not create a problem for consistency, since the effect is continuous through the cutoff. The takeup rate of brothers is lower in Figure 3, since they have their children earlier in the sample period, on average, compared to coworkers.

Appendix Figure A4, we present local linear regression graphs for coworker's and brother's leave take up. If anything, the jump at the reform cutoff date is even larger for these local estimates.

5.2 Regression Results

Having shown the raw patterns of leave taking behavior around the reform cutoff, we now turn to regression-based estimates. Table 1 presents the baseline RD estimates for the peer effects of fathers on their male coworkers and brothers. The specifications use daily data, exclude observations in a one-week window on either side of the discontinuity, include separate linear trends in birth day on each side of the discontinuity, and employ triangular weights. We include pre-determined control variables for father's and mother's years of education, father's and mother's age and age squared at birth, parent's county of residence and marital status prior to the birth, and an indicator for the gender of the child. As a reminder, the workplace sample is restricted to firms which have only one birth of any parity to male employees in the one-year interval straddling the reform. Coworkers must have their first child after the peer father's child is born and after the reform.

Column 1 of Table 1 estimates the first stages and corresponds to Figure 2. For both the workplace and family network, the estimate is a little over 30 percentage points. This is a sizeable direct effect on paternity leave, with an increase in take-up from roughly 3% to 35%.

Table 1. Regression discontinuity estimates for peer effects of fathers on their coworkers and brothers.

	First stage (1)	Reduced form (ITT) (2)	Second stage (2SLS) (3)	N (4)
A. Workplace network				
Take up of leave	.317***	.035***	.110***	26,851
	(.026)	(.013)	(.043)	
	[.03]	[.67]	[.67]	
B. Family network				
Take up of leave	.304***	.047**	.153**	12,495
	(.014)	(.020)	(.065)	
	[.026]	[.57]	[.57]	

*Notes: See text for the details of the RD setup and a list of included covariates. Standard errors clustered by firm in panel A and by family in panel B. Comparison mean in brackets. *p<0.10, **p<0.05, ***p<0.01.*

To estimate the workplace peer effect, in column 2 we first estimate the reduced form (or ITT) effect. The RD estimate of a coworker's leave take up at the cutoff date for their peer father's child's birthdate is 3.5 percentage points, a point estimate which is statistically significant at the 1% level. This estimate corresponds to Figure 3. To convert this into an estimated peer effect, we divide the reduced form coefficient in column 2 by the first stage coefficient in column 1. This yields a second stage estimate (which can be calculated via 2SLS since the system of equations is exactly identified) of 11.0 percentage points. This estimated peer effect is large relative to the average take-up rate of 67% for coworkers of untreated fathers.

In panel B of Table 1, we find strong evidence for peer effects among brothers as well. Brothers of reform-window fathers who were eligible for leave are 4.7 percentage points more likely to take paid leave after the birth of their first child. This implies a peer effect estimate of 15.3 percentage points. This represents a substantial increase in take up given the average take-up rate is 57% for brothers of untreated fathers.[21]

5.3 Robustness Checks

In this section we probe the stability of our baseline estimates to alternative specifications. We conclude that our estimated peer effects are remarkably robust to the usual specification checks performed in RD studies.

In Table 2, we first exclude all control variables from the regressions, and find virtually no change in the estimates for either the workplace or family networks. This is to be expected, since the values of pre-determined covariates should not affect the estimated jump at the cutoff date in a valid RD design. We next explore what happens when we use separate quadratic or cubic trends on each side of the discontinuity, rather than separate linear trends. The estimated reduced form and second stage coefficients are slightly larger, although the cubic trend estimate is no longer significant for the workplace results. The next set of robustness checks estimate RD regressions without a one-week donut around the reform date and with a two-week donut, respectively. The results remain significant, and if anything, get somewhat larger the bigger the donut. We also try a specification which includes all of the predicted non-eligible fathers, which yields similar results compared to our baseline estimates. Finally, note that we have been clustering our standard errors at the firm level or the family level. An alternative is to cluster at the level of the running variable, which is the day of birth. This alternative clustering does little to the standard errors.

We perform a variety of additional robustness checks. Appendix Table A4 varies

[21]The average take-up rate is higher for coworkers compared to brothers since take up increases over time and brothers have their children earlier in our sample period.

Table 2. Specification checks for coworker and brother peer effects.

	First stage	Reduced form	Second stage	N
	(1)	(2)	(3)	(4)
A. Workplace network				
Baseline	.317***	.034**	.109**	26,851
	(.026)	(.013)	(.043)	
No Controls	.318***	.034**	.106***	26,851
	(.024)	(.012)	(.040)	
Quadratic trends	.321***	.043**	.134**	26,851
	(.041)	(.021)	(.068)	
Cubic trends	.298***	.050	.168	26,851
	(.062)	(.032)	(.111)	
No donut	.323***	.024*	.074*	27,856
	(.024)	(.013)	(.040)	
Two week donut	.311***	.042***	.135***	25,736
	(.028)	(.015)	(.050)	
Non-eligibles included	.247***	.033***	.133***	34,749
	(.021)	(.012)	(.049)	
Cluster s.e.'s on	.317***	.035***	.110***	26,851
day of birth	(.026)	(.013)	(.043)	
B. Family network				
Baseline	.304***	.047**	.153**	12,495
	(.014)	(.020)	(.065)	
No controls	.303***	.046***	.152***	12,495
	(.013)	(.018)	(.059)	
Quadratic trends	.319***	.062**	.193**	12,495
	(.021)	(.030)	(.094)	
Cubic trends	.329***	.080*	.245**	12,495
	(.029)	(.042)	(.129)	
No donut	.308***	.043**	.141**	12,779
	(.013)	(.019)	(.061)	
Two week donut	.303***	.042**	.138**	12,204
	(.015)	(.021)	(.068)	
Non-eligibles included	.220***	.043***	.197***	17,835
	(.011)	(.017)	(.075)	
Cluster s.e.'s on	.304***	.047**	.153***	12,495
day of birth	(.014)	(.020)	(.066)	

*Notes: Specifications mirror the baseline specifications described in Table 1. Standard errors clustered by firm in panel A and by family in panel B. *p<0.10, **p<0.05, ***p<0.01.*

the window size of our baseline specification. For the workplace network, in panel A we find that windows of 3 months, 4.5 months, and 6 months (our baseline) yield similar results which all remain statistically significant. The estimates using a smaller window are somewhat larger, but also have larger standard errors. A similar set of findings holds in panel B for the family network. As a reminder, since we have fewer brothers compared to coworkers, we use wider windows of 6 months, 9 months, and 12 months (our baseline) in panel B. As with the workplace network, estimates for the brother sample using a smaller window are somewhat larger, with larger standard errors, but remain statistically significant.

An alternative approach to using polynomials on each side of the reform cutoff is to use local linear regression. This estimation method may be more robust to trends away from the cutoff point. In Appendix Table A5, we estimate local linear regressions for the workplace and family networks with bandwidths of varying size. Whether we use a bandwidth of 60 days, 90 days, or 120 days for the coworker sample, we find statistically significant peer effects of 13 to 14 percentage points. A similar finding of robustness holds for the brother sample when we use bandwidths of 120 days, 180 days, or 240 days.

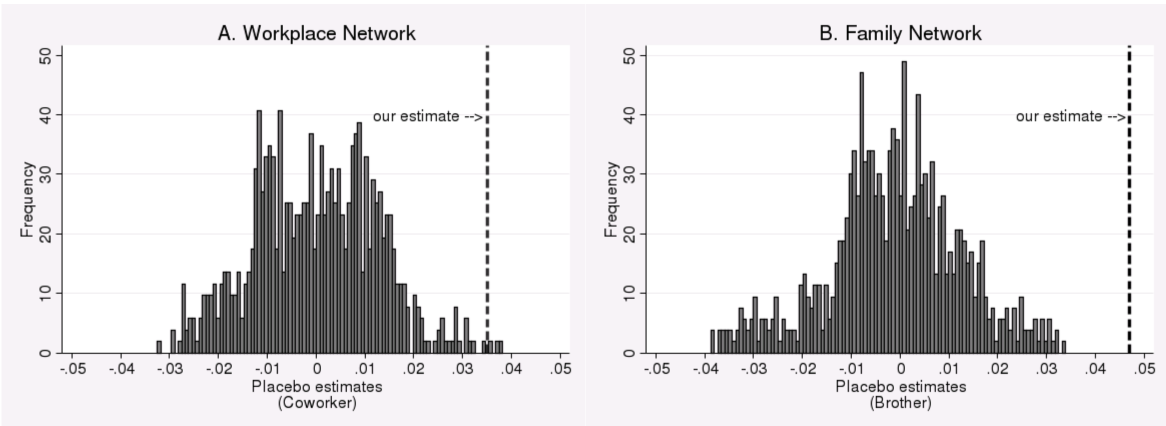


Figure 4. Placebo estimates of the peer effect.
Notes: *Each placebo estimate first assigns a window around a false reform date, and then uses an RD to estimate a reduced form peer effect. There are 730 estimates for each graph (2 years of estimates), where each estimate increases the false reform date by one day.*

As a further check, we run a series of placebo tests. To do this, we first assign a window around a false reform date, and then use the RD approach described in section 3.3 to estimate a reduced form peer effect. We run 730 placebo tests for each network (2 years of estimates), where each estimate increases the false reform date by one day.[22] To avoid having these placebo estimates be influenced by any jump at the true cutoff, the placebo windows start after the true reform date of April 1, 1993. Figure 4 graphs the distribution of placebo estimates for both the workplace

[22]It should be noted the placebo estimates are not independent of one another, since the windows contain a significant amount of overlap in observations.

and family network. As the graphs make clear, the true peer effect (from Table 1) is more extreme than all of the placebo estimates for brothers and almost all of the placebo estimates for coworkers. These findings indicate the odds of finding peer effects as large as we do merely due to chance are small.

6 Mechanisms

To clarify the nature of the peer effects we estimate, in this section we explore possible mechanisms. We first examine if there are other direct effects of the reform on the peer father besides increased take up which might serve as mediating relationships. We next assess three different channels through which the increase in peer father's leave may affect the leave taking behavior of his coworker or brother.

6.1 Mediating Relationships

In Appendix Table A6, we test for a variety of direct effects of the reform on other outcomes, but find no measureable changes in these potentially mediating outcomes. Except for changing the dependent variable, the RD estimates in the table use the same specification as the first stage estimates appearing in Table 1. There is no evidence of a statistically significant discontinuity in the future employment and earnings of fathers or mothers, or in the relative employment and earnings of mothers versus fathers. There is also no evidence of a direct effect on the grade point average of the child in middle school, completed fertility, or long-term marital status. As documented in the table, these estimates are close to zero and never statistically significant. The only estimate which approaches statistical significance is father's total earnings, but the estimated effect is small, amounting to less than a 2% reduction in earnings. The lack of other direct effects suggests that mediating relationships do not play an important role in explaining the peer effects we estimate.[23]

6.2 Peer Effect Channels

Because variation in the cost of paternity leave near the reform cut-off is as good as random, the peer effects estimates are not picking up common time effects such as general changes in societal norms. There are, however, several other channels through which the reform-induced increase in a peer father's leave may affect the leave taking behavior of his coworker or brother.

[23] Our results for mediating outcomes are broadly consistent with Rege and Solli (2010) and Fiva et al. (2011). Using difference-in-differences approaches, these two studies evaluate the direct effects of the paternity leave reform.

The first possible channel is sharing of information about how to enroll in the program.[24] As discussed in Section 3.1, the parental leave system is universal, simple, and well-known (including details about eligibility, benefit amounts, and the application process). To apply for parental leave benefits, the spouses must inform their employers and submit a joint application to the government. Because almost all eligible women take leave and the family must specify maternity and paternity leave on the same form, the introduction of the paternal-leave taking quota had few, if any, practical implications for the application process. For these reasons, we do not think a key mechanism for the estimated peer effects is information about either the existence of the program or how to sign up.

The second possible channel is leisure complimentarities or direct consumption externalities. Since the births are temporally distant, coworkers and brothers do not take leave at the same time as the original peer father. As a consequence, there is limited scope for complimentarities or externalities arising from the reform-induced take-up of paternity leave. Another piece of evidence against this channel is that the peer effect is present even if brothers live in different municipalities.[25]

The third channel is information about the costs and benefits of participation, including how employers will react and whether there is a social stigma. In our setting of paternity leave, information about costs and benefits is initially scarce since prior to the 1993 reform, almost no fathers were taking paternity leave. However, the reform generates random variation in the take up of peer fathers and therefore changed the information set of a subgroup of brothers and coworkers. This exogenous increase in information reduces uncertainty, which should increase take-up among risk averse individuals with unbiased expectations.[26]

Without data on subjective expectations and individual information sets, it is difficult to assess what type of information transmission is driving the estimated peer effects. However, we expect differing pieces of information to be transmitted in the workplace versus family network. In particular, a coworker can reveal important information about the firm-specific consequences of paternity leave, while a brother is more likely to pass on information related to the family setting (or the labor market more broadly). Interestingly, we find several pieces of suggestive evidence which indicate that workplace and family networks do indeed transmit different types of information about the costs and benefits of participation.

[24] Figlio et al. (2011) show that neighborhood networks can be important in spreading information about eligibility rules and benefits among immigrants.

[25] The estimated peer effect is .134 (s.e.=.89) for brothers who live in the same municipality, and .170 (s.e.=.094) for brothers who live in different municipalities.

[26] Competing explanations in social psychology include imitation and herding behavior.

Table 3. Mechanisms in the workplace network.

Characteristic of Peer father	First stage (1)	Reduced form (2)	Second stage (3)	N (4)
A. Predicted manager	.311***	.072**	.233**	4,272
	(.049)	(.031)	(.103)	
Not predicted manager	.316***	.028*	.088*	22,579
	(.029)	(.015)	(.047)	
B. Low unionization (≤33%)	.358***	.079***	.219***	6,834
	(.034)	(.026)	(.074)	
High unionization (>33%)	.306***	.036**	.117**	16,225
	(.028)	(.017)	(.055)	
C. Private firm	.301***	.051***	.170***	17,977
	(.027)	(.016)	(.055)	
Public firm	.377***	.032	.084	5,076
	(.041)	(.029)	(.077)	
D. Low tenure firm (<10 yrs)	.307***	.045***	.148**	20,128
	(.030)	(.016)	(.053)	
High tenure firm (≥10 yrs)	.328***	.009	.029	6,723
	(.051)	(.025)	(.075)	

*Notes: Specifications mirror those in Table 1. Peer father predicted to be a manager if he is the first or second highest earner in the firm. Unionization defined at the industry level. Firm tenure type defined by the average tenure of workers in the firm. Sample size can vary across subgroups due to missing values. Standard errors clustered by firm in panel A and by family in panel B. *p<0.10, **p<0.05, ***p<0.01.*

The first piece of evidence relates to the idea that the informational value about the firm specific consequences of taking leave is likely to be higher if the peer father is a senior manager in the firm. Since we do not have information about the management hierarchy within the firm, we assume managers are the employees with the first or second highest wage in the firm. Table 3 reveals the estimated peer effect is over two and a half times larger if the peer father is predicted to be a manager in the firm as opposed to a regular coworker.

We next compare leave take up by type of firm. Workers in firms in highly unionized industries tend to have more secure jobs and regulated pay scales. Consequently, they do not need to worry as much about an employer reacting badly to paternity leave. For workers with high job security, the benefit of learning about a peer father's leave-taking experience should therefore be less valuable. In Table 3, we examine whether this is true. Consistent with the job security hypothesis, the estimated peer effects are twice as large in low unionization workplaces. We find a similar pattern of estimates for private sector jobs versus relatively secure public sector jobs. We also break up firms based on the average tenure of workers within a firm. In the approximately 25% of firms where average tenure is 10 years or more, the estimated peer effect is close to zero. In contrast, for less established firms with higher worker turnover, the peer effect is large and statistically significant. Taken together, these firm-type results suggest the benefit of workplace-specific information is more valuable in settings where there is more job uncertainty.

Table 4. Peer effects on additional days of leave.

	Reduced form (1)	Second stage (2)	Reduced form (3)	Second stage (4)
	A: Workplace network		**B: Family network**	
More than 1 week	.033**	.105**	.047**	.153**
	(.014)	(.043)	(.020)	(.064)
	[.666]	[.666]	[.563]	[.563]
4 weeks or more	.033**	.103**	.039**	.129**
	(.014)	(.046)	(.020)	(.066)
	[.607]	[.607]	[.525]	[.525]
More than 4 weeks	.009	.028	.032**	.104**
	(.011)	(.035)	(.014)	(.047)
	[.172]	[.172]	[.148]	[.148]
More than 8 weeks	.010	.033	.023**	.077**
	(.009)	(.029)	(.012)	(.039)
	[.107]	[.107]	[.095]	[.095]
More than 12 weeks	.008	.025	.016	.051
	(.008)	(.025)	(.011)	(.034)
	[.083]	[.083]	[.074]	[.074]

*Notes: Specifications mirror the baseline specification described in Table 1 and have the same first stage estimates. N = 26,851 for the workplace network and N = 12,495 for the family network. Standard errors clustered by firm in the workplace network and by family in the family network. Comparison mean in brackets. *p<0.10, **p<0.05, ***p<0.01.*

The final piece of evidence exploits that the perceived productivity signal to the employer is likely to change discontinuously if the father's leave period exceeds the four-week quota. The reason is that the family loses the 4 weeks of paid paternity leave if not taken by the father, whereas additional days of paternity leave simply crowd out maternity leave. Taking more than 4 weeks of paternity leave could serve as a signal to employers that a worker is less committed to the job in a way that taking exactly 4 weeks of leave does not. In Table 4, we estimate peer effects on paternity leave spells exceeding 4 weeks. We find that brothers are 10.4 percentage points more likely to take more than 4 weeks of leave because of the reform-induced increase in paternity leave of the peer father; in contrast, there is no evidence of peer effects leading to a crowding out of maternity leave in the workplace.

7 Strength of Ties

It is natural that some peer groups might exert a stronger influence than others. In a seminal study, Granovetter (1973) classifies interpersonal ties into three categories: strong, weak, or absent. Formally, the strength of ties is defined by the overlap in network members; operationally, the strength of a tie is usually defined by the nature and duration of the relationship as well as by the frequency and intensity of interactions. In this section, we use this operational definition to explore heterogeneity in peer effects based on the strength of interpersonal ties between peers.

While weak ties play an important role in Granovetter's setting, in our setting it is strong ties that are likely to matter most.[27] The reason is that prior to the 1993 reform, almost no fathers were taking paternity leave, with the result that few fathers had direct knowledge about costs and benefits initially. Peers with strong ties are more likely to interact with each other and trust each other's opinions, increasing the chance that information will actually be transmitted and acted upon.

In Section 5, we documented sizeable and robust peer effects among brothers and male coworkers. These peer groups have strong ties as judged by the nature, duration, intensity, and frequency of social interactions. Brothers have known each other for a long time, share a familial bond, and are likely to keep in touch with each other. Similarly, male coworkers are likely to have frequent and time-consuming interactions with each other in small to medium sized firms.[28]

Is there any evidence for peer effects when ties are weaker? To answer this question, we first turn to extended family and extended workplace networks. In Table 5, we estimate whether a peer father influences his brother-in-law. This tie is arguably weaker than between brothers both in duration and intensity. We find no evidence of any peer effect in this weaker family network. In the second panel of Table 5, we estimate whether a peer father affects his female coworker's husband. We find no evidence of a significant effect, which is as expected given that a female coworker's husband is a relatively weak tie who generally works for a different firm.

To explore the peer strength of neighbors, the final panel in Table 5 defines peer groups by geographical neighborhoods. We have the street address of all fathers in Norway, so we measure neighborhoods very precisely. We define the two closest

[27] In Granovetter's setting of job finding, he argues for the importance of weak ties since more novel information flows from peers who are part of different social circles. In contrast, he argues that strong ties are less important, since these peers have information sets about available jobs that overlap considerably with what one already knows.

[28] There is little overlap in the two networks. Four percent of coworkers are brothers and 9% of brothers are coworkers in our two samples. While we do not have enough of these observations to estimate the combined peer effect, omitting these observations does not appreciably change our baseline estimates.

Table 5. Peer effects in networks with weak ties.

	First stage (1)	Reduced form (2)	Second stage (3)	N (4)
A. Extended family network – husband of sister				
Take up of leave	.320***	-.004	-.013	8,876
	(.017)	(.023)	(.072)	
	[.043]	[.54]	[.54]	
B. Extended workplace network – husband of female coworker				
Take up of leave	.318***	.015	.047	25,583
	(.037)	(.016)	(.049)	
	[.040]	[.52]	[.52]	
C. Neigborhood network – two closest households on each side				
Take up of leave	.274***	.002	.008	38,550
	(.012)	(.012)	(.043)	
	[.03]	[.58]	[.58]	

*Notes: Specifications described in Section 7. Standard errors clustered by family, firm, and neighborhood, respectively. Comparison mean in brackets. *p<0.10, **p<0.05, ***p<0.01.*

households (prior to the reform) on each side of a father as neighbors. Similar to what we did previously, we limit the sample to "neighborhoods" where there is one birth in a one year window surrounding the reform. We then look at first births to neighbors who had children after the reform and after the peer father.

Interestingly, neighbors defined in this way exert no peer influence on each other for paternity take up. This result holds even if we define neighborhoods more broadly; we find similar results using the four closest households or the entire street. Apparently, in this setting, neighbors are not important peers. When interpreting this evidence, however, it is important to draw a distinction between neighborhoods and neighbors; neighborhoods include an entire vector of attributes, of which neighbors are just one element. So our finding does not mean that neighborhoods play no role in people's decisions, but rather that neighbors defined strictly by close geography seem to have little influence on program participation in our setting.

In each of these weaker networks, the coefficients are small and not significantly different from zero. By way of comparison, the sample sizes and standard errors for the extended workplace and extended family networks are similar to the brother and male coworker networks. So the finding of no significant effect is not due to overly imprecise estimates. Indeed, for the neighborhood network, the sample size is larger and the standard errors are smaller than those for our baseline estimates appearing in Table 1.

8 Snowball Effects

Peer effects can play an important role in the evolution of program participation, because peer effects cascade through a network as the first peer interacts with a second peer, the second peer interacts with a third peer, and so on. In our setting, the peer effect could amplify over time within a firm, with each subsequent birth exhibiting a snowball effect in response to the original reform. This causal chain is initiated by the direct effect of the reform, inducing peer fathers (coworker 1) of children born after April 1, 1993 to take up paternity leave. The second link in the chain is the first subsequent coworker to have a child (coworker 2); his leave behavior is influenced directly by the (reform-induced increase in) leave taking of the peer father. The third link is coworker 3 who has a child after coworkers 1 and 2: the direct influence of the peer father is now amplified by a snowball effect due to the (peer-father-induced increase in) take up of coworker 2. The causal chain continues in this fashion, such that the direct influence of the peer father on coworker i is amplified by a snowball effect operating through the $i-2$ previous coworkers.

In Section 5 we estimated the total peer effect, which included both the direct influence of the peer father and any indirect snowball effects operating through the increase in take up of other coworkers. The goal of this section is to decompose the total peer effect into the direct effect and the snowball effect, and graph the relative importance of these effects over time.

8.1 Identifying Snowball Effects

The following diagram illustrates both the direct effects of a peer father's influence and the indirect, or snowball, effects for the case with four coworkers:

$$\begin{tikzcd}
\text{reform} \arrow[r] & \text{peer father} \arrow[r, bend right] \arrow[rr, bend right] \arrow[rrr, bend right] & \text{coworker 2} \arrow[r, bend left] \arrow[rr, bend left] & \text{coworker 3} \arrow[r, bend left] & \text{coworker 4}
\end{tikzcd}$$

The reform directly influences the peer father, as captured by the horizontal arrow. The peer father directly affects the next coworker after him who has a child, and similarly directly influences coworkers 3 and 4. These direct effects are captured by the bottom arrows in the diagram. But the peer effects of the reform do not stop there. The peer father's effect on other colleagues continues, since coworker 2, who was influenced directly by the peer father, now affects both coworkers 3 and 4. Moreover, coworker 3, who was influenced by both the peer father and by coworker

2 because of the reform, also affects coworker 4. These snowball effects are captured by any path that travels through the top arrows in the diagram.

To make the idea of a snowball effect more precise, we need some notation. Continuing with the case of three coworkers, the causal chain is described by the following system of equations:

$$\begin{aligned} y_{1g} &= \alpha + \lambda p_{1g} \\ y_{2g} &= \alpha_1 + \beta_1 y_{1g} \\ y_{3g} &= \alpha_2 + \beta_2 y_{2g} + \beta_1 y_{1g} \\ y_{4g} &= \alpha_3 + \beta_3 y_{3g} + \beta_2 y_{2g} + \beta_1 y_{1g} \end{aligned}$$

where the price, p_{1g}, of program participation for the peer father (with subscript label 1) varies randomly across firms (denoted by g), and coworkers are sorted by birth order so that coworker j is the jth father in the firm that has a birth.

Random variation in p_{1g} can be used to identify a set of reduced form coefficients:

$$\frac{dy_2}{dp_{1g}} = \frac{dy_2}{dy_1}\frac{dy_1}{dp_{1g}} = \beta_1\lambda = \pi_2$$

$$\frac{dy_3}{dp_{1g}} = \frac{dy_3}{dy_2}\frac{dy_2}{dy_1}\frac{dy_1}{dp_{1g}} + \frac{dy_3}{dy_1}\frac{dy_1}{dp_{1g}} = (\beta_2\beta_1 + \beta_1)\lambda = \pi_3$$

$$\begin{aligned} \frac{dy_4}{dp_{1g}} &= \frac{dy_4}{dy_3}\frac{dy_3}{dy_2}\frac{dy_2}{dy_1}\frac{dy_1}{dp_{1g}} + \frac{dy_4}{dy_3}\frac{dy_3}{dy_1}\frac{dy_1}{dp_{1g}} + \frac{dy_4}{dy_2}\frac{dy_2}{dy_1}\frac{dy_1}{dp_{1g}} + \frac{dy_4}{dy_1}\frac{dy_1}{dp_{1g}} \\ &= (\beta_3\beta_2\beta_1 + \beta_3\beta_1 + \beta_2\beta_1 + \beta_1)\lambda = \pi_4 \end{aligned}$$

The total peer effect on the take-up of coworker j is given by π_j divided by the first stage coefficient λ. By comparing the estimated π's across coworkers, we can identify the snowball effects. The second coworker identifies the direct effect, β_1, as π_2 divided by λ. Subtracting off this direct effect, the snowball effect on the third coworker, $\beta_2\beta_1$, is given by $\pi_3 - \pi_2$ divided by λ; the snowball effect on the third coworker, $(\beta_3\beta_2\beta_1 + \beta_3\beta_1 + \beta_2\beta_1)$, is given by $\pi_4 - \pi_2$ divided by λ.

In general, estimating the indirect effect of peers is difficult since it is hard to know who is influencing who in a network. In our setting, the spatial ordering of births makes the identification problem much simpler. Assuming that fathers who already have births can influence fathers who subsequently have births, and not the other way around, the reduced form coefficients above capture the snowball effects.

To estimate the snowball effects, we follow the RD design described in subsection 3.3. We estimate λ as the jump in take up at the reform date cutoff in a first stage

RD regression. We then estimate the π's from a reduced form RD regression, and use the differences in the estimated π's to estimate the snowball effects.

For simplicity of presentation, the system of equations above implicitly assumes the direct and indirect peer effects are independent of when the coworker's child is born.[29] In reality, the influence of a peer is likely to decay over time, with a smaller peer effect for coworkers having children temporally distant from the peer father. This decay is a nuisance parameter which is not of immediate interest, but which must be accounted for in order to consistently estimate the snowball effects.

8.2 Empirical Results

Table 6 displays the estimated total reduced-form peer effect for each coworker in a firm. Note the first stage coefficient λ is the same for all coworkers, and therefore does not affect the relative size of the snowball effect compared to either the direct or total peer effect. With this note in mind, we proceed by using estimates of the π's to decompose the total peer effect into the direct effect and the snowball effect, and graph the relative importance of these effects over time.

Table 6. Snowball effects on coworkers within a firm.

	Total effect (1)	Percent snowball (2)	Total effect (3)	Percent snowball (4)	Total effect (5)	Percent snowball (6)
	A. No decay		**B. Cubic decay**		**C. Quintic decay**	
Coworker 2 (π_2)	.034**	0%	.028**	0%	.027**	0%
	(.017)		(.014)		(.013)	
Coworker 3 (π_3)	.037**	11 %	.038**	26%	.035**	25%
	(.017)		(.017)		(.016)	
Coworker 4 (π_4)	.050***	44 %	.064***	56%	.060***	55%
	(.018)		(.023)		(.021)	
Coworker 5+ (π_5)	.025	-18 %	.083	66%	.078	66%
	(.016)		(.054)		(.051)	
F-test for snowball	1.01		2.84		2.84	
p-value	[.387]		[.036]		[.037]	

*Notes: Sample includes all coworkers having a child before 2002. Total effect is the total reduced form peer effect, accounting for decay as indicated in the specification headings. Snowball columns indicate the amount of the total effect that can be attributed to the snowball effect. The F-test for snowball efects is a joint test of $\pi_5=\pi_4=\pi_3=\pi_2$. N = 22,869. *p<0.10, **p<0.05, ***p<0.01.*

The first column presents coworker estimates which do not account for decay. This

[29] A possible concern is that the spacing between a coworker's birth and the peer father's birth is affected by the reform. In Appendix Figure A5, we verify that spacing is continuous through the reform and therefore not a source of bias.

regression mirrors our baseline specification, but allows for a separate discontinuity for each coworker. Even without subtracting out decay, the total reduced-form peer effect increases in magnitude from coworkers 2 through 4. Because we do not have enough observations to separately estimate effects within the firm for fifth and later coworkers, we estimate the average peer effect for this group. The total peer effect for this group declines, which is not suprising if decay is sizeable for this group who have children later in the sample period.

To identify the snowball effects, it is necessary to account for decay. We exploit the fact that coworker 2 does not experience a snowball effect since there are no intermediate births in between him and the peer father (coworker 1). Hence, any change over time in the estimated peer effect for coworker 2 can be attributed to decay. We run a preliminary RD regression using the subsample of coworker 2 observations to estimate decay, and then adjust the estimates appearing in column 1 of Table 6 to account for depreciation.[30]

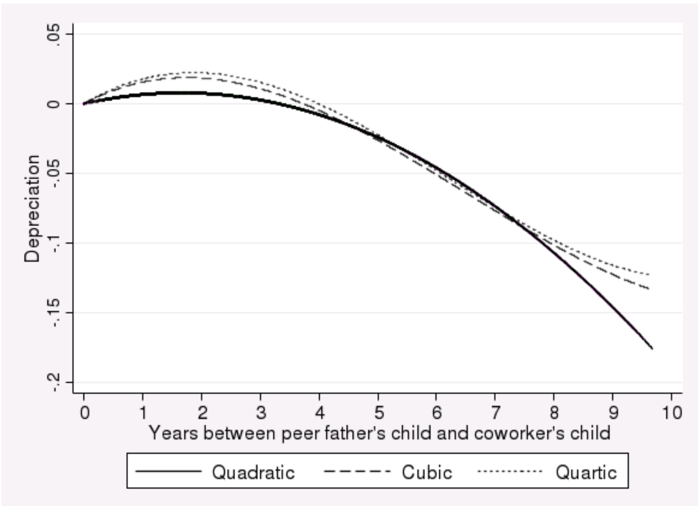


Figure 5. Decay in the estimated peer effect over time.
Notes: Estimated decay based on the subsample of coworker 2 observations (i.e., the first coworker to have a birth after the peer father).

To estimate decay, we augment equation (7) to include a polynomial in the timing difference between the birth date of the peer father's and coworker 2's child and an interaction term between these polynomial terms and the reform cutoff. The coefficients on the interaction terms divided by the coefficient on the reform cutoff identify the depreciation parameters. We plot the implied decay over time based on

[30]Because we estimate the decay parameters based on a coworker 2 subsample, and because we do not have enough coworker 2's who have births later than 2002 in our dataset (less than 5%), our estimates of the snowball effects are restricted to 1993 to 2002.

these estimates in Figure 5. Interestingly, the peer effect appreciates for the first 1.7 years before starting to decline again, with the depreciation term not becoming negative until approximately 3.5 years. This pattern makes sense once one realizes when fathers take leave from their firm. Although fathers generally sign up for leave before the birth of their child, most fathers do not begin their 4 weeks of leave until approximately 9 to 11 months after their child's birth.[31] While there is likely to be some immediate information transmission (e.g., coworker 2 knows early on that the peer father has signed up for leave), more information is revealed after the peer father returns to work. At that point, not only does the peer father have first-hand experience taking leave, but there is also an opportunity in the ensuing months to observe how the employer treats the peer father after his return to work.[32]

Our key results are found in columns 3 through 6 of Table 6, which report decay-adjusted estimates. Whether decay is modeled as a third or fourth order polynomial makes little difference to the estimates. Focusing on the specification which allows for cubic decay, the reduced form coefficient for coworker 2 is estimated to be .028. This coefficient represents only the direct influence of the peer father on coworker 2, since there are no intermediate coworkers to create a snowball effect. It is smaller compared to the estimate in column 1, since on average, coworker 2's have their children early on when there is still appreciation. For coworker 3, the total reduced form peer effect rises to .038. The snowball effect accounts for 26% of the total peer effect.[33] As expected, the snowball effect is even larger for coworker 4 since there are more intervening coworkers. Coworker 4's reduced form estimate is rises to .064, with 56% of the total peer effect attributable to the snowball effect. For fifth and higher coworkers as a group, the snowball effect accounts for 66% of the total effect, although the total effect is imprecisely estimated. As the table documents, these snowball effects are jointly statistically significant after accounting for depreciation. The pattern of increasing peer effects with each subsequent coworker captures the amplification of the original peer father's influence over time within a firm.

Figure 6 graphs the relative importance of the direct peer effect and the snowball effect over time, allowing both the direct peer and snowball effects to decay. The top line in the graph shows the actual leave take up for all coworkers having children

[31] This is because mothers and fathers cannot take leave at the same time, and mothers generally take all their leave before the father starts his leave. Many mothers exercise the option to take 48 weeks of leave at 80% earnings replacement rather than 38 weeks at 100% replacement.

[32] The interval between the peer father and subsequent fathers giving birth in a network is also likely to vary systematically with firm characteristics (such as the size of the firm), in which case our estimates of decay will also include this type of heterogeneity.

[33] The percent of the total peer effect accounted for by the snowball effect for coworker j is calculated as $(\hat{\pi}_j - \hat{\pi}_2)/\hat{\pi}_j$.

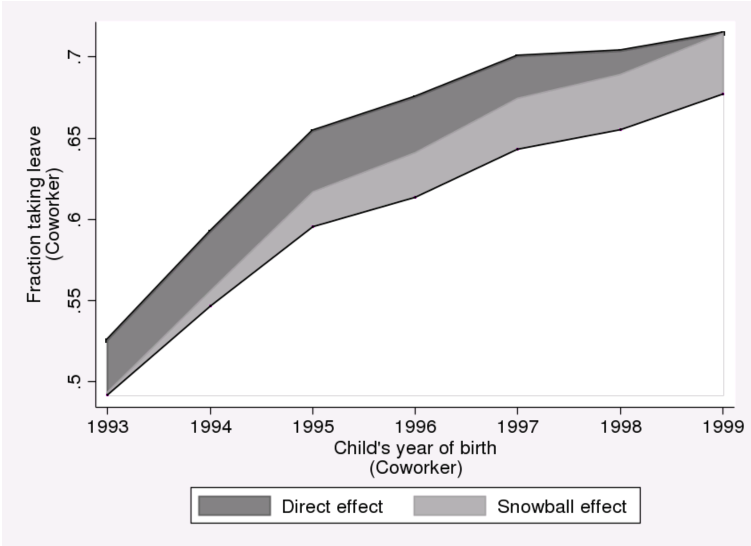


Figure 6. Direct peer and snowball effects over time.
Notes: The top line in the graph shows the actual leave take up for all coworkers having children after the original peer father. The bottom line subtracts the estimated total peer effect originating from the peer father. The grey shading decomposes the gap into direct peer and snowball effects.

after the original peer father. The bottom line subtracts the estimated total peer effect from the total leave take up. To construct this line, we use the estimated effects from column 3 in Table 6 to predict the size of the peer effect originating from the peer father (coworker 1), accounting for the mix of births in each year (coworker 2, 3, 4, and 5+ births) and adding back in depreciation.[34] The difference between the upper and lower lines illustrates how much lower leave take up would have been in each year had the original peer father not influenced any of his coworkers, either directly or indirectly. Figure 6 shows that this counterfactual gap, which includes depreciation, is sizeable and actually gets slightly larger over time.

Even more interesting is the decomposition of this counterfactual gap into direct peer effects and indirect snowball effects over time. The dark gray area in the graph indicates the direct effect of the peer father on coworkers, while the light gray area indicates the snowball effect. In 1993, virtually all of the estimated effect can be attributed to the direct effect, as there is little opportunity for intervening births to create a snowball effect. However, over time, the direct effect which can be mapped back to the original peer father (coworker 1) decays. By 1999 decay is large enough that the original peer father's direct effect on a coworker completely fades away. In

[34]We only plot the period from 1993 to 1999; extrapolating past 1999 is noisy and implies depreciation in excess of 100%. A likely reason is most coworkers have their children long before 1999; the majority of observations past 1999 are to 5th or higher order coworkers in a firm.

contrast, the snowball effect gets larger over time as more coworkers have a child within a given firm. Even though the snowball effects also decay, the accumulation of effects from intervening coworkers more than offsets this decay. By the end of the period, the snowball effect makes up almost 100% of the predicted total peer effect.

Figure 6 illustrates how important early peers are for future take up of social programs. From 1993 to 1999, program participation went from a little over 50% to over 70% of eligible coworkers. Much of this increase is due to common time effects, such as changes in societal norms, and the influence of other peer groups not captured by our estimates. However, even six years after the implementation of the program, the peer effects which can be traced back to the original father account for 21% of the total increase in program participation relative to 1993. These findings are especially important for the rollout of new social programs, as they indicate that participation rates early on can have long-lasting effects on future participation.

9 Conclusion

We find strong evidence for substantial peer effects of program participation in both workplace and family networks. Coworkers and brothers are 3.5 and 4.7 percentage points, respectively, more likely to take paternity leave if their peer father was eligible versus not eligible for paternity leave around the reform cutoff. These estimates imply sizeable peer effects of 11 and 15 percentage points for coworkers and brothers. The most likely mechanism is information transmission about costs and benefits, including increased knowledge of how an employer will react, and not leisure complementarities. We find substantial heterogeneity based on the strength of interpersonal ties between peers; while there are strong effects for long-term familial relationships and among male coworkers, there is no evidence for peer effects in weaker extended workplace and family networks or in neighborhoods defined by geography. Finally, we find the estimated peer effect gets amplified over time within a firm, with each subsequent birth exhibiting a snowball effect in response to the original reform.

Taken together, our results have important implications for the peer effects literature and for the evaluation of social programs. Our study points out that both the workplace and family can serve as important networks in settings where information about the benefits and costs of program participation is scarce and perceptions are in their formative stage. This finding may be of particular importance for the ongoing debate about policies aimed at promoting gender equality, ranging from family policy to affirmative action programs. Advocates of such public interventions often argue that traditional gender roles in both the family and the labor market can be changed or modified through peer influence. Our study also highlights that peer effects can have long-lasting effects on program participation, even in the presence of decay, since any original peer effect cascades through a network over time. This is especially important when considering the design and implementation of new social programs, since the initial group of participants can play a large and lasting role in the evolution of take-up patterns. Social interactions can reinforce the direct effects on take up due to a program's parameters, leading to a long-run equilibrium take-up rate which can be substantially higher than would otherwise be expected.

References

Aizer, A., and J. Currie (2004): "Networks or Neighborhoods? Correlations in the Use of Publicly-funded Maternity Care in California," *Journal of Public Economics*, 88, 2573–2585.

Angelucci, M., and G. De Giorgi (2009): "Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles' Consumption?," *American Economic Review*, 99, 486–508.

Angelucci, M., G. De Giorgi, M. Rangel, and I. Rasul (2010): "Family Networks and School Enrolment: Evidence from a Randomized Social Experiment," *Journal of Public Economics*, 94, 197–221.

Atkinson, A., L. Rainwater, T. Smeeding, O. for Economic Cooperation, and Development (1995): *Income Distribution in OECD Countries: Evidence from the Luxembourg Income Study*. Organisation for Economic Co-operation and Development Paris.

Babcock, P., K. Bedard, G. Charness, J. Hartman, and H. Royer (2011): "Letting Down the Team? Evidence of Social Effects of Team Incentives," Discussion paper, NBER 16687.

Baird, S., A. Bohren, C. McIntosh, and B. Ozler (2012): "Designing Experiments to Measure Spillover and Treshold Effects," Discussion paper.

Bandiera, O., I. Barankay, and I. Rasul (2009): "Social Connections and Incentives in the Workplace: Evidence from Personnel Data," *Econometrica*, 77, 1047–1094.

——— (2010): "Social Incentives in the Workplace," *Review of Economic Studies*, 77(2), 417–458.

Bandiera, O., and I. Rasul (2006): "Social Networks and Technology Adoption in Northern Mozambique," *The Economic Journal*, 116, 869–902.

Bayer, P., S. Ross, and G. Topa (2008): "Place of Work and Place of Residence: Informal Hiring Networks and Labor Market Outcomes," *Journal of Political Economy*, 116, 1150–1196.

Bertrand, M., E. Luttmer, and S. Mullainathan (2000): "Network Effects and Welfare Cultures," *Quarterly Journal of Economics*, 115, 1019–1055.

Bobonis, G., and F. Finan (2009): "Neighborhood Peer Effects in Secondary School Enrollment Decisions," *Review of Economics and Statistics*, 91, 695–716.

Bursztyn, L., F. Ederer, B. Ferman, and M. Yuchtman (2012): "Understanding Peer Effects in Financial Decisions: Evidence from a Field Experiment," Discussion paper.

Carrell, S., R. Fullerton, and J. West (2009): "Does your Cohort Matter? Measuring Peer Effects in College Achievement," *Journal of Labor Economics*, 27, 439–464.

Carrell, S., and M. Hoekstra (2010): "Externalities in the Classroom: How Children Exposed to Domestic Violence affect Everyone's Kids," *American Economic Journal: Applied Economics*, 2, 211–228.

Carrell, S., M. Hoekstra, and J. West (2011): "Is Poor Fitness Contagious? Evidence from Randomly Assigned Friends," *Journal of Public Economics*, 95, 657–663.

CARRELL, S., F. MALMSTROM, AND J. WEST (2008): "Peer Effects in Academic Cheating," *Journal of Human Resources*, 43, 173–207.

CARRELL, S., B. SACERDOTE, AND J. WEST (2011): "From Natural Variation to Optimal Policy? the Lucas Critique Meets Peer Effects," Discussion paper, NBER 16865.

CASE, A., AND L. KATZ (1991): "The Company You Keep: The Effects of Family and Neighborhood on Disadvantaged Youths," Discussion paper, NBER 3705.

COOLS, S., J. FIVA, AND L. KIRKEBØEN (2011): "Causal Effects of Paternity Leave on Children and Parents," Discussion paper, Discussion Paper, Statistics Norway.

CULLEN, J., B. JACOB, AND S. LEVITT (2006): "The Effect of School Choice on Participants: Evidence from Randomized Lotteries," *Econometrica*, 74, 1191–1230.

DING, W., AND S. LEHRER (2007): "Do Peers Affect Student Achievement in China's Secondary Schools?," *Review of Economics and Statistics*, 89, 300–312.

DUFLO, E., AND E. SAEZ (2003): "The Role of Information and Social Interactions in Retirement Plan Decisions: Evidence from a Randomized Experiment," *Quarterly Journal of Economics*, 188, 815–842.

DUNCAN, G., J. BOISJOLY, M. KREMER, D. LEVY, AND J. ECCLES (2005): "Peer Effects in Drug Use and Sex Among College Students," *Journal of Abnormal Child Psychology*, 33, 375–385.

EVANS, W., W. OATES, AND R. SCHWAB (1992): "Measuring Peer Group Effects: A Study of Teenage Behavior," *Journal of Political Economy*, 100, 966–991.

FIGLIO, D. N., S. HAMERSMA, AND J. ROTH (2011): "Information Shocks and Social Networks," Discussion paper, NBER 16930.

GAVIRIA, A., AND S. RAPHAEL (2001): "School-based Peer Effects and Juvenile Behavior," *Review of Economics and Statistics*, 83, 257–268.

GLAESER, E., B. SACERDOTE, AND J. A. SCHEINKMAN (1996): "Crime and Social Interactions," *Quarterly Journal of Economics*, 111, 507–548.

GRANOVETTER, M. (1973): "The Strength of Weak Ties," *American Journal of Sociology*, 78, 1360–1380.

HANUSHEK, E., J. KAIN, J. MARKMAN, AND S. RIVKIN (2003): "Does Peer Ability Affect Student Achievement?," *Journal of Applied Econometrics*, 18, 527–544.

HENSVIK, L., AND P. NILSSON (2010): "Businesses, Buddies and Babies: Social Ties and Fertility at Work," Discussion paper, IFAU.

HOXBY, C. (2000): "The Effects of Class Size on Student Achievement: New Evidence from Population Variation," *Quarterly Journal of Economics*, 115, 1239–1285.

IMBERMAN, S., A. KUGLER, AND B. SACERDOTE (2009): "Katrina's Children: Evidence on the Structure of Peer Effects from Hurricane Evacuees," Discussion paper, NBER 15291.

JACOB, B. (2004): "Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago," *American Economic Review*, 94, 233–258.

KATZ, L., J. KLING, AND J. LIEBMAN (2001): "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment," *Quarterly Journal of Economics*, 116, 607–654.

Kling, J., J. Liebman, and L. Katz (2007): "Experimental Analysis of Neighborhood Effects," *Econometrica*, 75, 83–119.

Kremer, M., and D. Levy (2008): "Peer Effects and Alcohol Use among College Students," *Journal of Economic Perspectives*, 22, 189–3A.

Kremer, M., and E. Miguel (2007): "The Illusion of Sustainability," *Quarterly Journal of Economics*, 122, 1007–1065.

Kuhn, P., P. Kooreman, A. Soetevent, and A. Kapteyn (2011): "The Effects of Lottery Prizes on Winners and their Neighbors: Evidence from the Dutch Postcode Lottery," *The American Economic Review*, pp. 2226–2247.

Lalive, R., and M. Cattaneo (2009): "Social Interactions and Schooling Decisions," *Review of Economics and Statistics*, 91, 457–477.

Lefgren, L. (2004): "Educational Peer Effects and the Chicago Public Schools," *Journal of Urban Economics*, 56, 169–191.

Ludwig, J., G. Duncan, and P. Hirschfield (2001): "Urban Poverty and Juvenile Crime: Evidence from a Randomized Housing-mobility Experiment," *Quarterly Journal of Economics*, 116, 655–679.

Manski, C. (1993): "Identification of Endogenous Social Effects: The Reflection Problem," *Review of Economic Studies*, 60, 531–542.

Markussen, S., and K. Røed (2012): "Social Insurance Networks," Discussion paper, IZA 6446.

Mas, A., and E. Moretti (2009): "Peers at Work," *American Economic Review*, 99, 112–145.

Maurin, E., and J. Moschion (2009): "The Social Multiplier and Labor Market Participation of Mothers," *American Economic Journal. Applied Economics*, 1, 251–272.

Moffitt, R. (2001): "Policy Interventions, Low-level Equilibria, and Social Interactions," in *Social Dynamics*, pp. 45–82. Cambridge: MIT Press.

Monstad, K., C. Propper, and K. Salvanes (2011): "Is Teenage Motherhood Contagious? Evidence from a Natural Experiment," Discussion paper.

Munshi, K. (2003): "Networks in the Modern Economy: Mexican Migrants in the US Labor Market," *Quarterly Journal of Economics*, 118, 549–599.

Rege, M., and I. Solli (2010): "The Impact of Paternity Leave on Long-term Father Involvement," Discussion paper.

Rege, M., K. Telle, and M. Votruba (2009): "Social Interaction Effects in Disability Pension Participation: Evidence from Plant Downsizing," *UiS Working Papers in Economics and Finance*.

Ross, S. L. (2009): "Social Interaction within Cities: Neighborhood Environments and Peer Relationships," *University of Connecticut Working Paper 31*.

Sacerdote, B. (2001): "Peer Effects with Random Assignment: Results for Dartmouth Roommates," *Quarterly Journal of Economics*, 116, 681–704.

Stinebrickner, R., and T. Stinebrickner (2006): "What Can be Learned about Peer Effects using College Roommates? Evidence from New Survey Data and Students from Disadvantaged Backgrounds," *Journal of public Economics*, 90, 1435–1454.

Zimmerman, D. (2003): "Peer Effects in Academic Outcomes: Evidence from a Natural Experiment," *Review of Economics and Statistics*, 85, 9–23.

For Online Publication:
Appendix Figures and Tables

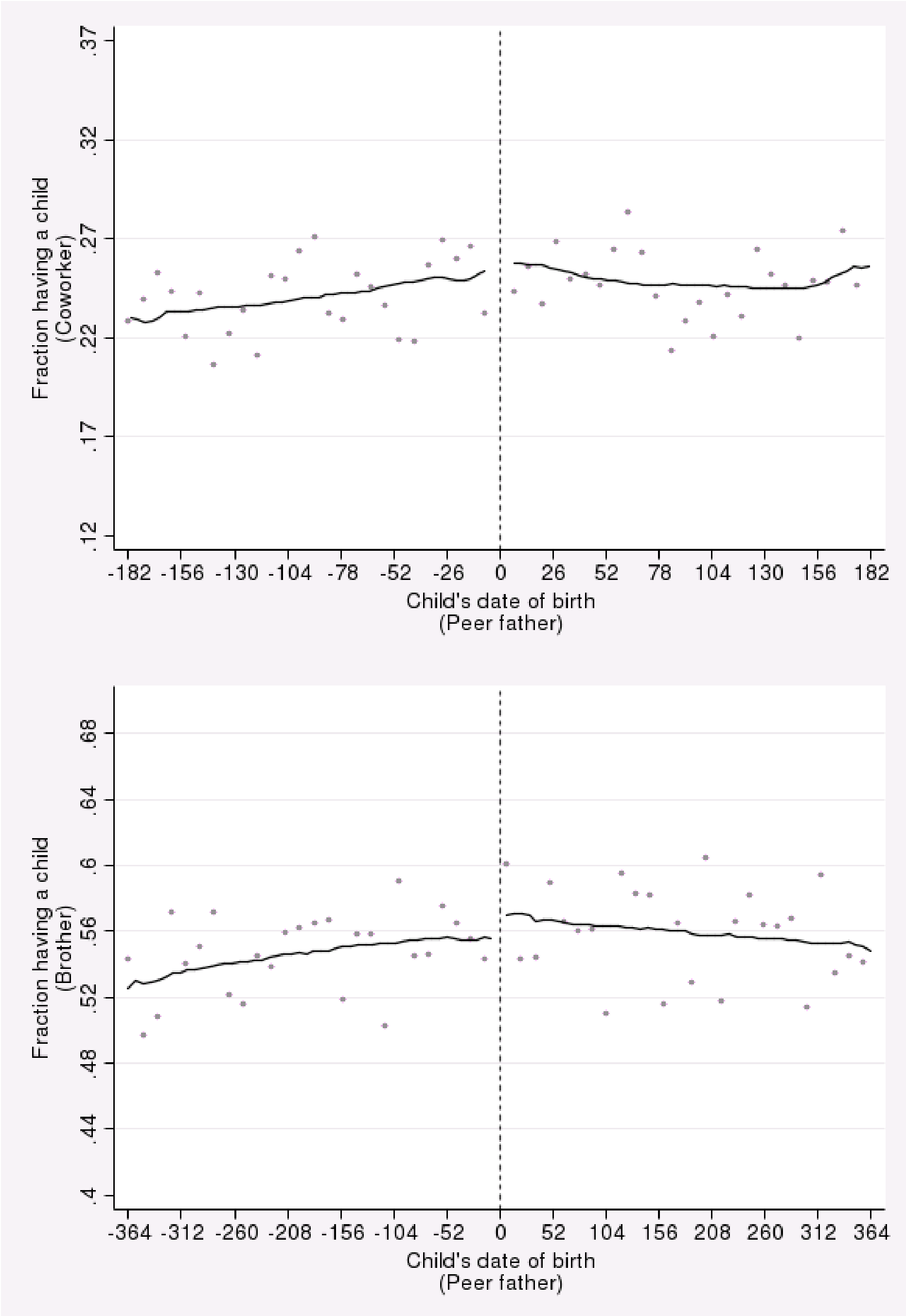


Figure A1. Coworker's and brother's fertility.

Notes: The top graph is for coworkers and the bottom graph is for brothers. Each observation is the average number of children born to coworkers/brothers in a bin, based on the birthdate of the peer father's child. The top graph uses one week bins, the bottom graph uses two week bins. The plotted local linear regression lines are based on daily, individual-level data. Dashed vertical lines denote the reform cutoff of April 1, 1993, which has been normalized to zero.

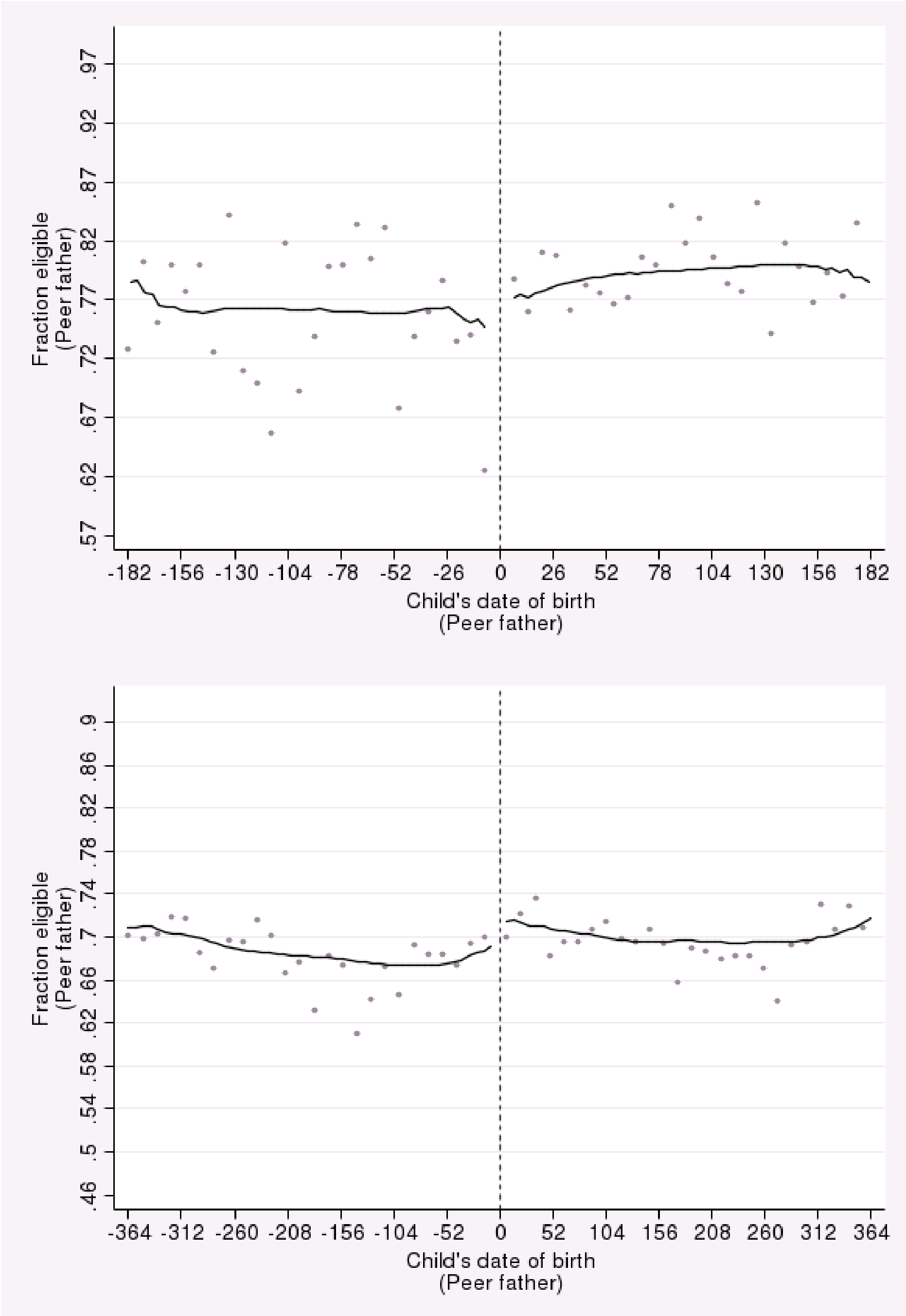


Figure A2. Peer father's eligibility.

Notes: The top graph is for coworkers and the bottom graph is for brothers. Each observation is the average number of children born to coworkers/brothers in a bin, based on the birthdate of the peer father's child. The top graph uses one week bins, the bottom graph uses two week bins. The plotted local linear regression lines are based on daily, individual-level data. Dashed vertical lines denote the reform cutoff of April 1, 1993, which has been normalized to zero.

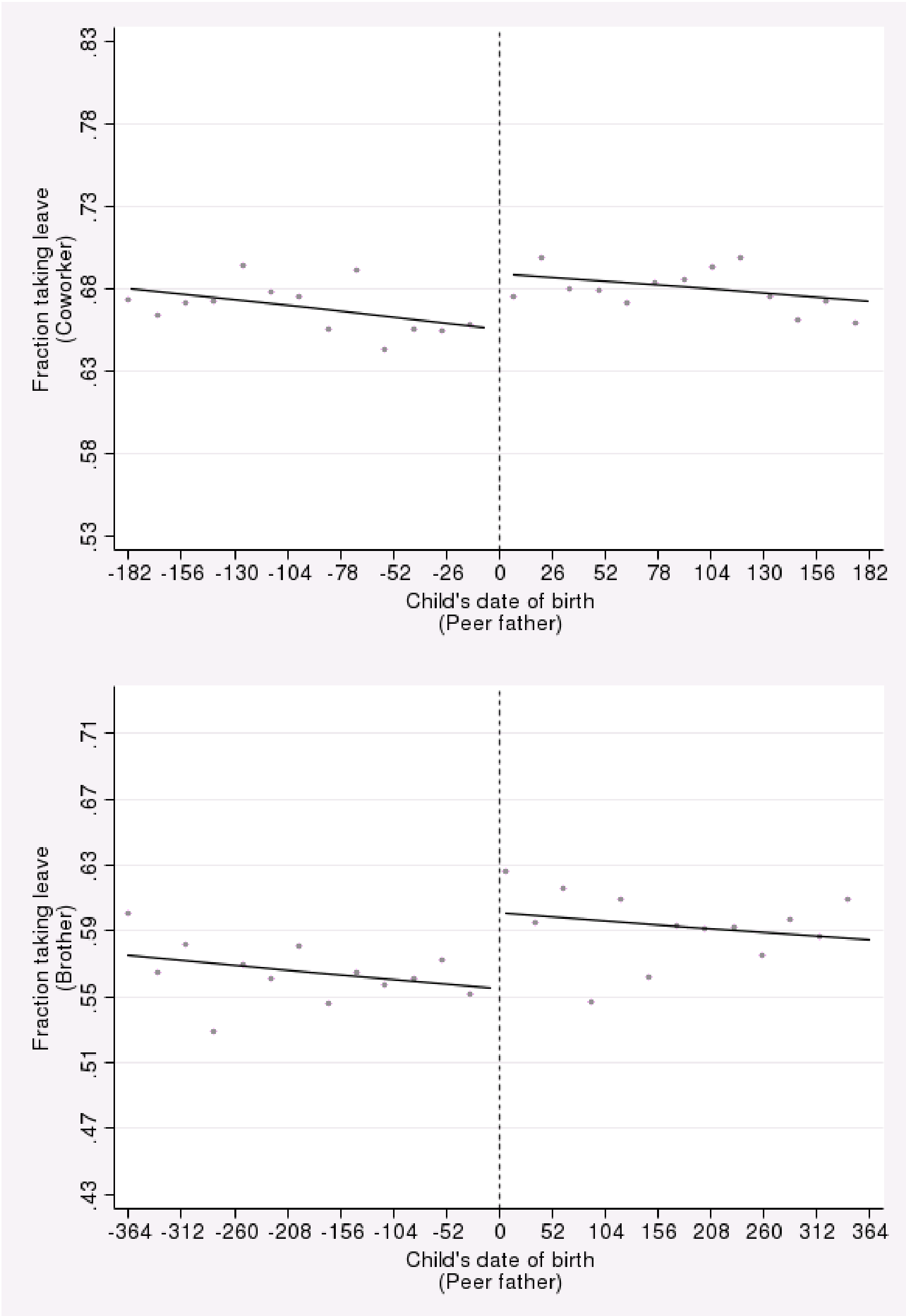


Figure A3. Coworker's and brother's leave take up using wider bins.

Notes: Each observation is the average number of coworkers taking paternity leave in two-week bins (top panel) or brothers taking paternity leave in four-week bins (bottom panel), based on the birthdate of the peer father's child. Dashed vertical lines denote the reform cutoff of April 1, 1993, which has been normalized to zero.

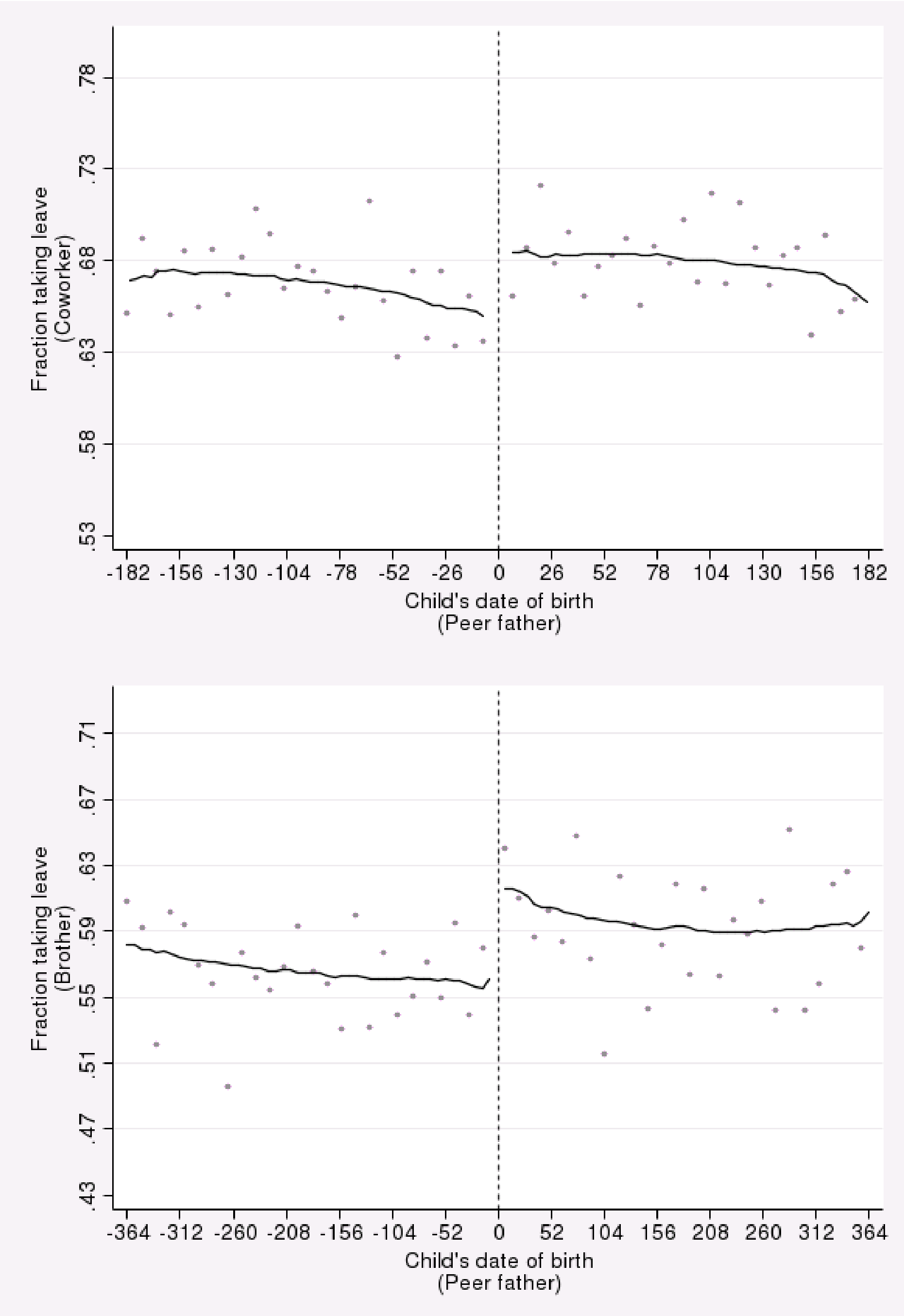
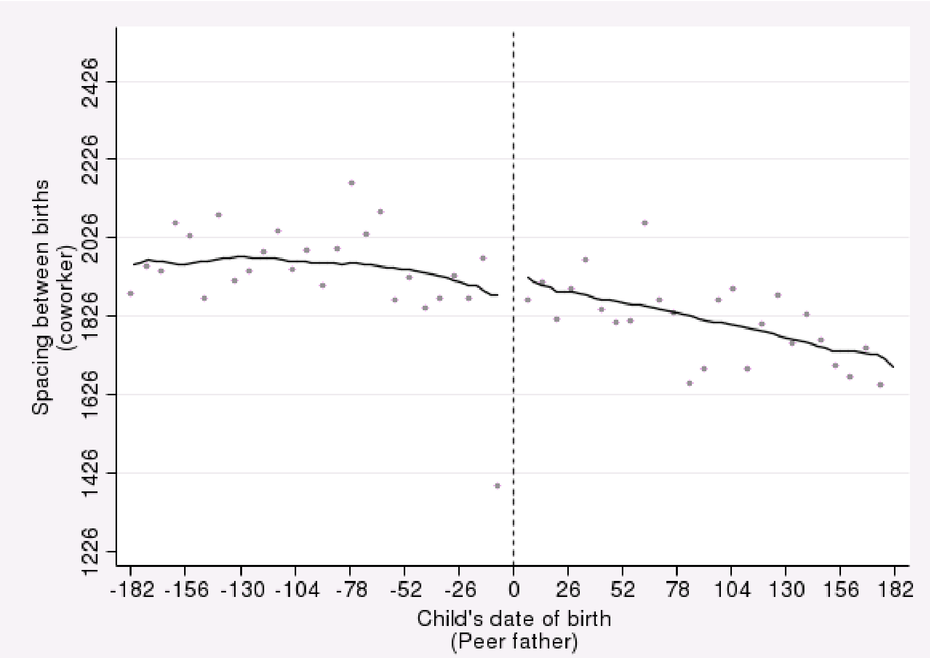


Figure A4. Local linear regression graphs for coworker's and brother's leave.

Notes: The plotted local linear regression lines are based on daily, individual-level data. The top graph is for coworkers and the bottom graph is for brothers. For comparison, dots for the average number of coworkers/brothers taking paternity leave in one week intervals (coworkers) and two week intervals (brothers) are also included in the figure, based on the birthdate of the peer father's child. Dashed vertical lines denote the reform cutoff of April 1, 1993, which has been normalized to zero. See notes to Table A5.



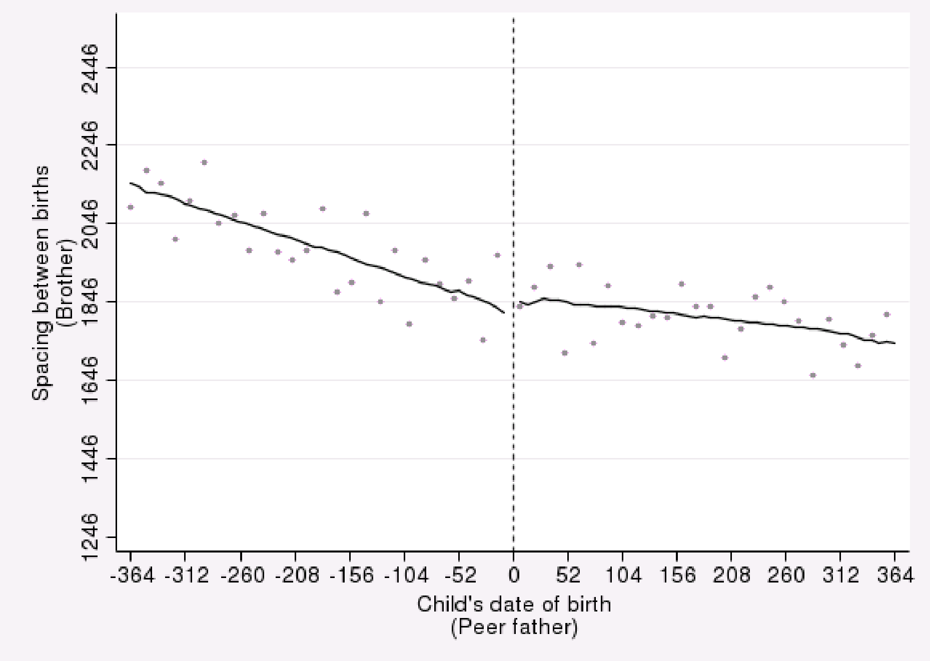


Figure A5. Spacing between the coworker's/brother's and the peer father's births.

Notes: The top graph is for coworkers and the bottom graph is for brothers. Each observation is the average number of days between births to a coworker/brother and the peer father in a bin. The top graph uses one week bins, the bottom graph uses two week bins. The plotted local linear regression lines are based on daily, individual-level data. Dashed vertical lines denotes the reform cutoff; the reform cutoff date of April 1, 1993 has been normalized to zero.

Table A1. Descriptive statistics for fathers in the workplace and family networks.

	One year window		Two year window	
Father	Coworker sample	All fathers	Brother sample	All fathers
characteristics	(1)	(2)	(3)	(4)
Some college	.23	.28	.26	.27
	(.42)	(.44)	(.44)	(.45)
Age at birth	31.3	31.9	28.9	31.9
	(5.4)	(5.5)	(4.0)	(5.5)
Married	.45	.48	.39	.48
	(.50)	(.50)	(.49)	(.50)
Child a girl	.50	.49	.49	.49
	(.50)	(.50)	(.50)	(.50)
Number of children	2.7	2.8	2.7	2.8
	(1.98)	(1.04)	(1.92)	(2.03)
N	7,504	38,958	10,823	81,913

Notes: Column (1) is our estimation sample of reform-window fathers in firms which have just one birth within 6 months on either side of the reform, and who also have a coworker whose first child is born after the father and after the reform. Column (2) is a comparison sample of all eligible fathers in Norway in the corresponding one year window. Column (3) is our estimation sample of reform-window fathers who have brothers, where the brother has a first child after the father and after the reform. Column (4) is a comparison sample of all eligible fathers in Norway in the corresponding two year window. There are 50, 134, 23, and 285 missing observations for the married variable and 166, 805, 68, and 1,684 missing observations for the some college variable in columns (1), (2), (3), and (4), respectively.

Table A2. Timing of fertility around the reform window of April 1, 1993.

Birthdate of child	Coefficient
March 4 - 10, 1993	1.44
	(4.58)
March 11 - 17, 1993	2.21
	(4.58)
March 18 - 24, 1993	-3.05
	(4.58)
March 25 - 31, 1993	-9.92**
	(4.58)
April 1 - 7, 1993 (first week post reform)	10.72**
	(4.58)
April 8-14, 1993	4.27
	(4.58)
April 15-21, 1993	2.74
	(4.58)
April 22-28, 1993	2.10
	(4.58)
N	5,479

Notes: Regression of daily birth rates on dummy variables for birth weeks around the reform window. Control variables include day of week, month, and year dummies, as well as 365 day of year dummies. Sample includes all births between 1992 and 2006 to fathers eligible for any type of parental leave. On average, there are 840 births per week to eligible fathers in all of Norway. Standard errors in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Table A3. RD estimates for direct effects of the April 1, 1993 reform on covariates.

	Workplace network (1)	Family network (2)
1. Father has some college	.034	-.011
	(.030)	(.016)
	[.22]	[.25]
	26,178	12,340
2. Mother has some college	-.015	.007
	(.031)	(.017)
	[.28]	[.28]
	26,502	12,240
3. Father's age at birth	-.375	-.106
	(.371)	(.164)
	[31.2]	[28.8]
	26,851	12,495
4. Mother's age at birth	-.521	-.091
	(.340)	(.167)
	[28.7]	[27.1]
	26,851	12,491
5. Marital status at birth	-.036	.001
	(.035)	(.019)
	[.44]	[.39]
	26,708	12,495
6. Child is a girl	-.010	-.001
	(.035)	(.019)
	[.48]	[.49]
	26,427	22,262
7. Father's firm size	-4.5	–
	(5.0)	–
	[45.1]	–
	26,851	–
8. Father predicted to be eligible	.033	.020
	(.027)	(.015)
	[.78]	[.70]
	34,385	17,696

Notes: Regressions use daily data, include linear trends in birth day on each side of the discontunity, and employ triangular weights. Sample restrictions and control variables are the same as those in Table 1. For each regression, coefficient estimates, standard errors in parentheses, Standard errors in parentheses, clustered by firm in column (1) and by extended family in column (2). Comparison mean in brackets based on peer fathers with births in the pre-reform window. Number of observations reported below the comparison means. *p<0.10, **p<0.05, ***p<0.01.

Table A4. Window robustness checks for coworker and brother peer effects.

Window	First stage (1)	Reduced form (2)	Second stage (3)	N (4)
	Panel A: Workplace network			
90 days	.312***	.043**	.138**	14,069
	(.036)	(.018)	(.060)	
135 days	.320***	.035**	.109**	20,498
	(.028)	(.015)	(.047)	
180 days (baseline)	.317***	.034**	.109**	26,851
	(.026)	(.013)	(.043)	
	Panel B: Family network			
180 days	.318***	.063**	.198**	6,083
	(.020)	(.029)	(.091)	
275 days	.309***	.053**	.171**	9,179
	(.016)	(.023)	(.074)	
365 days (baseline)	.304***	.047**	.153**	12,495
	(.014)	(.020)	(.065)	

Notes: Specifications mirror the baseline specifications described in Table 1, changing the window size on each side of the reform. Standard errors clustered by firm in panel A and by family in panel B. *p<0.10, **p<0.05, ***p<0.01.

Table A5. Local linear regression estimates for coworker and brother peer effects.

Bin width	First stage (1)	Reduced form (2)	Second stage (3)	N (4)
		Panel A: Workplace network		
60 days	.317***	.045*	.141*	9,030
	(.047)	(.024)	(.085)	
90 days	.313***	.042**	.134**	13,939
	(.037)	(.018)	(.063)	
120 days	.306***	.039**	.128**	18,055
	(.030)	(.016)	(.056)	
		Panel B: Family network		
120 days	.316***	.066**	.208**	4,079
	(.025)	(.033)	(.104)	
180 days	.312***	.050*	.160*	6,052
	(.020)	(.027)	(.083)	
240 days	.307***	.052**	.170**	8,104
	(.017)	(.023)	(.071)	

Notes: Samples mirror the baseline samples described in Table 1. Estimates based on local linear regressions with a uniform kernel with no control variables included. N is based on the number of observations in the bin width. Bootstrap standard errors, clustered by firm in panel A and by family in panel B, based on 2,000 replications in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Table A6. Regression discontinuity estimates for direct effects of the reform on other outcomes.

	Total Years Employed (max=12)			Total Earnings (12 year annuity)			GPA of child	Married (in 2006)	# kids (in 2006)
	Father	Mother	Ratio F/M	Father	Mother	Ratio F/M			
	(1)	(2)	(3)	(3)	(4)	(6)	(7)	(8)	(9)
	-.006	-.038	-.001	-6,261	-2,032	-.000	.003	.004	.005
	(.031)	(.043)	(.002)	(3,793)	(1,636)	(.003)	(.019)	(.007)	(.007)
	[11.5]	[10.6]	[.47]	[356,707]	[195,871]	[.37]	[4.10]	[.67]	[2.6]
N	81,794	81,794	81,794	81,794	81,794	81,794	79,076	81,794	80,762

Notes: Specification uses daily data, includes linear trends in birth day on each side of the discontunity, and employs triangular weights. Sample includes all fathers with a child of any parity born within one year of the reform who are eligible to take any type of parental leave. Employment in a year defined by whether an individual's earnings exceed the "substantial gainful activity" level (approximately NOK 72,900 in the year 2010 or $12,500), total earnings defined as the sum of annual gross earnings (including self-employment earnings) annuitized over 12 years, GPA of child defined as the average of the exam scores taken at the end of compulsory lower secondary school (scores ranges from one to six) normalized to be mean zero and standard deviation one, married is a dummy variable which equals 1 if the parents are still married in 2006, and # kids is total fertility as of 2006. Standard errors clustered by family in parentheses. Comparison mean in brackets.*p<0.10, **p<0.05, ***p<0.01.