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Peer Effects in Program Participation

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Peer Effects in Program Participation

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. 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 was exogenously induced to take up leave. The most likely mechanism is information transmission about the costs and benefits of taking paternity leave, including increased knowledge of how an employer will react. The estimated peer effect snowballs over time, as the first peer interacts with a second peer, the second peer interacts with a third peer, and so on. This leads to long-run participation rates which are substantially higher than would otherwise be expected.

JEL-Code: H530, J130, I380.

Keywords: program participation, peer effects, social interactions.

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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 (Manski, 1993). 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 groups and exploit variation in the "price" of a social program for a random subset of individuals. 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] With random variation in treatment (and group membership determined prior to treatment), the triple threats identified by Manski no longer bias the estimates.

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

[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), Evans, Oates, and Schwab (1992), Gaviria and Raphael (2001), Glaeser, Sacerdote, and Scheinkman (1996), Hensvik and Nilsson (2010), Markussen and Roed (2012), Maurin and Moschion (2009), Monstad, Propper, and Salvanes (2011), Munshi (2003), and Rege, Telle, and Votruba (2012).

[2] See, for example, Babcock, et al. (2011), Bandiera, Barankay, and Rasul (2009, 2010), Carrell, Fullerton, and West (2009), Carrell and Hoekstra (2010), Carrell, Hoekstra, and West (2011), Carrell, Sacerdote, and West (2011), Cullen, Jacob, and Levitt (2006), Duncan et al. (2005), Hanushek et al. (2003), Hoxby (2000), Imberman, Kugler, and Sacerdote (2012), 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 this empirical strategy, which Moffitt (2001) labels the "partial population" approach. See Angelucci et al. (2010), Baird et al. (2012), Bobonis and Finan (2009), Bursztyn et al. (2012), Duflo and Saez (2003), Hesselius, Nilsson, and Johansson (2009), Kremer and Miguel (2007), Kuhn et al. (2011), and Lalive and Cattaneo (2009).

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 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. To encourage more fathers to take leave, the 1993 reform stipulated this extra month of paid leave could only be taken by fathers.

We focus on 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 of paternity leave for some reform-window 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.

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 percentage points, 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.

We explore several possible channels for these effects. We find suggestive evidence for information transmission about costs and benefits. In particular, 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. Indeed, we find larger effects when the peer father is a senior manager in the firm and in work environments where there is less job security. These

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

findings are consistent with survey data collected prior to the paternity leave reform which suggested many fathers were reluctant to take leave because they were worried how employers and coworkers would react.[5]

A key result is that 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 effect 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. Decomposing these effects over time, most of the estimated peer effect can be attributed to the direct influence of the peer father in the early years after the reform. Over time, however, the contribution of the snowball effect dominates as the direct effect decays and more coworkers have a child within a given firm.

Taken together, our results have important implications for the peer effects literature and for the evaluation of social programs. Both the workplace and family can serve as important networks in settings where information is sparse and social norms are changing. This is particularly relevant 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 labor markets can be changed or modified via peer influence. Our study also highlights that peers 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.

The remainder of the paper proceeds as follows. Section 2 discusses our identification strategy. In Sections 3 and 4, we discuss the 1993 reform, our data, and threats to identification. Section 5 presents our main findings on peer effects and Section 6 explores possible mechanisms. Section 7 estimates how peer effects cascade through the workplace network. The final section offers some concluding remarks.

[5]See Brandth and Kvande (1992) and the Norwegian government's white papers from 1991 and 1995 on parental leave (NOU, 1991; NOU, 1995). The government's 1995 white paper, referring to why many fathers do not take paternity leave, stated "fathers are concerned that both employers and coworkers will perceive them as less invested in their careers if they exhibit a large commitment to family." The report further stated "one reason fathers fail to exercise their rights may also be fear of deviating from the usual pattern in the workplace."

2 Identification strategy

2.1 Using Experimental Variation within Naturally Occurring Peer Groups

The objective of this paper is to estimate peer effects in naturally occurring peer 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.

To fix ideas, consider an experiment with two individuals in each group g, where the price, p_{1g}, of program participation for individuals with the label 1 varies randomly across groups but there is no change for any individuals with the label 2. Letting y_{ig} denote the outcome for individual i in group g, the system of simultaneous equations for peer effects is:[6]

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

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

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.

Since p_{1g} is random, it will be uncorrelated with x_{1g}, x_{2g}, w_g, e_{1g}, and e_{2g}. This implies that λ can be identified from a regression of y_{1g} on p_{1g}. As long as individual 2 makes their choice after individual 1, 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 $\hat{\lambda}$.

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}.

2.2 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:

[6] Manski's formulation replaces the covariates x_{ig} and y_{ig} with expectations; we use Moffitt's (2001) approach, which is more general since it allows for e_{1g} to affect y_{2g} (and e_{2g} to affect y_{1g}).

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:[7]

$$y_{1g} = \alpha_1 + 1[t \geq c](g_l(t-c) + \lambda) + 1[t < c]g_r(c-t) + e_{1g} \tag{3}$$

$$y_{2g} = \alpha_2 + \beta y_{1g} + 1[t \geq c]f_l(t-c) + 1[t < c]f_r(c-t) + e_{2g} \tag{4}$$

where c is the cut-off date, and f_l,f_r,g_l, and g_r are unknown functions. 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 near 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 (3). 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):

$$y_{2g} = \gamma_2 + 1[t \geq c](h_l(t-c) + \pi) + 1[t < c]h_r(c-t) + u_{2g} \tag{5}$$

where h_l and h_r are unknown functions, and 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.

An advantage of the reduced form model is that it requires fewer assumptions to estimate the sign of the peer effect. Absent manipulation of the assignment variable, the reduced form consistently estimates the effect of having a peer father exposed to the new versus old regime. To consistently estimate the size of the peer effect via 2SLS, one also needs to assume the only channel for coworkers or brothers to be affected is through the peer father's take up of paternity leave. For example, a potential concern is that program participation means something different in the pre- and post-regimes, with fathers taking leave under the new policy sending a weaker signal to their peers about the costs of taking leave.

To our knowledge, RD has not previously been used to estimate peer effects within naturally occurring peer groups. 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.

[7]See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for details on the implementation and assessment of RD designs.

In an RD design, a window surrounding the cutoff (i.e., the reform date) needs to be specified, which raises several issues in a many-to-one setting. First is how to define the running variable when there is more than one peer father in the window, particularly when there are peer fathers both before and 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 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? A final issue is that for large networks, an RD approach will have little power; 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.

As discussed in Section 3, we sidestep these issues by looking at networks where there is a single peer father in the reform window.

3 Background and Data

3.1 Paternity Leave

Governmental paid parental leave has a long history in Norway.[8] 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. Apart from a few weeks reserved for the mother, parents could share the leave between them as desired before 1993, but few fathers took any amount of leave.

In an effort to promote gender equality, in October 1992 the labor party introduced a paternal-leave taking quota in their proposed national budget for 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.[9] 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, with the restriction that mothers and fathers could not both take leave at the same time.

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

[8] Our description builds on Rege and Solli (2010) and Cools et al. (2011).

[9] 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. We believe the paternity quota is the driving force behind the increased fraction of fathers taking leave, as none of the previous extensions of shared parental leave increased father's leave take-up.

parents (whether married or cohabiting) working at least 6 of the last 10 months. Both the father's and mother's earnings in the prior 10 months needed to exceed the "substantial gainful activity" threshold (approximately NOK 72,900 in the year 2010 or $12,500). 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. 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.[10] 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.

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 on the application form.

The introduction of the paternity quota led to a large increase in take-up rates of parental leave by fathers, as shown in Figure 1. 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 from 1992 to 2006. We link this data with administrative registers provided by Statistics Norway, a rich longitudinal database that covers every resident from 1967 to 2006. For each year, it contains individual demographic information and unique identifiers that allow us to match

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

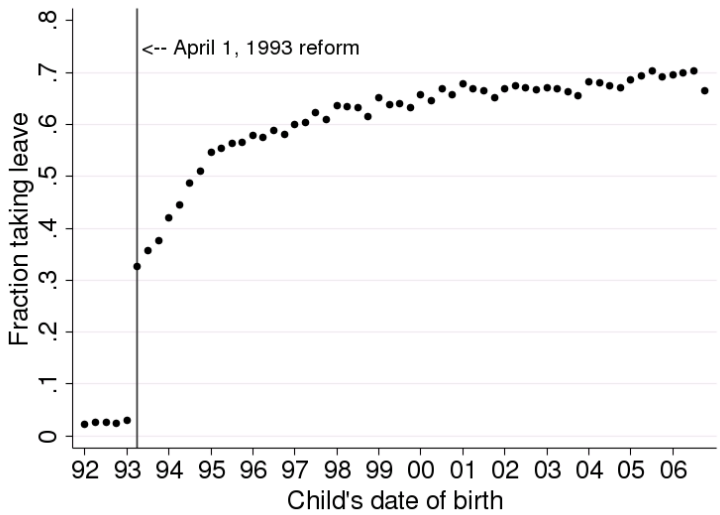


Figure 1. Paternity leave take up by quarter of year for all eligible fathers, 1992-2006.

spouses and parents to their children. We further merge these data sets with linked employer-employee data that contains complete records of all firms and workers for the period 1992 to 2006.

For both coworker and brother networks, we restrict the sample to fathers predicted to be eligible in order to gain precision. 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 described in the previous subsection.[11]

We further refine the sample to allow us to cleanly identify a single peer father and use a straightforward RD design, as discussed in Section 2.2. 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 workplace 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.

One implication of our approach is that 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. These small and medium

[11] Average take-up for predicted eligible fathers is 60% over the entire post-reform period, while it is only 4% for predicted non-eligible fathers.

firms are ideally suited for a study of peer effects, because it is likely that employees in these types of firms interact with each other directly.

In Online Appendix Table A.1, we document the characteristics of fathers in each of our networks. Our coworker sample contains approximately 20% of the entire population of eligible fathers, while our brother sample contains 13%. There is little overlap in the two networks; 4% of coworkers are brothers and 9% of brothers are coworkers in our two samples.

4 Threats to Identification

The validity of our RD design requires that individuals cannot manipulate the assignment variable, which is the birthdate of the peer father's child. There is little opportunity to strategically time conception, as the implementation date for the reform was announced less than nine months in advance. However, it is still possible that mothers with due dates close to the cutoff date could postpone induced births and planned cesarean sections.[12] As shown in Online Appendix Table A.2, there is some evidence that a small number of births are delayed (10 fewer births the week before the reform for all of Norway, relative to an average of 840 births per week). 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 on each side of the reform, or no donut at all, does not materially affect our findings.

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. Given the timing of the reform, there is little opportunity for eligibility manipulation. An RD regression in Online Appendix Table A.3 confirms there is no significant jump in predicted eligibility of peer fathers around the cut-off date.

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 Online Appendix Table A.3, we test whether covariates are directly affected by the 1993 reform. It is reassuring to find these RD estimates are close to zero and always insignificant.

Our analysis estimates peer effects in the take up of paternity leave, conditional on coworkers or brothers having children after the reform. It is possible that having a

[12] See e.g. Dickert, Conlin, and Chandra (1999).

peer father exposed to the reform could also affect the fertility behavior of coworkers and brothers. However, as illustrated in Online Appendix Figure A.1, there is no measurable effect on the fertility of coworkers or brothers of peer fathers.[13]

5 Peer Effect Results

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 before turning to a more detailed regression-based analysis.

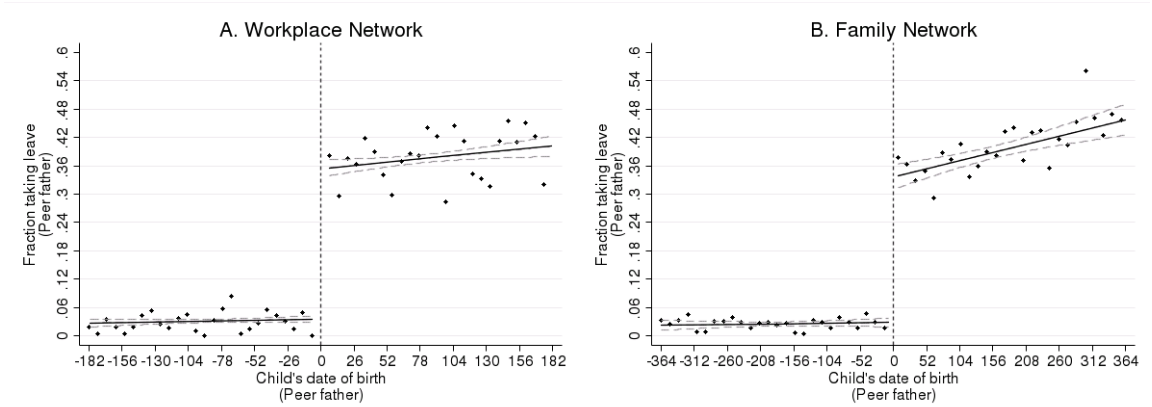


Figure 2. Fraction of peer fathers taking leave.
Notes: Each observation is the average number of peer fathers taking paternity leave in one-week bins (left panel) or two-week bins (right panel), based on the birthdate of their child. Dashed vertical lines denote the reform cutoff of April 1, 1993 (normalized to 0).

Figure 2 displays the fraction of peer fathers (i.e., reform-window fathers) taking any amount of paid leave in a window surrounding the reform, for both the workplace and family networks. 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 use unrestricted means for two-week bins since we have fewer observations.[14] For both networks, there is a sharp jump in the take-up rate of peer fathers at the cutoff, with program participation rising from roughly 3% to 35%. These graphs provide

[13]A formal statistical test, mirroring the RD regression specification of Table 1, yields an ITT estimate for fertility of -.004 (s.e.=0.10) for coworkers and -.001 (s.e.=.014) for brothers.

[14]There are 242 brothers (with 233 peer fathers) and 550 coworkers (with 153 peer fathers) on average in a one-week interval.

strong evidence that the reform had large direct effects on the leave behavior of peer fathers.

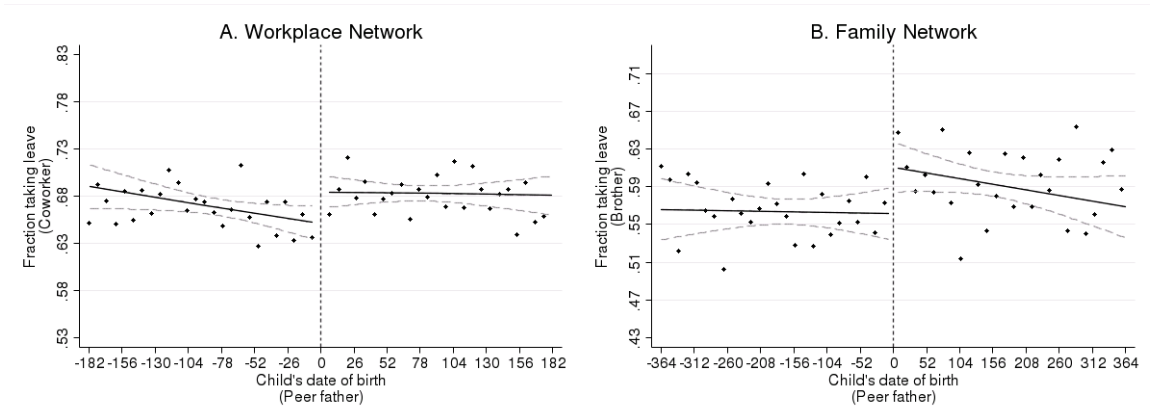


Figure 3. Coworker's and brother's leave take up.

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 (normalized to 0). Each graph sets the scale of the y-axis to $\pm.3$ *standard deviations of the respective variable.*

Figure 3 illustrates the reduced form model. 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 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.

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 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.[15]

[15]There is a negative slope as a function of the running variable, both before and after the cutoff. This negative slope reflects 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.

In our appendix, we provide further visual evidence for a sizable reduced form peer effect in both the workplace and family networks. Online Appendix Figure A.2 presents graphs similar to Figure 3, but aggregating the raw data into bigger bins. In Online Appendix Figure A.3, we present local linear regression graphs for coworker's and brother's leave take up. Both figures reveal similarly-sized jumps at the cutoff.

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. To gain precision, 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.

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

	First stage	Reduced form (ITT)	Second stage (2SLS)	N
	(1)	(2)	(3)	(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: 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. Control variables include 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. 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.*

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 sizable direct effect on paternity leave, with an increase in take-up from roughly 3% to 35%.

The second column of Table 1 reports the reduced form (ITT) estimates corresponding to Figure 3. Panel A shows that coworkers are 3.5 percentage points more likely to take paternity leave if their colleague was eligible versus ineligible for paternity leave around the reform cutoff. 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 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, 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.[16]

The peer effect estimates presented in this section represent a weighted average of the peer effects for coworkers or brothers having children at different points in time. Coworkers and brothers having children later in the sample period could experience smaller effects if the the direct effect of the peer father decays over time. But the effect could also grow over time if the original peer father's influence gets amplified with each subsequent birth in a firm. We explore these issues in more detail in Section 7.

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 Online Appendix Table A.4, 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. Results are also essentially unchanged if we do not use triangular weights. 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

[16]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.

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. Online Appendix Table A.5 varies 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. 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 Online Appendix Table A.6, we estimate local linear regressions for the workplace and family networks with bandwidths of varying size, including the optimal bandwidth indicated by Imbens and Kalyanaraman (2012). Regardless of bandwidth choice, the estimates are robust and statistically significant for both the workplace and family networks.

As a final 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 2.2 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. 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. Online Appendix Figure A.4 graphs the distribution of placebo estimates for both the workplace 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.

5.4 Peer Effects in Other Networks

Is there any evidence for peer effects in networks where ties are weaker? To answer this question, in Table 2 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; the estimated peer effect is close to zero and statistically different from the effect found for brothers in Table 1 (p-value = .09).

Table 2. Peer effects in networks with weaker ties.

	First stage (1)	Reduced form (2)	Second stage (3)	N (4)
A. Brothers-in-law				
Take up of leave	.320***	-.004	-.013	8,876
	(.017)	(.023)	(.072)	
	[.043]	[.54]	[.54]	
B. Neighbors				
Take up of leave	.274***	.002	.008	38,550
	(.012)	(.012)	(.043)	
	[.03]	[.58]	[.58]	

*Notes: Brother-in-law defined as the husband of a sister to the peer father. Neighbor defined by taking the two closest households on each side of the peer father. Specifications mirror those in Table 1. Standard errors clustered by family in panel A and by neighborhood in panel B. Comparison mean in brackets. *p<0.10, **p<0.05, ***p<0.01.*

The second panel in Table 2 defines peer groups by geographical neighborhoods. Using street addresses, we define the two closest households (prior to the reform) on each side of a father as neighbors. Similar to before, 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. The positive peer effects found for coworkers and brothers in Table 1 are statistically different from the neighbor estimates (p-value of .06 when compared to coworkers and p-value of .04 when compared to brothers). 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.

6 Mechanisms

6.1 Other Consequences of the Paternity Leave Reform

In Online Appendix Table A.7, we estimate the impact of the paternity leave reform on several outcomes of the peer fathers as well as their spouses and children. These estimates are useful in understanding not only whether there are other direct effects of the reform on the peer father and his family besides increased take up, but also as background for understanding the type of information likely to be transmitted among peers about the costs and benefits of taking paternity leave.[17]

As documented in Online Appendix Table A.7, we find 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 completed fertility, long-term marital status, or the grade point average of the child in middle school. 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 while the paternity leave quota may have encouraged fathers to stay home with their infants in the short run, it did not have other long-lasting effects. In particular, the reform did not achieve the goal of improving gender equality more broadly in the workplace.

6.2 Peer Effect Channels

There are several channels through which a peer father could affect the leave taking behavior of his coworker or brother. The first is sharing of information about how to enroll in the program. As discussed in Section 3.1, the parental leave system is universal, simple, and well-known. 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

[17] There is a substantial literature evaluating the impact of maternity and paternity leave reforms on parental and child outcomes. Our estimates are broadly consistent with Rege and Solli (2010) and Cools et al. (2011). Using difference-in-differences approaches (and a different sample), these two studies evaluate the direct effects of the Norwegian paternity leave reform.

or how to sign up. This is supported by previous research suggesting that fathers' leave taking behavior were not constrained by lack of information about eligibility, benefit amounts, and the application process (Brandth and Kvande, 1992; NOU, 1995).

The second possible channel is leisure complementarities 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 complementarities 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.[18]

The third channel is information about the costs and benefits of participation. In our setting of paternity leave, information about the consequences of taking paternity leave is initially scarce, since prior to the 1993 reform very few fathers were taking leave. At the same time, survey data collected prior to the paternity leave reform suggested many fathers were reluctant to take leave because they were worried how employers and coworkers would react (see footnote 5). 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.[19]

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 (i) a worker's boss to transmit more valuable information and (ii) workers in more uncertain work environments to react more strongly to increased information from a peer.

We first examine whether 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. Panel A of 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. For workers with high job security, the benefit of learning about a peer father's leave-taking experience should

[18]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.

[19]An alternative is that fathers tend to overestimate the costs of taking paternity leave. Other 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. Separate Regressions				
1. Predicted manager	.311***	.072**	.233**	4,272
	(.049)	(.031)	(.103)	
Not predicted manager	.316***	.028*	.088*	22,579
	(.029)	(.015)	(.047)	
2. Low unionization (≤33%)	.358***	.079***	.219***	6,834
	(.034)	(.026)	(.074)	
High unionization (>33%)	.306***	.036**	.117**	16,225
	(.028)	(.017)	(.055)	
3. Private firm	.301***	.051***	.170***	17,977
	(.027)	(.016)	(.055)	
Public firm	.377***	.032	.084	5,076
	(.041)	(.029)	(.077)	
4. 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)	
B. Combined Regression Contrasts [p-values]				
1. Manager vs. not manager		[.08]		26,851
2. Low vs. high unionization		[.36]		
3. Private vs. public		[.22]		
4. Low vs. high tenure		[.03]		

*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.*

be less valuable, as they do not need to worry as much about an employer reacting badly to paternity leave. In Table 3, we examine whether this is true. Consistent with the job security hypothesis, in panel A 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 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

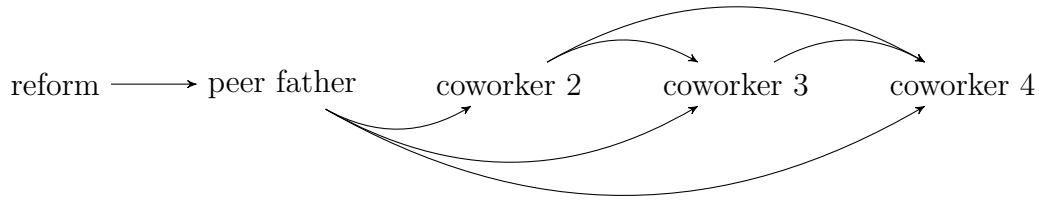
where there is more job uncertainty.[20]

Since the estimates for each row in panel A come from a separate subsample, they are often imprecise. To gain precision, we also estimate an alternative specification using a single combined regression. In this regression, the subsamples have a common first stage and common slopes for the linear trends in birth day (one common trend before and one common trend after the discontinuity). Because of perfect multicolinearity,[21] we cannot identify eight separate peer effects. But we can estimate whether the four key contrasts (manager vs. not manager, low vs. high union, private vs. public and low vs. high tenure) are statistically significant. Panel B reveals the manager vs. not manager and the low vs. high tenure firm contrasts are statistically significant while the other two contrasts are not. A joint test on all four contrasts has an F-statistic of 3.11 with an associated p-value of .01.

7 Snowball Effects

7.1 Model

Peers 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. The following diagram illustrates how peer effects could travel through a network. The direct effects of the peer father on his coworkers are captured by the bottom arrows in the diagram. The indirect, or snowball effects, are captured by any path from the peer father that travels through the top arrows in the diagram.



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

[20] In Online Appendix Table A.8, we explore additional dimensions of heterogeneity. We find larger, but noisily estimated, peer effects in smaller firms and for coworkers who start at the firm in the same year.

[21] Because manager + not manager = low union + high union = private + public = low tenure + high tenure = 1, the set of these eight groupings interacted with the reform cutoff dummy has only five degrees of freedom.

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 a coworker is amplified by a snowball effect operating through the intervening coworkers.

We model the causal chain by the following system of equations:

$$y_{1g} = \alpha + \lambda p_{1g} \tag{6}$$

$$y_{2g} = \alpha_1 + \beta_{11} y_{1g} \tag{7}$$

$$y_{3g} = \alpha_2 + \beta_{22} y_{2g} + \beta_{12} y_{1g} \tag{8}$$

$$y_{4g} = \alpha_3 + \beta_{33} y_{3g} + \beta_{23} y_{2g} + \beta_{13} y_{1g} \tag{9}$$

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. The model makes two key assumptions: sequential ordering of peers and additive separability. The assumption of sequential ordering implies that fathers who already have births influence fathers who subsequently have births, and not the other way around. Additive separability implies that, except for the ordering of births within a firm, peer effects from coworkers do not have an interactive effect.

7.2 Identification

To identify the snowball effects we additionally assume $\beta_{11} = \beta_{12} = \beta_{13} = \beta_1$ and $\beta_{22} = \beta_{23} = \beta_2$ (for consistency in notation, we also relabel β_{33} as β_3). This parameterization of six coefficients down to three implies the peer effect of the first coworker and the peer effect of the second coworker is the same for all subsequent coworkers. Importantly, however, the assumption allows for different peer effects from the first, second, and third birth in a firm. If fathers are primarily learning about how an employer will respond to paternity leave, this formulation makes sense: the first coworker to take leave is likely to reveal more novel information and therefore be a more influential peer than the second or third coworker to take leave.

With these assumptions, random variation in p_{1g} can be used to identify a set of reduced form coefficients $\pi_2 = dy_2/dp_{1g} = \beta_1\lambda$, $\pi_3 = dy_3/dp_{1g} = (\beta_2\beta_1 + \beta_1)\lambda$, and $\pi_4 = dy_4/dp_{1g} = (\beta_3\beta_2\beta_1 + \beta_3\beta_1 + \beta_2\beta_1 + \beta_1)\lambda$. 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. The percent of the total peer effect accounted for by the snowball effect for coworker j can then be calculated as $(\pi_j - \pi_2)/\pi_j$.

The model in equations (6)-(9) is related to the one-sided feedback model of peer effects in Glaeser, Sacerdote, and Scheinkman (2003). Their model also assumes sequential ordering and additive separability. Their model differs in that they reduce the set of six β coefficients to two parameters; their model assumes $\beta_{11} = \beta_{22} = \beta_{33} = \gamma$, $\beta_{12} = \beta_{23} = \gamma\delta$, and $\beta_{13} = \gamma\delta^2$. While this parameterization is useful for some applications, it is less suited to our setting as it would imply the most recent birth in a firm has the largest peer effect. In part because of the differing parameterizations, our concept of the snowball effect is related, but not identical, to Glaeser et al.'s social multiplier.

So far, our model implicitly assumes the direct and indirect peer effects are independent of when the coworker's child is born in time. In reality, the influence of a peer is likely to decay over time, with smaller peer effects for coworkers having children temporally distant from one another. To adjust for this decay, we exploit the fact that coworker 2 does not experience a snowball effect as 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 assume the decay between any two coworkers due to birth spacing is the same as the decay identified between coworker 2 and coworker 1. Decay adjusted estimates for coworker j can then be calculated by dividing π_j by $1 + r_j$, where r_j is the percent decay for a given birth spacing.

7.3 Empirical Results

Table 4 displays the decay adjusted reduced form peer effects $(\pi_j/(1 + r_j))$ 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 the decay adjusted reduced form estimates to decompose the total peer effect into the direct effect and the snowball effect, and graph the relative importance of these effects over time.

The first column presents coworker estimates which do not account for decay (i.e., r_j=0 for all coworker groups). This regression mirrors our baseline specification, but allows for a separate discontinuity for each coworker. Even without subtracting

Table 4. Snowball effects on coworkers within a firm.

	Reduced form (1)	Percent snowball (2)	Reduced form (3)	Percent snowball (4)	Reduced form (5)	Percent snowball (6)
	A. No decay		**B. Cubic decay**		**C. Quintic decay**	
Coworker 2	.033**	0%	.026**	0%	.024**	0%
$\pi_2/(1+r_2)$	(.017)		(.013)		(.012)	
Coworker 3	.039**	15 %	.037**	30%	.034**	29%
$\pi_3/(1+r_3)$	(.017)		(.017)		(.015)	
Coworker 4	.049***	33 %	.060***	57%	.055***	56%
$\pi_4/(1+r_4)$	(.018)		(.022)		(.020)	
Coworker 5+	.025	-32 %	.083	69%	.077	69%
$\pi_5/(1+r_5)$	(.016)		(.053)		(.049)	
Snowball F-test	1.01		2.84		2.84	
p-value	[.387]		[.036]		[.037]	

Notes: Sample includes all coworkers having a child before 2002. The reduced form peer effects are adjusted for decay as indicated in the specification headings. Snowball columns indicate the amount of the peer effect that can be attributed to the snowball effect. The F-test for no snowball effects is a joint test of $\pi_5/(1+r_5) = \pi_4/(1+r_4) = \pi_3/(1+r_3) = \pi_2/(1+r_2)$. *N = 22,922. *p<0.10, **p<0.05, ***p<0.01.*

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 reduced form peer effect for this later group declines, which is not surprising if decay is sizable.

Details on how decay is estimated are found in Online Appendix B. Here we merely report, for each order coworker j, the average decay rate, r_j, and average birth spacing relative to the original peer father, s_j.[22] There is appreciation for coworker groups 2 and 3 (r_2=.27, s_2=2.6 years; r_3=.04, s_3=3.6 years), and depreciation for higher order coworkers in a firm (r_4=-.18, s_4=4.3 years; and r_5=-.71, s_5=5.6 years). This pattern makes sense once one realizes that 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. 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

[22] A possible concern is that birth spacing between a coworker and the peer father is directly affected by the reform. In Online Appendix Figure A.5, we show this is unlikely to bias the estimates.

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.[23]

Our key results are reported in columns 3 and 4 of Table 4, which report decay-adjusted estimates allowing for cubic depreciation. The decay adjusted reduced form coefficient for coworker group 2 is estimated to be .026. 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 group 3, the decay adjusted reduced form peer effect is .037, with the snowball effect accounting for 30% of the total peer effect. Since there are more intervening coworkers, the snowball effect is even larger for coworker group 4 (57% of the total peer effect) and coworker group 5+ (69% of the total peer effect), although the effect for the latter group is imprecisely estimated. As the table documents, these decay-adjusted snowball effects are jointly statistically significant.

As a robustness check, we also tried modeling decay as a fourth order polynomial. As seen in columns 5 and 6, this makes little difference to the estimates. Since the decay function is estimated using the relatively small sample of coworker 2's, we perform a sensitivity analysis instead of directly accounting for the sampling variability of the decay estimates in our snowball tests. Specifically, we explore the sensitivity of the joint snowball tests to differing amounts of depreciation. In Online Appendix B, we plot what the decay function would look like if it were three-fourths or half as large as the one we actually use. Using these flattened decay functions still results in joint significance for the snowball effects; the three-quarters decay function yields a p-value of .051 and the halved decay function yields a p-value of .104. Steeper decay functions than the one we actually use would increase statistical significance.

7.4 Importance of Snowball Effects

Figure 4 illustrates the relative importance of the direct peer effect and the snowball effect over time. 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 reduced form peer effect from the total leave take up.[24] The difference

[23]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.

[24]To construct the bottom line, we use the estimated π_j's (the reduced form estimates not adjusted for decay) to predict the size of the peer effect originating from the peer father (coworker

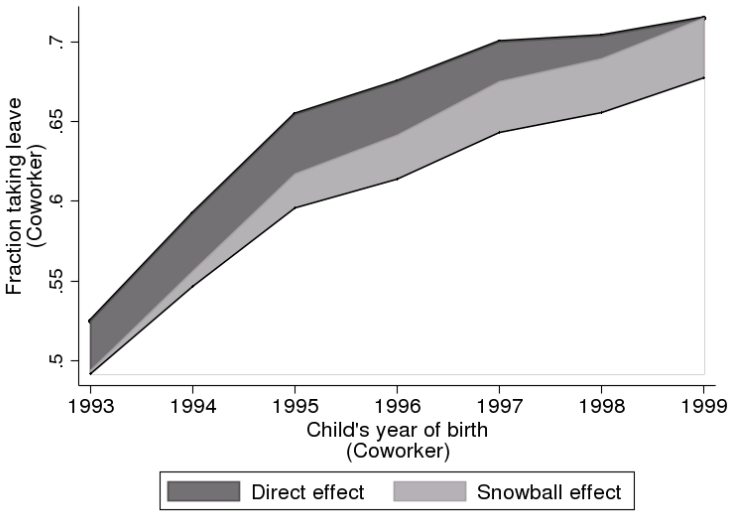


Figure 4. Direct peer and snowball effects over time.

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.

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. This counterfactual gap, which includes depreciation, is sizable and actually gets slightly larger over time.

The shading in Figure 4 decomposes this counterfactual gap into the direct effect of the peer father on coworkers (dark grey) and the indirect snowball effect (light grey) over time. 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 decays. In contrast, the snowball effect grows and starts to dominate over time as more coworkers have a child within a given firm.

Figure 4 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

1), accounting for the mix of births in each year (coworker 2, 3, 4, and 5+ births). We only plot the period from 1993 to 1999; extrapolating past 1999 is noisy and implies depreciation in excess of 100%. Most coworkers in our dataset have their children before 1999.

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.

8 Conclusion

We find strong evidence for substantial peer effects in 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 sizable 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. 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 peer effect estimates point out that both the workplace and family can serve as important networks in settings where information is scarce and perceptions are in their formative stage. Peer effects may be of particular importance in the ongoing debate about policies aimed at promoting gender equality, ranging from family policy to affirmative action programs. Although the snowball analysis requires stronger assumptions, it highlights that peer influences 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 take-up patterns. Social interactions can reinforce the direct effects 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.

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Online Appendices

Online Appendix A: Tables and Figures

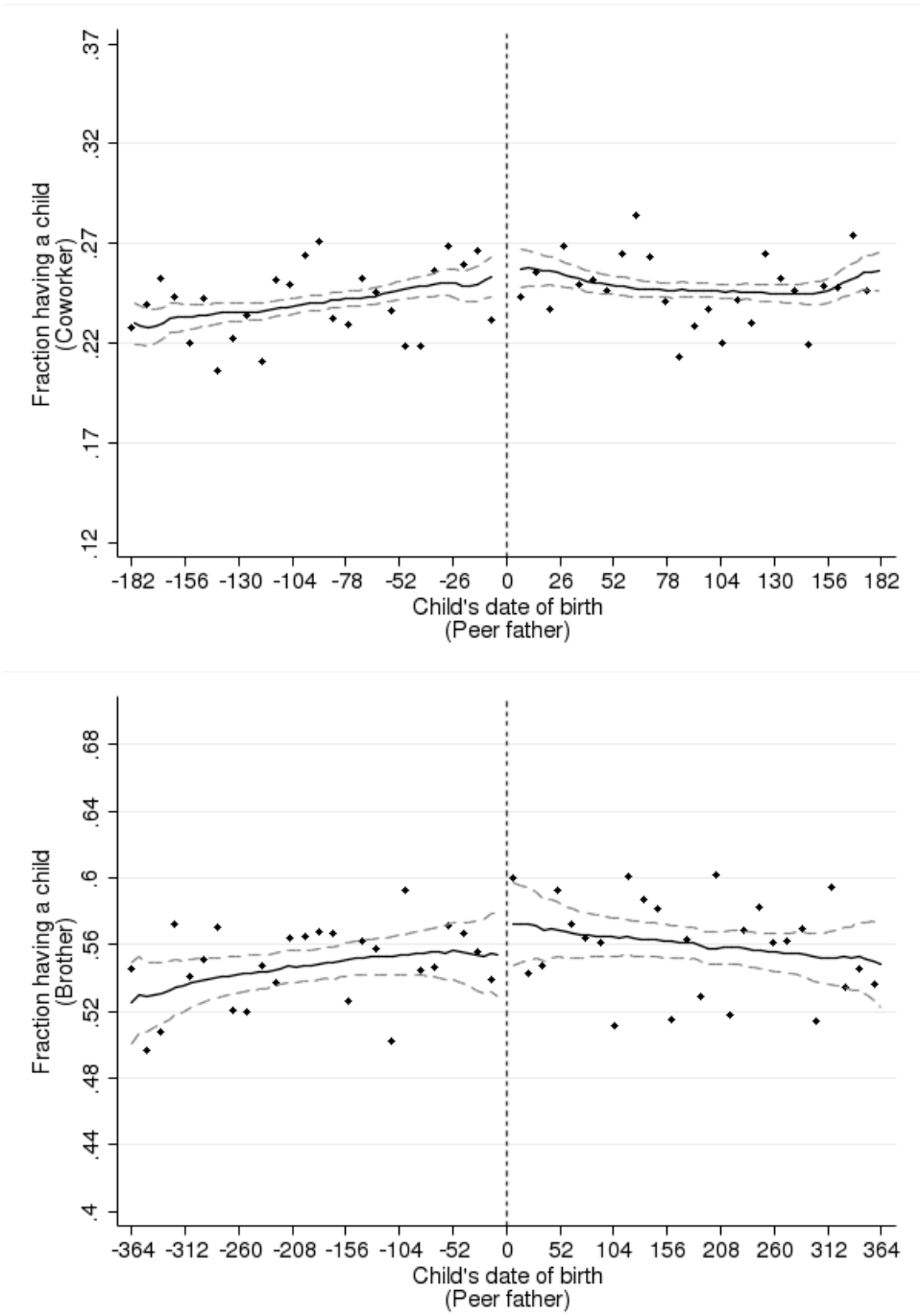

Figure A.1. Coworker's and brother's fertility.

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.

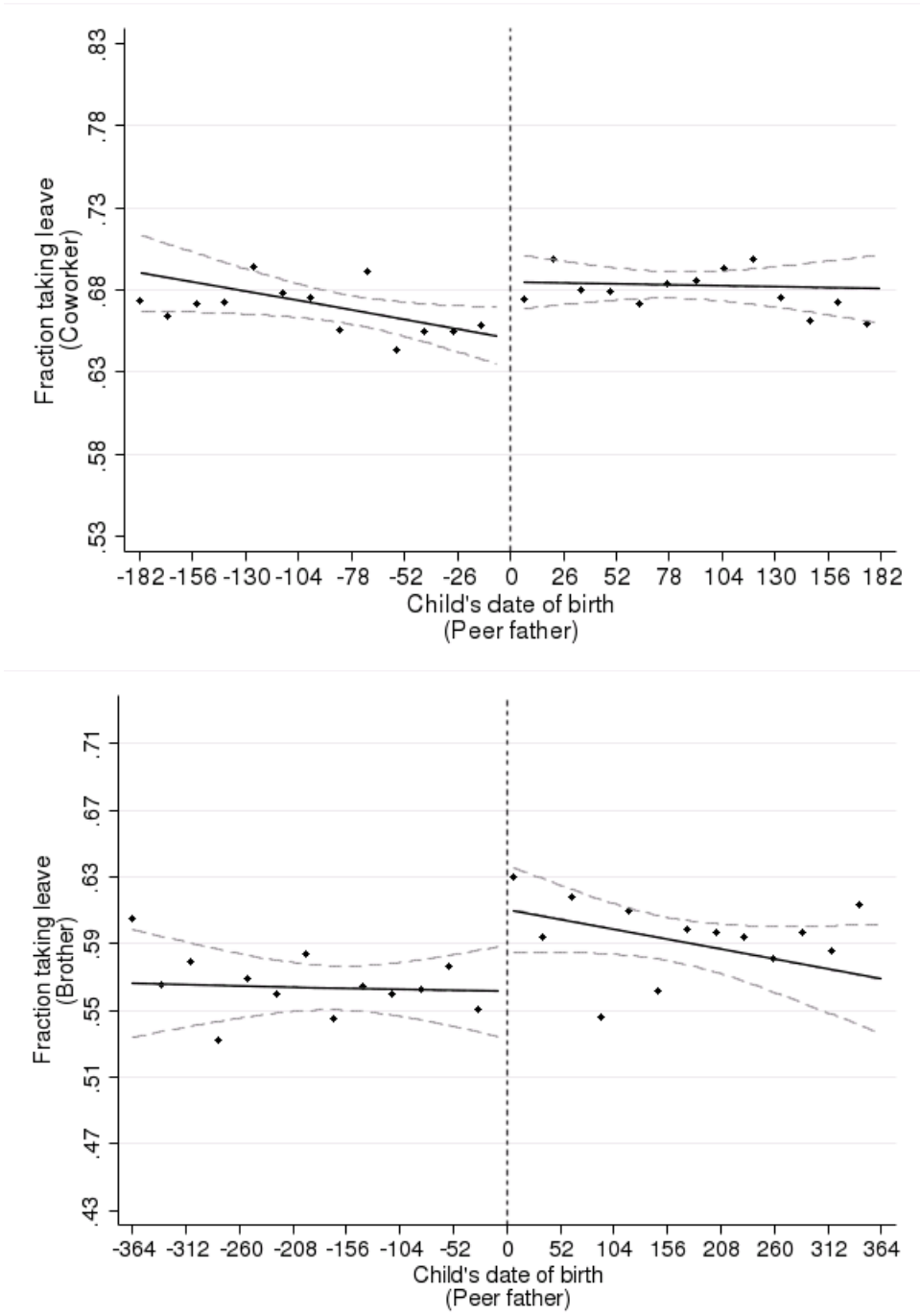

Figure A.2. Coworker's and brother's leave take up using wider bins.

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.

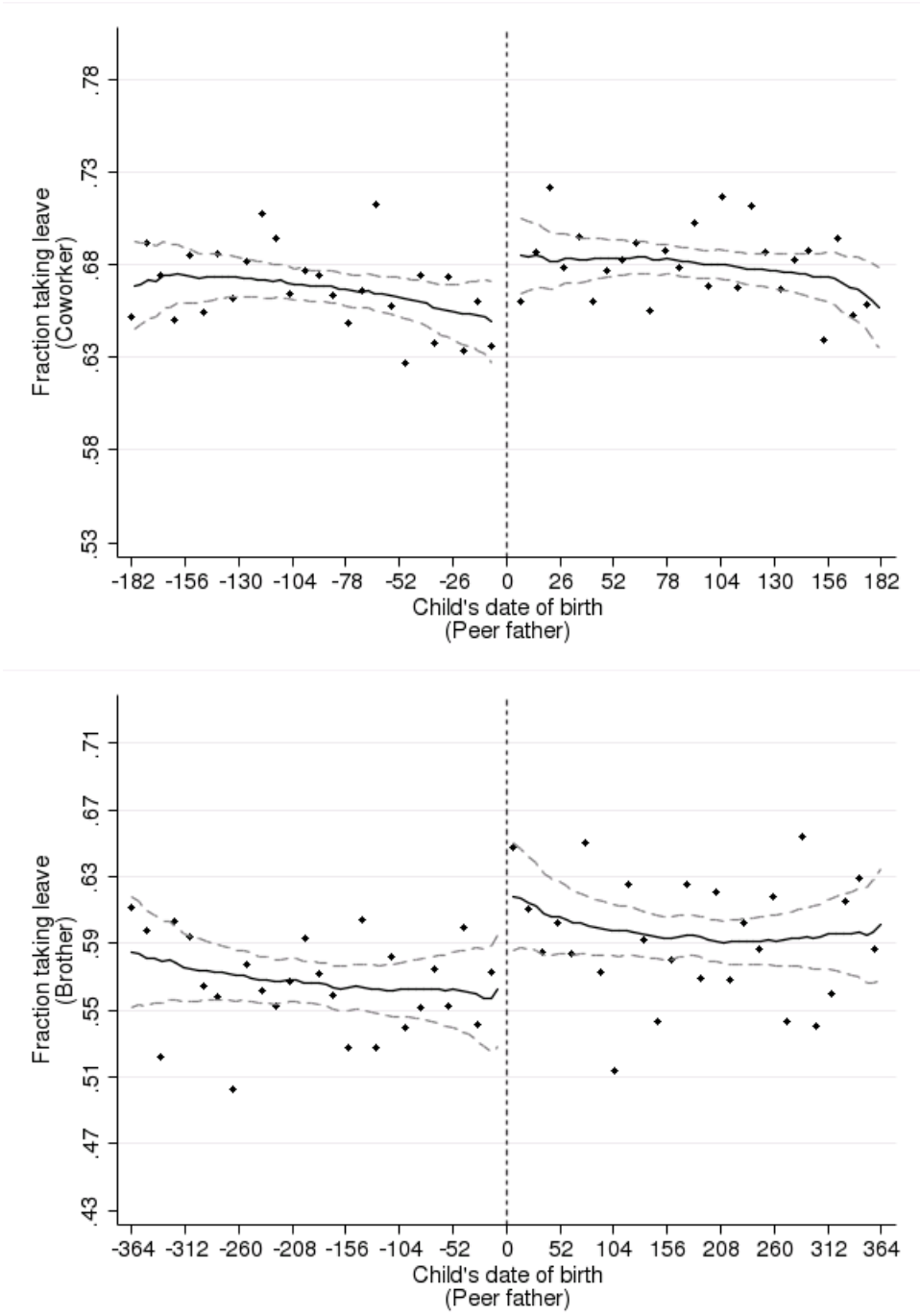

Figure A.3. Local linear regression graphs for coworker's and brother's leave.

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 A.6.

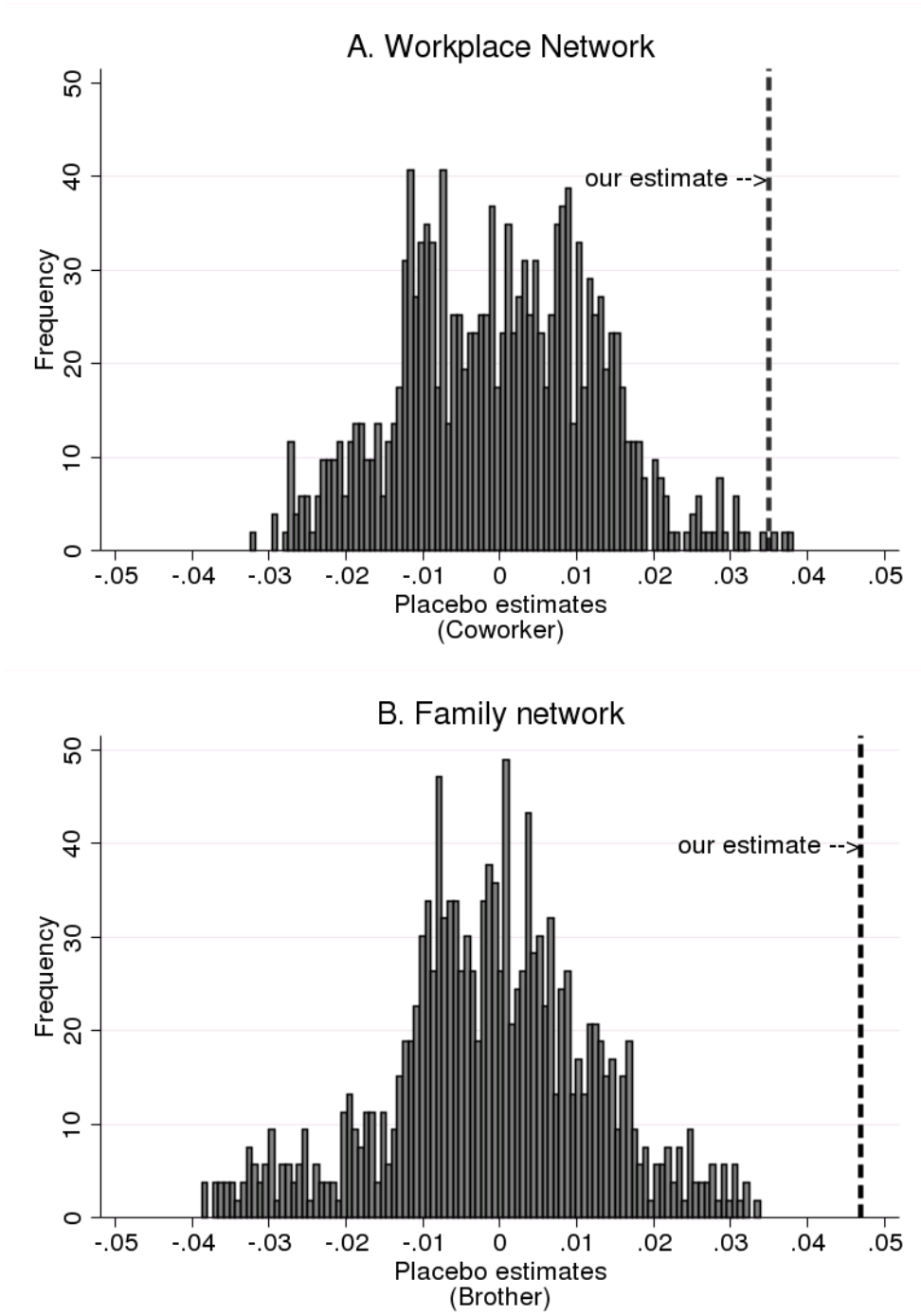

Figure A.4. Placebo estimates of the peer effect.

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. Note the placebo estimates are not independent of each other, as the samples overlap.

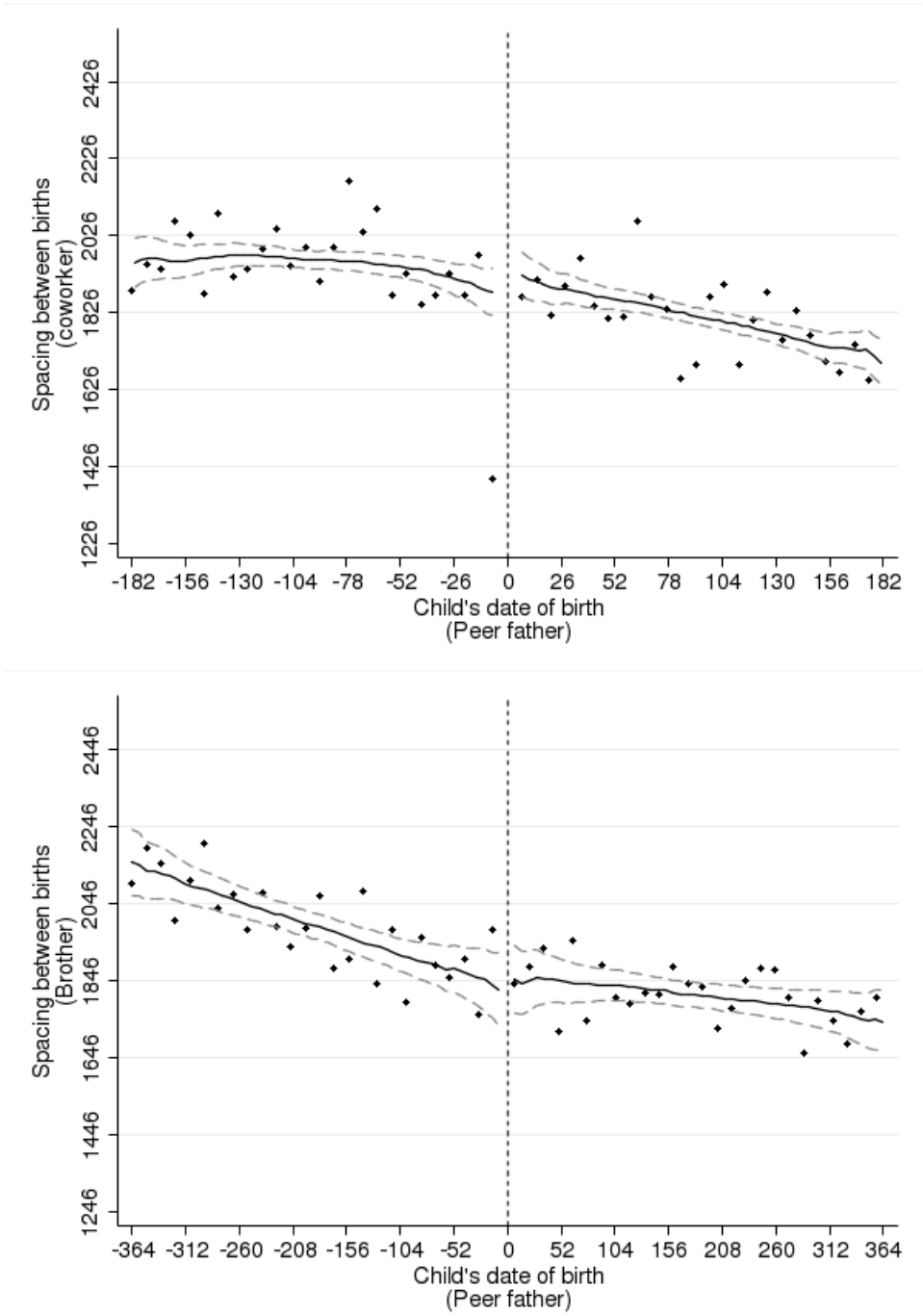

Figure A.5. Spacing between the coworker's/brother's and the peer father's births.

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 A.1. 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 A.2. 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: This table tests for strategic timing of birth by regressing the birthdate of the child on dummies for one week intervals before and after the reform date of April 1, 1993. Control variables include day of week, month, and year dummies, as well as 365 day of year dummies. To increase precision, for this regression we use all births to fathers eligible for any type of parental leave in Norway between 1992 and 2006, and not just those in the family or workplace networks. 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 A.3. 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 discontinuity, 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 A.4. Specification checks for coworker and brother peer effects.

	First stage (1)	Reduced form (2)	Second stage (3)	N (4)
A. Workplace network				
Baseline	.317***	.034**	.109**	26,851
	(.026)	(.013)	(.043)	
No Controls	.318***	.034**	.106***	26,851
	(.024)	(.012)	(.040)	
No triangular weights	.313***	.033**	.105***	26,851
	(.024)	(.013)	(.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)	
Ineligibles 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)	
No triangular weights	.301***	.043**	.143***	12,495
	(.013)	(.018)	(.060)	
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)	
Ineligibles 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 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.

Table A.5. 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 A.6. Local linear regression estimates for coworker and brother peer effects.

Bandwidth	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)	
159 days	.316***	.032**	.101**	23,596
	(.026)	(.014)	(.045)	
		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)	
341 days	.303***	.046**	.152**	11,487
	(.014)	(.019)	(.063)	

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 bandwidth. The optimal bandwidths based on Imbens and Kalyanaraman (2012) are used in the last row of each panel. 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 A.7. Regression discontinuity estimates for direct effects of the reform on other outcomes.

	Total Years Employed (max=12)			Total Earnings (12 year annuity)			GPA of	Married	# kids
	Father	Mother	Ratio F/M	Father	Mother	Ratio F/M	child	(in 2006)	(in 2006)
	(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 discontinuity, 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.

Table A.8. Additional Workplace Estimates.

Workplace Characteristic	First stage (1)	Reduced form (2)	Second stage (3)	N (4)
1. Coworkers with start dates	.235***	.059**	.251**	6,841
within one year of each other	(.032)	(.027)	(.120)	
Coworkers with start dates	.340***	.024	.071	20,010
more than one year apart	(.029)	(.015)	(.045)	
2. Firm size $<$ 30	.313***	.042**	.135**	14,301
	(.026)	(.018)	(.059)	
Firm size $\geq$ 30	.319***	.021	.065	12,550
	(.047)	(.020)	(.061)	

*Notes: Specifications mirror those in Table 1. Sample size can vary across subgroups due to missing values. Standard errors clustered by firm. *p<0.10, **p<0.05, ***p<0.01.*

Online Appendix B: Decay Estimation

This appendix describes how the decay-adjusted estimates in Section 7 are calculated. As explained in the paper, we exploit the fact that coworker 2 does not experience a snowball effect as 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 first run a preliminary RD regression using the subsample of coworker 2 observations. Specifically, we take the subsample of coworker 2 observations and augment equation (5) to include a polynomial in the spacing between the birth date of the peer father's and coworker 2's child, s, and an interaction term between these polynomial terms and the reform cutoff. Using a third order polynomial for spacing,

$$\begin{aligned} y_{2g} = \quad & \gamma_2 + 1[t \geq c](h_l(t-c) + \pi) + 1[t < c]h_r(c-t) \\ & + \psi_1 s + \psi_2 s^2 + \psi_3 s^3 + \delta_1 1[t \geq c]s + \delta_2 1[t \geq c]s^2 + \delta_3 1[t \geq c]s^3 + u_{2g} \end{aligned}$$

Estimation of this regression mirrors the baseline RD specification in Table 1. The estimated interaction coefficients are $\hat{\delta}_1$=.023 (s.e.=.054), $\hat{\delta}_2$=-.008 (s.e.=.014), and $\hat{\delta}_3$=.0004 (s.e.=.0010), with a joint F-statistic of 2.73 (p-value=.064).

The top graph in Online Appendix Figure B.1 plots the depreciation rate over time based on these estimated interaction terms. The graph plots depreciation for cubic and quartic polynomials. Focusing on the cubic specification, the peer effect appreciates for the first 1.8 years before starting to decline again, with the depreciation term not becoming negative until approximately 3.8 years. As explained in the paper, this pattern makes sense once one realizes when fathers take leave from their firm. The bottom graph in Online Appendix Figure B.1 plots the histogram which shows the distribution of coworker 2 observations based on the spacing between the peer father's child and coworker 2's child. Online Appendix Figure B.2 plots what the decay function would look like if it were three-fourths or half as large as the one we actually use;, these are used for the sensitivity analysis reported in the paper.

The second step is to calculate the average spacing between the birth date of the peer father's and each order coworker's child. These are labeled as s_2, s_3, s_4 and s_5.

The third step is to calculate the decay rates as $r_j = \frac{\hat{\delta_1}}{\hat{\pi}} s_j + \frac{\hat{\delta_2}}{\hat{\pi}} s_j^2 + \frac{\hat{\delta_3}}{\hat{\pi}} s_j^3$. Note the estimated coefficients from the preliminary RD regression based on the coworker 2 sample are used to identify the decay rates for all order coworkers, and not just coworker 2.

The four symbols in Online Appendix Figure B.1 plot the average spacing, s_j, and depreciation rates, r_j, for each of the j coworker groups. The values for these four coordinates are reported in the main text; the average depreciation rates are

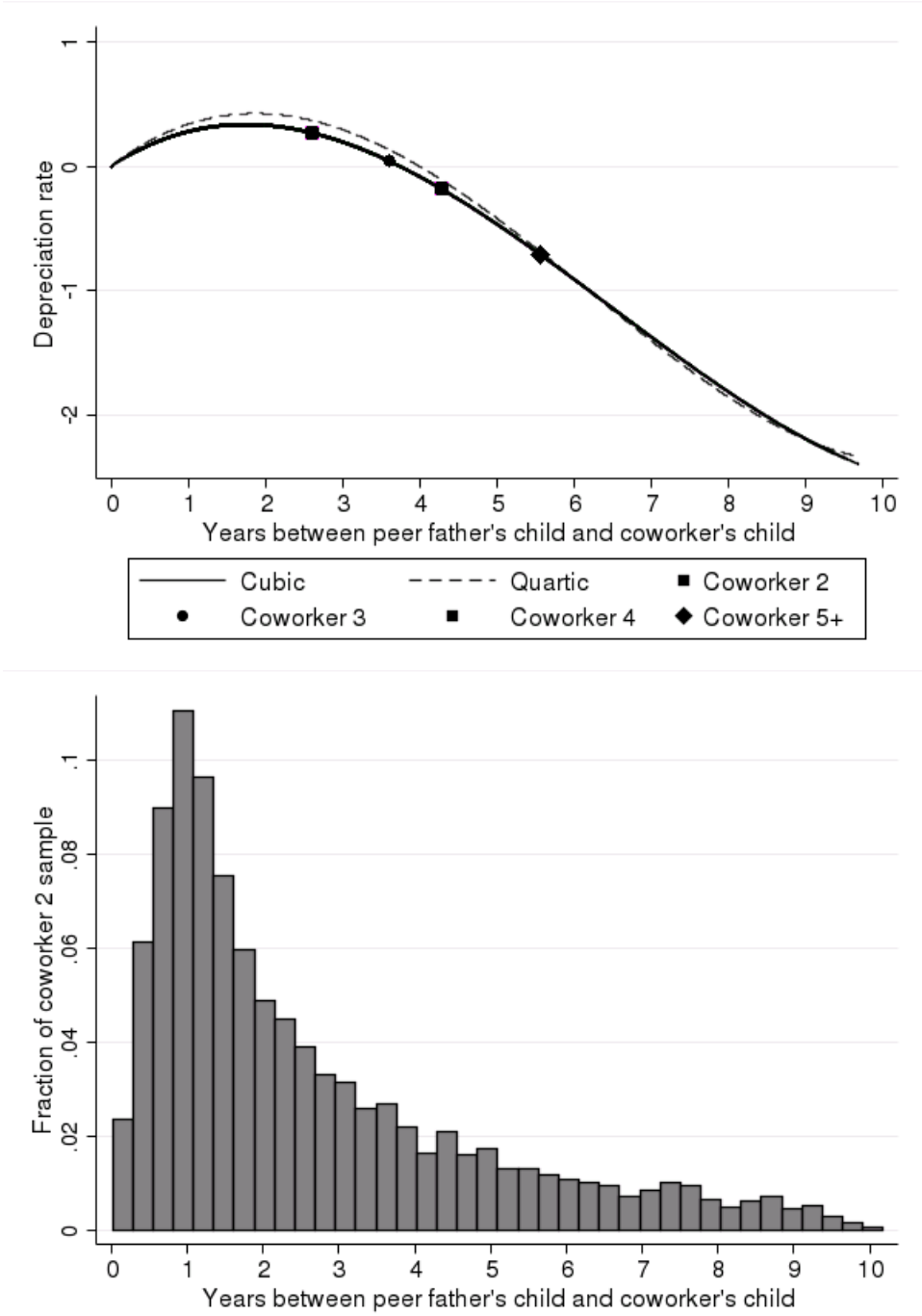

Figure B.1. Decay in the estimated peer effect over time.

Notes: The top graph shows estimated decay for different order polynomials based on the subsample of coworker 2 observations (i.e., the first coworker to have a birth after the peer father). The four symbols plot the average spacing and implied depreciation rates for each coworker group. The bottom graph shows the distribution of coworker 2 observations based on the spacing between the peer father's child and coworker 2's child.

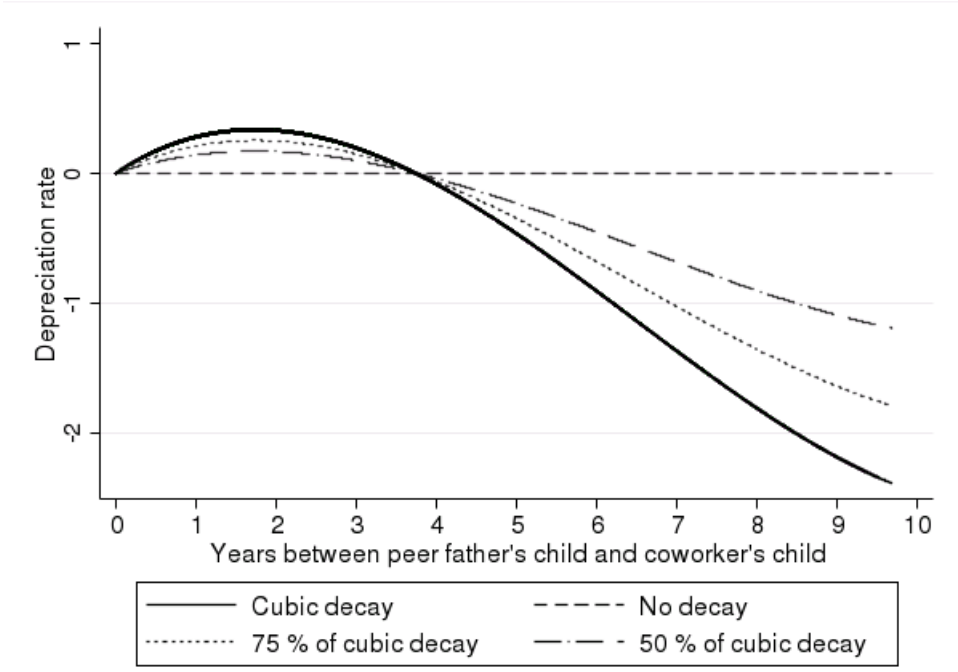

Figure B.2. Flatter decay functions for sensitivity analysis.

used to calculate the decay adjusted reduced form estimates for each coworker group. While estimated depreciation exceeds 100% after about 6.3 years in the figure, it is important to remember that this is the region of the data where we do not have many observations in our coworker 2 sample (see bottom panel of Online Appendix Figure B.1). Moreover, none of the estimated depreciation rates used in the analysis are in this region.

The decay-adjusted snowball estimates are then calculated by dividing the reduced form peer effect estimates in column 1 of Table 4 by $1 + r_j$ for each group j.