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Parenthood and the Gender Gap in Workplace Authority

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Abstract

This article answers several related questions: does parenthood affect whether women hold positions of authority? Is there a parenthood effect on authority for men? Is the gender gap in authority explained by a more deleterious effect of parenthood on women's in comparison to men's representation in positions of authority? Past studies of the relationship between parenthood and workplace authority have been limited in their ability to assess a causal effect of parenthood because most have employed a static approach, measuring the presence of children and the type of job held concurrently, using cross-sectional data. Using retrospective life course data from four rounds of the Family Survey of the Dutch Population and distributed fixed-effects models, we study within-person changes in having supervisory authority among women and men in the years before, around, and after the birth of their first child. The findings show a moderate negative effect of motherhood on women's representation in authority, which is entirely explained by a reduction in the number of hours worked. Fatherhood has no effect on men's representation in authority. The gender gap in supervisory authority between women and men grows over time but is already very large years before the transition to first-time parenthood.

Introduction

Parenthood has received substantial attention in studies of the gender gap in earnings, but its influence on women's and men's representation in workplace authority remains little understood. The issue is important for at least two reasons. First, theoretically, parenthood is seen as pertinent to women's underrepresentation in workplace authority, given strong cultural ideas about motherhood and fatherhood that shape employer discrimination and couples' gendered responses to parenthood. Second, scholars have pointed to the potential relevance of parenthood effects on authority for explaining parenthood effects on earnings, although extant evidence suggests that representation in authority plays only a small role in the pay gaps between men and women, and within women, between mothers and other women (Budig and England, 2001; Magnusson and Nermo, 2017). The idea that motherhood is *the main* explanation of women's under-representation in positions of authority is strongly endorsed in popular opinion (Belkin, 2003; Ely, Stone and Ammerman, 2014).

The vast majority of analyses of the relationship between parenthood and workplace authority have used cross-sectional data. When limited to cross-sectional data, authors typically examine regression-adjusted differences in the authority status of parents and those without children, entering controls for human capital dimensions such as education or experience. This approach is prone to selection bias, for parents may differ from childfree individuals on characteristics that predict aspiring to or being qualified for positions entailing authority. Beyond issues of selection, a dynamic life course, rather than a static approach to the study of the relationship between parenthood and workplace authority is preferable because the effects of parenthood may unfold over the life course and linger on long after the transition to parenthood.

We study the effects of the transition to parenthood on women's and men's representation in workplace authority using life course data from the Family Survey of the Dutch Population (1998–2009). Rather than comparing parents and childfree individuals, we model within-person changes in having workplace authority before, around, and after the birth of the first child in a sample of women and men who have transitioned to parenthood. Our analysis of years of data surrounding the birth of men's and women's first child allows us to assess the causal effect of parenthood with more confidence and examine how any gender gap in authority changes across the years before and after the birth of a first child. Bygren and Gähler (2012) is to our knowledge the only other study to examine the effects of the transition to parenthood on the attainment of workplace authority with longitudinal data. Using individual fixed-effects models in a panel of Swedish women and men (1968–2000), the authors find that parenthood has no direct effect on women's chances of having supervisory authority. Men, on the other hand, receive an authority premium when they become fathers.

Our study adds to the scant literature on the role of parenthood in women's and men's representation in workplace authority using data from the Netherlands. Our approach differs from Bygren and Gähler (2012) in using distributed fixed-effects models to predict women's and men's probabilities of having workplace authority both before and after the transition to parenthood. To study parenthood effects, Bygren and Gähler (2012) employ three dummies for children's age (0–6 years, 7–20 years, and 20+ years). Like Bygren and Gähler (2012), we use person-years as units of analysis, but our analysis differs from theirs in that our predictor of interest is a variable indicating the number of years before and after the transition to first-time parenthood, with a maximum of 7 years *before* and 15 years *after* the transition to parenthood, rendering the model a *distributed* fixed-effect model. Mapping the period preceding *and* following the transition to parenthood in a detailed manner aids causal inference and allows us to observe when exactly potential parenthood effects start taking place.

Background

The transition to parenthood can affect individuals' attainment of workplace authority as a function of employer discrimination, gendered responses to parenthood, or both. One reason employers may discriminate against women in demanding jobs entailing authority is because they see them as less competent and status worthy. Theories of status posit widespread beliefs about the competence of different 'types' of people that result in a hierarchical ordering of individuals (Berger, Cohen and Zelditch, 1972; Ridgeway, 2001, 2014). Gender and parental status are two social positions along which such status ranking occurs.

Gendered evaluations of parenthood in the work context are in line with the 'ideal worker' norm, which defines the ideal worker as one whose work takes precedence over family life (Benschop and Doorewaard, 1998; Davies and Frink, 2014). Cultural ideas about fatherhood are not at odds with ideal worker narratives because the view on good fatherhood involves being the primary breadwinner for the family. This strong attachment to the workplace expected of fathers is incompatible with cultural ideas of motherhood that regard women as bearing the primary responsibility for child rearing. In other words, being a good mother does not permit strong involvement in the workplace the way of being a good father does. Motherhood ideals may be at tension with ideal worker norms especially when it comes to positions of workplace authority, given that expectations of strong emotional attachment to the workplace and total work devotion are particularly embedded in high-status positions such as managerial and professional jobs (Kanter, 1977; Blair-Loy, 2005; Cha and Weeden, 2014).

A large body of experimental research has shown that employers view working mothers as less competent and committed than childfree women, whereas competence evaluations of men do not change or even improve when they become fathers (e.g. Correll, Benard and Paik, 2007 [U.S.]). In the Netherlands, Mari and Luijkx (2020) recently find that employers expect women to be less committed to their jobs than childfree women, while fathers and childfree men are evaluated as equally committed. Although not all studies find a gendered evaluation of parenthood status (e.g. Bygren, Erlandsson and Gähler, 2017 [Sweden]), the most common finding in other contexts too is that mothers are evaluated as less competent and committed, and fathers are not evaluated differently from childfree men, with significant consequences for hiring and promotion (Cuddy, Fiske and Glick, 2004 [U.S.]; Fuegen *et al.*, 2004 [U.S.]; Hipp, 2019 [Germany]).

So far, we have described employers' beliefs about gender and parenthood without attention to whether they, at least on average, reflect large supply-side differences in how mothers and fathers respond to parenthood for cultural reasons, or whether employers hold inaccurate perceptions that fabricate or exaggerate supply-side gender differences in the response to parenthood. Notions of statistical discrimination assume the former, while status-based theories of gender discrimination assume the latter (on the distinction between the two, see Correll and Benard, 2006). We cannot adjudicate between these as explanations of gender differences in effects of parenthood on authority, but we are able to assess how much of the effect of parenthood is explained by a change in hours worked after a birth. Of course, such a change might itself reflect (anticipated) discrimination based on birth, but it is also likely to be a supply-side response based on cultural notions of who is responsible for breadwinning versus child care.

The 'ideology of intensive mothering' (Hays, 1998), commonplace in the Netherlands (Van Engen *et al.*, 2009), has generally led individuals to prioritize men's careers and women's role in child rearing. This ideology has two parts—first a belief that children need very intensive parental attention to flourish, and second that mothers are more appropriate than fathers for providing this. The Dutch institutional setting reflects and simultaneously sustains this ideology. Dutch maternity leave law has changed only slightly over the decades, from 12 weeks in 1930 when it first came into force, to 16 weeks as of 1990. Paternity leave has been virtually non-existent, with 2 days of paid leave for fathers as of 2001. Unpaid parental leave of 13 times the weekly work hours was introduced in 1991 and expanded to 26 times the weekly hours in 2009. While available to both mothers and fathers upon fulfilment of eligibility criteria, mothers make use of the unpaid parental leave to a much greater extent than fathers: 42 per cent of mothers in 2003 and 57 per cent in 2013 took unpaid leave, as opposed to 15 and 23 per cent respectively among eligible fathers (Merens and van den Brakel, 2014).

Minimal job-protected leave policies in the Netherlands—among the least generous in Europe (Budig, Misra and Boeckmann, 2016)—have historically been coupled with low availability of formal childcare facilities for preschool children and low labour market participation among women. The increasing availability of part-time jobs in the 1980s, followed by a move toward legal equality of full- and part-time workers and law on co-financing childcare by parents, employers, and the state in the 1990s enabled women to enter the labour market in large numbers. These changes resulted in the current dominant one-and-a-half earner model (Merens and van den Brakel, 2014). Women tend to work part-time more often than men and while about 35–40 per cent of Dutch preschool children are enrolled in formal day care, the share of those in day care for 30 hours or more per week is among the lowest in Europe: 6 per cent as opposed to the European average of 17 per cent over the last decade (Eurostat, 2020).[1]

After first childbirth, women in the Netherlands work fewer hours, whereas men's work hours are not substantively susceptible to the transition to parenthood (Begall and Grunow, 2015; Fouarge and Baaijens, 2004). These patterns are relevant because cultures of long work hours are particularly strong in managerial and professional jobs and at the top of occupational hierarchies (Cha and Weeden, 2014; Goldin, 2014). Thus, in our analyses, we assess how parenthood affects changes in women's and men's representation in workplace authority with and without controls for hours worked, bearing in mind that we cannot be sure whether hours are endogenous or exogenous to whether one is in an authority position.

While past research on parenthood and authority is limited, based on the broader literature on gender and work, we expect a parenthood penalty in authority for women, and no effect or a premium among men. Some of the available cross-sectional evidence[2] is in line with these expectations, finding a negative association of parenthood with authority among women (Ishida, 1995 [Japan and Britain]; Hultin, 1998 [Sweden]; Rosenfeld, Van Buren and Kalleberg, 1998 [9 countries]; Maume, 2004 [U.S.]; Blommaert *et al.*, 2019 [The Netherlands]), and positive association among men (Wolf and Fligstein, 1979 [U.S.]; Hopcroft, 1996 [U.S.; only for supervisory authority, no effect for decision-making]; Rosenfeld, Van Buren and Kalleberg, 1998 [U.S.]). Other studies find no association between parenthood and authority for women (Wolf and Fligstein, 1979 [U.S.]; Bridges and Miller, 1981 [U.S.]; Jaffee, 1989 [U.S.]; Adler, 1994 [U.S.]; Wright, Baxter and Birkelund, 1995 [7 countries]; Hopcroft, 1996 [U.S.]; Maume, 1999 [U.S.]; Mitra, 2003 [U.S.]; Huffman and Cohen, 2004 [U.S.]) and men (Bridges and Miller, 1981; Jaffee, 1989; Adler, 1994; Ishida, 1995; Wright, Baxter and Birkelund, 1995; Hultin, 1998; Maume, 1999; Mitra, 2003; Huffman and Cohen, 2004).

A possible explanation for the absence of association between parenthood and authority is the common use of the presence of (young) children *in respondents' household* as a measure of having children (Bridges and Miller, 1981; Jaffee, 1989; Adler, 1994; Wright, Baxter and Birkelund, 1995; Maume, 1999; Huffman and Cohen, 2004). The initial intention in using this measure in many of these studies is to proxy individuals' household commitments (due to parenting responsibilities) more so than parenthood itself. Much of the literature however refers to these studies' findings as evidence for the absence of association between parenthood and authority. This is an erroneous inference as the comparison group here are not only childfree individuals but also parents whose (older) children have left the parental home. Comparing individuals whose children live at home to childfree individuals *and* individuals with children who have left the parental home, as done in these studies, may result in an underestimation of parenthood effects if these effects are long lasting, having an impact even well after children have left the parental home.

Using retrospective occupational career course data from the Family Survey of the Dutch Population and individual fixed-effects models, we are able to overcome some selection issues stemming from parents and childfree individuals potentially differing on relevant authority-associated characteristics, as well as limitations related to ambiguous comparison categories commonly employed in cross-sectional studies. Our distributed fixed-effects approach allows us to pinpoint the timing of effects of parenthood by considering the period before the transition to parenthood and not averaging effects over all years before or after the first birth. What happens before the transition to parenthood also affects conclusions regarding the causal effect of the transition to parenthood. A pre-transition trend that continues into the post-transition trend in the same way is ambiguous evidence of a causal effect of parenthood; indeed, the less change there is in the trajectory (as opposed to the level) with birth, the more the data suggest the conclusion that a process other than parenthood drives both the pre-transition and post-transition trend. By contrast, a clear change in trajectory or level right at the birth or even a bit before or after it suggests direct effects of parenthood. The models we use allow us to observe such potential scenarios.

Methods

Data and Sample

We use retrospective occupational career course data from four rounds (1998, 2000, 2003, and 2009) of the Family Survey of the Dutch Population (hereafter FSDP)[3]. The FSDP is a large-scale life course survey of the Dutch-speaking population of the Netherlands aged 18–70, with an oversample of the married and cohabiting population. Complete life and occupational career courses of respondents are documented retrospectively[4] through structured face-to-face interviews and self-completion questionnaires (De Graaf *et al.*, 1998, 2000, 2003; Kraaykamp, Wolbers and Ruiter, 2009). Other studies have used these data to study labour market exits and reduction in work hours around first childbirth (Begall and Grunow, 2015) and other outcomes such as early retirement (Visser *et al.*, 2016).

For each year after respondents left full-time education for the first time for at least three months (defined as the start of their career), we know whether they were employed and when their job started and (if applicable) ended. Only periods in which the main activity of the respondent was working count as jobs. This means that periods of education combined with a part-time job are classified as intermediate periods and are missing from the analysis. Additional to this, we exclude periods of unemployment (not having a job but looking for one), incapacity for work due to illness, being out of employment to do homemaking and child care, self-employment, and working in a family business. The authority status and number of hours worked are known for each job. Within-job changes in authority status and hours worked are also documented.

Given that we aim to identify authority effects of transitions to parenthood, we select individuals who have transitioned to first-time parenthood during the observed period. The four harmonized rounds of the FSDP contain person-year[5] observations from 6,573 ever-parents (50 per cent women) who had at least one job that was not self-employment or working in a family business. We made the following sample restrictions. Since we use fixed-effects models, which rely on within-person changes, we exclude 270 respondents who did not have at least two non-missing years in which they were employed for which we know their authority status. We also exclude 182 individuals with missing or inconsistent information on any of the dates (e.g. if the respondent's birth date is too close to the start of their career), 135 individuals who transitioned to first-time parenthood before they left full-time education, and 4 individuals who transitioned to parenthood before they turned 15 or after they turned 50. Altogether, using these exclusion criteria we drop 9 per cent of the respondents. For the remaining respondents, we drop an additional 5 per cent of person-years by excluding person-years when the respondent worked as selfemployed or in family business, was younger than 16, and had missing information on their authority status.

The final analytic sample consists of 2,998 women with 33,207 person-years in employment (out of their total 53,017 person-years that include those not in our sample because of non-employment) and 2,922 men with 51,710 person-years in employment (out of a total of 53,428 person-years, including non-employment years). Our data are not subject to the type of selective sample attrition common in panel data because information about the occupational career is asked retrospectively—all jobs prior to and including the job at the time of the survey are documented. We do not use person-year observations for years in which the respondent is not employed; this is not a weakness of the data, but rather a matter of choosing the analytic sample relevant to our research question of whether a first birth affects whether, if employed, individuals are in positions of authority. We also lack data on years of individuals' careers that occurred after the survey; accordingly, we have fewer years of data for individuals who were relatively young at the time of the survey. However, we still observe a good share of the respondents 15 years after the transition to parenthood: half of the men and a third of the women.

The fact that we use only years in which respondents are employed does mean, however, that those who withdraw from employment for many years contribute fewer observations than those employed more continuously. On average, women are observed as employed in 11 out of the 23 years we observe, and men in 18. Before the transition to parenthood, we observe both women and men in employment for an average of 6 years (out of the 7 years we observe). After the transition to parenthood, women are observed as employed for an average of 8, whereas men for 12 years (out of 15). Seven per cent of the women are employed in all 23 years; for men, this figure is 32 per cent. The average age at the transition to first-time parenthood among women is 27 and among men 29. Our sample covers a wide range of cohorts that left full-time education between the 1930s and the 2000s, though most of them started their careers in the 1960s or later.

Measures

Supervisory authority

Our measure of workplace authority is a dichotomous measure of supervisory authority (the survey question reads: 'Do you supervise other employees at your workplace?'). Respondents had supervisory authority in 27 per cent of the person-years in which they were employed (36 per cent among men and 13 per cent among women). Out of the samples of 2,998 women and 2,922 men, 519 women (17 per cent) and 1,232 men (42 per cent) changed authority status during the observed period. Of those who switched between having and not having supervisory authority, both men and women changed on average 1.4 times. Fifty-nine per cent of all changes among women are moves *into* authority (41 per cent are *out of* authority). Among men, 70 per cent of all changes are moves into authority, and 30 per cent are moves out of authority.

Years before/after transition to first-time parenthood

Our predictors of major interest are variables indicating the number of years before and after the transition to first-time parenthood. The time window we observe, chosen such that we could draw on a sufficient number of cases, includes up to 7 years prior and 15 years after the birth of the first child. The majority of respondents are observed (as employed) at 7 years before transition to first-time parenthood (70 per cent among men and 64 per cent among women). Among men, at least 50 per cent of all respondents are observed as employed in each of the years before/after the transition to first-time parenthood. Among women, this figure is 30 per cent.

Control variables

All our models control for respondent age and period to account for life-course patterns (changes with age) and period trends (such as changes in the economy) in respondents' chance of having workplace authority. We use 4-year and 5-year intervals of age and period, respectively, to break perfect collinearity between the two[6]. These specifications were chosen based on model fit; however, our results are robust to using dummies and longer or shorter age and period intervals. We also include the interaction between respondent age and respondent age at first child birth to account for selectivity in the timing of transition to parenthood on unobserved factors that may also affect the attainment of a job with authority. We further include the interaction between respondent age and highest completed education (coded as lower secondary, higher secondary, and higher education) at the time of the survey to capture heterogeneity among educational groups in the development of authority status across the life course[7].

In some additional models, we also control for the transition to cohabitation and marriage, work experience, and hours worked. The transition to cohabitation or marriage is operationalized with a categorical variable indicating for each year whether the respondent

Table 1. Descriptive statistics by gender

	Women	Men
Supervisor at first observation	9%	22%
Age at first observation		
Mean	21.16	23.11
SD	(4.05)	(4.44)
Age at first child		
Mean	27.01	29.15
SD	(4.34)	(4.58)
Educational attainment at survey		
Lower secondary	41.19%	36.41%
Upper secondary	34.89%	31.52%
Higher education	23.92%	32.07%
Cumulative number of months not employed at last observation		
Mean	26.09	7.99
SD	(48.12)	(18.32)
Percentage ever cohabitating or married	87%	98%
Work hours at first observation		
Mean	37.44	41.76
SD	(8.50)	(8.60)
Observations	2,998	2,922

Data source: Family Survey of the Dutch Population 1998 to 2009.

was single, cohabiting, or married (and living with their partner). Time-varying work experience is measured by two variables: the number of months spent unemployed or out of the labour force in the previous year, and the cumulative number of months spent unemployed or out of the labour force in the past. We do not control for the cumulative number of months in employment because this measure is too highly collinear with age for men. A limitation of our measure of work hours is that for changes in the number of work hours within the same workplace, the survey recorded only changes of more than 8 h. Table 1 provides descriptive statistics for the variables central to our analyses.

Analytic Strategy

We estimate individual fixed-effects linear probability (OLS) models predicting the probability of having supervisory authority in the given year. Separate models are run for women and men, limiting each sample to employed person-years of those who ever experienced the transition to parenthood. Our models in essence identify the effect of parenthood by comparing the probability that the *same* individual had of being in a position entailing authority before and after the transition to parenthood. As mentioned before, this approach addresses selectivity issues better than approaches making cross-sectional comparisons between parents and childfree individuals. Individuals who transition to parenthood may differ from childfree individuals in authority-associated characteristics, in which case the outcomes observed would (in part) be due to selection rather than an effect of the transition to parenthood.

We use distributed fixed-effects models by including indicator variables for 7 years before, the year of, and 15 years after the transition to parenthood. The coefficients on these indicator variables represent the difference between the predicted year and 7 years prior to the child birth (the omitted reference category). This dynamic approach, used elsewhere to study wage effects of events such as the transition to marriage (e.g. Dougherty, 2006; Cheng, 2015), maps effects of the transition to parenthood on authority as they unfold over the life course, rather than treating the transition to parenthood as a one-time event (as done with conventional fixed-effects models). Standard errors are clustered at the respondent level.

We estimate several distributed fixed-effects models beginning with the baseline model described above. We then estimate a model that adds controls for the respondent's union status (single, cohabiting, or married) and the number of months spent unemployed or out of the labour force in the past (cumulative and in the previous year). A final model adds hours worked in the current job. From each of these models, we compute and plot predicted probabilities of employment for women or men at each of the 7 years before the birth, at the year of the birth, and at each of the 15 years after the birth. The predictions assume the sample distribution of controls in the model across all years as the distribution for each year, an average marginal effects approach. Each gender's predictions come from the coefficients from their gender-specific regression and their gender-specific sample distributions on controls. In one further set of analyses, we generate predictions for women if they had men's means on the controls; this allows us to see whether differences in means on controls between men and women explain any of the gender gap in authority.

Results

Figure 1 plots the results from the distributed individual-fixed effects models for women and men. The figure shows the average predicted probabilities (y-axis) of having supervisory authority for women and men at each year up to 7 years before, and 15 years after the transition to parenthood (x-axis). Year 0 on the x-axis represents the 12-month period starting in the month in which the first child was born up until and including the 11th month of life of the child. We work with 12-month periods around the birth of the first child, not calendar years. Year −1 on the x-axis thus ends in the month before the child was born, and year 1 begins when the child turned 1 year of age. The figure shows the results from three models: (i) a baseline model controlling for age, period, the interaction between age and age at first childbirth, and the interaction between age and education; (ii) a model adding the transition to cohabitation or marriage and work experience to the baseline model; and (iii) a final model additionally controlling for hours worked. The coefficients behind the plots are shown in Supplementary Table A1.

We first discuss the results for women. Focusing on the baseline model, among women, we see an initial increasing probability of being in a position of authority up until 2 years before the transition to parenthood, although the rate of growth of women's probabilities

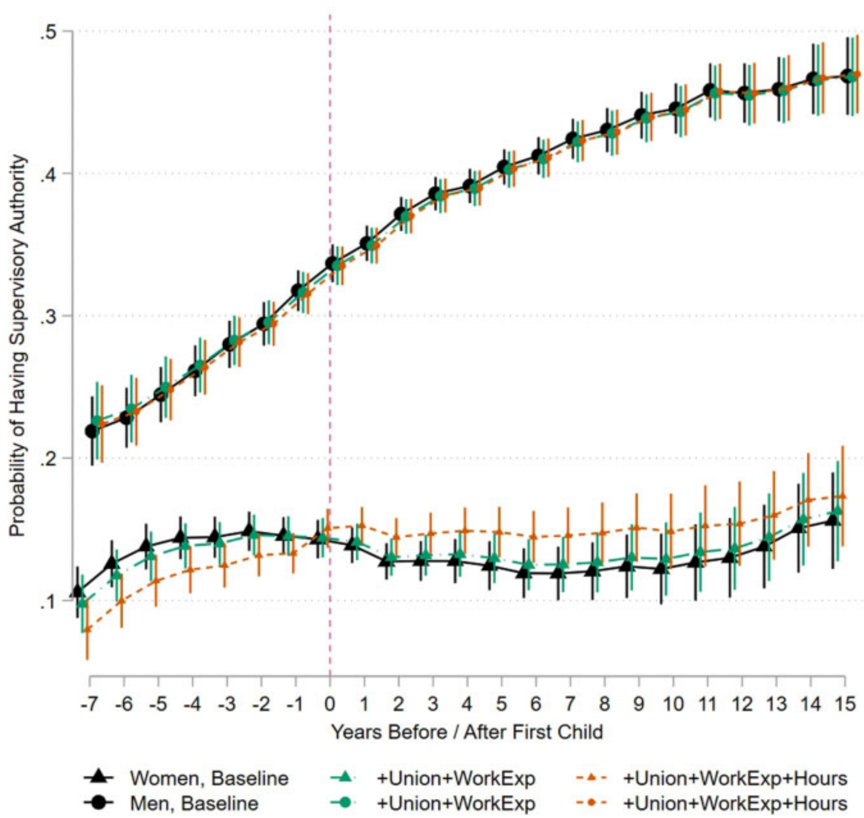

Figure 1. Predicted probabilities of having supervisory authority by years before/after transition to parenthood. *Data source*: Family survey of the Dutch population 1998 to 2009. *Notes*: 95% confidence intervals shown. Figures plotted using Stata's plottig scheme (Bischof 2017).

slows down at 5 years prior to childbirth. Women's predicted probability of having supervisory authority increases from about 11 per cent at 7 years to 15 per cent at 2 years before the transition to parenthood ($P = 0.000$). At 2 years prior to the birth women's probability of having supervisory authority starts decreasing, reaching a probability of 0.12 in year 6 after the transition to parenthood ($P = 0.040$). The largest decrease (of 1 percentage point) between adjacent years in this period happens between year 1 and year 2 after childbirth. Fourteen years after the transition to parenthood, women's probability of having supervisory authority recovers back to the 15 per cent observed at 2 years prior to the transition to parenthood.

These results suggest that the transition to parenthood has a negative effect on women's representation in supervisory authority that starts in anticipation of parenthood, 2 years prior to childbirth. The total decrease in the predicted probability of 3 percentage points (20 per cent) is recovered by the end of the observation period. The slowing down of the rate of growth of women's probabilities at 5 years prior to childbirth could also be interpreted as an effect of the anticipation of parenthood, albeit not unambiguously.

To investigate whether the start of the decrease we observe at 2 years before the transition to parenthood (or the slowing down of the rate of growth of women's probabilities at 5 years prior to childbirth), is due to the transition to cohabitation or marriage (which leads both to transitioning to parenthood *and* a possible decrease in women's chances of having workplace authority), in the next model (not shown), we control for the transition to cohabitation or marriage (with single as reference category). The addition of this variable scarcely changes the results. Perhaps surprisingly, our results also indicate that women do not lower their probability of being in a position of authority by taking time out of employment after birth; we can infer this from the lack of change in the trajectory of probabilities of being in authority when we add the two measures of experience to the model. Figure 1 plots the predicted probabilities from a model containing both the transition to cohabitation/marriage and work experience because neither contributes to a substantive change of the results (see Supplementary Table A1 for the step-by-step model build-up).

Finally, we look at the extent to which the trend can be explained by changes in the number of hours worked. We find that among women changes in hours worked account for the decrease in the probability of having supervisory authority starting 2 years prior to the transition to parenthood mentioned above. Moreover, the drop from year 1 to year 2 that we earlier observed is no longer statistically significant. This suggests that the relatively modest declines in being in jobs with authority associated with the transition to motherhood are largely explained by the fact that some women move to part-time work, and this entails demotions from authority or hurts their chances of promotion to positions of authority.

In additional analyses (not shown), we add a control for whether the person has had a second birth. We find that the shape of the post-first-birth trajectories shown in Figure 1 is not meaningfully changed. We suspect that the explanation for this lack of distinctive effect of a second birth on authority is that the timing of subsequent births for most women (99 per cent of the women in our sample have their second child within 9 years after the first child is born) is in the period during which women keep working the reduced number of hours they went to after their first birth. Given the centrality of work hours for explaining the effect of the first-time transition to motherhood, we observe no changes around subsequent births because work hours are already reduced.

The findings in Figure 1 show that how parenthood is associated with workplace authority is substantially different for men and women. Among men, we find an increasing pre-transition trend that continues into the post-transition period. Men's average predicted probabilities of having supervisory authority increase over the years, with statistically significant increases between almost all adjacent years up until age 12 of the child. At 7 years before the transition to first-time parenthood, men's average predicted probability of having supervisory authority is 22 per cent. At the end of the observed period, this probability has doubled to 47 per cent with continuous monotonic increases.

The results for men suggest to us that there is no causal effect of the transition to parenthood on men's holding a job with authority. Of course, if one were to do a simple before and after comparison, as standard person fixed-effects models do, using our data, it would show men having much more authority after than before the birth of their first child. However, our distributed approach reveals that the slope of men's upward trajectory is not steeper after than before a birth, casting doubt on either the hypothesis that they increase their efforts at promotions into jobs with authority when they become fathers, or that employers favour them for promotions to positions of authority more after a birth.

As we did for women, we use models that add controls to examine the role of the transition to cohabitation and marriage as processes that might propel men into positions of authority, lead to transitioning to parenthood, and continue after the transition to parenthood. As Figure 1 makes clear, adding the transition to cohabitation and marriage to men's model has a trivial effect on the level and slope of the curve expressing the relationship between years before and after parenthood and workplace authority among men, and so do the additions of work experience and hours worked. Consistent with Bygren and Gähler (2012), we find that the transition to cohabitation or marriage itself does not have a statistically significant effect on having supervisory authority among either men or women (see Supplementary Table A1). This is in line with evidence of absence of association between union status and workplace authority from numerous cross-sectional studies (Wright, Baxter and Birkelund, 1995; Wolf and Fligstein, 1979; Bridges and Miller, 1981; Jaffee, 1989; Adler, 1994; Hopcroft, 1996; Maume, 1999; Mitra, 2003; Huffman and Cohen, 2004; Maume, 2004; Blommaert *et al.*, 2019).

What do our findings mean for the gender gap in workplace authority among individuals who eventually transition into parenthood? Figure 1 showed a large average gap in levels of supervisory authority between women and men starting at least 7 years before the transition to parenthood, a gap that grows steadily larger over time but does not change appreciably at birth. However, one problem with the analysis in Figure 1 for understanding the gender gap is that women and men in our sample are not fully comparable in terms of authority-associated characteristics, and Figure 1 is based on adjusted covariates within but not between genders. Men on average score higher on characteristics that are positively associated with having workplace authority. For example, they are older at the parenthood transition and work more hours, especially after becoming parents.

To show the net gender gap in authority, in Figure 2, we show three predicted trajectories. Two are repeated from Figure 1—predictions for men and for women given their own gender's coefficients from the final model discussed earlier, which controls for the transition to cohabitation and marriage, work experience, and hours worked. What is new in Figure 2 is the predicted trajectory for women from the model with their own coefficients but men's means (dash-dotted line). If we compare the predicted trajectory for women if they had men's means to the men's trajectory, the difference tells us what part of the gender gap is explained by gender differences in the controls at any particular year before or after the first birth.

Using this comparison, we see a substantial gender gap in having authority of about 11 percentage points at 7 years prior to the transition to parenthood even when women have men's means on controls, and this net-of-controls gap rises to 16 percentage points by the year of the transition to parenthood. At the end of the observed period, this gap has increased to 27 percentage points. Gender differences in the controls explain about 25 per cent of the gender gap in authority 7 years prior to the transition to parenthood, and 13 per cent of the gap 15 years after the transition to parenthood. Thus, the larger share of the gender gap in authority is not explained by gender differences in levels of the authority-associated characteristics we observe[8]. While women never resume the trajectory they were on before the transition to parenthood, the gender authority gap would have remained large even if they did resume their pre-transition trajectory.

In Figure 2, we did not control for education because, as explained above, as measured at the survey year it is a time-invariant variable so drops out of the models. This is potentially problematic because a larger share of men in our sample are highly educated than women, and this could explain some of the gender gap in authority. Figures 3 and 4 show the results equivalent to those shown respectively in Figures 1 and 2, separately by educational attainment, so that all gender comparisons hold education constant. This also allows us to see whether the parenthood effect on authority varies by education for either men or women. The coefficients behind the plots are shown in Supplementary Table A2. The three groups here (lower secondary, higher secondary, and higher education) are made based on the highest completed education at the time of the survey[9].

Figure 4 repeats what is shown in the fullest model in Figure 3 and then adjusts for between-gender differences in means on covariates by plotting the predicted trajectory of women's probability of authority given men's means on covariates, separately by educational attainment. The figure shows a large controls-adjusted gender gap in authority for all three groups. The absolute gender authority gap at 7 years prior to the transition to parenthood is comparable for the three groups: 12 percentage points for the lower and upper secondary group, and 13 percentage points for the higher education group. At the end of the observed period, this gap has grown, respectively for all three groups, to 21 (1.8 times), 26 (2.2 times), and 34 percentage points (2.6 times the initial gap). Thus, given that a large gender gap exists at all three educational levels, education clearly does not explain much of the gender gap that remains after adjusting for the other covariates. Indeed, the average predicted probabilities of having supervisory authority of women in the highest education category are lower than those of men with the lowest level of

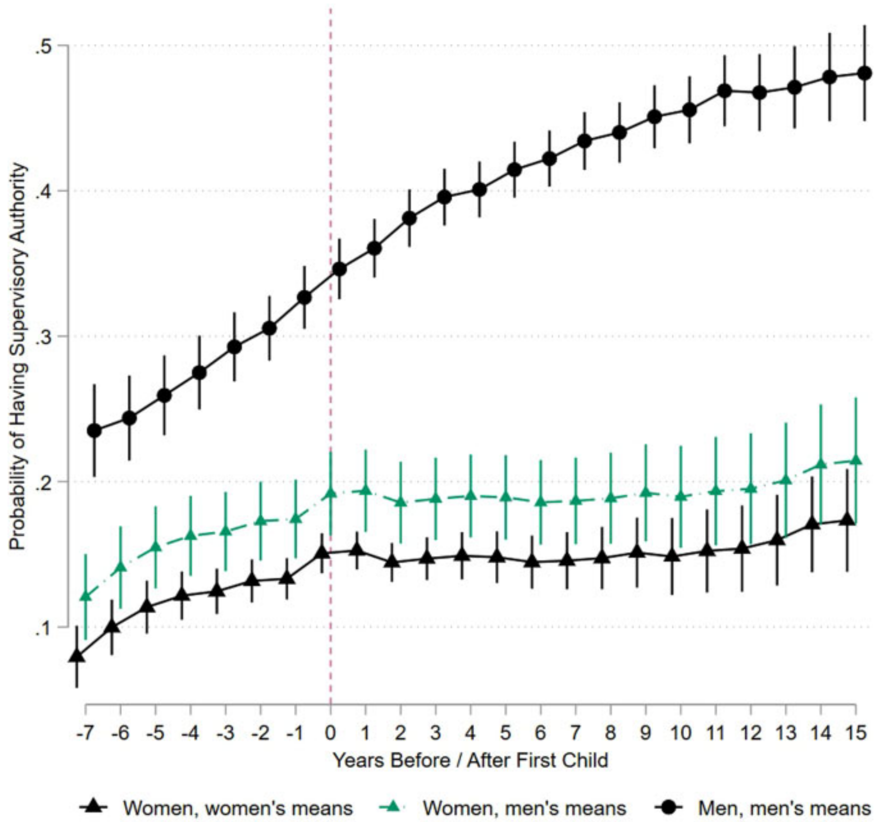

Figure 2. The net gender gap in supervisory authority by years before/after transition to parenthood. *Data source*: Family survey of the Dutch population 1998 to 2009. *Notes*: 95% confidence intervals shown. Women's coefficients were used to estimate predicted authority for women when using either women's or men's means, and men's coefficients were used to estimate predicted authority for men with their own means. See Endnote 8 for decomposition results when giving women men's slopes.

education (lower secondary) throughout the observation period[10].

Discussion

The gender gap in workplace authority has been seen as a source of gender inequality because authority positions are associated with more power and higher job rewards than positions that do not entail authority. Additionally, having women better represented in positions of authority may also decrease segregation and gender earnings gaps among those they supervise (Cohen and Huffman, 2007; Stojmenovska, 2019). A substantial body of literature has concluded that human capital investments and employees' positions in the structures of the economy do not explain the gender gap in workplace authority (Smith, 2002). Whilst long seen as theoretically relevant, the role of parenthood in women's and men's representation in workplace authority has remained understudied (Longarela, 2017; Smith, 2002).

Using retrospective life course data and distributed fixed-effects models, we study the relationship between parenthood and representation in supervisory authority in a sample of Dutch women and men. Our study addresses some selection issues and problems associated with ambiguous comparison categories commonly employed in cross-sectional studies. Instead of comparing parents and childfree individuals at one point in time, we observe individuals years before and after first childbirth and study within-person changes in authority status as individuals transition to first-time parenthood.

We find that the transition to parenthood has a modest negative effect on women's representation in supervisory authority that is recovered at 14 years following the transition to parenthood. The negative effect of parenthood on women's trajectories, which starts in anticipation of parenthood, 2 years prior to childbirth, is

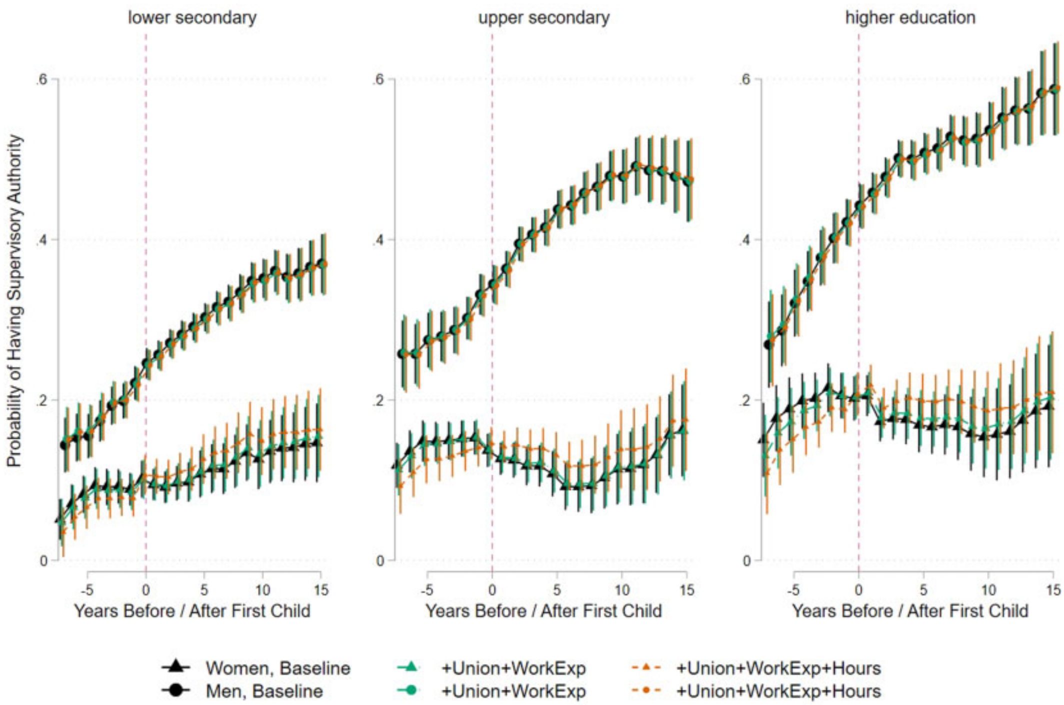

Figure 3. Predicted probabilities of having supervisory authority by years before/after transition to parenthood, by educational attainment. *Data source*: Family survey of the Dutch population 1998 to 2009. *Notes*: 95% confidence intervals shown.

largely explained by reduced work hours around the time of birth. This finding supports the notion that authority positions are, or at least are seen, as incompatible with part-time work. Even though legal equality between part- and full-time workers in the Netherlands is among the most extensive anywhere (Fouarge and Baaijens, 2009), the work-devotion narratives surrounding authority (Blair-Loy, 2005; Cha and Weeden, 2014) serve to exclude part-time workers, who are often women, from the exercise of authority. Women often reduce their hours after a birth, whether this is because they want more hours at home with their children than is compatible with full-time work, or because of inadequate childrearing contributions from their partners and poor non-parental child care options. Whatever the reason for their hours reduction, our results show that, in the Netherlands, this explains all of the effects of becoming a parent on women's likelihood of working in a position of authority.

Among men, we find no evidence of an effect of parenthood on being in a supervisory position. Men's probabilities of having supervisory authority increase before the transition to parenthood, and continue increasing after childbirth in a similar way. One could possibly argue that men's probabilities of having supervisory authority increase prior to the transition to parenthood as a function of anticipating parenthood, and afterward as an effect of parenthood. However, given that the trend prior to the transition to parenthood continues into the post-transition trend in much the same way, we believe the more parsimonious conclusion is that the monotonic trend reflects men's being on trajectories that increase their chances of having authority across the life course.

Comparing our findings to those from the Swedish context (Bygren and Gähler, 2012) is complicated by the use of different methods. Had we adopted a similar approach using our data, we would also have found that men's probabilities of having supervisory authority are higher after than before the transition to parenthood. However, our distributed approach casts doubt on the fatherhood authority premium thesis, given that the slope of men's upward trajectory is not steeper after than before the first birth. For women, Bygren and Gähler (2012) find that their chances of having authority are unaffected when they become mothers. With the caveat of different approaches used, substantively the difference in findings regarding motherhood could be driven by the fact that the Dutch institutional context is more conservative relative to the Swedish context.

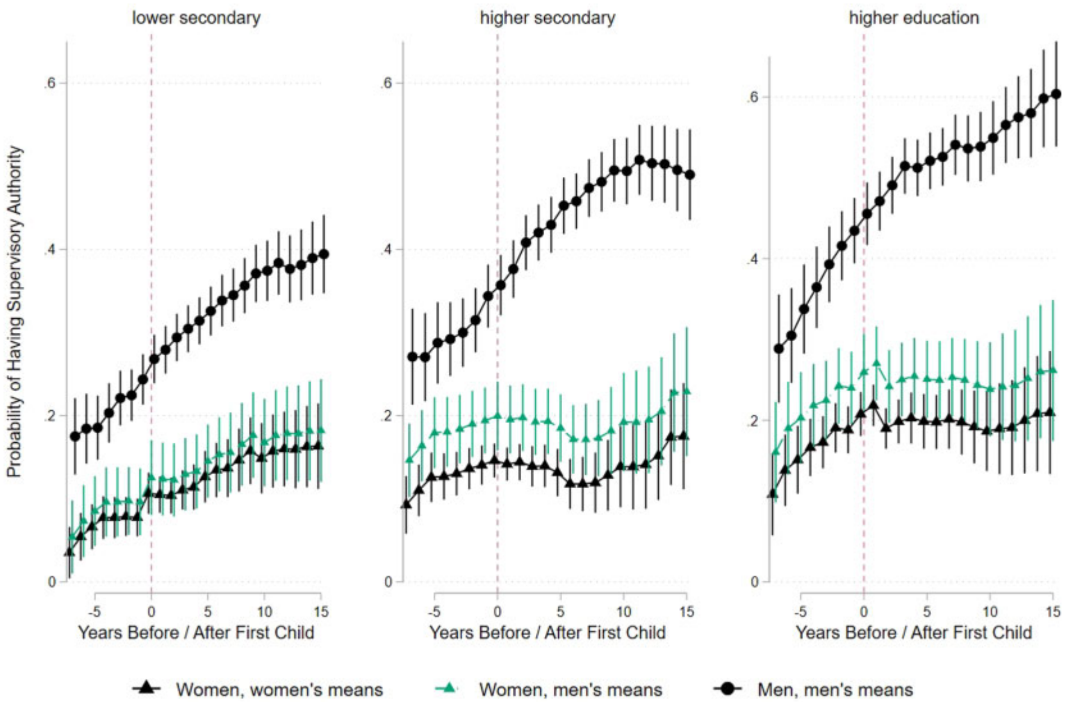

Figure 4. The net gender gap in supervisory authority by years before/after transition to parenthood, by educational attainment. *Data source*: Family survey of the Dutch population 1998 to 2009. *Note*: 95% confidence intervals shown.

We additionally find a large gender gap of about 11 percentage points in authority, 7 years before the transition to parenthood. While this gap grows after the transition to parenthood (to 27 percentage points 15 years after the transition to parenthood), it would have remained large even if women had continued their pre-transition trajectory immediately after their first childbirth. Thus, the majority of the gender gap in authority has nothing to do with child rearing. Our finding of a gender authority gap long before the transition to parenthood squares with recent findings that the gender gap in earnings opens long before motherhood (Combet and Oesch, 2019).

Given that our analyses are based only on individuals who transitioned to parenthood, the conclusions concerning the gender gap in authority can only be generalized to this population, which constitutes a large majority of the population. The gender gap in authority among individuals who never transition to parenthood could be smaller than the gap we show before the first birth or in the years after the effect of the first birth fades. This would be expected if individuals who become parents differ from those who never have children such that, in anticipation of parenthood, they make more gendered career choices long before they have a child. This might imply that our estimate of the total effect of parenthood on the gender gap in authority is an underestimate because our model ignores adaptations to future parenthood made more than 7 years before a birth. We note, however, that evidence regarding whether anticipated parenthood affects individuals' career choices is mixed (see, for example, Bass, 2015 versus Cech, 2016).

Nonetheless, what we observe with certainty—with the caveat that our conclusions are limited to supervisory authority as one form of workplace authority—is that the gender authority gap among individuals who eventually transition to parenthood in the Netherlands is not *created* at the transition to parenthood. The relatively modest change in women's probability of being in a supervisory position around the transition to parenthood pales in comparison with the gender gap in authority even years before the transition to parenthood.

Notes

1 Daycare for preschool children is typically available 3 months after childbirth until the child turns 4, when they start kindergarten. Older children can go to daycare after school until they start primary

school at age 12. Up until 2011, the largest share of childcare costs was covered by the state, with the employer and parents each covering less than a third of the costs (Roeters and Bucx, 2018). In 2019 paternity leave was prolonged to 5 days, and as of July 2020 fathers can request an additional 5 weeks (70 per cent paid). This period is however not in our study.

2 Most of these studies look at supervisory authority (Wolf and Fligstein, 1979; Bridges and Miller, 1981; Jaffee, 1989; Adler, 1994; Ishida, 1995; Hopcroft, 1996; Hultin, 1998; Rosenfeld *et al.*, 1998; Mitra, 2003; Huffman and Cohen, 2004; Bygren and Gähler, 2012; Blommaert *et al.*, 2019). Others (additionally) look at managerial/formal hierarchical position (Ishida, 1995; Wright *et al.*, 1995; Maume, 1999; Maume, 2004), decision-making (Jaffee, 1989; Adler, 1994; Ishida, 1995; Hopcroft, 1996; Wright *et al.*, 1995), and sanctioning authority (Wolf and Fligstein, 1979; Wright *et al.*, 1995). See Smith (2002) for a review of types of workplace authority.

3 The four rounds are comparable in terms of data collection and survey questions. For more information see the respective data documentation (de Graaf *et al.*, 1998, 2000, 2003; Kraaykamp *et al.*, 2009).

4 Methodological studies show that memory bias in retrospectively collected employment career data is limited and that the great majority of respondents makes no errors at all (Manzoni *et al.*, 2010; Shattuck and Rendall, 2017).

5 The original file contains person-month observations. Monthly information was aggregated to yearly information by selecting the mode information of 12-month periods. For example, if a respondent had authority in 4, and no authority in 8 out of 12 months, they are coded as not having authority in that year. Respondents were considered as employed only if they worked at least 6 out of the 12 months.

6 We put age and cohort into categories that differ in length to break their perfect collinearity for readers interested in their separate coefficients; we have no discussion of these effects as our interest was just to control for them. Our coefficients for either age or period on authority might absorb some effects of cohort, given the age-period-cohort problem, which we do not address in this article.

7 The two interactions we included each entail one time-invariant measure—age at first birth and education at the time of the survey. Time-invariant measures are part of the individual fixed-effect and thus the main effects of age at first birth and education drop out of the models. In the case of education, the dataset includes retrospectively reported educational attainment for each year, but most respondents did not increase their educational attainment across the years of employment we analyse. A sensitivity test shows that using this measure instead changes none of our substantive conclusions.

8 In additional Kitagawa-Oaxaca-Blinder decomposition analyses (not shown), we find that gender differences in authority returns to authority-associated characteristics explain more of the gender gap in authority than mean gender differences in these characteristics.

9 To address the possibility that after entering the labour market some individuals pursue (or stop pursuing) more education depending on the type of job they get, as a sensitivity test we run these analyses using the highest completed education at 7 years prior to the transition to parenthood instead. These analyses do not yield different results.

10 The replication package to this article is available at the corresponding author's website (http://www.stojmenovska.com).

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Supplementary Data

Supplementary data are available at ESR online.

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