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Is There a Fatherhood Wage Premium? A Reassessment in Societies With Strong Male-Breadwinner Legacies

Objective: *This study examines whether fatherhood sparks the wage attainment of men or rather entry into fatherhood is simply more typical for high-earning men and at times of wage growth during the career cycle.*

Background: *Fatherhood premiums may contribute to gender economic inequalities, particularly in countries with strong male-breadwinner legacies such as Germany and the United Kingdom. Yet, as male-breadwinner norms have waned and policies have started fostering men's role as carers, wage premiums could be a thing of the past. Also, the mechanisms usually invoked to account for fatherhood premiums—effort allocation, couple specialization, and employer discrimination—seem of little relevance even in these countries. Entry into parenthood spurred by wage attainment is therefore scrutinized as an alternative source of the apparent premiums on average and across cohorts.*

Method: *The author uses long-running panel data for both countries and three regression-based approaches (pooled ordinary least squares, fixed effects estimation, and fixed effects individual-slope estimation).*

Results: *Overall, fatherhood wage bonuses could not be detected on average as well as across birth cohorts. At best, estimates were compatible with modest premiums among older cohorts of men. Positive selection on both prior wage levels and wage growth was found to be largely responsible for the apparent wage boost. The contribution of selection on prior wage levels though has been fading across cohorts, meaning that men select into fatherhood less and less on the basis of time-invariant characteristics positively related to both wages and the chance of becoming a father.*

Conclusion: *The link between fatherhood and wages appears to be more of a selection story than a causal one, even in contexts with strong male-breadwinner legacies.*

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Motherhood wage penalties are now key drivers of gender wage gaps in high-income countries (for a review, see Ponthieux & Meurs, 2015). Counterpart to wage losses for mothers, fathers may receive more modest wage premiums (Killewald, 2013; Lundberg & Rose, 2000; Petersen, Penner, & Høgsnes, 2011). Yet, although the question of whether motherhood causally affects wages has come under intense scrutiny (e.g., Elwert & Winship, 2014), few

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have asked the same about fatherhood (for an exception, see Loughran & Zissimopoulos, 2009).

I first reconsider the possibility that fathers earn more due to how men select into fatherhood (e.g., Kravdal & Rindfuss, 2008; Trimarchi & Van Bavel, 2017). Previous research has carefully accounted for the fact that high-earning men are more likely to become fathers. Selection, though, may also operate through the superior wage growth of fathers-to-be rather than just through their wage levels (Ludwig & Brüderl, 2018). Accounting for both, this study provides a more severe test for the causal effect of fatherhood on wages.

Second, adding to a literature mainly based on the United States, I compare here Germany and the United Kingdom. These two countries have long supported male breadwinning both through policy and culturally. The inability to support a causal story for fatherhood and wages would thus be particularly meaningful in such contexts. As male-breadwinner norms have waned (Knight & Brinton, 2017) and policies in both countries have extended family-leave rights to men (Blum, Koslowski, Macht, & Moss, 2018), rich longitudinal data also allows investigating heterogeneity across cohorts. This has been overlooked in previous studies, perhaps partly because of the invariance of the policy context of fatherhood in the United States (at least at the federal level, cf. Baum & Ruhm, 2016).

If present in the past, wage premiums may have declined and particularly in Germany. Comprehensive evidence has pointed to a (small) shift in effort from the market to the household for recent cohorts of German men (Leopold, Skopek, & Schulz, 2018; Pollmann-Schult & Reynolds, 2017; Tamm, 2018). Notably, German fathers have increasingly accessed a generous and relatively long parental leave provision put in place since 2007 (e.g., Bünning, 2015). In the United Kingdom, evidence points to similar changes in men's contribution to household production (e.g., Altintas & Sullivan, 2016; Huerta et al., 2014), but policy change regarding fathers has been less extensive (Lewis, 2002; Lewis & Campbell, 2007; Tanaka & Waldfogel, 2007). This further motivates a cross-country, cross-cohort comparative perspective. If fatherhood wage premiums prove persisting, they may further deepen gender wage gaps or make it such that, even if the mommy penalty will narrow down, gender wage gaps will endure (Petersen, Penner, & Høgsnes, 2014).

Considering whether fatherhood to this day causally affects men's wages is thus paramount to disentangle the sources of gender economic inequality. In short, I ask here (a) if fatherhood premiums are causal or rather a by-product of the process by which men select into fatherhood; (b) whether premiums are found in contexts with strong, yet fading, male-breadwinner legacies; and (c) whether premiums for fathers have faded too or have rather persisted across cohorts.

Background

Why There Could Be a Fatherhood Premium: Reviewing Previous Evidence and Mechanisms

Considering previous evidence, the fatherhood wage premium seems generally modest in size and is not always detected across contexts. Both findings are in stark contrast with research on motherhood wage penalties. Estimates of the gross premium for all fathers have been the largest in North America, up to a range of 6% to 13% in Canada and the United States (Cooke & Fuller, 2018; Hodges & Budig, 2010; Lundberg & Rose, 2000). In Norway and Denmark, premiums have not been ascertained (Cools, Markussen, & Strøm, 2017; Kleven, Landais, & Sogaard, 2018) or have at best amounted to 1% to 2% (Petersen et al., 2011). Few studies have examined the premium in Germany and the United Kingdom, finding for the former a 2% to 3% wage boost for higher order parities (Pollmann-Schult, 2011). For a comparison, gross motherhood wage losses have typically been estimated in excess of 15% in Germany (Beblo, Bender, & Wolf, 2008; Gangl & Ziefle, 2009; Kühhirt & Ludwig, 2012) and of around 12% in the United Kingdom (Gangl & Ziefle, 2009; see also Harkness, 2016). Beyond Germany and the United Kingdom, substantial economic losses for mothers seem rather universal across contexts (e.g., Kleven, Landais, Posch, Steinhauer, & Zweimüller, 2019).

When detected, wage premiums for fathers have been traced back to individual changes in work effort, couple specialization, and employer discrimination. Based on previous evidence and theoretical considerations, though, the explanatory power of each of these mechanisms can be called into question. For one, fatherhood may elicit an increase in men's work effort (e.g., Eggebeen & Knoester, 2001). Yet studies based on household data have not been able to assess this due to the lack of precise measures of productivity, relying instead on proxies of effort or productivity such as working hours. As previously acknowledged (Killewald, 2013), working hours may be poor proxies of effort or only affect wages in the long run, as hard-working employees signal themselves to employers and secure thereby better-paid positions in internal labor markets (Gibbons & Waldman, 1999). In the short run, working longer hours may increase total earnings rather than wage rates, but studies of the fatherhood premium have typically been concerned with the latter (for an exception, see Cooke, 2014).

Furthermore, fatherhood may propel a man to increase his work effort only conditional on whether his earning potential exceeds that of his partner (Becker, 1981). Fatherhood wage premiums should thus be observed particularly for those men whose partner reduces working hours or leaves paid work come parenthood. Indeed, U.S.-based studies have found that married fathers, and among them especially those whose wives interrupt employment or cut back work hours, add extra working hours of their own and gain a wage premium (Killewald, 2013; Killewald & Gough, 2013; Lundberg & Rose, 2000).

Assessing the impact of couple-level specialization on the wages of fathers, however, presents additional complications due to the "endogeneity of family" (Lundberg, 2005, p. 603). Couple formation is often a transitory event, considering that splitting and repartnering are commonplace (Elzinga & Liefbroer, 2007). If specialization affects couple formation and stability, conditioning the analyses of fatherhood premiums on the presence and characteristics of a partner may induce selection bias (Elwert & Winship, 2014). For example, if couples in which women do not specialize in household production are more likely to split (e.g., Kalmijn, Loeve, & Manting, 2007; Lepinteur, Fleche, & Powdthavee, 2016; for a review, see Cooke & Baxter, 2010), surviving couples at any given point in time may be the ones more likely to (have) specialize(d). The role of couple specialization on the wages of fathers may thus be exaggerated, yet studies have neglected this point so far (e.g., Killewald & Gough, 2013).

Moving on from the behaviors of fathers and couples, wage premiums for fathers may also result from employer discrimination. Employers may prefer fathers, expecting them to be more productive, competent, or committed than their childless counterparts (cf. Correll, Benard, & Paik, 2007; Phelps, 1972). Even regardless of factual productivity differences between male employees, men could be differentially treated in the workplace depending on parental status. In a lab experiment with U.S. undergraduate students, Correll et al. (2007) indeed found that fathers were evaluated as more committed, would have been hired more often, and would have been offered higher starting salaries than childless men, holding job applicants' features equal. Yet, in the companion field experiment, differences in call-back rates between equivalent fathers and childless men were not detected. The same inconclusive evidence has recently emerged from a large field experiment across multiple job titles in Sweden (Bygren, Erlandsson, & Gähler, 2017).

Hence, a review of the size and generative mechanisms of the fatherhood bonus motivates asking whether expecting a causal effect of fatherhood on wages is warranted in the first place.

Contextual Underpinnings of the Wage Trajectories of Fathers in Germany and the United Kingdom

Although addressing wage determination at the individual level, contextual factors that may shape the fatherhood premium (and its drivers) have been overlooked in the literature (cf. Cooke, 2014). One might expect that a strong male-breadwinner norm, both culturally and institutionally enforced, may foster shifts in market effort after the transition to fatherhood, a traditional specialization pattern within couples, or employer bias in favor of fathers.

Looking at the United Kingdom and Germany, I compare two countries with strong yet drifting male-breadwinner legacies (e.g., Crompton, 1999). Culturally, male-breadwinner norms have been stronger in former West Germany rather than in the United Kingdom (Knight & Brinton, 2017; Trappe, Pollmann-Schult, & Schmitt, 2015). Unfavorable attitudes toward mothers' employment—whether full-time or in general during a child's preschool years—persist in both countries (Dechant & Rinklake, 2016;

O'Reilly, Nazio, & Roche, 2014). Still, attitudes in both countries have shifted away from traditionalist views that assigned to men the role of (sole) breadwinners and to women the role of full-time carers and homemakers (Knight & Brinton, 2017).

As for policy, fatherhood has long been synonymous with providing. This has been emphasized, for one, via mandated cash transfers from fathers to mothers in case of couple dissolution (Hobson, 2002). Lately, however, and to a somewhat greater extent in Germany, policy reform started targeting fathers for care obligations rather than for cash provision (Adler & Lenz, 2017). In 2007, Germany introduced two bonus months of paid parental leave granted to households in which both parents take some portion of the leave period. As a result, father involvement in the household appears to have increased (e.g., Tamm, 2018) in times of a broader, if slow, gender convergence in the division of labor (Leopold et al., 2018; Pollmann-Schult & Reynolds, 2017). The United Kingdom, differently, opted for a paid statutory paternity leave (2003), with high uptake rates but lasting only 2 weeks, and a parental leave scheme (2015) only partly paid and so far largely not exploited by new parents (Blum et al., 2018).

Changes in the cultural and policy contexts surrounding fatherhood thus further enrich the chance to understand the contextual underpinnings of fatherhood premiums (if any) over time in the two countries. First, fatherhood may act as a transforming event and spur men's work effort particularly in contexts where male breadwinning is culturally reinforced (Townsend, 2002). Yet the transition to fatherhood does little to change men's attitudes toward work and family, as men seem not to become more (or less) traditional after the birth of a child (Grinza, Devicienti, Rossi, & Vannoni, 2017; Kuziemko, Pan, Shen, & Washington, 2018). Coherently, findings regarding fathers' working hours as a proxy for effort suggest that, even in former male-breadwinner regimes, men do not commit more fully to breadwinning after the birth of a child. Indeed, average working hours actually decline after the transition to fatherhood in most European countries (Bünning & Pollmann-Schult, 2016). In Germany, becoming a father prompts an increase in working hours for men born prior to 1960, but a decrease in working hours for men belonging to younger cohorts, and both changes are modest in size (1 hour of paid work at most; Pollmann-Schult & Reynolds, 2017). For Britain, previous studies provide little evidence that the presence of children affects men's working hours at all (Bryan, 2007; Paull, 2008; Schober, 2013).

Men could still increase their work effort conditional on their partner's investment in the household, consistent with a traditional mode of couple specialization. Tax policies, for example, may provide incentives for particular household arrangements regarding paid and unpaid work. Pooling the income of both partners to determine personal income tax, Germany discourages paid employment among secondary earners (Bick & Fuchs-Schündeln, 2017; Smith, Dex, Vlasblom, & Callan, 2003), and may thereby foster traditional couple specialization. Differently, Britain switched from joint to individual taxation in 1990, further pursuing tax credit policies since the end of the 1990s with the aim of encouraging maternal labor supply (Francesconi, Rainer, & Van Der Klaauw, 2009; Francesconi & Van der Klaauw, 2007). Perhaps surprisingly, though, couple specialization in both the United Kingdom and Germany deviates somewhat from the Beckerian model when it comes to the transition to parenthood. Mothers indeed trade off employment hours with time spent in housework and child care, yet fathers' allocation of time to either paid or unpaid work is hardly affected by parenthood regardless of their partner's behavior (e.g., Kühhirt, 2012; Schober, 2013).

Employer bias in favor of fathers may still rest on the assumption, however justified, that men will maintain or even increase their commitment to paid work after the birth of a child. In recent years, such an assumption may also have eroded because of fathers' use of paternity and parental leaves, which may signal a parallel and potentially conflicting commitment to the family sphere. In Britain, around 80% of fathers now take time off around the birth of a child, although mostly in the form of the 2-week paternity leave introduced in 2003 (Blum et al., 2018). In Germany, fathers have been entitled to paid parental leave provisions since the end of the 1980s, but fathers' uptake became substantial only after the aforementioned 2007 reform. According to the latest figures, more than 30% of German fathers now use parental leave provisions, typically using up the 2-month bonus (Bünning, 2015; Kluve & Tamm, 2013). Notably, these figures for German fathers approach those of their counterparts in Sweden. Swedish fathers taking parental leave have been found to experience modest wage penalties after returning to work, a finding scholars have interpreted as evidence of adverse signaling (Albrecht, Thoursie, & Vroman, 2015; Evertsson, Grunow, & Aisenbrey, 2016). Also, wage penalties for fathers taking leave may stem from a reorientation of effort from the market to the household (Rege & Solli, 2013). It is at best unclear, therefore, if employers may still assume increased commitment to work among fathers and discriminate in their favor, particularly in modern-day Germany.

Broadly, as male-breadwinner norms waned and policies have shifted their emphasis from cash to care, it could be that fatherhood premiums are at best a thing of the past in both countries, and possibly even more in Germany than in the United Kingdom. In addition to the average causal effect of fatherhood on wages, I will thus look into its possible heterogeneity across cohorts.

Why There Might Not Be a Fatherhood Premium After All: The Role of Selection

Support for the idea of a fatherhood wage premium is thus mixed, even when considering contexts with strong male-breadwinner legacies. It is natural to ask, then, whether the fatherhood premium is causal or rather driven by selection into fatherhood. I consider here two sources of selection: selection on prior wage levels and selection on prior wage growth.

Similar to selection into marriage (for a review, see Ludwig & Brüderl, 2018), selection on prior wage levels entails that high-earning men are, by the same token, more likely to become fathers. As Cooke and Fuller (2018, p. 783) put it, "positive selection might account for the gross (wage) premium if the men who become fathers have unmeasured characteristics such as loyalty and commitment valued similarly by employers and potential partners." If such positive selection holds, ignoring it would lead to an overestimation of fatherhood wage premiums.

Research on what kind of men eventually become fathers is relatively underdeveloped (Balbo, Billari, & Mills, 2013; Kreyenfeld & Konietzka, 2017). Across countries, highly educated men have better chances of becoming a father than do low-educated men, yet much appears to be due to selection into union (e.g., Trimarchi & Van Bavel, 2017). If fatherhood and union formation are a compound, one might expect selection into fatherhood to overlap with selection into marriage, and both of them will be positive (for a review, see Ludwig & Brüderl, 2018). As long as drivers of positive selection are unobserved, cross-sectional estimates of the bonus (e.g., Cooke, 2014; Cooke & Fuller, 2018; Petersen et al., 2011, 2014) might thus suffer from selection bias due to omitted variables in the regression equation. Relying on panel data, differently, one can add individual fixed effects (FE) to curb estimates from selection on such time-invariant unobservables. Comparing the latter to cross-sectional estimates has highlighted negative selection may be at play, as panel estimates of the daddy bonus are typically bigger than their cross-sectional counterparts (Hodges & Budig, 2010; Lundberg & Rose, 2000). Yet, once again, these studies have only focused on the United States. They could thus speak to the specificity of the American context where fatherhood—or at least early fatherhood—may go hand in hand with markers of earning and life-course disadvantage such as dropping out of high school or incarceration (e.g., Dariotis, Pleck, Astone, & Sonenstein, 2011). Hence, also the direction of selection on prior wage levels is at best ambiguous, much like the effectiveness of mechanisms purportedly leading to a causal bonus discussed in the previous sections.

Yet selection could also operate through a different path. Men may select into fatherhood, depending on their wage growth rather than simply on their wage levels (Ludwig & Brüderl, 2018). The transition to parenthood, much like that to marriage (Killewald & Lundberg, 2017), may simply occur at times of fast wage growth in the career cycle. For one, as men who become fathers are disproportionately better educated (Kravdal & Rindfuss, 2008; Trimarchi & Van Bavel, 2017), they do not simply enjoy high wages but also steep wage growth paths (e.g., Lagakos, Moll, Porzio, Qian, & Schoellman, 2018)—steeper, possibly, than that of relatively less-educated men who are more likely to remain childless. This might hold especially for men belonging to younger cohorts, as they typically have children later in life after considerable accumulation of experience and wages in the market (McMunn et al., 2015; Struffolino, Studer, & Fasang, 2016). Once again, the United

States might not provide the best context to assess this selection dynamic, as mean age at first birth has not increased much across cohorts, and the transition to parenthood is still prominent in men's early 20s when labor market careers are not yet consolidated (e.g., Mills, Rindfuss, McDonald, Te Velde, & the ESHRE Reproduction and Society Task Force, 2011).

Previous comparative studies for European countries, including the United Kingdom and Germany, have indeed found that the wages of men grow already in the period prior to fatherhood (Smith Koslowski, 2011). Yet such literature has not drawn one important implication out of this finding. If the wage spikes prior to fatherhood are comparable to the wage spikes observed after fatherhood, then speaking of a bonus sparked by fatherhood is unwarranted given the observed data pattern (e.g., Killewald & Lundberg, 2017; Ludwig & Brüderl, 2018). The tests I devise in this study, therefore, will try to detect a spike in wages occurring in the aftermath of fatherhood, once one accounts for men's selection into fatherhood based on both wage levels and on wage growth.

Methods

Data and Samples

I employ long-running household panel data, namely, the German Socio-Economic Panel (SOEP v31; Goebel et al., 2018) and the British Household Panel Survey (BHPS; University of Essex, Institute for Social and Economic Research, 2018). Both are multipurpose household surveys following the lives of a representative sample of each country's residents (Buck & McFall, 2011; Goebel et al., 2018; Taylor, Brice, Buck, & Prentice-Lane, 2010). Both datasets also comprise fertility history files (respectively, Goebel, 2017; Pronzato, 2011) that I used to recover information on the transition to fatherhood.

For the United Kingdom, I rely on all BHPS sample members. The BHPS was temporarily discontinued in 2009, but its sample started being interviewed again in 2010 to 2011 within the framework of the U.K. Household Longitudinal Study (UKHLS). My analyses thus rely on the full data available for the BHPS sample, covering, despite the gap, the period from 1991 to 2016. For Germany, I employ samples A to H as well as refreshment samples J and K, covering the period 1984–2014. In the main analyses, I focus on men aged 20 to 50, working as dependent employees, with nonmissing information on their fertility history as well as on their current wage. Furthermore, given my focus on the transition to fatherhood, I restrict my analyses to men who, when first observed in the panel, had no children. During the observation period, part of this initial pool will experience the transition to fatherhood (treated group), whereas the rest will remain childless (control group). For some of the men in this latter group, family histories may be truncated: They might become fathers after the last data point in each panel or drop out of the panels prior to their transition to fatherhood. If arguments on selection hold, this subgroup of men among the controls might actually be more similar to fathers(-to-be) than childless men, and this would in turn attenuate estimates of the fatherhood premium. Hence, to ensure that the control group does not include prospective fathers who might be on a similar wage trajectory to that of the treated, I further limit my analyses to men who have been observed at least until age 40, thereby selecting cohorts of men born not after 1974 for Germany and not after 1976 for the United Kingdom. The cut-off point at age 40 is assumed to be indicative of completed fertility for men, as less than 10% of the transitions to fatherhood occurred past age 40 in the final samples for both countries (see also Kleven et al., 2018). Applying or not this sample restriction, however, does not alter the substantial conclusions of this article (for sensitivity checks and a discussion, see Section 8 of Appendix S1).

Finally, due the requirements of one of the statistical models I will use (FE individual-slope [FEIS] model, see below), I further limit my analyses to men who have been observed for at least three waves in the panel (Brüderl & Ludwig, 2015; Ludwig & Brüderl, 2018). Together, these restrictions result in a sample of 2,713 men (1,013 of which will become first-time fathers) and 34,925 person-year records for Germany, and a sample of 1,253 men (572 of which will become first-time fathers) and 15,470 person-year records for the United Kingdom. In both samples, men are followed for an average of roughly 12 waves. After (prior) the birth of a child, in particular, first-time fathers are followed for an average of roughly 12 (five) waves in the German sample and 10 (five) waves in the British sample.

Table 1. *Summary of Sample Restrictions and Relative Sample Sizes*

	SOEP		BHPS-UKHLS	
	N person-years (*N* individuals)	%	*N* person-years (*N* individuals)	%
All men aged 20 to 50	146,334 (20,109)	100	74,462 (10,883)	100
In dependent employment	106,054	72.5	53,550	71.9
	(16,452)	(81.8)	(8,544)	(78.5)
Complete fertility history	88,088	60.2	52,917	71.1
	(11,879)	(59.1)	(8,227)	(75.6)
First-time fathers and childless men	58,625	40.1	33,902	45.5
	(7,591)	(37.7)	(5,395)	(49.6)
Observed at least until age 40	37,076	25.3	17,437	23.4
	(3,236)	(16.1)	(1,649)	(15.1)
Nonmissing on current wage	35,923	24.5	16,086	21.6
	(3,160)	(15.7)	(1,539)	(14.1)
Observed for at least three waves	35,344	24.1	15,743	21.1
	(2,747)	(13.6)	(1,285)	(11.8)
No child when first observed	34,925	23.8	15,470	20.8
	(2,713)	(13.5)	(1,253)	(11.5)

Note: BHPS = British Household Panel Survey; SOEP = German Socio-Economic Panel; UKHLS = U.K. Household Longitudinal Study.

Table 1 sums up all sample restrictions and relative sample sizes. Notably, restricting the sample to individuals observed for at least three waves has a minor impact on the final sample counts. My main findings, coherently, are unaltered by this choice (see Figure 1 in Appendix S1).

With the exception of the selection of suitable treatment and control groups, the most substantial drop in sample size is observed because of the use of fertility history files for the SOEP. For Germany, the SOEP started collecting men's fertility histories only from 2000 onward (Goebel, 2017). Respondents that dropped from the panel prior to that date are not included in the analyses. Nevertheless, replicating the analyses for Germany using fertility info from the core file (i.e., number of children in the household) does not alter any of the conclusions on the impact of fatherhood on wages for German men (see Section 6 in Appendix S1).

Measures

The outcome variable in this study is the log of real hourly wages. Hourly wages are computed by dividing gross monthly pay by the amount of weekly working hours multiplied by 4.35, the approximate number of weeks in a month. For BHPS, I sum weekly working hours and hours of overtime (Bryan & Sevilla-Sanz, 2011). For SOEP, I use actual working hours or, when missing, the sum of contractual working hours and of overtime (Kühhirt & Ludwig, 2012). As per Table 1, listwise deletion on missing wages only lead to marginal sample losses of 0.8% and 1.8% of the potential sample in the SOEP and BHPS, respectively.

Wages are then indexed at 2014 prices (2016 for the United Kingdom) and values below 1 or above 100 are trimmed, following standard practices in the literature (e.g., Kühhirt & Ludwig, 2012). Taking the natural logarithm of real wages then enables the interpretation of coefficients in terms of percentage effects on wage levels, as the log scale well approximates the percentage-point scale as long as coefficients lie in the $[-0.25, 0.25]$ interval.

I deploy two measures to single out the effects of the transition to fatherhood. The first is a simple dummy ($Child_{it}$) that equals 0 prior to the birth of the first child and 1 in its aftermath. In a second model specification, I consider the effect of the transition to fatherhood as distributed along the life course of men (e.g., Dougherty, 2006). Instead of a single dummy switching from 0 to 1 after the transition to fatherhood, I operationalize fatherhood via a series of dummies k for each year t since the first child's birth. Whenever relevant data were available (thus, with the exclusion of the UKHLS arm), both measures are corrected for the month in which the child is born. If the child's birth occurred in the interview year, but months prior to the interview, the relevant dummy is set to 0. Only if the interview occurred in the same month of birth or after, the relevant dummy is set to 1.

Focusing on the transition to fatherhood, and thus on the first parity, is consistent with part of the literature on family events and wages (e.g., Kleven et al., 2018; Ludwig & Brüderl, 2018). Many studies in the field have rather employed dummies for different parities (e.g., Petersen et al., 2011; Pollmann-Schult, 2011) or a single counter for the number of children (e.g., Cooke, 2014; Gangl & Ziefle, 2009). Yet the arguments developed in the previous sections deal, by and large, with the transition to fatherhood and not with the effect of specific (higher order) parities. It is posited, for example, that employers treat differently fathers and childless men rather than discriminate among fathers depending on their number of children. As for selection dynamics, the core contrast is between men who will eventually become fathers and men who will not.

Differently, it could be argued that the need for increasing work effort and for couple specialization might be more pressing after higher order births, as additional children demand higher income. Even if of sure interest, the effects of different parities might not be easily disentangled in the regression framework, considering that higher order births might be endogenous to previous births. Notably, one birth may causally affect the chances of a subsequent birth via its effect on a parent's labor market standing. If men, say, receive a wage premium after the birth of a first child, this might increase their chances of having a second one because they can afford a larger family size. If, in turn, the second birth propels men to specialize in paid work even more and get even higher wages, part of the effect of a first birth on wages will work through the second birth. Focusing on the transition to fatherhood, I therefore look for the total effect of fatherhood on wages: Such total effect includes wage responses to higher order parities yet avoids the empirical hurdles of disentangling separate effects for each endogenous parity.

Models throughout also include age and age squared to net out pure lifecycle effects. The results are unchanged when opting for different polynomial forms (quadratic, cubic, and quartic) or for a full set of dummies for age, potential labor market experience, and (for SOEP only, due to data availability) actual labor market experience. Period dummies are also included, grouping years in 3-year bans (with only two broader residual categories: 2011–2014 for SOEP, 2012–2016 for BHPS). I grouped together multiple years considering that interviews are carried out annually in one of the panels (SOEP). When applying the within-individual transformation in FE models, age and interview year thus increase of one unit each year creating collinearity between the two variables in the FE regression model. Grouping period dummies circumvents this issue. For consistency, I deploy 3-year dummies for the U.K. arm of the analysis as well, although BHPS interviews naturally span over multiple years.

For cross-cohort comparisons, I identify three birth cohorts, distinguishing men born between 1950 and 1959, 1960 and 1967, and between 1968 and 1976 (1974 for Germany). Such cut-off points ensure enough cell size in each group. I also follow previous literature on Germany, suggesting that the relationship between fatherhood and labor market participation (and thus, perhaps, wages) changed starting from men born in the 1960s (Pollmann-Schult & Reynolds, 2017). For the United Kingdom, I will not report estimates for the 1950 to 1959 British cohort because they would be based on relatively older men (aged 32 or older) experiencing the transition to fatherhood.

Fatherhood and Wages: Model Specifications

I start from a simple ordinary least squares (OLS) specification of the wage equation,

$$y_{it} = \alpha + \beta Child_{it} + \gamma_1 Age_{it} + \gamma_2 {Age_{it}}^2 + \varphi_t + \varepsilon_{it} \tag{1}$$

where y_{it} is the log of real hourly wages. Apart from age (Age_{it} and ${Age_{it}}^2$) and period (φ_t), no other variables are adjusted for on the right-hand side of the equation (e.g., Killewald & Lundberg, 2017; Loughran & Zissimopoulos, 2009). My goal here is to test whether data are compatible with a causal fatherhood wage bonus. Parsimony in model specification allows, first, to estimate such gross bonus if any. Second, parsimony shields estimates from the risk of overcontrol bias (Elwert & Winship, 2014), that is, of muting the effect fatherhood has on wages by accounting for the channels through which the effect could manifest in the first place. Adjusting for working hours, for example, could already account for part of the bonus in accordance with the work effort mechanism. I refrain from including this and similar variables in the analyses, as the question of whether data supports a causal fatherhood wage bonus predates asking what mediates the bonus if present.

Still, pooling together all observations as if they belonged to different units, the OLS model does not distinguish person-year records belonging to the same person from person-year records belonging to a different one. Estimates of β in Equation 1, therefore, simply contrast records in which the transition to fatherhood has occurred ($Child_{it} = 1$) to records in which the transition has not occurred ($Child_{it} = 0$). The latter group of observations includes both fathers-to-be prior to first childbirth and men belonging to the control group of childless men.

Differently, to focus on within-individual change, and thus on the transition to fatherhood, I contrast Equation 1 to the following FE specification:

$$y_{it} = \beta Child_{it} + \gamma_1 Age_{it} + \gamma_2 Age_{it}^{\ 2} + \varphi_t + \theta_i + \varepsilon_{it} \quad (2)$$

The estimation of β associated with $Child_{it}$ in Equation 2 rests only on within-unit variance because of the inclusion of individual FE θ_i. β can now be interpreted as the average one-off shift in wages that men experience when becoming fathers (for details on how this average is computed, see Borusyak & Jaravel, 2016; Imai & Kim, 2017). Furthermore, individual FE curb estimates of β from time-invariant sources of selection into fatherhood. The role and direction of such type of selection can be assessed in two ways. First, I contrast OLS and FE estimates of the β coefficient in both equations. Negative selection on time-invariant individual characteristics should be signaled by an increase in the magnitude of β in the FE model; positive selection, vice versa, by a decrease. Second, I look at the correlation coefficient $r(\theta_i, Child_{it})$, expressing the sign and magnitude of selection into fatherhood in terms of the correlation between wage-relevant time-constant unobservables θ_i and the variable for fatherhood, $Child_{it}$ (e.g., Gangl & Ziefle, 2009).

Although the model in Equation 2 nets out time-invariant sources of selection, it still makes a number of assumptions. One is that the change in wage levels come fatherhood can be expressed as a one-off change, summarized by a single coefficient β. Research on male marital wage premiums and motherhood wage penalties alike has now well established that family events impact wages in a dynamic fashion (e.g., Kleven et al., 2018; Korenman & Neumark, 1991; Loughran & Zissimopoulos, 2009). In other words, one is better off modeling parenthood effects year by year after the birth of a child, as short-, medium-, and long-run shifts in wage levels may differ. This first assumption is relaxed by deploying an event-study design (e.g., Kleven et al., 2018), including leads k of $Child_{it}$ as follows (see also Imai & Kim, 2017):

$$y_{it} = \sum_{k=0}^{k=10} \beta_k \cdot \mathbb{1}(k = t - Child^i) + \gamma_1 Age_{it} + \gamma_2 Age_{it}^{\ 2} + \varphi_t + \theta_i + \varepsilon_{it} \quad (3)$$

With $Child^i$ I now indicate the year in which the child's birth occurs for an individual i. Calendar time keeps being signaled by the subscript t. $\mathbb{1}(k = ...)$ is the indicator function whose argument can be either 1 or 0, that is, a (set of) dummy variable(s). Equation 3 then includes event-time dummies k for each year t since the year of first childbirth and up to the 10th after. I cap the last indicator variable ($k = 10$), coding 1 all years after the 10th since first childbirth. The results from Equation 3 are unaltered by this choice (see also Section 3 in Appendix S1; for cell sizes in each year k, see Table 1 in Appendix S1).

With Equation 3, I am thus able to capture the dynamic evolution of wages after fatherhood. Yet, prior to fatherhood, fathers to be and childless men could be on different wage growth paths. This would violate another assumption behind the FE estimator, namely, the parallel trend assumption for a treatment (here, $Child_{it}$). Equations 2 and 3 indeed require that outcomes evolve in parallel for controls and treated prior to the treatment (e.g., Brüderl & Ludwig, 2015; Wooldridge, 2010). In my case, childless men and fathers-to-be (prior to fatherhood onset) should experience similar wage growth. This might not be the case considering, for example, that much of selection into fatherhood operates through selection into a union (e.g., Trimarchi & Van Bavel, 2017) and men who enter a union are on steeper wage growth profiles compared with men who remain single (e.g., Ludwig & Brüderl, 2018). Given that entry into a union has little causal effect per se on men's wages (Killewald & Lundberg, 2017; Loughran & Zissimopoulos, 2009; Ludwig & Brüderl, 2018), there is little risk that fatherhood wage premiums are actually marital wage premiums. Adjusting estimates for men's entry into union is thus likely unnecessary to achieve credible causal inference on fatherhood and wages (cf. Ludwig & Brüderl, 2018; see also Figure 4 in Appendix S1). Rather, a process of selection on the basis of wage growth might underlie family formation (union entry and fatherhood), and failing to account for it may lead to incorrectly claim the existence of a positive wage boost brought about by fatherhood.

Hence, to complement the evidence obtained via the FE estimators in Equations 2 and 3, one can ask if any effect of fatherhood on wages can be detected once any individual trend in wage growth is netted out (Ludwig & Brüderl, 2018). A more restrictive specification of this kind fully accounts for the chance that fathers-to-be and childless men may be on different wage-growth paths. It could permit wage growth to depend on any (time-invariant) individual characteristic, as each man in the sample would basically have his own wage slope. A FEIS model (e.g., Brüderl & Ludwig, 2015) is thus fitted in several steps. First, one estimates separate OLS regressions for each individual i, regressing log-wages y_{it} on a constant and a linear term for age. Second, the predicted values for each individual regression are subtracted from y_{it}, thus obtaining wage values for each individual i that are both demeaned (constant term in Step 1) and detrended (linear term for age in Step 1). Third, all independent variables are similarly demeaned and detrended. Finally, one can run an OLS on the transformed data. All of these steps are automated in the STATA (StataCorp, College Station, TX) routine xtfeis (Brüderl & Ludwig, 2015), which I deploy. A more compact formulation of the model is as follows (Wooldridge, 2010, p. 377):

$$y_{it} = \beta Child_{it} + \gamma_2 Age_{it}{}^2 + \varphi_t + \mathbf{W}_{it}{}'\theta_i + \varepsilon_{it} \tag{4}$$

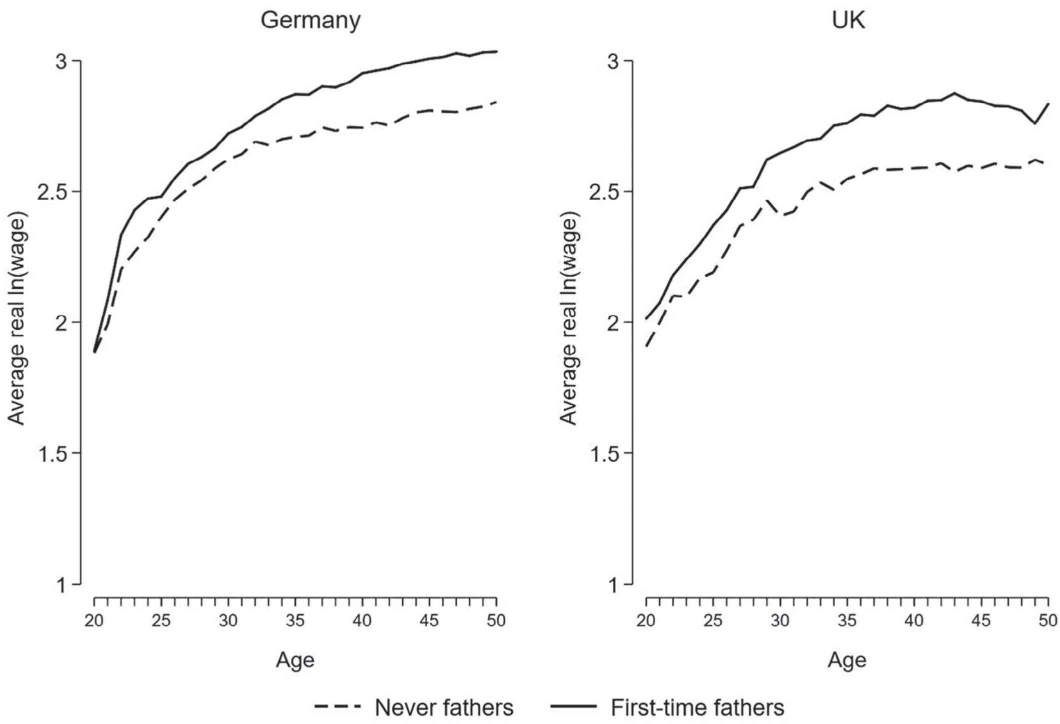
With respect to the FE specification in Equation 2, I allow now for the product of individual FE θ_i and some observable variable, namely, the linear term for age contained in the vector $\mathbf{W}_{it}{}'$. I ran such FEIS estimation both using the single dummy variable $Child_{it}$ and, separately, the event-study approach of Equation 3. Notably, given that FEIS estimation is based on an OLS model for each single individual (Step 1) including two parameters (the constant term plus a linear slope for age), at least three observations per person are needed, hence the aforementioned sample restriction (Ludwig & Brüderl, 2018). Throughout, standard errors are handled via the Huber-White estimator to account for the clustering of observations within each individual (via the option robust in STATA 14.0).

Findings

Pooling data from all the available waves, Figure 1 depicts the average wage levels from age 20 onward and separately for men who will eventually become fathers and for men who will not. Trends were similar in Germany and Britain. Wages at the mean grew at a faster pace for fathers relative to nonfathers, already since their late 20s and early 30s. After that, the gap somewhat widened. Such a pattern could be consistent with the existence of a (modest) fatherhood wage bonus, as wage trajectories diverged more markedly around the age of first-time fatherhood in both countries (McMunn et al., 2015; Struffolino et al., 2016). The picture could not be conclusive, however, on whether men experienced a wage premium come parenthood or some antecedent factors boosted the wages of fathers-to-be.

Turning to statistical models, OLS estimates in the first column of Table 2 and Table 3 reflect the patterns displayed in Figure 1. Fatherhood was associated with an average wage gain of about 14% in Germany ($p < .001$) and 17% in Britain ($p < .001$). In terms of magnitude, these estimates were compatible with those highlighted for the United States in previous studies (Hodges & Budig, 2010; Lundberg & Rose, 2000). Yet OLS regression pooled all observations in the panel and did not distinguish records belonging to the same individual from records belonging to a different individual. As a result, OLS estimates in the first column of both tables simply contrast person-year records of men once they have become fathers with person-year records of men who are not (or not yet) fathers. I thus quantified the differential already shown in Figure 1, but could not address the potential bias stemming from selection into fatherhood.

FIGURE 1. AVERAGE LOG HOURLY WAGES BY AGE OF THE RESPONDENT, SEPARATELY FOR CHILDLESS MEN AND FATHERS.



Sources: German Socio-Economic Panel 1984 to 2014, British Household Panel Survey 1991 to 2016.

Table 2. *OLS, FE, and FEIS Models for the Log of Real Hourly Wages: German Sample (German Socio-Economic Panel 1984–2014)*

	1 OLS	2 FE	3 FEIS
First-time father (ref. childless), β (SE)	0.142^{***} (0.005)	0.038^{***} (0.011)	−0.010 (0.010)
$r(\theta_i, Child_{it})$		0.13	
R^2 (within-R^2 in Columns 2 and 3)	0.14	0.23	0.07
Number of individuals	2,713	2,713	2,713
Number of person-years	34,925	34,925	34,925

Note: All models include period dummies and a quadratic for age. A linear term for age is allowed to vary across individuals in the FEIS specification. Standard errors are clustered at the individual level. FE = fixed effects; FEIS = fixed effects individual slope; OLS = ordinary least squares; ref. = reference. $^{*}p \leq .05$. $^{**}p \leq .01$. $^{***}p \leq .001$.

A step forward in this latter direction came from FE estimates in the second column of Tables 2 and 3. Such estimates focused on within-individual variation and therefore could be interpreted as the one-off shift in wage levels brought about, on average, by the transition to fatherhood. Netting time-invariant differences between men by means of individual FE, the estimates were reduced. The shift in wages brought about by fatherhood stopped at around 3.8% in Germany ($p = .001$). For British men, similarly, the fatherhood wage premium halted at 3% ($p = .028$). The decrease in size compared with OLS estimates was opposite to what previous studies have found for the United States. Selection into fatherhood in both Germany and Britain seemed to be positive on average, that is, high-earning men in both countries were

Table 3. *OLS, FE, and FEIS Models for the Log of Real Hourly Wages: British Sample (British Household Panel Survey 1991–2016)*

	1 OLS	2 FE	3 FEIS
First-time father (ref. childless), β (SE)	0.172^{***} (0.008)	0.030^{*} (0.014)	0.002 (0.013)
$r(\theta_i, Child_{it})$		0.17	
R^2 (within-R^2 in Columns 2 and 3)	0.11	0.22	0.09
Number of individuals	1,253	1,253	1,253
Number of person-years	15,470	15,470	15,470

Note: All models include period dummies and a quadratic for age. A linear term for age is allowed to vary across individuals in the FEIS specification. Standard errors are clustered at the individual level. FE = fixed effects; FEIS = fixed effects individual slope; OLS = ordinary least squares; ref. = reference. $^{*}p \leq .05$. $^{**}p \leq .01$. $^{***}p \leq .001$.

more likely to become fathers. At the bottom of both tables, correlations $r(\theta_i, Child_{it})$ further supported this conclusion. For both countries, I found small and positive correlations between time-invariant unobserved heterogeneity θ_i and my indicator variable for fatherhood. Unobserved, time-invariant factors relevant for wage determination were thus positively correlated with the transition to fatherhood.

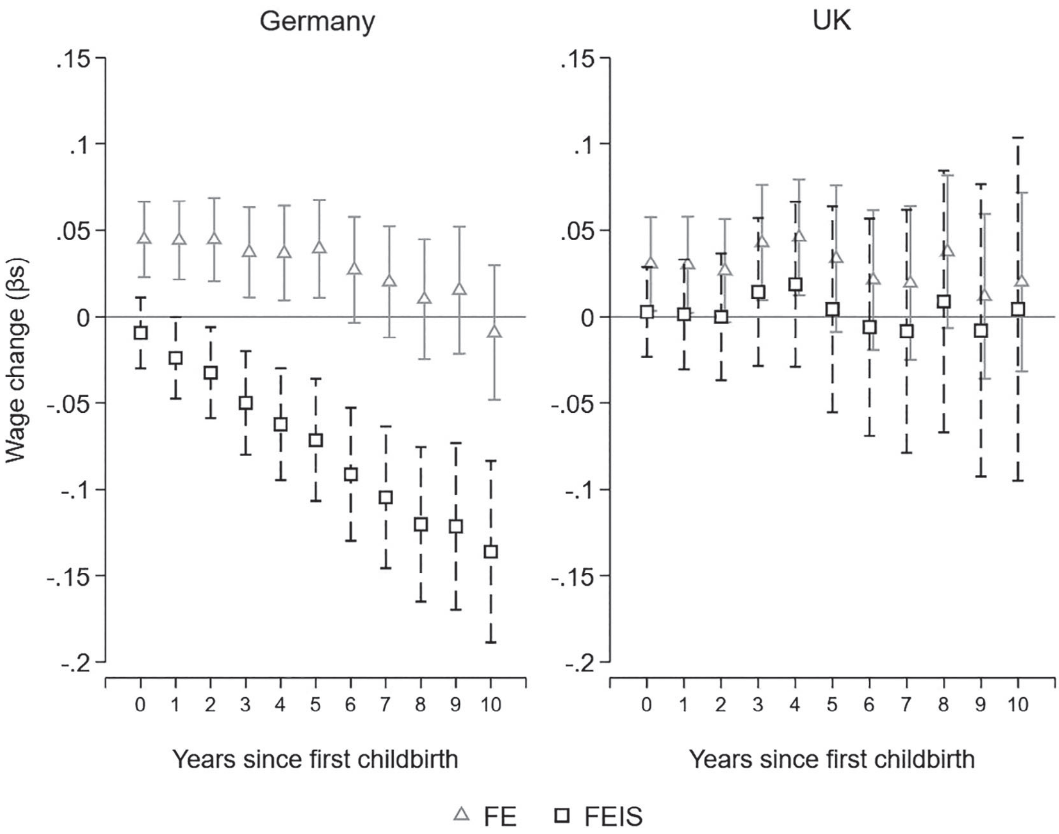
Even if netting out time-invariant heterogeneity between individuals substantially reduced the bonus, FE estimates could still be biased if the wages of men who eventually become fathers grew at a faster pace than those of their childless counterparts. In the third column of Tables 2 and 3, FEIS estimates addressed this by letting the wage-age profile vary between men. Now, including individual slopes, I could not reject the hypothesis that the effect fatherhood had on wages was actually nil for German men ($\beta = -.010$, $p = .301$). The same could be said for British men looking at FEIS estimates in Table 3 ($\beta = .002$, $p = .845$). Hence, once the possibility of divergent individual wage trajectories was accounted for, the evidence did not support a causal story for the fatherhood wage bonus (as for U.S. men in Ludwig & Brüderl, 2018).

So far, I have assumed that fatherhood may bring about a one-off shift in men's wage levels. Such an assumption could be unwarranted. For example, men could increase their work effort or specialize in paid work particularly in the first years following a child's birth, often compensating for mothers' work interruptions and subsequent income loss during that period. As many applied and methodological contributions have shown (e.g., Borusyak & Jaravel, 2016; Korenman & Neumark, 1991), a simple dummy for "before-after" the event of childbirth will not help retrieving more complex, dynamic effects. Figure 2 thus displays estimates from an event-study approach in which the wage response to fatherhood was singled out for each year after that of a child's birth—as per Equation 3. For German men, the figure supports the presence of a fatherhood wage bonus, especially in the short term. FE estimates in the first 5 years were all positive and around the size previously assessed with the single-dummy approach (≈4%). For British men, point estimates were in a similar range, being compatible with wage bonuses of around 2% to 4% points in the aftermath of fatherhood.

Yet, turning to FEIS estimates and thus accounting for idiosyncratic wage growth, the alleged premium reduced further. For Germany, in particular, estimates turned negative already in years 0 ($p = .373$), 1 ($p = .049$), and 2 ($p = .016$). In later years, the estimates were not just negative in sign but substantial in size. Yet, as the number of fathers observed in such later years shrunk, the precision of FEIS estimates in the long run became questionable with respect to both the sign of the point estimates and to the width of confidence intervals (see also Ludwig & Brüderl, 2018; Section 3 of Appendix S1 for sensitivity checks). It was therefore unwarranted to consider such estimates as evidence of some substantial long-run fatherhood wage penalty in Germany.

FEIS estimates for the United Kingdom were close to 0 in the first years after childbirth and quite noisy thereafter. For the aforementioned sensitivity of such long-run estimates, caution should be applied this time to the idea that fatherhood spurred substantial wage premiums for British fathers in the long run.

FIGURE 2. POINT ESTIMATES AND 95% CONFIDENCE INTERVALS FOR THE EVENT STUDY OF FATHERHOOD AND WAGES.
Note: FIXED EFFECTS (FE) ESTIMATES AND FIXED EFFECTS INDIVIDUAL-SLOPE (FEIS) ESTIMATES ON DISPLAY.



Sources: German Socio-Economic Panel 1984 to 2014, British Household Panel Survey 1991 to 2016.

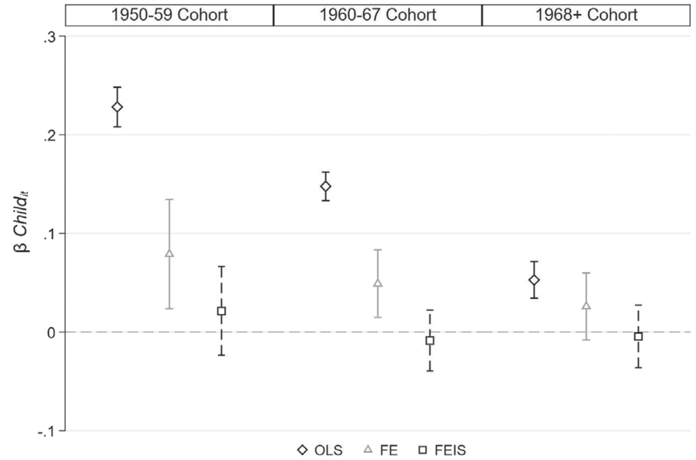
Hence, even if the wage trajectories of fathers and nonfathers depicted in Figure 2 differed markedly in both countries, there was no strong evidence to conclude that fatherhood boosted men's wages in Germany and the United Kingdom. Rather, the apparent wage premium could be traced back, by and large, to positive selection both on the basis of wage levels (static) and on the basis of wage growth (dynamic). Already prior to their child's birth, that is, fathers to be earned relatively higher wages and were on superior wage growth paths than their childless counterparts.

Heterogeneous Effects? A Comparison Across Cohorts

On balance, the evidence presented in the previous section could not support the idea of a causal fatherhood wage premium. Regardless of the estimation method though, all of my analyses tested for the presence of a premium for all men on average. In the remainder, I examine cohort differences: Fatherhood wage premiums might simply be a thing of the past, as the United Kingdom and Germany shifted away from a traditional male-breadwinner model in recent decades.

In Figure 3, I repeat the analyses for Germany separately by birth cohort. OLS estimates suggested that fatherhood was associated with higher wages for men, especially for those belonging to the older cohorts (up to around 23% for the 1950–1959 cohort). After the inclusion of individual FE, estimates were substantially reduced for the older cohorts, and they remained small but stationary for the youngest. Finally, looking at the FEIS specification, estimates

Figure 3. Point Estimates and 95% Confidence Intervals for the Coefficient of First-Time Fatherhood on Log Wages.



Note: Separate models for different birth cohorts. *Source*: German Socio-Economic Panel 1984 to 2014. FE = fixed effects; FEIS = fixed effects individual slope; OLS = ordinary least squares.

further reduced and were no longer compatible with the idea of a wage premium particularly in the youngest cohorts (for the 1950–1959 cohort, somewhat differently, $\beta Child_{\text{it}} = .02$, $p = .350$).

The results for Britain in Figure 4 exhibited similar patterns across model specifications. In the FEIS specification, in particular, fatherhood brought about a wage gain of around 2% for the 1960 to 1967 cohort ($p = .266$), whereas the point estimate for the youngest cohort was even negative ($-.004$, $p = .811$).

Given the partitioning of the sample in different cohorts, I refrain from presenting here the event-study part of my analyses for each separate group due to the small cell size. Nonetheless, event-study results (see Figures 5 and 6 in Appendix S1), with both FE and FEIS specifications, gave no strong indication of a fatherhood bonus across cohorts in the United Kingdom and Germany.

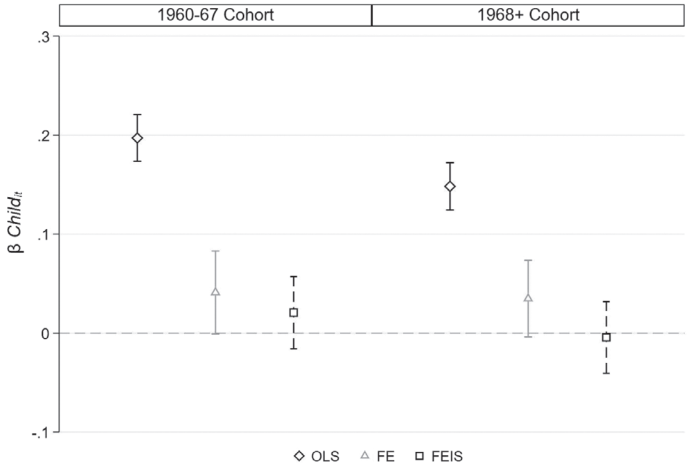
At best, small causal fatherhood premiums are indeed a thing of the past in Germany and the United Kingdom. Notably, cross-cohort analyses also revealed that selection into fatherhood on the basis of wage-relevant, time-invariant characteristics might have become less positive across subsequent cohorts. This was evidenced by the smaller and smaller contribution that including individual FE made to estimates of the premium across cohorts in Figures 3 and 4.

Discussion and Conclusions

In this study, I ask whether men get a wage premium when they become fathers in countries that have long supported male breadwinning. I propose that observed premiums could be a statistical artifact if men who become fathers have a higher earning potential and experience steeper wage growth than men that will eventually remain childless. By deploying several model specifications that variously account for such selection dynamics, I cannot reject the null of no effect of fatherhood on men's wages. What is more, across cohorts, both the size of premiums and the importance of selection into fatherhood appear to be fading in Germany and the United Kingdom. Over time, fatherhood may have thus become less of a marker of breadwinning in the labor market, and traits associated with breadwinning may have become less important for the transition to fatherhood in the marriage market.

Such findings stand in contrast with previous studies in a twofold sense. First, pertaining to selection into fatherhood, I find that British and German men positively select into parenthood on the basis of time-invariant unobserved factors. In the United States, such selection dynamic appears to be negative instead (Hodges & Budig, 2010; Killewald & Gough, 2013; Lundberg & Rose, 2000). The portion of

Figure 4. Point Estimates and 95% Confidence Intervals for the Coefficient of First-Time Fatherhood on Log Wages.



Note: Separate models for different birth cohorts. *Source*: British Household Panel Survey 1991 to 2016. FE = fixed effects; FEIS = fixed effects individual slope; OLS = ordinary least squares.

the premium due to positive selection also has been decreasing across cohorts in both Germany and the United Kingdom. This differs from findings from cross-cohort analyses carried out in Nordic countries, typically finding persistent or even increasing positive selection of men into parenthood on the basis of wage levels or on antecedents of earning power such as educational attainment (Hart, 2015; Jalovaara et al., 2018; Kravdal & Rindfuss, 2008). As the question of which men become fathers has broad implications—not limited to men's own wage attainment, but extending to income inequality across households or the intergenerational transmission of (dis)advantage (e.g., Huerta et al., 2014; McCall & Percheski, 2010)—this study motivates research on the demography of fatherhood and whether and how it is changing over time (see, e.g., Autor, Dom, & Hanson, 2017; Kearney & Wilson, 2018).

Second, differently from the U.S.-based literature, considering selection on both time-invariant unobserved factors and on prior wage growth, I cannot conclude that fatherhood sparks the wage attainment of men in the United Kingdom and Germany. As a consequence, highlighting the crucial role played by selection dynamics casts doubts on the credibility of estimates of fatherhood premiums derived by cross-sectional data, lest they will account for selection into fatherhood as well. So far largely confined to the role of unobservables, results in this study prompt further research on those features (personality, genes, noncognitive skills, beauty, etc.; see, e.g., Bowles, Gintis, & Osborne, 2001) that may matter for both wage attainment and the transition to fatherhood to be considered in a comparative and cross-cohort perspective.

Surely, the inability to reject the null in this study could reflect the fact that fatherhood indeed has a negligible (causal) effect on men's wages, but it may also stem from (a) lack of statistical power, (b) measurement error, and (c) deficiencies in the study design. Sample sizes in this study are highly comparable to previous longitudinal studies on fatherhood and wages (e.g., Killewald & Gough, 2013; Smith Koslowski, 2011). Yet, if true effects are small or very small I may have failed to detect them due to a lack of statistical power. Across both my main and subgroup analyses, at best fatherhood premiums amounted to 3% to 4%. If even smaller premiums exist and could not be detected here, however, it is worthwhile to question what their substantial significance would be, especially when contrasted, on the other hand, with the order of magnitude of motherhood wage penalties (e.g., Cools et al., 2017; Gangl & Ziefle, 2009; Harkness, 2016; Kleven et al., 2018).

Findings in this article may also be invalidated by measurement errors. Measurement error could affect the computation of log hourly wages as well as the construction of the key independent variable operationalizing the transition to fatherhood. For the former, I relied on well-established practices in the literature (e.g., Bryan & Sevilla-Sanz, 2011; Gangl & Ziefle, 2009; Kühhirt & Ludwig, 2012), and I can only note that my conclusions on fatherhood and wages are consistent with those of studies using perhaps more precise register data, albeit for different countries (Cools et al., 2017; Kleven et al., 2018). Furthermore, using fertility history files has enabled me to detect the precise timing of the transition to fatherhood, a crucial requirement for the event-study part of the analyses presented here.

Finally, even if the study design of this article has sought to curb estimates of the fatherhood bonus from multiple sources of selection bias, evidence from alternative causal designs could complement my findings. Examples of alternative designs could include quasi-experimental studies matching childless men and fathers on time-constant and, especially, time-varying confounders (e.g., for the male marital wage premium, Mincy, Hill, & Sinkewicz, 2009) or approaches based on instrumenting the transition to parenthood (e.g., Cools et al., 2017; Kleven et al., 2018). Experimental studies, so far failing to detect a preference for fathers in terms of hiring chances in field settings (Bygren et al., 2017; Correll et al., 2007), could also complement the evidence of this study by investigating employers' wage offers to prospective male employees, depending on parental status. In addition, this study has only focused on the transition to fatherhood and possible heterogeneities by cohort. Future research could deploy the same stringent tests to assess how men's wages respond to higher order parities and across other social groupings. Parity- and group-specific mechanisms should of course motivate these analyses.

All in all, the evidence amassed in this study cannot support the idea of a causal wage premium for fathers in Germany and the United Kingdom. One consequence is that as much as gender wage gaps in the two countries are driven by the transition to parenthood, the penalty for mothers rather than the premium for fathers really drives the gap in labor markets. The wage response to motherhood, however, has yet to be put to test using FEIS, accounting thereby for heterogeneous individual wage growth (but see Loughran & Zissimopoulos, 2009; Livermore, Rodgers, & Siminski, 2011). If, on average, mothers-to-be are on worse (better) wage growth paths than women who will not become mothers, motherhood penalties could have actually been overestimated (underestimated) in the literature so far. This study motivates future research into the reciprocal relationship between wages and motherhood, too.

Absent wage boosts for fathers, nonetheless, it appears wage losses for mothers will not be compensated for within the confines of households alone. Evidence on men's wages in this article thus indirectly calls for continued research on those policies that may mitigate the motherhood wage penalty and reduce gender economic inequalities more at large. Notably, my findings for Germany and the United Kingdom are consistent with those of similar event-study designs for Scandinavian countries (for Denmark, Kleven et al., 2018; for Norway, Cools et al., 2017), countries that have a much longer tradition of support for men's role as carers and women's role as earners. As increasingly inclusive care policies are implemented in former male-breadwinner societies, especially in the form of parental leaves, future research could evaluate their impact on men's labor market outcomes (e.g., for Scandinavia, Rege & Solli, 2013; Albrecht et al., 2015; Evertsson et al., 2016). The evidence in this study cannot support a causal story linking wages and fatherhood per se, after all.

Note

Thanks to Lynn Prince Cooke, Renske Keizer, Volker Ludwig, Josef Brüderl, and to the participants at the RC28 Spring Meeting 2019 for their comments and feedback. All errors and omissions are the author's own. Data from the British Household Panel Survey and U.K. Household Longitudinal Study were made available through the U.K. Data Archive (University of Essex, Institute for Social and Economic Research, 2018), whereas data from the German Socio-Economic Panel were made available by the German Institute for Economic Research, Berlin, 2016. Neither the original collectors of the data nor the archive bear any responsibility for the analyses or interpretations presented here.

Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Appendix S1. Supplementary information.

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