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Which Mothers Pay a Higher Price? Education Differences in Motherhood Wage Penalties by Parity and Fertility Timing

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Abstract: Upon becoming mothers, women often experience a wage decline—a "motherhood wage penalty." Recent scholarship suggests the penalty's magnitude differs by educational attainment. Yet education is also predictive of when women have children and how many they have, which can affect the wage penalty's size too. Using fixed-effects models and data from the National Longitudinal Survey of Youth 1979, I estimate heterogeneous effects of motherhood by parity and by age at births, considering how these relationships differ by education. For college graduates, first births were associated with a small wage penalty overall, but the penalty was larger for earlier first births and declined with higher ages at first birth. Women who delayed fertility until their mid-30s reaped a premium. Second and third births were associated with wage penalties. Less educated women instead faced a wage penalty at all births and delaying fertility did not minimize the penalty.

Keywords: motherhood wage penalty; parity; fertility timing; education; NLSY79

Upon becoming mothers, women often experience a decline in wages, commonly known as the motherhood wage penalty, a phenomenon that has sparked considerable interest in both popular discourse and academic literature (England et al. 2016; Gough and Noonan 2013; Miller 2017, 2018; Wade 2017; Yu and Kuo 2017). Although earlier research has considered the motherhood wage penalty in the aggregate (Avellar and Smock 2003; Budig and England 2001), recent scholarship has demonstrated that the magnitude of the penalty received varies across women (Budig and Hodges 2010; England et al. 2016; Yu and Kuo 2017). Some research points to differences by education, although whether penalties are higher for more or less educated women remains contested (for reviews, see Gough and Noonan [2013] and Wilde, Batchelder, and Ellwood [2010]).

In addition to its relationship with motherhood effects on wages, women's educational attainment is also associated with their fertility schedules. Since the 1970s, college-educated women have been substantially delaying having children, whereas less educated women have experienced less change, leaving recent cohorts with significant education differences in the timing of childbearing (Cherlin, Talbert, and Yasutake 2014; Isen and Stevenson 2010; McLanahan 2004). Not only do women with less than a college degree have children earlier but they also have more children by the end of their childbearing years than do college-educated women (Isen and Stevenson 2010; Musick et al. 2009). Because fertility patterns are distinct by education, women with and without a college degree may have notably different work–family experiences that develop across their childbearing years.

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Given large education differences in fertility schedules, it is possible that timing and parity play a role in driving the heterogeneity in motherhood effects on wages by education. A number of studies have found that delayed childbearing is associated with lower motherhood penalties and that the benefits of delay are especially pronounced for women with at least a college degree (Amuedo-Dorantes and Kimmel 2005; Herr 2016; Miller 2011; Taniguchi 1999). Higher-order births also compound motherhood's costs, so if women with more education have fewer children, they may avoid these additional penalties (Kahn, García-Manglano, and Bianchi 2014; Sigle-Rushton and Waldfogel 2007). The combination of these documented patterns would suggest that because highly educated women are more likely to delay fertility and have fewer children, they will have lower wage penalties on average, which could contribute to inequality across women and their families. Still, past work has not empirically tested the interplay of the timing of each birth and parity of births and how this varies by educational attainment. If this differential parity and timing by education is related to differential wage effects, these diverging fertility patterns may be an overlooked but contributing factor to socioeconomic inequality and its rise in recent decades.

To what extent are the timing and parity of births associated with variation in the motherhood wage penalty—the within-person difference in wages in years prior to and after becoming a mother—and to what degree does this differ by women's education? Using fixed-effects models and data from the National Longitudinal Survey of Youth 1979 (NLSY79), I estimate heterogeneous effects of motherhood by parity and the age at first (and later) births, considering how these effects differ for college-educated and non–college-educated women. Estimating a single effect of motherhood by education, as is common in the literature, hides this potential variation by fertility schedules even when controlling for these characteristics. In fact, it may even offset effects running in opposite directions for different groups.

I build on past literature by theoretically and empirically disentangling how parity and timing shape differences in motherhood wage penalties for women with versus without a college degree. First, I consider education differences in the estimate of this aggregate motherhood wage penalty that does not give attention to parity or timing. Second, I draw attention to parity, its association with motherhood wage penalties, and how this may vary by education. Third, I account for fertility timing and how each parity transition's timing may shape its effect on wages for women in each education group. Finally, I consider whether and how family and work characteristics mediate these relationships. As I discuss below, family and work factors are often used to explain these education differences, but to what extent do they account for the change in wages associated with motherhood, parity transitions, and fertility timing? By understanding the way differential fertility schedules are related to the magnitude of motherhood wage penalties by education, we gain greater insight as to whether differences in timing and parity narrow or widen wage inequality between college-educated and non–college-educated mothers.

Education and Motherhood Wage Penalties

Women with and without a college degree are systematically different on a wide range of characteristics, including expected wage trajectories, levels of various forms of capital, and views toward work and family, all of which suggest that motherhood will differentially affect their wages (DiPrete and Buchmann 2006; Edin and Kefalas 2005; Hochschild and Machung 1989; Williams and Boushey 2010). However, it is unclear which group will experience larger penalties, given the nature of these differences and the possibility that both groups may face some advantages and other disadvantages that may counterbalance each other (Gough and Noonan 2013; Wilde et al. 2010).

One possibility is that women with a college degree face smaller effects of motherhood on wages because they can balance work and family through mechanisms unavailable to less educated women. In the workplace, college-educated women's level of job autonomy tends to be higher, which is associated with a lower penalty (Yu and Kuo 2017). Autonomy is associated with more flexible schedules compared to women with more team-based work environments, making these jobs more compatible with motherhood (Goldin 2014). Highly educated women may also have more job options and thus potentially more family friendly but high-wage choices (Amuedo-Dorantes and Kimmel 2005; Hewlett 2007). In the home, highly educated women are not only more likely to be married than less educated women, but their husbands are also more likely to be highly educated and have higher incomes (Isen and Stevenson 2010; Schwartz and Mare 2005). Highly educated women are thus more likely to have the resources to outsource childcare and housework, either via their own earnings or higher overall household income (Gonalons-Pons 2015; Gupta 2007; Hochschild and Machung 1989).

Consistent with this theoretical perspective, some empirical evidence using data on a cohort of women born between 1943 and 1953 (National Longitudinal Survey of Young Women 1968 [NLS-YW]) has supported a smaller penalty for highly educated mothers (Taniguchi 1999). Other research has even identified a wage premium for highly educated mothers in a cohort of women born between 1957 and 1964 (NLSY79) (Amuedo-Dorantes and Kimmel 2005).

By contrast, there are also reasons to expect that highly educated women may have larger wage penalties than less educated women. The professional and managerial jobs that these women occupy may require a high level of work commitment, so they may be less time flexible than other jobs and more difficult to combine with motherhood (Gough and Noonan 2013; Stone 2007; Wilde et al. 2010; Williams 2010). Many academic studies and popular press accounts have stressed the difficulty of combining motherhood with professional jobs (Belkin 2003; Hewlett 2007; Hochschild and Machung 1989; Slaughter 2015; Stone 2007). Additionally, employers may be more reluctant to hire or promote mothers to positions expecting high levels of work commitment (Gough and Noonan 2013; Wilde et al. 2010).

Finally, because college-educated women are more likely to be married to men with high-status jobs, marriage to these men may instead have the opposite effect on labor force participation. Instead of allowing them to ease work–family conflict through outsourcing, these women may leave the labor force or move into lower-wage or part-time work to compensate for their spouse's overwork or because they can afford to stay home (Cha 2010; Cotter, England, and Hermsen 2007). Both shifting to part time and temporarily leaving the labor force decrease cumulative work experience, which is associated with lower wages upon returning to work. Supporting this perspective, there is also evidence of larger penalties for highly educated women in both the NLS-YW and the NLSY79 cohorts (Waldfogel 1997; Wilde et al. 2010). Given these conflicting findings, I investigate whether wage penalties are larger for highly educated mothers compared to less educated mothers, or vice versa, and whether these work and family characteristics account for the penalty's magnitude.

Parity and Motherhood Wage Penalties by Education

Women with less than a college degree are more likely to have more children than women who complete college, but are there effects of higher-order births on wages and are the wage effects of multiple children the same for all women? Having more than one child adds another dimension of complexity to being a working mother. If motherhood's relationship with wages varies by education, parity will likely also be differentially related to the magnitude of motherhood effects by education (Abendroth, Huffman, and Treas 2014; Kahn et al. 2014). Given education differences in the ways that work and family tensions manifest themselves and the mechanisms through which women can alleviate these tensions (Williams and Boushey 2010), the effects of parity may vary by education.

Conventional wisdom and some qualitative empirical studies suggest that second children may shift the balance of work and family more drastically than first births (Hewlett 2007; Hirshman 2006; Stone 2007), a phenomenon that has been referred to as "second child syndrome" (Abrams 2001; Slaughter 2015). Typically, this argument is made about highly educated women, suggesting that women with more education can more easily adjust to being working mothers at the first birth and would thus see smaller initial motherhood penalties.

Factors including heightened discrimination from employers and the need for further workplace flexibility to accommodate multiple children's needs and schedules may lead highly educated mothers to experience a larger wage penalty at the second child than at the first, possibly due to the need to make job changes or denial of some opportunities for career advancement (Abrams 2001; Slaughter 2015; Stone 2007). These accounts suggest that, by comparison, less educated mothers may be more likely to face these setbacks upon their first birth, given that they already needed to make these more drastic work–family shifts upon becoming mothers due to less accommodating or flexible workplaces and fewer resources for childcare (Williams and Boushey 2010). Although Doren (2019) found evidence against this hypothesis for the first labor force exit, especially for mothers with a college degree, it remains plausible that those who remained in the labor force after their first birth may have been more likely to experience setbacks in wages after their second child than their first because of changes in work behaviors or job characteristics. In this article, I address whether the magnitude of wage penalties associated with each parity transition vary by women's educational attainment.

Fertility Timing and Motherhood Wage Penalties by Education

College-educated women have children later than women with less education, but does this affect the magnitude of the wage penalties they experience? Delayed fertility is associated with smaller negative effects of motherhood on wages because women are able to develop their careers prior to facing the work–family conflicts associated with motherhood (Amuedo-Dorantes and Kimmel 2005; Miller 2011; Taniguchi 1999). The few studies examining the role of timing among nonteenage mothers typically suggest smaller wage penalties for women who delay fertility until their late 20s or early 30s (Amuedo-Dorantes and Kimmel 2005; Gough 2017; Gough and Noonan 2013; Miller 2011; Taniguchi 1999). However, they often use a single dummy variable for "delay" based on a cutoff age, for instance, delaying motherhood until after 30 (Amuedo-Dorantes and Kimmel 2005) rather than looking at effects across the distribution of ages at first birth. It is therefore unclear at what age the benefits of delay begin to appear or whether the benefits increase with further delays.

Previous research also suggests that less educated mothers may not benefit from delaying fertility to the same degree as college-educated mothers (Amuedo-Dorantes and Kimmel 2005; Miller 2011; Wilde et al. 2010). If there is little room for wage growth, delaying fertility may be less beneficial. Accordingly, I ask whether the magnitude of the motherhood penalty declines with age at first birth and, if so, whether this is only true for college-educated women.

Data and Measures

I use data from the NLSY79 to estimate motherhood wage penalties—the change in women's wages prior to and after having children. NSLY79 is ideal for this analysis because it not only provides comprehensive longitudinal data for women who have completed their fertility but it is also used in many studies estimating motherhood effects and can thus be compared with past research (Amuedo-Dorantes and Kimmel 2005; Avellar and Smock 2003; Budig and England 2001; England et al. 2016; Miller 2011). Given my focus on timing and parity, observing women throughout the full span of their childbearing years is imperative. Although this cohort of late baby boomers may not reflect the experiences of today's women of childbearing age, these data are the most recent available where women have completed their fertility.

NLSY79 respondents were born between 1957 and 1963, and they were first interviewed in 1979 when they were 14 to 22 years old. Interviews were conducted annually from 1979 to 1994 and biennially thereafter. This article uses data from 1979 through 2014. These data are well suited for the current project given the frequent, detailed collection of data on work, family, and fertility during women's childbearing years. My analysis includes observations from female respondents from the nonsupplemental sample; supplemental samples were not interviewed for the entire follow-up period and were therefore unlikely to have completed their childbearing while in the sample. The full nonsupplemental sample consists of 3,108 women across 80,808 person–years. I drop the 2,219 person–years observed prior to age 18, when wages were first measured. My analyses also exclude the 559 women across 14,151 eligible person–years who had their first child prior to age 20. Teen births, however, were not atypical in this cohort, especially among less educated women (for which they represent 28 percent of births compared to 6 percent for women who get college degrees), and thus my sample is not fully representative of all births especially for less educated women. Still, because the estimate of the motherhood penalty targets within-person change in wages before and after having children, it would be theoretically and empirically impossible to estimate the motherhood effect for those who do not work (or earn wages in a meaningful capacity in relation to the later wage trajectory) prior to having children. My sample is therefore representative of women in this cohort who had their first birth at age 20 or later and worked in at least two years. In addition to these restrictions, 28,623 person–years with missing information are dropped, the overwhelming majority (88 percent of missing person–years) of which are missing on the outcome variable, log wages, thus rendering multiple imputation inappropriate. Applying the NLSY79 baseline weights, the final analytic sample consists of 2,377 female respondents across 35,815 person–years.

As noted, the outcome variable is an annual measure of the natural log of women's hourly wages, as is standard in the literature (Budig and England 2001; England et al. 2016; Kahn et al. 2014).[1] By taking the log, I can discuss wage penalties in terms of the percent change, easing comparison across education groups with different wage distributions in which a dollar change is unlikely to have a comparable meaning for both. However, it necessarily eliminates the 21 percent of person–years with nonmissing wages where women report zero wages in a given year from my analysis, so my findings are specific to women in the labor force, which is also standard in the literature (Avellar and Smock 2003; Budig and England 2001; Yu and Kuo 2017).[2] If women leave the labor force, they are excluded in that year and in subsequent years they do not work, but they re-enter the sample if they return to work. Alternatively, if women only work prior to or after becoming mothers or making a particular parity transition, their motherhood effect cannot be estimated because of the lack of variation in the motherhood or parity variables in years with an available outcome variable. A limitation of this method is that it is necessarily unable to quantify the consequences of leaving the labor force unless women re-enter. If they do re-enter and their lost experience has a negative effect on wages, this penalty will be picked up in their lower endowment of cumulative experience. In a sensitivity analysis, I test whether using predicted wages for observations in which women are out of the labor force rather than excluding those observations affects my findings. Under this specification, selection out of the labor force would no longer affect selection into my sample. However, results are similar to those of the main analysis (see Table 2 of the online supplement).

I measure a woman's educational attainment by whether she ever reported having earned a bachelor's degree or more.[3] Because I aim to classify women into groups, this variable is time invariant. Another possible operationalization would be to classify women based on educational attainment at the time of their first birth, but this would necessarily exclude women who did not have children. To ensure that the inclusion of women who finished their degree after becoming mothers does not provide misleading estimates, I ran a sensitivity analysis excluding women who had children prior to completing college but who ultimately finished the degree. Results are comparable to my main analyses (results available upon request).

Further motivating my classification of women based on their ultimate educational attainment, education is associated with a host of characteristics, including views toward work and family as well as cultural, social, or human capital. In addition, it likely reflects selection on a range of skills, cognitive and noncognitive, as well as the career path a woman aspires to follow in the long term. Many of these traits are underlying factors that do not necessarily appear upon women's completion of their education, but their presence throughout her life is signaled by the level of education she ultimately attains. It is these underlying characteristics selecting women into educational attainment that I aim to pick up by classifying women based on whether they ever completed college.

Still, educational attainment in a given year has ties to women's wages in that year, independent of her ultimate educational attainment. I therefore also include a time-variant control for years of schooling, which accounts for how changes in women's education may affect wages, consistent with past literature (Budig and England 2001; Kahn et al. 2014).

Motherhood is measured with a time-variant dummy variable for whether women have had a child by the year of observation.[4] Current parity is measured with three time-variant dummy variables for whether women have had one child, two children, or three (or more) children by that year. Parity is defined based on the number of live births a woman has ever had. I treat twins as two births that occurred in the same year.[5]

For estimating heterogeneous motherhood effects by timing of births, I categorize birth timing into approximately five-year intervals for each parity transition. Given my restriction of first birth timing to 20 or older and the biological end of women's childbearing years around age 50, I group women's age at each birth into five age groups: 20 to 22, 23 to 27, 28 to 32, 33 to 37, and 38+.[6] These variables are a time-variant series of dummies as well, which I define more technically in the Methods section. The narrow interval from 20 to 22 makes it possible to discern variation in motherhood status across age groups even for young births while still separately estimating effects for young mothers. The wider grouping at the upper tail end provides larger cell sizes given the rate of fewer births at these ages. However, for all birth orders and for both education groups, the vast majority of births are concentrated between ages 38 and 41 and are clustered at the younger side of the age range. Substantive results are consistent when ages at birth are instead pooled into 2-year ranges (results available upon request).

The model also includes a set of time-varying controls that may confound some of the effects of motherhood and parity transitions. A series of dummy variables for current age accounts for age patterns of wages. Results are robust to alternate specifications, including the natural log of age and a quadratic age specification. The series of dummy variables had the best model fit, according to Akaike information criterion (AIC) statistics. As noted above, I control for current years of educational attainment. Finally, I include a set of dummy variables for survey year.

I also estimate effects of motherhood net of important time-variant family and work characteristics to test mediation, given that many of the explanations for differential motherhood penalties center on these factors. Additionally, the variables I include mirror past literature, increasing the comparability between my findings and past work (Avellar and Smock 2003; Gough and Noonan 2013; Kahn et al. 2014). For family characteristics, I include current marital status: never married, currently married, previously married (divorced, separated, or widowed). I also account for spouse's earnings, which is set to zero for unmarried women and women whose spouse did not work. I track spacing of children using a counter of the number of years since each birth. For work characteristics, I include a set of dummy variables for whether women worked part time ($<$1,750 hours in that calendar year; 35 hours per week $\times$ 50 weeks per year), full time (35 to 60 hours per week), or "overworked" ($>$60 hours per week) in each year, following Cha (2010). I measure tenure in a job as number of years (or fractions thereof) women have worked with a given employer. I measure experience based on women's cumulative work hours and also include experience squared. Finally, I include occupation, which I grouped into five categories: professional, managerial, and technical; sales and office; service; farm, production, and craft; operation. Results from these models provide some insight as to why these patterns are observed and whether mediating factors are similar by education.

Methods

Following past research, I estimate the effects of motherhood on women's wages using fixed-effects regression models (Amuedo-Dorantes and Kimmel 2005; Avellar and Smock 2003; England et al. 2016; Kahn et al. 2014). A complication of estimating motherhood effects is selection into motherhood by unobservable characteristics. Because fixed-effects models estimate the difference in a woman's wages between the years before and the years after a change in motherhood status, they difference out these unobserved selection characteristics that remain constant within each woman over time, making these models well suited for this type of analysis. Selection into parity and timing of childbearing are strong as well, making this analytic strategy of estimating heterogeneous effects of motherhood by parity and timing especially appropriate here. Still, it cannot produce a truly causal effect of births, given that many unobserved selection characteristics can and do change over time, including the desire to have a child or achieve a certain family size. Additionally, I only observe outcomes for women who do, either by choice or by circumstance, become mothers, have a certain number of children, or have children at certain ages. The estimates therefore do eliminate unchanging selection characteristics over time, but they do not produce explicit causal effects as would an experimental design that randomly assigned parity transitions and birth timings to women.

To assess the variation in wage changes associated with motherhood, I estimate wage penalties at the initial transition to motherhood while also allowing for heterogeneous effects by parity and timing of each birth by education. To capture differences by educational attainment, I stratify models by a dummy variable measuring whether women ever attained a bachelor's degree. I begin by examining only the effect of the transition to motherhood with model 1, which estimates the effects of change in motherhood status (the transition from zero to one child) on log wages within individuals. This baseline model, which I estimate separately by education (less than college versus college or more), is as follows:

$$\ln(wage)_{it} = \beta_0 + \Sigma\beta(mom_{it}) + \Sigma\beta(age_{it}) + \Sigma\beta X_{it} + \alpha_i + u_{it} \tag{1}$$

where i represents individuals, and t represents age in one-year increments. Mom_{it} is a time-variant dummy variable that equals zero before mother's first birth and one when mothers reach the age at which they have their first birth. Age_{it} is a series of time-variant dummy variables capturing a woman's current age at each observation to account for the age-patterning and nonlinearities in life-course wages. X is a vector of time-varying controls for years of education and current year. In my second set of models, which assess mediation, this vector first includes all family characteristics and then all family and work characteristics, as described above. α_i represents individual fixed effects, and u_{it} represents the person–period error term.

Next, I consider variation in motherhood effects by parity, stratified by education. Model 2 estimates education-specific effects of changes in parity for women's first three births in the same framework (the transition from zero to one on a dummy variable for each parity), estimating the effects of parity transitions separately for the first, second, and third children:

$$\ln(wage)_{it} = \beta_0 + \Sigma\beta_p(mom_{itp}) + \Sigma\beta(age_{it}) + \Sigma\beta X_{it} + \alpha_i + u_{it} \tag{2}$$

where p represents parity (first child, second child, or third child). Mom_{itp} is a series of parity-specific time-variant dummy variables (mom_{it1}, mom_{it2}, mom_{it3}) that have values of 0 before women have made a particular parity transition and values of 1 thereafter. If a woman never makes that parity transition, her value on that variable will be time-invariant and will thus not contribute to the estimation of the coefficient for that parity transition in the fixed-effects model. Note that because effects reflect within-person change, the comparison group is an individual in their own past, not a different group of women.

I then incorporate timing of births, estimating effects of parity transitions separately by age at each parity transition, which allows for heterogeneous effects by age at first and higher-order births:

$$\ln(wage)_{it} = \beta_0 + \Sigma\beta_{pb}(mom_{itpb}) + \Sigma\beta(age_{it}) + \Sigma\beta X_{it} + \alpha_i + u_{it} \tag{3}$$

where b represents age group at p birth. Age groups b are grouped into approximately five-year intervals, but values on t still increase in one-year increments. As with the previous models, I estimate this separately for women who do and do not earn a bachelor's degree. Mom_{itpb} is thus a series of age-at-birth–specific time-variant dummy variables for each five-year age group (e.g., for second births: $mom_{it2,20}$, $mom_{it2,25}$, $\ldots$, $mom_{it2,35}$, $mom_{it2,40}$) and estimates the fixed effects of motherhood at specific age groups at each parity transition. For instance, if a woman had her first birth at 25, $mom_{it1,25}$ will have values of 0 at ages below 25 and values of 1 at ages 25 and higher, signaling the woman's transition to first birth. She will have values of zero at all ages on $mom_{it1,20}$ and $mom_{it1,30}$ to $mom_{it1,40}$, which represent women who have their first births in those age groups, and she will not contribute to the estimation on those coefficients in the fixed-effects model because of the lack of variation. A woman whose first birth was at 30 will likewise contribute only to the estimate of the $mom_{it1,30}$ coefficient, and her values on that variable will switch from 0 to 1 at age 30, when she makes the transition. Again, because these are within-person estimates, the comparison group is not a different group of women but individuals' past selves.

Results

Descriptive Results

Figure 1 shows age-specific rates of first, second, and third births for women without a college degree and with a college degree or more, displaying the expected group variation in women's fertility timing and parity. Although women with less than a college degree were more likely to have children in their early 20s, women with a college degree or more had children in their late 20s and throughout their 30s. These trends show that women with and without a college degree had children at very different ages. Across parities, the patterns were predictably consistent, all peaking earlier and having more right-skewed distribution for less educated women. As Table 1 shows, 79.4 percent of women without a college degree had children by the end of their childbearing years, whereas the same was true for only 70.3 percent of women with a college degree or more. Second and third births were also slightly more common among less educated women than among women with college degrees—58.4 percent of less educated mothers had second births and 21.7 percent had third births, compared to 54.4 percent and 19.4 percent of college-educated women, respectively. Table 1 displays descriptive statistics by educational attainment for the full set of variables.

Multivariate Results

Table 2 presents motherhood penalties by education from the regression models estimated with Equation 1 through 3. Model 1 estimates a single coefficient for motherhood on log wages. Differences by education are determined based on the significance of the interaction of bachelor's degree or more with each motherhood variable in a fully interacted model. For women with less than a college degree, the transition to motherhood was associated with about an 11.3 percent wage penalty ($p < 0.001$). Women with a college degree or more experienced a penalty of about half the magnitude, about 4.8 percent ($p < 0.10$). The difference in these estimates by education is statistically significant only at the 10 percent level, but the difference in magnitude suggests that the transition to motherhood may have represented a smaller shift in women's wage trajectories for bachelor's degree

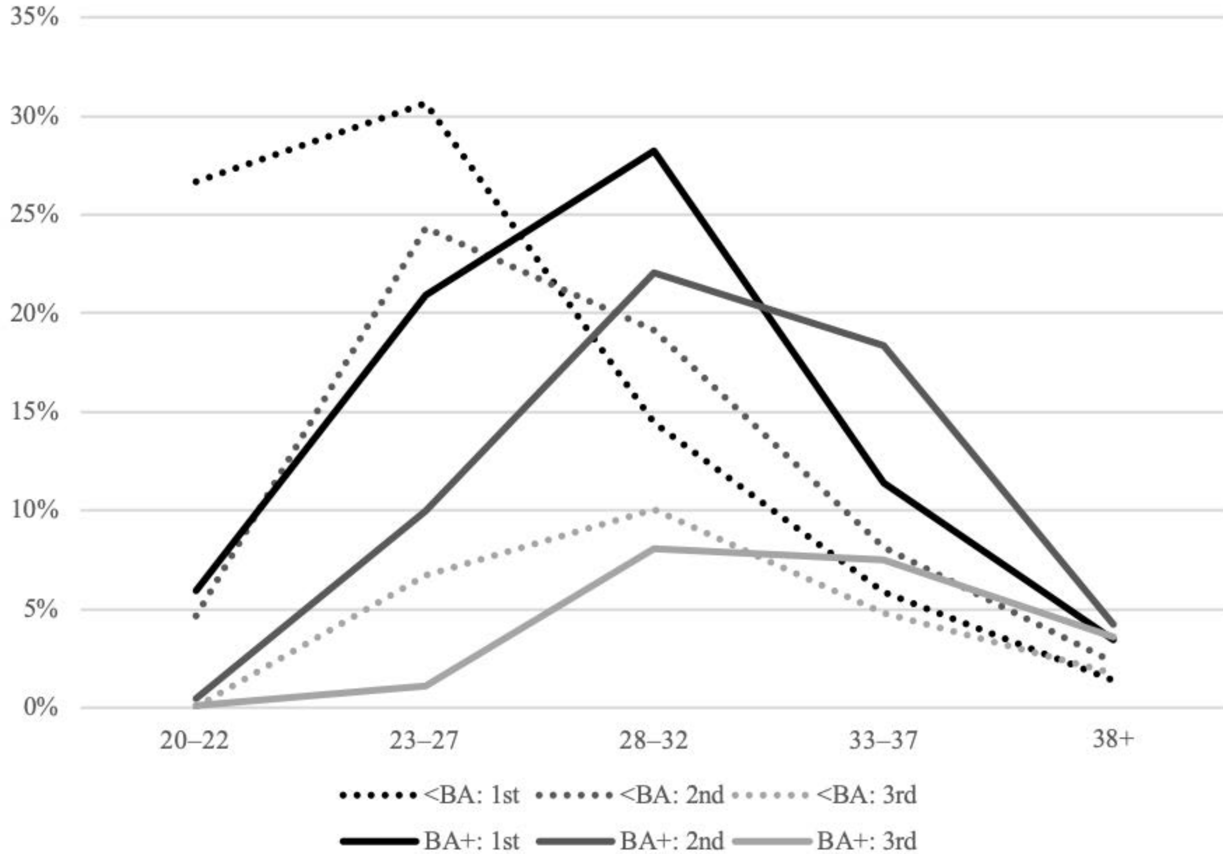

Figure 1: Percentage of women with first, second, and third births in each age range, by education. Source: National Longitudinal Survey of Youth 1979, 1979–2014. Weighted using baseline weights. BA, bachelor's degree.

holders. This finding provides some evidence that women with more education will have penalties of a smaller magnitude.

Model 2 estimates separate coefficients on motherhood by parity for women's first through third births. Women with less than a bachelor's degree experienced about a 10.1 percent wage penalty at the initial transition to motherhood ($p < 0.001$). Having a second child was associated with a 6.3 percent penalty ($p < 0.01$); having a third was associated with a 9.7 percent penalty ($p < 0.01$). Women who earn a bachelor's degree, on the other hand, had more variation across parity transitions, which was hidden in the aggregate estimates from model 1. Unlike the initial model, model 2 shows that the transition to motherhood had a significantly different association with wages for college graduates as compared to the wage penalty experienced by less educated mothers ($p < 0.01$). Controlling for later births revealed a 0.8 percent wage premium with the first birth for college-educated mothers, an effect of small magnitude that was not statistically significant. Later births, however, were associated with wage penalties of more substantial magnitudes. Although neither of these estimates differ at the 5 percent level from those for less educated mothers,

Table 1: Descriptive statistics

	<BA Mean or percentage	SD	BA+ Mean or percentage	SD
Log of Annual Hourly Wage	2.5	0.8	2.9	0.8
Parity				
Percent with first birth ever	79.4%		70.3%	
Percent with second birth ever	58.4%		54.4%	
Percent with third birth ever	21.7%		19.4%	
Birth Timing				
Age at first birth (for women with first births)				
20–22	33.7%		8.5%	
23–27	38.7%		29.9%	
28–32	18.3%		40.4%	
33–37	7.5%		16.3%	
38+	1.8%		5.0%	
Age at second birth (for women with second births)				
20–22	7.9%		0.9%	
23–27	41.5%		18.1%	
28–32	32.7%		40.0%	
33–37	13.9%		33.3%	
38+	3.9%		7.7%	
Age at third birth (for women with third births)				
20–22	0.2%		0.6%	
23–27	28.9%		5.4%	
28–32	42.9%		39.6%	
33–37	20.6%		36.7%	
38+	7.4%		17.6%	
Family Characteristics				
Years since each birth (zero if never had that birth)				
Years since first birth	8.0	9.5	5.0	8.0
Years since second birth	4.8	7.9	3.1	6.5
Years since third birth	1.5	4.9	1.0	3.8
Marital Status				
Never married	26.8%		37.4%	
Currently married	55.2%		52.2%	
Divorced, widowed, or separated	18.1%		10.4%	
Spouse earnings, if married (in $1,000s)	47.1	38.7	67.6	58.9
Work Characteristics				
Occupation group				
Professional, managerial, and technical	22.7%		59.9%	
Sales and office	44.5%		28.4%	
Service	20.5%		8.9%	
Farm, production, and craft	4.5%		1.1%	
Operation	7.9%		1.7%	
Degree of labor force participation				
Part time (<35 hours/week)	38.2%		38.7%	
Full time (35–60 hours/week)	59.1%		57.5%	
Overwork (>60 hours/week)	2.8%		3.8%	
Tenure at current job in years	4.9	5.8	4.6	5.6
Years of experience in full-time year equivalents	12.4	9.6	12.4	9.9
Years of education	12.5	1.2	16	1.7
Age	33.6	9.6	33.5	9.6
Respondents	1,603		774	
Person–years	22,955		12,860	

Notes: All data were from the National Longitudinal Survey of Youth 1979, 1979–2014. Estimates are weighted using baseline weights. BA, bachelor's degree.

second children were associated with a 12.5 percent penalty ($p < 0.001$), and third children were associated with a 16.7 percent penalty ($p < 0.01$). These findings suggest that highly educated women incurred lower wage-related consequences at the first birth than the second birth, at which point they faced a wage penalty. Less educated women instead had less variation in associations of births with changes in wages across parity transitions, but the penalty of highest magnitude occurred at the first birth.

Model 3 presents estimates of heterogeneous associations of motherhood with changes in wages by timing of the transition to motherhood, controlling for the timing of subsequent parity transitions.[7] For women with less than a college degree, the magnitude of wage penalties was not associated with a linear age patterning based on the timing of the transition to motherhood. For the youngest age group, there was a 5.7 percent statistically insignificant wage penalty. The penalty rose to 15.4 percent for women who became mothers between ages 23 and 27 ($p < 0.001$) and then attenuated across the next two age groups, falling to a 9.9 percent penalty ($p < 0.01$) at 28 to 32 years, reaching a 4 percent statistically insignificant penalty at 33 and 37 years. The penalty's magnitude then increased again in the oldest age group, who experienced a 21.3 percent wage penalty ($p < 0.05$). This suggests that although there may be a somewhat attenuated motherhood penalty for delaying childbearing until the early or mid-30s, an even larger penalty arose for women who delayed until their late 30s and beyond.

For college-educated women, examining heterogeneous associations of timing with wages shows that models 1 and 2, which pool effects across all ages at births, cover substantial variation in the magnitude of motherhood wage penalties (or premiums) by birth timing. When estimating coefficients separately by timing of first birth, a clear age patterning of associated changes in wages emerges. Women who became mothers in their 20s through early 30s experienced a wage penalty that declined with age at first birth. Women transitioning to motherhood between 20 and 22 had an 11.0 percent penalty (not significant, likely due to small cell sizes); women becoming mothers between 23 and 27 had a 10.1 percent penalty ($p < 0.05$); and women becoming mothers between 28 and 32 had a 5.9 percent penalty ($p < 0.10$). Although these coefficients do not significantly differ from and have similar magnitude to those of less educated women, an age pattern indicating benefits of fertility delay is present among bachelor's degree holders. The picture starts to shift more drastically for women who delay fertility into their mid-30s and beyond.[8] Having a first birth at 33 to 37 years was associated with a 15.2 percent premium ($p < 0.05$), which increased to a 21.4 percent premium for women who had their first child at 38 or older ($p < 0.05$). The estimates at ages at which the premium appears are statistically significantly different by educational attainment at or lower than the 5 percent level. These findings reveal benefits to fertility delay, but they primarily apply to highly educated mothers. Additionally, this pattern adds complexity to the findings from model 2 (which, like model 3, accounts for later births), suggesting that although in the aggregate, the first child is not associated with a detectable wage change for college-educated women, and a notable premium is present for women who delay their fertility into their 30s, which is aligned with past findings (Amuedo-Dorantes and Kimmel 2005; Taniguchi 1999). When the estimates are

Table 2: Fixed-effects estimates of motherhood, parity, and timing on log wages by education

	Model 1			Model 2			Model 3		
	<BA	BA+	Diff?	<BA	BA+	Diff?	<BA	BA+	Diff?
First child	$-0.113^{\dagger}$	-0.048		$-0.101^{\dagger}$	0.008	†			
	(0.020)	(0.027)		(0.020)	(0.026)				
Second child				$-0.063^{\dagger}$	$-0.125^{\dagger}$				
				(0.022)	(0.034)				
Third child				$-0.097^{\dagger}$	$-0.167^{\dagger}$				
				(0.032)	(0.053)				
Timing of first child									
20–22							-0.057	-0.110	
							(0.070)	(0.095)	
23–27							$-0.154^{\dagger}$	-0.101^{*}	
							(0.028)	(0.048)	
28–32							$-0.099^{\dagger}$	-0.059	
							(0.034)	(0.034)	
33–37							-0.040	0.152^{*}	*
							(0.049)	(0.061)	
38+							-0.213^{*}	0.214^{*}	†
							(0.086)	(0.085)	
Constant	$1.228^{\dagger}$	$1.519^{\dagger}$		$1.246^{\dagger}$	$1.560^{\dagger}$		$1.238^{\dagger}$	$1.583^{\dagger}$	
	(0.228)	(0.150)		(0.229)	(0.149)		(0.228)	(0.149)	
Person–years	22,955	12,860		22,955	12,860		22,955	12,860	
Respondents	1,603	774		1,603	774		1,603	774	
AIC	39,450.54	21,136.94		39,406.78	21,046.75		39,398.5	20,984.11	

Notes: All data are from the National Longitudinal Survey of Youth 1979, 1979–2014. All models include time-varying controls for age (a series of dummy variables), years of schooling, and dummy variables for survey year. Model 3 also includes measures of timing of second and third births. Full results for later birth timings can be found in Table 4 of the online supplement. Significance of the group differences (Diff) is determined based on significance of the interaction of a bachelor's degree (BA) or more with each motherhood variable in a fully interacted model. Regressions are weighted using baseline weights. Standard errors are in parentheses. $^{*}p < 0.05$, $^{\dagger}p < 0.01$.

pooled across ages at first births, the penalties associated with earlier first births appear to cancel out the premiums associated with later first births.

Mediation

To understand why such patterns arise, I test mediating effects of family and work characteristics. First, I estimate motherhood effects net of family characteristics: current marital status; years since first, second, and third birth; and spouse's income. Overall, the story remains consistent. Figure 2 displays coefficients from the main model alongside those from the two models with mediators for the purpose of comparison. Table 3 shows the full set of results. Accounting for family characteristics does not greatly change the story, but it adds nuance. Differences by education in the motherhood, first birth, and timing coefficients for women who become mothers at age 33 or older are statistically significant ($p < 0.05$). The most notable deviation from the initial model is that adding these family characteristics somewhat attenuates the third-child effect for college-educated mothers from 16.7 percent ($p < 0.01$) in the initial model to 11.0 percent ($p < 0.10$) with family characteristics controlled. This indicates that about one-third of the third-child penalty for college-educated mothers is accounted for by family characteristics.

Table 3: Fixed-effects estimates of motherhood, parity, and timing on log wages by education with family characteristics

	Model 1			Model 2			Model 3		
	<BA	BA+	Diff?	<BA	BA+	Diff?	<BA	BA+	Diff?
First child	−0.112†	−0.032	*	−0.094†	0.008	$\dagger$			
	(0.021)	(0.029)		(0.021)	(0.027)				
Second child				−0.081†	−0.116†				
				(0.025)	(0.039)				
Third child				−0.129†	−0.110				
				(0.040)	(0.066)				
Timing of first child									
20–22							−0.057	−0.106	
							(0.070)	(0.094)	
23–27							−0.151†	−0.093	
							(0.029)	(0.048)	
28–32							−0.091†	−0.056	
							(0.033)	(0.036)	
33–37							−0.027	0.146*	*
							(0.050)	(0.061)	
38+							−0.192*	0.212*	$\dagger$
							(0.089)	(0.087)	
Married	−0.030	0.051	*	−0.025	0.049	*	−0.028	0.044	
	(0.024)	(0.029)		(0.024)	(0.029)		(0.024)	(0.029)	
Previously married	0.017	0.078*		0.016	0.071*		0.014	0.075*	
	(0.029)	(0.035)		(0.029)	(0.035)		(0.029)	(0.035)	
Years since first birth	−0.001	−0.006		0.000	−0.006		0.000	−0.004	
	(0.003)	(0.004)		(0.003)	(0.004)		(0.003)	(0.004)	
Years since second birth	0.000	−0.002		0.003	0.003		0.003	0.004	
	(0.002)	(0.004)		(0.002)	(0.004)		(0.003)	(0.004)	
Years since third birth	−0.002	−0.008*		0.002	−0.005		0.002	−0.005	
	(0.002)	(0.004)		(0.003)	(0.005)		(0.003)	(0.005)	
Spouse earnings ($1,000)	0.001*	0.000		0.001*	0.000		0.001*	0.000	
	(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)	
Constant	1.221†	1.511†		1.258†	1.546†		1.254†	1.572†	
	(0.227)	(0.150)		(0.229)	(0.148)		(0.228)	(0.148)	
Person–years	22,955	12,860		22,955	12,860		22,955	12,860	
Respondents	1,603	774		1,603	774		1,603	774	
AIC	39,441.87	21,056.96		39,393	21,025.75		39,380.3	20,972.54	

Notes: All data are from the National Longitudinal Survey of Youth 1979, 1979–2014. All models include time-varying controls for age (a series of dummy variables), years of schooling, and dummy variables for survey year. Model 3 also includes measures of timing of second and third births, which can be found in Table 4 of the online supplement. Significance of the group differences (Diff) is determined based on significance of the interaction of a bachelor's degree (BA) or more with each motherhood variable in a fully interacted model. Regressions are weighted using baseline weights. Standard errors are in parentheses. $^{*}p < 0.05$, $^{\dagger}p < 0.01$.

As for the effects of the covariates themselves in the model accounting for parity and timing (model 3), being married (versus never married) is associated with a 4.4 percent nonsignificant wage premium for college-educated women, whereas being formerly married (versus never married) is associated with about a 7.5 percent wage premium ($p < 0.05$). Less educated women, conversely, do not have a statistically significant association of marital status with wages. However, if they are married, each $10,000 increase in spouse's earnings is associated with a 1 percent increase in their own wages ($p < 0.05$). There is no association for their more educated counterparts. This suggests that household resources do not affect wages of college graduates but may slightly help less educated women have slightly

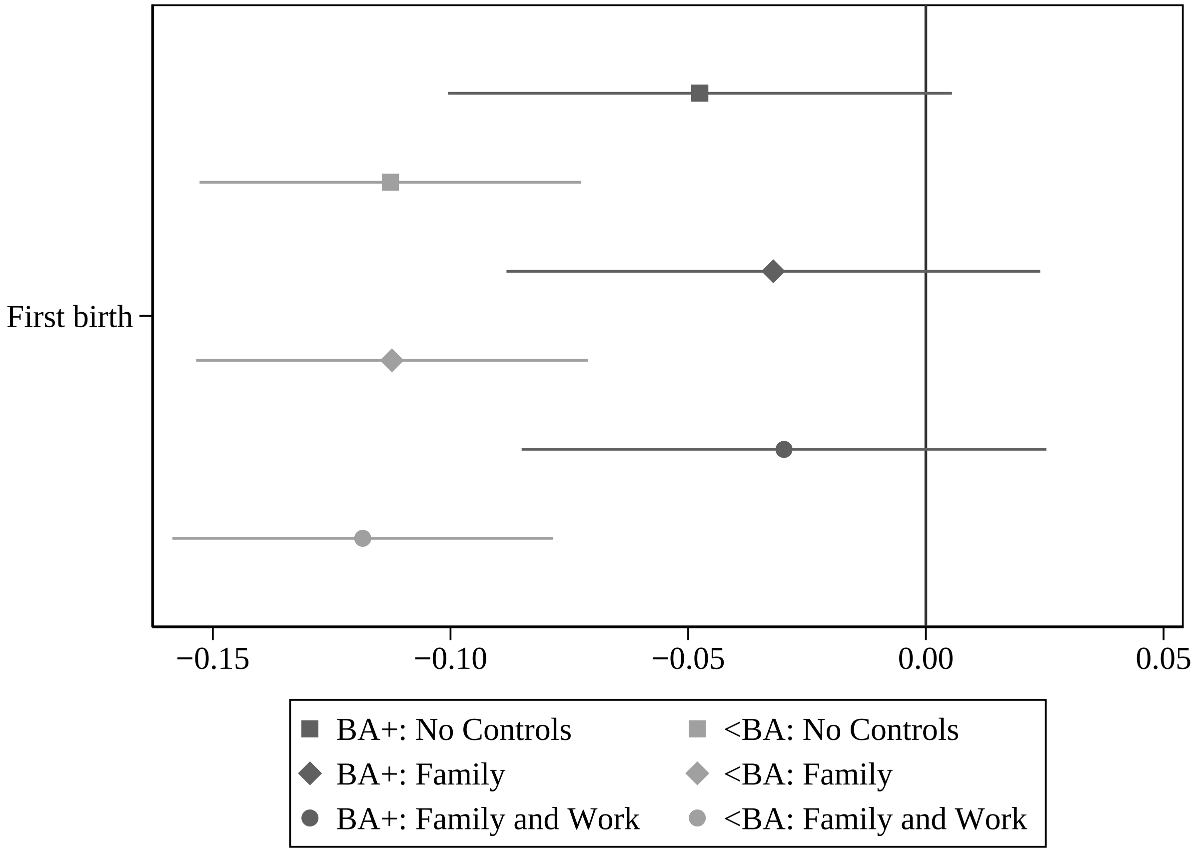

Figure 2: Coefficients across specifications, model 1: transition to motherhood. Coefficient estimates are from model 1 in Table 2 ("No Controls"), Table 3 ("Family"), and Table 4 ("Family and Work"). BA, bachelor's degree.

higher engagement in the labor force. Timing since births also does not have an effect of notable magnitude or significance for either group.

The final model adds work characteristics: occupation group, part time/full time/overwork, tenure, experience, and experience squared. Again, coefficients on motherhood variables, as shown in Table 4 and displayed in Figures 2-4, alongside coefficients from the previous two sets of models for comparison are mostly consistent with the earlier models. The key exception is that, in the parity models, the second birth coefficient for college graduates is somewhat attenuated, falling by about three percentage points from 11.6 percent in the model with only family characteristics to an 8.6 percent penalty when work characteristics are added ($p < 0.05$). Changes in work characteristics are responsible for approximately a third of the magnitude of the penalty associated with second births, suggesting that college-educated mothers do make changes in their work behaviors around their second birth, leading to this penalty.

Table 4: Fixed-effects estimates of motherhood, parity, and timing on log wages by education with family and work characteristics

	Model 1			Model 2			Model 3		
	<BA	BA+	Diff?	<BA	BA+	Diff?	<BA	BA+	Diff?
First child	$-0.118^{\dagger}$	-0.030	*	$-0.107^{\dagger}$	-0.002	*			
	(0.020)	(0.028)		(0.021)	(0.027)				
Second child				-0.060^{*}	-0.086^{*}				
				(0.024)	(0.037)				
Third child				-0.088^{*}	-0.098				
				(0.039)	(0.065)				
Timing of first child									
20–22							-0.049	-0.119	
							(0.070)	(0.094)	
23–27							$-0.143^{\dagger}$	-0.066	
							(0.028)	(0.044)	
28–32							$-0.120^{\dagger}$	-0.058	
							(0.032)	(0.035)	
33–37							-0.041	0.106	
							(0.048)	(0.060)	
38+							-0.151	0.200^{*}	†
							(0.080)	(0.091)	
Married	-0.033	0.020		-0.030	0.020		-0.031	0.019	
	(0.023)	(0.028)		(0.023)	(0.028)		(0.023)	(0.028)	
Previously married	0.021	0.072^{*}		0.021	0.067^{*}		0.019	0.072^{*}	
	(0.028)	(0.034)		(0.028)	(0.034)		(0.028)	(0.034)	
Years since first birth	0.000	-0.004		0.001	-0.004		0.001	-0.003	
	(0.003)	(0.003)		(0.003)	(0.003)		(0.003)	(0.003)	
Years since second birth	0.002	0.002		0.004	0.006		0.004	0.007	
	(0.002)	(0.004)		(0.002)	(0.004)		(0.002)	(0.004)	
Years since third birth	-0.002	-0.003		0.000	0.000		0.001	0.000	
	(0.002)	(0.004)		(0.003)	(0.005)		(0.003)	(0.005)	
Spouse earnings ($1,000)	0.000	0.000		0.000	0.000		0.000	0.000	
	(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)	
Occupation group (vs. Professional, Managerial, and Technical)									
Sales and office	$-0.068^{\dagger}$	$-0.064^{\dagger}$		$-0.069^{\dagger}$	$-0.063^{\dagger}$		$-0.069^{\dagger}$	$-0.062^{\dagger}$	
	(0.015)	(0.018)		(0.015)	(0.018)		(0.015)	(0.018)	
Service	$-0.229^{\dagger}$	$-0.243^{\dagger}$		$-0.228^{\dagger}$	$-0.240^{\dagger}$		$-0.229^{\dagger}$	$-0.236^{\dagger}$	
	(0.021)	(0.030)		(0.021)	(0.030)		(0.021)	(0.030)	
Farm, production, craft	-0.081^{*}	-0.117		-0.081^{*}	-0.117		-0.080^{*}	-0.116	
	(0.035)	(0.065)		(0.035)	(0.065)		(0.035)	(0.063)	
Operation	0.004	-0.019		0.006	-0.016		0.007	-0.016	
	(0.026)	(0.054)		(0.026)	(0.054)		(0.026)	(0.053)	
Full time (vs. part time)	$-0.073^{\dagger}$	$-0.040*$		$-0.076^{\dagger}$	$-0.046^{\dagger}$		$-0.075^{\dagger}$	-0.045^{*}	
	(0.013)	(0.018)		(0.013)	(0.018)		(0.013)	(0.018)	
Overwork (vs. part time)	$-0.430^{\dagger}$	$-0.431^{\dagger}$		$-0.433^{\dagger}$	$-0.436^{\dagger}$		$-0.429^{\dagger}$	$-0.431^{\dagger}$	
	(0.033)	(0.033)		(0.033)	(0.033)		(0.033)	(0.033)	
Tenure	$0.013^{\dagger}$	$0.011^{\dagger}$		$0.013^{\dagger}$	$0.011^{\dagger}$		$0.013^{\dagger}$	$0.011^{\dagger}$	
	(0.002)	(0.002)		(0.002)	(0.002)		(0.002)	(0.002)	
Experience	$0.042^{\dagger}$	$0.067^{\dagger}$	*	$0.038^{\dagger}$	$0.064^{\dagger}$	*	$0.037^{\dagger}$	$0.062^{\dagger}$	*
	(0.005)	(0.009)		(0.005)	(0.009)		(0.006)	(0.009)	
Experience squared	$-0.001^{\dagger}$	$-0.001^{\dagger}$		$-0.001^{\dagger}$	$-0.001^{\dagger}$		$-0.001^{\dagger}$	$-0.001^{\dagger}$	
	(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)	
Constant	$1.286^{\dagger}$	$1.614^{\dagger}$		$1.311^{\dagger}$	$1.638^{\dagger}$		$1.313^{\dagger}$	$1.647^{\dagger}$	
	(0.233)	(0.147)		(0.234)	(0.146)		(0.232)	(0.145)	
Person–years	22,955	12,860		22,955	12,860		22,955	12,860	
Respondents	1,603	774		1,603	774		1,603	774	
AIC	38,575.15	20,440.54		38,552.86	20,422.07		38,552.8	20,388.63	

Notes: All data are from the National Longitudinal Survey of Youth 1979, 1979–2014. All models include time-varying controls for age (a series of dummy variables), years of schooling, and dummy variables for survey year. Model 3 also includes measures of timing of second and third births, which can be found in Table 4 of the online supplement. Significance of the group differences (Diff) is determined based on significance of the interaction of a bachelor's degree (BA) or more with each motherhood variable in a fully interacted model. Regressions are weighted using baseline weights. Standard errors are in parentheses.
$^{*}p < 0.05$, $^{\dagger}p < 0.01$.

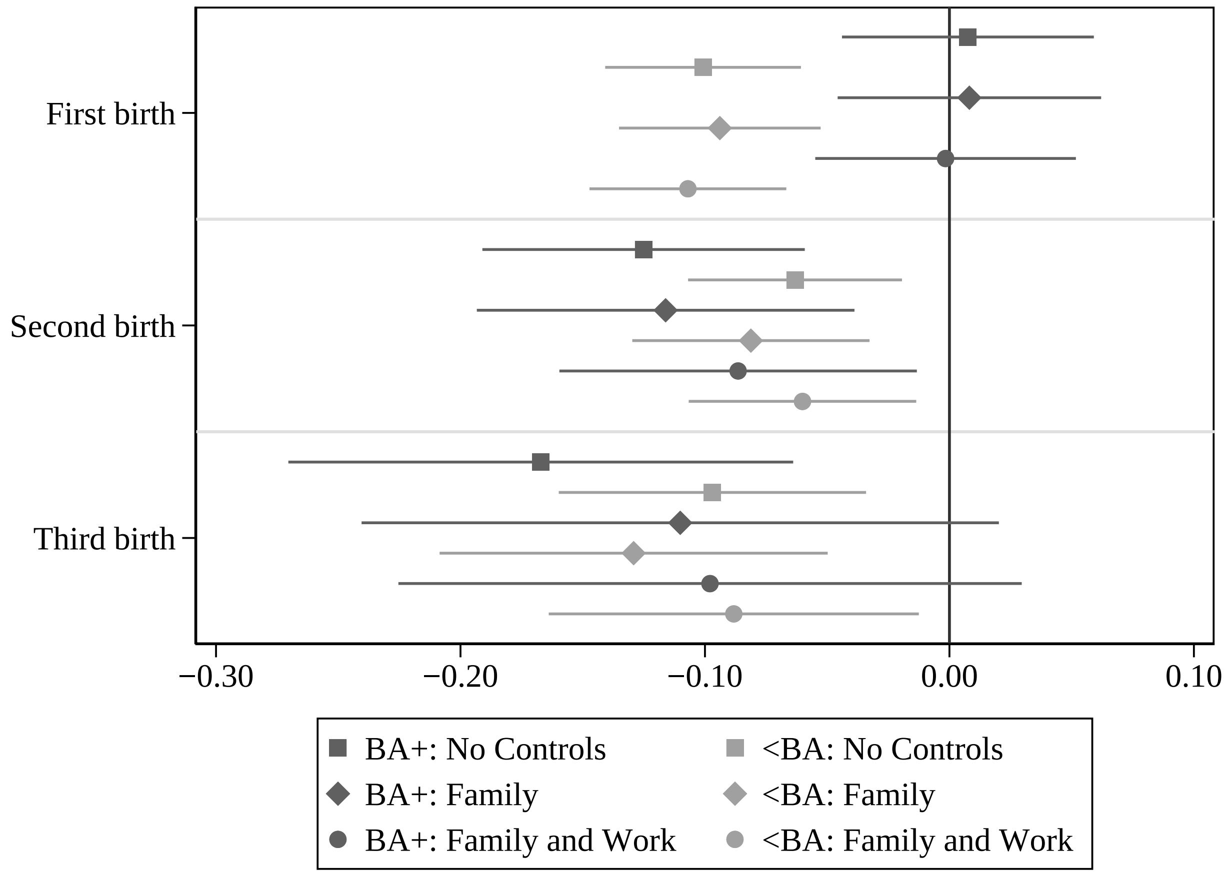

Figure 3: Coefficients across specifications, model 2: parity. Coefficient estimates are from model 2 in Table 2 ("No Controls"), Table 3 ("Family"), and Table 4 ("Family and Work"). BA, bachelor's degree.

In addition, for mothers without a college degree, the large motherhood penalty associated with having a first birth at age 38 or older attenuates by about one-quarter from the previous model to a 15.1 percent penalty that is only marginally significant ($p < 0.10$). This suggests that work characteristics partially account for this pronounced wage penalty these mothers experience. Unlike mothers with younger transitions to motherhood, these women may be experiencing a larger penalty due to changes they make in work factors around the time of the birth that lessen their pay. For college-educated mothers, the wage premium associated with having a first child from 33 to 37 also slightly attenuates to a marginally significant 10.6 percent premium ($p < 0.10$). This suggests these more educated mothers may be making beneficial changes in work characteristics that are partially responsible for the wage premium they experience.

As for the effects of work characteristics themselves, changes in occupation groups are associated with wage effects, suggesting part of the motherhood penalty is accounted for by women moving out of higher-status occupation groups into

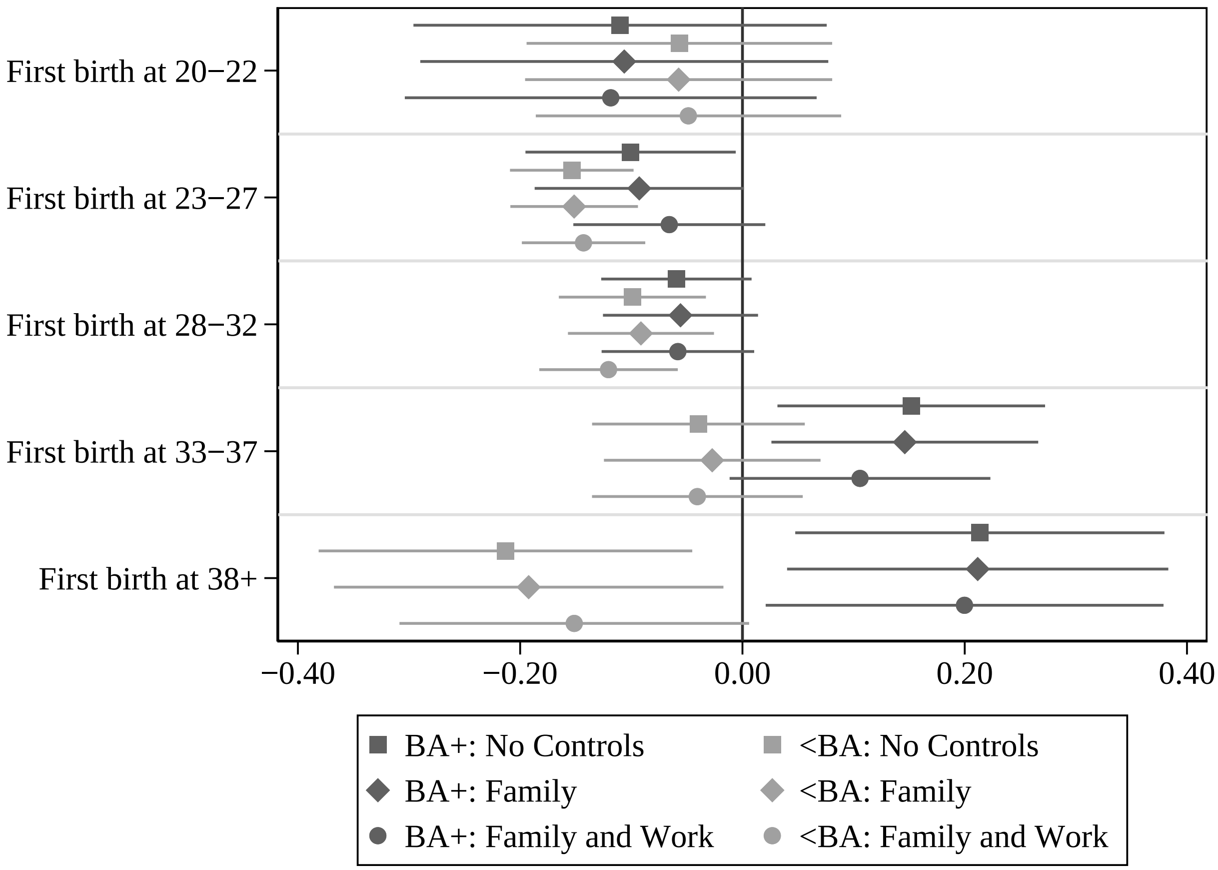

Figure 4: Coefficients across specifications, model 3: timing. Coefficient estimates are from model 3 in Table 2 ("No Controls"), Table 3 ("Family"), and Table 4 ("Family and Work"). BA, bachelor's degree.

lower-status groups. In model 3, working full time, compared to part time, is actually associated with lower earnings of 4.5 percent for college-educated mothers ($p < 0.05$) and about 7.5 percent for less educated mothers ($p < 0.001$), but this negative association (as opposed to a positive association) only appears when the coefficient is estimated net of this wide range of controls. Working overtime is similarly associated with substantially lower wages ($p < 0.001$). Each year of tenure with an employer is associated with an 11 percent wage increase for college graduates and a 13 percent increase for less educated mothers across models ($p < 0.001$). Each year of experience increases college-educated mothers' wages by 6.2 percent ($p < 0.001$) and less educated mothers' wages by 3.7 percent ($p < 0.001$), suggesting a penalty for lost experience or time out of the labor force for all, but one of significantly larger magnitude for more educated mothers ($p < 0.05$). The quadratic experience term shows the expected slightly declining return to experience as it continues accumulating.

Although these family and work characteristics do attenuate the effect of motherhood, they do not alter the substantive conclusions that estimating a single effect of motherhood hides substantial variation in the effects by parity and timing, especially for college-educated women. However, work and family characteristics primarily have similar effects by education. This suggests that the relationship between family and work characteristics and motherhood wage penalties are actually similar by educational attainment, even if the overall magnitude of the penalty differs. Additionally, these models add to our understanding of what drives the differential parity effects for college graduates—the second-child effect is partly explained by work characteristics, and the third-child effect is partly explained by family characteristics.

Discussion and Conclusions

Although becoming a mother represents an important transition for all childbearing women, exploring variation in wage changes associated with motherhood revealed that both parity and timing of first births contributed to variation in the education-specific wage penalties (or premiums) that mothers experienced. For women with a college degree, motherhood was met with a small penalty at the aggregate level. Yet first births were not associated with a change in wages when accounting for second and third births, which were tied to larger wage penalties than were revealed when the motherhood penalty was estimated as a single coefficient. A portion of these effects of second and third births were accounted for by work and family characteristics, respectively. Furthermore, a wage premium was even reaped by women who delayed fertility until at least their mid-30s, and its magnitude increased with further delays. Less educated women, on the other hand, faced a wage penalty at all births, and delaying fertility did not linearly minimize the penalty. If college-educated women have fewer children and have children later than women with less education, these findings suggest that growing education differences in when and how many children women have may contribute to rising wage inequality in the United States. However, my findings also indicate that because delay is not associated with lower penalties for less educated women, wage inequality would not be lowered if women without a college degree had the same fertility timing as women with a college degree or more.

Past research on education differences in the motherhood penalty, which has primarily estimated a single effect of motherhood by group but controlled for characteristics like parity and timing, has glossed over this variation. As my analysis shows, the heterogeneous effects in models 2 and 3 reveal variation in education-specific motherhood effects by parity and timing that are not visible in model 1, which estimated a single coefficient for each group. These findings suggest that education differences in motherhood penalties are perhaps more complex than much of the literature has acknowledged. Parity and timing not only shape motherhood's association with changes in wages, but the characteristics of the variation they induce also varies by women's educational attainment, thus differentially affecting aggregate estimates of education-specific motherhood effects.

Why did college-educated women experience a premium upon becoming mothers if they delayed fertility? Why was this not the case for less educated mothers? This is likely related to the types of jobs and career trajectories college-educated women may have. These women may be working in jobs at which the potential for wage growth is substantial and early career trajectories are important for staying on track. Additionally, they may provide benefits like paid maternity leave or programs to allow a cleaner transition back to work after having a child (Hewlett 2007; Williams and Boushey 2010). Because these options are available, college-educated women may make more explicit decisions about when to have children and how many to have. Perhaps they are intentionally timing births to minimize negative consequences, such as having them shortly after they have received a promotion so that they will not be denied that career advancement as a result (Armenti 2003). Or, an expectation of this pay increase may encourage childbearing. Less educated women may not have this type of upwardly mobile career structure that values uninterrupted early careers (Hewlett 2007). They may also not have access to the family friendly benefits that are more common for college-educated women. If less educated women's career structures have fewer payoffs to this type of planning, even if women were to make these decisions, they would not equate to the same minimized wage penalties.

Why did college-educated women encounter a wage penalty with the second child, but not before? The popular notion of this second-child effect has been speculated about both in the media and by academics, suggesting that these high-status women can maintain a workable work–family balance with one child, but the second child brings about the negative effect of motherhood on their career (Abrams 2001; Hewlett 2007; Hirshman 2006; Slaughter 2015; Stone 2007). The mediation analysis supports this assertion, suggesting that changes in work characteristics account for about one-third of the effect. Still, the majority of the effect persists net of these traits, suggesting that women who have multiple children may already have been less committed to the work force even prior to having these subsequent children (Doren 2019). Less educated women encounter the negative effect as early as with their first child, suggesting that in the types of work and family situations they face, the changes made by these women or their employers at the first birth may be more likely to decrease their subsequent wages compared to more educated women. Lower wage rates, less job flexibility, fewer benefits like childcare or paid leave, and fewer prospects for upward career mobility may make work less desirable or feasible, leading to earlier shifts in level of work attachment (Cotter et al. 2007; Williams and Boushey 2010; Yu and Kuo 2017).

Given the rapidly changing nature of the relationship among gender, work, and family, it is likely that these processes vary across cohorts. Fertility delay has become even more common in cohorts since the NLSY79 (Cherlin et al. 2014; Livingston 2018), and completed parity may be changing as well, suggesting that their motherhood experiences even follow different patterns from those of same-education women in the past. Without younger cohorts having yet completed their fertility, however it is difficult to know whether they will ultimately "catch up" with past cohorts in how many children they ultimately have (Stone 2018). Yet it is possible to speculate about how effects of parity and timing may have changed for women with and without a college degree.

Workplaces have made explicit efforts to become more family friendly, although there remains much room for progress (Hewlett 2007; Rossin-Slater, Ruhm, and

Waldfogel 2013; Slaughter 2015). With more accommodating workplaces for college-educated women, the shift at the second child may decline, and the premium associated with fertility delay may become less pronounced compared to earlier births if interruptions at any time are less detrimental. Still, these benefits may be restricted to professional women, given that other jobs are less likely to invest as many resources into retaining their workers and easing their work–family conflict after they have children (Hewlett 2007; Williams and Boushey 2010).

At home, men have begun to do more housework and childcare, making it easier for (partnered) women to maintain their careers upon becoming parents (Bianchi 2000; Bianchi and Milkie 2010). The trend toward overwork among men in high-status jobs, however, may be stalling progress among their wives, who may compensate by decreasing their attachment to the workforce (Cha 2010; Cha and Weeden 2014; Weeden, Cha, and Bucca 2016). Falling marriage rates, especially for less educated women, may also exacerbate penalties in younger cohorts and create more pronounced education differences. Over time, motherhood penalties have begun to fall (and even disappear) for married women, whereas unmarried women have seen a persistent penalty, and it is likely that these trends will continue into the future (Pal and Waldfogel 2016). Thus, motherhood wage penalties, the relative benefits of delaying fertility, and the additional negative effects of later children on wages should decline over time, especially for college-educated women. Countervailing processes like the rise of overwork and falling rates of marriage, however, may stall the rate of change. Once more recent cohorts with available data (e.g., NLSY 1997) have completed their fertility, these hypotheses about changes across cohorts and how these changes vary by education should be tested.

Finally, these findings have important implications for social stratification and the reproduction of disadvantage. College-educated women not only reaped the financial benefits of their education (Autor 2014) but they also had access to the resources and flexibility to minimize the consequences of motherhood. Less educated women had lower baseline pay, likely lacked job choices, and, unlike their more educated peers, their motherhood wage penalty appeared immediately upon the transition to motherhood and was unchanged by postponing childbearing. This larger motherhood penalty thus only enhanced their relative socioeconomic vulnerability. As the polarization of fertility schedules becomes even more pronounced, these unequal motherhood wage penalties may exacerbate the diverging destinies of children and the differences in resources their mothers have available to offer (McLanahan 2004).

Notes

1 I measure hourly wage as annual earnings from salaries, wages, and tips from all jobs worked divided by annual hours from all jobs worked. Results are robust to using an alternate specification of respondent-reported hourly wages from their main job (Table 1 of the online supplement).

2 These nonmissing, zero-wage person–years account for 42 percent of "missing" log wage observations.

3 Results are similar when BA+ is defined by whether women had attained a BA by age 25 (available upon request). With this alternate definition, are 235 fewer women who are classified as BA+ than in my main analysis, because they attained their college degree after age 25.

4 I only include biological births. Adding nonbiological children could introduce substantially more heterogeneity to the sample and the way women experience motherhood. Adoptive children are likely more carefully planned, so that would almost certainly add an even stronger form of selection into parity and timing of births.

5 In a sensitivity analysis, I test the robustness of my findings to omitting twin births given that twin births likely represent a greater shock than singleton births. Excluding these observations does not affect my findings (Table 3 of the online supplement).

6 The oldest age at birth in my analytic sample is 50.

7 Coefficients for timing of later births are available in Table 4 of the online supplement.

8 Two-year as opposed to five-year age groupings also identify this age range as the turning point from penalty to premium.

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