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Do Highly Paid, Highly Skilled Women Experience the Largest Motherhood Penalty?

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Abstract

Motherhood reduces women's wages. But does the size of this penalty differ between more and less advantaged women? To answer this, we use unconditional quantile regression models with person-fixed effects, and panel data from the 1979 to 2010 National Longitudinal Survey of Youth (NLSY79). We find that among white women, the most privileged—women with high skills and high wages—experience the highest total penalties, estimated to include effects mediated through lost experience. Although highly skilled, highly paid women have fairly continuous experience, their high returns to experience make even the small amounts of time some of them take out of employment for childrearing costly. By contrast, penalties *net of* experience, which may represent employer discrimination or effects of motherhood on job performance, are not distinctive for highly skilled women with high wages.

Keywords

motherhood, gender, labor markets, wage trajectories

Being a mother lowers women's hourly earnings. The mechanisms proposed to explain this wage penalty for motherhood include employers' discrimination against mothers (Correll, Benard, and Paik 2007), reduced job performance due to the demands of motherhood (Azmat and Ferrer forthcoming), and the wage growth forgone during any time spent out of the labor force for childrearing (Budig and England 2001).

Mothering, the quintessential care work, produces public goods that many of us enjoy as free riders. Although we pay nothing to their mothers, we benefit from having spouses, friends, community members, coworkers, and employees whose good qualities arose in part through their mothers' unpaid efforts (England 2005). Given the public goods produced by mothering, many feminists see it as poignant—indeed, downright unjust—that the work of mothering is not only unpaid, but also reduces one's pay when one holds a job. Moreover, given that there is no parallel fatherhood wage penalty, the motherhood penalty contributes to the overall gender gap in pay (Waldfogel 1998). This pay gap, in turn, reduces the economic well-being of single women and their children, and, to the extent that "money talks" in relationships, reduces the bargaining power of women in heterosexual couples (Bittman et al. 2003).

In this article we ask an intersectional question about differences between groups of women in the size of the penalty they experience for motherhood. Taken as a proportion

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of their wage, is this penalty higher for women at the top or bottom of hierarchies of cognitive skill and wages? Highly skilled women with high wages are privileged in many ways. To foreshadow our findings, we find that one aspect of this privilege, their higher rates of return to experience, has a price—a higher proportionate wage penalty for motherhood. Although very few of these privileged women drop out of employment, the little time some do take out is very costly to their future wage, because the wage growth they lose while at home or working part-time is substantial. Thus, when the motherhood penalty is estimated to include that portion of the penalty incurred because of lost experience and tenure, these privileged women have the highest penalties. The net penalty, tapping employer discrimination or the effect of motherhood on performance, by contrast, does not differ consistently by wage or skill.

One motivation for our inquiry comes from two articles that, taken together, present a puzzle. Wilde, Batchelder, and Ellwood (2010) find higher motherhood penalties for women with higher cognitive skill levels, as measured by test scores, whereas Budig and Hodges (2010) find higher penalties at lower wage levels. This combination of findings is puzzling, given that cognitive skills and wages are positively correlated (Farkas et al. 1997). Moreover, both studies use the same panel dataset, the National Longitudinal Survey of Youth (NLSY79), and both use models with person-fixed effects to remove omitted variable bias. In this article, we assess whether more advantaged or more disadvantaged women—on the dimensions of cognitive skill and wage level—suffer larger motherhood penalties.

As we use the term "motherhood penalty," it does not imply that all of what we identify as a penalty is discrimination against mothers by employers, although there is strong evidence that such discrimination exists (Correll et al. 2007). Another mechanism hinges on the fact that employers reward experience and tenure with higher wage rates; thus, even absent discrimination, when motherhood leads women to interrupt their employment, it lowers their future wages upon their return to employment (Budig and England 2001; Staff and Mortimer 2012). Motherhood may also adversely affect wages through lowering job performance (Azmat and Ferrer forthcoming; Becker 1985) or through leading women to trade wages for "mother-friendly" jobs. We are interested in the sum total of all these effects of motherhood, which we refer to as the "total penalty" for motherhood. We also examine "net penalties" that are revealed by adjusting out any part due to motherhood encouraging a loss of experience, tenure, or part-time employment. As is standard, we express all penalties as a percent of a woman's hourly wage.

PAST RESEARCH

Are Motherhood Effects Causal?

Causation is seldom certain outside of random-assignment experiments, but the now mature research on motherhood wage penalties suggests that associations between motherhood and wages are not merely reflective of differential selectivity of women who are already destined to have low wages into motherhood, but rather are, at least in part, causal (Amuedo-Dorantes and Kimmel 2005; Anderson, Binder, and Krause 2002, 2003; Avellar and Smock 2003; Budig and England 2001; Budig and Hodges 2010; Correll et al. 2007; Gangl and Ziefle 2009; Glauber 2007; Korenman and Neumark 1992; Miller 2011; Waldfogel 1997; Wilde et al. 2010). Some researchers have assessed causal effects of motherhood by using instrumental variables that plausibly predict fertility but not wages (Amuedo-Dorantes and Kimmel 2005; Korenman and Neumark 1992; Miller 2011).[1] Other work shows evidence of causal effects of motherhood using person-fixed-effects models that control for all unchanging, unobserved characteristics of women that might affect their wages (Anderson et al. 2002; Avellar and Smock 2003; Budig and England 2001; Glauber 2007; Miller 2011; Waldfogel

1997). One type of selectivity into motherhood is not adequately dealt with by fixed-effects models—the selectivity that arises when changing events negatively affect a woman's career trajectory, leading women to decide to have a child. However, Wilde and colleagues (2010) present descriptive evidence from panel data that women's wages do not tend to fall before, but do often fall after, a birth.

How Motherhood Penalties Vary by Women's Skill and Wage Levels

The two articles that prompted our inquiry give very different impressions of whether advantaged or disadvantaged women suffer larger motherhood penalties. The first is a National Bureau of Economics Research paper by Wilde and colleagues (2010), using panel data from the National Longitudinal Survey of Youth, begun in 1979. To measure cognitive skill, they used a standardized test, the Armed Forces Qualifying Test (AFQT), administered shortly after the first wave. Such tests tap the kinds of cognitive skills taught in schools. Nonetheless, the authors believed it to be cleaner for causal inference to use the test score rather than education as their measure of skill; the tests were given shortly after the first wave, so test scores are thus exogenous to fertility and wage experience thereafter. By contrast, many women continue getting more education for years. Combining women of all races, Wilde and colleagues (2010) find the motherhood penalty to be much larger for women with higher skills, both before and after controls for experience. No prior paper had examined whether motherhood penalties vary by women's cognitive skill levels as measured by test scores. A number do consider the related questions of whether women with more education or those in higher-level jobs suffer larger penalties, but their findings are conflicting.[2]

The 2010 article by Budig and Hodges reports that, for white women, the proportionate wage penalty for motherhood is much larger at low levels of the wage distribution, both before and after controls for experience are included. Theirs was the first article in the literature on motherhood penalties to deploy quantile regression, a technique that provides separate estimates for how much an independent variable, in this case motherhood, affects an outcome at different percentiles (called "quantiles") of the dependent variable. Standard regression models (sometimes called "mean regression") assess how much an increase in an independent variable at its *mean* is associated with a change in the dependent variable at its *mean*; quantile regressions provide much more information.

Because test scores and wages are moderately positively correlated (England, Christopher, and Reid 1999; Farkas et al. 1997; Neal and Johnson 1996), such that many women with high wages have high cognitive skills, and vice versa, it is unclear what mechanisms would lead to low penalties for women with low cognitive skill but high penalties for women with low wages. We attempt to solve this puzzle by examining how penalties differ by various combinations of skill and wage level.

We use a relatively new statistical model, the unconditional quantile regression model (hereafter UQR), following the lead of Killewald and Bearak (2014). Budig and Hodges (2010) introduced the use of *conditional* quantile regression (hereafter CQR) to the literature on motherhood penalties. In their comment on Budig and Hodges (2010), Killewald and Bearak (2014) argue that to answer the question posed by Budig and Hodges—whether women who have high or low wages experience a larger motherhood penalty—one should use the less well known *unconditional* quantile regression, introduced into the econometric literature by Firpo, Fortin, and Lemieux (2009). Covariates in UQR models help net out spurious associations between motherhood and wages, just as they do in CQR, but in UQR (but not CQR) the inclusion of covariates in the model has no effect on the wage quantile at which a given observation falls. In UQR, the quantile of each observation is decided by the value of wages at various percentiles of the univariate

distribution of wages among the observations in the analysis. By contrast, when using CQR, as Budig and Hodges (2010) did, if a model controls for education, then, for example, a finding that the motherhood penalty is 5 percent for women whose wage is at the 80th quantile informs us about those at the 80th wage percentile of wages *among women of their own education level*. Women with low wages relative to others at their education level might still be at a high wage quantile unconditionally—that is, compared to all women, not just those at their education level. After arguing for the appropriateness of UQR to answer Budig and Hodges's (2010) question, Killewald and Bearak (2014) re-estimate Budig and Hodges's (2010) simplest baseline model, and they find little difference in motherhood penalties by quantile; this contrasts with Budig and Hodges's CQR findings of higher penalties for lower-wage women. In their reply, Budig and Hodges (2014) accept that UQR is the preferred technique.

In this article, we build on the emergent consensus that UQR is the preferred technique for assessing how motherhood penalties vary across the wage distribution, and we innovate by using UQR to assess whether penalties are lower at low wage levels (as Budig and Hodges [2010, 2014] suggest) at all cognitive skill levels, and whether penalties are higher for women with high cognitive skill (as Wilde and colleagues [2010] suggest) at all wage levels. That is, we look at how penalties vary according to skill across wage levels.

In addition, in an online supplement (http://asr.sagepub.com/supplemental), we provide a parallel analysis for black women, allowing us to examine how race affects motherhood penalties as well as racial differences in how penalties vary by skill and wage level. Past research shows smaller motherhood penalties for black than white women (Hill 1979; Glauber 2007, 2013; Waldfogel 1997), but the differentials remain unexplained.[3] Wilde and colleagues (2010) included women of all races in their analyses without exploring interactions of motherhood and race, but Budig and Hodges (2010) did preliminary analyses that found little variation in motherhood penalties by wage quantiles for black women, and thus limited analyses shown in their paper to white women. We too find lower penalties for black women than for white women, and black women's penalties do not vary much by skill and wage quantile. Given that we failed to find an explanation of black women's lower and less variable penalties, we limit findings shown in this article to white women; we show findings for black women in the online supplement, but we summarize them briefly here when discussing our results.

The Motherhood Penalty and Why It Varies by Group: Past Research and Theory

Despite strong agreement that, on average, motherhood carries a wage penalty, researchers differ in their views about why it occurs, and about why penalties might vary by skill and wage level. As mentioned earlier, we will distinguish between what we call the "total" motherhood penalty, estimated in models not controlling for experience, and the "net" penalty, estimated in models controlling for experience to adjust out any effects that arise because motherhood takes women out of employment (or reduces their hours) for a time. The total penalty is affected by motherhood-induced experience differentials, as well as other factors, such as employer discrimination based on motherhood or lowered job performance because of motherhood. The net penalty is affected only by mechanisms other than experience; we thus treat it as the combined effect of discrimination and performance differences among women with the same experience.

Motherhood lowers pay because of forgone experience. Some women respond to having a child by spending some months or years out of employment or by reducing the hours they work per week. Although there is no agreement on whether experience is rewarded with higher wage rates because experience increases productivity, as human

capital theory posits, or because of institutional practices undertaken for other reasons, it is clear that wages rise with experience and thus that forgoing experience lowers future wages. Some research includes measures of overall years of experience as well as the experience one has accumulated with the present employer (i.e., "tenure" or "seniority"). Most studies show that estimated penalties are substantially reduced, but not eliminated, when models add a control for employment experience and tenure; that is, they explain part, but not all, of total motherhood penalties (Anderson et al. 2002, 2003; Budig and England 2001; Budig and Hodges 2010; Waldfogel 1997; Wilde et al. 2010).[4] (Staff and Mortimer [2012] explain the entire gap with experience-related measures; they examined women up to 31 years of age in Minnesota.)

Because experience is the mechanism for a significant portion of the total motherhood penalty, we would expect the total penalty to be larger in groups that interrupt their employment experience for longer periods at home. Thus, we should see higher total penalties for women with low wages, because they are more likely to drop out of employment (Blau and Kahn 2007). We know of no research on whether cognitive skill affects how much motherhood reduces women's employment. But given the correlation between skill and education, and the more continuous employment of more-educated women (Byker 2016; England, Garcia-Beaulieu, and Ross 2004; England, Gornick, and Shafer 2012), we might expect skilled women to suffer lower total motherhood penalties because they stay employed more continuously.

How much a given amount of time spent out of employment lowers wages depends on the return to experience one would have had if one stayed employed. Given this, if we compared two groups with an equal amount of experience lost to motherhood, we would expect groups of women with higher rates of return to experience to have higher total motherhood penalties.[5] Predictions about differences in penalties between groups defined by skill or wage follow from this.

If higher cognitive skills allow workers to learn more effectively on the job, increasing their productivity more quickly, and if productivity affects wages, this will lead more-skilled workers to have higher returns to experience, and thus higher total motherhood penalties. Some evidence suggests this: Killewald and Gough (2013:484) show higher returns to experience for more-educated women, and Wilde and colleagues (2010) show that, before childbearing, women with high cognitive skills have steeper wage trajectories.

What would we expect about penalties varying by wage level? If low-wage workers have lower returns to experience, they should also have a lower total motherhood penalty. In the 1970s and 1980s, authors theorizing segmented labor markets posited that firms' low profits and lack of unionization led to jobs that not only paid low starting wages but also had little wage growth, because they were not attached to the job ladders of internal labor markets (Doeringer and Piore 1971; Edwards, Reich, and Gordon 1975; Tolbert, Horan, and Beck 1980). This suggests that low wages often go with low returns to experience, but researchers then were working with datasets that lacked measures of experience, so such claims were not tested. If the claims of an association between low wages and low returns to experience are correct, then we would expect lower total motherhood penalties in lower-wage jobs.

To summarize, we argued that groups with lower experience (including lower tenure) should have higher total penalties, and groups with higher returns to experience should have higher total penalties. Indeed, the amount of any group's motherhood penalty that is due to experience should be a multiplicative function of the amount of experience they have lost to motherhood and their returns to experience. This means making predictions about group differences in total penalties is difficult, because our review suggests that groups that are more advantaged in skill and wage may have higher levels of experience *and* higher returns to experience. If so, these two features of their situations will have

contradictory effects on the size of their total motherhood penalties. We thus formulate two contrasting hypotheses that differ on which part of this multiplicative effect should dominate group differences in penalties:

Hypothesis 1: Groups with higher rates of return to experience will have higher total motherhood penalties than groups with lower rates of return to experience. Women who have high skills and high wages will thus have higher total motherhood penalties, because they also have relatively high rates of return to experience.

Hypothesis 2: Groups with lower levels of experience will have higher total motherhood penalties than groups with higher levels of experience. Women with low skills or low wages will thus have higher penalties, because they have lower levels of experience.

Motherhood lowers pay because it affects performance or because employers discriminate against mothers. Some mechanisms for the motherhood penalty have nothing to do with levels of, or returns to, experience; they affect the penalty estimated net of experience, and they also affect the total penalty, because the net penalty is part of the total penalty. One such mechanism is discrimination against mothers by employers. This discrimination could entail offering lower pay to mothers, less willingness to hire mothers in high-paying jobs, or offering fewer promotions to mothers. Correll and colleagues' (2007) audit study provides evidence of hiring discrimination against mothers in elite jobs. They sent equivalent résumés as applications to real job ads, varying randomly whether the résumé indicated indirectly that a woman was a mother by saying she was a PTA officer. (The alternative "non-mother" résumé said the woman was an officer of a neighborhood association.) Résumés of mothers received significantly fewer calls in response to job applications.[6] The positions in Correll and colleagues' audit study were high-skilled professional jobs and the applicants were college graduates. We lack audit studies illuminating whether discrimination against mothers varies by skill or wage. In our analysis, the inference of discrimination will not be clear if there is a penalty net of experience, because the net penalty could also result from the effect of motherhood on women's performance in their jobs.

Becker (1985, 1993) claimed that motherhood reduces women's productivity on the job, thus affecting pay; if women do "home production" when not on the job, they have less energy left for job performance at work. His argument was theoretical; he had no data with measures of productivity. However, one recent study of a representative sample of U.S. lawyers admitted to the bar in 2000 proposed "billable hours" as a measure of productivity, which is commonly accepted in law firms because of its direct effect on revenue. Using this measure, Azmat and Ferrer (forthcoming) show that gender differences in the number of "billable hours" explain a substantial share of the gender gap in pay, and that having children reduced billable hours for women but not for men.[7] Thus, there is some evidence that motherhood lowers productivity, but the hypothesis is untested for most occupations.

The productivity portion of the motherhood penalty is undoubtedly also a function of how gender, family, and jobs are socially organized (Acker 1990; Williams 2004; Williams and Bornstein 2008). For example, motherhood would be unlikely to affect productivity in paid work more than fatherhood does if gender did not structure who does childrearing and associated household work. Moreover, governmental and employer policies may affect the link between motherhood and productivity. For example, in cases where working long hours is highly rewarded, with little flexibility in *which* hours are worked, women, especially mothers, will be more disadvantaged (Cha and Weeden 2014; Goldin 2014; Herr and Wolfram 2012). Where women are offered paid leave after a birth, this may increase their ability to return to the same employer, and thus not lose the benefit of their accumulated tenure (Baker and Milligan 2008; Waldfogel 1998). In this analysis, however,

we are unable to model these variations, or even to estimate how much of the penalty results from productivity versus discrimination averaged across contexts. The closest we can come is to estimate the net-of-experience penalty, which is our best estimate of the portion of the penalty that results from how motherhood affects how well one actually performs, *or* how well, through discriminatory lenses, one is perceived to meet job demands.

Do these net penalties, picking up discrimination or productivity, differ by skill? Wilde and colleagues (2010) hypothesized that highly skilled women suffer higher motherhood penalties because they are in jobs most sensitive to effort. Putting their point more expansively, highly skilled women often hold jobs critical to their organizations' profits or other goals, so if motherhood causes any reduction in effort or performance (real or perceived), it may have a large impact on the organization's (real or perceived) "bottom line," giving employers an especially large incentive to make wages responsive to performance. Findings by Glass (2004) are consistent with this claim; among women who have a child and stay with the same employer, those who used family-friendly policies, such as reduced hours or working from home, had slower wage growth, a difference most pronounced for women in professional and managerial jobs. These patterns suggest that using family-friendly policies reduces productivity, or that employees who take employers' offers of flexibility options are seen as less productive, and thus are penalized even if their productivity is not reduced.

In contrast to the argument of Wilde and colleagues (2010) that penalties are higher for more cognitively skilled women, Budig and Hodges (2010) offer reasons why high-wage jobs (which typically also entail higher skills) would have *lower* net motherhood penalties. They suggest that jobs paying lower wages are more inflexible, and their incumbents have little power to negotiate changes that would help accommodate motherhood demands. The result may be productivity reductions that are reflected in wages, or terminations that force women to take worse jobs after unanticipated events, such as when childcare arrangements fall through. Budig and Hodges (2010) also argue that low-wage women have less income from their own earnings to purchase things such as a car or flexible childcare. Thus, when they have children, they may be less able to avoid absences that get them relegated to even lower wages.

The predictions just discussed relate to that part of the motherhood penalty not coming through the effect of motherhood on experience, but rather the net penalty, estimated in models that control for amount of experience, and theorized to result from effects of motherhood on performance, or on employers' biased perceptions or discriminatory treatment of mothers. This leads to two distinct predictions, one suggesting that advantaged women and one that disadvantaged women have larger penalties:

> *Hypothesis 3:* Advantaged women (women with high skills and high wages) will have higher net motherhood penalties. They are more likely to be in jobs in which any decline in performance (real or perceived) due to motherhood has a larger effect (or perceived effect) on their organizations' bottom line.

> *Hypothesis 4:* Disadvantaged women (women with low skills or low wages) will have higher net motherhood penalties. They have less power to negotiate job flexibility and less money to purchase services needed to avoid negative effects of motherhood on their performance (or perceived performance).

DATA AND METHODS

Data

Like Wilde and colleagues (2010) and Budig and Hodges (2010), we use nationally representative panel data from the National Longitudinal Survey of Youth 1979 (NLSY79). The cohort was born between 1958 and 1965 and first interviewed in 1979 at age 14 to 21. We use data through the 2010 interview when the cohort was age 45 to 52.[8] This cohort is, roughly speaking, the second half of the baby boom, and is now in middle age and largely

through the childbearing years. These data include detailed employment and family information collected repeatedly throughout the adult lives of the respondents.

Our main analyses are limited to non-Hispanic white women (hereafter called "white"). We limit the main analyses to white women in part to be consistent with the analysis of Budig and Hodges (2010), who limited their analysis to white women. Another reason for limiting the sample to white women is that, for the most part, the motherhood penalty does not differ by skill or wage level for black women, but we cannot be sure if this null finding is due to limited statistical power, given the relatively small sample size of black women. We will briefly summarize results for black women here, and we provide regression results in the online supplement. We exclude women of other races because we do not have enough statistical power for separate estimates for these groups.

Our models, discussed in the next sections, take person-years as the units of analysis; thus, our analytic sample consists of 37,063 person-year observations from 3,216 non-Hispanic white women. The online supplement shows results from a parallel analysis of 18,520 person-year observations from 1,442 non-Hispanic black women. To get separate estimates of the motherhood penalty by race, we include one indicator variable for race and interact it with all variables.[9]

The person-years in the analytic sample exclude years during which women were enrolled in school (secondary school or higher education), because wages in those years may be misleading; however, in broad terms, our conclusions are not affected if we retain these person-years (results available upon request).[10]

Variables

Dependent variable. Our dependent variable, consistent with most past research, including that of Budig and Hodges (2010, 2014) and Wilde and colleagues (2010), is the natural logarithm of the hourly wage a woman earned at the time of the given year's survey. We convert wages to constant 1996 dollars and then take the natural logarithm.

Cognitive skill: interacted with all other independent variables. We interact the *cognitive skill* variable with all other variables to provide separate estimates of the motherhood penalty by skill. To measure cognitive skills, we use age-adjusted scores from the Armed Forces Qualifying Test (AFQT), administered to all respondents in 1980; this is the measure Wilde and colleagues (2010) used. We use women of all races to assess cutting points for thirds to follow Wilde and colleagues (2010).[11] Because preliminary results showed that motherhood penalties differed little between the middle and bottom third, we dichotomized the variable into the top third versus the bottom two thirds, which we will, for brevity, refer to as "high" and "low" skill. Our use of AFQT does not imply a belief that individuals' scores measure something entirely determined innately. Scores are potentially affected by genetically inheritable factors; socially determined learning environments at school, at home, and in the community; and their interactions. Our goal is simply to ascertain whether motherhood penalties vary by skill within categories of wage level. Moreover, we make no claim that AFQT, or the broader construct of cognitive skills the test taps, are the only kind of skills relevant to labor market success. We do note, however, that this test predicts earnings net of education and race, and does so for white, black, and Hispanic respondents (England et al. 1999; Farkas et al. 1997; Neal and Johnson 1996).

Why examine how motherhood penalties vary by skill? Broadly, our motivation is to see whether women toward the top and bottom of job and reward hierarchies experience different motherhood penalties. We do this in part by examining how penalties vary by wage level, using quantile regression. But skill is another aspect of job hierarchies. Although there is no one-to-one match between individuals' skills and the skill demands of their jobs, these aspects are positively correlated: people with more skill are more likely to be

hired in jobs requiring more cognitive skill (Farkas et al. 1997). Moreover, there is evidence that individual-level skill affects wage differences between individuals in jobs with comparable skill demands, perhaps because skill affects performance (Farkas et al. 1997). We chose an individual-level measure of cognitive skill to tap this crucial skill dimension of inequality. We do this in part to revisit Wilde and colleagues' (2010) analysis, which used the same AFQT measure. We could have stratified by occupational measures of skill demands, or by broad occupational categories, and future research could usefully do this. We could also have used education as a proxy for skill, as other studies have done, with mixed results. However, Wilde and colleagues (2010) argue that using scores from one test administered only slightly after Wave 1 of the survey is a better strategy, because this one early measurement ensures that the scores are exogenous to all future fertility and wage experience. One could similarly ensure exogeneity by taking respondents' education at Wave 1, but for respondents who were very young at Wave 1 it is a much worse indicator of ultimate education than is the skill measure. If instead we used education at each wave to measure skill, it might be endogenous to number of children.

The key independent variable – motherhood. Our main independent variable is *motherhood*, measured by the number of children a woman has ever given birth to (or adopted) by the person-year. This way of measuring motherhood follows the practice of much past research, including Budig and England (2001), Budig and Hodges (2010, 2014), and Killewald and Bearak (2014). As a sensitivity test, in the Appendix we show results from a specification similar to that used by Wilde and colleagues (2010), which measures the penalty with indicator variables for having been a mother different amounts of time (relative to not being a mother), with a control variable for number of additional children past the first.[12]

Control variables. Other variables in all of our models include indicator variables for each year. We also include respondent's age and its square in the given year.[13] We control for respondent's geographic location in the given year with indicator variables for four regions of the country (Northeast, West, South, Midwest), and for whether the respondent lives in a central city in a Metropolitan Statistical Area (MSA), elsewhere in an MSA, or outside an MSA.[14] We also enter education attained by the given year, with indicators for college or more, some college, or less than high school, where high school is the reference category.[15] We include interactions between the age variables and each education indicator, allowing us to model the potentially greater wage gains with age experienced by women with higher education. Equation 1 includes motherhood and these controls.

Our next most saturated model, Equation 2, adds the woman's marital status (in three categories: not yet married; married and still together; and separated, divorced, or widowed). It also includes her spouse's annual earnings in the previous year, set to 0 if she is not married. The control for marital status "removes" the effect of these 0s from the coefficient on spousal earnings. Spousal earnings is also 0 if a woman is married but her husband had no earnings the past year. We include spousal earnings to control for other income available in the family, which might affect how much women will try to maximize earnings.

Equation 3 adds person-fixed effects to remove potential selectivity into motherhood on unmeasured variables. In models with fixed effects, coefficients on measures of motherhood reveal the within-person (across year) change in wages associated with women's changes in motherhood, after adjusting for observed covariates that also change across years. Such models control for all unmeasured, unchanging characteristics of persons that contribute additively to the estimation of their wages (Allison 2005). One type of selectivity into motherhood is not dealt with by fixed-effects models—when an

event that starts a woman upon a negative wage trajectory also makes her decide to have a child. If this occurs, models with fixed effects will overestimate the negative effect of motherhood on wages—attributing to motherhood some negative effects of changes that preceded and caused motherhood. Thus, a key assumption in our claim that our coefficients are unbiased is that negative career *trajectories* do not affect when or if women have children. As this is a strong assumption, it is fortunate that Wilde and colleagues (2010) present a convincing descriptive analysis of the path of wages before and after births, showing no evidence that women's wages typically fall before a birth; in fact, wages more typically rise before a birth.

Finally, our most saturated model, Equation 4, includes work experience, tenure, whether the current job is part-time versus full-time, and the interaction of each of these measures with education. To measure experience, we use a measure of the cumulative number of hours worked for pay since the first wave of the survey for each person-year observation. This measure, also used by Wilde and colleagues (2010), is superior to the more customary years (or weeks) of experience variable, which treats a year of full-time and part-time experience equally. To allow effects to be nonlinear, we enter the square of experience as well.[16] To measure tenure, we use a measure of the cumulative number of hours worked for pay since a woman began working for her current employer, and we include the square of this measure to allow effects to be nonlinear. (As is common practice, we include tenure in the experience measure.) Age, experience, tenure, and the square of each are also each interacted with each education dummy, to adjust for the potentially higher rates of return to experience or tenure obtained by women with high education (Killewald and Gough 2013).[17] We enter experience, tenure, the square of each, and part-time status[18] in the same model, but examinations (not shown) of models that do not include part-time status and tenure show that reductions in the motherhood penalty between Equations 3 and 4 come largely from the control for experience.[19]

Regression Models

As described earlier, our most saturated model is as follows:

$$
\begin{aligned}
q_\tau(\ln wage_{it}) = \sum_{j=0}^{1}\sum_{k=0}^{1}(&\beta_{0jk\tau} + kids_{ijkt}\ \beta_{1jk\tau} + age_{ijkt}\beta_{1jk\tau} + age^2_{ijkt}\beta_{2jk\tau} \\
&+ EDUCATION_{ijkt}\beta_{3jk\tau} + age_{it} \times EDUCATION_{ijkt}\beta_{4jk\tau} \\
&+ age^2_{ijkt} \times EDUCATION_{ijkt}\beta_{5jk\tau}\beta_{3jk\tau} + MARRIAGE_{ijkt}\beta_{7jk\tau} \\
&+ spouse\text{'}s\ earnings_{ijkt}\beta_{8jk\tau} + spouse\text{'}s\ earnings^2_{ijkt}\beta_{9jk\tau} \\
&+ spouse\text{'}s\ earnings\ unknown_{ijkt}\beta_{10jk\tau} + urban_{ijkt}\beta_{11jk\tau} + REGION_{ijkt}\beta_{12jk\tau} \\
&+ MSA_{ijkt} + YEAR_{jkt}\beta_{13jk\tau} + experience_{ijkt}\beta_{14jk\tau} + experience^2_{ijkt}\beta_{15jk\tau} \\
&+ experience_{ijkt} \times EDUCATION_{ijkt}\beta_{16jk\tau} \\
&+ experience^2_{ijkt} \times EDUCATION_{ijkt}\beta_{17jk\tau} + tenure_{ijkt} \times EDUCATION_{ijkt}\beta_{18jk\tau} \\
&+ tenure^2_{ijkt} \times EDUCATION_{ijkt}\beta_{19jk\tau} + fulltime_{ijkt}\ \beta_{20jk\tau})
\end{aligned}
$$

The dependent variable is the natural logarithm of a woman's hourly rate of pay in 1996 constant dollars. We compute separate estimates for each quantile of the outcome distribution, indexed by τ; the method for doing this will be explained below. The subscript i indexes women; the subscript t indexes the wave of the survey; and the it uniquely identify the person-year observations. The subscript j indexes skill category, and the subscript k indexes race category. Because we report findings for black women in the online supplement, we interact all variables with race, with white women as the reference category. To obtain separate penalties by skill level, we interact all variables with the cognitive skill dichotomy, with low skill as the reference. With respect to race, for example, for $kids_{ijkt}$, $kids_{ij0t}$ equals the number of children born to woman i by wave t; $kids_{ij1t}$ is an interaction term and equals the same for black women but is scored 0 for white women. Put another way, $ij0t$ indicates a baseline, and $ij1t$ an interaction term, with respect to race. Similarly, but with respect to skill categories, for $kids_{ijkt}$, $kids_{i0kt}$ equals the number of children born to woman i by wave t; correspondingly, $kids_{i1kt}$ is an interaction term and equals the same for highly skilled women, but 0 for lower-skilled women. Put another way, $i0kt$ indicates a baseline term, whereas $i1kt$ indicates an interaction term, with respect to skill. Other variables are entered as described above, with effects estimated for black and white women at each skill level by fully interacting everything with skill and race. As mentioned earlier, other variables included in all models are age and its square, education categories, the interaction of the age variables with the education categories, year indicators, and variables capturing geography. A more saturated model also includes marital status, husband's earnings, and its square. The most saturated model also includes work experience, its square, tenure, its square, whether the current job is part-time versus full-time, and the interaction of each of these measures with education.

Computing estimates of the motherhood penalty at varying percentiles of women's wage distribution. As mentioned previously, we use unconditional quantile regression (UQR) to estimate separate coefficients for a number of quantiles of women's wage distribution. One of the articles that motivated our work—Budig and Hodges (2010)—used conditional quantile regression (CQR; see Hao and Naiman 2007; Koenker 2005; Koenker and Bassett 1978), but we use UQR, which was introduced into the econometric literature more recently by Firpo, Fortin, and Lemieux (2009). UQR is appropriate if we want to compare motherhood penalties between women whose wage levels are high versus low in an absolute sense, whereas CQR is appropriate if we want to compare motherhood penalties at different points in the conditional wage distribution—that is, across women who differ in whether their wage is higher or lower than would be expected given their scores on covariates in the model, for example, whether women with low wages *compared to others with similar education* have higher motherhood penalties.[20] Our interest is in the former, so UQR is appropriate, as Killewald and Bearak (2014) suggested, and Budig and Hodges (2014) now agree. In UQR, as in CQR, covariates in the model help net out spurious associations between motherhood and wages, but in UQR the inclusion of covariates has no effect on which person-years are defined to be at which quantile of the wage distribution. This means that in the series of nested UQR models we estimate, the same observations are at the 20th and 80th (or any other) quantile across models, which is not true with CQR.

UQR can be estimated using a simple ordinary least squares (OLS) regression on a transformed dependent variable, the recentered influence function (RIF), which is defined in the following way:

$$RIF\left(Y; q_\tau, F_Y\right) = q_\tau + \left(\tau - 1\{Y \le q_\tau\}\right) \Big/ f_Y\left(q_\tau\right)$$

τ is a given quantile. q_τ is the value of the outcome variable, Y, at the τth sample quantile. $f_Y(q_\tau)$ is the density of Y at q_τ. **1** is the indicator function. For example, suppose we are interested in the motherhood penalty for women at the 20th quantile of the wage distribution ($\tau = .2$). The wage at the 20th percentile in the sample is then q_τ. The density of the wage distribution, estimated at the 20th percentile wage is $f_Y(q_\tau)$. The indicator function $1\{Y \leq q_\tau\}$ creates a dummy variable set to 1 if a given woman's wage is below the 20th percentile wage in the sample. The UQR estimate of the motherhood penalty for women at the 20th percentile of the wage distribution can then be obtained by OLS regression on the transformed dependent variable. Effects at other quantiles can be estimated analogously.

Locating the various wage quantiles is done in the preliminary stage of UQR regression estimation. We use wages across white women's person-years to locate the percentiles we use for analyses of white women, presented here, as well as analyses of black women, presented in the online supplement. This allows us to compare results to those of Budig and Hodges (2014) and Killewald and Bearak (2014), who limited their analysis to white women, and to use common quantiles for our white and black analyses, so that, for example, the 20th quantile will be the same actual wage when we compare black and white women's penalties at that wage percentile.

Standard errors are bootstrapped to incorporate the uncertainty involved in estimation of the RIF. Each bootstrap simulation repeats each stage of the procedure, starting from the beginning. Coefficients or differences between coefficients are referred to as significant if $p < .05$ on a two-tailed test.

Our UQR regressions are unweighted, given that weights can cause as well as correct biases (Winship and Radbill 1994), and we provide separate estimates by race (from models fully interacted by race), so that the over-representation of black women in the regressions is not biasing coefficients even if effects differ by race.

Our tables with quantile regression results show coefficients for effects of motherhood at the 20th, 50th, and 80th percentile of white women's logged wage, with the separate effects at each of these quantiles obtained using UQR as described earlier. We take the 20th and 80th quantiles to represent low- and high-wage workers, respectively. We did not choose extreme percentiles, as these may be more influenced by measurement error. The online supplement shows effects at more detailed quantiles. Our main emphasis is on results from models that show separate effects by wage levels within skill categories, but we also show results for standard mean regressions (not differentiating effects by quantile) for models that do not interact motherhood (and other variables) with skill and for models that do neither.

Does UQR make sense with panel data? We refer to observations at the 20th and 80th quantiles as "groups" differing in their wage level. The "groups" near a particular quantile are groups of person-years defined by wage, not groups of persons. A question relevant to how to interpret our results is how much individual women tend to have approximately the same rank order in wages in different years. Let us use the term "rank-invariance" for the extreme situation where no woman ever changes her rank. Clearly, we cannot require perfect rank-invariance for quantile results to be meaningful, because if women varied in their number of children, but rank-invariance of wages was perfect, there could be no motherhood penalty. But if women's rank often changes dramatically across years, then it may not make sense to think of effects of motherhood surrounding a particular quantile of person-years to be indicative of effects experienced by a group of women. Thus, we examine the extent of rank-invariance in Figure 1, which shows the proportion of white women[21] whose percentile in the wage distribution differed by at least one third (33 percentage points) or one sixth (17 percentage points) between 1996 (chosen as a year near the center of the period) and each other year. The asymmetry in the curves reflects the

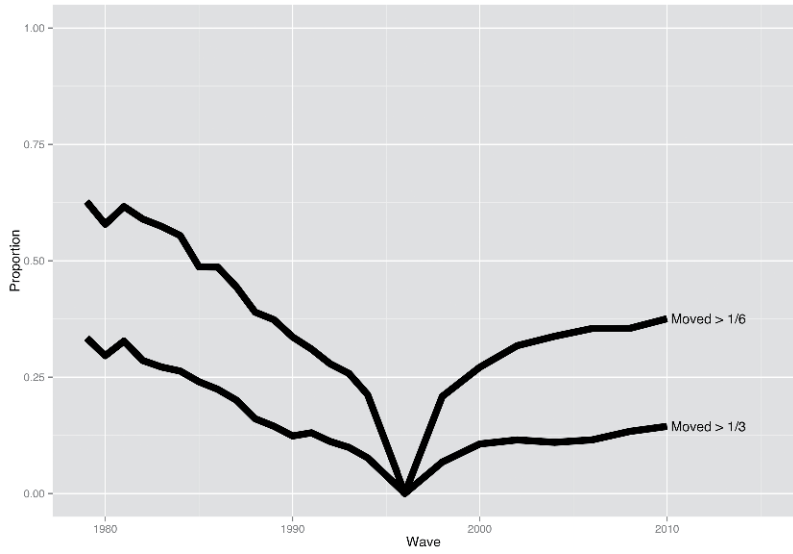

Figure 1. Proportion of White Women Whose Wage Rank Changes by One Third or One Sixth, Between 1996 and Other Waves

well-known fact that wages among young adults in their first years of employment are often erratic and less predictive of their later wages; this is why high proportions of women deviate at least a sixth or a third from their 1996 rank in the early years of the survey (they were 14 to 21 years of age in 1979, but years when women had a wage but were also a student are removed from all calculations). Focusing on the right-hand half of the curves, the "moved > 1/3" curve is at .14 for 2010, showing that only 14 percent of women moved at least 33 percentage points in rank between 1996 and 2010. The "moved > 1/6" line is at .38 for 2010, showing that 38 percent of women changed their percentile by 17 points or more between 1996 and 2010. If we consider moving less than a third an acceptable degree of rank-invariance, it is encouraging that in all pairs of years, less than a third of women moved that far in rank, even when we include the early, more erratic years.

Getting at the same issue a different way, Figure 2 shows the bivariate correlations (Pearson R) between a woman's wage percentile in 1996 and each other year. The correlations range from a low of .25 in the early years, to 1 (by definition) for 1996, to a low in post-1996 years of .59 in 2010.

Overall, Figures 1 and 2 show there is a good deal of stability in how a given woman's wage ranks relative to her peers across the years. This gives us confidence that referring to wage "groups" and comparing these groups' motherhood penalties is sensible.

Computing wage and skill groups' rates of return to experience and tenure from regression results. To explore our hypothesis that groups with higher rates of return to experience have higher motherhood penalties, we compute the rates of return to experience and tenure for groups defined by wage quantile and skill from regression results. Equation 4 includes experience and tenure (along with all other variables), providing rates of return at various quantiles. However, one cannot read these rates of return directly from regression coefficients, due to complexities of our models—the quadratic form in which experience and tenure are entered, and the fact that experience, tenure, and their squares are each interacted with education. To obtain an average rate of return

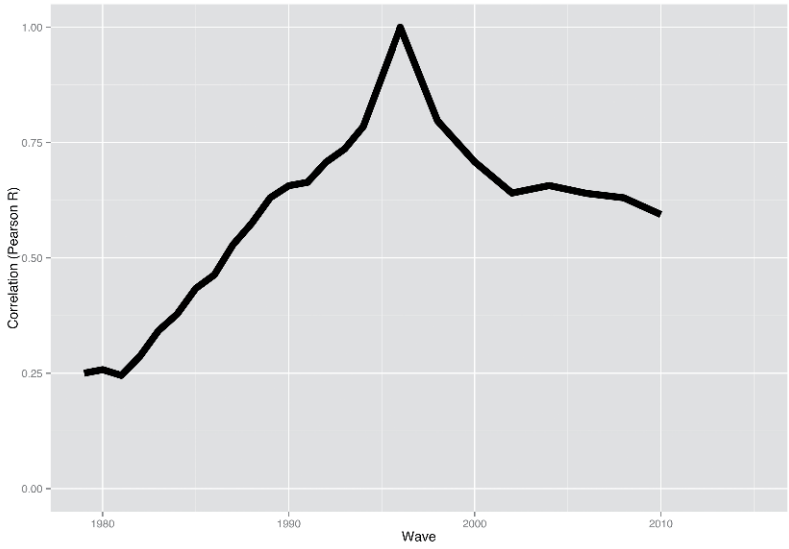

Figure 2. Correlation between White Women's Wage Ranks, between 1996 and Other Waves

for experience from additive and interactive coefficients involving experience and in our estimated Equation 4, we compute point estimates of coefficients for experience and its square for each race-by-skill-by-education group at two quantiles of interest, the 20th and 80th (to represent low and high wages). Then, using the coefficients for each experience and its square, for each race-by-skill group, at each of the two quantiles (20th and 80th), we take a weighted average across the four education groups, weighting by the race- and skill-specific proportion of the sample (of person-years) that is in each education group. We use these averaged coefficients of experience and its square for each race-by-skill-by-wage group to predict the increase in ln wage women gain for each group in each successive year of experience, 1 to 30. After computing these point estimates, we repeat this process 1,000 times on bootstrapped samples, to compute standard errors, and then repeat these steps for each year 1 to 30. For the standard errors of the differences between any two rates of return (e.g., more and less skilled white women at the 80th quantile), we perform the analogous procedure, bootstrapping the differences. We follow the same procedure to get returns to tenure at the 20th and 80th wage quantiles, separately by combinations of race and skill.

RESULTS: HOW PENALTIES VARY BY SKILL, WAGE, AND RACE

Total Motherhood Penalties

Hypotheses 1 and 2: effects of group differences in levels and returns to experience on total penalties. The first two hypotheses disagree on whether disadvantaged or advantaged women will have higher motherhood penalties. Hypothesis 1 posits that advantage is associated with a higher return to experience, so any time out of employment for childrearing leads to greater wage loss upon return and thus higher motherhood penalties. Hypothesis 2, however, posits that advantage is associated with lower penalties, because advantaged women are employed more continuously and have more experience.

Our test of this begins in Table 1. Equation 3, with fixed effects, our preferred specification for the total penalty, shows that, among white women, highly skilled women with high wages have the highest motherhood

penalty per child. Highly skilled women with wages at the 80th percentile lose 10 percent in wage for each child, and this penalty is significantly larger than that for highly skilled women with wages at the 20th percentile, or that for less skilled women with wages at either the 20th or 80th percentile. These latter three groups have lower penalties, between 4 and 7 percent. Although we prefer Equation 3 because fixed effects provide some protection from omitted variable bias, in fact, Equations 1, 2, and 3 all yield estimated penalties of 10 to 12 percent for highly skilled women with high wages (Table 1).

The conclusion that the highest total penalties are to women who combine high wages and high skill is also supported when, as a sensitivity test, we use an alternative specification similar to that used by Wilde and colleagues (2010), with an indicator for whether the woman is eight or more years beyond having a first child (relative to a reference of having no child), with a control for number of additional children beyond the first. Results for that specification show that penalties increase across time (results not shown). For simplicity, Table A2 in the Appendix just shows penalties at their highest point at eight or more years past the birth. In Equation 3, highly skilled women at the 80th percentile of wages have a 21 percent penalty, whereas the penalties for the other three groups (high/low, low/high, and low/low) are all lower, between 6 and 15 percent.

Taking all results together, we conclude that Wilde and colleagues (2010) were right that highly skilled women have higher motherhood penalties, but with the caveat that this is true *only* if they also have high wages. Of course, highly skilled women *are* typically earning higher wages; for example, in 1996, 64 percent of white women in the top third of skills had wages in the top third, whereas only 32 percent of white women in the bottom two thirds of the skill distribution were in the top third of wages. (Results calculated from the observations row of Table 2.) Budig and Hodges's (2010) conclusion that lower-wage women have higher penalties no longer holds, mainly due to our use of UQR instead of CQR. Among women with lower skills, we find some hint of their finding, in that the penalty is significantly higher for women at the 20th than the 80th wage quantile (Table 1, Equation 3), but the difference is only 2 percentage points (from 5 to 7 percent); moreover, an examination of more detailed quantiles (see Table S1 in the online supplement) shows that, among women with lower skills, the relationship is actually curvilinear, with penalties slightly larger near the middle wage quantiles.

Hypothesis 1 describes the higher penalties for more-skilled women with high wages flowing from the higher rates of return to experience that advantaged women will have. Figure 3 shows that, among white women, more-skilled women do indeed have higher rates of return to experience. The figure shows rates of return to experience for white women in four groups—high- and low-skill women, at the 20th and 80th quantiles—calculated from the quantile regressions in Table 1, Equation 4. Women with high wages and high skill have the highest rates of return to experience. At every number of years of experience 1 through 30, their returns are significantly higher than all three other groups (results on significance not shown). Figure S1 in the online supplement shows that these women also have the highest returns to tenure; the four groups have the same rank order in returns to tenure and experience. Thus, the evidence for white women is consistent with Hypothesis 1: white women with high skill and high wages have the highest rates of return to experience, whether it is all experience or the portion that is tenure with their current employer. Their high returns to experience and tenure mean that loss of every year of work caused by motherhood is much more costly for their future wages, even in proportionate terms, than it is for other groups of women. The remaining three wage/skill groups all have significantly lower rates of return to experience and tenure, rates that do not differ dramatically from each other. Thus, we do not try to explain these smaller differences.

Table 1. White Women's Motherhood Penalty per Child by Skill: At Selected Quantiles and From Mean Regressions

	Quantiles			Gradient		
	.2	.5	.8	.8–.2	Diagonals	Mean Regression
Equation 1: Wave, Age, Geography, Educational Attainment, and Age × Educational Attainment						
High Skill	–.054***	–.080***	–.102***	*	/ *n.s.*	–.093***
Difference between Skill Groups	*n.s.*	*n.s.*	**			*
Low/Mid Skill	–.060***	–.062***	–.032**	*	\ *n.s.*	–.059***
Equation 2: All of the Above, Marital Status, and (if married) Spouse's Earnings						
High Skill	–.052***	–.085***	–.118***	***	/ **	–.103***
Difference between Skill Groups	*n.s.*	*n.s.*	**			**
Low/Mid Skill	–.062***	–.063***	–.036***	*	\ *n.s.*	–.062***
Equation 3: All of the Above and Woman-Fixed-Effects						
High Skill	–.044***	–.066***	–.098***	***	/ **	–.084***
Difference between Skill Groups	*	*n.s.*	***			*n.s.*
Low/Mid Skill	–.066***	–.080***	–.045***	*	\ *n.s.*	–.063***
Equation 4: All of the Above, Experience, Experience × Educational Attainment, Whether Works Full-Time, Whether Works Full-Time × Educational Attainment, Tenure, and Tenure × Educational Attainment						
High Skill	–.023***	–.013	–.037**	*n.s.*	/ *n.s.*	–.040***
Difference between Skill Groups	*n.s.*	*n.s.*	*			*n.s.*
Low/Mid Skill	–.031***	–.023***	–.002	**	\ *	–.024**

Note: Table entries are coefficients from regression models. Entries in the Gradient column show the significance tests for differences between coefficients at quantiles .2 and .8. Entries in the Diagonals column show significance tests for coefficients on the diagonals: "/" indicates the difference between the coefficients for high-skilled women at quantile .8 and low-to-middle-skilled women at quantile .2; "\" indicates the difference between the coefficients for high-skilled women at quantile .2 and low-to-middle-skilled women at quantile .8.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Table 2. White Women's Means by Wage Third and Skill at Three Selected Waves (1986, 1996, and 2006)

		1986			1996			2006		
		Wage Third			Wage Third			Wage Third		
	Skill	Bottom	Middle	Top	Bottom	Middle	Top	Bottom	Middle	Top
Hourly wage (1996$)	High	5.099	9.074	15.621	5.166	9.132	23.001	5.669	9.089	24.240
	Low/Mid	5.008	8.765	14.820	5.412	8.961	17.779	5.507	8.989	18.670
Mother	High	.531	.320	.279	.889	.740	.659	1.000	.783	.755
	Low/Mid	.577	.426	.361	.882	.791	.683	.887	.863	.797
Educational attainment										
Less than high school	High	.083	.030	.011	.062	.038	.016	.059	.022	.007
	Low/Mid	.328	.209	.084	.272	.200	.063	.268	.164	.055
High school	High	.483	.294	.165	.358	.313	.136	.353	.304	.182
	Low/Mid	.520	.537	.487	.506	.522	.376	.470	.531	.432
Some college	High	.248	.260	.232	.235	.344	.211	.412	.283	.248
	Low/Mid	.125	.168	.225	.171	.206	.270	.202	.233	.313
College	High	.186	.416	.592	.346	.305	.637	.176	.391	.563
	Low/Mid	.028	.085	.204	.052	.072	.292	.060	.073	.200
Marriage										
Single, not yet married	High	.262	.424	.397	.099	.137	.136	.088	.054	.091
	Low/Mid	.307	.330	.366	.081	.097	.138	.060	.061	.065
Married, living together	High	.628	.485	.515	.765	.687	.743	.765	.696	.748
	Low/Mid	.549	.554	.492	.645	.684	.683	.613	.676	.684
Separated, divorced, or widowed	High	.110	.091	.088	.136	.176	.122	.147	.250	.161
	Low/Mid	.144	.115	.141	.275	.219	.179	.327	.263	.252
Works full-time	High	.634	.831	.835	.457	.740	.721	.353	.620	.811
	Low/Mid	.598	.829	.764	.572	.756	.815	.577	.779	.816
Cumulative years of work experience	High	4.822	5.660	5.984	11.538	13.383	14.656	17.148	20.028	23.824
	Low/Mid	3.959	5.797	6.492	9.818	13.242	14.898	16.404	20.675	23.617
Cumulative years of tenure at current job	High	1.349	2.102	2.760	2.303	4.880	6.021	3.523	4.954	9.258
	Low/Mid	1.246	2.470	2.785	2.099	4.517	6.434	2.992	6.810	9.662
Observations	High	145	231	272	81	131	369	34	92	286
	Low/Mid	577	469	191	346	320	319	168	262	310

Note: Cumulative work experience and tenure are in years: the sum total of hours worked, divided by 2000. Skill is measured by test scores, adjusted for age; although this table pertains to white women only, cutting points for the thirds refer to the weighted sample of all race/ethnic groups of women prior to any deletions.

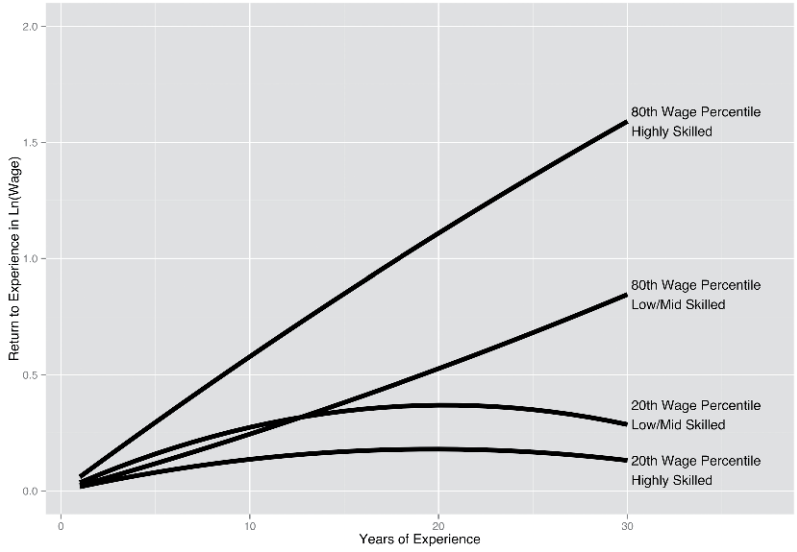

Figure 3. White Women's Return to Experience, by Wage Quantile and Skill
Note: Based on results from Equation 4, Table 1. Curves show how much ln wage increases from 0 to each year of experience for each of the four groups. Differences in groups' initial ln wage levels are not shown, but set to 0 when experience is equal to 0.

Examining the occupations of women/years that are in the highest skill third as well as in the top third of wages[22] may help the reader envision the kinds of work in which women are experiencing high wages, high returns to experience and tenure, and high motherhood penalties. The 10 most common occupations for white women in the top third of skills and wages are nurses, teachers, therapists, accountants, and managers of various types (financial, medical and health service, marketing and sales, human resource, managers of administrative support workers, and a residual category). (Detailed results available upon request.) It is a mix of predominantly male and predominantly female occupations. The relatively high rates of return to experience and tenure these women enjoy may reflect access to raises and promotion in these largely professional and managerial occupations, compared with occupations the other three groups of women are typically in. The most common occupations in the other three groups, featuring either lower skills or low wages, are not professional or managerial; they include secretarial and administrative positions, food service, home health aides, and retail sales.

Why was Hypothesis 2, which said that groups with less experience will have higher penalties, not upheld? First, we note that Hypothesis 2 *is* correct in positing that more advantaged women have more experience, as past research shows. In these data, among white women, highly skilled women with high wages have the highest levels of experience. For example, Table 2 shows that in 2006, white women in the top third of skills and the top third of wages averaged 24 years; white women with low or medium skills, and wages in the bottom third, however, averaged only 16 years. Indeed, in later years, the cumulative experience of women in the top third of skills is very close to their estimated potential experience, computed as their age minus 6 more than the years of schooling they completed; they are employed quite continuously, more so than any other wage/skill group (results not shown). Thus, other things being equal, based on their higher experience *levels*, we would expect lower penalties for women who are highly skilled and have high

wages, as Hypothesis 2 says, yet we found the opposite. This is probably because, to the extent that experience is relevant, group differences in penalties are a multiplicative function of group differences in experience levels and in returns. Highly skilled, high-wage women have more experience, which, other things being equal, reduces their penalties; but other things are not equal, as these women also have the highest returns to experience. Their high returns appear to dominate in affecting group differences in penalties through making even the small amounts of time taken out of employment for childrearing quite damaging to their future wages.

The online supplement provides results regarding penalties for black women, which we summarize briefly here. We found that total penalties are lower for black than for white women; when we pool across skill groups and do simple mean regressions, penalties are 7 to 8 percent for white women but only 3 to 4 percent for black women (see Table A1 in the Appendix for white women and Table S1 in the online supplement for black women; Equations 1, 2, and 3). Black women undoubtedly experience race discrimination that white women do not, but it is unclear how this would lead to lower motherhood penalties. Moreover, black women's lower total penalties are not due to lower returns to experience or tenure; race differences in these returns are not significant (results not shown). Black women do have lower wages and lower average cognitive skill (at least as measured by AFQT), but this does not appear to be the reason for their lower motherhood penalties. We conclude this from two findings in the online supplement (Table S2): (1) Unlike the pattern for white women, total penalties do not generally differ significantly for black women by skill or quantile, suggesting that if their average wages and skill were to rise, this would not increase their penalty. (2) When we compare black and white women at the same (white) wage quantile and skill level, black women's penalties are generally lower. The lower penalties for black than for white women fit our general conclusion that more advantaged women have higher penalties, but we are unable to offer an explanation of these racial differences in penalties.

Hypotheses 3 and 4: effects of group differences in how motherhood affects productivity or discrimination, and thus affects penalties net of experience. Hypotheses 3 and 4 concern penalties that do not flow through experience but relate to other mechanisms, involving performance or discrimination, and are seen net of experience. Hypothesis 3 says that advantaged women, those with high skills and high wages, will have higher net motherhood penalties because they are more likely to be in jobs in which any decline in performance because of motherhood has a large effect (or perceived effect) on their organizations' bottom line. Hypothesis 4 says that disadvantaged women, with low skills or low wages, will have higher net motherhood penalties because they have less power to negotiate job flexibility and less money to purchase services needed to avoid negative effects of motherhood on their performance (or perceived performance).

To examine these competing hypotheses, we consider the net penalties from Equation 4 in Table 1, which contains controls for experience, tenure, and hours. When these controls are entered, penalties drop by half or more (compare Equations 3 and 4 in Table 1), so this half or more of the motherhood penalty results from losing experience or tenure.[23] Hypothesis 3 says that women with high wages and high skills suffer the highest penalties, and Table 1 (Equation 4) shows this is true. Their penalty is 4 percent per child, and this is significantly more than the near 0 penalty of women with low skill and high wages. However, it is not significantly different than the 2 to 3 percent penalty experienced by women at low wage levels and high or low skill levels, respectively. Our alternative specification in Table A2 in the Appendix also shows that the net motherhood penalty is largest for women with high skills and high

wages, but this penalty is not significantly larger than that suffered by low-wage women, whether their skills are high or low. [24] Hypothesis 4 says that women with low skills or wages have higher penalties. As Table 1 shows, contrary to the hypothesis, the penalty for women with low skill and low wages, 3 percent, is nonsignificantly different from the penalty for women with high skill and low wages (2 percent). However, consistent with the hypothesis, among low-skilled women, penalties do rise significantly as wages go from the 80th to 20th quantile, albeit only from 0 percent to 3 percent. Thus, we find some support for each of the two conflicting hypotheses as regards net penalties in our main specification, but the differences are quite small. Overall, we conclude that penalties net of experience and seniority do not differ consistently or strongly by wage and skill among white women. Moreover, our analysis in the online supplement (Table S3) shows that net penalties do not differ by skill or wage for black women. We take these estimated net penalties to tap effects of motherhood on performance or employer discrimination based on motherhood, and our conclusion is that, for the most part, these penalties are not distinctive for highly paid women with high skills.

CONCLUSIONS AND DISCUSSION

Who suffers larger motherhood wage penalties—advantaged or disadvantaged women? We found that, among white women, the total motherhood penalty is highest—10 percent per child—for women with high skills and high wages. This finding is consistent with our hypothesis that high penalties are associated with high rates of return to experience; among white women, highly skilled high-wage women have the highest rates of return to experience and tenure, as well as the highest motherhood penalties. Group differences in the part of the total penalty explained by experience will necessarily be a multiplicative function of group differences in rates of return to experience and group differences in levels of experience. In the case of highly skilled white women with high wages, what is striking is that they have the highest penalties *despite the fact* that they have the most continuous experience of any group of women, which, other things being equal, would reduce their penalties. The evidence suggests that their penalties are highest because when some of these women lose experience to childrearing, by dropping out or shifting to part-time work for a short time, their steep experience-wage slopes make even these small amounts of lost experience very expensive; they lose the steep wage growth they would have enjoyed had they worked continuously.

Revisiting the two articles that inspired our analysis, we conclude that Wilde and colleagues (2010) were correct that highly skilled women have higher total penalties, but with the important caveat that this applies only to women who have high wages as well. Budig and Hodges's (2010) previous finding that higher-wage women suffer smaller penalties is modified by our analysis, because we adopt the appropriate unconditional quantile regression (UQR) model and examine penalties separately by skill: among skilled women, high-wage women have worse total penalties, the opposite of Budig and Hodges's finding. Among women with lower skills, total penalties are curvilinear and highest at the middle, although the difference is small and the explanation unclear.

We also estimated *net* motherhood penalties, those estimated controlling for experience; these analyses cannot distinguish how much of net penalties result from employer discrimination against mothers or reduced job performance due to motherhood. Rather, these two are lumped together in the net penalty. This net penalty was not significantly higher (or lower) for women with high skills and high wages than it was for women with low wages, whether their skill was high or low. Thus, there is no clear evidence that this group suffers any more or less discrimination on the basis of motherhood, or that their job performance is more or less affected by motherhood.

Our analysis focused on white women, but we also showed that, taken as a whole, penalties are lower for black than for white women, although there are not significant differences in the returns to experience between the groups. Among black women, penalties did not differ significantly by skill or wage.

Overall, our analysis shows that privilege (on race, wage, or skill) has its price—larger proportionate motherhood penalties. In the case of privilege on wage and skill, the penalty arises because when highly skilled women are in high-wage jobs, they have high rates of return to experience, and these steep wage trajectories make them lose large amounts of wage growth during the typically small amounts of time they take out. One clear lesson from our results is the importance of not confusing the disadvantage of low skill, low wage levels, or membership in a disadvantaged racial group, with the disadvantage of high motherhood penalties. These disadvantages typically afflict different women. Black women have lower wages than white women, but they have lower total penalties for motherhood. Among white women, those with the highest total motherhood penalties are in an advantaged group with high skills and high wages; even after they become mothers and suffer the steepest penalty, they are typically quite affluent because their own earnings are still high relative to those of other women, and many of them are married to relatively high-earning men. Nonetheless, when it comes to their own pay, motherhood is the most costly for them.

APPENDIX

Table A1. White Women's Motherhood Penalty per Child: At Selected Quantiles and From Mean Regressions

	Quantiles			Gradient	
	.2	.5	.8	.8−.2	Mean Regression
Equation 1: Wave, Age, Geography, Educational Attainment, and Age × Educational Attainment	−.058***	−.067***	−.053***	*n.s.*	−.069***
Equation 2: All of the Above, Marital Status, and (if married) Spouse's Earnings	−.060***	−.071***	−.062***	*n.s.*	−.075***
Equation 3: All of the Above and Woman-Fixed-Effects	−.057***	−.075***	−.066***	*n.s.*	−.071***
Equation 4: All of the Above, Experience, Experience × Educational Attainment, Whether Works Full-Time, Whether Works Full-Time × Educational Attainment, Tenure, and Tenure × Educational Attainment	−.028***	−.020***	−.016*	*n.s.*	−.030***

Note: Table entries are coefficients from regression models. Entries in the Gradient column show the significance tests for differences between coefficients at quantiles .2 and .8.
*$p < .05$; ***$p < .001$ (two-tailed tests).

Table A2. White Women's Motherhood Penalty Eight or More Years After the First Birth by Skill: At Selected Quantiles and From Mean Regressions

	Quantiles			Gradient		
	.2	.5	.8	.8–.2	Diagonals	Mean Regression
Equation 1: Wave, Age, Geography, Educational Attainment, and Age × Educational Attainment						
High Skill	–.086***	–.202***	–.275***	**	/ *n.s.*	–.200***
Difference between Skill Groups	*n.s.*	*n.s.*	*			*n.s.*
Low/Mid Skill	–.119***	–.161***	–.093*	*n.s.*	\ *n.s.*	–.126***
Equation 2: All of the Above, Marital Status, and (if married) Spouse's Earnings						
High Skill	–.089***	–.214***	–.311***	**	/ *	–.220***
Difference between Skill Groups	*n.s.*	*n.s.*	**			*n.s.*
Low/Mid Skill	–.128***	–.163***	–.097*	*n.s.*	\ *n.s.*	–.135***
Equation 3: All of the Above and Woman-Fixed-Effects						
High Skill	–.105***	–.186***	–.211***	*	/ *n.s.*	–.187***
Difference between Skill Groups	*n.s.*	*n.s.*	***			*n.s.*
Low/Mid Skill	–.148***	–.185***	–.055*	**	\ *n.s.*	–.156***
Equation 4: All of the Above, Experience, Experience × Educational Attainment, Whether Works Full-Time, Whether Works Full-Time × Educational Attainment, Tenure, and Tenure × Educational Attainment						
High Skill	–.066***	–.097***	–.146***	*n.s.*	/ *n.s.*	–.133***
Difference between Skill Groups	*n.s.*	*n.s.*	***			*n.s.*
Low/Mid Skill	–.071***	–.078***	.020	***	\ **	–.077**

Note: Table entries are coefficients from regression models. Entries in the Gradient column show the significance tests for differences between coefficients at quantiles .2 and .8. Entries in the Diagonals column show significance tests for coefficients on the diagonals: "/" indicates the difference between the coefficients for high-skilled women at quantile .8 and low-to-middle-skilled women at quantile .2; "\" indicates the difference between the coefficients for high-skilled women at quantile .2 and low-to-middle-skilled women at quantile .8. Models also adjust for additional children; see Appendix Table A3.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Table A3. White Women's Motherhood Penalty per Additional Child After the First by Skill: At Selected Quantiles and From Mean Regressions

	Quantiles			Gradient		
	.2	.5	.8	.8–.2	Diagonals	Mean Regression
Equation 1: Wave, Age, Geography, Educational Attainment, and Age × Educational Attainment						
High Skill	–.038**	–.037	–.053	*n.s.*	/ *n.s.*	–.060**
Difference between Skill Groups	*n.s.*	*n.s.*	*n.s.*			*n.s.*
Low/Mid Skill	–.039***	–.032*	–.020	*n.s.*	\ *n.s.*	–.039***
Equation 2: All of the Above, Marital Status, and (if married) Spouse's Earnings						
High Skill	–.037**	–.039	–.057	*n.s.*	/ *n.s.*	–.063**
Difference between Skill Groups	*n.s.*	*n.s.*	*n.s.*			*n.s.*
Low/Mid Skill	–.040***	–.034**	–.022	*n.s.*	\ *n.s.*	–.041***
Equation 3: All of the Above and Woman-Fixed-Effects						
High Skill	–.021*	–.030**	–.078***	**	/ *	–.058**
Difference between Skill Groups	*n.s.*	*n.s.*	*n.s.*			*n.s.*
Low/Mid Skill	–.041***	–.049***	–.048***	*n.s.*	\ *n.s.*	–.038**
Equation 4: All of the Above, Experience, Experience × Educational Attainment, Whether Works Full-Time, Whether Works Full-Time × Educational Attainment, Tenure, and Tenure × Educational Attainment						
High Skill	–.011	.011	–.011	*n.s.*	/ *n.s.*	–.016
Difference between Skill Groups	*n.s.*	*n.s.*	*n.s.*			*n.s.*
Low/Mid Skill	–.019*	–.005	–.009	*n.s.*	\ *n.s.*	–.010

Note: Table entries are coefficients from regression models. Entries in the Gradient column show the significance tests for differences between coefficients at quantiles .2 and .8. Entries in the Diagonals column show significance tests for coefficients on the diagonals: "/" indicates the difference between the coefficients for high-skilled women at quantile .8 and low-to-middle-skilled women at quantile .2; "\" indicates the difference between the coefficients for high-skilled women at quantile .2 and low-to-middle-skilled women at quantile .8. Models also adjust for years since first birth; see Appendix Table A2.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Authors' Note

Jonathan Bearak's affiliation is included for informational purposes only; this work was not conducted under the auspices of the Guttmacher Institute. The views expressed here are those of the authors and do not necessarily reflect the views of the Guttmacher Institute.

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Notes

1. See Wilde and colleagues (2010) for a critical discussion of the instruments used in this literature.
2. Budig and England (2001) find that motherhood penalties differ little by occupational level, and Loughran and Zissimopoulos (2009) find no significant relationship between education and the penalty. Amuedo-Dorantes and Kimmel (2005) and Taniguchi (1999) find higher penalties for less educated women, Waldfogel (1997) finds them higher among well-educated women, and Anderson and colleagues (2002, 2003) find them higher at middle levels of education. Buchmann and McDaniel (2016) find no motherhood penalty in very recent data for women in high-level male-dominated professions (they do not examine nonprofessional women). Pal and Waldfogel (2016) find a complicated pattern of differences between education groups in levels and trends in motherhood penalties.
3. Recent studies have complicated this finding. Glauber (2013) used successive Current Population Survey cross-sections and reports that black women's motherhood penalties, initially much smaller than white women's, increased, moving toward convergence in the 1990s, and then diminished again somewhat in the 2000s. Pal and Waldfogel (2016) report similar findings. These analyses using the CPS could not control for experience, as the survey does not gather this information. Also, we can be less sure that motherhood effects in these cross-sectional analyses are causal than is the case for results from panel data with fixed effects.
4. Past studies have also found that women earn less per hour when working part-time, and that, because mothers are more likely to work part-time, this explains part of the penalty (Budig and England 2001; Staff and Mortimer 2012; Waldfogel 1997). This is presumably because employers often pay less per hour in such jobs. Working part-time could also have a longer-term influence on wage growth if the rate of return to experience in part-time jobs is less than in full-time jobs. As we will discuss, our measures of experience and tenure calibrate them by the hour, such that a week of 20 hours/week employment counts half as much as a week of 40 hour/week employment. Thus, effects of *past* part-time (rather than full-time) work are part of our "experience effect." We also control for whether the current job is part-time in our model assessing net penalties.
5. It is important to distinguish between the earnings forgone during nonemployment (when one is earning nothing), and the forgone increases in one's post-return *wage rate* from the same period of nonemployment. Our models and our discussion about returns to experience affecting the motherhood penalty refer to the latter, not the former.
6. Correll and colleagues (2007) also present a random-assignment experiment with undergraduate subjects asked to evaluate candidates for a position and assign a salary. They were given a packet of materials on the candidates, including past performance information, and a mention of whether they have children. With comparable packets and past reviews, mothers were treated less favorably and seen as less able. Correll and colleagues argue that this is status-based stereotyping, not merely statistical discrimination, given that substantial information on past productivity was available about each candidate.
7. Gallen (2015) provides more indirect evidence that women and men differ in productivity because of women's motherhood burden. Using Dutch data, she finds that firms with a higher proportion of women workers had lower revenue, and this differential was associated with mothers.
8. Respondents in the NLSY79 were interviewed annually up to the 1994 survey and bi-annually thereafter. We use one more wave of data than Wilde and colleagues (2010), and two more than Budig and Hodges, because they were available. However, we did sensitivity tests deleting the last two years, and they show this is unimportant to differences between our results and those of either set of authors.
9. Another reason to model interactions between motherhood and race is to avoid biased conclusions about how penalties vary by cognitive skills. Because black women average lower test scores, and past research has found black women to have lower motherhood penalties, the lower penalties Wilde and colleagues (2010) attributed to lower cognitive skills may actually have been caused by some other distinctive features of black women's situations. The additive control for race implicit in fixed-effects modeling does not adequately deal with this problem. Given NLSY oversampling, black women are 28 percent of our analytic sample of person-years, but when we divide our analytic sample into women who are in the top third versus the bottom two thirds of the (all race) AFQT distribution, the top third is 6 percent black, whereas the bottom two-thirds is 40 percent black. Thus, penalties for the top third reported by Wilde and

colleagues (2010) are almost entirely penalties experienced by white women. Our analyses do not have this problem, because we produce separate estimates by race crossed with skill.

10. We exclude person-years in which women were enrolled in school part-time or full-time in a school that gives either a high school degree or a bachelor's or graduate degree. The rationale is as follows: effects of motherhood in fixed-effects models reveal average within-person wage differences between years before and after motherhood; thus, including years in which women had low-paying, part-time jobs while in school may lead to underestimation of the motherhood penalty. That is, if women hold low-paid jobs in school, then graduate and work in higher-paying jobs, then have a child, then return to work at a wage that reflects some motherhood penalty relative to their post-school, pre-child wages, fixed-effects estimation will underestimate this differential because the "pre-motherhood" wages are an average of their lower and higher pre-child wages.
11. Another reason for using women of all races to determine thirds of the AFQT distribution is so the cutting points between categories are the same for white and black women. This is important, because, although our main analyses are only for white women, in the online supplement we show results for a parallel analysis for black women. Because we used the full sample of all races to form test score thirds, in our analyses the same test score cuts the skill groups for both black and white women. But are test scores biased for black women? Rodgers and Spriggs (1996) use the 1991 wave of the NLSY79 and divide the AFQT into two scales, one that deals with reading and another that deals with math. They show that reading skills increase the wage earned by blacks but not whites, and math skills increase the wage earned by whites but not blacks. We examined this for our analytic sample of black and white women's person-years (through 2010), using an OLS regression to predict wage from the two AFQT scales, education, year, geography, and number of children (all variables operationalized as in our models in Table 1). We found significant positive effects of the math skill scale for both black and white women. The puzzling finding was that reading skills, which had significant positive effects for black women, had significantly negative effects for white women. However, when these scales are combined into the overall AFQT measure we use here, it has a significant positive effect on the earnings of both black and white women and the effects are significantly stronger for black women. Thus, although we do not know whether AFQT measures black women's skills as accurately as it does those of white women, we believe it is a meaningful measure of skills that are rewarded in the labor market for black as well as white women. Our purpose in using AFQT is not to explain race differences in earnings, but to test Wilde and colleagues' (2010) hypothesis that motherhood penalties are higher for women with higher cognitive skills, for both black and white women.
12. Specifically, we enter a set of indicator variables for whether, in this person-year, the number of years that have passed since the woman first became a mother is less than one year, one year, two to four years, five to seven years, or eight or more years. The reference category is years by which women have not given birth. (Women who never have a first child, as well as women who have a first but never have a second, are scored 0 on the indicator variable for all their person-years; eliminating women who never became mothers changes results little.) The analysis allows us to see how the effects of being a mother spread out over time. We add a single control variable for the number of additional children (after the first) the woman has had.
13. Budig and Hodges (2010) control for age, although not its square, whereas Wilde and colleagues (2010) control for potential experience (age minus education minus 5) and its square.
14. We also include a category for "MSA not known," which applies to a tiny fraction of cases.
15. Budig and Hodges (2010) and Wilde and colleagues (2010) measure educational attainment in years, forcing the effect to be linear. Our categories are formed from number of years, assuming that less than 12 is less than high school, 12 is high school only, 13 to 15 is some college, and 16 or more is college graduate.
16. Like Wilde and colleagues (2010), we derive years of experience from cumulative hours, whereas Budig and Hodges (2010) derive it from cumulative weeks, without attention to how many hours/week a woman worked. We control for experience and its square, like Wilde and colleagues (2010); however, unlike Wilde and colleagues (2010), we also add tenure (with current employer). Budig and Hodges (2010) use a linear term for experience and for tenure. Budig and Hodges (2010) also include the respondent's usual weekly work hours, the number of weeks the respondent worked in the prior year, and whether the respondent changed employers between waves; we exclude all of these. Like Budig and Hodges (2010), we follow standard practice by including tenure in experience. Thus, if an individual has a total of 10 years of employment experience, of which 3 are with her current employer, she is coded as having 10 years of experience and 3 years of tenure.
17. Fortunately, including both age and experience in the same models does not create problematic collinearity, because women vary substantially in how much experience they accumulate with age. One could, however, be concerned about our inclusion in our most saturated models of interactions

between education and experience, given that this interaction term might be highly correlated with the (also included) interaction between education and age. We ascertained that the returns to experience we estimate for wage-by-skill categories among white women are virtually identical if we instead estimate them from a model that excludes the interaction of education and experience. (See later text for how these experience returns are estimated.)

18. Budig and Hodges (2010) also control for whether the current job is part-time, whereas Wilde and colleagues (2010) do not.
19. The failure of the variable indicating whether a woman is currently working part-time to explain much of the motherhood penalty may arise partly because the measure of hours of experience itself captures directly some of the effects of having worked part-time in the past; working 20 hours/week over a year will lead to cumulating only half as many hours as working 40 hours/week over the same year.
20. As an example, imagine a CQR with the log of wages as the outcome and just two regressors: motherhood and education. In such a CQR model, if the motherhood penalty is 5 percent at the 80th percentile and 15 percent at the 20th percentile, this means mothers who have high wages for their education level have a smaller wage penalty than do mothers who have low wages for their education. Therefore, results from a CQR model cannot be interpreted as the effect of motherhood on high-wage and low-wage workers, because, since education strongly affects earnings, even women who have low wages for their high education level may earn more than many workers whose wages are low compared to women overall.
21. Figures 1 and 2 on wage rank-invariance are almost identical if black women are included in the computations.
22. One might be interested in the size of this high-wage, high-skill group, with its distinctively high experience returns, relative to the other three groups. Figure 3 pertains to high or low-to-medium (called low) skill groups of women at the 80th and 20th quantiles of person/years of wages, but there is almost no one at a single wage percentile, so to get an idea of the relative size of groups of observations that are in the various wage-by-skill groups, we approximate high wage as the top third (of person-years) and low wage as the bottom third. These are in turn divided into observations belonging to women in the high- or low-skill groups. We find that high (top third) wage observations belonging to women in the top third of skills make up 18 percent of all person-years. A larger group, 26 percent, are low on both: these women are in the bottom third of the wage distribution and the bottom two-thirds of the skill distribution. As for the two discordant groups, with low/high or high/low combinations, only 7 percent of observations are in the top third of the skill and the bottom third of the wage distribution, and 15 percent are in the bottom two thirds of the skill and top third of the wage distribution.
23. In results not shown, it is apparent that most of this reduction comes from the addition of experience. The small effect of controlling for whether the current job is part-time may be because the experience indicator measures cumulative hours worked since the year the survey began, and thus captures years worked as well as how much of past employment was full- or part-time. Adding tenure also leads to little change in the coefficients on number of children. Thus, having to move one's tenure to 0 after a birth is not the main motor of the effects of lost experience.
24. Our alternative specification in Table A2 examines penalties for the first child eight years after the birth, controlling for any additional children born after the first. Net penalties are largest, 15 percent, for women with high skill and high wage, but this penalty is not significantly different than that of either women with low wages and low skill or women with high skill and low wages. It is vastly and significantly larger than the 2 percent penalty for high-wage women with low skills, however.

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