THE WAGE PENALTY FOR MOTHERHOOD

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Motherhood is associated with lower hourly pay, but the causes of this are not well understood. Mothers may earn less than other women because having children causes them to (1) lose job experience, (2) be less productive at work, (3) trade off higher wages for mother-friendly jobs, or (4) be discriminated against by employers. Or the relationship may be spurious rather than causal—women with lower earning potential may have children at relatively higher rates. The authors use data from the 1982–1993 National Longitudinal Survey of Youth with fixed-effects models to examine the wage penalty for motherhood. Results show a wage penalty of 7 percent per child. Penalties are larger for married women than for unmarried women. Women with (more) children have fewer years of job experience, and after controlling for experience a penalty of 5 percent per child remains. "Mother-friendly" characteristics of the jobs held by mothers explain little of the penalty beyond the tendency of more mothers than non-mothers to work part-time. The portion of the motherhood penalty unexplained probably results from the effect of motherhood on productivity and/or from discrimination by employers against mothers. While the benefits of mothering diffuse widely—to the employers, neighbors, friends, spouses, and children of the adults who received the mothering—the costs of child rearing are borne disproportionately by mothers.

DOES motherhood affect an employed woman's wages? We provide evidence of a penalty for the cohort of American women currently in their childbearing years, and we investigate its causes. Five explanations for the association between motherhood and lower wages have been offered. First, many women spend time at home caring for children, interrupting their job experience, or at least interrupting full-time employment. Second, mothers may trade off higher wages for "mother-friendly" jobs that are easier to combine with parenting. Third, mothers may earn less because the needs of their children leave them exhausted or distracted at work, making them less productive. Fourth, employers may discriminate against mothers. Finally, perhaps the association is not really a penalty *resulting* from motherhood and its consequences at all. What appears in cross-sectional research to be a causal effect of having children may be a spurious correlation; some of the same unmeasured factors (such as career ambition) that discourage child-bearing may also increase earnings.

We build on Waldfogel's 1997 study. She uses a fixed-effects model to avoid spuriousness. Analyzing panel data spanning 1968 to 1988, and after controlling for marital status, experience, and education, she finds a wage penalty of 6 percent for mothers with one child and 13 percent for mothers with two or more children. (She does not provide information on the size of the motherhood effect before partialling out that portion caused by motherhood reducing job experience.) We

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use a similar statistical model, but analyze more recent data (1982 through 1993) and include more detailed measures to assess whether the loss of full-time experience and seniority caused by motherhood explains most of the penalty. Ours is the first analysis to distinguish among the four categories of years of full-time experience and seniority and years of part-time experience and seniority. (Seniority refers to experience with one's present employer.) We include a measure of number of employment breaks. By adding controls for a large number of job characteristics, we attempt to assess how much, if any, of the motherhood penalty results from mothers choosing or being confined to lower paying but more "mother-friendly" jobs. We also examine whether the motherhood penalty varies by marital status since a growing number of mothers are single.

WHY CARE ABOUT THE WAGE PENALTY FOR MOTHERHOOD?

A wage penalty for motherhood is relevant to larger issues of gender inequality. Most women are mothers, and women do most of the work of child rearing. Thus, any "price" of being a mother that is not experienced by fathers will affect many women and contribute to gender inequality. Of course, lower pay for employed mothers at one point in time is only the tip of the iceberg. Lifetime earnings are also lowered for those women who have a period with no earnings because they stay home caring for children full time (Davies and Joshi 1995; Joshi 1990). Gender inequality in earnings affects other gender inequalities. Lifetime earnings affect private pension income. For married women, lower earnings may affect their bargaining power with their husbands (Blumstein and Schwartz 1983; England and Kilbourne 1990). For single mothers, the motherhood penalty contributes to the gap in poverty rates between households headed by a single woman and those containing an adult male (McLanahan and Kelly 1999).

Penalties for motherhood are also relevant for theory and policy because child rearing creates broad social benefits (Coleman 1993; England and Folbre1999; Folbre1994a, 1994b; Risman and Ferree 1995). All work confers benefits on those who consume what is produced. Work that produces a physical product or a business service often has few beneficiaries beyond those who buy the product and thus indirectly pay the worker. In contrast, "caring" labor also benefits those who make no payments to the worker. Good parenting, for example, increases the likelihood that a child will grow up to be a caring, well-behaved, and productive adult. This lowers crime rates, increases the level of care for the next generation, and contributes to economic productivity. Most of those who benefit—the future employers, neighbors, spouses, friends, and children of the person who has been well reared—pay nothing to the parent. Thus, mothers pay a price in lowered wages for doing child rearing, while most of the rest of us are "free riders" on their labor.

PAST RESEARCH ON LINKS BETWEEN MOTHERHOOD AND WAGES

Several recent studies find a wage penalty for motherhood in the United States (Lundberg and Rose 2000; Neumark and Korenman 1994; Waldfogel 1997, 1998a; 1998b). A motherhood penalty has also been found in the United Kingdom (Harkness and Waldfogel 1999; Joshi and Newell 1989) and Germany (Harkness and Waldfogel 1999). Men suffer no such penalty—their wages are either unaffected (Loh 1996:580) or even increase after having a child (Lundberg and Rose 2000).

MOTHERHOOD AND REDUCED EMPLOYMENT EXPERIENCE

Mothers have high rates of employment today—for example, over 40 percent of women with children under one year of age are in the labor force (Klerman and Leibowitz 1999). Nonetheless, many women lose at least some employment time to child-rearing (Klerman and Leibowitz 1999). One explanation of the wage penalty for motherhood is based on this fact; some mothers take time out of employment, and loss of work experience affects later wages. Human capital theory predicts that experience and seniority have positive returns because they involve on-the-job training that makes workers more productive.

In this view, workers pay for a part of this training with an initially lower wage, but employers raise wages with seniority to retain their more productive, experienced workers. A return to experience is also compatible with institutional theories that see the reward as a result of organizational policies and inertia that reward experience for reasons other than its link to productivity. Either view implies that mothers will earn less if they lose any job time in child rearing.

Past studies are unclear about what part of the child penalty is explained by work experience because some authors report only penalties with or without controls for experience. Also, studies vary in how they measure experience. An early study by Hill (1979:589) reports that controlling for experience and tenure explained all the negative effect of children on women's pay. Lundberg and Rose (2000) find a 5-percent penalty for women's first birth, but they did not include controls for job experience. Waldfogel (1997:214) finds a penalty net of experience: 6 percent for one child, declining to 4 percent if controls are added for whether the current job is part-time and how much of past experience was part-time. But she doesn't report the gross penalty that includes the effects of experience that would be obtained by leaving experience out of the regression. Korenman and Neumark (1992: 246–47) find no net or gross penalty for motherhood; they find no difference in wage change across a two-year period (1980–1982) between women who experienced a birth during the period and those who had not, regardless of whether women's work experience during that interval was controlled. Perhaps the two-year interval they examined was too short to reveal the effects of motherhood on wages. None of the prior studies distinguished full-time from part-time work experience *and* whether the experience is general (i.e., with any employer) or is entirely with the current employer. These types of work experiences may differ in their returns. Corcoran, Duncan, and Ponza (1984) find smaller returns to part-time work experience compared with full-time experience. Waldfogel (1997) distinguishes between full-time and part-time experience, and finds almost identical returns, but she doesn't distinguish between general employment experience and seniority with one's current employer. Korenman and Neumark (1992) make this distinction but not between full-time versus part-time within either category. Waldfogel (1997) also distinguishes between whether the woman's current job was part-time, but Korenman and Neumark (1992) do not. Hill's (1979) analysis makes most of these distinctions (but not whether seniority was part-time). However, she uses much older data (1976) and applies an OLS statistical model that does not control for unobserved differences between mothers and non-mothers.

We distinguish among years of full-time experience, part-time experience, full-time seniority, and part-time seniority. We also include a measure of the number of employment breaks the woman has taken because continuity may influence wages—that is, among women with equal years of experience, those with more *continuous* experience may have higher earnings. For example, Felmlee's (1995) analysis of 1968–1973 data shows that women who changed employers but maintained continuous employment (defined as a break of no more than a month) were less likely to have a reduction and more likely to see an increase in wages compared with women who were out of the labor force between jobs. We examine the "gross" motherhood penalty, and then we estimate a "human capital model" that controls all these measures of experience.

Motherhood Reducing Job Effort and Productivity

According to human capital theory, losing job experience adversely affects mothers' wages because the mothers are less productive; that is, more experienced workers are more productive, therefore they are paid more. However, is there a link between motherhood and productivity that exists even among women with equal human capital? Becker's (1991) "new home economics" argues that mothers may be less productive on the job than non-mothers because they are tired from home duties or because they are "storing" energy for anticipated work at home. The assumption is that non-mothers spend more of their nonemployment hours in leisure instead of in child care or other household work and that leisure takes less

energy—thus leaving more energy for paid work. In this same vein, mothers may spend time while at work worrying about their children, calling them at home, or scheduling appointments for them. They may take sick leave to deal with children's illnesses. Mothers also may choose or be relegated to less demanding occupations because of this extra burden of the "second shift" (Hochschild 1989). This second mechanism, operating via occupational choice or placement, is explored in a later section on "mother-friendly" jobs.

No study has directly measured the effort or productivity of mothers versus non-mothers, or men versus women; prior research has approached these questions only indirectly. Bielby and Bielby (1988) analyze data from a national survey that asked respondents how "hard" their jobs require them to work, how much "effort, either physical or mental" their jobs require, and how much "effort they put into their jobs beyond what is required." Women reported slightly *more* effort than men. This is striking since other research finds that men generally overestimate and women underestimate their merit or performance (Colwill 1982). As far as we know, no research has compared mothers to non-mothers on effort measures. Since it is women's responsibility for the care of children that is presumed to create differences in effort between women and men, the absence of sex differences in effort in past research suggests that mothers and non-mothers may not differ in effort.

Our data, too, lack measures of productivity or effort. Thus, we must see this "effort" explanation of the motherhood penalty as consistent with a residual effect of motherhood not explained by other variables for which we do have measures.

Low Wages in Mother-Friendly Jobs: Compensating Differentials

Mothers may seek "mother-friendly" jobs. The features of these jobs that make them easier to combine with motherhood may compensate for their lower earnings, as predicted by economists' theory of compensating differentials. For example, following Becker (1991), mothers may choose jobs that require less energy or that have parent-friendly characteristics, such as flexible hours, few demands for travel or weekend or evening work, on-site day care, or availability of a phone to check on children. The theory of compensating differentials states that competition eventually requires all jobs to be equally attractive to the worker at the margin when both pecuniary (wage) and nonpecuniary benefits are taken into account. In this view, employers can fill jobs for lower wages if they offer nonpecuniary amenities that some workers will trade off against wages. How much the amenity reduces the market wage is determined by the preferences of the worker at the margin (England 1992:69–72). "Mother-friendliness" is just one of many nonpecuniary amenities that the theory predicts can compensate for lower wages. If mothers are more willing than other workers to trade off wages for "mother-friendly" jobs, then mothers will earn less.

The most obvious mother-friendly job characteristic is being able to work part-time. Waldfogel (1997) finds that, net of experience and education, the wage penalty of 6 percent for having one child was reduced to 4 percent when she added a control for whether the job was part-time and whether past experience was part-time; the penalty for having two or more children was reduced from 15 percent to 12 percent.

No study has tested whether or how much other job characteristics explain the motherhood penalty. However, two studies use data from the Quality of Employment Study (QES), which contain workers' self-reports of characteristics of their jobs, to explore whether women or mothers are especially likely to be employed in parent-friendly jobs. Glass (1990) found that predominantly male jobs had more flexible schedules, unsupervised break time, and paid sick leave and vacation, all features seen as parent-friendly. Glass and Camarigg (1992) constructed indices of schedule flexibility and ease of job performance. Among workers employed roughly full-time, and net of education, experience, tenure, marital status, and firm size, mothers were no more likely to be in jobs with these characteristics than non-mothers, nor were these characteristics more common for those in predominantly female occupations.

Direct measures of features of jobs that make them more compatible with parenting would be ideal for studying mother-friendly jobs—for example, whether employers allow flexible hours or choice about overtime work, provide on-site day care, or allow parents to make personal phone calls during work. But few direct measures exist, especially for national probability samples. Given data limitations, our strategy is indirect—we try a large number of the available job measures, enter them into our models and determine if they explain any observed motherhood penalty. Our hope is that this broad array of job characteristics includes job features that determine or correlate with mother-friendliness, even if they do not directly measure this construct.

One approach we take is to examine whether mothers are employed in more heavily "female" jobs, to see if this explains the lower earnings of mothers compared with non-mothers. Prior research shows that "female" jobs pay less than "male" jobs, even after controlling for skill levels (England 1992; Kilbourne et al. 1994). Is some portion of the reduced wages of female jobs a differential compensating for "mother-friendly" features not controlled for in previous analyses? The studies by Glass (1990) and Glass and Camarigg (1992), discussed above, do not support this idea that "female" jobs are more conducive to parenting. Desai and Waite (1991) indirectly explore the possibility that "female" jobs are mother-friendly by examining whether such jobs helped women stay employed continuously around a birth. They did *not* find that women who worked in occupations employing a higher percentage of females stay employed longer during their pregnancies or return to work sooner after a birth than do those in other jobs. However, Okamoto and England (1999) find that mothers are more likely to be employed in occupations with a high percentage of females than are other women. We assess here whether the sex composition of jobs explains any of the motherhood wage penalty.

Some employer policies are "mother-friendly" (Glass and Estes 1997; Glass and Fujimoto 1995; Glass and Riley 1998). Based on their work, we suspect that large firms and public-sector organizations offer more family-friendly policies, and that the presence of unions has equivocal effects (more provision for leave, but less child-care aid). Mothers may turn to self-employment to accommodate child-care responsibilities (Connelly 1992; Glass and Fujimoto 1995; Presser 1994). Mothering may also be accommodated by working in child-care, either because it is done at home or because the mother can enroll her child where she works, sometimes at a discount. We include many job characteristics in our models to see if they explain some of the wage penalty for motherhood.

Employer Discrimination against Mothers

Another possible explanation of the motherhood penalty is employer discrimination—treating women differently because of their motherhood status (e.g., placing mothers in less rewarding jobs, promoting them less, or paying them less within jobs). Such discrimination is distinct from sex discrimination that is based on the probabilistic assumption that most women are or will become mothers. Sex discrimination creates a sex gap in pay, but not a gap between mothers and other women.

Economists distinguish between discrimination based on "taste" and on statistical discrimination. In the taste model, an employer makes no assumption about mothers' lesser productivity but simply finds it distasteful to employ them. Sometimes it is co-workers or customers who have this taste, and employers find it expensive to offend them. If such differential treatment of mothers exists, it should show up in our models as a residual effect of motherhood after human capital and the mother-friendliness of jobs have been controlled. (Of course *prior* discrimination could affect the accumulation of experience, encouraging labor force withdrawals.) Or if some of the motherhood penalty is reduced by controlling for job characteristics that determine reward level, discrimination could explain why mothers were relegated to lower paying jobs; in this case discrimination could explain more than just the residual penalty after controlling for job characteristics.

A second discrimination model is statistical discrimination. Suppose that, net of types

of human capital that employers can screen cheaply, such as education and experience, mothers are, on average, less productive. The statistical discrimination model is part of economists' consideration of costs of information. The idea is that it is expensive to measure individual productivity before hiring, so employers use averages based on informal or formal data gathering to predict how individuals will perform. On this basis, they might treat women with (more) children less favorably. In economists' thinking, employers would create the degree of pay gap between mothers and non-mothers (or any other two groups to whom statistical discrimination applies) that is commensurate with their estimated productivity gap. In most statistical discrimination models offered by economists, the group that is discriminated against is paid, on average, approximately commensurate with the groups' *average* productivity; in taste discrimination the group's average pay is less than that based on their average productivity.[1] Of course, in such a scheme, individual mothers who are more productive than the average mother are being paid less than commensurate with their productivity. How would this show up in our regression models? If we had accurate measures of individual productivity (measures assumed to be expensive for employers to acquire before hiring), productivity was controlled, and statistical discrimination was the only source of the motherhood penalty, we would find a coefficient of 0 for the presence of children. However, if productivity is unmeasured, then the regression coefficient for the presence of children would pick up any statistical or taste discrimination. Social psychological research on stereotyping suggests that a more realistic model, similar to the statistical discrimination model, features employers observing real differences, exaggerating them, and thus producing an average pay gap between groups *more* than commensurate with group differences in productivity. It is also possible to perceive group differences where none exist. These types of discrimination would show up in the residual effect of motherhood on wages.

U.S. federal law prohibits sex and race discrimination in two forms. Differential treatment involves treating women differently than men because of their sex rather than any individual qualification. This standard prohibits both taste and statistical discrimination. U.S. law, however, does not explicitly prohibit discrimination based on parenthood status, but if differential treatment on the basis of parenthood were applied only to women, the courts might well see such treatment as sex discrimination, provided that qualifications and productivity were equivalent between the groups of women.

A second kind of legal claim of sex or race discrimination involves *disparate impact.* This doctrine states that policies are considered discriminatory and illegal if they use some screening criterion for hiring or promotion that screens out more women than men *and* the screening criterion is not a "business necessity." "Business necessity" is defined loosely to include anything that results in more productive workers or reduces costs. Consider the analogous concept of business policies that have a disparate impact on mothers: Policies that require long or inflexible work hours, do not allow sick days to care for children, do not permit personal phone calls from the job, and do not provide for maternity leave will adversely affect mothers. A disparate impact claim of discrimination against mothers parallel to the present legal standard regarding sex and race would prohibit any such policies, *unless* having such policies saves employers money or increases output.

Acker's (1990) and Williams's (1995) notion of gendered organizations can be seen as a kind of disparate impact model. Both researchers argue that many workplace policies are gendered—that they were formed around an idealized image of a male worker who has a wife at home and no family responsibilities other than to contribute money. Few people, male or female, have a full-time homemaker backing them up today, but some careers have requirements that

[1] Theoretical reasoning suggests a strong tendency for the group pay gap to equal the productivity gap under statistical discrimination. But the two may not be equal when mismatches between worker and employer are costly (Aigner and Cain 1977), employers are risk averse (Aigner and Cain 1977), or human capital accumulation is endogenous to the discrimination (Lundberg and Startz 1983).

seem most consistent with this gendered image. Such policies probably do have a disparate impact on mothers, and as such have a disparate impact by sex. But most such policies probably would not be deemed discriminatory by the courts because, despite their disparate impact, employers probably get more output from the employee, and thus employers could probably meet their burden of proof under the loosely defined "business necessity" standard.

But we are interested in a broader notion of disparate-impact discrimination than courts would allow. Thus, we ask where the effects of policies that have a disparate impact on mothers would show up in our statistical models. Such policies should affect the motherhood penalty net of work experience (although this could be an underestimate to the extent that experience is endogenous to such policies, i.e., they force women out of employment upon a birth). Effects of policies that limit mothers to lower paying (more mother-friendly) jobs would be netted out when relevant job characteristics were controlled.

The foregoing discussion should serve as a caveat that the interpretation of motherhood effects net of human capital depends on a number of assumptions. Researchers' inability to directly measure productivity or employer discrimination means that either may show up in our analysis as an unmeasured residual effect of motherhood on wages.

Spurious "Effects" of Motherhood on Wages Due to Unmeasured Heterogeneity

It is possible that there is no causal effect of motherhood on wages, but rather that some of the same individual characteristics that cause lower earnings for mothers also lead to childbearing at higher rates. For example, women with lower academic skills may be more likely to have children early because they know their career prospects are not good and thus think children will yield more satisfaction. Or perhaps women who care less about affluence are more likely to have (more) children and are more apt to trade earnings for other job values. Or perhaps a "present" orientation (e.g., an inability to delay gratification) makes it more likely that women will become pregnant unintentionally *and* that they exhibit low self-discipline at work, which leads to lower wages. Each of these hypotheses involves some characteristic that is exogenous to both fertility and earnings that affects both, thereby creating a correlation between earnings and the presence of children that is not causal.

Past studies have dealt with this possible heterogeneity through the explicit inclusion of control variables or by using fixed-effects models. All studies include some control variables, but data sets lack measures of many relevant characteristics, such self-discipline or the taste for affluence. We believe that the best way to deal with heterogeneity on unmeasured characteristics is to combine the inclusion of available control variables with person-specific fixed-effects modeling. Three studies have used person fixed-effects models: Korenman and Neumark (1992), Lundberg and Rose (2000), and Waldfogel (1997).[2] We use the same approach.

Person fixed-effects models require panel data that measure variables at least two points in time. Although computing algorithms vary, the coefficients obtained are those one would get if dummy variables for persons and years were entered into an OLS model run on the pooled sample of person-years. The inclusion of dummy variables for persons controls for unchanging characteristics of the person that are unmeasured but have additive effects on earnings. Person fixed-effects models have the limitation that if an unmeasured characteristic affects number of children and interacts with another variable in affecting wages, the models will not eliminate bias. For example, if career ambition lowers fertility and interacts with job experience to create steeper wage trajectories rather than having a simple additive wage increment of a certain percentage at each year, the coefficient purporting to rep-

[2] Another approach is sibling fixed-effects models. Such models assess how differences in sisters' wages are related to fertility differences, assuming that the relevant sources of heterogeneity that bias models seeking to estimate child penalties are held constant within pairs of siblings. This is a questionable assumption, but, using this method, Neumark and Korenman (1994) found a child penalty of about 7 percent, which fell to 4 to 5 percent when job experience was controlled.

resent the effect of the presence of children on the log of wages would be biased. Coefficients for motherhood could also be biased if women decide to become pregnant when they see a period of low wages coming (e.g., if their industry or town is in recession). In this case, the anticipated low wage would be causing the birth rather than the child causing the low wage. Yet fixed-effects models, by removing certain classes of bias arising from omitted variables, are a vast improvement on OLS models.

Interactions with Marital Status and Other Variables

We examine whether the child penalty differs between married and unmarried mothers (unmarried mothers are divided into never-married and divorced or separated). Most prior research has simply entered child status and marital status additively. Married mothers might be more able to spend time at home or to choose a more mother-friendly job, given another adult's income for support, leading to a higher child penalty for married women. But, absent a sex-based division of market labor versus household labor, we might hypothesize the opposite, that married women have someone to share child rearing duties, thus enabling them to better optimize earnings. Moreover, unmarried women with children may find that they can't earn enough after paying for child-care expenses to do better than welfare, which could lead to a greater experience deficit for mothers relative to non-mothers among the unmarried. And we might find no difference in the wage penalty by motherhood status if employer discrimination were the mechanism, unless employers single out married or unmarried mothers for discrimination. We also examine interactions of number of children with race, and whether any race differences in child penalties result from racial differences in marital status. Two studies have found smaller child penalties for black women compared with white women (Hill 1979; Waldfogel 1997).

DATA, MEASURES, AND MODELS

We pooled the 1982–1993 waves of the National Longitudinal Survey of Youth (NLSY), a national probability sample of individuals aged 14 to 21 in 1979; blacks and Latinos are oversampled.[3] NLSY respondents are interviewed annually. We limit our sample to women employed part-time or full-time during at least two of the years from 1982 to 1993, since fixed-effects models require at least two observations on each person. Out of the total of 6,283 women in the 1979 NLSY, we had at least two years of employment for 5,287 women. After deletions of person-years with missing values on one or more variables, our analyses are based on 41,842 person-years as units of analysis, which is an average of 7.9 years (waves) of data for each of the 5,287 women. Only 6 percent of person-years were lost because of missing values and women with less than two years of employment.

From the 1990 census, we calculated the percent female in each detailed occupation/industry cell (U.S. Bureau of the Census 1993). NLSY data are coded into 1980 occupation and industry codes starting in 1982, but these codes were easily mapped onto 1990 occupation and industry codes. Because pre-1982 occupations and industries were coded using 1970 census codes, which do not easily map onto 1990 census codes, we limited our sample to the 1982 through 1993 years.

The *Dictionary of Occupational Titles* (U.S. Department of Labor 1977) contains data on approximately 12,000 occupations. Department of Labor observers coded occupations according to their demands. DOT variables were transformed into averages for each 1980 detailed census occupation (England 1992, chap. 3).

Measures of effort (occupational average) were provided from the 1977 Quality of Employment Survey (Quinn and Staines 1979).[4] These averages were merged with our data according to 1980 census occupation codes.

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[3] Waldfogel (1997) used 1968–1988 waves of the NLS–Young Women. Neumark and Korenman (1994) also used NLS-YW, 1973–1982; the 1980 and 1982 waves were used by Korenman and Neumark (1992). Lundberg and Rose (2000) used the Panel Study of Income Dynamics (PSID) 1980–1992. Hill (1979) used the 1976 wave of the PSID.

[4] The authors thank Randall Filer for these data.

VARIABLES

The dependent variable is the natural log of hourly wage in the respondent's current job. We omitted person-years whose hourly wages appeared to be outliers (i.e., below $1 or above $200 per hour). The principal independent variable is the total number of children that a respondent reported by the interview date of each year (1982 through 1993). In alternate specifications, we measure children with dummy variables for one child, two children, and three or more children (with "no children" as the reference category). Dummy variables for marital status include married and "divorced." (The divorced category actually includes divorced, separated, and widowed respondents, although in this young sample there were few widows.) "Never-married" is the reference category.

Measures of human capital include education, years of full-time and part-time work experience, and years of full-time and part-time seniority (i.e., experience in the organization for which one currently works). These measures cover the entire life cycle back to 1978. Experience includes seniority in one's present workplace. Finally, the total number of breaks in employment is included. A break is defined as time out of employment lasting longer than 6 weeks since one's first full-time job of at least 6 weeks duration. Models controlling for human capital variables also include a measure of whether the respondent is currently enrolled in school, since this is likely to affect employment and type of job.

We include a large number of job characteristics. A dummy variable is included for whether the respondent's current job is part-time, defined as less than 35 hours per week. (In results not shown we substituted hours worked per week and its square for the dummy variable for part-time work, and the coefficients for presence of children were virtually unchanged.) Union status is a dummy variable coded 1 if the respondent reported that wages in her job were set by collective bargaining. A dummy variable is coded 1 for work in the public sector (local, state, or federal government). Another dummy variable is coded 1 if the respondent's job is one of the two census occupational titles for child care (child-care worker, private household; other child-care workers). Authority is a dummy variable coded 1 for census detailed occupational categories with titles containing the words "management," "supervisor," or "foreman" (England 1992:137–39).

We measure the cognitive skill demanded by an occupation with a scale created by England (1992:134–35). The scale was created from a factor analysis of numerous items, most taken from the *Dictionary of Occupational Titles*. The scale score was merged with NLSY respondents' records according to their detailed (1990) census occupational category. Measures of specific vocational preparation, the physical strength demanded by the job, and the physical hazards associated with one's occupation are averages of variables taken from the *Dictionary of Occupational Titles* and are merged with these data according to NLSY respondents' detailed occupation.

Finally, several measures, created from the Quality of Employment Survey (QES) are included as continuous variables. Two were measures used by Bielby and Bielby (1988) measuring how much "effort they put into their jobs beyond what is required" and "how much effort, either physical or mental" their jobs require. A third measure is the ratio of the amount of effort their job requires to the amount of effort respondents said it takes to watch television. We also include two more indirect measures: percentage of time spent not actually working while at work (e.g., waiting), and the percentage of time spent goofing off while at work.

The percentage of females in respondent's job in 1990 is calculated from 1990 census data. It is the percent female among persons employed in a cell of a matrix cross-classifying detailed 1990 three-digit occupational category with detailed three-digit industry category.

A measure of the number of employees in the respondent's current work location is included to model the effects of firm size on the motherhood penalty. The NLSY began collecting data for this measure in 1986. Thus, this measure is not included in our main models but is used in a supplementary analysis on those years for which we had the measure—1986 through 1993.

A measure of time (in minutes) spent commuting to one's current place of employment, a variable available only in the NLSY 1988 and 1993 surveys, is not included in our main models. But we do include it in a supplementary analysis using only 1988 and 1993 data.

Statistical Models

We use fixed-effects regression models to analyze NLSY data arranged in a pooled time-series cross-section with person-years as units of analysis. Effects are fixed for years and persons. Person fixed-effects models eliminate bias created by the failure to include controls for unmeasured personal characteristics that have additive effects. Thus, fixed-effects models control for effects of unchanging aspects of cognitive aptitude, preferences resulting from early socialization, life cycle plans, tastes for affluence, future orientation, and other unmeasured human capital. The model is:

$$Y_{it} = b_0 + \Sigma b_k X_{kit} + \varepsilon_{it},$$

where

$$\varepsilon_{it} = u_i + v_t + w_{it}.$$

Regression coefficients are denoted by b, k indexes measured independent variables (X's), i indexes individuals, t indexes time periods, ε is the error term, u is the cross-sectional (individual) component of error, v is the timewise component of error, w is the purely random component of error, and b_0 is the intercept. The dependent variable, Y, is the natural logarithm of hourly earnings.

For all models, the Hausman test was conducted to assess whether random-effects models were adequate. In each case, the test indicated a need for fixed-effects models. We also present results from ordinary-least-squares (OLS) regression models for comparison. Because OLS models presumably contain greater omitted-variable bias, the comparison provides some insight into whether those who have (more) children have lower earning-potential based on their unobserved characteristics. Because the multiple observations on each individual are not independent, we use the Huber-White method to correct the standard errors in the OLS models (although this correction never substantially changed standard errors). We place more confidence in the fixed-effects models for causal inference.

We do not add Heckman-type selectivity corrections to our models. However, if women for whom the motherhood penalty would be the worst are the most likely to remain out of the labor force, our models will underestimate the motherhood penalty.

RESULTS

Additive Effects of Motherhood and Causal Mechanisms

Table 1 presents means for variables used in the analysis by marital status and motherhood status. Models intended to capture causal effects begin in Table 2. We refer to models with fixed-effects, except where otherwise noted. Table 2 presents only those coefficients indicating the effect of the total number of children a woman has on the natural log of hourly wage. (The complete regression results for the model including all variables are presented in Appendix Table A.)

The models capturing the "gross" effect of motherhood include no controls other than person-specific and year-specific fixed-effects. They indicate that the wage penalty for each child is 7 percent. The OLS models show only a slightly higher gross child penalty, 8 percent. This suggests only slight negative selectivity into having (more) children on unmeasured pay-relevant characteristics.[5]

Adding marital status to the model increases the estimated motherhood penalty slightly (by –.005 in fixed-effects models). Inspection of the full regression results (Appendix Table A) shows that marriage actually increases women's earnings, so marriage is a suppressor. These are average effects of marriage across child statuses; we will see below that presence of children and marriage interact to affect wages.

[5] If we compute gross OLS models on a cross-section of the most recent year, 1993, the gross child penalty is 11 percent, which is larger than the penalty in OLS pooled data. This suggests that selectivity into motherhood creates a worse bias for cross-sectional than for pooled OLS models.

Table 1. Means and Standard Deviations (in Parentheses) for Variables Used in the Analysis, by Marital Status and Motherhood Status: NLSY, 1982 to 1993

	Never-Married		Married		Divorced[a]	
Variable	Childless	Mother	Childless	Mother	Childless	Mother
Hourly wage	5.99	5.29	6.48	6.35	6.42	6.08
	(1.71)	(1.63)	(1.74)	(1.77)	(1.77)	(1.66)
Ln hourly wage	1.73	1.67	1.87	1.85	1.86	1.81
	(.54)	(.49)	(.55)	(.57)	(.57)	(.51)
Human Capital Variables						
Education (in years)	13.45	12.31	13.18	12.52	12.46	11.90
	(2.16)	(1.89)	(2.37)	(2.21)	(2.34)	(1.82)
Enrolled in school	.26	.08	.07	.04	.08	.06
	(.44)	(.28)	(.26)	(.19)	(.27)	(.23)
Number of breaks in employment	1.95	2.25	2.07	2.37	2.58	2.95
	(1.75)	(1.93)	(1.80)	(1.98)	(2.09)	(2.14)
Full-time seniority (in years)	1.37	1.56	1.90	2.02	1.76	1.75
	(2.21)	(2.48)	(2.49)	(2.88)	(2.55)	(2.53)
Part-time seniority (in years)	.31	.26	.35	.58	.21	.26
	(.85)	(.75)	(.94)	(1.40)	(.66)	(.80)
Full-time experience (in years)	3.05	3.44	4.37	4.75	4.62	4.74
	(3.04)	(3.33)	(3.10)	(3.55)	(3.33)	(3.50)
Part-time experience (in years)	2.18	1.54	2.22	2.39	1.98	1.84
	(1.93)	(1.62)	(2.10)	(2.38)	(1.97)	(1.83)
Family Characteristics						
Number of children	—	1.67	—	2.00	—	2.08
		(.94)		(1.03)		(1.06)
One child	—	.56	—	.36	—	0.34
		(.50)		(.48)		(.47)
Two children	—	.29	—	.40	—	.38
		(.45)		(.49)		(.48)
Three or more children	—	.16	—	.25	—	.29
		(.37)		(.43)		(.45)
Race/Ethnicity						
Latina	.14	.15	.10	.21	.10	.19
	(.35)	(.36)	(.31)	(.41)	(.30)	(.40)
Black (non-Hispanic)	.23	.65	.10	.21	.15	.38
	(.42)	(.48)	(.30)	(.41)	(.36)	(.48)

(Table 1 continued on next page)

Table 2 shows that reduced experience is clearly part of the explanation of the motherhood penalty. Controlling for the human capital variables shown in Table 1, reduces the child penalty by 36 percent, from about 7 percent to 5 percent.[6]

We next explore whether something about the jobs held by mothers explains their lower wages. Mothers may trade wages for "mother-friendly" jobs, or lowered productivity could cause women to choose less de-

[6] OLS models show an even larger reduction in the child penalty when the human capital variables are added (from 8 percent to 2 percent compared with from 7 percent to 5 percent in fixed-effects models). The larger drop in OLS suggests that some of the unobserved human capital difference between mothers and non-mothers is exogenous to both motherhood and measured human capital, and affects each. Because this component is netted out of both the gross and human capital models under fixed-effects, the motherhood coefficients differ less. It is unclear why in the models that control human capital variables,

(Table 1 continued from previous page)

Variable	Never-Married		Married		Divorced[a]	
	Childless	Mother	Childless	Mother	Childless	Mother
Job Characteristics						
Part-time job	.31	.27	.23	.32	.19	.22
	(.46)	(.44)	(.42)	(.46)	(.40)	(.41)
Work effort ratio (QES)	.56	.56	.56	.56	.56	.56
	(.05)	(.04)	(.05)	(.05)	(.04)	(.04)
Work effort required (QES)[b]	3.68	3.64	3.69	3.67	3.67	3.65
	(.17)	(.17)	(.16)	(.17)	(.17)	(.16)
Extra work effort (QES)[b]	3.49	3.46	3.50	3.48	3.49	3.46
	(.17)	(.17)	(.17)	(.18)	(.17)	(.18)
Percent of time waiting on job (QES)	.17	.17	.17	.17	.17	.17
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Percent of time goofing off on job (QES)	.06	.05	.06	.06	.06	.05
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Hazardous conditions (DOT)	8.23	10.95	7.31	8.98	9.36	10.80
	(18.69)	(21.24)	(17.27)	(19.44)	(19.74)	(21.50)
Strength requirement (DOT)	2..00	2.20	1.96	2.06	2.05	2.15
	(.66)	(.70)	(.67)	(.69)	(.69)	(.68)
Specific vocational training (DOT)	17.77	13.20	19.51	17.21	17.39	14.97
	(16.47)	(12.79)	(16.78)	(15.34)	(15.35)	(13.38)
Cognitive skill (DOT)	1.032	.507	1.175	.920	.878	.639
	(1.664)	(1.139)	(1.724)	(1.507)	(1.475)	(1.222)
Authority (DOT)	.07	.04	.09	.07	.10	.07
	(.26)	(.20)	(.29)	(.25)	(.30)	(.26)
Percent female in occupation/industry	.67	.67	.68	.68	.67	.66
	(.25)	(.25)	(.25)	(.25)	(.26)	(.26)
Government job	.10	.09	.08	.08	.08	.07
	(.29)	(.29)	(.28)	(.28)	(.27)	(.26)
Union member	.15	.19	.13	.15	.15	.15
	(.36)	(.39)	(.34)	(.36)	(.35)	(.35)
Child-care job	.02	.02	.02	.04	.02	.02
	(.12)	(.15)	(.14)	(.20)	(.12)	(.14)
Self-employed	.03	.03	.07	.08	.06	.05
	(.17)	(.17)	(.25)	(.28)	(.23)	(.21)
Number of person-years	13,019	4,151	7,241	11,789	2,088	3,554

[a] Category includes separated, divorced, and widowed.

[b] Variables are coded such that low scores indicate more average effort reported.

manding jobs. Or job characteristics could explain the motherhood penalty if employers discriminated against mothers, excluding them from high-paying jobs with demands they believed mothers would fulfill less well. Table 2 shows that support for these ideas is weak. Including *all* the job characteristics lowers the (marital status– and human capital–adjusted) penalty for each child from –.047 to –.037. Although this is a 21-percent reduction, a decline in the a child penalty to wages from about 5 percent to about 4 percent seems small. The reduction in the OLS model is even smaller. Moreover, half of the reduction in the fixed-

child coefficients are larger in the fixed-effects model than in the OLS model, whereas in the gross models the child penalty is larger in the OLS model.

Table 2. Unstandardized Coefficients for the Effect of Total Number of Children (Continuous Variable) on Women's Hourly Wage (ln), from Fixed-Effects Models and OLS Models: NLSY, 1982 to 1993

Control Variables in Model	Fixed-Effects Model	OLS Model
Gross (no controls)	−.068**	−.081**
	(.004)	(.002)
Marital status	−.073**	−.081**
	(.004)	(.002)
Marital status and human capital variables [a]	−.047**	−.018**
	(.004)	(.002)
Marital status, human capital variables, and job characteristics [b]	−.037**	−.012**
	(.004)	(.002)

Notes: OLS models include age and year, each in linear, squared, and cubed form. Numbers in parentheses are standard errors. Standard errors in OLS models were corrected using the Huber-White method.

[a] Measures of human capital include education, full-time seniority, part-time seniority, full-time experience, part-time experience, number of breaks in employment, and whether currently enrolled in school.

[b] Job characteristics include the QES and DOT measures listed in Table 1, whether the current job is part-time, percent female of the respondents' occupation by industry category, dummies for whether the job is in government, unionized, in a child care occupation, or self-employment, and industry dummies.

*$p < .05$ **$p < .01$ (two-tailed tests)

effects model is achieved by simply including a single job characteristic: whether the woman is working part-time. Working part-time reduces hourly pay, either directly or through forcing women into less desirable jobs that offer part-time hours.

No other job characteristic, when added alone to the human capital model, changes the child penalty to any nontrivial extent (results not shown). Mothers are less likely to be in jobs involving authority and more likely to work in jobs involving child care (Table 1). But neither of these variables, when added to the model, reduces the child penalty by even one percentage point. Controlling for the sex composition of the woman's job had no effect on the child penalty (results not shown). Although "female" jobs pay less (Appendix Table B), mothers are no more likely than non-mothers to be in them (Table 1). In fact, the zero-order correlation between number of children and the percent female of one's job is slightly negative (results not shown). Thus, there is no evidence that women select female jobs because they are more mother-friendly. Occupational sex segregation and the wage penalty for working in a female job appear orthogonal to having children and the wage penalty for children.

We also experimented with adding groups of job variables, but no group of related variables had a nontrivial effect on the child penalty. The five QES measures of effort required by the occupation do not change the penalty by even one percentage point. Interestingly, Table 1 shows similar means for mothers and non-mothers on these variables, and Appendix Table B shows that not all the effort measures have the predicted effects on earnings. Similarly, if all the dummy variables for industry are added to the human capital model, the child penalty is reduced by less than one percentage point.

Two supplementary analyses added job characteristics that were available only for certain years of the NLSY panel (results not shown). Limiting the analysis to those years for which we had data on firm size (1986–1993) revealed no change in the size of the child penalty with the inclusion of firm size. A second supplementary analysis assessed whether the child penalty arises because mothers sacrifice higher pay for a shorter commute. Because commuting time was measured only in 1989 and 1993, we ran fixed-effects models including those women employed in both 1989 and 1993. Adding commuting time to the human capital model had no effect on the estimated motherhood penalty.

Given that job characteristics do not substantially mediate the effect of motherhood on wages, we need not worry about whether any such indirect effect comes from voluntary selection of mother-friendly jobs, employer discrimination relegating mothers to worse jobs, or some other process. Motherhood does not seem to have its effects through the kinds of jobs women hold, with the important exception of working part-time.

Table 3. Unstandardized Coefficients for the Effect of Number of Children (Dummy Variables) on Women's Hourly Wage (ln), from Fixed-Effects Models and OLS Models: NLSY, 1982 to 1993

	Fixed-Effects Models			OLS Models		
Control Variables in Model	One Child	Two Children	Three or More Children	One Child	Two Children	Three or More Children
Gross (no controls)	−.020*	−.125**	−.217**	−.080**	−.191**	−.280**
	(.008)	(.011)	(.015)	(.006)	(.007)	(.008)
Marital status	−.038**	−.142**	−.232**	−.080**	−.192**	−.280**
	(.008)	(.011)	(.015)	(.006)	(.007)	(.008)
Marital status and human capital variables	−.045**	−.112**	−.151**	−.039**	−.071**	−.053**
	(.008)	(.010)	(.015)	(.006)	(.006)	(.008)
Marital status, human capital variables, and job characteristics	−.032**	−.089**	−.121**	−.026**	−.051**	−.034**
	(.008)	(.010)	(.014)	(.005)	(.006)	(.007)

Note: Numbers in parentheses are standard errors. Standard errors in OLS models were corrected using the Huber-White method. For descriptions of models and variables, see notes to Table 2.

*$p < .05$ **$p < .01$ (two-tailed tests)

Table 3 presents a check on whether measuring "motherhood" with a continuous variable counting total number of children obscured nonlinear or nonmonotonic relationships. We measured the presence of children with three dummy variables (one child, two children, and three or more children), each relative to a reference category of no children. Table 3 shows that the gross penalty is 2 percent for one child, 13 percent for two children (i.e., an additional 11 percent for the second child), and 22 percent for three or more children. Controlling for marital status and all of the human capital variables, the penalties are 5 percent, 11 percent (an additional 6 percent for the second child), and 15 percent. As with the models entering number of children as a continuous variable, the addition of job variables reduces the penalty little. The penalty for having one child is small and none of it is explained by lost experience (the penalty goes up slightly in the model including the human capital variables). Having a second child has a much larger incremental effect than does having the first child. Women may be more likely to take a break from employment when there are two children at home because the difference between their earnings and the cost of care for two children makes employment no longer compelling. But, this is not the whole story, because most of the incremental loss in wage after the second child is present in the human capital model, which controls for experience. Given that effects are at least monotonic, if not perfectly linear, our judgment is that the imprecision introduced by measuring number of children as a continuous variable in our analyses is worth the gain in simplicity.

Which Women Suffer Larger Child Penalties?

Next we consider interactions to investigate what characteristics of women or their jobs increase the size of motherhood penalties. Table 4 shows results from interacting dummy variables for marital status with number of children. The left column of Table 4 presents coefficients for total number of children, which, in this model including interactions, tell us the effect of each child on wages for never-married women.[7] The columns to the right present effects for married

[7] Coefficients for "additive" terms in models including an interaction involving that variable give the effect of that variable when all other variables with which it has been interacted equal 0. When both marital status dummy variables equal 0, this indicates never-married status since "never-married" is the reference category.

Table 4. Effect of Number of Children (Continuous Variable) on Women's Hourly Wage (ln) from Fixed-Effects Models and OLS Models, by Marital Status

	Fixed-Effects Models			OLS Models		
Control Variables in Model	Never-Married	Married	Divorced[a]	Never-Married	Married	Divorced[a]
Gross (no controls)	−.035**	−.079**	−.079**	−.101**	−.073**	−.081**
	(.007)	(.006)	(.007)	(.004)	(.005)	(.006)
Human capital variables	−.026**	−.051**	−.046**	−.025**	−.015**	−.025**
	(.007)	(.006)	(.007)	(.004)	(.005)	(.006)
Human capital variables and job characteristics	−.019**	−.040**	−.038**	−.014**	−.014**	−.014**
	(.007)	(.006)	(.007)	(.004)	(.004)	(.005)

Note: Numbers in parentheses are standard errors. Standard errors in OLS models were corrected using the Huber-White method. Effects are calculated from unstandardized coefficients in models containing interactions between marital status and number of children (using a continuous measure).

For descriptions of models and variables, see notes to Table 2.

[a] Includes separated, divorced, or widowed.

*$p < .05$ **$p < .01$ (two-tailed tests)

and divorced or separated women obtained by adding the coefficient for number of children and the coefficient for the relevant interaction. The fixed-effects models show that women who have never been married experience lower child penalties than do married or divorced women, both before and after adding controls for human capital variables and job characteristics.[8] This result holds if we combine married and previously married women into one category (results not shown). In the OLS models, never-married women show child penalties as high as or higher than those in the fixed-effects models. We are more confident in the fixed-effects models for drawing conclusions concerning causation, particularly because in recent cohorts, women with more earning power are also more likely to marry. Thus, fixed-effects modeling is needed to net out the selectivity into marriage.

The fact that marriage increases the child penalty suggests that at least some part of the penalty arises because the ratio of time and energy mothers allocate to children versus jobs is affected by whether they have a source of financial support other than their own earnings. Without assuming a sex-based division of labor, the direction we would predict for this interaction would not be clear. Husbands could, in principle, provide money that allows married mothers to focus more on their children than single women can; or they could simply be a second person to share child-care responsibilities, allowing married mothers to focus more on their jobs than single mothers. The higher child penalty for married mothers suggests that the first scenario is more common.

The higher penalty for married mothers also suggests that child penalties are not *entirely* a matter of discrimination against mothers, unless we believe that employers discriminate more against married mothers than against single mothers.

It is puzzling that married and divorced women have similarly high child penalties. After all, divorced women do not have husbands to provide financial support and they usually get relatively little child support. The similarity implies that the larger penalties experienced by married women are long-lasting, enduring even if the marriage ends. Perhaps the penalties operate through missed promotions, or cumulative impacts of impressions made, or small raises earned early in one's employment history.

The fact that marriage increases the penalties for children does not mean there is a

[8] Appendix Table B shows results from models that interact dummy variables for number of children with marital status. It shows greater penalties for married and divorced women as in Table 4, but these are largely limited to second and higher order births.

marriage penalty. In fact, on average there is a marriage premium: Marriage has positive effects in all models that do not interact marriage with children (see Appendix Table A). The interaction between marital status and the presence of children implies not only that the child penalty varies by marital status but also that the effect of marriage varies by child status. Calculations from Table 4's (gross or human capital) models with interactions show that marriage has a wage premium for women with one child or no children, and no effect for women with two children. But for women with more than two children, marriage has a net wage penalty. Thus, marriage increases the child penalty, while children reduce the marriage premium, turning it into a penalty for mothers with more than two children.

To test whether more skilled women experience higher penalties, we interacted human capital with number of children (results not shown). There was no interaction between years of education and number of children. We found that women with more full-time experience suffer larger child penalties, but the opposite was true for full- or part-time seniority. Thus, there is no clear evidence that more skilled or committed women experience higher penalties.

Do women with higher level jobs incur a larger motherhood penalty? This might be true if such jobs are organized on a "male" model that penalizes any behavior that appears to be less than a full commitment, whether or not the behavior affects productivity. To test this, we interacted number of children with job characteristics (models also included human capital measures). The child penalty is higher for women in full-time jobs than for those in part-time jobs. The penalty is slightly lower for women in more heavily male jobs. Penalties were no higher in jobs requiring more on-the-job or vocational/professional training or more cognitive skill (they were trivially but significantly lower). Finally, we created a variable intended to capture high-level male jobs. We coded this dummy variable 1 if the job was classified as professional or management in the census's broad occupational categories *and* the job's percent female (of the occupation-by-industry category) was no more than 35 percent. We interacted this dummy variable with number of children in a model that also controlled for marital status and the human capital variables. Women in these heavily male professional and managerial jobs actually had smaller (1 to 2 percentage point) child penalties. Thus, it appears that high-level, "male" jobs penalize women a bit less for having children.

Finally, we considered whether child penalties differ by race. Limiting this analysis to Latinas, non-Hispanic blacks, and non-Hispanic whites, we interacted race with number of children (results not shown). For the gross model, the penalties for number of children did not differ by race. After adjusting for the human capital variables, black/white penalties still did not differ, but Latinas had smaller penalties. When we used dummy variables for number of children, allowing nonlinear effects, it was only for mothers with three and more children that we found smaller penalties for blacks and Latinas (whether or not the human capital variables were controlled). Of course, most women have fewer than three children. There were no three-way interactions between marital status, children, and race in any model, implying that the lower penalties that women of color experience for third or higher parity births are not explained by the fact that more of their births occur outside marriage. Waldfogel (1997) and Neumark and Korenman (1994), using models that control for human capital variables, also report a smaller penalty for black women compared with white women in an earlier NLS data set. Our findings show that this difference exists only for women with more than two children.

CONCLUSIONS

We find a wage penalty for motherhood of approximately 7 percent per child among young American women. Roughly one-third of the penalty is explained by years of past job experience and seniority, including whether past work was part-time. That is, for some women, motherhood leads to employment breaks, part-time employment, and the accumulation of fewer years of experience and seniority, all of which diminish future earnings. However, it is striking that about two-thirds of the child penalty remains after

controlling for elaborate measures of work experience.

We added numerous job characteristics to models to assess whether mothers earn less because their jobs are less demanding or because they offer mother-friendly characteristics. These factors had only a small effect in explaining the child penalty, and about half of the effect came from a single job characteristic—whether the current job is part-time. Most job characteristics had no effect on the motherhood penalty—either because the characteristics don't affect pay or because motherhood does not affect whether women hold these jobs.

In what social locations are motherhood penalties the steepest? Black women and Latinas have smaller penalties, but only for the third and subsequent births. Never-married women have lower child penalties than married or divorced women. Second children reduce wages more than a first child, especially for married women. There is no evidence that penalties are proportionately greater for women in more demanding or high-level jobs, or "male" jobs, or for more educated women, although the penalties are higher for women who work full-time and already have more work experience.

Our use of fixed-effects modeling gives us some confidence that the effects of motherhood identified here are causal rather than spurious. Further, our detailed measures of work experience assure us that no more than one-third of the motherhood penalty arises because motherhood interrupts women's employment, leading to breaks, more part-time work, and fewer years of experience and seniority. Finally, we find that little of the child penalty is explained by mothers' placement in jobs with characteristics associated with low pay. However, we did not have direct measures of many job characteristics that would make jobs easier to combine with parenting. Thus, we may have underestimated the importance of this particular factor. For future research to be able to answer this question and generalize to the nation as a whole, we need the inclusion of questions about job characteristics that accommodate parenting on national surveys using probability sampling, preferably panels.

What explains the approximately two-thirds of the 7-percent-per-child penalty not explained by the reductions motherhood makes in women's job experience, if little of it is from working in less demanding or mother-friendly jobs? The remaining motherhood penalty of about 4 percent per child may arise from effects of motherhood on productivity and/or from employer discrimination. A weaknesses of social science research is that direct measures of either productivity or discrimination are rarely available. Thus, new approaches to measuring productivity or discrimination would be a welcome contribution. In the meantime, our analyses provide indirect evidence that at least part of the child penalty may result from mothers being less productive in a given hour of paid work because they are more exhausted or distracted. Net of human capital variables, women earn less with each subsequent child, and children reduce women's pay more if the mothers are married or divorced than if they are never-married. Employers may discriminate against all women by treating them all like mothers, or they may discriminate against all mothers relative to other women. But is it plausible that employers discriminate by number of children, and discriminate more against married mothers than single mothers (but give a premium for marriage when women have no child or one child)? This seems far fetched. This does *not* mean that *none* of the child penalty is discriminatory. It may be that a base amount is discriminatory, and that the portion that is related to productivity is the portion that varies by number of children and marital status, because those factors affect decisions about how time and energy is allocated between child rearing and jobs.

How should public policy respond to wage penalties for motherhood? Because distinguishing between discriminatory and nondiscriminatory differences by race and sex is institutionalized in our legal system, it is tempting to conclude that a motherhood penalty is not of public concern unless it results from employers' discrimination. We don't know how much of the penalty arises from discrimination in the form of "differential treatment" of equivalently qualified and productive mothers and non-mothers. Nor do we know how many policies that have a disparate impact on mothers would fail the legal standard of being a "business necessity."

But we think there is a serious equity problem, *even if* the penalty were found to be entirely explained by mothers having less work experience, lower productivity, and choosing mother-friendly jobs, and *even if* employers' policies had the intent and effect only of maximizing output relative to costs. In short, we think there is a serious equity problem when we all free ride on the benefits of mothers' labor, while mothers bear much of the costs of rearing children. At this point we depart from the narrow scientific analysis, and articulate our findings with a normatively based notion of equity.

Reducing the extent to which mothers bear the costs of rearing children is a worthy goal, in our view. Broadening the concept of discrimination to include anything about how jobs are structured or what is rewarded that has a disparate impact on mothers, and making employers change such policies, would be one way to approach this. But should employers have to get rid of *any* policy that penalizes mothers? We suspect that this would reduce the net output of organizations because policies that reward experienced workers and workers who can work long hours when needed by the employer would need to be changed. Of course, the net effect on output is an empirical question; in some cases the productivity gains resulting from increased morale and continuity of mothers' employment would offset costs.

But if there are costs to employers of restructuring work to eliminate the motherhood penalty, deciding who should pay them is part of the larger question of who should bear the costs of raising the next generation. A general equity principle is that those who receive benefits should share in the costs. As Marxist feminists pointed out in the 1970s, capitalist employers benefit from the unpaid work of mothers, who raise the next generation of workers. But employers are not the only ones who benefit when children are well reared—we all free ride on mothers' labor. Thus, mandating that employers share in these costs makes sense only as part of a broader redistribution of the costs of child rearing.

Those who rear children deserve public support precisely because the benefits of child rearing diffuse to other members of society. Indeed, child rearing (whether unpaid or paid), broadly construed, creates more diffuse social benefits do than most kinds of work. In our view, the equitable solution would be to collectivize the costs of child rearing broadly—to be paid not just by employers but by all citizens—because the benefits diffuse broadly. While most U.S. mothers today are employed, mothers continue also to bear the lion's share of the costs of rearing children. Yet other industrial democracies have collectivized the costs to a much greater extent than has the United States (albeit often with other, pronatalist, motivations). Costs can be socialized through family allowances, child care, and medical care that are financed by progressive taxes. Adopting such policies in the United States would not eliminate the fact that motherhood lowers wages, although it might reduce some of the gross effect if the presence of subsidized child care increased women's employment. Such policies would put a floor under the poverty of families with mothers, and would redistribute resources toward those who now pay a disproportionate share of the costs of rearing children. In a period when most mothers are employed, when welfare mothers are being required to take jobs, and when the economy is generating budget surpluses unthinkable a decade ago, there may be a political opening for creative proposals that would increase equity for mothers while also helping children.

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Appendix Table A. Unstandardized Coefficients from the Regression of Women's Hourly Wage (ln) on Selected Independent Variables: NLSY, 1982 to 1993

Independent Variable	Fixed-Effects Coefficient	OLS Coefficient
Intercept	.953**	.078
	(.238)	(.108)
Family Characteristics		
Total number of children	−.037**	−.012**
	(.004)	(.002)
Married	.035**	.021**
	(.006)	(.004)
Divorced/separated/ widowed	.041**	.026**
	(.009)	(.008)
Human Capital		
Education (in years)	.047**	.046**
	(.003)	(.001)
Enrolled in school	−.125**	−.051**
	(.007)	(.006)
Number of breaks in employment	−.011**	−.009**
	(.003)	(.001)
Full-time seniority (in years)	.015**	.015**
	(.001)	(.001)
Part-time seniority (in years)	.012**	.021**
	(.003)	(.002)
Full-time experience (in years)	.023**	.029**
	(.002)	(.001)
Part-time experience (in years)	.011**	.014**
	(.002)	(.001)
Job Characteristics		
Part-time job	.001	−.020**
	(.005)	(.005)
Work effort ratio	−.385**	−.988**
	(.051)	(.048)
Work effort[a]	−.201**	−.304**
	(.015)	(.015)
Extra work effort[a]	.068**	.067**
	(.014)	(.014)
Percent of time waiting on job	−.308	−1.121**
	(.166)	(.164)
Percent of time goofing off on job	−.368	−1.257**
	(.350)	(.336)
Hazardous conditions	.000	−.000**
	(.000)	(.000)
Strength requirement	−.002	−.032**
	(.005)	(.005)
Specific vocational training	.001**	.002**
	(.000)	(.000)
Cognitive skill	.012**	.032**
	(.003)	(.002)
Authority	.005	.001
	(.009)	(.009)
Percent female in occupation/industry	−.063**	−.058**
	(.012)	(.011)
Government job	.004	−.029**
	(.008)	(.008)
Unionized job	.085**	.126**
	(.006)	(.006)
Child-care occupation	−.397**	−.493**
	(.014)	(.014)
Self-employed	−.041**	.011
	(.009)	(.009)
Industry (Reference Category is Agriculture, Mining, Forestry)		
Public administration	−.149**	.071**
	(.020)	(.019)
Finance, insurance, and real estate services	−.142**	.049**
	(.019)	(.018)
Professional services	−.178**	.002
	(.017)	(.017)
Personal services	−.357**	−.173**
	(.019)	(.018)
Business and repair services	−.178**	.037*
	(.018)	(.018)
Communications	−.119**	.132**
	(.027)	(.023)
Wholesale trade durables	−.128**	.054*
	(.024)	(.024)
Wholesale trade non-durables	−.172**	.013
	(.024)	(.025)
Retail trade	−.273**	−.106**
	(.017)	(.017)
Entertainment and recreation services	−.215**	−.022
	(.022)	(.022)
Utilities	−.051	.162**
	(.032)	(.028)
Transportation	−.114**	.198**
	(.022)	(.021)
Construction	−.082**	.091**
	(.025)	(.024)
Food, tobacco, textile manufacturing	−.132**	−.030
	(.018)	(.017)
Chemical, petroleum, rubber, and leather manufacturing	−.091**	.081**
	(.023)	(.022)
Lumber, furniture, stone, glass manufacturing	−.130**	.018
	(.027)	(.026)
Metal industries manufacturing	−.081**	.090**
	(.028)	(.027)

(Appendix Table A continued on next page)

(Appendix Table A continued from previous page)

Independent Variable	Fixed-Effects Coefficient	OLS Coefficient	Independent Varaible	Fixed-Effects Coefficient	OLS Coefficient
Industry (Continued)			$(Age)^2$	—	$-.001^{**}$ (.000)
Machinery manufacturing	$-.100^{**}$ (.020)	$.126^{**}$ (.019)	Interview year	—	$.036^{**}$ (.007)
Equipment manufacturing	−.048 (.027)	$.179^{**}$ (.026)	$(\text{Interview year})^2$	—	$.012^{**}$ (.001)
OLS Control Variables			$(\text{Interview year})^3$	—	$-.001^{**}$ (.000)
Age	—	$.061^{**}$ (.006)			

Note: Numbers in parentheses are standard errors. Standard errors in OLS models were corrected using the Huber-White method. Age is not included in fixed-effects models, but is implicitly controlled because period is controlled, the person fixed-effects cancel out cohort, and period and cohort together uniquely determine age.

[a] Variable was coded such that high scores indicate a low average effort reported by those in the occupation. Signs on regression coefficients are reversed so that a positive coefficient indicates a positive effect on earnings.

$^{*}p < .05$ $^{**}p < .01$ (two-tailed tests)

Appendix Table B. Effects of Number of Children (Dummy Variables) on Women's Hourly Wage (ln) from Fixed-Effects Models and OLS Models, by Marital Status: NLSY, 1982 to 1993

	Fixed-Effects Models			OLS Models		
Control Variables in Model and Marital Status	One Child	Two Children	Three or More Children	One Child	Two Children	Three or More Children
Gross (No Controls)						
Never-married	−.023 (.014)	$-.056^{**}$ (.019)	$-.115^{**}$ (.027)	$-.152^{**}$ (.011)	$-.224^{**}$ (.014)	$-.312^{**}$ (.019)
Married	−.023 (.014)	$-.162^{**}$ (.019)	$-.245^{**}$ (.026)	$-.029^{**}$ (.014)	$-.175^{**}$ (.017)	$-.250^{**}$ (.021)
Divorced[a]	$-.082^{**}$ (.022)	$-.180^{**}$ (.025)	$-.245^{**}$ (.031)	$-.067^{**}$ (.020)	$-.154^{**}$ (.022)	$-.312^{**}$ (.019)
Human Capital Variables						
Never-married	$-.043^{**}$ (.013)	$-.054^{**}$ (.018)	$-.078^{**}$ (.026)	$-.060^{**}$ (.010)	$-.073^{**}$ (.013)	$-.044^{*}$ (.017)
Married	$-.043^{**}$ (.013)	$-.128^{**}$ (.019)	$-.166^{**}$ (.025)	$-.024^{**}$ (.013)	$-.073^{**}$ (.013)	$-.044^{*}$ (.017)
Divorced[a]	$-.043^{**}$ (.013)	$-.127^{**}$ (.024)	$-.149^{**}$ (.030)	$-.060^{**}$ (.010)	$-.073^{**}$ (.013)	$-.044^{*}$ (.017)
Human Capital Variables and Job Characteristics						
Never-married	$-.026^{*}$ (.012)	$-.039^{*}$ (.017)	$-.059^{*}$ (.025)	$-.038^{**}$ (.009)	$-.045^{**}$ (.012)	−.021 (.016)
Married	$-.026^{*}$ (.012)	$-.102^{**}$ (.018)	$-.133^{**}$ (.024)	$-.013^{*}$ (.011)	$-.045^{**}$ (.012)	−.021 (.016)
Divorced[a]	$-.026^{*}$ (.012)	$-.101^{**}$ (.023)	$-.128^{**}$ (.029)	$-.038^{**}$ (.009)	$-.045^{**}$ (.012)	$-.070^{**}$ (.021)

Note: Numbers in parentheses are standard errors. Standard errors in OLS models were corrected using the Huber-White method. Effect sizes are calculated from unstandardized coefficients in models containing interactions between marital status and dummy variables for number of children. For descriptions of models and variables, see note to Table 2.

[a] Includes separated, divorced, or widowed.

$^{*}p < .05$ $^{**}p < .01$ (two-tailed tests)

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