THE MOTHERHOOD WAGE PENALTY REVISITED: EXPERIENCE, HETEROGENEITY, WORK EFFORT, AND WORK-SCHEDULE FLEXIBILITY

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This paper seeks an explanation for the well-documented wage disadvantage of mothers compared to women without children. An analysis of data from the 1968–88 National Longitudinal Survey of Young Women shows that human capital inputs and unobserved heterogeneity explain 55–57% of the gap. Further analysis suggests that mothers tended to face the highest wage penalty when they first returned to work. A finding that medium-skill mothers (high school graduates) suffered more prolonged and severe wage losses than either low- or high-skill mothers casts doubt on the work-effort explanation for the wage gap, according to which women reduce work effort in response to childcare duties. The authors instead cite variable time constraints: high school graduates are likely to hold jobs requiring their presence during regular office hours, and are unlikely to gain flexibility by finding work at other hours or by taking work home in the evening.

It is well documented that mothers earn less than women without children. A variety of factors could explain this wage penalty, including reduced investment in wage-enhancing human capital, unobserved heterogeneity between mothers and nonmothers, and lower work effort by mothers. In this paper, we extend previous studies of the motherhood wage penalty in two ways.

First, we consider heterogeneity among mothers in the timing of their return to the labor force. If working mothers of infants and toddlers avoid a wage penalty because they are more career-oriented and more likely to return to the same job than mothers who delay their return to the labor force, then wage penalties estimated for a pooled sample of all mothers will mask the true penalty, as well as its source.

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A data appendix with copies of the computer programs used to generate the results presented in the paper is available from Deborah Anderson at the College of Education, University of Arizona, Tucson, AZ 85721. Data extracts were created using SAS; all statistics reported in this study were produced with the software Stata.

Industrial and Labor Relations Review, Vol. 56, No. 2 (January 2003).
0019-7939/00/5602 $01.00

Second, we investigate the source of the motherhood wage penalty that persists even after human capital and unobserved heterogeneity have been controlled. In particular, we consider whether mothers bear a wage penalty because they exert less effort at work, and we examine to what extent, if any, work schedule conflicts reduce mothers' wages. The work effort explanation offers several testable implications for the pattern of wage penalties by age of children, education, and race. Since young children require more physical care (lifting, holding, diapering, chasing, and so on) and are more likely than older children to wake up and scream at night, mothers of young children could easily be more exhausted and thus less productive at work than mothers of older children. Although older children present their own challenges, especially as occasioned by short school days and extracurricular activities that require parental shuttling, it is likely that these tasks affect wages through conflicts with work schedules rather than by draining energy. A declining wage penalty as children grow older would be consistent with the work effort explanation; a persistent wage penalty would suggest that work schedule conflicts are important.

The work effort explanation also implies that mothers in jobs requiring more effort will experience larger penalties than mothers in jobs requiring little effort. The 1977 Quality of Employment Survey (QES) suggested that work effort rises with education: 59% of women without a high school degree reported that their job required "a lot" of effort, compared to 68% of women who had a high school diploma but no college degree and 84% of women who were college graduates. Thus a wage penalty that rises with education would also be consistent with a work effort explanation. The absence of such a pattern, on the other hand, would suggest the need for other explanations. Finally, some researchers report that black mothers bear a smaller wage penalty than white mothers. If this is the case, then the work effort explanation becomes less plausible, because it is difficult to imagine how race would systematically affect energy availability at work. Again, other explanations would need to be considered.

Measuring the Motherhood Wage Gap

We estimate a 10% motherhood wage penalty from a pooled cross-section of women ages 14 to 44 in the 1968–88 National Longitudinal Survey of Labor Market Experience of Young Women (NLSYW).[1] This estimate in part reflects human capital differences between mothers and non-mothers. For example, in the NLSYW, women who are never mothers attain 13.2 years of schooling on average, compared with 12.5 years for mothers. Even more striking, the gap between potential work experience (age – schooling – 6) and actual work experience is three years for mothers in the NLSYW, compared with only 1.5 months for non-mothers. In addition to differences in experience, mothers who do not return to the same job may suffer from the loss of firm-specific human capital.

Nevertheless, cross-sectional analyses have typically identified significant wage penalties associated with having children even when controls for differences in human capital are included. Reported penalties range from 2% to 10% for one child and from 5% to 13% for two or more children (Budig and England 2001; Waldfogel 1995, 1997, 1998a).

[1]A description of these data, on which our analyses are based, follows. To get this particular estimate, we use OLS (in a pooled cross-section of women) to regress log wage on a variable that takes the value of one if the woman is a mother (and is otherwise zero). In order to reduce the weight placed on multiple observations per woman, we weight each observation by the total number of *possible* observations per woman divided by the *actual* number of years that the woman appears in the unbalanced panel. While the NLSYW data provide information about an earlier generation of women, these data are comparable to those used by many other researchers who have explored this question. Furthermore, the estimated penalty is remarkably similar to the 11% penalty generated by running an analogous regression on women 14 to 44 years of age in the March 1999 CPS. Future work should replicate the following analyses with more recent data to see if the results have changed over time.

An obvious drawback of the use of cross-sectional analysis is the possibility that mothers are different from non-mothers in ways that are not observed in the data. Budig and England (2001), Korenman and Neumark (1992), Lundberg and Rose (2000), and Waldfogel (1995, 1997) addressed this concern by estimating fixed effects models.[2] Of these studies, only that of Korenman and Neumark, who analyzed differences in an arguably too-short two-year period, found that the estimated motherhood penalty is greatly reduced by controlling for unobserved heterogeneity. Lundberg and Rose (2000), focusing on several years surrounding the birth of the first child, estimated that mothers' wages fall by 5%, on average, after the first birth; mothers who are continuously employed, however, face no penalty at all. In the longer periods of twelve years or more studied by Budig and England (2001) and Waldfogel (1997), penalties estimated with fixed effects models are quite similar to those estimated in a cross-section.

Previous studies do not give a clear picture of race and education patterns in the motherhood wage penalty. On the one hand, Budig and England (2001) found no variation by education level and only small racial differences in the motherhood wage penalty.[3] On the other hand, Hill (1979) and Waldfogel (1997) both reported that black mothers bear a smaller penalty than white mothers do, if they bear one at all. And Waldfogel and Mayer (2000) reported conflicting cross-sectional estimates regarding how the motherhood penalty varies with education.

Other Explanations for the Motherhood Wage Gap

Since human capital measures and unobserved heterogeneity do not fully account for the lower wages of mothers, other explanations may be relevant. Frank (1978) proposed that wives subordinate their careers to their husbands' careers, particularly in location decisions. As a result, they tend to accept jobs for which they are overqualified and, relative to better-matched men with the same skills, underpaid. If mothers also subordinate their careers to the needs of their children, their choice of jobs will be further restricted, leading to worse matches and lower wages, compared to non-mothers. For example, mothers may accept a lower wage in return for schedule flexibility. Discrimination is also a possibility: if employers assume that mothers are less productive, they will be inclined to pay them less. Finally, mothers may in fact be less productive at work because they have dissipated their energy caring for their children, a supposition formalized as Becker's (1985) work effort hypothesis.

We focus on Becker's hypothesis both because it is the most commonly invoked explanation for the persistence of the motherhood penalty after every conceivable observable variable has been controlled (Budig and England 2001; Korenman and

[2]Although an improvement over cross-sectional analysis, fixed effects analyses also have limitations. In a study of displaced workers, Jacobson et al. (1993) found that workers' earnings begin to decline even before the displacement occurs. One could imagine that women who plan to become mothers might reduce their effort prior to leaving the work force and experience the same pattern of declining wages. A before and after comparison will therefore underestimate the wage penalty. Waldfogel (1998b), however, reported no wage deterioration in the NLSY in the two or three years prior to a birth, and only a slight reduction in the year immediately preceding the birth. Thus, the expected size of this bias is unclear.

Another limitation concerns unobserved effects that are not "fixed." For example, a deterioration in labor market conditions for women may lower their prospective wages and encourage women to have more children (since a reduction in wages lowers the opportunity cost of having children), leading to an overestimate of the wage penalty imposed by children. This is probably not a problem for a general labor market deterioration, since adverse labor market conditions would also affect the labor market prospects of fathers and, by increasing uncertainty and reducing income, discourage childbearing. Still, an individual-specific shock may bias the estimates.

[3]Budig and England (2001) reported evidence of smaller wage penalties for black and Latina mothers, relative to white mothers, among women with three or more children.

Neumark 1992; Waldfogel 1997) and because few studies have explicitly tested it. Budig and England (2001) studied whether mothers choose less energy-demanding occupations and concluded that "mother-friendly" job characteristics explain very little of the motherhood wage penalty. Waldfogel and Mayer (2000) also found that occupational controls do not eliminate the penalty. Hersch and Stratton (1997) found that married women's wages are negatively correlated with time spent on housework, but they did not explicitly consider mothers. Stratton (2001) demonstrated that neither reduced work effort nor compensating wage differentials associated with more flexible jobs can explain the housework penalty, again without reference to mothers. Bielby and Bielby (1988) found that mothers of pre-school children report less work effort while on the job compared with other women, but they did not test whether lower effort corresponds to lower wages.[4]

We explore another set of testable implications of Becker's hypothesis. First, since younger children demand more physical energy from care-givers than older children do, wage penalties should fall as children grow older.[5] Indeed, Bielby and Bielby (1988) found that, compared to female coworkers who were not mothers, mothers of pre-schoolers reported significantly lower effort on the job, but mothers of older children did not. If the work effort hypothesis holds, then these effort patterns should affect wages systematically. In fact, Waldfogel's (1998b) study was consistent with this wage-effort effect in finding that the wage benefits conferred by maternity leave job protection diminish over time. Second, racial differences in the size of the penalty, when measures of human capital, family structure, and household resources are controlled, would be inconsistent with the productivity story.[6] Finally, higher-skilled jobs typically require more effort (as discussed above), so wage penalties should rise with mothers' years of schooling.

Discussions in both the academic and popular press of the difficult "balancing act" required of working mothers reflect the relevance of Becker's model (see, for example, Spain and Bianchi 1996) and also suggest that, given dual responsibilities, scheduling poses real problems for working mothers. In a 1997 Pew Research Center survey, 73% of the 457 mothers interviewed rated a flexible work schedule as "very important" in choosing a job. Moreover, less than one-third of mothers who work full-time said they prefer this option (Pew Research Center 2000). Thus mothers' labor market returns may be reduced not only by energy constraints, but also by binding time constraints.

Data and Empirical Approach

When the NLSYW surveys began in 1968, a nationally representative sample of 5,159 women, ages 14 to 24 at that time, was interviewed. In each subsequent round, data were gathered on each respondent's educational attainment, employment, and fertility since the preceding round. As of the 1988 survey, 3,508 women (currently ages 34 to 44) were still being interviewed,

[4]Bielby and Bielby summed the responses to three items from the 1977 QES as their measure of effort: (1) "My job requires that I work very hard," scaled by 1 = strongly disagree to 4 = strongly agree; (2) "Altogether, how much effort, either physical or mental, does your job require?" scaled by 1 = none, 2 = only a little, 3 = some, and 4 = a lot; and (3) "And how much effort do you put into your job beyond what is required?" scaled by 1 = none to 4 = a lot.

[5]As mentioned earlier, the demands of older children tend to be time-intensive rather than effort-intensive.

[6]School quality is also an important correlate of wages. If black women are more likely than non-black women to attend low-quality schools, they may find themselves in jobs where their effort is rewarded less and, correspondingly, their children impose less of a penalty. Despite this possibility, and anticipating our results, our analyses find no evidence of racial differences in the motherhood wage penalty.

representing 68% of the original sample. For our analyses, we restrict the sample to non-Hispanic white and black women who are currently (as of the interview date) working and not currently enrolled in school.[7] Further, we restrict the sample to person-year observations for which information is available regarding education, actual labor market experience, and other regression variables, and in which the hourly wage is between $1 and $150 in 1997 dollars.[8] The final sample includes an unbalanced panel of 4,246 women (2,993 whites and 1,253 blacks) observed up to 15 times between 1968 and 1988; on average, each woman is observed 6.4 times, resulting in 27,204 woman-year observations.

Table 1 provides summary statistics for the pooled cross-section (27,204 woman-year observations) and a sample including the last observation per woman (4,246 observations). For each sample, statistics are presented for all women and separately for mothers and non-mothers; means and standard deviations are weighted to account for the over-sampling of blacks. A few differences are worthy of note. Mothers are much more likely than non-mothers to be black, married, and part-time workers. On average, mothers are about four years older and have completed almost one year less education than non-mothers. Finally, focusing on the last observation per woman, we see that by the end of the survey period, women who were ever mothers earned wages that were nearly 6% lower than those of women who were never mothers.

To show the effect on the motherhood wage penalty of adding different controls, we begin by regressing log hourly wage on race, marital status, and motherhood status using the pooled cross-section; this provides the total motherhood wage gap. We then add human capital measures, which always include years of schooling and incrementally include quadratics in potential experience, actual experience, and actual experience and age. Since time out of the labor force is approximately equal to (age – schooling – time in the labor force – 6), the inclusion of age effectively controls for years absent from the labor market. Finally, we control for current part-time status, occupation, and household resources available to working mothers. The most complete wage equation is

$$\begin{aligned}(1)\quad \ln\text{WAGE}_{it} = {} & \alpha + \beta_1\text{BLACK}_i + \beta_2\text{MARRIED}_{it} \\ & + \beta_3\text{CHILDREN}_{it} + \beta_4\text{EDUCATION}_{it} \\ & + \beta_5\text{EXPERIENCE}_{it} + \beta_6\text{EXPERIENCE}^2_{it} \\ & + \beta_7\text{AGE}_{it} + \beta_8\text{AGE}^2_{it} + \beta_9\text{PARTTIME}_{it} \\ & + \beta_{10}\text{OCCUPATION}_{it} + \beta_{11}\text{RESOURCES}_{it} + \nu_{it},\end{aligned}$$

where i indexes individual women ($i = 1 \ldots 4{,}246$ in the full sample), t indexes time ($t = 1968 \ldots 1988$), and ν_{it} is an error term.[9]

LNWAGE is the natural log of the hourly wage in real 1997 dollars; BLACK and MARRIED are indicator variables for race and marital status (married, spouse present), respectively.[10] CHILDREN is a vector of two

[7]Workers include employed and self-employed women. We eliminate 158 Hispanic women (920 woman-year observations) from the sample because they are too small a group to examine separately.

[8]We use the CPI for all urban consumers, not seasonally adjusted, to adjust all wage and income variables for the effects of inflation.

[9]In all pooled cross-section models, we account for the unbalanced panel and multiple observations per woman by (1) weighting each observation by the total number of *possible* observations per woman (here, that is the 15 years in which the survey took place) divided by the *actual* number of years that the woman appears in the pooled cross-section sample, and (2) reporting robust clustered standard errors.

[10]In OLS regressions (pooled cross-sections) that do not fully control for differences in human capital and household resources, the motherhood wage penalty for two or more children is somewhat larger for married women than for single women. However, once all observable characteristics are included and unobservable heterogeneity is controlled for using fixed effects, the motherhood penalty is the same for single and married women. Therefore, we choose to pool single and married women into one sample rather than to estimate the model separately by marital status.

Table 1. Weighted Means: All Women, Mothers, and Non-Mothers. (Standard Deviations for Continuous Variables in Parentheses)[a]

	Pooled Cross-Section			*Last Observation per Woman*		
Variable	*All Women*	*Mothers*	*Non-Mothers*	*All Women*	*Mothers*	*Non-Mothers*
Wage (1997 $)	10.81	10.46	11.20	10.20	10.01	10.60
	(5.60)	(5.66)	(5.49)	(5.58)	(5.53)	(5.67)
Black	.127	.162	.088	.128	.147	.090
Married, Spouse Present	.610	.776	0.425	.590	.716	.332
Children < Age 18 in Home	.944	1.79	n/a	.87	1.30	n/a
	(1.107)	(.90)		(1.11)	(1.11)	
Education	12.85	12.53	13.21	12.81	12.62	13.20
	(2.27)	(2.19)	(2.30)	(2.35)	(2.30)	(2.41)
Potential Experience	9.17	11.59	6.49	7.62	8.94	4.89
	(6.34)	(5.59)	(6.03)	(6.07)	(6.19)	(4.76)
Actual Experience	7.54	8.59	6.36	6.16	6.70	5.06
	(4.80)	(4.50)	(4.86)	(4.40)	(4.44)	(4.10)
Age	28.13	30.24	25.78	26.55	27.69	24.20
	(6.42)	(5.84)	(6.23)	(6.24)	(6.35)	(5.28)
Part-Time	.199	.276	.114	.227	.262	.154
Professional	.213	.184	.246	.200	.187	.226
Managerial	.058	.056	.059	.047	.045	.052
Sales	.047	.052	.042	.053	.052	.055
Clerical	.404	.382	.428	.398	.390	.416
Craft	.014	.015	.012	.012	.012	.012
Operative	.118	.146	.086	.111	.129	.075
Service	.139	.155	.121	.168	.176	.153
Children Age 18+ in Home	.07	.08	.05	.05	0.07	.001
	(.32)	(.34)	(.29)	(.27)	(0.33)	(.031)
Other Adults in Home	1.07	.93	1.22	1.17	1.060	1.390
	(0.87)	(.60)	(1.08)	(.95)	(0.782)	(1.199)
Husband's Income (1997 $)	19,187	25,600	12,055	17,764	22,034	8,970
	(22,129)	(22,772)	(19,000)	(21,165)	(21,822)	(16,585)
Own Non-Labor Income	618	867	341	486	599	252
(1997 $)	(2,494)	(2,744)	(2,148)	(2,029)	(2,018)	(2,032)
Observations	27,204	15,268	11,936	4,246	2,958	1,288

[a]Means and standard deviations are weighted to account for the over-sampling of blacks. Husband's income is zero for unmarried women; observations with zeroes are included in computing averages for number of children, number of other adults, husband's income, and average nonlabor income. For the pooled cross-section, "Mother" is defined as a woman having at least one child under the age of 18 at home during a given year. For the individual women sample, "Ever Mother" is defined as a woman *ever* having at least one child under the age of 18 at home. All differences in means (mothers versus non-mothers) are statistically significant at the 5% level except the following: managerial occupation in the pooled cross-section; managerial, sales, clerical, craft, and service occupations in the individual women sample.

dummy variables for one child and two or more children (under the age of 18) living in the household, following Korenman and Neumark (1992) and Waldfogel (1997). Measures of human capital investment include EDUCATION (years of completed education), quadratics in labor market experience (EXPERIENCE and EXPERIENCE^2) and age (AGE and AGE^2), PARTTIME (an indicator variable for usually working less than 35 hours per week), and OCCUPATION (a series of indicator variables for the following categories: professional, technical and kindred; managers, officials, and proprietors;

clerical and kindred [the omitted category]; sales workers; crafts, foremen, and kindred; operatives and kindred; service workers including private household workers; and other occupations, including farmers and farm managers, farm laborers and foremen, laborers, and armed forces).

Finally, RESOURCES is a vector of variables that measure each woman's access to resources in the household that may mitigate (or exacerbate) the motherhood wage penalty: number of adult children (age 18 and older) in the household, number of other (related or unrelated) adults (age 18 and older) in the household, husband's income (measured in thousands of real 1997 dollars),[11] and own non-labor income (measured in thousands of real 1997 dollars).[12] On the one hand, we might expect greater household resources to reduce the motherhood penalty by providing working mothers with more adults to help in the care of children and more income with which to purchase childcare, restaurant meals, and other substitutes for home production. On the other hand, these same resources may, in fact, increase the motherhood penalty if the other adults in the household also need care (for example, aged parents) or if husbands with greater income contribute less to home production.[13]

We then proceed to a fixed effects analysis in order to control for unobserved heterogeneity between mothers and non-mothers. We use a specification similar to equation (1), except that all variables pertain to mean-differenced values across years for each woman, and the error term is composed of a fixed component (α_i) and time-varying component (μ_{it}). In addition, for some models, we include a more detailed vector of children variables equal to the number of children in each of five age groups, corresponding to five distinct stages in children's lives—infants and toddlers (birth through 2 years old), pre-school children (3–5 years old), elementary school children (6–10 years old), middle school children (11–13 years old), and high school children (14–17 years old)—in order to test whether the penalty is largest for working mothers of young children.

Finally, we address heterogeneity among working mothers and test other implications of the work effort hypothesis by estimating the wage penalties for different subgroups of mothers. Mothers who return to work when their children are infants may differ in unobservable, wage-enhancing capital from mothers who stay home until their children are older, either through a greater commitment to their career or by returning to the same job and thereby benefiting from a good job match and a pre-existing stock of firm-specific capital.[14] These differences would be only partly captured in measures of time out of the labor force. To control for this heterogeneity, we interact all independent variables by a discrete return-timing variable that groups mothers by the age of their youngest child in any year they worked. For example, a woman who leaves the labor force for four

[11]This variable is equal to all income (labor and non-labor) of the respondent's husband; it includes half of income that was reported jointly for wife and husband. This variable is set to zero for unmarried women.

[12]This includes all income reported by the respondent *except* earnings from wage and salary, own business or farm, or unemployment insurance. If the respondent is married, this variable includes half of income that was reported jointly with her spouse.

[13]Hersch and Stratton (1994) found that a husband's share of housework declines with increases in his labor income. This is consistent with both a time allocation model (in which the husband's greater income contribution reflects his greater marginal productivity in the labor market, relative to his marginal productivity at home) and a household bargaining model (in which the husband's greater income leads to greater bargaining power within the relationship). Bittman et al. (2001), however, pointed to the importance of the husband's share of income in determining his wife's housework hours: the more unequal the husband/wife shares, the more hours the wife spends in housework, even if it is the wife who is contributing more income to the household.

[14]For example, Waldfogel (1998b) found a statistically significant positive effect of maternity leave on both the likelihood of returning to the same employer after childbirth and the post-interruption wage.

Table 2. Wage Effects of Children: Pooled Cross-Section and Fixed Effects Models. (Standard Errors in Parentheses)[a]

	Pooled Cross-Section						*Fixed Effects*	*Fixed Effects by Age of Child at Return to Work Force*		
								0–2 Years	*3–5 Years*	*6–17 Years*
Independent Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
One Child	−.069***	−.089***	−.076***	−.056***	−.049***	−.052***	−.030***	−.027***	−.024	−.017
	(.013)	(.012)	(.011)	(.012)	(.012)	(.073)	(.007)	(.008)	(0.021)	(.027)
2 or More Children	−.134***	−.161***	−.122***	−.082***	−.071***	−.073***	−.055***	−.055***	−.006	−.042
	(.013)	(0.014)	(0.012)	(.014)	(.014)	(.013)	(.008)	(.010)	(.026)	(.033)
Education		X	X	X	X	X	X	X	X	X
Experience		X	X	X	X	X	X	X	X	X
Experience^2		X	X	X	X	X	X	X	X	X
Experience Type		Potential	Actual	Actual	Actual	Actual	Actual	Actual	Actual	Actual
Age				X	X	X	X	X	X	X
Age^2				X	X	X	X	X	X	X
Part-Time					X	X	X	X	X	X
Occupation & Household Resources					X	X	X	X	X	
R-squared	.029	.247	.281	.297	.302	.375	.186	.183	.160	.162
Observations			27,204 Woman-Years				4,246 Women	2,197 Women	490 Women	271 Women

	Percentage Point Difference in Motherhood Penalty from Previous Specification[b]					Total Cols. 2–6 (% of Col. 1)	Percentage Point Difference in Motherhood Penalty from Previous Spec.[b] (% of Col. 1)[c]			
Kid1	+1.8	−1.2	−1.9	−0.7	+0.3	−1.6 (24%)	−2.1 (56%)	−0.1 (57%)	+0.7 (46%)	−1.2 (73%)
Kid2+	+2.3	−3.4	−3.6	−1.0	+0.2	−5.5 (44%)	−1.7 (57%)	−0.2 (59%)	−4.2 (91%)	−1.4 (69%)

X indicates variables that were included in the regression.

[a]In the pooled cross-section regressions, we weight each observation by the total number of *possible* observations per woman divided by the *actual* number of years that the woman appears in the sample and estimate robust clustered standard errors in order to control for an unbalanced panel and multiple observations per woman. In the fixed effects regressions, we report the within R-squared. All models include controls for race and marital status.

[b] The wage penalty for children as a percentage is $(e^{\beta} - 1)$, where β is the estimated regression coefficient; the "percentage point difference" is calculated using this transformation.

[c]"% of col. 1" is the percentage of the actual motherhood penalty estimated in column 1 that is explained by the full specification in each column.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

years and has two children who are two and four years of age when she goes back to work would be given the fixed classification of returning with a child 0–2 years of age. Other possible designations are returning with a child 3–5 years of age and returning with a child 6–17 years of age.[15] We also test whether the penalty varies by race, and if it is higher for more educated mothers.

Human Capital, Heterogeneity, and Sample Composition: Results

The first six columns of Table 2 show the effect on the motherhood wage penalty of controlling for various factors.[16] Column (1) shows ordinary least squares (OLS) results for the pooled cross-section, controlling only for race, marital status, and the presence of one child or two or more children. We interpret the coefficients of the children variables as the total motherhood penalty, including the direct effect of children on wages as well as any indirect effects of children such as reduced labor market experience, lower on-the-job effort, heterogeneity between mothers and non-mothers, and employer discrimination. The presence of one child reduces a mother's wage by about 7%; the presence of two or more children reduces her wage by nearly 13%. These estimates are similar in magnitude to those found by Waldfogel (1997) in regressions that included measures of human capital. Column (2) shows that the penalties *rise* by 1.8 and 2.3 percentage points, respectively, when we include education and a quadratic in potential experience, the standard arguments of the Mincer wage equation.[17] The increase probably arises because mothers tend to be older than non-mothers and thus have greater potential experience.

Potential experience overestimates women's actual work experience if women take any time off to bear and raise children. This is apparent in the data: when we measure experience directly[18] (see column 3 of Table 2), the effects of one child and two or more children fall by 1.2 and 3.4 percentage points, respectively. The addition of age and age squared in column (4) further controls for number of years out of the work force, and reduces the motherhood wage penalties by another 1.9 and 3.6 percentage points for one and two-or-more children, respectively. Since our goal in this paper is to estimate and interpret the "residual" motherhood wage penalty that persists even after all readily controlled factors are included in the regression, we control for actual labor market experience and time out of the labor force in all analyses from here forward.[19] Controlling for

[15]Since we also control for presence of children in each age group, this approach distinguishes heterogeneity in return timing from the effect of having children of particular ages.

[16]Appendix A reports the coefficient estimates for all of the controls.

[17]Potential experience is equal to the lesser of (age – education – 6) or (age – 16). This helps to reduce measurement error, in terms of too much potential work experience, for individuals with very little educational attainment.

[18]Note that "actual labor market experience" is measured using information on actual weeks worked in each year as reported in the fifteen surveys. If a woman works 50 or more weeks in a given year, she is credited with a full year of labor market experience; otherwise, her labor market experience for that year is equal to actual weeks worked divided by 50. This is the standard definition of a full-year worker according to the Bureau of Labor Statistics (see, for example, Hayghe and Bianchi 1994; Cohen and Bianchi 1999).

Total labor market experience is equal to the sum of actual labor market experience in every year between 1968 and 1988, plus "potential labor market experience" prior to 1968. Given the young age of women in 1968—ages 14 to 24—the use of potential experience for this period is unlikely to induce much measurement error.

Gaps in the available information on work experience make it necessary to impute actual experience in some cases. Specifically, if information on weeks worked is missing in year t but not missing in years $t-1$ and $t+1$, then weeks worked in year t are estimated to equal the average of weeks worked in $t-1$ and $t+1$. Thus, we impute labor market experience for any given year *only* if we have valid information on actual labor market experience in the two surrounding years; only 1,520 woman-year observations (5.6% of the sample) have imputed experience.

[19]Recall, however, that children may also indirectly lower wages by reducing mothers' work experience and increasing mothers' labor market interruptions.

part-time work, occupation, and measures of household resources in columns (5) and (6) has a net effect of reducing the penalty for one child by 0.4 percentage point and the penalty for two children by close to one percentage point. This results in a 5% penalty for one child, which is similar to the penalties reported by Korenman and Neumark (1992) in their cross-sectional model and by Waldfogel (1997). However, the estimated penalty for two or more children (7%) is smaller than the penalty found by others.

Taken together, human capital, occupational, and household resource variables account for 24% of the total wage penalty for one child and 44% of the total wage penalty for two or more children. Actual experience and years out of the labor force alone account for 19% to 37% of the observed total motherhood penalties.

Column (7) of Table 2 reports estimates for a fixed effects model using the same control variables as column (6); this reduces the penalty by an additional 2.1 percentage points for one child and an additional 1.7 percentage points for two or more children.[20] Thus we find that unobserved heterogeneity between mothers and non-mothers accounts for a significant portion—up to 32%—of the motherhood wage gap. Controlling for both observed and unobserved differences between mothers and non-mothers, we find a 3% wage penalty for having one child and a 5% penalty for having two or more children. Our estimates of the unexplained penalty lie between those reported by Korenman and Neumark (1992), who reported no unexplained penalty in a short differences model, and Waldfogel (1997), who reported penalties of 4% for one child and 12% for two or more children.[21]

[20]The fixed effects results in Table 2 are robust with respect to the inclusion or exclusion of part-time, occupation, and household resource measures.

[21]We find lower penalties than did Waldfogel (1997) even when we use a nearly identical model specification. We suspect that the difference lies in our more conservative rule for imputing work experience, since Waldfogel retained more observations in her sample of working women.

Our estimates thus far represent the average penalty across all working mothers. Columns (8)–(10) of Table 2 show results for models estimated separately for mothers who returned to work when their youngest child was an infant or toddler (0–2 years old), of pre-school age (3–5 years old), and of school age (6–17 years old), respectively. Mothers who returned to work with infants or toddlers comprise the vast majority (74%) of working mothers: their subsample penalties are just about the same as those calculated for the entire sample. Mothers who returned to work with children 3–5 years of age (17% of working mothers) experience a slightly higher wage penalty for one child, but that penalty is only marginally distinguishable from zero. The wage penalty for those women with two or more children and for mothers who returned to the work force when their children were 6–17 years of age are not statistically significant. Although these results suggest that mothers who return most quickly to the work force bear the brunt of the motherhood penalty, the decompositions in the following section show that this is not, in fact, the case.

Who Bears the Motherhood Wage Penalty?

We begin our decompositions by evaluating the wage effect of children at different ages. As discussed above, if the work effort hypothesis holds, then the motherhood wage penalty will diminish as children grow older. Column (1) of Table 3 shows a 2.7% wage penalty per child for infants and toddlers (0–2 years old) for all women. The wage penalty for older children ranges between 1.1% and 1.7%. F-tests reject equality of the wage effects of all age groups. As expected, infants and toddlers impose the largest estimated penalty, and F-tests reject equality of this penalty with the penalties for older children at the 15% significance level or better. However, it is also clear that the wage penalties, although small in magnitude, persist even as children age.

Columns (2)–(4) of Table 3 show estimates for a fixed effects model fully inter-

Table 3. Wage Effects of the Number of Children of Different Ages by Timing of Return to the Work Force: Fixed Effects Models. (Standard Errors in Parentheses)[a]

	(1)	(2)	(3)	(4)
		Age of Youngest Child at Return to Work		
Age Group	*All Women*	*0–2 Years*	*3–5 Years*	*6–17 Years*
0–2 Years Old	−.027***	−.026***	—	—
	(.006)	(.006)		
3–5 Years Old	−.016***	−.010*	−.038***	—
	(.005)	(.005)	(.015)	
6–10 Years Old	−.012***	−.009*	.009	−.026
	(.004)	(.005)	(.013)	(.017)
11–13 Years Old	−.017***	−.006	−.016	−.021
	(.005)	(.006)	(.016)	(.018)
14–17 Years Old	−.011**	−.008	.015	.006
	(.005)	(.007)	(.015)	(.017)
Number of Woman-Years	27,204	16,559	2,872	1,439
Number of Women	4,246	2,197	490	271
Null Hypothesis:		F-Value (P-Value)		
Equal Effects for All Age Groups	1.83	4.55	3.15	1.58
	(.120)	(.0004)	(.014)	(.191)
Ages 0–2 = Ages 3–5	2.53	6.08	—	—
	(.112)	(.014)		
Ages 3–5 = Ages 6–10	.66	.03	8.90	—
	(.418)	(.854)	(.010)	
Ages 6–10 = Ages 11–13	.68	.16	2.70	.09
	(.410)	(.693)	(.101)	(.770)
Ages 11–13 = Ages 14–17	.88	.07	3.35	1.82
	(.349)	(.797)	(.068)	(.177)
Ages 0–2 = Ages 6–10	6.21	8.13	—	—
	(.013)	(.004)		
Ages 0–2 = Ages 11–13	2.26	7.63	—	—
	(.133)	(.006)		
Ages 0–2 = Ages 14–17	5.74	5.82	—	—
	(.017)	(.016)		
Ages 3–5 = Ages 14–17	.93	.05	9.52	—
	(.335)	(.816)	(.002)	
Ages 6–10 = Ages 14–17	.08	.01	.19	3.05
	(.777)	(.929)	(.660)	(.081)

[a]Models include all variables in the full specification (Col. 7, Table 2).
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

acted by timing of the mother's return to work. This decomposition shows that at least some of the persistence in the penalty is due to sample composition. For the majority of women who return to work with a child five years of age or younger, the wage penalty is greatest when they first return. The penalties in the first years of return to work are consistent with the possibility that mothers experience a period of adjustment in managing child and job responsibilities. For example, mothers who return to work when their children are infants experience only a 1% penalty for each pre-school child, compared with a nearly 4% penalty for mothers who recently returned to work. This pattern is also consistent with a job-matching story. Under

Table 4. Wage Effects for Number and Age of Children, by Race. (Standard Errors in Parentheses)[a]

	Ordinary Least Squares			*Fixed Effects*		
	White	*Black*	*Diff.*[b]	*White*	*Black*	*Diff.*[b]
One Child	−.052***	−.052***		−.030***	−.020*	
	(.013)	(.020)		(.008)	(.011)	
2 or More Children	−.075***	−.064***		−.047***	−.054***	
	(.016)	(.021)		(.010)	(.014)	
R-Squared	.364	.389		.193	.192	
Number of Children						
0–2 Years Old	−.061***	−.055***		−.023***	−.032***	
	(.011)	(.014)		(.007)	(.009)	
3–5 Years Old	−.023**	−.021*		−.013**	−.020***	
	(.010)	(.012)		(.006)	(.007)	
6–10 Years Old	−.031***	−.002	xx	−.014***	−.010	
	(.009)	(.009)		(.005)	(.007)	
11–13 Years Old	−.040***	−.004	xx	−.026***	−.0005	xx
	(.012)	(.014)		(.007)	(.008)	
14–17 Years Old	.004	.010		−.002	−.017**	
	(.011)	(.013)		(.007)	(.008)	
R-Squared	.365	.390		.193	.192	
Observations						
Woman-Years	19,206	7,998				
Women				2,993	1,253	

[a]In the pooled cross-section regressions, we weight each observation by the total number of *possible* observations per woman divided by the *actual* number of years that the woman appears in the sample and estimate robust clustered standard errors in order to control for an unbalanced panel and multiple observations per woman. In the fixed effects regressions, we report the within R-squared. Models include all variables in the full specification (Col. 7, Table 2).

[b]An "xx" indicates that the white and black coefficients are significantly different at the 5% level based on point estimates of race-interacted coefficients for models that pooled the white and black samples.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

this scenario, the first job a mother takes upon her return to the labor market may not be the best match. Over time, however, she is likely to improve her match. Finally, we observe that penalties associated with children do diminish as they grow older; these patterns are consistent with the work effort hypothesis.[22]

Table 4 shows results for models estimated separately by race. To facilitate comparisons with previous research, the table reports both OLS (pooled cross-section) and fixed effects wage penalty estimates for both the presence of one or two or more children (top panel) and the number of children by age group (bottom panel). The

[22]These patterns are also consistent with sample selection bias. Stratton (1995) suggested that workers who receive low reentry wages relative to their wages prior to a labor force interruption are more likely to leave the labor force again. In our analysis, their presence in the group of women first returning to the labor force will raise the estimated penalty for children; their absence thereafter will lower it. As we show in what follows, the decline in the wage penalty over time does not apply to all educational groups, suggesting that sample selection does not fully explain the pattern of our results. Nevertheless, this bias may exaggerate the pattern where it does exist.

Table 5. Wage Effects of Children by Mother's Race and Return Timing, Fully Interacted Fixed Effects Models. (Standard Errors in Parentheses)[a]

Main Effect: White Women Who Returned to Work When Their Child Was 0–2 Years Old	
Number of Children	
0–2 Years Old	−.024*** (.007)
3–5 Years Old	−.007 (.007)
6–10 Years Old	−.012* (.006)
11–13 Years Old	−.015* (.009)
14–17 Years Old	−.003 (.009)
Number of Woman Years	11,027
Number of Women	1,448

	Age of Youngest Child at Return to Work		
Number of Children	*0–2 Years*	*3–5 Years*	*6–17 Years*
	Relative Effect: Other White Women		
3–5 Years Old	See	−.037**[(a)] (.018)	
6–10 Years Old	Main	.016 (.016)	−.020[(a)] (.021)
11–13 Years Old	Effects	−.007 (.020)	−.037*[(a)] (.023)
14–17 Years Old	Above	.027 (.020)	.013 (.022)
Number of Woman Years	11,027	1,965	1,134
Number of Women	1,448	312	212
	Relative Effect: Black Women		
0–2 Years Old	−.005[(a)] (.013)		
3–5 Years Old	−.005[(b)] (.011)	−.052**[(b)] (.026)	
6–10 Years Old	.004 (.010)	.008 (.024)	−.005 (.049)
11–13 Years Old	.019 (.013)	−.002 (.028)	.080*[(a)] (.048)
14–17 Years Old	−.011[(c)] (.014)	−.018 (.028)	−.013 (.051)
Number of Woman Years	5,532	907	305
Number of Women	749	178	59

[(a)], [(b)] and [(c)] denote significant total effect in models run separately for each subgroup at the .05, .10, and .15 levels, respectively.

[a]Models include all variables in the full specification (Col. 7, Table 2).

*Significant main or relative effect at the .10 level; **at the .05 level; ***at the .01 level.

top panel shows that the overall estimated motherhood penalties for black and white mothers are very similar. The second panel shows slightly larger penalties for black mothers of young children and significantly smaller penalties for black mothers of middle-school children. However, when we control for return timing, these small racial differences become less apparent.

Table 5 compares wage penalties by race in a fixed effects model that is fully interacted by both race and return timing. The top panel of Table 5 shows main wage effects for the biggest subgroup: white women who returned to work with an infant or toddler. The second and third panels show wage effects relative to the main group for white and black women, respectively. Table 5 shows that the motherhood wage penalty experienced by early returning black mothers is statistically indistinguishable from that experienced by their white counterparts. Both black and white mothers who return when their youngest child is preschool-aged face a large penalty upon reentry. In sum, there do not appear to be systematic differences between white and black women, a finding that again accords with the work effort hypothesis.[23]

[23]Our results by race are robust with respect to the exclusion of occupation and household resources

Table 6. Wage Effects for Number and Age of Children, by Education. (Standard Errors in Parentheses)[a]

	Ordinary Least Squares							*Fixed Effects*						
Schooling Level	*High School Dropout*	*Diff. from HSG*[b]	*High School Graduate*	*Some College*	*Diff. from HSG*[b]	*College Graduate*	*Diff. from HSG*[b]	*High School Dropout*	*Diff. from HSG*[b]	*High School Graduate*	*Some College*	*Diff. from HSG*[b]	*College Graduate*	*Diff. from HSG*[b]
One Child	.0003		−.050***	−.097***		−.086***		.004	xx	−.041***	−.057***		.005	xx
	(.028)		(.016)	(.036)		(.031)		(.020)		(.009)	(.019)		(.019)	
2 or More Children	.018	xx	−.078***	−.084**		−.183***	xx	−.0002	xx	−.058***	−.112***	x	−.031	
	(.028)		(.020)	(.038)		(.043)		(.023)		(.012)	(.024)		(.025)	
R-Squared	.230		.256	.301		.174		.139		.181	.183		.201	
Number of Children:														
0–2 Years Old	−.051***		−.059***	−.044*		−.084***		−.035**		−.037***	−.022		.004	xx
	(.019)		(.013)	(.023)		(.031)		(.015)		(.008)	(.015)		(.016)	
3–5 Years Old	.004		−.027**	−.052*		−.052*		.0001		−.019***	−.035***		−.015	
	(.016)		(.011)	(.029)		(.031)		(.012)		(.007)	(.014)		(.016)	
6–10 Years Old	.021*	xx	−.019*	−.055***		−.108***	xxx	.016	xxx	−.019***	−.052***	xx	−.015	
	(.012)		(.011)	(.022)		(.024)		(.010)		(.007)	(.013)		(.015)	
11–13 Years Old	.001	x	−.045***	.003		−.102***		.012	xxx	−.030***	−.032**		−.045**	
	(.018)		(.013)	(.027)		(.033)		(.012)		(.008)	(.017)		(.021)	
14–17 Years Old	.027		−.007	.008		−.049		.008	xx	−.019**	−.025		−.005	
	(.017)		(.015)	(.029)		(.038)		(.012)		(.008)	(.017)		(.024)	
R-Squared	.234		.257	.300		.178		.142		.181	.182		.202	
Observations	4,114		11,794	3,044		3,876		770		1,801	485		637	

[a]Sample includes 22,828 woman-year observations for 3,693 women. In the pooled cross-section regressions, we weight each observation by the total number of *possible* observations per woman divided by the *actual* number of years that the woman appears in the sample and estimate robust clustered standard errors in order to control for an unbalanced panel and multiple observations per woman. In the fixed effects regressions, we report the within R-squared. Models include all variables in the full specification (Col. 7, Table 2).

[b]A symbol in these columns indicates that the coefficient in the preceding column is significantly different from the high school graduate coefficient based on point estimates of race-interacted coefficients for models which pooled all education groups as follows: x = .10 level; xx = .05 level; xxx = .01 level.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Finally, we estimate the motherhood wage penalty for women with different educational levels. According to our earlier discussion, the effort requirement of work should rise with education. To clarify the analysis, we restrict the sample to women whose educational group does not change over their work history.[24] Approximately 85% of the sample (3,693 of 4,246 women, and 2,518 of 2,958 mothers) meets this criterion. We compare four distinct educational groups: high school dropouts (less than 12 years of schooling), high school graduates (exactly 12 years of schooling), those with some college (13–15 years), and college graduates (16 or more years).

Table 6 shows OLS and fixed effects results for the two specifications of children's presence. The fixed effects models in the top panel reveal a clear pattern by skill: both the least and the most skilled mothers bear no wage penalty for the presence of children, while those at the medium-skill level (those with at least 12 years of schooling but without a college degree) bear significant wage penalties, ranging from 4% for one child to almost 11% for two or more children. Note that the estimated OLS wage penalties for women with a college degree are among the largest for any education group, but these penalties all but vanish in the fixed effects specification. Clearly, this prediction of the work effort hypothesis—that the motherhood wage penalty should rise with skill—is not supported by the data.

The second panel shows a decomposition of the wage penalty by children's age groups. Here we see that the least skilled mothers of infants experience about the same wage penalty as high school graduates, but that older children impose no penalty on those with less than 12 years of schooling. By contrast, the wage penalty persists for medium-skilled mothers with children of all ages. Finally, highly skilled mothers appear to experience a wage penalty only for their middle-school children. This result appears, however, to be the result of heterogeneity among women in the timing of their return to work, as shown in Table 7.

Table 7 presents the estimates for a fixed effects model fully interacted by education and return timing. The first panel of Table 7 reports the main effects for the largest education-timing subgroup: high school graduates who returned to work when their youngest child was an infant or toddler. All other figures represent effects relative to the base group. The base group experienced a 3% motherhood penalty for each infant and toddler when they returned to work. The penalty, however, falls to only 1% for older children. Moreover, the estimates of older-children penalties are below conventional confidence levels, with p-values of 30%.

The experience of early returning high school dropouts is not statistically distinguishable from that of the base group, except for a marginally significant wage premium associated with elementary school–age children. This suggests that high school dropouts also experience a wage penalty when their children are very young. Nevertheless, all of the relative effects for this group are positive and, beginning with preschool-age children, are large enough to offset the negative base group effects. High school dropouts who return to work when their youngest child is 3–5 years of age appear to experience no wage penalty initially, and, like earlier returning dropouts, receive a wage premium for their school-age children, relative to the base group. The premium for this group is statistically significant at the 10% level.

By contrast, high school graduates who delay their return to work experience a statistically significant penalty of more than

from the regression. When we estimate a similar model using the "one child" and "two or more children" specification, late-returning black women appear to avoid the penalty. This group is probably the source of the diminished penalties reported in Hill (1979) and Waldfogel (1997).

[24]The results are quantitatively very similar, and our conclusions are unchanged, when we perform this analysis using initial education level to stratify the sample by education groups.

Table 7. Wage Effects of Children by Mother's Education and Return Timing, Fully Interacted Fixed Effects Models. (Standard Errors in Parentheses)[a]

Main Effect: High School Graduates Who Returned to Work When Their Child Was 0–2 Years Old

Number of Children	
0–2 Years Old	−.032***[(a)] (.009)
3–5 Years Old	−.009 (.009)
6–10 Years Old	−.009 (.008)
11–13 Years Old	−.010 (.010)
14–17 Years Old	−.011 (.011)
Number of Woman Years	7,312
Number of Women	909

	Age of Youngest Child at Return to Work		
Number of Children	*0–2 Years*	*3–5 Years*	*6–17 Years*
Relative Effect: Mothers with Less Than 12 Years of Schooling			
0–2 Years Old	.002[(a)] (.017)		
3–5 Years Old	.011 (.016)	.008 (.031)	
6–10 Years Old	.022 (.014)	.050*[(b)] (.027)	−.028 (.038)
11–13 Years Old	.012 (.017)	.034 (.030)	.039 (.039)
14–17 Years Old	.009 (.017)	.030 (.029)	.015 (.040)
Number of Woman Years	2,611	612	376
Number of Women	416	130	79
Relative Effect: Mothers with 12 Years of Schooling			
3–5 Years Old	See	−.058**[(a)] (.024)	
6–10 Years Old	Main	−.033 (.023)	−.012 (.038)
11–13 Years Old	Effects	−.058**[(a)] (.027)	−.057 (.038)
14–17 Years Old	Above	.001 (.027)	−.021 (.034)
Number of Woman Years		1,291	535
Number of Women		220	101
Relative Effect: Mothers with More Than 12 Years of Schooling, but without College Degree			
0–2 Years Old	.018 (.019)		
3–5 Years Old	.003 (.019)	−.058[(b)] (.057)	
6–10 Years Old	−.004 (.019)	−.027 (.053)	−.102[(b)] (.083)
11–13 Years Old	.014 (.024)	−.033 (.056)	−.059 (.089)
14–17 Years Old	.031 (.025)	−.061 (.057)	−.039 (.070)
Number of Woman Years	1,777	246	114
Number of Women	241	38	22
Relative Effect: Mothers Who Are College Graduates			
0–2 Years Old	.040** (.019)		
3–5 Years Old	.003 (.020)	−.010 (.068)	
6–10 Years Old	.003 (.021)	.022 (.065)	.021 (.067)
11–13 Years Old	−.019 (.029)	.079 (.089)	−.118*[(a)] (.065)
14–17 Years Old	−.005 (.035)	.100 (.101)	.045 (.072)
Number of Woman Years	2,133	160	165
Number of Women	306	28	28

[(a)] and [(b)] denote significant total effect in models run separately for each sub-group at the 5% and 10% levels, respectively.

[a]Models include all variables in the full specification (Col. 7, Table 2).

*Significant main or relative effect at the .10 level; **at the .05 level; ***at the .01 level.

6% per child when they first return, and, moreover, the penalty persists even as their children grow older. Compared to the marginally significant 1% penalty associated with school-age children of high school graduates who returned to work within three years of the birth of a child, those who returned to work three to five years after a birth experienced wage penalties of 4–6% for elementary school and middle-school children. These effects are also statistically significant in models that restrict the sample to later-returning high school graduates. Note that the pattern for those with some college is similar, with no discernible difference from the base group for early returners and higher penalties for late returners.

Finally, we consider the wage penalty experienced by college graduates relative to the base group. Mothers who return right away actually experience nearly a 1% wage premium for their infants and toddlers. For the majority of mothers who return before their children enter school, the wage penalty is not statistically distinct from that for the base group. College graduates who return to work when their children are pre-schoolers exhibit no significant penalty. Those postponing their return until their children are of school age exhibit a strong penalty for middle school children only. This aberration, although statistically significant, may be attributable to outliers within this small subsample of mothers.

The results for mothers of different educational levels are striking. Specifically, it is medium-skilled mothers—those who graduated from high school but did not complete four years of college—who bear the brunt of the wage penalty, especially if they delay their return to work until their children are pre-schoolers. Mothers who are high school dropouts experience a wage penalty only for infants and toddlers; almost all college graduates experience no wage penalty at all. The non-monotonic effect of education and the persistence of the penalty for later-returning medium-skilled mothers are particularly at odds with the work effort explanation.

Flexible Schedules and Medium-Skilled Workers

Bielby and Bielby (1988) found that women with less labor market continuity (in our sample, the late returners) report less effort while at work than other women, and that more educated women report more effort. By assigning these different effort levels to different groups of mothers, and by assuming that high school dropouts also expend more effort on the job, at least once their children are older, one could argue that our findings do not contradict the work effort hypothesis. We are uncomfortable, however, with a model that relies on extreme and unsystematic heterogeneity among subgroups of mothers in expending work effort. We therefore propose an alternative, and perhaps complementary, hypothesis based on the work-schedule inflexibility of some types of jobs.

While Becker's analysis focuses on finite energy as the primary constraint, we believe that time, and in particular time spent at work during the middle of the day, also presents a restrictive constraint. Suppose that a worker's productivity, and thus her wage, are determined by (1) standard office-hour time at work (that is, 9 a.m. to 5 p.m.); (2) time spent working outside the 9-to-5 norm, whether at the workplace or at home; and (3) effective effort. Further, effective effort is an increasing function of education. This would be true under either the theory that education increases productive human capital (Becker 1964; Mincer 1974) or a signaling theory by which more ambitious workers obtain more education because doing so is less costly for them than it is for other workers (Spence 1973).

A worker who is time- or effort-constrained may be able to avoid a wage penalty by making offsetting adjustments among the three inputs. Whether this is possible will depend on the degree of substitutability among these factors. Jobs vary by the extent to which productivity depends on each input and by the extent to which one input is substitutable for another. For soli-

tary activities like writing, other time can be freely substituted for office hours. Similarly, in some jobs, putting forth greater effective effort can reduce time spent working.

Jobs that require a college degree are more likely than other jobs to require relatively high levels of effective effort. This follows from the relationship between effort and education specified above. Further, college graduates are likely to have more autonomy in their jobs with respect to both working hours and working methods than are less-educated workers, and therefore likely to enjoy greater substitutability among inputs. High school graduates are those most likely to have clerical jobs that require their presence during office hours, and are less likely than college graduates to have sufficient autonomy in those jobs to allow them to substitute either effort or other time for actual office hour time. Finally, many of the jobs that are available to high school dropouts, including food service, housekeeping, and manufacturing, entail shift work, including hours outside the 9-to-5 norm. For these jobs, time generally matters more than effort, and time outside of office hours is no less valuable than regular office hours.

To complete this explanation, suppose that the same factors that determine a mother's productivity generate disutility for her, again at different rates and for different reasons. Just as spending more time at work reduces leisure time, spending more effort at work will eventually undermine a mother's energy level at home. Office hours may present particular problems for two reasons: they coincide with the time of day when children are most active and thus need the most supervision, and they are the hours during which a helping husband is most likely to be working, himself. Mothers who have the strongest distaste for office-hour jobs, either because they lack support at home or because they strongly prefer spending those hours with their children, are the most likely to delay re-entry into the labor force. When they do return to the work force, particularly if they are high school graduates, they will most likely find jobs that require their presence during regular office hours. If they have higher absenteeism (for the same reasons they delayed re-entry) and are unable to substitute other time or increased effort, they will suffer a wage penalty relative to early returning mothers who have evidenced a milder disutility for office hour work. College graduates and low-skilled workers avoid the penalty by substituting effort and flexible time.[25]

Table 8 summarizes work schedule flexibility measures by education, as reported in the May 1991 CPS Supplement. The first four rows of the table suggest that schedule flexibility declines with education. Compared to mothers with higher educational attainment, high school dropouts are more likely to work on weekends and are likely to experience greater variability in starting and stopping times. College graduates are the least likely to work on the weekend and are the most likely to work during the day. However, the last two rows of Table 8 indicate that college graduates are much more likely to work at home as part of their job or to have jobs that allow for some flexibility. High school graduates and those with some college are caught in the middle. Like the college graduates, they work during standard office hours; unlike college graduates, they are not able to work at home or to take advantage of flexible scheduling. Thus the CPS data provide some support for the work schedule flexibility explanation; more direct tests of this explanation await future research.

Conclusions and Policy Implications

Others have identified a wage penalty among working mothers and have investi-

[25]For separate, education-level–based labor markets to co-exist, the high school graduate's wage with the penalty must still be greater than the non-penalized high school dropout's wage. If not, the high school graduates with strong disutility for working during office hours would take the lower-skilled, but more flexible, jobs.

Table 8. Work Schedule Flexibility by Education Level for Women 24–44 Years of Age.

Description	*High School Dropout*	*High School Graduate*	*Some College*	*College Graduate*
Proportion Who Work on the Weekend	0.226	0.146	0.125	0.105
Standard Deviation of Usual Hour Start Time of Job	3.86	3.54	3.49	2.62
Standard Deviation of Usual Hour End Time of Job	3.89	3.46	3.32	2.68
Proportion Who Work a Day Shift	0.751	0.799	0.793	0.871
Proportion Who Work Flextime or on a Schedule That Allows for Variation in When They Begin and End the Work Day	0.096	0.143	0.183	0.212
Proportion Who Work at Home as Part of Their Job	0.032	0.078	0.122	0.377
N	1,095	5,934	3,701	4,368

Source: Authors' calculation from the May 1991 CPS Supplement on Work Schedules.
Our sample includes women ages 24–44 who worked or had a job in the week preceding the survey, were not self-employed, and were not enrolled in school.

gated reasons for its existence. In this paper, we use a detailed panel data set to consider each of the proffered explanations. Like researchers before us, we control for human capital inputs and for unobserved heterogeneity. These controls explain 55–57% of the wage gap between mothers and non-mothers, leaving an unexplained wage penalty of 3–5%. We extend previous research by addressing heterogeneity among mothers identified by the timing of their return to the work force. We find that mothers tend to face the highest wage penalty when they first return to work, even if their children are older. This may reflect learning to manage the dual responsibilities of work and children, the greater incidence of illnesses among children when they first enter a daycare or school environment, or poor match quality, on average, in the first jobs that are taken on reentry.

We also investigate the incidence of the penalty by further decomposing the sample by age of children and by mothers' race and education. We find that younger children impose a higher penalty than older children and that black and white mothers face the same penalty, patterns that are consistent with a work effort explanation. But the largest differences in the penalty arise among education groups. Although more educated women are likely to have jobs in which effort is relatively important, we find that college-educated mothers do not, in fact, face any penalty for having children. And while high school dropouts face a 3% penalty if they work when their children are infants and toddlers, they do not bear any penalty for older children. Thus high school dropouts who delay their return to the work force until their children are older bear no motherhood wage penalty at all. By contrast, high school graduates—especially those who return to work when their children are older—face persistent penalties of 4–6% up until their children enter high school.

The results for wage gap differentials by education cast doubt on the work effort hypothesis as a complete explanation for the wage penalty. It is unlikely that high school graduates reduce work effort in response to childcare duties at home when mothers with less and more education do not. Instead, we suggest that time, and in particular time during the middle of the day, poses a binding constraint that may

contribute to the motherhood penalty. High school graduates are the most likely to have jobs that require their presence during regular office hours and the least likely to gain flexibility either by finding work at other hours or by taking work home in the evening. The work-schedule flexibility model provides a compelling explanation for observed education patterns in the motherhood wage penalty.

Appendix A

Regression Results for Non-Child Variables in Pooled Cross-Section and Fixed Effects Models Reported in Table 2[a]

	Pooled Cross-Section						*Fixed Effects*	*Fixed Effects by Age of Child at Return to Work Force*		
								0–2 Years	*3–5 Years*	*6–17 Years*
Independent Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
Black	−.107***	−.035***	−.029***	−.039***	−.046***	−.008	—	—	—	—
	(.014)	(.011)	(.011)	(.011)	(.011)	(.010)				
Married, Spouse Present	.044***	−.004	−.005	−.010	−.006	−.076***	−.028***	−.028***	.003	−.047
	(.011)	(.009)	(.009)	(.009)	(.009)	(.012)	(.007)	(.008)	(.024)	(.031)
Education		.088***	.081***	.086***	.085***	.055***	.025***	.030***	−.001	.065***
		(.002)	(.002)	(.003)	(.003)	(.003)	(.004)	(.005)	(.013)	(.017)
Experience		.039***	.045***	.045***	.042***	.041***	.059***	.053***	.057***	.060***
		(.003)	(.003)	(.004)	(.004)	(.004)	(.003)	(.004)	(.009)	(.011)
Experience^2		−.001***	−.001***	.0001	.0001	.0001	−.0004***	−.0003**	−.00001	−.001***
		(.0001)	(.0002)	(.0002)	(.0002)	(.0002)	(.0001)	(.0001)	(.0003)	(.0004)
Age				.052***	.051***	.037***	.029***	.036***	.022	−.014
				(.008)	(.008)	(.008)	(.004)	(.006)	(.014)	(.016)
Age^2				−.001***	−.001***	−.001***	−.001***	−.001***	−.001***	−.0001
				(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0002)	(.0003)
Part-Time					−.083***	−.024**	−.014**	−.025***	−.005	−.045*
					(.012)	(.012)	(.006)	(.007)	(.018)	(.023)
Professional						.161***	.111***	.100***	.173***	.133***
						(.013)	(.009)	(.011)	(.033)	(.038)
Managerial						.146***	.100***	.074***	.099***	.109***
						(.019)	(.010)	(.013)	(.033)	(.039)
Sales						−.118***	−.051***	−.063***	−.031	−.066
						(.020)	(.011)	(.014)	(.035)	(.046)
Craft						.040	.101***	.088***	.089*	.050
						(.030)	(.018)	(.023)	(.054)	(.070)
Operatives						−.042***	.094***	.091***	.084***	.066
						(.012)	(.009)	(.011)	(.031)	(.043)
Service						−.285***	−.166***	−.161***	−.189***	−.061
						(.015)	(.009)	(.010)	(.029)	(.037)
Other Occupations						−.104***	.073***	.040	.076	.172**
						(.034)	(.023)	(.028)	(.067)	(.086)
# Children Age 18 or Older in HH						.001	.003	.002	.028	−.004
						(.013)	(.007)	(.008)	(.018)	(.020)
# Other Adults in HH						−.023***	−.007**	−.008**	−.003	−.018
						(.005)	(.003)	(.003)	(.010)	(.015)
Husband's Income						.002***	.0004***	.001***	−.0012**	.002***
						(.0003)	(.0002)	(.0002)	(.0005)	(.001)
Own Non-Labor Income						.005***	.001	−.001	.003	.004
						(.002)	(.001)	(.001)	(.003)	(.003)
Constant	2.236***	0.960***	0.993***	0.364***	0.392***	1.042***	1.417***	1.259***	1.810***	1.511***
	(.010)	(.033)	(.033)	(.103)	(.103)	(.107)	(.073)	(.091)	(.232)	(.298)

[a]The omitted occupation is clerical. Columns (1) and (2) use potential experience; all other columns use actual experience. See Table 2 for other details.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

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