CONSEQUENCES OF FAMILY EVENTS:
THREE PAPERS ON FAMILY CHANGE AND SUBSEQUENT OUTCOMES

by

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CHAPTER 1

Introduction

My dissertation follows a three-paper model in which I examine individuals'

experiences of events within the family and labor market. I consider the impact of each of

the following events in turn: job loss, the birth of a second child, and parental divorce.

These events have implications for socioeconomic inequality and gender inequity across

the life course. In the first paper (Chapter 2) I look at households to study the relationship

between job loss and changes in the division of housework. In the second paper (Chapter

3) I use a new approach to study the motherhood penalty: I examine the relationship

between birth spacing and mothers' long-term labor market outcomes. In the third paper

(Chapter 4) I estimate the association between parental separation and children's test

scores, as mediated by behavioral problems. Each of these papers provides a window into

the role the family or labor market plays in individuals' everyday lives.

My dissertation focuses in part on the intersection of the family and the labor

market. Arguably, there has always been tension between family life and participation in

the labor market for both sexes, but this tension has increased in salience over the past 40

years as female labor force participation has risen sharply. No longer specializing in

unpaid labor within the household, many women have accumulated dual

responsibilities—to the household and to the workplace—that have changed the nature of

both contexts. Women saw their wages rise more substantially than men's wages,

changing the nature of the dual-earner couple. At the same time as opportunities for

women in the labor market increased, women also became more likely to pursue higher education. In fact, women overtook men in college-completion rates in the 1990s, a shift that can be expected to have long-term implications for household dynamics and for women's opportunities in paid labor. These changes inform my work on household labor and wage penalties in Chapters 2 and 3.

Chapter 2 examines the relationship between household labor and time in the labor market. Specifically I consider how both the division of housework within a family and the total amount of housework completed change when one partner becomes unemployed. I find that when one member of a couple becomes unemployed, the division of labor between partners is altered. There are gender differences in response to unemployment with regard to household labor: while both men and women experience increases in housework time while unemployed, the unemployment of the female partner is associated with twice the increase in housework hours as for unemployed male partners. Furthermore, I am the first to show that the total amount of housework completed within the household also significantly rises in response to unemployment. This paper highlights the importance of considering both relative and absolute time in studies of housework and other forms of labor as they relate to the family. In particular, if one examines only relative time in labor, one may overlook increases or decreases in the total amount of work performed. Additionally, my results have implications for theories of compensatory gender display and deviance neutralization. These theories posit that wives increase their time in housework, and their husbands decrease their time in housework, when they earn more than their husbands. This is presumed to be a way to make up for gender-atypical labor market behavior. I demonstrate that in the most

extreme case of a female breadwinner household—one in which the male partner is unemployed—these male partners do not reject housework as a way to protect their masculinity and these female breadwinners do not increase their time in housework. The paper uses data from the Panel Study of Income Dynamics (PSID).

In Chapter 3, I shift my focus away from the effect of labor market changes on the household to consider the effect of family events on labor market outcomes. Specifically, I examine how birth spacing, along with birth timing, affects mothers' long-term labor market outcomes. A long line of research finds an association between entry into motherhood and lost labor force time and lower wages. This phenomenon facilitates earnings inequality within couples and can have detrimental effects on mothers' financial well-being in the event of divorce. I found it problematic that many researchers studying the motherhood penalty took into account age at first birth (since postponement of a first birth seems to reduce the costs of motherhood), but very few accounted for birth spacing. This is surprising, since the time between subsequent births is a key element in studying fertility and the majority of mothers in the United States have more than one child. Thus, in this paper I consider whether the birth spacing interval, along with birth timing, has an impact on mother's midlife financial outcomes. I employ a potential outcomes framework and inverse probability weighting to estimate the average treatment effect on the treated. I find only minimal evidence of a wage or earnings penalty at any of the time points representing different birth interval lengths. However, I do find a few statistically significant differences by education and age at first birth, which suggest that the average results may be obscuring some heterogeneity in effects. This analysis uses data from the National Longitudinal Survey of Youth 1979 (NLSY79).

In Chapter 4, I consider the context of the family separately from the labor market context. I hypothesize that behavioral changes act as a mediating variable between parental separation and children's test scores. A large body of literature has indicated that divorce has a negative impact on children's cognitive and behavioral outcomes. Researchers have proposed and tested a variety of causal mechanisms to explain these relationships, but in general they have found these play a role but do not explain the entire association between divorce and outcomes. Could divorce directly affect intelligence? Given the relative stability of intelligence over the life course this seems fairly unlikely. Perhaps there is a direct effect of divorce on behavior, and such behavioral changes explain the impact of divorce on cognitive outcomes? This seems more likely, since the family environment around the divorce period is known to be stressful. I postulate that any direct effect of divorce on children's cognition is small but that divorce has substantial effects on children's psychological and emotional states. These psychological and emotional effects can then manifest in a variety of behavioral problems, which in turn may result in lower test scores and lower general academic performance, even though the child's intelligence remains unchanged. These negative consequences may persist throughout a child's educational career.

To test this hypothesis, I first estimate the reduced-form effect of parental separation on math and reading test scores. I then add blocks of covariates to determine whether the association between separation and test scores is explained by demographic characteristics, family characteristics, lagged behavior measures, and finally lagged test scores. I find little evidence for an association between parental separation and test scores after controlling for demographic characteristics. The results are more consistent with a

selection story than a resource- or stress-based argument. I use two data sets in this chapter—the Early Childhood Longitudinal Study (ECLS)-Kindergarten cohort and the ECLS-Birth cohort—because previous research has suggested divorce may affect young children and school-age children differently, perhaps because of differences in psychological maturity.

These three papers are all tied together by their focus on the mutual interactions between events within families and other life outcomes, particularly interactions between family and labor, and family and education. These papers are based in the idea that it is important to understand the connections between family events and later outcomes in order to be able to draw conclusions that are relevant to a broad audience and that also may be applicable to policy interventions.

CHAPTER 2

Unemployment in Families: The Case of Housework[1]

2.1 Introduction

Paid labor is an integral part of many Americans' lives, and unemployment can have a

devastating effect on individuals' financial stability, career trajectories, and mental and

physical health (Burgard, Brand, & House, 2007; Jacobson, LaLonde, & Sullivan, 1993).

Unemployment—the state of being not currently employed, but seeking employment—is

not limited to isolated individuals but also affects the well-being and daily routines of

families. For example, job loss is associated with increased probability of divorce

(Charles & Stephens, 2004). For those who remain married, the experience of

unemployment may alter specialization decisions within the household. The question of

whether spouses (mostly wives) of individuals who become unemployed increase their

labor supply (the "added-worker effect") has been extensively studied and debated

(Heckman & MaCurdy, 1980; Lundberg, 1985; Maloney, 1987). Stephens (2002) found

large increases in wives' labor supply following husbands' job losses, which in the long

term compensated for one fourth of their husbands' lost income. But the literature on

changes in wives' labor supply in response to their husbands' unemployment has

typically ignored changes in spouses' time in nonmarket productive activities following a

job loss. Just as changes in one spouse's employment hours may affect the employment

hours of the other spouse, so too may job loss motivate the unemployed spouse to increase household labor, whereas the other spouse may decrease time in household labor, particularly if she is employed.

The effects of unemployment on household labor may not be limited to reallocation of tasks from one spouse to another. In addition to changing who does housework, unemployment may change the total amount of housework the couple does. For example, unemployment is expected to reduce the household's financial resources, which may reduce the outsourcing of housework and increase the household's total housework burden. In this paper we ask whether unemployment leads to both a rise in the couple's total time in housework and a shift of housework to the unemployed spouse.

Furthermore, we hypothesize that responses to unemployment will vary according to the gender of the unemployed spouse. Time spent in housework is a normative way in which women "do gender," whereas men do gender through housework avoidance (West & Zimmerman, 1987). Thus, we expect that while unemployed a wife may pour additional time into housework, whereas an unemployed husband may be reluctant to take on additional housework if he believes that housework is the realm and responsibility of his wife. This belief may be intensified during periods of unemployment, if his inability to fulfill the gendered, socially normative role of breadwinner makes further deviation from gender norms through participation in female-typed tasks especially distasteful (Brines, 1994). In this way, we hypothesize an interactive effect between gender and time availability, with gender affecting the "starting points" of employed men's and women's housework time and the degree to which they reallocate effort to domestic labor in the face of a sudden reduction in paid labor time.

We use data from the Panel Study of Income Dynamics (PSID, 2009) and estimate the effect of shifts into unemployment on both the reallocation of housework hours between spouses and the amount of total household production. The PSID is a uniquely appropriate dataset for answering this question because it has asked household respondents to report the housework hours of both the head of the household (by default the husband for married couples) and his wife for more than two decades. Thus, for many members of the PSID sample, we have repeated annual measures of both spouses' housework time and can estimate the changes in a couple's housework time that occur when either spouse experiences unemployment.

2.1.1 Literature Review

The changes in spouses' housework time that we expect during periods of unemployment depend on our prior assumptions about how spouses make decisions about household labor. Job loss provides husbands the potential to reduce their working wives' "second shift" (Hochschild, 1989) by assuming more housework and child care responsibilities at home. Alternatively, husbands may find increased time in homemaking socially uncomfortable and undesirable, due to gendered norms of behavior that define housework as women's work. In the former situation, spouses may be able to smoothly reallocate tasks in the event of a husband's job loss, but in the latter case husbands' unemployment may increase the burden on wives to participate in paid labor, without offering any relief from household labor. These competing responses illustrate two of the literature's main theoretical perspectives on housework division: time availability theory and gender-based perspectives of household labor.

Time availability theory suggests that couples rationally allocate time in housework on the basis of spouses' relative hours in the paid labor market and the amount of housework to be done (Bianchi, Milkie, Sayer, & Robinson, 2000; England & Farkas, 1986). Therefore, the spouse with less time spent in paid labor is expected to take on the greater responsibility for housework. Past research into time availability has run into a temporal ordering problem. By assuming that couples allocate time in housework based on their relative labor market hours, time availability theory implicitly assumes that couples first decide how to allocate each spouse's time in the paid labor market and then decide how to allocate each spouse's time in household labor based on the number of hours each spends in paid labor. Thus, it has been common in empirical work to test time availability or to account for time availability by including measures of spouses' market work time as an independent variable in models of housework hours (Bianchi et al., 2000; Bittman, England, Sayer, Folbre, & Matheson, 2003; Brines, 1994; Evertsson & Nermo, 2004; Greenstein, 2000). If housework and labor force hours are jointly determined, with gendered expectations about the allocation of housework influencing spouses' time in paid labor, modeling time spent in housework in a way that takes the labor force decision as given may understate the effect of gender and overstate the effect of time availability. Because of the potential for joint determinacy, time in the labor force is endogenous to time in housework.

The involuntary and sudden nature of job loss allows us to address these shortcomings. In our study, we can exploit the fact that job loss is an exogenous shock to labor force hours and examine how couples respond to this shock. The involuntary nature of job loss provides us with a rare situation in which it is appropriate to view labor

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market outcomes as determined prior to, rather than jointly with, decisions about time in household labor.

Along with altering the relative time availability of spouses, job loss changes their relative earnings. The theory of relative resources suggests that spouses allocate housework based on the relative resources (monetary or otherwise) each contributes to the family. The spouse contributing the larger share of resources to the family is expected to spend less time in housework than the spouse contributing the smaller share, as the advantaged spouse is expected to view housework as undesirable and to use resources to bargain out of housework time (Bittman et al., 2003; Brines, 1994; Evertsson & Nermo, 2004). Thus, the relative resources theory of housework division implies that job loss will result in a shift of some additional household labor to the unemployed spouse, above and beyond the effect of time availability, because of this spouse's loss of income.

The loss of income associated with unemployment may also affect housework time in absolute terms. Given that wives' earnings are negatively associated with their own time in housework, and that these reductions do not appear to be offset by increases in husbands' housework time (Gupta, 2007), we expect that a wife's unemployment, in particular, will lead to increased housework hours for her due to the reduction in income. This earnings–housework association may arise because wives' earnings are used to outsource housework. The positive association between expenditures on market substitutes for wives' own time in housework and both total family income and wives' earnings (Cohen, 1998; Oropesa, 1993) is consistent with this hypothesis and suggests that the loss of income from unemployment may lead couples to use their own labor to produce household goods that they previously purchased.

Time availability and relative resources have been criticized for ignoring the potential role of gender in the allocation of housework tasks within families. The "doing gender" perspective in the housework literature suggests that housework is a space for the symbolic enactment of gendered behavior (Berk, 1985; West & Zimmerman, 1987). Cultural norms about which spouse should dedicate time to the labor force and which should dedicate time to domestic labor (husband and wife, respectively) also influence individuals' time in housework. These norms may act as disincentives for couples to adopt nontraditional divisions of household labor, as couples may fear being stigmatized or experience psychological unease (Atkinson & Boles, 1984; Brines, 1994; Hochschild, 1989; Tichenor, 2005).

For wives, unemployment leads to changes in time availability that are compatible with gendered norms of behavior, whereas for husbands the predictions of time availability and relative resources are at odds with norms of behavior for men. Thus, we expect that gender will condition and constrain the extent to which individuals adjust their time in household labor in response to unemployment, with greater increases in housework time for unemployed wives than unemployed husbands.

With the loss of the financial provider role for unemployed husbands, it is even possible that husbands will resist housework even more strongly than when they were employed, leading to declines in husbands' housework time when they are unemployed. This is consistent with the theory of gender display (Brines, 1994) and its similar counterpart, gender deviance neutralization (e.g., Greenstein, 2000), which suggests that when the wife is the primary breadwinner husbands will do less housework, and wives more, than if the spouses had earnings parity, as spouses compensate for nontraditional

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labor force outcomes with a traditional division of labor at home. Existing evidence on gender display is mixed (Bittman et al., 2003; Brines, 1994; Evertsson & Nermo, 2004; Greenstein, 2000; Gupta, 2007; Killewald & Gough, 2010), with critics having argued that it is wives' own earnings that determine their time in housework, rather than wives' earnings relative to those of their husbands (Gupta, 2007; Killewald & Gough). Unemployment provides a special case in which to examine couples' responses to a female breadwinner and, as a result, our study provides a test of whether the predictions of gender display hold in the context of unemployment. Brines (1994) is one of the few researchers to examine this case with unemployed husbands; she found that recently unemployed husbands spent more time in housework than fully employed husbands, but long-term unemployed husbands spent no more, and perhaps less, time in housework than fully employed husbands. She suggested that these long-term unemployed husbands resisted housework in response to their dependency on their wives.

Although Brines (1994) found resistance to housework among unemployed husbands, other researchers have not found these differences. Burda and Hamermesh (2010), using cross-sectional data from the American Time Use Survey, estimated that unemployed men and women spent about 10.5 more hours per week in household production than employed men and women. A weakness of this study, however, is that if the unemployed are different from others in ways that are correlated with housework time but not observed in the data, such as in their gender ideology or health, these results may be biased.

Two studies have used short longitudinal data sets to examine the relationship between unemployment and housework. Shamir (1986) studied a small sample of Israelis

who had registered as unemployed with the Employment Service of the Israeli Ministry of Labor and Welfare and their spouses. He found that both men and women who became unemployed increased their share of household tasks modestly during the period of unemployment and then reduced their share of household tasks following re-employment (Shamir). More recently, Ström (2002) studied Swedish couples using the Swedish Longitudinal Study among the Unemployed and the Swedish Level of Living survey. She found that men who were unemployed at the first wave had higher housework hours at the second wave than continuously employed men, even if they had been re-employed by the second wave (Ström). Women who were unemployed at the first wave spent more time in housework than continuously employed women at the second wave, but there were no significant differences if the women had been re-employed by the second wave (Ström).

2.1.2 The Present Study

Our work contributes to the literature in four ways. First, we employ fixed effects models, which allow us to examine how the housework hours of both spouses differ during periods of unemployment from those during periods of employment. These models net out time-invariant differences, so results are unbiased even in the case of unobserved time-invariant characteristics of individuals that are correlated with both their likelihood of unemployment and their housework time. Previous panel studies of the relationship between unemployment and housework time have been limited by small sample sizes and short durations of the panels.

Second, we note that the existing panel studies of couples' housework responses to unemployment come from countries other than the United States. Given evidence of significant cross-national variation in the division of household labor (Cooke, 2010; Gupta, Evertsson, Grunow, Nermo, & Sayer, 2010; Sayer, 2010; van der Lippe, 2010), rates of female labor force participation and hours worked by women (van der Lippe), rates of unemployment, and unemployment benefits (Baker, Glyn, Howell, & Schmitt, 2005), it is unclear whether the results would translate directly to the United States. Thus, we contribute to the literature by estimating the relationship between unemployment and housework hours specifically within the United States.

Our third contribution is our most significant. Unlike most past research, we specifically consider the family context in which both unemployment and housework occur. In doing so, we look at the housework time of both the individual who becomes unemployed and his spouse. In this way, we are able to test whether changes in housework time result primarily from a reallocation of housework between spouses or from a change in the total amount of housework the couple performs. Unemployment reduces the opportunity cost of housework, as time spent in housework does not come at the cost of foregone time in paid labor. Additionally, unemployment lowers household income, reducing the family's ability to outsource household labor. Thus, we expect that the total time spent in household labor will rise for households with an unemployed spouse. Furthermore, the unemployment of one spouse alters the relative labor force commitments of spouses, as well as their relative bargaining positions, both of which should tend to shift household production to the unemployed individual and away from his spouse. We expect to see both reallocation of housework time toward the unemployed

spouse, consistent with the time availability and resource-based theories, and increases in total household production. Our study is unique in considering changes in total household production as well as substitution of one spouse's time for another.

Although we expect to see both reallocation of housework and increases in household production, we acknowledge that couples' accustomed division of labor may have "stickiness": Couples may change their division of housework less than would be expected on the basis of purely economic motives. Either because of gender norms, because spouses have developed routines and patterns of housework that require effort to renegotiate, or because spouses have developed task-specific skills that make reallocation of housework difficult, we expect changes in spouses' household labor time during periods of unemployment to be moderate in size.

Furthermore, again acknowledging the importance of examining unemployment and housework in the context of total family decisions, we expect that changes in spouses' housework hours will vary according to the family employment context in which they occur, a possibility ignored in previous research. Given that much of the research on housework has focused on the role of spouses' *relative* endowments of time and resources in shaping the allocation of household labor, this neglect is puzzling. We expect that when the spouse of an unemployed individual is heavily committed to the labor force, couples will have a greater incentive to shift housework to the unemployed spouse in order to reduce the disparity between spouses in total work hours (market labor plus household labor). In other words, the response of a partnered individual to his job loss will not be purely individual, but will depend on what makes sense in the context of the couple's joint decisions about employment and housework. Specifically, we examine

how husbands' responses to unemployment in housework time vary by the employment status of their wives.

Finally, we contribute to the literature by testing the extent to which gender conditions time availability, either by further boosting the increase in housework time for unemployed wives or by attenuating the increase expected because of time availability for unemployed husbands. In particular, we can examine whether there is evidence for compensatory gender display in the face of unemployment as Brines (1994) found using cross-sectional models.

2.2 Method

We use data from the 1979–2007 waves of the PSID. The PSID is a longitudinal study conducted by the University of Michigan that began in 1968 with a sample of 4,800 American households. It has since reinterviewed members of those original households and their descendants annually or biannually (beginning in 1997). Our period of study begins in 1979 because it was the first year that the PSID collected reports of both spouses' employment statuses. The panel nature of the PSID makes it an ideal data set for evaluating how couples change their time spent in household labor in response to changes in their labor force participation and rewards.

We restrict the sample to married and long-term (1 or more years) cohabiting couples, with both partners present in the household, censoring them once one partner is over the age of 60 so that we avoid including couples who are approaching the normative age of retirement. We exclude couples who have been cohabiting for shorter periods of time because in these cases the PSID does not collect housework hours for both partners.

For simplicity, we refer to all partners as "spouses," "husbands," and "wives," even though some are not married. Responses to survey questions are provided by the household member who is better able to answer the array of questions on financial and other matters in the study (Achen & Stafford, 2005). The percentage of respondents who are wives increases over the period of study to about 50%, as shown in Table 2.1.

We estimate match-specific individual-level fixed effects models, which allow us to control for time-invariant unobserved heterogeneity in housework hours that may be correlated with unemployment status. By *match-specific* we mean that fixed effects for the individual are fixed only within the context of one specific couple, and we treat subsequent marriages as separate observations. As a result, our estimates of the relationship between unemployment and housework hours are identified based on the difference between average housework hours during spells of unemployment and average housework hours during periods of employment, net of other controls, for those who are employed in at least one year and unemployed in at least one year. Under the fixed effects framework, we assume that individuals' (*i*) housework hours (*hswk*) across time (*t*) can be modeled as a function of time-varying predictors (*X*), individual-level match-specific fixed effects (α), and time-varying individual-level variation (ε), as follows:

$$hswk_{it} = X'_{it}\beta + \alpha_i + \varepsilon_{it}$$

Dependent Variables. We use two dependent variables: the weekly housework hours of the husband and those of the wife. The question reads, "About how much time do you spend on housework in an average week? I mean time spent cooking, cleaning, and doing other work around the house." For the question about the spouse, "do you" is replaced with "does she" or "does he." Time in direct child care is not explicitly included

in this measure, although it is possible that some respondents included time in child care when giving their answers. We recode values above the 99th percentile to take the value of the 99th percentile to guard against outliers that would unduly influence the results. Absolute time spent in housework is frequently used as the dependent variable in studies of household labor, including for married couples (Baxter, Hewitt, & Haynes, 2008; Bianchi et al., 2000; Bittman et al., 2003; Brines, 1994; Gupta, 1999, 2007). Alternatively, for those primarily interested in questions of the allocation of housework time between partners, the share of total housework done by each spouse may be more appropriate (Coltrane & Ishii-Kuntz, 1992; Gershuny, Bittman, & Brice, 2005; Greenstein, 2000; Shamir, 1986). In our work, it is most appropriate to use absolute housework time, as we wish to highlight the effects of unemployment on the total amount of time the couple spends in housework, as well as the allocation between spouses. We present results of models of the share of housework done by each spouse in the "Alternative Specifications" section.

Stylized questions about the number of hours spent in housework, such as the question employed in the PSID, consistently yield higher estimates of time in housework than time diary measures (Juster, Ono, & Stafford, 2003). Yet, such single summary measures may be less susceptible to double counting than measures asking about time spent in specific tasks, given that many respondents multitask (Geist, 2010). Will measurement error bias our results? First, we note that any consistent upward bias in housework time by a given couple—even if the amount of bias varies across couples— will be absorbed by the fixed effects. Thus, if all respondents overestimate, and some respondents overestimate more than others, the coefficients will remain unaffected,

provided that the extent of overestimation is constant for any given couple. Even if there is variation across years in the extent of overestimation by a couple, if the measurement error is classical the coefficients will remain unbiased. Thus, the coefficients will only be biased if the measurement error in the reports of housework time is correlated with the time-varying component of employment status. For example, if individuals truly experience no change in housework time when they become unemployed, but those who are unemployed want to appear productive during their unemployment spell, they may overreport housework time more than they did when employed. In this case, our coefficients would be biased upward.

Independent Variables. In our first model, we estimate the average change in spouses' housework hours associated with transitions to unemployment for either spouse. Two dummy variables for employment are created. The first is set to 1 if the husband reports being unemployed at the time of the survey, and the second is set to 1 if the wife reports being unemployed at the time of the survey. Thus, both husbands' and wives' housework hours are allowed to vary with changes in the employment status of either spouse. In this model, we aggregate together the experiences of all unemployed persons of the same gender, assuming that the relationship between unemployment and housework does not vary with the employment status of the unemployed individual's spouse.

Our second model relaxes this assumption. In this model, the dummy variable for the husband's unemployment is interacted with the employment status of his wife in the year prior to the start of the current unemployment spell. Wives may be housewives, part-time workers (less than 35 hours per week but more than zero), or full-time workers (at

least 35 hours per week) in the year prior to the husband's job loss. We retain controls for other groups, such as unemployed wives, but do not allow separate interactions of these groups with the unemployment status of the husband. We do not interact the variable for the wife's unemployment with the employment status of the husband in the year prior to the beginning of her unemployment spell because the sample of husbands working less than full time is small and highly selective.

Because our primary interest is in comparing housework time during periods of unemployment to periods of employment, we construct a series of dummy variables for whether either spouse is a student or out of the labor force, where the latter category includes those who report keeping house and those who report that they are retired at the time of the survey. Employment is the omitted category. We are particularly concerned with estimating the effect of being involuntarily unemployed on spouses' housework time, as it is in this case that nonemployment can be viewed as an exogenous shock to individuals' time in paid labor.

Control Variables. In both models, we control for the same set of time-varying covariates that may be correlated with both unemployment and the amount of housework each spouse performs, but may be plausibly treated as exogenous to unemployment. Because the presence of children in the household is associated with increases in housework time, particularly for women (Baxter et al., 2008; Bianchi et al., 2000; Sanchez & Thomson, 1997), we control for the presence of at least one, at least two, and at least three children in the household, using individual indicator variables, as well as the age of the youngest child. We also include a linear measure of the year, to account for secular trends in spouses' housework time. We control for the state-level annual

unemployment rate for the respondent's state of residence, as locally poor labor market conditions may affect spouses' decisions about household labor (Burda & Hamermesh, 2010). We control for whether the family owns the home in which they reside, as homeowners may have a larger amount of housework to be completed than non-homeowners and may also have greater incentives to maintain their homes. Finally, we include a dummy variable to indicate whether the husband or wife provided the PSID interview, as previous research has shown that in the PSID married men report about 3 hours per week more time in core housework tasks than their wives report for them, although husbands and wives report similar levels of housework time for wives (Achen & Stafford, 2005).

We do not control for time-varying covariates that are potentially endogenous with unemployment, such as family income or mental health. As such, our results estimate the total average change in housework time resulting from unemployment, rather than the residual change that remains after controlling for possible causal pathways that link unemployment to housework. For example, households are likely to have a lower family income when one spouse is unemployed than when both spouses are employed. Lower family income is expected to reduce the extent to which the household outsources domestic labor (through dining out or hiring a domestic worker). Reduced outsourcing is then expected to be associated with increased housework time for the couple. Controlling for family income in the regression will effectively net out any of the association between unemployment and housework that operates through reduced household income. Thus, it does not estimate the average real change in housework time experienced by couples with an unemployed spouse.

Although we include time-invariant variables such as race and education in the descriptive statistics, we do not include these variables in the fixed effects models, as time-invariant variables do not contribute to the fixed effects estimation, which capitalizes on within-couple changes over time. In order to examine variation by these characteristics, we conduct supplemental analyses based on the main models, as described in the "Alternative Specifications" section.

We weight the data according to the year-specific PSID household-level weights, which have been rescaled so that they average one in each year. Our sample includes both the Latino sample (1990–1995) and the immigrant sample (1997–2007) in the years in which they were included in the PSID sample, and the weights reflect this. For fixed effects models, it is necessary to assign each couple a single, time-invariant weight. Arbitrarily, we assign the couple their household sample weight from the first year in which they appear in the sample, although results using the sample weight from the last year in which the couple appears are similar.

We drop observations with missing values on the dependent variable (1,342 observations; 1.5%), observations for which the respondent's employment status is unknown (325 observations, 0.4%), and observations in which the respondent resides outside of the 50 states or the state of residence is unknown and, therefore, their state-level unemployment rate cannot be assigned (2,261 observations; 2.6%). We require information about the employment hours of the unemployed individual's spouse in the year prior to unemployment, so we drop observations in which respondents are unemployed during the first year in which they are observed (5,834 observations deleted; 6.7%). Finally, we drop all observations for which the current year-specific weight (986

observations; 1.1%) or the longitudinal weight (1,141 observations; 1.3%) is equal to zero. Observations with zero weight pertain to households that were nonrespondents in the current wave of data collection but had valid responses in other years or were recontacted after a period of nonresponse (Gouskova, Heeringa, McGonagle, Schoeni, & Stafford, 2008). This leaves us with 74,881 observations on 10,390 couples, who were observed 7.2 times each on average.

2.3 Results

The data in our sample span the period 1979–2007. Descriptive statistics for three periods are shown in Table 2.1: 1979–1985; 1986–1992; 1993–2007. The average age of husbands in the early period was 40.0 years, and increased to 41.8 in the late period. The corresponding ages for wives were 37.5 years in the early period and 39.8 in the late period. Median annual earnings for husbands (in 2009 dollars) declined across the period from $50,190 in the early period to $47,265 in the late period, consistent with stagnating men's wages during this period (Morris & Western, 1999). Conversely, median annual earnings for wives more than doubled from $10,300 in the early period to $21,750 in the late period, reflecting in part increases in married women's employment, as evidenced by the increase in wives' hours in paid labor across the period, from an average of 19.4 hours per week in the early period to 26.1 hours per week in the late period.

About 7% of families in the sample included a Black husband. Because the PSID asked only about the race of the head of the household prior to 1985 (and, for married couples, the head of the household is the husband by default), we used only the husband's racial identification. The percentage of families in the sample that included a Latino

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husband rose from 3% in the early period to 6% in the late period. A similar rise was seen for the percentage of families with a husband who was an immigrant—from 3% to 7% across the periods. The percentage of respondents holding college degrees increased over the period, from 27% of husbands in the early period to 32% of husbands in the late period, and from 19% to 27% for wives. About 80% of respondents owned the home in which they resided, with the majority of the remaining respondents living in rental dwellings. The percentage of respondents who are wives rose over the period from 20% in the early period to 50% in the late period, whereas the percent who are husbands declined over the period from 80% in the early period to 49% in the late period.

The trends in wives' average time in housework observed in this sample followed trends documented elsewhere (Bianchi et al., 2000; Gershuny & Robinson, 1988), declining from 27.0 hours per week in the early period to 19.1 in the late period. Yet, we found far less change in husbands' housework hours across the period, which averaged between 6.4 and 7.2 hours per week in each period. Others have documented a rise in men's housework time (Bianchi et al., 2000; Gershuny & Robinson, 1988; Juster et al., 2003), although they found a leveling out after the mid-1980s. The small increase we did see over the period of study is consistent with the results from Juster and colleagues for the same time period and using the same data set. Averaged across all years in the sample, for husbands the distribution of the housework hours variable is such that the 25th percentile was 2 hours per week and the 75th percentile was 10 hours per week. The corresponding values for wives were 10 hours per week at the 25th percentile and 30 hours per week at the 75th percentile. Thus, three quarters of wives did as much or more housework than the three quarters of husbands who did the least housework.

Table 2.1

Descriptive Statistics

	1979–1985		1986–1992		1993–2007		Range
	Mean	*SD*	Mean	*SD*	Mean	*SD*	
Median husband earnings	$50,190	$47,221	$48,569	$62,832	$47,265	$82,928	$0–$339,200
Median wife earnings	$10,300	$18,111	$15,617	$22,441	$21,750	$30,702	$0–$96,000
Wife weekly work hours	19.35	16.85	23.67	17.00	26.06	17.11	0–60
Husband housework hours	6.40	6.82	7.16	6.94	7.01	6.72	0–35
Wife housework hours	26.95	16.01	22.48	14.38	19.13	13.15	0–80
Husband age	40.00	10.90	40.54	10.13	41.83	9.87	16–60
Wife age	37.53	10.58	38.21	9.83	39.81	9.70	15–60
Wife is respondent	0.20		0.35		0.50		0–1
Husband is respondent	0.80		0.65		0.49		0–1
Black[a]	0.07		0.07		0.07		0–1
Latino[b]	0.03		0.05		0.06		0–1
Immigrant[c]	0.03		0.03		0.07		0–1
Husband college degree[d]	0.27		0.27		0.32		0–1
Wife college degree[e]	0.19		0.21		0.27		0–1
Own home	0.79		0.77		0.79		0–1
Rent home	0.19		0.20		0.19		0–1
Husband unemployed[f]	0.02		0.02		0.02		0–1
Wife unemployed[g]	0.01		0.02		0.02		0–1
Husband total losses	1.25	0.59	1.47	0.93	1.49	1.08	1–12
Wife total losses	1.09	0.31	1.19	0.46	1.24	1.08	1–5
Husband keeping house[h]	0.02		0.03		0.03		0–1
Wife keeping house[i]	0.37		0.26		0.22		0–1
Husband is student	0.01		0.01		0.01		0–1
Wife is student	0.01		0.01		0.01		0–1

Note. All values are weighted. *N* = 10,390.
[a]Black: 0 = *non-Black husband*, 1 = *Black husband*. [b]Latino: 0 = *non-Latino husband*, 1 = *Latino husband*.
[c]Immigrant: 0 = *non-immigrant husband*, 1 = *immigrant husband*. [d]Husband college degree: 0 = *no college degree*, 1 = *college degree*. [e]Wife college degree: 0 = *no college degree*, 1 = *college degree*. [f]Husband unemployed: 0 = *not unemployed*, 1 = *unemployed*. [g]Wife unemployed: 0 = *not unemployed*, 1 = *unemployed*. [h]Husband keeping house: 0 = *not keeping house*, 1 = *keeping house*. [i]Wife keeping house: 0 = *not keeping house*, 1 = *keeping house*.

On average, 2% of husbands and 1%–2% of wives were unemployed at the time

of the survey over the 3 periods. Among those who experienced unemployment in at least

1 period, the average number of survey waves in which they were observed to be

unemployed was slightly higher for husbands than wives, ranging between 1.3 and 1.5 for

husbands and between 1.1 and 1.2 for wives. On average 2%–3% of husbands were

keeping house or retired at the time of the survey over the 3 periods. Consistent with

rising female labor force participation rates, the percentage of wives keeping house or

retired dropped from 37% in the early period to 22% in the late period. In all years,

approximately 1% of both husbands and wives were students at the time of the survey.

The results for the aggregate model are presented in Table 2.2. In the aggregate

model, husbands spent an average of 3.2 hours ($p < .001$) per week more in housework

when they were unemployed as opposed to employed, whereas they spent an average of

1.6 fewer hours ($p < .001$) per week in housework when their wife was unemployed than

when she was employed. In the aggregate model for wives' housework hours, wives

spent an average of 1.3 fewer hours ($p < .01$) per week in housework when their husband

was unemployed than when he was employed, but they spent an average of 6.4 hours

more per week ($p < .001$) in housework when they were unemployed than when they

were employed.

Thus, consistent with the predictions of the time availability and relative resources

perspectives, for both spouses unemployment increased housework time for the

unemployed spouse, compared to periods of employment and decreased housework for

the other spouse. Furthermore, there was evidence of gender asymmetry in the responses

of spouses to the experience of unemployment: Both husbands and wives increased their

housework time if they became unemployed, but the increase for wives was twice as large. The reduction in housework hours of 1 to 2 hours per week experienced by the spouse of the unemployed individual was similar for both husbands and wives. Regardless of which spouse became unemployed, the total time in housework by couples increased during unemployment, consistent with our expectation that unemployment will reduce the opportunity cost of housework and the ability to pay for housework substitutes.

Table 2.2

Summary of Fixed Effects Analysis for Variables Predicting Husband's and Wife's Housework Hours

Variable	Husbands		Wives	
	b	*SE*	*b*	*SE*
Husband's unemployment	3.19***	.25	-1.29**	.38
Wife's unemployment	-1.55***	.23	6.39***	.46
Husband keeping house	3.91***	.36	-2.42***	.45
Wife keeping house	-1.59***	.10	9.71***	.22
1+ children	1.36***	.13	6.05***	.25
2+ children	0.07	.10	1.96***	.19
3+ children	0.04	.15	2.17***	.29
Age youngest child	-0.10***	.01	-0.31***	.02
Respondent is husband	2.11***	.14	N/A	
Respondent is wife	N/A		-0.98***	.23
Year	0.04***	.01	-0.14***	.01
State unemployment rate	-0.02	.02	0.04	.04
Rent home	-0.83***	.12	-1.22***	.20
Constant	5.20***	.24	18.12***	.38
R^2 overall	.06		.26	
Variance explained by fixed effects	.50		.52	

Note. Year centered on 1978. Models also control for student status of husbands and wives. All values are weighted. $N = 10,390$.
*$p < .05$. **$p < .01$. ***$p < .001$.

Turning to the covariates in our fixed effects models of husbands' housework time, a first child was associated with an increase of 1.4 hours ($p < .001$) per week in housework, and with each year the youngest child grew older, there was a decline of about 0.1 hours (6 minutes; $p < .001$) per week in housework. Husbands' reported housework hours were an average of 2.1 hours ($p < .001$) per week higher in the periods in which they reported their own housework time, indicating that husbands reported significantly more housework time for themselves than did other members of the household. This was consistent with, though slightly smaller in magnitude, previous work on reporting of housework hours in the PSID (Achen & Stafford, 2005). Husbands' housework time increased yearly by about 0.04 hours (2 minutes; $p < .001$) per week. Thus, although husbands' housework hours have risen over time, the change was slight. Changes in the annual state-level unemployment rate were not significantly associated with changes in husbands' housework time. Finally, renters spent about 0.8 hours (48 minutes; $p < .001$) per week less in housework than homeowners.

In the fixed effects model of wives' housework time, it was clear that children had a substantial effect on wives' housework hours. A first child was associated with a 6.1-hour ($p < .001$) increase in weekly housework hours, whereas a second child was associated with an additional 2.0 hour ($p < .001$) increase, and a third or higher-order birth was associated with an additional 2.2 hour ($p < .001$) increase. Wives' housework time declined by about 0.3 hours per week (18 minutes; $p < .001$) with each year the youngest child aged. Although both spouses spent more time in housework when children were present in the household, the effects were substantially larger for wives than for husbands. If the wife was the respondent she reported about 1.0 hour less housework per

week than if another member of the household was the respondent. Interestingly, these results do not imply that each spouse over reported his or her own time in housework compared to the reports of other household members. Instead, it suggests that husbands reported more time in housework for both themselves and their wives than their wives reported. With each passing year, wives' housework time declined by 0.14 hours (8 minutes; $p < .001$) per week. Consistent with the results in Table 2.1, changes across time in wives' housework hours have been larger than changes in husbands' housework hours. As in the model of husbands' time, the state-level unemployment rate was not associated with wives' housework hours. Wives in families that rented their dwellings spent about 1.2 hours ($p < .001$) per week less in housework than those in families that owned their homes, also consistent with the results for husbands.

The results also indicated large differences between changes in housework time during unemployment and changes during periods of being voluntarily out of the labor force, particularly for wives. In the model of husbands' housework hours, his being voluntarily out of the labor force was associated with 3.9 more hours of housework per week ($p < .001$) than when he was employed and 2.4 fewer hours per week in his wife's housework time ($p < .001$). Recall, for comparison, that a husband's unemployment was associated with 3.2 hours per week more of his housework time and 1.3 fewer hours per week in his wife's housework time than during periods in which he was employed. Thus, the changes associated with being voluntarily absent from the labor force were slightly larger than for unemployment. This effect was magnified in the results for wives' time. When wives were voluntarily out of the labor force, their housework hours were an average of 9.7 ($p < .001$) hours more per week than in periods when they were employed,

compared to 6.4 hours more per week when they were unemployed. Given that neither group is working, what might explain this difference? First, wives who were voluntarily out of the labor force may have had more time available for housework than wives who were unemployed, if unemployed wives were spending some of their time seeking employment. Second, it is possible that some wives who voluntarily exited the labor force were motivated in part by a desire to increase household production (see e.g., Hochschild, 1989; Stone & Lovejoy, 2004). When wives were voluntarily out of the labor force, their husbands' housework hours were an average of 1.6 hours per week ($p < .001$) lower than during periods in which she was employed, similar to when she was unemployed.

2.3.1 Interactions by Wives' Employment Status

Table 2.3 shows the results for the models that interact the husband's job loss with the wife's employment status in the year prior to his transition to unemployment. Our findings are consistent with the results from the aggregate model but bring the family context of unemployment and housework to the forefront. Husbands who were unemployed had the largest increases in housework time, as compared to when they were employed, if their wives worked full time (3.9 hours; $p < .001$) and the smallest increases when their wives were homemakers (2.2 hours; $p < .001$). In terms of changes in husbands' housework hours, unemployed husbands whose wives worked part time fall in between, although we cannot reject the null hypothesis that the husband's increase was the same as if the wife worked full time ($F(1, 10389) = 1.87$; $p = .17$). In some sense, these results are surprising, as homemakers have, on average, the highest household labor

burden and therefore the greatest potential for reduction during periods of their husbands'

unemployment. But couples with a traditional division of labor may be least likely to

perceive increased household responsibility by the husband as an appropriate response to

his job loss. The results suggest that the division of household labor responded to the

relative market work commitments of each spouse: Unemployed husbands were more

likely to take over household tasks for their wives when the wife's time was highly

committed to the labor force.

Table 2.3
Selected Coefficients of Fixed Effects Analysis Predicting Housework Hours with Interactions for Wife Work Hours—Husbands, Wives

	Interactions by Wife's Work Hours			
	Husbands		Wives	
Variable	*b*	*SE*	*b*	*SE*
Husband's unemployment/wife homemaker	2.18***	0.49	-0.50	0.94
Husband's unemployment/wife PT	3.17***	0.37	-1.98***	0.55
Husband's unemployment/wife FT	3.91***	0.40	-1.03*	0.49
Wife's unemployment	-1.55***	0.23	6.37***	0.46
Husband keeping house	3.92***	0.36	-2.42***	0.45
Wife keeping house	-1.59***	0.10	9.71***	0.22
Constant	5.20***	0.24	18.12***	0.38
R^2 Overall	0.06		0.26	
Variance explained by fixed effects	0.50		0.52	

Note: Year centered on 1978. Models also control for student status of husbands and wives. All values are weighted.
N=10,390.
PT = engaged in part-time work. FT = engaged in full-time work.
*$p < .05$. **$p < .01$. *** $p < .001$.

Wives who worked part time saw a statistically significant 2.0 hour per week ($p <$

.001) reduction in housework hours during periods of husbands' unemployment, which

was larger in magnitude than the reduction for either homemakers (0.50 hours) or wives

working full time (1.0 hours; $p < .05$), although we cannot reject the null hypothesis that

it is equivalent to the reduction seen by wives working full time ($F(1, 10389) = 1.73$; $p =$

.19). It is possible that wives who worked part time decreased their housework time to a

greater extent because they expected to increase their time in market work, which may

not have been an option for wives already working full time.

2.3.2 Alternative Specifications

In order to examine whether there was heterogeneity underlying the results presented in

the previous section, we performed subgroup analyses as well as a model with an

alternative specification of the dependent variable. For simplicity, we confined our

alternative specifications to the aggregate model.

First, we checked the consistency of our results for various subgroups: Black

versus non-Black, Latino versus non-Latino, immigrant versus nonimmigrant, college

degree versus no college degree, parents versus nonparents, cohabiters versus married

couples (overall and only in years when they can be separately identified, i.e., post-1982),

and by time period (1979–1985, 1986–1992, 1993–2007). In each case, we tested

whether the three principal conclusions of our main analysis held: (a) that individuals'

housework hours rose during unemployment, while their spouses' housework hours fell;

(b) that the total time spent in household production increased during unemployment; and

(c) that unemployed wives increased their housework hours more than unemployed

husbands. All three conclusions held for all subsamples as well as the main sample, with

three exceptions for conclusion (b): for cohabiters, for Latinos, and for immigrants. In the

case of the female partner's unemployment, for all three of these groups, couples also

experienced an increase in total household production, but in the case of the male

partner's unemployment, couples experienced a small decrease in total household

production as female partners reduced their time in housework more than their

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unemployed partners increased their time in housework. This may indicate real differences in the responses of these subgroups to unemployment, although we are cautious in drawing conclusions from the relatively small numbers of unemployment spells observed in these groups (between 100 and 250 spells of unemployment for each gender within each subgroup). Future research, either qualitative or quantitative, may wish to explore variation by ethnicity and marital status in couples' household labor responses to unemployment.

Finally, although we believe absolute hours are more informative as a dependent variable for our research question because they allow us to identify changes in total household production along with changes in housework allocation, we estimated models using the share of housework hours completed by each spouse as the dependent variable. This allowed us to more closely test the deviance neutralization hypothesis as defined by Greenstein (2000) because the hypothesis rests on a relative measure of housework division. The patterns were consistent with our previous results in both sign and statistical significance, suggesting a lack of evidence for deviance neutralization: Spouses who became unemployed increased the share of the total housework they performed, regardless of their gender.

2.3.3 Limitations

As discussed in the Method section under "Dependent Variables," the stylized measure used in the PSID to report time in housework likely resulted in a slight overestimate of time in housework although, for the reasons previously discussed, this will only lead to biased coefficient estimates in certain circumstances.

A second limitation related to the housework measure available in the PSID is that it emphasizes time in female-typed tasks. Therefore, we may have underestimated husbands' increases in housework time if they substantially increased their time in male-typed tasks such as yard work and home and vehicle maintenance. Estimates from the National Survey of Families and Households indicated that married men spent about 40% of their total household labor time in these male-typed tasks (yard and household maintenance and auto maintenance), compared to only 6% for married women (Noonan, 2001). The gender differences we observed in husbands' and wives' responses to unemployment might have resulted from men increasing their time in male-typed household responsibilities, which are underreported in the PSID. In the most extreme form, gender norms might not affect the amount of time that unemployed husbands and wives add to their household labor responsibilities, but would greatly affect the type of work.

Because we are concerned with capturing measures of housework time during periods of unemployment and the housework data are collected only once per survey wave, we have limited our study to examine only unemployment at the time of the survey. In this way, labor force status and housework time were measured concurrently. This means that we did not observe the relationship between unemployment and housework for all spells of unemployment, only for those that were ongoing at the time of the PSID interview. This was a necessary limitation, as we did not know the housework time during unemployment for those whose unemployment spells fell between survey waves.

Finally, the data did not allow us to examine the influence of gender ideology on couples' responses to unemployment. The absence of data on gender ideology only matters in terms of our results if these attitudes are time-varying. If gender ideologies are consistent over time, these effects will be absorbed into the fixed effects and our estimation will be unbiased. However, there is some evidence that gender ideology is not a time-invariant trait for individuals (Davis, 2007; Fan & Marini, 2000). Thus, we consider three cases whereby a relationship between gender ideology, job loss, and housework could occur and discuss their implications. First, we could suppose the experience of unemployment would change an individual's gender ideology (see Fan & Marini for a similar idea), which would have a subsequent impact on time in housework. This is not problematic for our study because in this case gender ideology is a mediating variable but not an omitted variable. Therefore, we would not want to control for it for the same reasons we do not control for mental health or income. Second, we could suppose a change in gender ideology leads to a job loss. Although possible, this seems unlikely. Third, an individual could have a change in gender ideology that results in quitting a job. This is possible, and this is why it is important to examine how *involuntary* changes in labor force participation affect time in housework. This case would also explain why the coefficients for our out-of-the-labor-force results were larger than the coefficients for unemployment. However, the absence of a gender ideology measure does prohibit a study of either the mediating or the moderating influence of gender ideology on spouses' responses to job loss.

2.4 Discussion

In this study we have taken a closer look at the relationship between unemployment and couples' divisions of household labor using a large sample of couples followed over an extended period. Our results indicate that, at base, both men and women act rationally in the face of unemployment. Both unemployed husbands and unemployed wives increase their time in housework during the period of unemployment as compared to periods when they are employed; spouses of unemployed individuals decrease their time in housework during the unemployment period. Thus, we see evidence of a reallocation of housework in the direction of the unemployed spouse, consistent with both the time availability and relative resources theories. Furthermore, husbands' responses to unemployment vary with the wife's labor force status: Husbands whose wives are least available for housework increase their housework time the most.

Yet reallocation is only half of the change that occurs within couples. The total time in household production also increases. That is, the increase in housework time by the unemployed spouse is only partially offset by the decrease in housework time by the other spouse. This finding highlights the importance of studying unemployment within the larger family context rather than focusing only on shifting allocations between spouses or the effect of an event such as unemployment on the individual's time in housework. Absolute time in housework increases within the household during a spell of unemployment, a change that may be explained at least in part by the loss of income the family experiences, which reduces the ability to purchase substitutes for housework time, a possible mechanism that deserves further study.

At the same time, although individual couples may respond to unemployment differently, on average our results do not indicate a wholesale renegotiation of household roles during unemployment, perhaps because unemployment is perceived as only a temporary position. Spouses may only gradually re-evaluate their division of labor as time passes to assess whether it is reasonable under the new conditions of spouses' time availability.

Although the increases in housework time that we find for unemployed individuals, and the corresponding decreases in housework time by their spouses, are consistent with time availability and relative resources, our results indicate that even though both theories are posited as gender-neutral, their realization is gendered. Although unemployed husbands increase their time in housework an average of 3 hours per week, unemployed wives increase their time in housework by double that amount, an average of 6 hours per week. Our results indicate that time availability and gender-based explanations of the division of household labor cannot be treated additively; rather, they are interactive: It is not merely the case that wives do more housework than husbands, controlling for time in paid labor, but that wives' time in housework increases more rapidly with decreases in employment hours than does husbands'.

Our results might be interpreted in parallel to Brines' (1994) theory of gender display: She argues not that spouses' relative resources are irrelevant for their time in housework, nor that gender simply operates to give wives more housework responsibility than their husbands, even if they have equal resources, but that the relationship between relative resources and housework time is conditioned by gender. But, our results are *not* consistent with a gender deviance neutralization story. We see no evidence that husbands

compensate for becoming unemployed by reasserting their masculinity through rejection of housework. Complementarily, our results provide no evidence that wives increase their time in housework when their husbands become unemployed as a way to emphasize their own femininity and preserve their husband's masculinity. On the contrary, husbands increase their time in housework during unemployment, while their wives decrease their time in housework. These results hold whether we use absolute housework hours as Brines (1994) used or the relative share of housework each spouse provides as Greenstein (2000) used.

Thus, our key findings are twofold. First, during periods of unemployment there is a reallocation of housework in the direction of the unemployed spouse as theory suggests should be true. Equally, if not more important, there is an increase in the total amount of housework within the household. This increase in total household production is substantial as a fraction of changes in housework magnitude. Unemployed husbands increase their housework time by more than twice the amount that is necessary to offset declines in their wives' housework time. For unemployed wives, housework increases are four times the amount needed to compensate for their husbands' reductions. Thus, viewing unemployment primarily as ushering in a reallocation of housework responsibilities between spouses is inappropriate: The larger change is in the total amount of household production.

Second, although the pattern of increases and decreases seen in the results is consistent with the supposedly gender-neutral time availability and relative resources theories of housework division, the magnitudes of the increases indicate that these responses are gendered, but the results are not supportive of a gender deviance

neutralization explanation. Wives increase their housework time during unemployment on average at a magnitude of twice the increase of unemployed husbands, a phenomenon that requires further study.

Our findings suggest that we should move away from purely individual-level or divisional analyses in the study of housework. Just as the added-worker literature developed in economics to better understand the patterns of labor force participation within the family that followed unemployment, so too must we study the relationship between unemployment and housework within the broader family context. Time in housework is not only about the relationship between spouses. Rather it is also about the complex relationships between market labor, home production, outsourcing, and standards of cleanliness within the household. We hope that as data become available for researchers to examine features of the relationship between unemployment and nonmarket labor during the recession at the end of the 2000s, which produced high rates of unemployment, they will incorporate the family context into their studies, going beyond the level of the individual and the reallocation-only study of the spousal dyad.

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CHAPTER **3**

Re-imagining the Motherhood Penalty: The Role of Birth Spacing

3.1 Introduction

The majority of mothers in the United States participate in the labor force, including

those with small children (Dye 2008), and the intersection of work with family continues

to be a key area of interest for sociologists. Both men and women who contemplate

parenthood face a decision about the tradeoffs between the time required for childbearing

and childrearing and time in the labor force. However, because women tend to be the

primary caregivers for small children (Hochschild 1989), along with the child bearers,

motherhood typically entails more costly tradeoffs in terms of employment than does

fatherhood[2]. Given such high stakes, women often face a series of difficult decisions

about how to achieve their preferred combination of family- and career-related goals.

They must decide whether to have children, and then among those who choose to have

children, they must decide when the timing is best to bear those children. These

decisions do not end with the first child: after each birth, a woman must decide whether

to have a subsequent child and, if so, when. The potential economic consequences of

these decisions are large. Entry into motherhood is associated with significant costs in

lost labor force time and lower wages, conditional on labor force participation and

experience (Budig and England 2001; Crittenden 2001). Thus, motherhood is one

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[2] Entry into fatherhood may actually provide men with a wage premium in contrast with the penalty
experienced by women after their entry into motherhood (Glauber 2008; Lundberg and Rose 2000).

mechanism by which gender inequality in wages is maintained. In fact, some researchers now contend that motherhood is *the* critical factor in the remaining gender wage gap (England 2005; Epstein 1988; Waldfogel 1998). For married and partnered couples, parenthood is also instrumental in facilitating an earnings disparity between spouses (Lundberg and Rose 2000), which may lower wives' bargaining power within the household (McElroy and Horney 1981; Lundberg and Pollak 1996; Pollak 2005) and lead to poorer financial outcomes for women in the event of divorce (Holden and Smock 1991). However, existing work suggests that women who delay fertility can substantially mitigate these costs (Amuedo-Dorantes and Kimmel 2005; Wilde, Batchelder, and Ellwood 2010).

Given the magnitude of the consequences of fertility decisions for women's economic outcomes and the probable impact of fertility timing on these consequences, it is puzzling that there has been so little research on the financial implications of the length of the birth interval between children. Furthermore, since the financial penalty experienced by mothers of two children as compared to mothers of one is often estimated to be even larger than the penalty for one child as compared to remaining childless (Anderson, Binder, and Krause 2003; Budig and England 2001; Glauber 2007), if there is heterogeneity by birth spacing in the second-child penalty this may have considerable implications for women's wages. I argue that the implications of birth spacing are no less relevant to study than those of birth timing for women's long-term financial well-being, given that nearly 80% of women in the United States who become mothers go on to bear two or more children (Dye 2008).

Timing and spacing are intimately linked—for biological reasons the longer a woman postpones a first birth, the less time she has for bearing additional children. As such, the effects of timing and spacing may be confounded in existing estimates. For example, if women who bear their children later tend also to space them closer together, and close spacing is advantageous, the reported financial benefits to postponing fertility will be overstated. Additionally this implies that there may be an interactive effect between timing and spacing: Perhaps delaying fertility is advantageous, but only up to the point that it precludes a long birth interval. In this case, women who desire to have more than one child will have a different optimum age at first birth than women who desire to have only one child, and the implications for women who change their minds after the first birth are unknown.

I argue that spacing has direct and indirect effects on labor force outcomes. Spacing directly affects women's labor force outcomes by altering their labor supply decisions, changing the likelihood and duration of employment interruptions, and changing the level of labor force participation for employed mothers. Indirectly, the outcomes of spacing decisions may impact women's satisfaction with their efforts to combine labor force participation and childbearing. In this paper I do not identify the precise mechanisms by which spacing decisions affect women's labor force outcomes, focusing instead on the economic consequences associated with different spacing patterns, regardless of how these patterns arise.

I contribute to the literature in two primary ways. First, I go beyond work that estimates the effect of spacing on the likelihood of different labor market attachment patterns (e.g., Peltola 2004; Troske and Voicu 2009), instead estimating the effects of

spacing on women's mid-life annual earnings and hourly wage rates, after most childbearing has been completed. Second, I employ a potential outcomes framework that is implemented using inverse probability weighting and ex post outcome regressions. This strategy is designed to produce causal estimates of spacing effects and to allow me to identify heterogeneity in effects.

I begin by reviewing the motherhood penalty literature, including the birth timing and spacing literatures that inform this study. I then discuss how the present study advances work in this area. I follow with a detailed discussion of the analytic framework. I then discuss the data and present the results. Finally, I discuss the implications of the results as they contribute to understanding the intersection between women's fertility decisions and labor force outcomes.

3.2 Background

The effect of spacing on women's labor force outcomes may be considered under the framework of the motherhood penalty. Previous research has indicated that mothers have poorer labor force outcomes than non-mothers, even after accounting for mothers' lower levels of labor force experience (Anderson et al. 2003; Avellar and Smock 2003; Budig and England 2001; Glauber 2007; Loughran and Zissimopoulos 2009). For example, researchers have estimated a motherhood wage penalty of anywhere from 0% to 4% for one child, with estimates upwards of 5% for two or more children (Anderson et al. 2003; Avellar and Smock 2003; Glauber 2007; Loughran and Zissimopoulos 2009). Although neither the precise magnitude of this motherhood penalty nor the mechanism through which it arises is fully agreed upon, several theories have been posited.

There are five major theories in the motherhood penalty literature: human capital, productivity, job characteristics/compensating differentials, employer discrimination, and selection (Gough and Noonan w.p.). First, human capital theory suggests that mothers invest less in market human capital because they will have fewer periods of labor force participation over which to reap the benefits of their human capital. As a result they receive lower wages than childless women (Becker 1985). Because mothers take time out of the labor market to raise children, they have lower levels of work experience on average. In addition, they are more likely than non-mothers to work part time, and part-time work generally pays less per hour than full-time work (Blank 1990). Lower human capital generally explains a portion of the motherhood penalty in the empirical literature (about 50% typically), but much of the penalty remains unexplained (Budig and England 2001; Anderson et al. 2003).

Second, mothers may expend less effort in the workplace than non-mothers, resulting in lower productivity. Becker (1985) argues that individuals have only a limited amount of effort to expend, so mothers will deplete more of their effort at home, leaving less available for paid work. This then translates into lower productivity, which results in lower wages, all else constant. This theory is very difficult to test directly, and the few studies that have attempted to estimate the role of productivity in the motherhood penalty come to conflicting conclusions (Anderson et al. 2003; Kalist 2008).

The third theory of compensating differentials suggests that mothers may accept lower wages in return for jobs that are more flexible or that have attractive benefits. Jobs that demand less effort, have flexible hours, or require little travel may be more appealing to mothers than non-mothers (Becker 1985), and all else constant, jobs with such

characteristics will offer lower wages (Filer 1985). There is some evidence supporting this theory (Amuedo-Dorantes and Kimmel 2008), particularly with regard to the availability of maternity leave (Waldfogel 1998). However, others have found little evidence that mothers select into jobs with family friendly policies (Glass and Camarigg 1992).

Fourth, employers may discriminate on the basis of motherhood status. For example, employers may believe that mothers are less likely to be productive, especially in time-intensive jobs in which the demands of market work and the demands of motherhood are perceived to pose a greater conflict (Ridgeway and Correll 2004) but make no attempt to determine whether this is true. So, even if mothers and non-mothers are equally productive, a wage penalty may arise. Audit studies have indicated that mothers are perceived as less competent and less committed to work than are women without children (Correll, Benard, and Paik 2007). Discrimination can also occur when employers consciously favor non-mothers over mothers because they believe that mothers should not be in the labor force (Gough and Noonan w.p.), although this is probably rare.

Finally, the motherhood penalty may be due entirely to selection. That is, women who become mothers may differ from non-mothers even before childbirth in ways that are related to productivity in the workplace (Gough and Noonan w.p.). Additionally, women may choose to have children at times when their careers are stagnating or their wages are low because it reduces the cost of childbearing. Lundberg and Rose (2000) find some evidence for negative selection into motherhood on this basis.

These theoretical perspectives suggest ways of explaining the evidence that postponing a first birth reduces the extent to which mothers experience negative consequences in the labor market. With regard to human capital, women who postpone the first birth have more time to accumulate work experience prior to birth, which, given a typical wage growth profile, means they have higher wages when they first take a leave from the labor market than if they had not postponed. Since wages are known to follow a quadratic with experience (Mincer 1974), remaining engaged in the labor market full time during the years of rapid earnings growth that occur immediately following completion of schooling should advantage women who postpone fertility. This improves their market position upon return to the labor force. The implications for birth spacing are that if certain intervals improve women's ability to accumulate human capital in the interim period between births, or make it easier to return to full-time employment, these women may see a smaller penalty from having a second child than women without these advantages. One might hypothesize that spacing one's children farther apart would facilitate a return to full-time employment. One might have similar assumptions about work productivity, although it is very difficult to test. Women who delay childbearing until a point in the life course when they have higher earnings may also benefit from having more financial resources that they may use to reduce the demands placed on them outside of the labor market making it easier to keep up with pre-birth productivity or reducing the pressure to make a change to a part-time or family friendly job. They may be able to purchase help with household chores or, perhaps more importantly, an acceptable child care substitute.

From an employer discrimination perspective, work interruptions may be less costly when they follow a long period of uninterrupted employment, given that the worker's employment history may be viewed as a signal of her commitment to the labor force. Furthermore, women who delay fertility will, on average, have accumulated more tenure with their employers prior to the birth of the first child than women who do not delay. As a result, employers may make less use of stereotypes about mothers when assessing the employee's performance. Finally, there is undoubtedly some role for selection in explaining differences in motherhood penalties regardless of how women time and space their children. I will attempt to account for this selection in my analysis.

Extending the findings of the postponement literature to birth spacing suggests that on average the extremes of birth spacing (either very short or very long) may result in a larger motherhood penalty. Having two children in close succession may signal to employers that the mother is less invested in market work, reducing her potential for wage growth. In addition, with two or more small children at home, the demands on mothers' time are likely to be intensified, reducing the effort that they can dedicate to market work. Close birth spacing may also increase mothers' incentives to trade financial rewards in the workplace for family friendly characteristics during the period of intensive childrearing. Finally, the intensity of the childrearing period for women with short birth intervals is likely to encourage longer work interruptions, particularly as child care costs relative to the woman's foregone wage will be higher for women with two children under preschool age at home.

Alternatively, women whose children are spaced very far apart (e.g., 8 years apart) will be considerably older at the time of the second birth than at the first and may

have less energy to devote to both work and childrearing. They may also be more likely to select into a second birth on the basis of characteristics that are associated with labor force outcomes such as an unemployment spell or a second marriage following a divorce. Women with mid-range spacing intervals may be more likely to remain attached to the labor force and experience shorter work interruptions. This may improve these women's ability to maintain employment with the pre-birth employer and hence retain job-specific social and human capital. Taking these factors together, having non-normative (i.e., extreme) birth intervals may be negatively associated with women's labor force outcomes.

Life-course research (Elder 1977), suggests that the employment decisions made by women during the period of childrearing will have long-term effects on their career trajectories. Thus, if extreme birth intervals are negatively associated with labor market outcomes, women with such birth intervals will be more disadvantaged in the labor market than those with mid-range birth intervals even at mid-life.

3.2.1 Previous Research

Researchers have examined the motherhood penalty in a number of ways, but little research has focused on birth spacing as a possible source of labor market penalties faced by mothers. Only two studies in recent literature address the topic. Using data from the National Longitudinal Survey of Youth 1979 (NLSY79), Peltola (2004) found that shorter birth intervals facilitated labor market re-entry, but women with that pattern were more likely to enter part-time work with lower hours than women with longer birth intervals. As noted previously, part-time work is generally associated with lower wages

and larger motherhood penalties. Troske and Voicu (2009) also used data from the NLSY79 and found that longer birth intervals reduced the negative effect of motherhood on the probability of working full time, although it increased the positive effect on the probability of working part time. They also found heterogeneous effects by race and education. In particular, women with higher education had more to gain from having a second birth after a longer interval, but they were also more likely to space their births close together (Troske and Voicu 2009), which may have resulted in part from biological limitations that arose after having postponed a first birth.

Prior research has identified a number of sources of heterogeneity in the motherhood penalty, which are relevant for my analysis. The majority of evidence on age at first birth and postponement indicates that postponement of a first birth reduces the magnitude of the motherhood penalty (Amuedo-Dorantes and Kimmel 2005; Herr 2011; Miller 2011; Taniguchi 1999). Some researchers have also found a larger motherhood penalty for married women than unmarried women (Budig and England 2001; Budig and Hodges 2010), but other researchers have found no difference by marital status (Killewald and Gough w.p.; Taniguchi 1999; Wilde et al. 2010). Highly educated, highly skilled women are usually found to have a smaller motherhood wage penalty than those with less education or lower skills (Herr 2011; Miller 2011; Taniguchi 1999). Some researchers have even found a motherhood premium for such women (Amuedo-Dorantes and Kimmel 2005; Anderson et al. 2003). Conversely, Wilde et al. (2010) found larger penalties for high-skill women resulting in twice the lost lifetime earnings as low-skill women. Finally, scholars have found racial and ethnic differences in the motherhood penalty. Generally, white women are found to pay a larger penalty than black mothers

(Glauber 2007; Hill 1979; Neumark and Korenman 1994; Waldfogel 1997), but there is less known about differences in penalties for Hispanic women and Asian-American women.

3.3 The Present Study

I make two primary contributions to the literature. First, Peltola (2004) and Troske and Voicu (2009) only examined the probabilities of different types of labor force re-entry following first and second births. Whether and when a mother returns to work and at what level of labor force attachment is important, but it is also important to examine the long-term financial implications of these choices. Therefore, I contribute to the literature by estimating the effects of different birth intervals on financial outcomes measured at mid-life, namely annual earnings and hourly wages.

Whether the length of the birth interval matters for women's long-term economic outcomes is an unresolved question, though each of the two previous studies of spacing suggests possible mechanisms for an effect. At the heart of this uncertainty is a lack of knowledge about the relative costs of tradeoffs women make when considering strategies to combine work and family over the life course. For example, is the wage cost of a second job interruption comparable to that of a first interruption? Does the actual or perceived depreciation of human and social capital occur at a constant rate during time out of the labor force? Are the returns to two years of part-time experience greater than or less than the returns to a single year of full-time experience? And perhaps most important for the decision-making process, do women have a sense of the cost of these tradeoffs when deciding how to time and space their births? While I cannot answer these questions with this study, they provide a framework for understanding estimates of the net cost or

benefit women receive from particular birth spacing patterns, which is an important first step in examining these issues, as there is currently no consensus on whether, and under what circumstances, particular birth intervals may affect economic outcomes.[3]

Because of the inherent complications in comparing long-term outcomes among individuals with different starting points, different numbers of spells out of the labor force, and different labor market trajectories, I examine the effects at a single point in each individual's time, at age 45, allowing elements of their previous histories to be absorbed into the net effect I observe at that age. I examine two measures of mid-life outcomes: annual earnings and hourly wages. I am ultimately interested in women's cumulative work behavior and financial well-being. If some women accept a short period out of the labor force, which earns them no income in that period, in return for higher lifetime earnings than other women who spend more time in part-time work, it would be a mistake to believe that the women who take this time out of the labor force are disadvantaged in the long-run.

My second major contribution to the literature is that I employ a potential outcomes framework to estimate causal effects of different birth spacing patterns on mid-life labor market outcomes. I implement the framework by using a reweighting

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[3] It is important to note that a number of mechanisms may lead to different birth spacing intervals, not just their subsequent outcomes. Women may choose to space their births far apart as a way to facilitate attachment to the labor force and eschew the homemaker role. They may also view this as a way to reduce the family's expenditures on child care to the extent that they can justify continuing to work outside the home, as child care costs for two young children are high. Women may also decide to postpone a second birth if they experience a partnership disruption, waiting to have a second child until they have established a new long-term partnership. Longer birth intervals may also result for more unexpected reasons. For example, either the first or second birth may be mistimed. The second birth may be an unexpected birth for a woman who intended to have only one child. Women may experience difficulty conceiving a second child, particularly if they postponed their childbearing to the point when their ability to conceive was already declining. Arguably these "unexpected" or "unintended" long birth intervals may have different consequences for women's economic outcomes than intended and planned long birth intervals. However, in this paper I am interested primarily in determining the individual economic consequences of different birth spacing patterns regardless of the mechanisms behind them. This will serve as a first step in examining the underexplored nature of the relationship between birth spacing and women's labor outcomes.

procedure[4]. I also consider heterogeneity across subgroups, in an attempt to better identify variation in outcomes resulting from different characteristics of mothers and their birth spacing, and to achieve an improved estimate of the association between birth spacing and mid-life economic outcomes, net of other characteristics that may affect labor force attachment and earnings growth. In particular, I consider heterogeneity by age at first birth, race, and education.

3.4 Data and Methods

To examine whether having a second child at different points in a woman's life affects her mid-life labor market outcomes, I employ a potential outcomes framework (e.g., Rubin 1974) and use the corresponding treatment language. Let Y_1 be the outcome in the "treated" state (i.e., having a second birth at a particular time point). Let Y_0 be the outcome in the untreated state (i.e., no second birth at that particular time point). I am interested in the effect of the treatment D on the outcome Y. Therefore, the observed outcome will be the result of the following equation:

$$Y = DY_1 + (1 - D)Y_0 \tag{1}$$

However, equation (1) does not itself provide the parameter of interest, namely, the average effect of treatment on the treated (ATET), defined as the average impact of the treatment among those participating in the treatment. In my study, the average effect of treatment on the treated will refer to the average impact of having a second birth in a

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[4] Many studies in the motherhood penalty literature have used fixed-effects models to estimate the relationship between motherhood status and current wages. Such models have been invaluable. However, they are not well-suited to the present research question because they compare within-person outcomes across periods. Since women do not experience more than one birth interval between their first and second child, fixed-effects methods are inappropriate to the question at hand.

particular time period among those having the second birth as compared to those who have not yet had (or will never have) a second birth. The ATET is denoted as follows:

$$\Delta^{TT} = E(Y_1 - Y_0|D = 1) = E(Y_1|D = 1) - E(Y_0|D = 1) \tag{2}$$

Now that the parameter of interest has been defined, it is straightforward to see why this analytic framework is called potential outcomes. This is because we observe either Y_1 or Y_0 for each individual, but we do not observe the untreated outcome for the treated individual or the treated outcome for the untreated individual. More concretely, we observe the midlife wages and earnings of women with a second birth at a particular time point, but we do not observe what their wages and earnings would be if they had not had a second birth at that time point. This is the evaluation problem.

To solve what is essentially a missing data problem, it is necessary to impute the missing outcomes. For example, for the treatment effect on the treated in equation (2) we have two terms. The first term (denoted (A) below) is the expectation of the treated outcome given that treatment occurs. This is easily constructed. The second term (denoted (B) below) is the expectation of the untreated outcome given that treatment occurs. We do not observe this. If we simply substitute $E(Y_0/D=0)$ for this term, we will incur bias in the results because of selection. This bias is equivalent to $E(Y_0/D=1)-E(Y_0/D=0)$.

$$\Delta^{TT} = E(Y_1|D = 1) - E(Y_0|D = 1)$$

$$\qquad\qquad (A) \qquad\qquad (B)$$

3.4.1 Solving the Evaluation Problem

Solving the evaluation problem by imputing the missing outcomes requires making assumptions about selection. In particular, it requires making the conditional independence assumption (CIA), which states that treatment status is random conditional on a set of *X* observed covariates (Rosenbaum and Rubin 1983; Rubin 1978). Thus, for ATET this means that $(Y_0 \perp D)|X$. The variables in *X* cannot be affected by the treatment, and also *X* must include all of the variables that affect both the treatment and the outcomes. It is not possible to test whether the CIA is valid without conducting an experiment, but sensitivity analyses can help with assessment of validity. The variables in *X* are chosen based on theory, past research, and other factors such as institutional knowledge (Sianesi 2004).

An additional issue arises when conditioning on *X*. This is referred to as the dimensionality problem. Essentially, with many variables in *X* there may be so many cells that some cells will not have untreated observations to match to the treated observations (Rosenbaum and Rubin 1983; Smith and Todd 2005). Rosenbaum and Rubin (1983) suggested solving this problem through the use of the propensity score and showed that matching treated and untreated cases together using *P(X)=Pr(D=1|X)* can substitute for matching on *X* directly. I employ this strategy in my own analysis.

3.4.2 Extension to the Dynamic Context

Thus far, I have laid out the potential outcomes framework and evaluation problem within a static context. However, my interest in this paper is in whether the timing of the second birth matters. This requires extending the standard potential outcomes model to

the dynamic context. The dynamic framework is especially useful for examining the impact of a treatment at a particular time (e.g., *M* months into unemployment) compared to not having received the treatment up to that point (Sianesi 2004). Lechner (2009) has referred to the dynamic potential outcome model as an interesting framework for studying the interaction of fertility and labor market decisions at different ages because it allows careful consideration of selection issues, and it allows explicit consideration of dependence on the labor market and fertility history realized up to that point. For the purpose of implementing this framework, it is convenient to think about elapsed duration as discrete (Sianesi 2004); in my analysis, people who are eligible for a second birth (the treatment) at time *t* are those who still have not had a second child after *t* years irrespective of what happens after *t*.

Robins (1986) suggested an explicitly dynamic causal framework that allows defining causal effects of dynamic interventions and systematically addressing the type of selection problem that could arise over a sequence of events. The approach is based on the key assumption that it is possible to control for the variables jointly influencing outcomes and selection at each particular step of the sequential selection process. In other words, because there is now a timing aspect involved, it is necessary for the CIA to hold in terms of future second births. Each effect by year of treatment requires the CIA to hold at the margin (i.e., birth at *t* vs. postponing to at least *t+1*). It does not have to hold in terms of a "once-and-for-all decision" (Sianesi 2004). Thus, the CIA becomes the dynamic CIA (DCIA) (Lechner 2009).

At the initial period of treatment, everyone is in the same treatment state. For my analysis, this means that each woman has one just-born child at the initial period. This is

followed by 10 periods in which different treatment sequences could be realized (i.e., in each period a woman can have a second child or not). Rather than estimating one propensity score model to resolve the dimensionality problem, I must estimate 10 models, each modeling the probability of a second birth in year t, conditional on X and on having reached the post-first birth duration of t. This approach has been applied by Sianesi (2004) and others in studies of active labor market programs.

3.4.3 Inverse Probability Weighting

The estimation of the propensity score is the first step in estimating the ATET. In each of the 10 years following a first birth, probit models are estimated that predict the probability of a woman having a second child in that year. The propensity scores are then employed in a reweighting procedure to estimate the ATET for each period. This reweighting procedure is called inverse probability of treatment weighting. The procedure allows me to align the treatment and controls groups dynamically by year since first birth and estimate a yearly treatment effect (Fitzenberger, Sommerfeld, and Steffes 2012).

Before estimating the ATET, it is important to examine whether conditioning on the propensity score results in balance on the covariates between the treated and the untreated cases. I use a regression-based balancing test to check for balance (Smith and Todd 2005), wherein I regress each conditioning variable in X on a polynomial in the propensity score and an interaction between the treatment dummy and the polynomial. If balance is achieved, the coefficients on the interactions will be equal to zero. If balance is not achieved, it may be necessary to revisit the propensity score specification. However,

it is important to note that the balancing test does not provide information about the validity of the CIA.

3.4.4 Estimator

Equation (3) provides the normalized estimator for the ATET for a single time period, where n_1 is the number of treated cases, n_0 is the number of untreated cases, Y is the outcome, D is the treatment, and $\hat{P}(X_i)$ is the propensity score (Busso, DiNardo, and McCrary 2011). The parameter of interest, Δ_{TT}, is the effect on midlife wages and earnings of having a second birth at time t versus not yet having a second birth. Therefore, the counterfactual for each period is those women who have not yet had a second birth by time t and those women who will never have a second birth.

Using a normalized estimator ensures that the weights will sum to one within the sample. This is important because unnormalized estimators can result in weights that are arbitrarily large, which may lead to problems, including impossible treatment effect estimates (Busso et al. 2011). In contrast to Frölich (2004), Busso et al. (2011) show that inverse probability weighting is a good method for this type of analysis but only if the weights are normalized. In this analysis, the outcome Y is wages (or earnings) measured at age 45, and the treatment D is a second birth at a particular time point. $\hat{P}(X)$ is the estimated probability of a second birth at a particular time point.

Normalized estimator for ATET:

$$\widehat{\Delta_{TT}} = \frac{1}{n_1}\sum_{i=1}^{n} Y_i D_i - \frac{1}{n_0}\sum_{i=1}^{n}\left(\frac{1}{n_0}\sum_{i=1}^{n}\frac{\hat{P}(X)*(1-D_i)}{1-\hat{P}(X)}\right)^{-1}\left(\frac{\hat{P}(X_i)*Y_i*(1-D_i)}{1-\hat{P}(X_i)}\right) \tag{3}$$

One complication with using a two-step estimation procedure is that standard errors must be adjusted to account for the first-stage model that estimates the propensity

score. I follow the literature in estimating bootstrapped standard errors for the estimated treatment effects (e.g., Fitzenberger et al. 2012, Sianesi 2004).

3.4.5 Assessing Effect Heterogeneity

Finally, after obtaining the ATET for each time period, I follow the strategy of Abadie and Imbens (2011) and Fitzenberger et al. (2012) and estimate ex post outcome regressions to examine effect heterogeneity. In particular I examine heterogeneity by age at first birth, race, and education. These regressions will be useful in determining whether there is heterogeneity in effects that is obscured in the overall ATET.

3.4.6 Data

The data for the analysis come from the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 has been used in much of the literature assessing the motherhood penalty (Amuedo-Dorantes and Kimmel 2005; Budig and England 2001; Loughran and Zissimopoulos 2009) and is also the dataset used in the two recent papers that examine spacing effects (Peltola 2004; Troske and Voicu 2009). It is particularly appropriate for this research because it focuses on the experiences of young adults and captures nearly all of their work experiences up until middle age. Initiated in 1979 as a sample of 12,686 men and women ages 14-22, NLSY79 has surveyed respondents annually through 1994 (when respondents were ages 29-37), and biannually thereafter. NLSY79 therefore provides a large sample of young women experiencing the transition to motherhood. By the year of the last publicly available wave of data, 2010, the

respondents were ages 45-53, which means that most women in the sample had completed their childbearing.

The analysis requires a number of sample restrictions. First, I exclude from the sample respondents who were married in 1979 and also those who were self-employed in any of the waves (1,569 women; 1,068 women). Because I am focusing on mid-life labor force outcomes, it is important that I have long work histories available. I exclude the military subsample, which was not re-interviewed after 1984, and the poor non-black, non-Hispanic subsample, which was not interviewed after 1990 because these samples do not allow for analysis of long work histories. This results in the loss of 69,456[5] person-year observations. In addition, I drop men from the sample, which results in the loss of 115,362 observations. I further restrict the sample to women without a birth by 1979 (22,501 observations). These restrictions allow me to focus on women with a potential for a long work history in standard work who had not started a family by the time of the first wave of observation. To ensure enough time for women to have more than one birth, I restrict the sample to women with a first birth no later than age 35. I also require women to be at least 18 at the time of the first birth to allow for the completion of at least secondary schooling. These two restrictions result in the loss of 27,614 observations. To allow the wages of women with a second birth to recover by the time I measure them at age 45, I restrict the sample of women with a second birth to those whose birth occurred no later than age 40 (335 observations). This does not eliminate women who are not observed to have a second birth from the analysis. In order to include all non-attriting women from the NLSY in the sample, regardless of their age in 1979, I measure all women's labor force outcomes at age 45, which includes wages and earnings through the

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[5] All following observation numbers listed in this section are person-year observations.

previous year at age 44[6]. In addition, I eliminate from the sample women with a sample

weight equal to zero (5,429 observations). Finally, because I am interested in birth

spacing, I eliminate women with twins (587 observations). These women are arguably a

very interesting group to study because both children arrive simultaneously, but there are

too few observations for a meaningful analysis in this data set.

I use list-wise deletion to address the problem of missing data for the dependent

variables and covariates[7]. I drop 1,900 observations for missing values on ASVAB factor

1, 1 observation for missing education, 4 observations for missing marital status, 2,033

observations for missing experience, 289 observations for missing region, 208

observations for missing information on the number of children the respondent wants,

1,828 observations for missing lagged nonwork, and 4,481 observations for missing

income among women who reported being in the labor force at the time of the interview.

For the outcome variables, I drop 1,708 observations for respondents missing income

measures at age 45 who reported being in the labor force and 7,849 observations for

respondents missing wage measures at age 45. This leaves me with a sample of 36,263

observations on 1,917 women, interviewed in an average of 19 waves each.

3.4.7 Variables

There are two outcome variables in the models—log annual earnings and log hourly

wages, both measured at age 45. Annual earnings and hourly wages are adjusted to 2011

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[6] The measures for some women are based on reports at age 46 because after 1994 the survey was collected
biannually. For these women their wages and earnings refer to age 45. Nonetheless, I will refer to measures
collected at age 45 throughout the analysis.
[7] For the lagged part-time work variable, family resources, and lagged cumulative experience, I use
indicator dummies for the missing data instead of listwise deletion so as to retain more cases. The sample
retention rate of the survey over the period of observation is 76.8%.

dollars. Each of these two outcome variables provides an aspect of the respondent's long-term labor force outcomes, which may be impacted by birth spacing. In particular, earnings captures labor market time along with wage rate. The values these variables take are simply those reported in the survey obtained at age 45 and referring to the previous year.

As discussed above in the description of the method, I first estimate the probability of having a second child in each time period. This is estimated using a probit model with a second birth in that time period as the outcome variable and controlling for a number of background characteristics that influence both childbearing and labor force outcomes. I control for two factors of the Armed Services Vocational Aptitude Battery (ASVAB) administered to respondents in 1981. The ASVAB is considered a proxy for skill level. I construct the factors using factor analysis. The first factor is more heavily verbal, with stronger loadings on the general science, word knowledge, and paragraph comprehension subtests of the ASVAB. The second factor is more heavily mathematical, with stronger loadings on arithmetic reasoning and mathematics knowledge. In addition to controlling for ASVAB factors, I control for other background characteristics, including education, race and the number of children wanted. Education is measured as a categorical variable and indicates the highest level of education achieved by the respondent before the second birth (if a second birth exists): less than high school, high school, some college, and college. Race is measured as a categorical variable with categories for white, black, and Hispanic. The number of children wanted was measured in 1979 and refers to the number of children the respondent indicated she wanted to have in the future.

Along with background demographics, I control for concurrent demographics in the probit models. I control for marital status, age at first birth, and region. Marital status is measured as never married or widowed, married, and separated or divorced. Age at first birth is a continuous variable. Region refers to the Northeast, North Central, South, and West regions of the country. Finally, the models include controls for labor market status, lagged one year prior to the year of interest. I include dummy variables for lagged part-time work and lagged non-work. Both part-time work and not working have been shown to be associated with childbearing. I also control for lagged cumulative experience. Drawing on prior research, women who want to have a second child and have more cumulative work experience should be more likely to have the second child now rather than waiting because the cost of the interruption will be lower than for women with less experience. Additionally, I control for lagged financial variables, as these may be causally related to having a child. I control for lagged income of the respondent and lagged family resources, which is a measure of the net family income minus the respondent's income. This measure indicates whether the respondent has additional financial resources to draw on if she were to give birth. The values of these variables are inflated to 2011 dollars using the Consumer Price Index. Finally, I interact marital status and race in the probit models, as important differences between race and ethnicity groups exist with regard to childbearing in the context of marriage, and marriage may also be related to wage outcomes (Killewald and Gough w.p.). Following Sianesi (2004) I provide selected estimates of the coefficients from the propensity score models in Appendix Table 1.

After estimating the propensity scores for each time point (t=1 to t=10), where t indicates the number of years since the first birth, I calculate the normalized weights from equation (3). Plots of the propensity scores by treatment status are found in the appendix. The plots indicate that overlap is very good. Before estimating the ATET, I run regression-based balancing tests as discussed above. The balancing tests indicate that the weights do a good job of balancing the covariates. Not one of the tests of difference approaches statistical significance. To estimate the ATET I use the weights in a set of weighted regressions of the treatment on the outcomes. I bootstrap the standard errors using 500 replications. These initial regression models only estimate the ATET from equation (3). After estimating the ATET from equation (3), I add covariates to the models to examine whether there is heterogeneity by race, education, or age at first birth.

3.5 Results

Table 3.1 presents the sample-weighted descriptive statistics of the sample at entry into eligibility for a second birth (i.e., these women have just had a first birth). Fifty percent of the sample has an education of high school or less, while the remaining women are split about equally between the some college and college degree categories of education. More than 80% of the sample is white, while 11% of respondents are black and about 6% are Hispanic. Age at first birth in this analytic sample is quite high, averaging 25.8 years. This is not completely unexpected because women in the NLSY79 who bore children before they were first observed or before age 18 were eliminated from the analytic sample. Women in the sample had relatively high expectations for the number of children they wanted when they were interviewed in 1979. Additionally, the

vast majority of women in the analytic sample were married at the time they became

eligible for a second birth (77%), but nearly 20% were never married or widowed as

well.[8] With regard to labor-related characteristics, nearly one-third of women were

working part time in the year before the first birth, while about 10% were not working.

The average income of the women in the sample was about $25,000 annually, while the

average annual amount of family resources, not including the respondent's income, was

about $45,000. Finally, the average amount of work experience accumulated up to one

year prior to the year of interest was 9,824 hours, or about 4.7 full-time years.

In Figure 3.1 I show the distribution of birth intervals across the 10 years

following a first birth. In the figure we see that the highest frequencies of second births

occur between two and four years after the first birth. Very few births occur only one

year after the first birth or more than eight years after the first birth.

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[8] The number of widowed women in this age group is very small. They accounted for less than 1% of the observations in the sample after restrictions were imposed but prior to listwise deletion for missing covariates.

Table 3.1 Descriptive Statistics of the Sample at Entry into Eligibility for Second Birth

	Mean (SD)
ASVAB Factor 1	0.27 (0.74)
ASVAB Factor 2	0.22 (0.82)
Education	
Less than HS	7.82%
High School	42.34%
Some College	23.95%
College	25.89%
Race	
White	83.04%
Black	11.25%
Hispanic	5.71%
Age at First Birth	25.76 (4.52)
# Children Wanted	2.64 (1.44)
Lagged PT work[a]	.29
Lagged cumulative experience (hours)	9824.28 (8771.41)
Marital Status	
Never Married/Widowed	19.44%
Married	77.13%
Separated/Divorced	3.44%
Lagged Non-working	.09
Income	25352.80 (22648.18)
Family Resources	45115.21 (118943.40)
Region	
Northeast	19.25%
North Central	29.43%
South	33.59%
West	17.73%
Fam. resource missing	.17
Cum. experience missing	.10
Income missing	.04
N	1666

[a]Standard deviations not provided for proportions.

Figure 3.1 Frequency Distribution of Interval between First and Second Births
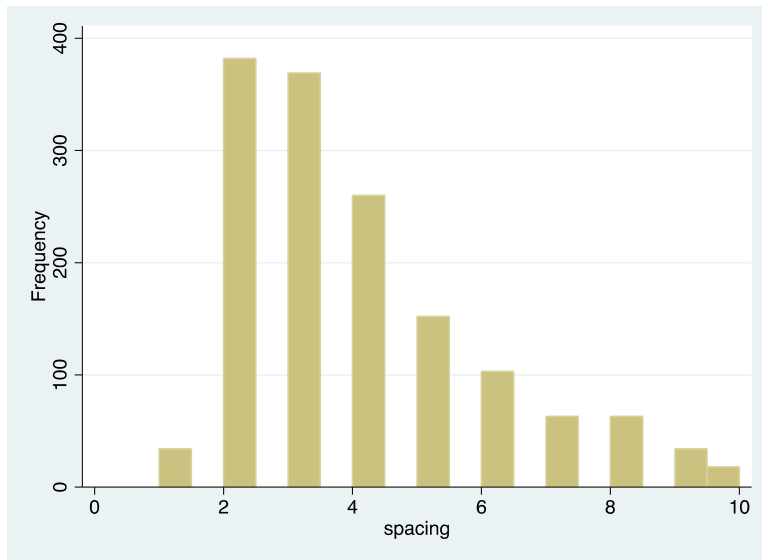
Having examined the distribution of birth intervals, I calculate the ATET to see whether different birth intervals have effects on women's midlife labor force outcomes. Table 3.2 contains the ATET estimates from the weighted regressions at each time point, without the addition of covariates. There is only one marginally significant treatment effect ($p<.10$) in the model of the effect on log annual earnings at age 45 of having a second birth as opposed to not having a second birth in the interval that occurs four years after the first birth. The coefficient suggests that women who have a second birth four years after the first achieve an earnings premium over those women who do not experience a second birth four years after experiencing a first birth. These women without a second birth four years after the first may still experience a second birth in a later year. Thus, although I had hypothesized that women with birth intervals at the extremes might have worse outcomes than women with mid-range birth intervals, I find

no statistically significant evidence to suggest that this is the case. However, average

treatment effects may obscure heterogeneity by subgroups, which I turn to next.

Table 3.2 Estimated Average Treatment Effects on the Treated for the Wages and
Earnings of Women at Age 45, By Birth Interval

	Log Annual Earnings	Log Wages
One-year interval *(N=1917)*	-0.05	0.01
	(-0.13; 0.14)	(-0.17; 0.32)
Two-year interval *(N=1883)*	-0.15	-0.08
	(-0.16; 0.01)	(-0.11; 0.02)
Three-year interval *(N=1508)*	-0.10	0.01
	(-0.27; 0.08)	(-0.12; 0.18)
Four-year interval *(N=1146)*	0.19[+]	-0.03
	(0.13; 0.38)	(-0.15; 0.16)
Five-year interval *(N=898)*	0.05	0.02
	(-0.21; 0.25)	(-0.26; 0.19)
Six-year interval *(N=751)*	0.003	-0.002
	(-0.39; 0.27)	(-0.29; 0.25)
Seven-year interval *(N=651)*	0.28	0.22
	(0.29; 0.39)	(0.16; 0.43)
Eight-year interval *(N=590)*	-0.24	-0.15
	(-0.73; 0.01)	(-0.49; 0.18)
Nine-year interval *(N=527)*	0.17	0.22
	(-0.02; 0.49)	(0.12; 0.47)
Ten-year interval *(N=493)*	0.09	-0.10
	(-0.43; 0.83)	(-0.54; 0.50)

Note: 95% bias-corrected bootstrapped confidence intervals (500 repetitions). [+]p<.10.

3.5.1 Subgroup Analyses

Although I found minimal evidence of in the sample overall that women with a

second birth at a particular time point achieved different wages and earnings than

comparable women who did not have a second birth at that time point, there may be

heterogeneity that is obscured by these average estimates. Therefore, I estimate three

additional sets of regressions to explore heterogeneity by race, education, and age at first

birth. Estimates from models with statistically significant interaction effects are included

in Table 3.3[9]. I find a few statistically significant effects for education and age at first birth. With regard to education, I find some statistically significant effects for the 1-year interval, 5-year interval, and 10-year interval. For the 1-year interval the results suggest that having at least some college negates the penalty that women who have a second birth one year after the first experience, as compared to women who do not have a second birth one year after the first. The coefficient instead indicates a substantial premium for college-educated women over high school-educated women with a birth at the 1-year interval, as compared to the women without a second birth at the 1-year interval. This finding is in contrast to Troske and Voicu (2009) who found that more highly educated women would gain from waiting longer to have a second child.

At the 5-year interval, there are statistically significant effects by education for both wages and earnings. The results suggest that high school educated women experience a premium as compared to women who do not have a second birth five years after the first birth. However, in contrast to the results for the 1-year interval, women with a college degree experience a smaller wage premium with a second birth than the women with a high school education, and the interaction term wipes out the positive effect of a college degree on wages and earnings.

At the 10-year interval, the results for wages indicate that women with some college who have a second birth 10 years after the first actually experience a large penalty compared to women with a high school degree also having a child at the 10-year interval. By contrast the women with a high school degree who have a child at the 10-year interval experience a wage premium compared to women who do not have a second birth 10 years after the first birth. For earnings, the interaction effect wipes out the

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[9] Complete results available from author on request.

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positive effect of college for college degree-holders, and for those with some college, it results in a penalty as with wages. This may be consistent with a productivity argument, as women with at least some college may hold jobs that are more demanding than women with a high school degree on average, and having a second child a decade after the first may make them tired and unable to perform at a high level in the workplace. However, it could also be the case that these women have a second child at this late point because they have experienced changes in their work or home lives that lead them to select into childbearing and that are correlated with labor force outcomes, such as a layoff. Additionally, because these women have a second child so much later in life, it may also be the case that their wages have not had enough time to recover by age 45 from the interruption to their labor force participation.

Along with heterogeneity by education, I also found some heterogeneity by age at first birth. In particular, I found effects for the 4-year interval and the 5-year interval. For the 4-year interval there was a statistically significant interaction effect in the model of log wages. The results suggest that as the age at first birth rises, the wage premium experienced by women having a second birth four years after the first (as compared to women who do not have a second birth four years after the first) declines slightly. This pattern is also seen in the model of log annual earnings for the 5-year interval. For the 5-year interval in the wage model I also found statistically significant effects. I find that the interaction term wipes out the positive effect of age at first birth on log wages. Nonetheless, women with a birth at the 5-year interval still experience a large premium compared to women who do not have a second child five years post-first birth. There is no obvious explanation for the heterogeneity by age at first birth, but for the 4-year and

5-year intervals, the results indicate either no real difference by age at first birth or a

small advantage to having a first birth earlier if the second birth occurs after 4 or 5 years.

Table 3.3. Average Treatment Effects on the Treated for Selected Birth Intervals, Interactions by Education and Age at First Birth.

	Log Wages				Log Annual Earnings			
	1-Year Int.	4-Year Int.	5-Year Int.	10-Year Int.	1-Year Int.	4-Year Int.	5-Year Int.	10-Year Int.
Panel 1. Interactions by education								
Second birth	-0.4 (0.30)		0.30 (0.14)*	0.71 (0.14)***	-0.72 (0.32)*		0.40 (0.20)*	1.17 (0.23)***
Education (HS is omitted)								
Less than HS	-0.80 (0.14)***		-1.01 (0.22)***	-1.22 (0.25)***	-0.94 (0.18)***		-1.11 (0.27)***	-1.37 (0.31)***
Some college	0.21 (0.06)**		0.27 (0.09)**	0.40 (0.11)***	0.36 (0.09)***		0.29 (0.12)*	0.65 (0.15)***
College	0.40 (0.09)***		0.65 (0.14)***	0.50 (0.19)**	0.97 (0.08)***		1.13 (0.14)***	1.21 (0.19)***
Second birth X less than HS	0.51 (0.61)		-0.15 (0.53)	-1.50 (1.09)	0.77 (0.79)		-0.60 (0.66)	-2.12 (1.45)
Second birth X some college	0.87 (0.50)+		-0.14 (0.26)	-1.95 (0.61)**	1.51 (0.43)***		0.01 (0.31)	-2.12 (0.75)**
Second birth X college	1.44 (0.45)**		-0.74 (0.29)*	-0.12 (0.29)	1.67 (0.51)**		-1.04 (0.33)**	-1.16 (0.41)**
Panel 2. Interactions by age at 1st birth								
Second birth		1.20 (0.52)*	1.18 (0.54)*			1.10 (0.62)+	2.40 (0.77)**	
Age at 1st birth		0.03 (0.01)**	0.05 (0.01)***			0.04 (0.01)**	0.07 (0.01)***	
Second birth X age at 1st birth		-0.05 (0.02)*	-0.05 (0.02)*			-0.04 (0.02)	-0.09 (0.03)**	

Note: *p<.05; **p<.01; ***p<.001.

3.5.2 Limitations

The major limitation of this study is the sample size. At the tail ends of the birth interval

distribution, the number of cases is quite small as Figure 3.1 indicates. Only 34 women in

the sample have a second birth in the year following their first birth. By 6 years out from

the first birth, the number of women having second children has declined below 100 on a

yearly basis, and at 9 and 10 years post-first birth, only 34 and 18 women, respectively, have second births. The size of the sample makes it particularly difficult to look for heterogeneity in outcomes within each period.

There is also considerable missing data within the data set that I have addressed in this paper using list-wise deletion. However, the combination of missing data and relatively small sample size does reduce my statistical power.

3.6 Conclusion

The goal of the analysis was to determine the effect of having a second birth at a particular time on women's mid-life economic outcomes to provide insight into whether birth spacing, like birth timing, may have the potential to mitigate the effects of the motherhood penalty. There was only very minimal statistically significant evidence of an average treatment effect on the treated for women with a 4-year interval in the model of log annual earnings. Yet, there were a few instances of statistically significant heterogeneity in effects by education and age at first birth. Thus, heterogeneity by these characteristics warrants further research. In particular, it would be useful to examine the mechanisms that drive differences by education, including whether they result from the different types of jobs women with different education levels hold or something else. This could provide information about whether the penalty arises from lower human capital accumulation or compensating differentials. With regard to age at first birth, it may be illuminating to examine models that treat age at first birth as a categorical variable, rather than a continuous variable, to better understand whether there are patterns

of outcomes that occur through the combination of age at first birth and the length of the birth interval.

It is important to note that even though the main models show only one marginally significant effect, the results do not necessarily indicate that different birth intervals have no effect on economic outcomes, only that for each period there are no statistically significant differences between the women having a second birth and those not having a second birth with regard to measures of *long-term* economic outcomes. The negative effects of certain birth spacing patterns for women's labor force participation found in previous research (Peltola 2004; Troske and Voicu 2009) were measured quite proximate to the second birth. A comparison of those results with the results of this analysis suggests that while perhaps in the short term certain birth spacing patterns may be more or less advantageous for women in the labor force, if such a pattern of advantages and disadvantages exists, it may not persist to affect midlife wages or earnings.

The results of this analysis may be reassuring for women interested in having more than one child. The analysis provides suggestive evidence that on average at any particular time point after a first birth, having a second child versus not having a second child is not associated with long-term negative effects on labor market outcomes. At the least, there is minimal statistically significant evidence, even in the subgroup analyses, that there are detrimental long-term effects. Thus, it may be the case that the transition to the first child is the crucial transition underlying the motherhood penalty and that penalties do not compound with additional children. If so, researchers should focus future work on identifying the precise mechanisms responsible for the motherhood penalty at

the birth of the first child and should also focus on explaining the common finding that the penalty grows with additional children.

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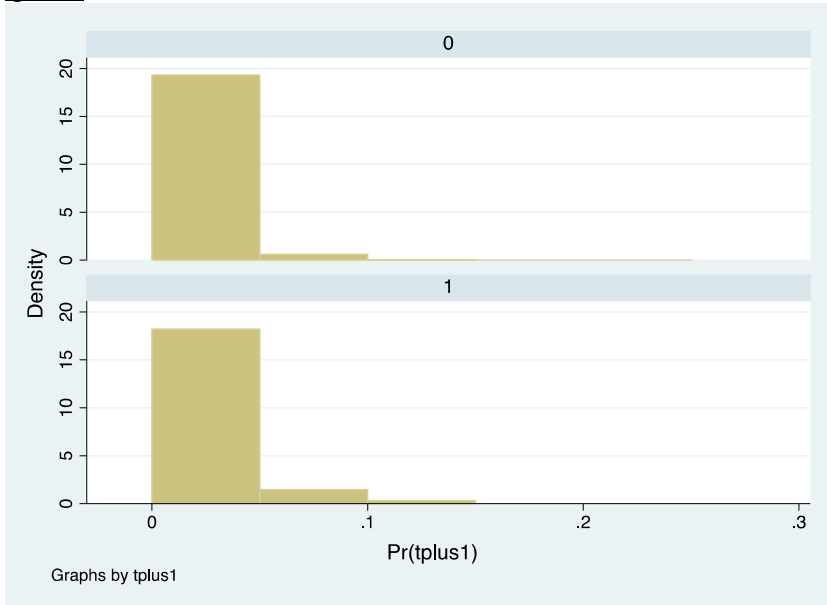
Appendix A
Table A1. Probit Coefficients of Propensity Score Estimation by Year of Second Birth

	1 Year	3 Years	5 Years	7 Years	9 Years
ASVAB factor 1	-0.21 (0.12)	0.02 (0.06)	-0.05 (0.10)	-0.16 (0.12)	-0.21 (0.15)
ASVAB factor 2	-0.13 (0.16)	0.10 (0.06)	0.10 (0.10)	0.01 (0.11)	-0.20 (0.17)
Education (HS is omitted)					
Less than HS	-0.25 (0.22)	-0.01 (0.10)	-0.07 (0.18)	-0.06 (0.21)	0.11 (0.28)
Some college	-0.47 (0.25)	0.15 (0.09)	0.04 (0.14)	0.02 (0.19)	0.24 (0.31)
College	-0.30 (0.22)	0.35 (0.12)**	0.21 (0.19)	0.36 (0.29)	0.96 (0.59)
Marital status (never married is omitted)					
Married	0.20 (0.11)	0.10 (0.06)	0.31 (0.09)***	0.16 (0.13)	-0.07 (0.21)
Separated/Divorced	0.30 (0.15)*	-0.05 (0.11)	-0.20 (0.15)	-0.26 (0.18)	-0.22 (0.17)
Race (white is omitted)					
Black	0.07 (0.23)	-0.30 (0.13)*	-0.32 (0.18)	-0.07 (0.27)	-0.45 (0.36)
Hispanic	-0.13 (0.23)	-0.24 (0.14)	-0.19 (0.20)	-0.24 (0.29)	-0.19 (0.37)
Age at 1st birth	0.002 (0.02)	-0.002 (0.01)	-0.01 (0.01)	-0.04 (0.02)*	-0.16 (0.03)***
Region (Northeast is omitted)					
North Central	0.11 (0.27)	-0.04 (0.12)	0.29 (0.19)	0.34 (0.26)	-0.98 (0.48)*
South	-0.24 (0.25)	-0.12 (0.12)	-0.09 (0.18)	0.09 (0.23)	0.10 (0.31)
West	-0.56 (0.26)*	-0.17 (0.14)	-0.14 (0.21)	0.50 (0.30)	0.09 (0.29)
Number of children wanted	0.03 (0.08)	0.03 (0.03)	0.07 (0.04)	0.04 (0.06)	-0.04 (0.07)
Lagged part-time work status	0.20 (0.08)*	0.16 (0.06)	0.19 (0.07)**	0.06 (0.08)	0.21 (0.17)
Lagged nonworking status	-0.01 (0.06)	-0.05 (0.05)	-0.01 (0.07)	-0.07 (0.09)	-0.28 (0.12)*
Lagged cumulative experience	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.00)
Income	-0.00 (0.003)	0.001 (0.001)	0.001 (0.002)	-0.004 (0.003)	-0.001 (0.002)
Family resources	0.00 (0.001)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Constant	-2.26 (0.49)***	-0.75 (0.29)**	-1.07 (0.42)*	-0.51 (0.50)	2.17 (0.81)**
# eligible women included in estimation	1859	1473	864	629	506

Note: Models also control for missing indicators for lagged part-time status, income, family resources, and lagged cumulative experience. They also include a set of race X marital status interactions. *p<.05; **p<.01; ***p<.001.

Appendix B. Histograms showing distributions of propensity scores by treatment status.

<u>tplus1</u>

Graphs by tplus1

<u>tplus2</u>

Graphs by tplus2

tplus3

Graphs by tplus3

tplus4

Graphs by tplus4

tplus5

Graphs by tplus5

tplus6

Graphs by tplus6

tplus7

Graphs by tplus7

tplus8

Graphs by tplus8

tplus9

Graphs by tplus9
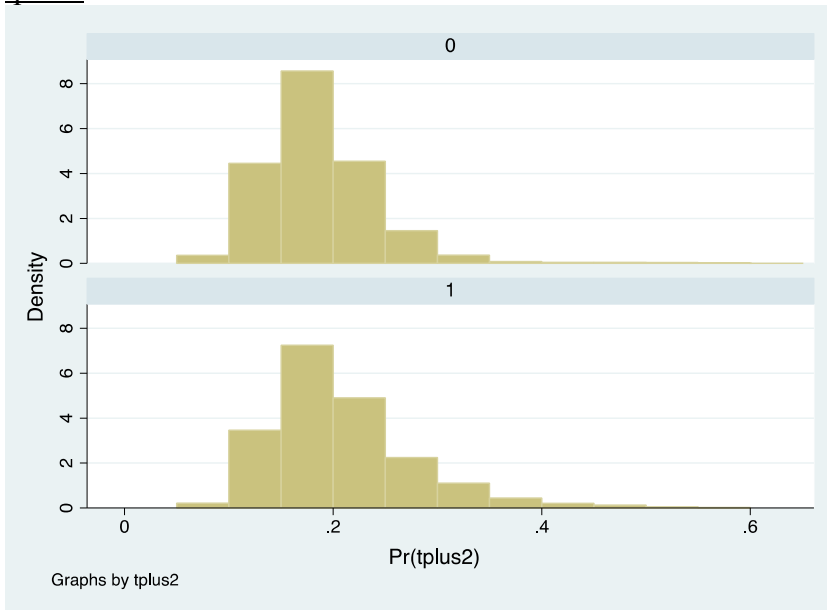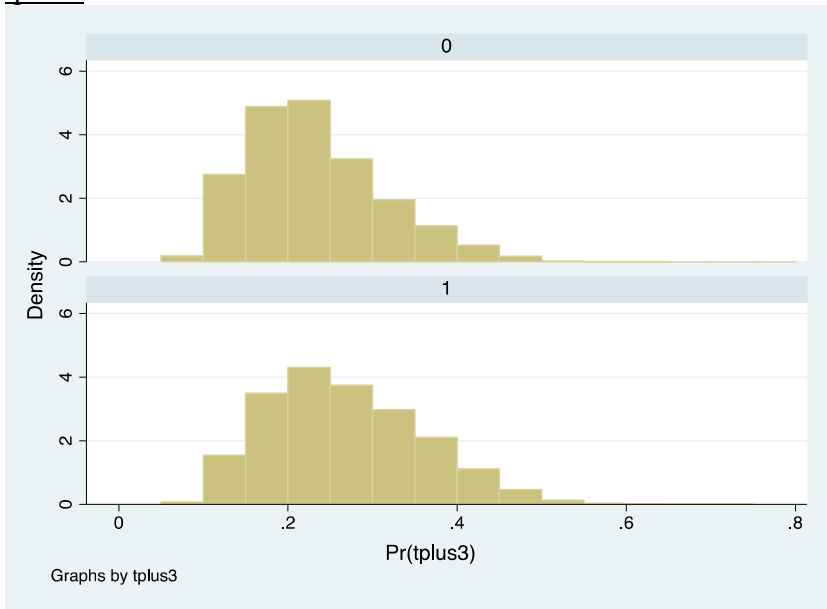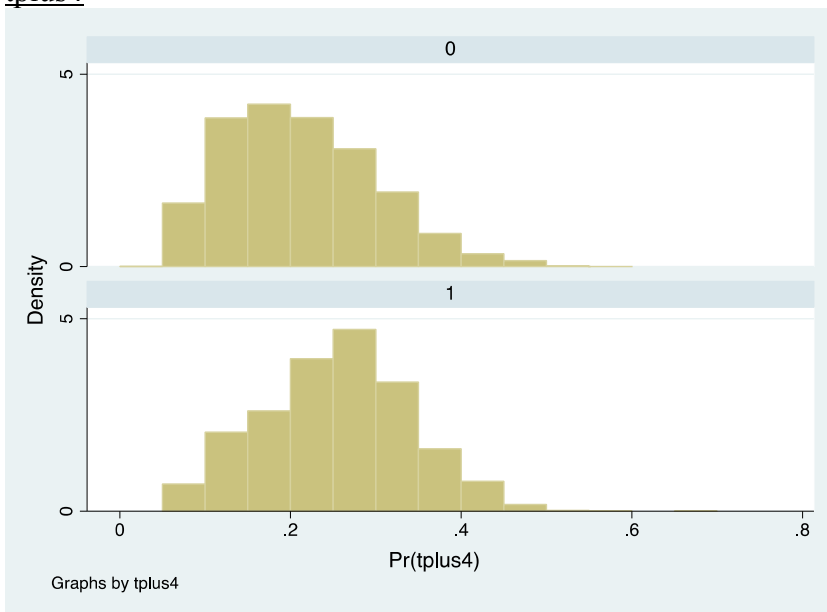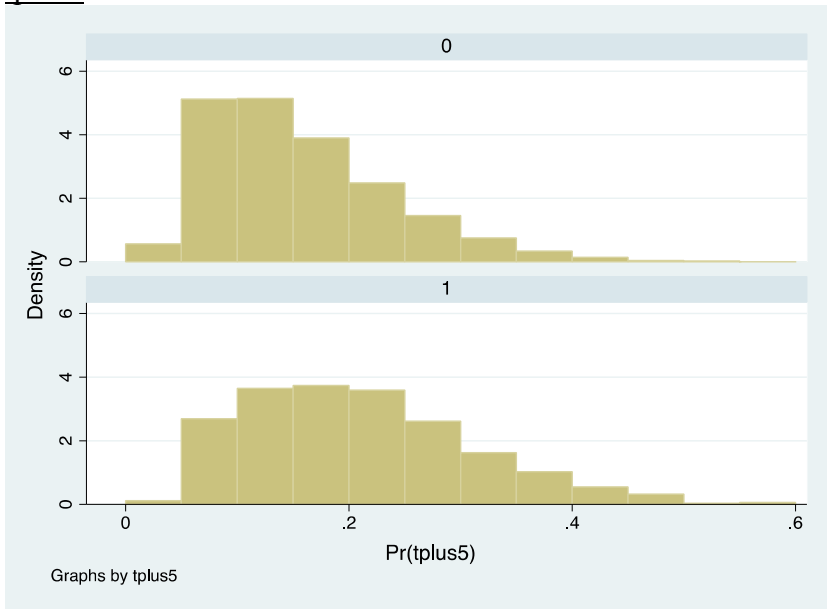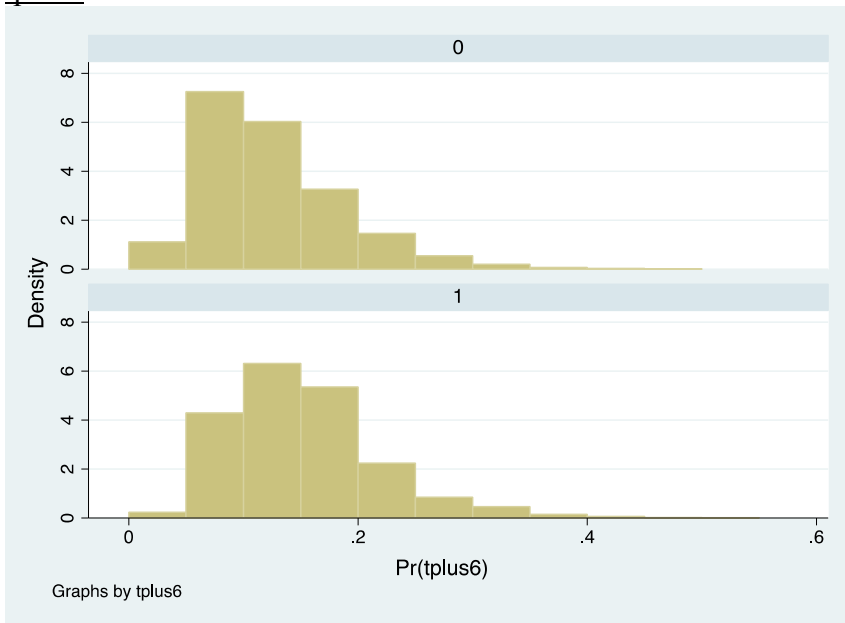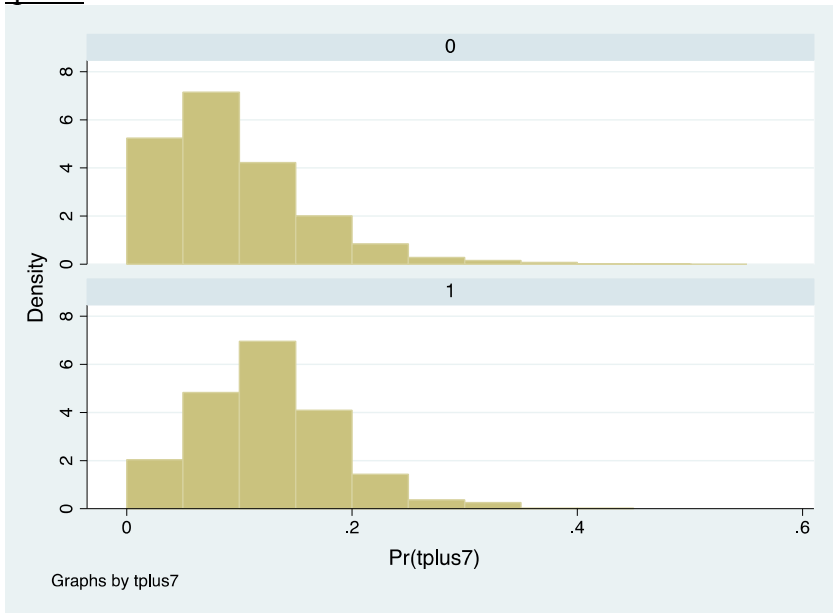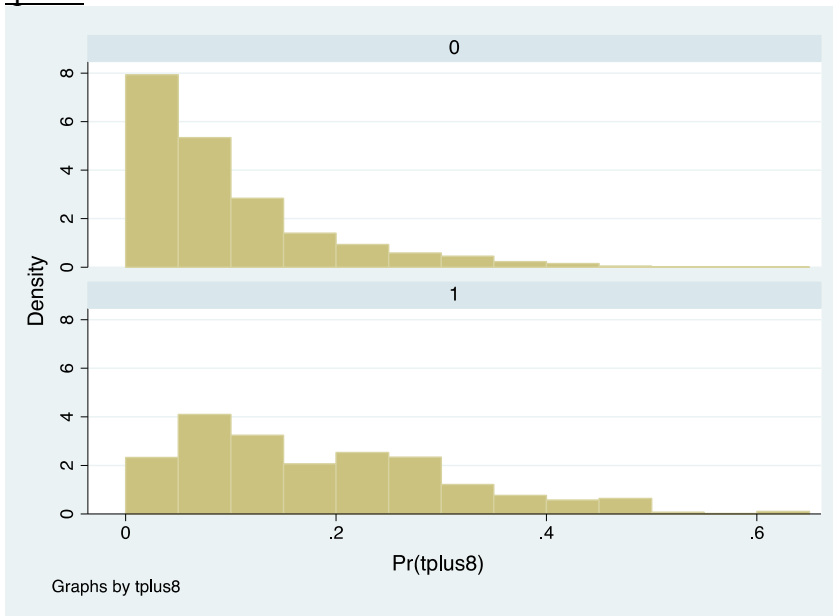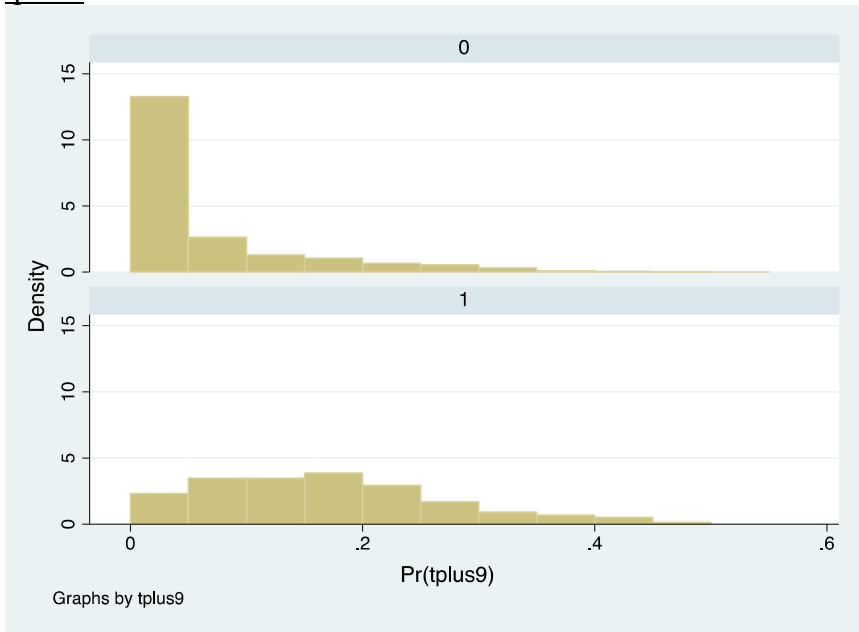
tplus10

Graphs by tplus10
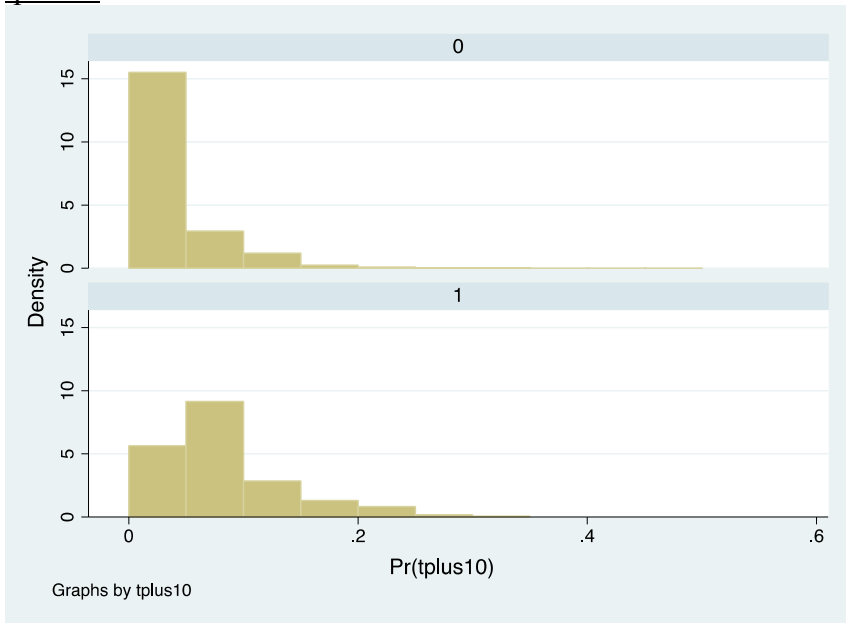
CHAPTER **4**

Do Behavior Problems Mediate the Relationship between Parental Separation and Children's Educational Outcomes?

4.1 Introduction

Although the divorce rate in the United States has fallen in many segments of the

population, it continues to be high (Amato 2010). Furthermore, researchers estimate that

slightly more than half of all divorces involve families with children under the age of 18

(Amato 2000). This is concerning given a plethora of past research indicating that

parental divorce is associated with negative outcomes for children (e.g., Furstenberg and

Cherlin 1991; McLanahan and Sandefur 1994). At the same time as the divorce rate has

remained stable or fallen, the percentage of children at risk of exposure to family

disruption has grown. This is the case because there has been a steady increase in the

percentage of births occurring to unmarried, cohabiting parents over the past three

decades (Bumpass and Lu 2000; Kennedy and Bumpass 2008), and rates of union

dissolution among such couples are relatively high (Osborne, Manning, and Smock

2007).

A large body of literature has demonstrated that family instability can be

detrimental to children's well-being (Amato and Cheadle 2008; Brown 2006; Cavanagh

and Huston 2008; Fomby and Cherlin 2007; Haveman and Wolfe 1995; Heard 2009;

Magnuson and Berger 2009; McLanahan and Percheski 2008; McLanahan and Sandefur

1994; Osborne and McLanahan 2007; Wolfinger, Kowalski-Jones, and Smith 2003).

However, by the turn of the century there were indications that the instability of cohabiting unions was declining--dissolution rates within cohabitations declined substantially between the early 1990s and about the year 2000 (Kennedy and Bumpass 2008). As such, cohabiting unions may be becoming more similar to marriages, not only as a venue for childbearing but also with regard to dissolution patterns.

In response to changes in the family, such as the growing proportion of births to cohabiting couples, research conducted over the past 25 years on the relationship between changes in family structure and children's well-being has evolved significantly, expanding beyond the early focus on parental divorce to examine instability among cohabiting and dating parents, and to consider the role of multiple transitions into and out of coresidential unions. This expansion has been spurred both by the changing demographics of families and made possible by the collection of a number of child-focused longitudinal data sets, including the National Longitudinal Survey of Youth 1979 Children and Youth, the Early Childhood Longitudinal Studies, the Fragile Families and Child Wellbeing Study, and the National Longitudinal Study of Adolescent Health. Research on parental divorce and family transitions has also benefitted from methodological innovation that has allowed researchers to better distinguish between selection effects and causal effects, and to tease out nuances of parental separation (e.g., Kim 2011).

In this paper I contribute to the literature in three ways: First, I am the first to estimate models of parental separation on children's cognitive outcomes (test scores) that explicitly consider behavior (non-cognitive skills) as a potential mediating variable. I study test scores as a measure of cognitive skill because test scores are considered quite

malleable. Furthermore, they can be affected by non-cognitive skills like behavior. Additionally, IQ is generally set by age 10, if not sooner, but non-cognitive skills are still developing at that age and may influence test scores (Carneiro, Crawford, and Goodman 2007; Carneiro and Heckman 2003; Heckman 2000). Finally, it is important to study cognitive skills because they play a large role in determining socioeconomic status in adulthood (Carneiro et al. 2007; Heckman, Stixrud and Urzua 2006).

Second, I include children from two age groups to see whether the effects of separation vary by developmental stage. Third, I include the separation of cohabiting parents along with that of married parents for the younger cohort, rather than limiting separation to marital dissolution. This allows me to capture a greater number of children experiencing the dissolution of their parents' relationship, given the larger portion of children in this cohort born into cohabiting unions.

In the remainder of this paper I discuss family instability theories and past empirical evidence. I then discuss the ways in which this study is a departure from past research and can provide valuable insights. I follow with a discussion of data and methods. I then present the results of the analysis and discuss their implications.

4.2 Theoretical Framework

I draw on three theories that are often used to explain the association between parental divorce and children's outcomes. The first theory is resource theory. Resource theory covers a variety of resources, but most often researchers focus on financial resources. Becker (1981) and others (e.g., Bergstrom 1997) have argued that greater financial resources should improve child well-being because these resources allow parents to

purchase high-quality goods, such as education and extracurricular activities. They also

may reduce stress within the family (McLoyd 1990). However, once parents separate,

there is a reduction in financial resources within the child's household. This is because

children usually live with their mothers, and women generally have lower hourly wages

than men (McLanahan and Percheski 2008). Thus, researchers have theorized that the

loss of resources accompanying parental separation may have negative effects on child

well-being. For example, Amato (1998) notes that when nonresident fathers pay child

support, children seem to benefit. He theorizes that the underlying mechanisms for this

relationship are the same as in two-parent families: the financial resources directly

improve child health and nutrition, improve the home environment, and improve

children's access to educational resources; they also indirectly lower the level of stress

experienced by the mother.

In addition to a loss of financial resources, children may also experience a loss of

social and community resources (McLanahan and Sandefur 1994). This branch of

resource theory posits that parental separation weakens the connections between children

and their nonresident parent (usually the father), which reduces the nonresident parent's

willingness to invest in the child. The separation is also expected to weaken a child's ties

to other adults and institutions within the community, either through loss of connection

with the nonresident parent, or through residential mobility necessitated by the separation

(McLanahan and Sandefur 1994). The loss of social and community resources is thought

to act in part by creating trust issues with children and uncertainty about the future.

Additionally, it is thought that the loss of these resources may reduce children's

willingness to invest in themselves by lowering their expectations and reducing

motivation (McLanahan and Sandefur 1994).

The implications for my study are two-fold. First, a reduction in financial

resources may lead to lower test scores because the child's household no longer has

sufficient income to provide high-quality educational resources. Second, a loss of social

and community resources may lead to lower test scores if children become less motivated

to perform well in the classroom.

The second theoretical perspective is stress theory. This theory is closely linked to

the third theory, which is parenting theory. As research relating to parental divorce has

grown to encompass more types of family instability and to consider the frequency of

family structure changes, the stress perspective has gained dominance (Cavanagh and

Huston 2006; Fomby and Cherlin 2007; Heard 2009; Osborne and McLanahan 2007).

The general idea is that changes in family relationships lead to disruptions in resources

and routines, and these disruptions interfere with the mother's psychological functioning

and relationship with her child. This interference consequently affects child well-being

(Osborne and McLanahan 2007). The theory also considers the possibility that

experiencing a stressful event such as parental separation may negatively affect child

development (Haveman and Wolfe 1995).

The implications for test scores are indirect. First, stress may negatively affect

parenting, and this could affect child behavior (McLanahan and Sandefur 1994). Second,

the stress of divorce might directly affect the mental health of children, increasing

internalizing behaviors like depression and anxiety, and externalizing behaviors such as

acting out (Kelly 2003). These behavioral changes could lead to direct negative effects on

test scores. Therefore, stress theory implies that behavior changes may be an intervening mechanism in the relationship between parental separation and children's test scores.

The third theory is sometimes referred to as parenting theory. It suggests that stressful life experiences, like those associated with divorce, lead to parental strain and distress, which is then reflected in problems such as parental depression, anxiety, antisocial behavior, and health problems (Hetherington, Bridges, and Insabella 1998; Osborne and McLanahan 2007). The distress experienced by parents can reduce their ability to parent their children. It is difficult to consider parenting theory separately from resource theory and stress theory because both resources and stress are theorized to affect parenting behaviors (McLanahan and Percheski 2008). Because parental separation may increase the need for mothers to work longer hours in the labor market, they may have less time or energy to devote to childrearing. Furthermore, their parenting abilities might be further impacted by high levels of stress as they attempt to fill the roles of breadwinner and parent simultaneously (McLanahan and Sandefur 1994; Sigle-Rushton and McLanahan 2004). These difficulties may lead to inconsistent parenting, which may lead parents to become overly permissive or overly authoritarian, neither of which are ideal strategies for childrearing (McLanahan and Sandefur 1994). For example, a reduced ability to parent may lead the resident parent to monitor the children less closely, which could result in children developing problem behaviors (Hill, Yeung, and Duncan 2001).

For the purpose of studying test scores, the implication of parenting theory is nearly identical to those of stress theory. A reduced ability to parent might have negative effects on children's behavior and thereby indirectly affect children's test scores.

These three theories suggest ways in which separation might affect younger and older children differently. Cavanagh and Huston (2008) theorize that family instability may be especially salient in early childhood because young children may be more sensitive to parents' distress and disruptions in the home environment. Furthermore, the stress parents face during this period may leave them unable to provide the level of support young children need. They suggest that early childhood instability will impact child development in ways that affect later social relationships and interpersonal skills. Consequently such children may be lonelier and less able to navigate peer relationships (Cavanagh and Huston 2008). Kelly and Lamb (2000) posit that very young children may be hurt most by divorce because they lack maturity in the cognitive, emotional, and language realms that is necessary for understanding and coping with the changes occurring in their relationships with their parents. Alternatively, if a loss of resources affects children's willingness to invest in themselves, we would expect such changes to be limited to older children. Thus, the importance of the timing of a parental separation for negative educational outcomes depends on the mechanism that underlies poor performance.

The theories also suggest reasons that children of cohabiting parents that separate might have different outcomes than children of married parents that divorce. On the one hand, children of cohabiting parents could experience more negative outcomes because their families may already have lower economic resources prior to the separation (Brown 2004). In addition, these families may have lower levels of social and community support to draw on than married families (Harknett and Knab 2007). On the other hand, if financial resources are the key mechanism in the relationship between separation and

children's outcomes, children of cohabiting parents may be no worse off than children of married parents after a separation because cohabiting parents often do not pool resources to the same extent as married parents (Kenney 2004). As such, there may be a less drastic change in the standard of living for mothers and children after the separation.

4.3 Evidence from Past Research

To support the causal theories for the association between parental separation and children's outcomes described above, empirical research must demonstrate that separation causes changes to children's outcomes. In other words, the relationship between separation and children's outcomes cannot be entirely explained by parents' pre-existing personality characteristics and cognitive abilities. A large number of researchers have sought to provide evidence for these theories.

In their highly influential study, McLanahan and Sandefur (1994) found that growing up with a single parent increased the risk of school failure, delinquency and teenage childbearing. With regard to resources, they found that loss of income could explain as much as half of the disadvantage of living with a single parent. In addition, children in single-parent families spent less time with both fathers and mothers than children in two-parent families, and they lived in communities with fewer resources. With regard to parenting, they found that single mothers exercised less control over their children than mothers in two-parent families, and that parenting practices accounted for the majority of the difference in high school dropout between children in single-parent and two-parent families. Their results suggest important roles for resource theory, stress

theory, and parenting theory in explaining the negative outcomes of children in divorced families.

A number of more recent studies have indicated that parental separation, and sometimes other family transitions, exacerbate children's already existing behavioral problems or cause new behavioral problems (Amato and Cheadle 2008; Fomby and Cherlin 2007; Hao and Xie 2002; Magnuson and Berger 2009; Ram and Hou 2003). There is also evidence that parental separation reduces educational attainment and cognitive scores (Heard 2009; Ram and Hou 2003; Wolfinger et al. 2003; McLanahan and Percheski 2008). These studies generally attribute part of the negative effects to selection and part to reduced resources. Researchers have also found evidence that poor parenting and parental stress arising from a separation, increases behavior problems (Osborne 2007; Osborne and McLanahan 2007). Thus, as in McLanahan and Sandefur (1994), recent research provides evidence for all three theories.

Evidence is mixed with regard to the ways in which the timing of the transition and the theories interact, and with regard to differences between the separations of cohabiting and married parents. Cavanagh and Huston (2008) found evidence supporting the theory that younger children would be harmed more by parental separation than older children for reasons that are most consistent with stress theory. Children experiencing family instability at younger ages reported being lonelier, were less competent with their peers, and were somewhat more prone to externalizing behavior problems. Conversely, family instability occurring in middle childhood did not have these effects. Similarly, Heard (2009) found that only early childhood transitions had significant negative effects on cumulative GPA when measured at the end of high school, not transitions in middle

childhood. Yet Hao and Xie (2002) find negative effects on behavior with instability occurring in middle childhood, and Magnuson and Berger (2009) also find some behavior effects though little difference in achievement.

With regard to cohabitation, the body of literature is fairly limited (Amato 2010), but researchers have found evidence that children born to cohabiting parents are much more likely to experience a parental separation than children born to married parents (Osborne et al. 2007). However, how similar these two different types of separations are is a bit murky. Examining children's asthma-related emergencies Harknett (2009) found that there was a 4-fold increase the odds of an emergency for children whose parents experienced a marital disruption compared to children with stably married parents. Yet there was no effect for the dissolution of cohabiting unions. Although health-related outcomes may have different antecedents than cognitive and non-cognitive outcomes, the study suggests a need to be cautious when examining different types of parental separation.

The existing research has also seen a number of methodological advances in recent years, which have allowed for better estimation of causal effects. A recent paper by Kim (2011) advances the literature by integrating the divorce literature with the emerging literature on the effects of events and interventions on cognitive and non-cognitive outcomes. The work provides an important starting point for thinking about causal pathways, including how divorce might affect children's non-cognitive outcomes (interpersonal social skills, externalizing and internalizing behaviors) as well as cognitive outcomes (math and reading scores), and how changes in non-cognitive outcomes might impact later achievement. He estimates models using stage-specific ordinary least squares

(OLS), a counterfactual matching estimator, and a piece-wise growth curve model in an effort to obtain causal estimates. Including controls for lagged outcome variables in his models, Kim (2011) finds some negative effects of parental divorce on math scores during and after the divorce period but not on reading scores. In addition, he found a negative effect of divorce on interpersonal skills during and after divorce, and a positive effect of divorce on internalizing behavior problems during the divorce period. One challenge in interpreting these results is that the controls for lagged outcomes may obscure direct effects of divorce on children's test scores because divorce is a process (Furstenberg and Cherlin 1991), and it is difficult to pinpoint the starting time of the events and conditions within the family that lead up to the legal event.

4.4 The Present Study

I depart from previous research in this area in three ways. First, I contribute to the literature by focusing on the outcomes of children experiencing a parental separation before the age of 5 using data from the Early Childhood Longitudinal Study-Birth cohort (ECLS-B), along with the outcomes for school-age children (using Kim's (2011) Early Childhood Longitudinal Study-Kindergarten cohort data set). This is important for two reasons: Young children are relatively understudied in this literature, and children under 5 are in a developmental stage where both cognitive (i.e., IQ) and non-cognitive skills are still rapidly developing. Therefore, effects of parental separation on cognitive outcomes may be more evident in younger children than older children who have more developed cognitive skills and more exposure to learning in a classroom setting, not only the family setting.

Second, for the sample of young children, I examine the separations of both biological cohabiting parents and biological married parents. Even though the proportion of children being born into cohabiting families is growing, much of the literature on family transitions has focused on either divorce of married parents or transitioning from a single-parent to two-parent family (Amato 2010; Amato and Cheadle 2008; Aughinbaugh, Pierret, and Rothstein 2005; Burnett and Farkas 2009; Furstenberg and Cherlin 1991; Harknett 2009, Kim 2011; Magnuson and Berger 2009; Potter 2010; Wolfinger et al. 2003; but see Cavanagh and Huston 2006; Cooper, Osborne, Beck and McLanahan 2008). By estimating models that include both groups of parents, along with models separated by parental marital status, I can shed additional light on the similarities and differences between cohabiting and married parents in terms of the consequences of relationship dissolution for children.

Finally, I extend the work of Kim (2011) and estimate models of parental separation on children's cognitive outcomes, measured using math and reading test scores, while explicitly considering the role behavior may play as a potential mediating variable in the relationship between separation and later test scores.

The outcomes typically studied in research on parental separation can be mapped to the literature on cognitive and non-cognitive skills or traits. This link was made explicit in Kim (2011). Test scores and IQ are both measures of cognitive skill. Non-cognitive skills can take a number of forms, from specific behaviors to interpersonal skills. Test scores have been shown to be quite malleable, although IQ is generally set between ages 8 and 10. At that age, non-cognitive skills are more malleable than cognitive skills (Carneiro et al. 2007; Carneiro and Heckman 2003; Heckman 2000). This

is partly because test scores are affected by both family background and schooling. Research on the effect of non-cognitive and cognitive skills on later life economic outcomes has indicated that non-cognitive skills, such as behavior, affect later cognitive skills as measured by test scores and educational achievement (Jacob 2002). Non-cognitive skills also play a large role in determining socioeconomic success in adulthood, together with cognitive skills (Carneiro et al. 2007; Heckman et al. 2006). Finally, low-quality parenting disadvantages children in terms of non-cognitive skill development, and although non-cognitive skills continue to develop as children age, early interventions to produce non-cognitive skills in disadvantaged children are most successful (Carneiro and Heckman 2003; Heckman 2000). To explicitly relate this to parental separation, I suggest that parental separation at all ages may have a negative effect on the development of non-cognitive skills. Furthermore, if parental separation reduces the quality of parenting as the stress and parenting theories suggest, the negative effect of the separation on the development of non-cognitive skills may persist long after the event itself. Finally, given that non-cognitive skills have been shown to affect later educational outcomes, less developed non-cognitive skills (as could result from separation) are likely to be associated with lower test scores. For these reasons, I estimate models of the association between parental separation and test scores with and without controls for non-cognitive skills. I test three hypotheses:

> Hypothesis 1: Parental separation is negatively associated with math and
> reading test scores.
>
> Hypothesis 2: Demographic and family characteristics explain some, but
> not all, of the negative association between separation and test scores.

Hypothesis 3: Parental separation has no direct negative association with test scores after controlling for non-cognitive skills. In other words, parental separation will be associated with lower non-cognitive skills, and after accounting for these skills, there will no longer be any significant association between separation and test scores.

4.5 Data and Method

The data for this study come from two sources: the Early Childhood Longitudinal Study Birth cohort (ECLS-B) and the Early Childhood Longitudinal Study Kindergarten cohort (ECLS-K), both of which are collected by the National Center for Education Statistics (NCES). I use the ECLS-B for the analysis of young children's experiences of parental separation. The data set is the best available for answering questions about whether behavior mediates the relationship between parental separation and children's test scores. The sample is nationally representative of the 2001 birth cohort in the United States, and the survey has a longitudinal design, which allows for examining events within their temporal ordering. The sample is also large, which allows for comparison of children in disrupted families (both cohabiting and married families) with children in intact families even within demographic subgroups. Furthermore, the data set contains measures of math and reading test scores collected at preschool and kindergarten, and teacher reports of positive and negative behaviors, which previous research has shown to be quite useful in understanding children's behavior (Verhulst, Koot, and Van der Ende 1994). Children in this sample were followed from birth to kindergarten.

I use the ECLS-K for the analysis of older children's experiences. As with ECLS-B, this data set is arguably the best available for answering the questions at hand. The data set followed kindergartners in the 1998-1999 class through the 8th grade. It is nationally representative, and it uses a multistage probability design. Test score and behavior measures are available at all waves of the survey. As with ECLS-B, the behavior measures come from teacher reports. This is the data set used by Kim (2011), and for the purpose of my analysis, I replicate his restrictions exactly so that the two samples are comparable. However, in my analysis I use fewer variables than Kim to preserve comparability with my ECLS-B analysis.

The ECLS-K sample restrictions include using list-wise deletion for missing values on variables included in the analysis and omitting the 1st grade fall survey because it was administered to only a subsample of children. In addition, I omit the 8th grade survey, since it was not available for Kim's analysis. Given the periods covered by this edited data set, I am able to analyze parental divorces that occurred between the spring of 1st grade and the spring of 3rd grade, with outcomes measured in the spring of 5th grade. See Kim (2011) for further details. The final sample size for the ECLS-K is 3,585.

My construction of the analytic data set for the ECLS-B is similar, although I include separations of biological cohabiting parents along with separations of biological married parents in my analysis. Both groups are examined separately in a set of specification checks. I make this adjustment for a few reasons. First, by 2001 when the ECLS-B cohort was born, childbearing within the context of cohabitation was already quite common (Kennedy and Bumpass 2008). Second, parental divorce at young ages is considered relatively rare, but other research has suggested that dissolution of cohabiting

unions when children are young is more common (Osborne et al. 2007). Third, including

children born to cohabiting parents widens the socioeconomic base of the analytic

sample, as less socioeconomically advantaged parents are more likely to bear children

within the context of cohabitation than within the context of marriage (Manning and

Brown 2006). For the ECLS-B sample, I continue with a list-wise deletion approach to

missing data, and I limit the sample to children living with two biological parents

(married or unmarried) at the time of the first survey, which was conducted when

children were about 9 months of age. Parental separation could occur at any point

between the first survey and the third survey, which was conducted at preschool age.

Outcomes are measured at kindergarten. The control variables are measured at the first

wave of data collection when the household was still intact. I further limit the sample to

children for whom at least one survey for the mother and one survey for the father exist.

My final sample size for the ECLS-B ranges from 2,040 to 3,069 depending on the

covariates used (particularly preschool test scores and behavior scores).

4.5.1 Analytic Strategy

     I estimate ordinary least squares (OLS) models. In Model 1 I estimate a reduced-

form model of the effect of parental separation on children's test scores. In Model 2 I add

controls for standard demographic characteristics, measured at baseline. These variables

include age in months, gender, race/ethnicity, socioeconomic status (composite variable),

child disability (ECLS-K) or child's poor health (ECLS-B), urbanicity, and region of

residence. Model 3 includes additional controls for family characteristics, including a

categorical variable for the number of siblings, the mother's happiness in her marital or

cohabiting relationship with the biological father, and an index of the mother's

psychological well-being. In Model 4, I add controls for lagged behavior. These behavior

scores are measured more proximate to the parental separation. In the ECLS-K these

measures are taken from the 3rd grade survey, while in ECLS-B they are taken from the

preschool survey. In Model 5, I substitute lagged test scores, consistent with Kim's

(2011) models, for the lagged behavior scores. Finally, in Model 6, I include both the

lagged behavior scores and lagged test scores. I estimated both unweighted and weighted

models, consistent with Kim (2011)[10]. The weighted models are estimated using a paired

jackknife procedure (Tourangeau et al. 2006) that employs the 90 replicate weights

provided in each of the data sets. This weighting procedure follows the recommendations

of the two surveys' user guides, and it accounts for the complex survey design. The

weights are used to account for attrition from the sample.

The main outcome variables are normalized Item Response Test math and reading

scale scores. In the ECLS-K models the lagged behavior measures are scales of

interpersonal social skills, externalizing behavior, and internalizing behavior.

Externalizing behaviors are usually described as aggressive behaviors, while internalizing

behaviors are described as anxious/depressive behaviors. For more about the ECLS-K

variable construction see Kim (2011). The range of the variables is from 0 to 3, with 3

being the greatest amount. In the ECLS-B models the behavior measures are scales of

good behavior and negative behavior. The scale of good behavior is constructed using

measures of kindergarten teacher reports of the child's ability to make friends, pay

attention, and share with others in the classroom. This scale maps most closely to the

interpersonal social skills scale in the ECLS-K. The scale of negative behavior is

---

[10] Results from unweighted models are not shown but are available from the author on request.

constructed using measures of kindergarten teacher reports of how often the child is impulsive, disruptive, overly active, has trouble concentrating, is restless, throws tantrums, behaves aggressively, and annoys others in the classroom. This scale maps most closely to the externalizing behavior scale in the ECLS-K.

As indicated, I control for child gender, child race, socioeconomic status, child disability or poor child health, region, and urbanicity in the models. Child gender is a dummy variable with a value of 1 for female and 0 for male. Child race is a categorical variable where the omitted race category is white. Additionally included are race categories of black, Hispanic, and "other," where "other" is a combination of Asian/Pacific Islander, American Indian/Native Alaskan, and multiracial. Socioeconomic status is a constructed index variable. Child disability and poor child health are dummy variables. Additionally, region is a categorical variable with the omitted category of Northeast. The additional region categories are Midwest, South, and West. Finally, urbanicity is a categorical variable with an omitted category of city. The other possible categories are living in a large town and living in a small town.

With regard to family characteristics, the number of siblings variable is a categorical variable with zero siblings as the omitted category. The variable takes a value of 1 for one sibling, 2 for two siblings, and 3 for three or more siblings. The mother's happiness with her relationship is a categorical variable with the categories of "not too happy" (omitted), "fairly happy," and "very happy," where "very happy" takes the highest value. Finally, the mother's psychological well-being is measured using items from the depression instrument and has been rescaled to take a value between 0 and 3, with a value of 3 indicating the worst psychological well-being.

4.6 Results

Descriptive statistics are provided in Appendix A. Weighted descriptive statistics using

ECLS-K suggest that about 6% of the sample experiences parental divorce. The mean

math score at 5th grade is 98, and the mean reading score is 126. The mean interpersonal

social skills score is 2.19. The mean externalizing behavior score is 0.61, and the mean

internalizing behavior score is 0.58. About 29% of the sample lives in a city, while 45%

live in large towns, and 26 % live in small towns. Nearly 50% of the sample has one

sibling at baseline, and another 30% have two siblings. Mean socioeconomic status is

0.28. About 77% of mothers reported being "very happy" in their relationship with the

child's biological father at baseline. Finally, about 77% of the sample is white, 6% is

black, 11% is Hispanic, and 6% holds some other racial designation.

Weighted descriptive statistics using ECLS-B suggest that almost 13% of children

experience the separation of their parents by preschool. About 71% of these separations

are parental divorces, and the other 29% are separations of cohabiting biological parents.

The mean math score at kindergarten is 43, and the mean reading score is 42. The mean

of good behavior is 11.5. The mean of negative behavior is 6. Mean socioeconomic status

in quintiles is 3.6. About 37% have one sibling at baseline, and 16% have two siblings.

About 9% of the children in the sample have parents who are cohabiting at the first wave,

as opposed to married. About 62% of the sample lives in a city, while 11% live in large

towns and 16% live in small towns. About 79% of mothers reported being "very happy"

in their relationship with the child's biological father at baseline. Finally, about 61% of

the sample is white, 6% is black, 16% is Hispanic, and 7% holds another racial

designation.

In the weighted OLS models estimating the association between parental

separation and math scores using the ECLS-K, I find a statistically significant negative

association between divorce and math scores in Model 1 (-6.87, p<.001). This is

consistent with Hypothesis 1. The results in Table 4.1 indicate that this association no

longer persists after the addition of the demographic characteristics in Model 2. The

characteristics that appear most important in explaining the negative association between

divorce and math scores include age, gender, being black, socioeconomic status, and

disability. The magnitudes of the coefficients on black and socioeconomic status are

particularly large (-10.94, p<.01 and 8.42, p<.001, respectively). This suggests that

Hypothesis 2 is partially supported. Demographic characteristics explain some of the

association between divorce and math scores, but without adding even the family

characteristics, there is no longer any statistically significant association. That is, contrary

to Hypothesis 2, after the addition of demographic variables, there is no statistically

significant unexplained association between parental separation and math scores. In

subsequent models family characteristics and baseline measures of test scores and

behavior are associated with math scores, but they do not statistically significantly

change the association between divorce and math scores. Thus, there is no residual

association to explain that would be required to test Hypothesis 3 for math scores.

Table 4.1. Estimated Effect of Divorce on ECLS-K Math Scores

Math Scores	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Divorce	-6.87 (1.93)***	-2.49 (1.95)	-1.98 (2.10)	-2.30 (2.34)	1.20 (1.68)	0.95 (1.74)
Age in months at 1st grade		0.67 (0.14)***	0.70 (0.13)***	0.64 (0.13)***	-0.24 (0.09)**	-0.24 (0.09)**
Gender (Male is omitted)						
Female		-5.52 (1.03)***	-5.58 (0.98)***	-6.65 (1.01)***	-4.62 (0.70)***	-5.01 (0.74)***
Race (White is omitted)						
Black		-10.94 (3.04)***	-10.03 (3.00)***	-10.19 (2.92)***	-4.14 (2.12)*	-4.37 (2.10)*
Hispanic		-3.83 (2.18)*	-3.46 (2.08)	-3.65 (2.10)*	0.22 (1.57)	0.07 (1.60)
Other		-4.16 (2.41)*	-4.19 (2.43)*	-3.64 (2.40)	-3.17 (1.53)*	-2.97 (1.54)*
Baseline SES		8.42 (0.68)***	7.53 (0.78)***	7.11 (0.77)***	2.67 (0.69)***	2.60 (0.66)***
Disability		-6.01 (2.02)**	-5.31 (1.89)**	-4.62 (1.82)**	-1.47 (1.37)	-1.28 (1.34)
Urbanicity (city is omitted)						
Large town		-0.41 (1.27)	-0.42 (1.28)	-0.34 (1.25)	-0.93 (1.04)	-0.87 (1.02)
Small town		-3.38 (1.60)*	-2.91 (1.61)*	-2.71 (1.68)	-1.22 (1.21)	-1.19 (1.22)
Region (Northeast is omitted)						
Midwest		0.81 (1.50)	1.43 (1.49)	2.18 (1.54)	0.50 (1.19)	0.78 (1.18)
South		0.33 (1.55)	0.81 (1.55)	1.26 (1.61)	0.50 (1.33)	0.66 (1.33)
West		-1.84 (2.18)	-0.91 (2.17)	-0.58 (2.23)	-1.25 (1.60)	-1.10 (1.60)
Siblings (zero is omitted)						
One			-1.32 (1.69)	-1.39 (1.71)	0.13 (1.21)	0.14 (1.22)
Two			-3.41 (1.67)*	-3.36 (1.65)*	-0.75 (1.19)	-0.69 (1.21)
Three or more			-3.34 (2.09)	-3.21 (2.09)	-0.40 (1.54)	-0.28 (1.54)
Marital birth			-0.08 (2.30)	-0.48 (2.17)	0.73 (1.66)	0.59 (1.62)
Mother's happiness with marriage (not too happy is omitted)						
Fairly happy			-2.80 (2.86)	-3.81 (2.78)	-2.49 (2.45)	-2.92 (2.48)
Very happy			-2.90 (2.51)	-4.50 (2.52)*	-2.39 (2.28)	-3.06 (2.32)
Mother's psychological well-being			-2.07 (1.35)	-1.89 (1.27)	-0.83 (1.10)	-0.78 (1.05)
Teenage mother			-4.72 (1.93)**	-4.44 (1.91)*	-3.32 (1.23)**	-3.28 (1.22)**
Interpersonal behavior at 3rd grade				3.78 (1.27)**		1.94 (0.89)*
Internalizing behavior at 3rd grade				-5.45 (1.08)***		-2.08 (0.65)**
Externalizing behavior at 3rd grade				0.65 (1.23)		0.87 (0.94)
Baseline math score					1.11 (0.03)***	1.09 (0.03)***
Baseline reading score					0.09 (0.04)*	0.08 (0.04)*
Constant	98.55 (0.71)***	43.26 (11.99)***	46.76 (11.90)***	48.23 (12.29)***	80.31 (8.73)***	78.66 (8.40)***
R^2	0.01	0.18	0.19	0.22	0.55	0.55
N	3585	3585	3585	3585	3585	3585

Note: Tests are one-sided. * p<.05; ** p<.01; *** p<.001.

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To compare these results with Kim's (2011) total effect results it is necessary to look at Model 6, which most closely mimics his models. Contrary to Kim's findings, Model 6 indicates a positive non-statistically significant coefficient on divorce for the model of math scores. Kim found a negative and non-significant coefficient.

In Table 4.2, I show the results of the weighted ECLS-K regressions of the association between divorce and reading scores. I find a marginally statistically significant (-5.59, p<.10) negative association between divorce and reading scores. This provides weak evidence for Hypothesis 1. After the addition of demographic characteristics in Model 2 there is no longer even a marginally statistically significant negative association between divorce and reading scores. Thus, as in the math score models, there is partial evidence for Hypothesis 2. Models 3-6 indicate that family characteristics are associated with reading scores, as are baseline measures of test scores and behavior, but they do not statistically significantly affect the association between divorce and reading scores. Since there was no statistically significant association between parental separation and reading scores to explain after adding demographic characteristics, Hypothesis 3 is irrelevant.

These results are consistent with Kim's (2011) non-statistically significant total effect findings in the models of reading scores. However, Kim found a negative and non-significant coefficient, whereas in Model 6 I find a positive, non-significant result.

Turning to the results of the ECLS-B analysis of parental separation and math scores in Table 4.3, the results are similar to those in the ECLS-K analysis. In Model 1 there is a statistically significant negative association between separation and math scores at kindergarten (-1.73, p<.05). This is consistent with Hypothesis 1. With the addition of

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demographic characteristics in Model 2 there is no longer a statistically significant negative association between parental separation and math scores. Thus, as with ECLS-K there is partial support for Hypothesis 2 because demographic characteristics reduced the negative association to non-significance. The strongest demographic predictor in Model 2 is socioeconomic status. Models 3-6 indicate that family characteristics and baseline measures of test scores and behavior are statistically significantly associated with math scores, but they do not statistically significantly affect the association between parental separation and math scores. As with the ECLS-K models, there is no residual association with which to test Hypothesis 3.

In Table 4.4, I show the comparable results for the ECLS-B models of parent separation and reading test scores. I find no statistically significant association between separation and reading scores in the reduced-form model. Thus, I find no support for Hypothesis 1. Models 2-6 indicate that demographic and family characteristics, and baseline behavior and test scores are statistically significantly associated with reading scores, but they do not statistically significantly change the association between separation and reading scores.

Because the ECLS-B models include separations of both cohabiting and married parents, I conducted a sensitivity analysis to examine whether the associations between separation and test scores differed by type of relationship. The results (not shown) suggest that any negative associations between parental separation and math and reading scores in families with cohabiting parents can be explained by demographic characteristics. Only in the reduced-form models (Model 1) are the associations statistically significant. The

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Table 4.2. Estimated Effect of Divorce on ECLS-K Reading Scores

Reading Scores	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Divorce	-5.59 (3.55)+	-1.66 (3.29)	-0.82 (3.35)	-1.03 (3.55)	2.05 (3.23)	1.86 (3.28)
Age in months at 1st grade		0.89 (0.16)***	0.92 (0.15)***	0.84 (0.15)***	0.10 (0.14)	0.09 (0.14)
Gender (Male is omitted)						
Female		4.40 (1.01)***	4.30 (1.01)***	2.81 (1.04)**	4.38 (0.84)***	3.46 (0.90)***
Race (White is omitted)						
Black		-5.44 (3.02)*	-4.60 (2.94)	-4.59 (2.94)	-0.36 (2.40)	-0.50 (2.42)
Hispanic		-1.77 (2.92)	-1.37 (2.83)	-1.53 (2.81)	1.38 (2.35)	1.19 (2.35)
Other		-0.38 (2.87)	-0.40 (2.87)	0.21 (2.88)	-0.29 (2.34)	-0.02 (2.39)
Baseline SES		10.20 (0.75)***	9.21 (0.86)***	8.74 (0.79)***	4.63 (0.76)***	4.54 (0.73)***
Disability		-6.66 (2.35)**	-5.58 (2.19)**	-4.86 (2.05)*	-1.83 (1.90)	-1.62 (1.82)
Urbanicity (city is omitted)						
Large town		-2.70 (1.60)*	-2.64 (1.55)*	-2.52 (1.49)*	-3.06 (1.31)*	-2.95 (1.28)*
Small town		-4.77 (1.67)**	-4.29 (1.68)**	-4.00 (1.70)*	-2.54 (1.50)*	-2.41 (1.48)
Region (Northeast is omitted)						
Midwest		-1.30 (1.63)	-0.64 (1.57)	0.42 (1.63)	-0.98 (1.41)	-0.29 (1.44)
South		-0.49 (1.60)	-0.20 (1.57)	0.45 (1.65)	-0.58 (1.43)	-0.16 (1.46)
West		-2.52 (2.15)	-1.38 (2.06)	-0.90 (2.11)	-1.45 (1.73)	-1.11 (1.74)
Siblings (zero is omitted)						
One			-1.25 (1.30)	-1.45 (1.38)	0.12 (1.16)	-0.04 (1.18)
Two			-4.06 (1.55)**	-4.24 (1.55)**	-1.22 (1.39)	-1.43 (1.37)
Three or more			-7.05 (2.30)**	-7.22 (2.40)**	-3.53 (1.77)*	-3.77 (1.80)*
Marital birth			-1.19 (2.69)	-1.77 (2.56)	-0.43 (2.30)	-0.82 (2.22)
Mother's happiness with marriage (not too happy is omitted)						
Fairly happy			-6.15 (2.99)*	-7.15 (2.95)**	-6.62 (2.45)**	-7.12 (2.50)**
Very happy			-4.39 (2.80)	-6.12 (2.89)*	-4.73 (2.24)*	-5.66 (2.35)**
Mother's psychological well-being			-1.88 (1.45)	-1.76 (1.43)	-0.76 (1.21)	-0.79 (1.19)
Teenage mother			-5.50 (2.16)**	-5.07 (2.10)**	-4.11 (1.96)*	-3.91 (1.93)*
Interpersonal behavior at 3rd grade				3.93 (1.45)**		2.39 (1.23)*
Internalizing behavior at 3rd grade				-5.86 (1.38)***		-2.84 (1.03)**
Externalizing behavior at 3rd grade				-0.81 (1.30)		-0.68 (1.08)
Baseline math score					0.72 (0.05)***	0.68 (0.05)***
Baseline reading score					0.36 (0.07)***	0.36 (0.06)***
Constant	125.99 (0.71)***	47.92 (14.29)***	55.07 (14.01)***	58.75 (14.17)***	81.30 (12.58)***	81.53 (12.08)***
R^2	0.004	0.16	0.19	0.22	0.41	0.42
N	3585	3585	3585	3585	3585	3585

Note: Tests are one-sided. + $p<.10$; * $p<.05$; ** $p<.01$; *** $p<.001$.

Table 4.3. Estimated Effect of Parental Separation on ECLS-B Math Scores

Math Scores	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Parental separation	-1.73 (0.85)*	1.58 (0.75)	1.42 (0.77)	1.27 (0.88)	-0.02 (0.55)	-0.82 (0.60)
Age in months at 1st interview		0.56 (0.12)***	0.53 (0.12)***	0.37 (0.15)**	0.24 (0.10)**	0.13 (0.11)
Gender (Male is omitted)						
Female		0.24 (0.50)	0.33 (0.52)	-0.67 (0.57)	-0.84 (0.34)**	-1.05 (0.42)**
Race (White is omitted)						
Black		-1.95 (1.00)*	-1.67 (1.01)	-1.15 (1.22)	-1.52 (0.76)*	-0.84 (0.91)
Hispanic		-1.78 (0.71)*	-1.83 (0.72)**	-3.15 (0.93)***	-0.91 (0.61)	-1.43 (0.73)*
Other		0.42 (0.79)	0.35 (0.80)	0.41 (0.85)	0.26 (0.61)	0.61 (0.74)
Baseline SES		2.69 (0.24)***	2.63 (0.24)***	2.18 (0.30)***	0.59 (0.18)**	0.46 (0.21)*
Poor child health		-1.02 (2.02)	-0.87 (2.05)	1.61 (2.57)	-1.51 (1.60)	-0.25 (2.20)
Urbanicity (city is omitted)						
Large town		-1.71 (0.83)*	-1.59 (0.81)*	-1.99 (0.79)**	-0.09 (0.58)	-0.10 (0.70)
Small town		-1.78 (0.80)*	-1.68 (0.80)*	-2.12 (0.92)*	-0.46 (0.63)	-0.62 (0.74)
Region (Northeast is omitted)						
Midwest		-0.80 (0.94)	-0.84 (0.98)	-0.53 (1.05)	0.03 (0.74)	0.28 (0.66)
South		-0.96 (0.88)	-1.01 (0.92)	0.38 (0.98)	0.15 (0.72)	1.05 (0.66)
West		0.11 (0.86)	0.23 (0.88)	0.90 (0.98)	1.14 (0.73)	1.21 (0.68)*
Siblings (zero is omitted)						
One			-0.63 (0.45)	-0.82 (0.62)	0.59 (0.40)	0.44 (0.46)
Two			-1.28 (0.65)*	-0.75 (0.77)	1.18 (0.53)*	1.32 (0.60)*
Three or more			-2.84 (1.08)**	-2.08 (1.24)*	0.48 (0.76)	0.32 (0.96)
Mother's happiness with marriage (not too happy is omitted)						
Fairly happy			-0.95 (1.78)	-0.57 (2.40)	-0.91 (1.02)	-1.00 (0.92)
Very happy			-1.22 (1.73)	-0.01 (2.25)	-1.62 (0.97)*	-1.52 (0.87)*
Mother's psychological well-being			-0.41 (0.42)	0.40 (0.49)	-0.21 (0.29)	0.22 (0.36)
Positive behavior at preschool				1.46 (0.28)***		0.34 (0.22)
Negative behavior at preschool				-0.07 (0.09)		0.01 (0.07)
Preschool math score					0.56 (0.03)***	0.55 (0.04)***
Preschool reading score					0.20 (0.03)***	0.18 (0.03)***
Constant	43.07 (0.30)***	28.37 (1.83)***	30.87 (2.46)***	26.07 (3.76)***	16.86 (1.83)***	17.57 (1.99)***
R^2	0.003	0.15	0.16	0.18	0.54	0.52
N	3069	3022	3022	2106	2897	2040

Note: Tests are one-sided. * $p<.05$; ** $p<.01$; *** $p<.001$.

results from the analysis of married parents suggest that divorce has no statistically significant association with math and reading scores in the reduced-form models in this age group. Thus, there appear to be minimal differences that are accounted for after controlling for demographic characteristics.

4.6.1 Limitations

The findings in this paper should only be taken as preliminary, given the limitations of the analysis. First, the narrow time frames for the occurrence of parental separation plus the large numbers of cases lost to missing data mean the sample sizes are considerably reduced from their original sizes. Approximately one-fifth of original cases are retained in each data set. This limits the opportunity for more sophisticated analyses that stratify by the likelihood of parental separation or allow for multiple interactions. Including the 8th grade data in the ECLS-K would allow for studying a wider period of opportunity for parental separation. This should be done in future research. In addition, applying multiple imputation may allow for the retention of more cases than list-wise deletion for cases with missing data. Second, although the separations of cohabiting and married parents appear to be similar, this paper cannot examine differences and similarities between these groups in detail, as they are beyond the scope of the paper. Therefore, the evidence is only suggestive that these two states may now be relatively similar with regard to children's cognitive outcomes after union dissolution.

Table 4.4. Estimated Effect of Parental Separation on ECLS-B Reading Scores

Reading Scores	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Parental separation	-1.35 (1.13)	2.61 (1.09)	2.35 (1.06)	1.63 (1.41)	0.51 (0.78)	-0.62 (0.92)
Age in months at 1st interview		0.83 (0.18)***	0.77 (0.18)***	0.58 (0.22)**	0.29 (0.14)*	0.16 (0.17)
Gender (Male is omitted)						
Female		1.73 (0.89)*	1.96 (0.90)*	1.17 (0.97)	0.26 (0.71)	0.32 (0.82)
Race (White is omitted)						
Black		0.34 (1.25)	1.02 (1.33)	2.35 (1.94)	0.80 (1.10)	1.98 (1.50)
Hispanic		-0.96 (1.16)	-0.91 (1.18)	-2.26 (1.33)*	1.37 (0.88)	0.75 (1.00)
Other		2.84 (1.27)*	2.65 (1.23)*	2.50 (1.41)*	1.98 (1.05)*	2.21 (1.29)*
Baseline SES		3.74 (0.37)***	3.64 (0.37)***	2.81 (0.44)***	0.53 (0.25)*	0.24 (0.31)
Poor child health		-0.03 (2.82)	-0.29 (2.76)	4.82 (2.49)*	-0.68 (1.84)	2.77 (1.55)*
Urbanicity (city is omitted)						
Large town		-2.24 (1.19)*	-1.97 (1.17)*	-2.94 (1.26)*	0.30 (0.98)	0.11 (1.07)
Small town		-2.83 (1.35)*	-2.72 (1.39)*	-3.49 (1.48)*	-1.06 (1.17)	-1.55 (1.20)
Region (Northeast is omitted)						
Midwest		-0.80 (1.42)	-0.74 (1.49)	-0.67 (1.75)	0.67 (0.98)	0.69 (1.02)
South		0.57 (1.32)	0.56 (1.38)	2.31 (1.64)	2.42 (1.09)*	3.51 (1.15)**
West		1.09 (1.26)	1.37 (1.29)	2.07 (1.57)	2.84 (0.99)**	3.00 (1.07)**
Siblings (zero is omitted)						
One			-2.94 (0.79)***	-2.42 (1.04)*	-0.94 (0.64)	-0.55 (0.73)
Two			-3.82 (0.94)***	-2.68 (1.10)**	0.01 (0.78)	0.47 (0.83)
Three or more			-5.77 (1.36)***	-5.54 (1.66)***	-0.81 (1.09)	-1.87 (1.37)
Mother's happiness with marriage (not too happy is omitted)						
Fairly happy			0.90 (2.88)	0.21 (2.32)	1.24 (2.44)	0.55 (3.06)
Very happy			1.04 (2.78)	1.49 (2.07)	1.04 (2.51)	0.66 (3.19)
Mother's psychological well-being			0.17 (0.63)	0.83 (0.74)	0.15 (0.48)	0.45 (0.53)
Positive behavior at preschool				1.68 (0.47)***		0.37 (0.39)
Negative behavior at preschool				-0.14 (0.16)		-0.05 (0.14)
Preschool math score					0.49 (0.04)***	0.49 (0.05)***
Preschool reading score					0.61 (0.04)***	0.57 (0.05)***
Constant	41.69 (0.50)***	18.70 (2.44)***	20.62 (3.24)***	17.20 (5.02)**	2.03 (3.13)	4.29 (4.63)
R^2	0.001	0.13	0.15	0.16	0.53	0.51
N	3069	3022	3022	2106	2897	2040

Note: Tests are one-sided. * p<.05; ** p<.01; *** p<.001.

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4.7 Discussion

In this paper I tested three hypotheses using data from the ECLS-K and the ECLS-B. Recall Hypothesis 1: Parental separation is negatively associated with math and reading test scores. I found limited support for this hypothesis. Parental separation was associated with lower test scores for math in the reduced-form models using both data sets, but there was only a marginally statistically significant association with reading scores in ECLS-K. This might indicate that different environmental or familial influences affect children differently in terms of skill acquisition in math and reading. However, there is no longer any statistically significant negative association between separation and test scores after the addition of demographic variables. This indicates that demographic characteristics associated with parental separation are also associated with math scores and reading scores. The strongest candidate for a mediating variable in the models is socioeconomic status.

Recall Hypothesis 2: Demographic and family characteristics explain some, but not all, of the association between separation and test scores. I found some support for Hypothesis 2 for math scores in both data sets and for reading scores in ECLS-K. In particular, with the addition of demographic characteristics, the negative association between separation and test scores was no longer statistically significant.

Recall Hypothesis 3: Parental separation has no direct association with test scores after controlling for non-cognitive skills. I could not test this hypothesis because there was no statistically significant residual association to explain once the demographic variables had been added to the models.

A second goal of this paper was to examine whether the relationship between parental separation and test scores varied by developmental stage. I find that differences between the two cohorts appear to be minimal. As such, it does not appear that younger children have worse outcomes than school-age children and vice versa, contrary to the findings of several previous studies.

Finally, I was interested in whether, in the ECLS-B cohort, the children of cohabiting parents and the children of married parents had different experiences of the dissolution of the parental union with regard to its association with test scores. The children of cohabiting parents do not appear to have worse outcomes following the dissolution of the parental relationship than children of married parents. This is consistent with previous research suggesting that cohabitation may be becoming more like marriage in recent years than it was a few decades prior.

The findings of the analysis are generally inconsistent with resource theory, stress theory, and parenting theory. Rather, they are more consistent with a selection explanation, since the association between parental separation and test scores can be explained by controlling for demographic characteristics. This does not mean that parental separation is a stress-free and resource-neutral event, but instead it suggests that in these data there may be no statistically significant relationship between parental separation, changes in children's behavior, and test scores. Perhaps with a longer period of observation or a greater number of cases experiencing parental separation further insight could be garnered.

Because of the preliminary and descriptive nature of these results, it is inappropriate to draw conclusions regarding potential policy implications. However, the

study suggests additional areas to be examined by researchers in the future. First,

researchers should attempt to improve the sample sizes of these types of analyses when

using ECLS-K and ECLS-B, possibly through dealing with missing data through multiple

imputation. Large segments of these samples have been lost to missing data in this

analysis, and the weights may not fully adjust for these losses. Second, it may be useful

to consider incorporating interactions into the models because the average coefficients in

an analysis such as this one may mask subgroup heterogeneity. Third, the finding that

children of cohabiting parents do not appear to fare worse than those of married parents

in the ECLS-B suggests that researchers should more carefully examine similarities and

differences between and within these groups to determine whether cohabitation really is

becoming more like marriage, and if so, what this implies for future generations. Finally,

there may be value in stratifying children by their risk of parental separation and

examining whether the associations between separation and test scores vary along this

dimension.

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Appendix Table 4.1. ECLS-B Descriptive Statistics

Variable	
Percent experiencing parental separation between 9 months and preschool	12.6%
Percent experiencing parental divorce between 9 months and preschool	8.9%
Percent experiencing separation of cohabiting parents between 9 months and preschool	3.7%
Percent female	48.2%
Mean age at baseline (months)	10.35 (0.06)
Mean kindergarten math score	42.86 (0.29)
Mean kindergarten reading score	41.53 (0.49)
Mean kindergarten good behavior score	11.47 (0.08)
Mean kindergarten negative behavior score	5.99 (0.26)
Mean preschool math score	31.57 (0.26)
Mean preschool reading score	27.64 (0.31)
Mean preschool good behavior score	5.07 (0.04)
Mean preschool negative behavior score	3.06 (0.11)
Mean SES at baseline (quintiles)	3.60 (0.03)
Percent in poor health at baseline	1.7%
Mean mother's psychological well-being at baseline (0-3)	0.38 (0.01)
Percent with one sibling at baseline	37.2%
Percent with two siblings at baseline	16.3%
Percent with three or more siblings at baseline	8.4%
Percent with cohabiting parents at baseline	9.0%
Percent with married parents at baseline	88.8%
Percent living in a city at baseline	62.0%
Percent living in a large town at baseline	11.3%
Percent living in a small town at baseline	16.7%
Percent white	61.1%
Percent black	5.7%
Percent Hispanic	15.8%
Percent other race/ethnic category	7.4%
Percent mothers not too happy with relationship	0.8%
Percent mothers fairly happy with relationship	20.4%
Percent mothers very happy with relationship	78.8%

Appendix Table 4.2. ECLS-K Descriptive Statistics

Variable	
Percent experiencing parental divorce between 1st grade and 3rd grade	6.1%
Percent female	50.5%
Mean age at baseline (months)	87.19 (0.15)
Mean 5th grade math score	98.14 (0.71)
Mean 5th grade reading score	125.65 (0.69)
Mean 5th grade interpersonal skills score	2.19 (0.02)
Mean 5th grade internalizing behavior score	0.58 (0.02)
Mean 5th grade externalizing behavior score	0.61 (0.01)
Mean baseline math score	36.33 (0.33)
Mean baseline reading score	43.60 (0.39)
Mean baseline interpersonal skills score	2.25 (0.02)
Mean baseline internalizing behavior score	0.51 (0.02)
Mean baseline externalizing behavior score	0.55 (0.02)
Mean SES at baseline	0.28 (0.02)
Percent with a disability at baseline	12.9%
Mean mother's psychological well-being at baseline (0-3)	0.39 (0.02)
Percent with one sibling at baseline	49.1%
Percent with two siblings at baseline	29.8%
Percent with three or more siblings at baseline	11.5%
Percent with married parents at birth	93.1%
Percent living in a city at baseline	45.5%
Percent born to teenage mother	18.2%
Percent living in a large town at baseline	25.3%
Percent living in a small town at baseline	29.2%
Percent white	77.2%
Percent black	5.9%
Percent Hispanic	10.5%
Percent other race/ethnic category	6.4%
Percent mothers not too happy with relationship	1.1%
Percent mothers fairly happy with relationship	21.5%
Percent mothers very happy with relationship	77.4%

CHAPTER 5

CONCLUSION

In this dissertation I have investigated the relationships between family events and subsequent outcomes with a focus on the events of job loss, the birth of a second child, and parental separation. In Chapter 2, I showed that the unemployment of one member of a couple changes the distribution of housework between the members of the couple and also increases the household's total time in housework. Furthermore, this process is gendered, with unemployed wives increasing their time in housework to a greater extent than unemployed husbands. In Chapter 3, I showed that on average different patterns of second births do not appear to have significant effects on mothers' wages or earnings as measured at age 45 whether or not they occur soon or much longer after the first birth. However, there were also indications that there may be some heterogeneity in effects by education and age at first birth. This type of heterogeneity will require further analysis in the future. In Chapter 4, I demonstrated that negative associations between parental separation and children's math and reading test scores could be explained by the addition of demographic characteristics. The results suggest that selection plays a significant role in explaining the association between parental separation and children's test scores. These papers all add new insights to the literature, and they also suggest areas for future research.

5.1 Future Research in Housework

There are four major areas that should be explored in future research on housework. The first two areas are directly related to the relationship between unemployment and housework. First, there is a lack of information about whether the relationship between unemployment and changes in housework varies based on the concurrent state of economic growth. For example, do unemployed male and female partners increase their time in housework more during times of recession than during times of normal economic growth because jobs are difficult to find and unemployment periods may be lengthy? Conversely, do male and female partners experience smaller changes in time in housework during unemployment when the economy is experiencing significant growth? With the Great Recession fresh in the minds of so many individuals, it makes sense to consider how the broader economy may influence the ways in which men and women divide labor within the home, particularly in the face of unemployment or underemployment. This is potentially consequential for policy reasons as well as for gender relations within households.

Second, future research should investigate whether gender and class differences in the receipt (and amount) of unemployment insurance impact the division of labor within households in ways that may perpetuate labor market inequality. Unemployment insurance is an important source of income in many households, especially during economic downturns. Yet, it does not always factor into discussions about how unemployment affects paid and unpaid labor within a household. Given important differences by gender and class in the dollar value of unemployment insurance, not

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accounting for this form of income may lead researchers to overlook potential sources of long-term labor market inequality that persist long after the economy has recovered.

The third and fourth areas to be explored are similar and relate to data. I argue that researchers who study the division of labor within families need to be able to draw on longitudinal time-use data. Such data do not currently exist in the United States, as the American Time Use Survey is cross-sectional. The detail and accuracy of time-use data are very appealing to researchers studying the division of labor because it is possible to examine household labor in a more nuanced way than it is with many of the global measures available in survey data. Yet, analysis of cross-sectional data can be challenging because there is often unobserved heterogeneity that affects both labor market outcomes and household labor. With longitudinal data and panel models it is possible to account for the time-invariant unobserved heterogeneity, but this is not the case with cross-sectional data. Barring funding for a national longitudinal time-use survey, researchers should consider adding time-use components to long-standing surveys such as the PSID, but they should ensure that the time-use measures are collected as proximately as possible to the interviews themselves. This reduces the risk of missing important events that occur between the time-use data collection and the interview.

5.2 Future Research on the Motherhood Penalty

A large body of literature establishes the magnitudes of motherhood penalties. While a number of researchers have examined the kinds of characteristics associated with reductions in these penalties, such as postponement of a first birth, there is still ample room to explore in more detail the mechanisms that underlie the wage penalties (or

premiums) experienced by both mothers and fathers. More attention should be paid to the employment context and to the financial stability of a household. As an example, researchers should investigate whether differential access to, or length of, maternity leave plays a role in mitigating or exacerbating the wage penalties that arise in conjunction with entry into motherhood. Furthermore, while access to maternity leave or other benefits that may facilitate job return is important, researchers must compare the availability of such programs with the take-up of these programs. If take-up is low, the policy is most likely failing to do what it was designed to do.

A second challenge for future research on the motherhood penalty is to be able to more fully incorporate the characteristics of spouses/partners and other adults living in the household. Surveys such as NLSY79 do a very good job of capturing labor market information and background characteristics from respondents, but the data available on respondents' partners is limited. To the extent possible, researchers should consider ways of adapting current surveys to be more conducive to a household-level analysis of parenthood penalties and premiums so as to better model what are likely complex, joint decisions that couples make about childbearing, childrearing, and labor market participation.

5.3 Future Research on Parental Separation

Similar to Amato (2010) I argue that there are four major areas that should be examined in future research on parental separation. First, there must be more attention to the dissolution of cohabiting unions and the effects these dissolutions have on children. Given that cohabiting relationships are increasingly a setting for childbearing, it is

important to understand how this setting, and instability in this setting, affects children's outcomes. If it leads to more volatility in children's living arrangements than instability in other types of parental relationship, and therefore negative consequences for skill acquisition and achievement, scientists and policymakers will need to identify solutions to mitigate these negative effects. Yet, this can only be done if the research exists to suggest the types of interventions that would work.

Second, researchers must make an effort to study the effects of the dissolution of same-sex couples for their children. Data on these types of families are limited, so researchers may need to design qualitative studies to examine the similarities and differences in the experiences of children with same-sex parents that break up as compared to children with opposite-sex parents. Furthermore, because same-sex marriage is not legal in many states, researchers will need to classify different types of relationships in a more nuanced way than is often necessary with opposite-sex parents.

Third, it would be helpful to have more studies of children's outcomes over long trajectories. For example, it could be informative to track children who experience parental separation as young children through middle childhood, adolescence, and into adulthood. Many studies exist that cover one of these periods, but often researchers are limited by data availability. This may be another area in which qualitative work or a small-scale location-based survey could make this a more manageable feat because following individuals over a long period of time, particularly as they transition through life stages, is very challenging.

Finally, future research should acknowledge the possibility of heterogeneous effects of a separation on children's outcomes and model such heterogeneity

appropriately. One example is to use a multi-level modeling approach, wherein children are stratified based on the likelihood of parental separation, as predicted by observable characteristics. Then the effects of separation are estimated within these strata, and researchers examine whether there is a pattern of effects across strata. It seems likely that there are heterogeneous effects of separation arising from parental characteristics and that some children might experience stronger negative responses to parental separation than others. Furthermore, some research suggests that children whose parents have a number of risk factors for separation may actually benefit from a separation. The results from this future work might be particularly informative in thinking about family-focused social policies that may reduce child poverty.

5.4 Consequences of Family Change

Families have been adapting to events like unemployment, births, and parental separations for centuries, although the nature of these adaptations has evolved substantially over time. Social science research has made clear that some of these adaptive behaviors may have long-term consequences, whether positive or negative, and much research has been devoted to identifying and understanding these consequences. The past several decades have brought many new contributions to the literatures on division of labor, the motherhood penalty, and divorce. The contributions in this dissertation provide additional insights, but there is still considerable room for expansion as I have indicated in earlier sections of this conclusion. The family is rapidly changing, and it is unclear what impact this will have on our understanding of past findings and on future research. Although these rapid changes may make researching family events more

challenging than ever, the need to understand their antecedents and consequences is as strong as ever. To address large-scale social issues like social and economic inequality, we need research that tackles the impacts of family events, and the interactions between families and other institutions.