**What Money Buys and Family Costs:**
**Three Papers on the Work-Family Intersection**

by

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# TABLE OF CONTENTS

**LIST OF FIGURES**

Figure

# LIST OF TABLES

Table

CHAPTER 1

**Introduction**

The second half of the 20<sup>th</sup> century witnessed large-scale changes in American gender roles and family formation. The traditional model of marital specialization, with men investing in paid labor and women in unpaid labor, including child-rearing (Becker 1981), has eroded. Women's labor force participation rates rose (Fischer and Hout 2006; Juhn and Potter 2006) while their time in housework declined (Bianchi et al. 2000). At the same time, nonmarital child-bearing, divorce, and remarriage increased, while time spent married declined (Teachman, Tedrow, and Crowder 2000). These changes in patterns of work and family life create new challenges in balancing work and family responsibilities for American adults.

While the entry of married women into the labor force overturned the traditional model of husbands specializing in paid labor and wives in unpaid labor, the traditional gendered division of unpaid labor has been much slower to change. In opposite-sex married couples, wives continue to perform the majority of household labor, even in households in which both partners work full-time (Kamo 1988). Working wives face decisions about how to manage their dual burden of work and family. They may choose to reduce the total level of domestic production in the household, to use their earnings to outsource domestic production, or to enlist the aid of other household members, perhaps

including their husbands. Not all wives will be equally able to deploy these strategies: some working wives may have greater resources available that allow them to reduce time in housework. Chapters 2 and 3 are concerned with a particular resource – earnings – and its capacity to allow working wives to reduce their unpaid labor burden.

Chapter 2, "Money Isn't Everything: Wives' Earnings and Housework Time", which is co-authored with Margaret Gough, explores the limitations in wives' ability to use their financial resources to reduce their household labor time. The autonomy perspective of housework time predicts that wives' housework time falls steadily as their earnings rise, because wives use additional financial resources to outsource or forego time in housework. We argue, however, that wives' ability to reduce their housework varies by household task. That is, we expect that increases in wives' earnings will allow them to forego or outsource some tasks, but not others. As a result, we hypothesize more rapid declines in wives' housework time for low-earning wives as their earnings increase than for high-earning wives who have already stopped performing household tasks that are the easiest and cheapest to outsource or forego. Using fixed-effects models and data from the Panel Study of Income Dynamics, we find considerable support for our hypothesis. We further conclude that past evidence that wives who out-earn their husbands spend additional time in housework to compensate for their gender-deviant success in the labor market is due to the failure to account for the non-linear relationship between wives' absolute earnings and their housework time.

In Chapter 3, "Opting Out and Buying Out: Wives' Earnings and Housework Time," I focus more closely on the mechanism explaining the negative relationship between wives' earnings and their housework time. It has been proposed that the negative

association between wives' earnings and their time in housework is due to greater

outsourcing of household labor by households with high-earning wives, but this

hypothesis has not been tested directly. In a sample of dual-earner married couples in the

Consumption and Activities Mail Survey of the Health and Retirement Study, use of

market substitutes for women's housework was found to be only weakly associated with

wives' time cooking and cleaning. Furthermore, expenditures on market substitutes

explain less than 15% of the earnings–housework time relationship. This suggests that

use of market substitutes plays a smaller role in explaining variation in wives' time in

household labor than has previously been hypothesized.

Changing patterns of family life have affected men as well as women. Since

children of non-coresidential parents are more likely to live with their mothers than their

fathers (Kreider 2008), increases in nonmarital childbearing, divorce, and remarriage lead

to increases in the fraction of men who are biological or social fathers, but do not follow

the traditional model of living only with biological children who are also the children of

their wives. This raises the possibility that men understand the roles and responsibilities

of fatherhood, including the role of providing financially for children, differently

depending on whether the relationship is biological or social, residential or

nonresidential, and reinforced through marriage to the mother, a nonmarital relationship,

or a former relationship only. Chapter 4, "A Reconsideration of the Fatherhood

Premium," specifically examines heterogeneity in the relationship between fatherhood

and men's wages, depending on the form of fatherhood. Past research has asserted a

fatherhood premium for men, while ignoring the heterogeneity of men's fathering

contexts. Using panel models and data from the 1979 cohort of the National Longitudinal

Survey of Youth, I find that married residential fatherhood is associated with wage gains of 4.3%, unmarried residential fathers experience no statistically significant wage changes, and nonresidential fatherhood is associated with a wage loss of 3.8%. Men's wages are 4.3% lower when they are stepfathers compared to being married and childless; this means that the marriage premium for men marrying women with children is only about 1/3 as large as the marriage premium for men marrying childless women. Thus, the fatherhood wage premium, far from being universal, is confined to married, biological, residential fathers. As a result, men's wages are 13% lower when they are divorced, nonresidential fathers than when they are married, residential fathers, and the majority of this difference is due to the transition from residential to nonresidential fatherhood, not the loss of the marriage premium.

Work and family outcomes are mutually dependent. For women, much attention has been paid to the possibility that housework and parenting responsibilities depress women's labor force outcomes (Avellar and Smock 2003; Budig and England 2001; Noonan 2001; Hersch and Stratton 1997). Less attention, however, has been paid to the ways in which the rewards that women receive in the labor market affect their decisions about unpaid labor, which is the focus of Chapters 2 and 3. For men, too, family, especially marriage, is acknowledged to affect labor force outcomes (Chun and Lee 2001; Hersch and Stratton 2000; Loh 1996). The increasing diversity of family forms and its effect on the relationship between work and family for men has been recognized by Cohen (2002), who shows that the rise of cohabitation is an important determinant of the changing returns to marriage. However, the heterogeneity of contemporary fatherhood forms and the possibility, discussed in Chapter 4, for differences across fatherhood types

in the effect of family on work, has been given little attention. Thus, each of the three empirical chapters of this dissertation uses quantitative analysis and data from a different nationally-representative dataset to shed new light on a different aspect of the work-family intersection.

**Money Isn't Everything: Wives' Earnings and Housework Time[1]**

2.1 Introduction

Among married couples, wives perform the majority of household labor even when both spouses work full time (Kamo 1988) and when wives earn as much as their husbands (Evertsson and Nermo 2007). This inequality in the division of household labor contributes to a gender gap in leisure time between fully-employed husbands and wives and may also contribute to the gender gap in wages, if wives' more extensive housework responsibilities reduce the intensity of their labor market work (Hersch and Stratton 1997; Noonan 2001).

Brines (1994) proposed a provocative explanation for this phenomenon: that couples with "gender-deviant" relative earnings – that is, where the wife earns more than the husband – will compensate by adopting a gender-traditional division of household labor. Under this theory, wives' housework hours will fall as they contribute a larger

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[1] Chapter 2 is co-authored with Margaret Gough. A version of this chapter appears as: Killewald, Alexandra and Margaret Gough. 2010. "Money isn't everything: Wives' earnings and housework time." *Social Science Research* 39: 987-1003. doi:10.1016/j.ssresearch.2010.08.005

share of the couple's income, up to the point that they contribute half of the couple's income. However, as wives' income share increases beyond this point, their housework hours will rise. Brines terms this pattern "gender display." To avoid confusion with the broader use of this term (West and Zimmerman 1987), we refer to Brines' model as "compensatory gender display", emphasizing that this is a behavior enacted by breadwinner wives to compensate for their gender-deviant labor force outcomes.

The key empirical prediction of compensatory gender display is that breadwinner wives – wives who out-earn their husbands – will perform more housework than wives who have earnings parity with their husbands, and that, among breadwinner wives, housework hours will continue to rise as the wife's share of the couple's income continues to increase.

In contrast, the autonomy perspective hypothesizes that wives' own earnings are a better predictor of their time in household labor. Although the causal mechanism has not been directly tested, one possibility is that wives' increased earnings provide increased financial resources to purchase market substitutes for their housework time. The autonomy perspective predicts consistent declines in wives' housework time as their earnings rise.

This paper challenges the predictions of compensatory gender display, but also argues that the autonomy perspective has insufficiently considered the constraints that lead even wives with high earnings to spend substantial time in housework. We hypothesize that limits in wives' ability to outsource or forego time in household labor will lead to small additional reductions in housework time for wives at the high end of the

earnings distribution. We further hypothesize that evidence previously interpreted as indicative of compensatory gender display behavior is instead an artifact of failing to account for the non-linear relationship between wives' absolute earnings and their housework time. By appropriately controlling for this non-linear relationship, as well as using fixed-effects models to control for time-invariant attitudes and behaviors, we provide a rigorous evaluation of the theory of compensatory gender display. If no evidence is found for compensatory gender display, the supposition that wives are disadvantaged in terms of household labor time when they out-earn their husbands must be overturned.

Thus, the first goal of this paper is to test the validity of the assumption that the relationship between wives' earnings and their time in housework is linear. If a non-linear relationship is found, the second goal is to assess whether the evidence for compensatory gender display is robust to models that allow a more flexible relationship between wives' own earnings and their housework time. We begin by reviewing the existing literature on time in household labor, focusing on several resource- and gender-based theories. Next, we summarize our research questions and propose several reasons that the relationship between wives' earnings and their time in housework may be non-linear. We then describe our data and analytic strategy. We follow with the presentation of our results and discussion of their robustness to alternative specifications. We conclude with a discussion of our findings and their implications.

2.2. Background

*2.2.1 Resource-Based Theories of Household Labor*

Wives' financial resources are acknowledged to affect their household labor time,

although the form of this relationship is contested. A core question is whether wives'

household labor time responds more strongly to their absolute earnings or their earnings

relative to their husbands' earnings. We label these the autonomy perspective and the

relative resources perspective, respectively. In both perspectives, spouses' financial

resources are presumed to influence time in household labor net of time in the labor

market. In other words, spouses with higher earnings are assumed to do less housework

not simply because they spend, on average, more time in the labor market and therefore

have less time available for household labor, but because they are advantaged by

controlling greater financial resources. As a result, both perspectives imply that spouses'

resources should influence household labor time even after controlling for labor market

hours.

The relative resources perspective (referred to sometimes as the bargaining

perspective or dependency perspective), assumes that the spouse who controls more

resources will have a more powerful bargaining position and, thus, can better achieve his

or her desired outcome (Blood and Wolfe 1960). If housework is assumed to be an

undesirable activity for both spouses, then, other things equal, the spouse with greater

resources is expected to perform less housework than his or her partner (Bittman et al.

2003; Brines 1994; Evertsson and Nermo 2004). Under the relative resources perspective,

wives' housework hours should fall *whenever* their financial resources rise relative to

those of their husbands, as greater resources give them greater power to bargain out of undesirable household chores.

Spouses' relative financial resources may affect the balance of power within the relationship in two ways. First, spouses with higher wage-earning *potential* will have greater ability to support themselves in the event of a divorce. The spouse who is less dependent on the marriage for well-being will have a better bargaining position (Lundberg and Pollak 1996; McElroy and Horney 1981). Under this framework, spouses' relative financial resources are best operationalized by the spouses' potential wages in the event of divorce (Pollak 2005).

Alternatively, spouses' *current* financial contributions to the marriage may influence spouses' bargaining positions, as they influence what is perceived as a fair exchange between spouses. Thus, if both spouses spend the same amount of time in the labor market, but one spouse earns more, it may seem "fair" or "appropriate" to both spouses that the breadwinner spouse performs less household labor. As a result, spouses' relative financial resources can be measured by the share of the spouses' current earnings that are provided by the wife (or the husband). Our work follows this second operationalization, as relative earnings have been the dominant operationalization of spouses' relative financial resources in the empirical sociological literature on housework (see, Baxter, Hewitt, and Haynes 2008; Bianchi et al. 2000; Bittman et al. 2003; Brines 1994; Evertsson and Nermo 2004, 2007; Greenstein 2000; Gupta 2006, 2007; Presser 1994).

Empirical evidence has tended to support the predictions of the relative resources perspective, finding that wives' time spent on housework is negatively associated with their earnings relative to their husbands' (Baxter et al. 2008; Bianchi et al. 2000; Bittman et al. 2003; Presser 1994).

In contrast, the autonomy perspective emphasizes the role of the absolute level of wives' earnings in determining their household labor time. The causal mechanism for this relationship has not been directly tested, but the outsourcing of household labor has been suggested as a likely cause (Gupta 2006, 2007). Under this perspective, it is economically rational for wives to reduce their time in housework as their earnings rise, as their greater financial resources allow them to purchase market substitutes for their household labor. This perspective is supported by findings that wives' time in housework falls more rapidly with increases in their own earnings than with increases in those of their husbands (Gupta 2006, 2007; Gupta and Ash 2008). It is also consistent with evidence that spending on market substitutes for women's household labor, such as housekeeping services and meals away from home, rises more quickly with wives' earnings than with husbands' (Cohen 1998; Oropesa 1993; Phipps and Burton 1998). Even if spouses pool their incomes, this suggests that wives exercise greater control over the use of their own earnings than their husbands'.

More broadly, the autonomy perspective may be conceived of as encompassing any causal mechanism linking wives' absolute earnings to lower time in household labor. Gupta (2006, 2007) proposes, for example, that high-earning wives may simply feel a reduced obligation to perform housework, even if they do not purchase a market substitute for their own household labor. It is also possible that high-earning wives are

able to convince their husbands to take over more of the household labor, although Gupta (2006, 2007) does not find evidence for this hypothesis. The autonomy perspective has generally been specified empirically as a linear relationship between wives' earnings and their time in housework (Gupta 2006, 2007).

*2.2.2 Gender-Based Theories of Household Labor*

Neither the relative resources perspective nor the autonomy perspective can explain why women with full-time jobs who earn as much or more than their husbands continue to perform the majority of household labor. Rather, it is clear that norms about gender reduce wives' abilities to use their financial resources to reduce their hours of housework. Broader social norms may lead both spouses to systematically discount women's earnings (Agarwal 1997; Blumberg and Coleman 1989), giving wives less bargaining power than their financial resources would predict. From the standpoint of wives' own perceptions, the resulting division of labor may seem fair, though it is not consistent with a gender-neutral model of bargaining (Hochschild 1989; Lennon and Rosenfield 1994).

Furthermore, because housework has a performative quality to it, embodying ideals of feminine and masculine behavior (West and Zimmerman 1987), a gendered division of market and domestic labor may produce the social and psychological rewards of conforming to traditional gender roles (Berk 1985). Conversely, women who deviate from these gendered cultural norms and reduce their housework substantially may experience social stigma and guilt (Atkinson and Boles 1984; DeVault 1991; Tichenor 2005). These socially-imposed costs may lead spouses to a division of labor that deviates

from what would be expected from a gender-neutral logic based only on spouses' relative incomes.

Thus, while spouses may negotiate the division of household labor based in part on what they *perceive* as a fair exchange, gendered norms of behavior and the discounting of wives' financial contributions will yield greater responsibility for housework for wives than husbands, even when their earnings are similar.

*2.2.3 Compensatory Gender Display*

Compensatory gender display provides an alternative to the assumptions and predictions of a gender-neutral relative resources perspective, but articulates a narrower hypothesis than the gender-socialization or gender-performance perspectives previously discussed. The compensatory gender display framework posits that partners use housework to affirm traditional gender roles in the face of gender-atypical economic circumstances.

The compensatory gender display hypothesis was operationalized by Brines (1994) and other researchers (Bittman et al. 2003; Evertsson and Nermo 2004; Greenstein 2000; Gupta 2007) as a quadratic relationship between the share of the couple's household income that is provided by the wife or the husband and the housework hours of either spouse.[2] Wives' housework hours are expected to follow a U-shaped pattern, with wives' housework time falling with increased relative earnings up to the point that they contribute about half of family income, and then rising as they out-earn their

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[2] Greenstein (2000) also introduces a model in which the dependent variable is the proportion of housework done by each spouse.

husbands by progressively larger amounts. Concomitantly, husbands' housework hours are expected to increase as wives' earnings rise relative to theirs but fall once their wives contribute more than about half of family income. These predictions contrast with those of the relative resources perspective, which suggest that wives' housework hours should decline (and husbands' rise) with increases in wives' relative earnings, even among couples in which the wife earns more than the husband.

The core implication of the compensatory gender display framework is not its particular functional form[3], but its claim that women who out-earn their husbands, instead of using their own financial resources to achieve greater gender equity in the division of household labor, are penalized at home for their success at work, doing more housework than they would have if they had not out-earned their husbands.

Empirical tests of compensatory gender display have generally supported its tenets, with two important challenges. Brines (1994) originally found evidence of compensatory gender display for men using a cross-sectional sample from the Panel Study of Income Dynamics (PSID). Subsequent work using data from the National Survey of Families and Households (NSFH) (Bittman et al. 2003; Greenstein 2000),

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[3] For example, a quadratic form specification of wives' earnings relative to their husbands' that fits the housework data well, in which the minimum housework hours were predicted to occur for wives who earned 100% of the couple's income, would not be interpreted as providing evidence for compensatory gender display, since wives' housework hours would be predicted to fall as their relative earnings rose over the entire distribution of wives' relative earnings. Conversely, if wives who out-earn their husbands do more housework than wives who have earnings parity with their husbands, but the shape of the relationship between wives' relative earnings and their housework time is not a quadratic, this would still be consistent with the theory of compensatory gender display.

Australian time-use data (Bittman et al. 2003), and the PSID (Evertsson and Nermo 2004) found evidence of compensatory gender display for at least one gender. Among samples of American couples, support for compensatory gender display has been found using both the NSFH and the PSID (Bittman et al. 2003; Brines 1994; Evertsson and Nermo 2004; Greenstein 2000), although individual studies may find evidence consistent with compensatory gender display on the part of only one gender.

Gupta (1999) criticized Brines' findings by showing that they were sensitive to the inclusion of the 3% of men who were most highly dependent on their wives. In later work using the NSFH, he showed that the observed quadratic relationship between relative resources and housework time found by Brines and others is an artifact of including as a control variable only the household's total income, rather than separate controls for husbands' earnings and wives' earnings, to reflect the stronger relationship between wives' own earnings and their household labor time (Gupta 2007). Gupta challenges both compensatory gender display and the relative resources hypothesis and suggests that autonomy is the most appropriate framework through which to view the relationship between wives' earnings and household labor time.

2.3. The Present Study

Gender-based challenges to resource theory have challenged the economic logic of bargaining and specialization and have attempted to explain why couples in which the wife earns the most *divide* housework in a way that is not economically rational. Little attention has been given to the question of why high-earning wives continue to do

housework *themselves* rather than purchasing market substitutes for their own time or lowering the total amount of domestic production. While Gupta's (2007) finding demonstrates the importance of wives' earnings in determining their household labor time, it does not consider ways in which constraints in wives' desire or ability to forego and outsource household labor may moderate the degree to which wives' behavior follows the predictions of autonomy. Although Gupta (2006) and Gupta and Ash (2008) find some evidence that the earnings-housework relationship is flatter at the high end of the earnings distribution, the small sample size of the NSFH makes it difficult to formally test the assumption of linearity, and the implications of this empirical result are not discussed in detail.

There is good reason to believe that the association between wives' earnings and their housework time may not be linear. We propose that wives face heterogeneity in the costs associated with foregoing or outsourcing specific household tasks. Even among households with significant financial resources, constraints in households' ability or desire to outsource or forego household labor may arise for several reasons. For example, Baxter, Hewitt, and Western (2009) show that attitudes about whether it is appropriate, affordable, and efficient to hire a domestic worker are related to the likelihood that a household pays for regular help with housework, even after controlling for differences in households' financial resources. Transaction costs associated with outsourcing, particularly the costs of monitoring service providers, may also reduce the ease with which households can outsource household production (de Ruijter, van der Lippe, and Raub 2003). Furthermore, even among high-earning wives, doing housework is tied to a desire to be "good wives" (Atkinson and Boles 1984; Tichenor 2005). The husbands of

high-earning wives also express a reluctance to let their wives' career success interfere

with her household production, suggesting that they may pressure their wives to do some

household labor (Atkinson and Boles 1984; Hochschild 1989). Thus, the social

construction of gender may constrain the ability of high-earning wives to forego

housework time.

If households' attitudes toward the outsourcing of domestic labor can be captured

with a single, time-invariant measure, then these attitudes cannot explain changes in

wives' housework hours that are associated with changes in their earnings. Similarly, if

trust problems in outsourcing, a lack of availability of domestic workers, or gendered

norms of behavior simply depress outsourcing by a constant amount, they cannot explain

the relationship between wives' earnings and their housework time.

The heterogeneity in the ease and desirability of outsourcing or foregoing

different household tasks, however, provides a mechanism by which the non-linear

association between wives' earnings and their time in housework may arise. De Ruijter et

al. (2003) suggest that outsourcing will be inhibited when the costs of monitoring service

providers are high, when outsourcing involves a loss of privacy for the household, and

when it is more difficult to find providers who are deemed to provide an adequate quality

of service or good. Compared to the outsourcing of meal preparation, hiring domestic

workers may be less appealing to households because it is difficult to monitor the effort

and quality of the service, the worker must be admitted into the home, often

unsupervised, and domestic workers may be in relatively short supply in some areas.

Likewise, households may view some household tasks as appropriate and efficient to

outsource or forego, but not others. For example, it may be difficult to hire a domestic

worker to handle unexpected and time-sensitive tasks, such as the cleaning up of spills. Without outsourcing household labor, it may be possible to forego some time cleaning by increasing the period of time between dustings, but less possible to forego the frequency with which meals are prepared. Wives are also less likely to forego or outsource tasks that have symbolic meaning or are associated with appropriate behavior for wives or mothers. For example, a wife may be willing to hire a domestic worker to dust the home, but not to prepare birthday meals for family members. What all of the proposed mechanisms have in common is that they recognize sources of heterogeneous constraint in wives' ability to use their earnings to reduce their time in household labor.

Wives with low earnings may spend considerable time in housework because they lack financial resources to outsource this labor, and they may feel less free than high-earning wives to forego it, as they do not provide substantial financial resources to the household. Thus, when wives with low earnings experience an increase in earnings, this should translate into relatively large reductions in household labor time, as they outsource or forego household tasks for which they view this change to be easy, affordable, and appropriate. As wives' earnings rise, we expect that they will increasingly forego or outsource housework, first giving up tasks that are perceived as the least costly to outsource or forego, and then gradually giving up tasks that incur higher costs, either financial or non-financial, when they are not done.

As earnings continue to rise, wives are left with household tasks that are difficult to forego or outsource – either because of difficulties in procuring an adequate substitute or because substitution is not perceived as appropriate. In other words, wives with high earnings are left with tasks that are performed primarily for non-financial reasons: further

increases in earnings will not make outsourcing or foregoing these tasks more feasible. As a result, we predict that earnings increases for high-earning wives will have a smaller effect on their housework time, as the majority of the housework that remains is done for non-financial reasons and, hence, is less likely to be outsourced or foregone. Thus, the ability of high-earning wives to outsource or forego housework time is constrained, *though they still do less housework than they would if they earned less*.

Our analysis is not designed to determine the precise cause of the non-linear relationship between wives' earnings and their housework time. Instead, having outlined several theoretical reasons why such a relationship might occur, we propose to test empirically whether a non-linear relationship exists and, if it does, to determine whether failure to account for this relationship has led to spurious evidence in favor of compensatory gender display.

We now consider how our theory challenges existing empirical evidence for compensatory gender display. By assuming that financial resources of either the household or the individual facilitate declines in wives' housework time at a constant rate, existing models have not allowed for the possibility of a non-linear relationship between wives' earnings and their housework time. Compensatory gender display theory has, to date, been tested by including both linear and quadratic terms for spouses' relative earnings and examining the sign and significance of the quadratic term. If, however, the relationship between wives' absolute earnings and their time in housework is non-linear, constraining the relationship between absolute earnings and housework to be linear may lead to a spurious non-linear relationship between the share of household income wives provide and their housework hours. This is because wives' absolute earnings are

19

positively correlated with their share of household income. We use a more flexible

specification of wives' absolute earnings – a linear spline – and then measure the

relationship between wives' share of household income and their housework hours.

Compensatory gender display is hypothesized to have explanatory power even

after accounting for other predictors of spouses' housework time, including their

demographic characteristics, labor market hours, and absolute earnings. Therefore, if this

theory as it has been articulated by Brines and others is correct, the quadratic relationship

between wives' relative earnings and their housework time should not disappear when a

more flexible specification of wives' absolute earnings is introduced to the model.

In addition, previous evaluations of compensatory gender display have not

utilized longitudinal data that can control for the fact that couples in which the wife out-

earns the husband may differ from other couples in systematic ways that affect their

housework time. For example, these wives may also have high levels of energy and

motivation that lead them to invest heavily in both market work and housework, or it may

be the case that wives who are efficient in the labor force are less efficient at home,

leading to high earnings but also long hours in housework. Likewise, evaluations of the

autonomy perspective have made use of cross-sectional data (Gupta 2006, 2007).

However, it is possible that high-earning wives spend less time in household labor not

*because* of their earnings, but simply because wives with high earnings have fixed,

unobserved traits that are correlated with lower levels of domestic production, such as a

greater distaste for housework. In this case, it could not be said that wives' earnings give

them autonomy to reduce their time in household labor, as the relationship is spurious.

Our analysis, which uses panel data and fixed-effects models, can control for such

unobserved attributes of wives, as long as they do not vary over time. To our knowledge, we are the first researchers to directly test whether *changes* in couples' labor force outcomes are associated with *changes* in their housework hours in a way that supports either the autonomy perspective or compensatory gender display.

2.4. Data and Methods

We use measures of spouses' time in housework from the 1976-2003 waves of the Panel Study of Income Dynamics (PSID) (Panel Study of Income Dynamics 2009)[4], as these are the years for which we can match these measures to earnings records from the same year. The panel nature of the PSID makes it an ideal dataset for evaluating how changes in spouses' housework hours are associated with changes in their labor force outcomes and also provides us with a much larger sample size than the NSFH.

Our sample includes members of the core sample (1976-2003) and immigrant sample (1997-2003).[5] Because our analyses make use of weighted data, we exclude all couple-year observations that have zero weight in either the cross-sectional or the panel analyses. This allows us to maintain a consistent sample for each model, although individual couples enter and leave the sample in different years. Each individual couple

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[4] In 1968, the year the PSID began, 76% of families selected to be interviewed were interviewed (Hill 1992). By 1989, approximately 50% of both adults and children in 1968 households had attrited from the sample, although, with weights, its cross-sectional representativeness has remained quite good (Fitzgerald, Gottschalk, and Moffitt 1998a, 1998b).

[5] We exclude members of the Latino sample (1990-1995) because of the short span of this subsample and because key labor force outcomes variables are not computed in 1993 and 1994 for this sample.

may appear in the sample in one or more years, depending on the number of years in which the couple is observed by the PSID and satisfies the sample restrictions. We restrict our analysis to married or long-term cohabiting heterosexual couples in which neither partner is above the age of 60.[6] Before restricting the sample further, we re-code the top 1% of time use and earnings values to the 99th percentile, in order to avoid unduly influential observations.

We restrict our sample to couples in which both spouses are employed full time, defined as an average of at least 35 hours per week during the year. We discuss this decision in more detail below. However, as long as we adjust for the time spent in the labor force by spouses, our main results concerning compensatory gender display also hold in a sample restricted to husbands employed full time and wives employed part time (at least 20, but fewer than 35 hours per week), a sample of couples in which the wife works full time and the husband has any labor force status (including unemployed), and a sample of all couples in which the wife earns at least as much as her husband or will do so in the following year.

Although our results do not depend on analyzing only couples with two full-time workers, we present the results from this sample because in more heterogeneous samples

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[6] The PSID collects housework hours only for individuals designated the head of household or his wife or "wife." A woman is considered the "wife" of the head of household if the couple is unmarried but has been cohabiting for at least 12 months, or since the prior interview. Because housework hours are not collected for the same-sex partner of the head of the household, same-sex partners cannot be included in the analysis, nor can couples that do not include the household head. For brevity, we still refer to "spouses," "husbands," and "wives," although not all couples are married.

it is difficult to avoid confounding the effects of labor specialization and resources.

Studies that include couples with varying work hours typically include controls for the

weekly hours spent in market work by each spouse or for the employment status (part-

time, full-time, not employed) of each spouse in an attempt to distinguish the effects of

time and financial resources. However, because earnings are the product of wages and

labor market hours, this strategy will only be effective if the hours–housework

relationship is properly specified. For example, the relationship between wives' labor

market hours and time in housework may be non-linear, or may vary depending on the

husband's labor market hours. In this case, a linear control for the spouses' time in the

labor market will not fully adjust for differences in labor market time. Studying couples

in which spouses are relatively similar in their time availability allows us to evaluate how

spouses' housework hours change in response to changes in their earnings, holding

constant their employment status. The effect of employment changes on spouses'

housework hours has been discussed elsewhere and has not yielded results consistent

with the predictions of compensatory gender display (see, for example, Gershuny,

Bittman, and Brice 2005; Ström 2002).

## 2.4.1 Housework Hours

Following most previous evaluations of compensatory gender display, the key dependent

variable is the individual's weekly hours spent in housework. PSID respondents are

asked: "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?" This question

does not impose a specific definition of housework. Although we estimated analogous models for husbands' and wives' time in housework, we present only the results for wives' housework time in the main section. We found no evidence for compensatory gender display in any of the models of husbands' time in housework using our main analytic sample (see appendix at the end of this chapter).

## 2.4.2 Financial Resources

We measure spouses' financial resources with two separate variables – one for husbands' annual earnings and one for wives' annual earnings – to address evidence that wives' absolute earnings are a stronger determinant of their housework hours than are their husbands' earnings (Gupta 2006, 2007; Gupta and Ash 2008). Annual labor income, as constructed by the PSID, includes overtime and bonuses as well as regular pay. Annual earnings are standardized to 2008 dollars using the Consumer Price Index (CPI). The functional form of the wife's absolute earnings varies across models: first a single linear term is considered and then a linear spline with three knots. The knots are placed at $23,925, $33,671, and $47,939, corresponding to the 25th, 50th, and 75th percentiles of the weighted earnings distribution for wives. The spline specification constrains the relationship between wives' earnings and their housework time to be linear between any two knots of the spline, but allows for different slopes between different pairs of knots. This allows a flexible relationship between wives' earnings and their housework time. Husbands' earnings are constrained to have a linear relationship with the housework

hours of both spouses, for simplicity. Alternative models that allowed a spline

specification of husbands' earnings did not substantially alter the results.

We measure spouses' relative financial resources as the share of the couple's total

annual earnings that is provided by the wife. This reflects the view that spouses' current

financial contributions affect the division of household labor. We discuss the results

when spouses' relative wages are included in the discussion of alternative model

specifications. In the main models, we follow the standard specification of compensatory

gender display, including both a linear and quadratic term for the wife's share of the

couple's earnings (Bittman et al. 2003; Brines 1994; Evertsson and Nermo 2004;

Greenstein 2000; Gupta 2007).

*2.4.3 Control Variables*

In both the cross-sectional and panel models, we include covariates to adjust for time-

varying characteristics of couples that may be correlated with both the financial variables

and the household labor hours of each spouse. The first set of controls adjusts for life-

cycle effects. Binary variables for the presence of at least one, at least two, and at least

three children in the household, as well as a linear control for the age of the youngest

child, are included to control for the association between the presence of children and

women's household labor time (Baxter et al. 2008; Bianchi et al. 2000; Sanchez and

Thomson 1997). In the cross-sectional models, linear controls for the ages of both the

husband and the wife are included, as is a linear control for the year of the survey, to

account for differences in housework hours across both the life course and time periods.

In the panel model, only the control for the survey year is retained, due to the inability to separately identify age and period effects in fixed-effects models.

While the main models require that each spouse averages at least 35 hours of paid work per week during the year, we further control for the mean weekly paid work hours of each spouse, to adjust for residual differences in labor force hours. Previous analyses have often found a negative relationship between individuals' market labor time and their housework time and a positive relationship between individuals' market labor time and their spouses' housework time (Bianchi et al. 2000; Bittman et al. 2003; Evertsson and Nermo 2004). Weekly labor force hours are constructed by dividing the annual market work hours of the individual by 52. The values are then centered around 40.

We include an indicator variable for whether the couple owns their home, because home ownership may induce a preference for higher levels of domestic production and may also increase the amount of housework to be done.

Because the PSID collects all information in a given survey year from a single respondent, we also include a dummy variable that indicates whether the wife or another household member was the respondent in that year to guard against the potential for proxy response bias in spouses' reported housework hours (Achen and Stafford 2005; Berk 1985). Because each couple-year observation includes information from two different survey years (labor force outcomes for year $t$ are reported in survey year $t+1$), we include separate indicator variables for the respondent's identity in the year in which

the demographic and housework information was collected and for the year in which the labor force information was collected.[7]

Finally, our cross-sectional models include time-invariant characteristics of couples that have been found to be associated with spouses' housework hours: whether each spouse has a bachelor's degree and whether the husband is African-American or not.[8] More educated couples (Baxter et al. 2008; Presser 1994; Sanchez and Thomson 1997) and African-American couples (Pittman and Blanchard 1996; Sanchez and Thomson 1997) have been found to be more egalitarian in the division of household labor than their less educated or white counterparts. For couples that are missing information on the race of the husband or the education of either spouse in a given year, we use information from the closest preceding non-missing year to impute these values. If no such information is available, we use information from the closest subsequent year.

*2.4.4 Missing Data*

From the original sample of 21,674 couple-year observations in which both spouses are working full-time, 182 observations fail to report valid data on the wife's weekly

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[7] After the PSID switched to a biannual format in 1997, we use an average of the labor force outcomes in *t-1* and in *t+1* to construct an approximation of the labor force outcomes in *t*.

[8] Because the core PSID sample was drawn in 1968, the representation of respondents who are neither white nor African-American is small. Couples may be interracial, but the PSID did not begin asking the race of the wife until 1985, so we rely only on information about the race of the husband.

housework time and are excluded.[9] We drop 1,279 observations in which either spouse reports annual work hours and earnings that imply an hourly wage of less than $4 per hour (in 2008 dollars), as this is below the minimum wage in every year. In particular, of these observations, 527, or 41% of them, were likely unpaid workers in family businesses as they reported no earnings even though they reported working more than 35 hours per week. Models of wives' housework time that included observations with wages greater than $0 but less than $4 per hour produced results similar to those presented in the main models. Our final sample thus includes 5,059 couples, who are observed approximately 4.0 times each on average, for a total of 20,213 couple-year observations.

For covariates with non-zero missing data – race, education, the identity of the respondent, and home ownership – less than 2% of the sample has missing data. For education, race, and respondent identity, we create three dummy variables set to one if the observation lacks valid data for the item. The missing data dummy variable associated with a covariate is included in any model that includes the covariate. Only one observation is missing valid data for the home ownership variable. We re-code this observation into the "neither rents nor owns" group.

### 2.4.5 Analytic Approach

Our multivariate analysis proceeds in two stages. In the first stage, we document the relationship between wives' earnings and their time in housework, without including

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[9] We do not impute missing data because of the sensitivity of fixed-effects models to noise in the measurements.

a measure of spouses' relative earnings. We do this using three models. Our first model uses ordinary least squares (OLS) and a linear specification of both husbands' and wives' annual earnings. Our second model retains the linear specification of both spouses' earnings, but makes use of the panel nature of the PSID and is estimated using fixed effects. By comparing the results from these two models, we can assess the extent to which controlling for time-invariant attributes of couples affects our results. In particular, we are able to determine how much of the negative relationship between wives' earnings and their housework time can be attributed to unobserved differences between high-earning and low-earning wives, rather than to a causal relationship. Our third model retains the fixed-effects specification but specifies the relationship between wives' earnings and their housework hours as a linear spline with three knots.

In the second stage, we replicate the first three models, adding measures of spouses' relative incomes, to test whether the evidence for compensatory gender display is robust to the use of fixed effects and a more flexible specification of the relationship between wives' earnings and their time in housework.

Our OLS models are estimated with standard errors clustered at the couple level to account for the correlation across observations from the same couple. 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[10] fixed effects (   ), and time-varying individual-level variation (   ), as follows:

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Under OLS models, a single error term,                      , is implied. The clustered standard errors adjust for the lack of independence of the error terms for a given individual, since they share a common    .

However, the coefficients estimated by OLS models are only unbiased if it can be assumed that      is uncorrelated with **X**. If individuals' tastes for housework and preferences for the level of domestic production in the household differ systematically across couples in ways that are correlated with the observed predictors of individuals' housework time, there will be a correlation between these unmeasured individual-specific traits and the predictors of housework time, resulting in biased estimates of the OLS coefficients. For example, Oropesa (1993) finds that wives employed full time are less uncomfortable with an unclean house and like to cook less than other wives. Because the PSID does not measure attitudes toward domestic production, these attitudes and preferences are unobserved traits that may be correlated with spouses' labor force hours and earnings. Fixed-effects models eliminate bias in the estimates of the coefficients that is due to a correlation between the predictors and time-invariant individual-specific effects,    .

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[10] Because housework hours may be affected by relationship context, we assume that fixed effects for the individual are fixed only within the context of that couple, so we treat subsequent marriages as separate cases. Any couple that transitions from cohabitation to marriage is treated as an ongoing couple.

To summarize, we estimate one cross-sectional and two panel models, which differ by whether the relationship between wives' absolute earnings and their housework hours is constrained to be linear or is allowed to vary across the earnings distribution as a linear spline. We then test for evidence of compensatory gender display in each model, investigating the stability of the evidence when either panel models or more flexible functional forms are introduced.

Both descriptive statistics and regression results are presented making use of the PSID household weights, which are re-scaled to average one in the full sample of each year, to make the weights from different years comparable. For panel models, the weight must be constant for each couple, so we use the household weight from the first year the couple is observed.[11]

## 2.5. Results

In Table 2.1, descriptive statistics for our primary analytic sample are presented for three periods: 1976-1984, 1985-1993, and 1994-2003. The average couple in our sample is observed in their late 30s or early 40s and 8-9% of the couples include an African-American man. The fraction of husbands and wives with college degrees rises from 22% and 13%, respectively, in the early period to 31% and 29% in the late period. Husbands' median earnings are flat, falling between $49,500 and $51,500. Wives' earnings show growth, as expected from the greater labor force experience and education of women in

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[11] Using either no sample weights or the weight from the last observation of the couple yielded similar results.

the later period, increasing from \$30,810 in the early period to \$37,032 in the latest period. This growth is reflected in a modest rise in the median share of couples' earnings provided by wives, from 0.39 in the earliest period to 0.42 in the latest period. However, the fraction of wives earning more than their husbands rises substantially, from 15% to 26%, and the fraction of wives earning at least 50% more than their husbands rises from 4% to 9%. Hours spent in the paid labor market each week remain roughly constant for both husbands and wives. Husbands' median weekly hours increase slightly from 41.9 hours in the early period to 43.5 hours in the late period, and wives' median weekly hours rise from 38.5 to 39.2.

Husbands' average housework hours are stable around 7 hours per week while wives' average housework hours fall substantially, from 19.5 hours per week in the early period to 14.5 hours per week in the late period. The trends in wives' average time in housework observed in this sample follow trends documented elsewhere, although we find little change in husbands' housework hours over the period, while others have found a rise in men's housework time (Bianchi et al. 2000; Gershuny and Robinson 1988). We do, however, find a decline in the fraction of husbands who report doing no housework at all, from 15% in the early period to 8% in the late period.

### 2.5.1 Results for Linear Absolute Earnings

The earnings variables are the key independent variables of interest, so we discuss the results for these variables first. The first two columns in Table 2.2 report results from OLS and fixed-effects models that include a single linear term for the relationship

Table 2.1. Descriptive Statistics

|  | 1976-1984 (N=4824) | 1985-1993 (N=8403) | 1994-2003 (N=6986) |
|---|---|---|---|
| Median Husband's Annual Earnings | $50,787 | $51,200 | $49,650 |
|   Standard Deviation | ($28,364) | ($34,295) | ($38,368) |
| Median Wife's Annual Earnings | $30,810 | $33,245 | $37,032 |
|   Standard Deviation | ($15,983) | ($19,416) | ($21,606) |
| Median Wife's Share of Earnings | 0.39 | 0.41 | 0.42 |
|   Standard Deviation | (0.11) | (0.13) | (0.13) |
| Proportion of Wives Out-earning their Husbands | 0.15 | 0.21 | 0.26 |
| Proportion of Wives Earning at Least 50% more than their Husbands | 0.04 | 0.07 | 0.09 |
| Mean Husband's Weekly Housework Hours | 6.95 | 7.37 | 7.03 |
|   Standard Deviation | (6.61) | (6.31) | (5.89) |
| Mean Wife's Weekly Housework Hours | 19.53 | 16.14 | 14.46 |
|   Standard Deviation | (10.32) | (9.27) | (8.65) |
| Proportion of Husbands with no Housework Hours | 0.15 | 0.09 | 0.08 |
| Median Husband's Weekly Employment Hours | 41.92 | 43.19 | 43.48 |
|   Standard Deviation | (9.40) | (9.33) | (8.49) |
| Median Wife's Weekly Employment Hours | 38.46 | 38.85 | 39.23 |
|   Standard Deviation | (4.52) | (5.43) | (5.62) |
| Mean Husband's Age | 38.83 | 39.29 | 41.28 |
|   Standard Deviation | (10.46) | (9.40) | (9.26) |
| Mean Wife's Age | 36.26 | 37.04 | 39.34 |
|   Standard Deviation | (9.98) | (9.11) | (8.94) |
| Proportion of African-American Husbands | 0.08 | 0.09 | 0.08 |
| Proportion of Husbands with College Degree | 0.22 | 0.28 | 0.31 |
| Proportion of Wives with College Degree | 0.13 | 0.21 | 0.29 |
| Proportion of Couples Owning their Home | 0.79 | 0.79 | 0.83 |
| Proportion of Housework Reports Provided by Wife | 0.17 | 0.33 | 0.48 |

Notes: Reported sample sizes reflect couple-year observations.

between wives' earnings and their time in housework. Wives' earnings are significantly negatively related to their time in housework in both models, but the magnitude of the coefficient drops by 44% in the panel model. This suggests that a substantial portion of the observed negative association between wives' earnings and housework time in cross-sectional models is due to unobserved differences between high-earning and low-earning wives, such as differences in tastes for housework, rather than to a causal relationship between earnings and housework time. In the cross-sectional model, each $10,000 increase in a wife's earnings is associated with a predicted decrease in her weekly housework time of 0.82 hours (49 minutes), while in the panel model the predicted reduction is only 0.46 hours (28 minutes).

As expected from the autonomy perspective, wives' own earnings are stronger predictors of their housework hours than are their husbands' earnings. We are able to reject the null hypothesis that the coefficients on husbands' and wives' earnings are the same in both the cross-sectional ($F(1, 5058) = 62.45$, $p$-value $< 0.001$) and panel ($F(1, 5058) = 29.99$, $p$-value $< 0.001$) models. Thus, with the standard linear specification, there is support for the autonomy perspective. While the magnitude of the relationship between wives' earnings and their housework time is reduced in the fixed-effects model, it is not eliminated, and wives' earnings remain more important than husbands' in determining wives' housework time.

*2.5.2 Results for Spline Models*

The third column in Table 2.2 reports results from the fixed-effects model that specifies the relationship between wives' earnings and housework time as a linear spline. The relationship between wives' earnings and housework is strongly non-linear and the assumption of linearity is rejected ($F(3, 5058) = 7.41$, *p*-value $< 0.001$). In the first piece of the spline, for wives in the lowest quartile of the earnings distribution, weekly housework time falls 1.9 hours with a $10,000 annual earnings increase. In the second quartile, a $10,000 increase is associated with a decline of 1.0 hour per week. Past the median, at about $34,000, the rate of the decline in wives' housework hours is substantially reduced and never exceeds 0.25 hours for every $10,000 increase in earnings. The same earnings increase translates into housework reductions that are four times larger for a wife in the second quartile than for a wife above the median and nearly eight times larger for a wife in the first quartile than for a wife above the median.

If all wives reduced their housework hours at the rate implied by the lowest-earning wives, a change from the 10th percentile to the 90th percentile of the earnings distribution would decrease weekly housework hours by 8.8 hours. Holding the other covariates constant at their mean, and using the results of parallel models of husbands' housework time, this would have the effect of closing the gap in housework time between spouses by 75%, to 2.8 hours per week. Given the non-linearity, however, a change from the 10th to

Table 2.2. Results from Models of Wives' Housework Hours

| | Linear Earnings | Spline Earnings | |
| --- | --- | --- | --- |
| | Cross-Section | Panel | Panel |
| Annual Earnings | | | |
| His Annual Earnings[a] | -0.01 (0.04) | 0.06 (0.05) | 0.06 (0.05) |
| Her Annual Earnings[a] | -0.82 (0.08)*** | -0.46 (0.09)*** | N/A |
| First Quartile | N/A | N/A | -1.85 (0.44)*** |
| Second Quartile | N/A | N/A | -1.02 (0.38)** |
| Third Quartile | N/A | N/A | -0.21 (0.25) |
| Fourth Quartile | N/A | N/A | -0.25 (0.11)* |
| Children | | | |
| Age of youngest child | -0.10 (0.03)** | -0.17 (0.03)*** | -0.16 (0.03)*** |
| >=1 child | 3.46 (0.38)*** | 3.65 (0.37)*** | 3.59 (0.37)*** |
| >=2 children | 1.39 (0.30)*** | 0.71 (0.27)** | 0.69 (0.27)* |
| >=3 children | 2.03 (0.46)*** | 1.20 (0.49)* | 1.15 (0.48)* |
| Labor Force Hours | | | |
| Husband's LF Hours | 0.04 (0.02)* | 0.01 (0.01) | 0.00 (0.01) |
| Wife's LF Hours | -0.03 (0.03) | -0.03 (0.02) | -0.03 (0.02) |
| Year | -0.24 (0.02)*** | -0.13 (0.02)*** | -0.13 (0.02)*** |
| $R^2$ / $R^2$ overall | 0.16 | 0.10 | 0.10 |
| Rho (fraction of variance due to individual-specific fixed effects) | N/A | 0.61 | 0.60 |

Notes:  Results shown are regression coefficients with standard errors in parentheses. The sample includes 20,213 observations from 5,059 couples. In the cross-sectional models, standard errors are clustered at the couple level. All significance tests are two-tailed. All models also control for whether the couple owns their home, rents, or neither owns nor rents, and whether the wife or another member of her household was the respondent in each wave. The cross-sectional model also controls for the ages of each spouse, whether each spouse has a bachelor's degree, and whether the husband is African-American. The knots of the spline are placed at the 25th, 50th, and 75th percentiles of the weighted earnings distribution for wives:  $23,925, $33,671, and $47,939.
[a]Earnings are measured in $10,000s
*p<.05; **p<.01; ***p<0.001.

the 90th percentile of the earnings distribution only reduces wives' housework time by 2.9 hours and closes the housework time gap between spouses by only 31%, leaving a residual gap between spouses of 7.8 hours per week, even for wives at the 90th percentile of the earnings distribution. Thus, the ability of high-earning wives to achieve parity with their husbands in household labor is limited.

These results indicate a strong violation of the assumption of linearity that has traditionally been imposed in previous studies. At low levels of earnings, changes in wives' absolute earnings are associated with substantial changes in their housework hours. Past the median, however, the decline in housework hours associated with increases in earnings is much flatter.

Given the results from Table 2.2, compensatory gender display does not appear to be the only way to explain the high housework hours of high-earning wives. Instead, our results indicate that high-earning wives do not do *more* housework than other wives, and they do not do high levels of housework *because* of their high earnings. Rather, they spend considerable time in housework *in spite of* their financial resources: their earnings buy significantly less relief than a linear relationship between earnings and housework would predict.

*2.5.3 Non-linearity and its Implications for Compensatory Gender Display*

How might failing to account for the non-linearity shown in Table 2.2 lead to spurious evidence in favor of compensatory gender display? Imposing a linear relationship between wives' earnings and their housework time will over-predict housework hours for

wives at some points of the earnings distribution and under-predict them at other points. The differences between the predictions of the linear and spline specifications of wives' earnings are illustrated in Figure 2.1. The dotted line shows the predicted weekly housework hours of wives at various points in the earnings distribution, using the estimates of the constant linear specification panel model and holding covariates constant at their means. The solid line shows predicted weekly housework hours based on the spline panel model. The linear model under-predicts the housework hours of wives with the lowest earnings by 2.3 hours per week compared to the predictions of the spline model and over-predicts the housework hours of wives at the median by 0.6 hours. Thus, traditional linear models of wives' time in household labor under-estimate the household labor of wives with the fewest financial resources and over-estimate that of middle-income wives.

Additional analyses indicate that wives' absolute earnings are positively correlated with the share of family income that they provide (results not shown, available from the authors upon request). The bivariate correlation is 0.46, and non-parametric, smoothed (lowess) plots show a positive relationship between wives' absolute earnings and the wife's share of family income across the entire range of wives' earnings, although the relationship flattens out at higher earnings levels.[12] Thus, in models that constrain the relationship between wives' earnings and their time in housework to be linear, but allow the relationship between relative earnings and housework to be

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[12] This is in contrast to Gupta's (2007) suggestion that high-earning wives are concentrated among wives at the middle of the relative earnings distribution, but may be explained by the fact that our sample is limited to couples in which both spouses are working full time.
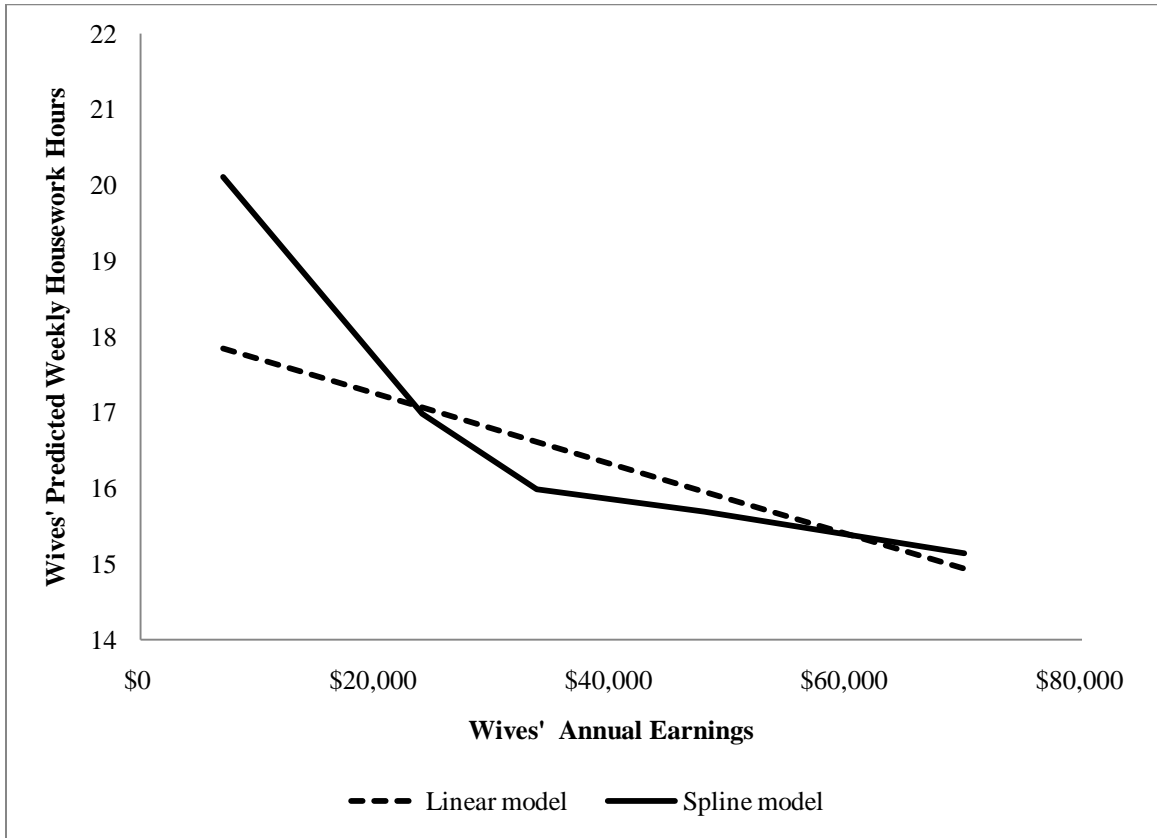
Figure 2.1. Wives' Predicted Weekly Housework Hours, by Earnings

quadratic, the quadratic term of relative earnings picks up a non-linearity in the relationship between absolute earnings and time in housework. Because the linear model under-predicts the weekly hours for low-earnings wives and over-predicts them for median earners, the quadratic term for relative earnings will correct these prediction errors as much as possible. A positive quadratic term for relative earnings, then, tends to increase predicted housework hours of low-earning wives, who tend to contribute the least to family income, while decreasing the predicted hours of wives near the middle of the earnings distribution, who tend contribute a moderate share to family income. This term is then frequently interpreted as providing evidence for compensatory gender display.

Given these results, findings from previous studies that are consistent with compensatory gender display may be an artifact of assuming a linear relationship between wives' earnings and their housework time. To test this hypothesis, we repeat the models shown in Table 2.2 but add the traditional linear and quadratic terms for the wife's share of family income. If ignoring the non-linear relationship between wives' earnings and their housework hours is the cause of evidence consistent with compensatory gender display, we would expect to see results consistent with compensatory gender display in the OLS and fixed-effects models that constrain the earnings-housework relationship to be linear, but not in the model that allows for a more flexible earnings-housework relationship. We discuss only the results for the measures of spouses' relative incomes, as the coefficients on the other variables are largely unchanged from the models that excluded the relative incomes measures.

*2.5.4 Results for Relative Earnings*

The first two columns of Table 2.3 present the results from the OLS and fixed-effects models of wives' housework hours with only constant linear terms for spouses' absolute earnings. In these models, the coefficients on the linear and quadratic terms for relative earnings are consistent with compensatory gender display: in both the panel and the cross-sectional model the linear term is negative and significant, and the quadratic term is positive and significant. In both models it is possible to reject the joint null hypothesis that both coefficients are zero (F(2, 5058) = 9.45, $p$-value < 0.001 in the cross-section, F(2, 5058) = 5.60, $p$-value = 0.004 in the panel). In both models, wives' minimum housework hours are predicted to occur when the wife provides more than half, but less than 100%, of the couple's income; in other words, wives who out-earn their husbands the most do more housework than wives who out-earn their husbands by less. Thus, both models support compensatory gender display.

The cross-sectional result contrasts with Gupta's (2007) finding that including separate (linear) terms for husbands' and wives' earnings eliminates the relationship between spouses' relative incomes and their housework hours. In our PSID data, we do not find this strategy sufficient to reject predictions of compensatory gender display. To test whether this result is due to our exclusion of couples in which the husband works less than full time, who are included in Gupta's sample but not in our own main sample, we repeat the cross-sectional analysis with a sample that restricts only the wife's employment to at least 35 hours per week and wages to at least $4 per hour, but allows

Table 2.3. Results from Models of Wives' Housework Hours, Including Relative Earnings

| | Linear Earnings | | Spline Earnings |
| --- | --- | --- | --- |
| | Cross-Section | Panel | Panel |
| Relative Earnings | | | |
| Wife's Share of Earnings | -18.42 (5.26)*** | -16.39 (5.12)** | -2.49 (6.58) |
| $(\text{Share})^2$ | 10.97 (4.84)* | 12.41 (4.76)** | 2.41 (5.50) |
| Annual Earnings | | | |
| His Annual Earnings[a] | -0.29 (0.08)*** | -0.12 (0.07) | 0.04 (0.09) |
| Her Annual Earnings[a] | -0.34 (0.13)** | -0.17 (0.12) | N/A |
| First Quartile | N/A | N/A | -1.74 (0.60)** |
| Second Quartile | N/A | N/A | -0.98 (0.42)* |
| Third Quartile | N/A | N/A | -0.19 (0.30) |
| Fourth Quartile | N/A | N/A | -0.24 (0.13) |
| Children | | | |
| Age of youngest child | -0.10 (0.03)** | -0.16 (0.03)*** | -0.16 (0.03)*** |
| >=1 child | 3.47 (0.38)*** | 3.62 (0.37)*** | 3.59 (0.37)*** |
| >=2 children | 1.33 (0.30)*** | 0.67 (0.27)* | 0.69 (0.27)* |
| >=3 children | 1.96 (0.46)*** | 1.17 (0.49)* | 1.15 (0.48)* |
| Labor Force Hours | | | |
| Husband's LF Hours | 0.04 (0.02)* | 0.00 (0.01) | 0.00 (0.01) |
| Wife's LF Hours | -0.03 (0.03) | -0.03 (0.02) | -0.03 (0.02) |
| Year | -0.23 (0.02)*** | -0.13 (0.02)*** | -0.13 (0.02)*** |
| $R^2$ / $R^2$ overall | 0.16 | 0.10 | 0.10 |
| Rho (fraction of variance due to individual-specific fixed effects) | N/A | 0.60 | 0.60 |

Notes:  Results shown are regression coefficients with standard errors in parentheses. The sample includes 20,213 observations from 5,059 couples. In the cross-sectional models, standard errors are clustered at the couple level. All significance tests are two-tailed. All models also control for whether the couple owns their home, rents, or neither owns nor rents, and whether the wife or another member of her household was the respondent in each wave. The cross-sectional model also controls for the ages of each spouse, whether each spouse has a bachelor's degree, and whether the husband is African-American. The knots of the spline are placed at the 25th, 50th, and 75th percentiles of the weighted earnings distribution for wives:  $23,925, $33,671, and $47,939.
[a]Earnings are measured in $10,000s
*p<.05; **p<.01; ***p<0.001.

the husband to have any level of labor force commitment. In this sample, too, the results are consistent with compensatory gender display. Gupta's (2007) finding that, in the NSFH, controlling separately for husbands' and wives' earnings suffices to eliminate the evidence in favor of compensatory gender display, does not appear to be robust with the PSID.

Here, the way in which our work moves beyond the specification of Gupta (2007) becomes important: the non-linearity of the relationship between wives' *absolute* earnings and housework hours entirely explains the non-linear relationship between wives' *relative* earnings and their housework hours. In the final column of Table 2.3, after including a more flexible specification of wives' earnings, the magnitudes of the coefficients on the relative resources terms shrink considerably, both the linear and the quadratic terms for the share of income provided by the wife are no longer significant, and it is no longer possible to reject the joint null hypothesis that the coefficients on both relative resources terms are zero ($F(2, 5058) = 0.10$, $p$-value $= 0.90$). This eliminates any evidence for compensatory gender display. However, the negative relationship between wives' absolute earnings and their housework time remains significant ($F(4, 5058) = 3.49$, $p$-value $= 0.008$) and the assumption of linearity is still strongly rejected ($F(3, 5058) = 4.04$, $p$-value $= 0.007$). Thus, consistent with the autonomy perspective, we find no evidence for compensatory gender display and find that wives' absolute earnings are always negatively associated with their housework hours. However, we also find, as in Table 2.2, that the housework reductions associated with increased earnings diminish considerably at higher levels of wives' earnings.

In summary, across specifications our results show that housework hours decline as earnings increase throughout the distribution of wives' earnings. However, at higher earnings levels, the reduction in housework when earnings increase is smaller than it is when earnings increase for wives below the median level of earnings. In contrast to compensatory gender display, high-earning wives do less housework than low-earning wives, even when they earn more than 50% of their family's total earnings.

*2.5.5 Results for Control Variables*

Because the relationships between the control variables and wives' housework hours are similar across the three models presented in Table 2.2, we discuss the results from all models together. In all models, a first child is associated with an average increase of around 3.5 hours per week of wives' housework, while the additions of second and third children have significant, but smaller positive associations with housework time. In both the cross-sectional and panel models, wives' housework hours decline modestly with increases in the age of the youngest child. Support for the time availability hypothesis is weak in this sample, as changes in neither husbands' nor wives' weekly labor market hours are significantly associated with changes in wives' time in housework in the panel models.

*2.5.6 Specification Checks*

We check both whether our results are robust to alternative model specifications and whether the results hold for subgroups based on race, education, age, marital status, and parental status, as well as for observations from different time periods. We discuss our alternative model specifications and the results in more detail in this section (full results available from the authors upon request).

One critique of the preceding results might be that they are the artifact of either an insufficiently flexible specification of the husband's earnings or relative earnings, or of the number and placements of the knots in the linear spline model. To address the first concern, we consider models that included the husband's earnings as well as the wife's as a linear spline, as well as models that specify both the wife's earnings and spouses' relative earnings as linear splines, always choosing knots that roughly divide the sample into quartiles. To address the second concern, we consider models that included up to six knots in the spline for wives' earnings. In these models there is no evidence consistent with compensatory gender display, and it is never possible to reject the joint null hypothesis of no relationship between the share of income provided by the wife and her housework hours.

Allowing additional knots in the earnings-housework relationship also allows us to explore more fully the shape of the non-linear relationship between wives' earnings and their time in housework. As in the main models, the median of the earnings distribution appears to be a key point of change: in the model with five knots, we find that in each of the three pieces of the spline below the median wives' housework hours

45

fall at least one hour per week for every $10,000 increase in annual earnings, while in the three pieces above the median they fall no more than 0.4 hours for every $10,000 increase in annual earnings. Again, the spline results support our finding that housework reductions associated with increased earnings are much smaller for high-earning wives than low-earning wives. We also consider models with alternative specifications of the dependent variable, using either the share of the spouses' total housework time that is done by the wife, or the difference between the spouses' housework hours. Neither of these alternative specifications provides evidence consistent with compensatory gender display.

For our race, education, age, marital status, parental status, and period subgroup analyses, we consider six pairs of subgroups: pre-1990 and post-1989 observations; couples in which the husband is African-American and those in which he is not; couples in which the wife has a bachelor's degree and those in which she does not; couples in which the wife is more than 40 years of age and those in which she is not; couples who have children and those who do not; and couples who are married as opposed to those who are cohabiting (in years in which it is possible to make this distinction). We find evidence consistent with compensatory gender display for only one of the six subgroup pairs – women married to African-American men. These results may suggest a need for greater attention in future research to differences by race in the evidence for compensatory gender display, although the smaller sample size of African-Americans makes us cautious in interpreting these results. In particular, the result is not significant when the analysis is further restricted to wives who earn at least as much as their husbands and whose husbands are African-American, suggesting that the result may

46

reflect a non-linear relationship between earnings share and housework hours for wives who are out-earned by their husbands, rather than that breadwinner wives spend more time in housework than those who have earnings parity with their husbands. Furthermore, one prediction of compensatory gender display is that wives' housework hours should continue to rise as they out-earn their husbands by greater amounts. However, we find no evidence that wives who are married to African-American men and substantially out-earn their husbands (by more than 50%) spend more time in housework than wives who out-earn their husbands by smaller amounts.

Note that the estimated coefficients in fixed-effects models are determined by the relationship of changes in couples' characteristics across years to changes in their housework hours across years. If there is little variation in spouses' earnings across years, these coefficients may be problematic, especially if couples are observed only a small number of times. To test this hypothesis, we repeat both our main models and all of our subsample analyses using OLS models that include the same spline in wives' earnings, as well as the control variables used in the OLS models presented in the main analysis. In both the full sample and all other subgroups, the results are entirely consistent with the results from the fixed-effects models: there is still no evidence for compensatory gender display, except among the women married to African-American men, and we again find a strongly non-linear relationship between wives' earnings and their time in housework. Therefore, our main conclusions are not dependent on our decision to use fixed-effects models.

To test the predictions of the relative resources perspective, we repeat the model from the third column of Table 2.3, but exclude the quadratic measure of spouses'

relative incomes. If the predictions of the relative resources perspective are correct, we would expect that the coefficient on the linear term would be negative and significant, but we find that it is positive and not significant in the panel model and negative and not significant in the cross-sectional model. As discussed earlier, bargaining power between spouses may also be thought of as determined by spouses' relative earnings power, typically measured as the ratio of their wages. Replacing the relative incomes measures with relative wages produces no evidence of either relative resources or compensatory gender display once we control for the non-linear relationship between wives' wages and their housework time. Therefore, we find no evidence for the relative resources perspective.

We consider the possibility that our results may be biased by the inclusion of proxy reports of wives' housework time. While we have included controls for whether the wife reported her own housework hours, it is possible that the extent of proxy response bias varies with the earnings of the wife. To test this hypothesis, we repeat the models from Table 2.2, column 3 and Table 2.3, column 3, restricting the sample to couples in which the wife was the respondent for both her housework hours and the spouses' earnings. There is no evidence in favor of compensatory gender display in this sample, and again wives' housework hours fall most rapidly with earnings increases when they are in the first quartile of the earnings distribution and least rapidly when they are above the median. Furthermore, we repeat the model from Table 2.2, column 3, which excludes the relative earnings terms, and allow the respondent's identity to interact with the coefficients on wives' earnings. The estimated earnings coefficients do not differ significantly depending on whether the husband or the wife was the respondent,

suggesting that proxy response bias is not responsible for the estimated coefficients in the main models.

Lastly, we performed several supplemental analyses using the measure of expenditures on food away from home (the only market substitute about which the PSID collects information). We find no evidence of a non-linear relationship between wives' earnings and household expenditures on food away from home. Furthermore, models that control for expenditures on food away from home show the same non-linear pattern observed in the main models.

*2.5.7 Limitations*

The present study has a few limitations. In terms of measurement, we lack information on wives' time spent in child care, which is an important component of wives' non-market work. However, the exclusion of time in child care from analyses of housework time is standard (Coltrane 2000), including in previous assessments of compensatory gender display. This exclusion is in part because it is not possible to separate the leisure and labor components of child care (Blair and Lichter 1991), and evidence suggests that parents view time with children differently from either housework or leisure (Guryan, Hurst and Kearney 2008).

Analytically, while fixed-effects models account for unobserved time-invariant differences across couples, they cannot prevent bias introduced by a correlation between the individual-year error term and the covariates. For example, the PSID does not include annual measures of gender role attitudes, a variable that may be associated with both

wives' earnings and their time in housework. Any time-invariant component of this measure – an individual's average attitudes during the period she is observed – will be absorbed by the fixed effects and will not affect our results. However, year-to-year fluctuations in gender role attitudes may be correlated with changes in both housework hours and earnings, and the fixed effects do not account for this correlation.

Lastly, while we have established that a negative and non-linear relationship exists between wives' earnings and their housework time, we acknowledge that it is not possible for us to determine the causal mechanism responsible for this relationship. Wives may decrease their time in housework as their earnings rise either because they are outsourcing domestic labor or because they are foregoing housework without purchasing a substitute for their own time. Similarly, it is not possible to determine whether the non-linear relationship between wives' earnings and their time in housework is due to a general discomfort with outsourcing, a reluctance to outsource or forego household tasks with symbolic significance, missing markets for some forms of outsourcing, distrust of providers of substitutes for household labor, or some other reason. Thus, further research is needed to identify the causal mechanism responsible for these relationships.

2.6. Discussion and Conclusion

Our results highlight both the importance of and limits of financial resources in shaping wives' time in household labor. Consistent with the autonomy perspective, we find that wives' housework time declines with earnings increases at every point in the earnings distribution. This implies that wives have achieved partial success in changing the terms

of the heterosexual partnership, as they are able to reduce their domestic labor when their financial contributions to the marriage are high. In other words, wives have some discretion in the type of goods – financial or domestic – that they provide to a partnership. This is consistent with work indicating that conceptions of appropriate behavior for women now include paid labor as well as domestic production (Riggs 1997; Sayer 2005), and that husbands enjoy the financial rewards provided by their wives' careers (Atkinson and Boles 1984). Clearly, individual financial resources matter.

However, we estimate a smaller effect of wives' earnings on their housework time than is hypothesized by the simplest form of the autonomy perspective. First, we find that this relationship is reduced considerably in the panel models, indicating that it is explained in part by unobserved differences between wives with low and high earnings, rather than being exclusively due to increased out-sourcing or foregoing of domestic labor as wives' earnings rise. Second, we find that low-earning wives decrease their housework hours more than others as their earnings increase, while increased earnings above the median of the wives' earnings distribution lead to only small reductions in household labor time. If wives' time in housework were the result of a straightforward market decision, we would not expect so little additional decline in housework as wives' earnings rise past the median of the earnings distribution. While wives' housework time falls as their earnings rise throughout the earnings distribution, the overall decline is modest.

Our data do not permit us to determine whether the constraints on wives' housework reductions emerge due to wives' desire to do housework in order to "do gender" (Berk 1985; West and Zimmerman 1987), or to express love for family members

(DeVault 1991), or because of limits in the outsourcing of household production that are not due to gender norms, such as the lack of availability of substitutes for certain types of household labor. What is certain, however, is that wives experience a limitation in housework reductions that does not apply to husbands. That is, there is something about the experience of being a wife, as opposed to a husband, that causes even high-earning wives to spend considerably more time in housework than their husbands, even when they out-earn them. Thus, even causal mechanisms that are gender-neutral in theory have gender-asymmetric effects on spouses' housework time, as it is wives, not husbands, who perform the majority of household labor that is not outsourced or foregone by couples. As a result, wives cannot fully compensate for their disadvantaged role *as women* by leveraging their advantaged financial position. In other words, women cannot easily buy their way to equality with men when it comes to household labor responsibilities.

In addition to calling for greater attention to limits in wives' ability to outsource or forego domestic labor, our work questions the predictions of compensatory gender display. Once we have accounted for the non-linear relationship between wives' absolute earnings and their housework time, we find no evidence of compensatory gender display. In contrast to the predictions of compensatory gender display, we find no evidence that wives are penalized at home for their success in the labor market: in terms of household labor, it is never *worse* to earn more. Thus, contrary to compensatory gender display, wives' earnings are best seen as a resource for reducing household labor, not as a liability.

While rejecting the narrow hypothesis of compensatory gender display, our findings highlight the importance of the gendered division of household labor in shaping

the behavior of women at all income levels. The continued high levels of housework by high-earning wives show that more than money is needed for wives to achieve parity with their husbands in household labor time. Furthermore, our results indicate not only the limits of financial resources in determining wives' time in housework, but also heterogeneity in the ways in which gender and financial resources interact to shape women's lives: low-income wives are constrained to perform domestic labor by their *lack* of financial resources, while high-income wives are constrained *in spite* of them.

2.7. Appendix

Table 2.4. Results from Models of Husbands' Housework Hours, Including Relative Earnings

| | Linear Earnings | | Spline Earnings |
| | Cross-Section | Panel | Panel |
| --- | --- | --- | --- |
| Relative Earnings | | | |
| Wife's Share of Earnings | 2.85 (3.49) | 0.99 (3.54) | -1.79 (4.38) |
| (Share)$^2$ | -1.25 (3.51) | 1.69 (3.31) | 3.45 (3.81) |
| Annual Earnings | | | |
| His Annual Earnings[a] | -0.05 (0.05) | 0.01 (0.05) | -0.03 (0.05) |
| Her Annual Earnings[a] | 0.07 (0.09) | -0.05 (0.10) | N/A |
| First Quartile | N/A | N/A | -0.10 (0.36) |
| Second Quartile | N/A | N/A | 0.38 (0.28) |
| Third Quartile | N/A | N/A | 0.20 (0.20) |
| Fourth Quartile | N/A | N/A | -0.14 (0.11) |
| Children | | | |
| Age of youngest child | -0.08 (0.02)*** | -0.10 (0.02)*** | -0.10 (0.02)*** |
| >=1 child | 1.40 (0.26)*** | 1.59 (0.27)*** | 1.60 (0.27)*** |
| >=2 children | 0.46 (0.21)* | 0.14 (0.20) | 0.13 (0.20) |
| >=3 children | 1.30 (0.31)*** | 0.12 (0.31) | 0.13 (0.31) |
| Labor Force Hours | | | |
| Husband's LF Hours | -0.07 (0.01)*** | -0.04 (0.01)*** | -0.05 (0.01)*** |
| Wife's LF Hours | 0.04 (0.02)* | 0.03 (0.01)* | 0.03 (0.01)* |
| Year | 0.05 (0.01)*** | 0.03 (0.02) | 0.03 (0.02) |
| $R^2$ / $R^2$ overall | 0.06 | 0.06 | 0.06 |
| Rho (fraction of variance due to individual-specific fixed effects) | N/A | 0.56 | 0.56 |

Notes: Results shown are regression coefficients with standard errors in parentheses. The sample includes a subset of the main analytic sample, restricted to observations in which the husband provides a report of his weekly housework time. This subsample includes 20,119 observations from 5,034 couples. In the cross-sectional models, standard errors are clustered at the couple level. All significance tests are two-tailed. All models also control for whether the couple owns their home, rents, or neither owns nor rents, and whether the wife or another member of her household was the respondent in each wave. The cross-sectional model also controls for the ages of each spouse, whether each spouse has a bachelor's degree, and whether the husband is African-American. The knots of the spline are placed at the 25th, 50th, and 75th percentiles of the weighted earnings distribution for wives in the full sample: $23,925, $33,671, and $47,939.
[a]Earnings are measured in $10,000s
*p<.05; **p<.01; ***p<0.001.

**Opting Out and Buying Out: Wives' Earnings and Housework Time[13]**

Wives continue to spend more time than their husband doing housework, even when both spouses work full-time (Kamo 1988; Killewald and Gough 2010). For these couples, domestic labor is a source of gender stratification, as it contributes to unequal leisure time between spouses. Furthermore, wives' time in housework is negatively associated with wages, making women's greater domestic burden a contributor to the gender gap in wages (Hersch and Stratton 1997; Noonan 2001).

Thus, it is natural to ask what resources wives may use to reduce their time in household labor. Existing studies indicate that wives' earnings are negatively associated with their time in housework, even after controlling for time spent in market work (Gupta 2006, 2007; Killewald and Gough 2010). Given that wives' earnings are positively associated with household expenditures on market substitutes for their household labor and negatively associated with their time in housework, it has been hypothesized that wives' earnings allow them to outsource household production (Cohen 1998; de Ruijter, Treas, and Cohen 2005; Gupta 2006, 2007; Gupta and Ash 2008): wives use their

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earnings to "buy out" of time in housework. Nevertheless, the explanatory power of the buying-out hypothesis has, to my knowledge, never been directly tested. An alternative cause of the negative earnings – housework relationship is that higher earnings lead wives to reduce their household labor hours, without purchasing a market substitute for their own time. In other words, high earners "opt out" by doing less housework.

Testing the buying-out hypothesis has been difficult because most datasets do not include information on both housework time and household expenditures. Brines (1994), using data from the Panel Study of Income Dynamics (PSID) found that expenditure on dining out relative to food consumed at home was negatively associated with wives' time in housework, but the PSID lacks data on expenditures on other types of market substitutes for housework. By linking time use and expenditure measures from the Consumption and Activities Mail Survey (CAMS) to earnings measures from its parent study, the Health and Retirement Study (HRS), I directly measured the extent to which use of market substitutes is associated with wives' time in household labor. Furthermore, by comparing the results of models of wives' housework time before and after the inclusion of a measure of use of market substitutes, I measured the extent to which these expenditures explain the negative relationship between wives' earnings and their housework time.

Understanding whether wives' earnings lessen the time they spend in household labor primarily by increasing spending on market substitutes has implications for understanding the intersection between household responsibilities and market work. Acknowledging that wives may not fully compensate for their reduced housework time by increased use of market substitutes introduces a new parameter into the household

decision-making model: the level of household production. Households make choices about, for example, the degree of cleanliness of the home and the quality of the food that household members consume. Household labor need not be viewed as purely a task of allocation between spouses or between private production and purchased commodities. Rather, households are also making trade-offs between the amount of domestic production and the amount of other items the household values, including leisure time and consumption goods.

3.1 Buying Out and Opting Out

The relationship between wives' earnings and their time in housework is both statistically significant and practically large. Using data from the National Survey of Families and Households (NSFH), Gupta (2006) found that, compared to wives in the lowest quartile of the earnings distribution, those in the top quartile spent 13 fewer hours per week in housework, implying a 40% reduction. Among couples in which both spouses worked full-time, wives in the second quartile of the earnings distribution spent 26 hours per week in housework, compared to 18 hours for women in the highest quartile, a 30% reduction. Thus, the relationship is not entirely due to differences in labor market work by high- and low-earning wives.

Households face decisions about both the use of their members' time and the ways to spend available financial resources. Domestic production, such as meals and a clean home and clothes, can be created either directly, through the time inputs of household members, or indirectly, using the financial rewards from market work to

purchase the services of others. If wives' earnings are negatively related to their housework hours, this may be because earnings are negatively correlated with the total amount of household production, or because they are positively correlated with the fraction of domestic production that is outsourced or allocated to other household members, or both.

The buying-out explanation for the negative relationship between wives' earnings and their housework hours suggests that wives' earnings give them the purchasing power to buy market substitutes for their own household labor. This explanation focuses on the positive correlation between wives' earnings and the fraction of domestic production that is outsourced. The assumption that higher-income households use their financial resources to purchase market substitutes that allow household members to reduce their own time in housework is frequently given as a reason to control for household income in models of individuals' time in housework (e.g., Bittman et al. 2003; Brines 1994; Evertsson and Nermo 2004). The buying-out hypothesis is also invoked in recent studies that recognize the distinct effects of husbands' and wives' earnings on wives' housework time (Gupta 2006, 2007; Gupta and Ash 2008).

The buying-out hypothesis is appealing in part because it draws on a standard economic model of consumption: conditional on the amount of time spent in the labor force, wives with higher earnings have greater financial resources to outsource domestic labor, thereby "purchasing" additional leisure time for themselves. Assuming that wives enjoy leisure more than housework and again conditioning on time in paid work, wives with greater earnings should devote less time to housework and more to leisure. If wives use their earnings to buy out of time in household labor, we would expect that household

expenditures on market substitutes for household labor rise with household income, as richer households have greater financial resources to "purchase" the wife's leisure time. This does not imply that there are no constraints on households that prevent them from outsourcing household labor, merely that, all else equal, households with more financial resources are better able to overcome these constraints and reduce the wife's time in household labor than are households with fewer financial resources.

Furthermore, the wife is likely to value her own leisure time more highly than her husband does. Even if spouses pool their incomes, existing evidence indicates that spending on goods that wives value or within wives' sphere of traditional responsibility rises more quickly with wives' earnings than with husbands' (Phipps and Burton 1998). As a result, the buying-out hypothesis predicts that expenditures on market substitutes for wives' household-labor time rise more quickly with wives' earnings than with husbands'. As expected, couples' spending on market substitutes for housework typically performed by women, including child-care services and cleaning services, have been found to have a stronger positive association with wives' earnings than with husbands' (Cohen 1998; Oropesa 1993; Phipps and Burton 1998; Soberon-Ferrer and Dardis 1991), although some studies have found that dining out responds equally strongly to husbands' and wives' earnings (Cohen 1998; Oropesa 1993). This is also consistent with past evidence that wives' time in housework falls more rapidly with their own earnings than with the earnings of their husbands (Gupta 2006, 2007; Gupta and Ash 2008; Killewald and Gough 2010).

If use of market substitutes is an important mechanism by which wives reduce their time in household labor as their earnings rise, use of market substitutes should be

strongly negatively related to wives' time in housework. Furthermore, if the use of market substitutes fully explains the negative relationship between wives' earnings and their housework hours, then models of wives' housework time that include a measure of households' use of market substitutes should show no remaining association between wives' earnings and their time in housework. In other words, the link between wives' earnings and their time in housework is entirely indirect, operating through increased expenditures on market substitutes.

Buying out, however, is not the only possible source of the negative relationship between wives' earnings and their housework hours. It is possible that housework hours are lower for wives with higher earnings because the average level of domestic production in their households is lower. This may occur for two reasons: opting out and selection. Wives may respond to earnings increases by opting out of housework, foregoing time in household labor without purchasing a market substitute. Wives with higher earnings may have a preference for lower levels of domestic production because the rewards for high levels of domestic production are not as great for them. High-earning wives may face less social pressure to perform the traditionally female tasks of household production, as has sometimes been suggested (Gupta 2006, 2007; Gupta and Ash 2008). All women may feel pressure to perform housework as a way to "do" gender and to express affection for household members (Berk 1985; DeVault 1991; Hochschild 1989; West and Zimmerman 1987), but this pressure may not operate with equal force on all women. High-earning women may be particularly likely to derive more personal satisfaction and social status from their labor market roles, making the status ascribed for performing traditional household production less important.

Furthermore, the effort required to procure adequate, trustworthy substitutes for wives' time in household labor, as well as reluctance to trust service providers (especially when their behavior is not easily observed by their employers) may reduce households' desire to outsource many aspects of market production (de Ruijter et al. 2003). Therefore, households that choose to reduce wives' time in housework may still not be willing to bear the financial and non-financial costs of purchasing market substitutes and may choose instead to reduce household production. If wives are both opting out and buying out in response to earnings increases, expenditures on market substitutes will only partially mediate the earnings-housework relationship, and models that include a control for expenditures on market substitutes will continue to show a residual association between wives' earnings and their housework time.

It is also possible that the observed negative relationship between wives' earnings and their time in housework is entirely spurious, due to differences among women in taste for housework or domestic production that are correlated with, but not caused by, differences in earnings. In this case, too, we would expect to see lower levels of domestic production and less time spent in housework for wives with higher earnings and controlling for expenditures on market substitutes would not eliminate the earnings-housework relationship.

There is some evidence that use of market substitutes may reduce women's time in housework. Van der Lippe, Tijdens, and de Ruijter (2004), using data from the Dutch National Time Budget survey, found that frequency of takeout meals is associated with lower time cooking for both men and women and that use of cleaning services reduces women's time cleaning. Similarly, Bittman, Rice, and Wajcman (2004), using the

Australian 1997 Time Use Survey, found that having hired someone to clean the home in the previous two weeks was associated with significantly less total time in housework for women, although the number of times restaurant or takeout meals were purchased over the same period was not. Nonetheless, neither study tested whether the use of these market substitutes explained the negative association between women's earnings and their housework time.

3.2 Method

The sample was drawn from the Health and Retirement Study (HRS) (Health and Retirement Study; RAND HRS Data, Version J). The Consumption and Activities Mail Survey (CAMS), which in each wave included a subsample of households from the HRS sample, asked individuals about their own time in housework and household expenditures on various items. The 2003, 2005, 2007, and 2009 waves of CAMS were merged with the HRS Core surveys of 2002, 2004, 2006, and 2008. In the 2005-2009 waves of CAMS, the spouses of primary respondents were asked to complete a shorter interview that included measures of the respondent's time use, but did not repeat the household expenditure measures. In these cases, I imputed to both spouses the level of household expenditure that the primary household respondent reported. For the periods considered here, the HRS is a representative sample of the U.S. population over the age of 50, although the sample included younger spouses as well. This article highlights the experiences of a relatively under-studied group: mature couples, typically living without young children in the

household, but before retirement. As the population ages, understanding the experiences of this population becomes increasingly valuable.

In the analyses that follow, the dependent variables are wives' self-reported time in meal preparation and clean-up (cooking) and cleaning house, washing, ironing, and mending (cleaning) in the week before the survey. Together, these tasks – cooking, doing dishes, ironing, washing, and cleaning house – account for more than 70% of women's time in non-care household activities, including for women aged 45-64 (Krantz-Kent 2009).

*3.2.1 Measuring Substitution*

Before discussing the measure of market substitutes employed here, it is useful to think about what such a measure would ideally capture. By "market substitutes," I mean any good or service that an individual purchases in order to increase household production, without performing household labor herself. Purchasing takeout food, hiring someone to clean the home or mow the lawn, or sending shirts to a laundry are all examples of using market substitutes, sometimes also referred to as outsourcing. In order to test the extent to which wives substitute purchased services for their own time in housework, it would be ideal to determine the amount of wives' own time that the purchased goods replace. For example, if a wife hires a domestic cleaner, what we wish to know is how many hours the wife would have spent to accomplish the work done by the cleaner.

There is not, of course, such an ideal measure. Instead, households' expenditures on dining out (cooking) and housekeeping or laundry services (cleaning) are here

considered to be expenditures on market substitutes for women's housework time. This is consistent with existing research (Cohen 1998; de Ruijter et al. 2005; Oropesa 1993; Treas and de Ruijter 2008), although spending on laundry services is sometimes excluded (Cohen 1998; Oropesa 1993).

In CAMS, individuals were asked to report their expenditures on "Housekeeping, dry cleaning and laundry services: hiring costs for housekeeping or home cleaning, and amount spent at dry cleaners or laundries" and "Dining and/or drinking out: items in restaurants, cafes, and diners, including take-out food." This measure is not without limitations. Expenditures in these areas may increase without reducing wives' housework time. For example, consuming alcoholic beverages in restaurants rather than at home would register as increased spending on food away from home, but would be a poor measure of money spent reducing wives' time cooking. Likewise, a household may spend more money on laundry services simply because the members have purchased more clothes that require dry-cleaning.

Additionally, particularly for food, variation in spending on market substitutes may reflect variation in the quality of services purchased rather than the amount of the wife's time that is replaced. Alternatively, wives' greater earnings may lead to greater tolerance of food that is purchased but ultimately goes uneaten by household members. These effects introduce measurement error into the substitution measure, which will lead to a downward bias in the estimated relationship between the use of market substitutes and wives' housework time.

To the extent that increasing expenditures on market substitutes reflect quality increases and tolerance for wastage, a similar effect should also lead to increased spending on groceries and, to a lesser extent, cleaning supplies, which are complements to rather than substitutes for wives' housework time. CAMS also asked individuals about expenditures on "Housekeeping supplies: cleaning and laundry products" and "Food and beverages: food and drinks, including alcoholic, that you buy in grocery or other stores." To capture more accurately the extent of substitution for the wife's housework, a measure of the share of the household's expenditures in the domain of cooking or cleaning that is spent on market substitutes, rather than complements was constructed. This is given as follows:

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Brines' (1994) measure of spending on restaurant meals relative to spending on food prepared at home is similar in spirit. It is expected that this measure is more strongly related to wives' housework time than is the absolute level of expenditures on substitutes, as it more strongly indicates the extent to which market substitutes, rather than complements, are used. Nonetheless, the measure is still not perfect. For example, if wives choose to purchase prepared foods that are more expensive than raw ingredients but require relatively less time to cook, this inflates expenditures on groceries, but in fact indicate greater use of market substitutes.

*3.2.2 Model Specification*

The analytic technique was ordinary least squares (OLS). Wives' hours spent cooking and cleaning in the previous week were the dependent variables. Wives' annual earnings in the calendar year before the HRS survey were the primary independent variable. Husbands' earnings (also in the calendar year before the HRS survey), the usual weekly labor market hours of each spouse in main and secondary jobs at the time of the HRS survey, and dummy variables for whether the wife is African-American and whether each spouse has a bachelor's degree were included as covariates. Because race, education, and labor-force participation are correlated with household expenditures on market substitutes and with housework time (Baxter et al. 2008; Bellante and Foster 1984; Cohen 1998; de Ruijter et al. 2005; Pittman and Blanchard 1996; Sanchez and Thomson 1997; South and Spitze 1994), but are also associated with earnings, failure to control for these variables would risk confounding their effects on expenditures and housework time with those of earnings.

Two measures were used to adjust for differences across households in the demands for domestic production caused by people and space: the number of rooms in the family's home and a dummy variable set to 1 if members of the household include anyone other than the couple.

A measure of the wife's employment history and the number of children ever born to the wife were included as indicators of her relative taste for home production as opposed to market work. Furthermore, a wife's employment and fertility history may affect the household's current division of labor, net of current employment hours, if

household roles negotiated earlier in the marriage shape her own and her husband's expectations of behavior later in life. The employment history measure was constructed by dividing the wife's years of employment to date by the difference between her present age and 14. This approximates the share of the wife's adult life that she has spent employed, although it is a coarse measure and subject to measurement error in respondents' reported work history.

Of the 12,052 observations of women from CAMS, 237 (2.0%) were excluded because their information could not be matched to HRS reports from the previous calendar year. In order to focus the analysis on married, working-age couples, women whose marital status was something other than married were excluded (5,513 observations, 45.7%), as were couples in which either spouse was over the age of 65 (4,084 observations, 33.9%). Couples in which either spouse was not in the labor force at the time of the HRS survey or reported no earnings in the previous calendar year, were excluded from the analysis (1,374 observations, 11.4%). This restricts the focus of the analysis to dual-earner couples. Retired couples were excluded because their earnings are a poor measure of their financial resources. Household sample weights of 0 led to the exclusion of 11 observations (0.09%). An additional 11 observations (0.09%) were excluded because they were missing data on one of the variables used to define the sample: marital status, or the age, earnings, or retirement status of either spouse.

After excluding couples who did not meet the age or employment criteria, 822 couple-year observations remained in the sample. Of these, 12 observations (1.5% of the remaining sample), were removed because of reporting no expenditure at all in the domains of either cooking or cleaning, suggesting that these couples have some other

means of household production that direct spending on household goods or services does not capture. Missing values on the dependent variable – the wife's time in housework – led to a loss of 14 observations, or 1.7% of the remaining sample. The final analytic sample included 796 observations from 449 wives.

In the analytic sample, 2.1% of the observations were missing data on the usual hours spent in paid work by the husband and 2.3% were missing this information for the wife. The number of rooms in the house was missing for 7.2% of the sample and the number of children ever born was missing for 0.1% of the sample. Lastly, 5.6% of the sample was missing data on one of the expenditure variables related to cleaning, and 5.1% was missing data on one of the cooking expenditure variables. For each of these variables, an indicator variable was created that is set to one if the observation is missing data on this covariate. The indicator for missing data was included in any model that includes the associated covariate.

The HRS household-level weights, normalized to average one in each year in the full sample of HRS households in each wave, were used to weight the sample in all analyses. Because wives may be represented in multiple waves, all analyses clustered the standard errors at the individual level. The top 5% of both time-use and financial variables were re-coded to the 95th percentile, as were the variables for the number of rooms in the home and the number of children ever born. In order to adjust for inflation during the period, financial variables were scaled to 2009 dollars.

3.3 Results

Table 3.1 presents descriptive statistics for the sample. The mean annual earnings were $43,111 for wives and $68,352 for husbands. Husbands also spent somewhat more time in the labor market – an average of 44.9 hours per week, compared to 38.0 hours for wives. Thus, in this sample of dual-earner couples, husbands both substantially out-earned their wives and were more engaged in the labor market. A bachelor's degree was held by 36% of wives and 40% of husbands. African-Americans comprised 4% of the sample. Couples had an average of 7.2 rooms in their homes, and 44% of couples lived with at least one other resident. On average, wives had spent 73% of their years since age 14 in the labor market and had given birth to 2.2 children. Wives were 54.1 years old on average, compared to 56.8 years old for husbands.

Wives spent an average of 6.6 hours per week cooking and 8.0 hours per week cleaning and doing laundry. Almost all households reported some spending on groceries, cleaning supplies, and dining out: more than 97% of the sample households reported some expenditure on each of these items. By contrast, only 56% of households reported any spending on housecleaning or laundry services in the previous year. Among those who buy such goods or services, mean monthly expenditures were $66 for laundry and housecleaning services, $35 for cleaning and laundry supplies, $217 for dining out, and $485 for groceries.

Table 3.1. Spouses' Characteristics: Descriptive Statistics (N = 796)

| Variables | M | SD |
|---|---|---|
| Wife's annual earnings | $43,111.17 | $31,828.80 |
| Husband's annual earnings | $68,351.66 | $43,050.04 |
| Wife has bachelor's degree[a] | 0.36 | 0.48 |
| Husband has bachelor's degree[b] | 0.40 | 0.49 |
| Wife is African-American[c] | 0.04 | 0.20 |
| Wife's weekly labor market hours | 38.03 | 11.67 |
| Husband's weekly labor market hours | 44.93 | 11.63 |
| Residents other than couple[d] | 0.44 | 0.50 |
| Rooms in the home | 7.23 | 1.86 |
| Share of wife's adult years employed | 0.73 | 0.20 |
| Number of children ever born to wife | 2.16 | 1.31 |
| Wife's age | 54.05 | 4.91 |
| Husband's age | 56.81 | 4.51 |
| Wife's weekly hours cooking | 6.62 | 4.29 |
| Wife's weekly hours cleaning | 7.97 | 5.44 |
| Monthly spending on cleaning/laundry services, if positive | $66.32 | $77.85 |
| Monthly spending on cleaning/laundry supplies, if positive | $35.33 | $27.41 |
| Monthly spending on dining out, if positive | $217.32 | $177.50 |
| Monthly spending on groceries, if positive | $485.34 | $255.98 |

[a]Wife has bachelor's degree: 0 = no; 1 = yes. [b]Husband has bachelor's degree: 0 = yes. [c]Wife is African-American: 0 = no; 1 = yes. [d]Residents other than couple: 0 = no; 1 = yes.

*3.3.1 Multi-variate Results*

Table 3.2 presents two models of wives' time in household labor. Model 2 is identical to Model 1, except that it also includes the measure of the household's level of use of market substitutes. The reduction in the size of the coefficient on wives' earnings between Model 1 and Model 2 indicates the extent to which greater use of market substitutes in households in which wives have higher earnings explains the relationship between wives' earnings and their time in housework.

In Model 1, wives' earnings were significantly negatively associated with their time in both cooking and cleaning, consistent with existing evidence. For each $10,000 increase in a wife's annual earnings, her weekly time in cleaning was predicted to be 0.21 hours (13 minutes) lower, and her weekly time cooking was predicted to be 0.19 hours (11 minutes) lower. Consistent with past research, in models of wives' time in both cleaning and cooking, it was possible to reject the null hypothesis that the coefficients on husbands' and wives' earnings are equal ($F(1, 448) = 7.49$, $p$-value $< 0.01$ and $F(1, 448) = 8.65$, $p$-value $< 0.01$), and in fact husbands' earnings were positively and not significantly associated with wives' time both cooking and cleaning. This suggests that increased husbands' earnings do not translate into less housework for wives.

Model 2, of Table 3.2, presents the results from the models that include the measure of the household's use of market substitutes. For both cooking and cleaning, wives' housework time was predicted to be lower when their households made greater use of market substitutes, but the relationship was weak and only marginally significant in the model of time cleaning. An increase of 1 percentage point in a household's reliance

Table 3.2. Summary of OLS Regression Analyses for Wives' Weekly Housework Hours (N = 796)

| | Cleaning | | Cooking | |
| --- | --- | --- | --- | --- |
| | Model 1 | Model 2 | Model 1 | Model 2 |
| Wife's annual earnings ($10Ks) | -0.21 (0.08)** | -0.19 (0.08)* | -0.19 (0.06)** | -0.18 (0.07)** |
| Husband's annual earnings ($10Ks) | 0.07 (0.05) | 0.09 (0.05)† | 0.06 (0.05) | 0.06 (0.05) |
| Wife has bachelor's degree[a] | -2.21 (0.48)*** | -2.12 (0.48)*** | -0.35 (0.44) | -0.26 (0.43) |
| Husband has bachelor's degree[b] | 0.09 (0.54) | 0.17 (0.54) | -0.18 (0.41) | -0.20 (0.41) |
| Wife is African-American[c] | 1.09 (1.60) | 1.26 (1.62) | 0.20 (1.18) | 0.16 (1.17) |
| Wife's weekly labor market hours | -0.03 (0.02) | -0.03 (0.02) | -0.02 (0.02) | -0.02 (0.02) |
| Husband's weekly labor market hours | 0.01 (0.02) | 0.01 (0.02) | -0.01 (0.02) | -0.00 (0.02) |
| Residents other than couple[d] | 0.68 (0.47) | 0.60 (0.46) | 0.72 (0.38)† | 0.59 (0.37) |
| Rooms in the home | -0.33 (0.12)** | -0.34 (0.12)** | -0.17 (0.11) | -0.16 (0.10) |
| Share of wife's adult years employed | 1.64 (1.18) | 1.75 (1.18) | 0.13 (1.04) | 0.27 (1.04) |
| Number of children ever born to wife | 0.44 (0.20)* | 0.44 (0.20)* | 0.17 (0.15) | 0.19 (0.15) |
| Share of expenditures on substitutes | ----- | -0.01 (0.01)† | ---- | -0.02 (0.01)* |
| Constant | 9.83 (1.36)*** | 9.64 (1.36)*** | 8.63 (1.38)*** | 8.94 (1.41)*** |
| $R^2$ | 0.15 | 0.16 | 0.06 | 0.07 |

Note: Results shown are regression coefficients with standard errors in parentheses. All models also include four indicator variables for whether the respondent is missing valid data on the labor market hours of the husband, the labor market hours of the wife, the number of rooms in the home, and the number of children ever born to the wife. Model 2 includes an indicator variable for whether the respondent is missing valid data on the share of expenditures on substitutes. All standard errors are clustered at the individual level.

[a]Wife has bachelor's degree: 0 = no; 1 = yes. [b]Husband has bachelor's degree: 0 = no; 1 = yes. [c]Wife is African-American: 0 = no; 1 = yes. [d]Residents other than couple: 0 = no; 1 = yes.

† $p < .10$; *$p < .05$; **$p < .01$; ***$p < 0.001$.

on market substitutes was associated with a predicted decline of 0.01 hours (1 minute) in a wife's weekly time cleaning and of 0.02 hours (1 minute) in her weekly time cooking. Even considerable changes in the use of market substitutes were therefore associated with small changes in wives' housework time.

Controlling for use of market substitutes reduces the negative association between wives' earnings and their time spent in housework, but the reduction was moderate in both models: 12% in the model of time spent cleaning and 2% in the model of time spent cooking. The greater role of market substitutes in mediating the cleaning-earnings relationship as compared to the cooking-earnings relationship is consistent with the results of models of households' expenditures on market substitutes, which showed that wives' earnings were positively and significantly associated with their reliance on substitutes for wives' time cleaning, but not with reliance on substitutes for wives' time cooking (see Table 3.3, in the appendix at the end of this chapter).

After controlling for the use of market substitutes, wives' earnings and the level of use of substitutes were the only significant predictors in the model of wives' time cooking, and the model explained only 7% of the variation in wives' time cooking. A larger number of control variables were significant in the model of wives' time cleaning, which explained 16% of the variation in wives' time cleaning, after including the measure of use of market substitutes. Wives with bachelor's degrees spent, on average, 2.1 fewer hours per week cleaning than other wives, even after including controls for earnings, labor-force hours, and use of market substitutes. The number of children ever born was positively and significantly associated with wives' time cleaning, as expected. Each additional child born to the wife was associated with a predicted increase of 0.4

hours per week in her time cleaning, net of other controls. Each additional room in the house was associated with a predicted *decline* of 0.3 hours per week in the wife's time cleaning, contrary to expectations. One possibility for this counterintuitive finding is that larger homes are less cluttered and therefore easier to clean. Neither the share of the wife's adult life spent working nor the presence of residents other than the couple in the household was significantly associated with the wife's housework time.

*3.3.2 Husbands' Housework*

The main results may be limited by their neglect of husbands' time in housework. First, it is possible that wives' earnings enable them to negotiate greater time in housework by their husbands. If this is the case, including husbands' housework hours in the model of wives' time use should reduce the negative relationship between wives' earnings and their time spent on housework. Additionally, increased spending on substitutes may be used to reduce husbands' housework time, rather than wives', leading to a weak association between households' use of market substitutes and wives' housework time.

To test these hypotheses, I used the subset of 362 wives (*n*=609) from the 2005-2009 CAMS for whom husbands' housework time was also available. The husbands in this subsample reported spending an average of 3.1 hours per week cleaning and 3.0 hours per week cooking. Including husbands' housework hours as a covariate in the models of wives' housework time revealed that husbands' time cleaning was positively and not significantly associated with wives' time cleaning, whereas husbands' time cooking was negatively and marginally significantly associated with wives' time cooking

(full results in Table 3.4, in the appendix at the end of this chapter). Nonetheless, controlling for husbands' time in housework slightly *increased* the negative association between wives' earnings and their time both cooking and cleaning. Therefore, although wives cooked less when their husbands cooked more, the negative association between wives' earnings and their time in housework does not appear to be due to high-earning wives reallocating housework to their husbands.

Second, I repeated the analysis of Table 3.2 using husbands' time in housework as the outcome (full results available in Table 3.5, in the appendix at the end of this chapter). If high-earning wives were reallocating housework to husbands, we would expect to see a positive association between wives' earnings and their husbands' housework time. If high-earning wives use their earnings to purchase substitutes for their husbands' housework time, we would expect to see a negative relationship between wives' earnings and their husbands' housework time, and we would expect that this association would be substantially reduced after controlling for the household's use of market substitutes. Wives' earnings were not significantly associated with husbands' time cooking, but were marginally significantly and negatively associated with husbands' time cleaning once the measure of use of market substitutes was included. Despite this, the household's use of market substitutes was not significantly associated with the husband's time cooking or cleaning and the negative relationship between wives' earnings and their husbands' cleaning time was slightly *greater* after the measure of use of market substitutes is added to the model. Thus, although husbands married to high-earning wives spend less time cleaning, this does not appear to be because wives' earnings are used to purchase substitutes for their husbands' household labor time. Instead, it is possible that

high-earning wives not only opt out of housework themselves, but also allow their husbands to do so.

### 3.3.3 Alternative Specifications

This section considers a variety of possible sources of heterogeneity that may be associated with both wives' earnings and their time in household labor, as well as alternative specifications of several independent variables. Under each alternative specification, the models presented in Table 3.2 were repeated, to test whether the inclusion of the additional variables alters the main conclusions. Spouses' ages, whether either spouse is under the age of 50, whether either spouse is in poor health, household income other than the earnings of spouses, and the year of the report were all tested as possible omitted variables. The fraction of the earnings-cleaning relationship explained by the inclusion of the market substitute measure ranged from 11% to 16%, and from 1% and 3% for the earnings-cooking relationship. These results are therefore quite similar to those in the main models.

Alternative specifications of the financial variables were also considered. First, I used absolute rather than relative expenditures on market substitutes in the time-use models, but found that the inclusion of these variables reduced the magnitude of the earnings-cleaning relationship by only 4% and the earnings-cooking relationship by less than 1%, and the level of expenditure was not a significant predictor of time in either cleaning or cooking. Second, because spouses' wages rather than earnings determine the opportunity cost of each hour of housework, I repeated the models replacing spouses'

earnings with their hourly wage. Wives' wages were negatively and marginally significantly related to their time cleaning, and the use of market substitutes explained 8% of the relationship. Wives' wages were positively and not significantly related to their time cooking. Third, I included a linear measure of the share of the couple's earnings that the wife earns, because spouses' bargaining positions may affect both their division of household labor and their expenditure decisions. The relative earnings measure was not significant in any of the models. It is also possible that spouses' relative earnings may be non-linearly related to their time in housework (Bittman et al. 2003; Brines 1994), but when I added a quadratic term for spouses' relative earnings to the linear term, neither term was significant, nor were they jointly significant.

3.4 Discussion

The results presented here go beyond existing work by explicitly considering the relationship among wives' earnings, household expenditures on market substitutes for wives' time in household labor, and wives' housework time. Household expenditures on domestic substitutes were negatively related to wives' time in household labor, although the association is far weaker than might be supposed. The inclusion of measures of the use of market substitutes in models of wives' time in household labor explained 12% of the negative association between wives' time in cleaning and their earnings and 2% of the relationship between wives' time in cooking and their earnings.

Throughout, I found stronger associations in the domain of cleaning than cooking. The model explained a greater share of the variation in wives' time cleaning than in their

time cooking, and the inclusion of the measure of use of market substitutes reduced the cleaning-earnings association more than the cooking-earnings association. One reason for this may be that cleaning is a more uniformly undesirable activity, whereas some forms of cooking are enjoyable for at least some wives, so wives are more likely to use their earnings to outsource cleaning. Furthermore, dining out is a recreational activity for spouses, as well as a market substitute for wives' time cooking (Cohen 1998; Oropesa 1993). At a minimum, the results suggest that it may be inappropriate to treat women's time in household labor as homogenous.

This study has several limitations. First, the sample size is small, which limits the power of the statistical tests. Second, although the analyses presented suggest that buying out is not the only explanation for the negative relationship between wives' earnings and their time in housework, they do not distinguish between opting out and unobserved heterogeneity among wives as the source of the residual earnings-housework association, net of use of market substitutes. As the primary focus of this analysis was to call into question the power of the buying-out hypothesis, this ambiguity does not threaten the main conclusion. More extensive panel data on earnings, housework time, and use of market substitutes would provide an even richer test of the source of the earnings-housework relationship.

The measures available also limit the analyses. First, housework hours and household expenditures were measured in the calendar year following the collection of information on spouses' earnings, labor-force hours, and household composition, so the measures are not contemporaneous. Second, the available measure of substitution is limited. Although cooking, cleaning, and laundry constitute a substantial share of

women's non-care housework time, the analyses did not include all forms of household labor. In addition, not all household expenditures intended to reduce wives' housework time are included in the measure of expenditures on market substitutes. For example, purchases of labor-saving devices have the potential to reduce wives' household labor time, although the evidence of their effectiveness in this regard is mixed (Bittman et al. 2004; van der Lippe et al. 2004). Furthermore, among the types of expenditures considered, households may report their expenditures with error and, as discussed earlier, individuals report the dollar amount spent on goods, rather than the goods' capacity to reduce household labor time. Such measurement error may partially explain the low predictive power of use of market substitutes in models of wives' housework time. If this is true, it minimally suggests that the documented positive relationship between wives' earnings and household expenditures on domestic services and restaurant meals cannot be interpreted as evidence that wives are using these goods to substitute for their own time in housework.

The extent to which the findings presented here generalize to younger samples of women is unknown. Younger women, particularly those with children in the household, may experience stronger time pressure that motivates them to use increased earnings to buy some relief in terms of household work. In addition to age differences, cohort differences may exist. Women born in later birth cohorts may be more willing to outsource household labor when they possess the financial resources to do so.

Despite these limitations, the results of this study shed light on the degree to which use of market substitutes explains variation in wives' time in housework and, in particular, to what extent increased use of market substitutes by high-earning women

explains the negative relationship between wives' earnings and their time in housework. For those hypothesizing a role of market substitutes in mediating the earnings-housework relationship, this provides the first direct evidence in favor of the buying-out story. At the same time, the results indicate that the use of market substitutes is insufficient to explain all, or even the majority, of the difference between high-earning and low-earning wives' time in household labor. Although it is not possible in this study to determine the reason for the residual negative relationship between wives' earnings and their housework time, the results suggest that future research should recognize the potential for wives to opt out of housework.

Furthermore, although wives' time in housework falls as their earnings rise, the rate is quite slow. A $10,000 increase in wives' annual earnings is associated with a predicted decline in her combined cooking and cleaning time of only 0.4 hours. The fact that wives do not make extensive use of market substitutes to replace their own time in household labor suggests that non-financial concerns may motivate even high-earning women to invest time in domestic production, even though this reduces the amount of time available for either market work or leisure. Even high-earning wives may feel compelled to perform at least some household labor themselves, whether because they or their families do not perceive goods purchased in the market as adequate substitutes for the wife's own time or because norms of doing gender suggest that it is appropriate for women to spend some time in housework, regardless of the economic logic of this behavior. The idea that gendered norms of behavior operate to give greater responsibility for household labor to women than to men is not new (Berk 1985; Hochschild 1989; West and Zimmerman 1987), but the results presented here suggest that these norms may

also operate to keep domestic production in the household, rather than outsourcing it to market goods and services.

This analysis suggests a need for more research understanding how couples make decisions about the level of household production in their home. Just as the allocation of housework between spouses provides insight about processes of household decision-making and gender inequality in the household, exploration of the circumstances in which households reduce domestic production provides insight about the more general question of work-family tradeoffs that households make. Existing research has too often treated the amount of housework to be done as exogenous. As a result, we know little about the social processes by which individuals and households construct definitions of appropriate levels of domestic production and make decisions about time in housework as opposed to other activities, or expenditures on domestic production as opposed to other goods. Households are not bound to produce a fixed level of domestic production, with the only decisions being how much to outsource and how to allocate the remaining labor among household members. Instead, households also make decisions about how much they value domestic production as opposed to other goods and activities, as well as which forms of household production to retain and which to let go. Certainly, individuals do not have perfect freedom to choose the level and kind of domestic production they prefer, but it is equally erroneous to assume that households have no control over these decisions. An understanding of domestic production decisions as households experience them must therefore take seriously the full complement of domestic production options families and individuals face, including the decision to opt out of certain types of domestic production.

3.5 Appendix

In the models of households' use of market substitutes, the dependent variable was the substitutes measure discussed in the main text: the share of the household's spending on cooking or cleaning that is on substitutes for the wife's household labor time rather than complements. In each case, the sample was restricted to households with non-missing expenditure data. For the model of use of cooking substitutes, ordinary least squares (OLS) was used ($n = 756$). The large number of households that reported no spending at all on cleaning and laundry substitutes made OLS less appropriate for the models of use of cleaning substitutes. Instead, a probit model was first used to model households' decisions to use *any* cleaning substitutes ($n = 753$), and then an OLS model was used to predict the fraction of cleaning-related spending that is on substitutes, among those with non-zero spending ($n = 416$). Reported coefficients from the probit model are probit coefficients, not marginal effects.

Table 3.3 presents the results of models of households' use of market substitutes. Wives' earnings were positively and significantly associated with both the probability that a household uses cleaning and laundry services and, among those that use them, the share of spending on cleaning that is spent on substitutes. Among households with some spending on cleaning and laundry services, each $10,000 increase in a wife's annual earnings was associated with a predicted increase of 2.8 percentage points in the share of the household's spending on cleaning that is on services rather than supplies. Husbands' earnings were also positively and significantly associated with both the probability that a household spends money on cleaning and laundry services and the fraction of spending that is on substitutes, among those who use them. For each $10,000 increase in a

husband's annual earnings, the share of cleaning expenditures that are on substitutes was predicted to rise by 0.9 percentage points. In the model of the likelihood of any use of substitutes, it was not possible to reject the null hypothesis that the coefficients on husbands' and wives' earnings are the same ($\chi^2$ (1) = 0.87, $p$-value = 0.35), but it was possible to reject the null hypothesis of equality in the model of the level of use of cleaning substitutes, among those who use them (F(1, 263) = 8.30, $p$-value < 0.01). In the domain of cleaning, therefore, this is evidence that the level of substitute use in the household depends not only on the total financial resources of the couple, but also on whose paycheck provides them, with increases in wives' earnings predicted to lead to a greater increase in reliance on market substitutes than equivalent increases in husbands' earnings.

The share of food spending that is on food away from home, rather than groceries, was positively associated with both husbands' and wives' earnings, but not significant in either case, and the coefficients were also not statistically different from each other (F(1, 424) = 0.01, $p$-value = 0.92). Furthermore, the model explained only 5% of the variance in the share of food expenditures that are on dining out, whereas it explained 22% of the variation in the share of cleaning expenditures that are on cleaning services, among those who use them. Taken together, the results suggest heterogeneity in the outsourcing process for different kinds of household production, with use of cleaning services responding more strongly than dining out to household demographics.

Table 3.3. Summary of Analyses for Household Use of Market Substitutes

| | Cleaning | | Cooking |
|---|---|---|---|
| Model Type | Probit | OLS | OLS |
| Outcome | Any Substitutes[e] | Substitute Level, If Non-zero | Substitute Level |
| Wife's annual earnings ($10Ks) | 0.10 (0.03)*** | 2.83 (0.54)*** | 0.33 (0.33) |
| Husband's annual earnings ($10Ks) | 0.07 (0.02)*** | 0.85 (0.33)* | 0.28 (0.22) |
| Wife has bachelor's degree[a] | 0.19 (0.16) | 0.21 (3.71) | 2.65 (2.09) |
| Husband has bachelor's degree[b] | 0.19 (0.15) | 5.14 (3.58) | -1.70 (2.01) |
| Wife is African-American[c] | 1.12 (0.23)*** | 4.63 (4.68) | -0.76 (3.68) |
| Wife's weekly labor market hours | 0.00 (0.01) | 0.11 (0.12) | 0.03 (0.07) |
| Husband's weekly labor market hours | -0.00 (0.01) | 0.26 (0.12)* | 0.04 (0.06) |
| Residents other than couple[d] | -0.24 (0.12)† | -2.16 (3.07) | -4.60 (1.78)* |
| Rooms in the home | 0.08 (0.04)* | -0.83 (0.86) | 0.78 (0.51) |
| Share of wife's adult years employed | -0.20 (0.34) | -6.62 (8.11) | -0.13 (4.28) |
| Number of children ever born to wife | -0.08 (0.05) | -2.05 (1.32) | 0.03 (0.82) |
| Constant | -1.09 (0.44)* | 27.21 (11.16)* | 19.84 (6.01)** |
| $\chi^2(14)$ | 99.61 | N/A | N/A |
| $R^2$/pseudo $R^2$ | 0.16 | 0.22 | 0.05 |
| n | 753 | 416 | 756 |

Note: Results shown are regression coefficients with standard errors in parentheses. All models also include four indicator variables for whether the respondent is missing valid data on the labor market hours of the husband, the labor market hours of the wife, the number of rooms in the home, and the number of children ever born to the wife. All standard errors are clustered at the individual level.
[a]Wife has bachelor's degree: 0 = no; 1 = yes. [b]Husband has bachelor's degree: 0 = no; 1 = yes. [c]Wife is African-American: 0 = no; 1 = yes. [d]Residents other than couple: 0 = no; 1 = yes. [e]The three observations that are missing data on the number of children ever born to the wife are dropped from this analysis because all of them make some use of cleaning substitutes.
† $p < .10$. *$p < .05$. **$p < .01$. ***$p < 0.001$.

Table 3.4. Summary of OLS Regression Analyses for Wives' Weekly Housework Hours, Controlling for Husbands' Weekly Housework Hours (n=609)

| | Cleaning | | Cooking | |
| --- | --- | --- | --- | --- |
| | Model 1 | Model 2 | Model 1 | Model 2 |
| Wife's annual earnings ($10Ks) | -0.23 (0.09)* | -0.20 (0.09)* | -0.20 (0.08)* | -0.20 (0.08)* |
| Husband's annual earnings ($10Ks) | 0.13 (0.06)* | 0.14 (0.06)* | 0.05 (0.05) | 0.06 (0.05) |
| Wife has bachelor's degree[a] | -2.26 (0.55)*** | -2.22 (0.55)*** | -0.35 (0.50) | -0.30 (0.49) |
| Husband has bachelor's degree[b] | 0.29 (0.63) | 0.37 (0.64) | 0.02 (0.49) | 0.01 (0.48) |
| Wife is African-American[c] | 0.65 (1.95) | 0.82 (1.98) | 0.74 (1.49) | 0.62 (1.53) |
| Wife's weekly labor market hours | -0.01 (0.02) | -0.01 (0.02) | -0.02 (0.02) | -0.01 (0.02) |
| Husband's weekly labor market hours | 0.00 (0.02) | 0.01 (0.02) | -0.01 (0.02) | -0.01 (0.02) |
| Residents other than couple[d] | 0.62 (0.55) | 0.56 (0.55) | 0.78 (0.42)† | 0.69 (0.42)† |
| Rooms in the home | -0.35 (0.13)** | -0.34 (0.13)** | -0.25 (0.11)* | -0.23 (0.11)* |
| Share of wife's adult years employed | 1.73 (1.36) | 1.73 (1.37) | 0.28 (1.20) | 0.29 (1.19) |
| Number of children ever born to wife | 0.51 (0.23)* | 0.49 (0.23)* | 0.14 (0.18) | 0.15 (0.18) |
| Husband's weekly housework hours | 0.04 (0.10) | 0.04 (0.10) | -0.12 (0.07)† | -0.12 (0.07)† |
| Share of expenditures on substitutes | ----- | ----- | ----- | ----- |
| Constant | 8.74 (1.60)*** | 8.65 (1.61)*** | 9.31 (1.53)*** | 9.73 (1.54)*** |
| $R^2$ | 0.14 | 0.15 | 0.08 | 0.09 |

Note: Results shown are regression coefficients with standard errors in parentheses. All models also include four indicator variables for whether the respondent is missing valid data on the labor market hours of the husband, the labor market of the wife, the number of rooms in the home, and the number of children ever born to the wife. Model 2 includes an indicator variable for whether the respondent is missing valid data on the share of expenditures on substitutes. All standard errors are clustered at the individual level.
[a]Wife has bachelor's degree: 0 = no; 1 = yes. [b]Husband has bachelor's degree: 0 = no; 1 = yes. [c]Wife is African-American: 0 = no; 1 = yes. [d]Residents other than couple: 0 = no; 1 = yes.
† $p < .10$; *$p < .05$; **$p < .01$; ***$p < 0.001$.

Table 3.5. Summary of OLS Regression Analyses for Husbands' Weekly Housework Hours (n=609)

| | Cleaning | | Cooking | |
|---|---|---|---|---|
| | Model 1 | Model 2 | Model 1 | Model 2 |
| Wife's annual earnings ($10Ks) | -0.09 (0.06) | -0.10 (0.06)† | 0.05 (0.06) | 0.06 (0.06) |
| Husband's annual earnings ($10Ks) | -0.01 (0.03) | -0.01 (0.03) | -0.03 (0.03) | -0.04 (0.03) |
| Wife has bachelor's degree[a] | -0.18 (0.38) | -0.18 (0.38) | 0.50 (0.38) | 0.49 (0.37) |
| Husband has bachelor's degree[b] | -0.22 (0.35)* | -0.22 (0.35) | -0.19 (0.33) | -0.20 (0.33) |
| Wife is African-American[c] | 1.53 (0.89)† | 1.44 (0.91) | 0.14 (0.65 | 0.16 (0.64) |
| Wife's weekly labor market hours | 0.04 (0.01)** | 0.04 (0.01)** | 0.01 (0.01) | 0.01 (0.01) |
| Husband's weekly labor market hours | -0.02 (0.01)† | -0.02 (0.01)† | -0.02 (0.01)† | -0.02 (0.01)† |
| Residents other than couple[d] | 0.23 (0.34) | 0.26 (0.34) | -0.03 (0.27) | -0.04 (0.28) |
| Rooms in the home | 0.00 (0.09) | -0.00 (0.09) | 0.05 (0.07) | 0.05 (0.07) |
| Share of wife's adult years employed | 1.95 (0.80)* | 2.04 (0.81)* | 1.91 (0.77)* | 1.83 (0.77)* |
| Number of children ever born to wife | 0.03 (0.14) | 0.04 (0.14) | -0.12 (0.12) | -0.13 (0.12) |
| Share of expenditures on substitutes | ---- | 0.00 (0.01) | ---- | -0.00 (0.01) |
| Constant | 1.50 (1.04) | 1.43 (1.05) | 1.81 (0.94)† | 1.95 (0.97)* |
| $R^2$ | 0.07 | 0.07 | 0.08 | 0.08 |

Note: Results shown are regression coefficients with standard errors in parentheses. All models also include four indicator variables for whether the respondent is missing valid data on the labor market hours of the husband, the labor market hours of the wife, the number of rooms in the home, and the number of children ever born to the wife. Model 2 includes an indicator variable for whether the respondent is missing valid data on the share of expenditures on substitutes. All standard errors are clustered at the individual level.

[a]Wife has bachelor's degree: 0 = no; 1 = yes. [b]Husband has bachelor's degree: 0 = no; 1 = yes. [c]Wife is African-American: 0 = no; 1 = yes. [d]Residents other than couple: 0 = no; 1 = yes.

† $p < .10$; *$p < .05$; **$p < .01$; ***$p < 0.001$.

CHAPTER 4

**A Reconsideration of the Fatherhood Premium**

Fatherhood has recently been under the sociological microscope (Marsiglio et al. 2000; Nelson 2004), and wage-earning is acknowledged as one way in which men support their children (Ihinger-Tallman, Pasley, and Buehler 1993; Nock 1998b; Seltzer 1991). However, little is known about the relationship between fatherhood and men's wages, leading to claims that the fatherhood wage premium is under-studied (Loughran and Zissimopoulos 2009; Lundberg and Rose 2000). Previous research has found that biological fathers generally experience a wage premium relative to childless men (Cornwell and Rupert 1997; Glauber 2008; Hersch and Stratton 2000; Lundberg and Rose 2000, 2002; Whitehouse 2002). Given ample evidence of a motherhood wage penalty (Avellar and Smock 2003; Budig and England 2001; Glauber 2007; Taniguchi 1999; Waldfogel 1997), a wage premium for fathers exacerbates both the gender gap in wages in the labor force and the wage gap between partners in heterosexual couples, potentially weakening women's bargaining position within the household.

The possibility of heterogeneity in the fatherhood wage premium has been given little attention, despite increasingly heterogeneous fathering arrangements. It is unclear whether the wage gains associated with fatherhood are experienced by all men,

or are conditional on the types of fatherhood – residential and biological, nonresidential and biological, and/or stepfatherhood. Likewise, the fatherhood premium literature is underdeveloped theoretically. While an extensive literature documents the positive association between marriage and men's wages and investigates the causes of this relationship (Chun and Lee 2001; Gray 1997; Hersch and Stratton 2000; Loh 1996), the relationship between fatherhood and men's wages has not been subjected to the same scrutiny.

Heterogeneity in the wage premium by type of fatherhood has implications for understanding not only how family structure affects men's wages, but also how family structure is implicated in wage inequality by other demographic traits. If the wage benefits of fatherhood differ by family form, and selection into different family forms is correlated with other demographic traits, like race and education, then family structure may play a role in either exacerbating or reducing wage inequality by other demographic traits. Furthermore, heterogeneity in the fatherhood premium may also contribute to variation across family structures in the financial resources available to children.

This paper addresses both theoretical and empirical gaps in the literature on the fatherhood wage premium. Theoretically, I use the lens of social role theory to hypothesize about variations by fatherhood forms in the wage changes associated with fatherhood. I then estimate panel models using data from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY79) to test whether wage changes for men are different for residential biological fatherhood, nonresidential biological fatherhood, and stepfatherhood.

The remainder of the paper is organized as follows. First, I discuss possible sources of a fatherhood premium, including how the context of fatherhood might moderate each mechanism. I then introduce my data and methods and present the results. I close with a discussion of the results and their implications.

## 4.1 Theoretical Framework

The primary goal of this paper is to test for heterogeneity in the fatherhood wage premium, not to establish the cause of any observed heterogeneity. However, there are several reasons why fatherhood may be associated with changes in men's wages and a consideration of these reasons can help to generate hypotheses about which fathers will earn the highest wage premiums.

First, fatherhood may lead to positive or negative changes in men's labor market productivity. On the one hand, fatherhood involves new responsibilities outside the labor force, which may decrease men's productivity while at work. On the other hand, fathers may be more motivated than childless men to increase their productivity at work, in order to better provide financially for their families. In either case, changes in labor market productivity should be reflected in wages. Fatherhood may also be causally related to wage changes if employers discriminate in favor of or against fathers. Lastly, a non-causal association between wages and fatherhood may arise if men are selected into fatherhood on the basis of unobserved traits that are correlated with wages. In this section, I discuss these three explanations for the fatherhood premium – productivity, discrimination, and selection – in more detail.

*4.1.1 Role Theory and Productivity Implications*

Social role theory provides a lens through which to understand the potential relationships between men's labor force productivity and their fatherhood status. Fatherhood is a social role – a social position with associated norms, or rules of appropriate behavior (Rodgers 1964). Entry into fatherhood introduces men to the position of father and creates a new tie of kinship, which may encourage behavioral changes that comply with the expectations of this role and relationship. Fatherhood can thus be a transformative process that encourages men to put the needs of their children above their own, to build social capital, to increase institutional involvement, and even to desist from crime (Florsheim and Ngu 2006; Knoester and Eggebeen 2006; for a review, see Marsiglio et al. 2000).

However, fatherhood status may affect men's behavior heterogeneously. The greater commitment an individual has to an identity, the more it is expected to affect his behavior (Stryker 1980). Fathers for whom the fatherhood role is more salient are expected to demonstrate a greater commitment to it – a greater willingness to devote energy and resources to behavior consistent with that role (Ihinger-Tallman et al. 1993). As a result, the developmental effects of parenthood are expected to be greater for those for whom the parenthood role is more salient and, therefore, invest more heavily in it (Palkovitz 1996).

As a result, there may be differences in the average level of commitment to the fatherhood role and the strength of kinship ties among residential fathers, nonresidential

fathers, and stepfathers. The stepparent role has been found to have low salience and ties of kinship between stepparents and stepchildren are less automatic than between biological parents and their children (Cherlin and Furstenberg 1994). Similarly, co-residence with children facilitates repeated interactions between fathers and their children, but nonresidential fathers may lack this opportunity, leading to diminished ties of kinship and role salience. For example, Knoester and Eggebeen (2006) find that the positive effects of fatherhood on men's social integration and well-being are larger for residential fathers than nonresidential fathers. Minton and Pasley (1996) find that divorced nonresidential fathers are less satisfied and report giving up fewer other relationships in order to enact the fatherhood role than do residential married fathers. These results suggest that efforts to alter behavior to comply with the expectations of the fatherhood role are diminished for nonresidential as compared to residential fathers.

Individuals not only have beliefs about the norms for their roles, but also have a level of certainty about those beliefs (Fine, Ganong, and Coleman 1997). Along with the diminished role salience and strength of kinship relationships for nonresidential fathers and stepfathers, role ambiguity may lower their commitment to the fatherhood role compared to that of residential fathers. In other words, fathers are likely to invest less in children when the expectation of investment is less clear. For residential, biological fathers, legal and social expectations of supporting children, including financially, are clear, especially if they are married to the child's mother (Nock 1998b). By contrast, Cherlin (1978) proposes that remarriage is an incomplete institution, with vague norms about behaviors for its members, including stepfathers. As a result, ties between stepparents and their stepchildren are more discretionary and there is less agreement

91

about their mutual responsibilities (Cherlin and Furstenberg 1994; Marsiglio 1992), so that stepfathers may experience less normative pressure to provide financially for their stepchildren.

Role ambiguity is also likely to be higher for nonresidential than residential fathers. Men's responsibilities and relationships to children are often defined by their relationship to the mother, leading to greater role ambiguity among nonresidential fathers who are no longer partnered with the child's mother (Ihinger-Tallman et al. 1993; Seltzer 1991; Tach, Mincy, and Edin 2010). Nock (1998a) argues that child support obligations may *discourage* wage-earning by nonresidential fathers, some of whom may view child support as a tax that reduces their own returns to paid labor and thus decreases their incentive to work.

Men's union status may also affect the fatherhood role. To the extent that cohabitation, like remarriage, is an incomplete institution (Cherlin 2004; Nock 1995), the fatherhood role for unmarried cohabitors may be associated with greater role ambiguity. For unmarried residential fathers living singly, the lack of a partner to reinforce the fatherhood role may also lead to higher levels of ambiguity: when men are partners to the mothers of their children, the roles of partner and father may be mutually reinforcing, but single fathers lack this opportunity.

In addition to commitment to the fatherhood role and clarity regarding its expectations, fathers' behavior will also reflect their own beliefs about what it means to be a "good father" (Ihinger-Tallman et al. 1993). Thus, any individual's role as a father is defined by his internalized understanding of appropriate paternal behavior (Minton and

Pasley 1996). The discourse of "new fatherhood" advocates greater involvement by men in the day-to-day lives of their children, broadening the role of father to include routine child care and emotional support for children (Henwood and Procter 2003; Kaufman and Uhlenberg 2000; Wilcox 2004). Nonetheless, wage-earning among fathers remains socially normative (Nock 1998b; Riggs 1997). For fathers who view providing for children financially as a normative component of the fatherhood role and are committed to that role, entry into fatherhood should increase productivity in the labor market and result in higher wages.[14] The greater role salience and lower role ambiguity of residential fatherhood is expected to lead to greater wage increases for residential fathers as compared to nonresidential fathers or stepfathers.

Fathers may increase their productivity at work in many ways. They may work harder for each hour spent on the job or work more hours to accumulate more experience, leading indirectly to higher wages. Alternatively, they may take jobs with higher financial rewards and lower non-financial rewards (Reed and Harford 1989). As previously discussed, fatherhood may also prompt men to make greater investments in human, social, and institutional capital, each of which may have returns in the labor market.

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[14] It is possible that partnered fathers experience productivity gains because the birth of a first child leads to a more specialized division of labor between opposite-sex partners, with men assuming fewer household responsibilities (Becker 1981; Glauber 2008). However, entry into fatherhood is, almost by definition, associated with some increase in time spent with children, and fatherhood is also associated with flat or increasing time in household labor (Bianchi et al. 2000; Hersch and Stratton 2000; Sanchez and Thomson 1997; see Baxter et al. 2008 for an exception). Thus, fatherhood does not appear to be associated with declines in unpaid labor time for men.

*4.1.2 Discrimination*

Men's wages may change after becoming fathers for reasons other than their own behavior. If employers discriminate in favor of fathers, fatherhood may be causally associated with increased wages, even if fathers' productivity is not higher than that of childless men. Employers may raise fathers' wages because they perceive fathers' financial needs as greater or because fatherhood generates biased employer assessments of productivity. In a laboratory experiment, Correll, Benard, and Paik (2007) found that, when evaluating equally qualified same-gender applicants who differed on parental status, undergraduates rated fathers more favorably than childless men in terms of their anticipated commitment to the job and recommended a higher starting salary. However, discrimination in favor of fathers may be limited to residential fathers, particularly when they are married. Employers may even discriminate against nonresidential fathers as compared to childless men if nonresidential fatherhood is perceived as a signal of irresponsibility. As a result, employer discrimination may lead not only to a wage premium for fathers relative to childless men, but to variation in the fatherhood premium across fatherhood forms.

*4.1.3 Selection*

A final possibility is that any association between fatherhood and wages is due to the selection of men into fatherhood, not to a causal relationship between fatherhood and

wages. Sensitive to this possibility, most assessments of the fatherhood premium estimate fixed-effects models to eliminate the effect of selection into fatherhood that is correlated with fixed, but unobserved, traits that are also correlated with wages. Because fixed-effects and first-difference estimates of the fatherhood premium tend to be positive and statistically significant (Cornwell and Rupert 1997; Glauber 2008; Hersch and Stratton 2000; Lundberg and Rose 2000, 2002), selection is generally rejected as providing the entire explanation for the fatherhood premium.

However, other forms of selectivity are not addressed by fixed-effects models. First, selection into fatherhood forms may follow wage changes, rather than precede them. For example, a wage increase may give a man the financial security he desires before becoming a father. If individuals' wages are serially correlated, this wage increase is likely to persist following the transition to fatherhood, so that the post-fatherhood periods will have higher average wages, even if no causal relationship exists between fatherhood and wages. Furthermore, because marital fertility is more likely than non-marital fatherhood to be intended (Kost and Forrest 1995), this form of selection may particularly be a concern for the fatherhood premium for married men.

Another selectivity concern is that men are selected into different forms of fatherhood not only on the basis of their wages, but based on their expected effort fulfilling the father role. Perhaps, for each man, the amount of effort that he would spend to increase his wages upon entry into fatherhood is invariant with respect to the type of fatherhood, but varies across men in a way that is correlated with the type of fatherhood they enter. In this case, heterogeneity in the observed premium across different types of fatherhood may occur, but is due to variation across men, not variation in any given

95

man's commitment to different forms of fatherhood. For example, Hofferth and Anderson (2003) find that stepfathers are negatively selected on time spent with children: stepfathers do spend, on average, less time with their stepchildren than biological fathers spend with their biological children, but men who are stepfathers *also* spend less time with their coresidential biological children than do biological fathers who are not also stepfathers. Compared to residential fathers, nonresidential fathers and stepfathers may be negatively selected on their commitment to the fatherhood role. Thus, a limitation of this analysis is that it estimates only the average fatherhood premium for the men who self-selected into various forms of fatherhood, not the average premium that would be expected by a man randomly chosen from the population, were he to become a father of that type.

Regardless of the mechanisms considered – productivity, discrimination, or selection – residential fathers are predicted to experience larger wage gains than either nonresidential fathers or stepfathers. Among childless men, nonresidential fathers, and stepfathers, the expected order is ambiguous. The goals of the empirical analysis, therefore, will be, first, to assess which fathers experience a wage premium as compared to childless men and, second, whether the wage gains are larger for residential fathers than nonresidential fathers or stepfathers.

### 4.1.4 Previous Research

Although less attention has been paid to the fatherhood premium than to either the male marriage premium or the motherhood penalty, a few previous studies have included

the fatherhood premium as a focus of their analyses (rather than a by-product estimating the marriage premium). Using both random-effects and fixed-effects models and a sample of married couples from the Panel Study of Income Dynamics, Lundberg and Rose (2000) find that married men's wages are approximately 9% higher following the first transition to fatherhood. Lundberg and Rose (2002), again using data from the PSID and fixed-effects models, but including both married and unmarried men, find that a first child increases men's wages by about 7%, the second child by an additional 6%, and further children have no effects on men's wages. Loughran and Zissimopoulos (2009), in a sample from the NLSY79, also use conventional fixed-effects models show a wage premium of about 4% associated with fatherhood. [15] Neither work includes stepchildren nor do they distinguish between biological children who live with their fathers and those who do not.

Glauber (2008), also using fixed-effects models and data from the NLSY79, distinguishes between married and unmarried fathers and finds a fatherhood premium of 7% for married African-American fathers and 9% for married White fathers and married Latino fathers. There is no wage premium for unmarried fathers of any race, regardless of whether they are never-married or previously-married. However, Glauber also excludes stepchildren and does not distinguish between unmarried coresidential fathers and

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[15] The authors preferred model, however, is a fixed-effects model that treats as its outcome the first difference in wages across subsequent years. In this way, the authors are able to control for the possibility that marriage and parenthood are associated not only with unobserved traits that are correlated with wage *levels* but also unobserved traits that are associated with wage *growth*. Using this model, they find no effect of fatherhood on men's wages.

unmarried nonresidential fathers so it is unclear, for example, whether fathers who live

with their child and cohabit with the child's mother experience a fatherhood premium

similar to married fathers.

4.2 Data and Methods

I use a sample of men in the 1979 cohort of the National Longitudinal Survey of Youth

(NLSY79) (Bureau of Labor Statistics 2008) to assess whether there is heterogeneity

across fatherhood contexts in the wage changes associated with fatherhood. There are

6,403 men ages 14-22 in the original sample; they were re-interviewed annually through

1994 and biannually thereafter. The most recent available data come from the 2008

survey wave, at which point the respondents are ages 43-51; each man who remained in

the sample for the entire period is thus observed 23 times. The longitudinal nature of the

study allows the experiences of these men to be followed through middle-age, when most

union formation and parenthood status changes have been completed.

The military and poor non-Black non-Hispanic subsamples were not re-

interviewed throughout the entire period, so members of these subsamples are dropped,

leaving a sample of 4,837 men. Men who become fathers of any type before the age of 18

are excluded, because they do not have sufficient pre-parenthood observations of wages

to meaningfully compare wages before and after the transition to fatherhood. This

reduces the sample to 4,697 men, or 94,582 observations. Observations that occur when

the individual is under the age of 18 (5,767 observations), currently enrolled in school

(9,807 observations), in the active armed forces (2,737 observations), self-employed

(5,964 observations), or working a job not for pay (335 observations), are excluded from the analysis because wages during these periods are often poor indicators of the individual's long-term earnings potential. Observations that occur after the man's oldest biological child has reached the age of 18 are excluded (5,662 observations), as many children leave the parental home at this point. Thus, men who do not live with their children who are 18 years or older are not "nonresidential fathers" in the same way as are men who do not co-reside with younger children.[16] The 106 observations in which the man is a widower are excluded, as there are insufficient observations to analyze this group as a separate union status.

*Hourly Wages.* The dependent variable is the log of the respondent's direct report of his hourly wage rate in his current or most recent regular job since the last interview, adjusted to 2009 dollars. The top and bottom 1% of reported wages are re-coded to the values at the 99th and 1st percentiles of the wage distribution of the analytic sample, respectively. The focus on hourly wages is consistent with previous research and with the theoretical focus on the ways in which fatherhood alters the financial returns to men's labor, rather than time spent in the labor force. Furthermore, the fact that earnings and labor supply for a given year are not reported until the subsequent survey wave introduces difficulties in the matching of these variables with marriage and fertility histories (Loughran and Zissimopoulos 2009).

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[16] Furthermore, the effects of having been a residential father to a child who has now left home are unclear: there may be a residual effect of this relationship, particularly for fathers who are providing financial support to their young adult children – for example, college expenses.

*Fatherhood.* A dummy variable is created for each fatherhood type – (biological) residential fatherhood, (biological) nonresidential fatherhood, and stepfatherhood – and is set equal to one if the man has any children of this type.[17] The excluded group is childless men. In general, counts of residential and nonresidential biological children are determined by the man's reports of the usual residence of each of his biological children in each survey wave. The presence of stepchildren in the household is identified from the household roster. Unfortunately, it is not possible to distinguish stepchildren from adopted children. The construction of these variables is discussed in greater detail in the appendix at the end of this chapter.

*Union Status.* Men's marital status is collected in each wave.[18] Three dummy variables for union status are created: one for whether the man is currently cohabiting, one for whether he is currently married, and one for whether he is currently divorced. The omitted group is never-married men who are not cohabiting, a group referred to as "single" for brevity. A man is considered to be cohabiting if he is not presently married and his household roster includes an individual who is designated as his partner.

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[17] Supplemental analyses also considered alternative specifications of the fatherhood variables, including using the count of children in each group, rather than a single dummy variable. Results were qualitatively similar to those presented in the main analysis.

[18] The year-specific marital status reports are supplemented with the respondent's report of his marital history. If an individual is missing marital status information in a given year, or if the year-specific status report conflicts with the marital history, the marital history information is used. Because marital history is reported as the start and end date of all marriages, this has the effect of eliminating artificial changes in marital status that are caused by measurement error.

*Control variables*. The key comparison in this model is the wages of men before and after their transition to fatherhood. In order to make this comparison meaningful, adjustments are made to account for the fact that older men are both more likely to have fathered a child and have higher average wages. Because the log of wages is typically assumed to follow a quadratic with labor market experience (Mincer 1974), a quadratic for the man's potential labor market experience is included as a covariate. Potential experience is constructed as the individual's age minus his years of education, minus 5. Furthermore, the returns to experience vary by education (Heckman, Lochner, and Todd 2003). Therefore, the quadratic in potential experience is interacted with the man's educational attainment. Education is measured by two dummy variables, one of which is set to one if the individual has completed at least the 12th grade but fewer than four years of college at the time of the survey, and the second of which is set to one if the individual has completed at least four years of college at the time of the survey.[19] Men who have not completed the 12th grade are the omitted group.

An indicator variable is set to one if the individual reports (in the current survey wave) a health limitation in the amount or kind of work he can perform. Lastly, a series of dummy variables for the region of residence of the respondent (northeast, north central, south, west, and missing) are included to control for regional differences in both wages and family forms.

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[19] Alternatively, education was specified as two dummies, one of which was set equal to one if the man had received at least one year of postsecondary education and the other of which was set equal to one if the man had received at least four years of college (with those who had not completed any postsecondary education being the omitted category). Results were similar to those presented in Table 4.3.

Results are presented from an individual-level fixed-effects model that estimates the relationship between men's fatherhood status and their wages. Fixed-effects models are common in the estimation of the relationship between union status or parenthood and wages (Avellar and Smock 2003; Budig and England 2001; Cornwell and Rupert 1997; Glauber 2007, 2008; Hersch and Stratton 2000; Taniguchi 1999; Waldfogel 1997). Adapting the notation of Cornwell and Rupert (1997), the fixed-effects equation can be summarized as follows:

$$;$$

where the subscript $i$ indicates the individual, $t$ indicates the year of the observation, $W$ is the hourly wage, $X$ is the vector of control variables, $F$ is the vector of fatherhood variables, and $U$ is the vector of union status variables, all as previously defined. $\alpha_i$ is the individual's time-invariant individual-level error term (or individual fixed effect), and $\varepsilon_{it}$ is the individual- and year-specific error term. In other words, the fixed-effects model estimates the association between fatherhood and wages, net of the association between unobserved but time-invariant traits of the individual that are potentially correlated with both fatherhood and wages ($\alpha_i$).

In the main models, no controls are included for individuals' job traits and labor supply, such as work hours, tenure with current employer, occupation, or industry. This is because the changes in wages that result from fatherhood may operate through changes in any of these variables. If fatherhood is associated with wage gains for men because fatherhood prompts men to transition to higher-paying occupations, this is still a real wage change due to fatherhood; it merely operates through another observed variable.

The exclusion of these control variables is therefore appropriate in order to understand the gross association between fatherhood and wages.

Past research on the fatherhood premium is varied on this point. Glauber (2008) includes a rich set of control variables, including actual experience, education, tenure, and region. Both Loughran and Zissimopoulos (2009) and Lundberg and Rose (2002) exclude controls for occupation and industry because of a concern for endogeneity. However, Loughran and Zissimopoulos (2009) exclude controls for education and region on the same grounds, but include a control for actual experience, while Lundberg and Rose (2002) exclude the control for actual experience and include controls for education. In subsequent models, I consider the potential role of job traits and labor supply to mediate the association between fatherhood and wages and include a richer set of control variables.

In terms of missing data, three observations (less than 0.01% of the sample) are deleted because the individual reports an educational attainment and age that implies fewer than zero years of post-schooling work to date. Three hundred and ninety-five observations (0.62% of the sample) are deleted because the individuals did not provide sufficient information to determine the union or fatherhood status of the individual. An additional 19 observations (0.03% of the sample), from two individuals, are deleted because the individual never provided valid education data, making it impossible to assign the individual to an education-specific age-wage profile.

A final type of missing data must be considered: 10.8% of the remaining unweighted sample (6,887 observations) is excluded from the wage analysis because they

did not report a wage. In all but 0.09% of these cases, this is because the individual did not report any regular employment since the last interview. The exclusion of observations with missing wage data is consistent with past research on the fatherhood wage premium (Glauber 2008; Loughran and Zissimopoulos 2009; Lundberg and Rose 2000, 2002).[20] As a result, the estimates that follow are of the average wage changes associated with fatherhood for men who are employed, rather than the average wage changes that would be experienced by all men who become fathers, were they to remain continuously employed. The final analytic sample for the wage regressions includes 56,898 observations from 4,555 men.

4.3 Results

Descriptive statistics, weighted using the NLSY79 weights, of the sample along the key dimensions of union status and fatherhood are shown in Table 4.1. In a given period, a man may belong to any of the four union statuses, which are mutually exclusive by construction. Most men (54%) in the sample are married; being single is quite common as well (36%). Cohabitation and divorce, by contrast, are relatively uncommon (6% and 5%, respectively). A man may also belong to one or more of the three fatherhood statuses. While men may belong to multiple fatherhood categories simultaneously, less

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[20] Models that adjusted for selectivity into the labor force by using household non-labor income and the presence of adults other than the man and a partner or spouse in the household as time-varying instruments for employment yielded results almost identical to those presented in the main models.

than 10% of the sample does. Among the three types of fatherhood, residential fatherhood is the most common: 37% of the sample is residential fathers, compared to 12% who are nonresidential fathers and 8% who are stepfathers. Looking at the combinations of fatherhood statuses, it is most common to be exclusively a residential father (30%), followed by exclusively a nonresidential father (9%), followed by a residential father and a stepfather (5%).

Union status and fatherhood type are correlated. Among never-married men living singly, only 8% have children. Among cohabiting men, being childless is again most common, although 42% are fathers and 27% have biological children living with them. Among married men, 75% are fathers and 63% have biological residential children. Nonresidential fatherhood is the most common fatherhood type only among divorced men: 47% have nonresidential children. Finally, stepfathers are a distinctive group among married men because they are the only group for which dual status is more common than any single status: only 2.1% of the sample is exclusively stepfathers, while 4.8% are both stepfathers and residential biological fathers. Table 4.1 also reveals that, despite considerable heterogeneity in family forms, just three groups – single childless men, married childless men, and married men living with residential children (and without nonresidential children or stepchildren) – comprise 74% of the sample; no other group is more than 5% of the sample.

Table 4.2 shows other weighted descriptive statistics of individuals (Panel A) and observations (Panel B) in the sample. Eighty-one percent of the men in the sample are observed to marry at some point between 1979 and 2008, with an average age at first

Table 4.1. Union Status and Fatherhood Status, Cell Frequencies

|  |  | Union Status | | | | | |
|  |  | Single | Cohabiting | Married | Divorced | *Total* | Unweighted Count |
| Fatherhood | Childless | 33.02% | 3.30% | 13.54% | 2.00% | 51.86% | 27,980 |
| Status | Residential Only | 0.28% | 1.24% | 27.63% | 0.46% | 29.61% | 16,233 |
|  | Nonresidential Only | 2.62% | 0.82% | 3.28% | 2.05% | 8.77% | 6,859 |
|  | Stepfather Only | N/A | N/A | 2.09% | N/A | 2.09% | 1,113 |
|  | Residential and Nonresidential | 0.06% | 0.30% | 1.19% | 0.10% | 1.65% | 1,290 |
|  | Residential and Stepfather | N/A | N/A | 4.77% | N/A | 4.77% | 2,584 |
|  | Nonresidential and Stepfather | N/A | N/A | 0.75% | N/A | 0.75% | 484 |
|  | Residential, Nonresidential, and Stepfather | N/A | N/A | 0.51% | N/A | 0.51% | 355 |
|  | *Total* | 35.98% | 5.66% | 53.76% | 4.61% | 100% |  |
|  | Unweighted Count | 21,815 | 3,871 | 28,638 | 2,574 |  | 56,898 |

Table 4.2. Descriptive Statistics

| Panel A: Individuals (*N=4,555*) | Mean (S.D.) |
|---|---|
| Ever marry | 0.81 |
| Among those marrying | |
| Age at first marriage | 25.34 (5.36) |
| Cohabit before marriage | 0.24 |
| Ever divorce | 0.32 |
| Ever father | 0.74 |
| Among fathers | |
| Age at entry to fatherhood | 26.72 (5.42) |
| Race | |
| White | 0.80 |
| African-American | 0.14 |
| Hispanic | 0.06 |
| | |
| Panel B: Person-Years (*N=56,898*) | Mean (S.D.) |
| Hourly wage | $19.02 ($12.05) |
| Age | 30.08 (7.20) |
| Education | |
| Completed $12^{th}$ grade | 0.65 |
| Completed at least 4 years of college | 0.21 |
| Health Limitation | 0.04 |
| Region | |
| Northeast | 0.20 |
| North Central | 0.30 |
| South | 0.33 |
| West | 0.18 |

marriage of 25.3.[21] This result and all subsequent results are affected by sample attrition. Men who attrite from the sample are younger at the time of exit and therefore less likely to have completed transitions to marriage by the time they exit.

Of the men who marry, 24% cohabited before marriage and 32% eventually divorce. Seventy-four percent of men are observed to make a transition to fatherhood of some kind. The average age at entry into fatherhood is 26.7 years. Racially, 80% of the sample is White, 14% African-American, and 6% Hispanic.[22]

As shown in Panel B of Table 4.2, among the employed, the average hourly wage is $19.02 in 2009 dollars. On average, the men are 30.1 years old when they are observed. Twenty-one percent have completed at least four years of college and an additional 65% completed at least 12th grade. Men report work-related health limitations in only 4% of the observations. Twenty percent of the observations are from the northeast region, 30% from the north central region, 33% from the south, and 18% from the west.

Preliminary analyses revealed that, conditional on union status, fatherhood states may be treated additively (results available from the author upon request). That is, there is no statistically significant interaction between the three fatherhood types within any given union status ($F(7, 4554) = 1.42$, $p$-value $= 0.19$). Therefore, fatherhood statuses are treated as additive and non-exclusive: men who live with, for example, both biological children and stepchildren will be classified as both residential fathers and stepfathers.

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[21] The statistics in Table 4.2, Panel A are calculated prior to censoring observations when the respondent's oldest biological child reaches 18.

[22] Racial identification is based on the assignment by the NLSY79 of the individual to race-identified sample stratum.

I also tested for interactions between fatherhood status and union status (full results available from the author upon request). For example, transitions to residential fatherhood may have different effects on men's wages depending on whether the man is living singly, cohabiting, married, or divorced. For nonresidential fathers, there are no statistically significant interactions between union status and fatherhood ($F(3, 4554) = 1.82$, $p$-value $= 0.14$). For residential fathers, however, differences in the fatherhood premium across union types are statistically significant ($F(3, 4554) = 3.11$, $p$-value $= 0.03$). However, there are no statistically significant differences in the fatherhood premium among residential fathers who are living singly, cohabiting, or divorced ($F(2, 4554) = 0.21$, $p$-value $= 0.81$). All subsequent analyses thus compare four groups of fathers: married[23] residential fathers, unmarried residential fathers, nonresidential fathers, and stepfathers.

Multivariate results from the main fixed-effects wage equation are shown in column 1 of Table 4.3 (Model 1). In addition to the fatherhood premiums, the results in column 1 of Table 4.3 show differences in wages among men of different union statuses. These results can be interpreted as the wage premium associated with different union

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[23]  Although the current wife of the man is not necessarily the same as the mother of his residential children, the number of biological children that the man has fathered since the beginning of his current marriage and the number of biological children he currently lives with is the same in 73.7% of all observations in which he has at least one biological residential child or has fathered at least one child since the start of his current marriage. In only 14.7% of the cases does the number of biological residential children exceed the number of marital births, which gives an upper bound on the presence of children from previous unions. However, some of these cases may also be premarital births from the current union, or measurement error.

statuses for childless men. Compared to their wages when they are single, men earn

statistically significantly more when they are married and when they are cohabiting. For

childless men, the cohabitation premium is 4.2% and the marriage premium is 6.6%.

Men's wages when divorced, however, are not statistically significantly different from

when they are single.

The estimates for the relationship between fatherhood and wages indicate the

marginal effect of fatherhood on men's wages, conditional on union status. In terms of

fatherhood status, the null hypothesis of equal wage premiums for married residential,

unmarried residential, nonresidential, and stepfathers is rejected (F(3, 4554) = 22.87, $p$-

value < 0.001). Of the three types of fathers, only biological, married, residential fathers

experience a statistically significant fatherhood wage premium. Men's wages are, on

average, 4.3% higher when they live with their biological children and are married than

when they are childless and married. [24] By contrast, men's wages are 3.8% lower when

they are nonresidential fathers[25] than when they are childless, and 4.3% lower when they

are stepfathers than when they are married and childless, both of which are statistically

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[24] Lundberg and Rose (2002) found that sons increased men's wages more than daughters, although only

for men born in or before 1950. Using data from the household roster, residential biological children and

stepchildren were each subdivided into four groups: boys age 6 and under, boys age 7 and older, girls age

6 and under, and girls age 7 and older. There was no evidence for either residential biological children or

stepchildren that the wage effects of fatherhood vary by the age or gender of the children.

[25] It is possible that the relationship between nonresidential fatherhood and men's wages varies according

to whether the man previously lived with his children. To test this hypothesis, I divide nonresidential

fathers into two groups, depending on whether or not they were previously a residential father, but the

difference is not statistically significant (F(1, 4554)=0.76, $p$-value=0.38).

Table 4.3. Multivariate Results, ln(Hourly Wage)

|  | Model 1 | Model 2 |
|---|---|---|
| Fatherhood |  |  |
| Married Residential | 0.043 (0.009)*** | 0.035 (0.008)*** |
| Unmarried Residential | -0.010 (0.016) | -0.014 (0.015) |
| Nonresidential | -0.038 (0.013)*** | -0.025 (0.012)* |
| Stepfatherhood | -0.043 (0.008)** | -0.036 (0.008)*** |
| Unions |  |  |
| Cohabitation | 0.042 (0.011)*** | 0.036 (0.011)** |
| Married | 0.066 (0.009)*** | 0.051 (0.009)*** |
| Divorced | 0.016 (0.016) | 0.009 (0.015) |
| Person-year observations | 56,898 | 56,898 |
| Individuals | 4,555 | 4,555 |
| Overall $R^2$ | 0.19 | 0.33 |

*p<.05; **p<.01; ***p<0.001.

Note: Results presented are coefficients with standard errors in parentheses. Childless, never-married men living singly are the excluded category. All models control for the region of residence of the respondent, whether his health limits his work, biological children who live in other residential arrangements, a quadratic in the respondent's potential experience, his educational attainment, and their interaction. Model 2 also controls for tenure with the current employer, whether the man is working part-time, his work hours if he is working full-time, a quadratic in his work experience to date, the occupation and industry of the job, whether the job is covered by a collective bargaining agreement, whether his job is in the government or non-profit sector (as opposed to private for-profit).

significant differences from the wages of childless men. For unmarried residential fathers, wages are no different than for childless men of the same union status. When it comes to wages, while partnered men benefit from their union status, only married, residential, biological fathers can be said to benefit from fatherhood.

The results from Table 4.3 are represented graphically in Figure 4.1, with two possible union and fatherhood trajectories for hypothetical men.[26] In the first trajectory, a never-married, childless man who lives singly transitions first to cohabitation and then to marriage, receiving first the cohabitation premium of 4.2%, and then an additional, statistically significant, wage gain of 2.4% when he marries ($F(1, 4554) = 4.07$, $p$-value = 0.04). A subsequent transition to married, biological, residential fatherhood leads to an additional gain of 4.3%, for a total gain of 10.9%. If the man then divorces and his children do not live with him following the divorce, his wages fall 13.1% so that they are now 2.2% less than and not statistically significantly different from when he was never-married, living singly ($F(1, 4554) = 1.43$, $p$-value = 0.23). The marriage premium itself (as compared to divorce) is only 5% (6.6%-1.6%), so the majority of the wage loss for married men with children who divorce and do not retain custody of their children is due to the transition from being a residential father to being a nonresidential father, not to the loss of the marital union itself.

As shown in Trajectory 2 of Figure 4.1, changes in union status and fatherhood type may occur simultaneously. For example, for stepfathers, transitions to

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[26] In other words, the figure does not represent the observed wage changes for men who follow the hypothetical trajectory, but the predicted wage changes for a hypothetical man, based on the estimates in Table 4.3, column 1.
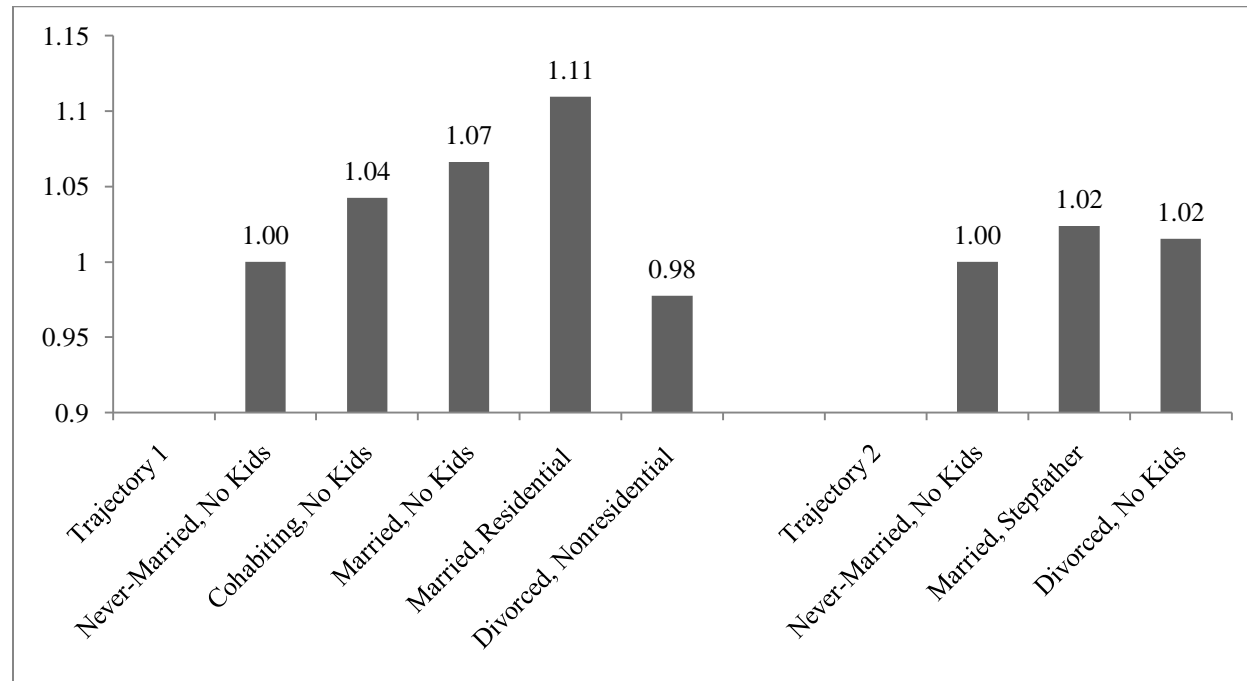
Figure 4.1. Men's Wages Relative to When Childless, Never-Married, Living Singly

stepfatherhood occur at the same time as marriage. From the point estimates, we can see that the gains from marriage (6.6%) are large enough to offset the stepfatherhood penalty (4.3%), leading to a wage gain of 2.3% for never-married, childless men who become stepfathers, although the difference is only marginally statistically significant ($F(1, 4554)$ = 3.77, $p$-value = 0.05) and is statistically significantly less than the gains experienced by men who marry women without children ($F(1,4554)$ = 26.75, $p$-value < 0.001). [27] In contrast to the married man with residential children, if a man with only stepchildren divorces, his wages do not change statistically significantly ($F(1,4554)$ = 0.29, $p$-value = 0.59).

*4.3.1 How Much Can Be Explained by Labor Supply and Job Traits?*

In Model 2, I consider the possibility that the observed heterogeneity in the fatherhood wage premium is due to differences in the labor supply, human capital, or jobs held by different types of fathers. In particular, fatherhood has been found to be positively associated with hours of paid labor for at least some groups of men, although the results are mixed (Astone et al. 2010; Glauber 2008; Kaufman and Uhlenberg 2000; Lundberg and Rose 2000, 2002).[28] If married residential fathers increase their market work hours more than other fathers, this suggests that their higher wages post-fatherhood

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[27] There is no evidence that the remarriage premium differs from the premium for a first marriage ($F(1,$ 4554) =2.44, $p$-value =0.12). In the main models, all currently married individuals are included in the married group.

[28] Results of fixed-effects models estimating the relationship between fatherhood and men's annual work hours are shown in Table 4.4 in the appendix associated with this chapter.

reflect increased work effort. If the premium for residential fathers and penalties for nonresidential fathers and stepfathers disappears when these traits are controlled for, this suggests that the heterogeneity is not due to within-job pay discrimination – although discrimination in hiring cannot be ruled out. I repeat the wage models, but add controls for the occupation, industry, and sector of the respondent's current job, and whether it is covered by a collective bargaining agreement. I also control for whether the respondent is currently working part-time (fewer than 35 hours per week), his weekly work hours if he works full-time, his experience, and the duration of his tenure with his current employer. The results are shown in column 2 of Table 4.3. Unlike Model 1, which measures the gross association between fatherhood and wages, the results in Model 2 estimate the association that remains after accounting for the effect of fatherhood on labor supply, job tenure, and job traits.

These controls improve the fit of the model considerably (overall $R^2 = 0.33$ compared to 0.19 in column 1, Table 4.3), and the coefficients on the fatherhood variables are reduced: married residential fatherhood is associated with wage gains of 3.5%, nonresidential fatherhood is associated with wage losses of 2.5%, and stepfatherhood with wage losses of 3.6% as compared to being married and childless, each of which is statistically significantly different from childless men. As before, transitions to unmarried residential fatherhood are not associated with statistically significant wage changes for men. The job-related traits explain about 1/5 of the premium for married residential fathers and penalty for stepfathers, and about 1/3 of the penalty for nonresidential fathers.

Thus, married residential fatherhood appears to be associated with wage gains for men in part because of increased labor supply, greater human capital, and placement in jobs with wage-enhancing characteristics, and the reverse is true for nonresidential fatherhood and stepfatherhood. However, married residential fathers as well as nonresidential fathers and stepfathers, the majority of the wage changes associated with fatherhood is not explained by the controls for human capital and job traits. This may be because the set of controls does not capture some relevant measures of human capital or wage-related traits of jobs, or may indicate differences in within-job productivity that are not due to these factors, within-job discrimination by employers, or selection into fatherhood types based on commitment to the fatherhood role.

*4.3.2. Does Fatherhood Follow Wage Changes?*

As discussed previously, fatherhood status transitions might follow wage changes, particularly for marital births. In this case, the premium for married, residential fathers may occur because married men choose to have children after experiencing a wage increase, and this wage increase endures into the fatherhood period. If this is true, the higher wages for married, residential fathers cannot be attributed to the experience of fatherhood. To test this possibility, four dummy variables were created that were set equal to one only in the year immediately prior to the transition to each form of fatherhood. None of these variables was statistically significant, nor were they jointly significant ($F(4, 4554) = 1.07$, *p*-value $= 0.37$). Thus, there is little evidence that fatherhood transitions (quickly) follow wage changes, as wages in the year immediately

prior to entry into fatherhood are not statistically significantly different from those in other childless years (full results available from the author upon request).

*4.3.3 Race and Education Subgroups*

Lastly, I test whether the results are robust across race and education subgroups. First, I allow each fatherhood status to interact with the respondent's race (White, African-American, or Hispanic). None of the race differences in the fatherhood premium are statistically significant, nor are they jointly significant in either Model 1 or Model 2 ($F(8, 4554) = 1.15$, $p$-value $= 0.33$; $F(8, 4554) = 1.05$, $p$-value $= 0.40$). In Model 1, there are some differences by race in union premiums ($F(6, 4554) = 2.67$, $p$-value $= 0.01$), which are driven by a difference between African-Americans and Whites. Compared to Whites, African-Americans experience smaller cohabitation and marriage premiums and a larger divorce penalty.[29]

A similar pattern emerges for education subgroups. There are no statistically significant differences in the fatherhood premiums earned by men who completed at least four years of college as compared to men with less education ($F(4, 4554) = 0.70$, $p$-value $= 0.59$; $F(4, 4554) = 0.82$, $p$-value $= 0.51$), but there are significant differences in union premiums, in both Model 1 and Model 2 ($F(3, 4554) = 3.40$, $p$-value $= 0.02$; $F(3, 4554) = 2.87$, $p$-value $= 0.04$). Compared to men with less education, men with four or more years

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[29] While Glauber (2008) finds lower fatherhood premiums for non-white men, this may be because she considers only men's current marital status and the man's number of biological children, without considering either the role of cohabitation or distinctions between residential and nonresidential fatherhood.

of college have significantly smaller marriage premiums and significantly larger divorce penalties.

4.4 Conclusion

The fatherhood premium documented in past research (Cornwell and Rupert 1997; Hersch and Stratton 2000; Lundberg and Rose 2000, 2002; Whitehouse 2002) is far from universal. Consistent with Glauber (2008), I find no fatherhood premium for unmarried fathers. Furthermore, I find no fatherhood premium for nonresidential fathers or stepfathers. In no model using the NLSY79 is unmarried residential fatherhood, nonresidential fatherhood or stepfatherhood associated with wage gains for men as compared to childless men of the same union status. By contrast, entrance into married residential fatherhood is associated with wage gains of around 4.3%, above and beyond the marriage premium of 6.6%. Consistent with past research, both cohabitation and marriage are associated with wage gains for men, although the marriage premium is larger (Cohen 2002; Loh 1996).

Not only are men's wages affected by both their union status and their role as fathers, but, at least for residential fathers, unions and parenthood interact: only married residential fathers experience significant wage gains compared to childless married men, while cohabiting, never-married, and divorced residential fathers do not experience significant wage gains compared to childless men of the same union status. The crucial role of marriage in reinforcing men's incentives to provide for their children financially is consistent with the understanding of fatherhood as a "package deal" – that is, fatherhood

takes on meaning in men's lives when it is coupled with a relationship to the mother (Tach et al. 2010), especially when the relationship is marital (Ihinger-Tallman et al. 1993; Nock 1998b; Seltzer 1994). A fatherhood premium is observed only for men for whom ties of marriage to the mother, ties of biology to the child, and co-residence with the child are mutually reinforcing. Residential, biological fathers who are not married receive no fatherhood premium, nor do stepfathers who live with children and are married to the mother: all three ties of marriage, biology, and residence are required to create a fatherhood wage premium.

Married, biological, residential fatherhood remains the most common form of fatherhood, and thus many men experience a fatherhood wage premium. Nonetheless, more than 40% of the observations of fathers in the sample are *not* married with exclusively residential, biological children, and for these men fatherhood does not lead to wage gains.

The study has several limitations. First, it uses men's reports of their fathering experiences, which may contain errors. When mothers' reports are used as a benchmark, nonresidential fathers are found to be underrepresented by between 25% and 50% in the Survey of Income and Program Participation (SIPP), National Survey of Families and Households (NSFH), and Panel Study of Income Dynamics (PSID) (Rendell et al. 1999; Sorensen 1997). Undercount may occur because of either underrepresentation of nonresidential fathers among survey respondents in a way that is not accounted for by the survey weights, such as the exclusion of institutionalized populations from the sampling frame, as well as because of under-reporting of fatherhood status by survey respondents (Rendell et al. 1999; Sorensen 1997). Furthermore, non-reporting of fatherhood status is

119

particularly likely for when the birth was nonmarital (Rendell et al. 1999). In particular, men who are unaware of the children they father will not be included, and men may disproportionately fail to report the children with whom they are least involved. As a result, the group of nonresidential fathers observed in the NLSY79 may be biased in favor of men with higher levels of commitment to their fatherhood role. This implies that the wage penalty estimated for nonresidential fathers in Table 4.3 may underestimate the average wage penalty for all nonresidential fathers.

Second, the analysis does not consider the possibility that fatherhood may affect men's wages through their probabilities of cohabitation, marriage, and divorce. Nock (1998a) argues that men who father children prior to marriage suffer socioeconomic disadvantage in part because of the negative association between premarital parenthood and future marriage and cohabitation. By conditioning on union status, this indirect path linking fatherhood to wages is not considered a part of the fatherhood premium or penalty. Instead, the wage changes reported are the *direct* effects of fatherhood, not indirect effects that operate through union formation and dissolution.

Third, as previously discussed, the results estimate the relationship between fatherhood and wages for men who enter each fatherhood form and are employed. If men select into fatherhood forms based on their expected fatherhood premium or penalty, the estimates presented here do not represent the wage gains or losses that a randomly-selected man could expect to receive, were he to enter that fatherhood form and remain employed. If fathers who have the smallest wage gains (biggest losses) from fatherhood disproportionately choose not to become fathers or not to be employed, the estimates in Table 4.3 will be biased upward.

Lastly, this study does not fully identify the mechanism by which married residential fathers receive higher wages following entry into fatherhood, while other fathers do not, although it provides suggestive evidence. While some of the heterogeneity in the association between fatherhood types and wages appears to be related to differences in changes in men's job traits and labor supply, these job-related characteristics are not enough to explain the entirety of either the wage premium for married residential fathers or the penalty for nonresidential fathers and stepfathers. There is no evidence that transitions to fatherhood follow wage changes. This leaves open the possibility that residential fathers receive higher wages, conditional on the jobs they hold, because they are more efficient and productive workers, but may also be due to employer discrimination in favor of residential fathers and against nonresidential fathers and stepfathers.

The larger wage gains for married residential fathers as opposed to other fathers is in theory consistent with equal investments in children by all fathers, but larger investments in wage-earning for married residential fathers and in other types of investments in children by unmarried residential fathers, nonresidential fathers, and stepfathers. Because the NLSY79 lacks data on time spent with children, it is not possible to test whether fathers who receive the largest wage premiums are those who spend the least time with their children. However, an extensive literature documents that nonresidential fathers and stepfathers spend significantly less time with their children than do residential biological fathers (Cooksey and Fondell 1996; Hofferth and Anderson 2003; Seltzer 1991). This cast doubt on the hypothesis that heterogeneity in the fatherhood wage premium is due to differences in the ways in which fathers invest in

their children, as stepfathers and nonresidential fathers, who contribute the least time to their children, do not appear to compensate for this with increased wage-earning.

The association between marriage and residential fatherhood means that married fathers receive both a marriage premium and a residential fatherhood premium. As a result, the returns to remaining married are large in terms of men's wages: compared to when married with residential children, men earn, on average, 13% less when they are divorced with nonresidential children. The marriage premium for childless men as compared to childless divorced men is 5%, implying that more than half of the returns to staying married for fathers is due not to marriage itself, but to the association between marriage and residential fatherhood.

Regardless of the causal mechanism, the observed heterogeneity in the fatherhood wage premium has implications for wage differences among men, as well as for sex stratification. Within married two-parent homes, the gains for residential fathers are in opposition to the wage losses of mothers (Avellar and Smock 2003; Budig and England 2001; Glauber 2007). The birth of a child, therefore, will tend to contribute to the within-couple gender gap in pay, giving men an improved bargaining position within the household. Differences in family structure also contribute to wage differences among men, with men who have the "package deal" of marriage and coresidential fatherhood experiencing wage gains that no other combination of union status and fatherhood achieves.

4.5 Appendix: Variable Construction

*Residential and Nonresidential Children, Method 1.* In survey waves 1982 and 1984-2008, respondents are asked to indicate the usual residence of each of their biological children. If a child is reported to live at least part-time with the individual, s/he is considered a residential child. If the child is reported to live with someone other than the respondent in a parent-like environment, the child is considered to be nonresidential. Parent-like situations include children who live with his/her other parent, other relatives, in foster care, with adoptive parents, with the respondent's partner's family, or with in-laws. Finally, if the child does not live with the respondent, but also does not live in a parent-like environment, or if the parent refuses to report or does not know the child's residence, s/he is considered to be in an "other residential" situation. Other residential situations include children living in long-term care institutions, away at school, in prison, in the military, in an independent residence, in some other kind of housing situation, or in a temporary residential arrangement. Only 1.4% of observations in the analytic sample include at least one child living in an "other" residential situation. Children who are deceased prior to the interview are excluded.

*Residential and Nonresidential Children, Method 2.* In all years, a household roster is collected that includes information on the relationship of each member of the household to the respondent. Individuals identified as the son or daughter of the respondent are considered residential children. Using the fertility files, the date of the birth and death of all of the respondent's children are constructed at the year-month level and are compared to the current year and month. Since the dates of birth of all children are updated in each survey wave, it is possible that the respondent will report a different birth date of his

children in different survey waves. I use the reported birth date in the first wave in which a child of that parity is reported, under the assumption that recall error will increase with time since the birth of the child. All children who are born prior to the current month, died after the current month, and are less than 18 years old at the time of the interview, are considered potential residential children of the respondent. The difference between this measure of potential residential children and the reported number of residential children from the household roster is the count of the respondent's nonresidential children. When the month of the birth is unknown, the birth is assigned to December of the year in which it took place. This is done in order to be conservative in estimating the number of nonresidential children: that is, I want to avoid assuming that a child is nonresidential when, in fact, s/he may simply have been born in a later month than the interview took place. Similarly, all deaths of unknown month are assumed to have occurred in January.

Because the usual residence of biological children is not asked in four years (1979, 1980, 1981, and 1983), the second method of constructing fatherhood status is used for these years. For all other years, the first method is used. If an individual never reported a year of birth for one or more of his children, and there is no valid data for the year of birth of a child of earlier parity to indicate that the earlier parity child was born after the current year, the observations from that individual for the years 1979-1981 and 1983 are deleted.

Among observations following the birth of at least one biological child (therefore, both measures should produce a count of at least one residential or nonresidential child), for whom both measures are available, the match rates are 94.7% for residential children

and 73.4% for nonresidential children. Results that used the second method for all years produced results very similar to those presented in the main results.

*Stepchildren.* The number of stepchildren in the household is determined by the number of individuals identified in the household roster as the stepson or stepdaughter of the respondent. This measure relies on respondents' own reports of their relationship to the children in their household. It is possible that stepfathers who are the most invested in their children are more likely to refer to the biological children of their wives as sons and daughters, rather than a stepson or stepdaughter. In this case, those who identify as stepfathers may be a negatively selected subset of true stepfathers.

4.6 Appendix: Labor Supply

Table 4.4: Multivariate Results, Annual Hours of Market Work, among
Those with Non-zero Hours

|                          | Coeff. (S.E.)       |
| ------------------------ | ------------------- |
| Fatherhood               |                     |
|   Married residential     | 14.85 (14.03)       |
|   Unmarried residential   | 47.71 (27.51)       |
|   Non-residential         | -44.91 (22.29)*     |
|   Stepfatherhood          | -14.76 (12.92)      |
| Unions                   |                     |
|   Cohabitation            | 51.58 (18.53)**     |
|   Married                 | 37.26 (14.65)*      |
|   Divorced                | 17.96 (26.54)       |
| Person-year observations | 51,920              |
| Individuals              | 4,479               |
| Overall $R^2$            | 0.07                |

*$p<.05$; **$p<.01$; ***$p<0.001$.
Note: Childless, never-married men living singly are the excluded category. The model
controls for the region of residence of the respondent, whether his health limits his work,
biological children who live in other residential arrangements, a quadratic in the
respondent's potential experience, his educational attainment, and their interaction. The
sample is restricted to the subsample of the main sample for which valid, non-zero annual
work hours were reported in the subsequent survey wave ($t+1$), concerning the previous
year ($t$). During the period in which the NLSY79 was conducted biannually, the reports
of annual work hours for the following ($t+1$) and preceding ($t-1$) years were averaged.

CHAPTER 5

**Conclusion**

In this chapter, I review the main conclusions and implications of Chapters 2-4 and

discuss avenues for future research. Each of the empirical chapters of this dissertation

makes use of a different nationally-representative panel dataset to perform quantitative

analysis that explores a different aspect of the work-family intersection. Chapter 2

provides evidence against the theory of compensatory gender display. Instead of wives

who out-earn their husbands spending more time in housework than wives who have

earnings parity with their husbands, I find that increases in wives' earnings uniformly

lead to declines in housework time, regardless of their husbands' earning levels. At the

same time, declines in wives' housework time with increases in their earnings are

modest, particularly for wives above the median of the earnings distribution. Thus, while

financial resources do not disadvantage wives in terms of their household labor burden,

even high-earning wives continue to spend, on average, far more time in housework than

their husbands.

Chapter 3 focuses specifically on the purchase of market substitutes for wives'

time in housework as a mechanism for the negative relationship between wives' earnings

and their time in housework. The fact that wives' earnings are positively associated with

households' expenditures on goods like cleaning services and child care, coupled with the

negative association between wives' earnings and their housework time, has suggested that high-earning wives may reduce their time in housework primarily through using their earnings to outsource housework to paid providers. Nonetheless, I find that less than 15% of the earnings-housework relationship is eliminated by the inclusion of controls for the household's reliance on market substitutes for time cooking and cleaning. Instead, high-earning wives may be choosing to spend less time in housework without purchasing a substitute for their lost time, leading to lower levels of domestic production in the household.

Chapter 4 examines the relationship between fatherhood and men's wages. I find that married residential fathers experience a wage premium of a little over 4% compared to childless married men, but unmarried residential fathers, nonresidential fathers, and stepfathers do not experience wage gains compared to childless men of the same union status. Thus, the fatherhood premium is only a premium for men who are married and live with their children. In fact, the wage premium for married residential fatherhood as compared to nonresidential fatherhood comprises more than half of the wage differences between married residential fathers and divorced nonresidential fathers. Thus, the returns to marriage for men with children, in terms of wages, operate in large part through the association between marriage and residential fatherhood.

5.1 Future Research on Housework

Although housework is a well-studied topic in sociology, research has disproportionately focused on understanding housework through the lens of gender, calling attention primarily to the decisions opposite-sex couples make about the division of housework

within the home. While gender differences in housework time contribute to sex stratification, and gender is an important determinant of housework time, gender is not the *only* lens through which to understand individuals' time in housework. Housework may also be considered as worthy of study as an element of time use, family life, and even a signal of class. However, less is understood about the ways in which households make decisions about the total level of domestic production in the household and about the determinants of housework time among those who are not in opposite-sex coresidential unions.[30]

Chapters 2 and 3 focus on a particular determinant of variation among married women in time spent in housework – labor income – but future research should consider additional factors, such as variation in taste for domestic production (as both a commodity and an activity) and in the availability of market substitutes (for example, geographic differences in the supply of domestic workers). More generally, more research is needed to understand the mechanisms by which demographic traits are linked to time spent in housework. Chapter 3 suggests that outsourcing is not the primary cause of the negative relationship between wives' earnings and their time in housework, but the relative importance of alternative mechanisms, such as selection, an ability to substitute the social rewards and esteem of paid labor for those of domestic production, or a social network with lower standards of household production, remains unclear. Likewise, although variations in housework time by race and education are well-documented (Baxter et al. 2008; Pittman and Blanchard 1996; Presser 1994; Sanchez and Thomson 1997), little is known about the reasons for these differences.

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[30] For a discussion of housework among same-sex couples, see Carrington (2002).

Qualitative interviews with individuals, single or in couples, may shed light on the ways in which decisions are made not only about how to allocate housework time between members of a couple, but what housework is appropriate or desirable to outsource, how to consider the financial costs of outsourcing against the time costs of performing housework oneself, or the tradeoffs between time and money spent in domestic production as opposed to other goods or activities. For example, how do households construct standards of what is "clean enough"?  Likewise, qualitative research can provide more information about the source of the observed non-linear relationship between wives' earnings and their housework time. While qualitative research has already been used to understand why high-earning wives continue to do more housework than their husbands (Tichenor 2005), it would also be valuable to understand what prevents them from further outsourcing or opting out of time in housework.

Quantitative research, too, might be used to better understand the processes at work in decisions about household labor. As discussed in Chapter 3, more datasets that include information on household expenditures on domestic services of various kinds as well as housework time are needed in order to replicate the analyses of Chapter 3 for a broader sample of adults. Although it would require adding several additional questions to existing survey instruments, measures of outsourcing of domestic labor could be improved by asking individuals to estimate how much of their own time is saved through expenditures on domestic services.

The results in Chapter 3 also highlight the heterogeneity in the relationship between earnings and housework time for cooking as opposed to cleaning, as well as the relative importance of outsourcing as an explanation for this relationship. This variation

suggests that grouping all housework time into a single category (or clustering together time in "female-typed" housework tasks) may mask important variation in the ways in which individuals think about housework. The National Survey of Families and Households (NSFH), which collects task-specific information on housework time for a variety of household tasks, might be used to understand whether variations by earnings, race, and education in housework time are robust across housework type, or are driven by a few key tasks.

Lastly, as discussed in Chapter 2, the operationalization of spouses' relative financial resources in models of housework time remains contested. Women's financial resources relative to their husbands may be operationalized as a function of spouses' relative wages, consistent with divorce-threat bargaining, or spouses' relative earnings, consistent with the idea that spouses' current social roles affect what division of household labor seems fair or natural. However, we know little about which operationalization has greater predictive power.

5.2 Future Research on Fathers

Future research is needed to explain the mechanisms that give rise to the heterogeneity in fatherhood wage premiums. In Chapter 4, I consider the possibilities that the variation across fatherhood types in the fatherhood wage premium may be caused by productivity, selection, or discrimination. Although audit studies and laboratory experiments have sometimes been used to assess the likely effects of discrimination on the wages or employment prospects of parents (Correll et al. 2007; Riggs 1997), similar research has

not, to my knowledge, been conducted to explore whether these effects vary by marital status and coresidence with the child.

If the observed heterogeneity in the fatherhood wage premium is due to productivity changes, more research is needed to understand how these changes come about. In Chapter 5, labor supply, occupation, industry, tenure, sector, and collective bargaining status were considered as possible mediators of the relationship between fatherhood and wages. However, even after controlling for these factors, married residential fathers receive a wage premium, while other fathers did not. It is possible that married, residential fathers are more likely to invest in wage-enhancing training, expend more effort while at work, or change jobs in order to receive higher wages at the expense of non-financial rewards (in some way not captured by the models in Chapter 5). Exploratory qualitative research is well-suited to provide information not only about how men respond to fatherhood, but specifically how fatherhood changes their orientation toward work. Quantitative analyses, including with the NLSY79, could also be used to examine more carefully the relative contributions of changes in occupation and industry, increased labor supply or tenure, or job traits or human capital investments, to the wage premium for married, residential fathers and penalties for nonresidential fathers and stepfathers.

Lastly, Chapter 5 does not examine within-group heterogeneity in the fatherhood premium. For example, it is possible that nonresidential fathers who have more frequent contact with their children experience a smaller fatherhood wage penalty (or even a wage premium), consistent with greater investment in the fatherhood role. Similarly, stepfathers who assume their role when the child is younger may experience a fatherhood

132

premium. Recent research suggests that nonresident fathers' involvement with their children declines more in response to *mothers*' new relationships than fathers' (Tach et al. 2000). Role ambiguity may be greater for both nonresidential fathers and stepfathers when both are involved in a child's life, leading to greater fatherhood penalties for fathers in this situation. Provided that adequate data exist, future research should examine whether the observed wage penalties or premiums associated with fatherhood vary by these determinants of men's fatherhood commitment and role ambiguity.

5.4 Work and Family: Stability and Change

The questions raised in Chapters 2-4 respond to new developments in the areas of family or women's work in the United States. The results highlight areas of both change and stability in the work-family intersection. Housework hours for women have fallen in recent decades (Bianchi et al. 2000), including among the sample in Chapter 2 of wives working full-time. Furthermore, Table 2.1 shows that, among couples in which both spouses work full-time, the fraction of wives who out-earn their husbands has risen from 15% in 1976-1984 to 26% in 1994-2003. At the same time, these changes have not been enough to overturn the traditional division of household labor: in the samples of married and cohabiting couples in Chapter 2 and 3, wives continue to spend, on average, more than twice as much time in housework as their husbands. Furthermore, both chapters find that wives' earnings have a relatively modest relationship with time spent in housework, suggesting that further gains in wives' financial resources may do little to close the gender gap in housework.

For men, increases in divorce, remarriage, and non-marital child-bearing (Teachman et al. 2000) have increased the diversity of fatherhood types. The analysis presented in Chapter 4 responds to this, examining the fatherhood wage premium separately for married and unmarried residential fathers, nonresidential fathers, and stepfathers. Here, too, there is some evidence of stability. Only the traditional role of residential father in conjunction with marriage is associated with wage gains for men. Consistent with the traditional norm of breadwinning for married fathers (Nock 1998b), married men receive a fatherhood premium. Thus, in the face of considerable changes in gender roles and family forms, some traditional work-family relationships for men and women remain.

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