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Essays on Relationship Quality and Outcomes

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Essays on Relationship Quality and Outcomes

by

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A thesis submitted to the
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of the requirements for the degree of
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Department of Economics

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This thesis entitled:
Essays on Relationship Quality and Outcomes
written by Erin K. Fletcher
has been approved for the Department of Economics

Terra McKinnish

Prof. Ann Carlos

Date _____

The final copy of this thesis has been examined by the signatories, and we find that both the content and the form meet acceptable presentation standards of scholarly work in the above mentioned discipline.

Fletcher, Erin K. (Ph.D., Economics)

Essays on Relationship Quality and Outcomes

Thesis directed by Prof. Terra McKinnish

The heterogeneous nature of romantic relationships leaves much to be studied in terms of the effects of relationship status on economic decision-making and allocations. In particular, it may be that there is greater variation within what are generally considered 'like' groups—e.g. married couples or cohabiting couples—than there are between these groups. This dissertation examines how this heterogeneity is associated with preferences for investing in children and susceptibility to violence in a relationship. Using two nationally representative, US data sets—the Fragile Families and Child Wellbeing Study and the National Survey on Families and Households—I examine outcomes associated with various types and quality of romantic relationships. In two papers, I exploit the detailed nature of Fragile Families to account for previously unobserved heterogeneity within couples, including their likelihood to marry, relationship quality, estimation of the partner's character and the likelihood of financial support from a partner. This study employs linear regression, propensity score matching and ordered probit regression to link relationship quality and expectations of support to different types of investment in children such as reading to children and bringing children to the doctor. I find that attitudinal questions concerning relationship quality and expectations of support are significant predictors of investments in children in some cases and significance is tied to the type of the investment. In the third chapter, I examine heterogeneity of violence and violent relationships and find that, somewhat contrary to a traditional understanding of family violence, women who leave violent relationships tend to forge new relationships without violence and violence can end within a relationship.

Dedication

For my family

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Any errors or omissions are my own.

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Chapter 1

Match Quality and Maternal Investments in Children

1.1 Introduction

It is widely believed that parents' early investments in children have a significant effect on their performance in school and on subsequent tests of intelligence. While some of the variation in measures of intelligence like IQ, grades, and standardized exams is likely attributable to genetics, parents are encouraged to read to and with their children for the simple reason that such actions are also thought to stimulate intellectual curiosity and development.

In turn, parents likely are endowed with a set of characteristics that lead them to invest more or less in their children's cognitive skills. Education, own parents' investments, and cultural background might influence cultural norms and personal preferences that influence time spent with children. Income and work status represent time constraints on such activities as well as differences in willingness to pay for goods like museum trips or private school. However, such characteristics do not fully explain variation in parents' time spent with children and there are likely unobservable characteristics that exert an additional influence. In this paper, I examine one of these previously unobservable characteristics, whether the relationship of a mother's investment in her children is related to the quality of her relationship with her children's father.

This is the first paper, to my knowledge, that empirically links what I term subjective match quality, or the perception of the quality of one's relationship, to the amount of time spent with a child. I show that women who rate their relationships highly on average report spending more time reading with their child than women who rate their relationship with the child's father poorly.

Though the estimates range from 0.8 days a week to 1.6 days a week extra, the result is highly statistically significant and robust to several different specifications and the inclusion of comprehensive controls for individual characteristics. I explore a number of potential theoretical mechanisms for this difference and test them by exploiting the longitudinal design of the Fragile Families and Child Wellbeing Study. The primary implication of this study is that there is a broader range of quality within marriages and other romantic unions that may affect the child's welfare before divorce or union dissolution occurs—if ever. Anticipation of divorce and current match quality can have effects before divorce or separation is imminent.

It is possible that the effect captured is individual, person-specific characteristics and not relationship quality. It may be that parents with lower preferences for investing in their children form unhappy relationships and thus we see that those who rate their relationship lower are the ones who are investing less in their children. I attempt to control for this unobserved heterogeneity by including controls for prenatal investments in the child and a measure of baseline match quality.

1.2 Background

There is a significant literature showing that investments in children such as reading to children or enrolling them in private schools are linked to measures of cognitive ability such as test scores and educational attainment.¹ For instance, an increase in mother's child care time is associated with additional years of schooling (Datcher-Loury, 1998). In addition to observed characteristics, Fink and Mukherjee (2007) note that unobservable, family- and mother-level characteristics are associated with differences in outcomes for children. On the relationship side, it has been shown that stability with regard to parental presence, and divorce more particularly, is associated with a significant effect on a child's cognitive outcomes (Craigie 2008). McLanahan & Sandefur (1994) show that children from single-parent families are more likely to drop out of high school, experience teen births and are less likely to go to college than children living with two parents. However, a causal link between divorce and adverse outcomes is not apparent (Ermisch & Francesconi, 2001).

¹ Haveman & Wolfe (1995) present a comprehensive review of the literature on this topic.

With the assumption that there is a relationship between investments and outcomes, I examine the links between happiness in a marital or non-marital romantic union and investments in children.

In addition to the above work in the economics literature, this paper is also related to a strand of literature, which unites and pulls from diverse fields such as psychology, psychopathology and biochemistry, that links environment to children's outcomes. Through biochemical or other processes, marital discord, maltreatment, and unloving environments are linked to adverse social and psychological outcomes (Cicchetti & Toh, 2005), as well as increased rate of disease (Corso, Edwards, Fang & Mercy, 2008).² A model of family stress by Conger, et.al. (1992) postulates that marital discord or unhappiness affects children's adjustment behavior through parenting and parental investments. Recently, some work has been done to try to identify biochemistry as the missing link between chaotic households and adverse outcomes (Miller & Chen, 2010). While I do not directly link investments to outcomes in this paper, I do provide an alternative hypothesis that parents' choices given marital or relationship discord affect investments, which might, in turn, affect outcomes.

Other, more closely related, work examines directly the link between match quality and household behaviors. These include investments in children as well as other factors that dictate investment levels in children. These papers define match quality using marital status, with much of the analysis hinging on marginal marriages and divorces. Theoretical models such as Brown and Flinn (2007) show the interdependency of child quality and match quality—or really match survival—and lay out the implications of divorce on investments in children. Chiappori, Fortin and Lacroix (2002) show how the divorce decision affects women's labor supply. Aizer and McLanahan (2006) link monetary investments in children to child support paid. My analysis unites some of the ideas in these papers and adds to them by including unmarried couples with children and allowing for more nuance within the match quality category by employing questions posed to mothers about the quality of their relationship. This is the first paper, to my knowledge, to include unmarried

² Several more papers on this topic can be found on the Center for Disease Control's website <http://www.cdc.gov/nccdphp/ace/publications.htm>

mothers in the sample and assess the greater impacts of relationship discord on investments in children.

The closest paper to mine, Schmierer (2010), presents a model showing that anticipation of divorce results in fathers decreasing time spent with children. Schmierer also presents some empirical evidence to support his model using different data and finds no effect for mothers, whereas I find a statistically significant effect for mother. My work differs from Schmierer in several key ways. First, the addition of the subjective match quality variable allows me to examine the state of the relationship before the union dissolves, if it ever dissolves. In this way, I can compare bad relationships that end to bad relationships that do not end, whereas without such a variable, we can only compare relationships that end to those that do not, with little information about their quality. I also include far more mother-father pairs that are unwed, an important addition given the growing demographic of unwed parents, and additional variation in the outcome variables not captured by the dichotomous variables he employs.

1.3 Theoretical Mechanism

Though there are many, varied explanations, I examine three mechanisms by which we would expect mothers to decrease the time spent with their children given poor match quality. The first and second are guided by the time constraint and the third is informed by literature in economics about children as a public good. In any case, this is primarily a story of opportunity costs.³ These are explanations that to some extent can be tested using these data.

Firstly, due to natural time constraints, it can be posited that couples who argue more have less time to spend with their children because they spend more of their time bargaining among themselves. In the data, I show that couples who argue more rate their relationship lower on average.

An alternative explanation is that women who are unhappy in their relationships are acting

³ Datcher-Loury (1998) shows that higher opportunity costs decrease maternal child care time and that there is great variation in the quantity and quality of time inputs among mothers, even among those with similar levels of education.

in anticipation of the end the relationship and thus, the steps that follow that end. Perhaps women who see their relationships as ending, particularly in the near future, are more likely to spend time investing in themselves—taking a class, purchasing conspicuous consumption goods, or returning to the workforce, for example. When the relationship appears to be ending, the opportunity cost of not investing in one’s future match potential becomes higher. Much of the theoretical work on match quality mentioned above hinges on this idea and thus we see women who are unhappy in their relationships investing more in themselves. With a time constraint, this might result in decreased investment in children.

As a corollary, we can view children as a public good in which both parents invest and receive utility. Union dissolution, in most cases, diminishes the value of the child as public good through decreased time spent with the child as a result of custody agreements or perhaps by decreasing future returns such as care in old age. As a result, anticipation of the union’s dissolution would cause an anticipatory decrease in investment in the child due to a decreased future benefit stream.

While these mechanisms are not mutually exclusive and there are certainly other possibilities by which we might expect a relationship to affect investments, I can test directly for the time-constraint argument using the Fragile Families data set and questions posed about argument frequency. I also test the anticipation explanation by controlling for the future relationship status of couples. For the 1-year and 3-year follow-up surveys, I can control for whether the parents are still together in subsequent waves. While this does not cover the entire range of relationships that eventually end, I at least can observe variation in reading days given the most imminent relationship changes. I am unable to distinguish whether the anticipation effect is a result of the public goods argument or the investing more in one’s future match quality argument. Likely, there is some combination of mechanisms acting on mothers.

In addition, there is the possibility of an effect in the opposite direction, whereby poor match quality results in more time spent with children. One possible mechanism is apparent in the case where you have very high quality parents. In this situation, poor match quality might be perceived as affecting the child and thus parents would strive to spend more time with the child to compensate.

If this effect is strong, we would see a downward bias in the coefficient on match quality, perhaps more so from highly educated parents. For this reason, I include parental characteristics as controls to account for education, income and parental quality where possible.

1.4 Data

The Fragile Families and Child Wellbeing Study (Fragile Families) is a four-wave, longitudinal data set that was collected to facilitate the study of issues within “non-traditional” families, which includes unmarried parents, blended families, and single parents. It consists of a representative sample of women living in 20 large cities in the United States who gave birth in 1998, 1999 or 2000 and a significant oversample of unwed mothers who gave birth in the same time period. The study follows both parents of a focal child over several years. Follow-up surveys were conducted one year, three years and five years after birth of the child with the mother and father, individually. The baseline surveys take place in the hospital right around the time of birth and provide baseline measurements for characteristics such as parent quality and match quality. Subsequent surveys are given over the phone and ask similar questions about parent and match quality as well as how time is spent with the child. In particular, I am interested in investment in children’s cognitive skills as measured by how much time is spent reading with a child on a weekly basis.

For this paper, I employ the baseline and follow-up surveys as administered to the mother of the child when she remains the primary caretaker, and so long as she reports some sort of intimate relationship with the child’s father. Mothers who work or have someone look after the child are included, though I do control for whether a child is in someone else’s care besides the mother’s. If the mother is doing something besides spending time with her child—such as working or job-searching—we expect her to spend less time reading with her child. Mothers who report that their child is living with someone other than the mother are excluded from the sample.

I work with multiple cuts of the data. I divide the relevant sample as described above into three separate samples by conditioning on romantic involvement with the father in each previous wave: women who are involved in the first two waves, then the first three waves, and finally all four

waves. Women who are in a relationship with someone other than the reported father of the child are dropped.

The first sample is called the ‘1-year sample’ and consists of all women in relationships with the father of the focal child at the baseline and at the time of the one-year follow-up survey. For this group, characteristics such as whether the child is ever in someone else’s care, earnings, and in particular, subjective match quality are measured as they are reported at the one-year follow-up survey. This sample has 1,902 observations at the mother level.

The second sample is termed the ‘3-year sample’ and is made up of respondents from the 1-year sample minus any respondents who report having separated or divorced her spouse or otherwise dissolved the romantic relationship with the child’s father. For this group, characteristics such as whether the child is ever in someone else’s care, earnings, and in particular, subjective match quality are measured as they are reported at the three-year follow-up survey. There are 1,382 women in this group. It should be noted that this and the following sample decrease in size for two reasons. There is some survey attrition, but most of the participants who are in the 1-year sample but not the 3-year drop out due to having ended their romantic relationship with the child’s father.

The final sample is of women who are involved with the father of the child in every wave, from baseline to the five-year follow-up survey. This allows for pooling and fixed effects specifications on a sample of 1,087 women. I call this group the ‘5-year sample’ and characteristics not marked as “at birth” come from the five-year follow-up survey and consists of 1,087 mothers.

Summary statistics for the 1-year follow-up respondents in relationships and the longitudinal sample are provided in Table 1.1. Those who remain in a relationship with the focal child’s father tend to be slightly more educated and older. They are more likely to be white and to have sought prenatal care earlier in the pregnancy. They are less likely to have collected unemployment or received public assistance at the time of the birth of the focal child and less likely to have used drugs, cigarettes or alcohol during the pregnancy.

Table 1.1:

Baseline Characteristics			
All Mothers and those Always with Father			
	1-Year	3-Year	5-Year
Mother's Age	26.07	26.74	27.20
%Female Child	52.71	52.13	52.13
Low Birthweight?	8.12	7.61	6.75
%Married	38.10	44.18	49.24
% Cohabiting	43.09	41.20	38.32
% In Public Housing	7.58	7.20	6.80
% Mother US Born	82.81	81.38	80.26
Earnings (\$1000s)	6.47	6.76	7.04
Public Asst (\$1000s)	0.56	0.50	0.43
Unemployment(\$1000s)	0.28	0.28	0.27
Num Other Kids	1.10	1.10	1.07
% Prenatal Care	0.99	0.99	0.99
Month of First Prenatal Visit	2.35	2.27	2.20
% Alcohol Used During Preg	1.75	1.64	1.69
%Drugs Used During Preg	2.41	2.63	2.37
% Cigarettes Used During Preg	15.60	14.64	13.69
%White	38.17	41.86	44.42
% Black	42.07	37.63	35.60
%Asian	3.35	4.00	4.37
%Native American	1.65	1.79	1.56
%Other	14.64	14.67	13.97
% Latina	26.57	27.07	26.25
% Less than 8th Grade	3.24	3.10	2.44
% Some High School	22.59	20.70	19.02
% HS Diploma	25.13	24.21	23.99
%GED	4.55	4.27	4.05
% Some College	0.25	25.38	25.06
% Tech or Trade School	2.93	2.57	2.98
% BA/BS	10.99	12.92	14.90
%Graduate School	5.56	6.84	7.56
N	1902	1382	1087

1.4.1 Measures of Match Quality

The Fragile Families data include a number of unique measures of match quality, of which I use several. The primary variable of interest is mother's estimation of relationship quality, but I also add controls for a baseline measure of quality measured by whether the couple is married, their reported chances of marrying if unmarried, frequency of arguments and whether the relationship ends in a future wave.

Mothers' report of relationship quality is measured in the three follow-up surveys with the question: How would you rate your relationship with child's father? The options are "Excellent", "Very Good", "Good", "Fair", and "Poor". For each of the responses, I create a dichotomous variable taking a value of one for the answer which the respondent gave and zero for the others. I exclude "Poor" from the regression specifications as the constant.

The relationship quality question was not asked in the baseline survey, but I do have an alternative measure of baseline match quality. I consider whether a parent is married at the birth of the child under the assumption that if they are married, they must have considered the match good at some point. If unmarried, the mother reports that a marriage to the child's father is "Certain", of "High" probability, "Low" probability, or a "50-50" chance. This provides a proxy for match quality before the child is born. For purposes of Table 1.3, I combine and dichotomize this variable, calling it 0-1 Baseline Match Quality. Respondents who are married at the baseline or report a "Certain" or "High" chance of marriage are given a 1 and others are given a 0.

I also measure argument frequency. In the follow-up surveys, mothers are asked how often they argue with the focal child's father "about things that are important". Answers are coded "Always", "Often", "Sometimes", "Rarely" and "Never". The questions on argument frequency were also different in the baseline survey. I employ principal components analysis to combine seven questions related to argument frequency on various topics asked in the baseline survey into a single index of baseline argument frequency.

Finally, I also consider the future status of the couple. For each wave, I determine whether the

Table 1.2:

Mother's Relationship Rating by Wave						
	1-yr Follow-Up		3-yr Follow-Up		5-yr Follow-Up	
	Number	%	Number	%	Number	%
1 Excellent	906	26.77	715	23.15	595	22.09
2 Very Good	1,033	30.52	870	28.17	733	27.21
3 Good	664	19.62	598	19.37	513	19.04
4 Fair	413	12.2	421	13.63	418	15.52
5 Poor	369	10.9	484	15.67	435	16.15
Total	3,385	100	3,088	100	2,694	100

couple breaks up—separates, divorces, or the mother reports they are no longer intimately involved—and create a dichotomous variable with a value of 1 if the relationship ends and 0 if it continues. In waves where there is at least one wave following available, I can use whether the relationship ends in a subsequent wave as a additional control. Though I don't have information for the entire trajectory of each relationship regarding the status of these relationships, relationships that end soon after the birth of the child are likely to be different from those that end later. More immediate union dissolutions may be better anticipated than ones far into the future and thus more likely to exert an effect. The percentage of women reporting each relationship status by wave is displayed in Table 1.2.

Measures of match quality are interesting in their own right and are likely affected by cultural norms and individual characteristics. Observable characteristics such as age and education level can be directly controlled for. It is problematic, however, if all high-quality parents, and thus those that read to their children more, report being in good relationships. The definition of a high-quality parent is certainly debatable and generally unobservable, but I take into account parental quality using observable, baseline characteristics to control for this phenomenon to some extent.⁴ Table 1.3 presents regression results of each wave's 0-1 measure of match quality on these baseline characteristics as well as education, race, and earnings and welfare information. Though black women rate their relationships more poorly on average, most observable characteristics that we

⁴ Heckman and Robb, 1985 discuss selection on observables in non-randomly selected samples.

would associate with parental quality are not predictive of match quality. Whether a woman did drugs during pregnancy is predictive, but the number of women reporting prenatal drug use is small, around 5.5% of the baseline sample.

1.4.2 Measures of Investments in Cognitive Skills

For a measure of investments in cognitive skills, I focus here on reading days per week. Reading to children has been shown to be an important predictor of children's outcomes (Raikes 2006) and given the age of these children, we do not observe investments such as school choice or outcomes such as test scores. Mothers are asked both how many days each week they read to the focal child and how many days per week the father reads to the focal child. Other measures of time investments are asked in a similar manner about activities such as time spent playing inside and watching television. The outcome variable, thus, is measured discretely and takes values between zero and seven. Though one third to one half of the sample reports reading to their child every day of the week ($y=7$), there is significant variation in the responses and they do vary over time. Table 1.4 shows the distribution of reading days per week as reported by the mother by wave.

It is likely that parents' investments in children's cognitive abilities are somewhat endogenous to children's displayed abilities (Brown & Finn 2007). For instance, children that show signs of learning more words might be read to more often, or it may be the case for children who show signs of learning fewer words. The lack of test scores at the young ages available in the Fragile Families mean that most parents do not have an objective measure of their child's cognitive ability. However, this does not mean that they are not aware of it and not reacting to it, only that their measure is not as readily comparable to other children's. Thus, though we expect some endogeneity in the measure of reading days per week, the understanding of a child's abilities is imperfect and thus should not perfectly predict investments.

Figures 1.1, 1.2, and 1.3⁵ show graphically that the average number of reading days reported

⁵ The boxplots represent the distribution of days per week spent reading with the focal child, delineated by the mother's report of relationship quality in that wave. The ends of the plot (or the 'whiskers') represent the maximum and minimum responses. The top and bottom of the box represent the first and third quartile and the line in the

Table 1.3:

Baseline Characteristics on Good-Bad Relationship Indicator by Wave				
0-1 Indicator of Quality	Baseline	1-yr	3-yr	5-yr
Mother's Age	0.00552*** (0.00156)	-0.00763*** (0.00204)	-0.00427** (0.00209)	-0.00254 (0.00234)
Female Child	0.00172 (0.0143)	0.0225 (0.0183)	0.00662 (0.0191)	0.00217 (0.0212)
Mother Born in US	-0.0742*** (0.0237)	-0.0192 (0.0303)	0.0412 (0.0303)	0.0194 (0.0331)
Log of Income	0.0718*** (0.00869)	0.0226* (0.0117)	0.0168 (0.0126)	0.0284** (0.0140)
Total Num Kids	0.0195*** (0.00653)	0.0146* (0.00880)	0.00375 (0.00911)	0.00641 (0.0104)
Black	-0.147*** (0.0174)	-0.118*** (0.0223)	-0.111*** (0.0235)	-0.0897*** (0.0264)
Asian	-0.0620 (0.0484)	0.0156 (0.0605)	0.0327 (0.0573)	0.0236 (0.0642)
Native American	-0.0640 (0.0402)	0.0247 (0.0723)	-0.0212 (0.0526)	-0.0675 (0.0568)
Other	-0.103*** (0.0251)	-0.0658** (0.0306)	-0.0420 (0.0323)	-0.136*** (0.0353)
Hispanic	-0.261 (0.266)	-0.155 (0.309)	-0.658 (0.439)	-0.719 (0.440)
Some High School	0.0835** (0.0376)	0.0132 (0.0583)	0.0598 (0.0554)	-0.0640 (0.0616)
HS Diploma	0.121*** (0.0387)	0.128** (0.0589)	0.142** (0.0564)	0.0178 (0.0625)
GED	0.160*** (0.0485)	0.0636 (0.0699)	0.134* (0.0705)	0.0122 (0.0794)
Some College	0.184*** (0.0396)	0.142** (0.0594)	0.104* (0.0570)	0.00236 (0.0635)
Tech or Trade School	0.134** (0.0537)	0.0927 (0.0778)	0.227*** (0.0798)	0.00974 (0.0849)
BA or BS	0.305*** (0.0471)	0.223*** (0.0665)	0.152** (0.0639)	0.0171 (0.0701)
Graduate School	0.329*** (0.0550)	0.211*** (0.0733)	0.189*** (0.0699)	0.0176 (0.0769)
\$ Public Assistance	-0.0202*** (0.00519)	0.00721 (0.00698)	0.0108 (0.00791)	0.00347 (0.00937)
Unemployment Collected?	-0.00633 (0.0246)	0.00331 (0.0316)	-0.0215 (0.0334)	-0.0607 (0.0371)

Continued on next page

Continued

Baseline Characteristics on Good-Bad Relationship Indicator by Wave

0-1 Indicator of Quality	1-yr	3-yr	5-yr
Living in Public Housing	0.0540 (0.0347)	0.00878 (0.0373)	0.0307 (0.0431)
Any prenatal care?	-0.0660 (0.0884)	-0.0932 (0.0916)	-0.0394 (0.105)
First prenatal visit month	-0.00962 (0.00715)	-0.00963 (0.00764)	-0.00453 (0.00865)
Parents Married at Birth?	0.0911*** (0.0244)	0.0913*** (0.0247)	0.0782*** (0.0274)
Mother drank alcohol during pregnancy	-0.0103 (0.0722)	0.0517 (0.0765)	0.0342 (0.0819)
Mother used drugs during pregnancy	-0.124** (0.0563)	-0.100* (0.0577)	-0.177** (0.0687)
Mother smoked cigarettes during pregnancy	-0.00686 (0.0267)	-0.0624** (0.0292)	-0.0390 (0.0330)
Constant	0.676*** (0.168)	0.644*** (0.176)	0.563*** (0.201)
Observations	1902	1382	1087
R-squared	0.073	0.060	0.062

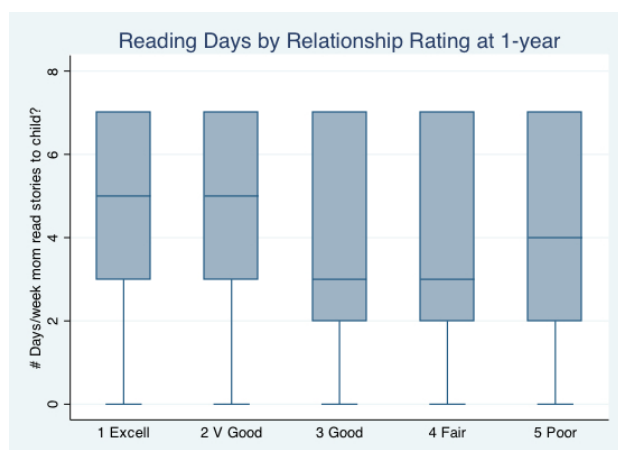
Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Table 1.4:

Mother's Reading Days per Week by Wave

	1-yr Follow-Up		3-yr Follow-Up		5-yr Follow-Up	
	Number	%	Number	%	Number	%
0 None	223	6.62	73	2.38	63	2.37
.5 d/wk	25	0.74	—	—	—	—
1 d/wk	189	5.61	81	2.64	112	4.21
2 d/wk	375	11.12	218	7.1	244	9.18
3 d/wk	618	18.33	337	10.98	409	15.39
4 d/wk	352	10.44	308	10.04	316	11.89
5 d/wk	400	11.87	352	11.47	433	16.29
6 d/wk	71	2.11	82	2.67	90	3.39
7 d/wk	1,118	33.17	1,618	52.72	991	37.28
Total —	3,371	100	3,069	100	2,658	100

Figure 1.1: Reading Days by Relationship Rating for Follow-up Survey 1



is in fact different for mothers reporting various levels of satisfaction in their relationships. Interestingly, and especially in the first wave, mothers who rate their relationship as excellent report the highest number of reading days, but mothers who rate their relationship as poor report a higher average number of reading days than those who rate their relationship as good or fair. This may be an indication that the mothers who are most likely to see their relationship ending are aware of their changing behavior and somehow attempt to compensate for it.

While similar questions were asked of fathers about reading days per week, the response rates are much lower than they are for women and participation degrades much more quickly over time. In an attempt to get at the same measure, mothers are also asked how many days per week the father reads with the child. I choose not to include this measure in the regression analysis because I believe it to be more reflective of the mother's preferences for reading than the father's. This is seen in the high correlation between mother's and father's reading days, as reported by the mother. Table 1.5 shows correlation coefficients for father's reading days with both mother's reading days and mother's relationship rating. Perhaps unsurprisingly, father's reading days as reported by the mother are negatively correlated with relationship rating. That is, women who rate their relationship more highly (= 1 are more likely to report a higher number of reading days. In a limited sense, the mother's estimation of the father's reading days may also be reflecting her

middle of the box represents the median.

Figure 1.2: Reading Days by Relationship Rating for Follow-up Survey 2

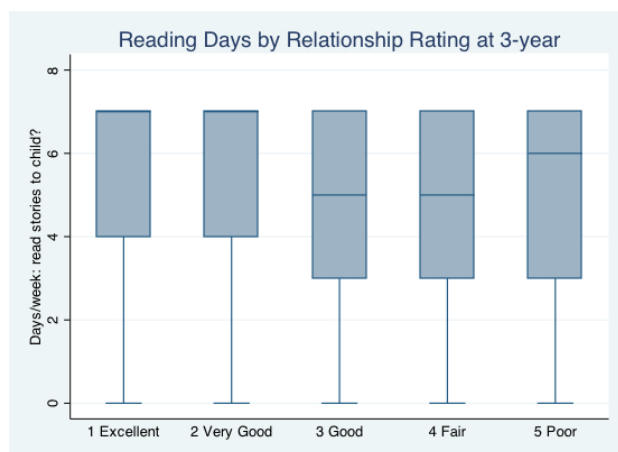


Figure 1.3: Reading Days by Relationship Rating for Follow-up Survey 3

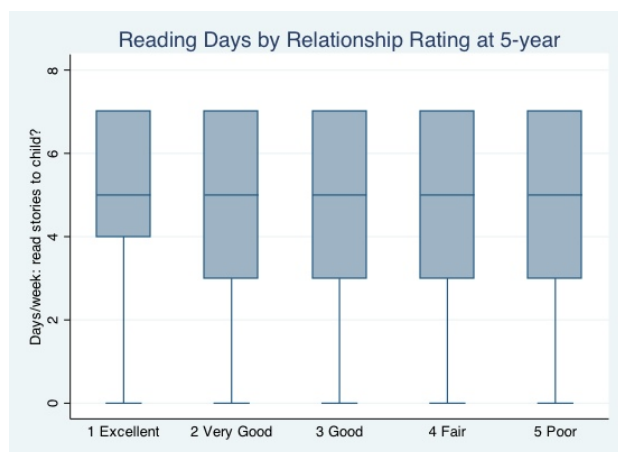


Table 1.5:

Correlations–Father’s Reading Days			
	Father’s Reading Days		
	1-year	3-year	5-year
Mother’s Reading Days	0.4141	0.3824	0.3310
Mother’s Relationship Rating	-0.3135	-0.3644	-0.3599

opinion about the father’s involvement in the child’s life as well as her relationship with him.

1.4.3 Data and Conceptual Issues

The ordered nature of the measured variables introduces a wide margin for error in this test. Time reading with a child is measured in days per week, for example, and frequency of arguments is coded as “Often”, “Sometimes”, or “Never”. Though I do account for the categorical variables by creating a set of dichotomous variables for each answer, the result is that the magnitudes are somewhat difficult to interpret.

In addition, mothers are asked, implicitly in hindsight and on average, how many days per week they read with the child as opposed to having some check-off process or time-use survey where we could see actual days or actual hours spent reading. This combined with the discrete nature of the outcome variable indicate that, rather than a precise recount of days spent reading every week, there is a sort of underlying process by which mothers arrive at the number of days they read with their child. A mother might try to recall and average over the past two or three or more weeks, or she might average long periods of reading in a single day over several days. Thus, when we do see a significant effect of argument frequency on days spent reading, the magnitudes of the coefficients are somewhat murky, but can at least be interpreted for their sign and significance. These issues make the use of an ordered probit specification more useful. This is discussed at the end of the empirical section and preliminary results are given at the end of the results section.

1.5 Empirical Strategy

1.5.1 Baseline Specification

The baseline specification is:

$$y_i = \alpha + \sum_{j=1}^4 \beta_j \times MatchQuality_{j,i} + X_i \gamma + \epsilon_i \quad (1.1)$$

where y indicates the number of days per week that a mother reads with her child, $MatchQuality$ is a vector of dichotomous variables for match quality where one of the five entries takes a value of one and the others zero. X is a vector of socio-economic and individual characteristics including race, education, mother's age and immigrant status and child's gender as well as the baseline socio-economic characteristics described above.

This model is estimated separately on each of the three regression samples: the 1-Year Sample, the 3-Year Sample and the 5-Year Sample.

1.5.2 Prenatal Investments

I control for unobserved heterogeneity and mediate omitted variable bias by controlling for the mother's baseline match quality and investment in the children. Baseline match quality as measured by a combination of marital status and unmarried mothers' report of the chances of an eventual marriage is added to control for effects that child quality might have on match quality. Measures of prenatal investment include whether prenatal care was sought, the month in which the first doctor's visit occurred, whether the mother used drugs and alcohol or cigarettes during pregnancy.⁶ Additionally, I control for whether the child was ever breastfed. The lack of variation over time in the match quality and reading variables does not allow for a traditional fixed effect strategy,

At baseline, participants are asked whether they are married, and if they are not married, they are asked what the chances of marrying the child's father is. I use these two measures to

⁶ I also added a control for whether the child was of low birthweight, but the variable is sparsely populated for the relevant sample. This, combined with lack of significance on the coefficient, led me to exclude it.

control for baseline match quality under the assumption that if a couple married, at some point they would have considered their match high enough to take that step. And similarly, if they intend to marry, there must be some perception of a high quality match. The baseline survey does not include the subjective match quality questions asked in future waves.

As the baseline survey takes place at the hospital around the time of birth, there is no measure of reading days. Instead, I use measures of prenatal behaviors to control for mothers' initial level of investment in the child.

The measures of a mother's prenatal investments in the child include whether prenatal care was received, at what point in the pregnancy prenatal care was sought, and behaviors such as alcohol, drug and cigarette use during pregnancy. Prenatal investments are indicated by the vector Z_i . I control for baseline match quality as measured by marital status and respondents' report of the chances of imminent marriage. Respondents report a chance of marriage as "Certain", "Good", "50-50", "A Little", or "No Chance".

$$y_i = \alpha + \sum_{j=1}^4 \beta_j \times MatchQuality_{i,j} + \sum_{k=5}^9 \beta_k \times BaselineMatchQuality_{i,k} + X_i \gamma + Z_i \psi + \epsilon_i \quad (1.2)$$

where y_i is reading days again. Here we add Z_i , which is a vector of controls for prenatal investments and $BaselineMatchQuality_i$ which is a vector of dichotomous variables on marital status and chances of marriage reported at the baseline.

1.5.3 Argument Frequency

Next, I employ a similar strategy as above but this time controlling for argument to frequency in order to test whether increased argument frequency leads to decreased time spent reading. The Fragile Families Data, in addition to this subjective measure of relationship quality, offers a few measures of relationship quality that might be considered more objective. In the first case, mothers are asked in each wave about how often they argue with the child's father. This constitutes a measure of how time may be spent when not with the child as well as a measure of match quality.

In addition to the baseline controls, I control for argument frequency as asked in the wave

and baseline argument frequency. As the argument frequency questions are asked differently in the baseline survey, I use principal components analysis to account for the variation in the questions posed on argument frequency at the baseline. The baseline survey asks five separate questions about argument frequency while subsequent surveys ask how often the couple argues about “things that are important” to them. I use principal components analysis on the five questions and retain the first two components to use as regressors in the next specification, named PCArg1 and PCArg2.

7

$$y_i = \alpha + \sum_{j=1}^4 \beta_j \times MatchQuality_{i,j} + \sum_{l=1}^4 \eta_l \times ArgumentFreq_{i,l} + \quad (1.3)$$

$$\phi_1 PCArg1 + \phi_2 PCArg2 + X_i \gamma + Z_i \psi + \epsilon_i \quad (1.4)$$

The specification is similar to those above, but now includes a vector of dichotomous variables for Argument Frequency in the wave being examined and the additional controls for baseline argument frequency as indicated by the principle components, PCArg1 and PCArg2.

1.5.4 Future Status

Most of the literature on match quality tends to focus on the marginal decision of marriage or divorce and hinges on the assumption that this decision is indicative of quality of the match. While this is certainly one indicator of quality, it is likely a noisy one and does not allow for a measure of match quality for unmarried couples. Marriage and divorce may be more or less expected or acceptable in different cultures and individuals, as well as cultural groups, seek divorce for different reasons. As my sample includes both married and unmarried parents, I focus on the question of union dissolution as reported by the mother. Ideally, I would have the measure for all couples, knowing when and if every relationship ended. However, with the number of surveys, we can only look a few years into the future. Other studies show that the extent to which parents will change their investment behaviors in expectation future relationship status are limited to a horizon of a few years, so the data used here should span a sufficiently long timeline to show an effect, if one is

⁷ A screeplot of the eigenvalues is available in Figure 1.8.

present. I cannot ultimately distinguish whether some of the behaviors I see are more attributable to anticipation of union dissolution for mothers who remain in relationships for all four waves due to the limited time horizon.

I perform the first analysis adding measures of the couple's relationship status in the future for the 1-year and 3-year follow-up surveys.⁸ In addition, I test whether the dissolution of marriages affects child investments more or less than the dissolution of relationships of unwed couples. I do this by interacting marital status in that wave with future relationship status for each of the waves available. Thus, for the specification on reading days in the one-year follow-up survey, the variable *MarriedbyRelEnd3* indicates that the couple was married during the 1-year follow-up survey, but separated or divorced by the time of the interview for the 3-year follow-up survey.

$$y_i = \alpha + \sum_{j=1}^4 \beta_j I_{i,j} \times MatchQuality_{i,j} + \theta_1 RelEnd3 + \theta_2 RelEnd5 + \theta_3 MarriedbyRelEnd3 + \theta_4 MarriedbyRelEnd5 + X_i \gamma + Z_i \psi + \epsilon_i$$

where *RelEnd3* is a dichotomous variable that takes the value of one if the relationship ends by separation, divorce or break-up as reported by the mother in the three-year follow-up survey and zero otherwise. *RelEnd5* is a similar indicator, taking a value of one if the relationship ends by the five-year follow-up survey. The preceding specification is for the days per week spent reading as measured in the one-year follow-up survey. The specification for the 3-year Sample is as follows but without variables *RelEnd3* and *MarriedbyRelEnd3*.

1.5.5 Fixed Effects and Ordered Probit

The panel nature of the data naturally leads to a fixed effects specification. I measure the change in reading days over the change in relationship status over time for each individual.

$$y_i = \alpha + \sum_{j=1}^4 \beta_j \times MatchQuality_{j,i,t} + X_{i,t} \gamma + \rho_i + \epsilon_{i,t} \quad (1.5)$$

⁸ Future status information is not currently available for the 5-year follow-up survey, but is slated to be released in late 2012.

Due to the nature of the left-hand side variable, a discrete variable that takes on values zero to seven, I also use an ordered probit model to estimate the model. While the measure of days per week surely indicates the need for estimation with a count or probit model, the best model is not immediately apparent. Count models, such as the Poisson or Negative binomial, are likely more appropriate in situations where the count is very clearly taken for each observation, such as parking tickets. If mothers were asked to keep track of the days of the week that they read to their children each week, a count model might be more appropriate. However, the design of the question, which asks respondents to estimate the number of the days per week they read with the child, implies some underlying process by which respondents remember and likely average the number of days spent reading over weeks or months. This may be related to a preference for reading with a child or an averaging of time over several days or weeks or perhaps an understanding by the respondent of what the “right” answer is. While it is clear that 2 days is more than 1 per week, the question does not make clear that 2 days is actually twice as many as 1, or that 4 is twice as many as 2, as a count model would require. The ordered probit model is useful for ordered, discrete outcomes, such as reading days per week, where it is assumed that there is some underlying cut-off which moves responses from one discrete outcome to each higher one. The ordered probit model is:

$$S_i = \alpha + \sum_{j=1}^4 \beta_j \times MatchQuality_{j,i} + X_i \gamma + \epsilon_i \quad (1.6)$$

$$y_i = 0 \text{ if } S_i \leq \mu_0; \text{ Prob}[y_i = 0] = \Phi[\mu_0 - X'_{it}\beta] \quad (1.7)$$

$$y_i = 1 \text{ if } 0 \leq S_i \leq \mu_1; \text{ Prob}[y_i = 1] = \Phi[\mu_1 - X'_{it}\beta] - \Phi[\mu_0 - X'_{it}\beta] \quad (1.8)$$

$$y_i = 2 \text{ if } \mu_1 \leq S_i \leq \mu_2; \text{ Prob}[y_i = 2] = \Phi[\mu_2 - X'_{it}\beta] - \Phi[\mu_1 - X'_{it}\beta] \quad (1.9)$$

$$\dots \quad (1.10)$$

$$y_i = 7 \text{ if } S_i > \mu_6; \text{ Prob}[y_i = 7] = 1 - \Phi[\mu_1 - X'_{it}\beta] \quad (1.11)$$

where y_i is the number of reading days predicted by the model and S_i is the predicted index from the ordered probit model. The μ_j are unknown parameters or cut-offs that reflect some ordered, underlying probability of a mother to read to her child on a weekly basis.

1.6 Results

Tables 1.6 and 1.7 show results from regression analysis of mother's reading days per week on subjective match quality variables and displays a persistent, positive link between happiness in a relationship and child investment. Though much of the results seem to be driven by individuals who rate their relationships as excellent, it is apparent that individuals who rate their relationship more highly read more, on average, to their children on a weekly basis than those who report dissatisfaction in their relationship. These findings are in accordance with theoretical predictions made in this paper as well as in the match quality literature. In the 1-year follow-up survey, women who describe their relationship with their child's father as "Excellent" spend about 1 day more per week reading with their child and those who describe it as "Poor". Effects for smaller jumps, from Poor to Fair, say, are not generally distinguishable from zero. Tables 1.6 and 1.7 show results for each sample. The relevant sample in each wave consists of the women who have remained in a romantic relationship with the focal child's father up to and including that wave.

These results show the robustness of the results to the inclusion of controls for individual characteristics, socio-economic status, race and prenatal investments. Controlling for whether the child is ever in someone's care besides the mother's, though exerting a significant effect, does not seem to detract from the strength of the results. Characteristics of the mother at the baseline, including her own reports of prenatal care, economic status and prenatal behaviors do not affect the strength of the results though individual coefficients occasionally come in as significant.⁹

Controlling for baseline match quality also does change slightly the magnitude and strength of the results. We expect some endogeneity in match quality and child quality, particularly as the child ages. Baseline match quality, in most cases, comes in as very significant, indicating that there is some inherent level of quality in the relationship that persists from the beginning of the relationship through the child's first few years, affecting household and parental behaviors. Baseline

⁹ These controls are jointly significant for the 1-yr and 3-yr follow-up surveys with F stats of 2.65 and 2.04 for the respective regressions shown in Table 1.6. For the 5-yr follow-up survey, they lose significance, with an F stat of .87 for the regressions shown in Table 1.6.

Table 1.6: Reading Days on Subjective Match Quality using Ordered Probit, Unweighted

	Days per Week that Mother reads with Child					
	(1)	(2)	(3)	(4)	(5)	(6)
	1-Year	1-Year	3-Year	3-Year	5-Year	5-Year
Subjective Match Quality						
Excellent	0.354*	0.288	0.628**	0.638**	0.332	0.331
	(0.210)	(0.214)	(0.300)	(0.301)	(0.323)	(0.324)
Very Good	0.154	0.0895	0.436	0.442	0.00441	4.38e-05
	(0.209)	(0.212)	(0.299)	(0.300)	(0.322)	(0.323)
Good	-0.0485	-0.119	0.287	0.298	-0.294	-0.292
	(0.212)	(0.214)	(0.302)	(0.303)	(0.326)	(0.328)
Fair	-0.129	-0.185	0.173	0.185	-0.121	-0.114
	(0.224)	(0.224)	(0.316)	(0.317)	(0.345)	(0.348)
Married or Chances of Marriage at Birth						
Married at Birth		0.346		0.602**		0.150
		(0.353)		(0.264)		
Certain Chance		0.472***		0.561**		0.242
		(0.181)		(0.244)		(0.239)
Good Chance		0.513***		0.367		0.143
		(0.184)		(0.248)		(0.245)
Fifty Fifty Chance		0.413**		0.348		0.152
		(0.182)		(0.251)		(0.250)
A Little Chance		0.857***		0.462		0.00668
		(0.223)		(0.325)		(0.350)
Male Child	-0.0822*	-0.0800	-0.0140	-0.00959	-0.177***	-0.178***
	(0.0494)	(0.0495)	(0.0615)	(0.0617)	(0.0668)	(0.0670)
Married in wave	0.0424	0.165	-0.0751	-0.160	0.0693	0.0924
	(0.0661)	(0.320)	(0.0792)	(0.117)	(0.0883)	(0.106)
Log of Earnings	0.00852	0.00737	-0.00830	-0.0135	0.0693	0.0924
	(0.0177)	(0.0178)	(0.0267)	(0.0270)	(0.0269)	(0.0270)
Black	0.0365	0.0380	-0.111	-0.0937	-0.169**	-0.168**
	(0.0613)	(0.0616)	(0.0759)	(0.0763)	(0.0816)	(0.0821)
Asian	-0.196	-0.180	-0.301	-0.296	-0.0709	-0.0757
	(0.168)	(0.168)	(0.188)	(0.188)	(0.196)	(0.196)
Native American	0.0135	0.00498	-0.143	-0.149	0.134	0.119
	(0.195)	(0.195)	(0.244)	(0.244)	(0.284)	(0.284)
Other	-0.284***	-0.272***	-0.214**	-0.199*	-0.0879	-0.0874
	(0.0841)	(0.0842)	(0.105)	(0.105)	(0.115)	(0.116)
Hispanic	-1.085	-1.074			-1.023	-0.995
	(0.718)	(0.719)			(1.028)	(1.031)
HS Diploma	0.193	0.155	0.193	0.155	0.612**	0.608**
	(0.199)	(0.200)	(0.244)	(0.245)	(0.295)	(0.296)
Bachelor's Degree	0.408**	0.396**	0.616***	0.589***	0.702***	0.710***
	(0.191)	(0.191)	(0.224)	(0.226)	(0.262)	(0.266)
Grad School	0.558***	0.536**	0.854***	0.824***	0.684**	0.692**
	(0.208)	(0.209)	(0.246)	(0.248)	(0.279)	(0.283)

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	Continued					
	(1)	(2)	(3)	(4)	(5)	(6)
	1-Year	1-Year	3-Year	3-Year	5-Year	5-Year
Mother's Age	-0.00144 (0.00504)	-0.00118 (0.00505)	-0.0102* (0.00602)	-0.0108* (0.00615)	-0.0129** (0.00630)	-0.0115* (0.00652)
Num other kids in HH	-0.0567*** (0.0212)	-0.0545** (0.0213)	-0.0609** (0.0261)	-0.0579** (0.0261)	-0.0328 (0.0288)	-0.0329 (0.0288)
Any prenatal care?	-0.493** (0.238)	-0.511** (0.238)	-0.410 (0.325)	-0.454 (0.327)	-0.176 (0.392)	-0.215 (0.394)
Month of first prenatal	-0.0575*** (0.0197)	-0.0609*** (0.0198)	-0.0614** (0.0251)	-0.0632** (0.0253)	-0.0610** (0.0296)	-0.0635** (0.0297)
Child ever in other care?	-0.161*** (0.0514)	-0.168*** (0.0515)			-0.110 (0.0922)	-0.102 (0.0926)
Amt Public Assistance	-0.00231 (0.0197)	-0.000431 (0.0197)			-0.00735 (0.0303)	-0.00802 (0.0305)
Amt Unemployment	-0.0141 (0.0194)	-0.0129 (0.0194)			-0.0143 (0.0263)	-0.0138 (0.0263)
In Public Housing?	-0.124 (0.0922)	-0.135 (0.0924)			-0.206 (0.130)	-0.196 (0.131)
Mother US-Born	0.257*** (0.0855)	0.267*** (0.0858)			0.119 (0.112)	0.114 (0.112)
Disabled Child	-0.0750 (0.181)	-0.0806 (0.181)			0.216 (0.336)	0.221 (0.337)
Ever Breastfed?	0.00802 (0.0546)	0.00131 (0.0547)			-0.120 (0.154)	-0.114 (0.155)
Prenatal Substance Use						
Alcohol Use	0.0257 (0.189)	0.0256 (0.189)	-0.0675 (0.239)	-0.0946 (0.240)	0.365 (0.261)	0.352 (0.261)
Drug Use	-0.274* (0.150)	-0.297** (0.150)	-0.366* (0.189)	-0.352* (0.190)	0.188 (0.215)	0.195 (0.215)
Cigarette Use	0.0326 (0.0724)	0.0373 (0.0726)	0.165* (0.0962)	0.172* (0.0967)	-0.185* (0.104)	-0.190* (0.105)
Observations	1,902	1,902	1,382	1,382	1,087	1,087

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Reading Days on Subjective Match Quality using Ordered Probit, Weighted

	Days per Week that Mother reads with Child					
	(1)	(2)	(3)	(4)	(5)	(6)
	1-Year	1-Year	3-Year	3-Year	5-Year	5-Year
Subjective Match Quality						
Excellent	0.0779 (0.366)	-0.0184 (0.386)	0.828* (0.465)	0.824* (0.467)	0.332 (0.323)	0.331 (0.324)
Very Good	-0.302 (0.362)	-0.399 (0.378)	0.494 (0.488)	0.479 (0.490)	0.00441 (0.322)	4.38e-05 (0.323)
Good	-0.289 (0.364)	-0.403 (0.383)	0.647 (0.479)	0.636 (0.481)	-0.294 (0.326)	-0.292 (0.328)
Fair	-0.382 (0.551)	-0.476 (0.567)	0.504 (0.536)	0.519 (0.537)	-0.121 (0.345)	-0.114 (0.348)
Married or Chances of Marriage at Birth						
Married at Birth		0.582 (0.713)		0.969** (0.426)		0.150 (0.250)
Certain Chance		0.627 (0.427)		1.149*** (0.339)		0.242 (0.239)
Good Chance		0.700 (0.426)		1.154** (0.360)		0.143 (0.245)
Fifty Fifty Chance		0.522 (0.415)		0.825** (0.367)		0.152 (0.250)
A Little Chance		1.289*** (0.434)		1.306*** (0.426)		0.00668 (0.350)
Male Child	-0.178 (0.120)	-0.168 (0.118)	-0.0113 (0.137)	-0.00852 (0.139)	-0.177*** (0.0668)	-0.178*** (0.0670)
Married in wave	0.0301 (0.151)	0.0635 (0.604)	0.201 (0.198)	0.342 (0.302)	0.0693 (0.0883)	0.0924 (0.106)
Log of Earnings	0.0478 (0.0306)	0.0431 (0.0287)	-0.00810 (0.0465)	-0.0217 (0.0467)	-0.0253 (0.0816)	-0.0237 (0.0821)
Black	-0.147 (0.148)	-0.155 (0.148)	-0.00810 (0.190)	-0.0217 (0.192)	-0.169** (0.0816)	-0.168** (0.0821)
Asian	0.166 (0.288)	0.186 (0.285)	0.0682 (0.400)	0.0718 (0.402)	-0.0709 (0.196)	-0.0757 (0.196)
Native American	-0.558** (0.269)	-0.557** (0.272)	0.383 (0.472)	0.352 (0.472)	0.134 (0.284)	0.119 (0.284)
Other	-0.344* (0.175)	-0.340* (0.176)	-0.238 (0.267)	-0.253 (0.271)	-0.0879 (0.115)	-0.0874 (0.116)
Hispanic	-1.128*** (0.237)	-0.987*** (0.282)			-1.023 (1.028)	-0.995 (1.031)
Mother's Age	0.00355 (0.0126)	0.00283 (0.0127)	-0.0252* (0.0143)	-0.0253* (0.0145)	-0.0129** (0.00630)	-0.0115* (0.00652)
Num other kids in HH	-0.140*** (0.0472)	-0.136*** (0.0472)	-0.155** (0.0606)	-0.151** (0.0605)	-0.0328 (0.0288)	-0.0329 (0.0288)

Continued on next page

	Continued					
	(1)	(2)	(3)	(4)	(5)	(6)
	1-Year	1-Year	3-Year	3-Year	5-Year	5-Year
Any prenatal care?	-1.303** (0.592)	-1.247** (0.588)	-1.186* (0.668)	-1.174* (0.684)	-0.176 (0.392)	-0.215 (0.394)
Month of first prenatal	-0.0405 (0.0588)	-0.0405 (0.0573)	-0.144** (0.0592)	-0.151** (0.0612)	-0.0610** (0.0296)	-0.0635** (0.0297)
Child ever in other care?	-0.312** (0.122)	-0.334*** (0.123)	0.154 (0.0592)	0.151 (0.0612)	-0.110 (0.0922)	-0.102 (0.0926)
Amt Public Assistance	-0.0680 (0.0443)	-0.0646 (0.0451)	0.0189 (0.0536)	0.0248 (0.0545)	-0.00735 (0.0303)	-0.00802 (0.0305)
Amt Unemployment	0.0214 (0.0300)	0.0237 (0.0302)	0.0561 (0.0648)	0.0644 (0.0655)	-0.0143 (0.0263)	-0.0138 (0.0263)
In Public Housing?	0.0240 (0.168)	0.0239 (0.170)	-0.298 (0.243)	-0.330 (0.241)	-0.206 (0.130)	-0.196 (0.131)
Mother US-Born	0.533*** (0.180)	0.545*** (0.181)	0.103 (0.259)	0.0970 (0.262)	0.119 (0.112)	0.114 (0.112)
Disabled Child	0.222 (0.291)	0.279 (0.250)	1.143* (0.626)	1.176* (0.631)	0.216 (0.336)	0.221 (0.337)
Ever Breastfed?	-0.128 (0.134)	-0.120 (0.134)	-0.120 (0.154)	-0.112 (0.155)	0.365 (0.261)	0.352 (0.261)
Substance Use During Pregnancy						
Alcohol Use.	0.0743 (0.296)	0.0349 (0.314)	0.622* (0.333)	0.593* (0.334)	0.365 (0.261)	0.352 (0.261)
Drug Use	-0.656* (0.385)	-0.699* (0.388)	-0.876** (0.344)	-0.952*** (0.344)	0.188 (0.104)	0.195 (0.105)
Cigarette Use	-0.0241 (0.141)	-0.0206 (0.142)	0.315* (0.182)	0.316* (0.183)	-0.185* (0.104)	-0.190* (0.105)
Baseline Controls	No	Yes	No	Yes	No	Yes
Education	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,399	1,399	1,029	1,029	1,087	1,087

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

match quality was determined by the couple's marital status and, if they were unmarried at the time of the first interview, the mother's expressed chances for marriage.

Interestingly, the strength of the results seems to fade in subsequent waves. By the 5-year follow-up survey, there is no statistically significant difference in reading days per week by mother's report of subjective match quality. In fact, virtually all variables of interest lose their significance, with much of the results being driven by differences in race and education. As relationships end and the pool of women in relationships with the father dwindles, we may see a convergence on similar investment behaviors and similar subjective match quality. Because in earlier waves we see that conflict drives investment, it follows that relationships that persist match better on investment behavior as well as other characteristics.

Alternatively, by the 5-year follow-up, many of the children in question are attending some sort of educational program. Many have entered kindergarten and others are in preschool or Head Start programs. Any of these might provide instruction to parents to read with the children during the week or give the children homework. This reduces variation in the reading days variable as more and more children enter school. Figure 1.3 supports this interpretation, as mothers of children in the 5-year follow-up survey report reading five days per week on average at every level of relationship quality.

1.6.1 Marginal Effects

The ordered probit effects that mirror Table 1.6 are in Table 1.8. In order to calculate the marginal effects, I create two representative mothers from my sample, one married and one unmarried.¹⁰ The representative respondents are defined by the average values for a married and unmarried respondent and the most probable of those values when the value is categorical or dichotomous. Table 1.8 holds marginal effects. It shows, for each of these average women, how changing her answer to the relationship question changes the index, S_i and how that subsequently

¹⁰ Given the heterogeneity of the sample, the number of dichotomous variables, and the large oversampling of non-marital births, it did not seem useful to calculate the marginal effects at the average.

changes her answer to the number of days spent reading to their child per week (y_i). The predicted values reported in Table 1.8 reflect the coefficients calculated for the regression specifications labeled (2) and (4) in Table 1.6.

1.6.2 Results on Argument Frequency

In this section, I directly test one of the possible theoretical mechanisms for how perception of match quality relates to investments in children's cognitive abilities. In particular, I test whether there is a trade-off between arguing or bargaining and time spent with children. In the baseline survey, a series of questions are posed about how often the mother argues with the child's father about a number of different subjects including drugs, money and the pregnancy. In the two subsequent waves, respondents are only asked about general argument frequency. The question does not appear in the 5-year follow-up survey, so only two surveys are examined here.

In order to have a baseline measure that reflects overall argument frequency, I calculate and retain two principle components from the various measures of argument frequency and supplant the many measures in the regressions. In general, this measure does not predict argument frequencies reported in subsequent follow-up surveys, but is included as a control of prenatal or baseline argument frequency.

In accordance with a theory of time constraints and opportunity costs, I would expect that mothers who report arguing about things that are important with their spouse "Often" or "Sometimes" would spend less time reading with their children than mothers who report arguing "Never" ("Always" is the excluded category). The results, however, are inconclusive, varying greatly in magnitude when included in these specifications. The signs are as expected for some answers, but most responses are not significant. In the 3-year sample, the variable "Argue Never" is dropped for collinearity. This may mean that argument frequency and subjective match quality are measuring different unobservable characteristics that are correlated. In any case, I cannot use this test to show that mothers who argue more with their child's father spend more or less time reading with their child. Match quality variables, however, retain some of the significance displayed in Table 1.9.

Table 1.8: Marginal Effects from Ordered Probit

Effect on S_i and y_i of Varying Subjective Match Quality				
Days per Week that Mother reads with Child				
	Married	Unmarried	Married	Unmarried
	1 yr Sample		3 yr Sample	
	(2)		(4)	
Excellent				
predicted S	0.18	0.11	0.15	-0.38
predicted y	5 days	5 days	7 days	5 days
Very Good				
predicted S	-0.017	-0.09	-0.04	-0.57
predicted y	5 Days	5 days	7 Days	5 days
Good				
predicted S	-0.23	-0.30	-0.19	-0.72
predicted y	4 days	3 days	6 days	4 days
Fair				
predicted S	-0.31	-0.38	-0.30	-0.83
predicted y	3 days	3 days	5 days	4 days
Poor				
predicted S	-0.12	-0.19	-0.49	-1.01
predicted y	4 days	4 days	5 days	3 days

Table 1.9: Reading Days on Argument Frequency using Ordered Probit, Weighted

	Days per Week that Mother reads with Child					
	(a)	(1)	(2)	(b)	(3)	(4)
	1-Year	1-Year	1-Year	3-Year	3-Year	3-Year
Argument Frequency						
Argue Never	0.118 (0.482)	-0.428 (0.394)	-0.366 (0.399)			
Argue Rarely	-0.0919 (0.438)	-0.726** (0.349)	-0.689** (0.350)	-0.574 (0.501)	-0.967** (0.442)	-0.972** (0.433)
Argue Sometimes	-0.171 (0.437)	-0.652* (0.341)	-0.610* (0.346)	-0.752 (0.503)	-1.112** (0.438)	-1.124*** (0.428)
Argue Often	-0.201 (0.457)	-0.612* (0.357)	-0.604* (0.361)	-0.629 (0.524)	-1.042** (0.466)	-1.037** (0.455)
Subjective Match Quality						
Excellent					0.915** (0.369)	0.959** (0.373)
Very Good		-0.271 (0.382)	-0.395 (0.400)			
Good		-0.290 (0.385)	-0.425 (0.407)		0.713* (0.388)	0.748* (0.393)
Fair		-0.378 (0.547)	-0.489 (0.561)		0.657 (0.467)	0.715 (0.474)
Married or Chances of Marriage at Birth						
Married at Birth			0.613 (0.724)			0.940** (0.423)
Certain Chance			0.633 (0.441)			1.066*** (0.335)
Good Chance			0.674 (0.443)			1.094*** (0.346)
Fifty Fifty Chance			0.498 (0.429)			0.688* (0.366)
A Little Chance			1.281*** (0.444)			1.264*** (0.423)
Controls for Baseline	No	Yes	Yes	No	Yes	Yes
Observations	1,227	1,227	1,227	1,029	1,029	1,029

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

1.6.3 Results on Future Status

I use the variables related to future status of the relationship which indicate whether the intimate relationship ends in a future wave either by separation, divorce or break up. I exploit the panel nature of the data to perform analysis on two waves, the second and third, using these variables, the results of which are displayed in Table 1.10. As theorized above and by Brown and Flinn (2007), the future status of the parents' relationship does exert a measurable effect on mother's reading days with the child. The effect, unlike shown in Schmierer (2010), is significant for mothers' investments, and is associated with a large deviation from the mean. Mothers who divorce in a future wave spend on average about one day per week fewer reading with their child.

Interestingly, this effect is jointly significant with the coefficients on subjective match quality. When controlling for future relationship status, the coefficients on match quality are close in magnitude to the baseline estimates. Future relationship status, then, does not fully account for differences in investments in children. The match quality coefficient, in this case, could be proxying for couples who divorce even later than observed in the data set, allowing for the all coefficients to be significant. This result is extremely important as it shows that we can account for much of the difference in investments in children by posing questions about subjective match quality.

There may be some confounding effects here because the sample consists of both couples that are married and unmarried. It is likely that the costs of ending a marriage are different than the costs of ending a relationship that may or may not have legal ties, which may, in turn, affect investments. Thus, I also allow for marital status to be interacted with the future relationship status. This coefficient is small in magnitude and not statistically significant for married couples that divorce before the 3-year follow-up survey. There is a differential effect, however, for married respondents whose relationships end by the 5-year follow-up survey. These results, when combined, show a positive overall effect on reading days for married parents whose relationship ends. This suggests there may be something different about married respondents who eventually divorce and speaks to the effect in the opposite direction I predicted for high-quality parents. Though I cannot

Table 1.10: Reading Days on Future Relationship Status using Ordered Probit, Weighted

	Days per Week that Mother reads with Child					
	(a)	(1)	(2)	(b)	(3)	(4)
	1-Year	1-Year	1-Year	3-Year	3-Year	3-Year
Relationship End 3-yr	-0.0583 (0.199)	0.208 (0.169)	0.235 (0.170)			
Relationship End 5-yr	-0.273 (0.572)	-0.909*** (0.342)	-0.996** (0.507)	0.0102 (0.361)	-1.036** (0.517)	-1.173** (0.548)
Married x Rel Ends at 3-yr		-0.531 (0.376)	-0.580 (0.384)			
Married x Rel Ends at 5-yr		2.903*** (0.570)	2.990*** (0.687)		1.547** (0.615)	1.631** (0.637)
Subjective Match Quality						
Excellent		0.438 (0.408)	0.423 (0.443)		0.327 (0.300)	0.300 (0.293)
Very Good		0.0746 (0.381)	0.0609 (0.415)		-0.00901 (0.302)	-0.0431 (0.294)
Good		0.114 (0.401)	0.0923 (0.418)		0.136 (0.307)	0.105 (0.305)
Poor		0.535 (0.547)	0.634 (0.557)		-0.422 (0.529)	-0.486 (0.535)
Male Child	-0.191 (0.121)	-0.170 (0.119)	-0.156 (0.117)	0.0166 (0.138)	-0.00799 (0.138)	-0.00677 (0.139)
Married (in wave)	0.0760 (0.150)	0.0944 (0.164)	0.0203 (0.645)	0.265 (0.179)	0.293 (0.181)	0.353 (0.305)
Log of Earnings	0.0369 (0.0339)	0.0553* (0.0321)	0.0504* (0.0297)			
Married or Chances of Marriage at Birth						
Married at Birth			0.770 (0.743)			0.924** (0.423)
Certain Chance			0.708* (0.427)			1.118*** (0.337)
Good Chance			0.747* (0.423)			1.125*** (0.359)
Fifty/fifty Chance			0.568 (0.411)			0.737** (0.346)
A Little Chance			1.357*** (0.426)			1.279*** (0.422)
Observations	1,399	1,399	1,399	1,031	1,031	1,031

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

attribute it to only high-quality parents, there is some evidence of an effect in the other direction.

1.6.4 Weighted Results

Perhaps the most problematic part of the results discussed above is external validity. The Fragile Families dataset is unique in that it brings significant attention to a previously understudied group. However, this increased attention means that the sample is not random and not representative of the population as a whole, leading to complications when searching for causal effects and policy implications. I present both sets of results because the distinctions call attention to the idea of heterogeneous effects for different groups.

Fragile Families offers a number of different weights based on its varied sampling strategy. I use the national weight in the following specifications; each observation is weighted such that its appearance in the data set represents its occurrence in the nationally representative sample. This greatly reduces the weight on observations where the mother remains unmarried. I repeated each of the specifications as discussed above using the national weights.

Inclusion of the weights decreases the importance of subjective match quality and interestingly, often increased the significance of what I call more objective measures of match quality. While the quality of the match appears to be important for reading days regardless of the marital status of the average group member, the type of measure that most appropriately predicts reading days is different for different groups.

In the weighted regressions, argument frequency and future status increase in significance and magnitude in terms of their effect on reading days when compared to the unweighted sample. The standard errors on match quality increase, though the magnitudes do not change significantly.

The reasons for this switch are unclear. Perhaps married mothers are more likely to revise upward their estimation of their marital happiness, so we do not observe their true estimation of match quality, but an upwardly biased one. If married women are more likely to lie about their happiness then we would see no effect on reading days.

This story is consistent with a marked difference in significance on future relationship status

and subjective match quality, but does not explain why the results on argument frequency are stronger. It seems unlikely that a mother who would change her answer about match quality would not also change her answer about argument frequency.

1.6.5 Pooling and Fixed Effects Results

Despite the emphasis that the survey puts on unmarried mothers, it is interesting to find that there is a large number of women that reports some sort of intimate relationship with the father of the focal child for all four surveys. This sample allows for pooling and fixed effects specifications; the fixed effects specifications are reported in Table 1.11. In the pooled specifications, the coefficient estimates, are, as expected, similar to the cross-sectional estimates and highly significant.

When individual fixed effects are added, the coefficient estimates are still within the range of estimates from cross-sectional results, but lose significance. Though there is significant cross-sectional variation in number of reading days and relationship quality, these measures are fairly stable at the individual level. A woman who rates her relationship as excellent and reads to her child six days a week in the first follow-up is likely to report an excellent relationship and six reading days in the third follow-up. The resulting lack of variation over time makes a traditional fixed effect strategy not very informative. The strong association of the baseline match quality variable and subsequent match quality variables also reflects the lack of variation over time in the measure.

1.7 Robustness Checks and Extensions

1.7.1 Investments in Cognitive Skills versus Time Spent with Children

Despite the strong results on days spent reading with a child, it is still unclear whether there is something particular about investments in cognitive skills that makes them especially affected by match quality or if parents who are happier with their partners are more likely to report higher averages of any time activity their child does.

In order to test this, I run similar regressions to those on reading days per week, but using

Table 1.11: Reading Days on Subjective Match Quality using Fixed Effects

Days per Week that Mother reads with Child				
Subjective Match Quality				
Excellent	0.153 (0.371)	0.157 (0.372)	0.159 (0.372)	0.218 (0.394)
Very Good	0.121 (0.367)	0.124 (0.367)	0.125 (0.367)	0.156 (0.389)
Good	-0.0297 (0.369)	-0.0242 (0.369)	-0.0223 (0.369)	-0.0343 (0.392)
Fair	0.000440 (0.373)	-0.00476 (0.374)	-0.00131 (0.374)	0.0648 (0.393)
Married	0.306** (0.140)	0.305** (0.141)	0.316** (0.142)	0.0910 (0.150)
Mother's Age				0.135*** (0.0204)
Log of Earnings				0.00251 (0.0371)
Num of Kids		0.00438 (0.0524)	0.00746 (0.0528)	-0.0919 (0.0573)
Child Ever in Other Care?			-0.0624 (0.127)	0.227* (0.134)
Hours Child in other care			0.00327 (0.00386)	0.00247 (0.00394)
Constant	4.636*** (0.372)	4.624*** (0.394)	4.597*** (0.397)	0.777 (0.782)
Observations	4,106	4,102	4,102	3,807
R-squared	0.003	0.003	0.003	0.021
Number of newid	1,374	1,374	1,374	1,361

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

alternate variables of interest of days per week that a child watches television and days per week that mothers play inside with their children. While these activities are time investments, they are not necessarily investments in children's cognitive abilities, as reading is. In this case, the specifications are identical to Equations 6.1 and 6.2, except that the left-hand side variable measured is days per week that the child watches television or days per week that the child spends playing inside. Television days are only available for the five-year follow-up survey and days playing inside is available for each of the samples.

Television viewing time is an area where we might expect to see the opposite effect, that couples unhappier in their relationships are more likely to set their child in front of the TV while they attend to other matters. Television viewing is limited to the final wave of the analysis and thus I only have results for the 5-year Sample, but match quality is insignificant in all specifications. It seems that better or worse match quality does not induce or relate to increased or decreased average TV watching. There is no statistically significant coefficient on the individual dummy variables for match quality.

In the case of days spent playing inside, there is some statistical significance in the most parsimonious of regressions. When we add controls for baseline maternal investments and socioeconomic status, the results become insignificant for measures of subjective match quality. These results on time spent playing inside and television viewing indicate that subjective match quality is associated with changes in investment-heavy activities, but not necessarily all time spent with children. In addition, there does not appear to be an 'opposite' to investments in cognitive skills, at least as measured by television viewing time.

1.7.2 Direction of causation between parenting and relationship

While the results on match quality are significant and strongly correlated with the baseline match quality, I still have not effectively ruled out the possibility that people who are inherently bad parents are necessarily those who get into bad relationships. Finally, in order to address this question, I add various measures of estimation of the parents' personalities by the mother. Though

these are not necessarily indicative of parenting quality, I control for whether a mother sees her child's father as a good or bad person. This is an attempt to control for omitted variable bias arising from the possibility that bad people or bad parents get into bad relationships. Interestingly, the addition of controls for mother's estimation of the father's character is not associated with any effect on the number of reading days in the same way that estimation of the relationship quality does. Mothers who report that their partner is "often" or "sometimes" "fair and willing to compromise" do not report significantly different reading frequencies than those who report that their partner is "never" "fair and willing to compromise".

This could be interpreted as the inability to reject the hypothesis that bad parents are necessarily those who get into bad relationships. Mothers seem to invest less in their children if they see the relationship as ending or if they are unhappy in the relationship, but not necessarily if they see their partner as a bad person.

1.8 Conclusions

A mother's subjective assessment of the quality of her relationship with a child's father is an important predictor of how much time she will invest in her children's cognitive abilities. These results are robust to the addition of controls for several individual characteristics that account for parental quality, cultural norms and socio-economic status. Mothers who report an excellent relationship with the father of their child spend up to 1.2 days more per week reading with the child than a mother who reports a poor relationship with the father. Importantly, these results show that relationship discord can have an indirect impact on children through how it affects their parents' decisions to invest in their children. That these effects might be in place before or even in the absence of divorce or union dissolution reflects a unique and important contribution to the literature.

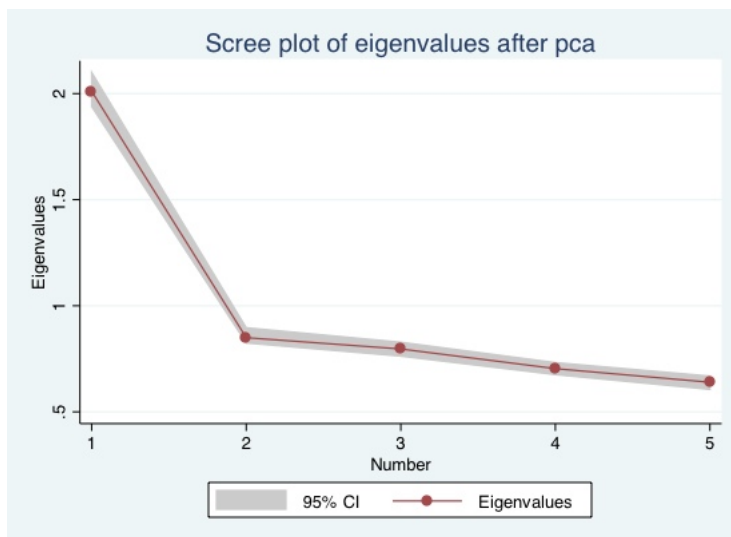
In addition, I test some of the theoretical mechanisms by which we hypothesize that match quality could affect parental investment decisions. In particular, I find that argument frequency does not have a significant direct impact. Though we might expect relationship discord to have a

directly measurable opportunity cost, we cannot measure it with reported argument frequency. The correlation of argument frequency and subjective match quality likely affects these specifications.

The inclusion of future relationship status in part of the analysis shows that while an upcoming divorce does significantly affect time spent reading with children, it is over and above the effect of a mother's estimation of the match quality. This result is important as researchers can use subjective match quality as a proxy for future relationship status in assessing investments in children, but should not necessarily use it to predict future relationships status. The union dissolution decision, while important, does not fully encompass the range of satisfaction or happiness in an intimate relationship and those gradations can exert an important effect on parents' behaviors, investments in children or otherwise, within the relationship. Likely, this relationship could be explored more to examine the link between match quality and other behaviors such as health or education monetary outlays as related to children or investments in oneself.

Further research is needed to examine a link, if any exists, between match quality and children's cognitive abilities as well as a link between match quality and time spent on other activities and monetary investments. The difference between effects on time investments in cognitive ability and time spent on other activities is also an area for further examination.

Figure 1.4: The scree plot of the eigenvalues for the principle components analysis is intended to visually assist in selecting how many components to retain for the analysis. I retained the first two components as the marginal value of additional components drops significantly after the second component as seen in the leveling of the screeplot after the second point.



Chapter 2

Expectations of Support: Health Investments and Promises of Financial Assistance for Children

2.1 Introduction

An evolving notion of parenthood in the United States has called attention not only to the diversity of types of families, but also variation within types, particularly as regards the quality of romantic relationships and notions of financial and emotional responsibility for raising children. This heterogeneity within unmarried parents is largely understudied. In addition, the set of parents that are unmarried is expanding (Pew 2010). The makeup of families is evolving and the traditional role of fathers as providers for life has been circumscribed by working mothers, single mothers and various legal or informal, financial and emotional arrangements to cover the costs of raising children (Pew 2010). As such, the relationship between investments in children and arrangements that include children, but not necessarily marriage, merits further investigation.

In terms of financial arrangements, theory tells us that decisions about household expenditure are made with an eye on current income and expectations of future income (Hall 1978). In addition, income from child support has been shown to improve outcomes for children more than income from other sources, so the promise or expectation of that income might be particularly powerful in affecting expenditures (Knox 1996). If the promise of support is powerful and credible, we would expect to see an adjustment in household expenditure and most critically, adjustments in investments in children.

Here, I specifically examine the relationship between investments in children's health and expectations of financial support using a subsample of the Fragile Families and Child Wellbeing data of parents who are unmarried at the birth of the child. For outcomes, I focus on two health investments deemed critical by the American Academy of Pediatrics. The first is on-time doctor's visits; I use the elapsed time since the last doctor's visit to create a dichotomous variable indicating whether the child was late to their last recommended checkup for a sample of 12-18 month olds. Secondly, I examine the number of months a child was breastfed as reported by the mother. The key right-hand-side variable of interest is whether a promise of financial assistance was given to the mother by the father at or around the birth of the child.

I first use a basic OLS strategy, testing for associations of the promise of support with recent checkups or extended breastfeeding. In order to address the heterogeneity of the sample, I include key controls such as prenatal care, characteristics of the father, and other measures of child support. To compare to the OLS results, I also analyze the sample using propensity score matching to test whether the relationship is robust to comparisons within more like groups. In addition, I test whether these verbal agreements to financially support a child affect health investments differently than the actualization of these promises through child support, either formal or informal. In order to do this, I add controls for whether a financial support agreement is in effect for the follow-up wave, and whether those payments are being made on time. For all specifications, I separate by race to test for whether there are different effects for different groups.

This work is related to several strands of literature: one that seeks to explain parents' involvement in their children and one that links early childhood health care to eventual outcomes, but also is associated more generally to research around expectations of future income and how childrearing differs by race. There is a broad ethnographic and sociology literature that aims to differentiate among socioeconomic status and race according to attitudes about marriage, childbearing and investments in children (e.g. Edin & Kefalas 2005; Smock, Manning & Porter, 2005, Osborne & McLanahan 2007). In addition, these results confirm findings in the medical literature showing that Black parents are more likely to follow "alternative vaccination schedules", or take their children late to routine appointments, if at all (Dempsey, et.al. 2011). The question of a promise of financial support also hints at questions of partnership and financial stability and how that affects children (e.g. Craigie 2008, Ryan, et.al. 2008; Smock, et.al. 2005). To my knowledge, this is the first paper that associates expectations of child support with investments in children. I also add to the literature by examining the use of attitudinal questions in surveys.

While OLS regressions on the whole sample show no effect on whether the child is late for his last scheduled doctor's visit there is an effect when the regression is run only for Black women. Black women who received the promise of support were less likely than those who didn't receive the promise to miss their last scheduled doctor's checkup by at least a month, but little to no effect

for White women. Interestingly, we also see an effect on investments like breastfeeding that are perhaps seen as less cost-intensive. With a full set of controls, White women who received the promise breastfed their children on average 2.4 months longer than White women who did not. These differing effects indicate that there may be significant differences in the way the question of support is understood by mothers in different communities. In addition, it may be that the promise of support has entirely different meanings to different types of fathers (Edin & Kefalas, 2005; Osborne & McLanahan, 2007; Gibson-Davis, et.al 2005).

An alternative strategy involving propensity score matching fails to identify the effect as strongly, though differences by race and by type of investment persist. The magnitude of decreased probability of being late is much smaller, on the order of 5 percentage points, and significance is only achieved with the exclusion of important controls. For breastfeeding, the effect is persistent for White mothers, though of a smaller magnitude—about half of the OLS effect—and with certain matching methods, a similar effect appears for Black mothers where there was none with an OLS strategy.

The rest of this paper is organized as follows. In Sections 2, I discuss the data. Section 3 presents greater detail on the sample and key variables, descriptive statistics, and methods. In Section 4, I present OLS and propensity score matching results. Section 5 concludes.

2.2 Data

Comparisons within the set of unmarried women have been previously difficult due to small sample sizes. Until recently, surveys with a representative sampling design surveyed few never-married women. A recent increase in non-marital births in the United States combined with a heightened interest in outcomes associated with non-traditional families has led to increased presence in surveys. In particular, the Fragile Families and Child Well-being (FF) data set is well-suited to analysis of non-traditional families due to its significant oversample of non-marital births. Fragile Families is a longitudinal study with about 4000 initial respondents who gave birth in 1998, 1999 or 2000 and three follow-up waves, taking place around the focal child's first, third and fifth

birthdays.¹ I use information provided by the mothers in the baseline and one-year surveys about relationship quality and investments in the focal child.

2.3 Methods

2.3.1 Sample

The relevant sample consists of mothers who are the primary caretakers of the focal child as of the first follow-up survey. Fathers as primary caretakers are not included and mothers must have knowledge about the identity of the father of the child and be able to answer questions about his personality and commitment to raising the child. I use several characteristics as reported in the baseline survey, including socio-economic characteristics, mother's estimation of father's character and commitment to raising children, and father's race and education.

As I am interested in the promise of support, I concentrate on mothers unmarried at the birth of their child who answered the question of whether a verbal promise of support was made by the father by the time of the first survey. Further, I restrict the sample to women who are primary caretakers for the focal child and mothers who were interviewed for the one-year followup when their child was aged 12 to 18 months.

I truncate the data in order to concentrate on an age when children are due for several crucial doctors visits to receive vaccinations and ensure child well-being. A small number of respondents were interviewed before the child's one-year birthday. I do not consider these respondents. Due to the fact that twelve months is the minimum recommended age for administration of certain vaccines, I exclude children whose mothers were interviewed before their first birthday. After the child turns 18 months old, the frequency of recommended well-child visits drops according to the AAP. On the upper end, I restrict the sample to children 18 months or younger in order to avoid cases of older children who have a longer scheduled lag between doctor's visits. Table 2.1 shows the distribution of all children in the Fragile Families Data aged 12 months and up at the one-year follow-up survey. Most children are in the 12 to 18 months range, so the analysis is relevant for a

¹ The 9-year follow-up survey is expected to be released in 2012.

significant portion of respondents.

Another important aspect of the sample is that I split the sample by race, concentrating on the two largest racial groups represented in the data, Black women and White women. The differing marriage rates between races leaves likely very different samples of women unwed, the women who are considered here. Though many of their observable characteristics are the same, fewer Black women (12.9% of Black women in the whole sample are married than White women (49.2%). This is representative of statistics nationwide. In the 2000 Census, about 36.3% of White respondents lived in a household with a spouse, while only 26% of Black respondents did. There is also evidence that marriage and other forms of romantic commitment are viewed differently within different racial communities (Edin & Kefalas 2006). Thus I split the sample to examine whether financial commitments are associated with significant differences in investments in children by race. The data bear out this prediction, exhibiting different effects for various types of investments.

2.3.1.1 Breastfeeding Sample

The subsample of mothers who ever breastfed was much smaller than the sample of women for whose children there was complete health information. Less than half of the women in the sample as described above report having ever breastfed the child. The breastfeeding specifications were performed using the subsample who reported having ever breastfed,

2.3.2 Key Variables

In terms of outcome variables, I choose to concentrate on breastfeeding and doctor's visits as these two interventions are high priorities of the American Academy of Pediatrics and have very different cost structures. I include a number of key controls to account for unobserved heterogeneity in the sample. I use age of child fixed effects instead of controlling for age linearly to account for differences by age.

Table 2.1: Baby's Age in Months at time of Mother's 1-year Interview

	Freq.	Percent	Cum.
12	842	22.03	22.03
13	647	16.93	38.96
14	422	11.04	50.00
15	159	4.16	54.16
16	362	9.47	63.63
17	454	11.88	75.51
18	321	8.40	83.91
19	219	5.73	89.64
20	135	3.53	93.17
21	78	2.04	95.21
22	47	1.23	96.44
23	32	0.84	97.28
24	37	0.97	98.25
25	17	0.44	98.69
26	9	0.24	98.93
27	4	0.10	99.03
28	13	0.34	99.37
29	16	0.42	99.79
30	8	0.21	100.00
Total	3,822		100.00

2.3.2.1 Outcome—Late to doctor’s visits

Doctor’s visits are summarily important in a child’s health and development. The AAP dictates vaccinations and regular checkups at 9, 12, 15 and 18 months of age. Between twelve months and fifteen months, a child is due for 10 vaccines.² The data do not provide a comprehensive timeline of doctor’s visits, but rather ask the mother when was the last time the child saw a doctor for a regular checkup. So, while I cannot ask whether a child was late to a particular visit—say, the nine-month visit—I can ask whether the child has been to the doctor within the last three months. When children are not brought in to the doctor within the recommended window of time, pediatricians become concerned, and so I create a variable that indicates whether the child has been to the doctor in the last three months as a proxy for whether the child was late to his last regularly scheduled checkup.

Given the AAP guidelines, for children between 12 and 18 months of age, all other things equal, the last doctor’s visit should have been within the last three months. I use the date of the last doctor’s visit and the interview date to construct a variable representing the months elapsed since the last regular doctor’s visit. If more than three months have elapsed between a child’s last doctor’s visit and the interview, a dummy variable called “late” takes a value of one and zero otherwise. As these are critical months to be getting vaccinations, it is worrisome to have more than three months elapse between visits. I assume that if a longer span of time has elapsed since the last doctor’s visit, it signifies a lower preference for investment in the child or a lower ability to pay. I cannot distinguish between the explanations, but do examine whether there is a link between these decisions and an earlier promise of financial support.

Doctor’s visits are costly in monetary terms, particularly in the absence of health insurance or assistance, but less costly in terms of time. Though a doctor’s visit might require a mother to take off work or school, they require much less time than say, breastfeeding. For this reason, we might consider doctor’s visits less of a time investment and more of a financial investment.

² <http://aapredbook.aappublications.org/resources/IZSchedule0-6yrs.pdf>

2.3.2.2 Outcome—Breastfeeding

Breastfeeding is associated with “health, nutritional, immunologic, developmental, psychologic, social, economic and environmental benefits” (AAP 2005). As of 2005, the American Academy of Pediatrics recommends exclusive breastfeeding for at least the first six months. As an outcome variable, I use the number of months that a mother reported breastfeeding the focal child.

I concentrate on breastfeeding here because it seems that breastfeeding would differ from doctor’s visits in terms of the kinds of costs associated with it. The costs associated with breastfeeding are primarily time-oriented; a mother must find a time and place to feed her child or pump at regular intervals. But there are not significant financial costs associated with breastfeeding as there are with doctor’s visits.

2.3.2.3 Key Controls

In order to account for the non-random selection of participants into the promise group, I rely on “selection on observables” (Heckman & Robb, 1985) by controlling for pre-treatment characteristics where available. I include measures of prenatal preferences for investing in children by accounting for prenatal health investments. These include when prenatal care was first received and whether the mother used drugs, alcohol or cigarettes while pregnant.

I also test whether conditioning on additional information has any effect. I include information about the father’s characteristics at the birth of the child to proxy for the mother’s belief that the father will follow through with his promise of support. I also check whether current child support agreements and on time payments have any effect.

2.3.2.4 Father characteristics

The rich data associated with Fragile Families gives a large number of father characteristics, as reported by the mother, with which we might associate credibility, reliability or other desirable traits in a caregiver. I include controls for father’s socioeconomic characteristics where available, as reported by the mother, but also add variables that may proxy for the above traits that may be

affect how much the mother believes in the promise. Father's education and race are very highly correlated with the mother's education and race, respectively.

The analysis also controls for whether the father was in jail at the time of birth of the child, whether the father was working around the time of the first interview and whether the mother believes the father has difficulty holding a job due to drugs. The inclusion of father characteristics is a unique contribution afforded by the data. While we might expect that a woman's propensity to consume is based on her expectations of future income, the credibility of that income source is also paramount. A promise of financial support from a partner or former partner who is seen as stable, loving and dependable should have different effects than a promise of support from a man seen as unstable and violent.

2.3.2.5 Other Child Support Variables

The question of whether a promise of support in the past is exerting a measurable effect might be tempered by the actualization of child support later on. For a reduced sample, I am able to determine whether any sort of agreement, legal or otherwise, is in effect by the one-year follow-up survey and whether the payment is relatively on time. I use a dichotomous variable to indicate whether the mother reports having a legal or informal support agreement with the father and condition on that variable in addition to the full set of controls described above.

The late payment variable is equal to one if the mother reports that half or more of the agreed upon child support generally arrives late or never. It is equal to zero if the agreed upon child support is paid on time always or most of the time. Including these two additional variables allows me to test whether the promise of support exerts an effect, conditioning on current income streams.

2.3.3 Descriptive Statistics

Table 2.1 shows the cumulative probability of the ages of children whose mothers were interviewed at birth and around the child's first birthday. Most children's mothers were interviewed

within a few months of their birthday, but in some extreme cases, the mother was not interviewed until the child was 20 months or more. I restrict the sample to children aged 12-18 months. During this time period, as described above, a child should have seen the doctor within three months.

Figures 2.1 and 2.2 show histograms and corresponding kernel density function for the distribution of elapsed time since a doctor's appointment for women who received a promise of support (2) and those who did not (1). Though both distributions have a long right tail, and observations are clustered near the origin, it is clear that women who received the promise were much more likely to have taken their child to the doctor recently.

Figures 2.3 and 2.4 show distributions of months a child was breastfed by whether the mother received a promise of support. Women who received a promise of support were slightly more likely to have breastfed their children at all, and there is much more variation in the right tail of their distribution.

Table 2.2 shows descriptive statistics for mothers in the baseline survey, split by promise of support. The first column shows unmarried respondents who reported that the child's father had promised financial support. The second column shows responses by unmarried respondents who reported that the child's father had not promised financial support. While the average responses are similar on many measures, there are some key differences that merit further attention. Women receiving the promise are about a year younger and more likely to have been born in the US and have their own income. Almost 70% of the women receiving the promise were Black as opposed to 50% of the non-promise population. Differences in education do not show much of a discernible pattern.

Women reporting a promise of financial support tend to answer questions in such a way that suggest a better relationship with the father. They are less likely to report arguing over the pregnancy and other relationship issues. They are more likely to indicate that they want the father around and less likely to indicate that the father suggested an abortion (15% versus 29%). Women who received a promise are also more likely to predict high chances of eventual marriage with the father. It is interesting to note, however, that the timing of the survey might affect answers about

Figure 2.1: Distribution of Time Elapsed Since Last Doctor's Visit for Children whose Mothers Report No Promise of Support

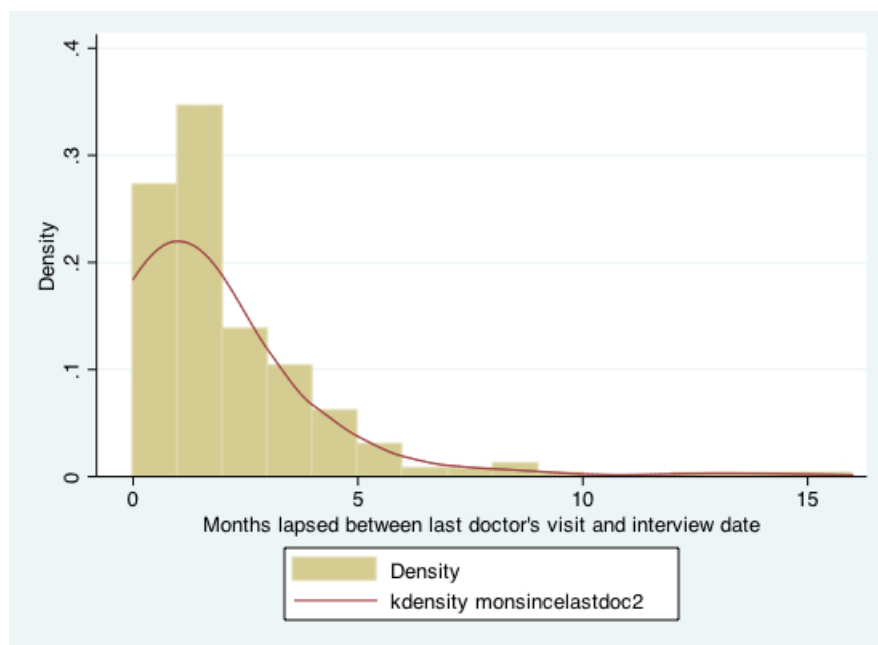


Figure 2.2: Distribution of Time Elapsed Since Last Doctor's Visit for Children whose Mothers Report a Promise of Support

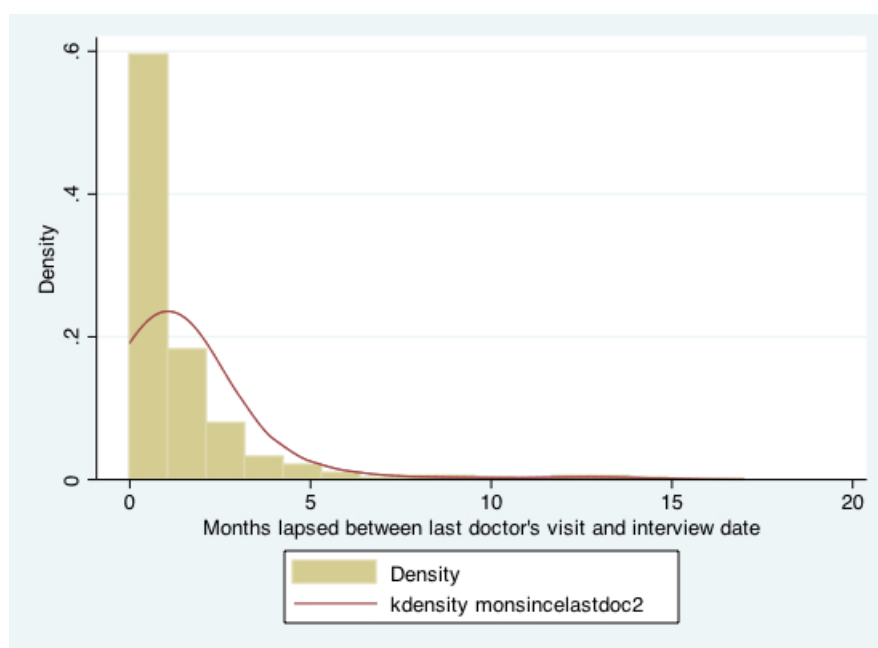


Figure 2.3: Distribution of Months Child Breastfed for Children whose Mothers Report No Promise of Support

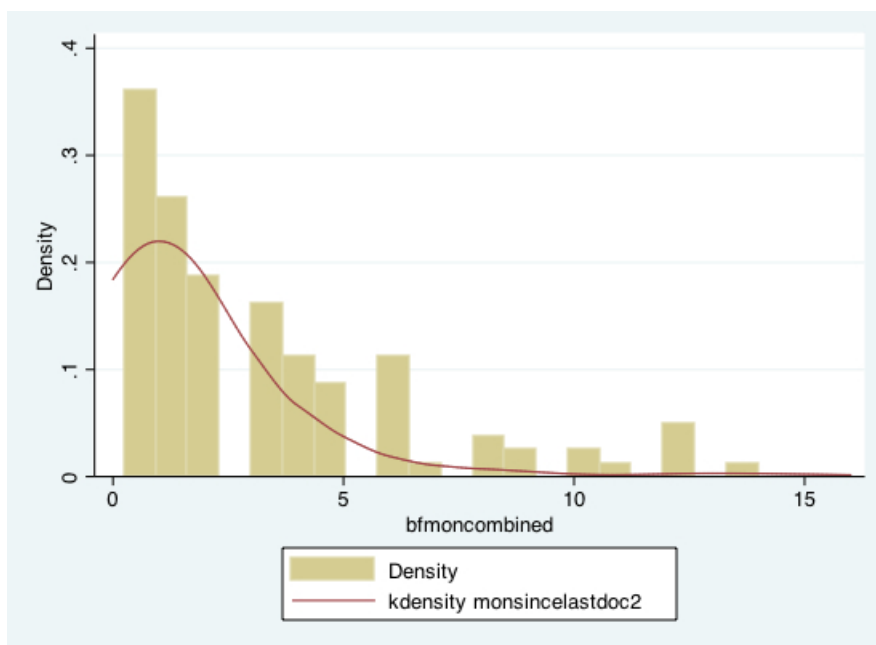


Figure 2.4: Distribution of Months Child Breastfed for Children whose Mothers Report a Promise of Support

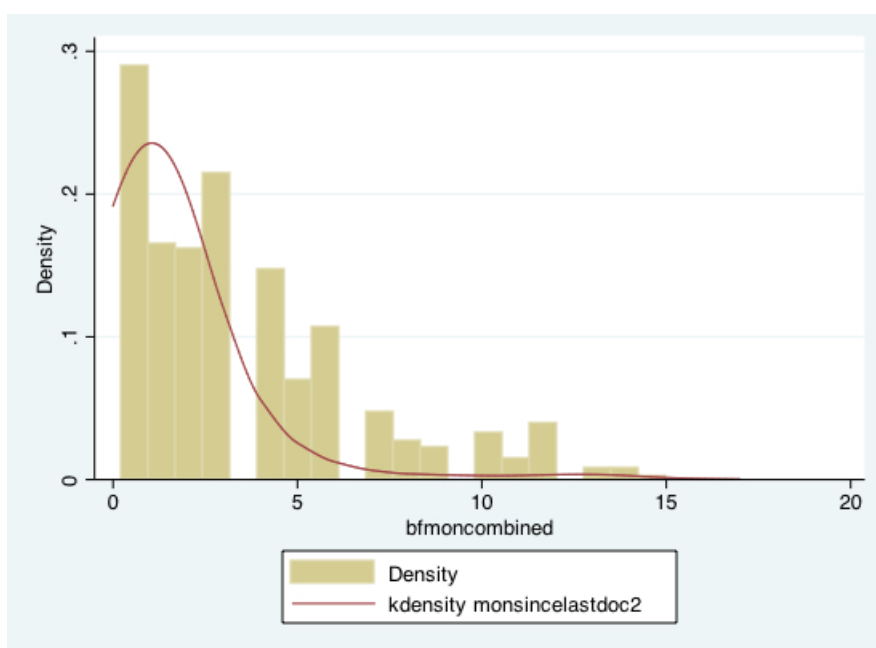


Table 2.2: Baseline Characteristics of Unmarried Mothers

	Promise Mean	No Promise Mean	T for difference
Mother's Age	22.8	23.3	0.72
% Male Child	56.5	52.9	-0.60
% Low Birth Weight?	11.2	15.7	1.05
Average Frequency of Disagreements-1 is 'Always, 3 is 'Never'			
Money	2.3	2.4	0.64
Time	2.19	2.19	-0.02
Sex	2.61	2.56	-0.59
Pregnancy	2.61	2.32	-2.99
Drugs	2.76	2.61	-1.86
Cheating	2.51	2.37	-1.411
% Mother wants Father around	94.9	70.5	-4.61
% Father wants to be around	98.3	71.9	-3.06
% Father Promised \$	100	0	
% Father Suggested Abortion	14.5	29.4	2.86
% In Public Housing	17.5	21.4	0.81
% Mother US-Born	94.8	92.9	-0.66
% Mothers Earn Any Income	70.	67.9	-0.46
% Receiving Public Asst.	45.9	35.7	-1.78
% Receiving Unemployment	10.4	8.3	-0.63
Num other Kids	0.99	0.92	-0.44
% Any Prenatal Care	97.0	98.8	1.29
Month of first Doc Visit	2.9	2.6	-1.150
% Used During Pregnancy			
Alcohol	1.8	3.5	0.83
Drugs	4.6	4.7	0.05
Cigarettes	19.3	22.4	0.63
%White	22.3	32.9	1.96
%Black	63.0	47.1	-2.73
%Asian	0.8	0	-2.01
%Native American	2.4	0.05.9	1.32
%Other	11.6	14.1	0.64
% Latina	22.1	28.6	1.22

Baseline Characteristics of Mothers continued

	No Promise Mean	Promise Mean	T for difference
Proportion with level of education			
No Formal	0	0	
Less than Grade 8	3.0	7.1	1.41
Some HS	33.2	34.1	0.16
HS Diploma	30.8	25.9	-0.95
GED	5.4	5.9	0.19
Some College	20.9	21.2	0.06
Tech/Trade School	3.8	2.4	-0.77
BA/BS	2.8	2.4	-0.24
Graduate School	0.2	1.2	0.82
Earnings (1999 US\$)	23869	21804	-0.97
% Reporting Chances of Marriage at Birth of Child			
No Chance	21.3	61.9	7.20
A little Chance	14.5	13.1	-0.36
Fifty Fifty Chance	20.7	9.6	-3.03
Good Chance	22.3	8.3	-3.93
Certain Chance	21.1	7.1	-4.15
Father characteristics at birth			
%Father in Jail	6.4	10.6	1.20
% Father Worked Last Week	69.0	41.2	-4.83
% Father has drug problem	6.4	23.6	3.61
Child Support Variables for Wave 2			
% Any Child Support Agreement	44.6	39.3	-0.92
% Late Payments	19.7	31.4	1.40
Breastfeeding Duration	3.5	3.1	-0.94
N	503	85	

Late support payment is conditional on any support agreement

relationship quality. Edin & Kefalas (2007) tell of the “magical moment” of pregnancy, whereby everything seems like it will work out for the best and parents are more likely to report high satisfaction with their relationship and expectations for the future.

The pattern of support shows both positive and negative selection. While we might expect that a promise of support was correlated with higher socio-economic status, this does not appear to be the case. Women who report better relationships with the father are more likely to have received a promise of support, but race, income and education are not strong predictors of the promise. Black women are more likely to report a promise than White or Latina women, as are women born in the US. Younger women, also, were more likely to report a promise.

The effect of a financial promise before birth on child support during the first follow-up survey is not statistically different from zero. While about 44.6% of those receiving a promise have a formal or informal child support agreement, while 39.3% of women who did not receive a promise have an agreement later. This effect is not statistically different from zero. Nor is it for the propensity of late payments. Most of those reporting no promise and a child support agreement also reported that payments tend to be late.

Table 2.3 shows descriptive characteristics of women interviewed at the baseline by whether a promise of support was given, this time examining Black women and White women separately. Within race, there are surprisingly few differences by promise of support. Each group reports an average age of around 23. Differences in average education, income and responses to other questions, when broken out by race seem to disappear when comparing those who received a promise to those who didn't. Where socioeconomic differences do arise, they do not point to a pattern of negative or positive selection for a promise. For instance, Black women who receive a promise of support are slightly more likely to be receiving unemployment (10.6% versus 3.9%). White women who receive the promise are slightly more likely to have received a bachelor's degree. In terms of propensity score matching, this is both useful as the issue of common support is readily solved. There exist people in the middle who are both receiving the treatment and not receiving the treatment.

Despite their outward similar appearance, the inclusion of attitudinal questions and additional

Table 2.3: Characteristics of Black and White Respondents by Promise of Support

	Black mothers			White mothers		
	Promise	No Prom.	T diff	Promise	No Prom.	T diff
Mother's Age at birth	23.34	23.83	0.92	23.64	24.30	0.79
%Male child	53.9	51.1	-0.59	56.0	51.5	-0.57
Num Other Kids	1.24	1.16	-0.49	0.82	0.95	0.83
% Low birthweight	14.2	19.4	1.50	9.3	9.1	-0.05
Frequency of Arguments 3 is Never, 1 is Always						
Money	2.32	2.39	0.96	2.27	2.45	1.71
Time	2.19	2.39	2.63	2.19	2.19	-0.05
Sex	2.58	2.56	-0.29	2.65	2.65	-0.04
Pregnancy	2.64	2.38	-3.93	2.57	2.27	-2.77
Drugs	2.84	2.77	-1.39	2.69	2.44	-2.51
Cheating	2.45	2.49	0.58	2.54	2.39	-1.28
% Mother wants father involvement	96.8	66.1	-13.45	92.5	51.7	-7.99
% Father wants involve	98.4	83.5	-4.56	94.2	62.5	-4.29
% Father suggested abortion	15.7	32.6	4.70	17.2	29.0	2.02
% Cohabiting at birth	1.4	0	-1.37	2.7	0	-1.34
% in public housing	18.1	18.9	0.21	8.6	12.1	0.83
% Mother US born	97.7	96.9	-0.59	92.0	89.4	-0.64
Latina	2.6	1.6	-0.69	40.1	43.1	0.42
Economic Characteristics						
% Rec. any public asst	50.8	44.5	-1.32	52.4	41.5	-1.51
% Rec. unemployment	10.6	3.9	-2.39	9.2	12.3	0.71
Income (1999\$)	19459	18900	-0.33	31087	24861	-1.53

Continued
 Characteristics of Black and White Respondents by Promise of Support

	Black mothers			White mothers		
	Promise	No Prom.	T diff	Promise	No Prom.	T diff
Prenatal Behaviors						
% Any Prenatal Care	97.5	96.9	-0.40	97.8	100	1.19
Month of First Prenatal	2.86	3.03	0.95	2.66	2.8	0.57
% On/Off Rel. with Father	60.0	39.5	-4.41	51.9	53.0	0.16
% Alcohol	2.5	5.4	1.84	1.1	3.0	1.09
% Drugs	6.8	7.0	0.08	7.0	3.0	-1.17
% Cigarettes	21.6	26.4	1.21	31.0	27.3	-0.57
Education						
% No Formal Educ.	0	0		0	3.0	2.40
% Less than 8th Grade	1.8	2.3	0.43	8.0	7.6	-0.11
% Some HS	35.6	33.6	-0.45	29.4	28.8	-0.10
% HS Diploma	31.3	32.0	0.16	26.2	22.7	-0.56
% GED	5.8	6.33	0.18	3.7	10.6	2.10
% Some College	19.1	20.3	0.33	21.9	22.7	0.13
% Tech/Trade degree	3.5	3.1	-0.19	6.4	3.0	-1.03
% BA/BS	2.1	2.3	0.14	2.7	0	-1.34
% Grad Degree	0.7	0	-0.96	1.6	1.5	-0.05
Stated chances of marriage at birth of child						
% No Chance	20.5	75.8	14.3	35.8	70.0	4.97
% A Little Chance	15.0	6.3	-2.63	14.9	15.1	0.03
% 50/50 Chance	24.1	7.0	-4.39	13.9	9.1	-1.01
% Good Chance	22.1	6.3	-4.23	0.13	4.5	-1.97
% Certain Chance	18.4	4.7	-3.91	21.9	0.02	-3.94
One-year child characteristics						
Child's Age at 2nd interview	16.3	16.8	1.4	14.3	14.30	0.09
% Disabled Child	3.2	2.3	-0.54	3.8	4.5	0.28
Times saw doctor for illness	2.88	3.72	1.60	4.25	3.88	-0.37
Times went to ER	1.55	1.79	1.17	1.16	1.41	1.16
% Ever Breastfed	41.6	38.8	-0.61	49.7	57.6	1.09

information about the relationship and the father's character do uncover some differences. Those who received the promise, both Black and White women, report higher measures of subjective relationship quality. They tend to say that they want the father involved with the child more, and are less likely to report that the father suggested she get an abortion (32.6% versus 15.7% for Black women and 29% versus 17.2% for White women). They report arguing less on average, particularly about the pregnancy, and are more likely to say they expect to marry the baby's father. However, 60% of Black women receiving the promise reported being in an on-off relationship with the father, versus only 39.5% of the Black, non-promise population. This measure is mediated by more Black women reporting no lingering romantic relationship with the baby's father.

2.3.4 Methods

One potential problem is that mothers have significantly more information about their child's father than is observable. It may be that mothers are not responding to the commitment device itself, but some other, unobservable quality of the father. In the case that mothers who do not take their child to the doctor are the same mothers who had a child with delinquent fathers, this test cannot distinguish between parents who choose not to invest due to the level of security afforded by the commitment device or the level of security afforded by a responsible father. I attempt to control for mothers' proprietary information with various questions the mother answers about the father's trustworthiness, tendency to violence and other qualities. While marriages are said to be assortatively matched on the couple's preferences for investing in children's wealth (Peters & Siow 2002), it is unclear whether such matching is present in couples that do not marry, but do choose to bear children together.

In an ideal world, I would be able to observe the same mother and father pair in separate realities: for instance, when they chose to enter into a verbal or legal agreement about financial support but remained unmarried and then separately the same pair when the father does not offer a verbal promise of support. Barring the possibility of time travel, propensity score matching is also one way to replicate that experiment in part. By matching women with similar observable

characteristics and particularly answers about the child’s father, I can compare their children’s health status with the hope of identifying an effect from the commitment device itself. More specifically, I use the mother’s characteristics and reports of the father’s characteristics to create propensity scores that predict whether a promise of financial support had been given at the time of birth of the child. This strategy hinges on the assumption that mother-father pairs with similar observable characteristics are also similar on unobservables. In the case where this is not true, this test will yield spurious results.

It is likely that unobserved heterogeneity plays a large role in how often a mother takes her child to the doctor. A mother’s organizational skills, her own health status and experience with doctors as well as proximity to doctors she trusts may influence the number of times she takes her child to the doctor. In order to attempt to control for such heterogeneity, I control for a number prenatal health decisions made by mothers including drug and alcohol use and when prenatal care was first sought.

2.3.4.1 OLS with Father characteristics

I begin with a basic OLS specification of a health outcome on a 0-1 promise of support and various controls:

$$y_i = \alpha + \beta_1 promise_i + X_i\gamma + Z_i\psi + \sum_{i=0}^N babyage + \epsilon_i \quad (2.1)$$

where y is an outcome variable indicating mother’s investments in the child’s health. y is a 0-1 variable indicating whether the child was late for the last recommended doctor’s visit (“late”) or months the child was breastfed. ‘Promise’ is a dichotomous variable indicating whether the father made a promise of financial support to the mother by the time the baseline interview was conducted. X is a vector of controls including race, education, family characteristics, earnings and work status. Z is a vector of dichotomous variables about the mother’s opinion of the father’s character and trustworthiness.³

Babyage represents a series of indicator variables for the child’s age in months at the time of

³ See Table 4 for the full list of variables

the interview. A child of thirteen months will have a value of one for the 13-month dummy and a value of zero for all other age dummies. As the follow-up interview conducted around the child's first-year birthday was not collected at the same time for each child, the age variable requires some additional consideration. All other things equal, a child of 18 months will necessarily have a longer amount of time elapsed since his last check-up than a child of a 17 months. Controlling for age in this manner gives a separate constant to each month cohort, reducing the possibility that we ascribe reductions in months elapsed to the promise of support when it is actually only due to the passage of time. The equation is estimated on the full sample and then separated out by race for Black and White mothers.

2.3.4.2 Conditioning on current support agreements

A second set of OLS specifications conditions on the current state of support agreements between the mother and father. Using additional information given at the follow-up interview, I estimate the effects of a promise of support on health investments in the following way:

$$y_i = \alpha + \beta_1 promise_i + X_i \gamma + Z_i \psi + \sum_{i=0}^N babyage + \beta_2 AnySupportAgreement + \beta_3 LatePayment + \epsilon_i \quad (2.2)$$

where y , X , Z and $babyage$ are as described above. $AnySupportAgreement$ is a 0-1 variable for whether the mother reports having a child support arrangement at the time of the follow-up interview. And $LatePayment$ is a 0-1 variable for whether that arranged child support is paid late (1) or on-time (0).

2.3.4.3 Propensity Score Matching

A number of matching methods exist with which I could estimate the effect of a promise of financial support on health investments in children. Though each of them should, in theory, present the same estimates as observations approach infinity, with finite observations, the choice of method is important. I follow Caliendo & Kopeinig (2008) and Black & Smith (2004). I estimate

propensity scores for a promise of support using a logit model where the probability of receiving a promise is a function of observable characteristics.

Thus a mother’s propensity score is calculated by:

$$P(\textit{Promise} = 1|X) = \alpha + \beta_1\textit{promise}_i + X_i\gamma + Z_i\psi + \sum_{i=0}^N \textit{babyage} + \epsilon_i \quad (2.3)$$

where X, Z and babyage are the full set of controls found in the OLS regression labeled (1) in Table 4 as well as father characteristics and age dummies. I then use the propensity scores in place of the treatment–or support promised–to create a matched sample and estimate the effects on the probability of late doctor’s visits and duration of breastfeeding. I employ three methods of matching on propensity scores; nearest neighbor matching, kernel distribution⁴ and stratification.

In subsequent specifications, I estimate the propensity score conditioning on whether a current agreement of support is in place and excluding the additional father characteristics such as whether he was in jail.

2.4 Results

2.4.1 OLS

In the interest of external validity, I weight the sample using the national weights as constructed by the Fragile Families. The choice of weights, at least within those offered by the data, does not affect the point estimates significantly, though the weighted and unweighted results do diverge. Results on months elapsed since last doctor’s visit are in Table 2.4.

Table 2.4 shows a series of regressions for the whole sample (1), and then split out by race (2-5). The first column shows the regression with all controls–race, education, socioeconomic status, father characteristics–for the whole sample. Here, the promise of support shows a reduction in the probability that a child was late to his last recommended doctor visit, but the effect is not distinguishable from zero. The second two columns are for White mothers only. (3) adds upon (2) with additional controls for father characteristics such as whether the father was in jail for the

⁴ For kernel estimation, standard errors are bootstrapped using 200 repetitions.

Table 2.4: Late Doctor's visit on Support Promise using OLS

	All (1)	White (2)	White (3)	Black (4)	Black (5)
Father Promised \$ Support	-0.0421 (0.0493)	0.122* (0.0671)	0.105 (0.0771)	-0.234** (0.0988)	-0.246*** (0.0950)
Mother's Age at birth	-0.00342 (0.00498)	-0.00864** (0.00432)	-0.00811** (0.00398)	-0.000143 (0.00901)	0.000979 (0.00916)
Male Child	-0.0504 (0.0410)	0.0162 (0.0693)	-0.000659 (0.0629)	-0.0969* (0.0535)	-0.0825 (0.0527)
Father suggested abortion	0.00712 (0.0335)	0.136 (0.101)	0.158 (0.106)	-0.0550 (0.0474)	-0.0364 (0.0444)
Num Other Kids	-0.00649 (0.0175)	-0.0478 (0.0383)	-0.0541 (0.0379)	0.000166 (0.0265)	-0.00991 (0.0248)
Race, Education and Income					
Black	0.0735 (0.0801)				
Asian	-0.104 (0.0696)				
Nat American	0.153 (0.122)				
Other	0.0162 (0.0825)				
Latina	0.000681 (0.0511)	0.0191 (0.0502)	0.0287 (0.0511)	-0.0438 (0.0834)	-0.0303 (0.0813)
HS Diploma or GED	0.0175 (0.0426)	0.0285 (0.0783)	0.0539 (0.0734)	0.00714 (0.0669)	-0.00536 (0.0650)
Some college	0.0430 (0.0474)	0.000826 (0.0909)	0.0407 (0.0859)	0.0556 (0.0881)	-0.00269 (0.0835)
Tech/Trade School	0.0782 (0.0767)	0.186* (0.104)	0.222** (0.111)	-0.0556 (0.0883)	0.00672 (0.0912)
BA/BS	0.182 (0.136)	0.181 (0.122)	0.183 (0.156)	0.0599 (0.174)	0.0191 (0.174)
Grad School	-0.110 (0.179)	0.504 (0.357)	0.515 (0.409)	-0.201 (0.196)	-0.123 (0.182)
Log of income at birth	-0.0610** (0.0280)	-0.0957* (0.0563)	-0.113** (0.0568)	-0.0146 (0.0280)	-0.0236 (0.0279)

continued on next page

Late Doctor's Visit Continued					
	All (1)	White (2)	White (3)	Black (4)	Black (5)
Prenatal Investment Behaviors					
Month of first Prenatal visit	0.00108 (0.0104)	0.0133 (0.0188)	0.0303 (0.0226)	0.00285 (0.0124)	0.00206 (0.0123)
Any prenatal care?	-0.160 (0.144)	-0.396 (0.286)	-0.265 (0.288)	-0.206 (0.165)	-0.154 (0.150)
Cigarettes	-0.0585 (0.0370)	0.0318 (0.0822)	0.0368 (0.0787)	-0.0965** (0.0451)	-0.115** (0.0462)
Drugs	0.0351 (0.0719)	-0.210 (0.135)	-0.194 (0.132)	0.115 (0.101)	0.0553 (0.0685)
Alcohol	-0.0514 (0.0737)	-0.512** (0.230)	-0.568** (0.232)	-0.168 (0.114)	-0.0965 (0.0987)
Father characteristics at birth					
Father in jail	0.0208 (0.0540)		-0.329** (0.135)		0.0729 (0.0724)
Father worked last week	0.0716** (0.0348)		0.0213 (0.0584)		0.0237 (0.0487)
Father has drug problem	0.122 (0.0818)		0.0364 (0.0851)		0.379*** (0.123)
Constant	0.908*** (0.329)	1.425** (0.673)	1.383** (0.661)	1.013** (0.453)	1.013** (0.443)
Observations	588	140	140	357	355
R-squared	0.166	0.357	0.406	0.260	0.311

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Indicator variables for age are included.

e.g., If mother was interviewed when child was 13 months, age13=1 and all others =0

Controls for father's race and education are included

birth of the baby. Again, here, the effect of the support promise is indistinguishable from zero when all controls are included. The last two columns replicate (2) and (3) for Black mothers only, and here we see a large, measurable effect in the expected direction. Women reporting the promise were about 24 percentage points less likely to be late for the last recommended doctor's visit than women who did not receive the promise. While education and income do not appear to have a large impact on whether the one-year-old children have been to the doctor recently for Black mothers, the promise of support exerts a large effect. In contrast, income is important for White mothers, with additional income reducing the probability that a child will be late to a recommended checkup. These differences between the coefficients for White and Black women are statistically significant at the 1% level.

Additionally in Table 2.4, prenatal behaviors and objective estimations of the father's character exert different effects for White mothers and Black mothers. Whether the father worked around the time of the birth seems to be a strong indicator that a White mother will not be late for the child's checkup. Black women show a stronger association with whether the father has a drug problem, which increases the likelihood that the child will miss the recommended checkup by some period of time.

Though not reported here, additional specifications were performed that included a variable indicating whether the child is covered by Medicare or private health insurance at the time of the follow-up interview. The coefficients on these indicator variables are insignificant and do not have a measurable effect on the coefficient on the financial support promise. This may indicate that the financial aspect of these investments in children is less important than other determinants, perhaps unobserved, such as pre-birth decisions.

Table 2.5 shows results using breastfeeding as the outcome variable. The first regression is for the entire sample, while specifications 2-5 are split by race, with 2-3 representing White mothers and 4-5 representing Black mothers. White women who received a promise of support breastfed their children for two more months than White women who did not receive the promise. Education also exerts a strong effect. Women who graduated from college or even attended some

college breastfed the focal child for about 2 more months on average than those who had only some high school or less education. A few measures of prenatal investment are also particularly salient. White women who used cigarettes or drugs during pregnancy spent on average 3-4 fewer months breastfeeding the child.

2.4.1.1 Results on ‘Ever Breastfed’

The same specifications were performed using ever breastfed as an independent variable instead of months breastfed, but no significant coefficients emerged. There is some evidence, by way of descriptive statistics at the baseline, that mothers who breastfeed at all are slightly different than mothers who do not. Mothers who breastfed were slightly more educated and slightly older than mothers who did not attempt breastfeeding at all.

2.4.2 Results on Propensity Score Matching

The results associated with a propensity score matching strategy are decidedly different from the results of the parsimonious regressions, though different results by race are still present and magnitudes, as predicted, are smaller for propensity score matching than for OLS. The results are shown in Tables 2.6 and 2.7 and a propensity score distribution is in Figure 2.5. Table 2.6 is for a late doctor’s visit and Table 2.7 uses breastfeeding as an outcome variable. Each of Tables 2.6 and 2.7 shows three different methods of matching based on three iterations of controls included in calculating the propensity score. The first specification uses the full set of controls as listed in Table 2.5 for OLS. The second specification conditions on current agreements of support as in (2) and (4) of Table 2.6. The final specification leaves out the additional father characteristics such as whether he was in jail when the child was born as in regression (2) and (4) of Table 2.5.

Though the pattern of significance varies among which controls are included, there is no strong evidence for an effect of the promise on doctor’s visits. Including all important controls leads to weakly significant results, but they are highly dependent on specification. For instance, specification (1) for Black mothers, which includes the full set of controls, indicates some weak

Table 2.5: Months Breastfeeding on Support Promise using OLS

	All (1)	White (2)	White (3)	Black (4)	Black (5)
Father Promised \$	1.212** (0.596)	0.992 (1.129)	2.440* (1.271)	0.658 (1.028)	1.155 (1.108)
Mother's Age at birth	0.0833* (0.0472)	-0.0139 (0.0674)	-0.0181 (0.0571)	0.114 (0.0935)	0.120 (0.0960)
Male Child	0.256 (0.453)	-0.315 (0.821)	0.536 (0.815)	1.062 (0.727)	1.193 (0.731)
Father suggested abortion	1.466** (0.628)	-2.263 (1.597)	-1.467 (1.577)	2.314** (0.971)	2.222** (0.978)
Num Other Kids	0.851*** (0.247)	1.306* (0.674)	1.258** (0.522)	0.531* (0.283)	0.618** (0.285)
Race, Education and Income					
Black	1.587* (0.898)				
Asian	3.601 (2.536)				
Native American	-1.330 (1.1078)				
Other	-1.179 (0.869)				
Latina	1.254** (0.553)	-0.0877 (1.060)	-0.786 (0.814)	1.798 (1.315)	1.920 (1.298)
HS Diploma or GED	0.457 (0.629)	0.152 (1.009)	0.333 (0.899)	0.828 (1.113)	0.889 (1.127)
Some college	0.824 (0.662)	1.919 (1.439)	1.770 (1.473)	0.893 (1.263)	0.736 (1.252)
Tech/Trade school	-4.941*** (1.577)			-1.559 (1.767)	-3.045 (2.172)
BA/BS	0.557 (1.762)	3.057 (2.703)	4.293* (2.327)	-0.147 (2.029)	-0.737 (2.137)
Grad degree	-2.570 (2.413)	-1.300 (1.924)	3.369 (3.339)	-6.296** (2.750)	-5.363* (2.770)
Log of income at birth	0.667*** (0.231)	1.026 (0.644)	0.703 (0.482)	0.554 (0.404)	0.610 (0.402)

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Months Breastfeeding Continued					
Prenatal Investment Behaviors					
Month of First Prenatal	-0.350**	-0.258	-0.303	-0.204	-0.277
	(0.159)	(0.196)	(0.198)	(0.214)	(0.217)
Any Prenatal Care?	-5.505***	0.349	2.996	-4.675**	-4.369**
	(1.676)	(2.012)	(2.396)	(1.989)	(1.955)
Cigarettes	-1.027*	-2.698**	-3.178***	-0.540	-1.252
	(0.563)	(1.031)	(0.967)	(1.365)	(1.546)
Drugs	-0.339	-4.553*	-5.273**	1.618	0.626
	(0.986)	(2.250)	(1.940)	(1.283)	(1.226)
Alcohol	-2.130***			-1.043	0.274
	(0.765)			(1.980)	(2.023)
Father characteristics at birth					
Father in jail	-2.915**		-4.609**		-3.024*
	(1.230)		(1.891)		(1.605)
Father worked week before birth	-0.524		-3.739**		0.271
	(0.565)		(1.652)		(0.670)
Father has drug problem	0.892		0.243		3.030**
	(0.802)		(0.964)		(1.435)
Constant	-0.992	-6.478	-4.900	0.605	-1.113
	(3.528)	(7.404)	(5.305)	(4.995)	(5.119)
Observations	255	65	65	143	142
R-squared	0.474	0.595	0.667	0.590	0.616

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Indicator variables for age are included

e.g., If mother was interviewed when child was 13 months, age13=1 and all others =0

Controls for father's race and education are included

Table 2.6: Results on Late 1-year visit using Propensity Score Matching on Promise of Support

	(1) Full	(2) Add Agree	(3) No Add'l Father
Full Sample			
Nearest	-0.053	-0.098	0.048
Neighbor	(0.054)	(0.060)	(0.056)
	-0.972	-1.631	0.851
Kernel	-0.063	-0.061	0.018
	(0.047)	(0.069)	(0.061)
	-1.343	-1.277	0.294
Stratification	-0.067	-0.069	0.02
	(0.060)	(0.060)	(0.058)
	-1.108	-1.14	0.347
White Mothers			
Nearest	0.044	0.064	-0.079
Neighbor	(0.054)	(0.054)	(0.069)
	0.803	1.197	-1.159
Kernel	0.017	0.014	0.018
	(0.079)	(0.078)	(0.061)
	0.212	0.185	0.294
Stratification	0.06	0.061	-0.09
	(0.039)	(0.038)	(0.053)
	1.515	1.576	-1.697
Black Mothers			
Nearest	-0.085	-0.186	-0.079
Neighbor	(0.071)	(0.085)	(0.069)
	-1.199	-2.181	1.159
Kernel	-0.096	-0.094	-0.09
	(0.072)	(0.066)	(0.053)
	1.332	-1.421	-1.697
Stratification	-0.124	-0.121	-0.093
	(-)	(0.058)	(0.066)
		-1.107	-1.41

Standard errors in parentheses, associated t-stat below

*** p<0.01, ** p<0.05, * p<0.1

(1) includes the full set of controls as listed in Table 4

(2) is as (1) plus whether a support agreement is in effect

(3) is as (1) minus father characteristics such as drug problem

Kernel estimation standard errors are bootstrapped with 200 repetitions

Blanks indicate that analytical errors could not be calculated

Table 2.7: Results on Breastfeeding Duration using Propensity Score Matching on Promise of Support

	(1)	(2)	(3)
	Full	Add Agreement	No Add'l Father
Full Sample			
Nearest	0.771	0.821	1.275
Neighbor	(0.523)	(0.578)	(0.527)
	1.473	1.422	2.322
Kernel	0.657	0.677	0.922
	(0.506)	(0.488)	(0.379)
	1.299	1.388	2.431
Stratification	0.628	0.665	1.042
	(0.319)	(0.314)	(-)
	1.968	2.116	
White Mothers			
Nearest	1.116	1.016	1.018
Neighbor	(0.3556)	(0.55)	(0.501)
	2.006	2.09	2.031
Kernel	1.424	1.489	1.274
	(0.657)	(0.673)	(0.611)
	2.168	2.212	2.087
Stratification			1.157
	(-)	(-)	(-)
Black Mothers			
	full controls	full controls	no father char
Nearest	1.372	1.507	0.716
Neighbor	(0.632)	(0.723)	(0.563)
	2.176	2.083	1.27
Kernel	0.451	0.431	0.747
	(0.705)	(0.705)	(0.637)
	0.64	0.611	1.172
Stratification			0.995
	(-)	(-)	(-)

Standard errors in parentheses, associated t-stat below

*** p<0.01, ** p<0.05, * p<0.1

(1) includes the full set of controls as listed in Table 4

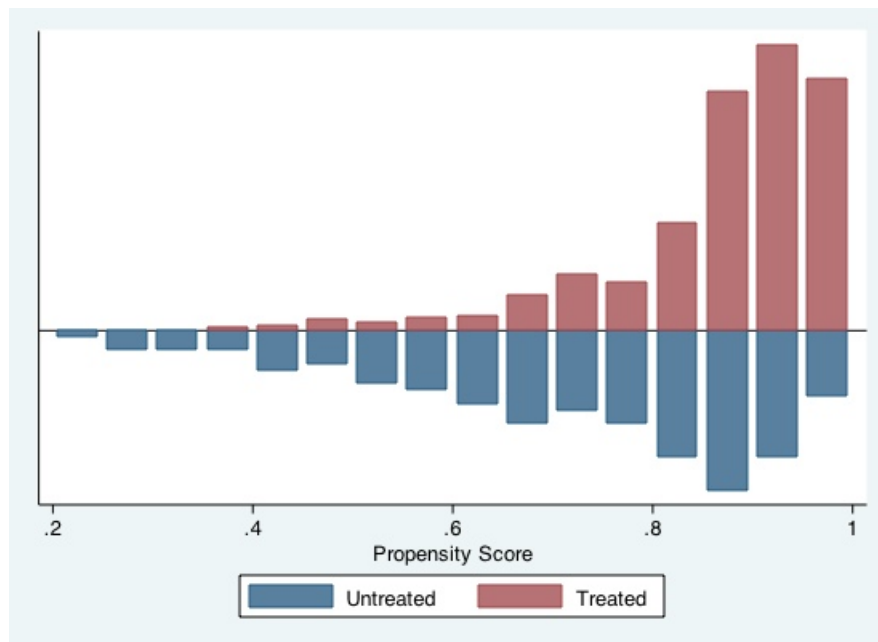
(2) is as (1) plus whether a support agreement is in effect

(3) is as (1) minus father characteristics such as drug problem

Kernel estimation standard errors are bootstrapped with 200 repetitions

Blanks indicate that analytical errors could not be calculated

Figure 2.5: Propensity Score Distribution for Full Set of Controls



evidence of an effect. When using nearest neighbor matching, Black mothers most likely to receive the promise are shown to be 18 percentage points less likely to be late than their counterparts unlikely to receive the promise. However, this result is not robust to different matching methods, or leaving out variables that are determined later.

For breastfeeding, the results are somewhat stronger and slightly more consistent, particularly for White mothers, though they too are sensitive to specification. For Black mothers, nearest neighbor matching leads to an effect of 1.3-1.5 additional breastfeeding months for those likely to receive the promise of support. For White mothers, nearest neighbor matching and kernel matching lead to a similar effect, on the order of 1.1-1.5 months, but with different sets of controls. In the full sample, effects are smaller and much more varied, ranging from 0.6-1.2 months. They are only significant, however, when using stratification matching, with an effect of about 0.6 months. The divergence here again indicates that there race and class may play a role in the interpretation of survey questions, particularly questions concerning expectations of child support.

2.4.3 Results on Actual versus Promised Financial Support

Perhaps the most interesting aspect of the question of whether promises of financial support affect behavior is whether, when we condition on the fulfillment of the promise, it continues to exert an effect. Though the Fragile Families Data Set does provide information on child support paid and the existence of legal and informal support agreements, the data are not well populated, so the work done here is with a reduced sample.

With the goal of maintaining the insight gained from the full sample results and deepening our understanding of the interaction between expected and actual income, I repeat the analysis on a subsample of the Fragile Families Data which includes women who answered questions about actual financial support from the child's father.

A replication of the earlier OLS regressions but adding the mother's report of how frequently the father pays child support on time is displayed in Tables 2.8 and 2.9. The first column is for the entirety of the above sample that answers questions about child support, the second two are for

White women, adding in the late payment variable for (3). Specifications (4) and (5) are for Black women, again adding the late payment variable in (5).

Table 2.8: Results on late visit using OLS with actualization of child support in 1st year. Full controls as used in earlier specifications are present.

	All (1)	White (2)	White (3)	Black (4)	Black (5)
Father Promise	-0.048 (0.049)	0.103 (0.076)	0.085 (0.076)	-0.25** (0.095)	-0.25*** (0.095)
Any Support Agreement	0.034 (0.038)	0.011 (0.062)	0.046 (0.069)	-0.011 (0.046)	0.006 (0.045)
Late support payment	0.08** (0.039)		0.12 (0.077)		0.11** (0.049)
Constant	0.80** (0.32)	1.36** (0.65)	1.25* (0.64)	1.02** (0.45)	0.90** (0.43)
Observations	588	140	140	355	355
R-squared	0.171	0.406	0.413	0.311	0.317

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Indicator variables for age are included

e.g., If mother was interviewed when child was 13 months, age13=1, all others =0

All include full set of controls are as listed in Table 4

For Black women, the promise of support reduces the probability of having gone late to the doctor by about 25 percentage points. Inclusion of a mother's report of late child support payments, (5), has no effect on the coefficient on the promise of support for Black women while late payments increase the probability that she will be late to the doctor's visit by about 11 percentage points. The variable of whether the father is late on payment also increases the probability that a mother will be late to take her child to the doctor. This result is especially robust for Black women and is around 11 percentage points. For White women, the effect is only present without other controls for father characteristics (not shown). While this may indicate some sort of assortative matching on preferences for investing in children, it also may be that the late payment induces the mother to put off the doctor's appointment because she cannot pay for it.

Other key variables, such as income, whether the mother smoked during the pregnancy, and

Table 2.9: Results on breastfeeding duration in months using OLS with actualization of child support in 1st year. Full controls as used in earlier specifications are present.

	All (1)	White (2)	White (3)	Black (4)	Black (5)
Father Promise \$	1.20** (0.59)	2.37* (1.36)	2.71* (1.43)	1.22 (1.09)	0.81 (1.12)
Any Support Agreement	0.75 (0.52)	-0.25 (1.14)	0.21 (1.17)	0.43 (0.72)	1.36* (0.78)
Late support payment	2.80*** (0.75)		1.83 (1.66)		3.53*** (1.04)
Constant	-4.12 (3.46)	-4.30 (6.40)	-6.14 (6.08)	-1.75 (4.93)	-6.49 (5.07)
Observations	255	65	65	142	142
R-squared	0.513	0.668	0.677	0.619	0.666

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Indicator variables for age are included

e.g., If mother was interviewed when child was 13 months, age13=1, all others =0

All include full set of controls are as listed in Table 4

whether the father worked in the week before the first interview, have different effects depending on race. Prenatal substance use is associated with decreased likelihood that the child was late to his doctor's appointment, though the effects are only strong for Black women who smoked and White women who drank alcohol during pregnancy. In terms of father characteristics, whether the father has a drug problem increased the likelihood that Black women would take their children late to the doctor. These differing effects might be due to the reduced sample size, but it might also be attributable to women assigning confidence to different types of signals in assessing their future income streams and ability to pay for their children's health care.

Table 2.9 shows results using breastfeeding as an outcome variable. Here, a promise of support retains a significant effect on duration of breastfeeding only for White women (specification 3) and is associated with almost three months longer breastfeeding durations. White women who received the promise of support breastfed their children almost three months longer than White women who did not, conditioning on the full set of controls including father characteristics and current support agreements. Late payments, interestingly, lead to longer breastfeeding for Black women as did (weakly) the presence of a support agreement, while these had no effect for White women. The earlier promise of support was more predictive of breastfeeding duration for White women than current agreements, while the opposite appears to be true for Black women.

2.5 Conclusions

The unique makeup of the Fragile Families dataset gives researchers a very different picture of how expectations of income may affect decision-making and gives insight into survey design. While economic theory suggests that future income, such as the knowledge of a coming raise or inheritance, generally causes individuals to increase their spending now in order to smooth consumption over time, it seems that not all sources of future income are the same. In this study, I examine the relationship between expectations of child support and key investments in children, namely taking the child in for regular checkups and breastfeeding duration. While regular doctors' visits in the first two years are crucial to a child's development and well-being, primarily due to the administration of

vaccines and the ability of physicians to intervene in cases of abuse, it is also a decision that might be limited by financial constraints. Without the urgency of a crisis situation, we may see these visits put off by the uninsured or those with little ability to pay for health care. Breastfeeding, on the other hand, is less financially constraining and we may not expect to see an effect on breastfeeding duration of promises of support.

Descriptive statistics on the individuals who receive promises of support reveals that within race or class, the observable differences between women who were promised financial support and those who were not promise support, are minimal. While the average White, middle-class woman who receives a promise is noticeably different from the average Black, poor woman who receives a promise, she is not much different than the average White, middle-class woman who doesn't receive a promise of support. This leads me to separate regressions out by race.

My results show that a while a simple regression attaches little to no weight to a promise of support in the mother's decision to take the child to the doctor, separating out the regression by race leads to strong results for Black mothers in particular. Black mothers who did not receive a promise of support are more likely to be behind in doctor's visits, particularly being late for doctor's visits occurring between 12 and 18 months more often more than women who did receive a promise of support. When propensity score matching is employed, no effect is found.

Breastfeeding is another health investment that, though not particularly financially constraining, is also affected by a father's promise of support. OLS suggests that the effect is only significant for White women, but propensity score matching leads to some evidence that there is an effect for Black women as well. OLS results show that Black women are more swayed by current income streams as measured by actualization of child support, as opposed to a promise in the past of financial assistance. Interestingly, the financial constraint of buying formula may bind in Black women as they are more likely to breastfeed longer if the baby's father does not pay child support on time. Propensity score matching reinforces the separation by race, with White mothers most likely to receive the promise reporting significantly longer breastfeeding duration than White mothers who did not receive the promise, though the magnitude of the effect is smaller than in OLS. The effect

is weakly present for Black mothers, but highly dependent on the matching estimator form.

The ethnographic and sociology literatures on motherhood, childbearing, and financial support among unmarried couples inform us to be wary of attitudinal questions when comparing groups with different socioeconomic status. The differences shown here by race indicate that it may be that a question of promised support means something very different to those in different communities. If so, the use of questions about expectations should be carefully vetted and tested for cultural norms before inclusion in studies.

It does appear that a question of whether financial support has been promised is a weak indicator of how the mother will invest in the child. This study cannot identify the mechanism by which the effect takes place. It may be that cultural expectations dictate behavior or that a promise of support directly affects her expectations of income. More research is needed to distinguish between these effects.

It is clear, though, that controlling for characteristics of the person making the promise is important. The dependability or credibility of the person promising financial support likely affects the mother's estimation of the promise's worth, and thus her expectations of future income. Including these controls in regression analysis leads to highly different results in some cases, suggesting that there are multiple dimensions to expectations of future income.

For future surveys, it would be useful to include questions in follow-up interviews that directly address the earlier promise. Whether the mother thinks the promise had been fulfilled, or was being fulfilled, would also be helpful in identifying the effect and providing checks. Inclusion of questions about expectations of father involvement in survey analysis in hopes of determining the effect of income expectations on investments in children, current or future, may yet have some merit, but more analysis is needed to test the robustness of the claims. The addition of further controls and more information about the couples may contribute to a greater understanding of the mechanisms behind a promise of support exerting effort on investments in children.

Chapter 3

Evaluating Conventional Notions of Intimate Partner Violence

3.1 Introduction

Conventional wisdom about domestic violence says that batterers will be violent regardless of who their partner is, while victims, once they leave violent relationships, tend to seek out new relationships without violence. Using the National Survey on Families and Households (NSFH), I examine the characteristics and behaviors of women in violent relationships as well as their movement out of those violent relationships. When women report having ended a violent relationship and entered a new relationship, they tend to report that the new relationship is free of violence. Conventional wisdom about domestic violence says that violence can only end with the relationship's end. In contrast, I show that many men and women who report violence early on in their relationship do not report it later, even when in the same relationship. This is likely a story of heterogeneity of types and severity of violence being captured by survey data. It may be that relationships in which violence ends were experiencing the least severe forms of violence. Alternatively, survey data may capture more types and levels of violence in relationships than in other methods of reporting such as hospital admissions or crime statistics. Survey data, in an attempt not to alienate respondents, may conflate different types of violence or obscure the underpinnings of violence in relationships.

The NSFH is a nationally representative sample of American families with an oversample of underrepresented groups. In the survey, 16.9% of married, separated or cohabiting women surveyed report violence in their relationships, while only 10.0% of men do. I find that more than

half of those who report violence in the first wave—10% of all women and 6% of men—do not report violence in subsequent waves. This indicates that a significant portion of violent relationships end, either through ending the relationship or through stemming violence within the relationship. The idea that violence can be stemmed within a relationship is highly inconsistent with literature that explains intimate partner violence as part of a cycle of power and control. The nature of power and control is such that violent relationships are only posited to end with the destruction of the relationship itself, but the data here show otherwise. Thus, I seek a deeper understanding of these changes and their causes.

An advantage of survey data is extensive information on earnings and individual characteristics, information which is often lacking in hospital or police reports. Given this flexibility, I seek to determine whether income and earnings can affect changes in violence status within a relationship. I first show the relationship between earnings levels and violence in the NSFH, and then examine whether a change in those earnings is associated with a woman leaving violent relationships in accordance with theoretical and applied work in economics (Bowlus and Steitz 1999, Aizer 2010). I find a statistically insignificant effect of a change in income on violence, though it is in the expected direction. Among women who report violence in the first wave, those whose earnings increase more are more likely not to report violence in wave two, regardless of whether they are in the same relationship or not, though again, the point estimate is statistically indistinguishable from zero.

The survey highlights an important dichotomy in violent relationships that merits attention from the policy community. If all intimate partner violence is pathological and permanent, a greater emphasis should be placed on prevention. In turn, if, as these data suggest, some violence can be stemmed within a relationship, further research is necessary to identify factors that lead to a change in violence status. Current programs designed to prevent intimate partner violence do not tend to distinguish between types of violence or the level of control, but rather assume that control dictates most violent relationships. This paper suggests that much intimate partner violence is misunderstood.

3.2 Perceptions and Measures of Domestic Violence

There is a small but expanding literature in economics on domestic violence, very little of which has employed the NSFH. Cross-sectional analysis of the waves of NSFH by Fox, Benson, DeMaris and Van Wyk (2004) shows that factors like location, race and income are not significant predictors of violence. However, structural model estimates of Bowlus and Seitz (1999)—using alternate data—indicate that changes in certain variables, particularly income and employment do have a significant effect. Aizer (2010) tests the effect of changes in the gender wage gap using hospitalization data and exogenous increase in wages in female-specific occupations. A few papers model violence in relationships using an intra-household bargaining framework (Tauchen, Witte and Long, 2001; Bowlus and Seitz, 1999), ultimately suggesting that an increase in victims' assets or earnings should decrease incidence of violence.

Much of the literature on domestic violence uses reported crimes, either reported by the victim or by hospitals. While indicative of a certain type and level of violence, relying on police or hospital reports restricts the sample to severe injuries and instances of severe violence. In addition, these data impart a selection bias and adds to the perception of domestic violence as a pathology. Using hospital reports, Aizer (2010) shows that the rate of domestic violence at any given time is 2%; this paper shows a much larger group reporting in a single year. Some 17% of the sample of female respondents in relationships—5.8% when accounting for the oversampling—report violence in their relationships. Separating couples are more likely to experience violence, supporting the commonly made claim that leaving can be the most dangerous time for a victim. Though the direction of causality is not established here, the dominant narrative indicates that the act of leaving a violent relationship can incite additional violence (Zorza, 1993).

The literature on domestic violence in the field of sociology defines battering, victimhood, or both as individual pathologies, specifically focusing on instances of violent control in relationships. Whether or not battering is defined as a disease, the dominant narrative paints violence within relationships as a tool of power and control and as a situation that is relatively difficult to escape.

Batterers continue to batter regardless of the identity or individual characteristics of themselves or their victims, though victims are generally considered amenable to rehabilitation once they have left an abusive relationship. At least one study has worked at distinguishing this very specific type of violence from other types of violence, splitting intimate violence into four categories, many of which are likely reported under the guise of “violent arguments” in the sample discussed below (Johnson 2006, Card & Dahl 2009).

Johnson distinguishes between “situational family violence”, “intimate terrorism”, “violent resistance”, and “mutual violent control” and indicates that patterns of abuse will be different depending on the type of violence that dominates (2006).¹ Many papers, such as Aizer (2010), focus on intimate terrorism, intentionally or not, through their choice of definition of violence. In situations marked by intimate terrorism, or violent control, violence is perpetrated by one partner against the other for the purpose of bullying and control. Card and Dahl’s 2009 paper is based on the argument that types of violence as explored by Aizer may actually be situational family violence. Johnson contends that survey data, like that of NSFH used here, tends to illuminate situational family violence while surveys taken at women’s shelters and police records are more indicative of intimate terrorism or mutual violent control. This paper supports that view. This distinction would be useful in crafting certain victim assistance policies, and enhances the importance of following those who report any kind of violence through their relationships. The prevalence of violence in subsequent waves of NSFH and the gender gap in reporting as well as some information about the severity of violence allow me to make some salient observations on victim behavior.

In the spirit of testing some of the hypotheses laid out in other work and contributing to a greater understanding of the nature of changes in violence levels over time, this study examines changes in the self-reported earnings of individuals, uses self-reports of domestic violence from a large, randomly selected population over two waves of a survey, and tracks movement in and out

¹ Johnson (2006) splits violence within families into four categories. Intimate terrorism is the term used for violence perpetrated primarily against one partner by the other as a means of controlling the partner. Violent resistance is generally a response to intimate terrorism. Mutual violent control is marked by similar power struggles, but is perpetrated by both partners. Situational family violence is marked by violent arguments, but lacks the power and control aspect.

of violent relationships.

3.3 Data

The data used in this paper come from the University of Wisconsin's National Survey on Families and Households (NSFH). The three-wave, longitudinal study was intended to supplement and expand the abilities of a diverse group of researchers to examine families beyond what was possible with large-scale, national surveys such as the Current Population Survey (CPS) and Population Survey of Income Dynamics (PSID). Though these were broad in scope and covering large segments of the population, researchers felt they had reached the limit of new knowledge on family dynamics and transitions provided by these surveys. Thus, they sought to design a survey specifically to enable research on the family experience. The increased focus on the family and aspects of family life, perhaps at the cost of more detailed work histories, was considered a necessity in order to keep the survey manageable. The unique focus of the NSFH would be borrowed by future surveys, such as the Fragile Families and Child Wellbeing Data for which Sara McLanahan was also instrumental, but these future surveys lack the specific questions about family violence that I explore in this paper.

The NSFH consists of a representative sample of American families with an additional sample focused on minorities, singles and cohabiting couples. The data provide an extensive look at marriage and cohabitation histories, income, family dynamics, and family violence. It is one of the few large-scale, nationally representative surveys that examines questions of violence within relationships and within families, making it an especially suitable tool for this work. The original participants, aged 16 to 97 and numbering over 13,000 in 1987, were recontacted in 1992-1994 and, to a lesser degree, in 2002-2004.

The survey, though designed with future waves in mind, was initially only funded for one wave. Every effort was made to maintain contact with participants and response rates remain high for the second wave, but a dramatically different sampling design for the third waves makes analysis of transitions, as undertaken here, practically impossible in wave three. In contrast, the

second wave had an even greater scope than the first. While initial respondents and their partners were re-interviewed regardless of current status (married, separated, divorced or newly single), new spouses and partners were also interviewed.

Unlike the PSID and even the Fragile Families Data, NSFH has a section on family violence, including questions about violence in relationships with a spouse or partner as well as with children. I focus here only on questions pertaining to violence within intimate relationships, either continuing, or ones that ended or separated recently enough that both the respondent and partner were interviewed.

The survey designers were also careful to note that the survey design would shed further light on under-reporting of violence. They surmise that some characteristics and behaviors, such as marital conflict, may be underreported, but that the pattern of reporting should be distributed across individuals, as opposed to across couples. This increases the need to interview both individuals in a couple in order to examine the whole picture of violence.

The second wave of NSFH was conducted with the intent of recontacting all respondents. Between surveys, respondents were sent a postcard each year encouraging them to update their address with the researchers in order to reduce attrition due to migration. Survey documentation for wave two shows that three households were deleted in the recontacting phase due to fears of family violence as expressed by the primary respondent. It is unknown if any additional survey attrition can be attributed to violence, but it is possible, due to the nature of the survey, that some exists. Respondents were recontacted for the third wave only if they had a child eligible for an interview in the previous wave or if they were over 45 at the time of wave three. The reason for the sampling change appears to be financial in nature, but severely restricts the usefulness of the wave. The third wave's interviewing strategy eliminates many respondents who report domestic violence in earlier waves. Particularly, younger women were more likely to report violence in waves one and two, but wave three does not interview many younger women without children. For this reason, results from wave three are expected to be severely biased, with far fewer women reporting violence than we would expect if we had the full sample, and is not examined here.

3.4 Sample

In this paper, I am particularly interested in transitions between violent and non-violent states in relationships. Violence, once present in a relationship, is generally thought to persist until the relationship ends and often even after (Zorza, 1994). These generalities come as a result of analysis on very select samples, mostly case studies in the field of psychology or sociology, and little analysis has been done on nationally representative samples of families relating to violence. I posit that a nationally representative survey will highlight many different levels and kinds of violence and power in relationships—such as those described by Johnson (2006)—, thus painting a different picture than the one provided by crime statistics, hospital admissions, or shelter surveys, all of which focus on extreme and physically violent instances, likely marked by violent control.

For the first wave, I examine all respondents, female and male, reporting violence and not, to highlight any observable differences between groups on their report of violence. I restrict the sample at the outset to primary respondents under the age of 65 in 1987 who are in romantic relationships. This allows me to follow participants over time while limiting attrition in the sample due to death of either spouse during the intervening years.

For the wave over wave analysis, I restrict the sample to women who report violence in their relationships in the first wave. I follow these women whether they report violence or not in the second, regardless of relationship status. While there is likely additional information to be gleaned from women who move from no violence to violence from wave one to two, the sample is relatively small, and thus not considered here. Also, I examine those who drop out of the survey. Given the knowledge that at least three respondents chose not to continue the survey based on fears of family violence, it is necessary to examine whether those who discontinue their participation, for any reason, are substantially different from those who continue to participate.

3.4.1 A note about survey design and questions about violence

In each wave, each respondent who reports being in a romantic relationship answers questions about the nature of her relationship with her spouse or partner.² Following questions on division of duties and happiness, participants are then asked a series of questions about violence perpetrated by a partner or spouse and about violence perpetrated by the respondent against his or her partner or spouse. For the purposes of examining severity, I focus on the question concerning number of violent arguments. The questions are asked in succession about halfway through overall the interview. The questions as worded are available in Section 3.4.2.

The questions do not go on to determine whether these incidents were reported as crimes nor prompt respondents to identify the primary aggressor. In addition, the response to questions on the number of arguments is limited to four. If the respondent indicates that there were more than four arguments in the past year, it is coded identically as four. Interviewers were also instructed to inform respondents that such information would be confidential and thus not shared with the authorities. Ideally, this line of questioning in the context of a household survey would be less prone to underreporting than other surveys where domestic violence is reported to authorities or sources such as crime data. Indeed, I find that reports of violence in this survey are higher than those generally reported in crime statistics or imputed by the US Department of Justice.

Willingness to answer the questions declines as the line of questioning continues. About 700 women answered affirmatively to the question on whether arguments become physical in the first wave, but many more women choose not to answer subsequent questions about the severity and frequency of violence and attrition in the group reporting violence is higher than in the entire sample, leaving about 400 women for whom we have information about violence in both waves. Information on severity in both waves is even further limited. While it is tempting to use the later questions to examine severity and frequency of violence, much of the variation in these questions is likely a result of inherent differences in those who are willing to talk about it and those who are

² Separated respondents are asked a slightly different set of questions about violence and relationships quality. See questions in subsequent section

not. In addition, all instances are likely to suffer from severe underreporting.

3.4.2 The Questions

All respondents answer the following questions about violence:

1. There are various ways that couples deal with serious disagreements. When you have a serious disagreement with your partner, how often do you: Discuss it calmly; hit or throw things;
2. Sometimes arguments between partners become physical. During the last year has this happened in arguments between you and your partner?
3. Have you been cut, bruised or seriously injured in a fight with him/her ?
4. Has your partner been cut, bruised, or seriously injured in a fight with you?

The wording for separated and divorced respondents for the first two questions is slightly different:

1. In the last year (before your separation), how many times did you and your husband or wife argue?
2. Did those arguments ever become physical?

In addition, married and cohabiting respondents answer the following set of questions following question 2:

- 2a. During the past year, how many fights with your partner resulted in YOU hitting, shoving, or throwing things at him/her?
- 2b. During the past year, how many fights with your partner resulted in HIM/HER hitting, shoving, or throwing things at you?

3.5 Descriptive Statistics

In the first wave, there are about 3500 women in the age range 17-65, who are in relationships or currently separated and thus were asked questions about domestic violence. Table 3.1 compares these women to a similarly composed sample of 3009 men. Of these women, 593 (16.9%) reported some sort of physical argument and I have full information on earnings for 344 of them (Table 3.2).

Table 3.1 shows that the majority of respondents were in relationships: 76%, were married, 6% were separated, and 8% were cohabiting.³ Table 3.2 shows that women who reported violence were, on average, significantly younger than the general population, even accounting for those unmarried. They had an average age of 33.4 while the entire population of respondents had an average age of 37.5. Age is perhaps the only persistent source of observed variation in those who report violence, with younger respondents more likely to say that they had been in a violent argument. A common misconception about intimate partner violence is that it is a problem of the poor. I reexamine this story later in the paper, testing whether earnings and changes in earnings have an effect on the presence of violence or a change in violence status. I choose not to impute earnings for women who do not report earnings and thus work with a limited sample that reports earnings over the first two waves. Of 344 women reporting violence for whom I have full earnings information, 47.9% said that her husband or partner had caused her serious injury in one of these arguments (Table 3.2). Only 11% claimed that she had caused her partner serious injury. This smaller sample is the group for which there is some information on the severity of violence.

In the NSFH total sample, 17% of married, separated or cohabiting women surveyed report violence in their relationships, while only 10% of men do. The difference is statistically significant at the 99% level. Tables 3.3 and 3.4 show how women and men, respectively, change their reports in violence over time along with whether they change relationship status. By the second wave, about half of those who reported violence initially no longer report violence. Many, but not all, of these respondents had left their wave one relationships. Separated respondents report violence at a much higher rate than other respondents in relationships. Many respondents in the same relationship report a change in their answers to questions about relationship violence. Of 577 women reporting violence in the first wave that we are able to follow, 267, or 46.3%, report no violent arguments and no relationship change in the second wave. The fact that these women report no violence in a previously violent relationships is contrary to conventional notions of power and control in violent

³ Marital status as reported in the tables is independent of relationship status. A respondent who reports being divorced and in a relationship answers questions about violence for his/her current relationship, not with the former spouse.

Table 3.1: Characteristics of Primary Respondents Age 17-65 in Relationships

	Females	Males	t for difference
	Females	Males	
Respondent's Age	37.3	39	-6.09
Years of Education	13.1	13.2	-1.29
Marital Status			
% Separated	5.9	3	5.8
% Divorced	14.4	10.7	4.56
% Widowed	5.7	0.17	2.74
% Single	3.9	5.1	-2.36
% Married	75.6	81	-5.74
% Cohabiting	7.5	9.4	-2.82
Length Current Marriage**	23	28	-1.03
% White	11.8	13.5	-2.05
% Black	79.8	77.6	2.28
% Asian	5.0	4.9	-0.01
% Reporting Physical Argument	16.9	10.0	8.25
% Reporting Self seriously injured	9.3	2.5	12.06
% Reporting Spouse Seriously Injured	2.0	2.0	-0.03
Number of physical arguments where			
Female is aggressor	0.08	0.1	-1.46
Male is aggressor	0.08	0.08	0.01
Total	0.17	0.18	-0.79
Respondent's Earnings	16965	32140	-9.15
N	3515	3009	

Earnings are reported in 1986 US dollars

**Months, for married respondents only

Table 3.2: Wave 1 Characteristics of Primary Respondents Reporting Violence. Age 17-65, Sample for Earnings Analysis

	(1) Females*	(2) Females	(3) Males	t for diff (2 to 3)
Age	33.4	31.6	35.1	-2.45
Total Yearly Earnings	12232	16330	20567	-8.02
% Separated	13.0	7.6	4.6	4.56
% Divorced	31.3	17.7	18.2	4.44
% Married	50.4	69.4	67.1	-2.89
% Single	4.38	5.5	16.4	-4.87
% Cohabiting	8.13	4.3	16.4	-3.4
Age at First Marriage	19.8	19.8	22.4	-8.3
Length Current Marriage**	14.2	9.9	24.1	-0.92
Years of Education	12.8	12.9	13.1	-1.72
% Reporting Self seriously injured	54.4	47.9	13.6	9.58
% Reporting Spouse Seriously Injured	12.4	11.3	20.31	-2.90
Number of physical arguments where				
Male is aggressor	0.46	0.65	0.82	-0.02
Female is Agressor	0.45	0.65	0.98	-0.04
Total Number	0.90	1.3	1.79	-0.03
N	593	344	280	

Earnings are reported in 1986 US dollars

*Earnings information for (1) is incomplete in wave 2

(2) is group used for earnings analysis in Tables 5-6

**Months, for married respondents only

relationships. It is possible that there is some measurement error in these statistics and it is also possible that these individuals represent what Johnson (2000) calls situational family violence as opposed to violent control. About the same number, 203, report no relationship or a different relationship, and no violence in the second wave.

One potential explanation is that people age out of violence. Overwhelmingly, men and women who report violence in either wave are much younger than the average age of respondents. As shown in Table 3.3, linear probability analysis on the incidence of violence in the past year shows little statistically significant relationship between violence and race, socioeconomic status, education level, tenure of marriage or other salient factors. The first two specifications are on the entire sample of respondents in any relationship, controlling for relationship status in (2). Specification 3 is only on married respondents in order to include controls for age at first marriage and tenure of the current marriage, factors that may be associated with maturity or experience in relationships. In each specification, black women are slightly more likely to report violence and additional earnings translate to a small but statistically significant decrease in the probability of reporting violence. Among married respondents, the coefficient on age occasionally is statistically significant and it has a strong, negative effect on violence. It may be that age is proxying for some other factor, such as maturity or relationship experience. Alternatively, underreporting may be more severe in older women.

However, age is only a significant factor in predicting violence for married respondents and suggests that older, married women are less likely to report violence than younger, married women by about 2.5 percentage points per year (See Table 3.3). It is likely that nationally representative survey identifies some instances of violence that are not marked by violent control, and thus not subject to the same expectation of repetition and a cycle of violence. This explains why we see some respondents 'aging' out of violence. The lack of distinction in the survey between violence that is a result of violent control and violence that may be situational in nature, and perhaps temporary, means that we cannot distinguish between the types of violence in the survey. However, the results discussed above indicate that much of the violence recorded in the NSFH does not follow the typical

trajectory as predicted by relationships marked by violent control, so is likely some other type of violence: situation family violence or otherwise. Further research is necessary to identify factors that lead to a decrease in violence within a relationship keeping in mind the distinction among these types of violence and how violence progresses in a relationship.

Interestingly, currently separated respondents report much higher levels of violence in the past year than married or cohabiting respondents in both waves. It may be that these couples are separating as a result of violence. One conjecture is that separated or divorced respondents have less to fear in terms of retribution from their abuser and are more likely to report. An alternate explanation is provided by the sociology literature, where separation is depicted as an especially dangerous time for victims of intimate partner violence and is often marked by homicide (Zorza 1993). In cases of violent control, it may be that violence escalates as a result of the separation. This may work through channels such as the batterer feels he is losing control over his victim and lashes out or perhaps increased stress of separation incites more violence in all parties regardless of aggressor status. There is a plausible bidirectionality of the effects of violence on separation and separation on violence, which makes it difficult to distinguish causation.

3.5.1 Attrition

The problem of attrition is significant in any type of longitudinal analysis, and here it is of particular concern as the presence of family violence may actually cause attrition. If violence causes attrition, then analysis of transitional states will not be able to account for the most severe cases of violence. However, there are only three documented cases of attrition that can be directly attributed to family violence. Among the female respondents who drop out of the sample between the first and second waves, the report of violence is rather similar. Female respondents who are recontacted reported violence 16.5% of the time in wave 1, while those who are not recontacted report violence about 17.5% of the time in wave 1, but the difference is not statistically significant.

While the similarities between the two groups persist with regard to other questions of violence, those who are recontacted and those who drop out display some differences in other observ-

Table 3.3: OLS Results using Report of Physical Argument in Wave 1

	(1)	(2)	(3)
Black	0.0595*** (0.0227)	0.0393* (0.0222)	0.0999** (0.0444)
Hispanic	-0.00121 (0.0297)	0.000335 (0.0289)	0.0348 (0.0691)
Native_American	0.193 (0.130)	0.203 (0.127)	-0.0279 (0.226)
Asian	-0.0287 (0.0700)	-0.0116 (0.0680)	0.381 (0.259)
Years of Education	-0.00250 (0.00172)	-0.00201 (0.00167)	-0.00388 (0.00690)
Earnings	-0.00112*** (0.000424)	-0.000619 (0.000414)	-1.75e-05 (0.00110)
Woman's age	0.00399 (0.00424)	-0.00421 (0.00419)	-0.0255** (0.0107)
Woman's Age Squared	-0.000102* (5.25e-05)	-3.62e-07 (5.18e-05)	0.000195 (0.000129)
Married1		-0.256*** (0.0212)	
Cohabiting		-0.223*** (0.0319)	
Separated		-0.0241 (0.0365)	
Tenure Current Marriage			-0.000168 (0.000141)
Age First Marriage			0.000894 (0.00488)
Constant	0.212*** (0.0810)	0.565*** (0.0848)	0.984*** (0.220)
Observations	2,816	2,816	1,033
R-squared	0.023	0.082	0.049

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Earnings are in thousands of 1986 US dollars

able characteristics. Those who drop out are more likely to be black and less likely to be married. Among those married, those who dropped out had been in their marriages much longer than those who were successfully recontacted.

3.5.2 Violent Control and Severity of Violence

While women and men report violence at different rates when asked if they have gotten into a violent argument with their spouse or partner, the difference is quite small when compared to differences in reports of the severity of violence. Table 3.1 shows that 47.9% of women who report violence say that their partner caused them serious injury while causing serious injury to 11.3% of their partners. Conversely, men report being seriously injured at a rate of only 13% and having injured their partners at a rate of 20%. These statistics highlight the differences between men and women in reporting violence or perhaps in their understanding of the questions asked or in their experience of what constitutes violence.

For both men and women, we see significant reporting bias. Men are much less likely to report having been injured and more likely to report having injured his spouse, but not quite at the same rate as women report having been injured themselves. In the case of symmetry—where it is taken that both sexes understood the question in the same way or had the same tendency to underreport—, we would expect men to report being injured 11.3% of the time and having injured their spouses 47.9% of the time. Both sets of statistics point to men being the primary aggressors, but also indicate, perhaps, an unwillingness to report having injured another person by both men and women. Alternatively, either sex could be downplaying their involvement in the arguments and exaggerating their partner's in an attempt to position themselves as the victim. Also, respondents may understand violence differently, so underreporting by men suggests that they may not see these behaviors as violent.

3.5.3 Transitions

Over two waves, 577 women answered questions about physical violence in their relationship in the first wave and were eligible and available to answer questions about violence in the second wave. That is, they were successfully recontacted. Some 163 of those (28.2%) did not answer questions about violence in the second wave, even though 46 of them would have been asked the questions as they were in some sort of relationship. Half of the 577 remained with their wave one partner or spouse, and half were in a new relationship.

Of the 414 women who did answer questions about violence, 267 of them reported being in the same relationship, but did not report violence in the past year. In other words, 46% of all women reporting violence in wave 1 appear to have stemmed violence within their relationship. They represent 82% of all women reporting no violence in wave two given reporting violence in wave one. Table 3.4 shows that the majority of women reporting any type of violence remain in the same relationship, but no longer report violence. A small percentage (6.9% of all reporting DV in wave one, or 40 women) continue to report violence and are in the same relationship. Even fewer (3.2% or 19 women) report being in the same relationship and continue to report violence; few report violence in a new relationship. These results show that women are likely to either stem violence within their relationship or leave it for another violent relationship.

Table 3.5 shows similar results for men. Contrary to the notion that men who commit acts of domestic violence will do so regardless of whom they are in a relationships with, men show similar patterns of changing reports of violence over time. 52% of men reporting violence in the first wave report being in the same relationship but do not report violence by the second wave. A small minority, only 1.3%, report being in a different relationship and still reporting physical arguments.

In addition, the nature of the question allows me to more closely examine the severity of violence in relationships. Women who report a serious injury to themselves in wave 1 represent about 47.9% of respondents reporting any type of violence. In Wave 2, they report similar types of changes in violence and relationship status as all those reporting violence. However, they have

Table 3.4: Relationship status versus DV status in Wave 2, from women who report any violence in Wave 1. 577 women reported violence in wave 1. % represents percent of women reporting violence in wave 1

Status/DV	DV	No DV	No response
Same Rel.	40	267	21
%	6.9	46.3	3.7
Different Rel.	19	30	25
%	3.2	5.2	4.3
No Rel.	25	28	127
%	4.3	4.6	22.0
Separated	18	15	13
%	3.1	2.6	2.3
Total	88	326	163

Table 3.5: Relationship status versus DV status in Wave 2, from men who report any violence in Wave 1. 132 Total men reported violence in Wave 1. % represents percent of men reporting violence in wave 1.

Status/DV	DV	No DV	No response
Same Rel.	26	121	2
%	11.2	52.2	0.9
Different Rel.	3	19	7
%	1.3	8.2	0.3
No Rel	1	8	28
%	0.4	3.5	12.1
Separated	4	8	3
%	1.7	3.5	1.3
Total	36	156	40

a much higher non-response rate for questions about relationship violence in the subsequent wave. Tables 3.6 and 3.7 show similar results for the smaller groups of men and women who report being seriously injured by their spouses. Even in the case of severe injury, most respondents do not report violence in the second wave, although a large portion of respondents do not answer the question.

Table 3.6: Relationship status versus DV status in Wave 2, from women who report serious injury to self in Wave 1. % represents percent of women reporting serious injury in wave 1.

Status/DV	DV	No DV	No response
Same Rel.	17	136	12
%	5.2	58.6	3.7
Different Rel.	14	20	26
%	4.3	7.1	8.0
No Rel	20	8	78
%	6.2	2.5	24.1
Separated	17	8	6
%	5.2	2.5	1.9
Total	46	173	104

The small sample sizes make it difficult to draw conclusions, but it is not apparent from these results that men are more likely to find themselves in a new relationship or the same relationship with violence in the second wave following reports of violence in the first wave. This also runs contrary to a conventional understanding of violent relationships in which power and control are key. Men, generally the primary aggressors, are said to continue to perpetrate violence regardless of the relationship. Again, this highlights that the nature of violence recorded by survey data

3.6 Work on Changes in Earnings

A natural question that arises from the above statistics is why violence is not reported in subsequent waves by the majority of respondents. One plausible explanation is that couples and individuals “age” out of violence. However, the fact that we see a stemming of violence within relationships as well as respondents leaving violent relationships suggests that there may

Table 3.7: Relationship status versus DV status in Wave 2, from men who report serious injury to self in Wave 1. 52 men reported serious injury to self in Wave 1. % represents percent of men reporting serious injury in wave 1.

Status/DV	DV	No DV	No response
Same Rel.	9	28	0
%	15.3	47.5	0.0
Different Rel.	0	5	3
%	0.0	8.5	5.1
No Rel	0	8	9
%	1.8	14.2	15.2
Separated	2	2	0
%	3.3	3.3	0.0
Total	11	37	12

be something different about how partners are responding to changes in incentives and outside options. Work by Bowlus and Steitz (1998) and Aizer (2010) suggest that a change in earnings should prompt lower levels of violence due to the increased bargaining power of the victim. Other work on victims of violence suggests that financial concerns are paramount to remaining in or leaving a violent relationship, so we would expect that additional income would prompt a change in the report of violence whether it was within the relationship or by leaving it.

I estimate the following simple specification over the two waves on the sample who report violence in the first wave for whom I have full earnings information in order to evaluate the effects of changes in earnings as opposed to levels.

$$\Delta DV_i = \alpha + \beta_1 X_i + \epsilon_i \quad (3.1)$$

Where i is the individual, ΔDV_i is the change in report of violence. As I limit the sample to respondents who reported violence in the first wave, ΔDV_i takes a value of one if there is a change in the report of violence and a value of zero if there is no change in the report of violence. X is a vector of controls.

$$\Delta DV_i = \alpha + \psi_1 \Delta Earnings_i + \beta_1 X_i + \epsilon_i \quad (3.2)$$

$\Delta Earnings_i$ represents the change in earnings reported by a female respondent from wave 1 to wave 2. As the specification is essentially a differences-in-differences, I eliminate all controls that do not change over time.

3.6.1 Results

Table 3.8 shows results on the change in whether domestic violence is reported using a sample of women who report violence in the first wave for women who report complete earnings information over two waves. The outcome variable, ΔDV_i , takes a value of one if violence is only present in the first wave and zero if it is reported in both waves. It shows that there are small, insignificant

effects on a change in the report of violence in the expected direction. The first specification shows only baseline controls, while the second adds a change in earnings over the period. Regressions (3) and (4) add the time elapsed between interviews and the interaction between a woman's age and the time elapsed between interviews.

Table 3.8: OLS Results on a Change in Report of DV vs Women's Earnings

	(1)	(2)	(3)	(4)
Chg in Earnings		-0.0006 (0.001)	-0.0006 (0.001)	-0.0006 (0.001)
Woman's Age	-0.016 (0.0234)	-0.015 (0.024)	-0.020 (0.024)	-0.029 (0.035)
Age Squared	0.00011 (0.00028)	8.96e-05 (0.0003)	0.00014 (0.0003)	0.00013 (0.0003)
Time elapsed b/w interviews			-0.053 (0.044)	-0.097 (0.13)
Age by Time				0.0018 (0.0045)
Constant	-0.32 (0.472)	-0.33 (0.47)	0.062 (0.59)	0.37 (1.04)
Observations	344	344	344	344
R-squared	0.022	0.022	0.027	0.027

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

A \$1000 increase in earnings over the period spanning the two waves is associated with a increased probability of 0.06 percentage points of changing the report of violence, but cannot be distinguished from zero. These specifications are not very sensitive to assumptions about what constitutes a change in earnings and do not change significantly when other measures of time lapse are included. Coefficients on time elapsed between interviews and the interaction between age and time elapsed are insignificant.

The coefficient on age is insignificant and small in several specifications. Being one year older at the time of the first wave translates to a slightly higher probability (less than three percentage points) of reporting violence in the second wave, but the point estimate is indistinguishable from

zero in each instance. Each wave of interviews happened over a range of 1-2 years. The variable labeled “Time elapsed between interviews” changes signs depending on the specification, but is always insignificant. Individuals who had more time elapse since their last report of domestic violence might be expected to be less likely to report violence as they’ve had more time for their situation to change, but we see no effect. It may be that the time difference, on the order of months, is too small to record a measurable effect and if we had longer time lags, it would provide a clearer picture.

In addition, it is likely that these changes in earnings are endogenous. It is possible that the characteristics that garner a respondent more income are the same ones that encourage them to be more assertive, or more independent, or less accepting of abuse. Thus, I cannot establish a causal link between changes in reported earnings and changes in violence status. Nonetheless, the consistently negative results of similar magnitudes indicate that survey data may be appropriate for future analyses on the effect of changes in earnings on violence.

Table 3.9 shows a similar set of specifications with changes in the number of arguments reported from wave one to wave two. The analysis is conducted on a smaller sample that answered all questions about the number of physical arguments, but the results are consistent with those of the larger sample on the change in presence of any violence. A negative predicted outcome here translates to fewer arguments in the second wave and a positive one translates to more arguments in the second wave. The magnitude of the coefficient on earnings is larger; a change in a woman’s reported earnings of \$1000 is associated with .03 fewer arguments (with a maximum of eight being reported per wave), but all estimates are insignificant. An additional year of age is associated with more arguments, but at a decreasing rate and again in no specification is age significant.

Table 3.9: OLS Results on Change in number of arguments vs Change in Women's Earnings

	(1)	(2)	(3)	(4)
Chg in Earnings		-0.026 (0.023)	-0.025 (0.024)	-0.026 (0.024)
Woman's Age	0.26 (0.46)	0.31 (0.46)	0.30 (0.47)	0.14 (0.73)
Age Squared	-0.003 (0.006)	-0.0034 (0.006)	-0.0034 (0.006)	-0.0038 (0.006)
Time elapsed b/w interviews			0.12 (1.07)	-0.65 (2.06)
Age by Time				0.035 (0.093)
Constant	-5.22 (8.61)	-6.13 (8.54)	-6.77 (9.75)	-1.82 (17.27)
Observations	43	43	43	43
R-squared	0.014	0.041	0.041	0.045

Robust standard errors in parentheses

Age squared is included in all specifications

*** p<0.01, ** p<0.05, * p<0.1

3.7 Further Issues to Consider

3.7.1 Recanting

One primary concern may be that victims tend to recant their stories. Victims of violence often recant their accusations of violence against their partners, either in court proceedings or to family members or other confidantes. The pattern of recanting is well-documented, but generally reflects a dichotomy of situational fears. In the moment of abuse or battering, the victim wants to end the violence. Later, the fear of future violence or the fear of losing companionship and financial support offered by the batterer takes hold, causing the victim to reconsider her decision to report him (Walker 1979). Given that there are no legal repercussions for reporting violence in the context of the survey, I assume that the instance of recanting is low.

3.7.2 “Backlash”

Though much of the literature points to positive changes in earnings theoretically leading to decreased violence within relationship, there is some speculation that increased earnings could actually inflame the batterer and lead to increased violence as a way of exerting continued control over a victim who is on her way to becoming more independent. It may be that insignificance of the coefficients on changes in earnings are reflecting an averaging over couples where changes in earnings can have a positive effect, reducing violence, and a negative effect, increasing tendencies to violence. More information about the direction and nature of the domestic violence might assist here in separating those effects.

3.8 Conclusion

Domestic violence affects one in four married women in their lifetimes and is thought to kill over 14,000 women annually in the United States. Despite the clear severity of the problem, our understanding of its magnitude is still limited by differential reporting and the difficulty of attaining accurate data. The sources of variation are numerous and include differences in the nature of the

question asked and how it is perceived by the respondent, the interview pool, and perhaps most importantly, the stigma attached to reporting – felt both by batterers and victims.

A 1998 study by the Department of Justice showed that 2% of women per year experienced some sort of domestic violence (Tjaden and Thoennes 1998). The disconnect in rates reported by various agencies and studies is due in part to the issues present in monitoring and recording instance of violence. here, I show the number of abused women to be much higher, on the order of 6-17% in a single year. This indicates that violence in relationships is a much larger problem than thought; at the same time, the changes in relationship status and reports of violence over time also indicate that part of it may be self-correcting. While overall reports of violence do not change much over time, most individuals that report violence in 1986 do not report violence in 1992. It may be that individuals age out of violence, or that the conventional wisdom about intimate violence is only pertinent in the most extreme cases. Those who do are often reporting violence about a marriage or relationship that has since ended. Here, there is no evidence that a change in earnings affects the level of violence in a relationship.

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