Rethinking the Case Against Divorce

by

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Advisor: Lawrence L. Wu
In the memory of my grandmother —
Li Lin Chao-Shu (1917-2007).
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My parents moved out our three-generation household to form a “modern” conjugal family when I was two years old. They took my younger sister with them, but not me—as a compromise in the intergenerational tug of war in a culture/society where a grandson, rather than a granddaughter, still conferred uniquely significant meaning for grandparents. My grandfather died when I was four. My grandmother became the most important person in my life. She gave me unconditional love, and shaped my character. She kept absorbing new knowledge throughout her life, and her passion for knowledge was contagious. Hence, here I am, spending all my life asking questions and looking for answers.

Nonetheless, I have not taken those educational milestones in my life seriously because the most important thing for me is to learn, not to achieve. Thus, I have not attended any graduation ceremony since high school. However, when I finally realized that how much seeing her beloved grandson in academic attire would mean to my grandmother (as she kept saying that she would travel to New York as long as she could still get on an airplane), she passed away. Probably no other student has ever felt so empty, at least emotionally, as I do, holding a completed doctoral dissertation, although I do understand that she would want me to carry on the journey. I thank her for her love, for everything she has given to me, and for all the cherishable memories of her, with love.
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Abstract

In this dissertation, I reconsider the case against divorce (1) by examining the effects of divorce on the well-being of children and adults and on the cultural beliefs concerning family migration decisions in three empirical studies, and (2) by making two methodological contributions that help unravel the several puzzles in the empirical analyses of this dissertation.

In the first study, I examine children’s emotional well-being, measured by behavior problems. Using panel data from the mother-child sample of the National Longitudinal Survey of Youth 1979 (NLSY79) and fixed-effects and random-trends models to control for selection on unobservables, I find that there is no effect of divorce on behavior problems for children of divorce.

In the second study, I discuss methodological issues and describe a propensity score method for studying the effect of an event. I then apply this method in examining the effect of divorce on health using data from the adult sample of the NLSY79. I find that divorce has a negative effect on mental health for both divorced men and women. Divorce also has a negative effect on divorced women’s physical health and general health status, but no effect on divorced men’s physical health and general health status.

In the third study, I develop a “computerized multivariate factorial survey” vignette method for studying the interrelated sociopsychological processes.
I then apply this method in examining cultural beliefs concerning marriage prospects and family migration decisions. I probe what a convenience sample of respondents believe the probability of divorce for fictitious couples would be and what they believe the same fictitious couples would do when one spouse receives a job offer that requires moving to another city. Using simultaneous-equation models with correlated errors, I find that the respondents are more likely to believe that a fictitious couple would choose to live apart for work, if the respondents also believe that the same couple has a higher probability of divorcing within five years. I also find a gender asymmetry in respondents’ beliefs, with respondents seeing a fictitious couple as more likely to take a job offer and move when the husband, rather than the wife, receives the job offer.
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Chapter 1

Introduction

This dissertation deals with the case against divorce, a narrower set of issues than the case for marriage that has occupied the center stage of policy forum regarding family change (Berger and Berger 1983; McLanahan 2004; Popenoe 1996; Stacey 1996; Waite and Gallagher 2000). Much of the debate is concerned with the effects of marital status on well-being. Proponents of marriage argue that married people enjoy higher level of well-being because marriage provides the benefits that non-marriage does not. They observe that, on average, married people are healthier, happier, financially better off, enjoy more frequent and better sex, and their children fare better than their counterparts in divorced and other nonmarried families. They contend that these differences reflect the causal effect of marriage, and by implication the causal effect of divorce, on individual well-being. The opponents argue that the proponents of marriage fail to consider the diversity of individuals in different family arrangements. They focus instead on within-group variation than between-group variation, and contend that the majority of people in nonmarried families do well. They also argue that the benefits may not necessarily come from being married. Pivotal in this debate is the extent to which observed differences between the married
and the nonmarried can be *causally* attributed to marital status, and to what extent a focus on individual differences will alter the empirical picture and the policy prescriptions by the proponents of marriage, who seek to direct government investments and community resources to preventing the disruption of all marriages except in extremely dysfunctional, abusive conditions.

In this dissertation, I hope to improve the quality of this debate by building on the counterfactual framework for causal inference. In the Chapters 2 and 3 (and the methodological Chapter 4), I reexamine the causal claim that the well-being of adults and children would be higher if adults/parents avoid a divorce and remain married. I note that this causal claim conflates two potentially distinct effects—the potential harm to well-being due to divorce and the potential benefits to well-being due to marriage. Hence, I attempt to tease apart these two distinct effects and estimate the causal effect of divorce, net of the selection effect of marriage, because when divorce is considered a “treatment” of interest, marriage should be regarded as a confounding factor whose effect is to be partialled out.

I also hope to suggest and identify another possible source for the effect of divorce—namely, how the anticipation of marital instability, especially in a high-divorce society, might affect people’s cultural beliefs and how people make life decisions that are traditionally considered as a family decision, rather than individual decision. In Chapter 5, I first develop a new methodology within the factorial survey tradition for studying interrelated sociobehavioral beliefs and judgments, and apply this method to examining lay people’s beliefs concerning marriage prospects and this “forward-looking effect” of divorce in lay people’s beliefs concerning family migration decisions.
1.1 Counterfactuals of Marriage versus Divorce

The overarching claim of the proponents of marriage is that marriage provides more benefits for economic, physical, and psychological well-being than do all its alternatives and, thus, is a superior form of family arrangement compared to singlehood, cohabitation, divorce, and widowhood. The social science research supporting or contesting this claim focuses on whether theoretical reasoning and empirical evidence can make “the case for marriage” (see the summary of literature in Waite and Gallagher 2000). In this dissertation, I do not address the full spectrum of “the case for marriage” but focus on “the case against divorce.” I pay particular attention to the causal claim that marriage benefits, while divorce hurts, the well-being of adults and children. Establishing causality is essential because commentators of both sides seek to influence public policy and devise interventions to improve human welfare. No policy intervention will be effective unless the proposed intervention has a causal effect on the outcome. A careful examination of causality is especially critical because self selection of people in “bad” marriages into divorce is highly likely but the data are seldom analyzed in a way that takes the selection problem seriously. The focus on “the case against divorce” helps pin down causality in light of recent developments in quantitative methodology, which requires clear specification of the counterfactuals for causal inference (Heckman 2006; Holland 1986; Sobel 1995), by restricting the comparison to a pair of clearly defined counterfactuals of marriage and divorce, stripping off additional counterfactuals of marriage and singlehood, marriage and cohabitation, and marriage and widowhood. The focus on “the case against divorce” also helps illustrate my major critique of this literature, which is the effect of marriage is often confounded with the effect of divorce.
In other words, while theoretical arguments in the literature suggest divorce is considered harmful to individual well-being both for being a detrimental event itself and for removing the protection of marriage, empirical analysis rarely tries to tease apart these two effects. This has important implications for the debate over the case against divorce. To attribute the association between divorce and well-being as the causal effect of divorce, the effect of marriage should be considered as selection bias and thus controlled. Since the adults and children in divorce also tend to have experienced a marriage that provides them with fewer benefits than their counterparts remaining married, they might suffer from a double jeopardy in their family life. For policy purposes, it is necessary to devise specific interventions that target uniquely at either marriage or divorce, depending on whether the apparent worse well-being for adults and children in divorce comes from marriage or from divorce.

1.2 Institutionalized Divorce?

Over half a century ago, Goode (1956) commented on the importance of divorce by arguing that theoretically divorce provides a solution to marital conflicts that is not yet “institutionalized”, and observed empirically that the divorce rate was steadily rising in all industrialized societies (see also, Goode 1993). Half a century later, divorce is almost already an “institutionalized” life event, with about half of the newly weds being expected to end their marriage before a spouse dies (Castro Martin and Bumpass 1989) and most states in the United States having no-fault divorce laws. A substantial proportion of children are now

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1In Li (2006), I documented substantial similarities in childbearing behaviors in stepfamilies and in intact families, which I interpreted as another aspect of the institutionalization of nontraditional families.
growing up in nontraditional families (Bumpass 1984; Bumpass and Rindfuss 1979). Therefore, many people in the modern society not only observe marital disruptions around them, but themselves have personal experiences about what it is like to be a member of a broken home.

The high prevalence of divorce and the realization that “’til death do us part” has become a myth may have an impact on cultural beliefs especially with respect to making those decisions traditionally considered as family-based (Bumpass 1990). Job-related migration, examined in Chapter 5, is such an example. The standard economic model by Mincer (1978) builds essentially upon the assumption that spouses maximize their joint well-being in deciding whether to move to another place. To what extent this is still true in a society where half of the people are projected to divorce and more than a third of the individuals grow up in non-traditional families is unclear. Nevertheless, it is difficult to study this kind of “cultural” effect of divorce because typically we can only observe their manifestation at the behavioral level, and infer their cultural values and beliefs at a deeper level. The factorial survey method (Rossi 1955; Jasso 2006) provides one way to look into the cultural beliefs concerning marriage prospect and family decisions.

1.3 Organization

Chapter 2 examines the average treatment effect of parental divorce on behavior problems for children of divorce. The focus is on children’s emotional well-being. For those who are interested the most recent research concerning the effect of marital status on children’s cognitive ability, please refer to Brown and Flinn (2005) and Tartari (2005). Chapter 3 examines the average treatment effect
of divorce on health for the divorced adults. Chapter 4 describes a propensity score method for studying the effect of an event, and discusses related methodological issues in such dynamic settings. These chapters employ theoretical and methodological thinking as well as advanced statistical models developed in the counterfactual causality framework to examine the causal claims concerning divorce and the well-being for adults and children of divorce.

Chapter 5 develops a computerized multivariate factorial survey method for studying interrelated social psychological processes and presents preliminary substantive results on beliefs concerning marriage prospect and family migration decisions. The methodological component of this chapter builds on recent developments in the factorial survey methods (Ganong and Coleman 2006; Jasso 2006) pioneered by Peter Rossi (Rossi 1955). It both moves this method into the era of modern computerized survey technology and expands its capacity to studying multiple sociopsychological mechanisms using advanced multilevel, multiprocess statistical techniques. The substantive findings fall beyond the predictions of the standard economic models of family migration.
Chapter 2
The Kids Are OK: Divorce and Children’s Behavior Problems

2.1 Introduction

For most people, the really pressing question they are likely to wrestle with at some point in their lives is . . . “Should I stay married, maybe even for the sake of the kids?” This heartfelt debate is central to the future of marriage as a permanent commitment. And it is a debate that is taking place, not only among people but within people.—Waite and Gallagher (2000)

To speak of fostering an emotional democracy does not mean being weak about family duties, or about public policy towards the family. Democracy means the acceptance of obligations, as well as rights sanctioned in law. The protection of children has to be the primary feature of legislation and public policy. Parents should be legally obliged to provide for their children until adulthood, no matter what living arrangement they enter into.—Giddens (2003)

Divorce and child well-being is a highly contentious social science question that
interests the general public and policy makers alike. Commentators across the ideological and political spectrums have all agreed that parents should be responsible for their child’s well-being. Hence, they should take into account their child’s well-being when contemplating a family decision, among which divorce is perhaps the most serious (Giddens 2003; Waite and Gallagher 2000). Yet these views implicitly suppose that (changes in) family structure is a cause of the differentials in child well-being, but if there were in fact no causal link between divorce and child well-being, there would be no scientific basis for public policy concern or social intervention.

Indeed, this belief that family structure plays a causal role in determining child well-being has been well articulated in the sociological literature, as manifested in the three recent presidential addresses delivered by sociologists to the Population Association of America. Waite (1995) argued that, on the average, marriage provides numerous benefits for both adults and children that alternative family arrangements do not. Her argument has been taken as providing a scientific foundation for supporters of policies intended to promote marriage. Cherlin (1999) reviewed similar issues with a narrower focus on children and concluded that, while the observed correlations cannot be the entire story, family structure matters for child well-being. McLanahan (2004) detailed how family changes impacting different social strata differently might be responsible for the diverging trend in social inequality for children from different backgrounds. Perhaps not surprisingly, the position of these eminent scholars represents where the current empirical literature stands. Although many social scientists suspect that selection plays a role behind the correlation between family structure and child well-being, few studies to date have convincingly challenged the belief in the causal claim that children would fare better should
their parents stay/become married. Indeed, some even argue that social scientists will forever debate about the causation between family structure and child well-being until a randomized experiment can be conducted (McLanahan and Sandefur 1994:11)—which ethical concerns will never allow to happen.

Although causation is best established using randomized experiments, I argue that the evidence in this chapter seriously questions the consensus that divorce and child well-being are causally linked by explicitly and rigorously modeling unobserved factors that may differentially select families into divorce. I reexamine the causal claim that divorce is detrimental to children’s emotional well-being (measured in terms of overall behavior problem), using multiwave panel data to eliminate selection biases due to unobserved confounding factors, an issue not adequately addressed in prior research. I discuss two distinct sets of theoretical mechanisms through which the selection may operate, and adjust for selection in two ways, by using the fixed-effects models and generalizations of such fixed-effects models that incorporate individual-specific linear trends. I also use longitudinal data with the largest sample sizes than most research to date; thus, the analysis in this chapter will have reasonable statistical power to detect effects of divorce even under a fixed-effects or individual-specific linear trend specification.

Once selection on unobservables is controlled, I find no effect of divorce on children’s behavior problems. This null finding suggests that divorce itself is not the culprit for the lower emotional well-being for children of divorce. Indeed, these findings suggest that to help children of divorce, social scientists and policy makers should seek to understand the process both before and after marriages come apart (Furstenberg and Cherlin 1991) and target interventions on proximate determinants of socioemotional development, rather than attempting
to prevent divorce itself.

2.2 Literature Review

Social scientists have consistently found that children of divorce have, on average, lower well-being than children in two-parent families after adjusting for socioeconomic background and other demographic factors (Amato 2000; McLanahan and Sandefur 1994; Seltzer 1994). This finding, however, is taken by many as evidence that divorce causes reductions in children’s well-being, especially among social scientists and commentators attempting to translate these findings for non-scholarly audiences (Popenoe 1996; Marquardt 2005), despite repeated warnings about the difference between association and causation. Thus, public controversies concerning divorce and child well-being typically reflects a disagreement (often implicit) over whether divorce causes observed differences in well-being between children of divorce and children in two biological parent families.

Early research on the effect of divorce on children has largely been based on clinical samples, sometimes even lacking a control group. These findings lack external validity and are thus difficult to generalize. Nonetheless, two early longitudinal studies based on unrepresentative samples offered important insights on potential selection issues by finding that many children of divorce had already been having various emotional disturbances, adjustment problems, and substance abuse prior to the disruption of their parents’ marriages (Block et al. 1986; Doherty and Needle 1991).

In a seminal paper, Cherlin and colleagues (1991) highlighted the importance of a prospective design. They noted two potential sources of selection
that had been overlooked in previous empirical work: marital conflicts and family dysfunction. Their findings yielded smaller effects of parental divorce after controlling for pre-disruption family conditions and pre-disruption measure of child well-being, suggesting that previous estimates of the effects of divorce were upwardly biased by not adequately addressing these selection issues. Their study thus suggests that, despite the robust and relatively strong association between divorce and child well-being documented in various cross-sectional studies, there is far less empirical evidence in support of a causal role of divorce, given the relative lack of studies using longitudinal data and methods that would allow researchers to address these selection issues (Ni Bhrolchain 2001).

The findings by Cherlin et al. (1991) have led many social scientists to conclude that parental divorce is causally linked to reduced children’s emotional well-being, but that the degree of harm is less than that indicated in the associations found in cross-sectional studies. Nonetheless, a detailed examination of the literature since Cherlin et al. (1991) suggests that this conclusion is more inconclusive than is sometimes acknowledged. Cherlin et al. (1991) followed an entire cohort of British 7 year-olds until they were 11 years old in the National Child Development Study, controlling for social class, race, scores of the same outcome measured at age 7, health visitor’s report of family problems and difficulties, physician’s report of physical handicap, mental retardation, or emotional maladjustment measured at age 7. They also presented a parallel analysis examining only children’s behavior problems using data from the U.S. National Survey of Children (NSC), with the respondents being slightly older than the British (at 7-11 years old at base survey) interviewed in 1976 and followed up in 1981. The effect of parental divorce for boys dropped by about a half and
no longer statistically significant from the model controlling for social class, race, whether or not mother was employed outside the home, to a model adding child’s behavior problem in 1976 as well as parental marital conflicts. However, they noticed an eccentric pattern for girls, with girls of divorced parents showing somewhat fewer behavior problems than girls of continuously married parents, and they were cautious about this unusual finding. In contrast, Baydar (1988), also analyzing the same NSC data but using an advanced dynamic statistical model described in Tuma and Hannan (1984), found entering a stepfamily after divorce, but not parental divorce itself, reduces certain aspects of children’s emotional well-being in a five-year window between 1976 and 1981. Morrison and Cherlin (1995) focused on a two-year window between 1986 and 1988 in the Children of National Longitudinal Survey of Youth 1979. They found that controlling for pre-disruption child outcomes drove down the coefficient of marital disruption to zero for girls (although the initial level of difference was not statistically significant), but did not change the coefficient for boys. The effect of parental divorce on boys’ behavior problems was partly explained by the decline in economic resources. Morrison and Coiro (1999) examined to what extent the effect of divorce interacts with parents’ marital conflicts, using child and parent data from the 1988-1994 waves of NLSY79. They found that, holding constant pre-disruption level of behavior problems, marital conflict and marital disruption both increased children’s behavior problems. Moreover, their results suggested that children with high-conflict parents who remained married to each other had the highest level of behavior problems. Jekielek (1998), using the 1988-1992 wave of the same data as Morrison and Coiro (1999), found that children in high-conflict families have lower levels of anxiety and depression if their parents divorced, which is inconsistent with the findings reported
in Morrison and Coiro (1999). Sun (2001) analyzed data from the 1990-1992 waves of National Education Longitudinal Study (NELS) and found a small, but statistically significant, effect of divorce on adolescents’ self-reported behavior problems after controlling for pre-disruption behavior problems. In addition, he finds no association between divorce and teacher-reported behavior problems or adolescent substance use after controlling for pre-disruption measures of family relations and parental characteristics. Painter and Levine (2000) examined the effect of parental divorce during a child’s high-school years on a white, non-Hispanic subsample in the 1988 NELS, which followed a sample of 8th graders through 1994. They found that, at the base 1988 survey, children whose parents were to divorce in the next few years had exhibited higher level of emotional and behavior problems than had children whose parents were continuously married during subsequent waves. However, they did not find significant differences in family economic conditions, parents’ education, and parenting behaviors between the two groups of children prior to divorce. Controlling for the above pre-disruption characteristics slightly reduces the association between parental divorce and high-school dropout, and substantially reduces the associated between parental divorce and out-of-wedlock childbearing. Furstenberg and Teitler (1994) found that children of divorce had lower education, economic well-being and psychological well-being, and controlling for pre-disruption factors—including child characteristics, family background, quality of parents’ marital relations, and parent-child relations—substantially reduces the association between divorce and subsequent child well-being.

Although longitudinal data allow researchers to control for pre-disruption child outcomes, many studies using longitudinal data have not exploited this opportunity. Although these studies provide useful descriptions that help unravel
the complex family transitions (Carlson and Corcoran 2001) and the process of
divorce (Lansford et al. 2006; Strohschein 2005; Sun and Li 2002; VanderValk
et al. 2005; Wu et al. 2006), their findings provide no rigorous evidence for any
causal link between divorce and child well-being.

In sum, several prior longitudinal studies have relied on non-representative,
clinical samples (Block et al. 1986; Doherty and Needle 1991; Forehand et al.
1997) so that their results may not generalize. Among studies using data col-
lected from a probability sample, many have not controlled pre-disruption child
outcomes and thus have not exploited the advantage of panel data in making
causal inference (Allison and Furstenberg 1989; Carlson and Corcoran 2001;
Peris and Emery 2004). Even studies using longitudinal data have used mul-
tiwave panel data for descriptive purposes or have only analyzed two waves
of data in their panel design with little attention to selection issues. Several
studies examining marital conflict often obtain different findings despite using
the same data source (Jekielek 1998; Morrison and Coiro 1999). Finally, the
handful of studies that might be thought to provide evidence for a “causal”
effect of parental divorce have sometimes yielded conflicting findings and have
analyzed narrow age ranges for children, short periods during which divorce can
occur, or an over-representation of young children born to young parents from
disadvantaged socioeconomic circumstances (Baydar 1988; Cherlin et al. 1991;
Morrison and Cherlin 1995; Sun 2001).
2.3 Theory

2.3.1 Causal Mechanisms of Parental Divorce

There are a variety of theoretical mechanisms by which marital disruption might lower children’s well-being. One such mechanism focuses on parental resources, with divorced parents less able, on average, to provide sufficient resources to fulfill children’s social, economic, and emotional needs than two biological-parent families (Seltzer 1994). Divorce often entails moving to a different location, transferring to a new school, and adopting an unfamiliar life routine, all of which may diminish the well-being of children following divorce (McLanahan and Sandefur 1994). Children’s adjustment can also be affected by stressful events as the custodial parent copes with singlehood, resumes dating, moves in with a new partner, and remarries (often to another person with children). Family change and instability may be another source of stress, which may in turn be causally linked to problem behaviors for children of divorce (Hao and Xie 2002; Wu and Martinson 1993; Wu and Thomson 2001).

Divorced families often experience a substantial and sudden decline in economic circumstances (Peterson 1996; Weitzman 1985), which McLanahan and Sandefur (1994) have argued is responsible for about half of the disadvantage for children of divorce. The difficulty of making ends meet, compounded by frustrations with delayed or missing child support payments, imposes additional burden on divorced mothers. In addition, mothers’ emotional reactions towards economic difficulties may be transmitted to children passively through parent-child interactions and actively through the child’s social learning.

Psychologically, the sudden departure of a parent may affect a child’s sense of security and sense of controllability of the environment. The behavioral
manifestation of psychological harm may be seen in their distress, anxiety, social withdrawal, irritability, and frustration (Wallerstein and Blakeslee 2003). The divorced parents’ inability to fulfill the children’s emotional needs may be exacerbated by the stress associated with protracted inter-parental conflicts (Cherlin et al. 1991). Separated parents may continue to fight over the division of property, child custody, and various other matters not only during separation but sometimes long after a divorce is finalized (Furstenberg and Cherlin 1991). Parents in conflict also can serve as a negative role model from which children learn to express their emotions in an inappropriate way, which in turn exacerbate behavior problems in children of divorce (Grych and Fincham 1990).

2.3.2 Selection Mechanisms

It is worth noting that the resources that parents are able and willing to provide for their children may vary drastically across marriages and across divorces. There are “good” parents and “bad” parents, just like there are “good” spouses and “bad” spouses. It is plausible that a “bad” spouse or parent may well have been a “bad” spouse or parent prior to marital disruption (and may thus have been a factor in causing the disruption), with the alternative hypothesis—that a spouse or parent turned “bad” after the disruption—being perhaps less plausible. If so, this is one mechanism by which presumed “consequence” of marital disruption may potentially be a precursor to it (Cherlin et al. 1991). Although divorced families, on average, are disadvantaged on various social and emotional measures relative to intact families, many of these disadvantages might well have been present had the parents remained married. If these disadvantages (e.g., parental conflict, family dysfunction, disengaged parenting) would have been present irrespective of the parent’s legal status or the presence or absence of
a parent, child well-being may not differ were the parents to have remained married or were they to have divorced. In other words, a causal claim regarding the effect of parental divorce on child well-being can only fare better were their parents to have remained married. That is, theoretically the dynamics of family circumstances if the parents—who are observed to divorce—had remained married. Hence, for divorce to have a causal effect on child well-being, some causal factor that accompanies divorce must change before and after divorce and the change must take place in a way that causes child well-being to deviate from its pre-divorce trajectory. Conversely, if there is no change in a causal factor affecting child well-being, observed differences in well-being for children of divorce will reflect the selection of families of divorce on these factors, which I will refer to as “static selection on time-invariant factors.” Furthermore, even were a causal factor to change pre- and post-divorce but if this change does not alter the child’s pre-disruption trajectory of well-being, the change in this causal factor will represent what I will refer to as “dynamic selection on time-varying factors.” I give several examples below to illustrate these two possible selection mechanisms.

**Static Selection on Time-Invariant Factors**

“Static selection” will occur if there exists an unobserved factor that remains unchanged before and after divorce but is associated with a higher risk of divorce. One possible static selection mechanism is through the social inheritance or genetic transmission of personality traits (Freese et al. 2003). For example, certain aspects of child temperament are relatively stable across development-

---

1 Note that economic resources typically do decline following a divorce; hence, child well-being would decline after divorce if the effect of declining economic resources on children’s behavior problems outweighed the effects of socioemotional factors.
tal stages and associated with parental personality traits possibly via heredity (van den Oord and Rowe 1997). If a child with a difficult temperament is also likely to have parents with divorce-prone personality traits (Jockin et al. 1996), the child will both be exposed to a higher risk of parental divorce and behavior problems. If so, a naive estimate of the effect of divorce that ignores this type of selection will be upwardly biased.

While a decline in income often is a consequence of divorce (Peterson 1996; Weitzman 1985), persistent poverty is likely to increase the risk of marital disruption. Similar stress mechanisms that operate in low-income divorced families may operate in low-income married families, and cause higher level of children’s problem behaviors through long-term elevated level of inter-parental conflicts. Low income families also tend to live in poor neighborhood with relatively low quality schools. Poor neighborhood conditions or poor school quality may cause the children to develop higher levels of behavior problems (Harding 2003). If low income, high divorce rates, and high levels of children’s behavior problems were to covary in these ways, a naive estimate of the effect of divorce that ignores selection on economic conditions prior to divorce is likely to be biased upwards.

The intensity and frequency of inter-parental conflict has been repeatedly cited as a key risk factor that may impair the socioemotional development of children (see, e.g., the review of studies by Grych and Fincham 1990). If high-conflict marriages have a higher propensity to divorce, and high levels of marital conflict cause children to develop behavior problems, these factors will also be sources of selection bias.

Because the majority of divorces stem from low-conflict marriages (Booth and Amato 2001), one might question the importance of selection on marital
conflict. However, divorce-prone low-conflict marriages may be harmful for children’s emotional well-being if parents in such marriages are disengaged. Loveless and/or disengaged parents, even in a materially affluent and low conflict household, may still cause problems in children’s socioemotional development. Suggestive evidence along these lines can be traced to the famous Harlow experiments of nearly a half century ago (Harlow 1958; 1959), in which infant monkeys deprived of parental warmth had severe developmental deficits, even though adequately fed. These results and a subsequent body of research have led developmental psychologists to believe that adequate socialization requires engaged parenting. Nevertheless, there is substantial variation empirically in father’s time and activities with his children both among intact families (Harris et al. 1998; Yeung et al. 2001) and after marital disruption (King 1994; King et al. 2004). This suggests that marital disruption is not the mechanism that may diminish the degree of a father’s care and attention to his children. If loveless and disengaged parenting reflects the loss of interest of a parent in the marriage, failing to take into account the selection on parental involvement will again upwardly bias naive estimates of the effect of divorce on children’s emotional well-being.

**Dynamic Selection on Time-Varying Factors**

As noted above, I refer to “dynamic selection on time-varying factors” when a behavioral factor changes over time but the outcome of interest remains on its previous trajectory. Dynamic selection may occur either through the contemporaneous effect of a time-varying factor or through the cumulative effect of a time-invariant factor. Consider a first scenario in which the level of marital conflict is associated with the likelihood of divorce and in which we observe
marital conflict increases as marriage condition worsens. Increasing level of inter-parental conflict may have a contemporaneous effect, increasing levels of problem behaviors (e.g., aggression or depression) of children through mechanisms of observational learning (Bandura 1977) or lack of parental discipline. This scenario, although similar to the static selection on conflict, will lead to a trending, rather than time-constant, effect of selection. Simply holding constant a fixed level (or the intercept) will not properly eliminate the biases due to the dynamic selection.

Consider a second scenario, in which marital conflict remains at a constant level but where the influence of marital conflict on children’s problem behaviors is cumulative over time, which again will yield a trending effect of selection. Children may also develop behavior problems gradually because of the loveless parent-child relationship and the lack of an appropriate role model of parent, with behavior problems diverging over time for children in high-conflict, loveless families and for children in low-conflict, loving families will diverge over time if the effect of marital conflict is cumulative. This scenario will also lead to a dynamic selection effect, which implies a model controlling only for static selection mechanisms will be misspecified, with the standard fixed-effects yielding an upwardly biased estimate for the effect of divorce.

Virtually all previous studies have assumed that the selection mechanisms have a static, time-invariant effect on children’s emotional well-being, and under such an assumption, when the pre-disruption child outcome is sampled is irrelevant. However, if the dynamic selection is present, controlling only for static selection will yield biased and inconsistent estimates; hence, it will be necessary to specify a model that accommodates dynamic selection.
2.4 Statistical Models and Hypotheses

2.4.1 Standard Regression Adjustment

I begin the analysis of this chapter using a standard OLS regression estimates to replicate results reported in prior research, in which researchers have found that divorce is strongly associated with children’s behavior problems. The variables in this replication include nearly all the variables that have been used in the previous research.

Following the cross-sectional designs of much previous work, I do not exploit any longitudinal feature of the panel design in this first replication attempt. Hence, this replication takes the form of pooled cross-sectional data with “standard regression adjustment”—or the “analysis of covariance” (Winship and Morgan 1999)—estimated by ordinary least-squares (OLS) techniques. Formally, the model can be written as follows:

\[ y_{it} = \beta \cdot x_{it} + \theta_i \cdot D_{it} + \epsilon_{it} \]

where \( y_{it} \) is the measure of children’s emotional well-being (specifically, behavior problem index, in this case), \( D_{it} \) is a time-varying dummy indicator for parental separation/divorce (whichever comes first), and \( x_{it} \) is the vector of socioeconomic and demographic control variables. This analysis, as those reported in previous studies, addresses the question whether children of divorce fare emotionally worse off than children whose parents are continuously married—coming from similar socioeconomic and demographic characteristics. If the sample characteristics and the distributions of the independent variables, dependent variable, and control variables in this replication are similar to those previous studies on divorce and children’s behavior problems, I should find co-
efficients of comparable magnitudes.

2.4.2 Fixed-Effects Model

In Model 2.1 above, the coefficient for parental divorce $E(\theta_i)$ can be interpreted causally only if the underlying assumptions hold. However, if static selection on time-invariant unobservables (e.g., child temperament, persistent poverty, etc.; see Section 2.3.2) is present, then Model 2.1 will be subject to omitted variable bias and will yield an inconsistent estimate of the effect of divorce.

To formalize the argument, consider the specification of a fixed-effects model that incorporates a child-specific, time-invariant unobserved component, $c_i$:

$$y_{it} = \beta \cdot x_{it} + \theta_i \cdot D_{it} + c_i + \nu_{it} \quad (2.2)$$

Comparing Models 2.1 and 2.2 shows that $\epsilon_{it} = c_i + \nu_{it}$. Thus, if $c_i$ and $D_{it}$ are correlated, the naive OLS regression adjustment estimator for Model 2.1 will yield an inconsistent estimate for $E(\theta_i)$, the effect of divorce. In other words, if children of divorce are subject to the static selection, then the coefficient of $E(\theta_i)$ under the fixed-effects model in Equation 2.2 should be smaller than that under the standard regression adjustment in Equation 2.1.

The fixed effects, $c_i$, is identified when there are repeated measures of $y_{it}$. To estimate (2.2), I apply a time-demeaned transformation, yielding

$$(y_{it} - \bar{y}_i) = \beta \cdot (x_{it} - \bar{x}_i) + \theta \cdot (D_{it} - \bar{D}_i) + (\nu_{it} - \bar{\nu}_i), \quad (2.3)$$

which can then be estimated by ordinary least-squares (Wooldridge 2002).

2.4.3 Random Trends Model, I

As noted above, we often may suspect that both dynamic and static selection mechanisms are present. If so, then the estimator for the coefficient $E(\theta_i)$ in
Equation 2.2 will overstate the effect of divorce in the presence of dynamic selection. To deal with this possibility, I further relax the assumption on the error term \( \nu_{it} \) in Equation 2.2 by including a unique slope, \( g_i \), for each individual child:

\[
y_{it} = \beta \cdot x_{it} + \theta \cdot D_{it} + c_i + g_i \cdot t + \omega_{it}.
\]  

(2.4)

This model is called the “random trends model” (Wooldridge 2002). The model in Equation 2.4 generalizes Equation 2.2 by letting \( \nu_{it} = g_i \cdot t + \omega_{it} \). Hence, if \( g_i \) is correlated with parental divorce \( D_{it} \), the estimate of \( E(\theta_i) \) in Model 2.2 will be inconsistent. In the presence of dynamic selection, the true effect of parental divorce may be even smaller than those estimated in the first two analyses.

One can estimate the random trends model by taking two transformations followed by ordinary least-squares estimation. A first step is to take first differences between adjacent observations:

\[
\Delta y_{it} = \beta \cdot \Delta x_{it} + \theta \cdot \Delta D_{it} + g_i + \Delta \omega_{it}.
\]  

(2.5)

A second step is to apply the time-demeaned transformation to obtain the fixed-effects estimator, which eliminates \( g_i \):

\[
(\Delta y_{it} - \overline{\Delta y_i}) = \beta \cdot (\Delta x_{it} - \overline{\Delta x_i}) + \theta \cdot (\Delta D_{it} - \overline{\Delta D_i}) + (\Delta \omega_{it} - \overline{\Delta \omega_i}).
\]  

(2.6)

One then applies ordinary least squares to the expression in Equation 2.6.

### 2.4.4 Random Trends Model, II

A potentially undesirable feature of the random trends model is that the time-path of behavior problems for each child is assumed to follow one and only one child-specific slope. Hence, the model assumes that, for children of divorce, the slope before parental marital disruption is the same as the slope after disruption.
This specification, in effect, borrows strength from both the pre- and post-disruption data to estimate a single slope for each child—yielding a potentially biased estimate of the slope and, thus, the other coefficients. To investigate this possibility, I estimate a second random trends model:

\[ y_{it} = \beta \cdot x_{it} + \theta_i \cdot D_{it} + \gamma_i \cdot (D_{it} \cdot U_{it}) + c_i + g_i \cdot t + \omega_{it} \]  

where \( U_{it} \) is the duration since divorce. Adding the interaction between divorce and duration since divorce allows children of divorce to have a post-disruption slope that differs from the pre-disruption slope by the magnitude of \( \gamma_i \).

Typically, I will not have enough data to estimate two different slopes for an individual child. Instead, I estimate an average post-disruption slope, \( \text{E}(\gamma_i) \), for all children. This post-disruption coefficient, and how other coefficients are affected by including this coefficient, will provide a sensitivity check for the equal-slope assumption in the first random trends model in Equation 2.4.

These models assume that the counterfactual trajectories of behavior problems for children of divorce (i.e., what would have happened after the observed date of divorce had the divorce never happened) would follow the same trajectory with the same intercept and the same slope. This assumption would be problematic if there is reason to believe that the counterfactual trajectories would follow a different pattern; then, one needs to come up with an alternative scenario based on best information available to the analyst and model it accordingly.

2.5 Data

In studying the effects of parental divorce on child well-being, I exploit a unique design of the National Longitudinal Survey of Youth 1979 (NLSY79)—the avail-
ability of a wealth of longitudinal data on all children born to women in the original NLSY79 sample that can be linked to equally rich data for their biological coresident mothers in the original NLSY79 (Wu and Li 2005). Children living in the same households as the NLSY79 women were surveyed every other year since 1986. Although the child sample cannot be regarded as a probability sample of any cross-section of the U.S. population, it is representative of biological children born to women living in the United States in 1979 who were born between 1957 and 1964. The longitudinal and intergenerational design of the NLSY79 mother-child sample is especially suitable for examining the effects of parental divorce on children. However, a limitation of these data is that findings can be only generalized to children living with their mother after a marital disruption, and not to children in other living arrangements after parental divorce. The analysis includes all waves of data up to 2002.

2.5.1 Behavior Problem Index

The outcome variable is the behavior problem index—a commonly used indicator for children’s socioemotional development in both scholarly work and clinical applications. Behavior problems are also an established predictor of educational attainment and socioeconomic status (McLeod and Kaiser 2004; Miech et al. 1999). Understanding the relationship between parental divorce and children’s behavior problems may help explain the relationship between family structure and social inequality (Biblarz and Raftery 1999). The behavior problem index has been documented to exhibit substantial continuity across the life course (Knoester 2003; Loeber 1982; Sampson and Laub 1992). Children of divorce consistently score higher on behavior problem index than children in intact families (for a review, see, Amato 2001; Amato and Keith 1991).
Table 2.1: Items in the Behavior Problem Index

<table>
<thead>
<tr>
<th>Item description</th>
<th>External</th>
<th>Internal</th>
<th>Subscale</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cheats or tells lies</td>
<td>X</td>
<td></td>
<td>Antisocial</td>
</tr>
<tr>
<td>Bullies or is cruel/mean to others</td>
<td>X</td>
<td></td>
<td>Antisocial</td>
</tr>
<tr>
<td>Does not feel sorry for misbehaving</td>
<td></td>
<td></td>
<td>Antisocial</td>
</tr>
<tr>
<td>Breaks things deliberately (&lt; 12 yrs)</td>
<td>X</td>
<td></td>
<td>Antisocial</td>
</tr>
<tr>
<td>Disobedient at school (&gt; 5 yrs)</td>
<td>X</td>
<td></td>
<td>Antisocial</td>
</tr>
<tr>
<td>Trouble getting along with teachers (&gt; 5 yrs)</td>
<td>X</td>
<td></td>
<td>Antisocial</td>
</tr>
<tr>
<td>Sudden change in mood/feeling</td>
<td></td>
<td>X</td>
<td>Anxious/Depressed</td>
</tr>
<tr>
<td>Feels/complains no one loves him/her</td>
<td>X</td>
<td></td>
<td>Anxious/Depressed</td>
</tr>
<tr>
<td>Too fearful or anxious</td>
<td>X</td>
<td>X</td>
<td>Anxious/Depressed</td>
</tr>
<tr>
<td>Feels worthless or inferior</td>
<td>X</td>
<td></td>
<td>Anxious/Depressed</td>
</tr>
<tr>
<td>Unhappy, sad or depressed</td>
<td>X</td>
<td>X</td>
<td>Anxious/Depressed</td>
</tr>
<tr>
<td>Clings to adults (&lt; 12 yrs)</td>
<td>X</td>
<td></td>
<td>Dependent</td>
</tr>
<tr>
<td>Cries too much (&lt; 12 yrs)</td>
<td>X</td>
<td></td>
<td>Dependent</td>
</tr>
<tr>
<td>Demands a lot of attention (&lt; 12 yrs)</td>
<td>X</td>
<td></td>
<td>Dependent</td>
</tr>
<tr>
<td>Too dependent on others (&lt; 12 yrs)</td>
<td>X</td>
<td></td>
<td>Dependent</td>
</tr>
<tr>
<td>High strung, tense, nervous</td>
<td>X</td>
<td></td>
<td>Headstrong</td>
</tr>
<tr>
<td>Argues too much</td>
<td>X</td>
<td></td>
<td>Headstrong</td>
</tr>
<tr>
<td>Disobedient at home</td>
<td>X</td>
<td></td>
<td>Headstrong</td>
</tr>
<tr>
<td>Stubborn, sullen, or irritable</td>
<td>X</td>
<td></td>
<td>Headstrong</td>
</tr>
<tr>
<td>Strong temper, loses it easily</td>
<td>X</td>
<td></td>
<td>Headstrong</td>
</tr>
<tr>
<td>Difficulty concentrating/paying attention</td>
<td>X</td>
<td></td>
<td>Hyperactive</td>
</tr>
<tr>
<td>Easily confused/in a fog</td>
<td>X</td>
<td>X</td>
<td>Hyperactive</td>
</tr>
<tr>
<td>Impulsive—acts without thinking</td>
<td>X</td>
<td></td>
<td>Hyperactive</td>
</tr>
<tr>
<td>Trouble with obsessions, etc.</td>
<td>X</td>
<td></td>
<td>Hyperactive</td>
</tr>
<tr>
<td>Restless, overly active, etc.</td>
<td>X</td>
<td></td>
<td>Hyperactive</td>
</tr>
<tr>
<td>Trouble getting along with others</td>
<td>X</td>
<td></td>
<td>Peer Problems</td>
</tr>
<tr>
<td>Not liked by other children</td>
<td>X</td>
<td></td>
<td>Peer Problems</td>
</tr>
<tr>
<td>Withdrawn, not involved with others</td>
<td>X</td>
<td></td>
<td>Peer Problems</td>
</tr>
</tbody>
</table>

The Behavior Problem Index (BPI) consists of a checklist of 28 items of behavior problems reported by the mother. I have coded an item 1 if the mother answered “often” or “sometimes true”, and 0 if the mother answered “not true.” The higher the score, the greater the level of the behavior problem and the lower the level of a child’s emotional well-being. Items for the BPI are listed in Table 2.1. The BPI scores are collected biennially for children ages 4 and over until 1992. From 1994 onwards, they are collected for children between 4 and 15 years of age. I restrict my analysis to only measurements taken for
children under age 15 at the time of the interview. Because of this age constraint,
there are up to 6 repeated BPI measures with intervals of roughly 2 years apart
for each child. I use the summated raw score of BPI (hence the range of the
scale is 28), and control for the age pattern using sex-specific linear splines with
a node at 9.5 years of age.\footnote{Although prior research typically used an age-standardized BPI score for the
dependent variable, Cronbach (1990) has shown that this common practice will lead
to biased estimate of the effect of divorce if parental divorce is correlated with child’s
age (p. 242). Because a fundamental demographic insight on exposure and probability
suggests that more and more children will experience a parental divorce as they
age, parental divorce will be correlated with child’s age, suggesting that an age-
standardized score is inappropriate. Instead, I control for the age- and sex-specific
pattern of change in BPI using covariates on the righthand side of the equation, rather
than using standardization on the lefthand side of the equation.}

Figure 2.1 presents the density histograms (with
normal curves imposed) of the BPI scores. On average, boys have almost 1
more behavior problem than girls (with respective means of 8.7 and 7.9). The
distributions of scores are slightly skewed, but log-transformations do not make
the distributions look “more normal.”

2.5.2 Control Variables

I include controls for demographic characteristics of the child. The child’s sex
is coded 1 for boys and 0 for girls. The race and ethnicity of the child are mea-
sured by two mutually exclusive dummy variables, coded 1 for black and for
Hispanic, with non-black-non-Hispanic being the reference category. The birth
order and the mother’s age at the birth of the child are included as continuous
variables. I also include of the mother socioeconomic and demographic charac-
teristics, including mother’s nativity, education, total family income at her first
marriage, mother’s age at first marriage, age at first birth, mother was raised as
a Catholic, mother’s religiosity in 1979; mother had her first sex before age 20,
mother’s family structure at age 14, any regular reading materials in mother’s household when she was at age 14, mother’s self-esteem measured in 1980 and AFQT percentile score, and dummy indicators for missing data on self-esteem and AFQT. These variables are invariant over time (see Appendix A for the definition and construction of these control variables).

I also include several time-varying control variables. Child’s age (in months) is measured at the time of each assessment of the behavior problem.\(^3\) Number of siblings changes value at each wave of child assessment in the same way as

\(^3\)Because there are two modes of data collection of the child assessment data, i.e., a mother supplement and a child supplement, the age of child in each survey year has two versions corresponding respectively to the supplements. For behavior problems, age of child corresponds to the child’s age when the “mother supplement” was administered.
I measure the age of child. I also constrain the value for number of siblings to stop accumulating after a parental separation (or divorce if there is no reported separation before a divorce). I construct the number of siblings for each child by comparing the dates when the behavior problems were measured and the dates of births in the 1982-2004 Fertility and Relationship History provided by the Center for Human Resource Research at the Ohio State University.

### 2.5.3 Sample Restrictions

I restrict the NLSY79 mother sample to ever married women as of the most recent interview (up to 2002), deleting all men ($N = 6,403$ of the original 12,686 respondents of both sexes) and never married women ($N = 1,361$). Among ever married women, I delete those who are childless as of 2002 or with missing data on age at first birth ($N = 739$). I also delete those women with missing data on family structure at age 14 ($N = 8$), reading materials at age 14 ($N = 39$), Catholic religion ($N = 13$), religiosity ($N = 4$), age at sex ($N = 72$), education at marriage ($N = 10$), total net family income at the formation of first marriage ($N = 13$), family income at the formation of first marriage ($N = 13$), and the reason first marriage was dissolved ($N = 46$).

The full child sample is matched to the mother sample, as restricted by the above criteria, with the exception of 404 children who were born outside of the

---

4 The status of never married, like other time-varying statuses (such as childlessness) is partly affected by sample attrition. If a respondent was no longer interviewed after a certain survey year and was never married as of the last survey interview, she would be considered “never married” and hence deleted from the current analysis even if she might have gotten married between the last time we interviewed her and the 2002 survey.

5 This is primarily due to noninterview in all three consecutive surveys between 1983 and 1985 in which the question was fielded.

6 Most of them are those whose first marriages dissolved before the 1979 survey, in which only information on reasons the most recent marriage ended was recorded.
mother’s first marriage (i.e., either before her first marriage formation or after her first marriage dissolved), which were excluded from the analysis. I further deleted observations taken when the child was more than 15 years of age at the time of survey interview because only pre-1994 surveys have BPI measures for children age 15 and over, who tend to be born to relatively young mothers. Finally, listwise deletion of missing data on the dependent variable of BPI drops 802 observations. This gives a sample of 6,332 children born to 3,124 mothers.

2.5.4 Strengths and Weaknesses of Data

A common criticism about the Children of NLSY79 data has been the way the sample is generated. These children enter the sample through birth to the original NLSY79 mother sample and do not represent any cross-section of children in the U.S. population. In particular, the early waves of the child sample tended to over-represent children born to younger mothers with disproportionately low socioeconomic background. However, as the sample of mothers and children has “aged”, the child sample has grown increasingly more representative with respect to age structure and socioeconomic background. Table 2.2 illustrates this point by displaying the patterns of observations across survey waves. In 1986, all mothers in the analytic sample were under age 24 when they gave birth to the child. By the 2002 wave, the oldest mothers were 40 years of age, which is about the range we typically see in the population. As a result, using the 1986-2002 waves of the child data in this chapter improves on analyses of the same data in prior studies both because of larger sample sizes and by its representativeness.

Also note that because the BPI is measured between age 4 and 15 and because the survey takes place only every two years, there are at most six ob-
Table 2.2: Design Features of Children of the NLSY79

<table>
<thead>
<tr>
<th>Mom’s child’s age at birth</th>
<th>Birth cohort</th>
<th>‘86</th>
<th>‘88</th>
<th>‘90</th>
<th>‘92</th>
<th>‘94</th>
<th>‘96</th>
<th>‘98</th>
<th>‘00</th>
<th>‘02</th>
</tr>
</thead>
<tbody>
<tr>
<td>8–15</td>
<td>1973</td>
<td>13</td>
<td>15</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>9–16</td>
<td>1974</td>
<td>12</td>
<td>14</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>10–17</td>
<td>1975</td>
<td>11</td>
<td>13</td>
<td>15</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>12–19</td>
<td>1977</td>
<td>9</td>
<td>11</td>
<td>13</td>
<td>15</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>14–21</td>
<td>1979</td>
<td>7</td>
<td>9</td>
<td>11</td>
<td>13</td>
<td>15</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>16–23</td>
<td>1981</td>
<td>5</td>
<td>7</td>
<td>9</td>
<td>11</td>
<td>13</td>
<td>15</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>17–24</td>
<td>1982</td>
<td>4</td>
<td>6</td>
<td>8</td>
<td>10</td>
<td>12</td>
<td>14</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>18–25</td>
<td>1983</td>
<td>.</td>
<td>5</td>
<td>7</td>
<td>9</td>
<td>11</td>
<td>13</td>
<td>15</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>19–26</td>
<td>1984</td>
<td>.</td>
<td>4</td>
<td>6</td>
<td>8</td>
<td>10</td>
<td>12</td>
<td>14</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>20–27</td>
<td>1985</td>
<td>.</td>
<td>.</td>
<td>5</td>
<td>7</td>
<td>9</td>
<td>11</td>
<td>13</td>
<td>15</td>
<td>.</td>
</tr>
<tr>
<td>22–29</td>
<td>1987</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>5</td>
<td>7</td>
<td>9</td>
<td>11</td>
<td>13</td>
<td>15</td>
</tr>
<tr>
<td>23–30</td>
<td>1988</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>4</td>
<td>6</td>
<td>8</td>
<td>10</td>
<td>12</td>
<td>14</td>
</tr>
<tr>
<td>25–32</td>
<td>1990</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>4</td>
<td>6</td>
<td>8</td>
<td>10</td>
<td>12</td>
<td>.</td>
</tr>
<tr>
<td>26–33</td>
<td>1991</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>5</td>
<td>7</td>
<td>9</td>
<td>11</td>
<td>.</td>
</tr>
<tr>
<td>28–35</td>
<td>1993</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>5</td>
<td>7</td>
<td>9</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>30–37</td>
<td>1995</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>5</td>
<td>7</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>31–38</td>
<td>1996</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>4</td>
<td>6</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>32–39</td>
<td>1997</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>5</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td>33–40</td>
<td>1998</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>.</td>
<td>4</td>
<td>.</td>
<td>.</td>
</tr>
</tbody>
</table>
Table 2.3: Number of Observations per Child

<table>
<thead>
<tr>
<th>Obs. / child</th>
<th>Number of children</th>
<th>Percent</th>
<th>Cumul. Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>807</td>
<td>12.7</td>
<td>12.7</td>
</tr>
<tr>
<td>2</td>
<td>967</td>
<td>15.3</td>
<td>28.0</td>
</tr>
<tr>
<td>3</td>
<td>1,005</td>
<td>15.9</td>
<td>43.9</td>
</tr>
<tr>
<td>4</td>
<td>1,386</td>
<td>21.9</td>
<td>65.8</td>
</tr>
<tr>
<td>5</td>
<td>1,803</td>
<td>28.5</td>
<td>94.3</td>
</tr>
<tr>
<td>6</td>
<td>364</td>
<td>5.7</td>
<td>100.0</td>
</tr>
<tr>
<td>Total</td>
<td>6,332</td>
<td>100.0</td>
<td></td>
</tr>
</tbody>
</table>

Observations for any child. Thus, children born to the oldest and youngest mothers will have fewer than six observations because of the design of the original survey. Survey nonresponse also yields an unbalanced panel for some children despite extremely lowest sample attrition in the mother-child data. Table 2.3 gives the distribution of the number of observations per child. Close to 90% of the children have two or more observations, which is required for estimating the child fixed-effects models. Over 70% of the children have three or more observations, which is required for estimating the two random-trends models.

The unbalanced panel data might create a methodological problem if sample attrition and survey nonresponse are correlated with parental divorce. Table 2.4 gives a closer look at the patterns of these panel data for children of divorce and children in intact families. In any wave (from 1986-2002, thus, a maximum of 9 observations per child), the symbol “x” represents observations with valid data of the dependent variable of BPI, and the symbol “.” represents either no data (structurally) or missing data (nonresponse). The top 20 frequent patterns for each group, which capture about three quarters of the respondents for each group, do not appear to be any different.
Table 2.4: Patterns of Panel Data by Parental Separation/Divorce

<table>
<thead>
<tr>
<th>Frequency</th>
<th>Percent</th>
<th>Pattern</th>
<th>Frequency</th>
<th>Percent</th>
<th>Pattern</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intact Family</td>
<td></td>
<td></td>
<td>Divorced Family</td>
<td></td>
<td></td>
</tr>
<tr>
<td>249</td>
<td>6.6</td>
<td>. . . . . . . x</td>
<td>263</td>
<td>10.2</td>
<td>x x . . . . . . .</td>
</tr>
<tr>
<td>231</td>
<td>6.2</td>
<td>. . . . . . . xxx</td>
<td>223</td>
<td>8.7</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>224</td>
<td>6.0</td>
<td>. . . . . . . xxxxx</td>
<td>215</td>
<td>8.4</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>219</td>
<td>5.8</td>
<td>. . . . . . . xxxx .</td>
<td>192</td>
<td>7.5</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>212</td>
<td>5.6</td>
<td>. . . . . . . xxxx .</td>
<td>142</td>
<td>5.5</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>204</td>
<td>5.4</td>
<td>. . . . . . . xxxx .</td>
<td>128</td>
<td>5.0</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>189</td>
<td>5.0</td>
<td>. . . . . . . xxxx .</td>
<td>101</td>
<td>3.9</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>181</td>
<td>4.8</td>
<td>. . . . . . . xxxx .</td>
<td>81</td>
<td>3.1</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>160</td>
<td>4.3</td>
<td>. . . . . . . xxxx .</td>
<td>76</td>
<td>3.0</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>157</td>
<td>4.2</td>
<td>. . . . . . . xxxx .</td>
<td>65</td>
<td>2.5</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>153</td>
<td>4.1</td>
<td>. . . . . . . xxxx .</td>
<td>62</td>
<td>2.4</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>137</td>
<td>3.7</td>
<td>. . . . . . . xxxx .</td>
<td>59</td>
<td>2.3</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>69</td>
<td>1.8</td>
<td>. . . . . . . xxxx .</td>
<td>49</td>
<td>1.9</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>68</td>
<td>1.8</td>
<td>. . . . . . . xxxx .</td>
<td>45</td>
<td>1.8</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>67</td>
<td>1.8</td>
<td>. . . . . . . xxxx .</td>
<td>40</td>
<td>1.6</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>67</td>
<td>1.8</td>
<td>. . . . . . . xxxx .</td>
<td>39</td>
<td>1.5</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>64</td>
<td>1.7</td>
<td>. . . . . . . xxxx .</td>
<td>39</td>
<td>1.5</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>63</td>
<td>1.7</td>
<td>. . . . . . . xxxx .</td>
<td>37</td>
<td>1.4</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>60</td>
<td>1.6</td>
<td>. . . . . . . xxxx .</td>
<td>33</td>
<td>1.3</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>54</td>
<td>1.4</td>
<td>. . . . . . . xxxx .</td>
<td>31</td>
<td>1.2</td>
<td>. . . . . . . xxxx</td>
</tr>
<tr>
<td>928</td>
<td>24.7</td>
<td>(other patterns)</td>
<td>656</td>
<td>25.5</td>
<td>(other patterns)</td>
</tr>
<tr>
<td>3756</td>
<td>100.0</td>
<td></td>
<td>2576</td>
<td>100.0</td>
<td></td>
</tr>
</tbody>
</table>
Table 2.5: Descriptive Statistics for Children of the NLSY79

<table>
<thead>
<tr>
<th>Variable</th>
<th>Girls</th>
<th></th>
<th>Boys</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Intact family</td>
<td>Divorced family</td>
<td>Intact family</td>
<td>Divorced family</td>
</tr>
<tr>
<td>Black</td>
<td>.13</td>
<td>.23</td>
<td>.14</td>
<td>.22</td>
</tr>
<tr>
<td>Hispanic</td>
<td>.19</td>
<td>.23</td>
<td>.18</td>
<td>.26</td>
</tr>
<tr>
<td>Birth order</td>
<td>1.79</td>
<td>1.80</td>
<td>1.79</td>
<td>1.79</td>
</tr>
<tr>
<td></td>
<td>(.90)</td>
<td>(.92)</td>
<td>(.86)</td>
<td>(.90)</td>
</tr>
<tr>
<td>Mom’s age at</td>
<td>25.78</td>
<td>23.43</td>
<td>25.65</td>
<td>23.48</td>
</tr>
<tr>
<td>child’s birth</td>
<td>(4.05)</td>
<td>(4.13)</td>
<td>(3.98)</td>
<td>(4.02)</td>
</tr>
<tr>
<td>N</td>
<td>941</td>
<td>738</td>
<td>1,021</td>
<td>742</td>
</tr>
</tbody>
</table>

2.6 Results

I conduct parallel analyses on the full analytic sample, on the sample of children with at least two observations, and on the sample of children with at least three consecutive observations. Although the OLS regressions can be estimated on all these sub-samples, the random-trends models can only be estimated on the sub-sample of children with at least three observations. Similarly, the child fixed-effects models can only be estimated on the sub-sample of children with at least two observations. To facilitate comparisons across models, I only present results based on at least three consecutive observations. I obtain similar results regardless of the sample restrictions imposed (see Appendix B).

2.6.1 Descriptive statistics

Table 2.5 presents descriptive statistics for children’s characteristics by the child’s sex and whether the mother was divorced. Children of divorce belong disproportionately to younger mothers, and to racial and ethnic minorities. Table 2.6 presents descriptive statistics for the mothers of the children by their status of divorce as of the 2002 survey. Divorced women are more likely to be black and Hispanic, to have grown up in a broken family, from lower socioeco-
nomic backgrounds, to have scored lower on the AFQT test on cognitive ability, to have lower levels of education completed, and to have married and have a first child at a slightly younger age.

2.6.2 Age Patterns of Behavior Problem Index

Because BPI varies substantially with age, I use two methods to explore and control for this relationship. A first exploratory method uses a variable span “super smoother” developed by Friedman (1984). It is a nonparametric method that helps identify the age pattern of BPI with minimal assumptions about the bivariate relationship between age and BPI. The second method models the multivariate relationship between age and BPI by using a linear spline for age with a knot at 9.5 years of age, where the placement of the knot is guided by the nonparametric analyses. The use of a spline specification is a flexible parametric method, and will be used as the basis for subsequent analysis. The results from the two methods are roughly similar, suggesting that a spline specification provides a reasonable approximation to the underlying age patterns (as shown in the nonparametric smoother) in the regression analysis.

For boys, BPI first increases and then decreases with age, with a peak around 9.5 years of age (Figure 2.2). The BPI for girls follow a similar, but much less curvilinear, pattern (Figure 2.3). Boys tend to have more behavior problems than girls at all ages, which confirm the descriptive statistics on mean levels. Children of divorce consistently have higher level of behavior problems. The differences are roughly the same across all ages, and more than 1.5 points for boys and more than 1 point for girls on the BPI scale.7

7To gauge the sensitivity of these results to the observation plan, I replicated the same analysis on samples with at least one observation per child, two observations per child and three observations per child. The results are indistinguishable from each
Table 2.6: Descriptive Statistics for NLSY79 Mothers

<table>
<thead>
<tr>
<th>Variable</th>
<th>Continuously Married</th>
<th>Separated/Divorced</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hispanic</td>
<td>.18</td>
<td>.24</td>
</tr>
<tr>
<td>Black</td>
<td>.14</td>
<td>.24</td>
</tr>
<tr>
<td>Foreign Born</td>
<td>.08</td>
<td>.07</td>
</tr>
<tr>
<td>Intact family at age 14</td>
<td>.78</td>
<td>.66</td>
</tr>
<tr>
<td>Mother only at age 14</td>
<td>.11</td>
<td>.16</td>
</tr>
<tr>
<td>Stepmother-father at age 14</td>
<td>.02</td>
<td>.02</td>
</tr>
<tr>
<td>Stepmother-mother at age 14</td>
<td>.05</td>
<td>.09</td>
</tr>
<tr>
<td>mag, papers, lib card in HH at age 14</td>
<td>.49</td>
<td>.39</td>
</tr>
<tr>
<td>Raised as Roman Catholic</td>
<td>.43</td>
<td>.37</td>
</tr>
<tr>
<td>Frequency of religious activity in 1979</td>
<td>3.58</td>
<td>3.38</td>
</tr>
<tr>
<td></td>
<td>(1.66)</td>
<td>(1.71)</td>
</tr>
<tr>
<td>AFQT percentile score</td>
<td>48.17</td>
<td>35.17</td>
</tr>
<tr>
<td></td>
<td>(27.20)</td>
<td>(24.17)</td>
</tr>
<tr>
<td>Age at 1st sex&lt; 20</td>
<td>.02</td>
<td>.03</td>
</tr>
<tr>
<td>&lt; 12 years of schooling at Mar1</td>
<td>.11</td>
<td>.21</td>
</tr>
<tr>
<td>12 years of schooling at Mar1</td>
<td>.36</td>
<td>.39</td>
</tr>
<tr>
<td>13-15 years of schooling at Mar1</td>
<td>.26</td>
<td>.28</td>
</tr>
<tr>
<td>&gt; =16 year of schooling at Mar1</td>
<td>.27</td>
<td>.12</td>
</tr>
<tr>
<td>age at 1st marriage as of 2002</td>
<td>21.89</td>
<td>20.15</td>
</tr>
<tr>
<td></td>
<td>(3.44)</td>
<td>(3.07)</td>
</tr>
<tr>
<td>age at 1st birth as of 2002</td>
<td>23.88</td>
<td>21.21</td>
</tr>
<tr>
<td></td>
<td>(4.34)</td>
<td>(3.99)</td>
</tr>
<tr>
<td>Self esteem, 1980</td>
<td>32.59</td>
<td>31.86</td>
</tr>
<tr>
<td></td>
<td>(3.91)</td>
<td>(3.81)</td>
</tr>
<tr>
<td>N</td>
<td>1,037</td>
<td>911</td>
</tr>
</tbody>
</table>
Figure 2.2: Age Patterns of BPI for Boys, by Parental Divorce

Figure 2.3: Age Patterns of BPI for Girls, by Parental Divorce
2.6.3 Results from Regressions Adjusting Observables

I begin by estimating the naive OLS model ignoring any possible selection on unobservables, which I refer henceforth as results obtained from ordinary least squares regression adjustment. The resulting pooled cross-sectional estimates in the first and third columns of Table 2.7 show that parental divorce is associated with a 1.63-point increase (standard error .27) in boys’ behavior problems and a 1.39-point increase (standard error .24) in girls’ behavior problems. As expected, both associations are highly significant, indicating children of divorce, on the average, have worse emotional well-being than their counterparts of the same age, sex, and race/ethnicity but in intact families.

Controlling for socioeconomic and demographic factors that may confound the relationship between divorce and children’s behavior problems reduces the coefficient to 1.16 for boys and to 0.98 for girls (both with the same magnitude of standard errors). Consistent with prior findings, these coefficients in the regression adjustment are of substantially smaller magnitudes (with a reduction of approximately 1/3) than the simple correlations reported earlier, but remain highly significant.\(^8\) These estimates, thus, are similar to those in previous studies, for example, the findings reported by McLanahan and Sandefur (1994), who concluded that children of divorce are worse off compared with children in two-parent families of similar socioeconomic and demographic backgrounds.

The magnitudes of the effects suggest that an “average” divorced mother will notice one more behavior problem in their child than their married counterpart with similar socioeconomic backgrounds, on a scale of mean of about 8 items.

\(^8\)The standard errors are smaller in the present analysis than most previous research and thus the \(p\) values for significance level are smaller because these data have the largest number of observations and divorce, and therefore, greater statistical power.
Table 2.7: Regressions Adjusting Observed Confounding Factors

<table>
<thead>
<tr>
<th></th>
<th>Boys</th>
<th>Girls</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS1</td>
<td>OLS2</td>
</tr>
<tr>
<td>Parental divorce</td>
<td>1.63** (.27)</td>
<td>1.16** (.27)</td>
</tr>
<tr>
<td>Child’s Control Variable</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>.37 .25</td>
<td>.38 .26</td>
</tr>
<tr>
<td>Hispanic</td>
<td>.53 .34</td>
<td>.21 .34</td>
</tr>
<tr>
<td>Child’s Age ≤ 9 years</td>
<td>.10** .07*</td>
<td>.06 -.08</td>
</tr>
<tr>
<td>Child’s Age &gt; 9 years</td>
<td>-.26** -.30**</td>
<td>-.33** -.30**</td>
</tr>
<tr>
<td>Mom’s age at child’s birth</td>
<td>-.27** -.28**</td>
<td>-.28** -.28**</td>
</tr>
<tr>
<td>Child’s birth order</td>
<td>.15 .10</td>
<td>.19 .10</td>
</tr>
<tr>
<td>Mother’s Control Variable</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Foreign born</td>
<td>-.55 .12</td>
<td></td>
</tr>
<tr>
<td>Education (&lt; high school)</td>
<td>.91** .37</td>
<td></td>
</tr>
<tr>
<td>Education (some college)</td>
<td>-.16 .02</td>
<td></td>
</tr>
<tr>
<td>Education (college grad)</td>
<td>-1.42** -.10</td>
<td></td>
</tr>
<tr>
<td>Total net family income</td>
<td>7.65**</td>
<td>7.71**</td>
</tr>
<tr>
<td>Age at first marriage</td>
<td>-.01 .02</td>
<td></td>
</tr>
<tr>
<td>Age at first birth</td>
<td>.15 .10</td>
<td></td>
</tr>
<tr>
<td>Self esteem</td>
<td>-.21** -.18**</td>
<td></td>
</tr>
<tr>
<td>Catholic background</td>
<td>-.59* -.13</td>
<td></td>
</tr>
<tr>
<td>Freq. of relig. activities</td>
<td>.01 -.15*</td>
<td></td>
</tr>
<tr>
<td>AFQT score</td>
<td>.01* .01</td>
<td></td>
</tr>
<tr>
<td>Age at first sex &lt; 20</td>
<td>-.24 1.36</td>
<td></td>
</tr>
<tr>
<td>Intact family at age 14</td>
<td>-1.05 -.63</td>
<td></td>
</tr>
<tr>
<td>Mother only family at 14</td>
<td>-.90 .28</td>
<td></td>
</tr>
<tr>
<td>Stepmother-father at 14</td>
<td>-1.29 .21</td>
<td></td>
</tr>
<tr>
<td>Stepmother-mother at 14</td>
<td>-.24 -.88</td>
<td></td>
</tr>
<tr>
<td>Any reading material at 14</td>
<td>-.43 -.62*</td>
<td></td>
</tr>
<tr>
<td>AFQT score missing</td>
<td>-.57 1.08</td>
<td></td>
</tr>
<tr>
<td>Self esteem missing</td>
<td>.65 -.46</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>7.43 19.35**</td>
<td>14.37** 19.35**</td>
</tr>
<tr>
<td>$R^2$</td>
<td>.02 .08</td>
<td>.05 .08</td>
</tr>
<tr>
<td>N of observations</td>
<td>7,872 7,872</td>
<td>7,519 7,519</td>
</tr>
</tbody>
</table>
on a checklist of 28. The effect size (0.19 standard deviations for boys, and 0.17 standard deviations for girls) is similar in magnitude to that reported in the meta-analysis by Amato and Keith (1991).

2.6.4 Results from Regressions Adjusting Unobservables

Columns 1 and 4 of Table 2.8 give estimates from the fixed-effects model specified in Equation 2.2. These results show that the coefficient for parental divorce drops to .45 for boys and .48 for girls. Both are less than half the magnitude of the naive estimator in columns 2 and 4 of Table 2.7). Neither fixed-effects coefficient is statistically significant. Note that the statistical insignificance in these results stems from the smaller fixed-effects coefficients and not from the larger standard errors. Substituting the OLS standard errors (.27 and .24) for the fixed-effects standard errors, for example, does not yield statistical significance, showing that the lack of statistical significance is not due to the loss of statistical power in the fixed-effects model.

Including the random trends yields somewhat different results for boys but not for girls (see the RT1 models in Table 2.8). For boys, the coefficient flips sign and declines further in magnitude, implying that a parental divorce is associated with nearly half a point reduction in boys’ BPI. Although not statistically significant, the point estimate nevertheless suggests, contrary to most previous research, that parental divorce may improve boys’ emotional well-being after controlling for dynamic selection. For girls, estimated coefficients are similar in the fixed-effects and random-trends models. Overall, both the fixed-effects and random-trends coefficients tell a similar story: Parental divorce has no statistically significant effect on children’s behavior problems, with the estimated effect small enough in magnitude that fewer than half of the divorced mothers
Table 2.8: Regressions Adjusting Selections on Unobservables

<table>
<thead>
<tr>
<th></th>
<th>Boys</th>
<th></th>
<th></th>
<th>Girls</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>FE</td>
<td>RT1</td>
<td>RT2</td>
<td>FE</td>
<td>RT1</td>
<td>RT2</td>
</tr>
<tr>
<td>Parental divorce</td>
<td>.45</td>
<td>-.41</td>
<td>-.42</td>
<td>.48</td>
<td>.44</td>
<td>.46</td>
</tr>
<tr>
<td></td>
<td>(.36)</td>
<td>(.60)</td>
<td>(.60)</td>
<td>(.39)</td>
<td>(.65)</td>
<td>(.66)</td>
</tr>
<tr>
<td>Time since divorce</td>
<td>-.04</td>
<td>.09</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.30)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Control Variable</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Child’s age ≤ 9.5 years</td>
<td>.06</td>
<td>-.02</td>
<td>-.01</td>
<td>-.09*</td>
<td>.19</td>
<td>.17</td>
</tr>
<tr>
<td></td>
<td>(.04)</td>
<td>(.52)</td>
<td>(.52)</td>
<td>(.04)</td>
<td>(.47)</td>
<td>(.48)</td>
</tr>
<tr>
<td>Child’s age &gt; 9.5 years</td>
<td>-.35**</td>
<td>-.44</td>
<td>-.43</td>
<td>-.30**</td>
<td>-.04</td>
<td>-.06</td>
</tr>
<tr>
<td></td>
<td>(.04)</td>
<td>(.53)</td>
<td>(.54)</td>
<td>(.04)</td>
<td>(.48)</td>
<td>(.50)</td>
</tr>
<tr>
<td>Constant</td>
<td>8.45**</td>
<td>.22</td>
<td>.23</td>
<td>8.75**</td>
<td>-.53</td>
<td>-.55</td>
</tr>
<tr>
<td></td>
<td>(.27)</td>
<td>(1.05)</td>
<td>(.95)</td>
<td>(.27)</td>
<td>(.95)</td>
<td>(.95)</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>7,872</td>
<td>5,902</td>
<td>5,902</td>
<td>7,519</td>
<td>5,649</td>
<td>5,649</td>
</tr>
</tbody>
</table>

would observe one item increase on the behavior problem index in their child.

Table 2.8 also shows little for the possibility that the child-specific random slope might vary before and after marital disruption. The random-trends models with an interaction between parental divorce and the time since marital disruption (the RT2 models in Table 2.8) give nearly identical results as the random-trends models with a single random slope. The estimates of the interaction effect are close to zero (-.04/year for boys and .09/year for girls, with standard errors of .30 and .32), suggesting that the slopes before and after marital disruption, on average, are not much different.

### 2.7 Discussion

This chapter asks a very straightforward question: Is the association between parental divorce and children’s emotional well-being documented in numerous
prior studies causal? The results presented in this chapter show no evidence that parental divorce causes any increase in children’s behavior problems. While I successfully replicate the “robust” finding regarding the association when controlling for a wide range of socioeconomic and family background factors of the child and the mother, the association disappears when I exploit the the longitudinal research design to eliminate selection biases on unobserved factors. Despite the strong belief among social scientists and the general public that divorce alike, the answer to this specific causal question on divorce and children’s behavior problems, based on these analyses of this chapter, is no.9

It is certainly important to clarify the qualifications for this negative answer, since this answer challenges a vast literature on parental divorce and children’s emotional well-being. The null finding presented in this chapter is specific to one outcome—the behavior problem index—for children in the age range between 4 and 15 years old, who were born within the mother’s first marriage, and who resided with the mother after divorce. It is thus inappropriate to extrapolate these results beyond this age range, to other outcome measures, to children living with divorced fathers, and to children born out of wedlock or in stepfamilies. For example, these results do not overturn the finding that children in divorced families suffer from a nontrivial financial loss (Peterson 1996; Weitzman 1985). Nor do they provide much insight for explaining the long-term intergenerational transmission of family behaviors (McLanahan and Bumpass 1988). It is also important to note that the estimate is what the methodologists

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9The fixed-effects models and random-trends models used in this chapter do not address the issue of reverse causality. If children’s behavior problems increase the parents’ probability of divorce, then the effect of divorce estimated in this chapter would be biased. If this is the case, then we should expect an even smaller effect of divorce if we specify models that eliminate the biases due to reverse causality.
call the “the average treatment effect on the treated” (Winship and Morgan 1999).\textsuperscript{10} That is, the fixed effects estimates in this chapter are intended to answer the counterfactual question of what would have happened to children’s emotional well-being should those parents who divorced were instead to have remained married. As such, these estimates say nothing about the well-being of children born to parents who are happily married and have rarely, if ever, pondered the possibility of a marital disruption. In fact, the empirical analysis not only acknowledges but emphasizes the fact that children of divorce may differ from children in two-parent families for a variety of reasons that might be related to their socioemotional development, the family environment in which they grow up, and the dynamics and frailty of their parents’ marriage. If these differences exist, the estimate for the effect of divorce on children of divorce will not be the same as that of the effect of divorce for an “average” child in the population.

2.7.1 Moving Out with a Divorce Decree

Setting aside these empirical caveats for the moment, how might we explain the findings of this chapter—that parental divorce has no effect on children’s behavioral problems—given the vast body of prior research that has found that divorce is detrimental to children’s emotional well-being? I believe that the consensus among researchers is correct in supposing that parental resources are crucial in facilitating children’s socioemotional development (Hetherington and Kelly 2002; McLanahan and Sandefur 1994; Seltzer 1994; Waite and Gallagher 2000). I also believe that they are also correct in asserting that divorced parents

\textsuperscript{10}The identification of the effects relies only on data from those children whose parents separated or divorced in the observation period (between 1986 and 2002), and not on data from those children whose parents were continuously married.
have fewer social, economic, and emotional resources for their children than parents in intact families, at least partly due to structural barriers—e.g., separated residences and weakened parent-child ties. Where I disagree, however, is that the divorced parents would, on average, be able to provide equivalent parental resources as their intact-family counterparts for the children were we to implement a policy that, for example, prohibited marital disruption. The caveat, “on average,” also means that I also believe that, despite various structural barriers, not all divorced parents are doomed to failure in their attempts to provide sufficient parental resources necessary for the healthy socioemotional development of their children—some will, and others will not. But most importantly, attributing these differences in parental resources to divorce requires rigorously addressing the issue of whether these differences are indeed a consequence of divorce and thus an unavoidable byproduct of marital disruption.

As I have argued in the theory section, if the behaviors that prior research has argued are the causal factors linking parental divorce to children’s behavior problems do not in fact change before and after divorce, they in fact reflect selection, rather than causes of behavioral factors. Because my empirical design explicitly models the influences of changes in both observed and unobserved factors that may vary before and after a parental separation, I interpret the null findings in this chapter as providing solid evidence for the theoretical role of selection laid out earlier. If so, then from a policy perspective, if these selection mechanisms can be dissociated from marital disruption, one may be able to devise policies that mitigate the consequences of these selection factors without requiring that a couple remain married. Indeed, because what marital disruption necessarily involves is a spouse “moving out with a divorce
decree.”11, reduced contact through separate residence and a change in legal status are virtually the only things that are inevitably accompany divorce. All other things—e.g., care and money the non-custodial contributed to the child—can, in principle, be dissociated from the event of a divorce and manipulated (or remedied) following a divorce. Thus, it is perhaps less surprising that the findings of this chapter suggest that these behaviors and child outcomes are very diverse (and thus potentially malleable) for divorced parents and their children.

In summary, the findings of this chapter are consistent with the selection arguments contained in the speculations of Cherlin et al. (1991): The dysfunctional family dynamics (possibly involving high conflicts or disengagement of the parents) for children of divorce—the potential real cause of lower emotional well-being—are likely to be present before and after marital disruption and are unlikely to emerge only after separation. A marital separation moves a parent out of the household and divorce brings a change in legal status, formalizing much of what has already happened (e.g., whom the child is to live with) and other parental obligations (e.g., visitation schedules and the amount of child support). Neither non-coresidence nor a change in legal status is likely to substantially change the emotional environment in which the sensitive youthful mind is nurtured. Although the analysis seems like attacking a straw man because no one argues that the effect of divorce comes from the legal paper and one parent no longer comes home for dinner and sleep, this is nevertheless what is logically implied if one holds that lower child well-being is a causal consequence of divorce without establishing the—both necessary and causal—link

11In fact, even moving out is not always necessarily implied because co-residence is no longer taken for granted for many modern (especially professional) “married couples living apart.”
between divorce and the intervening behavior mechanisms. Hence, that divorce might have no causal effect on children’s well-being is not inconsistent with a view that marriages confer benefits for children’s well-being, as argued by the advocates for marriage (Popenoe 1996; Waite and Gallagher 2000) if one also acknowledges that the marriages that, on average, confer benefits are also likely to be those that are successful and enduring.

All married couples have their ups and downs, and not every marriage will last “‘til death do us part.” It goes without saying that divorce has existed in all the societies for all of recorded human history (Goode 1993), which is the hard evidence reflecting the diversity of family life. If what marital disruption actually does is to selectively end those dysfunctional families—those parents who have failed to live up to their marriage vows and play their parental roles—while maintaining successful marriages, one might expect that child outcomes might very well be worse for bad marriages and parents and better for good marriages and parents, regardless of legal marital status and living arrangements. Among severely troubled families, it may even be the case that the “true” effect of divorce for those whose parents who ended a “bad” marriage will result in improvements in the well-being for these children of divorce.

2.7.2 Speculation on Gender Differences

Although findings in this chapter point to gender differences, these findings are difficult to interpret because the gender interactions are not statistically significant at the .05 level. Nevertheless, I believe that it is perhaps worthwhile speculating on these gender differences in light of the dynamic selection arguments and empirical results from the random trends models. As shown in Table 2.8, parental divorce is estimated to yield an increase of about .45 behavior
problems for both boys and girls, controlling only for selection on time-invariant effects of the unobservables. Once we control for the time-varying effects of the unobservables, divorce still yields an increase of .45 behavior problems for girls, but yields a decrease of .41 behavior problems for boys. At the risk of overinterpreting this result, it is possible that what might lie behind this finding is that father absence in a dysfunctional or conflictual family removes the negative “role model” for boys in a way different from girls, a speculation consistent with a recent study that finds the effect of father absence depends on the anti-social personality trait of the father (Jaffee et al. 2003). If so, it may be that continuing coresidence with mother does not change the gender-specific dynamics for girls. I leave the test of to what extent this explanation is valid for future research.

2.7.3 Policy Implications

This chapter finds no evidence for an effect of parental divorce on children’s behavioral problems, thus implying that divorce is neither harmful nor helpful for this measure of children’s well-being. If so, a potential implication is that public policy should be neutral with respect to marriage versus divorce. This prescription is indeed contradictory to not only what the advocates for marriage have advocated but for also to much of what is taken for granted in this country. For policy to remain neutral with respect to parent’s legal marital status will imply, for example, the removal of all the tax incentives (and penalties) on the basis of marital status. In other words, the standard deduction for a married couple should simply be twice of the amount for a single person.\textsuperscript{12} Of course, the tax laws are just an example of which public policies may influence

\textsuperscript{12}This is, interestingly, a tax law that the legislature in Taiwan has passed in 2005.
on. Other areas may include parental leave policies, welfare benefits, and so forth. As for the debate taking place within people, the results reported in this chapter suggest that the decision to end a marriage or stay together for the sake of the kids would neither hurt nor help their children’s emotional well-being. Hence, parents in an unhappy marriage should perhaps look elsewhere than over-emphasizing on the decision of divorce itself if they want to protect their children’s emotional well-being. This implication indeed echoes the observation made by psychologists about why counseling has often failed to save a marriage (Hetherington 2002). A family is unlikely to function well if there are “contextual factors”, such as financial difficulties, causing strains on family members (Karney and Bradbury 2005). The detriments to the well-being of family members caused by these other factors cannot be improved by the status of the marriage or by the counseling sessions. The results of this chapter and findings of these other researchers suggest that the direction for future research should perhaps be identifying what these specific proximate (or contextual) factors are and how they work to affect the well-being of family members, rather than identifying the effect of changes in marital status.

\[13\]

In the end, I predict the ultimate form of such policy neutrality will likely be a manifestation of the “institutionalized individualism” promoted by Beck and Beck-Gernsheim (2002). Again, I will not extrapolate my finding to suggest that we should stay policy neutral regarding marital status for other purposes than protecting the emotional well-being of children unless we have empirical studies that reach the same conclusion. Thus, I am not promoting the same modern view of the world as they are.
Chapter 3
What Money Can and Cannot Buy: Divorce, Gender, and Adult Health

3.1 Introduction

At the turn of millennium, data collected by the U.S. Census Bureau showed that 17% of divorced adults assessed their health status as fair or poor, compared to 11% married adults; 37% of divorced adults, compared to 30% of married adults, reported limitations in physical and social functioning; twice as many divorced adults than married adults had activity limitations in work, daily living, and instrumental daily living activities; divorced adults, compared to married adults, were also more likely to suffer from low back pain, headaches, serious psychological distress, and become heavy smokers and drinkers (Schoenborn 2004). These differences in health behaviors and health outcomes, not surprisingly, also reflect in the mortality rate differentials (Gove 1973; Goldman 1993; Lillard and Waite 1995). Despite the persistent and pronounced health dispari-

\[\text{1The only health indicator on which the married appeared to fare less well than the divorced is obesity. Married men, in particular, were more likely to be overweight or obese.}\]
ties by marital disruption, it is unclear whether divorce *causes* poor health, the less healthy is more likely to divorce, or both. A large social science literature has devoted to the task of identifying causation versus selection in explaining the post-divorce health disparities.

This chapter joins the conversation and contributes to the literature in several ways: First, much of the previous research has examined the differences in health by marital status, and infrequently conducted a specific and focused design to study the effect of marital disruption. It should be noted that the entry into marriage and exit from marriage are very different social processes. Pooling all marital statuses and transitions makes the estimation task extremely complicated because one has to distinctly identify the selection into marriage, the selection out of marriage, and the effects of various ways of exiting marriage by multiequation models. Only a handful of empirical studies (e.g., Lillard and Waite 1995; Lillard and Panis 1996) have taken on this task, and even those have done so inevitably rely on relatively strong statistical assumptions concerning the parametric relationships among social processes. I employ a simple design by restricting attention to the disruption of first marriages to focus on examining the effect of divorce. Second, I apply a propensity score method developed from the counterfactual causality framework in estimating the effect of divorce. The estimate can be explicitly interpreted as “the average treatment effect of divorce for the divorced”, and makes the counterfactual comparison with what a divorced individual would have fared had s/he not experienced the divorce. This way of posing the question is particularly relevant for the current debates on marriage promotion because social policy directed at preventing divorce should not be based on conclusions for an average individual randomly drawn from the population—who might be happily married and who may never
have considered a marital disruption. Third, while much of the empirical literature has looked at the effect of divorce on indicators of psychological well-being, especially depressive symptoms, a surprisingly few studies have examined self-reported general health status—an outcome frequently analyzed in the health-disparity literature—and no prior study has examined composite measures of mental health and physical health. Hence, although the advocates for marriage promotion have often made the sweeping claim that marriage makes people healthy and divorce makes people sick (instead of referring to specific health outcomes), the empirical evidence rarely extends beyond a limited number of specific health outcomes and hence does not speak to overall health. This discrepancy between claims and evidence is particularly perplexing because a close examination of the literature suggests that, despite strong associations in almost all health outcomes, not all of the associations appear to be causal. In addition, selection and causal mechanisms may work differently for different health outcomes and sometimes by gender. I follow the long-standing interest since Bernard (1972) and Gove (1973) to examine to what extent the health effects of divorce differ between men and women.

3.2 Causation versus Selection

Despite the strong association, it is unclear whether poorer health is caused by marital disruption. Not only are healthier individuals in higher demand on the marriage market, but spouses tend to select on other characteristics, often observed to the researcher, that are positively associated with health, e.g., education, income, and personality (Goldman 1993; Waldron et al. 1996; Wyke and Ford 1992). Therefore, the apparent health disadvantage for the
divorced may be due to socioeconomic or even psychological disadvantages for the divorced, rather than a consequence of divorce. For example, Yamaguchi and Kandel (1997), using longitudinal data of married couples in the State of New York, found that the fact that divorce is associated with higher marijuana use is largely due to selection. Spouses with higher tendency to use marijuana are also more likely to divorce and continue using drugs. On the supply side of the marriage market, Lillard and Panis (1996) noted an “adverse selection” mechanism by which they meant that the unhealthy have greater incentives to get married because they need someone to take care of them. The adverse selection works in the reverse direction and is expected to reduce the health disparities across marital status. Their analysis of men in the Panel Study of Income Dynamics showed that both selection mechanisms are in operation but under different circumstances. Adverse selection dominates for self-reported health and among older men, with those who were over 50 years of age and who perceived themselves as relatively unhealthy tending to remarry quickly and to stay married longer, whereas the positive selection into marriage dominates for never-married men on unmeasured characteristics such as health behaviors and life styles. Overall, the positive selection into marriage prevails and the high mortality rates among the divorced men are explained largely by their poorer health relative to the married men.

The association between divorce and health may be causal because divorce is a major life event that increases stress levels (Bloom et al. 1978). Coping with stress takes energy and resources, and exposure to stress is linked to lower psychological well-being, depressed immune functions, and poor physical health (Pearlin and Johnson 1977; Pearlin et al. 1981). Not only is divorce itself a stressor, it also induces a variety of other stressors in that the event
is often associated with a substantial loss in household income and changes in
residence and employment status (Holden and Smock 1991; South et al. 1998).
These stresses may create a mere temporary crisis or become long-lasting strains
(Amato 2000). Substantial declines in economic resources after divorce have also
been argued to be an independent source of health differential and to multiply
the effects of stress (Hahn 1993). For example, Aseltine and Kessler (1993)
found that, although increased financial difficulty after divorce itself cannot ex-
plain the association between divorce and depression, the divorced become more
emotionally vulnerable to external stressors.

Divorce may also be harmful to health because it removes the benefits pro-
vided by the higher level of social, economic, and emotional resources in mar-
riage. Married people typically pool resources and are economically better off
than their unmarried counterparts, and economic well-being is positively associ-
ated with health (Smith 1999). Marriage is a form of objective social integration
that prevents individuals from the detrimental health effects of social isolation
(House et al. 1988). Marriage may provide protective effects on mental health
through social support in intimate relationships (Ross et al. 1990). Marriage
may improve physical health through the social control of healthy behaviors.
Social control between spouses may exert its effects through internalization of
normative healthy behaviors (the commitment effect) and informal sanctions of
deviant healthy behaviors (the “nagging” effect). Married people are less likely
to engage in risk taking behaviors and substance use, and have better habits of
diet, sleep, and exercise (Umberson 1987; 1992). Consistent with this reason-
ing, Lillard and Waite (1995) found that, mortality risks decrease as marriage
duration cumulates; and after divorce, the mortality risks bounce back to the
level when individuals are never married.
Although the majority of the literature assumes that marriage benefits individual health whereas divorce does harm, the degree to which marriage benefits health varies. Hawkins and Booth (2005), for example, found that staying in unhappy marriages is associated with lower levels of overall health, happiness, and self-esteem, and life satisfaction. The variation may depend on the quality of the marriage, and so does the effect of marital disruption (Gallo et al. 2003; Gove et al. 1983). Ending a bad marriage may indeed bring a relief and improves mental health (Wheaton 1990). Kalmijn and Monden (2006) further showed that the effect of divorce differ by the various dimensions of marriage quality. Women who ended a relatively unsatisfying or unfair marriage had only slightly higher level depression than those who ended a relatively satisfying or fair marriage. Those who ended a marriage marked by higher conflict were about as depressed as those who ended a marriage with lower conflict. However, those who ended a highly aggressive marriage were more depressed than those who ended a less aggressive marriage.

Moreover, causal mechanisms and selection mechanisms need not be mutually exclusive. Indeed, most studies find that the association between marital disruption and divorce is smaller when selection factors are controlled but remains statistically significant—this pattern is generally interpreted as providing support for both hypotheses (Waldron et al. 1996). Two longitudinal studies using European data provide more direct evidence for the coexistence of causation and selection: Lucas (2005), using 18 years of longitudinal data from the German Socio-Economic Panel Study, found that life satisfaction declines prior to marital disruption and then gradually rebounds after divorce. However, the level of post-divorce life satisfaction is below the initial level in the marriage. This temporal pattern provides a relatively clear picture of how both
causal and selection mechanisms operate at the same time. Wade and Pevalin (2004), using longitudinal data from the British Household Panel Survey, found that, consistent with the selection argument, those who subsequently divorced had poor mental health prior to marital disruption. They also found that their mental health is the worst around the time of marital disruption and remains worse after the disruption, which they interpreted as consistent with the causal argument.

3.3 Variations by Health Outcome and Gender

Bernard (1972) made a famous observation that there are two realities of a marriage, one for men and the other for women. She argued that marriage benefits men but hurts women because the gender roles constrain women to take the majority of household responsibilities while men reap the benefits of women’s household production activities. Indeed, early research on the health disparities by marital status found evidence consistent with this gender difference. Waite and Gallagher (2000) disputed Bernard’s position, and argued that the health of both men and women benefit from being married, although the mechanisms might differ. They suggested marriage provides emotional support for both men and women; men are helped by wife’s specializing in household work and restraining husband’s risk-taking and unhealthy behaviors; for women, their husband’s additional income is crucial. By implication, divorce hurts both men’s and women’s health by reducing mutual emotional support as well as the social and economic resources spouses provide to each other. Empirical evidence regarding gender differences in the effects of divorce is mixed and varies by health outcomes.
Bruce and Kim (1992) found that divorce increases the psychiatric disorder of major depression for both men and women, but divorced men have a greater risk of a first-onset in a community sample. Aseltine and Kessler (1993), using two waves of longitudinal data of a community sample, found that divorce increases depression only among those who did not report serious marital problems prior to the separation, but the effect of divorce on depression among those who did not report having marriage problems is more pronounced for women than men.

An analysis of a 1974-1975 U.S. national sample by Umberson (1987) found that divorced people are more likely to engage in unhealthy, risk-taking behaviors (except marijuana use), controlling for race/ethnicity, age, education, and income. Divorced men are more likely to have drinking problems than divorced women. Using 1986-1994 waves of longitudinal data from the Americans' Changing Lives, Williams (2003) found no gender difference between divorced men and women on depression (CES-D) and life satisfaction but, among the divorced, remarried men fare better than remarried women.

Studies analyzing the 1987-1988 and 1992-1994 waves of panel data from the National Survey of Families and Households yield contradictory findings: Williams and Dunne-Bryant (2006) found that the effects of divorce on depression, alcohol use and global happiness at the second wave remain statistically significant after controlling for the same outcome measured at the first wave. Divorced women have more depressive symptoms than divorced men, but there is no gender difference in alcohol use and global happiness. Marks and Lambert (1998) found that divorced women appear to be slightly (i.e., mostly statistically insignificant coefficients) worse off than divorced men on a number measures of psychological well-being, e.g., depression, hostility, global happiness, self-
esteem, personal mastery, autonomy, personal growth, etc. Simon (2002) found that divorce increases depression and alcohol use for both men and women. Divorced women are more depressed than divorced men, but divorced men tend to (the coefficient is statistically insignificant) be more likely to increase their alcohol use than divorced women. Kalmijn and Monden (2006) found that divorce has no overall effect for men but increases the depressive symptoms for women.

Gove (1973) documented mortality differentials by marital status, with the divorced having highest mortality rates followed by the widowed and the married. The differential by divorce is more pronounced for men than for women. Goldman (1993) noted that much of the mortality differentials by marital status may be due to selection of the socioeconomically advantaged into marriage. Hemström (1996) found that the mortality differential by divorce is greater for men than for women. However, part of the differential can be explained by controlling for employment status and the presence of children. Rogers (1995) conducted a case-control study of mortality using the 1986 National Health Interview Survey, and found that divorced people have higher mortality rates, with the difference being more pronounced for men than for women. For women, divorced women have higher rates of death due to accidents and suicide. Divorced men have higher mortality rates for all causes of death.

The literature examining the effect of divorce on health outcomes other than specific mental health outcomes and mortality is surprisingly thin. No study thus far has examined composite measures of physical health and mental health. Blekesaune and Barrett (2005) examined sick leave and receipt of health-related benefits and found that people with poor health are more likely to be selected out of marriage through divorce. While there is a small effect of divorce, the
effect tends to be only temporary. Zhang and Hayward (2006), using five waves of panel data from the Health and Retirement Survey (HRS), found that marital disruption is associated with elevated risks of cardiovascular disease for women, but not for men; however, even the association for women is not causal and only reflects differences in socioeconomic status and emotional distress. Wu and Hart (2002) found that divorce decreases functional health and general health status and increases depression for Canadian men, but has no statistically significant effect for women. Williams and Umberson (2004), examining longitudinal data from the Americans’ Changing Lives Survey, found that divorce worsens self-reported general health status of older men, but improves that of younger men and all adult women, especially women at older ages.

3.4 Data

3.4.1 National Longitudinal Survey of Youth 1979

The National Longitudinal Survey of Youth 1979 (NLSY79) is a prospective survey of a nationally representative random sample of 12,686 young adults ages 14 to 22 in 1979, with follow-ups conducted annually until 1994 and biennially from 1996 onwards. The initial wave includes three components—a main sample of 6,111 individuals, an over-sample of 5,295 racial minorities and poor non-Hispanic whites, and a sample of 1,280 military personnel. All household members within the age range as of 1979 are included in the sample, and were interviewed annually from 1979 to 1994 and biennially from 1996 onwards. The sample attrition has been very low. The retention rates were either above or close to 90% for the 1979-1994 surveys, above 80% for the next 1996-2000 surveys, and 77.5% for the 2002 survey. The response rates were above 90% until
1994, and above 80% from 1996 to 2002. Over 80% of the original NLSY79 respondents remaining in the eligible sample were reinterviewed until 1987, over 70% from 1988 to 1994, and over 60% from 1996 to 2002. The racial compositions of the respondents are comparable (with less than 1% differences) between 1979 and 2002 (National Longitudinal Surveys, 2004, *NLSY79 User’s Guide: 1979-2002*).

### 3.4.2 Dependent Variables

The SF-12 (see Appendix C for a list of items in the scale) measures the respondents’ mental and physical health irrespective of their proclivity to use formal health services, and was administered to the NLSY79 respondents ages 40 and over in 1998, 2000, 2002, and 2004. These data were collected only once when the respondent first met the age criterion when interviewed, and thus provided only a snapshot of his/her health status in the middle age. I use three outcomes drawn from the SF-12 scale: A self-reported single item assessing the general health status of the respondent, the composite score of physical health, and the composite score of mental health. All three outcomes are coded such that a higher score means better health. As shown in Table 3.1, divorced men and women on average have worse health, although the differences are not very large: Divorced men scored .16 points lower than married on a five-point Likert

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2Response rate is defined as the percentage of base-year respondents remaining eligible and not known to be deceased who were interviewed in a given survey year. Retention rate is defined as the percentage of base-year respondents within each sample type remaining eligible who were interviewed in a given survey year. The difference is that the latter includes in the eligible sample those deceased and difficult-to-field respondents whom the National Opinion Research Center did not attempt to contact.

3The question asks “In general, would you say your health status is . . . .” The respondent will report from one of the following five categories: “excellent”, “very good”, “good”, “fair”, or “poor”.

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Table 3.1: Means and Standard Deviations for Dependent Variables: Divorced and Continuously Married Women and Men

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Men</th>
<th>Women</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Married</td>
<td>Divorced</td>
<td>Married</td>
<td>Divorced</td>
</tr>
<tr>
<td>General health</td>
<td>3.84 (0.92)</td>
<td>3.68 (1.02)</td>
<td>3.78 (0.96)</td>
<td>3.53 (1.06)</td>
</tr>
<tr>
<td>Physical health (SF12)</td>
<td>53.10 (6.54)</td>
<td>52.56 (7.46)</td>
<td>52.76 (7.25)</td>
<td>50.88 (9.49)</td>
</tr>
<tr>
<td>Mental health (SF12)</td>
<td>55.05 (6.04)</td>
<td>53.44 (8.49)</td>
<td>52.99 (7.81)</td>
<td>51.34 (9.21)</td>
</tr>
</tbody>
</table>

Note: General health is measured on a 5-point Likert scale,

scale for general health status, while the difference between divorced and married women is .25 points. The difference in physical health is .54 for men and 1.88 for women, and the difference in mental health is 1.61 for men and 1.35 for women—all on scales, by design, with a mean of 50 and a standard deviation of 10.4

3.4.3 Measures of Divorce and Marriage Duration

I focus the analysis of this chapter on the disruption of respondents’ first marriage. I construct a marital history for the respondents from the questions about their current marital status and the changes in marital status between survey interviews. I code marital disruption for their first marriage as the number of

4Table 3.1 shows that the analytic sample has means that are 2-5 points higher than the designed means, and standard deviations that are smaller than the designed standard deviations. This suggests that the sample might be selective on those who are, on average, healthier and more homogeneous than the general population (or the sample that the SF-12 developers used to construct the norm).

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months between the reported wedding date and the reported date of separation
or divorce—whichever comes first. I exclude those cases whose first marriages
ended in spousal death or for unidentified reasons. Thus, the “treatment” of
interest is marital disruption—whether a respondent’s first marriage ended in
either separation or divorce prior to last interview.

3.4.4 Control Variables

Controls for the respondent’s sociodemographic characteristics include dummy
variables for race and ethnicity (coded 1 for black and for Hispanic as mutually
exclusive categories, with non-black-non-Hispanic being the omitted reference
group), dummy variables for his/her education level, an age-standardized sum-
mary score on the Armed Forces Qualifying Test (AFQT), self-esteem measured
in 1980, respondent’s age at first birth, missing age at first birth (which stands
largely for childlessness). I control for the respondent’s family background with
the following variables: whether the respondent was raised in the Catholic faith;
an index ranging from 1 to 6 for how often the respondent attended church in
1979; mother’s years of schooling, socioeconomic index of the father (or male
adult in the household) when the respondent was 14 years old; dummy vari-
ables equal to 1 if the respondent’s father (or adult male) was unemployed or
absent when the respondent was 14; an index ranging from 0 to 3 defined by
adding dummy variables equal to 1 if magazines, newspapers, or library cards
were present when the respondent was 14; and whether the respondent lived
in the South or in an urban area at age 14. In addition, I control for dummy
variables indicating whether respondent’s biological mother and father had any
major health problems, which is part of the health module asked of respondents
over age 40. Ailing parents may impose stress on the respondent’s marriage,
which may both increase the likelihood of divorce and decrease the respondent’s perceived health. Appendix A describes the definition and construction of these control variables. Table 3.2 presents descriptive statistics for the background variables for both men and women.

Table 3.2: Unweighted Means and Standard Deviations for Background Variables: Divorced and Continuously Married Women and Men

<table>
<thead>
<tr>
<th>Background Variable</th>
<th>Men</th>
<th>Women</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Married</td>
<td>Divorced</td>
<td>Married</td>
<td>Divorced</td>
</tr>
<tr>
<td>Black</td>
<td>.19</td>
<td>.30</td>
<td>.20</td>
<td>.29</td>
</tr>
<tr>
<td>Hispanic</td>
<td>.19</td>
<td>.20</td>
<td>.18</td>
<td>.21</td>
</tr>
<tr>
<td>Education (&lt; 12 years)</td>
<td>.16</td>
<td>.26</td>
<td>.11</td>
<td>.20</td>
</tr>
<tr>
<td>Education (12 years)</td>
<td>.34</td>
<td>.40</td>
<td>.33</td>
<td>.37</td>
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<td>Education (13-15 years)</td>
<td>.21</td>
<td>.22</td>
<td>.25</td>
<td>.27</td>
</tr>
<tr>
<td>Education (&gt;= 16 years)</td>
<td>.30</td>
<td>.12</td>
<td>.31</td>
<td>.15</td>
</tr>
<tr>
<td>Age at 1st marriage</td>
<td>26.3</td>
<td>23.3</td>
<td>24.7</td>
<td>21.4</td>
</tr>
<tr>
<td></td>
<td>(5.4)</td>
<td>(4.2)</td>
<td>(5.7)</td>
<td>(4.3)</td>
</tr>
<tr>
<td>Age at 1st birth</td>
<td>26.5</td>
<td>24.1</td>
<td>24.9</td>
<td>22.0</td>
</tr>
<tr>
<td></td>
<td>(5.4)</td>
<td>(5.1)</td>
<td>(5.3)</td>
<td>(5.0)</td>
</tr>
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<td>.14</td>
<td>.11</td>
<td>.12</td>
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<td>.34</td>
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<td>.01</td>
<td>.01</td>
<td>.01</td>
</tr>
<tr>
<td>Religiosity 1979</td>
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<td>3.6</td>
<td>3.3</td>
</tr>
<tr>
<td></td>
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<td>(1.7)</td>
<td>(1.7)</td>
<td>(1.7)</td>
</tr>
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<td>.01</td>
<td>.01</td>
<td>.01</td>
</tr>
<tr>
<td>AFQT score</td>
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<td>46.5</td>
<td>35.3</td>
</tr>
<tr>
<td></td>
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<td>(26.9)</td>
<td>(27.9)</td>
<td>(25.0)</td>
</tr>
<tr>
<td>AFQT missing</td>
<td>.06</td>
<td>.05</td>
<td>.04</td>
<td>.04</td>
</tr>
<tr>
<td>Self esteem 1980</td>
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<td>32.5</td>
<td>32.5</td>
<td>32.0</td>
</tr>
<tr>
<td></td>
<td>(3.9)</td>
<td>(4.0)</td>
<td>(4.0)</td>
<td>(3.9)</td>
</tr>
<tr>
<td>Self esteem missing</td>
<td>.05</td>
<td>.04</td>
<td>.04</td>
<td>.05</td>
</tr>
<tr>
<td>Foreign born</td>
<td>.08</td>
<td>.07</td>
<td>.08</td>
<td>.07</td>
</tr>
<tr>
<td>Reading materials present</td>
<td>.47</td>
<td>.38</td>
<td>.50</td>
<td>.38</td>
</tr>
<tr>
<td>Intact Family</td>
<td>.76</td>
<td>.65</td>
<td>.77</td>
<td>.64</td>
</tr>
<tr>
<td>South residence</td>
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<td>.33</td>
<td>.39</td>
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<tr>
<td>Urban residence</td>
<td>.76</td>
<td>.80</td>
<td>.78</td>
<td>.79</td>
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*Continued on next page...*
### Table 3.2 continued

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<tr>
<td></td>
<td>Married</td>
<td>Divorced</td>
</tr>
<tr>
<td>Father’s education (years)</td>
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<td>10.7</td>
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</tr>
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<td>Mother’s education (years)</td>
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<td>10.7</td>
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<td></td>
<td>(3.3)</td>
<td>(3.0)</td>
</tr>
<tr>
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<td>.41</td>
<td>.42</td>
</tr>
<tr>
<td>Father health problem missing</td>
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<td>.08</td>
</tr>
<tr>
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<td>.40</td>
</tr>
<tr>
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<td>.02</td>
</tr>
<tr>
<td>Number of cases</td>
<td>1,520</td>
<td>1,119</td>
</tr>
</tbody>
</table>

Note: Standard Deviations are in parentheses.

*When respondent was 14 years old.*

### 3.4.5 Sample Restrictions

I restrict the NLSY79 sample to those 9,619 cases who have ever been married and with valid data on dates of first marriage. I further delete 181 cases whose first marriage has ended by the date of 2004 interview but without valid information on the date of marital disruption. Of the 7,790 respondents with valid data on the dependent variables of SF-12, I excluded (1) those who had never been married when the dependent measures were taken or illogical first marriage history (N=1,667), (2) those whose first marriages ended in spousal death or unknown reasons (N=115), and (3) those in the military sample (N=193). I then delete those respondents with missing data on highest grade completed, foreign-born status, family structure at age 14, and reading materials presence at age 14 (total N=92). These restrictions lead to a base sample of 5,723 for the analysis.
A unique design feature of the NLSY79 is that the initial 1979 survey included all youths within the age range of 14-22 living in the sampled household. Thus, there could be multiple respondents of varying relationships—including siblings, spouses, cousins, etc.—interviewed in a household. In fact, 53 percent of the original 12,686 respondents resided in a multi-respondent household (see NLSY79 User’s Guide, chap. 4, p. 195). In the siblings fixed effects analysis, I construct a sub-sample from household rosters to include only respondents with one or more siblings in the original NLSY79 sample. Table 3.3 compares the base sample and the restricted sibling sample.

Table 3.3: Means and Standard Deviations for Variables: Men and Women for Base and Sibling Samples

<table>
<thead>
<tr>
<th>Variable</th>
<th>Men Base</th>
<th>Men Sibling</th>
<th>Women Base</th>
<th>Women Sibling</th>
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<td>Treatment Variable</td>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Separation/Divorce</td>
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<td>.39</td>
<td>.50</td>
<td>.46</td>
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<td>Dependent Variables</td>
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</tr>
<tr>
<td>General health</td>
<td>3.77</td>
<td>3.77</td>
<td>3.65</td>
<td>3.67</td>
</tr>
<tr>
<td></td>
<td>(0.97)</td>
<td>(0.97)</td>
<td>(1.02)</td>
<td>(1)</td>
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<tr>
<td>Physical health (SF12)</td>
<td>52.87</td>
<td>52.87</td>
<td>51.81</td>
<td>52.28</td>
</tr>
<tr>
<td></td>
<td>(6.96)</td>
<td>(6.84)</td>
<td>(8.5)</td>
<td>(7.95)</td>
</tr>
<tr>
<td>Mental health (SF12)</td>
<td>54.35</td>
<td>54.56</td>
<td>52.16</td>
<td>52.36</td>
</tr>
<tr>
<td></td>
<td>(7.25)</td>
<td>(7.12)</td>
<td>(8.58)</td>
<td>(8.24)</td>
</tr>
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<td>Background Variables</td>
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<td>.25</td>
<td>.26</td>
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<td>Hispanic</td>
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<td>.19</td>
<td>.20</td>
<td>.18</td>
</tr>
<tr>
<td>Education (&lt; 12 years)</td>
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<td>.13</td>
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<td>.36</td>
<td>.38</td>
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<td>.34</td>
</tr>
<tr>
<td>Education (13-15 years)</td>
<td>.21</td>
<td>.20</td>
<td>.26</td>
<td>.26</td>
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*Continued on next page...*
... table 3.3 continued

<table>
<thead>
<tr>
<th>Variable</th>
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<th>Men Sibling</th>
<th>Women Base</th>
<th>Women Sibling</th>
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<tbody>
<tr>
<td>Education (&gt;= 16 years)</td>
<td>.22</td>
<td>.22</td>
<td>.23</td>
<td>.27</td>
</tr>
<tr>
<td>Age at 1st marriage</td>
<td>25</td>
<td>25.5</td>
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<td>(5.1)</td>
<td>(5.3)</td>
<td>(5.4)</td>
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<tr>
<td>Age at 1st birth</td>
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<td>25.7</td>
<td>23.4</td>
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<td>.12</td>
<td>.11</td>
<td>.12</td>
</tr>
<tr>
<td>Catholic</td>
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<td>.37</td>
<td>.39</td>
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<td>.01</td>
<td>.01</td>
<td>.01</td>
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<tr>
<td>Religiosity 1979</td>
<td>3.09</td>
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<td>3.46</td>
<td>3.64</td>
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<tr>
<td>Religiosity 1979 miss</td>
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<td>.01</td>
<td>.01</td>
<td>.01</td>
</tr>
<tr>
<td>AFQT score</td>
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<tr>
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<td>.04</td>
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<td>.03</td>
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<tr>
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<td>(3.9)</td>
<td>(3.9)</td>
<td>(3.9)</td>
<td>(4.0)</td>
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<tr>
<td>Self esteem 1980 missing</td>
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</table>
3.5 Method

3.5.1 Statistical Models

I use multiple methods to examine the effects of marital disruption. The ordinary least square (OLS) regressions control for selection on sociodemographic factors and family background by linearly adjusting the covariance:

\[ y_{ji} = \theta \cdot D_i + \beta x + e_{ji}, \] (3.1)

where \( y_{ji} \) is the health measure for individual \( i \) growing up in household \( j \).

The OLS regression models may be inadequate because it assumes selection only on observed variables. To eliminate potential bias from unobserved factors that are shared among family members, I estimate a fixed effects model that specifies a unique intercept \( c_j \) for all siblings in the same household \( j \) (Griliches 1979; Quesnel-Vallée 2004):

\[ y_{ji} = \theta \cdot D_{ji} + \beta x + c_j + \epsilon_{ji}. \] (3.2)

Under the sibling fixed-effects specification, if siblings growing up in the same household have worse health outcomes and a higher chance of divorce either through shared social and physical environment or through heredity, OLS estimates of marital disruption \( \theta \) will be upwardly biased.

The OLS model also assumes that a linear combination of the covariates will ensure comparability between the divorced and the continuously married. However, this parametric assumption may not hold. The problem worsens if there is lack of support at certain regions of distributions of potential confounding variables. The propensity score method described in Chapter 4 and results presented in Appendix D address these two potential pitfalls. Specifically, I
construct a propensity score for each individual using a logistic model regressing the log-odds of marital disruption on covariates used in the OLS model. I then subclassify the sample both by quintile of propensity score and quintile of time of exposure to the risk of divorce. For each stratum $k$ (where $m_k$ out of the $n_k$ cases in the stratum are divorced), I estimate a simple OLS regression:

$$y_{ki} = \theta_k \cdot D_{ki} + \nu_{ki}.$$  

(3.3)

I then obtain the average treatment effect of marital disruption for those who are divorced (or average treatment effect on the treated, ATT) by averaging the stratum-specific effect $\theta_k$ with the number of divorced cases $m_k$ being weights (Gelman and Hill 2007:204-6):

$$ATT = \frac{\sum \theta_k \cdot m_k}{\sum m_k},$$  

(3.4)

with a standard error averaging over the stratum-specific standard error $s_k$’s:

$$S_{ATT} = \sqrt{\frac{\sum (s_k^2 \cdot m_k^2)}{(\sum m_k)^2}}.$$  

(3.5)

### 3.5.2 Average Treatment Effect on the Treated

Subclassification on the propensity score and the exposure time is superior to OLS regression not only because it makes fewer parametric assumptions but also because it provides estimates that can be explicitly interpreted as “the average treatment effects on the treated” (hereafter, ATT). The ATT gives the effect of divorce for those who are divorced, rather than for an average person randomly drawn from the general population, which will inevitably include people whose marriage is so satisfying that they will never even consider a divorce. The ATT is also derived from the counterfactual causality framework in which the treatment
effect is explicitly defined as a comparison between what would have happened to a person’s health had s/he not experienced divorce. In the following, I briefly describe the ATT in a counterfactual framework, and the conditions under which the ATT will be the same as the “average treatment effect” for the general population (hereafter, ATE).

Recall the counterfactual model for causal inference introduced in Chapter 4. Let $D$ be the dummy indicator for marital disruption; then the outcome $Y$ will be observed according to the following equation (suppressing individual indicator $i$):

$$Y = D \cdot Y_1 + (1 - D) \cdot Y_0.$$  

(3.6)

Define

$$\begin{align*}
Y_0 &= E(Y_0|X) + U_0 = \mu_0(X) + U_0 \\
Y_1 &= E(Y_1|X) + U_1 = \mu_1(X) + U_1
\end{align*}$$

(3.7)

where $E(U_0|X) = 0$ and $E(U_1|X) = 0$. Insert the above into (3.6), we obtain:

$$Y = \mu_0(X) + D \cdot \{[\mu_1(X) - \mu_0(X)] + [U_1 - U_0]\} + U_0.$$  

(3.8)

The treatment effect is $\mu_1(X) - \mu_0(X)$, the effect for an average person with characteristics $X$, and $U_1 - U_0$, the idiosyncratic effect for a particular person.

The ATE with characteristics $X$ in the population is

$$E(\Delta|X) = \mu_1(X) - \mu_0(X).$$  

(3.9)

The ATT for persons with characteristics $X$ is

$$E(\Delta|X, D = 1) = \mu_1(X) - \mu_0(X) + E(U_1 - U_0|X, D = 1).$$  

(3.10)

If $E(U_1 - U_0|X, D = 1) = 0$, the two are the same quantity. This happens if $U_1 - U_0 = 0$, or if agents either do not know $U_1 - U_0$ or do not act on their
knowledge of $U_1 - U_0$. In brief, $U_1 - U_0$ is the idiosyncratic effect of divorce for a particular couple with characteristics $X$ that is unobserved/unknown to the analyst. When we want to know whether the behavioral model correspond to reality, we must ask whether a couple know their own $U_1 - U_0$, and whether a couple act upon their knowledge of $U_1 - U_0$. If $E(U_1 - U_0|X, D = 1) \neq 0$, the estimates should be interpreted as the ATT.

### 3.5.3 Heterogeneity of Effects

Following Morgan (2001), I will also explore the heterogeneity of the effects of divorce across strata. This exercise helps demonstrate to what extent an interpretation that implicitly assumes a constant effect for all individuals in the population (if the estimate is interpreted as ATE, and in the divorced population if the estimate is interpreted as ATT) is warranted. This speaks to the claim that divorced people experience a wide range of life trajectories, and some divorces may indeed yield a beneficial effect for the divorced. The other purpose of this examination of the heterogeneity of effects is to explore the adequacy of the “no-hidden-bias” assumption. Biases due to unobserved factors are plausible if there is substantial heterogeneity of the estimated effects across subgroups and particularly if the heterogeneous effects vary in a systematic way. If there are biases not captured by the observed covariates, then one cannot interpret the estimated coefficients as “effects.” The estimates can only be as good as the conditioning on the selected observables works. I will use the language of the effect of divorce for the divorced men and women (i.e., corresponding to ATT) in this chapter, although I do not imply that the statistically significant coefficients should necessarily be interpreted as causal.
3.6 Results

Despite wide belief in the gender differentials in the health benefits of marriage since the classic thesis of “her marriage” versus “his marriage” (Bernard 1972), I find gender differences in physical health only, and no difference by gender in general health status and mental health.

Table 3.4 presents results for the commonly used self-reported general health status. On average, divorced men score -.17 lower than married men and divorced women score -.25 lower than married women on a five-point scale (Model 1). These differences are statistically significant at the .01 level. Some of these differences appear to reflect differences in sociodemographic characteristics and, to a lesser extent, differences in family background. Controlling for these factors yields much smaller coefficients than the zero-order associations reported in Model 1. For both men and women, the coefficients decline by about 2/3 in magnitude in Models 4-6, and are statistically significant at the .05 level for women, but not for men.

Table 3.5 presents results for physical health measured as a composite score using the SF-12 scale. On average, divorced men score .57 points lower than married men and divorced women score almost 2 points lower than married women in their physical health composite score on the SF-12 scale. For women, about half of the difference is due to differences in sociodemographic and family background between the two groups and cannot be attributed to the effect of divorce. The coefficient reduces to about 1 point, but remains statistically significant. For men, marital disruption appears to have no effect on physical health. Controlling for family background factors reduces the coefficient by 5 The standard error in the fixed-effects model is inflated, so that the coefficient in Model 5 is insignificant for women.
about half (Model 3), and further controlling for sociodemographic factors flips
the sign of the coefficients. In Model 4 and Model 6, marital disruption improves
men’s physical health by at least .1 point, although this improvement is minimal
and statistically insignificant.

Table 3.6 presents results for mental health. On average, divorced men and
women score more than 1.5 points lower than married men and women in their
mental health composite score on the SF-12 scale. These differences cannot be
explained by selection on sociodemographic and family background factors. The
coefficients remain approximately the same across all six models. The results
suggest that marital disruption has a detrimental effect on the mental health of
both men and women that is not due to selection on observables.

In brief, these results on the average effect of divorce for the divorced suggest
that divorced women would have fared better general health, physical health
and mental health had they remained married. However, divorced men would
have only enjoyed better mental health, but not general health status or physical
health, had they remained married.

I obtain qualitatively similar results using a different estimation strategy
to obtain the average effect of divorce for the divorced that subclassifies both
on the time-varying propensity score \( r(t) \) and marriage duration.\(^6\) I present
these results in Table 3.7 – Table 3.9, and show the heterogeneity of effects by
substrata. It is perhaps not surprising that the ranges of the estimated effects
are quite wide in these three tables. This is mainly due to the small sample
sizes in certain cells that increase the sampling variability. However, it is worth

\(^6\)They are not too similar if you look at the magnitudes of the coefficients. I believe
this stems from how well the subclassification in a 5 by 5 table perform in balancing
the propensity scores and marriage duration.
noting how many cells yield a positive estimate of the effect of divorce, meaning that divorce improves his/her health outcome compared to the counterfactual if s/he had remained in the marriage: 11 out of 24 cells for men and 7 out of 25 cells for women on general health status in Table 3.7; 12 out of 24 cells for men and 7 out of 25 cells for women on physical health in Table 3.8. Even in Table 3.9, 5 out of 24 cells for men and 8 out of 25 cells for women yield a positive coefficient on mental health. Although these results may be interpreted as nothing but reflecting the loss of efficiency in the subclassification estimation strategy, I believe these results more likely reflect the fact that the effects of divorce may be very heterogeneous.

3.7 Discussion

Largely consistent with prior research on specific health outcomes, I find strong associations between marital disruption and general measures of overall health, mental health, and physical health for both men and women in their early 40s. Are these associations causal? My estimates of the treatment effects of divorce for the divorced suggest that, holding constant a number of demographic and family background characteristics in a relatively nonparametric way, divorce hurts women’s health on all three general indicators. Although divorce appears to hurt men’s mental health, divorced men do not seem to suffer from a decline in self-reported global health or physical health.⁷

Although the analyses presented in this chapter have taken advantage of statistical techniques such as the propensity score subclassification and the sibling

⁷Although I follow the conventional language and call my estimates average treatment “effects” on the treated, they are only “effects” and not associations if the underlying assumptions (e.g., selection only on observables) hold. Hence, the reader should be cautioned not to confuse the language with its actual meaning.
fixed-effects models, the design of the NLSY79 with health outcomes measured only cross-sectionally is far from ideal in teasing apart causation and selection. The propensity score method works only as good as to the extent that the assumption of no hidden bias holds. The sibling fixed-effects model controls only for unobserved factors that are shared by individuals growing up in the same household. However, the health selection is perhaps more likely to occur through assortative mating (on health, and other socioeconomic characteristics related to health and divorce, e.g., education) or unobservable factors associated with both the individual’s health trajectory (e.g., disability) and divorce. If so, then a couple fixed-effects model or an individual fixed-effects model will be a better strategy to identify causal effects of divorce on health.

To the extent we have confidence in these estimates, divorce seems a painful psychological experience that not only makes people depressed (Simon 2002) but also deteriorates the overall mental health for both men and women. This is consistent with the argument that marital disruption is a stressful event that negatively affects a person’s mental resources. It is also consistent with the social support argument that the marriage helps institutionalize the supply of emotional support for spouses, as well as their integration into the community (House et al. 1988; Ross et al. 1990). If so, then these causal mechanisms are socioemotional resources that money can’t buy, and provide a source of psychological well-being.

However, marriage is also an economic institution in which spouses pool incomes—the capital on which our physical health builds (Smith 1997). Because it is disproportionately women who experience a substantial decline in economic well-being following a divorce (Peterson 1996; Weitzman 1985), that divorced women, but not divorced men, suffer from declines in self-reported
general health status and overall physical health is consistent with Waite and Gallagher’s (2000) argument that women benefit from marriage through increased household income. However, these results do not necessarily support their conclusion that policy makers should seek to prevent the divorce of married couples, since if income were the causal mechanism producing gender differences in the effects of divorce on physical health and general health status, this points to what money can buy.

Note also that these estimates are for the effects of divorcing the first marriage, averaging over all sorts of trajectories following a divorce. Part of the gender difference may reflect the gender differences in life trajectories, for example, divorced men are more likely to remarry than divorced women (Casper and Bianchi 2002; Sweet and Bumpass 1987) and, thus, enjoy the benefits of marriage again with a new spouse. Because of the variation in the trajectories following a divorce, it may not be surprising that I observe a lot of variability in these estimates. Indeed, even divorced men vary substantially in their economic standing following the disruption (McManus and DiPrete 2001). Although the focus on the effect of the disruption of a first marriage limits my ability to examine these speculations empirically, this focus directly addresses the policy question: whether individuals should get a divorce (or not) based on the health consequences of divorce.8

If for some individuals, divorce opens up the opportunity of remarrying to a spouse that provides greater health benefits than does his/her first spouse, it is still part of the effect of divorcing his/her first marriage. Estimates of the effects

8Other research that attempts to estimate a range of marital status coefficients using a more complicated design is related, but it is harder to interpret their results in this focused policy context. This policy question concerns only with the decision to voluntarily exit the marriage, irrespective of what might happen after the disruption.
of divorce only for the divorced, rather than the general population, presented in the last columns of each table are particularly suitable by addressing both theoretically and empirically the counterfactual of what would have occurred for a divorced individual had s/he remained married and had not divorced not.

The estimates for the effects of divorce on the overall mental health and physical health have not been reported elsewhere in the literature. The gender difference in my estimates of the effects of divorce on self-reported general health status is different from that reported by Williams and Umberson (2004). Their analysis of the Americans’ Changing Lives Survey data found that divorce improves women’s and young men’s self-reported general health status, but hurts older men’s general health status. Because the health outcomes for the NLSY79 sample were measured in a narrow age range (the early 40s) and because the design presented here focuses on the effect of divorce only for those in a first marriage, it is unclear how to reconcile these different findings.

A large scientific literature and the public attention to policy debates demonstrate the importance of answering whether there is a causal link between divorce and health. The evidence presented in this chapter is consistent with the claim that divorce causes declines in overall mental health, but provides only conditional support for similar claims regarding overall physical health and general health status. Future research is needed to continue to unpack the specific underlying causal mechanisms.
Table 3.4: Gender-Specific Regression Coefficients (and Robust Standard Errors) of Separation/Divorce on General Health Status, Men and Women, National Longitudinal Survey of Youth, 1979-2004

<table>
<thead>
<tr>
<th></th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
<th>Model 5</th>
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Note: ** p < .01, * p < .05
Table 3.5: Gender-Specific Regression Coefficients (and Robust Standard Errors) of Separation/Divorce on Physical Health, Men and Women, National Longitudinal Survey of Youth, 1979-2004

<table>
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<tr>
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<th>Model 4</th>
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Note: ** *p < .01, * p < .05
Table 3.6: Gender-Specific Regression Coefficients (and Robust Standard Errors) of Separation/Divorce on Mental Health, Men and Women, National Longitudinal Survey of Youth, 1979-2004

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<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sibling Fixed Effects</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Logit Propensity Score and Exposure Time</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>X</td>
<td></td>
</tr>
</tbody>
</table>

Note: ** p < .01, * p < .05
Table 3.7: Stratum-Specific Effects on General Health Status by Propensity Score \( r(t) \) and Marriage Duration

<table>
<thead>
<tr>
<th></th>
<th>Men ( b = -.18 )</th>
<th>Propensity Score ( r(t) )</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Marriage Duration</td>
<td>1st Quintile</td>
<td>2nd Quintile</td>
<td>3rd Quintile</td>
<td>4th Quintile</td>
<td>5th Quintile</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1st Quintile</td>
<td>-1.86</td>
<td>.37</td>
<td>.38</td>
<td>-.02</td>
<td>.24</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2nd Quintile</td>
<td>.33</td>
<td>.21</td>
<td>-.09</td>
<td>.03</td>
<td>-.42</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3rd Quintile</td>
<td>.10</td>
<td>-.09</td>
<td>-.09</td>
<td>.28</td>
<td>-.20</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4th Quintile</td>
<td>-.21</td>
<td>-.66</td>
<td>-.18</td>
<td>.16</td>
<td>-.20</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5th Quintile</td>
<td>-.18</td>
<td>-.23</td>
<td>.37</td>
<td>.71</td>
<td>NA</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

|                  | Women \( b = -.28** \) | Propensity Score \( r(t) \) |          |          |          |
| Marriage Duration | 1st Quintile        | 2nd Quintile               | 3rd Quintile | 4th Quintile | 5th Quintile |
| 1st Quintile     | -.41                | -.77                        | -.83      | .60       | .38       |
| 2nd Quintile     | -.03                | .27                         | -.24      | -.02      | -.30      |
| 3rd Quintile     | -.12                | -.36                        | -.11      | .63       | -.15      |
| 4th Quintile     | -.60                | -.38                        | -.03      | -.39      | -.12      |
| 5th Quintile     | -.21                | -.63                        | .04       | .10       | .34       |

Note: ** \( p < .01 \), * \( p < .05 \)
Table 3.8: Stratum-Specific Effects on Physical Health by Propensity Score $r(t)$ and Marriage Duration

<table>
<thead>
<tr>
<th>Men $b = -1.26$</th>
<th>Propensity Score $r(t)$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Marriage Duration</td>
<td>1st Quintile</td>
</tr>
<tr>
<td>1st Quintile</td>
<td>-19.04</td>
</tr>
<tr>
<td>2nd Quintile</td>
<td>3.63</td>
</tr>
<tr>
<td>3rd Quintile</td>
<td>.43</td>
</tr>
<tr>
<td>4th Quintile</td>
<td>.96</td>
</tr>
<tr>
<td>5th Quintile</td>
<td>-3.37</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Women $b = -2.05^{**}$</th>
<th>Propensity Score $r(t)$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Marriage Duration</td>
<td>1st Quintile</td>
</tr>
<tr>
<td>1st Quintile</td>
<td>-1.31</td>
</tr>
<tr>
<td>2nd Quintile</td>
<td>-1.72</td>
</tr>
<tr>
<td>3rd Quintile</td>
<td>-2.39</td>
</tr>
<tr>
<td>4th Quintile</td>
<td>-4.88</td>
</tr>
<tr>
<td>5th Quintile</td>
<td>-1.67</td>
</tr>
</tbody>
</table>

Note: ** $p < .01$, * $p < .05$
Table 3.9: Stratum-Specific Effects on Mental Health by Propensity Score $r(t)$ and Marriage Duration

<table>
<thead>
<tr>
<th></th>
<th>Propensity Score $r(t)$</th>
<th>Men $b = -2.67^*$</th>
<th>Women $b = -1.57^*$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1st Quintile</td>
<td>2nd Quintile</td>
<td>3rd Quintile</td>
</tr>
<tr>
<td>1st Quintile</td>
<td>-14.36</td>
<td>-2.62</td>
<td>-6.35</td>
</tr>
<tr>
<td>2nd Quintile</td>
<td>-4.15</td>
<td>1.50</td>
<td>-1.55</td>
</tr>
<tr>
<td>3rd Quintile</td>
<td>.43</td>
<td>-2.44</td>
<td>-.66</td>
</tr>
<tr>
<td>4th Quintile</td>
<td>-5.41</td>
<td>-4.27</td>
<td>-2.06</td>
</tr>
<tr>
<td>5th Quintile</td>
<td>-1.92</td>
<td>-3.70</td>
<td>.21</td>
</tr>
</tbody>
</table>

Note: ** $p < .01$, * $p < .05$
Chapter 4

Issues in Studying the Treatment Effect of an Event Using Propensity Scores

4.1 Introduction

The propensity score method has increasingly become a popular strategy in studying treatment effects in sociological research (Berk and Newton 1985; Brand and Halaby 2006; DiPrete and Engelhardt 2004; DiPrete and Gangl 2004; Harding 2003; Lundquist 2004; Lundquist and Smith 2005; Meier forthcoming; Morgan 2001; Smith 1997; Sobel 1995; Winship and Sobel 2004). The rationale is that an unbiased estimate of the treatment effect may be obtained on observational data by conditioning on the “propensity score”—the probability for an individual being assigned to the “treatment” (Rosenbaum and Rubin 1983). The key assumptions of the propensity score method is “selection on observables” (Goldberger and Cain 1982) or what the statisticians call “no hidden bias” (Rosenbaum 2002). If this assumption holds, balancing on the estimated propensity scores balances the relevant covariates so that an unbiased estimate of the treatment effect can be achieved.
Despite much attention to violations of the selection-on-observables assumption and thus when use of propensity score may yield biased estimates of the treatment effect, there has been much less attention to how to correctly specify a model for estimating the propensity score (Rosenbaum and Rubin 1984; 1985; Rubin 1997; Rubin and Thomas 1996). The issue of model specification seems to be considered either as a mechanical application of standard statistical techniques or as an art beyond the scope of statistical science. Moreover, the literature on the theoretical developments and the empirical applications of the propensity score method has focused largely within the contour of program evaluation in which the treatment assignment mechanism is safely assumed to be an exogenous intervention. Relatively little research has contemplated the possibility where a dynamic social process is involved in determining who receives the treatment and who the control. This lack of attention is especially unfortunate for sociologists interested in studying the effects of life events—such as divorce (Cherlin et al. 1991), death of a spouse (Elwert and Christakis 2006), teenage childbearing (Furstenberg et al. 1987; Geronimus and Korenman 1993), first sex (Meier 2003).

Estimating the treatment effect of an event poses additional methodological challenges and usually requires longitudinal data. Existing efforts in the counterfactual causality tradition have largely depend on variants of the inverse-probability reweighting strategy within a discrete-time conceptual framework, and few have examined issues in applying the propensity score method in a continuous-time framework. For example, to address the issue of endogenous time-varying confounding factors in epidemiology, Robins and his colleagues (e.g., Robins 1999a; b) have developed methods for studying the effect of an event on the hazards of another event. Xie and colleagues (Brand and Xie
2007; Xie and Wu 2005) have proposed a “forward-looking” approach to identifying and estimating the effects of time-varying treatments on time-varying outcomes. Their approach is a general novel solution to causal inference in longitudinal studies, but requires the “strong ignorability” assumption to hold in the whole dynamic sequence of observations.

In this chapter, I discuss issues in applying the propensity score method to studying the treatment effect of an event. In doing so, I discuss a hazard-model approach to propensity score estimation that accommodates the dynamic treatment assignment mechanisms. This hazard-model approach subsumes the conventional approach to propensity score estimation using a logistic regression or linear probability model, and helps reveal problems in applying the propensity score method that previous applications have failed to recognize. While the approach itself is general, my discussion will focus primarily on the research question of the previous chapter—i.e., how to estimate the health effect of marital disruption in the unique design where the outcome is measured only once in a cross-section.

4.2 Propensity Score and Causal Inference

The counterfactual model makes a significant contribution to recent developments in causal inference using observational data. Its development in statistics closely follows the experimental paradigm and formalizes the conditions in which “treatment effects” can be identified (Holland 1986), with a parallel tradition in econometrics arriving at very much the same conclusions, albeit relying on a relatively explicit set of behavioral models (Heckman 2006). The propensity score method has been developed in this tradition as a strategy to estimate the
causal effect of a treatment. In this section, I briefly summarize the ideas of using the propensity score method for making causal inference in the counterfactual tradition.

4.2.1 Counterfactual Model for Causal Inference

To formalize the idea, let \( D_i \) indicate the treatment of interest, with \( D_i = 1 \) if unit \( i \) receives the treatment, and \( D_i = 0 \) if unit \( i \) receives the control. Further, let \( Y_{i0} \) denote the outcome for unit \( i \) under control and \( Y_{i1} \) denote the outcome under treatment. The treatment effect for the individual \( i \) can be defined as

\[
\tau_i = Y_{i1} - Y_{i0} \tag{4.1}
\]

under the “stable unit treatment value” (SUTVA) assumption that the treatment status for one individual \( i \) does not affect the potential outcome for another individual \( j \) (Rubin 1980). This model works perfectly well in the hypothetical world. In reality, we cannot observe \( Y_{i0} \) and \( Y_{i1} \) simultaneously (hence, the term “counterfactual”). Instead, we only observe \( D_i \) and \( Y_i \), where

\[
Y_i = D_i \cdot Y_{i1} + (1 - D_i) \cdot Y_{i0}. \tag{4.2}
\]

In other words, \( Y_{i1} \) is observed only if an individual is assigned to treatment (i.e., \( D_i = 1 \)), and \( Y_{i0} \) is observed only if an individual is assigned to control (i.e., \( D_i = 0 \)). Because only one potential outcome is observed, the treatment effect at the individual level cannot be identified. However, we can identify the average treatment effect of \( D \) on \( Y \). Moreover, if we assume the treatment assignment is “strongly ignorable” (Rosenbaum 1984b; Winship and Morgan 1999), we can estimate the “true” average treatment effect defined as

\[
\tau = \mathbb{E}[\tau(X)] = \mathbb{E}[Y_1 - Y_0 \mid X = x]. \tag{4.3}
\]
By “strongly ignorable”, we essentially assume that the observed outcome under treatment $Y^1_i$ and under control $Y^1_i$ are independent of the treatment $D_i$ conditioning on a set of observable covariates $X$, and some outcomes are observed under the treatment $Y^1_i$ while other outcomes are observed under the control $Y^0_i$ at all values of $X = x$. Formally, the two conditions for the strong ignorability assumption can be written as

$$(Y^0_i, Y^1_i) \perp D \mid X$$

and

$$0 < \Pr(D = 1 \mid X) < 1.$$ 

The “strong ignorability” assumption is essential because the treatment group and the control group might not balance on covariates $X$’s, in which case one cannot causally attribute the observed differences in observed $Y$ to the treatment status $D$.

### 4.2.2 The Role of Propensity Score

There are multiple ways of statistically conditioning on the $X$’s to remove the unbalances and estimate the average treatment effect. The propensity score method can be viewed as similar in spirit to an older literature that attempted to match individuals in treatment and control groups on selected $X$’s (Smith 1997).

The propensity score method provides a logical extension of this older matching strategy by extending matching to all $X$’s; by linking the matching process to a regression-framework, which yields a more nonparametric regression model; and by employing a variety of means to achieve balance on the $X$’s even when the observational design is unbalanced. A difficulty with the traditional matching approach is the so-called the “curse of dimensionality,” with the number of
combinations of the values of all $X$s quickly becoming intractable with the number of $X$s. In an important paper, Rosenbaum and Rubin (1983) provides a proof showing that the “strong ignorability” assumption holds not only by conditioning on $X$ but also on the probability of being assigned to the treatment given $X$:

$$(Y^0, Y^1) \perp \perp D \mid \lambda(X),$$

(4.4)

where $\lambda(x) = \Pr(D = 1 \mid X = x)$. This theoretical result, which effectively solves the dimensionality problem while maintaining the essential structure for causal inference in a nonparametric way, is an important methodological development in the counterfactual causality literature. The probability of being assigned to the treatment $\lambda(x)$ is called the “propensity score.” Researchers have applied the propensity score not only by matching but also in other ways of conditioning—such as subclassification and regression/covariance adjustment (Morgan and Harding 2006)—for the purpose of estimating the average treatment effect. Prior research has emphasized that, for the strong ignorability assumption to hold, there must be “free of hidden bias”—i.e., all unbalances between the treatment group and the control group are on the observable $X$’s.

### 4.3 Propensity Score Estimation

What has often been overlooked is the specification of the functional form for estimating the propensity score $\lambda(\cdot)$ is properly specified. The statistical theory builds on the knowledge of the “true” propensity score. Empirically, the true propensity score is unknown and must be estimated from data, yet neither the theoretical nor the empirical literature has thus far provided guidance on the specification of $\lambda(\cdot)$. Most existing applications of the propensity score method
have used either a linear probability model or a logistic regression model for estimating the propensity score (Rosenbaum and Rubin 1984; 1985)—which I call the “conventional approach.” The conventional approach works well when the treatment is an exogenous shock—e.g., program implementation. In this situation, when the treatment occurs is irrelevant. However, it is unclear how well it applies when the treatment is an event generated by a social process in that the timing of treatment assignment varies according to observed and unobserved factors or individual characteristics that may even change over time. In this section I briefly review the conventional approach and introduce a “hazard-model” approach to estimating the propensity score when the treatment is an event.

4.3.1 The Conventional Approach

Consider the logistic regression approach to propensity score estimation in which the log odds that an individual receives treatment versus control is a linear function of covariates $X$’s:

$$\log \left( \frac{\Pr(D = 1)}{1 - \Pr(D = 1)} \right) = b_0 + b_1X_1 + b_2X_2 + \ldots. \quad (4.5)$$

Estimate coefficient $b$’s by maximizing the likelihood function (Long 1997):

$$\mathcal{L} = \prod_{D=1} \Pr(D_i = 1 | x_i) \cdot \prod_{D=0} [1 - \Pr(D_i = 1 | x_i)]. \quad (4.6)$$

The propensity score for individual $i$ is then given by:

$$\lambda_i(X = x_i) = \Pr(D_i = 1 | x_i) = \frac{\exp (b_0 + b_1x_{1i} + b_2x_{2i} + \ldots)}{1 + \exp (b_0 + b_1x_{1i} + b_2x_{2i} + \ldots)}, \quad (4.7)$$

where $b$’s are estimated coefficients and $x_i$’s are observed values for $X$’s.
4.3.2 The Hazard Model Approach

In estimating the health effect of divorce in the design considered in the previous chapter, the estimation of propensity score using the conventional approach appears straightforward. The health outcome is measured in only one cross-section, and each ever-married individual has either experienced a divorce or remains married when the outcome is measured. However, there is no obvious way to include time-varying covariates in this conventional estimation approach. Also, a static treatment assignment mechanism is implicitly assumed in the conventional approach, thus ignoring the process underlying divorce. A hazard-model approach addresses these issues and sheds light on issues in applying the propensity score method to studying average treatment effects when the treatment is an event.

Consider the observational plan for estimating the health effect of divorce. Define the following quantities of time:

\[
\begin{aligned}
    t_i &= 0 : \text{time at start of marriage, i.e., start of exposure to the risk of divorce;} \\
    t_i &= T^d_i : \text{time at marital disruption—either separation or divorce;} \\
    t_i &= T^w_i : \text{time at spousal death;} \\
    t_i &= T^s_i : \text{time at survey interview, when } Y_i \text{ is measured.}
\end{aligned}
\]

We observe an event of separation or divorce, \( D_i = 1 \), at the time of marital disruption, \( T^d_i \), if \( T^d_i < T^w_i \) or \( T^d_i < T^s_i \), in which case the individual is in the treatment group. Otherwise, an individual is in the control group, \( D_i = 0 \), with no event observed with no event observed if case \( i \) is right censored at time \( C^d_i = \min(T^w_i, T^s_i) \), i.e., at either time of spousal death or time at survey interview, whichever comes first. Denote the instantaneous divorce rate as:

\[
r(t) = \lim_{\Delta \to 0^+} \frac{\Pr(T^d < t + \Delta \mid T^d \geq t)}{\Delta},
\]

(4.8)
with the corresponding survivor function of divorce:

\[ S(t) = \Pr(T^d > t), \quad (4.9) \]

with the hazard and survivor function linked via the integrated hazard \( H(t) \):

\[ S(t) = \exp[-H(t)] = \exp\left(-\int_0^t r(u)du\right). \quad (4.10) \]

\( S(t) \) is the probability of being assigned to the control group, that is, of not having experienced the event of divorce by time \( t \). The probability of being assigned to the treatment group at time \( t \) is \( 1 - S(t) \)—which, by definition, equals the propensity score. To estimate the propensity score, we need to estimate the hazard regression for \( r(t) \) and then decide which \( t \) to use.

Under the proportionality assumption, we have:

\[ r(t) = r_0(t) \cdot \exp(b_1 \cdot X_1(t) + b_2 \cdot X_2(t) + \ldots), \quad (4.11) \]

where the covariates \( X(t) \)'s may vary with time. Note that (4.11) captures the divorce process via \( r_0(t) \) and the covariates \( X(t) \). With the likelihood function \( \mathcal{L} \) in a sample of size \( n \) given by:

\[ \mathcal{L} = \prod_{i} f(t_i), \quad (4.12) \]

where \( f(t) = r(t)/S(t) \) is the probability density function. When there is right-censoring (i.e., \( t_i \leq t_i^* \)), the likelihood function can be written as

\[ \mathcal{L} = \prod_{t_i \leq t_i^*} f(t_i) \prod_{t_i > t_i^*} S(t_i^*). \quad (4.13) \]

From (4.10), we can estimate the propensity score as follows:

\[ \lambda_i^*(t) = 1 - S_i(t) = 1 - \exp\left(-\int_0^t r(u)du\right). \quad (4.14) \]
We now need to make a crucial decision about which value of $t$ to plug into (4.14). For example, in the analyses of the previous chapter, we observe the health outcome for adult $i$ at $T_i^s$, in which case inserting the estimates of $b$'s into the following equation gives the estimate of propensity score $\lambda_i'$ for individual $i$:

$$\lambda_i' = 1 - S(T_i^s) = 1 - \exp\left(-\int_0^{T_i^s} r(u)du\right). \quad (4.15)$$

Note that this choice of $t = T_i^s$ makes the propensity score $\lambda_i'$ estimated using the hazard-regression approach in (4.15) conceptually equivalent to $\lambda$ estimated by a conventional logistic regression approach in (4.7). Note, however, that although the propensity scores estimated by the two different approaches are conceptually similar, they will in general differ, with the conventional logistic regression approach providing a rough discrete-time approximation to (4.15); hence, the hazard-model approach can be viewed as subsuming the conventional approach when the treatment is an event.

### 4.4 Issues in Applying the Propensity Score Method to Studying the Effect of an Event

The hazard model described in the preceding section is more than just a more general approach to propensity score estimation. In fact, it reveals issues in applying the propensity score method to studying the effect of a treatment that I argue prior research has not adequately acknowledged. The most significant insight is that the model makes explicit the dynamic nature of the treatment assignment mechanisms. For example, as $t$ varies in (4.14), there will be an infinite number of corresponding $\lambda_i'(t)$, unlike the conventional approach. Selecting $t = T_i^s$ in (4.15), while equivalent to the conventional approach, uses one specific choice of $t$, with date of survey fixed by the observation plan but with
this choice of $t$ in (4.14) not obviously optimal for matching. It should thus be apparent that the choice of $t$ is arbitrary with respect to the observational plan; hence, the choice of a propensity score $\lambda_i^*$ (where $t = t^*$) from a pool of numerous $\lambda_i(t)$ is also arbitrary. This apparent arbitrariness in the selection of $t$ in (4.14) is, at least, partly due to the lack of context in the discussion on the observational plan—which again highlights the importance of research design in analyzing observational data for causal inference (Rosenbaum 2002; Rubin 2007).

4.4.1 Time-Varying Balancing

Under the conventional approach to propensity score estimation, the differences in the covariates between the treatment group and the control group are constant over time. Regardless of the observation schedule, the balancing (and lack of balancing) is fixed. However, as indicated in (4.14), when the treatment is an event, the balancing (and lack of balancing) between the two groups on the propensity score and the covariates may change over time.

Realizing the fact that the balancing (and lack of balancing) of covaraites between the two groups may change over time when the treatment is an event raises a serious methodological issue in applying the propensity score method to studying the treatment effect of an event. The key theoretical result by (Rosenbaum and Rubin 1983) that conditioning on the propensity score will function as conditioning on all the covariates $X$’s may not hold in the dynamic setting when the treatment is an event. Thus, it may not be appropriate to apply the propensity score estimated by the conventional approach to studying the treatment effect of an event. Intuitively, the problem with making causal inference for the treatment of an event stems from the fact that, at any given
time, whether an individual receives the treatment or control is not random. Conditioning on the probability of being assigned to treatment at a given time conditional on the individual has not received the treatment up to the point will adjust for the biases due to this non-randomness. This conditional probability is the instantaneous hazard rate defined in (4.8). In fact, a recent paper (Lu 2005) shows that, under the “strong ignorability” assumption, the same “conditional independence” property in (4.4) will hold when the treatment is an event if one conditions on the conditional probability, or hazard $r(t)$, rather than the unconditional probability, or propensity score $\lambda'(t)$. This result implies that not only the propensity score estimated by the conventional approach might be incorrect, that estimated by the hazard-model approach might also be incorrect.

Note, however, that the essential “strong ignorability” assumption implies selection on observables and no hidden bias. Since it is an assumption, it need not be true in empirical applications. The hazard-model approach suggests a way to deal with potential hidden bias. Recall that, in (4.14), there are two components of the propensity score when the treatment is an event—$t$ and an individual specific hazard function $r_i(t)$. Both components may contribute to the difference in propensity score so that two individuals with the same value of $\lambda'$ can be the product of either component or both. The proof presented in Lu (2005) shows that, if there is no hidden bias, balancing on $r_i(t)$ works as well as balancing on the potentially time-varying observables. According to (4.14), I argue that time $t$ may serve as a proxy for unobserved confounding factors. Thus, if there is hidden bias, the hidden bias may be captured by further conditioning on $t$. An intuitive way to look at this potential hidden bias captured in time $t$ is to recognize that $r_i(t)$ is itself a time-dependent trajectory, and the choice of time $t$—which may or may not be exogenous according to
the observational plan—will affect the result. Conditioning on both \(r_i(t)\) and \(t\), therefore, provides a double insurance in removing biases.\(^1\)

### 4.4.2 Observational Plan and Exposure Time

(4.14) also shows that the selection of \(t\) will affect the estimated propensity score through both the upper bound of the integral and the hazard \(r_i(t)\). Although \(r_i(t)\) is in theory beyond the control of the investigator, the investigator can often design the observational plan to determine \(t\). Planning the research design with \(t\) in mind leads to another possibility of applying the propensity score method to studying the treatment effect of an event.

Consider a simple design: We follow only one single marriage cohort over time and measure their outcome at the end of a fixed observation period. Every individual in the sample, thus, begins at \(t = 0\) and they all have the same \(T_i = k\) (where \(k\) is a constant) when the outcome is measured. Under this design, time \(t\) has no variation and is absolutely exogenous. Because \(k\) is a constant for all individuals in the sample, the propensity score is really only a function of the hazard rate at time \(k\), \(r_i(k)\). One can then apply the result in Lu (2005) to include both time-invariant and time-varying covariates in estimating the hazard or the propensity score—and needless to say, that the two quantities carry redundant information and have the same effect in estimating the treatment effect.

Now turn to the more commonly seen observational plan (e.g., the one con-

\(^1\)In fact, one can flip the argument regarding \(t\) and \(r(t)\) as follows: If selection on these unobserved characteristics can be completely captured by holding \(t\) constant, which implies that whether an individual will experience the event between \(t\) and \(t + \Delta t\) given that s/he has not experienced the event until time \(t\) is purely random, then it is sufficient only to condition on \(t\). However, if not, then we need to further condition on \(r(t)\), the conditional probability of experiencing the event at \(t\).
sidered in this chapter), we follow multiple marriage cohorts over time and then measured their outcome at different time t’s. The time of exposure to the risk of the event occurrence, $T_i^s$, will vary from individual to individual under this design. Not only because the longer the exposure the higher proportion of the sample will have experienced the event, but also because the time of exposure may be endogenous, the exposure time $t$ must be controlled. For example, in estimating the health effect of divorce considered in this chapter, those who are married at a younger age will be exposed to the risk of divorce for a longer period of time if the outcome is measured at the same age for all individuals in the sample. If early marriage is associated with higher divorce rates, then the time of exposure is endogenous. This reasoning leads to a similar strategy of propensity score applications as discussed earlier. One can condition on both the exposure time $T_i^s$ and the estimated propensity score $\lambda_i(t = T_i^s)$.

4.4.3 Estimation Issues

The hazard-model approach also make clear the importance of modeling the treatment assignment mechanism through an appropriate $r(t)$ in applying the propensity score method to studying the effect of an event. A key issue in specifying the $r(t)$ is considerations of the specification of the baseline hazard and the possibility of nonproportional hazards (Wu 2003). This suggests why the applications of the propensity scores estimated by the conventional approach might be inadequate when the treatment is an event. The logistic regression essentially approximates the hazard model with a one-period constant baseline hazard and this approximation can be poor, the estimated propensity scores may be substantially different between the conventional approach and the hazard-model approach.
The following exercise demonstrates this point by comparing estimated propensity scores under several common parametric specifications of the hazard model under proportionality assumption.\footnote{The specification in (4.11) is general and flexible about the baseline hazard, \( r_0(t) \). If the baseline hazard remains general and flexible and we use the partial likelihood to estimate the coefficients, it is the semi-parametric Cox model (Cox 1972). One can estimate the expected survivorship, but I will not discuss the details here. Interested readers should read Therneau and Grambsch (2000).} These models are then compared with the conventional approach with a logit model specification—which essentially assumes a one-period exponential model while rounding the time to event information to the upper bound. The discussion should make clear that the functional form of the baseline hazards has implications for the estimated propensity scores—which leads to a more general implication that the misspecification of the functional form of \( \lambda(\cdot) \) bears consequences. For example, the exponential model assumes the baseline hazard to be a constant:

\[
r_0(t) = e^{b_0} = q_0,\tag{4.16}
\]

Thus, the propensity score can be estimated using

\[
\lambda_0'(t) = 1 - \exp (-q_0 \cdot t \cdot \exp (b_1 x_1 + b_2 x_2 + \ldots)) \tag{4.17}
\]

Similarly, the baseline hazard for the Gompertz model is specified as follows:

\[
r_0(t) = e^{b_0 + c_0 t} \tag{4.18}
\]

Thus, the propensity score can be estimated using

\[
\lambda_0'(t) = 1 - \exp \left( \frac{1}{c_0} \cdot \exp (b_0 + b_1 x_1 + b_2 x_2 + \ldots)[1 - \exp(c_0 t)] \right) \tag{4.19}
\]

The one-period models are easy to extend to become piecewise models. In the Gompertz specification, one can impose equality constraints at the cost
of an additional degree of freedom to make it piecewise linear splined models. The estimated propensity scores based on different specifications of the baseline hazard are going to be substantially different from each other.

The second issue related to estimation, uniquely associated with the strategy where one estimates the propensity score \( \lambda' \) rather than the hazard \( t \), is how to code the covariates after event or censoring but prior to the end of observation at \( t = T_i^a \). On the one hand, these measurements might be indeed the consequence of the event and thus should not be partialled out (Rosenbaum 1984a). On the other hand, how the analyst codes these measurements is one way to construct the various scenarios in terms of how those covariates should change under the counterfactuals if the treatment had not occurred in this period. Different assumptions will undoubtedly lead to different estimates of the propensity score, and in turn different estimates of the treatment effects.

### 4.4.4 General Longitudinal Designs

The two empirical strategies discussed above can be easily applied to general longitudinal design with multiple repeated measures of the outcome. Because the propensity score plays the role of essentially a composite covariate, one can incorporate the propensity score method to typical modeling strategies for panel data. The only additional step is to estimate \( r(t) \) and \( \lambda'(t) \) for each and every panel (or repeated cross-section). For example, in the individual fixed-effects model for panel data, the \( r(t) \) becomes a time-varying covariates in the equation:

\[
g_i(t) = \tau \cdot D_i(t) + b \cdot r_i(t) + c_i + e_i(t),
\]

where \( e_i(t) \) is the error term that is independent, identically distributed, and \( c_i \) is the individual specific intercept capturing unobserved, within-individual
time-invariant confounding factors. \( r_i(t) \) captures the dynamic selection on observables that balances the observed time-invariant and time-varying covariates at time \( t \). Other panel models can also apply in a similar way.

4.5 Discussion

Propensity score methods have increasingly gained popularity in sociological research for making causal inference using observational data. Although the treatment of sociological interest is often an event, few prior studies have seriously considered issues of applying the propensity score method, originally designed in an experimental analogy under a program evaluation paradigm, to studying the treatment effect of an event. This chapter discusses an alternative approach to propensity score estimation based on hazard regression. This hazard-model approach helps unravel issues in applications of the propensity score method to studying the treatment effect of an event.

The hazard-model approach to propensity estimation is an improvement over the conventional approach using either the logistic regression or linear probability model because it acknowledges the process underlying the mechanism of treatment assignment and by permitting time-varying covariates. It also clarifies that the estimation of the propensity score is \textit{not} simply a technical issue, and that substantive knowledge about the social processes generating the event can aid in estimating treatment effects through a more substantively informed estimate of the propensity score. Finally, the hazard model is a more general approach that subsumes the conventional approach.

The hazard-model approach helps illuminate how methodologically the conventional approach may confound time and the actual social processes behind
the treatment assignment and how results based on the conventional approach to propensity score estimation might be biased due to misspecification. The propensity score method is not the only strategy to studying the treatment effect of an event. Prior methodological developments in the counterfactual tradition have considered alternatives to studying the treatment effect of an event (e.g., Brand and Xie 2007; Robins 1999a; b; Xie and Wu 2005), but few has discussed the applications of the propensity score method in a similar context. The hazard-model approach not only highlights several caveats in applying the conventional propensity score method to studying the treatment effect of an event but also provides potential solutions. Moreover, it redirects the attention to the importance of correct model specification in estimating the propensity score. This redirected attention is in line with the advice the methodologists have repeated but the empirical researchers have ignored—careful research design is the basis for the sound analysis of data. It is also in line with the more structural approach to causal inference (e.g., Heckman 2000; Lillard 1993) that stipulates an explicit behavioral model for social process generating the “treatment assignment.” The hazard-model approach might in the future provide a potential niche that integrates the counterfactual approach and the structural approach to causal inference.
Chapter 5

Beliefs Concerning Marriage Prospects and Family Migration Decisions: A Computerized Multivariate Factorial Survey

5.1 Introduction

This chapter has both a methodological objective and a substantive objective. First, it describes a methodological advancement over existing factorial survey method. Second, it examines the beliefs about marriage prospects and family migration decisions as joint sociopsychological processes. The computerized multivariate factorial survey (CoMFaS) method described in this chapter builds on recent developments in the “factorial survey” or ”vignette” methods” (Ganong and Coleman 2006; Jasso 2006) for studying interrelated beliefs and judgments. The factorial survey methods (Jasso 2006) have succeeded in examining the sociopsychological processes behind the complicated relations between “vignette dimensions” (independent variables in the hypothetical vignette world) in forming beliefs and judgments, but the factorial survey methods have not yet been extended to the studying of complicated relations between both
“vignette dimensions” and the participant’s responses or the rating tasks (dependent variables in the hypothetical vignette world). The CoMFaS method described in this chapter attempts is designed to study intertwined beliefs and judgments by exploiting computerized experimentation. I illustrate how to apply this method to studying the underlying sociopsychological processes in beliefs concerning marriage prospects and job-related family migration. The substantive results from this illustration suggest that the existing models of family migration may not adequately capture the social psychology of family migration in the contemporary American society where marriage is highly unstable.

5.2 Factorial Survey Methodology

5.2.1 Studying Beliefs and Judgments

Jasso (2006) proposed a unified framework, based on the factorial survey method pioneered by Rossi and colleagues (e.g., Rossi and Nock 1982), to study positive beliefs and normative judgments. This framework stipulates three types of equations in understanding the world. The first equation reflects the reality and is routinely estimated by the social scientists to examine the determinants—such as educational attainment, mental and physical health—as indicated by the outcome $Y$:

$$Y_j = \beta_0 + \sum \beta_k X_{kj} + \epsilon_j$$

Ordinary people, like social scientists, also seek to understand the reality of the social world and form beliefs and judgments about the determinants of various outcomes. We can write the corresponding models that represent the positive beliefs and normative judgments of a lay scientist or lay judge $i$ as follows:

$$Y_{ij} = \beta_{0i} + \sum \beta_{ki} X_{kj} + \epsilon_{ij}$$

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When (5.2) represents a lay person’s belief in “what the reality is” in the social world, it is termed a Type II equation. When it represents a lay person’s judgment about “what ought to be” in the social world, it is termed a Type III equation. The two types of equations follow the common form that differs from a Type I equation in (5.1) in that each individual \( i \) has idiosyncratic parameters of \( \beta_{ki} \) that may or may not accurately describe the “true” parameters \( \beta_k \) in the real world. Similarly, the set of determinants \( X_k \) operating in the real world may differ from the set of determinants \( X_{ki} \) a lay individual \( i \) believes or judges to be operating in his/her mind. The factorial survey methods are the powerful tool for the social scientists to understand the positive beliefs and normative judgments through probing respondents’ responses in a systematic way and estimating the idiosyncratic parameters of \( \beta_{ki} \).

As Jasso (2006) points out, Type II and Type III equations do not have to be restricted to single equations. A multiequation model may more faithfully describe the mental representations in the lay scientists and lay judges (pp. 335-36). With a specific goal of laying out the foundation of factorial survey methods to understand the beliefs and judgments, however, her exposition focuses exclusively on the single-equation model.

### 5.2.2 Multiple Segment Factorial Vignette Designs

Ganong and Coleman (2006) proposed a multiple segment factorial vignette (MSFV) approach to studying a richer set of sociocognitive processes. As in the typical factorial survey methods developed by Rossi and colleagues (e.g., Rossi and Nock 1982), the MSFV involves an experimental component in which the vignette dimensions are constructed in a way so that they are orthogonal to ensure unbiasedness in estimating the effect of these dimensions on the
beliefs or judgments. It also expands the typical factorial survey methods by providing the respondents more than one segment of the vignette stories, and asking additional responses in the procession of the segments. Hence, the MSFV incorporates strengths of both the traditional factorial survey design and the expanded vignette method including more than one segment (Finch 1987).

Although I agree with Ganong and Coleman (2006) and Finch (1987) on the importance of examining multiple beliefs and judgments in the vignette studies, their MSFV approach assumes independence in the multiple underlying psychological processes. Human minds are complicated, and often times we are interested in how multiple decisions and judgments are made in an interrelated and intertwined manner. However, the multiequation vignette study envisioned by Jasso (2006) is practically difficult to implement in the traditional paper-and-pencil format. The approach I propose here exploits the advantages of computerized experimentation techniques combined with advanced multivariate statistical models to achieve this goal.

5.3 The Computerized Multivariate Factorial Survey Approach

It is not only substantively interesting but theoretically plausible to consider beliefs and judgments as involving interrelated sociopsychological processes. Social thinkers rarely form and report beliefs and judgments in a vacuum of mind, but through a complicated and intertwined process with a rich set of background information in the context. In the example I use to illustrate this new method, the belief of marriage prospects (that is, a respondent’s perception of marital instability) may be interrelated with the decision to migrate as a family.
Therefore, we should try to examine and empirically model these beliefs and judgments as intertwined processes. In the computerized multivariate factorial survey approach described in this chapter, modeling the intertwined mental processes is achieved through computerized experimentation techniques. Instead of manipulating all of the vignette dimensions, I allow the response (a dependent variable) in a first task to become a vignette dimension (an independent variable) in the second task. I will illustrate this new approach in detail using an example examining the intertwined beliefs concerning marriage prospects and family migration.

To give a brief overview, in the example on marriage prospects and family migration, I ask the respondents to rate the probability of divorce for a set of hypothetical couples in the vignettes in the first session. In the second session, I ask the respondents to report the most likely decisions for the same set of vignettes in which the hypothetical couples are facing a job promotion opportunity offered to one spouse that requires relocation. In this session, the respondent-reported probability of divorce in Session I is then used as a vignette characteristic (or vignette dimension) of the hypothetical couple, based on which and other vignette dimensions the respondent report whether the couples would stay as a family, move as a family, or become a married couple living apart. Note that the reported probability of divorce is not manipulated by the investigators, and thus unmeasured mental processes that affect the respondent’s beliefs concerning both marriage prospects and family migration decisions will be unraveled if the full processes are adequately modeled.
5.3.1 Experimental Procedures

The computerized experimental procedure goes as follows: My collaborators and I recruit the respondents by distributing soliciting emails through listservs, or by going to the classrooms under the consent of both instructor and students. Those respondents who agreed to participate were each seated in front of a personal computer with pre-installed computerized experiment control protocols written in the commercialized software, MATLAB with the open-source code extensions of psychtoolbox (Brainard 1997; Pelli 1997). The experimenter then presented a consent form to the respondents explaining the rights of human subjects, which also includes a description of the purpose of the study and a brief description of the content of the experiment. After the first three respondents taking over 45 minutes to complete the experiment, I added an oral instruction asking the respondents to put down the first thing that comes to his/her mind. I decided to make this change because the factorial survey is intended not to examine the accuracy of the respondent’s calculus or the soundness of their decision-making logic, but to probe their inherent beliefs that are influenced by family background, socialization, prior experience, and social norms. This additional instruction helps reduced the time it took for the subsequent respondents to complete the computerized factorial survey, although for some respondents it still take over 45 minutes. This seems to indicate how intricate the issues are for making the decisions of marriage disruption and family migration even though the respondents themselves are not involved, but acting as an “observer.”

As briefly stated in the preceding section, there are two sessions in the survey. In the Session I, I ask the respondent to report their beliefs about the probability of divorce (0%–100%) for the hypothetical dual-earner married couple living in
New York City described in the vignettes. In Session II, I ask the respondent to report their beliefs about the most likely family migration decision for the same set of hypothetical couples when one spouse is facing a job promotion that requires moving from New York City to Washington DC. Although we follow prior research that presumes migration as a joint decision of family members, particularly the spouses in a typical conjugal, nuclear family, the respondent is allowed to choose a third option in which one spouse moves and the other stays.

Once the experiment started, the respondent read the instruction for Session I on the computer screen, and clicked the mouse to proceed when s/he understands the task. There were 3 practice trials, followed by a short pause controlled by the respondent so that s/he had the opportunity to ask questions about the survey. After clicking the mouse again to proceed, s/he completed the rest of the session including 37 real trials. With a pause controlled by the respondent, Sessions II repeated the same procedure—instruction followed by 3 practice trials and 37 real trials. Finally, the respondents were asked to provide basic demographic information including sex, age, race, marital status and immigrant status, which concluded the experiment. The instructions and sample vignettes for both sessions are included in Appendix E.

The experimenter was always in the same room to answer questions during the experiment, but neither sitting nor standing close to the respondent to avoid potential concerns about coercion.

5.3.2 Vignette Dimensions

I use the following dimensions to construct the vignettes of fictitious dual-earner married couples (see also the Appendix, which put these dimensions in context). There are 5 parallel vignette dimensions for spouses, and 3 vignette dimensions
about the couple. In Session II on family migration, I add 3 more dimensions to describe how situation on the job offer that requires relocation.

(Race-specific) name Each spouse is assigned a name that is perceived as disproportionately used by people of either the white race or the black race. The list of “race-specific” names is drawn from Bertrand and Mullainathan (2004), which includes 9 white female names, 9 black female names, 9 white male names, and 9 black male names. The experimental manipulation of race composition of the couples give us a unique leverage to examine how race operates in the beliefs about family behaviors.

Age The age of husband and the age of wife both range from 31-40 years old with an equal interval of 1 year—i.e., a total of 10 levels.

Education Education for husband and wife each has 9 levels, ranging from 10-18 years of schooling. Two levels—high school graduates (12 years of schooling) and (four-year) college graduates (16 years of schooling)—are over-sampled and appear three times more frequently than the other levels.

Earnings Both husband’s and wife’s earnings have 10 levels—15, 20, 25, 30, 35, 40, 50, 60, 70, 80 thousand dollars. The levels are unequally spaced to increase the range and to maintain the average earnings (as well as the implied household income) at a reasonable level.

1In their field experiment on labor market discrimination, Bertrand and Mullainathan (2004) found that resumes with white-sounding names receive 50 percent more callbacks for interviews than resumes with African American names. Because the “race of the names” is the crucial element of their study, they adopted a rigorous procedure to ensure the names they used were perceived as uniquely white or uniquely black. Building on their hard work, the names of husband and wife used in this present study are drawn from Table 8 of their paper.
Sex role attitudes Husband’s and wife’s sex role attitudes are either “egalitarian” or “traditional.” The description of a egalitarian sex role attitude reads “the husband and the wife should share housework/childcare and enjoy the same job opportunities”; and that of a traditional sex role attitude reads “it is ideal if the husband is the breadwinner and the wife takes care of the home and children.” The descriptions are adapted from Bielby and Bielby (1992), the most influential sociological study on this topic that demonstrated the importance of sex roles in family migration decisions.

Marriage duration The duration of marriage has 10 levels, ranging from 2 years to 11 years with an equal interval of 1 year.

Children The number and sex composition of children have 6 categories: no children, one son, one daughter, one son and one daughter, two sons, and two daughters.

Best friend The fictitious couples are said to be either each other’s best friend or not each other’s best friend.2

Agent One of the spouses (husband or wife), called “Agent” henceforth, is randomly selected as the one who receives a job promotion opportunity that requires moving from New York City to Washington DC. The spouse who does not receive the job promotion is, henceforth, called “spouse.”

Pay raise The additional earning if the spouse being offered the job promotion takes the offer has 9 levels, ranging from 10,000 dollars to 50,000 dollars

2Schwartz (1994) argued that peer marriage is an ideal relationship that many Americans aspire and are more stable than traditional marriages.
with an equal interval of 5,000 dollars. The respondent is presented with the total earnings (i.e., the sum of the original earnings for the “agent” and the pay raise in the vignettes). The pay raise is constrained to be smaller than the spouse’s current annual earnings and smaller than 2/3 of agent’s current annual earnings.

**Job prospects for “spouse”** The spouse is described in the vignettes to be either “likely” or “unlikely” to find a job in Washington DC that is comparable to the one in New York City.

The respondents make their judgments about the probability of divorce and family migration decision of each fictitious couple consisting of a unique combination of these dimensions. I then try to understand their social psychological processes by estimating the equations (Type II Equations discussed in the preceding section) that describe what the respondents think are the relationships between vignette dimensions and marriage prospects as well as family migration decisions.

I explicitly distinguish between the “vignette dimensions” and “predictive variables.” The vignette dimensions are characteristics of the vignettes, e.g., the composition of children. They are what we use to construct the vignettes as “experimental stimuli.” However, the vignette dimensions are unprocessed information based on raw experimental materials and may not represent the psychological concepts upon which the respondent operate when they do the rating tasks. A related issue is the functional form linking the vignette dimensions (and thus those psychological concepts) and the respondent’s ratings may not follow a simple linear function. Hence, when I estimate the equations that represent the respondents’ beliefs, it is possible to specify the “predictive vari-
ables” derived from the vignette dimensions in varying functional forms. The predictive variables are processed information hopefully capturing the mental representation in the respondents’ heads. For example, one can construct predictive variables for number of children as well as for whether the sibling is mixed-sexed from the vignette dimension of the composition of children. The predictive variable for the number of children need not follow a linear function, either, in predicting the probability of divorce.

5.3.3 Vignette Construction

I follow the general procedures described in Jasso (2006) to construct the vignettes. I first determine the vignette dimensions (and levels for each dimension) and then construct a population of vignettes by fully “crossing” the vignette dimensions. Crossing means to create one vignette for each unique combination of the vignette dimensions. Therefore, the size of the population equals the product of the levels of vignette dimensions. Samples of vignettes are drawn from the population of vignettes.

In this present study, I construct a vignette population crossing spouse-specific dimensions of names (18 levels), ages (10 levels), educations (9 levels with over-sampling on 2 levels), earnings (10 levels) and sex role attitudes (2 levels) for husband and wife; and crossing couple-level dimensions of marriage duration (10 levels), children (6 levels) and best friend (2 levels); and crossing “agent” (2 levels), pay raise (9 levels with restrictions) and job prospects for “spouse” (2 levels). I then draw samples of 40 vignettes (with replacement) from this population.
5.4 Substantive Issues on Family Migration

5.4.1 Economic Model of Family Migration

Mincer (1978) in his seminal work on family migration argued that both spouses in a conjugal family seek to maximize their joint well-being, measured in monetary and nonmonetary terms, in family migration decision-makings. When both spouses gain by either staying or moving, there is no question. When family migration increases one spouse’s well-being but decreases the others’s, the decision to move (or to stay) will depend on the comparison of absolute values of the potential gain and loss in well-being between the spouses. In this case, one spouse will be either “tied mover” or “tied stayer” when the migration decision is against his/her net change of his/her “private” well-being. Mincer argued that the “tied spouse” phenomenon exemplifies the internalization of “externality” from the family migration in that the “tied spouse” sacrifices his/her “private” well-being to realize the “public” well-being of the conjugal family.

Empirical results are by and large consistent with predictions of Mincer’s (1978) model. Because economic gains (or losses) are typically proportional to the earnings, those who have a more modest earning—usually the wife—are more likely to be “tied mover” and “tied stayer” in maximizing the “public” family well-being. When the structural constraints of the labor market make women systematically earn less than men, they are more likely to take jobs that are easy to transfer geographically, such as positions in the service and secretarial sectors. The causal mechanisms are intricate but research found, in general, dual-earner families are less likely to migrate than single earner families (Mincer 1978). The pattern of family migration also has implications for gender inequality: Husband’s earnings are found to increase, while wife’s earnings
and employment both are found to decrease after a migration (DaVanzo 1976; Duncan and Perrucci 1976; Lichter 1980; Long 1974; Maxwell 1988; Sandell 1977).

5.4.2 Gender Role and Family Migration

Despite the theoretical elegance and empirical support of Mincer’s (1978) model, recent studies disagreed that pure economic calculus is the predominant sociobehavioral mechanism of family migration decision. Bielby and Bielby (1992) argued that Mincer’s theory, assuming gender symmetry in the decision-making process, failed to explain why women have disproportionately been the tied spouse in family migrations. They showed that the economic calculus is highly contingent on “gender-role ideology.” Men holding traditional gender-role ideologies do not hesitate to take a job in elsewhere regardless of his wife’s income, while women holding traditional gender-role ideologies are sensitive to their husband’s income in making the same relocation decision. Similar findings have been reported in Bird and Bird (1985); Shihadeh (1991) and Jürges (2006).

However, gender role ideologies might be endogenous to labor force decisions. Consider the possible discrepancy between one’s gender role ideology and one’s actual employment status: for example, when a woman subscribing to a traditional gender-role ideology winds up being a working wife and when a man subscribing to a traditional gender-role ideology ends up having a working wife. Gerson (1985) explored such discrepancies of what a woman aspired earlier in life and what she did subsequently in her analysis of women’s work-family trajectories. Gender role ideology is influenced by childhood socialization, while the real choice between family and work is a complex decision under socioeconomic constraints. Gerson’s analysis implied that working wives with a
traditional gender role might happily reduce their employment if the economic necessities disappeared. If their husband’s work prospects improve dramatically with family relocation, the move may be a relief for these traditional women.

5.4.3 Conjugal Power and Family Decisions

Negotiated conjugal power, rooted in Blood and Wolfe’s (1960) relative-resource theory, is another alternative sociobehavioral mechanism to the economic calculus behind family migration decisions (Bielby and Bielby 1992; England 1989). Although the conjugal power theory also predicts that the spouse with greater resources dominates the family migration decision, the income effect is viewed as the consequence of power relations based on resources inequity between spouses, rather than the consequence of harmonious collective rational decision makings. In contrast to the altruistic couple in Mincer’s (1978) model who cooperate to maximize their public family well-being, the conjugal power theory hypothesizes that spouses selfishly compete to maximize their private well-beings (even at the expense of the other spouse’s private well-being). Resources not only found the basis of “bargaining power” in a marriage but make a potential divorce more “affordable” for one spouse than the other. Thus, the spouse with greater resources is more likely to get what she/he desires in general. The same logic applies to decisions of family migration, but no empirical study, to my knowledge, has tested it. Although it is extremely difficult to conceptualize and operationalize conjugal power, one manifestation of conjugal power struggle is reflected in marital instability (Brines and Joyner 1999; Jasso 1988). If one spouse is able to leave a marriage, as is usually the case in the contemporary American society where not only no-fault divorce is legal and common, but a substantial proportion of the population is divorced, then the joint economic
calculus should no longer be taken for granted in family migration decisions.

5.4.4 Marriage Prospects and Family Decisions

Marriage prospects have long been recognized as an important factor in the labor force decisions, especially for women. For example, the employment rates were found to be higher in a period prior to separation for those women who subsequently divorced (Johnson and Skinner 1988). The effects of marriage prospects on individual decisions may be especially pronounced in societies where divorces are prevalent. Individualization is probably not only a cause but also a consequence of the changing context of family cultures (Bumpass 1990). We should reconsider whether maximizing the public well-being of the conjugal family, rather than private well-beings of spouses, remains the rational foundation for family migration decisions. Without a promising marriage, the spouse will be irrational if she/he sacrifices her/his private well-being for the public well-being and becomes “tied” in family migration.³

³Mincer’s (1978) family-migration model incorporated the economic calculus of marriage (Becker et al. 1977), assuming that the gains of migration and the gains of marriage are additive on the same dimension. Spouses compare these competing gains and make mutually exclusive decisions either to migrate or to divorce: If the gains of migration exceed the gains of marriage, the marriage shall dissolve. Conversely, one spouse becomes the tied mover or stayer and the marriage survives. Elaborating Mincer’s insight that migration decisions and marriage decisions are interrelated, Holt (1997) proposed a structural approach that treats these decisions in dynamic, discrete choice framework, which took into account the uncertain stochastic nature of human decisions. Individuals are theorized in Holt’s model to be making discrete choices between life-course states on a continuous time scale, and the state space consists of combinations of employment status, marital status, and migration status (p. 76).
5.5 Empirical Analysis and Statistical Models

The basic empirical analysis of data collected in the computerized multivariate factorial survey approach is the same as the conventional factorial survey approach (Jasso 2006; Rossi and Nock 1982). In principle, the analyst begins by estimating two separate sets of equations representing the respondents’ beliefs about marriage prospects and in family migration decisions. Both the slopes(coefficients and intercepts may vary across individual respondents, and thus the estimation typically involves the set of models described in, e.g., Hsiao (2003). In the analysis in this chapter, I only consider models that allow varying intercepts, but not varying coefficients.\textsuperscript{4} To fully exploit the unique experimental design of the computerized multivariate factorial survey approach, I estimate statistical models that accommodate the possibility that the equations for marriage prospects and family migration decisions are interrelated in that the respondents may take into account the marriage prospects (rating in Session I) when they try to think about family migration decisions (rating in Session II), and with correlated error terms. I use the econometric software, aML, developed by Lillard and Panis (2003) to estimate the multilevel, multiequation models which estimates all the models using the maximum likelihood method. This multi-equation nature is what makes this new approach “multivariate” in the statistical sense.

The identification of the multilevel-multivariate models will be enhanced by “exclusion restrictions”—i.e., some exogenous (independent) variables appear

\textsuperscript{4}These models are multilevel by nature. Hence, it is possible to explore how the coefficients vary by the characteristics of the respondent. I will explore this possibility in future research.
only in one equation but not the other. This is a statistical issue with implications for the design of the computerized multivariate factorial survey. In some cases (e.g., the present study), the aforementioned distinction between “vignette dimensions” and “predictive variables” can be useful in this regard. The analyst can include one set of predictive variables in one equation and a different set of predictive variables in another equation even though the two sets of predictive variables are constructed on the basis of the same vignette dimension(s).

5.5.1 Statistical Models

Marriage prospects are measured as the perceived probability of divorce within five years, $\Pr_{ij}(\text{Div})$, for vignette $j$ rated by respondent $i$, and is a linear function of predictive variables $X$ constructed from vignette characteristics:

$$\Pr_{ij}(\text{Div}) = \beta X + u_i + e_{ij}, \quad (5.3)$$

where $u_i$ is an individual-respondent specific error term that is assumed to follow a normal distribution across respondents.

The multinomial logit model for beliefs about family migration is specified in (5.4), with “stay as a family” ($k = 2$) being the base outcome category. Thus, the two equations are “move as a family ($k = 1$) vs. stay as a family” and “commute ($k = 3$) vs. stay as a family.”

$$\begin{align*}
\log \left( \frac{\Pr(y_{ij}=1)}{\Pr(y_{ij}=2)} \right) &= \gamma \cdot Z + \delta \cdot \Pr(\text{Div}) + \epsilon_{1i}, \\
\log \left( \frac{\Pr(y_{ij}=3)}{\Pr(y_{ij}=2)} \right) &= \gamma \cdot Z + \delta \cdot \Pr(\text{Div}) + \epsilon_{3i},
\end{align*} \quad (5.4)$$

with the probability of divorce $\Pr_{ij}(\text{Div})$ entering as a predictive variable, along with other predictive variables $Z$. The error terms $\epsilon_{1i}$, $\epsilon_{3i}$ are specific to individual respondents, and assumed to follow a multivariate normal distribution.

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5In some cases, a model will only be identified with the exclusion restrictions.
Finally, in the multivariate/multiequation analysis, I estimate the above two models in (5.3) and (5.4) jointly, with the error terms following a three-dimensional multivariate normal distribution as follows:

\[
\begin{pmatrix}
  u_i \\
  \epsilon_{1i} \\
  \epsilon_{2i}
\end{pmatrix}
\sim N
\left[
\begin{pmatrix}
  0 \\
  0 \\
  0
\end{pmatrix},
\begin{pmatrix}
  \sigma^2_u & \rho_{u1} & \rho_{u3} \\
  \rho_{u1} & \sigma^2_1 & \rho_{13} \\
  \rho_{u3} & \rho_{13} & \sigma^2_3
\end{pmatrix}
\right].
\tag{5.5}
\]

The correlations between error terms, \(\rho_{u1}, \rho_{u3}, \rho_{13}\), indicate the intertwined decision making processes that are not captured in the predictive variables constructed from the vignette characteristics.

## 5.6 Results

### 5.6.1 Sample Characteristics

The sample consists of 29 adult respondents. They are either undergraduate students, graduate students or post-doctoral students at New York University or the University of Western Ontario in Canada, and/or (direct or indirect) acquaintances of the investigator. All of them have at least some college education. The distribution of other demographic characteristics for these respondents is presented in Table 5.1.\(^6\) Because it is a convenience sample, the findings of this study cannot be generalized to the general population without caution.

### 5.6.2 Descriptive Statistics of Responses

Table 5.2 presents the distributions of respondents’ ratings of probability of divorce and family migration decision for the fictitious couples. The ranges of

\(^6\)The relatively large number of missing data in the demographic information is largely due to a technical issue that the respondent needs to shift from the mouse to keyboard when answering the demographic information. I noted this problem only half way in the data collection process. This problem will be fixed in future research.
Table 5.1: Demographic Characteristics of the Respondents

<table>
<thead>
<tr>
<th></th>
<th>Number of Cases</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Sex</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>11</td>
<td>48%</td>
</tr>
<tr>
<td>Female</td>
<td>14</td>
<td>38%</td>
</tr>
<tr>
<td>Missing</td>
<td>4</td>
<td>14%</td>
</tr>
<tr>
<td><strong>Race</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Non-Hispanic White</td>
<td>14</td>
<td>48%</td>
</tr>
<tr>
<td>Non-Hispanic Black</td>
<td>2</td>
<td>7%</td>
</tr>
<tr>
<td>Asian</td>
<td>5</td>
<td>17%</td>
</tr>
<tr>
<td>Other</td>
<td>2</td>
<td>7%</td>
</tr>
<tr>
<td>Missing</td>
<td>6</td>
<td>21%</td>
</tr>
<tr>
<td><strong>Marital Status</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Never Married</td>
<td>15</td>
<td>52%</td>
</tr>
<tr>
<td>Married</td>
<td>4</td>
<td>14%</td>
</tr>
<tr>
<td>Separated/Divorced</td>
<td>4</td>
<td>14%</td>
</tr>
<tr>
<td>Remarried</td>
<td>2</td>
<td>7%</td>
</tr>
<tr>
<td>Missing</td>
<td>4</td>
<td>14%</td>
</tr>
<tr>
<td><strong>Immigrant Status</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Non-Immigrant</td>
<td>17</td>
<td>59%</td>
</tr>
<tr>
<td>Immigrant</td>
<td>8</td>
<td>28%</td>
</tr>
<tr>
<td>Missing</td>
<td>4</td>
<td>14%</td>
</tr>
<tr>
<td><strong>Age (in Years)</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>&lt;20</td>
<td>1</td>
<td>3%</td>
</tr>
<tr>
<td>20-29</td>
<td>17</td>
<td>59%</td>
</tr>
<tr>
<td>30-39</td>
<td>3</td>
<td>10%</td>
</tr>
<tr>
<td>&gt;=40</td>
<td>4</td>
<td>14%</td>
</tr>
<tr>
<td>Missing</td>
<td>4</td>
<td>14%</td>
</tr>
</tbody>
</table>
the respondent-rated probability of divorce vary substantially: Some respondents’ answers spread over 90 percentage points (ID = 1002, 1004, 1005, 1018, 1025, 1070, 1092, 1097), while others’ answers fall in a relatively narrow range. One respondent (ID = 1012) even believes that the marriage of all 40 fictitious couples will remain intact for another five years or longer. The means of the probability of divorce also vary substantially, with that of two respondents above 50% and that of 3 respondents below 10%. This suggests that these respondents may hold very divergent views on marriage prospects and how it varies with the characteristics in the fictitious couples.

The majority of respondents give a relatively large number of ratings that the fictitious couple would decide to commute, i.e., for one spouse to take the job offer and move to Washington DC and the other spouse to stay in New York City. Thirteen out of the 29 respondents give 10 or more such ratings (out of 40) and only 9 out of the 29 respondents give 5 or fewer such ratings (out of 40). This finding is somewhat unexpected because the literature on family migration has always assumed that the family should move as a unit and rarely considered this possibility of “married couples living apart”, while the respondents in this study are not shy about entertaining this seemingly new social trend. It is unclear whether the respondents think “married couples living apart” (Binstock and Thornton 2003) is a prelude to marital disruption from these descriptives, although those who give the lowest ratings of probability of divorce also tend to think the couples should make migration decisions as a unit and disfavor the “commute” option (e.g., ID = 1012, 1021, 1096, 1099).
<table>
<thead>
<tr>
<th>ID</th>
<th>Probability of Divorce (%)</th>
<th>Family Migration Decision</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>mean</td>
<td>min – max</td>
</tr>
<tr>
<td>1001</td>
<td>30.0</td>
<td>5 – 74</td>
</tr>
<tr>
<td>1002</td>
<td>25.4</td>
<td>0 – 96</td>
</tr>
<tr>
<td>1003</td>
<td>46.0</td>
<td>16 – 83</td>
</tr>
<tr>
<td>1004</td>
<td>38.9</td>
<td>1 – 91</td>
</tr>
<tr>
<td>1005</td>
<td>29.2</td>
<td>0 – 90</td>
</tr>
<tr>
<td>1006</td>
<td>12.7</td>
<td>1 – 52</td>
</tr>
<tr>
<td>1009</td>
<td>24.8</td>
<td>3 – 81</td>
</tr>
<tr>
<td>1012</td>
<td>0.0</td>
<td>0 – 0</td>
</tr>
<tr>
<td>1015</td>
<td>16.5</td>
<td>2 – 31</td>
</tr>
<tr>
<td>1017</td>
<td>50.3</td>
<td>20 – 80</td>
</tr>
<tr>
<td>1018</td>
<td>36.5</td>
<td>0 – 91</td>
</tr>
<tr>
<td>1021</td>
<td>6.2</td>
<td>1 – 17</td>
</tr>
<tr>
<td>1023</td>
<td>18.7</td>
<td>0 – 60</td>
</tr>
<tr>
<td>1024</td>
<td>17.6</td>
<td>0 – 61</td>
</tr>
<tr>
<td>1025</td>
<td>35.9</td>
<td>3 – 94</td>
</tr>
<tr>
<td>1026</td>
<td>42.5</td>
<td>11 – 69</td>
</tr>
<tr>
<td>1029</td>
<td>31.7</td>
<td>5 – 65</td>
</tr>
<tr>
<td>1034</td>
<td>15.8</td>
<td>0 – 40</td>
</tr>
<tr>
<td>1037</td>
<td>30.4</td>
<td>1 – 83</td>
</tr>
<tr>
<td>1038</td>
<td>12.4</td>
<td>0 – 79</td>
</tr>
<tr>
<td>1039</td>
<td>10.3</td>
<td>2 – 30</td>
</tr>
<tr>
<td>1070</td>
<td>28.7</td>
<td>0 – 92</td>
</tr>
<tr>
<td>1091</td>
<td>14.9</td>
<td>1 – 31</td>
</tr>
<tr>
<td>1092</td>
<td>26.4</td>
<td>1 – 91</td>
</tr>
<tr>
<td>1095</td>
<td>5.8</td>
<td>0 – 20</td>
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<tr>
<td>1096</td>
<td>11.5</td>
<td>2 – 46</td>
</tr>
<tr>
<td>1097</td>
<td>30.5</td>
<td>0 – 100</td>
</tr>
<tr>
<td>1098</td>
<td>51.1</td>
<td>15 – 75</td>
</tr>
<tr>
<td>1099</td>
<td>15.9</td>
<td>5 – 70</td>
</tr>
</tbody>
</table>
5.6.3 Beliefs Concerning Marriage Prospects

The left panel of Table 5.3 presents the results predicting marriage prospects, measured as the probability of divorce within five years. After completing the survey, several respondents were aware of the names for the fictitious couples being associated with certain race and asked the experimenter about this, but this did not seem to be universally the case and the coefficients for the races of the spouses are not statistically significant. The respondents did not believe that age (in the 10-year range presented in the fictitious couples) or education is associated with the probability of divorce either.

The respondents believed that couples with higher earnings are less likely to divorce, with about a 1% difference in the probability of divorce for every 10,000 dollars difference in annual earnings. They also seemed to believe that the probability of divorce is 1% higher for every 10,000 dollar the husband earns more than the wife.\(^7\)

The number of children is associated with lower probability of divorce in these respondents’ beliefs. Childless couples are most likely to divorce than couples with one or two children. However, the sex composition of children is not associated with divorce rates.\(^8\) The respondents believed that couples who are each other’s best friend are almost 20% less likely to divorce within five years than couples who are not. They also believed that couples who have been married longer have lower divorce rates.

\(^7\)The difference might be nonlinear in that the respondents think couples with similar or even equal earnings have the lowest probability of divorce (Brines and Joyner 1999; Jasso 1988), but I have not explored this possibility in this chapter.

\(^8\)The log-likelihoods for the two nested models only specifying the number of children and specifying the sex composition are nearly the same. Because the sex-composition specification uses three more degrees of freedom, the likelihood ratio test prefers the simpler model.
The gender role manipulation appears to work properly, despite its relatively lengthy wording. The respondents believed that couples have the lowest probability of divorce if both spouses hold an egalitarian gender-role attitude, followed by the couples that both hold a traditional gender-role attitude. The couples in which the husband holds a traditional gender role while the wife holds an egalitarian gender role are perceived to have the highest probability of divorce. These results seem to suggest that one need not be a feminist to sense that the traditional gender-role specialization in marriage is more to the husband’s advantage than the wife’s.

5.6.4 Beliefs Concerning Family Migration Decisions

The right panel of Table 5.3 presents the results for how the respondents believed in what contributes the decisions of family migration when one spouse receives a job opportunity that requires relocation. The most interesting finding in these results is the gender effect. When the husband is the “agent” who receives the job opportunity, the respondent is more likely to say the couple will take the job offer and move as a family, rather than give up on the job and stay in New York City, compared to when the wife is the “agent” who receives the job opportunity that requires relocation.

The respondents believed that education is positively associated with the likelihood that a couple would choose the living style of commuting between two cities for job-related reasons. Alternatively, they might consider highly educated people are more willing than the less educated to pursue career opportunities potentially at a higher cost (e.g., relocating and resettling into a new place) and/or even at the expense of traditional family living arrangement.
The respondents believed that the higher the couple’s earnings are and the husband’s earnings relative to the wife’s earnings are, the less likely they are going to take the job offer and move. The pay raise associated with the job offer, on the other hand, is positively associated with the likelihood of taking the job and moving to DC. I interpret the negative coefficients for earnings as reflecting the economic calculus in the respondent’s head that compares the gain of a pay raise in proportion to their current earnings as they imagine the fictitious couples making their family migration decisions. This interpretation can be tested with an alternative specification of the relationships among the economic variables in the vignettes, but is beyond the scope of the current analysis.

Current earnings are not associated with the likelihood that the respondents thought the fictitious couple would commute, rather than decline the job offer and stay in NYC as a family. However, the pay raise associated with the job offer increases the chances that the respondents said the fictitious couple would commute, and the coefficient is of the same magnitude as the coefficient for moving.

If the spouse is likely to find a job in DC comparable to what s/he has in NYC, the respondents believed that the couple would be more likely to take the offer and move, but the spouse’s likelihood of finding a job is not associated with the likelihood that the respondents believed that the fictitious couple would commute. The strong effect for spouse’s job prospects—consistent with a mutual support between spouses, although also consistent with the joint economic well-being maximization—is a factor that the existing theoretical models of family migration has not considered.

Marriage prospects of the fictitious couple, measured in terms of the prob-
ability of divorce within five years as an answer provided in Session I by the respondent, are associated with the respondents’ belief that the couple would consider the possibility for being physically apart and commute, but it is not associated with their belief in the decision whether the couple would move or stay. This is consistent with the argument that family migration decision is not believed to operate as maximizing the joint family well-being, but the private well-being when marriage is not viewed as a cohesive, permanent institution.

Gender role attitudes appear to affect the respondents’ beliefs about family migration decisions. The respondents believed that the fictitious couple is most likely to move when the husband holds an egalitarian gender role attitude and the wife holds a traditional gender-role attitude. One possible explanation for this pattern is a combination of two factors: (1) An egalitarian husband is believed to adjust the “gender bias” that when the wife, rather than the husband, receives a job offer, the couple is believed to be less likely for the wife to take the job and for both spouses to move. (2) A traditional wife is believed to reinforce this same gender bias so that the fictitious couple would be even more likely for the husband to take the job offer and move when the husband is the “agent.” It requires the specification with an interaction effect to test this explanation that will be carried out in future research. In terms of commuting versus staying, when both husband and wife hold a traditional gender role attitude, the respondents were least likely to say that they would commute. When the husband holds a traditional gender role attitude and the wife an egalitarian attitude, the respondents were most likely to say that they would commute. This suggests that the respondents believed that “married couples living apart” may be a life style that traditional couples would not consider. On the contrary, it may be a life style that provides an opportunity, other than
separation or divorce, for egalitarian wife married to a traditional husband to gain autonomy and furthers her career. Again, this speculation can be tested with an interaction effect with the gender of the “agent” for future analysis.

The respondents believed that the presence and the number of children will increase the likelihood that the fictitious couple decline the job offer and stay in New York City. However, the coefficients are not statistically significant, suggesting that the respondents held a relatively wide range of opinions in this regard. This might also reflect characteristics of the sample, as 18 out of the 29 respondents were under age 30 and might not have considered the implications of having children for family decisions. Future research should gather a more diverse sample, obtain additional information on the respondents’ own family conditions, and exploit the multilevel data structure to answer this question.

Marriage duration has no effect on family migration decisions. However, spouses being each other’s best friend decreases the likelihood that the respondent said a fictitious couple would commute. The zero-order association has an effect of -.64 and is statistically significant, but controlling for other factors reduce the effect to -.26 and brings it below the significance level.

5.6.5 Results for the Joint Model

Table 5.4 presents the results for the joint model of marriage prospects and family migration. The substantive results are generally comparable to the results reported in the previous table. Thus, I focus on the correlations between error terms. The correlated unobserved heterogeneities may be interpreted as reflecting other factors that, in deciding what they believed that the fictitious couples would do, the respondents considered but were not stipulated in the vignettes. The unobserved heterogeneities are positively correlated between move versus
stay and the probability of divorce and between commute versus stay and the probability of divorce, but the correlations are not statistically significant. The unobserved heterogeneities between the migration decisions are positively correlated and statistically significant, suggesting that the vignettes have not fully captured how the respondents thought about the decisions of moving, staying, and commuting. Controlling for the correlated errors in a multiequation model helps remove the potential biases the unobserved heterogeneity may yield for the predictive variables considered. Future research will continue to unpack what makes the respondents believe in which of the three family migration decisions a fictitious couple would make.

5.7 Discussion

This chapter describes the computerized multivariate factorial survey (CoMFaS) method, and applies this method to studying the intertwined beliefs about marriage prospects and family migration decisions. The CoMFaS improves on recent developments in the factorial survey methods (Ganong and Coleman 2006; Jasso 2006) by permitting the dependent variable in the first rating task predicting the probability of divorce for fictitious couples to be an independent variable in the second rating task predicting the family migration decisions. This improvement will allow the analyst to model the recursive structural equations that the respondents might potentially hold in their heads when considering what they believe to occur in the vignette world. Indeed, it is a first step to a full set of multivariate, structural-equation models to potentially describe the intertwined and interrelated sociopsychological processes that lay people use to understand the external world. The more complicated model has a greater potential to re-
fect the sociopsychological reality of the human minds than the single-equation models. Ganong and Coleman (2006) proposed a multiple segment vignette method, but did not suggest a sophisticated set of statistical model to analyze data obtained from their model. They did not intend to model the sociopsychological processes, as other applications of the vignette approach (e.g., Hechter et al. 1999; Jasso and Opp 1997), either. The CoMFaS method described in this chapter accomplishes the truly multiequation, multivariate model of the human minds Jasso (2006) envisioned in the factorial survey framework.

The substantive analysis of interrelated beliefs concerning marriage prospects and family migration decisions yields results that the dominant models of family migration do not predict. The most intriguing finding is that the respondents believe that fictitious couples are more likely to take a job offer and relocate to another place when the husband, relative to the wife, receives the job offer. The standard explanation, since Mincer (1978), of the gendered pattern in migration behaviors has always resorted to the gender differences in earnings. Husband, on average, earns more, and thus wife is more likely to be the “tied” mover/stayer who sacrifices for maximizing the joint family economic well-being. Bielby and Bielby (1992) suggested gender role might moderate the economic effect, but did not confront the potential gender bias in the dominant analysis presumably because it is very difficult to obtain data in support of any challenge to the economic model that is based on widely available economic data. This chapter provides the first piece of empirical evidence using an experimental design, though on a convenience sample, that the gender bias exists in the respondents’ belief in family migration decisions. Whether these respondents will contest this bias when they become the actors who face similar family decisions in their real life is unknown, but the gendered pattern exists net of other factors thought
to affect family migration decisions and net of unobserved heterogeneity of the respondents.

The doubt about potential discrepancy between what one believes and how one behaves has been casted for decades (LaPiere 1934; Pager and Quillian 2005). Thus, whether beliefs predict behavior may be a limitation of the vignette approach in general. However, others have argued that the discrepancy diminishes as the measurement of attitudes improve (Ajzen and Fishbein 1977; Manski 2004). A systematic examination of how beliefs and judgments measured from the factorial survey predict behaviors will be an important next step to advance this literature.

Many of the substantive results reported in this chapter require a close look to test the speculative interpretations, and tease apart competing hypotheses in future research. For example, how gender role attitudes are believed to affect family migration decisions seem to either adjust or reinforce the gender bias discussed earlier. However, even the simplest results challenge the basic assumption that migration decisions are made on the basis that the spouses maximizes their joint economic well-being. Especially when a marriage is not perceived to have a good prospect, the respondents obviously entertained the idea of commuting so that one spouse can pursue his/her own career advancement, independent of his/her spouse’s, even at the expenses of the arrangement of living apart. The effect of marriage prospects (or perceived marital instability) on family decisions, as shown in this chapter and in line with observations made by Bumpass (1990), should be an important aspect to be considered in future theoretical and empirical research.
Table 5.3: Coefficients (and Standard Errors) for Variables in the Model Predicting Marriage Prospects and in the Model Predicting Family Migration Decisions

| Variable | Pr(divorce) | Family Migration | | |
|----------|-------------|------------------|------------------|
|          | Pr(divorce) | move/stay commute/stay |
| husband education | .13 | .09 | .16** |
| education difference | .36 | .03 | .11** |
| husband earnings | -.10* | -.03* | -.01 |
| earnings difference | .10** | -.015** | .001 |
| traditional/traditional | 3.50* | .09 | -.28 |
| gender role (husb/wife) | (1.69) | (.24) | (.27) |
| traditional/egalitarian | 12.30** | .32 | .66* |
| gender role (husb/wife) | (1.73) | (.26) | (.27) |
| egalitarian/traditional | 4.74** | .48* | .21 |
| gender role (husb/wife) | (1.60) | (.24) | (.27) |
| one child | -3.04 | -.40 | -.36 |
| two children | -3.90* | -.35 | -.43 |
| marriage duration | -.64** | .03 | .01 |
| best friend | -18.73** | .02 | -.26 |
| husband receives job | 1.16** | .62** |
| pay raise if takes job | (.09** | .09** |
| likely/unlikely spouse finds a comparable job | .09 | (.01) |
| Pr(divorce) | .003 | .02** |
| | (.004) | (.004) |
Table 5.4: Coefficients (and Standard Errors) for Variables Predicting Marriage Prospects and Family Migration Decisions in a Joint Model

<table>
<thead>
<tr>
<th></th>
<th>Pr(div)</th>
<th>move/stay</th>
<th>commute/stay</th>
</tr>
</thead>
<tbody>
<tr>
<td>husband age</td>
<td>-.06</td>
<td>.04</td>
<td>.03</td>
</tr>
<tr>
<td>age difference</td>
<td>.06</td>
<td>(.04)</td>
<td>(.05)</td>
</tr>
<tr>
<td>husband education</td>
<td>.27</td>
<td>(.26)</td>
<td>.11*</td>
</tr>
<tr>
<td>education difference</td>
<td>.30</td>
<td>(.19)</td>
<td>(.04)</td>
</tr>
<tr>
<td>husband earnings</td>
<td>-.08*</td>
<td>(.03)</td>
<td>-.03**</td>
</tr>
<tr>
<td>earnings difference</td>
<td>.12**</td>
<td>(.02)</td>
<td>-.02**</td>
</tr>
<tr>
<td>traditional/traditional</td>
<td>3.46**</td>
<td>(1.30)</td>
<td>-.04</td>
</tr>
<tr>
<td>traditional/egalitarian</td>
<td>13.31**</td>
<td>(1.34)</td>
<td>.24</td>
</tr>
<tr>
<td>egalitarian/traditional</td>
<td>5.87**</td>
<td>(1.31)</td>
<td>.49</td>
</tr>
<tr>
<td>best friend</td>
<td>-19.24**</td>
<td>(.92)</td>
<td>.004</td>
</tr>
<tr>
<td>one child</td>
<td>-3.80*</td>
<td>(1.35)</td>
<td>-.03</td>
</tr>
<tr>
<td>two children</td>
<td>-4.50**</td>
<td>(1.26)</td>
<td>-.39</td>
</tr>
<tr>
<td>marriage duration</td>
<td>-.52**</td>
<td>(.16)</td>
<td></td>
</tr>
<tr>
<td>husband receives job</td>
<td>1.15**</td>
<td>(.19)</td>
<td>.65**</td>
</tr>
<tr>
<td>pay raise if takes job</td>
<td>.10**</td>
<td>(.01)</td>
<td>.10**</td>
</tr>
<tr>
<td>spouse likely finds a job</td>
<td>2.90**</td>
<td>(.21)</td>
<td>.16</td>
</tr>
<tr>
<td>Pr(divorce)</td>
<td>-.004</td>
<td>(.006)</td>
<td>.02**</td>
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<tr>
<td>constant</td>
<td>39.99**</td>
<td>(9.59)</td>
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<tr>
<td>$\sigma_u$</td>
<td>12.85**</td>
<td>(1.78)</td>
<td></td>
</tr>
<tr>
<td>$\sigma_e$</td>
<td>15.17**</td>
<td>(.32)</td>
<td></td>
</tr>
<tr>
<td>$\sigma_{e_1}, \sigma_{e_3}$</td>
<td>1.26**</td>
<td>(.18)</td>
<td>.56**</td>
</tr>
<tr>
<td>$\rho_{u1}, \rho_{u3}$</td>
<td>.21</td>
<td>(.26)</td>
<td>.06</td>
</tr>
<tr>
<td>$\rho_{13}$</td>
<td>.45**</td>
<td>(.09)</td>
<td></td>
</tr>
<tr>
<td>log likelihood</td>
<td>-5507.21</td>
<td></td>
<td></td>
</tr>
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</table>
Chapter 6

Conclusion

6.1 The Case Against Divorce Reconsidered

In their influential book, Waite and Gallagher (2000) attempt to establish the case for marriage. They stipulate the causal mechanisms through which married people become better off in all realms of life, and argue that marriage is the way towards a higher level of social, economic, and emotional well-beings. When they try to supply empirical data in support their case, they draw comparisons between married people and single people, married people and cohabiting people, between married people and divorced people, and between married people and widowed people. They claim that the evidence is overwhelmingly in favor of marriage over other arrangements, and the between-group differences reflect causal relationships.

I embark this dissertation research with a narrower focus, and only consider the case against divorce. This focus on divorce makes the comparison group “clean” because the selection and causal mechanisms associated with remaining single, cohabiting, spousal death and divorce may be very different. A clean comparison helps to clarify what are the selection mechanisms that need to be controlled and what are causal mechanisms to be identified. Take the analysis of
children’s emotional well-being for example, focusing on the case against divorce simplifies the analysis so that I do not need to consider the comparison with parental death. Nor do I have to consider those children born out of wedlock, and whose parents subsequently become married to each other or separated. Thus, I can employ the counterfactual thinking and stipulate the scenarios for the static selection and dynamic selection, and ask what would happen to a child of divorce had his/her parents remained married. This counterfactual thinking is important for making causal claims especially in the case against divorce because, for example, the seminal work by Cherlin et al. (1991) shows that many of the disadvantages for children of divorce have been present prior to parental marital disruption. The observed differences may have existed regardless of the change in their parents’ marital status. The counterfactual model has also been useful in developing statistical techniques for making causal inferences, e.g., the propensity score method. In this dissertation, I describe an extension of the propensity score method and discuss related methodological issues, and then apply it to studying the effect of divorce on adult health.

Do we have a case against divorce? In two of the empirical chapters in this dissertation, I find no effect of divorce on children’s behavior problems, no effect of divorce on men’s physical health and general health status. However, I find a negative effect on both men’s and women’s mental health and a negative effect on women’s physical health and general health status. Notably, the longitudinal data and panel statistical models used in the analysis of children’s behavior problems have greater ability to control for selection on unobservables. The analysis of adult health relies mainly on the assumption that there is no hidden bias. Hence, I place more confidence in the findings of the child chapter than those reported in the adult chapter. For the findings on adult health, it is more
appropriate to consider these estimates as an upper bound of the effect. That is, the effect on adult health might be smaller (or less negative) if we properly control for selection on unobservables with better data and a better research design.

A fundamental point can be drawn from the analyses presented in this dissertation and, more generally, from the counterfactual causal framework is that one needs to be cautious in making causal claims. The counterfactualists argue that any causal claim must involve an explicit statement of the treatment and comparison groups (Holland 1986), and this explicitness will help clarify theoretical arguments and refine the specific policy prescriptions. For example, the effects estimated in the chapters on child behavior problems and adult health outcomes should both be interpreted as “the effect of divorce for the divorced” and bears no relevance for an average individual randomly drawn from the population. Therefore, the null finding for parental divorce on children’s behavior problem suggests that divorcing parents should perhaps consider other venues to improve their children’s emotional well-being, rather than worrying about whether to get a divorce or stay married. It is important to emphasize that these findings apply to parents who are on the verge of divorce and hence speak to policy implications for this group. Thus, these estimates do not imply what a happily married couple should do regarding their marital status and the well-being of their children. Heckman and colleagues have discussed these issues in detail in a series of papers (for a most recent summary, see Heckman 2006).

The counterfactual thinking that guides this dissertation also explicitly recognizes the heterogeneity of treatment effects, and brings us from comparing average differences to contemplating a theory grounded in the individual behaviors. Hence, such theoretical arguments as children of divorce, on average,
enjoy a lower level of parental resources than children in two-parent families cannot validly lead to the prediction that children of divorce would have been better off had their parents avoided the divorce and remained married. What would have happened to them depends on whether, at an individual level, divorced parents would be willing and able to provide equal level of resources as they would have—had they remained married. The average treatment effect literally averages over these individual-level “counterfactual effects”, rather than average over “observed differences” at the aggregate level. Hence, I believe the counterfactual thinking will improve the quality of the policy debates that motivate the dissertation in that it helps us think through what factors necessarily change with divorce and what not. If the causal mechanisms leading to worse post-disruption well-being must change with divorce, e.g., legal status leading to different tax eligibility, then a policy prescription targeted at divorce is desirable. However, if the causal mechanisms do not have to be tied to divorce, e.g., parental love or even income, then perhaps the proper policy prescription should be targeted directly at these causal mechanisms, rather than indirectly at a remote factor, i.e., divorce, that may or may not effectively change the real causal mechanisms that matter. This is indeed consistent with a deeper, theoretical point that even the counterfactualists often ignore because they tend to define “treatment” in a relatively rigid way (e.g., emphasizing on the “manipulability” of a treatment so that race, a key sociological variable, can never conceivably have an effect). Rather than an over-emphasis on the “treatment,” which is often driven by practical or policy concerns, it might be worthwhile to step back and think through the fundamental sociobehavioral mechanisms (Jasso 2003). One can then derive practical/policy implications from fundamental sociobehavioral theories that do not have to correspond to an predetermined
policy implementation. For example, if strong and lasting love is what really matters to people’s health from a theoretical standpoint, then one can imagine policy implications that will increase and prolong love not necessarily through the formal and public institution of marriage, but through more individualistic and private routes.

6.2 The Cultural Effects of Divorce

The concluding remarks seem to side myself with one camp of the debate over marriage and family change. That is to view “modern coupledom” as nothing but intimate relationships (Kipnis 2004; Stacey 1996), and thus refutes the other camp’s arguments that the effects of marriage (and divorce) can only be provided by a publicly-sanctioned and community-supported institution (Waite and Gallagher 2000). I would dispute this charge. In fact, the chapter on the beliefs concerning marriage prospect and family migration decisions take a different view from the above two camps. Instead, I argue that the real effect of divorce following dramatic changes in the family over the past few decades lies in cultural beliefs about the family. Therefore, unlike those who hold a “modern-coupledom” view, I argue that the effect of divorce is not entirely private and personal because the overall societal change in the family, not changes in individual family behaviors, affects individual beliefs. Different also from those who argue that the effects of marriage and divorce come from public sanctions by law and the community, I argue that what has really changed in the “brave new families” (Stacey 1991) is individuals’ assessments of what are expected of them and where they stand, relative to other members in the society, regarding their family lives and marriage/childbearing trajectories. As Bumpass (1990)
pointed out in his presidential address to the Population Association of America, an effect of divorce (or more specifically, the high prevalence of divorce or the uncertainty about marriage as a life-long commitment in modern society) may be that people give up the “traditional” cultural beliefs that place a higher weight on marital cohesiveness than considerations of individual well-being. The results from the chapter on an aspect of the cultural beliefs, although hardly representative of the contemporary American society because of a convenience sample, do provide suggestive evidence for the existence of an effect of divorce that many cultural theorists have long speculated and most social demographers have long overlooked.

This type of effect that goes from the aggregate to the individuals indeed has a long tradition of theoretical interests in sociology (Coleman 1990). However, because it is difficult to observe these effects empirically and perhaps also because quantitative sociologists tend to avoid the hard-to-define term of culture, a relatively thin empirical literature, especially in social demography, has explored these effects and their implications for social behaviors. Nonetheless, the cultural beliefs of divorce are likely to have profound impacts on various aspects of our lives because they are more resistant to policy interventions and more difficult to “fix” than other mediating mechanisms underlying the effect of divorce (e.g., declines in income) discussed in this dissertation and elsewhere. The profoundness of culture can also be seen, for example, in that the respondents in the vignette study tend to believe that the family would be more likely to take the job offer and move if the husband, rather than the wife, is the person who receives a job offer—while all other economic factors are held constant by the experimental design. Although it is unclear to what extent and how quickly cultural beliefs may translate into behaviors, ignoring these cultural as-
pects regarding divorce, and more broadly the effects of family change, would lose a wealth of important information that might improve our understanding of family life and the implications of family change on human behaviors and well-being. In sum, I hope this dissertation has achieved two goals: to improve the quality of the policy debate on the case against divorce and to show the possibility of examining the cultural implications of divorce in the contemporary society.
Appendix A

Definitions of Control Variables

These control variables are constructed and defined as follows:

Background information

The race and ethnicity of the NLSY79 respondent (i.e., the mother) is measured by two dummy variables—coded 1 for black and for Hispanic—with non-black-non-Hispanic being the reference category. I use the variable (R02147.) based on the 1978 household screening, which provides the basis for weighting the NLSY79 data, to construct the variables for race and ethnicity. Nativity is coded 1 if mother was born in a foreign country. Catholic is coded 1 if the respondent reported in 1979 that she was raised in Catholicism. Religiosity is measured on a 6-point Likert scale for the frequency of respondent’s church going activities in 1979 (coded 1 for “not at all”, 2 for “infrequently”, 3 for “once in a month”, 4 for “2-3 times a month”, 5 for “once a week”, and 6 for “multiple times a week”). I also control the mother’s cognitive ability using the AFQT (Armed Forces Qualification Test) percentile score, with missing data substituted by the sample mean.
Home environments at age 14

I include two sets of measures of the mother’s home environment at age 14. The family structure is measured by four dummy variables, coded 1 for “living with both biological parents”, for “mother only family”, for “stepmother-father family”, and for “stepfather-mother family.” The reference group is all the other living arrangements. The presence of reading materials is constructed by combining three questions inquiring if any household member regularly received magazine, newspaper or possessed a library card, coded 0 if no household member received any magazine/newspaper or possessed a library card.

Family formation behaviors

Three controls of family formation behaviors of the mother are included. (1) The NLSY79 asked the respondent’s age at first sex in three waves—1983, 1984 and 1985. If there are multiple reports of age at first sex, I use the most recent one. An indicator for teenage sex is constructed, coded 1 if the mother reported a first sexual intercourse before age 20. Separate indicators are constructed for those who have no valid data in all three surveys and for those who haven’t had sex by the 1985 survey. The reference category is thus those who reported having had sex by the 1985 survey, but whose age at first sex is beyond 20 years of age. (2) Age at first birth is another control of family formation behavior. It is a continuous variable in years. Median substitution is used for missing data on age at first birth. (3) The third variable is age at first marriage, a continuous variable measured in years.
Education

The NLSY79 inquired the respondent’s detailed history of their educational attainment. Using the highest grade completed reported in each survey year, I construct a set of dummy variables for mother’s education approximated to the nearest survey year prior to the formation of her first marriage. These dummy variables indicate educational levels of “less than high school”, “high school graduate”, “some college” and “four year college and more.”

Marital history

The history of mother’s first marriage is constructed using the information collected in each wave, which asked the respondent to report up to three marriage events between consecutive interviews. The respondent also reported the month and year of these events. Because the consecutive surveys are 1-2 years apart, the recall bias associated with time elapse (Wu et al. 2001) is minimal especially compared to other retrospective surveys. For those first marriage ended, I create dummy variables for the reason of which her marriage was ended, coded 1 for “separation”, “divorce”, “spousal death”, and “unknown reason.” A major reason for the respondents to fall in the “unknown reason” category is if they were married for more than once in the first survey interview in 1979, in which only the reason for ending the most recent marriage was recorded. The end date of marriage is recorded when the corresponding event of separation, divorce, spousal death, or unknown cause occurred.

Marital happiness

A series of relationship satisfaction questions were asked in seven (i.e., 1988, 1992, 1994-2002) waves of interviews of those mothers living with a spouse or
opposite-sex partner. I use the overall happiness, which asks the respondent “Would you say that your (relationship/marriage) is very happy, fairly happy, or not too happy?” and dichotomize it so that the variable is coded 1 for being unhappy in her marriage.

**Total net family income**

Total net family income is measured in thousand dollars for the year prior to the survey, and adjusted to 2002 dollars using the Consumer Price Index. I impute missing data using interpolated or extrapolated values within a respondent across survey years unless there are no valid data for an individual throughout all rounds. If the interpolated/extrapolated value is negative, a value of 10 dollars is assigned. I construct a time-invariant variable of total net income at the formation of first marriage by averaging the total net family income of the three years prior to (including) the calendar year in which the respondent was reportedly married the first time.¹

**Attitudes**

Self esteem of the mother was measured by the Rosenberg (1965) Self-Esteem Scale, first in 1980 and again in 1987. The scale consists of 10 items designed to gauge the self evaluation of an adolescent or adult makes and customarily maintains, and describes the degree of approval or disapproval towards oneself. I only use the 1980 measure to avoid the potential endogeniety problem.

¹The specific procedure is a little complicated due to the survey design. For those whose marriages were contracted prior to 1978, the income of year 1978 is used. For those married in 1979, the average of 1978 and 1979 incomes is used. For those married between 1980 and 1993, the average of three years of incomes is used. For those married in 1994 and in the odd years after 1994, the average of two years of incomes is used, whereas those married in even years after 1994, only the income of the prior year is used.
Appendix B

Supplementary Analysis of the Effect of Divorce on Children’s Behavior Problems

To make sure that decision to exclude all children with fewer than three consecutive observations does not change the substantive finding in Table 2.8, I repeat the same analysis reported in the main text on the less-restrictive sample with only two or more observations per child (the minimal requirement of child fixed-effects model). Because non-response or sample attrition is not random, it is possible that those children who have better well-being are more likely to remain in the sample and be interviewed multiple times in a row. Hence, the concern is that the null finding on the effect of parental divorce reported in Table 2.8 may be an artifact because the analytic sample of at least three consecutive observations consists of those children of divorce who are emotionally better off than those children of divorce with fewer observations. If the concern is warranted, a less restricted analytic sample will give a larger estimate of the effect of parental divorce. Table B.1 compares the OLS and FE results based on only two or more observations per child (top panel), the minimal data required to estimate a fixed-effects model, with the results on at least three consecutive
Table B.1: Comparison of Two Analytic Samples

<table>
<thead>
<tr>
<th></th>
<th>Boys</th>
<th></th>
<th></th>
<th>Girls</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS1</td>
<td>OLS2</td>
<td>FE</td>
<td>OLS1</td>
<td>OLS2</td>
<td>FE</td>
</tr>
<tr>
<td>parental divorce</td>
<td>1.76**</td>
<td>1.20**</td>
<td>0.50</td>
<td>1.51**</td>
<td>1.00**</td>
<td>0.46</td>
</tr>
<tr>
<td>(2+ obs./child)</td>
<td>(0.23)</td>
<td>(0.24)</td>
<td>(0.33)</td>
<td>(0.21)</td>
<td>(0.22)</td>
<td>(0.37)</td>
</tr>
<tr>
<td>parental divorce</td>
<td>1.63**</td>
<td>1.16**</td>
<td>0.45</td>
<td>1.39**</td>
<td>0.98**</td>
<td>0.48</td>
</tr>
<tr>
<td>(3+ obs./child)</td>
<td>(0.27)</td>
<td>(0.27)</td>
<td>(0.36)</td>
<td>(0.24)</td>
<td>(0.24)</td>
<td>(0.39)</td>
</tr>
</tbody>
</table>

observations per child (bottom panel) reported in the main text. The estimates are very similar. Hence, the null finding appears not due to restrictions on the sample.
Appendix C

SF-12 Questionnaire

The SF-12 instrument includes the following 12 questions:

1. In general, would you say your health is . . .
   \[
   \begin{align*}
   1 & \text{ excellent} \\
   2 & \text{ very good} \\
   3 & \text{ good} \\
   4 & \text{ fair} \\
   5 & \text{ poor}
   \end{align*}
   \]

   The following items are activities you might do during a typical day. Does your health limit you in these activities?

2. . . . Moderate activities, such as moving a table, pushing a vacuum cleaner, bowling or playing golf?
   \[
   \begin{align*}
   3 & \text{ yes, limited a lot} \\
   2 & \text{ yes, limited a little} \\
   1 & \text{ no, not limited at all}
   \end{align*}
   \]

3. . . . Climbing several flights of stairs?
   \[
   \begin{align*}
   3 & \text{ yes, limited a lot} \\
   2 & \text{ yes, limited a little} \\
   1 & \text{ no, not limited at all}
   \end{align*}
   \]
During the past four weeks, have you had any of the following problems with your work or other regular daily activities as a result of your physical health?

4. ... Accomplished less than you would like?

   \[
   \begin{align*}
   &1 \quad \text{yes} \\
   &0 \quad \text{no}
   \end{align*}
   \]

5. ... Were limited in the kind of work or other activities?

   \[
   \begin{align*}
   &1 \quad \text{yes} \\
   &0 \quad \text{no}
   \end{align*}
   \]

During the past four weeks, have you had any of the following problems with your work or other regular daily activities as a result of any emotional problems (such as feeling depressed or anxious)?

6. ... Accomplished less than you would like?

   \[
   \begin{align*}
   &1 \quad \text{yes} \\
   &0 \quad \text{no}
   \end{align*}
   \]

7. ... Didn’t do work or other activities as carefully as usual?

   \[
   \begin{align*}
   &1 \quad \text{yes} \\
   &0 \quad \text{no}
   \end{align*}
   \]

8. During the past four weeks, how much did pain interfere with your normal work (including both work outside of the home and housework)?

   \[
   \begin{align*}
   &1 \quad \text{not at all} \\
   &2 \quad \text{a little bit} \\
   &3 \quad \text{moderately} \\
   &4 \quad \text{quite a bit} \\
   &5 \quad \text{extremely}
   \end{align*}
   \]

145
The next questions are about how you feel and how things have been with you during the past four weeks. For each question, please give the one answer that comes closest to the way you have been feeling. How often during the past four weeks....

9. ... have you felt calm and peaceful?
   \[
   \begin{array}{l}
   1 \text{ all the time} \\
   2 \text{ most of the time} \\
   3 \text{ a good bit of the time} \\
   4 \text{ some of the time} \\
   5 \text{ a little of the time} \\
   6 \text{ none of the time}
   \end{array}
   \]

10. ... Did you have a lot of energy?
   \[
   \begin{array}{l}
   1 \text{ all the time} \\
   2 \text{ most of the time} \\
   3 \text{ a good bit of the time} \\
   4 \text{ some of the time} \\
   5 \text{ a little of the time} \\
   6 \text{ none of the time}
   \end{array}
   \]

11. ... Have you felt down-hearted and blue?
   \[
   \begin{array}{l}
   1 \text{ all the time} \\
   2 \text{ most of the time} \\
   3 \text{ a good bit of the time} \\
   4 \text{ some of the time} \\
   5 \text{ a little of the time} \\
   6 \text{ none of the time}
   \end{array}
   \]

12. During the past four weeks, how much of the time has your physical health or emotional problems interfered with your social activities (like visiting with friends, relatives, etc.)?
   \[
   \begin{array}{l}
   1 \text{ all the time} \\
   2 \text{ most of the time} \\
   3 \text{ a good bit of the time} \\
   4 \text{ some of the time} \\
   5 \text{ a little of the time} \\
   6 \text{ none of the time}
   \end{array}
   \]
Appendix D

Balancing on Propensity Score, Exposure Time and Marriage Duration

A key problem in making causal inference with observational data is lack of comparability, which manifests itself usually in two aspects—imbalance and lack of complete overlap (or “support” in the econometric literature) on relevant factors (i.e., marriage duration and propensity score in the present analysis) (Gelman and Hill 2007). I first examine the comparability of these two factors separately and then address the issue of comparability using the joint distribution for both factors.

Figure D.1 plots the distribution of marriage duration by divorce status. The lack of comparability in marriage duration between the married and the divorced is clear. Those who were divorced when their health outcomes were measured in their early 40s had a much shorter marriage duration than those who were married. Figure D.2 presents the distribution of propensity score by divorce status. The two groups are incomparable on propensity scores too. The

1There is no gender difference. Hence, I only present the results for the overall sample.
divorced are more likely to fall in the fifth quintile of propensity score, while the married are more likely to fall in the first quintile. The lack of comparability worsens when one conditions jointly on marriage duration and propensity score, as shown in Figure D.3. The figure clearly shows that the divorced score higher on the propensity score and have lower marriage durations than those remaining in a first marriage. The overlap between the two groups on these two dimensions is modest.

To address this problem, I use only those cells with overlapping data—that is, I delete cells with zero cases for either the married or divorced groups. For example, the 88 divorced respondents in the 4th quintile of propensity score and the 1st quintile of marriage duration are excluded from the analysis for lack of
support (or an overlapped “comparable” comparison group) in calculating the weighted mean of the treatment effect (Gelman and Hill 2007).
Figure D.3: Scatter Plots of Marriage Duration and Propensity Score $r(t)$ for NLSY Men and Women
Appendix E

Factorial Survey Instruments

The computerized factorial survey consists of two sessions probing the respondents’ beliefs: the first one about marriage prospects and the second one about family migration decisions of the hypothetical couples in the vignettes.

Session I: Beliefs about Marriage Prospect

Instruction

Dear Respondent:

We are studying what people think about marriage and divorce. We have made up descriptions of fictitious married couples, and we would like to know what you think are the chances that they will stay together or get a divorce. In the couples you are about to see, both spouses are working for pay.

Please point to the number on the line, between 0% and 100%, that represents what you think is the probability that the couple will divorce within five years; then click “OK”: 0% means that you think their marriage will absolutely last for at least another five years, while 100% means that you think that there is no chance that their marriage will survive another five years.
Marriage prospect vignettes: An example

[husname] is [husage] years old; he completed [husedu] years of schooling, and has annual (pre-tax) earnings of [husearn],000 dollars.

[wifname] is [wifage] years old; she completed [wifedu] years of schooling, and has annual (pre-tax) earnings of [wifearn],000 dollars.

They have been married for [mardur] years, have [children], and [are, are not] each other’s best friend.

What do you think is the probability that this couple divorces within five years?

[Draw a line of 100% with ticks on.]

Session II: Beliefs about Family Migration

Instruction

You have now completed Session 1. Thank you. (We are already half way through.)

In Session 2, we will present the same set of married couples to you. This time, one spouse now has a job offer that requires moving from New York City to Washington DC. We will also provide information on the pay raise associated with the job offer, whether or not the other spouse is likely to find a job in DC comparable to the job in NYC, and the couple’s probability of divorcing within five years that you estimated in the previous session.

We would like to know what you think they will do: Will they decide to take the job offer and move to DC? Or will they decide to decline the job offer and
stay in NYC? Or will they decide that one stays in NYC and the other moves to DC?

**Family migration vignettes: An example**

[husname] is [husage] years old; he completed [husedu] years of schooling, and has annual (pre-tax) earnings of [husearn],000 dollars. He believes [hussexrole].

[wifname] is [wifage] years old; she completed [wifedu] years of schooling, and has annual (pre-tax) earnings of [wifearn],000 dollars. She believes [hussexrole].

They have [children]. [agent] now has a job offer, which requires moving from NYC to Washington DC. Taking the offer will increase [her/his] earnings to [agentearn],000 dollars. It is [likely, unlikely] that [spouse] will find a job in DC comparable to [his/her] job in NYC.

You estimated that their probability of divorcing within five years is [divpr].

What do you think this couple will do?

\[
\begin{align*}
(1) & \text{ take the offer and move to DC} \\
(2) & \text{ decline the offer and stay in NYC} \\
(3) & \text{ one moves to DC, the other stays in NYC}
\end{align*}
\]
Bibliography


Hechter, Michael, James Ranger-Moore, Guillermima Jasso, and Christine


