Nature + Nurture = Love?

A Within-Family Analysis of the Trivers-Willard Effect in Breastfeeding Initiation.

Dr. Sebastian Schnettler
(Email: sebastian.schnettler@uni-konstanz.de)

Dept. of Sociology, University of Konstanz, Germany

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1 Introduction

The biological and social sciences have had a long contentious history (Segerstråle, 1986, 2000, 2001). But in recent decades, the potential relevance of biological explanations has gained new interest in some of the social sciences (Freese, J. A. Li, & L. D. Wade, 2003; Schnettler, 2010), but the exact way the two paradigms can profit from each other and potentially be integrated still is matter of debate and practical problems (Freese, 2007, 2008; Freese & Shostak, 2009; Schnettler, 2010; Bearman, 2008; Gintis, 2004; see also Laland & Brown, 2002).

In this paper I wish to contribute to a more detailed engagement of biological with sociological theories on human behavior in one particular substantive area: mothers' breastfeeding behavior. Specifically, the question is if and why mothers differentially invest in their offspring. In an attempt to answer this question I bring together biological and non-biological social science explanations with a particular focus on the Trivers-Willard hypothesis of differential parental investment. The Trivers-Willard hypothesis predicts son-biased investment for parents with high resource-levels and female-biased investment for parents with few resources. Specifically applied to breastfeeding behavior, the hypothesis is: mothers with high levels of resources are more likely to breastfeed their sons, whereas mothers with few resources are more likely to breastfeed their daughters. The Trivers-Willard hypothesis has been put to numerous tests in a variety of animal species and humans, but the empirical evidence has been mixed so far. In this paper, I will improve on previous research by controlling for a number of biological and social influences on parental investment and by using a data set that allows to compare parental investment within the same families.

This paper is part of a larger project in which I examine the Trivers-Willard hypothesis for a range of different parental investment indicators and test a hypothesis as to the relative strength of the effect in different investment types. Compared to other indicators of parental investment that may have a more contemporary taste to them (e.g., going to the movies, helping with school-related tasks), breastfeeding represents a more “natural” or “ancestral” form of investment. It is therefore deemed as a good test case to examine the relative importance of biological and social factors in mothers' decision to invest in their offspring.

The motivation to chose parental investment as a particular area of substantive interest is that it is part of what defines the core interest in both frameworks, evolutionary theory of human behavior and sociological theory of human behavior. In evolutionary theory, the importance of the biological parent-child relationship in humans follows from the relatively long period of care that newborns require compared to other animal species. It further follows from the central role of parental care for improving parents' inclusive reproductive fitness (cf., Salmon, 2005; Geary, 2005). In sociology, a long tradition of research in socialization, social stratification, family sociology, and other fields of specialization has uncovered the central role of parental investment in sustaining and generating the social fabric. Through their investment, parents make children into more or less functioning members of society (cf. Parsons, 1991 [1951]; Maccoby, 1992), they contribute in reproducing existing patterns of social inequalities (cf., McLanahan & Percheski, 2008; Neckerman & Torche, 2007), and contribute in important ways to create social solidarity. Furthermore, life course and stratification research have demonstrated that parental investment can have long-lasting and consequential effects on individual outcomes later in the life of children (cf., Haveman & Wolfe, 1995; Mayer, 2009; Leibowitz, 1977).

The downside of the importance of parental investment is that if parents don't fulfill their roles (and if their contribution is not adequately replaced by other social institutions), negative consequences for individual children and society at large may arise, ranging from reduced psychological health and reduced chances of mobility for individuals to deviance, reduced social solidarity, and lower levels of aggregate human capital for society as a whole. Empirical research has shown that memories of unfavored treatment may be long-lasting and lead to more conflicted relationships between adult children and their parents (Bedford, 1992). Furthermore, differential treatment may trigger a cumulation such that differential treatment affects child emotionality negatively which in turn may elicit a further decrease in parental investment (see Brody, Stoneman, & McCoy, 1992). Children who felt treated less favorably were also shown to have higher rates of delinquency (Scholte, Engels, de Kemp, Harakeh, & Overbeek, 2007).

It is thus of utmost importance to understand what mechanisms drive parental investment. This resonates well with the recent upsurge in sociology of a mechanism-based, or analytical, sociology (cf., Hedström & Swedberg, 1998; Hedström, 2005) and calls for better explanatory theory (cf., Mayer, 2009, pp. 423-424). Both sociological and evolutionary theories on human behavior have contributed different explanations for differences in parental investment. Engaging these explanations with each other promises to bring new light to this important area of research and may help policy makers identify areas where parents need to be supported in their attempts to provide investment in their children.

2 Theory and Previous Research

2.1 Introduction

In this paper, I will analyze the factors that determine whether a mother breastfeeds her child or not. I will follow common terminology from the breastfeeding literature and define "*breastfeeding initiation"* as whether the mother has ever breastfed her child or not, "*breastfeeding continuation"* as whether the mother has continued to breastfeed her child beyond a specific duration, and *"breastfeeding exclusivity"* as whether the mother has exclusively breastfed her child during a given period or whether she has instead combined breastfeeding with bottle feeding in that period. There are several possible combinations: some mothers may never breastfeed their children, some mothers may exclusively breastfeed their children and introduce other types of food only after a given period, and yet other mothers may combine different feeding methods from the beginning. By discussing different factors that have been found to influence this decision-making process, I aim to identify necessary controls for an empirical test of the Trivers-Willard.

2.2 The Trivers-Willard Hypothesis

2.2.1 Introduction

Building on inclusive fitness theory and Fisher's research on population sex ratios (not reported here[1]), Trivers and Willard extended the theory of inclusive fitness to predict both differential sex ratios and differential parental treatment based on differences in parental condition (Trivers & Willard, 1973; Trivers, 2002). In the following I will refer to this prediction as the Trivers-Willard (TW) hypothesis. The logic of the predicted effect is the following: If (1) the condition of offspring correlates with mother's (parents') condition during pregnancy and/or through investment after birth, (2) parental condition influences the reproductive success of sons more strongly than that of daughters, (3) then parents' reproductive success would be higher if they favored sons when in good condition and daughters when in bad condition.

A vast amount of research shows that condition (1) holds for humans. The positive effects of parental social class on social mobility and children's attainment have long been shown shown in sociological research. Condition (2) holds in all species where males can potentially have a much higher maximum number of offspring than females. This is not only the case in many animal species

1 Fisher had shown that in a local breeding population, a sex ratio of 50:50 should be selected for.

but also in humans. For women, their maximum fertility as measured by the number of children is limited by the long gestational period of nine months and the limited life-time period in which women are fertile. Furthermore, the production of female gametes is more costly and more limited than the production of male gametes. Parents in good condition can thus increase the reproductive success by investing more in sons that potentially can have more offspring than daughters. On the other hand, parents in bad condition increase their reproductive fitness by investing more in daughters than in sons, because sons of parents in bad condition may end up having few to no children at all (because they are in competition for mating partners with sons of parents in better condition). For daughters of parents in bad condition the mating competition is lower than for males, thus daughters are in this case a less "risky" investment (cf., Hopcroft, 2005; Trivers & Willard, 1973). Following Trivers and Willard, it is thus very likely that mechanisms have evolved during the long process of evolution that lead to sex-preferential parental investment based on parental condition. However, there are important inter-species differences based on the relative degree of paternal to maternal investment. In species where fathers invest relatively more in their children (as is the case in humans), the difference in cost for reproduction between the sexes and thus the expected differences in variance of reproductive success are much lower. Trivers and Willard predict that the hypothesized effect should also be found in humans. But the effect should be much smaller as compared to species with lower or no paternal investment (Trivers & Willard, 1973; Trivers, 2002).

The TW hypothesis relies on the assumption that male reproductive success is higher in males of higher socioeconomic status than for males in lower socioeconomic status and that the variance of male reproductive success is higher in males than in females. Some studies explored whether this proposition held both in historical societies and in contemporary developed societies (e.g., Voland, 1995; Klindworth & Voland, 1995; Low & Clarke, 1992; Røskaft, Wara, & Viken, 1992; S. Scott & Duncan, 1999). Whereas behavioral ecologists would state that this link needs to hold in contemporary societies for the Trivers-Willard effect to still be active, for evolutionary psychologists the link between socioeconomic status and fertility is not a necessary precondition for the TW effect. They assume that our mind is largely adapted to an ancestral environment in which humans presumably lived as hunter gatherers in small bands (Hopcroft, 2005, p. 1114; Barkow, Cosmides, & Tooby, 1992). Therefore a positive correlation of resource levels or socioeconomic status and fertility needs to have existed long enough in our ancestral past that adaptive mechanisms like those determining the TW effect may have developed – but it doesn't need to exist anymore for the respective mechanisms to still be active today.

2.2.2 Empirical Findings from Research on Animals

Parental biasing may take different forms and Trivers and Willard have left it open what kind of specific mechanisms are likely to exist. At least two major forms of biasing may be distinguished: sex-ratio biasing (SRB) at birth (consequence of somatic processes) and at later ages (e.g., due to sex-biased infanticide or investment differences) and resource allocation biasing (RAB). The two are overlapping to a degree in that differential RAB may lead to differential SRB. An extreme example of this would be a case in which parents let their children starve. In using this distinction, I follow the terminology proposed by Keller et al. (2001).

Until today, empirical evidence for the TW hypothesis remains mixed, both for humans as well as for many animal species (cf., Trivers, 2002, pp. 120-122). Keller, Nesse, and Hofferth (2001) find that most studies that examine the TW hypothesis in animals focus on SRB: Of a total of 70 studies they perused, 62 are on SRB and only 8 on RAB. Of those studies that were on SRB, most found sex-ratio biasing in accordance with the TW prediction. Of those eight studies that examine RAB, about one half is in accordance with the TW hypothesis and one half in contradiction with it (Keller et al., 2001, p. 344).

A more recent meta-analysis of 35 studies covering a total of 15 non-human primate species from 23 populations shows that the effects sizes depend inversely on sample size (Brown & Silk, 2002, p. 11252). Given that many of the studies are based on small samples, the authors take these results as indication that empirically found SRB may be due to stochastic variation in small samples rather than a results of real parental biases. It is not clear how many of the research articles examined in this study overlap with those examined in Keller et al. (2001). Furthermore, there is evidence of a publication bias such that studies that don't find a meaningful interaction between parental condition and parental biasing are less likely to get published (Festa-Bianchet, 1996; Smith, 1983, p. 873).

2.2.3 Empirical Findings on Sex-Ratio Biasing in Humans

Studies on humans have tested the TW effect in a variety of historical societies and with different methods. In contrast to the trend in animal studies, most studies on the TW effect have examined RAB. In the following I first briefly summarize the results on SRB in humans and then proceed to a more detailed account of studies on RAB.

Based on data from a historical family reconstruction project in northern England 1600-1800, one study finds a slight, yet statistically non-significant, increase in the male-female sex ratio from 1.06 to 1.17 when moving from the lowest to the highest class examined (S. Scott & Duncan, 1999, p. 90). Further support comes from studies about Hungarian Gypsies, a subpopulation ranking relatively low in the regional hierarchy: for them the authors find a lower average sex ratio when compared to the coresident non-Gypsy population (Bereczkei & Dunbar, 1997).

For the contemporary US, Mackey and Coney (1987) analyzed the sex ratios of men and women who were listed in the *Who's Who*, a publication of biographies of notable persons, and compared them with sex ratios in the general population. They found that the comparison was consistent with the TW hypothesis for men but not for women listed. Stronger evidence comes from another study that is based on birth and death register data from the contemporary US between 1983–2001, covering at total of 48 million births and 310,000 deaths. The authors show that married, better educated, and younger mothers bore more sons, and that on the other hand males had a higher risk of infant deaths than females among young, unmarried mothers (Almond & Edlund, 2007). Although the authors interpret the results as supportive of the TW hypothesis, it remains unclear what exactly the status of mother's age as a resource is.

2.2.4 Empirical Findings on Resource-Allocation Biasing in Humans

With regard to RAB, some of the same data sources were used that were also applied to study SRB. But a number of studies also exclusively examined RAB. Scott and Duncan (1999, pp. 90-92) referred to the same community in northern England between 1600-1800 mentioned above. They found that female infant mortality was significantly lower than male infant mortality in the elite class. But there was no statistical difference in infant mortality for tradesmen and the lowest class. Although this seems to be inconsistent with the TW hypothesis, their explanation of this effect illustrates the intricacies of interpreting the TW effect and provides an example for the evolutionary psychological argument that evolved mechanisms can, under certain circumstances, lead to maladaptive outcomes: Wet nursing, that is, nursing by a woman who is not the biological mother of the infant, was a common practice among elite families. Given the contraceptive effects of nursing, elite mothers were able to decrease the spacing between births. Together with the practice of "outsourcing" the nursing of some of their children to wet nurses, elite mothers could thereby increase their overall fertility. At the same time, wet nursing was seen as inferior to nursing by the biological mother. Therefore, it was left mainly for daughters in elite families. Boys, on the other hand, were more likely to be nursed by the biological mother. But because mothers weaned their boys earlier than they had wet nurses wean their daughters, and given the benefits of extended breastfeeding for infant development that were unknown to mothers at that time, girls were actually left better nourished than boys (ebd.: 102-106). In other words, the intention of elite mothers was to invest more in their sons (breastfeeding versus wet nursing), but the consequences of their intentions and ensuing actions made boys worse off than girls. That is, mother's intentions but not the consequences of their actions are consistent with the TW hypothesis.

Bereczkei and Dunbar, who reported supportive evidence for the TW effect on SRB in a contemporary low-ranking Hungarian Gypsy population, also found supporting evidence for the TW hypothesis when examining parental investment indicators (breastfeeding, education) (1997). In another study that is based on birth register data of contemporary Venezuela and accounts for more than 500,000 births, the authors find SRB that is small, but consistent with the TW hypothesis (Chacon-Puignau & Jaffe, 1996). Furthermore, moderately supportive evidence comes from a contemporary sample of Polish respondents where the authors use birth spacing and breastfeeding as measures of investment (Koziel & Ulijaszek, 2001).

For the contemporary US, support for the TW hypothesis comes from a representative sample of 900 women with birth spacing, birth weight, and breastfeeding as measures of parental investment and income and presence of a father as indicators of parental status (Gaulin & Robbins, 1991). Using considerably larger samples from two different representative US surveys and using a variety of parental investment indicators, Freese and Powell (Freese & Powell, 1999) find no support for the TW hypothesis. The study is one of the few that examines parental investment in early adolescents (ebd.: 1713). In direct reaction to that article, Kanazawa criticized the choice of parental investment indicators of Freese and Powell. In his own analyses of families with data from the National Survey of Families and Households, he finds confirming evidence for the TW hypothesis (Kanazawa, 2001). However, in a subsequent defense of the first article and a critique of Kanazawa's analysis, the authors make a point in reinforcing their null-findings (Freese & Powell, 2001). Also Keller and others, using data from the Child Supplement to the Panel Study of Income Dynamics, don't find support for the TW hypothesis (Keller et al., 2001).

Hopcroft (2005) criticizes the choice of parental investment indicators in other studies, arguing that in the contemporary US investment in children's education is the most relevant type of investment. Looking at educational attainment of children from high- and low-status parents with data from the General Social Survey, she finds support for the TW hypothesis. Although parental investment in children's education is an important factor in influencing children's later life achievement, the study is problematic on two accounts: first, it is not clear whether parental investment that was used in the ancestral environment (e.g., breastfeeding) or investment that is more relevant in contemporary society (e.g., money for college) should be more salient with regard to a potential TW mechanism. Second, the use of educational achievement, measured as years of education completed, as an indicator of parental investment is highly problematic given that a number of other factors beyond parental investment affect children's educational attainment.

2.3 Social Science Research on Mothers' Breastfeeding Behavior

2.3.1 Introduction

Breastfeeding initiation, continuation, and exclusivity together make up one specific expression of parental investment. Ethnographic and survey research has illustrated empirically that cost-benefit calculations play a predominant role in women's consideration of which infant feeding method to apply (e.g., Hannon, Willis, Bishop-Townsend, Martinez, & Scrimshaw, 2000; Radius & Joffe, 1988). Whether to breastfeed or not to breastfeed can thus be seen as a conscious decision-making process. This process of decision-making may, however, simultaneously be biased by affective or habitual influences, a proposition that seems plausible following a moderate view of modularity of the mind: Evolutionary psychologists have proposed that our mind is made of domain-specific modules that are each adapted to very specific survival tasks (Barkow et al., 1992). The actual degree of the modularity of the mind is still much disputed. Although some evolutionary psychologists defend a version of extreme modularity that sees the number of domain-specific modules ranging between a few hundred and a few thousand (Barkow et al., 1992; Buss, 1995), the emerging consensus in psychology seems to be that a smaller number of such domain-specific mechanisms operates together with a domain-general architecture (cf., Baumeister, 2005).

If we accept even a moderate view of modularity of the mind that states the possible coexistence of domain-specific psychological mechanisms along with domain-general mechanisms (e.g., general intelligence, rational decision making), then a simple test of the Trivers-Willard hypothesis is not possible. Different, possibly conflicting influences on parental investment behavior may exist, and for these influences we need to control in an empirical analysis. These influences may, for instance, spring from genetically determined, emotionally mediated preferences, others from culturally determined constraints on individual decision making. In the past, sociologists often dismissed sociobiological theories without taking into account the full variety of biological predictions. At the same time, researchers with a background in some field of evolutionary biology neglected sociological explanations for differential parental investment. Therefore, I consider it essential to take into account both social and biological influences on parental investment.

2.3.2 Costs and Benefits of Breastfeeding

Breastfeeding has been demonstrated to carry a range of benefits for infants, most importantly a number of short- and long-term health benefits. In the literature on developing countries, reduced risk of morbidity and mortality due to infectious diseases is generally emphasized as the most important benefit of breastfeeding (Horta, Bahl, Martines, & Victora, 2007; WHO Collaborative Study Team on the Role of Breastfeeding on the Prevention of Infant Mortality, 2000). However, specific context matters, illustrated by the fact that due to maternal transmission of the HI Virus infants of HIV positive mothers are at a higher risk of infection when they are breastfed. This is a problem of particular concern in countries with extremely high HIV prevalence rates (Horvath et al., 2009; Dennis, 2002, pp. 13-14).

In the context of developed countries, a range of other health and developmental benefits for infants have been the focus, including long-term effects like a reduced risk of type-2 diabetes and obesity, lower blood pressure and cholesterol levels, as well as improved cognitive development (Cunningham, D. B. Jelliffe, & E. F. P. Jelliffe, 1991; Dennis, 2002; Horta et al., 2007; Heinig & Dewey, 1996; Ip et al., 2007). Health benefits of breastfeeding are not limited to infants, however, but also extend to mothers: For women with a history of lactation, researchers have identified benefits like a reduced risk of certain types of cancer, faster return to pre-pregnancy weight, decrease in postpartum bleeding, and decreased risk of type 2 diabetes (Dennis, 2002, p. 13; Ip et al., 2007; Labbok, 2001).

Whereas the health benefits of breastfeeding seem to be well established and generally accepted, more controversy exists around the question of the optimal duration and dose of breastfeeding to obtain those health benefits (Kramer & Kakuma, 2004; Raisler, Alexander, & O'Campo, 1999; Fewtrell et al., 2007). Until 2001, the general recommendation of the WHO was for mothers in developed countries to breastfeed their newborns 4-6 months exclusively and to introduce complimentary feeding thereafter (Fewtrell et al., 2007, p. 635S). Following the results of an extensive review commissioned by the WHO (Kramer & Kakuma, 2002), this recommendation was corrected upwards to six months of exclusive breastfeeding. The latest status of research on the optimal duration of breastfeeding suggests that prolonged breastfeeding for up to six months is beneficial with regard to certain health conditions, but without effects on others (Kramer et al., 2003; Fewtrell et al., 2007). Determining the optimal duration of exclusive breastfeeding is important because at some point the infant's development reaches a stage when breast milk alone doesn't provide sufficient nutrition necessary at that stage of development (Fewtrell et al., 2007). Exclusive breastfeeding beyond this stage could turn into a disinvestment. Although empirical evidence suggests that there are no deficits for infants breastfed up to 12 months of age (Kramer et al., 2002), prolonged breastfeeding for more than twelve months may lead to malnutrition (Caulfield, Bentley, & Ahmed, 1996).

One important type of cost of breastfeeding are potential health risks for infants of mothers in certain risk groups. Similar to the above-mentioned case of HIV positive mothers, a number of other conditions exist that may strongly contraindicate breastfeeding. In addition to mothers with diseases that can be transmitted through breast milk, mothers exposed to toxic substances that can accumulate in breast milk have to weigh the risks and benefits of feeding possibly contaminated breast milk to their infants. An example are women working in gold mines who are exposed to mercury vapors (Bose-O'Reilly, Lettmeier, Roider, Siebert, & Drasch, 2008; see also Dórea, 2009).

Other types of costs are related to the situation of the mother. Costs may be related to physical or emotional discomfort when breastfeeding or expressing breast milk for bottle feeding (cf. Angeletti, 2009, pp. 227-229). Furthermore, breastfeeding may be perceived as stressful, partly as a direct consequence of the time-related and physical demands of breastfeeding, and partly as a consequence of increased role expectations, especially for mothers who are employed (see Auerbach, 1984). Some women report feelings of inadequacy when they perceive their attempt to breastfeed as unsuccessful (Mozingo, Davis, Droppleman, & Merideth, 2000; cf. Hauck & Reinbold, 1996), that breastfeeding can be painful under certain conditions (e.g., Müller & Silva, 2009, p. 654), or that breastfeeding in public or at the workplace may be perceived as inappropriate by others (Ellis, 1983; Figert, 2000, p. 352). Yet another type of cost can occur when breastfeeding interferes with other preferences of mothers, e.g., the desire or necessity to work. Combining a work schedule with breastfeeding can make achieving both goals more taxing because women have to go to great lengths to find a suitable place or time to breastfeed their child during work or study hours (Figert, 2000, p. 352), they may fear that regular breastfeeding might jeopardize their job and chances on the labor market, a fear that may be especially strong for women in lower status jobs.

2.3.3 Institutional Influences

A number of external sources of influence exist that may affect the subjective cost-benefit ratio of breastfeeding for mothers to varying extent. These external sources, most importantly the public health sector, the formula food industry, and the media, have their own interests that can differ from those of the individual mother, and also compete with each other in their attempts to influence mothers' breastfeeding-related decision-making (see Greer & Apple, 1991).

Given the established health benefits of breastfeeding for mothers and infants, it has been on the agenda of many health organizations to increase the rate of women who breastfeed and to increase the duration and exclusivity of breastfeeding. Examples of these efforts are the promotion of breastfeeding by international organizations like the WHO and UNICEF since 1978 (Dennis, 2002, p. 12) and the Healthy People 2010 Initiative of the Department of Health and Human Services in the US. The latter set the goal to increase the proportion of mothers who initiate breastfeeding right after birth to 75%, of mothers who continue breastfeeding for six months postpartum to 50%, and of mothers who prolong breastfeeding for twelve months postpartum to 25% (Guise et al., 2003, p. 70; Rosenberg, Eastham, Kasehagen, & Sandoval, 2008, p. 290).

A large number of educational programs have been set up to educate mothers about the benefits of breastfeeding and support programs to help mothers reduce some of the costs that breastfeeding can entail (cf. Guise et al., 2003). A probably even larger number of studies has been published that evaluate such programs, and by now several reviews and meta-analyses exist which, in aggregate, evaluate interventions that have been undertaken as part of randomized clinical trials or other clinical studies. These reviews suggest that educational interventions that supply women with information on the benefits of breastfeeding and support interventions that supply women with breastfeeding instruction and support during the period of breastfeeding do have a positive effect on breastfeeding initiation, duration, and exclusivity (Bernard-Bonnin, Stachtchenko, Girard, & Rousseau, 1989; Pérez-Escamilla, Pollitt, Lönnerdal, & Dewey, 1994; Fairbank et al., 2000; Sikorski, Mary J. Renfrew, Pindoria, & A. Wade, 2003; Guise et al., 2003). Most interventions were conducted in a clinical context with mothers. But also in the school context it has been shown that providing adolescents with information on breastfeeding can positively influence their breastfeeding beliefs (Martens, 2001).

A common practice in hospitals, consequence of a longstanding marketing campaign of the baby food industry (Baumslag & Michels, 1995; Greer & Apple, 1991; Merewood & Philipp, 2000), has been to give away free commercial discharge packages of artificial formula or promotional material on artificial formula to new mothers upon leaving the hospital. Given that the financial cost-benefit ratio of breast- versus formula-feeding may affect the decision for or against breastfeeding (cf. Jarosz, 1993), it is not surprising that this practice has been shown to have adverse effects on breastfeeding initiation and duration of exclusive breastfeeding (Bergevin, Dougherty, & Kramer, 1983; Frank, Wirtz, Sorenson, & Heeren, 1987; Pérez-Escamilla et al., 1994; Donnelly, Snowden, M J Renfrew, & M W Woolridge, 2000; Rosenberg et al., 2008), if not always very strong effects (Bliss, Wilkie, Acredolo, Berman, & Tebb, 1997). Furthermore, the distribution of commercial packages of artificial formula in hospitals implies the endorsement of this type of baby food by health professionals (see Rosenberg et al., 2008), thus affecting not only financial considerations but also the beliefs that mothers hold about the relative health benefits of breast milk vs. artificial formula.

In addition to the public health sector and the baby food industry, the media constitute a third institutional actor that is a (partial) reflection of the relative power struggle of other actors in the public discourse and exerts its own influence. A content analysis of British newspapers and television programming showed that bottle feeding was shown more often than breast-feeding and that bottle feeding was depicted as associated with higher-status families—even at a time when the health-benefits of breast-feeding had already been well established (Henderson, Kitzinger, & Green, 2000).

2.3.4 Individual Correlates of Mother's Breastfeeding Decisions

The major personal characteristics of mothers that predict breastfeeding behavior are socioeconomic status, ethnicity, maternal age, smoking status, employment, and her personal network (cf., Dennis, 2002).

A common finding is that women of lower socioeconomic status, operationalized by education, occupational status, or income, show lower rates of breastfeeding initiation, duration, and exclusivity (Dennis, 2002, p. 15; Wright, Parkinson, & Drewett, 2004). One reason is that women of lower socioeconomic status need to return to their jobs more quickly than women of higher socioeconomic status (Angeletti, 2009, pp. 226-227). Furthermore, a regular breastfeeding routine may be difficult to reconcile with the demands at work, especially for women in low-status jobs that don't provide the same flexibility as some semi-professional or professional occupations do (Galtry, 1997). Yet another reason may be education itself: mothers with less information may also know less about breastfeeding and be less able to differentiate the truth-value of various informational offerings from external actors (cf. Bergevin et al., 1983).

Even within a given socioeconomic group, the specific arrangements of the work contract and the workplace may have a huge influence on enabling continued breastfeeding for working mothers: availability of (paid) maternal leave policies, the possibility of temporary part-time work, flexibility of the work routine itself, and provision of a comfortable place for mothers to breastfeed at work (Angeletti, 2009, pp. 226-227; Galtry, 1997, pp. 6-8; Müller & Silva, 2009, pp. 655-656; see also Roe, Whittington, Fein, & Teisl, 1999).

Several authors report that blacks are less likely to breastfeed than whites (Gibson-Davis & Brooks-Gunn, 2007) and that African Americans have the lowest rates of breastfeeding in the US (R. Li, Darling, Maurice, Barker, & Grummer-Strawn, 2005). There is some (mixed) evidence that socioeconomic difference may be the main driving force behind much of the ethnic difference in breastfeeding initiation and duration (Dennis, 2002, p. 15; Gibson-Davis & Brooks-Gunn, 2007). Another possible explanation is cultural: African American women are less likely to breastfeed because during slavery in the US, they were often forced to wet nurse the children of their slaveholders. Breastfeeding may thus carry extremely negative associations for women of families with a history of slavery (see Banton, 2009).

It has furthermore been found that mothers of vulnerable groups show lower rates of breastfeeding initiation, duration, and exclusivity. One such group are young mothers: In a study conducted in New Zealand, the authors found that young mothers below the age of twenty were less likely to breastfeed their child than older mothers (Ford et al., 1994), a finding that has been replicated in several other studies, including a large scale survey of women in the US (Dennis, 2002, p. 15). Another vulnerable group with lower rates of breastfeeding initiation and a shorter average duration of breastfeeding are mothers whose physical or mental health is affected, e.g., those who are suffering from (postpartum) depression (McCarter-Spaulding & Horowitz, 2007; Falceto, Giugliani, & Fernandes, 2004).

To conclude, all correlates of a mother's personal situation and her breastfeeding decision either affect the cost-benefit ratio of breast- versus formula feeding or the beliefs that women hold. They can thus be parsimoniously explained with reference to a rational-choice framework in which women are seen as rational decision makers who try to maximize the net benefits of breastfeeding based on their beliefs about the costs and benefits of breastfeeding.

The personal support networks of a mother can be an important moderator for many of the previously described effects. Members of the personal support network can be useful in providing important information about breastfeeding or in affecting the cost-benefit ratio of breastfeeding. Partners and spouses are probably the most important support persons (like in other important domains, cf. Schnettler, 2008). They can provide emotional support and lessen the burden of breastfeeding through their own supportive involvement in the care for the infant (Müller & Silva, 2009, p. 654; Gibson-Davis & Brooks-Gunn, 2007). The perceived paternal support of breastfeeding can thus be an important factor in predicting the likelihood of breastfeeding (cf. J. A. Scott, Landers, Hughes, & Binns, 2001). In addition to a partner or spouse, the broader family or friend network can be helpful in lessening the burden of breastfeeding (Müller & Silva, 2009, pp. 654-655).

2.3.5 Between-Family vs. Within-Family Differences

In the vast majority of the literature on breastfeeding, it is assumed that the benefits of the child are part of the preference set of the mother, and that this is the same for all children. This is in stark contrast to both sociobiological and economic research that explicitly states that parents may have differential preferences for children with differences in initial endowment (Becker & Tomes, 1976; Salmon, 2005, pp. 508-511). Both economic and sociobiological arguments can be used to predict that child endowment and gender can play an important role in mothers' inclination to breastfeed a particular child and that the direction of this effect may vary with mother's status.

Few empirical articles directly address the TW effect of differential breastfeeding. Where such differences are addressed, often between-family data are used that may be biased by unobserved between-family differences. There is some research, however, on main effects of child characteristics where an interaction between parental status and child characteristics is not explicitly taken into account. It has been found, for instance, that children born with low birth weight have a lower probability for being breastfed (Hill, Ledbetter, & Kavanaugh, 1997). At first glance, this seems to be a confirmation for a potential endowment effect in parental investment. But we cannot say whether mothers reduce their investment of children with low birth weight because they fear a lower return on reproductive or economic investment, they are just more careful with children of low birth weight, or they are prevented from breastfeeding because the newborn needs to stay in an incubator (cf. Hill et al., 1997). Furthermore it is also difficult to attribute causality to birth weight, because birth weight may be affected by the same factors as breastfeeding inclination: One could imagine a case of a mother who gave birth to an unwanted child. Negative affect towards the child could possibly drive the mother to be both more negligent to her fetus during pregnancy, leading to a higher risk of low-birth weight, and later on to choose the easier feeding method for her infant.

A considerable amount of research has been conducted on the main effects of child gender on food allocation by parents, especially with regard to developing countries. Some studies provide evidence for preferential breastfeeding of boys in certain regions in India and China (Singh, Kumar, & Rana, 1992; Ren, 1995), but a review of more than 300 studies that uses sex differentials in nutritional status to draw conclusions about sex differentials in food allocation doesn't find a clear pattern for female disadvantage in developing countries. There is even evidence to the contrary: only some of the reviewed studies describe a female disadvantage in nutritional status and more studies describe male disadvantage (Marcoux, 2002). Given the evidence of another study it seems that some of the differences may be explained by birth order differences (see Mishra, Roy, & Retherford, 2004). Status differentials in sex-based discrimination are not taken into account in these studies and thus the TW effect is not directly tested.

Evidence which is directly relevant to the TW hypothesis can be found in another review on sex-differentials in food allocation (see Haddad, Peña, Nishida, Quisumbing, & Slack, 1996). In this review, the authors conclude that a slight pro-male bias can be found that is not consistent over all regions. Furthermore, they find that male bias is reduced in households with higher incomes, the opposite of what would be predicted by the TW hypothesis. Positive evidence[2] for the TW hypothesis has been found in a historical population in northern England between 1600-1800 (S. Scott & Duncan, 1999). For the contemporary US, evidence on the existence of a potential TW effect is mixed: Keller et al. (2001) find neither evidence for the TW effect with regard to breastfeeding initiation nor with regard to breastfeeding duration. Gaulin and Roberts (1991), however, find a statistically significant effect based on an income-by-gender interaction. In both studies, data on two children per family were available, but in Keller et al. (2001) there is no indication that the data were used for conducting a within-family analysis. And in the study of Gaulin and Roberts (1991), only the oldest child was used to study the TW effect with regard to breastfeeding. Furthermore, their study lacks important controls

2 However, interpretation of the results is complicated by the occurrence of wet nursing. Elite mothers nursed their sons themselves but had their daughters wet nursed. Whereas the intention in this case was probably for these mothers to invest higher in their sons, the consequence were unintended health benefits for the daughters due to differences in nursing duration. These differences in nursing duration can be explained by the fertility aspirations of elite mothers. They weaned their sons earlier in order to have another child. The same restriction did not apply to daughters who were wet nursed (see S. Scott & Duncan, 1999)

of important social and biological factors that may confound the status-by-gender interaction. Something similar is true for the historical study in Northern England, because the data available for such historical family reconstitutions usually limit the number of possible controls considerably.

In addition to differential treatment that may spring from conscious or unconscious mechanisms, differential treatment may also be a consequence of changes in position over the life course. This is the case when these changes affect the cost-benefit ratio of or the beliefs about breastfeeding. The influence of education can serve as an example: higher education likely leads to higher rates of breastfeeding, if we can expect that higher education also leads to better information about the benefits of breastfeeding. This effect has been confirmed in several studies (Sharps, El-Mohandes, Nabil El-Khorazaty, Kiely, & Walker, 2003). Accordingly, when a mother gives birth to a child at the beginning of her educational career, she may be less likely to breastfeed her child than at a later point in life when she has concluded her education. A similar effect may be true for mother's age at birth. At the same time we can expect young mothers to have less resources in general. Furthermore, external influences can affect mothers' decisions for or against breastfeeding as period effect: Children born before a major public health effort to promote breastfeeding may be less likely to be nursed than children born after this effort. Also, relationship status changes over the life course with the consequence that mothers have more or less support after childbirth which is associated with higher or lower probabilities to initiate breastfeeding. To summarize, predictions for between-family differences can also become within-family differences through the differential timing of births over the life course. However, the sex of the child at birth can be seen as a random event that is outside the decision of parents. Thus, these timing differences should affect boys and girls about equally. Although it is true that there is also evidence for sex-ratio biasing as predicted by TW, there is no prediction about an additional interaction with the parity of a child. That is, even though poor parents may have more girls than boys, whether a child of one sex is born at a particular birth order position is still a random event.

3 Data and Modeling Strategy

3.1 Data

Data used for the following analysis were drawn from the National Longitudinal Study of Adolescent Health (Add Health). The Add Health study has a complex, clustered sampling design: "A sample of 80 high schools and 52 middle schools from the US was selected with unequal probability of selection.

Incorporating systematic sampling methods and implicit stratification into the Add Health study design ensured this sample is representative of US schools with respect to region of country, urbanicity, school size, school type, and ethnicity" (Harris et al., n.d.). In schools that participated in the survey, an in-school questionnaire was administered in which ultimately more than 90,000 students who were enrolled in grades 7 through 12 in the 1994-1995 school year were interviewed. Of those students that participated in the in-school questionnaire plus those students that could be identified using school rosters provided by the participating schools, a random sample was drawn for in-home interviews. This sample was stratified by school grade and sex and yielded a total core sample of 12,105 adolescents. The Add Health cohort has been followed over time with data collected in 1994-1995 for Wave 1, 1996 for Wave 2, 2001-2002 for Wave 3, and 2007-2008 for Wave 4.

The core sample was amended by a number of special samples of individuals who are underrepresented in the general population but of special theoretical interest. In the subsequent analyses, I use one of these special samples: the genetic sample of twins and siblings living in the same household. For this sample, participants were drawn from the core sample. For each selected child, siblings living in the same household were also interviewed if they had not already been part of the core sample themselves. The genetic sample of sibling pairs comprises over 3000 sibling pairs with different degrees of genetic relatedness: monozygotic twins, dizygotic twins, full siblings, half siblings, and unrelated siblings raised in the same household (Bearman & Brückner, 2002, pp. 1190-1192). See Table 2 for a summary of the respective case numbers for each type of sibling. In contrast to full siblings which occurred naturally in sufficient numbers in the core sample, all other types of siblings had to be oversampled to obtain sufficient case numbers (Harris, Halpern, Smolen, & Haberstick, 2006). The complex sampling procedure of the core probability sample requires that cases are weighted in any analysis in order to provide estimates that are representative for adolescents in the U.S. (see Chantala & Tabor, 1999). However, for those individuals in the genetic sample that were not part of the core probability sample, no such weights are available. Depending on the type of sibling pair, between 18% and 77% of pairs exist who do not have weight information for both siblings (see right column in Table 2, see also Chantala (2001). The sample is thus neither representative of the population nor do weights exist to correct estimates accordingly. I will thus provide a description of the sample used for all dependent and independent variables. See Table 3 for an overview of the number of cases and response rates in the genetic sample.

There are a number of advantages to the genetic sample with respect to testing the TW hypothesis: First, the sample includes at least two children of the same family. Through the use of fixed effects models it is thus possible to study the TW effect as it plays out within the same families. Second, child respondents are close in age, thus making alternative explanations for differential parental investment based on different positioning of these children in their life courses unlikely. Third, except in one case (breastfeeding), data are based on child responses, thus eliminating social desirability biases that may exist in studies based on parental responses.

In sum, the suggested research strategy improves previous research in three ways. First, it includes a larger variety of potential biological and social influences that may themselves lead to differential parental investment, thus also allowing for a comparison of the strength of different biological and social influences (including the TW effect). Second, the analysis explicitly focuses on within-family comparisons through the inclusion of family fixed effects. Third, the genetic sample of the Add Health study includes data on a variety of sibling ties, thereby allowing to compare the effects of twin status and biological parenthood.

For the analysis in this paper, the sample was further restricted to include biological mother-child ties only. This seems to be a sensible restriction, given that today the prevalence of wet nursing seems to be negligible, given its status as a socially unacceptable practice in contemporary Western developed countries like the US or the UK (see Groskop, 2007). Since the invention of artificial formula it is also less of a necessity. Furthermore, given women's increasing labor market integration and the increasing marginal wages for women, wet nursing would be too expensive. In the Add Health study, information on breastfeeding of biological mothers is limited to those respondents whose biological mother lived in the household and a number of additional cases in which the biological mother did not live in the household but where the respondent caretaker was still able to provide information on breastfeeding duration. However, a number of important control variables would not be available if we included the latter cases. They were thus deleted which left 4502 cases of the original genetic sample. The relative high percentage of excluded cases of about 17% (926 cases), that is, cases where the biological mother did not reside in the same household as the study child, resulted from the oversampling of cases of social parenthood. The subsample used in this chapter is the genetic sample restricted to co-residing biological mothers. The effective subsample was even smaller, because many covariates are only available when either the biological mother or her current partner / spouse were the respondent caretaker.

3.2 Introduction to the Empirical Analyses

In the following analyses, the particular focus lies on testing the Trivers-Willard hypothesis, that is, the interaction between parental status and gender of the child. Previous empirical research has led to highly conflicting results, one reason being the variety of parental investment and resource indicators that were used. Another important reason is that previous research has neglected that the TW effect should first and foremost be a within-family effect. The between-family analysis compares parental investment between families that have one child only, only children of one gender, and children of mixed gender. While it could be that parents of one socioeconomic class systematically invest more in children of a specific gender, even if they only have children of the same gender, such a strategy is maladaptive on evolutionary grounds. If such parents held off investing in their children in anticipation of the potential birth of a child with the other sex, but this anticipated birth never happened, they would risk lowering their inclusive fitness. In families that have children of mixed gender, however, resource allocation biasing and favoritism are more likely. Therefore, I expect that if something like a TW mechanism exists, the consequences should be more visible when parental investment is compared within families with children of mixed gender. This is what the fixed effects models contribute.

An added advantage of the fixed-effects models is that unobserved heterogeneity on the family level is controlled for. This is because all effects that are constant for siblings in the same family get eliminated. This means, these effects are controlled for but they cannot be estimated. Unobserved differences between families could affect the interaction effect between child gender and parental status in such a way that it produces a spurious TW effect or suppresses such an effect. Unobserved differences for effects that differentially apply to children in the same families are not controlled for. Therefore, it is important to identify and control for possible effects on parental investment as best as possible. In the subsequent analyses I will therefore control for both biological and social influences that were identified as relevant in the preceding chapter.

3.3 Dependent Variables

With data from the Add Health study, I could model two of three measures of breastfeeding investment: breastfeeding initiation and breastfeeding continuation. Both were based on information from the parental questionnaire. Breastfeeding exclusivity couldn't be tested because it was not assessed in the Add Health questionnaires. In the following analyses, I used a binary variable to indicate whether the child had been breastfeed (y=1) or not (y=0) as the first dependent variable to capture breastfeeding initiation. In the second part of the analyses, the dependent variable was a binary variable measuring breastfeeding continuation of six months or longer (breastfeeding for less than six month: y=0, breastfeeding for six months or longer: y=1).

3.4 Explanatory Variables

In the selection of explanatory variables I chose those factors that were established as influential on breastfeeding initiation in the respective literature on breastfeeding and that could be operationalized using information form the Add Health survey. In addition I included a number of further explanatory variables and interaction terms that are necessary to test the TW effect.

3.4.1 Mother's Education and Poverty as Status Indicators

The positive association between status and the probability of breastfeeding initiation pointed out in previous research suggested the inclusion of status variables as main effects. Furthermore, according to the TW hypothesis, parental socioeconomic status predicts gender-differential parental investment. It was thus also necessary to include interaction terms between status variables and the gender of the respective study-child. Because complete educational and income histories weren't available and the socioeconomic status at the date of childbirth was thus not known, I used the mother's highest educational achievement and poverty status at the date of the interview as proxies. Although data on parental income at the date of interview were available, I did not use them because the number of missing cases was extremely high for these variables. In order to test for possible nonlinear effects of education on breastfeeding initiation, and especially for the interaction between education and sex, I included dummy variables to indicate four different educational classes: less than high school, high-school degree, more than high school (i.e., high-school degree plus training or some college), and college degree or more, whereby the last group served as comparison in all models of this section. Poverty status was household-based and operationalized as a positive answer to at least one of a few poverty-related questions by either the respondent caretaker in the parental questionnaire or the study child in the in-home questionnaire of Wave 1. The respondent caretakers were asked if they or any member of the household received aid to families with dependent children, food stamps, and or public assistance such as welfare. Children were asked if their residential father and residential mother received public assistance such as welfare.

3.4.2 Mother's Relationship Status as Proxy for Support

Availability of social support has been shown to affect breastfeeding initiation and continuation rates positively. There was no data on complete support networks of biological mothers. But I had two variables that helped to capture the relationship status of the biological mother and thereby one of the most important forms of support. The first was dummy coded and indicated whether the mother had a spouse or partner at birth of the respective study child. From this variable, it couldn't be clearly identified whether the partner of the mother at childbirth was the biological father of the child—but given a sample maximum of six marriages or marriage-like relationships until the date of the interview, one can conclude that the probability of this being otherwise is relatively low. The second variable was also dummy coded and indicated if the biological father of the child lived in the same household as the biological mother at the date of the interview. Residential status of the biological father at the date of the interview cannot causally explain an event in the past, but it may still serve as a proxy for the support at childbirth because highly supportive relationships may also last longer. Partnership status has previously been shown to be important in explaining breastfeeding initiation, but it should be kept in mind that a better approximation would be to directly measure the degree of actual support from a partner prior to and during the decision process for or against breastfeeding. Unfortunately such detailed information was not available for the given subsample.

3.4.3 Ethnicity

Ethnicity was primarily based on the response of the respondent caretaker. When the biological mother herself was the respondent I used her own response on her ethnicity. When the partner was the respondent caretaker I used his answer to indicate the ethnicity of the biological mother of the study-child. When no ethnicity-information was available from either type of respondent-caretaker, I used child-responses on mother's ethnicity. Much of the literature points out that the strongest effect of ethnicity on breastfeeding behavior seems to be whether the mother is African American or not. Thus a series of logistic regression models that are not reported in the main analysis section was conducted to determined the best combination of ethnicity variables. I found that one dummy variable for ethnicity suffices. It indicates whether the mother is identified as African American or not. All other ethnicities had very similar estimated probabilities for breastfeeding initiation, a results based on Wald test that constrains all these effects to be equal.

3.4.4 Sibship Structure

The sibship structure was captured by a combination of variables to control for family size, birth order effects, and existence of opposite sex siblings: (1) As argued in the literature on birth order effects on intellectual development, increasing family size can lead to a dilution of resources (Blake, 1981; Downey, 1995, 2001). In this case, it may mean that a mother of many children might not have the motivation to go through the same efforts and duration of breastfeeding for each of her children than a mother of fewer children. In order to capture a possible effect of family size on the willingness to initiate and continue breastfeeding, I included a continuous variable indicating the number of biological siblings co-residing in the same household at the date of the Wave-1 interview. (2) In accordance with the theory of birth order effects on intellectual development (Cicirelli, 1978; Sulloway, 2007), increasing birth order may go along with a lower probability to be breastfed, breastfed for an extended period, and to be breastfed exclusively. I thus included a number of variables to control for possible birth order effects: a continuous variable measuring the birth order of the study-child among his or her biological siblings, a dummy variable indicating whether the respective child was first-born or not to test for possible non-linear effects of birth order (cf. Härkönen, 2009), and a quadratic term to test for a possible decrease of the birth order effect with increasing parity. As with family size, this variable was based on information about full biological siblings only. It excluded half-siblings who shared the same biological mother but not the same father. (3) In order to proxy the existence of an opposite-sex child, I used a variable that indicated whether an opposite-sex, biological sibling lived in the same household as the study-child at the date of the interview. It is necessary to test for the possibility that the TW effect was only activated in the presence of opposite sex siblings within the same family. The presence of an opposite-sex, biological sibling in the household at the date of the interview could only be an approximation, because it excluded those siblings that moved out of the parental household. Unfortunately, full information of the gender of all biological siblings was not available and could only be derived from the household roster.

3.4.5 Further Sibling-Variant Covariates

Mother's age at birth was included as an additional control variable. Previous research suggests that young mothers, either teenage mothers or mothers in their early twenties, are less likely to initiate breastfeeding. Thus I included three variables related to mother's age at birth: a continuous variable indicating mother's age in years, a dummy variable indicating whether the mother was younger than 18 years old at childbirth to indicate a teen birth, and a quadratic term for mother's age, testing for a possible diminishing strength of the age effect.

To test for possible endowment effects, I used two dummy variables to proxy the child's condition. One indicated whether the child was born with low birth weight (<2500 grams) and the other one whether the child was born with extremely low birth weight (<1500 grams). A number of other variables were relevant as endowment indicators (e.g. child health, personality, etc.). But because they were measured after the infant feeding period was over, they could be highly endogenous and be a consequence of differential breastfeeding or other forms of differential parental investment. Even birth weight and breastfeeding could possibly be endogenous, e.g., when positive or negative affect towards a particular child affected both fetus treatment during pregnancy and willingness to breastfeed the child. But birth weight remains one of the earliest indicators available, and in comparison to other endogenous variables, the time period of exposure to perinatal treatment differences is low.

3.4.6 Variables not Included

Other variables that were deemed important in the literature could not be captured with the Add Health study. Smoking status, for instance, has been found to correlate negatively with breastfeeding initiation and duration, but mother's smoking status at date of birth of her child was not available. Also, smoking status at date of birth is highly endogenous because it could by itself be interpreted as an indicator of parental (dis-)investment. Furthermore, the Add Health study did not provide detailed histories of employment, education, and poverty status. Thus, status variables at date of birth were not available and needed to be replaced with variables providing the status at the date of the interview. Furthermore, there was no information about the employment status of mothers before and after child birth available. Ideally we would want to know if mothers worked before and after child birth and how much this depended on economic necessity.

3.4.7 Analysis

To test which of the control variables were associated with breastfeeding, I ran a series of logistic regression models with breastfeeding initiation or continuation as binary, dependent variables. In an approach common for other types of grouped data (e.g., panel data or case-control studies), I compared the results of a series of pooled logistic regression analyses with the results from a similar series of logistic regression models with fixed effects for the grouping variable (in this case: family) in order to compare between-family with within-family results.

Pooled logistic regression: $$logit(y)_i = \alpha + \boldsymbol{\beta} \boldsymbol{X_i} + \epsilon_i \tag{3.1}$$

Fixed-effects logistic regression: $$logit(y)_{if} = \alpha + \boldsymbol{\beta} \boldsymbol{X_{if}} + \boldsymbol{\gamma} \boldsymbol{Z_f} + \mu_f + \epsilon_{if} \tag{3.2}$$

Index i indicates the individual child and index f stands for the individual family. In 3.1, **X** includes all covariates as they apply to child I. ε_i captures the child-specific error term. In 3.2, $\mathbf{X}_{if}$ includes all covariates that are variable within families and $\mathbf{Z}_f$ includes all those covariates that differ between families but are fixed within families (sibling-invariant). In the fixed effects transformation for these models, the **Z** term cancels out, thus these sibling-invariant effects are controlled for but cannot be estimated.

In order to find the model that best described the data, I used a multi-stage inclusion process. Following guidance from Hosmer and Lemeshow (2000, p. 95), I started with a series of univariable logistic regressions, one for each variable of interest. I kept only those variables that were significant on the .25 level and those that were of particular theoretical importance. In the next steps, I included multiple control variables simultaneously: once sibling-invariant effects only, once sibling-variant effects only, and once sibling variant and invariant effects at the same time. In subsequent steps, I included interaction terms as well as quadratic effects and took out certain variables that didn't prove useful. In order to determine whether an effect was useful for a given model, I compared full and restricted models using likelihood ratio and Wald tests.

4 Results

Tables 4-6 provide some summary statistics for the dependent and independent variables used in subsequent regression analyses, in each case for the complete subsample and for twins and singletons separately. We see in Table 4 that twins were less likely to having been breastfed than singletons, a finding that is in agreement with other research on breastfeeding initiation and duration: The percentage difference of children breastfed between twins and singletons was about 9 percentage points for breastfeeding initiation and about 11 percentage points for breastfeeding continuation for six months or longer.

There are some apparent differences between singletons and twins with regard to status variables and low birth weight: Mothers of twins in the subsample seemed to be of higher status on average[3], measured by education and poverty status (see Table 6). Furthermore, twins were about four times as likely as singletons to having been born with low birth weight (44.7% vs. 10.8%) and extremely low birth weight (9.4% vs. 2.4%), maybe a consequence of twins sharing the same resources during the gestational period.

The percentage of missing cases varied from almost 0 to close to 20% over the dependent and independent variables (see Tables 4-6). Because many of the missing cases occurred in the same persons, the overall number of missing cases when all variables were considered simultaneously was 893 out of a total of 4504 cases (≈19.8%).

4.1 Between-Family Variation in Breastfeeding Initiation

The results of the between-family analysis of breastfeeding analysis are presented in Tables 7 and 9. In Table 7, the results refer to the complete sample, and in Table 9, selected regression results are compared for all cases versus singletons. In Table 7, one sees the results of a series of logistic regression models proceeding from a number of univariable analyses to a number of multivariable models that test for the inclusion of a combination of variables determined to be important earlier in this chapter (Models 1-5). In the case of pooled regressions over families, the usual independence of observations criterion of maximum likelihood theory is violated. I accounted for the statistical dependence of cases from the same families by using sandwich estimators to obtain robust variance standard errors (Hilbe, 2009, pp. 136-139). Model 1 introduces all sibling-invariant and -variant covariates simultaneously. In a step not reported here, sibling-variant and sibling-invariant covariates were included separately - but the two respective models didn't differ in a statistically meaningful way. Model 2 is the result of a series of likelihood ratio and Wald tests to determine which of the main effects did not contribute to the model in a statistically significant way and whether any of the quadratic terms for mother's age or birth order of the child should be included. Models 3 and 4 build on this model and include separately the status-by-gender interactions needed to test the TW hypothesis. Finally, Model 5 includes both status-by-gender interactions simultaneously.

3 A possible explanation for this may be given by the higher median and mean age of twin mothers at birth as compared to mothers of singletons. It may be that higher educated women postpone their childbirth longer. Delayed childbirth increases the risk for infertility, accordingly more women in this group may have tried fertility treatment which in turn increases the chance of a twin birth.

Many of the effects that have been pointed out as important in the research literature on breastfeeding can also be confirmed here: socioeconomic status was positively associated with breastfeeding initiation, both when measured by education and poverty status. African American mothers had lower odds to initiate breastfeeding than mothers with a different ethnic background. The respective effects became somewhat smaller when proceeding from the univariable analysis to Model 1, but remained relatively stable in subsequent models. It is noticeable that the main effect of poverty status were not significant anymore once the poverty-by-gender interaction was also included in the model (see Models 4 and 5). Earlier I introduced a second poverty variable to indicate near-poverty status. This variable did not contribute significantly to any of the models and was excluded from the analysis based on several goodness-of-fit criteria.

The availability of social support has been named an important factor in determining whether a mother chooses to breastfeed or not. This was also confirmed in this analysis with regard to a partner or spouse: mothers who were in a relationships at childbirth had, ceteris paribus, significantly higher odds for breastfeeding initiation than mothers who were not in a relationship. In the univariate model, the respective odds ratio was about 2.51 (~ $e^{.919}$; CI: 2.09-3.01). When other variables were controlled for, the odds ratio ranged between 1.25 (~$e^{.225}$; CI: 1.01-1.56) and 1.36 (~$e^{.310}$; CI: 1.07-1.74). However, whether the biological father lived in the household during the date of the interview was only statistically significant in the univariable analysis and became unimportant once mother's relationship status at childbirth and other factors were controlled for.

Furthermore, there was also a positive association between mother's age and breastfeeding initiation: A one-year increase in mother's age boosted the odds of breastfeeding initiation by a factor of 1.05 (~$e^{.046}$; CI: 1.03-1.06). And having experienced a teen birth decreased the odds by a factor of .38 (~$e^{-.976}$; CI: .26-.54) in the univariable analysis. The same trends appeared once other variables were controlled for, but when a quadratic effect of mother's age was included in the model, the effect of teen birth became statistically insignificant and was thus not included in Model 2. This step found justification when conducting a likelihood ratio test of a model with and without the teen birth dummy variable. To conclude, mother's age alone seemed to capture the effect of teenage birth quite well if a quadratic effect allowed for a diminishing effect with increases in mother's age. In Models 2 – 5, which excluded the dummy variable for teen birth status, the main effect of mother's age was accordingly higher than in Model 1: Whereas the respective odds ratio was estimated at 1.03 (~$e^{.033}$; CI: 1.02-1.05) in the latter model, it was estimated at a value of about 1.31 (~$e^{.27}$; CI: 1.17-1.46) in the former models.

This is because now mother's age needed to capture the effect of teen birth. But the quadratic effect indicates that with each year of mother's age, the odds were reduced by a factor of .996 (~$e^{.004}$; CI: .994-.998). This corresponds to a .4 percent decrease of the odds. Combining the main effect with a quadratic effect of mother's age instead of a dummy variable for teen-birth status was also better justifiable on substantive grounds: it seemed more likely that, whatever the effect was that contributed to the lower probability of underage mothers to have lower rates of breastfeeding, didn't suddenly reverse at age 18 but rather diminished gradually.

Furthermore, there was a clear endowment effect of birth weight on breastfeeding initiation. Only in the univariate analysis did both low and extremely low birth status significantly reduce the odds of breastfeeding initiation. The respective estimated odds ratios were .51 ($e^{-.672}$; CI: .43-.60) for low birth weight statsu and .41 ($e^{-.898}$; CI: .28-.58) for extremely low birth weight status. Once both birth weight indicators were included in a model simultaneously, only the effect of low birth weight remained statistically significant. Because of the reduced effect size and the lack of statistical significance of the dummy variable for extremely low birth weight, and the results of a model comparison via a likelihood ratio test, I decided to remove this dummy variable in subsequent models.

Both the continuous variable indicating birth order of the child and the dummy variable for firstborn status had relatively small effects and were not statistically significant in the univariable analysis. However, once both were controlled simultaneously and along with a number of other covariates in Model 1, the continuous variable for birth order had an increased negative effect on breastfeeding initiation that was statistically significant: With each increase in birth order position, the odds for breastfeeding initiation decreased by a factor of .77 ($e^{-.259}$; CI: .69-.87), that is, by about 23%. First-born status, on the other hand, didn't contribute significantly to the variance explained and showed only a small effect with a corresponding odds ratio of 1.09 ($e^{.089}$; CI: .89-1.35). A likelihood ratio test comparing a model that included the respective dummy variable with a model that did not, confirmed that first-born status didn't significantly contribute to the model. It was therefore left out of subsequent models. As a consequence of leaving out first-born status, there was a small change in the effect of birth order in Models 2-5 when compared to Model 1.

Family size had a stable, positive effect on breastfeeding initiation in all multivariable models: each additional sibling increased the odds of breastfeeding initiation by a factor of 1.27 (~$e^{.238}$; CI: 1.18-1.37). This result runs counter to the prediction of the family dilution hypothesis about sibship structure and intellectual development. It seems plausible that the dilution argument does not apply to breastfeeding, because this is a form of investment that is provided sequentially rather than simultaneously. An explanation for the positive effect of family size on breastfeeding initiation may be a shared, unobserved cause, e.g., a high nurturing desire that is associated with a desire for a larger family and a higher willingness to invest in each child. In the absence of a direct measurement of these desires one cannot further interpret the cause of the family size effect.

Twin status significantly decreased the odds of being breastfed by a factor of approximately .66 in all models ($e^{-.42}$; CI: .55-.79). This confirmed the descriptive finding reported earlier that twins were less likely to be breastfed when compared to singletons.

4.2 Test of the Trivers-Willard Hypothesis Between Families

The previous variables served as control variables. In the main part of the between-family analysis, I tested the Trivers Willard hypothesis by using both a test of the status-by-gender interaction and a control of whether the presence of an opposite sibling made a difference. The main effect of gender was relatively small and not statistically significant. Model 3 included interaction terms between the dummy variables for the educational status of the mother and the gender of the child. To evaluate the TW hypothesis it was necessary to calculate odds ratios for breastfeeding initiation that compare daughters versus boys in each educational group. As a first step to obtain these odds ratios, I summed up the log odds estimates of the main gender and education effects as well as the gender-by-education interaction for boys and girls in each category of mother's education. As a second step, I transformed the summed log odds into the respective odds. As a last step I then divided the odds for breastfeeding initiation of daughters by the respective odds of the sons. This yielded one odds ratio for each educational group that can be interpreted as the odds of having been breastfed for daughters as compared to the odds of sons. Table 8 illustrates this on the basis of the results of Model 3 in Table 7. Daughters in the lowest educational category had lower odds of breastfeeding initiation than sons, but higher odds if the mother had at least a high school degree (odds ratios: .801, 1.158, 1.145, 1.185). The direction of effects speaks against the TW hypothesis which would predict that daughters should be favored over boys in the lowest educational group and boys over daughters in the upper educational groups. But as we can see in Table 8, the respective confidence intervals of the odds ratios are extremely large. They all include the value 1 which indicates that the estimates were not statistically significant on the .05 level. The same can be seen in Table 7: here none of the gender-by-education interaction terms was statistically significant.

A similar result was obtained in Model 4 which included a poverty-by-gender interaction instead of the education-by-gender interaction. This interaction effect was negative and thus counter to the TW prediction: The odds of having been breastfed were .94 (~ $e^{-.226 + .160}$; CI: .31-2.81) times lower for daughters than for sons in poor families. For children in families that were not poor the relationship was the opposite. Here the odds of girls were 1.13 (~$e^{.119}$; CI: .96-1.32) times larger than those for boys. But like in Model 3, the status-by-gender interaction was not statistically significant.

I further tested a number of models with three-way interactions, including the status-by-gender interaction and either twin status or the dummy indicating whether a child of the opposite gender lived in the household. Maybe the mechanism leading to the TW effect is only activated when a child of the opposite gender is present. It may also be that the TW effect has different effect sizes in twin versus singleton comparisons due to the higher similarity of twins. But neither of these three-way interactions contributed significantly to any of Models 3-5 on the basis of likelihood ratio tests. Thus the respective regression results are not included here. Judging from the goodness-of-fit statistics provided in Table 7, Model 3 seems to be the best choice. Although the rounded pseudo-R^2 measure are the same in both models, Model 3 has a lower AIC value than Model 4. Also in comparison with Model 5 it fares better: It is more parsimonious and has a lower AIC value.

Finally, Table 9 provides a comparison of two models for the full sample and the sample without twins to see how the inclusion or exclusion of twins affected the results. There were hardly any major changes with regard to the main effects between the full-sample models and the sample without twins, except that some effects were slightly more pronounced when twins were excluded. For instance, take a look at the effect of low birth weight on having been breastfed or not: In the full sample, low birth weight status reduced the odds by a factor of .57 (~$e^{-.560}$; CI: .47-.70) whereas it reduced the odds by even a factor of .48 (~$e^{-.742}$; CI: .36-.63) when twins were excluded. The reason might be a slight suppressor effect of unobserved variables that made twins more alike and were associated with low birth weight. When twins were excluded, the partner effect was not statistically significant anymore. But at the same time, the estimated odds had about the same effect size with 1.252 (~$e^{.225}$; CI: 1.005-1.560) and 1.246 (~$e^{.220}$; CI: .973-1.595) respectively.

More interesting with regard to testing the TW hypothesis is a comparison of the interaction effects between Models 1b and 2b. The results from Model 1b speak for the opposite of the TW effect: As we saw above, daughters had lower odds for breastfeeding initiation than sons when mothers were in the lowest educational group. But daughters had higher odds when mothers were in any of the three higher educational groups. For singletons the results were mixed (Model 2b): Here, like in Model 1b, daughters of mothers with the lowest educational degree had the lowest relative odds when compared to sons of mothers in the same educational group. But the group with the highest relative odds of breastfeeding initiation were daughters with a mother who had a high school degree only. Thus the estimated effects lacked a clear pattern for or against the TW effect. Given that the interaction effects were not statistically significant in either of these models, one can conclude that the null-hypothesis of no gender-by-status effect couldn't be rejected with regard to breastfeeding initiation.

4.3 Within-Family Variation in Breastfeeding Initiation

Before doing the fixed-effects analysis, I started with an empty random intercept model with breastfeeding initiation as dependent variable in order to estimate the intraclass correlation. In this case the intraclass correlation can be interpreted as the share of between-family variation of the total variation which is equivalent to the within-family correlation. That is, the higher the variance that is explained by between-family differences, the higher the correlation within families must be. A random-intercept model is given by the following formula:

$$logit(y)_{if} = \alpha + \gamma_f + \epsilon_{ij} \tag{4.1}$$

From the respective variance components of this model, that is, the variance of the family fixed effects γ_f and the individual residuals ε_{if}, one can derive the within-family correlation in breastfeeding initiation ρ. It is the variance of the family fixed-effects divided by the overall variance (see StataCorp, 2007, p. 211):

$$\rho = \frac{\sigma_\gamma^2}{\sigma_\gamma^2 + \sigma_\epsilon^2} \tag{4.2}$$

In this case, the result yielded an intraclass correlation of about 90.3%. That is, the vast majority of variance is explained by between-family differences. It is only the remaining variation on which the fixed-effects estimations were based, that is, only on those cases of children that had different rates of breastfeeding initiation within the same families. Given the extremely high intraclass correlation, very little variation was left for the fixed-effects analysis. This made finding any statistically significant effect very unlikely. For the group of twin siblings, there were fewer than five sibling dyads that differed in breastfeeding initiation status. They were thus excluded from the following analysis. Fixed-effects models that condition out all influences that are fixed for different children in one family (e.g., mother's ethnicity) and estimate the influence of within-family differences on the dependent variable, are most useful and appropriate when there is considerable within-family variation (Hilbe, 2009, p. 485). But I still compared the results of the fixed effects and pooled regression estimates to compare the direction of effects.

In Table 10, the results of the conditional fixed effects logistic regressions[4] on breastfeeding initiation are displayed. In conditional logistic regression, a conditional likelihood function is used, that is, the likelihood is calculated relative to each family. Furthermore, it conditions out the grouping factors from the statistical output. This is in contrast to the unconditional logistic regression where we would obtain an estimate of the intercept for each family. Because fixed-effects models can control for, but not estimate effects that are constant for members in the same family, Models 1 – 4 of the fixed effects analysis only include sibling-variant factors and interaction effects between sibling-invariant and -variant factors. The approach taken in this section was to start with two separate models that introduce the main effect of gender and one of two status-by-gender interactions: first education, then poverty status (Models 1 and 2). In addition to these interaction effects, Models 3 and 4 included both interactions simultaneously and a selection of additional main effects that were selected as relevant in the previous between-family analysis. In Model 3 I added mother's age at birth because this proved to be the only significant main effect in the fixed effects analysis. In addition, Model 4 included all other main effects that were deemed important. Even though none of the main effects remained significant in Model 4, the direction of effects was the same as in the between-family analysis: The odds of a child for being breastfed increased with mother's age, yet only up to a specific age which was indicated by the quadratic effect. Furthermore, if the mother had a partner at childbirth, this doubled the odds of being breastfed when controlling for other factors. Low birth weight, increased birth order, and the presence of a child of opposite gender in the household decreased the odds. With regard to the TW hypothesis, it is more important to examine the interaction effects between status variables and gender of the child.

Similar to the between-family analysis, there was no clear pattern for or against the TW effect in Models 1 (interaction between mother's education and gender) and 2 (interaction between mother's poverty status and gender). The results of Model 1 show that daughters had higher odds of having been breastfed than sons when the mother was in either the lowest or second highest educational group

4 Because the number of cases of the lower-level units (children) is low, the number of upper-level units (families) in the sample high, and the frequency of children within families unbalanced, results of an unconditional fixed effects logistic regression could be biased. I thus followed the recommendation to use conditional logistic fixed effects regression in this case (cf. Hilbe, 2009, p. 482).

($e^{-.29+1.517} \sim 3.41$ and $e^{-.29+.339} \sim 1.40$). But they had lower odds than sons when the mother was in the highest or second lowest educational group ($e^{-.29} \sim .75$ and $e^{-.29-.016} \sim .74$). However, as one sees in Table 10, only the interaction effect between child gender and mother's education was statistically significant. Therefore the TW hypothesis seems to finds some evidence. The inconsistent pattern in the higher educational groups could be chance. It is particularly interesting that the effect was opposite to the one in the between-family analysis where daughters had lower odds than boys in the lowest educational group. This is evidence for some important differences between the between- and within family dynamics. Looking at the results of Model 2 we can see that daughters had slightly higher odds of breastfeeding initiation than boys in households that were poor and those that weren't ($e^{.072+.005} \sim 1.08$ and $e^{.072} \sim 1.07$). The difference between the two odds ratios was too small to draw any conclusion regarding the TW hypothesis.

Model 4 included all status-by-gender interactions simultaneously along with the relevant main effects. It shows that the education-by-gender interaction yielded a clear gradient as predicted by the TW hypothesis: daughters relative odds of having been breastfeed as compared to the odds of sons decreased with mother's increasing education ($e^{1.640-.393} \sim 1.25$, $e^{.532-.393} \sim .14$, $e^{.294-.393} \sim -.10$, $e^{-.393} \sim -.39$). That is, daughters had higher odds of having been breastfed than boys in the two lower educational groups, but had lower odds in the upper two educational groups. Only the interaction between child gender and the lowest educational grouping was marginally significant at the .1 level. Also, the poverty-by-gender interaction had the reverse effect and was thus in opposition to the TW hypothesis: conditional on the other effects, being a daughter in a poor family came with a penalty, that is, their odds of having been breastfed were decreased by a factor of about .57 ($\sim e^{-.557}$). This effect was not statistically significant.

4.4 Regression Results on Breastfeeding Continuation

The higher the cost of a form of investment for parents, the stronger the effect of differential parental investment should be. Thus, in the case of breastfeeding, one can expect that differences may come out more clearly when looking at breastfeeding continuation for six months or longer instead of breastfeeding initiation only. In order to compare the results for breastfeeding initiation with those on breastfeeding continuation, I replicated the models reported in the preceding sections for breastfeeding continuation, using the same set of predictor variables. It can be expected that any effect of differential investment may come out more clearly in the case of breastfeeding continuation. But a difficulty in this analysis is that the case numbers were much lower: as displayed in Table 4, only 18% of the cases in the subsample were breastfed for six months or longer, whereas 44% were breastfed for at least some shorter duration.

A comparison of the pooled logistic regression models for breastfeeding continuation in Table 11 with those on breastfeeding initiation in Table 9 shows that the directions of the effects are the same in both cases—both for sibling-invariant main effects and sibling-variant main effects. An apparent difference is that for breastfeeding *continuation,* only the child's birth weight, birth order, and twin status yielded statistically significant results among the sibling-variant factors. In the analysis of breastfeeding *initiation* this was also true for mother's relationship status, mother's age at birth, and the quadratic effect of mother's age. It is an interesting finding that child endowment characteristics (low birth weight, sibling order) seemed to become more important than other life-course related factors of the mother (age, relationship status). But with regard to the Trivers Willard hypothesis, again the examination of the gender-by-status interactions is more relevant. Similar to the analysis of breastfeeding initiation (Table 9), none of the respective interaction effects was statistically significant. But the direction of the interaction effect between the lowest educational group and child gender was different in Model 2b (see Table 11 and the respective model in Table 9). However, when calculating the conditional odds ratios of girls versus boys for each gender-by-education combination, I obtained a similar pattern as for breastfeeding initiation: the odds ratio of breastfeeding continuation in daughters versus boys was highest when the mother had a high school degree only. A difference to the model for breastfeeding initiation is that in all three of the remaining educational groups, daughters had lower odds for continued breastfeeding than boys. But given that in both cases the values were very close to 1, this was a statistically insignificant difference.

Finally, I replicated the conditional fixed effects analysis from Table 10 for breastfeeding continuation and report the results in Table 12. With regard to the education-by-gender interaction I didn't find a clear pattern, neither in Model 1 (interaction-only model) nor in Model 4 (full model). With regard to poverty, I found an effect consistent with the TW hypothesis. But in contrast to the model for breastfeeding initiation, here neither of the interaction terms was statistically significant.

4.5 Summary and Conclusion

The estimated main effects reported in the analysis were, by and large, in accordance with the research literature on breastfeeding behavior. This speaks for the case that the data set used for the analysis didn't differ from representative samples in the underlying patterns of association. The focus of the analysis was on the estimated status-by-gender interactions. In the between-family comparison, the analysis of interaction effects between education and child gender showed no statistically significant support for the TW hypothesis. The respective patterns of estimated effects were very similar for breastfeeding initiation and breastfeeding continuation. Also with regard to poverty status, no clear pattern could be detected: In the models on breastfeeding initiation, being a daughter rather than a son came with a penalty in poor families, but with a positive effect in the case of breastfeeding continuation.

Another test of the theory was conducted by comparing pooled regression results with the results of a series of conditional fixed effects models. Whereas the former tells us about between-family differences, the latter tells us about within-family differences. In previous tests of the Trivers-Willard hypothesis, researchers have often examined between-family differences exclusively. But as I argued earlier, it is theoretically more sensible to apply the TW hypothesis to within-family differences. With regard to breastfeeding initiation, the within-family analysis yielded a clear gradient in accordance with the Trivers-Willard hypothesis for education but not for poverty status. In neither of the full models was any of the gender-by-status interaction effects statistically significant. Only the interaction effect between the lowest educational group and child gender was marginally significant on the .1 level. With regard to breastfeeding continuation the pattern was less clear again. Here, I didn't find a clear gradient for the education-by-gender interaction and daughters of mothers without a high school degree even had higher odds to of continued breastfeeding than daughters of college educated mothers.

There are many possible ways parents may invest in their children, but breastfeeding seems to be closest to our biological nature. Breastfeeding has been performed by women since our species evolved and has only recently, that is, about 100 years ago, found a possible substitute through artificial formula. It is reasonable to expect that a TW effect would most likely be found with this type of parental investment. Whereas this is indeed what I found, the effect was visible only under very specific conditions: The effect was not statistically significant and limited to education. And for breastfeeding duration the effect was not found. It is not quite clear why this would be the case. Given that breastfeeding continuation is a higher investment than breastfeeding initiation, we would expect a TW effect to be even stronger in the former case. Possibly, mothers who continued breastfeeding for six months or longer had a higher investment tendency along with a stronger norm for investment equity. With the current data we couldn't answer this question, however. Given that the overall degree of within-family variation was low, fixed effects estimation was based on a very small number of cases —and given that even fewer mothers continued breastfeeding for six months or longer, the respective fixed-effects estimation was based on even fewer cases than in the models on breastfeeding initiation. To summarize, there are hints that the TW effect may get activated within families. But given the current, small sample of those cases with within-family variation, this result remains tentative. Some of the contradictory effects may be due to random error. A replication of the fixed-effects analysis with a bigger data set should thus be considered.

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Appendix – Tables

Table 1: Degree of Genetic Relatedness Between Ego and Various Biological Kin

r = 1	r = .5	r = .25	r = .125
Identical twin	Full sibling Parent Child	Half-Sibling Grandparent Grandchild Aunt/uncle Nephew/niece	First cousin

Table 2: Overview of Genetic Sample, by Type of Sibling

Sibling type	*Original dataset*[(A)]	*Own dataset*[(B)]	*Number pairs w/o weight information*	*% pairs w/o weight information*
Monozygotic twin	289	282	56	19,86
Dizygotic twin	452	443	133	30,02
Undetermined twin	43	43	7	16,28
Full sibling	1251	1249	225	18,01
Half sibling	442	424	157	37,03
Unrelated sibling	662	657	506	77,02
Total	3139	3098	1084	34,99

Notes: (A) See Rowe and Jacobson (1998, pp. 4-5); (B) 41 respondents removed due to missing values on a large number of variables relevant to the analyses here.
Data: Add Health Genetic Sample

Table 3: Number of Cases and Response Rates in Add Health Genetic Sample

	N			% of Wave 1	
	W1	W2	W3	W2	W3
Individual siblings	5430	4945	4303	91,1	79,2
Sibling dyads (At least one sibling interviewed)	3098	2937	2775	94,8	89,6
Sibling dyads (Both siblings interviewed)	3098	2696	2127	87,0	68,7

Data: Add Health Genetic Sample

Table 4: Distribution of Dependent Variables, by Twin Status

		Yes	No	Total	Yes	No	Total	Yes	No	Total		All	Tw.	Sng.
Breastfeeding initiation	N	1728	2208	3936	391	669	1060	1337	1539	2876	N	568	139	427
	%	*.44*	*.56*	*1.00*	*.37*	*.63*	*1.00*	*.46*	*.54*	*1.00*				
Breastfeeding continuation	N	708	3228	3936	108	952	1060	600	2276	2876	$\%_{NA}$	*.13*	*.12*	*.13*
	%	*.18*	*.82*	*1.00*	*.10*	*.90*	*1.00*	*.21*	*.79*	*1.00*				

Note: Med. = median, M = arithmetic mean, SD = standard deviation, %NA = % missing cases
Data: Add Health Genetic Sample

Table 5: Summary Statistics for Continuous Predictor Variables, by Twin Status

	Med.	M	SD	$\%_{NA}$	Med.	M	SD	$\%_{NA}$	Med.	M	SD	$\%_{NA}$
Mother's age at birth	24.0	24.9	5.2	17.5	26.0	25.9	5.1	16.7	24.0	24.5	5.2	17.9
Child's birth order	2.0	2.0	1.3	.2	2.0	2.4	1.4	.3	2.0	1.9	1.2	.2
Family size (bio.)	2.0	2.0	1.6	.3	2.0	2.3	1.5	.6	2.0	1.8	1.6	.3

Note: Med. = median, M = arithmetic mean, SD = standard deviation, $\%_{NA}$ = % missing cases
Data: Add Health Genetic Sample

Table 6: Summary Statistics for Categorical Predictor Variables, by Twin Status

	All			Twins			Singletons		
	$\%_{valid}$	$\%_{all}$	$\%_{NA}$	$\%_{valid}$	$\%_{all}$	$\%_{NA}$	$\%_{valid}$	$\%_{all}$	$\%_{NA}$
Mother Black / Af.Am.	20.3	17.1	16.0	23.0	19.5	15.3	19.3	16.2	16.3
Mother's Education									
< high school	18.3	18.1	1.2	16.7	16.5	1.0	18.9	18.7	1.3
only high school	31.9	31.5		29.9	29.6		35.6	32.2	
high school and more	28.0	27.6		28.6	28.3		27.7	27.4	
university	21.9	21.6		24.9	24.7		20.8	20.5	
Household poor	20.1	20.1	.1	16.7	16.6	.5	21.3	21.3	.1
Household near poverty	35.8	35.8	.1	30.7	30.6	.3	37.7	37.6	.1
Father in HH at birth	56.3	56.3	.0	58.7	58.6	.2	55.4	55.4	.1
Mother partnered at birth	81.8	73.2	10.5	83.7	74.0	11.6	81.1	72.9	10.1
Teen birth	4.8	4.0	17.5	3.4	2.8	16.7	5.3	4.4	17.9
Child sex = female	50.0	50.0	.0	46.5	46.5	.2	51.3	51.3	.1
Low birth weight	20.0	17.2	13.8	44.7	39.1	12.6	10.8	9.3	14.3
Extremely low birth weight	4.3	3.7	13.8	9.4	8.2	12.6	2.4	2.0	14.3
Child first-born	40.3	40.3	.2	26.3	26.2	.3	45.4	45.3	.2
Child is a twin	26.6	26.6	.0	100.0	99.9	.2	.0	.0	.1
Opposite sex child in HH	50.7	50.7	.1	53.6	53.5	.3	49.7	49.7	.1

Note: $\%_{valid}$ = % based on valid N only; $\%_{all}$ = % based on all cases; $\%_{NA}$ = % missing cases
Data: Add Health Genetic Sample

Table 7: Pooled Logistic Regressions with Robust Variance Standard Errors; Dependent Variable: Breastfeeding Initiation (y/n)

	Univariable			Model 1			Model 2			Model 3			Model 4			Model 5		
	β	rSE	p	β	rSE	p	β	rSE	p	β	rSE	p	β	rSE	p	β	rSE	p
Intercept				-.080	.271		-3.098	.757	*	-3.162	.759	*	-3.098	.756	*	-3.155	.759	*
Sibling-invariant																		
Father in HH at interview	.537	.066	*	-.113	.092													
Houshold poor, y/n	-.772	.085	*	-.288	.106	*	-.254	.104	*	-.259	.104	*	-.160	.143		-.210	.146	
Mother's education																		
(... vs. university degree)																		
-- no high school degree	-1.179	.107	*	-1.006	.130	*	-.920	.131	*	-.716	.179	*	-.923	.131	*	-.734	.182	*
-- only high school	-1.250	.094	*	-1.135	.108	*	-1.094	.108	*	-1.083	.150	*	-1.095	.108	*	-1.089	.150	*
-- more than high school	-.585	.093	*	-.452	.104	*	-.427	.104	*	-.409	.143	*	-.427	.104	*	-.412	.143	*
Black/Afr.Am., y/n	-1.399	.098	*	-1.249	.108	*	-1.218	.107	*	-1.220	.108	*	-1.218	.107	*	-1.220	.108	*
Number of full siblings	.057	.021	*	.238	.040	*	.238	.038	*	.238	.038	*	.238	.038	*	.238	.038	*
Sibling-variant																		
Mother had partner at birth	.919	.093	*	.310	.123	*	.230	.112	*	.226	.112	*	.226	.112	*	.225	.112	*
Teen birth, y/n	-.976	.180	*	-.499	.216	*												
Mother's age at birth	.046	.007	*	.033	.009	*	.268	.058	*	.270	.057	*	.267	.058	*	.269	.057	*
(Mother's age at birth)^2							-.004	.001	*	-.004	.001	*	-.004	.001	*	-.004	.001	*
Gender of child = female	.036	.064		.092	.073		.088	.073		.170	.160		.119	.080		.176	.161	
Low birth weight (<2500g)	-.672	.086	*	-.493	.111	*	-.560	.102	*	-.561	.102	*	-.559	.102	*	-.560	.102	*
Extremely l.b.w. (<1500g)	-.898	.183	*	-.368	.223	°												
Birth order of child	-.024	.026		-.259	.060	*	-.289	.048	*	-.290	.048	*	-.289	.048	*	-.290	.048	*
Child was first-born	.053	.065		.089	.106													
Child is a twin	-.396	.074	*	-.400	.092	*	-.422	.091	*	-.416	.091	*	-.421	.091	*	-.416	.091	*
Opposite sex child in HH	.108	.064		-.060	.079		-.085	.078		-.083	.078		-.087	.078		-.084	.078	
Interaction effects																		
Education x gender																		
-- less than HS x female										-.392	.242					-.361	.250	
-- high school x female										-.023	.207					-.013	.209	
-- more than HS x female										-.035	.205					-.028	.206	
Household poverty x female													-.185	.194		-.094	.201	
N				3611			3611			3611			3611			3611		
Resid. deviance				4367.2			4361.4			4357.9			4360.5			4357.6		
AIC				4403.2			4391.4			4393.9			4392.5			4395.6		
Adj. McFadden				.19			.19			.19			.19			.19		

Note: rSE = robust standard errors, p = significance levels: () <.1 (*) <.05
Data: Add Health Genetic Sample

Table 8: Example Calculation for Evaluating the TW Hypothesis

		Estimates M3, Table 7			Lower 95% CI			Upper 95% CI		
		♀	♂	♀/♂	♀	♂	♀/♂[1]	♀	♂	♀/♂[1]
<HS	Gender	.170	.000		-.144	.000		.484	.000	
	Education	-.716	-.716		-1.067	-1.067		-.365	-.365	
	Interaction	-.392	.000		-.866	.000		.082	.000	
	LogOdds (Sum)	***-.938***	***-.716***		***-2.077***	***-1.067***		***.201***	***-.365***	
	OddsRatio (Sum)	***.391***	***.489***	***.801***	***.125***	***.344***	***.181***	***1.222***	***.694***	***3.552***
HS	Gender	.170	.000		-.144	.000		.484	.000	
	Education	-1.083	-1.083		-1.377	-1.377		-.789	-.789	
	Interaction	-.023	.000		-.429	.000		.383	.000	
	LogOdds (Sum)	***-.936***	***-1.083***		***-1.949***	***-1.377***		***.077***	***-.789***	
	OddsRatio (Sum)	***.392***	***.339***	***1.158***	***.142***	***.252***	***.313***	***1.080***	***.454***	***4.282***
>HS	Gender	.170	.000		-.144	.000		.484	.000	
	Education	-.409	-.409		-.689	-.689		-.129	-.129	
	Interaction	-.035	.000		-.437	.000		.367	.000	
	LogOdds (Sum)	***-.274***	***-.409***		***-1.270***	***-.689***		***.722***	***-.129***	
	OddsRatio (Sum)	***.760***	***.664***	***1.145***	***.281***	***.502***	***.320***	***2.058***	***.879***	***4.100***
UNI	Gender	.170	.000		-.144	.000		.484	.000	
	Education	.000	.000		.000	.000		.000	.000	
	Interaction	.000	.000		.000	.000		.000	.000	
	LogOdds (Sum)	***.170***	***.000***		***-.144***	***.000***		***.484***	***.000***	
	OddsRatio (Sum)	***1.185***	***1.000***	***1.185***	***.866***	***1.000***	***.866***	***1.622***	***1.000***	***1.622***

Note: This tables lists the log odds estimates from Model 3 in Table 7 by mother's education (<HS = less than a high school degree; HS = high school degree only; >HS = high school degree and advanced training; UNI = college/university education) and child gender (♀ = daughter, ♂ = son, ♀ / ♂ = daughter compared to son). Also listed are the respective lower and upper 95% confidence interval limits that can be calculated based on the log odds estimates and standard errors in Table 7. For each education-by-gender combination, the log odds estimates for the main effects of gender and education as well as for the interaction effect of education and gender are added up. The resulting sum ("LogOdds (Sum)") is then transformed to the respective odds ratio by exponentiation of e to the power of the sum of "LogOdds (Sum)". In the third of the shaded columns we find the odds ratio of breastfeeding for daughters compared to sons for each educational group (.801, 1.158, 1.145, 1.185).

[1] For the confidence interval in the daughter vs. boy comparison, the lower confidence limit results from dividing the lower CI value of the daughter by the upper CI value of the son. And the upper confidence limit results from dividing the upper CI value of the daughter by the lower CI value of the son.

Data: Add Health Genetic Sample

Table 9: Comparison of Pooled Logistic Regression Models (All Cases vs. Twins Excluded); Dependent Variable: Breastfeeding Initiation (y/n)

	All cases						Twins excluded					
	Model 1a			Model 1b			Model 2a			Model 2b		
	β	rSE	p	β	rSE	p	β	rSE	p	β	rSE	p
Intercept	-3.098	.757	*	-3.155	.759	*	-3.317	.853	*	-3.349	.860	*
Sibling-invariant												
Houshold poor, y/n	-.254	.104	*	-.210	.146		-.358	.115	*	-.343	.165	*
Mother's education												
(... vs. university degree)												
-- no high school degree	-.920	.131	*	-.734	.182	*	-.841	.153	*	-.747	.217	*
-- only high school	-1.094	.108	*	-1.089	.150	*	-1.084	.127	*	-1.163	.183	*
-- more than high school	-.427	.104	*	-.412	.143	*	-.340	.126	*	-.347	.180	°
Black/Afr.Am., y/n	-1.218	.107	*	-1.220	.108	*	-1.100	.123	*	-1.099	.123	*
Number of full siblings	.238	.038	*	.238	.038	*	.205	.044	*	.206	.044	*
Sibling-variant												
Mother partnered at birth, y/n	.230	.112	*	.225	.112	*	.226	.126	°	.220	.126	.
Teen birth, y/n												
Mother's age at birth	.268	.058	*	.269	.057	*	.292	.066	*	.296	.066	*
(Mother's age at birth)^2	-.004	.001	*	-.004	.001	*	-.005	.001	*	-.005	.001	*
Gender of child = female	.088	.073		.176	.161		.057	.085		.043	.196	
Low birth weight, y/n	-.560	.102	*	-.560	.102	*	-.736	.145	*	-.742	.145	*
Birth order of child	-.289	.048	*	-.290	.048	*	-.304	.056	*	-.305	.056	*
Child is part of twin dyad, y/n	-.422	.091	*	-.416	.091	*						
Opposite sex child in HH, y/n	-.085	.078		-.084	.078		-.050	.091		-.053	.091	
Interaction effects												
Education x gender												
-- less than HS x female				-.361	.250					-.182	.292	
-- high school x female				-.013	.209					.151	.247	
-- more than HS x female				-.028	.206					.011	.247	
Household poverty x female				-.094	.201					-.034	.225	
N	3611			3611			2643			2643		
Resid. deviance	4361.4			4357.6			3247.16			3245.29		
AIC	4391.4			4395.6			3275.16			3281.29		
Adj. McFadden R^2	.19			.19			.18			.18		

Note: rSE = robust standard errors, p = significance levels: (°) <.1 (*) <.05
Data: Add Health Genetic Sample

Table 10: Conditional Logistic Fixed Effects Regression on Breastfeeding Initiation (y/n) (Twins Excluded)

	Model 1			Model 2			Model 3			Model 4		
	β	SE	p	β	SE	p	β	SE	p	β	SE	p
Female	-.290	.413		.072	.256		-.317	.479		-.393	.481	
Child gender * mother's education												
(Comparison: female, university)												
-- female * less than high school	1.517	.718	*				1.364	.845		1.640	.949	°
-- female * high school degree	-.016	.578					.335	.669		.532	.684	
-- female * high school or more	.339	.587					.393	.660		.294	.701	
Child gender * household poverty				.005	.490		-.484	.620		-.557	.688	
Mother's age at birth							.136	.052	*	.485	.308	
(Mother's age at birth)^2										-.007	.006	
Mother had partner at birth										.696	.567	
Low birth weight (y/n)										-.052	.517	
Birth order of child										-.027	.143	
Child of opposite gender in HH (y/n)										-.133	.460	

Notes: SE = standard error, p = significance level: (°) <.1 (*) <.05
Data: Add Health Genetic Sample

Table 11: Comparison of Pooled Logistic Regression Models: All Cases vs. Twins Excluded; Dependent Variable: Breastfeeding Continuation for 6 Month or Longer (y/n)

	Model 1a			Model 1b			Model 2a			Model 2b		
	β	rSE	p	β	rSE	p	β	rSE	p	β	rSE	p
Intercept	-2.241	.944	*	-2.240	.951	*	-3.020	1.064	*	-2.990	1.074	*
Sibling-invariant												
Houshold (HH) poor, y/n	-.144	.139		-.218	.201		-.191	.149		-.265	.217	
Mother's education												
(... vs. university degree)												
-- no high school degree	-.998	.166	*	-1.010	.235	*	-.948	.182	*	-1.006	.257	*
-- only high school	-1.035	.129	*	-1.176	.185	*	-1.067	.141	*	-1.265	.206	*
-- more than high school	-.511	.118	*	-.500	.163	*	-.582	.132	*	-.608	.186	*
Black/Afr.Am., y/n	-1.423	.182	*	-1.425	.182	*	-1.393	.198	*	-1.395	.198	*
Number of full siblings	.240	.046	*	.241	.046	*	.220	.049	*	.222	.049	*
Sibling-variant												
Mother partnered at birth, y/n	.206	.157		.206	.158		.206	.168		.204	.168	
Mother's age at birth	.083	.071		.086	.071		.152	.080	.	.156	.080	°
(Mother's age at birth)^2	-.001	.001		-.001	.001		-.002	.001		-.002	.001	
Gender of child = female	.041	.091		-.039	.166		.000	.100		-.146	.187	
Low birth weight, y/n	-.603	.151	*	-.607	.151	*	-.828	.215	*	-.836	.214	*
Birth order of child	-.181	.057	*	-.182	.057	*	-.205	.064	*	-.206	.064	*
Child is part of twin dyad, y/n	-.890	.127	*	-.896	.127	*						
Opposite sex child in HH, y/n	-.001	.098		.000	.098		.026	.108		.026	.108	
Interaction effects												
Education x gender												
-- less than HS x female				.027	.311					.115	.338	
-- high school x female				.269	.253					.379	.278	
-- more than HS x female				-.027	.231					.043	.259	
HH poverty x female				.146	.276					.149	.298	
N	3611			3611			2643			2643		
Resid. deviance	3042.85			3040.77			2462.7			2460.01		
AIC	3072.85			3078.77			2490.7			2496.01		
Adj. McFadden	.18			.18			.16			.16		

rSE = robust standard errors, p = significance levels: (°) <.1 (*) <.05
Data: Add Health Genetic Sample

Table 12: Conditional Logistic Fixed Effects Regression; Dependent Variable: Breastfeeding Continuation for Six Months or Longer (y/n) (Twins Excluded)

	Model 1			Model 2			Model 3			Model 4		
	β	SE	p	β	SE	p	β	SE	p	β	SE	p
Female	-.339	.429		-.005	.254		-.471	.466		-.540	.471	
Child gender * mother's education												
(Comparison: female, university)												
-- female * less than high school	.253	.773					-.179	.876		-.269	.884	
-- female * high school degree	.175	.620					.473	.701		.483	.716	
-- female * high school or more	.698	.607					.749	.665		.704	.686	
Child gender * HH poverty				.151	.583		.316	.685		.066	.715	
Mother's age at birth							.164	.058	*	.429	.346	
(Mother's age at birth)^2										-.005	.006	
Mother had partner at birth										1.327	.745	°
Low birth weight (y/n)										-.807	.673	
Birth order of child										-.040	.173	
Child of oppos. gender in HH (y/n)										-.104	.622	

Notes: b = log odds estimates, SE = standard error, p = significance level (°) <.1 (*) <.05
Data: Add Health Genetic Sample